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 reform in Denmark that altered old-age benefit payouts through a discontinuous increase in pension eligibility ages contingent on birthdate. Using detailed administrative data and a regression discontinuity design, we identify the causal effects of the policy, leveraging our setting to study essentially the entire financial portfolio. We document responses over two distinct time horizons. First, we show a lack of responses after the reform was announced but before it was implemented, inconsistent with the notion that future differences in pension eligibility impact savings. Second, we show large savings responses after implementation, when delayed benefit eligibility induces individuals to extend employment. Specifically, we find increased contributions to both employer-sponsored and personal retirement accounts, whereas we find no evidence of adjustments to other savings vehicles, such as bank or stock market accounts. Additional analyses point to inertia as a leading explanatory channel. The increased savings in personal retirement plans is entirely driven by those who made consistent contributions in the past. Moreover, the increased savings in employer-sponsored plans is largely explained by continuing to contribute at employer default rates, highlighting a role for firm policies in mediating responses to social security reform.

Keywords: social security, private savings, pension reform
JEL codes: H55, D14, J26

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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.

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 wealth. 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 assets. Second, identification requires a compelling source of exogenous variation in 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 pension benefits. We exploit the policy change to identify causal effects, estimating discontinuities in outcome variables by birthdate, and we exploit the breadth of our detailed data to study separately the effect of the reform on several types of savings vehicles.

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 differences in benefit eligibility). 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 critical 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, since it is during these years that our analysis sample navigates through the early retirement program. Note the data are not yet available to study behaviors around the FRA.[1]

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 claiming 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. 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 or economically meaningful savings responses in anticipation of reaching pension eligibility ages. There is no evidence that individuals adjust savings through employer-sponsored retirement plans (analogous to 401(k)s), personal retirement plans (analogous to IRAs), bank accounts, stock market investments, or property wealth. These results are inconsistent with lifecycle models that call for forward-looking adjustments to savings after the announcement of the reform in response to future differences in pension benefit payouts.[2]

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 \$6,117 (13%). We find concurrent and large increases in contributions

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[1] The birth cohorts we study are age 65 in 2019, and our data extend through 2018.

[2] This takeaway is broadly consistent with recent work that focuses on labor supply and earnings in the context of pension reform and finds a lack of forward-looking responses (Gelber et al. 2016 and Haller 2019).

to employer-sponsored retirement accounts, amounting to \$765 (15.5%) on average, that accompany this increase in earnings. We also find significant impacts on personal retirement accounts, as individuals are 3.9 percentage points (30%) more likely to contribute to these plans. 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, earnings rise by 15%, contributions to employer-sponsored plans rise by 19%, and the likelihood of contributing to personal plans rises by 24%.

In contrast, during the non-critical years of 2015, 2017, and 2018, when the strong incentives for delayed retirement are not present, we find muted or null responses in earnings and savings in retirement accounts. Moreover, we consistently find no evidence of savings responses through any other financial vehicles, perhaps most notably bank accounts and stock market investments, in any year. That is, our results indicate savings respond *only* when the treatment group is induced to delay retirement to comply with the new pension eligibility ages and *only* in traditional retirement accounts, which are specifically earmarked for consumption in retirement.

What can explain our findings? To investigate mechanisms, we conduct a series of additional analyses, and the overall body of evidence points to inertial behavior. We first provide evidence against two alternative explanations for the lack of anticipatory responses. It is unlikely that a complete lack of awareness can explain the null responses after the reform is announced, as we show the policy was well-publicized and prompted large increases in relevant Google search activity. We also rule out an inability to respond as a leading explanatory channel; we find no evidence of anticipatory responses even for a subsample of individuals who have room to adjust contributions to voluntary retirement savings accounts and who may be more financially sophisticated.

Next, we unpack the positive savings responses in both personal and employer-sponsored retirement accounts during the critical years of extended employment, and we find evidence supporting inertia. Consistent with the reform leading to the continuation of previous savings behaviors, we show that the increases in contributions to personal retirement plans are entirely driven by those who had made frequent contributions to the accounts in the past. We then 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 et al. 2016) where unions, employer associations, and firms have a major influence in setting contribution rates. We show how these types of policies can dictate responses to a national reform.

Taken together, our results show that in response to increases in pension eligibility ages, individuals extend employment and accumulate more savings. The lack of anticipatory responses, the lack of responses during non-critical years, the lack of adjustments to savings outside of retirement accounts, and the continuation of savings behaviors within retirement plans exhibited before the reform suggest inertia as the most likely mechanism.

Our paper relates most directly to the important literature that studies how private savings respond to the provision of public pension benefits.[3] 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 et al. 2006, Aguila 2011, Feng et al. 2011, and Lachowska and Myck 2018).[4] Finally, using an RD design, Lindeboom and Montizaan (forthcoming) study how retirement expectations, retirement realizations, and savings decisions respond to a composite reform in the Netherlands which reduced pension wealth.

Our approach is to hone in on one prominent component of the major reforms studied in the literature—namely changes in pension eligibility ages—and to unpack the causal effects of this type of policy change on savings through the lens of a standard lifecycle framework.[5] In doing so, we make several contributions. We provide novel evidence on how savings respond

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[3]A second related literature studies pension eligibility ages and labor supply (e.g., Mastrobuoni 2009, Behaghel and Blau 2012, Staubli and Zweimüller 2013, Manoli and Weber 2016, Lalive et al. 2017, Geyer and Welteke 2019, Haller 2019, Nakazawa 2019, Deshpande et al. 2020, and Geyer et al. 2020). Our analysis also connects to the general literature on social security and retirement incentives, as reviewed by Krueger and Meyer (2002) and Blundell et al. (2016). For instance, Burtless and Moffitt (1985), Asch et al. (2005), Coile and Gruber (2007), Liebman et al. (2009), Brown (2013), and Manoli and Weber (2016) similarly analyze nonlinear budget constraints from pension systems.

[4]For cross-country empirical analyses on the topic, see Kapteyn and Panis (2005), Disney (2006), Hurd et al. (2012), and Alessie et al. (2013). In the context of Denmark, perhaps the most related findings come from Chetty et al. (2014), who find that a government mandatory savings program from 1998 to 2003 did not crowd out other savings among low-income individuals.

[5]Most similar to our empirical approach is Nakazawa (2019), who studies primarily how raising pension eligibility ages in Japan impacts labor supply but also uses survey data to study the stock of total savings.

to increases in pension eligibility ages using a compelling RD design and population-wide administrative data. We additionally leverage our data to analyze separate measures of third-party reported assets throughout essentially the entire financial portfolio, whereas the literature has been restricted to using survey measures of savings such as self-reported income minus self-reported consumption. We view this innovation as an important step forward, as different types of vehicles for savings are likely to differ in the extent to which they serve as natural substitutes for public pension wealth. We highlight in particular the distinction between retirement accounts and other savings, itself the subject of a related strand of literature.[6] Furthermore, we provide a more comprehensive exploration of mechanisms. Our panel data allow us to study how responses differ by previous savings behaviors, and we use employer-employee linkages in our data to highlight how firms can mediate individual-level savings responses to social security reforms.

Overall, our results have broad implications for social security policy and models of household behaviors. First, we find that 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. Second, our results lend support to models that give rise to inertia in savings behaviors, such as those including fixed costs of adjustment, and they underscore a tight link between savings and employment. Third, our study emphasizes the importance of considering interactions with firm policies, such as employer retirement savings programs, when designing and predicting the effects of public policies.

The rest of this paper is organized as follows. Section 2 provides an overview of the policy environment. Section 3 discusses the economic incentives and grounds our empirical work with a benchmark model. Section 4 describes the data. Section 5 lays out our regression discontinuity design. Section 6 presents the main results, documenting the causal effects of the reform. Section 7 investigates underlying mechanisms and discusses potential explanations for our findings. We conclude in Section 8.

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 before describing

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[6]For earlier work on the relationship between tax-advantaged retirement accounts and total savings, see, e.g., Poterba et al. (1996), Engen et al. (1996), and Bernheim (2002). For more recent papers, see Gelber (2011), Chetty et al. (2014), and Andersen (2018).

the policy reform. More background information can be found in Appendix B.

2.1   Private Retirement Savings Accounts

As is typical of other modern economies, defined-contribution private retirement savings accounts dominate the retirement savings landscape in Denmark and 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.[7]

Broadly speaking, in Denmark participation in employer-sponsored retirement savings plans 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 retirement savings accounts, and so 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. In contrast, contributing to personal retirement savings plans is completely voluntary.

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, and the majority of workers—about 70% of the individuals in the birth cohorts we study—choose to participate. We focus our study on those participating in the VERP program, as it has historically played a major role in determining labor supply and retirement patterns of the Danish population, and as those not participating in VERP only just became eligible for the OAP in 2019 (for which the

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[7]Our analysis focuses on these traditional retirement accounts. In 2013, Denmark introduced "Roth-style" retirement accounts to the economy. Contributions to these plans are not tax deductible, but distributions are not taxed. For completeness, we study these types of accounts in the appendix, though overall they are likely to make up a much smaller fraction of the financial portfolio for the birth cohorts we study, who were 59 years-old when the accounts were first introduced.

data does not yet exist). The two programs are closely connected; however, the provision of benefits from each program are governed by different rules and regulations.

2.2.1 Voluntary Early Retirement Pension

The VERP program grants participants access to 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 very 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 VERP, at which point they lock in their annual base benefits for the duration of the program. Benefits amount to roughly $27,000 (in 2010 U.S. dollars), which are then subject to strict means testing.[8] First, base benefits for the duration of the program are reduced against wealth held in private retirement accounts right before reaching age 60.[9] 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 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

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[8]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.

[9]The government collects information on retirement account balances for VERP-eligible individuals around age $59\frac{1}{2}$, and the $27,000 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.

payouts against drawdown income and hours worked remain.[10] 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 annual, flat-rate, old-age benefits until death. The key idea for our study is that OAP wealth largely does not depend on retirement age. 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, though 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 stipulated the phasing in of stepwise 6-month increases in pension eligibility ages, contingent on birthdate. Figure 1 graphically illustrates how the reform indexed each of the three key 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.[11]

Specifically, in our analysis we exploit the fact that those born on January 1, 1954 learn in 2011 that their ERA has increased to age $60\frac{1}{2}$, that their incentivized retirement age has increased to age $62\frac{1}{2}$, and that their FRA has increased to age $65\frac{1}{2}$. In contrast, those born one day earlier, on December 31, 1953, experience no change in their pension eligibility ages, which remain constant at 60, 62, and 65. Our identification strategy exploits the

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

[11]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, slightly increased the standard base benefit amounts, and implemented even stricter means testing policies against assets held in private retirement accounts. Importantly, all of these changes were phased in to impact later birth cohorts, and none of them affect the individuals at the birthdate discontinuity that we study.

discontinuous nature of the policy change; individuals born right around the birthdate cutoff should be similar in all aspects, yet face different retirement and savings incentives due to the reform.

3 Economic Framework

We use a simple lifecycle framework to model key features of the pension system as well as the changes in incentives brought on by the 2011 reform. Building directly on Laitner and Silverman (2007) and Hurd et al. (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 initial setup from Hurd et al. (2012). Consider economic agents making decisions throughout continuous time $t \in [0, T]$. Agents 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, we assume the rate of time preference, ρ, equals the interest rate r.

Formally, agents 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

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

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

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

Since we assume the utility discount rate equals the interest rate, individuals should perfectly smooth consumption. 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 time 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 setup 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. Figure 2 illustrates this notion graphically by plotting public pension wealth against retirement age for a representative worker from the pre-reform birth cohort.[12] 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.[13] Retiring right at 60 discontinuously increases pension

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[12]For illustrative purposes, we abstract from discounting, and the benefit amounts depicted in the figure are for a worker who is married, who lives until age 85, and who has $250,000 in private retirement savings accounts at age 60.

