Behavioral Responses to a Pension Savings Mandate: Quasi-experimental Evidence from Swiss Tax Data*

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Abstract

To boost retirement savings, workers in many countries are encouraged or even required to contribute part of their earnings to an employer-sponsored pension account. I study saving responses to the occupational pension savings mandate in Switzerland using detailed administrative tax data. Leveraging a discontinuity in mandate coverage at the earnings threshold, I find that workers respond to being forced to contribute to an occupational pension account by increasing voluntary forms of retirement savings. I provide evidence consistent with two mechanisms that could explain this crowding-in effect: an increase in information and salience, and resource sharing within the household. The additional retirement savings appear to be funded by reduced private savings, leaving total savings unaffected by the mandate, but this is imprecisely estimated.

JEL classification: D14, E21, H2, H3, J26

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1 Introduction

Demographic change has ramped up pressures on public pension systems, amplifying the need to increase retirement savings in order to facilitate adequate living standards in old age (Poterba, 2014). To tackle what some scholars have declared a 'retirement savings crisis' (Benartzi and Thaler, 2013), policymakers have implemented a wide range of pension policies that aim to improve individuals' financial preparedness for retirement while limiting the fallout on public finances. The menu of available policy tools includes financial incentives for pension savings through tax benefits and subsidies, behavioral interventions such as automatic enrollment and careful setting of defaults, as well as provision of information about the pension system and the personal retirement situation.

A straightforward instrument is requiring workers by law to contribute some fraction of their earnings to a (typically employer-sponsored) pension account that they can only access upon entering retirement. A common justification for this *pension savings mandate* is that individuals may be myopic and not saving optimally for retirement in absence of the policy. Under this assumption, the pension contributions directly add to total savings. But if people are in fact optimizing in line with standard neoclassical life-cycle models, the mandate may not lead to an increase in total savings because savings vehicles are substitutes (Chetty et al., 2014). From this viewpoint, workers are expected to respond to the mandate by reducing other types of savings, such as private savings or different retirement savings, offsetting the mechanical increase in pension contributions at least partially. Another possibility is that insufficient retirement savings are due to a lack of information or salience, in which case the mandate could crowd in additional pension savings if this channel outweighs potential substitution effects. This mechanism may be particularly important for workers with low levels of financial literacy (Lusardi and Mitchell, 2011*a*). Understanding individual saving behavior is key to evaluating and improving the design of retirement systems.

This paper tests the effects of an occupational pension savings mandate on voluntary forms of pension savings, private savings, and total savings, and sheds light on the mechanisms underlying the overall behavioral response using administrative tax data from Switzerland. The Swiss setting has three major advantages. First, the occupational pension system provides compelling identifying variation that can be leveraged using quasi-experimental research designs. Second, the universal coverage of the tax data facilitates the use of empirical strategies that require zooming in on specific parts of the earnings distribution and following people over time. Together, these two features allow me to disentangle the causal effect of contributing to an employer-sponsored pension account from unobserved heterogeneity in savings

preferences. Third, I can measure different forms of pension savings, private savings, and total savings at the individual level, enabling me to characterize the full saving response. The richness of the data also enables me to document effect heterogeneity along dimensions such as age, gender, marital status, household income, and past saving behavior.

The Swiss savings mandate requires employees whose earnings exceed a relatively low threshold (around the 30^{th} percentile of the earnings distribution) to contribute to an occupational pension account. Individuals subject to the mandate cannot opt out. As a consequence, it mechanically increases occupational pension savings, but the effect on other forms of savings are unknown. Contributions are generally calculated by applying a legally defined age-specific contribution rate to the 'coordinated salary' which is equal to earnings minus a deduction. For individuals whose earnings are only marginally above the mandate threshold, there is a minimum coordinated salary that the contribution rate is applied to. This causes a discontinuity in the occupational pension savings rate at the cutoff of about 2 percentage points, on average, meaning that otherwise similar individuals are required to contribute substantially different amounts to occupational pension accounts.

I combine the clean policy variation with administrative data providing detailed information on income, wealth, and savings of the entire adult population in the second-largest Swiss canton, Bern, from 2002 to 2017. To uncover the saving responses to a forced increase in occupational pension contributions, I draw on two complementary quasi-experimental research designs. First, I use a regression discontinuity design that leverages the aforementioned jump in occupational pension savings at the mandate threshold by comparing workers with earnings just above the cutoff to those just below. I find strong evidence of a 'crowding-in' effect of the mandate on voluntary retirement savings. Workers respond to being subject to the mandate by increasing their private pension savings rate by 0.7 percentage points and occupational pension buy-ins by 0.4 percentage points. These effects correspond to a 12% and a 66% increase relative to the mean in the estimation sample, respectively. This implies that the mandate has a positive impact on savings earmarked for retirement above and beyond the mechanical effect on occupational pension savings. This finding is particularly striking given that the different types of retirement savings considered have similar characteristics, including the advantageous tax treatment and the payout timing, suggesting that there should be some substitution effect. The increase in retirement savings appears to be funded by reduced private savings rather than lower current consumption, implying that total savings – the sum of all forms of pension savings and private savings – are not affected by the mandate. However, the effects on private savings and total savings are imprecisely estimated due to the high variance of the private savings measure, so I cannot rule out substantial

changes. In sum, the results suggest that the mandate does not change total savings but it shifts the composition of the savings portfolio towards retirement savings.

To validate the regression discontinuity results and assess how workers who are *newly* covered by the mandate respond, I conduct a difference-in-differences analysis of the 2005 reform that brought many more workers into the occupational pension system by lowering the earnings threshold. This allows me to shed light on the dynamics and mechanisms behind the saving response. The difference-in-differences results are highly consistent with those from the regression discontinuity approach. I find significant crowding-in effects on private pension savings and occupational pension buy-ins that are very similar in magnitude when scaled by the policy variation they are based on.

Investigating potential mechanisms that explain the crowding-in response, I find evidence consistent with the mandate reducing information frictions and increasing salience of retirement savings. Within the set of workers who became subject to the mandate due to the reform, I identify individuals who are less likely to be affected by information and salience effects: those who had already been covered by the mandate in at least one pre-reform year and those who have made private pension contributions at least once before the reform. Neither of these subgroups increases private pension savings in response to the reform, suggesting that information and salience are at the heart of the crowding-in effect. Conversely, workers who had not been subject to the mandate or who had not contributed to private pension accounts before the reform respond by strongly increasing private pension savings. Intuitively, being enrolled into an occupational pension plan may have provided new information about the pension system and encouragement to have a careful look at the personal retirement situation to individuals who have previously been less aware of the need to save for retirement and the preferential tax treatment of pension savings. These results are consistent with theoretical models incorporating information and salience effects that push in the opposite direction of substitution effects. In the Swiss context, the information and salience channel appears to be strong, leading to a crowding-in effect on voluntary retirement savings, on average.

Moreover, I provide evidence suggesting that resource sharing within the household facilitates crowding-in. Only workers with relatively high household income, most of whom are secondary earners, increase private pension savings in response to becoming subject to the mandate after the reform. This explains how some low-earners can afford to raise voluntary retirement savings despite their take-home pay decreasing due to the mandated occupational pension contributions. Individuals with low household income respond to the mandate by reducing private pension savings, suggesting that the mandate crowds out voluntary retirement savings for the subset of workers with limited financial means.

This paper builds on and contributes to the vast literature on saving responses to retirement policies. Effect estimates vary substantially, both across different types of policies and among similar interventions. The impact of financial incentives for pension savings on other types of savings is ambiguous, with some papers estimating negative effects, null effects, or even positive effects (e. g. Engen, Gale and Scholz, 1996; Hubbard and Skinner, 1996; Poterba, Venti and Wise, 1996; Attanasio and DeLeire, 2002; Benjamin, 2003; Gelber, 2011; Börsch-Supan, Coppola and Reil-Held, 2012; Chetty et al., 2014; reviewed in Bernheim, 2002; OECD, 2018*a*). 'Behavioral interventions' such as automatic enrollment and default options have often been found to boost retirement savings (Madrian and Shea, 2001; Thaler and Benartzi, 2004; Chetty et al., 2014), but recent evidence suggests that this effect may not be sustained in the long run (Choukhmane, 2021). I provide evidence drawing on credibly exogenous variation and detailed administrative data which allows me to overcome the data and identification challenges that may explain some of the discrepancies in previous research. The finding that there can be crowding-in of pension savings in the presence of information and salience effects highlights the importance of paying attention to the sometimes surprising interactions between different ways of saving for retirement. While crowding-in is a relatively novel result in this literature, it is in line with Gelber (2011) who finds that becoming eligible for employer-sponsored 401(k) plans raises savings in Individual Retirement Accounts (IRAs) in the US.

Evidence on the saving responses to pension savings mandates specifically is scarce, presumably due to a lack of identifying variation and high-quality data. To my knowledge, only two papers, both using data from Denmark, study this type of policy (Arnberg and Barslund, 2014; Chetty et al., 2014). They both find limited crowding-out effects and substantial increases in total savings. Complementing these results, I document a positive impact on other types of pension savings but a null effect on total savings. My findings are also related to the literature on the effects of automatic enrollment into employer-sponsored pension plans (Madrian and Shea, 2001; Choi, Laibson and Madrian, 2004; Choukhmane, 2021), because the occupational pension savings mandate works in a similar way. The key difference between the two policies is that the mandate does not allow covered workers to opt out.

I also contribute to the growing literature examining the impact of information provision and increased salience on retirement savings (Duflo and Saez, 2003; Duflo et al., 2006; Goda, Manchester and Sojourner, 2014; Dolls et al., 2018). In line with previous findings, my results suggest that information and salience effects can boost pension savings. Whereas papers studying this channel typically consider pure information and salience treatments, I show that being required to contribute to an employer-sponsored pension account may stimulate retirement planning on its own. This is consistent with Blumenstock, Callen and Ghani (2018) who find that default-

ing employees into contributing to a savings account changes their attitudes towards and interest in saving.

The remainder of this paper is organized as follows. In Section 2, I explain the relevant parts of the Swiss old-age provision system. Section 3 describes the administrative tax data and the data preparation. Section 4 presents evidence on the savings effects of the pension savings mandate following a regression discontinuity approach. In Section 5, I analyze the saving response to becoming subject to the mandate due to the 2005 reform using a difference-in-differences design. Section 6 discusses the interpretation of the findings and explores the underlying mechanisms. Section 7 concludes.

2 Institutional Setting

The Swiss old-age provision system consists of three pillars: (i) a standard pay-as-you-go system, (ii) a fully funded occupational pension system, which includes the savings mandate studied in this paper, and (iii) voluntary private pension accounts.[1] This section briefly introduces the relevant institutional context, focusing on the rules of the occupational pension system and voluntary private pension accounts in 2017, the most recent year covered by the data. There have been no major changes to the old-age provision system during the sample period 2002–2017, except for the reform of the occupational pension scheme between 2004 and 2006 used for identification in the difference-in-differences analysis in Section 5.[2] Appendix B presents the Swiss old-age provision system in more detail.

2.1 Occupational Pension Scheme

The occupational pension system requires most workers to contribute to an occupational pension account. Employees must be enrolled in a pension fund by their employer if the annual gross earnings in their main job exceed the legally defined threshold. The pension savings mandate applies to women between 25 and 64 years as well as men between 25 and 65 years of age. The mandate is binding: employees satisfying the conditions cannot opt out of making contributions. Accordingly, coverage is quite comprehensive. In 2017, 4.2 million individuals were enrolled in an occupational pension fund, representing around 83% of the Swiss labor force (Federal

[1] The three-pillar system in Switzerland is akin to old-age provision systems in other countries that consist of a government-backed defined-benefit plan, employer-sponsored defined-contribution plans, and preferentially taxed private pension accounts – for example Denmark or the US (see Chetty et al., 2014). Simplifying a bit, the corresponding three pillars in the US are Social Security, 401(k) plans, and IRAs.

[2] Section 5.1 provides the relevant information about the 2004–2006 reform.

Statistical Office, 2019). Total contributions equalled CHF 54 billion, equivalent to 8% of GDP.[3] These numbers demonstrate that occupational pension plans are one of the most important savings instruments in Switzerland.

The earnings threshold determining mandate coverage is CHF 21,150. It is set with respect to the maximum pension from the pay-as-you-go system in order to avoid overinsurance. Some occupational pension funds may enroll employees with earnings below the statutory cutoff on a voluntary basis. However, comparing information reported directly by occupational pension funds to administrative data collected by the social insurance system, Ecoplan (2010) concludes that there are only small discrepancies between the number of employees enrolled in occupational pension funds and the equivalent counts inferred from earnings data. The difference is attributed to employees being enrolled in funds at multiple employers, self-employed individuals joining a fund voluntarily, and employees not covered by the mandate enrolling voluntarily. I cannot observe whether individuals below the threshold contribute to an occupational pension account because these contributions are not recorded in the tax data. If anything, this could attenuate the effect estimates relative to a situation where those below the threshold make zero occupational pension contributions. Moreover, individuals below the threshold who contribute to an occupational pension account are not 'treated' by the mandate because they could choose not to make these contributions.

Occupational pension contributions are calculated by applying the contribution rate to the 'coordinated salary' which is defined as gross earnings minus a deduction. The deduction is CHF 24,675 and serves a similar purpose as the mandate threshold. The statutory age-specific contribution rates are 7% from age 25 to 34, 10% for ages 35–44, 15% for ages 45–54, and 18% for ages 55–64 among women and ages 55–65 among men. Employers must pay at least half the contribution. The share paid by employees is deducted from their earnings by the employer and directly transferred to the pension fund. The contributions are exempt from income tax.

Importantly, there is a minimum coordinated salary of CHF 3,525. For employees whose earnings exceed the mandate threshold by only a small amount, the contribution rate is not applied to the marginal earnings above the threshold or deduction but to that minimum amount. This creates a discontinuity in occupational pension contributions at the cutoff which is the identifying variation that I leverage to study the effects of the savings mandate. Appendix Figure A.1 visualises the calculation of contributions before and after the 2005 reform.[4]

In addition to regular contributions, many occupational pension funds allow

[3] In 2017, the Swiss franc (CHF) was trading roughly at parity with the US dollar and at EUR 0.9. Hence, I do not report separate figures in US dollar or Euro in the remainder of this paper.

[4] Equation (7) in Appendix Section C.4 shows how to compute occupational pension savings.

lump-sum 'buy-ins' which can similarly be deducted from taxable income.[5] I observe occupational pension buy-ins in the tax data and examine how they are affected by the savings mandate in Sections 4 and 5.

Upon retirement, benefits can be received in the form of a lifelong annuity, a lump-sum payment, or a combination of the two. While contributions and returns on investment are exempt from income tax (and there is no capital gains tax in Switzerland) and occupational pension wealth is exempt from wealth tax, pension benefits are taxed.[6]

2.2 Private Pension Savings

In addition to contributing to the pay-as-you-go system and occupational pension plans, employees can make voluntary contributions to designated private pension accounts. These contributions can also be deducted from taxable income up to an annual cap of about CHF 6,800. The access to those funds is restricted until individuals enter retirement. Private pension accounts need to be set up separately with a bank or insurance company. In the remainder of this paper, I refer to this savings vehicle as 'private pension savings.' Total private pension savings in designated accounts equal roughly CHF 10 billion per year, representing 1.5% of Swiss GDP (Schüpbach and Müller, 2019).

There are strong tax incentives for private pension savings, in particular for affluent people, because contributions can be deducted from taxable income and the accumulated capital is exempt from wealth tax. Upon retirement, private pension capital can be claimed as an annuity subject to income tax or lump sum taxed at preferential rates – very similar to occupational pension benefits.

Overall, occupational pension savings and private pension savings have very similar characteristics: they represent funded pension wealth, access to the funds is limited before retirement, they are paid out upon retirement in the form of an annuity or lump sum, and their tax treatment is similarly favorable compared to ordinary private savings. Because of these commonalities, it seems natural to assume that occupational pension savings and private pension savings are substitutes for most individuals. I investigate this conjecture empirically when analyzing the impact of the savings mandate on private pension savings in Sections 4 and 5.

[5] Individuals can make occupational pension fund buy-ins up to the level that they would have accumulated, had they been earning their current salary since they were 25 years old (Kuhn, 2020).

[6] The majority of OECD countries follow a similar 'exempt-exempt-taxed' regime (OECD, 2018*b*).

3 Data

Assessing the effect of the occupational pension savings mandate on saving behavior requires measuring various forms of savings at the individual level. In general, this is challenging because high-quality microdata on assets are scarce. Switzerland is a particularly well-suited context to study savings choices and wealth accumulation due to the availability of administrative tax data on wealth.[7] However, there is no individual-level dataset on wealth with nationwide coverage because the wealth tax is only collected at the cantonal and municipal level and not at the national level.

I draw on administrative tax data from the canton of Bern, providing detailed and comprehensive information on income, wealth, and savings.[8] The dataset is an annual panel covering all taxpayers in the canton between 2002 and 2017. It is based on the tax returns that virtually all adult residents must file. The dataset has several merits for studying saving behavior: *First*, the tax records cover the entire population. The composition of individuals in the data only changes due to migration or death. Full coverage of the population is key because the research designs used in this paper require zooming in on specific parts of the earnings distribution. *Second*, individuals can be followed over time, enabling me to employ panel methods. *Third*, the dataset links partners which allows me to include household-level variables in the analysis. *Fourth*, the tax records are verified by the tax authority for administrative purposes, limiting the extent of measurement error and misreporting. *Finally*, the canton of Bern is approximately representative for Switzerland (Brunner, Meier and Näf, 2020), which provides some support for the external validity of the findings in this paper. Appendix C describes the data and all steps of the preparation for the empirical analysis in more detail.

Because the occupational pension savings mandate applies at the individual level while the tax unit in Switzerland is the household, I split up married couples into individual observations. Most of the important variables for the analysis, including earnings from employment, private pension savings, and occupational pension buy-ins, are reported at the individual level in the tax data. For all income and wealth components reported only at the household level, I equally assign half to each partner (following Fagereng et al., 2020).

I focus on the effect of the pension savings mandate on a set of savings outcomes. Since the mandate applies to gross earnings before deductions, I compute gross earnings from net earnings reported in the tax return using the year-specific so-

[7] Since France replaced its wealth tax with a tax on real estate in 2018, only two OECD countries other than Switzerland still levy a wealth tax: Norway and Spain (OECD, 2018*c*).

[8] Bern is the second-largest Swiss canton by population, with 1.03 million residents in 2017. Population statistics from the Federal Statistical Office: https://www.bfs.admin.ch/bfs/en/home/statistics/population.html [accessed on 22 October 2021].

cial insurance and occupational pension contribution schedules. Due to the minimum coordinated salary, gross earnings slightly below and above the mandate threshold can result in the same net earnings recorded in the tax data. Thus, for a small number of individuals, I cannot unambiguously impute gross earnings. In the empirical analyses, I use a 'donut hole' approach removing the problematic observations within a narrow earnings range of CHF 350 below and above the threshold.

Whereas some savings measures are directly observable in the tax data, others can be computed based on information available in the dataset. Occupational pension contributions are not recorded in the data because they are withheld at source by the employer, so I impute these by applying the statutory contribution schedule to gross earnings. Private pension savings in designated accounts and occupational pension buy-ins are directly observed in the tax data. Finally, I compute private savings that are not explicitly earmarked for retirement as the change in net wealth relative to the previous year. This measure includes both 'active savings' and 'passive savings' from changes in asset prices (see Fagereng et al., 2021). It is highly variable due to fluctuations in asset prices which makes precise estimation of the effect on private savings difficult. Total savings equal the sum of all four savings components discussed above. I focus on savings *rates* as a percentage of gross earnings rather than savings *levels* for two main reasons. First, it is a more informative measure when comparing individuals with different levels of income. Second, the results can be more easily compared to other findings in the literature than effect estimates denominated in Swiss francs.

Besides information on income, wealth, and savings, the dataset contains basic demographic information including the year of birth, gender, marital status, the number of children, and the municipality of residence.

I restrict the estimation sample to individuals between 25 and 60 years of age as the savings mandate only applies to working-age individuals.[9] Additional sample restrictions for the empirical analyses are introduced in Sections 4 and 5, respectively. Summary statistics for the restricted sample containing roughly 7 million observations from around 770,000 unique individuals are presented in Appendix Table C.1 and described in Appendix Section C.6.

[9] I impose a lower maximum age threshold than the legal retirement age – 62 years (2002–2004) and 64 years (2005–2017) for women, 65 years for men – to keep the age composition consistent across time and genders and to exclude people who retire early. Early retirement is widespread in Switzerland (Dorn and Sousa-Poza, 2005).

4 Evidence from a Discontinuity in Mandate Coverage

Estimating the causal effect of contributing to an occupational pension account on savings is challenging because individuals simultaneously decide how much to save and how to allocate their savings into different vehicles. Even when using variation in features of employer-sponsored pension plans across firms, selection bias persists if workers sort into firms according to their unobserved preference for retirement savings. Selection-on-observables approaches are therefore unlikely to recover unbiased estimates of the causal effect of occupational pension contributions on other types of savings. Field experiments that randomly assign individuals to pension plans are a promising way of addressing this challenge, but they are often infeasible due to a combination of financial, legal, and logistical constraints.

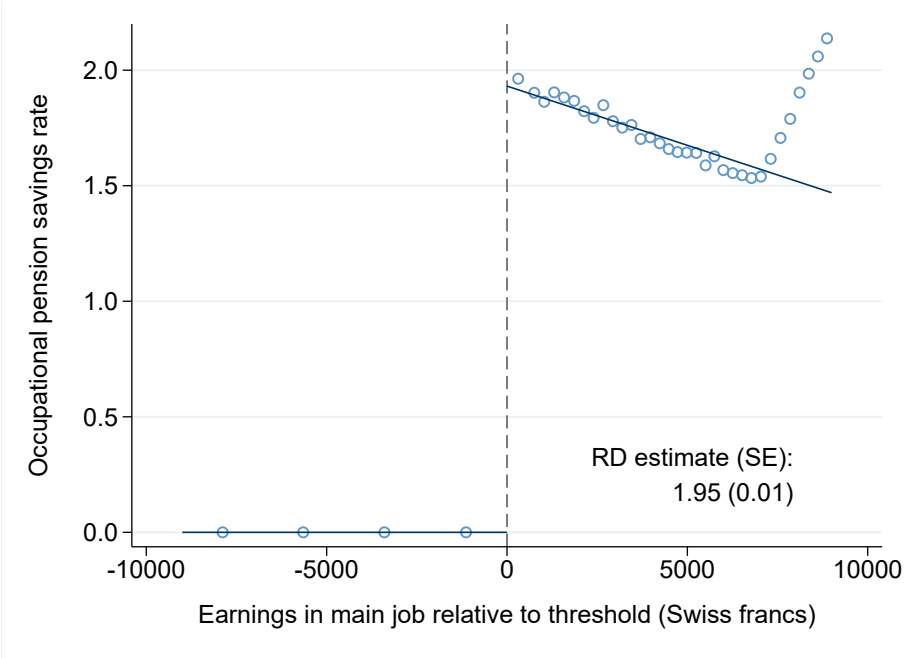
I approximate the ideal experiment using two complementary quasi-experimental research designs that exploit credibly exogenous variation in occupational pension savings. In this section, I employ a regression discontinuity design that leverages the earnings threshold of the Swiss pension savings mandate which requires otherwise similar individuals to contribute substantially different amounts to occupational pension accounts. In Section 5, I analyze the 2005 reform of the occupational pension system that substantially lowered the mandate threshold using a difference-in-differences design. This allows me to study the saving response of workers newly covered by the mandate, to pin down the effect dynamics, to shed light on the underlying mechanisms, and to validate the regression discontinuity results.

4.1 Discontinuity in Occupational Pension Savings

Employees with earnings above the mandate threshold automatically contribute a fraction of their earnings to an occupational pension account, while those below are not required to make those savings. Because contribution rates are applied to the minimum coordinated salary (CHF 3,525 in 2017) for those marginally above the threshold, annual occupational pension savings jump from zero to CHF 250–630. The exact amount depends on the age-specific contribution rate. Figure 1 depicts the relationship between occupational pension savings and earnings in the main job, which can be interpreted as the treatment assignment.[10] There is a discontinuity in the occupational pension savings rate of close to 2 percentage points at the threshold (equivalent to around CHF 400). This amount reflects the mechanical increase in savings induced by the mandate. I leverage this discontinuity using a sharp regression discontinuity (RD) design to estimate the effect of the mandate on saving behavior.

[10] As occupational pension savings are missing from the tax data (except for voluntary lump-sum buy-ins), contributions are predicted by applying the statutory contribution rate to gross earnings according to Equation (7) in Appendix Section C.4.

Figure 1: Predicted Occupational Pension Savings



Notes: Regression discontinuity plot showing the effect of the occupational pension savings mandate on occupational pension savings rates in 2017. Points are local sample means using non-overlapping quantile-spaced bins; lines are linear fits. Point estimates and standard errors are obtained from estimating Equation (2) employing a triangular kernel and using the occupational pension savings rate as the dependent variable. The running variable is recentered around the threshold of the pension savings mandate at CHF 21,150, indicated by the dashed vertical line. Occupational pension savings are predicted by applying the statutory contribution schedule to gross earnings in the main job following Equation (7). See Appendix C for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.

4.2 Identification and Estimation

Identification. Assuming that workers do not adjust their earnings to sort below or above the threshold, the mandate threshold generates variation in treatment assignment that is orthogonal to other individual characteristics. The fundamental idea is that individuals just below the cutoff can be used as a valid counterfactual for those just above the cutoff under the assumption that they are comparable in all relevant dimensions except for treatment status.

Formally, identification requires that the (average) potential outcomes are continuous across the threshold (Hahn, Todd and Van der Klaauw, 2001). The continuity assumption can be expressed in the following way. Let $Y_i(1)$ and $Y_i(0)$ denote the savings outcome of interest for individual i who is subject or not subject to the savings mandate, respectively. The sharp RD estimand τ_{RD} identifies the local average treatment effect of the mandate on individuals with near-threshold earnings X_i, provided

that the conditional expectation functions are continuous in x at the cutoff c:

$$\tau_{RD}(c) = \mathbb{E}[Y_i(1) - Y_i(0)|X_i = c] = \lim_{x \downarrow c} \mathbb{E}[Y_i|X_i = x] - \lim_{x \uparrow c} \mathbb{E}[Y_i|X_i = x] \quad (1)$$

The continuity assumption would be violated if workers or their employers were able to manipulate earnings strategically and precisely to impact workers' treatment status. In this case, potential discontinuities in observable and unobservable characteristics at the cutoff could confound estimates of the causal effects of the savings mandate. I discuss and test the validity of the identifying assumption in Section 4.3.

Estimation. Following the standard advice of the methodological RD literature (see in particular Gelman and Imbens, 2019), I estimate the target parameter τ_{RD} by $\hat{\beta}_1$ from a local linear regression model, allowing the slope of the control functions to vary on either side of the cutoff. The main RD specification is given by

$$Y_i = \beta_0 + \beta_1 \times \mathbb{1}\{X_i \geq c\} + \beta_2(X_i - c) + \beta_3(X_i - c) \times \mathbb{1}\{X_i \geq c\} + Z_i'\gamma + \epsilon_i, \quad (2)$$

where Y_i is a savings outcome of interest for individual i, X_i denotes earnings in the main job (the running variable), c is the mandate cutoff determining treatment assignment, and ϵ_i is the error term. Z_i represents a vector of controls consisting of age, gender, marital status, and number of children. Control variables are not necessary for identification, but their inclusion increases the precision of the estimates (Calonico et al., 2019). The outcome variable Y_i represents various types of savings, including the private pension savings rate, occupational pension buy-in savings rate, private savings rate, and total savings rate. The treatment variable is $\mathbb{1}\{X_i \geq c\}$ which indicates being subject to the savings mandate. Provided that ϵ_i does not change discontinuously at the threshold, $\hat{\beta}_1$ provides an unbiased estimate of the local average treatment effect of the savings mandate on savings outcome Y_i for individuals in proximity of the cutoff.

For ease of interpretation and to keep the effective estimation sample consistent, I use the same bandwidth for all outcomes in my main analysis. I choose it in a data-driven and transparent way by computing for each of the savings outcomes the bandwidth that minimizes the mean squared error (MSE) following the rate-optimal procedure proposed by Calonico, Cattaneo and Titiunik (2014*a*,*b*) and taking the unweighted mean. Based on that approach, I restrict the sample to individuals within CHF 9,000 of the earnings threshold in my main analysis.[11] Note that I exclude in-

[11]The outcome-specific MSE-optimal bandwidths are CHF 13,364 for private pension savings, CHF 6,704 for private savings, and CHF 6,408 for total savings. The mean is CHF 8,825 which I round to the nearest thousand. I do not include the bandwidth for occupational pension buy-ins in this calculation because the MSE-optimal bandwidth selector chooses a bandwidth of CHF 935, which is much lower than those of the other outcomes and leaves only few observations for estimation, given

dividuals within CHF 350 of the threshold in my main analysis because their gross earnings cannot be imputed unambiguously (see Appendix Section C.2). To give more weight to observations closer to the cutoff, I use the triangular kernel because it is MSE-optimal for point estimation when combined with an MSE-optimal bandwidth (Cattaneo and Titiunik, 2021).[12]

Inference. I compute standard errors using the heteroskedasticity-robust nearest-neighbor variance estimators proposed by Calonico, Cattaneo and Titiunik (2014*a*) and implemented in statistical software by Calonico, Cattaneo and Titiunik (2014*b*) and Calonico et al. (2017) since they are typically more robust in finite samples than conventional Huber-Eicker-White heteroskedasticity-robust standard errors (Calonico, Cattaneo and Titiunik, 2014*a*).

Sample restrictions. The main results are estimated using data from 2017 which is the most recent year in the dataset. I focus on working-age employees and remove individuals not observed in the preceding year from the sample because private savings cannot be measured for them (4% of the sample). This leaves me with an effective sample of 43,079 individuals within the estimation window.[13] Savings outcomes are winsorized at percentiles 1 and 99, except for occupational pension fund buy-ins as only roughly 0.8% of individuals in the estimation window make any buy-ins in a given year.

Robustness checks. In Appendix E, I demonstrate that the RD results are robust to different operationalization choices regarding the bandwidth, kernel function, size of the donut hole, order of the local polynomial, control variables, as well as using robust bias correction methods and different years in the data.

4.3 Validation of the Empirical Approach

The identifying assumption is that there is a discontinuity in treatment assignment at the threshold while average potential outcomes are continuous. This assumption has several implications that can be tested to assess the credibility of the empirical approach. Appendix D presents and discusses the results of these validity tests in detail. In this section, I highlight the key findings.

The main threat to identification stems from employees or employers manipulating earnings to sort around the threshold. To affect their treatment status, employees would need to adjust their working hours in their current position, demand a change

that employees within CHF 350 of the threshold are excluded.

[12]Estimation is performed using the Stata package `rdrobust` developed by Calonico, Cattaneo and Titiunik (2014*b*); Calonico et al. (2017).

[13]For reference, the full 2017 sample of individuals who are between 25 and 60 years old and have observable private savings consists of 444,398 individuals.

in the wage rate, or switch into a new job. On the other hand, employers might try to push the earnings of their employees below the threshold, so they can avoid paying the employer share of the occupational pension contributions (assuming some of the incidence falls on them).

I test for the presence of endogenous sorting by inspecting the continuity of the density of earnings and predetermined characteristics across the threshold. In the density test, there appears to be some excess mass below but close to the cutoff (Figure D.1). Assessing the continuity of covariates across the threshold, I find that individuals with earnings marginally above and marginally below the threshold have similar characteristics (Figure D.3). There is no significant discontinuity in gender, marital status, number of children, and net wealth in the previous year. I detect a small discontinuity in age, implying that individuals just above the threshold are on average half a year younger than those narrowly below.