[13]The y-intercept in the stylized graph is $300,000, which corresponds to 20 years (from age 65 to 85) of

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.[14]

The spikes in pension wealth at the critical ages translate to large discontinuities in life-time consumption, C^L. Graph (a) of Figure 3 plots lifetime consumption against retirement age, for the same representative worker from the pre-reform cohort.[15] The discontinuities at 60 and 62 should induce bunching in the retirement distribution, as those who would have otherwise located either just to the left or just to the right of these ages find it optimal to retire right at the critical ages.[16]

We let the data speak to the strength of these bunching incentives in our setting. Graph (a) of Figure 4 plots the empirical distribution of retirement ages for those born before the January 1, 1954 birthdate cutoff.[17] There are few retirements before the ERA, 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 Modeling the Reform: Benchmark Predictions

The 2011 reform increased pension eligibility ages. In the context of 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 $62\frac{1}{2}$, which changes the budget constraint as depicted by the maroon line in graph (b)

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standard OAP benefits ($15,000 per year).

[14]The negative slopes between 60 and 62 and between 62 and 65 result from the lack of actuarial adjustments when deferring claiming. Pension wealth for those who retire after age 65 is greater than just the OAP wealth due to quarterly bonus payments for working past age 62 (see Appendix B). Note the size of each spike depends on assets held in retirement accounts; the greater the balances in retirement accounts, the smaller the first spike (due to more reductions in base VERP benefits) and the larger the second spike (due to greater gains from avoiding the means testing).

[15]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.

[16]Note that incentive-induced bunching in retirement is not unique to the Danish system. 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; similarly, Manoli and Weber (2016) study bunching at the early retirement age in Austria. For a general review of the bunching literature, see Kleven (2016).

[17]Details on the monthly data used to produce this graph can be found in Section 4; the underlying sample consists of workers born within six months of January 1, 1954. Retirement ages are defined using an absorbing state measure. 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.

of 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 retirement distribution. Finally, guided by these responses borne out in the data, we use our framework to assess how savings should respond.

Given the strong retirement incentives attached to VERP pension eligibility ages, we expect the dominant forces at play to essentially shift 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 retirement ages in the data.

Graph (b) of Figure 4 shows how the empirical distribution of retirement ages shifts after the reform. The maroon line depicts the behavior of those born after the January 1, 1954 birthdate cutoff, who are affected by the reform and face budget constraints corresponding to the maroon lines in graph (b) of Figure 3. The graph shows how the reform clearly induces a 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 that are consistent with the lifecycle model. 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. The model calls for this extra income to be spread over the lifecycle in the form of increased consumption in every period. This change in the consumption profile yields two 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 earnings during this period are unchanged but consumption has increased. Second, during the reform-induced periods of extended employment (e.g., between ages 60 and $60\frac{1}{2}$), savings should *increase* on average. Consumption is still elevated, but income is higher from continued employment, and the increase in consumption cannot be greater than the increase in income; some of the extra income should be saved to finance increased consumption throughout later stages of the lifecycle.

4 Data

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

We have also gained access to a complementary, monthly-level administrative dataset that contains information on all employees in Denmark from 2008 to 2017.[18] We use these data to more finely track exits from the labor force and to conduct the bunching analysis of retirement ages 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, and thus form the ideal dataset for studying our research question. We avoid potential problems associated with using self-reported savings or imputed savings from self-reported income and consumption as outcome variables, and we exploit our data to study separately retirement savings accounts, bank accounts, stock market investments, and property values.

We observe flow variables that capture savings in traditional defined-contribution retirement accounts, which make up a dominant form of private saving in the economy and which might naturally be considered the closest substitutes to public pension wealth. We study as our main outcomes contributions to employer-sponsored accounts in levels and indicator variables for making positive contributions to personal accounts.[19] We also study annuitized distributions from these retirement accounts, but we are unable to distinguish between payments from employer-sponsored plans and personal plans. We winsorize contri-

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[18]This dataset, known in Denmark as the *eIncome* register, contains information on wages and salaries that firms report to tax authorities at a monthly frequency. See Kreiner et al. (2016) and Kreiner et al. (2017) for more discussion on this relatively new dataset.

[19]Our focus on extensive-margin responses to personal accounts is particularly informative in its own right, because contributions to personal plans are completely voluntary and thus less common than contributions to employer-sponsored plans. Mean contribution amounts in levels are often dominated by the large number of zeros. In Section 6, we discuss our approach to investigating contribution amounts to personal plans by using as outcomes indicators for making contributions of various sizes.

bution amounts at the 95th percentile, by year, in order to reduce the influence of outliers in our regressions, improve precision, and account for occasional observations of recorded contributions well-above annual contribution limits.[20]

For savings in bank accounts, stock market accounts, and property, we do not observe flow variables, but rather stock variables. Specifically, our measures of bank account balances and stock market account balances 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 corresponds to the year-end cash value of properties as assessed by the tax authorities directly. We use these measures to compute more noisy flow variables of savings in year t by subtracting year-end balances in year t with those from year $t-1$. We thus study changes in bank account balances, changes in stock market accounts, and changes in property values as our main outcomes. 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.[21]

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 for consistency. 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 amounts. 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. Specifically, starting with our data on the entire Danish population from 1985 to 2018, we carry out four main sample

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[20]Our analysis focuses on traditional retirement plans, though for completeness we analyze indicators for contributing to "Roth-style" retirement plans as well, in the appendix. As discussed in Section 2, Roth-style accounts were introduced to Denmark in 2013, when our analysis sample is 59 years-old, and thus likely form a substantially smaller part of the asset portfolio for the individuals we study.

[21]Still imprecision can present a challenge when studying these variables that capture changes in year-end assets within individuals, especially in relatively smaller samples. This general problem is discussed in more detail in Chetty et al. (2014); we follow their approach by additionally studying even more strictly winsorized versions of these outcome variables, at the 10th and 90th percentiles.

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.[22] Third, we balance the sample between the years 2006 and 2018. Fourth, we exclude the self-employed (defined during the pre-announcement period), who are subject to different rules and regulations concerning their early retirement options through the VERP scheme.

We are left with a sample of 40,042 individuals. Table 1 presents summary statistics for calendar year 2010, the year before the reform is announced. Columns (1) and (2) display the mean and standard deviation of key variables for the entire analysis sample. Columns (3) and (4) provide the same information for the 12,020 individuals who will ultimately make up the main estimation sample in our RD design, namely those born within 56 days (8 weeks) of the January 1, 1954 birthdate cutoff. Our sample contains active older workers, most of whom are married. Average earnings in 2010 amount to approximately $61,000. 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 just over $7,000.

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.[23] We derive identification from the policy-induced 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 birthdate cutoff.

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[22]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.

[23]Imbens and Lemieux (2008) and Lee and Lemieux (2010) provide reviews of RD designs in economics.

Specifically, to implement our RD design, we estimate equations of the following form:

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

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 of January 1, 1954, Z_i is a vector of pre-determined control variables, f and g are functions, 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 estimate separate linear polynomials in the running variable on either side of the cutoff, we use triangular weights, and we include as controls gender, pre-announcement marital status, and pre-announcement region of residence.[24] We choose our bandwidth to be eight weeks, or 56 days, on either side of the cutoff.

We probe the robustness of our results to these specification choices and discuss corresponding results in Section 6.3. In particular, we vary the bandwidth, drop the triangular weights, exclude controls, and estimate global linear polynomials in the running variable.

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 of the running variable, 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 long before the policy is announced. A separate 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

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[24]We control for pre-announcement marital status using a dummy variable for being married or cohabiting in 2010. We control for pre-announcement region of residence using dummy variables for residing in 2010 in each of the five administrative regions of Denmark: Hovedstaden (the capital region containing Copenhagen), Sjælland, Syddanmark, Midtjylland (containing Aarhus), and Nordjylland.

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. 2010, 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 superimposed 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 et al. (2019) using our baseline choice of bandwidth results in a p-value of 0.97. Overall, we fail to find evidence indicating the presence of any problematic discontinuity in the density of the running variable at the birthdate cutoff.

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.1 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 aggregate causal effects of increasing pension eligibility ages. We often lead with standard RD graphical analyses, which offer nonparametric representations of the causal effects of the reform. Specifically, we plot means of key outcome variables in one-week date-of-birth bins for individuals born around the birthdate cutoff, and we superimpose on these plots regression lines from estimating separate linear trends in the running variable for observations on either side of the cutoff. We then use regression-based estimates to quantify magnitudes and assess the statistical significance of our findings.

6.1 Anticipation Period

We begin our analysis by documenting impacts during 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 any anticipatory savings responses though. Figure 5 illustrates this result graphically. Each graph corresponds to a different key outcome variable, where the variables of interest are averaged over the anticipation time period. For instance, graph (a) illustrates the RD estimate of the policy reform on average annual contributions to employer-sponsored retirement accounts between 2011 and 2013. Over this time period, average annual contributions to these types of accounts were around \$6,000 for the control group, and the graph shows no evidence of any discontinuous change in this outcome variable at the birthdate cutoff. Graph (b) shows no impact on contributions to personal plans, where here the extensive-margin outcome variable is the fraction of years contributing to personal plans. Likewise, graphs (c) through (e) show a lack of savings responses through changes in bank account balances, stocks market investments, and property wealth, respectively. Graph (f) shows that there are also no discontinuities in earnings over this time horizon. Overall, the graphs make a strong visual case for a lack of savings responses. The pattern of the binned means indicate that the savings of those born just to the left of the cutoff look no different than the savings of those born just to the right.

Table 2 presents results from corresponding regression analyses. We report in the table RD estimates of β from estimating equation (6) using our baseline specification. Not only are the point estimates statistically indistinguishable from zero, they are also economically insignificant. The point estimate on employer-sponsored retirement accounts, for example, is a positive \$20.32, which at face value represents a 0.33% increase off of the control group mean. The point estimate for contributions to personal retirement plans is small and positive, whereas the estimates for other savings vehicles are negative in sign, but small. To attempt to gain more precision, we follow Chetty et al. (2014) and further winsorize our non-retirement account savings outcomes at the 10th and 90th percentiles, and we report the results in Appendix Table A.6. The first row presents the RD estimates for the anticipatory responses, which are very similar to our baseline results and more precise.

In general, a lack of anticipatory responses is not consistent with the notion that current savings respond to changes in future pension eligibility. We discuss potential explanations

and underlying mechanisms for these results in Section 7, after first establishing the causal effects of the reform over the early retirement period, which then allows us to assess and discuss the overall body of evidence as a whole.

6.2 Early Retirement Period

Here we 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 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 these two years "critical years," as they are the years during which 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, the first year during which 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 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 just over $6,000, which is a 13.7% increase off of a baseline mean of $44,449. These results are entirely consistent with the delayed retirement documented in Figure 4.

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 $765 to these retirement plans, which represents a meaningful 15.5% increase off of a mean of $4,928. Graph (d) illustrates how the treatment group is also 3.9 percentage points, or 27.9%, more likely to contribute to personal retirement accounts. Both of these point estimates are highly statistically significant, and the RD graphs provide visually compelling evidence that the reform causes individuals to save more in retirement accounts during the first critical

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year of policy-induced extended employment.

As mentioned in Section 4, we lead our analysis of contributions to personal plans with a binary indicator for contributing any positive amount. The large number of individuals contributing zero dollars makes it difficult to study contribution amounts in levels (see graph (a) of Appendix Figure A.2). To overcome this challenge, we use as outcomes indicators for making contributions of various sizes to personal plans. Specifically, we split the (pre-reform) 2010 distribution of positive contributions into quartiles, and use as outcome variables indicators for contributing between \$1 and \$Q, where Q corresponds to the relevant quartile. Graph (b) of Appendix Figure A.2 illustrates our approach. The histogram depicts the 2010 distribution of positive contributions, and the maroon vertical lines designate the quartiles used to define contribution bins. In graph (c), we plot the RD estimates and confidence intervals from estimating equation (6) on indicators for the various contribution amount bins. The point estimate furthest to the left mirrors the result in graph (d) of Figure 6: the policy causes a 3.9 percentage point decline in the likelihood of contributing \$0 to personal retirement plans. The subsequent point estimates show how in 2014 the reform caused increased contributions of meaningful amounts. The pattern of the point estimates, which are increasing as the contribution amount bins increase, suggests that the treatment group is more likely to make contributions of all sizes (except perhaps those over \$5,200).