To check whether these patterns reflect earnings manipulation that could bias the RD results, I repeat the analysis excluding the potential bunchers using a 'donut hole' approach (Bajari et al., 2011; Barreca et al., 2011). I find effect estimates similar to my main results for a wide range of donut hole sizes, alleviating concerns that the results are driven by sorting (see Appendix Section E.3). Moreover, the RD results are close to the difference-in-differences estimates (see Section 5.3) which are highly unlikely to be affected by strategic behavior since the treatment and control group are defined based on pre-reform earnings.

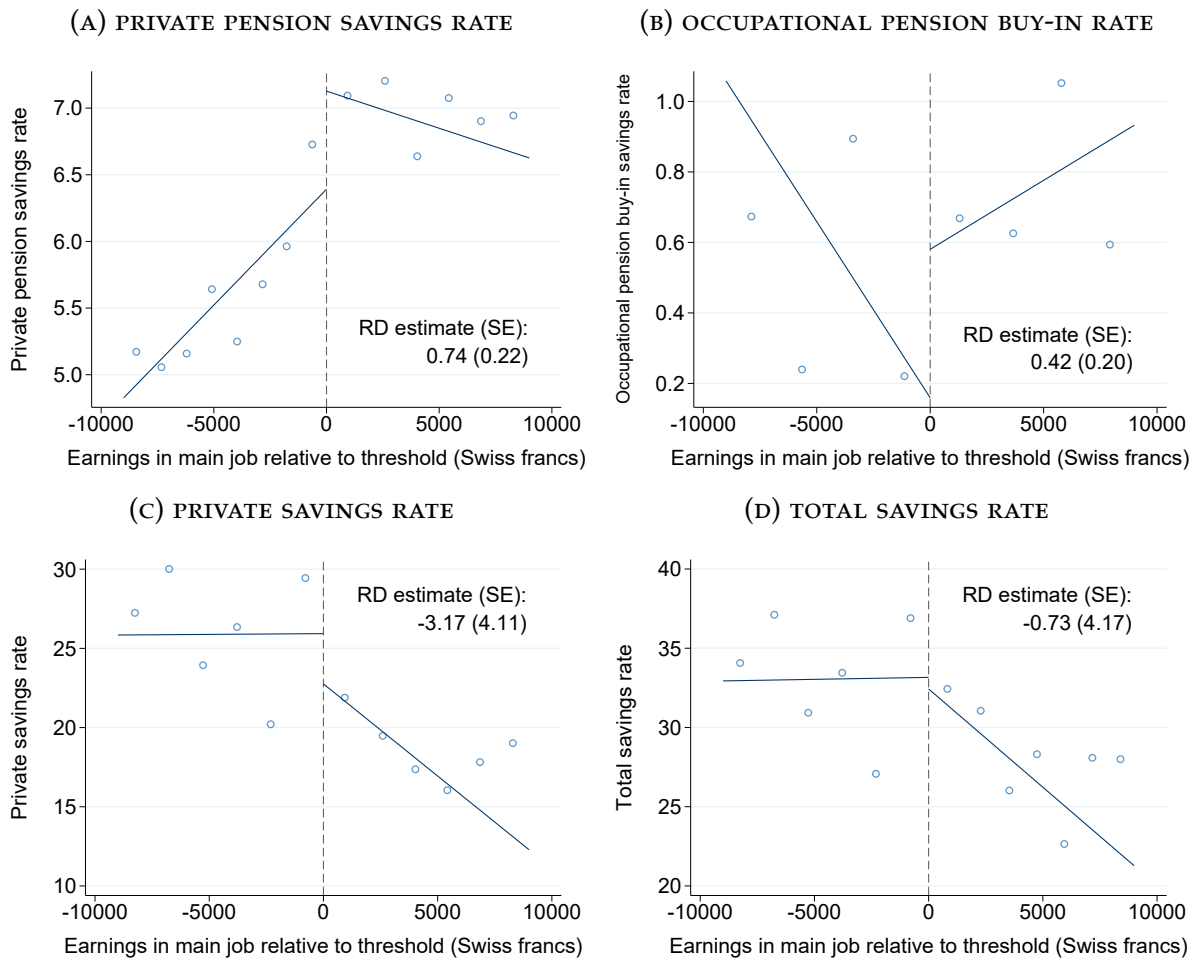
I further examine the validity of the RD approach by conducting a placebo test using hypothetical cutoff values at which no discontinuity in the savings outcomes should be detected (Figure D.4). I find that the significant treatment effect estimates for private pension savings and occupational pension buy-ins (documented in Section 4.4) clearly stand out from the estimates at the placebo thresholds which are all insignificant and close to zero.

4.4 Main Results

The mandate forces workers just above the threshold to contribute around 2% of their earnings to an occupational pension account. Below I present my estimates of the causal effect of the mandate on the private pension savings, occupational pension buy-ins, private savings, and total savings, obtained from estimating the RD model in Equation (2).

Figure 2 illustrates the main results by plotting the savings outcomes against the running variable. In lieu of a separate results table, it also reports the point estimates and standard errors for the treatment effect. Starting with Panel A, I estimate that the savings mandate *increases* the private pension savings rate by 0.7 percentage points.

Figure 2: Regression Discontinuity Results



Notes: Regression discontinuity plots showing the effect of the occupational pension savings mandate on a set of savings outcomes in 2017. Points are local sample means using non-overlapping quantile-spaced bins; lines are linear fits. The number of bins is selected using spacings estimators minimizing an asymptotic approximation to the integrated mean-squared error following the data-driven approach described in Calonico, Cattaneo and Titiunik (2015). Point estimates and standard errors are obtained from estimating Equation (2) using a triangular kernel. The running variable is recentered around the threshold of the pension savings mandate at CHF 21,150, indicated by the dashed vertical line. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. The effective estimation sample includes 43,079 workers. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation.
Source: Author's calculations based on administrative tax data from the canton of Bern.

This effect is statistically highly significant (p-value = 0.001). This is equivalent to a 12% increase relative to the mean within the estimation window. The graphical evidence shows that while the private pension savings rate increases with earnings below the cutoff, there is a clear jump at the threshold. Above the cutoff, the relationship is flat or even slightly decreasing.

I also find a statistically significant positive effect on occupational pension buy-ins shown in Panel B. The point estimate is 0.4 percentage points. This corresponds to a 66% increase relative to the mean in the estimation sample. The graphical evidence

for buy-ins is noisier because they are characterized by 'lumpiness,' with only 0.8% of individuals in the estimation window making any buy-in in 2017. Conditional on making a buy-in, the mean amount is CHF 17,200 – a sizeable amount for employees in the earnings range considered.

Panel C shows the RD results for private savings. There seems to be a discontinuity at the cutoff, suggesting that the savings mandate lowers the private savings rate by roughly 3 percentage points. However, that effect is imprecisely estimated due to the high variance of the private savings measure (see Appendix Section C.4).

How does the savings mandate affect the overall amount of savings that individuals set aside? Panel D depicts the impact of the mandate on the total savings rate. There is no discontinuity at the cutoff which suggests that there is no discernible effect on overall savings. However, the standard error is again large due to the variance of private savings, which are the largest component of total savings, so I cannot rule out substantial positive or negative effects.

In sum, there is strong evidence for a positive effect of the mandate on savings earmarked for retirement. Not only does the mandate mechanically increase occupational pension savings, it also raises voluntary contributions to private pension accounts and occupational pension buy-ins. These findings imply that requiring workers to contribute to employer-sponsored pension accounts does not crowd out but rather *crowds in* other forms of pension savings. This appears surprising through the lens of a standard neoclassical life-cycle model. I discuss the interpretation of this result and investigate potential mechanisms in Section 6.

At the same time, I do not find evidence that total savings respond to the mandate. This is because the increase in retirement savings appears to be funded by an equally sized reduction in private savings, rather than lower current consumption. However, the evidence for this is suggestive at best because effects on private and total savings are imprecisely estimated.

Together, these findings imply that total savings are unaffected by the mandate but there is some reallocation of savings towards pension accounts. Appendix F explores effect heterogeneity with respect to wealth, age, gender, and marital status.

5 Evidence from a Reform Extending Mandate Coverage

The RD approach provides causal evidence on the saving responses to being forced to contribute to an employer-sponsored pension account. But it does not directly tell us how workers *newly* covered by the mandate react and whether their response persists in subsequent years. I proceed by studying the 2005 reform that reduced the mandate threshold, thereby bringing many more low-income workers into the occupational

pension system. Using a difference-in-differences design, I compare workers who became covered by the mandate to those who narrowly missed out over time. This also serves as a reliability check of the RD approach as difference-in-differences and regression discontinuity rely on different identifying assumptions.

5.1 Reform of the Occupational Pension System in 2005

As part of a comprehensive reform of the occupational pension system that was implemented in three steps between 2004 and 2006, the earnings threshold of the savings mandate was lowered in 2005.[14] The purpose of the reduction of the threshold was to improve financial preparedness for retirement among low-income workers.

Figure 3 plots the evolution of the threshold since introduction of the occupational pension system in 1985, demonstrating the significance of the 2005 reduction. From 1985 until 2004, the threshold had been equal to the maximum pension in the pay-as-you-go system. Accordingly, it had been increasing one-for-one with the rising maximum benefit level during that period. The 2005 reform reduced the threshold to 3/4 of the maximum benefit level. As a consequence, it fell from CHF 25,320 in 2004 to CHF 19,350 in 2005.

Around 140,000 employees in Switzerland who otherwise would not have been subject to the mandate became covered due to the reform, the majority of which were female, working part-time, and had low hourly wages below CHF 25 (Ecoplan, 2010). This represents an increase in the number of workers covered by roughly 4%.

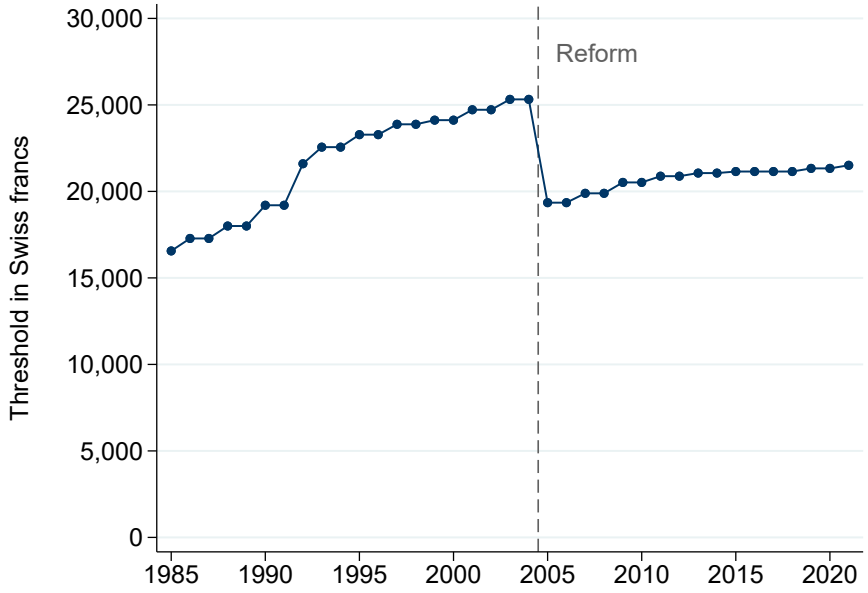
Appendix Figure A.1 depicts occupational pension savings as a function of earnings in the years immediately before and after the reform. The vertical difference between the lines represents the impact of the reform at each level of earnings. Most strikingly, workers in the earnings range between the 2004 threshold and the 2005 threshold had to start making positive contributions. Further, occupational pension savings increased at almost all earnings levels above the threshold because the reform also lowered the deduction from CHF 25,320 to CHF 22,575.[15]

The 2004–2006 reform was the first major reform of the occupational pension system since its introduction in 1985, and it remains the only one to date. It included further policy changes such as lowering the statutory rate for converting pension account balances into annuities from 7.2% to 6.8% over a transition period of ten years, increasing women's retirement age in the occupational pension system to 64

[14] Detailed information on the reform ('1. BVG-Revision') is available from the Federal Social Insurance Office: https://www.bsv.admin.ch/bsv/de/home/sozialversicherungen/bv/reformen-und-revisionen/revision-1-bvg.html [accessed on 30 October 2021].

[15] Before the reform, both the threshold and the deduction had been set equal to the maximum benefit from the pay-as-you-go system which was CHF 25,800 in 2005. Lowering the deduction aimed to offset some of the decline in pension benefit levels caused by the decrease in the conversion rate implemented in the same reform.

Figure 3: Earnings Threshold of Occupational Pension Savings Mandate



Notes: The figure displays the evolution of the earnings threshold determining coverage of the occupational pension savings mandate from the introduction of the occupational pension system in 1985 to 2021. The dashed vertical line indicates the timing of the reform that lowered the threshold in 2005. Employees with gross earnings in their main job exceeding the threshold are subject to the savings mandate. See Section 2.1 for more detailed information on the occupational pension system in Switzerland.

Source: Author's illustration based on information from the Federal Social Insurance Office.

years in line with the rules in the pay-as-you-go system, and aligning the contribution rate schedule for women with that of men. These changes are unlikely to confound the difference-in-differences estimates because neither the treatment nor the control group was contributing to occupational pension accounts before the reform.

5.2 Identification and Estimation

Identification. I leverage the exogenous variation in mandate coverage induced by the 2005 reform using a difference-in-differences (DD) design to estimate the causal effect on workers' savings. Identification relies on the assumption that savings rates of individuals subject to the mandate would have followed the same trend (on average) in absence of the policy as savings rates of individuals not covered by the mandate. If the parallel trends assumption holds, the evolution of savings rates of individuals not affected by the mandate (control group) can be used as a counterfactual trend for individuals who become subject to the mandate (treatment group).

I construct treatment and control group based on earnings in 2004, the year directly preceding the reform, to prevent endogenous selection into treatment. The group definitions follow from the provisions of the reform: the treatment group con-

sists of individuals with earnings in 2004 that are below the 2004 threshold but above the 2005 threshold. The control group includes individuals with 2004 earnings below but close to the 2005 cutoff. Thus, I define treatment group assignment as

$$T_i = \begin{cases} 1 & \text{if} \quad G_{i,2004} \in [c_{2005}, c_{2004}) \\ 0 & \text{if} \quad G_{i,2004} < c_{2005}, \end{cases} \tag{3}$$

where $G_{i,2004}$ denotes gross earnings in the main job of employee i in year 2004. The 2004 cutoff, c_{2004}, is CHF 25,320; the 2005 cutoff, c_{2005}, is CHF 19,350.[16] For the control group to cover a similar earnings range as the treatment group, I include individuals earning at least CHF 13,000 in 2004. Accordingly, the estimation sample includes individuals with 2004 earnings in a bandwidth of approximately CHF 6,000 above and below the 2005 threshold.

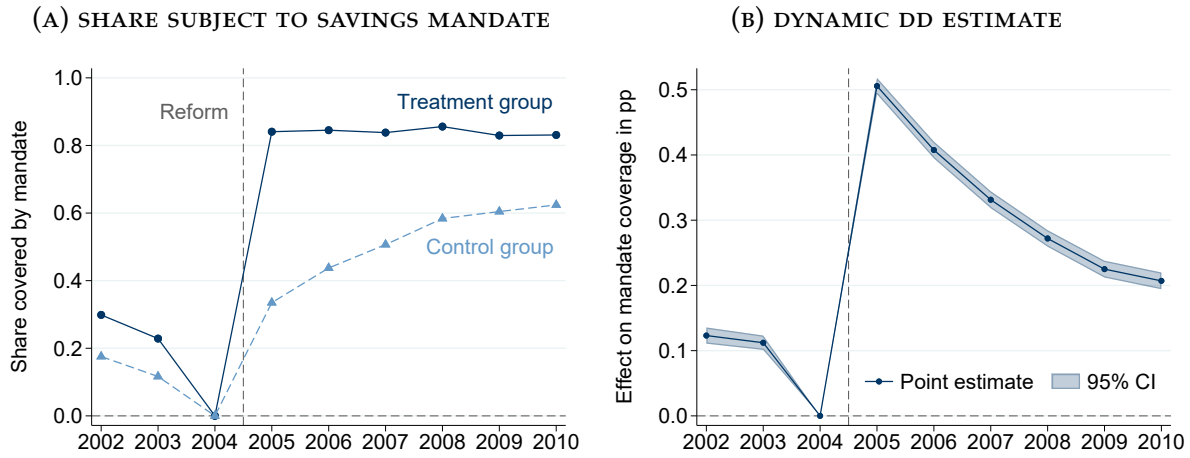
The parallel trends assumption would be violated if the savings of the treatment and control group were on different trends in absence of the mandate policy. As documented in Section 5.3, I do not find pre-reform differences in trends for any of the savings outcomes. This provides empirical support for the validity of the DD strategy, assuming that pre-trends are informative for (counterfactual) post-trends (Rambachan and Roth, 2021). Contamination from changes in other policies or the macroeconomic situation is unlikely because they would have had to occur at the same time as the reform and differentially affect the treatment and control group. Another reassuring fact is that RD and DD estimates are highly consistent.

Treatment variation. As the treatment and control group are defined based on pre-reform earnings, there is an exogenously higher probability that workers in the treatment group will be subject to the mandate relative to the control group, but the difference in the probabilities is not equal to one. Figure 4 illustrates the variation in mandate coverage. Panel A displays the evolution of the share subject to the mandate in the treatment and control group. By construction, nobody is covered in 2004. In the first year after the reduction of the threshold, the share subject to the mandate jumps to about 85% in the treatment group and 35% in the control group. While it stays constant in the treatment group in subsequent years, the control group gradually closes part of the gap due to workers' earnings growing over time and eventually rising above the cutoff. Panel B shows the corresponding dynamic effect on mandate coverage estimated from the DD model in Equation (4) using an indicator for being above the mandate threshold as the dependent variable.[17] This variation could

[16] The 2005 threshold of CHF 19,350 corresponds approximately to the 34th percentile of the distribution of main job earnings among working-age individuals in the canton of Bern in 2004.

[17] Appendix Figure A.2 displays the corresponding graphs for the occupational pension savings *rate*. It shows that the reform causes a persistent increase of close to 1 percentage point in the treatment group relative to the control group.

FIGURE 4: TREATMENT VARIATION



Notes: The figure displays the effect of the 2005 reform on the share of individuals subject to the occupational pension savings mandate. Individuals are sorted into treatment and control group based on whether their pre-reform earnings were below or above the 2005 mandate threshold, as defined in Equation (3). Panel A displays the share of individuals subject to the mandate separately for treatment and control group. Panel B displays year-specific difference-in-differences estimates relative to baseline year 2004 and corresponding 95% confidence intervals, obtained from estimating Equation (4) using an indicator for having earnings above the mandate threshold as the dependent variable. Standard errors are clustered at the individual level. The dashed vertical line indicates the timing of the 2005 reform that expanded mandate coverage. See Section 3 for more information on data preparation and variable construction as well as Section 5 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.

be used as the first stage in an instrumental-variable DD approach. I focus on the reduced-form results which should be interpreted as *intention-to-treat* (ITT) effects.

Estimation. To examine how the effect unfolds over time and to check the pre-trends for potential violations of the parallel trends assumption, I estimate dynamic DD specifications of the form

$$Y_{it} = \sum_{\substack{k=2002 \\ k \neq 2004}}^{2010} \beta_k \times \mathbb{1}\{t = k\} \times T_i + \mu_i + \lambda_t + \varepsilon_{it}, \tag{4}$$

where Y_{it} is a savings outcome for individual i in year t, μ_i denotes individual fixed effects, λ_t denotes year fixed effects, and ε_{it} represents the idiosyncratic error term. T_i is the treatment group indicator defined in Equation (3). The parameters of interest are β_k for $k > 2004$. They capture the effect of the savings mandate in post-reform year k relative to pre-reform year 2004. The coefficients β_k for $k < 2004$ can be interpreted as pre-trends to assess the credibility of the parallel trends assumption. β_{2004} is normalized to zero.

To increase power and aggregate the year-specific DD effects into an average post-

treatment effect, I also run static DD specifications of the form

$$Y_{it} = \beta \times \mathbb{1}\{t >= 2005\} \times T_i + \mu_i + \lambda_t + \varepsilon_{it}, \tag{5}$$

where all variables are defined as in the dynamic DD model in Equation (4).[18] The coefficient of interest, β, captures the average effect of the savings mandate in the post-reform period relative to the years before the reform.

Including individual fixed effects accounts for time-invariant confounders such as workers' permanent earnings potential or risk preferences. The year fixed effects absorb any shocks or structural trends affecting treatment and control group equally. In general, treatment and control group have similar characteristics because the sample is restricted to workers in a narrow earnings range (see Appendix Table A.1). I do not include time-varying covariates in the main analysis, but results are very similar when controlling for marital status, number of children, earnings, and net wealth.

Inference. Because the error terms are potentially serially correlated due to the panel structure, I cluster standard errors at the individual level (Bertrand, Duflo and Mullainathan, 2004). This approach is consistent with more recent work on clustering adjustment by Abadie et al. (2022) showing that it should account for clustered treatment assignment, because the policy is implemented at the individual level.

Sample restrictions. I restrict the sample to individuals between 25 and 60 years of age with some employment in each year considered.[19] As a consequence, I limit the estimation window to the period between 2002 and 2010 because enforcing a balanced panel with a wider window reduces the sample size and thus power. This is an innocuous restriction as I do not expect that it takes more than five years for the effects of the mandate to unfold.[20] Finally, I winsorize all savings outcomes at percentiles 1 and 99, except for occupational pension buy-ins as only 1.2% of all individual-year observations in the DD sample have positive buy-in amounts.

Appendix Table A.1 displays summary statistics separately for the treatment and control group in 2004. There are 8,905 individuals in the treatment group and 8,583 individuals in the control group. In total, the DD estimation sample includes 157,392 individual-year observations over the period 2002 to 2010. Comparing the pre-reform characteristics, the groups are very similar with respect to age, gender, and marital status. By construction, the treatment group has higher earnings than the control

[18]The static DD model increases power relative to the dynamic specification by both increasing the number of years used for estimation of the treatment effect and imposing parallel trends (and no anticipation effects) (Borusyak, Jaravel and Spiess, 2021).

[19]Specifically, I only include employees with annual earnings in the main job of at least CHF 2,500 in every year. The results are not sensitive to using different minimum earnings levels.

[20]This is confirmed by the results presented in Section 5.3. The effect estimates are somewhat noisier but qualitatively unchanged when extending the estimation window all the way to 2017.

group. Mean net wealth in the treatment group is also higher than in the control group, but the median is almost the same. The savings rates of the two groups before the reform are also very similar.

Robustness checks. I verify the robustness of the results to using smaller or larger earnings ranges when defining the control group as well as donut hole approaches that exclude workers in proximity of the 2005 mandate threshold in Appendix G.

5.3 Main Results

Figure 5 presents the dynamic effects of the occupational pension savings mandate on the savings outcomes of interest. The panels show year-specific effect estimates and pointwise 95% confidence intervals based on standard errors clustered at the individual level. Pre-trends in all panels are flat and statistically insignificant, providing empirical support for the validity of the parallel trend assumption. Note that for private and total savings, I can only estimate one pre-treatment coefficient because I cannot measure private savings (defined as the change in net wealth relative to the previous year) in 2002 as it is the first year in the dataset.

Panel A shows that private pension savings increase sharply in the treatment group relative to the control group after the reform in 2005. The results suggest that treated workers respond to the reform, which on average raises their occupational pension savings rate by 1 percentage point (relative to the control group), by *increasing* the private pension savings rate by 0.3–0.4 percentage points. The crowding-in effect arises instantaneously after the reform and is persistent over the succeeding five years, lending additional credibility to the interpretation that these estimates reflect a causal effect of becoming subject to the savings mandate.

Occupational pension buy-ins also increase in response to the reform, as depicted in Panel B. The year-specific effects vary between 0 and 0.4 percentage points, depending on the year. Similar to the RD results, the effect estimates are noisier than those for private pension savings because of the lumpiness of voluntary buy-ins.

Private savings in Panel C do not appear to respond to the policy change. There is a slight decrease in the two years after the reform, followed by a subsequent increase, but given the wide confidence intervals, this does not provide conclusive evidence of a negative or positive effect. Overall, the year-specific coefficients are centered around zero and statistically insignificant.

Panel D displays the effect on total savings. The point estimates hover around zero in the first few years after the reform and increase towards the end of the estimation window, but they are statistically insignificantly different from zero in each period. I cannot draw definite conclusions about the response of total savings, because the precisely estimated positive effects on private pension savings and occupational pen-

Figure 5: Dynamic Difference-in-Differences Estimates

Notes: Dynamic difference-in-differences plots showing the effect of the occupational pension savings mandate on a set of savings outcomes, exploiting the 2005 reform that expanded mandate coverage. The graphs depict year-specific effects relative to baseline year 2004 and corresponding 95% confidence intervals, obtained from estimating Equation (4). Standard errors are clustered at the individual level. The dashed vertical line indicates the timing of the 2005 reform. Treatment and control group are defined according to Equation (3). All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 5 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.

sion buy-ins are dominated by the noise from the high variance of the private savings measure.

Table 1 summarizes the aggregated post-treatment effects estimated using the static DD specification. In addition to the savings outcomes from Figure 5, column (1) reports the mechanical effect on the savings in occupational pension accounts driven by the mandate.[21] The occupational pension savings rate among workers in the treatment group increases by about 0.9 percentage points relative to the control group. Overall, the mandate leads to a clear increase in retirement savings, both mechani-

[21] Appendix Figure A.2 depicts the corresponding dynamic DD estimates.

Table 1: Static Difference-in-Differences Estimates

	Occupational pension savings rate (1)	Private pension savings rate (2)	Occupational pension buy-in savings rate (3)	Private savings rate (4)	Total savings rate (5)
Static DD	0.87***	0.31***	0.25***	1.00	2.40
	(0.028)	(0.088)	(0.068)	(1.98)	(1.99)
Individual fixed effects	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓
Observations	157,392	157,392	157,392	157,392	157,392

Notes: Difference-in-differences results for the effect of the occupational pension savings mandate on a set of savings outcomes, exploiting the 2005 reform that expanded mandate coverage. Estimates are obtained from estimating the static DD specification in Equation (5) using data from 2002 to 2010. Standard errors clustered at the individual level are reported in parentheses. Treatment and control group are defined according to Equation (3). The estimation sample includes individuals between 25 and 60 years of age with at least CHF 2,500 in earnings in every year considered. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 5 for more detail on sample restrictions and estimation. Stars indicate significance according to * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Author's calculations based on administrative tax data from the canton of Bern.

cally and by crowding in voluntary forms of pension savings. The point estimates for private savings and total savings are positive, but the standard errors are large, so I cannot rule out sizeable effects in either direction.

These findings are highly consistent with the RD results. To compare the magnitude of the savings effects in the RD and DD approach, I divide both sets of estimates by the respective treatment variation in occupational pension contributions that these designs leverage. The rescaled effects can be interpreted as the change in the savings rate in response to a 1 percentage point increase in the occupational pension savings rate. The treatment variation in the occupational pension savings rate is 1.95 percentage points in the RD approach (see Figure 1) and 0.87 percentage points in the DD approach (see Table 1). Focusing on the statistically significant effects on voluntary retirement savings, the rescaled coefficients are of remarkably similar magnitude: The effect of a 1 percentage point increase in the occupational pension savings rate on the private pension savings rate is 0.38 percentage points in the RD and 0.36 percentage points in the DD approach. The normalized effect on the occupational pension buy-in rate is 0.22 percentage points in the RD and 0.29 percentage points in the DD approach.

6 Interpretation and Mechanisms

The crowding-in effect of the occupational pension savings mandate seems surprising. The standard neoclassical life-cycle model predicts that individuals *reduce* voluntary retirement savings in response to a forced increase in occupational pension savings (Chetty et al., 2014), given that the pension savings vehicles considered here have similar features. To contextualize the findings, I first shed light on the characteristics of the affected workers. Then, I return to the saving response to the 2005 mandate expansion and document how it varies with workers' characteristics. Finally, I investigate potential mechanisms through which the mandate crowds in voluntary retirement savings. I find evidence that is consistent with intra-household resource sharing as well as information and salience effects playing a role.

When exploring effect heterogeneity and mechanisms, I focus on the DD analysis of the reform lowering the mandate threshold because the treatment variation is more likely to reflect workers entering the occupational pension system for the first time and it provides more statistical power to obtain precise effect estimates. I restrict attention to the impact on private pension savings because the crowding-in effect on occupational pension buy-ins is less surprising as employees subject to the mandate must be enrolled in an occupational pension plan which facilitates making additional, voluntary contributions to these accounts.

6.1 Characteristics of Workers Around the Threshold

Both empirical strategies use variation in mandate coverage created by the earnings threshold. Hence, they estimate a local average treatment effect specific to the group of workers in proximity of the cutoff. To characterize the marginal workers affected by the policy, Table 2 displays summary statistics on individuals between 25 and 60 years of age with main job earnings within CHF 5,000 of the threshold, pooling all years in the data from 2002 to 2017.

By construction, workers around the threshold have low earnings. Depending on the year, the mandate threshold corresponds to the 27–38th percentile of the distribution of main job earnings among working-age individuals. Although information on hours worked is not included in the tax records, given Swiss wage levels it is safe to say that the vast majority are part-time workers. This shapes the demographic make-up of this group: 82% of workers around the threshold are women; 68% of them are married. The average age is about 43 years and 75% of workers are between 34 and 51 years old. Total individual income is CHF 29,000, on average, the bulk of which is accounted for by earnings in the main job. This contrasts with mean total income among all working-age individuals which is CHF 59,000 (see Table C.1). Total house-

Table 2: Characteristics of Workers in Proximity of Mandate Threshold

	Mean	SD	P10	Median	P90
Female	0.82	0.39	0	1	1
Married	0.68	0.47	0	1	1
Age	42.65	10.11	28	43	57
Gross earnings in main job	21,515	3,465	16,834	21,569	25,796
Total individual income	29,209	36,923	14,949	24,058	52,274
Total household income	46,723	39,415	20,270	44,480	74,002
Net wealth	103,455	1,185,407	-17,265	19,471	241,546
Number of individuals			438,117		

Notes: Summary statistics, including mean, standard deviation, median, as well as percentiles 10 and 90, on characteristics of workers aged 25–60 with main job earnings within CHF 5,000 of the mandate threshold, pooling years 2002–2017. All monetary values are reported in Swiss francs. See Section 3 for more information on data preparation and variable construction.
Source: Author's calculations based on administrative tax data from the canton of Bern.

hold income is low on average, at CHF 47,000, but there is considerable variation – the 90th percentile is CHF 74,000. The distribution of net wealth is highly skewed: the mean is slightly above CHF 100,000, while the median is just CHF 19,000. However, these values are not much lower than mean and median net wealth among all working-age individuals which equal around CHF 130,000 and CHF 24,000, respectively.

These characteristics are correlated with saving behavior and may thus shape the response to pension policies. One should be careful when extrapolating the findings of this paper to groups of workers with very different characteristics than those considered here.