We present regression-based results for all main outcomes in column (1) of Table 3. The reform not only results in greater contributions to both employer-sponsored and personal retirement accounts, it also leads to a decrease in annuitized distributions received from retirement accounts. Treatment individuals receive payments from retirement accounts that are about \$263 (16.6%) less on average.[25] Panel (c) of Table 3 reports RD estimates for the other savings outcomes we study.[26] None of the estimates are statistically distinguishable from zero. The second row of Appendix Table A.6 shows how additional winsorizing of these outcome variables produces small point estimates that are closer to zero and more precisely estimated. Overall, results from the first critical year show that in response to the increases in pension eligibility ages, individuals earn more from continuing to work, and this extended employment results in the accumulation of more savings in retirement accounts, whereas there is no evidence of adjustments to other types of savings.

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[25]Recall from Section 4 that we unfortunately cannot distinguish between distributions from employer-sponsored and personal accounts.

[26]Results from analyzing indicators for contributing to Roth-style accounts, which were first introduced to the economy in in 2013, are reported in Appendix Table A.2; we find no evidence that the reform impacts contributing to these types of accounts (which likely make up a much smaller fraction of the retirement portfolio) in any year.

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 2015; only one point estimate appears statistically distinguishable from zero.

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. Similar to the first critical year, during 2016, treated individuals receive less VERP benefits and have 15.4% higher earnings. The extended employment again leads to more savings in retirement accounts: contributions to employer-sponsored plans increase by 18.8% and the likelihood of contributing to personal plans rises by 24.5%. Graph (d) of Appendix Figure A.2 suggests that the increased contributions to personal plans are primarily contributions under $2,200. The point estimate on distributions from retirement accounts is negative and similar to the one in 2014, though more imprecisely estimated in this year. We again find no evidence of savings responses through bank accounts, stock market accounts, or property, as the main RD estimates (as well as those subject to more stringent winsorizations reported in Appendix Table A.6) are statistically indistinguishable from zero.

Finally, in columns (3) and (4) 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.[27]

Before moving on to further unpack our main results and 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.

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[27]The point estimates in 2017 and 2018 for changes in bank account balances are fairly large (around $600) but imprecisely estimated and statistically insignificant; additional winsorizing yields smaller point estimates (see Appendix Table A.6).

6.3 Robustness and Specification Checks

We probe the robustness of our results along several dimensions by estimating our RD using various alternative specifications. We report results for the main outcomes in Appendix Table A.3 (for the anticipation period), Appendix Table A.4 (for critical year 2014), and Appendix Table A.5 (for critical year 2016). The tables are constructed as follows. Each row indicates an alternative specification, and each column corresponds to a different outcome variable. Row A reproduces baseline estimates. In rows B through E, we vary the bandwidth, both increasing and decreasing the size of the bandwidth in one-week intervals. In row F, we use a global linear polynomial rather than separate linear polynomials on either side of the cutoff. In row G, we exclude controls, and in row H, we do not use triangular weights.

Overall, our results are stable. The point estimates for outcomes over the anticipation period are broadly similar to one another and never statistically distinguishable from zero. The point estimates during the critical years do not appear sensitive. The estimates for earnings as well as contributions to retirement accounts are almost always highly statistically significant and do not fluctuate meaningfully with specification choices, and the point estimates for other savings outcomes are never statistically distinguishable from zero.

6.4 Placebo Exercises

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 Figure A.3 shows how our RD estimates for key outcome variables during each critical year 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 56 days on either side of the cutoff, the RD estimates corresponding to placebo cutoffs more than 56 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 contains a known discontinuity.

Finally, we replicate our entire analysis, but using placebo January 1 birthdate cutoffs for earlier birth cohorts who, to the best of our knowledge, are not impacted by policies that may result in discontinuities in outcomes as they age into the VERP program. 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.[28] Appendix Table A.8 reports the results; we find no evidence that being born just after these placebo January 1 cutoff dates impacts earnings or savings in retirement accounts at age 60 or 62.

7   Mechanisms

Taken together, the main results indicate deviations from benchmark theory and may point to inertial behavior as an underlying channel. We find that savings respond to the increase in eligibility ages only when the reform directly induces extended employment and only through retirement accounts. To explore mechanisms and directly assess the extent to which inertia might be driving the results, we first investigate the lack of anticipatory responses, and then we unpack the increases in contributions to retirement savings accounts during the two critical years.

7.1   Investigating the Lack of Anticipatory Savings Responses

Here we assess two natural alternative explanations for the lack of anticipatory responses other than inertia. First, it could be that a complete lack of awareness underlies the in-action: if individuals impacted by the reform are simply not aware of the changes to their eligibility ages until they reach age 60, then the lack of responses could be attributed to a deficiency of information. While we cannot rule out this explanation completely, we consider it an unlikely driving force behind the lack of anticipatory responses. In general, the major reform was well-publicized and a matter of political discourse. 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 altogether.[29] Overall, we view our setting as one in which general awareness was likely high. For some reference, Appendix Figure A.4 plots a Google search intensity index for "efterløn",

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[28]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).

[29]The prime minister of Denmark announced plans leading to the reform during his New Year's Day speech on the first day of 2011, while also suggesting an eventual elimination of the VERP program. Later phases of the reform make the entire scheme less financially attractive, and due to these changes, individuals wishing to opt out of the VERP program could in 2012 withdraw their contributions to the scheme. While likely a more attractive option for those younger than our analysis 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 the VERP scheme and shows a lack of responses along this potential margin.

which is the Danish word for the VERP program. The graph shows several large spikes in searches throughout the anticipation period.

A second candidate explanation could be the inability to respond. If "hand-to-mouth" or "wealthy hand-to-mouth" (Kaplan and Violante 2014, Kaplan et al. 2014) behavior is prevalent and individuals have little liquid financial assets, then it could be that they did not have room to adjust savings in response to the announcement of the reform. Two pieces of evidence suggest this is unlikely to be driving the null anticipatory responses in our context. First, average bank account balances for our analysis sample are relatively high (just over $26,000 in 2010) and constitute savings that are typically more liquid and easier to adjust. Second, we find no evidence of anticipatory responses when we estimate our RD using a subsample of individuals who are likely able to respond with more ease, namely those 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—but also have higher bank account balances on average ($35,535) and may be more financially sophisticated. We report the corresponding results in Table 5. Column (1) shows no evidence of any anticipatory savings responses in any of the savings vehicles we study for this subsample.

7.2 Investigating the Increased Savings in Retirement Accounts

We now turn to unpack the savings responses we find during the critical years, the large and meaningful increases in contributions to both employer-sponsored and personal retirement accounts.

7.2.1 Personal Retirement Savings Accounts

We start by investigating the increase in contributions to personal retirement plans. We study response heterogeneity by pre-announcement usage of these accounts. The goal is to assess whether the policy increases the likelihood of contributing 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 users of personal plans (who contributed in either 2 or 3 years between 2008 and 2010) and infrequent users (who contributed in either 0 or 1 year between 2008 and 2010). We then estimate our RD on contributing to personal plans in each critical year separately for each group, and we report results in Table 6.

Consistent with inertia and the continuation of previous savings behaviors, we find that the savings response is driven entirely by frequent users. The point estimates for frequent users represent increases of around 30% for each critical year, and indicate that the policy results in continued contributions during periods of policy-induced extended employment from those who had been contributing before the announcement of the reform. The point estimates for infrequent users are small and statistically indistinguishable from zero; there is no evidence the reform spurs these individuals to take up contributing to personal plans.

7.2.2 Employer-Sponsored Retirement Savings 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 has been shown to be especially true in Denmark (Fadlon et al. 2016), where there is additional evidence that individuals save passively and that employer-sponsored plans can play a key role in driving overall wealth accumulation (Chetty et al. 2014). In Denmark, collective bargaining agreements between unions and employer associations often stipulate minimum contribution rates for workers, and among those not covered by these agreements, firms often set 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-sponsored 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 informative exercises that directly investigation this question. To this end, we use our population-wide data to construct firms, and we proxy for employer default contribution rates using the median contribution rates at firms. All of our analyses center on firm contribution rates defined in 2010, the year preceding the announcement of the reform, so as to avoid defining firm characteristics of an individual based on, e.g., the endogenous choice of workplace in periods after the announcement of the reform.[30]

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[30]Our approach to constructing firms and inferring firm-default contribution rates broadly follows related strategies in Chetty et al. (2014) and Fadlon et al. (2016). We construct firms using our data on all individuals in Denmark; we keep individuals over 18 years of age and assign them to firms. We then compute individual-specific contribution rates by dividing contributions to employer-sponsored retirement accounts by labor market earnings. We infer the default contribution rate of the firm as the median contribution rate among individuals at the firm. Our sample sizes decrease slightly for these analyses due to our inability to define workplaces in 2010 for every individual in our sample; roughly 6% of individuals did not have positive labor

Graphical Anlaysis. First, we conduct a graphical analysis that compares deviations from employer default contribution rates, for our treatment and control group, before and after the reform. Figure 8 depicts the results. Each graph plots the distribution of deviations from default contribute rates. For example, the large spikes around zero in graph (a) show that individuals in both the treatment group and the control group tend to contribute at default rates; the fact that the two distributions lie on top of one other suggests that the propensity to deviate from the default rate did not differ by group in 2010, before the reform was announced. Graph (b) plots the same distributions during 2012; the graph shows no evidence that the behavior of the treatment and control group have diverged, despite the announcement of the reform. Graph (c) plots the distributions during 2014, the first critical year. The mass around zero has decreased more for the control group than the treatment group, with a corresponding rise in mass around negative ten percent, consistent with the control group beginning to retire and thus contributing less or not at all. (We note default contribution rates around 10% are common in Denmark.) In contrast, the mass of the treatment group remains higher around zero, suggesting they are more likely to still be contributing right around the default rate. The pattern continues in graph (d), the second critical year. This analysis points to an important role for employer defaults in shaping responses to the reform.

Regression Analysis. To better quantify the extent to which continuing to contribute at firm default rates can explain our findings, we conduct a regression-based analysis that compares actual contributions with predicted contributions according to default rates and earnings responses. Specifically, we define a new outcome variable, predicted contributions, as current earnings multiplied by the 2010 (pre-announcement period) 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 from responding to the reform by continuing to work at the same firm, which increases earnings, and continuing to contribute out of those earnings 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 the policy on actual contributions in 2014, but for the subsample of individuals for whom we could define firm default contribution rates in 2010. The subsample is 93.7% of our main RD estimation sample, and the $781 point estimate is very similar to our baseline estimate. Column (2) reports the estimate for the impact of the policy on predicted contributions in

market earnings in 2010.

2014, which is \$591. Taking these RD estimates at face value, the results indicate that in 2014, roughly $\frac{591}{781} = 76\%$ of the increase in contributions to employer-sponsored retirement accounts can be explained by continued contributions at firm default rates. Similarly, in 2016, the discontinuity in predicted contributions amounts to \$526, whereas the discontinuity in actual contributions is \$706, and thus firm default contribution rates can explain approximately 75% of the actual response during the second critical year. Overall, our results indicate that employers can play an important role in shaping how private savings ultimately respond to national social security reform.

8   Conclusion

In this paper, we provide novel evidence on the effects of increasing pension eligibility ages on private savings. We leverage rich, population-wide, linked employer-employee, administrative data on essentially the entire financial portfolio to study savings responses in a setting where strong labor supply incentives induce extended employment.

Our paper offers two main results. First, we find a lack of anticipatory responses, after the reform is announced but before it is implemented, inconsistent with the notion that future differences in pension eligibility impact current savings. Second, we find large and meaningful increases in contributions to retirement savings accounts—both personal plans and employer-sponsored plans—during periods of policy-induced extended employment. Then, through a series of additional analyses, we investigate mechanisms, and we view the overall body of evidence as pointing to inertia as a leading explanatory channel. In response to the reform, individuals continue working and continue saving in retirement accounts in a manner consistent with their behavior before the reform.

Our results carry important implications for policy. Pension eligibility ages are defining features of most social security systems, and similar reforms that increase eligibility ages have been enacted around the world in recent decades. A good deal of work investigates labor supply responses to these types of reforms, but understanding how raising eligibility ages will likely impact financial security throughout later stages of the lifecycle calls for an analysis of savings, a key resource used to finance consumption at older ages. We find that, in our setting, raising eligibility ages leads to longer working lives, increased earnings, and more private savings set aside in retirement accounts for shorter retirement time horizons.

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Figure 1: Pension Eligibility Ages by Birthdate
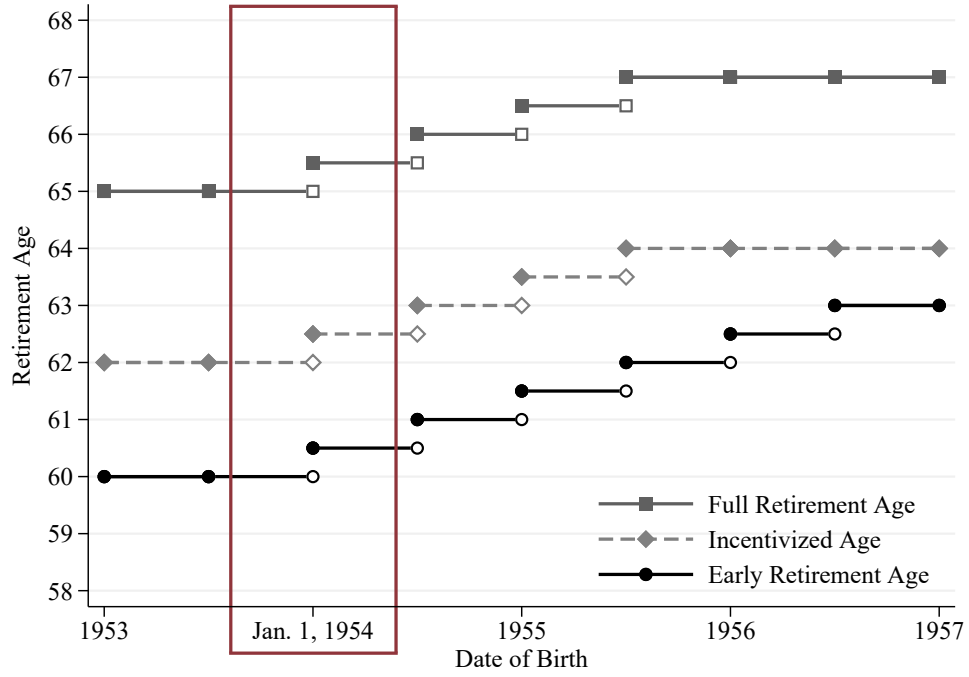
Notes: This figure graphically depicts the increases in pension eligibility ages due to the 2011 reform. Birth cohorts born before January 1, 1954 were unaffected by the reform. For these individuals, the key eligibility ages remained constant at 60, 62, and 65. Individuals born between January 1, 1954 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.
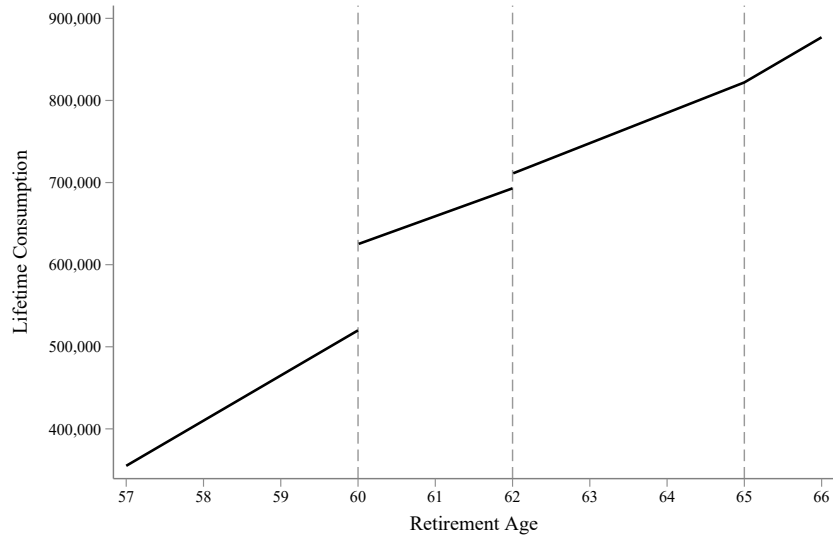
Figure 2: Pre-Reform Public Pension Wealth by Retirement Age
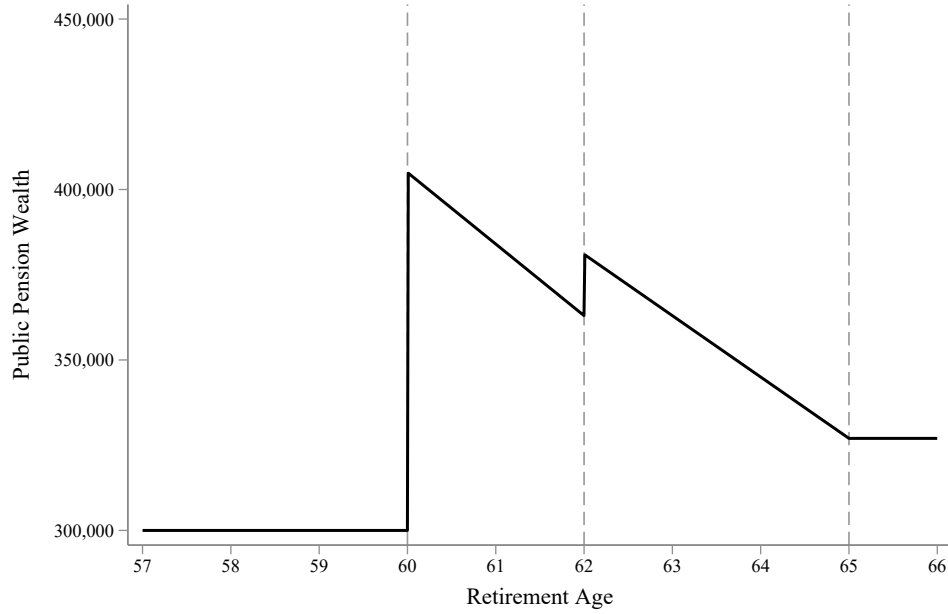
Notes: This figure plots public pension wealth against retirement age for a representative individual before the reform. For illustrative purposes, the benefit amounts depicted in the figure are for a worker who is married, who lives until age 85, and who has $250,000 in private retirement savings accounts at age 60. Note the *y*-intercept in the stylized graph is not zero, due to receiving OAP benefits after the early retirement program. The first spike in pension wealth at age 60 is due to the transition rule. Individuals retiring before 60 are not eligible to claim into the early retirement program and thus forfeit five years of early retirement benefits. The second spike in pension wealth at age 62 is due to the two-year rule. Retiring at age 62 eliminates the means-testing of early retirement benefits against private retirement savings accounts and produces higher benefits over the remaining three years of the early retirement program. The negative slopes between 60 and 62 and between 62 and 65 result from the lack of actuarial adjustments when deferring claiming. Pension wealth for those who retire after age 65 is greater than OAP wealth due to bonus payments for working past age 62 (see Appendix B).

Figure 3: Lifetime Budget Constraints

(a) Pre-Reform Budget Constraint

(b) Post-Reform Budget Constraints
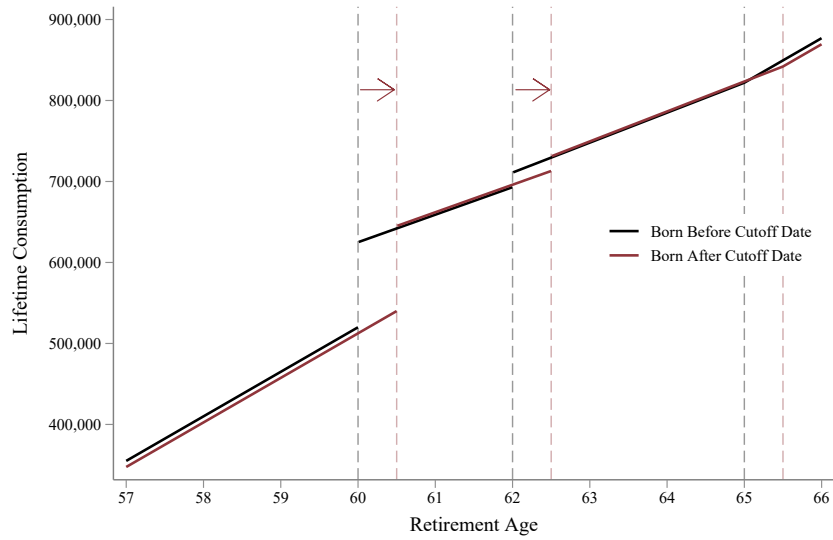
Notes: This figure plots lifetime consumption against retirement age for the same representative worker as in Figure 2. Lifetime consumption is the sum of public pension wealth and lifetime earnings. For illustrative purposes, annual earnings are assumed to be \$55,000 and lifetime earnings are earnings after age 57, the age of our sample when the reform is announced. Graph (a) depicts the lifetime budget constraint the worker faces before the reform. The spikes in pension wealth at age 60 and 62 translate to discontinuities in lifetime consumption. Graph (b) illustrates how the budget constraint changes due to the reform. If the worker was before the January 1, 1954 cutoff, the budget constraint is governed by the black line. If the worker was born on or after the cutoff, the budget constraint is governed by the maroon line. The key difference is the change in the location of the discontinuities in lifetime consumption.

Figure 4: Empirical Distributions of Retirement Ages

(a) Retirement Distribution for the Control Group
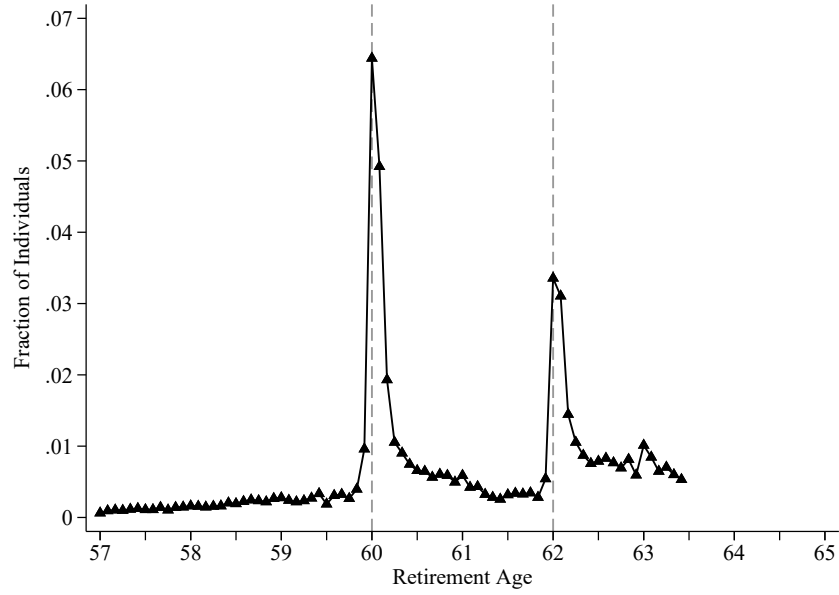
(b) Retirement Distributions for Treatment and Control Groups
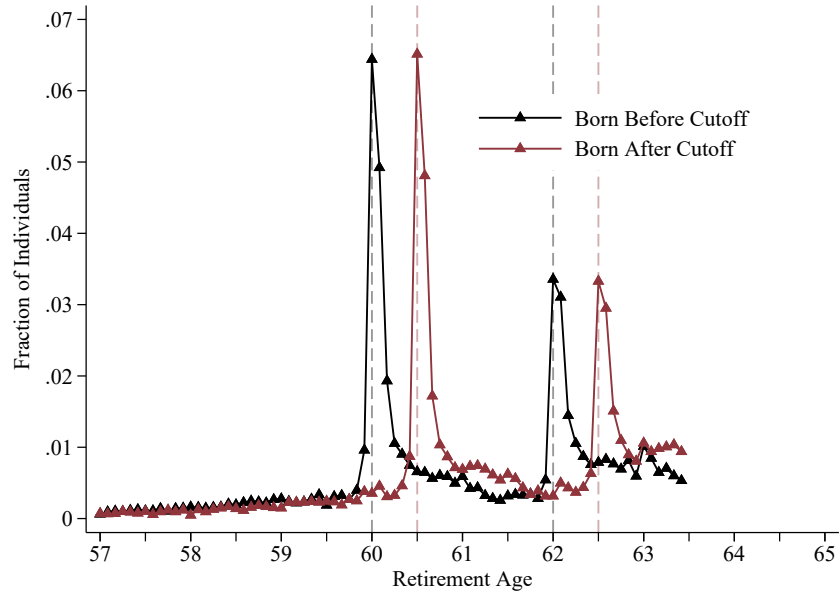
Notes: This figure plots empirical distributions of retirement ages. Retirement is measured as an absorbing state. Monthly retirement age is defined as the age of the individual in the last month during which earnings are positive, before permanently falling to zero. Graph (a) shows how those born before the January 1, 1954 birthdate cutoff tend to either retire right around 60 or 62. Graph (b) shows how, in response to the reform, those born after the birthdate cutoff tend to retire right around $60\frac{1}{2}$ or $62\frac{1}{2}$.