6.2 Effect Heterogeneity by Age, Gender, and Marital Status

To understand how the saving response to the mandate varies across workers with different characteristics, I estimate heterogeneous effects by including separate treatment indicators for different subgroups in the static DD specification in Equation (5). Table 3 reports the estimated effect for the whole treatment group as well as subgroup-specific estimates. Column 2 displays separate coefficients by age group. The high-age (low-age) group consists of individuals with age above or equal to (below) the median age in the treatment group in the pre-reform year 2004 which is 42 years. The aggregate treatment effect is purely driven by older individuals. For young workers, I find a precisely estimated zero effect. Column 3 shows effect estimates separately by gender. The effect is positive for women and strongly negative for men, suggesting that men fully offset the increase in occupational pension savings by a reduction in private pension savings. The aggregate effect is nevertheless posi-

Table 3: Private Pension Savings: Effect Heterogeneity

	Private pension savings rate			
	(1)	(2)	(3)	(4)
Static DD	0.31***			
	(0.088)			
Static DD × low age		-0.085		
		(0.10)		
Static DD × high age		0.66***		
		(0.11)		
Static DD × female			0.47***	
			(0.092)	
Static DD × male			-1.02***	
			(0.17)	
Static DD × unmarried				-0.84***
				(0.11)
Static DD × married				0.69***
				(0.097)
Individual fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	157,392	157,392	157,392	157,392

Notes: Difference-in-differences results for the effect of the occupational pension savings mandate on a set of savings outcomes by subgroup, exploiting the 2005 reform that expanded mandate coverage. Estimates are obtained from estimating the static DD specification in Equation (5) using data from 2002 to 2010, including separate treatment indicators for each subgroup. Standard errors clustered at the individual level are reported in parentheses. Treatment and control group are defined according to Equation (3). The estimation sample includes individuals between 25 and 60 years of age with at least CHF 2,500 in earnings in every year considered. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 5 for more detail on sample restrictions and estimation. Stars indicate significance according to * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Author's calculations based on administrative tax data from the canton of Bern.

tive because men account for only 11% of treated workers. Column 4 reports separate estimates by marital status, documenting that the effect is positive for married individuals and negative for unmarried individuals. All subgroup-specific coefficients are statistically significant, except for the estimate for young workers.

In sum, the crowding-in effect of the mandate is mainly driven by older, female, and married workers. Of course, these categories overlap which may account for some of the similarity in the subgroup-specific estimates – for example, men in the treatment group are much more likely to be unmarried than women.[22]

[22] 19% of women in the treatment group are unmarried in 2004, whereas the unmarried share is as

As married workers with low earnings typically have a partner who earns more than them, these results indicate that taking into account financial resources shared within the household may help to explain how they can afford to increase their voluntary retirement savings in response to becoming subject to the mandate.

6.3 Intra-household Resource Sharing

Because the Swiss tax data allow me to link married couples, I can directly test whether having another main earner in the household facilitates the increase in private pension savings in response to the reform. I estimate the static DD specification including separate treatment indicators for individuals with total household income below and above the median in the treatment group in 2004.[23] Column 2 of Table 4 reports the estimated effects. Individuals with household income above the median strongly increase their private pension savings rate in response to the reform, by roughly 1 percentage point. On the other hand, individuals with below-median household income reduce their private pension savings rate by 0.3 percentage points.

These results are consistent with the idea that the crowding-in of voluntary retirement savings among low-income workers is enabled by having another income source in the household, most importantly a partner in work. Liquidity constraints are more likely to be binding for individuals with low household income, so they respond to being forced to contribute to an occupational pension account by reducing their private pension savings. An alternative explanation for the differential response is that high-income households have stronger financial incentives to contribute to private pension accounts because they face higher marginal tax rates.

6.4 Information and Salience Effects

But why do workers increase their private pension savings in response to becoming covered by the mandate? Previous research has documented that information and salience are important causal drivers of retirement saving decisions. Dolls et al. (2018) find that people respond to receiving information about the pension system and their expected *public* pension payments by increasing *private* retirement savings in Germany. Using a field experiment, Goda, Manchester and Sojourner (2014) show that providing individual retirement income projections boosts contributions to employer-sponsored pension accounts.

high as 63% among men. But since 89% of treated workers are women, men still account for less than 30% of unmarried workers.

[23] Median household income in the treatment group in 2004 is around CHF 47,000. Median household income from employment is about CHF 44,000, suggesting that the partner's earnings are more important for most households than other types of income such as capital income, self-employment income, or transfer income.

Table 4: Private Pension Savings: Mechanisms

	Private pension savings rate			
	(1)	(2)	(3)	(4)
Static DD	0.31***			
	(0.088)			
Static DD × low household income		-0.33***		
		(0.097)		
Static DD × high household income		0.95***		
		(0.12)		
Static DD × not subject to mandate pre-reform			0.50***	
			(0.10)	
Static DD × subject to mandate pre-reform			-0.025	
			(0.12)	
Static DD × no private pension savings pre-reform				0.64***
				(0.091)
Static DD × positive priv. pension savings pre-reform				-0.46***
				(0.15)
Individual fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	157,392	157,392	157,392	157,392

Notes: Difference-in-differences results for the effect of the occupational pension savings mandate on a set of savings outcomes by subgroup, exploiting the 2005 reform that expanded mandate coverage. Estimates are obtained from estimating the static DD specification in Equation (5) using data from 2002 to 2010, including separate treatment indicators for each subgroup. Standard errors clustered at the individual level are reported in parentheses. Treatment and control group are defined according to Equation (3). The estimation sample includes individuals between 25 and 60 years of age with at least CHF 2,500 in earnings in every year considered. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 5 for more detail on sample restrictions and estimation. Stars indicate significance according to * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Author's calculations based on administrative tax data from the canton of Bern.

In a similar way, the savings mandate could help to reduce information frictions and increase salience of pension savings because workers who are enrolled into an occupational pension fund receive letters providing information about their expected pension contributions and benefits, the rules and regulations of their pension fund, and the pension system more generally. They are also likely to notice the pension contributions being deducted from their salary on their pay slip. This may encourage them to have a careful look at their retirement situation and provides information about available savings vehicles, including the tax advantages associated with both occupational and private pension savings, leading to the observed positive spillovers to voluntary retirement savings.[24]

[24]I cannot disentangle information effects, which occur if individuals lack relevant information about pension savings in absence of the policy, and salience effects, which are present when individuals know this information but do not act on it unless their attention is drawn to the subject (see Chetty, Looney and Kroft, 2009). For interpretation of my results, this distinction is of secondary importance.

I test for information and salience effects by estimating separate responses for subgroups of workers who are more or less likely to be affected by a lack of information or salience before the reform, based on their observed past saving behavior. First, I split up the treatment group by whether workers had been subject to the savings mandate in at least one year before the 2005 reform. Because by construction no one in the DD sample had earnings above the mandate threshold in 2004, this depends on their earnings in 2002 and 2003.[25] To be consistent with information and salience effects, crowding-in should be concentrated among workers who became covered by the mandate for the first time due to the reform. For workers who had already been subject to the mandate at some earlier point, the increase in information and salience should have been limited. Column 3 of Table 4 shows that this is precisely what I find in the data. Workers who had not been covered by the mandate before increased their private pension savings rate by 0.5 percentage points, while workers who had previously been covered did not respond at all.

Second, I estimate separate effects for workers who have had positive private pension savings at least once in the three-year period before the reform and those who have not contributed to private pension accounts at all prior to the reform.[26] If there were information and salience effects, crowding-in should be more prevalent among workers who have not made private pension contributions before the reform, as they appear to have been less aware of the need to save for retirement and the tax advantages of doing so. On the contrary, if their lack of private pension savings revealed their (weak) preference for retirement savings, they would not be expected to increase private pension savings in response to being forced to make occupational pension contributions due to the mandate. Column 4 of Table 4 shows that the positive effect on private pension savings is entirely driven by workers who have not contributed to private pension accounts prior to the reform. They increase their private pension savings rate by 0.6 percentage points. Conversely, workers with positive private pension savings before the reform reduce their savings rate by almost 0.5 percentage points. This finding is even more striking considering that workers who had not previously contributed to private pension accounts likely had to open a new account, which is associated with a (time and administrative) fixed cost.

Both these results are consistent with a reduction in information frictions and an increase in salience of the pension system driving the crowding-in effect of the occupational pension savings mandate. Information and salience effects are muted for individuals who had been subject to the mandate before and who were already aware of the benefits of contributing to private pension accounts. Being required by

[25] 64% of treated workers have not earned more than the threshold in both 2002 and 2003.

[26] 70% of workers in the treatment group have not made any private pension savings in the pre-reform period between 2002 and 2004.

the mandate to contribute to an occupational pension fund has no or even a negative effect on their private pension savings for those individuals. This suggests that there is some substitution between these savings vehicles that in aggregate is dominated by the information and salience effects.

7 Conclusion

Against the backdrop of demographic change, a wide range of pension policies aimed at boosting retirement savings and ensuring adequate living standards in old age have been implemented around the globe. Studying the impact of the occupational pension savings mandate on saving behavior in Switzerland, I document three main findings: *First*, being required to contribute to an occupational pension account *crowds in* voluntary retirement savings such as preferentially taxed private pension savings and occupational pension buy-ins. The results suggest that a 1 percentage point increase in the occupational pension savings rate raises the private pension savings rate by 0.4 percentage points and occupational pension buy-ins by 0.2–0.3 percentage points. Crowding-in is inconsistent with standard neoclassical life-cycle models, given that these different forms of pension savings have similar characteristics (Chetty et al., 2014). But it lines up with the positive impact of 401(k) eligibility on IRA assets in the US documented by Gelber (2011). *Second*, I find evidence consistent with reduced information frictions and increased salience driving the crowding-in effect. This is in line with the growing literature documenting a positive effect of information provision on pension savings (Duflo and Saez, 2003; Duflo et al., 2006; Goda, Manchester and Sojourner, 2014; Dolls et al., 2018) and the extensive literature demonstrating the important role of financial literacy for retirement planning (see Lusardi and Mitchell, 2007, 2011*a*,*b*). The results also suggest that resource sharing within the household facilitates the increase in voluntary retirement savings. *Third*, total savings do not appear to respond to the savings mandate, suggesting that the increase in retirement savings is funded by cutbacks in private savings rather than reduced current consumption. This result contrasts with earlier work that finds a positive impact of pension savings mandates on total savings in Denmark (Arnberg and Barslund, 2014; Chetty et al., 2014). However, the effects on private savings and total savings are imprecisely estimated, so I cannot rule out substantial changes.

The findings of this paper have implications for retirement policy. Together, they suggest that pension savings mandates do not necessarily increase total savings but shift the composition of the savings portfolio towards accounts earmarked for retirement. Thus, even if total savings are unaffected, mandates can improve financial preparedness for retirement by raising the share of assets that are only accessible

in old age. They also underline the importance of providing comprehensible and transparent information about and increasing the salience of the retirement system when policymakers aim to boost pension savings. Such efforts may have persistent effects as temporary policies to increase savings can change workers' attitudes towards and interest in saving (Blumenstock, Callen and Ghani, 2018), although competing evidence suggests that automatically enrolling employees into a workplace pension scheme does not create long-lasting saving habits (Choukhmane, 2021).

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Online Appendix

Behavioral Responses to a Pension Savings Mandate:
Quasi-experimental Evidence from Swiss Tax Data

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Contents

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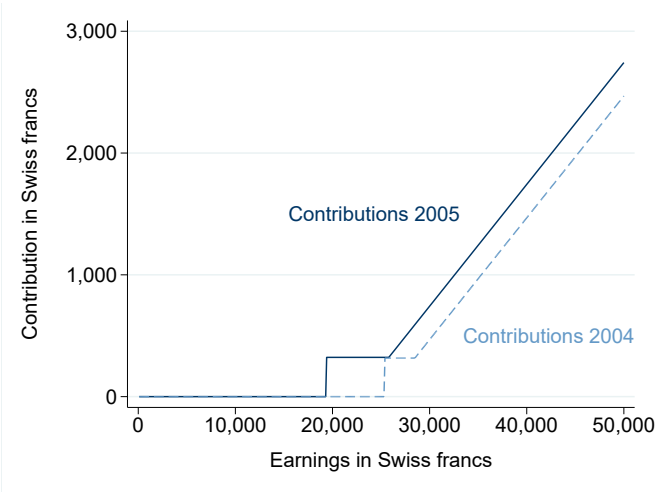
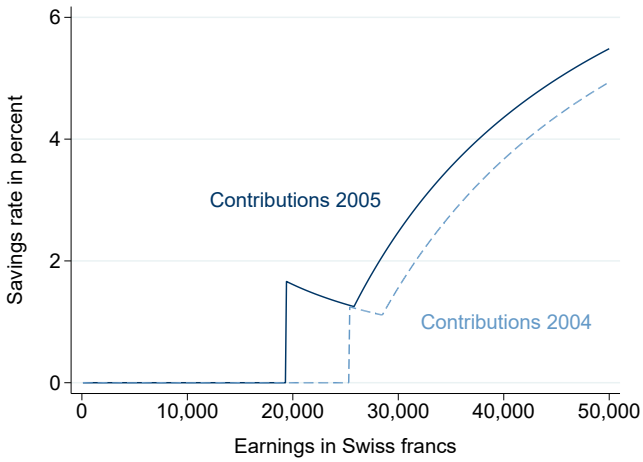
A Appendix Figures and Tables

Figure A.1: Occupational Pension Contributions in Relation to Earnings

(a) contribution levels in swiss francs



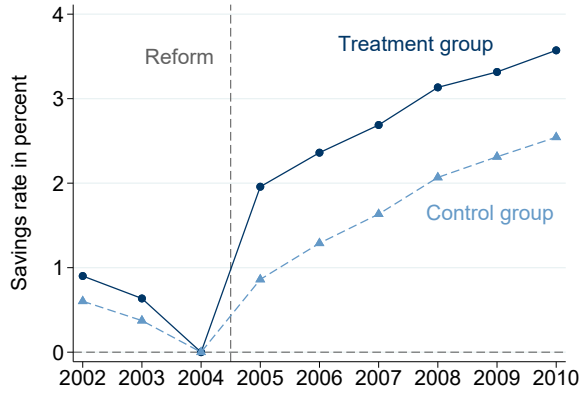
(b) savings rates as a percentage of earnings



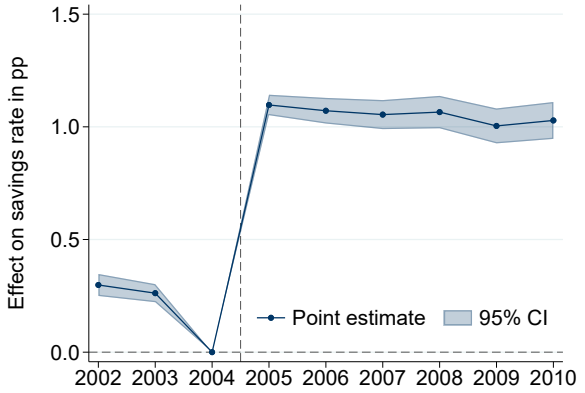
Notes: The figure displays the relationship between occupational pension contributions and gross earnings for an employee between 35 and 44 years of age, i. e. applying a statutory contribution rate of 10%, in 2004 and 2005. Panel A presents contribution levels in Swiss francs. Panel B shows savings rates as a percentage of gross earnings. The depicted years directly precede and succeed the reform of the occupational pension system described in Section 5.1. The relationship is similar in other years with slight variations depending on the year-specific parameters of the contribution schedule listed in Appendix Table B.1. See Section 2.1 for more detailed information on the occupational pension system in Switzerland.
Source: Author's illustration based on information from the Federal Social Insurance Office.

Figure A.2: Treatment Assignment: Occupational Pension Savings Rates

(a) mean occupational pension savings



(b) dynamic dd estimate



Notes: The figure displays the effect of the 2005 reform on occupational pension savings rates. Individuals are sorted into treatment and control group based on whether their pre-reform earnings were below or above the 2005 mandate threshold, as defined in Equation (3). Panel A displays the mean occupational pension savings rate separately for treatment and control group. Panel B displays year-specific difference-in-differences estimates relative to baseline year 2004 and corresponding 95% confidence intervals, obtained from estimating Equation (4) using the occupational pension savings rate as the dependent variable. Standard errors are clustered at the individual level. The dashed vertical line indicates the timing of the 2005 reform that expanded mandate coverage. See Section 3 for more information on data preparation and variable construction as well as Section 5 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.