Figure 5: Responses Over the Anticipation Period

(a) Employer-Sponsored Plans

(b) Personal Plans

(c) Bank Accounts

(d) Stocks

(e) Property

(f) Labor Market Earnings
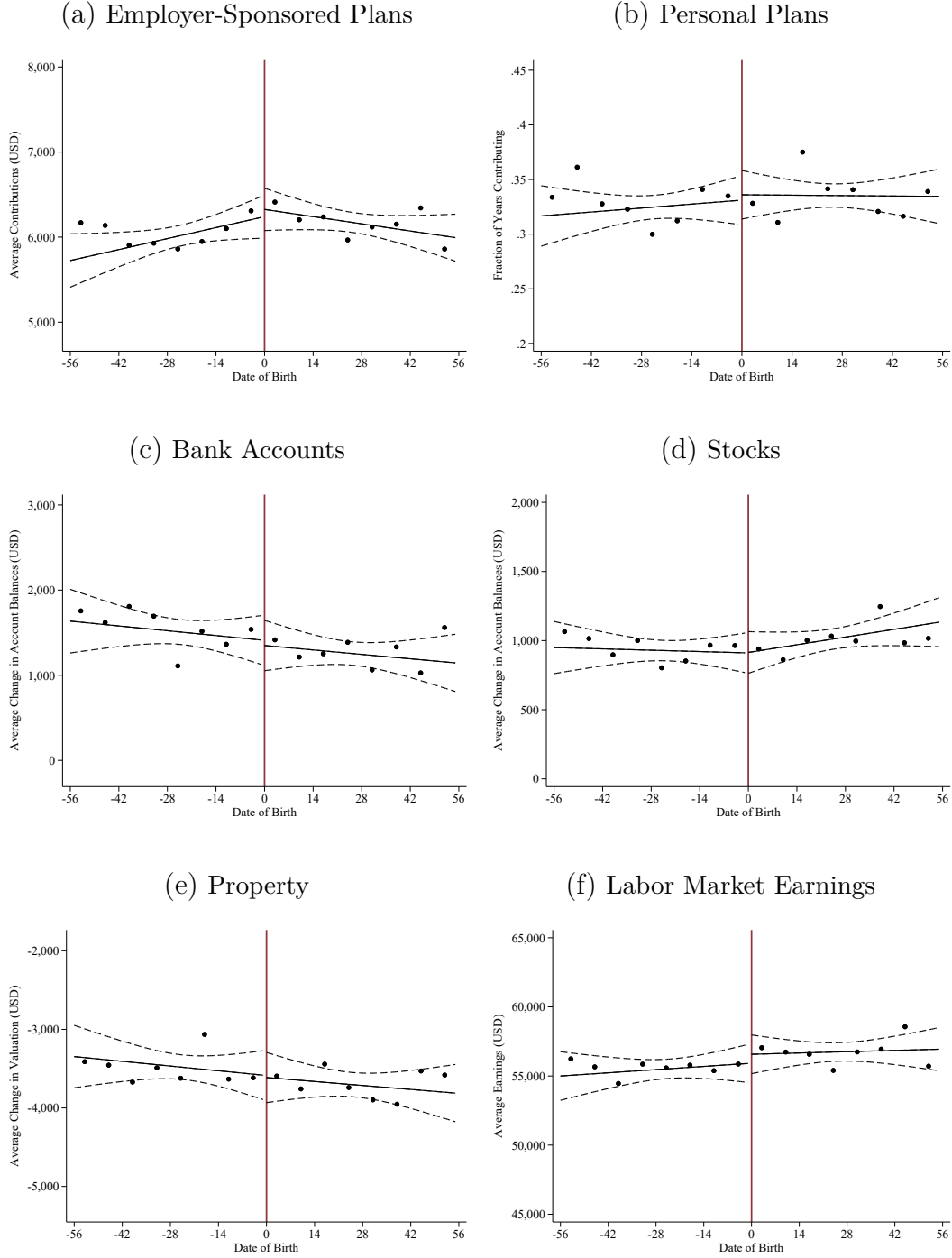
Notes: This figure illustrates the effect of the reform on key outcome variables over the anticipation time period. Each RD graph (a)–(f) corresponds to a separate outcome variable averaged over the three-year anticipation period, from 2011 to 2013. The graphs plot average outcomes in one-week date-of-birth bins. The maroon vertical lines designate the January 1, 1954 birthdate cutoff. The superimposed regression lines and 95-percent confidence intervals are based on the underlying unbinned data.

Figure 6: Responses During the First Critical Year 2014
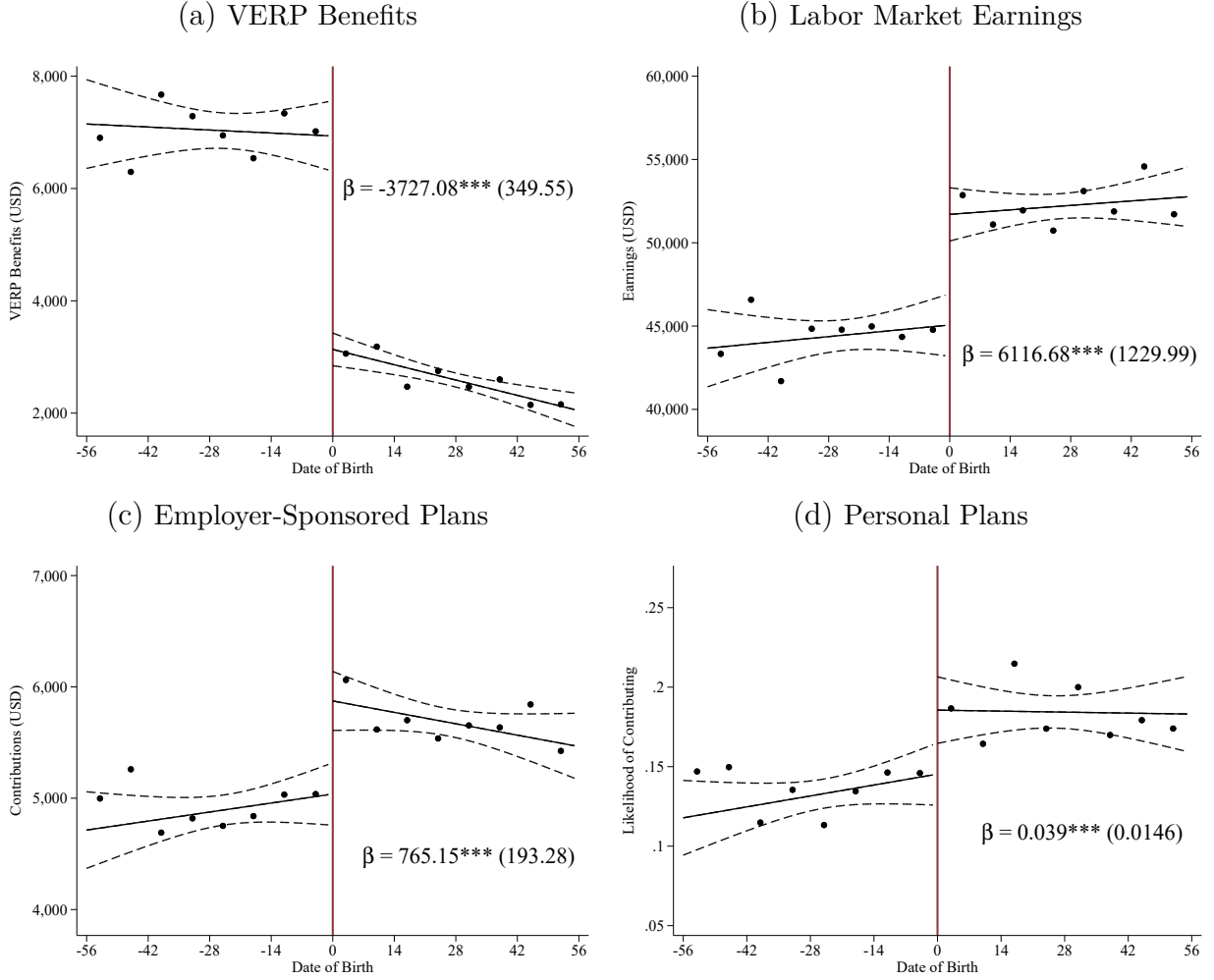
(a) VERP Benefits

β = -3727.08*** (349.55)

(b) Labor Market Earnings

β = 6116.68*** (1229.99)

(c) Employer-Sponsored Plans

β = 765.15*** (193.28)

(d) Personal Plans

β = 0.039*** (0.0146)

Notes: This figure illustrates the effect of the reform on labor market outcomes and contributions to retirement accounts during the first critical year, when individuals born at the cutoff date are age 60. Each RD graph (a)–(d) 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 and 95-percent confidence intervals 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 During the Second Critical Year 2016
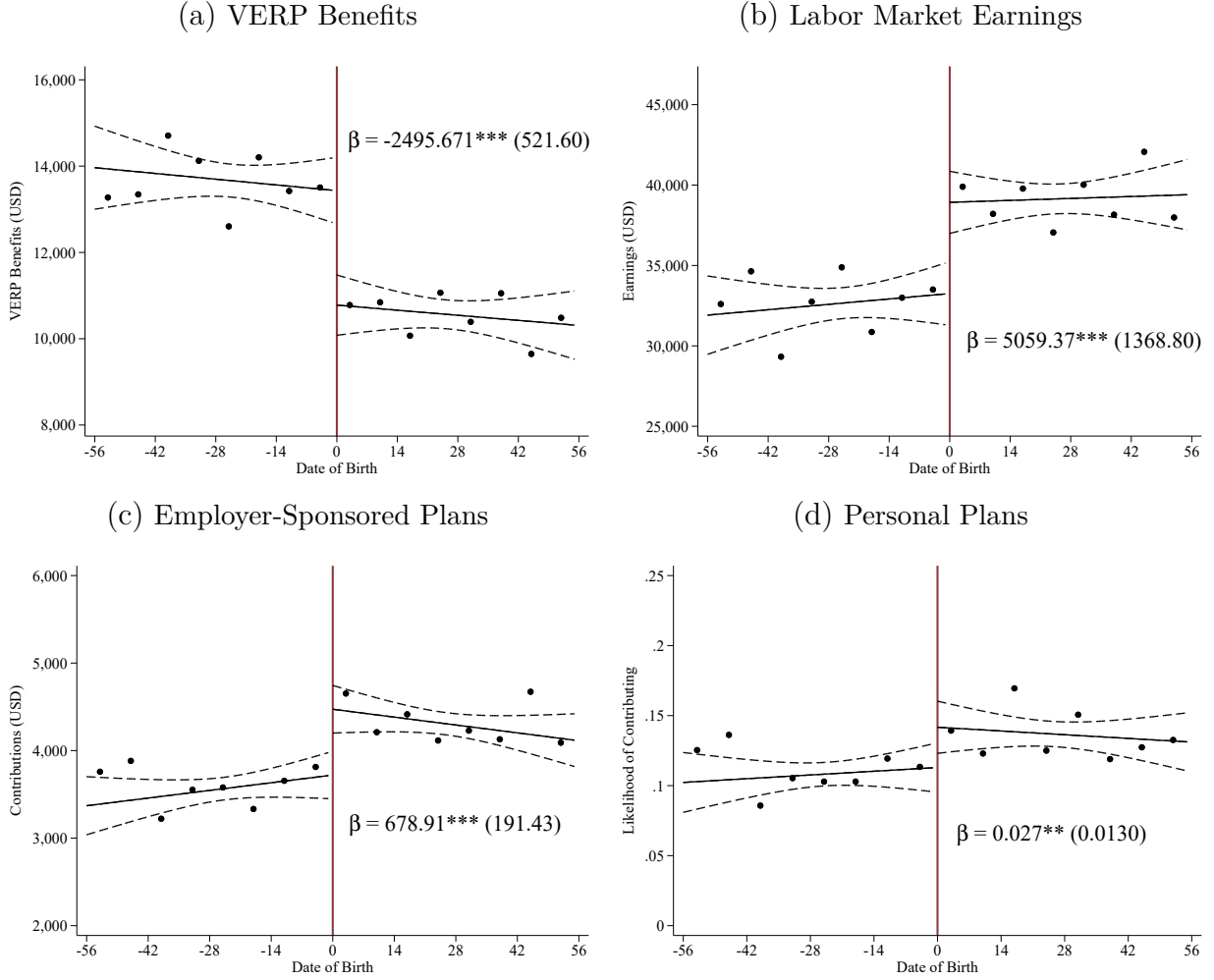
(a) VERP Benefits