Table A.1: Summary Statistics of DD Treatment and Control Group in 2004

	Treatment group				
	Mean	SD	P10	Median	P90
Age	41.92	7.33	31	42	52
Female	0.89	0.31	0	1	1
Married	0.77	0.42	0	1	1
Gross earnings main job	22,334	1,660	20,001	22,393	24,592
Net wealth	81,986	443,508	-19,948	24,377	215,059
Total savings rate (%)	21.1	159	-68.2	4.73	124
Occupational pension savings rate (%)	0	0	0	0	0
Occupational pension buy-in rate (%)	0.155	4.21	0	0	0
Private pension savings rate (%)	4.36	8.27	0	0	18.8
Private savings rate (%)	16.6	158	-71.9	2.19	115
Number of individuals			8,905		
	Control group				
	Mean	SD	P10	Median	P90
Age	41.15	7.28	31	41	51
Female	0.90	0.30	0	1	1
Married	0.78	0.41	0	1	1
Gross earnings main job	16,194	1,836	13,656	16,200	18,741
Net wealth	68,990	195,439	-16,862	23,564	194,577
Total savings rate (%)	22.1	178	-84.1	4.68	144
Occupational pension savings rate (%)	0	0	0	0	0
Occupational pension buy-in rate (%)	0.101	4.52	0	0	0
Private pension savings rate (%)	3.77	8.31	0	0	18.1
Private savings rate (%)	18.1	177	-87.2	2.56	134
Number of individuals			8,583		

Notes: Summary statistics, including mean, standard deviation, median, as well as percentiles 10 and 90, on key variables in the pre-reform year 2004 for individuals in the treatment and control group of the difference-in-differences analysis of the 2005 reform. Groups are defined based on gross earnings in the main job in 2004 according to Equation (3). Only individuals between 25 and 60 years of age with at least CHF 2,500 in earnings in every year between 2002 and 2010 are included. The bottom row in each panel reports the number of individuals in each group. All monetary values are reported in Swiss francs. Savings rates are calculated relative to gross earnings in the main job. All savings variables, except for occupational pension buy-ins, are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 5 for more detail on sample restrictions and estimation.
Source: Author's calculations based on administrative tax data from the canton of Bern.

B The Swiss Old-Age Provision System

This appendix provides a comprehensive description of the Swiss old-age provision system, focusing on years 2002 to 2017 which is the period covered by the data used in this paper.[2] Similar to other countries, the Swiss old-age provision system has three pillars: (i) a pay-as-you-go system ('old-age insurance'), (ii) an occupational pension system that is compulsory above an earnings threshold, and (iii) voluntary private pension accounts with a contribution cap. Appendix Table B.1 reports year-specific information on key parameters of the old-age provision system since the introduction of the occupational pension system in 1985.

B.1 Old-Age Insurance

Old-age insurance is organized as a pay-as-you-go scheme and compulsory for all individuals living or working in Switzerland between the age of 18 years and the statutory retirement age which is 64 years for women and 65 years for men. Between 2002 and 2017, the contribution rate applied to gross earnings was constant at 8.4%, of which employer and employees each pay half. The contribution of employees are deducted from their earnings by the employer and directly transferred to the social insurance administration. There is no cap on contributions, even if they do not lead to higher benefits. While benefit levels do depend on average earnings and the number of contribution years, the maximum benefit level is only double the minimum benefit level. From 2002 to 2017, the minimum annual benefit level was gradually increased from CHF 12,360 to CHF 14,100, while the maximum benefit level, accordingly, was rising from CHF 24,720 to CHF 28,200. This is relevant because key parameters of the occupational pension system are defined with respect to the maximum pension from old-age insurance as I explain in more detail in Section B.2. Benefit levels are usually adjusted every other year based on the evolution of an index reflecting the arithmetic mean of nominal wage growth and inflation.

B.2 Occupational Pension Scheme

In contrast to old-age insurance, the occupational pension system is fully funded. Female employees between 25 and 64 years of age as well as male employees between 25 and 65 years of age must be enrolled in an occupational pension fund by their employer if the annual gross earnings in their main job with the same employer

[2] For more information on the Swiss old-age provision system and relevant changes over time, see the website of the Federal Social Insurance Office: https://www.bsv.admin.ch/bsv/en/home/social-insurance.html [accessed on 20 October 2021].

exceed the threshold specified in the law. Employees cannot opt out of contributing to an occupational pension account if they meet the legal criteria.

Occupational pension funds are a key tool for wealth accumulation in Switzerland. Savings in occupational pension accounts total around 8% of GDP per year. Wealth accumulated in those accounts reached almost CHF 900 billion or 130% of GDP in 2017 (Federal Statistical Office, 2019).[3] According to data published by the Swiss National Bank, insurance and pension schemes – which mainly consists of capital in occupational pension funds – accounted for about 23% of total household wealth in Switzerland in 2020 (Annaheim and Heim, 2021).

The mandate threshold determining enrollment into occupational pension plans provides the identifying variation for both empirical strategies employed in the paper. From 1985 until 2004, the threshold had been equal to the maximum pension from old-age insurance in order to avoid overinsurance of the salary already covered by old-age insurance. As part of a multi-step reform of the occupational pension system that was implemented between 2004 and 2006, the threshold was lowered to 3/4 of the maximum benefit level in 2005, resulting in a drop of the threshold from CHF 25,320 in 2004 to CHF 19,350 in 2005. Before and after the reform, the threshold was gradually increased in proportion to the rising maximum old-age insurance benefit level.[4] Note that the mandate only applies to earnings in the main occupation.[5] Employees with multiple jobs who exceed the mandate threshold only when summing up multiple salaries can join an occupational pension fund on a voluntary basis. Yet, the number of individuals making use of that option seems to be negligible (Ecoplan, 2010).[6] Self-employed individuals can also voluntarily join an occupational pension plan in which case they subject themselves to the cap on tax-deductible private pension savings that applies to employees (see Section B.3).

To compute occupational pension contributions, the 'coordinated salary' is multiplied by the age-specific contribution rate.[7] The coordinated salary is equal to gross earnings minus a deduction. Similar to the mandate threshold, the deduction's purpose is to prevent overinsurance. It had been equal to the threshold between 1985 and

[3]Switzerland's GDP in 2017 was CHF 694 billion. Swiss GDP data are available from the State Secretariat for Economic Affairs: https://www.seco.admin.ch/seco/en/home/wirtschaftslage--wirtschaftspolitik/Wirtschaftslage/bip-quartalsschaetzungen-/daten.html [accessed on 19 October 2021].

[4]Figure 3 in Section 5.1 displays the full evolution of the earnings threshold over time.

[5]If employees are not working for the same employer for the whole year, the mandate threshold applies to the hypothetical earnings that they would have received if they had worked at that salary for the full year. However, employees must be on a permanent contract or work for the same employer for at least three months. I cannot distinguish these individuals from those who are not subject to the mandate because the tax data only contain information on annual earnings (see Appendix Section C.2). But this does not appear to affect a large number of individuals.

[6]This finding holds up in more recent data, see Schöchli (2021).

[7]See Equation (7) in Appendix Section C.4 for the computation of occupational pension savings.

2004, but in the 2005 reform the threshold was reduced by more than the deduction. Since then, the deduction equals 7/8 of the maximum benefit from old-age insurance while the threshold is 3/4 as mentioned earlier. The deduction ranges between CHF 22,575 and CHF 25,320 during the sample period.

The statutory contribution rates are 7% for employees aged 25–34, 10% for those aged 35–44, 15% for those aged 45–54, and 18% for women aged 55–64 and men aged 55–65.[8] Employers are legally obliged to pay at least 50% of the contribution. Occupational pension funds have some autonomy in designing their plans if they want to go beyond the minimum standards defined in the law (Dorn and Sousa-Poza, 2003).

For employees whose earnings are only slightly higher than the mandate threshold, there is a minimum coordinated salary equivalent to 1/8 of the maximum old-age insurance pension, varying between CHF 3,090 in 2002 and CHF 3,525 in 2017. Their contributions are calculated by applying the contribution rate to the minimum coordinated salary, not just to the marginal earnings above the threshold or deduction. Hence, there is a discontinuity in occupational pension savings at the cutoff.

If employees' earnings exceed an upper bound, there is a maximum coordinated salary equal to the upper bound minus the deduction. The upper bound is defined as three times the maximum pension from old-age insurance, gradually increasing from CHF 74,160 in 2002 to CHF 84,600 in 2017.[9] Furthermore, many occupational pension funds allow one-off 'buy-ins' that individuals can make above and beyond the regular contributions. These can also be deducted from taxable income.

To illustrate the calculation of contributions, and in addition the impact of the 2005 reform, Appendix Figure A.1 displays the relationship between occupational pension savings and gross earnings for an employee aged 35–44 (contribution rate of 10%). Panel A depicts the contribution level in Swiss francs; Panel B shows the savings rate as a percentage of gross earnings which is the transformation of savings outcomes that I use consistently throughout the empirical analyses. The discontinuity in occupational pension savings at the cutoff is clearly visible in this figure. This relationship follows the same pattern in all other years with slight differences based on the year-specific parameters listed in Appendix Table B.1.

Retirees can choose to receive occupational pension benefits as some combination of a lifelong annuity and a lump-sum payment. By law, they are entitled to receive at

[8] Women's legal retirement age in the occupational pension system was raised from 62 to 64 years in 2005 in order to be aligned with the rules of old-age insurance. Because of the lower retirement age, the statutory contribution rates applied to slightly different age groups for women before 2005 – specifically, 7% for the age group 25–31 years, 10% for the age group 32–41 years, 15% for the age group 42–51 years, and 18% for the age group 52–62 years.

[9] Most occupational pension funds provide insurance for the portion of the salary above the upper bound on a voluntary basis (Bütler, 2009). This practice is not relevant for employees in the earnings range studied in this paper.

least a quarter of their occupational pension capital as a lump sum.

The transformation of pension capital into an annuity is subject to a legally defined minimum conversion rate.[10] The rate was reduced gradually from 7.2% to 6.8% over the sample period as a consequence of the reform implemented between 2004 and 2006.[11]. Old-age insurance and occupational pension benefits are designed to jointly replace around 60% of pre-retirement earnings.

The tax treatment of occupational pension savings is quite favorable. Contributions and investment returns are exempt from income tax, and there is no capital gains tax in Switzerland. Moreover, pension capital is exempt from wealth tax. Benefits are subject to tax: while standard income tax is levied on annuities, lump-sum receipts are taxed at special, rather advantageous rates.

B.3 Private Pension Savings

Finally, there is the option of making voluntary contributions to designated private pension accounts. Because these savings can be deducted from taxable income, they are subject to a contribution cap. Employees enrolled in an occupational pension fund are allowed to make annual contributions to private pension accounts of up to 8% of the upper bound of the coordinated salary in the second pillar (equivalent to 24% of the maximum old-age insurance pension benefit). This cap has gradually increased from CHF 5,933 in 2002 to CHF 6,768 in 2017. Self-employed individuals who voluntarily join an occupational pension fund are subject to the same cap on private pension savings as employees.[12]

Private pension accounts are set up independently from occupational pension accounts, either at a bank or insurance company. The law allows contributions to standard savings accounts as well as investments into stocks and other securities. Use of this savings vehicle is fairly widespread: private pension capital in designated accounts totalled CHF 121 billion or 17.4% of GDP in 2017.[13]

Until people enter retirement, they have limited access to the accumulated capital in these designated private pension accounts. Once eligible, disbursement can be in

[10]The minimum conversion rate is only binding for the calculation of benefits based on pension capital deriving from the coordinated salary. It does not apply to voluntary occupational pension savings.

[11]The conversion rate translates the occupational pension capital into annual benefit entitlements. Accordingly, CHF 100,000 are converted into an annuity of CHF 6,800–7,200 depending on the year-specific conversion rate.

[12]Self-employed individuals who are not voluntarily enrolled in an occupational pension fund can contribute up to 20% of their income but at most 40% of the upper bound of the coordinated salary in the occupational pension system (ranging between CHF 29,664 in 2002 and CHF 33,840 in 2017) to private pension accounts.

[13]Statistics on the occupational pension and private pension system are available from the Federal Social Insurance Office: https://www.bsv.admin.ch/bsv/en/home/social-insurance/bv/statistik.html [accessed on 20 October 2021].

the form of an annuity or lump sum.

The tax treatment of private pension savings is similar to occupational pension savings: contributions can be deducted from taxable income, capital is exempt from wealth tax, while annuities are subject to income tax and lump-sum payments are taxed at preferential rates at the time of receipt.

Table B.1: Key Parameters of the Old-Age Provision System in Switzerland

	Old-age insurance		Occupational pension system				Private pension savings
Year	Minimum benefit	Maximum benefit	Threshold	Deduction	Upper bound	Min. coord. salary	Contribution cap
1985	8,280	16,560	16,560	16,560	49,680	2,070	-
1986	8,640	17,280	17,280	17,280	51,840	2,160	-
1987	8,640	17,280	17,280	17,280	51,840	2,160	4,147
1988	9,000	18,000	18,000	18,000	54,000	2,250	4,320
1989	9,000	18,000	18,000	18,000	54,000	2,250	4,320
1990	9,600	19,200	19,200	19,200	57,600	2,400	4,608
1991	9,600	19,200	19,200	19,200	57,600	2,400	4,608
1992	10,800	21,600	21,600	21,600	64,800	2,700	5,184
1993	11,280	22,560	22,560	22,560	67,680	2,820	5,414
1994	11,280	22,560	22,560	22,560	67,680	2,820	5,414
1995	11,640	23,280	23,280	23,280	69,840	2,910	5,587
1996	11,640	23,280	23,280	23,280	69,840	2,910	5,587
1997	11,940	23,880	23,880	23,880	71,640	2,985	5,731
1998	11,940	23,880	23,880	23,880	71,640	2,985	5,731
1999	12,060	24,120	24,120	24,120	72,360	3,015	5,789
2000	12,060	24,120	24,120	24,120	72,360	3,015	5,789
2001	12,360	24,720	24,720	24,720	74,160	3,090	5,933
2002	12,360	24,720	24,720	24,720	74,160	3,090	5,933
2003	12,660	25,320	25,320	25,320	75,960	3,165	6,077
2004	12,660	25,320	25,320	25,320	75,960	3,165	6,077
2005	12,900	25,800	19,350	22,575	77,400	3,225	6,192
2006	12,900	25,800	19,350	22,575	77,400	3,225	6,192
2007	13,260	26,520	19,890	23,205	79,560	3,315	6,365
2008	13,260	26,520	19,890	23,205	79,560	3,315	6,365
2009	13,680	27,360	20,520	23,940	82,080	3,420	6,566
2010	13,680	27,360	20,520	23,940	82,080	3,420	6,566
2011	13,920	27,840	20,880	24,360	83,520	3,480	6,682
2012	13,920	27,840	20,880	24,360	83,520	3,480	6,682
2013	14,040	28,080	21,060	24,570	84,240	3,510	6,739
2014	14,040	28,080	21,060	24,570	84,240	3,510	6,739
2015	14,100	28,200	21,150	24,675	84,600	3,525	6,768
2016	14,100	28,200	21,150	24,675	84,600	3,525	6,768
2017	14,100	28,200	21,150	24,675	84,600	3,525	6,768
2018	14,100	28,200	21,150	24,675	84,600	3,525	6,768
2019	14,220	28,440	21,330	24,885	85,320	3,555	6,826
2020	14,220	28,440	21,330	24,885	85,320	3,555	6,826
2021	14,340	28,680	21,510	25,095	86,040	3,585	6,883

Notes: Year-specific parameters of the old-age provision system in Switzerland since the introduction of the occupational pension system in 1985. All values are reported in Swiss francs. The option to contribute to preferentially taxed private pension accounts was established in 1987. The cap on these private pension savings applies to individuals enrolled in an occupational pension fund which includes all employees. See Appendix B for more detailed information on the Swiss old-age provision system.
Source: Official information from the Federal Social Insurance Office.

C Data Appendix

In this appendix, I describe the administrative data provided by the tax authority of the canton of Bern in more detail. I start by explaining the general preparation of the dataset for the empirical analysis. Subsequently, I turn to the income, wealth, and savings information primarily used in this paper. Finally, I discuss sample restrictions and present summary statistics.

C.1 Data Preparation

In Switzerland, the relevant tax unit is the household. Thus, married couples jointly file one tax return. Because the savings mandate applies at the individual level, I follow the approach of Brunner, Meier and Näf (2020) and Fagereng et al. (2020) and split up married couples into individual observations, equally assigning half of the income and wealth of those components that are only reported at the household level to each partner.[14] Most important for my analysis, earnings from employment are reported at the individual level, as are transfer income and pension income. The same is true for occupational pension buy-ins and private pension savings.

C.2 Income

The occupational pension savings mandate applies to gross earnings in the main job, before any deductions are applied. The tax records contain separate information for earnings in the main occupation and side jobs. The definition of earnings in the main job used by the tax administration overlaps to a large degree with the definition relevant for the savings mandate (see Section 2.1). Both refer to the total earnings with the same employer. There is a difference in that the tax authority includes positions at multiple employers as part of the main job if they are similar in terms of working hours or income received, while the savings mandate applies only to the main occupation at one employer. Yet, this distinction is unlikely to be relevant for many employees in the data, so the resulting measurement error is limited.

Employees report earnings net of social insurance and occupational pension contributions to the tax administration because these are deducted directly from their salary by the employer. Therefore, I compute gross earnings based on net earnings recorded in the tax data and the year-specific social insurance and occupational pension schedules. Because the social insurance and occupational pension contributions vary along the earnings distribution, the calculation must differentiate between cer-

[14] All wealth information is reported at the household level. Income variables reported at the household level include self-employment income, business income, financial income, and real estate income.

tain earnings ranges.[15] Taking an individual subject to the savings mandate for whom neither the minimum nor the maximum coordinated salary of the occupational pension system is binding as an example, the calculation must take into account that social insurance contribution rates are applied to total gross earnings and occupational pension contribution rates are applied to gross earnings above the deduction. Overall, gross earnings G_{it} in the main job of employee i in year t can be imputed from net earnings as

$$G_{it} = \begin{cases} \frac{N_{it}}{1-i^e_{it}} & \text{if} \quad N_{it} < (C_t \times (1-i^e_{it}) - p^e_{it} \times M_t) \\ \frac{N_{it}+p^e_{it}\times M_t}{1-i^e_{it}} & \text{if} \quad N_{it} \geq (C_t \times (1-i^e_{it}) - p^e_{it} \times M_t) \quad \text{and} \\ & \quad\; N_{it} < ((D_t + M_t) \times (1-i^e_{it}) - p^e_{it} \times M_t) \\ \frac{N_{it}-p^e_{it}\times D_t}{1-i^e_{it}-p^e_{it}} & \text{if} \quad N_{it} \geq ((D_t + M_t) \times (1-i^e_{it}) - p^e_{it} \times M_t) \quad \text{and} \\ & \quad\; N_{it} < (B_t \times (1-i^e_{it}) - p^e_{it} \times (B_t - D_t)) \\ \frac{N_{it}+p^e_{it}\times (B_t-D_t)}{1-i^e_{it}} & \text{if} \quad N_{it} \geq (B_t \times (1-i^e_{it}) - p^e_{it} \times (B_t - D_t)), \end{cases} \tag{6}$$

where N_{it} denotes net earnings, C_t is the cutoff value of the savings mandate, D_t is the deduction, M_t is the minimum coordinated salary, and B_t is the upper bound in the occupational pension system, while i^e_{it} represents the employee share of the social insurance contribution rate, and p^e_{it} represents the employee share of the age-specific occupational pension contribution rate.[16] Employees and employers each pay half of total social insurance contributions. Equally, I set the employee share of occupational pension contributions to 50 % which is the maximum employee share defined in the law. Some employers may bear more than 50% (which cannot be observed in the tax data), so this may result in slight measurement error.

Note that due to the minimum coordinated salary, gross earnings slightly below and slightly above the mandate threshold can result in the same net earnings recorded in the tax data. Thus, gross earnings cannot be unambiguously imputed from net earnings for a small number of individuals. This problem only concerns a narrow earnings range of not more than CHF 350 below and above the threshold (with the exact width depending on the age-specific contribution rate and the year-specific

[15] Note that the definition of the different earnings ranges with respect to net earnings in Equation (6) is equivalent to the definition in terms of gross earnings in Equation (7).

[16] Social insurance contributions include contributions for old-age insurance, invalidity insurance, loss of earnings compensation, and unemployment insurance. Above a certain high level of earnings, unemployment insurance contribution rates are reduced in most years that I have data on. I account for the schedule of unemployment insurance contributions when calculating gross earnings but omit it from Equation (6) because it does not matter for individuals in the earnings range of interest for this paper. More information on the structure of the social insurance system in Switzerland and historical contribution schedules are available on the website of the Federal Social Insurance Office: https://www.bsv.admin.ch/bsv/de/home/sozialversicherungen/ueberblick/beitraege.html [accessed on 23 October 2021].

minimum coordinated salary). As defined in Equation (6), I treat all individuals with ambiguous gross earnings as if they were above the mandate threshold. In the regression discontinuity analysis in Section 4 and the difference-in-differences analysis in Section 5, I address this issue by using a 'donut hole' approach that removes the problematic observations.

The tax records also include information on other types of income which are less relevant for this paper such as self-employment income, business income, financial income, real estate income, transfer income, and pension income.

C.3 Wealth

The tax records contain detailed information on wealth and its composition, including business wealth, financial wealth, real estate, other types of wealth, and debt. The Swiss wealth tax is quite comprehensive, covering all types of assets except for pension wealth in occupational and private pension accounts which is thus missing from the data. This is not a problem for the empirical analysis in this paper, because savings can be observed or computed even without directly observing pension wealth. Information on pension contributions is sufficient because pension wealth generally cannot be accessed during the work life.

It needs to be noted that the valuation of real estate for tax purposes systematically underestimates the true market value. As a rule of thumb, real estate is valued at around 60% of its market value in Switzerland (OECD, 2018*c*), although in individual cases the valuation may deviate significantly from that benchmark. To analyze the discrepancy between tax value and market value of real estate, the tax administration of the canton of Bern conducted an analysis comparing the observed price of all housing transactions in a given year to the value of these properties in the tax records.[17] On average, the tax value of real estate was 71% of its market value in 2002. Because there was no revaluation during the sample period and real estate prices in Bern have generally been increasing, the tax value has gradually declined to about 55% of the market value in 2017.

C.4 Savings

Various types of pension and private savings could be affected by the occupational pension savings mandate. Some of these are directly observable in the tax data; others need to be computed based on information available in the dataset.

[17] The analysis of real estate valuation for tax purposes is available on the website of the tax authority of the canton of Bern: https://www.sv.fin.be.ch/sv_fin/de/index/navi/index/steuersituationen/kauf-verkauf_liegenschaft/amtlicher_wert/allgemeine-neubewertung20.html [accessed on 23 October 2021].

As mentioned earlier, occupational pension contributions are withheld at source by the employer, so they are not recorded in the tax data. I impute these by applying the statutory contribution rates – the sum of the employer and employee shares – to individuals' gross earnings in the main job. Based on the contribution schedule explained in Section 2.1, occupational pension savings S_{it}^{occ} of employee i in year t are calculated as

$$S_{it}^{occ} = \begin{cases} 0 & \text{if} \quad G_{it} < C_t \\ p_{it} \times M_t & \text{if} \quad G_{it} \geq C_t \quad \text{and} \quad (G_{it} - D_t) < M_t \\ p_{it} \times (G_{it} - D_t) & \text{if} \quad G_{it} \geq C_t \quad \text{and} \quad (G_{it} - D_t) \geq M_t \\ p_{it} \times (B_t - D_t) & \text{if} \quad G_{it} > B_t, \end{cases} \tag{7}$$

where p_{it} is the age-specific total occupational pension contribution rate, i. e. employer and employee share. All other variables are defined as in Equation (6). Panel A in Appendix Figure A.1 illustrates how occupational pension savings are computed from gross earnings.

Private pension savings in preferentially taxed accounts as well as voluntary occupational pension buy-ins are directly observed at the individual level in the tax data because they must be reported in the tax return in order to be deductible from taxable income.

Finally, I compute private savings as the change in net wealth relative to the previous year. Hence, private savings $S_{i,t}^{priv}$ of individual i in year t are given by

$$S_{i,t}^{priv} = W_{i,t} - W_{i,t-1}, \tag{8}$$

where $W_{i,t}$ denotes net wealth of individual i in year t. Net wealth represents the difference between gross wealth – the summed value of all asset categories – and debt, and can be directly observed in the tax records. This savings concept can be described as *gross savings* as it includes accrued capital gains from changes in asset prices (which are sometimes called 'passive savings' in the literature) (see Fagereng et al., 2021). Note that my measure of private savings does not capture capital gains on real estate because there has not been a revaluation of real estate during the observed period (see Section C.3). I cannot separate gross savings into *net savings* (sometimes called 'active savings') and capital gains because realized capital gains are not observed as Switzerland does not have a capital gains tax and I do not have data down to the level of single assets or transactions.[18] An implication of this savings definition is that

[18]The latter is a common challenge in the literature on the measurement of savings. A few papers leverage very detailed administrative data to distinguish net savings and capital gains, including Bach, Calvet and Sodini (2020), Fagereng et al. (2020), and Fagereng et al. (2021). Fagereng et al. (2021) provide a highly insightful discussion of the distinction between gross and net savings and its implications, using both economic theory and an empirical application drawing on Norwegian data.

private savings are highly variable due to the fluctuations in capital gains. Another reason for the considerable variance of private savings is the purchasing timing of durable goods, services, and lumpy non-durables (Chetty et al., 2014). This makes obtaining precise estimates of effects on private savings more difficult.

Total savings are defined as the sum of all savings variables discussed above, including occupational pension savings, occupational pension buy-ins, private pension savings, and private savings.

For the empirical analyses, I transform the savings measures into savings rates as a percentage of gross earnings in the main job. The savings rate s_{it} of individual i in year t can be straightforwardly defined as

$$s_{it} = \frac{S_{it}}{G_{it}}, \tag{9}$$

where S_{it} is a savings variable of interest and G_{it} is gross earnings in the main job, as discussed before.

C.5 Sample Restrictions

To prepare the data for empirical analysis, I remove a number of observations that are unreliable or incomparable to standard taxpayers (similar to Brunner, Meier and Näf, 2020). This group includes individuals who are only taxed for part of the year because they move abroad or arrive from abroad (1.9% of all observations), individuals who failed to hand in a tax return and are assessed by the tax authority (2.8%), duplicate observations for individuals in the same year (0.6%), and observations with obvious errors in the reported information (0.1%). Because the savings mandate only applies to individuals between 25 years of age and retirement age (varying by gender and year), I restrict the sample to individuals who are between 25 and 60 years old. This step removes 39% of the observations in the data. In both empirical strategies pursued in the paper, the sample is restricted further as explained in Sections 4 and 5, respectively.

C.6 Summary Statistics

Appendix Table C.1 displays summary statistics on demographics, income, wealth, and savings for this dataset covering the full population of the canton of Bern between 25 and 60 years of age, pooling the time period between 2002 and 2017. I report mean, standard deviation, median, as well as percentiles 10 and 90. The panel dataset contains around 7.3 million individual-year observations from 767,369 unique individuals. The number of observations for which I have data on private savings and

total savings is somewhat smaller than the overall population as I cannot measure private savings in 2002 – the first year in the data – due to not having information on net wealth in 2001. Panel A presents information on demographic characteristics. Panel B shows statistics on all components of income. In the empirical analysis of the effects of the occupational pension savings mandate, I focus on gross earnings in the main job as it is the income definition that the mandate applies to. The table documents that it is by far the most important type of income for the average individual in working age. Panel C provides information on net wealth and its composition. Panel D displays information on total savings and its components. The mean total savings per year are roughly CHF 17,000. On average, the most important savings component is private savings, followed by occupational pension savings, and private pension savings. The fraction of individual-year observations reporting positive total savings is 82.5%. The corresponding share for the individual savings components is 68.7% for occupational pension savings, 2.6% for occupational pension buy-ins, 42.9% for private pension savings, and 66.3% for private savings.

Note that the characteristics of the employees studied in the regression discontinuity analysis in Section 4 and the difference-in-differences analysis in Section 5 may differ quite strongly from the summary statistics for the overall population because they are a particular subgroup with relatively low earnings. Accordingly, the population statistics reported in Table C.1 mainly serve to provide a broad overview of the overall distribution of demographics, income, wealth, and savings in the canton of Bern and a benchmark that the specific subpopulations can be compared to.

Table C.1: Summary Statistics on Working-Age Individuals

	Mean	SD	P10	Median	P90	Obs.
Panel A: Demographics						
Age	43.22	10.05	29	44	57	7,307,495
Female	0.51	0.50	0	1	1	7,307,495
Married	0.58	0.49	0	1	1	7,307,495
Number of children	0.77	1.06	0	0	2	7,307,495
Panel B: Income						
Total income	59,179	115,152	3,272	55,931	110,675	7,307,495
Gross earnings main job	52,806	53,241	0	50,682	107,895	7,307,495
Gross earnings side job	692	4,149	0	0	408	7,307,495
Self-employment income	4,535	25,362	0	0	6,404	7,307,495
Business income	482	11,181	0	0	0	7,307,495
Financial income	1,598	84,271	0	64	1,465	7,307,495
Real estate income	-3,541	22,122	-7,733	0	160	7,307,495
Transfer income	657	4,123	0	0	0	7,307,495
Pension income	1,486	7,552	0	0	0	7,307,495
Other income	465	46,649	0	0	263	7,307,495
Panel C: Wealth						
Net wealth	128,883	5,776,165	-19,891	24,125	259,116	7,307,495
Business wealth	10,681	126,461	0	0	2,769	7,307,495
Financial wealth	103,662	5,453,162	0	18,684	161,863	7,307,495
Real estate	106,128	388,002	0	0	301,225	7,307,495
Other wealth	7,728	268,638	0	0	9,450	7,307,495
Debt	-103,056	403,516	-300,000	-1,689	0	7,307,495
Panel D: Savings						
Total savings	16,978	2,211,579	-11,178	6,000	40,744	6,595,087
Occupational pension savings	3,262	3,286	0	2,527	8,721	7,307,495
Occupational pension buy-ins	698	10,216	0	0	0	7,307,495
Private pension savings	2,145	3,164	0	0	6,682	7,307,495
Private savings	10,668	2,211,494	-16,007	978	30,761	6,595,087

Notes: Summary statistics, including mean, standard deviation, median, as well as percentiles 10 and 90, on demographic characteristics, income, wealth, and savings of all individuals between 25 and 60 years old in the canton of Bern, pooling data from 2002 to 2017. All monetary values are reported in Swiss francs. The rightmost column reports the number of individual-year observations for each variable. The panel dataset contains 767,369 unique individuals. Individuals who are only taxed for part of the year, individuals who failed to hand in a tax return, duplicate observations for individuals in the same year, and observations with obvious errors are excluded. See Section 3 for more information on data preparation and variable construction.
Source: Author's calculations based on administrative tax data from the canton of Bern.

D Validity of the RD Assumptions

In this appendix, I provide empirical support for the validity of the RD approach by testing the implications of the continuity assumption. Before I present the results of these validity tests, some words on the a priori likelihood of a violation of the identifying assumption are in order.

The main threat to identification is manipulation of the running variable.[19] If individuals were able to manipulate their earnings strategically and precisely to sort around the threshold, potential discontinuities in savings outcomes could be driven by ex-ante differences in characteristics rather than the savings mandate. This behavior would result in the RD estimate being plagued by selection bias.[20]

The Swiss government usually announces the value of the mandate threshold between the middle of September and the middle of October before it comes into force on the 1st of January. It does not seem likely that employees can easily adapt their earnings in a precise manner by adjusting their working hours in their current position or switching into a new job on short notice (or demanding a lower or higher wage rate, for that matter). Note that taking on a second job would not affect treatment status because the mandate threshold applies to earnings in the main job only. Employers could also try to manipulate the position of their employees around the threshold in order to avoid paying the employer share of occupational pension contributions.