β = -2495.671*** (521.60)

(b) Labor Market Earnings

β = 5059.37*** (1368.80)

(c) Employer-Sponsored Plans

β = 678.91*** (191.43)

(d) Personal Plans

β = 0.027** (0.0130)

Notes: This figure illustrates the effect of the reform on labor market outcomes and contributions to retirement accounts during the second critical year, when individuals born at the cutoff date are age 62. Each RD graph (a)–(d) 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 and 95-percent confidence intervals 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: Differences Between Actual and Firm Default Contribution Rates

(a) Pre-Announcement Period: Year 2010

(b) Anticipation Period: Year 2012

(c) First VERP Critical Year: 2014

(d) Second VERP Critical Year: 2016
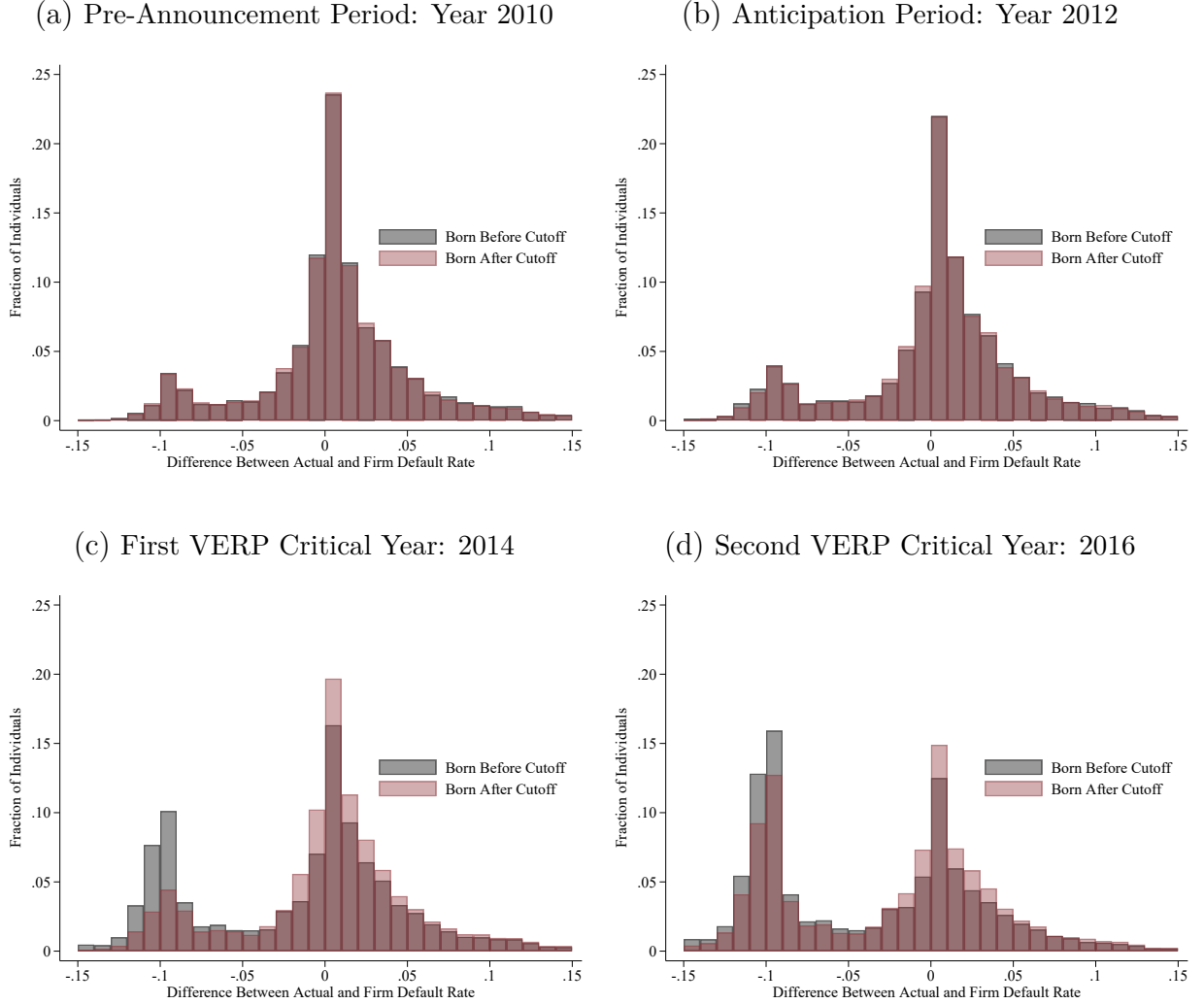
Notes: This figure illustrates how actual contribution rates to employer-sponsored retirement plans deviate from firm default contribution rates, over time, for both the treatment and control group. Firm default contribution rates are inferred as the median contribution rate among individuals working at the firm, as described in Section 7.2.2. 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	
	Mean (1)	SD (2)	Mean (3)	SD (4)
A: Demographics				
Age	56.99	0.29	56.99	0.09
Male	0.46	0.50	0.46	0.50
Married	0.72	0.45	0.72	0.45
Treated	0.52	0.50	0.52	0.50
B: Labor Market Earnings				
Any Earnings	0.94	0.23	0.94	0.24
Earnings	61,380	35,013	60,912	34,355
C: Retirement Savings (Flow Variables)				
Any Contribution to Employer Plans	0.89	0.32	0.89	0.32
Contributions to Employer Plans	6,508	4,951	6,430	4,888
Any Contribution to Personal Plans	0.41	0.49	0.41	0.49
Contributions to Personal Plans	1,192	2,130	1,171	2,111
D: Other Savings (Stock Variables)				
Bank Account Balances	26,505	46,790	26,238	45,558
Stock Market Account Balances	7,240	44,006	7,136	46,094
Property Wealth	152,541	189,923	151,354	182,384
Number of Individuals	40,042		12,020	

Notes: This table reports means and standard deviations of key variables, for the analysis sample and the main RD estimation sample, in 2010, the year before the reform. The analysis sample 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. The main RD estimation sample consists of the subset of individuals from the analysis sample who were born within 56 days of the birthdate cutoff.

Table 2: Responses Over the Anticipation Period

	Years: 2011–2013	
	RD Estimate (1)	Mean (2)
A: Labor Supply		
Average Earnings	186.09 (992.59)	55,621
B: Retirement Accounts		
Average Contributions to Employer Plans	20.32 (177.95)	6,048
Fraction of Years Contributing to Personal Plans	0.005 (0.016)	0.33
C: Other Savings		
Average Change in Bank Accounts	-66.22 (213.31)	1,543
Average Change in Stock Market Accounts	-4.00 (107.33)	944
Average Change in Property Wealth	-31.048 (225.04)	-3,494
Obs.	12,020	

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 for labor supply outcomes. Panel B presents results for contributions to retirement savings accounts. Panel C presents results for savings through bank accounts, stock market accounts, and property. 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 During Early Retirement Period Critical Years

	Critical Year: 2014		Critical Year: 2016	
	RD Estimate (1)	Mean (2)	RD Estimate (3)	Mean (4)
A: Labor Supply				
VERP Benefits	-3727.08*** (349.55)	6,995	-2495.67*** (521.60)	13,634
Earnings	6116.68*** (1229.99)	44,449	5059.37*** (1368.80)	32,737
B: Retirement Accounts				
Contributions to Employer Plans	765.15*** (193.28)	4,928	678.91*** (191.43)	3,603
Any Contribution to Personal Plans	0.039*** (0.0146)	0.14	0.027** (0.0130)	0.11
Distributions from Retirement Plans	-262.92*** (88.22)	1,584	-236.23 (163.73)	2,467
C: Other Savings				
Change in Bank Accounts	-120.84 (469.46)	1,876	370.12 (468.54)	801
Change in Stock Market Accounts	-295.57 (211.15)	1,843	31.56 (86.43)	312
Change in Property Wealth	-6.54 (22.03)	-522	0.40 (27.09)	-649
Obs.	12,020		12,020	

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 for labor supply outcomes. Panel B presents results for contributions to (and distributions from) retirement savings accounts. Panel C presents results for savings through bank accounts, stock market accounts, and property. 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 During Early Retirement Period Non-Critical Years

	Year: 2015		Year: 2017		Year: 2018	
	RD Estimate (1)	Mean (2)	RD Estimate (3)	Mean (4)	RD Estimate (5)	Mean (6)
A. Labor Supply						
VERP Benefits	-548.92 (481.58)	8,262	-1006.78** (583.33)	16,872	-856.75 (583.33)	17,236
Earnings	1925.14 (1387.34)	41,251	2780.50** (1356.15)	27,032	805.75 (1329.58)	24,133
B: Retirement Accounts						
Contributions to Employer Plans	327.76* (198.93)	4,575	258.31 (182.52)	3,023	36.68 (170.67)	2,476
Any Contribution to Personal Plans	0.015 (0.014)	0.12	0.006 (0.012)	0.10	0.004 (0.012)	0.10
Distributions from Retirement Plans	-141.34 (132.87)	1,956	-123.96 (195.70)	2,834	-51.86 (213.84)	3,282
C: Other Savings						
Change in Bank Accounts	-414.15 (476.21)	1,192	622.01 (467.66)	-17	610.25 (557.27)	4,229
Change in Stock Market Accounts	92.70 (236.40)	1,738	-51.86 (163.55)	1,193	-61.06 (184.00)	-1,754
Change in Property Wealth	15.30 (42.32)	-960	-18.78 (56.41)	-1,313	-56.47 (45.07)	-1,040
Obs.	12,020		12,020		12,020	

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 for labor supply outcomes. Panel B presents results for contributions to (and distributions from) retirement savings accounts. Panel C presents results for savings through bank accounts, stock market accounts, and property. 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: Anticipatory Responses for Users of Personal Retirement Plans

	RD Estimate (1)	Mean (2)
A: Labor Supply		
Earnings	-84.24 (1486.16)	56,739
B: Retirement Accounts		
Contributions to Employer Plans	319.51 (265.25)	5,962
Any Contribution to Personal Plans	0.001 (0.019)	0.71
C: Other Savings		
Change in Bank Accounts	68.07 (347.15)	1,554
Change in Stock Market Accounts	70.29 (174.67)	1,157
Change in Property Wealth	115.99 (344.96)	-3,712
Obs.	5,015	

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 for labor supply outcomes. Panel B presents results for contributions to retirement savings accounts. Panel C presents results for savings through bank accounts, stock market accounts, and property. 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

	RD Estimate (1)	Mean (2)
A. Frequent Users		
Any Contribution to Personal Plans in 2014	0.095*** (0.029)	0.28
Any Contribution to Personal Plans in 2016	0.062** (0.026)	0.21
Obs.	5,015	
B. Infrequent Users		
Any Contribution to Personal Plans in 2014	-0.001 (0.011)	0.04
Any Contribution to Personal Plans in 2016	0.003 (0.010)	0.04
Obs.	7,005	

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 in 2014	781.32***	590.74***
	(198.93)	(172.85)
Contributions in 2016	705.64***	525.63***
	(199.05)	(185.82)
Obs.	11,259	11,259

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 firm, as described in Section 7.2.2. 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
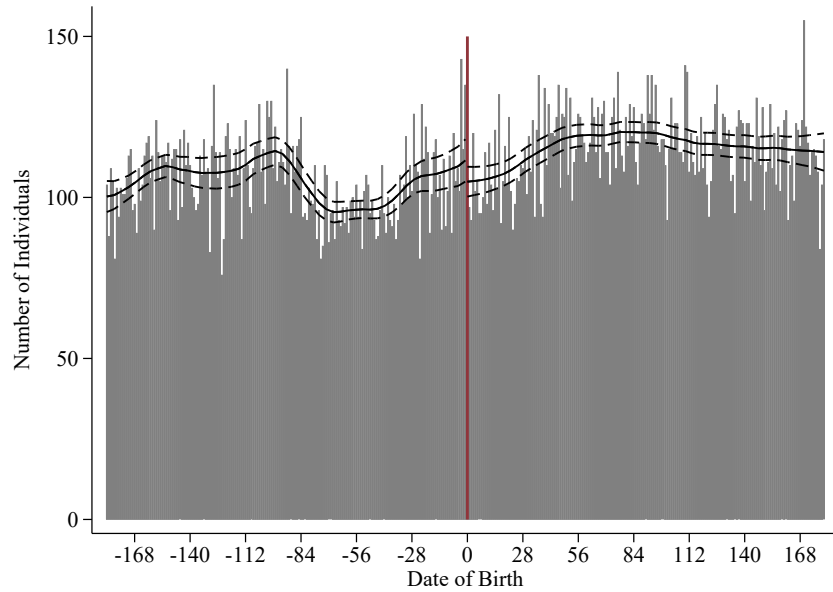
Notes: This figure depicts the density of the running variable, birthdate. The graph plots a histogram of the running variable for the entire analysis sample. Superimposed on top of the histogram are smoothed values and confidence intervals from local polynomial regressions of the number of individuals on birthdate. A formal density test as proposed by Cattaneo et al. (2019) using our baseline RD bandwidth of 56 days results in a p-value of 0.97.

Figure A.2: Analyzing Contribution Amounts to Personal Retirement Plans

(a) Unconditional Distribution

(b) Distribution of Positive Contributions

(c) RD Estimates: 2014
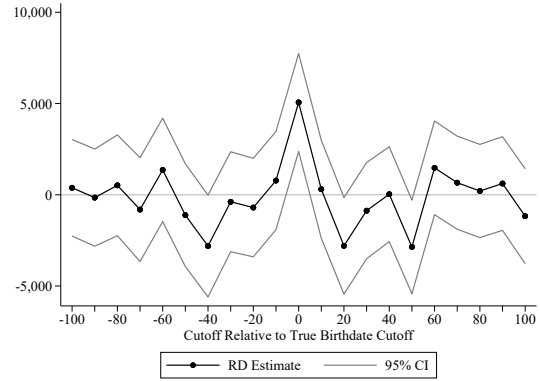
(d) RD Estimates: 2016
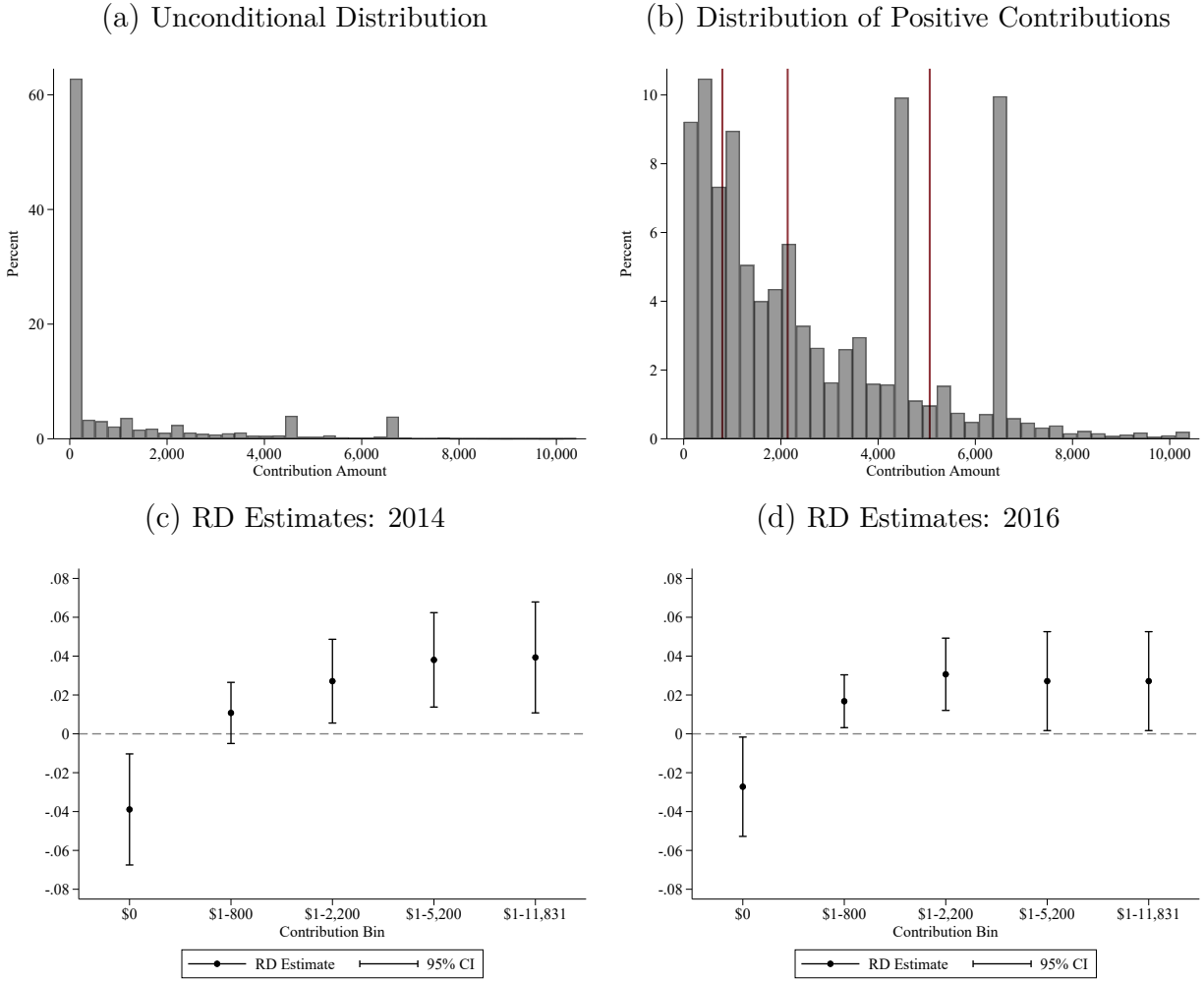
Notes: This figure illustrates the method of analyzing contribution amounts to personal retirement plans. Graph (a) plots the unconditional distribution of contribution amounts in 2010. The large number of small and zero contributions show why analyzing average contributions in levels is difficult. Graph (b) plots the distribution of positive contributions in 2010. The maroon vertical lines denote the quartiles of this distribution, which are used to define binary outcome variables for making contributions of various sizes. We use five indicator variables that capture contributions (i) that amount to $0, (ii) that are between $1 and the first quartile, (iii) that are between $1 and the second quartile, (iv) that are between $1 and the third quartile, and (v) that are greater than $1. Graph (c) plots the RD estimates from estimating equation (6) using as outcomes these indicator variables in 2014. Graph (d) plots the results for 2016.

Figure A.3: Psuedo Exercise: Psuedo Birthdate Cutoffs

(a) Labor Market Earnings: Year 2014
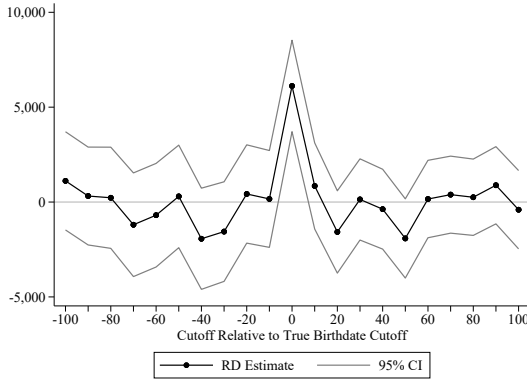
(b) Labor Market Earnings: Year 2016

(c) Employer Plans: Year 2014
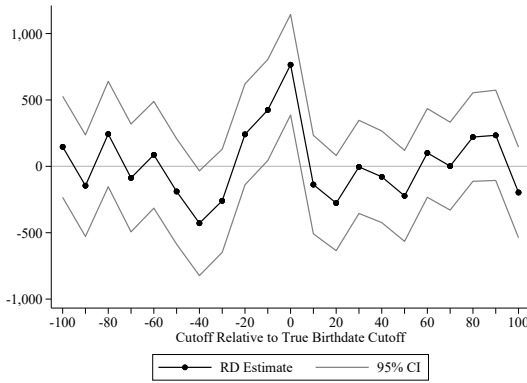
(d) Employer Plans: Year 2016
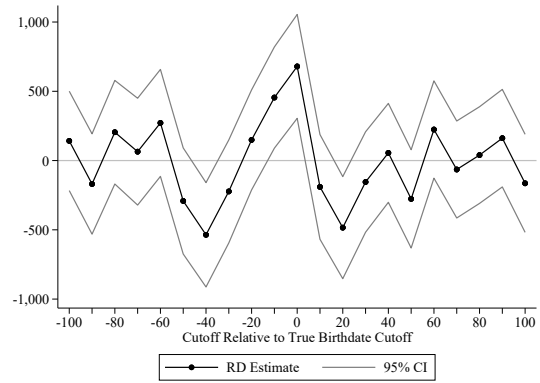
(e) Personal Plans: Year 2014
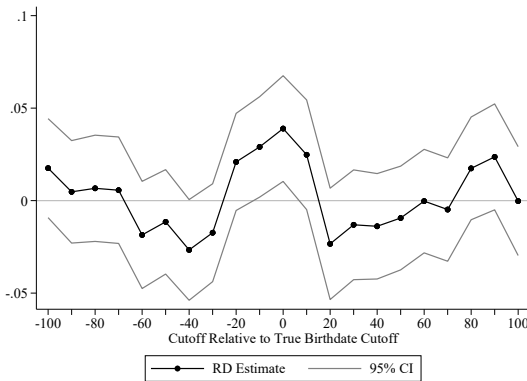
(f) Personal Plans: Year 2016
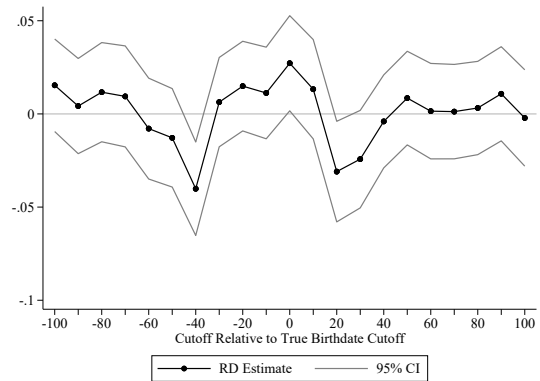
Notes: This figure illustrates how the RD estimates for labor market earnings and contributions to retirement plans, during each of the two critical years, change when placebo cutoffs are used rather than the true cutoff. Each graph (a)–(f) plots RD estimates and 95-percent confidence intervals from using the baseline RD estimating specification at various pseudo cutoffs.

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Figure A.4: Google Searches for Efterløn
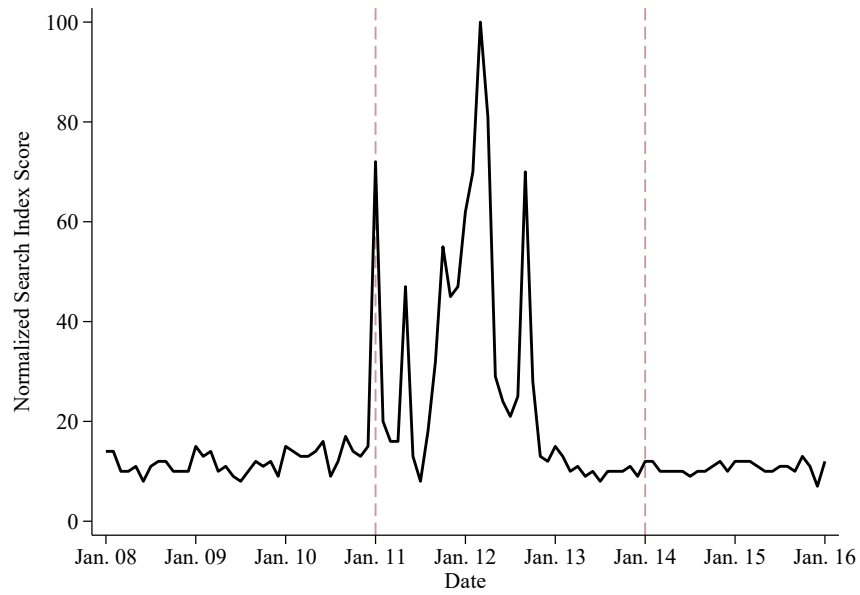
Notes: This figure plots a Google Trends search intensity index for "efterløn," which is the Danish word for the VERP program, between January 1, 2008 and January 1, 2016.

Table A.1: RD Estimates for Control Variables as Outcomes

	RD Estimate (1)	Mean (2)
Male	0.026 (0.020)	0.47
Married	0.018 (0.018)	0.69
Hovedstaden	-0.003 (0.013)	0.12
Sjælland	-0.010 (0.017)	0.25
Syddanmark	-0.005 (0.017)	0.24
Midtjylland	0.022 (0.017)	0.24
Nordjylland	-0.005 (0.014)	0.15
Obs.	12,020	

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.2: RD Estimates for Contributions to Roth-Style Plans

	Personal Plans		Employer Plans	
	RD Estimate (1)	Mean (2)	RD Estimate (3)	Mean (4)
Contribute in 2013	0.001 (0.011)	0.08	-0.003 (0.004)	0.02
Contribute in 2014	-0.010 (0.013)	0.12	0.003 (0.004)	0.01
Contribute in 2015	-0.007 (0.014)	0.14	0.001 (0.004)	0.01
Contribute in 2016	-0.015 (0.014)	0.15	0.000 (0.004)	0.01
Contribute in 2017	-0.004 (0.015)	0.16	0.002 (0.004)	0.01
Contribute in 2018	-0.022 (0.015)	0.18	-0.000 (0.010)	0.06
Obs.	12,020		12,020	

Notes: This table reports RD estimates for the impact of the reform on the likelihood of making any contribution to "Roth-style" retirement accounts. Outcome variables for both contributions to employer-sponsored and personal accounts are indicator variables for making any contribution to the plans. Roth-style plans were first introduced to the Danish economy in 2013. 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.3: Robustness to Alternative Specifications: Anticipatory Responses

	Employer Plans (1)	Personal Plans (2)	Bank Accounts (3)	Stocks (4)	Property (5)	Earnings (6)
A. Baseline	20.32	0.005	-66.22	-4.00	-31.05	186.09
	(177.95)	(0.016)	(213.31)	(107.33)	(225.04)	(992.59)
B. 70 Day Bandwidth	98.64	0.011	-60.89	32.61	-120.58	569.84
	(159.24)	(0.014)	(190.07)	(96.09)	(201.64)	(891.63)
C. 63 Day Bandwidth	71.97	0.009	-69.92	16.33	-75.47	392.29
	(167.83)	(0.015)	(200.72)	(101.23)	(212.36)	(938.25)
D. 49 Day Bandwidth	-32.61	-0.003	-94.29	-37.65	10.80	64.51
	(190.26)	(0.017)	(228.75)	(114.77)	(240.53)	(1058.08)
E. 42 Day Bandwidth	-55.72	-0.013	-142.11	-48.97	50.48	114.30
	(205.40)	(0.019)	(247.95)	(123.95)	(259.55)	(1138.09)
F. Global Polynomial	32.87	0.005	-66.58	-8.23	-31.31	190.27
	(177.95)	(0.016)	(213.34)	(107.39)	(225.10)	(992.24)
G. No Controls	84.95	0.005	-60.92	3.34	-26.18	645.80
	(180.98)	(0.016)	(213.34)	(107.62)	(230.82)	(1016.33)
H. No Triangular Weights	158.40	0.017	-89.89	55.05	-138.87	712.59
	(163.18)	(0.015)	(195.47)	(98.94)	(207.14)	(917.50)

Notes: This table reports results from assessing the sensitivity of the RD estimates over the anticipation time period to various specification checks. Each column corresponds to a different main outcome variable. Each row indicates the specification choice and how it differs from the baseline specification. Row A reproduces baseline estimates for ease of comparison. Row B increases the bandwidth by two weeks. Row C increases the bandwidth by one week. Row D decreases the bandwidth by one week. Row E decreases the bandwidth by two weeks. Row F uses a global linear polynomial rather than two separate linear polynomials on either side of the cutoff. Row G drops control variables from the regressions. Row H does not use triangular weights. Robust standard errors are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.4: Robustness to Alternative Specifications: Critical Year 2014

	Employer Plans (1)	Personal Plans (2)	Bank Accounts (3)	Stocks (4)	Property (5)	Earnings (6)
A. Baseline	765.15***	0.039***	-120.84	-295.57	-6.54	6116.68***
	(193.28)	(0.0146)	(469.46)	(211.15)	(22.03)	(1229.99)
B. 70 Day Bandwidth	797.83***	0.046***	-128.95	-206.62	-16.54	6275.60***
	(172.67)	(0.0131)	(420.78)	(188.02)	(19.69)	(1101.65)
C. 63 Day Bandwidth	793.50***	0.043***	-135.19	-247.66	-10.98	6203.09***
	(182.14)	(0.0138)	(443.17)	(198.54)	(20.77)	(1160.65)
D. 49 Day Bandwidth	733.37***	0.034**	-46.97	-366.64	-4.36	6079.54***
	(206.73)	(0.0156)	(501.41)	(226.63)	(23.57)	(1313.79)
E. 42 Day Bandwidth	725.82***	0.029*	102.11	-398.01	-2.07	6183.88***
	(223.17)	(0.0168)	(540.60)	(245.76)	(25.49)	(1415.75)
F. Global Polynomial	775.62***	0.039***	-63.97	-300.77	-7.47	6114.95***
	(193.07)	(0.015)	(469.43)	(210.48)	(22.06)	(1224.64)
G. No Controls	835.91***	0.040***	-118.17	-274.96	-15.12	6641.61***
	(196.79)	(0.015)	(469.41)	(211.75)	(22.49)	(1257.63)
H. No Triangular Weights	859.49***	0.051***	-108.84	-160.06	-11.55	6387.30***
	(176.47)	(0.0134)	(431.89)	(191.71)	(20.16)	(1130.06)

Notes: This table reports results from assessing the sensitivity of the RD estimates during the first critical year of 2014 to various specification checks. Each column corresponds to a different main outcome variable. Each row indicates the specification choice and how it differs from the baseline specification. Row A reproduces baseline estimates for ease of comparison. Row B increases the bandwidth by two weeks. Row C increases the bandwidth by one week. Row D decreases the bandwidth by one week. Row E decreases the bandwidth by two weeks. Row F uses a global linear polynomial rather than two separate linear polynomials on either side of the cutoff. Row G drops control variables from the regressions. Row H does not use triangular weights. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.5: Robustness to Alternative Specifications: Critical Year 2016

	Employer Plans (1)	Personal Plans (2)	Bank Accounts (3)	Stocks (4)	Property (5)	Earnings (6)
A. Baseline	678.91***	0.027**	370.12	31.56	0.40	5059.37***
	(191.44)	(0.0130)	(468.54)	(86.43)	(27.09)	(1368.80)
B. 70 Day Bandwidth	721.07***	0.031***	388.02	53.57	-5.59	5289.61***
	(171.07)	(0.0117)	(418.65)	(77.16)	(24.33)	(1226.75)
C. 63 Day Bandwidth	716.87***	0.029**	388.26	39.28	-1.50	5251.99***
	(180.44)	(0.0123)	(441.54)	(81.41)	(25.60)	(1292.08)
D. 49 Day Bandwidth	649.15***	0.023*	370.55	35.03	-0.60	4959.92***
	(204.72)	(0.0139)	(501.45)	(92.61)	(28.92)	(1461.54)
E. 42 Day Bandwidth	647.06***	0.019	359.95	43.67	-3.03	5063.54***
	(220.98)	(0.0150)	(542.03)	(100.34)	(31.20)	(1574.97)
F. Global Polynomial	688.72***	0.028**	369.31	35.70	0.68	5062.84***
	(191.49)	(0.0131)	(467.28)	(86.52)	(27.12)	(1368.00)
G. No Controls	751.88***	0.029**	390.65	34.99	-10.00	5672.21***
	(196.15)	(0.0131)	(468.85)	(86.53)	(27.64)	(1410.79)
H. No Triangular Weights	766.20***	0.037***	412.22	30.601	2.82	5535.10***
	(175.00)	(0.0121)	(427.67)	(78.68)	(25.09)	(1260.00)

Notes: This table reports results from assessing the sensitivity of the RD estimates during the second critical year of 2016 to various specification checks. Each column corresponds to a different main outcome variable. Each row indicates the specification choice and how it differs from the baseline specification. Row A reproduces baseline estimates for ease of comparison. Row B increases the bandwidth by two weeks. Row C increases the bandwidth by one week. Row D decreases the bandwidth by one week. Row E decreases the bandwidth by two weeks. Row F uses a global linear polynomial rather than two separate linear polynomials on either side of the cutoff. Row G drops control variables from the regressions. Row H does not use triangular weights. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.6: Additional Winsorizing of Flow Savings Variables Computed From Stock Variables

	Bank Accounts (1)	Stocks (2)	Property (3)
Anticipation	-59.41	-16.51	34.53
	(151.26)	(32.90)	(174.59)
2014	-37.27	-48.33	4.96
	(331.04)	(57.80)	(17.95)
2015	-293.30	32.47	20.91
	(328.95)	(53.75)	(32.73)
2016	423.54	5.52	14.94
	(328.00)	(20.14)	(22.15)
2017	473.24	-3.43	5.12
	(327.48)	(35.83)	(44.33)
2018	301.88	-59.63	-10.86
	(408.59)	(89.76)	(34.54)
Obs.		12,020	

Notes: This table reports additional RD estimates for the impact of the reform on savings in bank accounts, stock market accounts, and property, where outcome variables are more-stringently winsorized at the 10th and 90th percentiles. The columns denote the different type of savings vehicle, and the rows indicate 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

	Years: 2008–2010	
	RD Estimate (1)	Mean (2)
A: Labor Supply		
Earnings	692.77 (890.49)	59,778
B: Retirement Accounts		
Contributions to Employer-Sponsored Plans	-4.76 (195.79)	6,607
Any Contribution to Personal Plans	-0.003 (0.018)	0.25
C: Other Savings		
Change in Bank Accounts	-110.57 (209.89)	1,427
Change in Stock Market Accounts	-29.54 (45.04)	-186
Change in Property Wealth	-122.54 (615.83)	-12,614
Obs.	12,020	

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 for labor supply outcomes. Panel B presents results for contributions to retirement savings accounts. Panel C presents results for savings through bank accounts, stock market accounts, and property. 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

	First Critical Year	Second Critical Year
	RD Estimate	RD Estimate
	(1)	(2)
A: 1950/1951 Birth Cohorts		
Earnings	-729.20	-1194.96
	(1283.84)	(1331.95)
Contributions to Employer Plans	-215.25	-131.75
	(204.14)	(179.62)
Any Contribution to Personal Plans	0.013	-0.004
	(0.0192)	(0.0137)
Obs.	11,788	11,788
B: 1951/1952 Birth Cohorts		
Earnings	706.59	1243.32
	(1293.11)	(1344.74)
Contributions to Employer Plans	166.52	101.42
	(197.36)	(184.75)
Any Contribution to Personal Plans	0.016	0.004
	(0.019)	(0.014)
Obs.	11,810	11,810

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	-0.003 (0.0090)	0.94
Participate in 2012	0.005 (0.0099)	0.93
Participate in 2013	-0.009 (0.0106)	0.92
Obs.	12,020	

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Appendix B Additional Institutional Details

This section provides additional institutional details. The particular rules and regulations discussed pertain to our analysis time period and the birth cohorts relevant for our study.

B.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.

B.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 contributing to VERP 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.

B.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 but 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.