In the end, whether there is endogenous sorting is an empirical question. I investigate sorting in the following by examining the continuity of the density of the running variable and covariates across the threshold. To further assess the credibility of the RD assumptions, I subsequently conduct a placebo test using a range of hypothetical cutoff values for which no discontinuity in the outcomes should be found.

D.1 Density Test

I test for a discontinuity in the density of earnings at the mandate threshold following the idea of manipulation testing introduced by McCrary (2008). Figure D.1 shows the

[19] The other main concern about the validity of the identifying assumption in RD designs is the possibility of other policies using the same cutoff value (Imbens and Lemieux, 2008). This issue can be ruled out in this application as no other policy targets this exact threshold. The reason is that it is based on the specific structure of the Swiss old-age provision system, being defined as the part of the salary that is already covered by old-age insurance (see Appendix B for more detail).

[20] Manipulation could go both ways with individuals self-selecting to be above or below the threshold in line with their savings preferences. Being subject to the mandate implies lower take-home pay, while not being covered by the policy means losing the employer share of occupational pension contributions which has to be at least 50 percent (abstracting from issues of incidence). Studying increases in employer Social Security contributions in France, Bozio, Breda and Grenet (2020) find evidence of full pass-through to wages if there is a strong and salient tax-benefit linkage, as is the case in the Swiss occupational pension system.

Figure D.1: Frequency Distribution of Annual Earnings in Main Job

Notes: The figure displays the frequency distribution of annual earnings in the main job within CHF 9,000 of the threshold of the pension savings mandate in the canton of Bern in 2017. The dashed vertical line indicates the threshold at CHF 21,150. Individuals within CHF 350 of the cutoff whose gross earnings cannot be imputed unambiguously are excluded. The lighter bars flag potential bunchers who are removed from RD estimation in robustness checks in Section E.3. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions.

Source: Author's calculations based on administrative tax data from the canton of Bern.

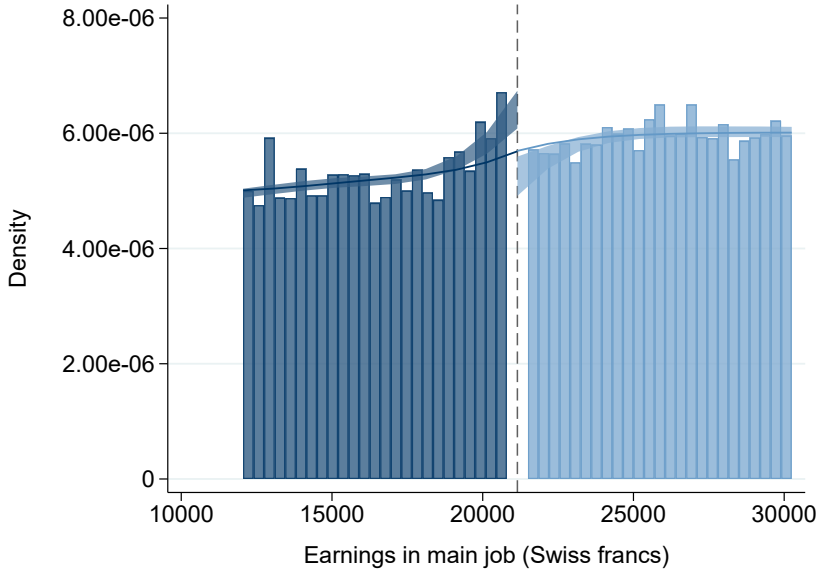
frequency distribution of annual earnings in the main job within CHF 9,000 of the cutoff in 2017. There is some missing mass around the threshold because individuals within CHF 350 of the cutoff are excluded from the analysis as their gross earnings cannot be imputed unambiguously (see Section C.2). This affects 1,290 individuals, representing 2.9% of the 44,369 individuals within the estimation window.

Although the density looks quite smooth overall, there appears to be some excess mass below but close to the cutoff, as indicated by the lighter bars in the graph. In Panel A of Figure D.2, I plot the results of the density test proposed by Cattaneo, Jansson and Ma (2018, 2020). The null hypothesis of no manipulation is rejected at the 1% level. This finding suggests that there could be some sorting below the threshold due to strategic behavior in response to the savings mandate.

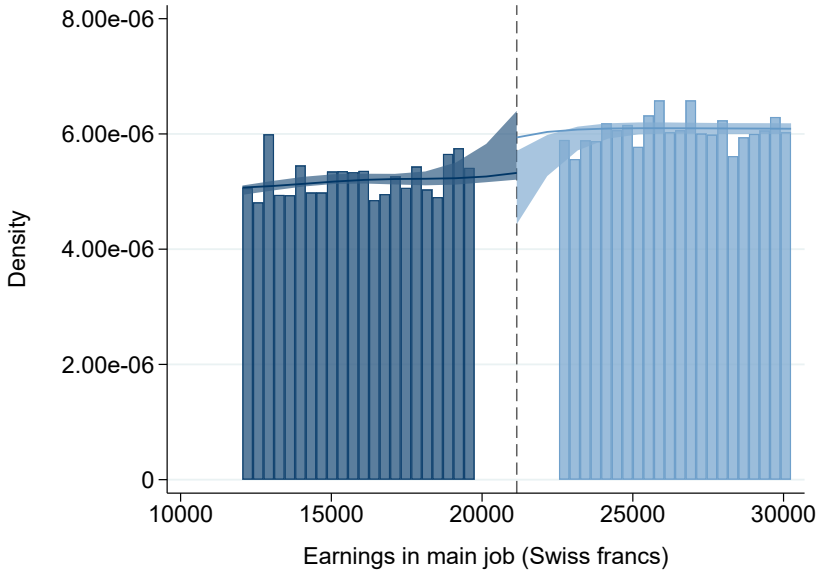
I investigate the possibility of earnings manipulation in numerous ways, concluding that it is unlikely to bias the RD results. *First*, the excess mass is modest compared to the total frequencies, suggesting that the impact of potential selection bias is limited. *Second*, there is no sign of a decline in the density just above the threshold which would usually be expected if some marginal earners bunched below the cutoff. *Third*, I repeat the RD analysis using a 'donut hole' approach that removes the potential

Figure D.2: Density Test for Annual Earnings in Main Job

(a) baseline distribution



(b) excluding potential bunchers



Notes: The figure displays the results of the density test proposed by Cattaneo, Jansson and Ma (2018, 2020) for the distribution of annual earnings in the main job within CHF 9,000 of the threshold of the occupational pension savings mandate in the canton of Bern in 2017. The bars represent the histogram of the distribution; the lines represent bias-corrected density estimates using a local quadratic model; the shaded areas represent valid confidence bands. The dashed vertical line indicates the threshold at CHF 21,150. Panel A shows the baseline distribution which excludes employees within CHF 350 of the cutoff whose earnings cannot be imputed unambiguously; the corresponding p-value for the null hypothesis of no manipulation is 0.000. Panel B depicts the distribution removing employees within CHF 1,400 of the cutoff; the corresponding p-value is 0.100. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions.

Source: Author's calculations based on administrative tax data from the canton of Bern.

bunchers from the estimation sample, finding estimates that are similar to my main results (see Section E.3). Excluding the observations highlighted in Figure D.1 (a donut hole of CHF 1,400 above and below the threshold), the null hypothesis of no manipulation cannot be rejected at the 5% level, as Panel B of Figure D.2 shows. *Fourth*, I do not find substantial discontinuities in predetermined characteristics at the threshold, as documented in Section D.2. This suggests that manipulation of the running variable may not be a significant problem as otherwise it would be expected to affect the distribution of relevant covariates around the cutoff. *Fifth*, the RD results are quantitatively and qualitatively similar to the DD estimates presented in Section 5.3. This is reassuring because it is highly unlikely that the reform-based DD approach is affected by endogenous manipulation. The reason is that I use pre-reform earnings in 2004 to define treatment and control group, when all individuals in both groups were not yet subject to the savings mandate (see Section 5 for more detail).

In sum, even if there appears to be some sorting below the threshold, the evidence suggests that it does not bias the RD estimates. Analyzing registry data on gross earnings of all employees in Switzerland collected by the social insurance system, Ecoplan (2010) detects a similar excess mass below the threshold and attributes it to employers aiming to avoid paying the employer contribution share.

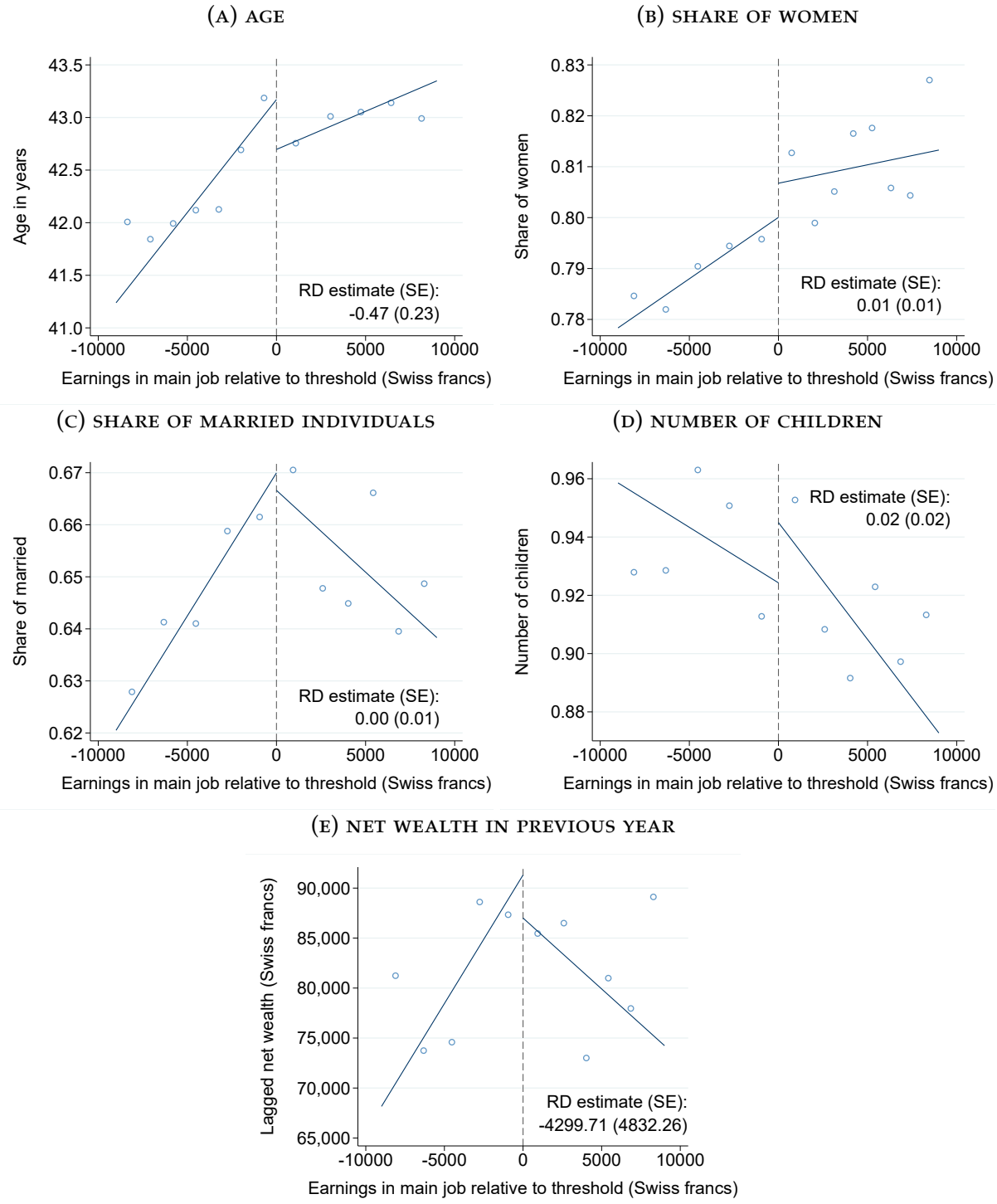
D.2 Continuity of Predetermined Covariates

Next, I consider the smoothness of a set of characteristics across the cutoff. If individuals were able to endogenously sort around the threshold, this would likely result in a discontinuity in predetermined covariates that are correlated with the savings outcomes. On the flip side, continuity of characteristics across the threshold suggests that individuals are not manipulating the running variable (although this condition is obviously neither necessary nor sufficient for identification).

I implement this balance test by separately running my main RD specification in Equation (2) without control variables using age, gender, marital status, number of children, and net wealth in the previous year as the outcome variable, respectively. The rationale for using lagged net wealth is the following: Wealth affects the (gross) savings rate (total change in net wealth, i. e. active savings plus accrued capital gains, see Section C.4), as Fagereng et al. (2021) show using very detailed administrative data from Norway, so it is an important covariate to check balance for.[21] That said, in order for net wealth to be a *predetermined* covariate not affected by the treatment, I take the lag by one year.

[21] Fagereng et al. (2021) find that net savings rates (excluding capital gains) are flat across most of the wealth distribution, while gross savings rates (including capital gains) increase considerably with wealth.

Figure D.3: Balance of Predetermined Covariates



Notes: Regression discontinuity plots of a set of predetermined covariates in 2017. Points are local sample means using non-overlapping quantile-spaced bins; lines are linear fits. The number of bins is selected using spacings estimators minimizing an asymptotic approximation to the integrated mean-squared error following the data-driven approach described in Calonico, Cattaneo and Titiunik (2015). Point estimates and standard errors are obtained from estimating Equation (2) without controls using a triangular kernel. The running variable is recentered around the threshold of the pension savings mandate at CHF 21,150, indicated by the dashed vertical line. Lagged net wealth is winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.

Figure D.3 shows the results of this exercise while simultaneously describing the general characteristics of individuals close to the cutoff. Panel A documents that the average age is roughly between 42 and 43 years. Panel B shows that around 80% are female which is not surprising given that women are much more likely to work part-time in Switzerland than men.[22] The share of married individuals depicted in Panel C is around two-thirds. The average number of children is slightly below one, as Panel D demonstrates. Panel E shows that mean net wealth is approximately between CHF 75,000 and CHF 95,000, depending on the position relative to the threshold. I explore heterogeneity in effect estimates with respect to these characteristics in Section F.

Concerning the balance of the predetermined covariates, Panel A shows that the RD estimate for age is significant at the 5% level (p-value = 0.043), suggesting that individuals narrowly above the threshold are on average half a year younger than those narrowly below. Although this does not seem like a large difference, I address concerns that this could reflect endogenous sorting by removing those observations close to the cutoff in a donut hole approach. The results presented in Section E.3 document that my estimates are not sensitive to excluding observations close to the threshold. Except for age, I do not find any statistically or quantitatively significant discontinuities of covariates at the threshold, implying that predetermined characteristics are smooth across the cutoff. Individuals with earnings just below and just above the threshold have on average similar characteristics which bolsters the case for comparing them in order to learn about the causal effects of the savings mandate.

D.3 Placebo Test

Finally, I conduct a placebo test. For that purpose, I re-run my main RD specification in Equation (2) using a range of hypothetical cutoffs. Because there is no discontinuity in the treatment assignment at the placebo thresholds, no significant effects on any savings outcome of interest should be found if the RD assumptions are valid. More formally, this amounts to testing whether continuity holds at other values of the support of the running variable. Although for identification the conditional expectation functions only need to be continuous exactly at the cutoff, it is implausible that this holds while there are discontinuities at other values of the running variable. For each placebo specification, I only use observations on one side of the threshold to avoid contamination from the (potential) discontinuity in the outcome at the true cutoff. I evaluate placebo thresholds of CHF 10,000 below as well as CHF 10,000, 15,000,

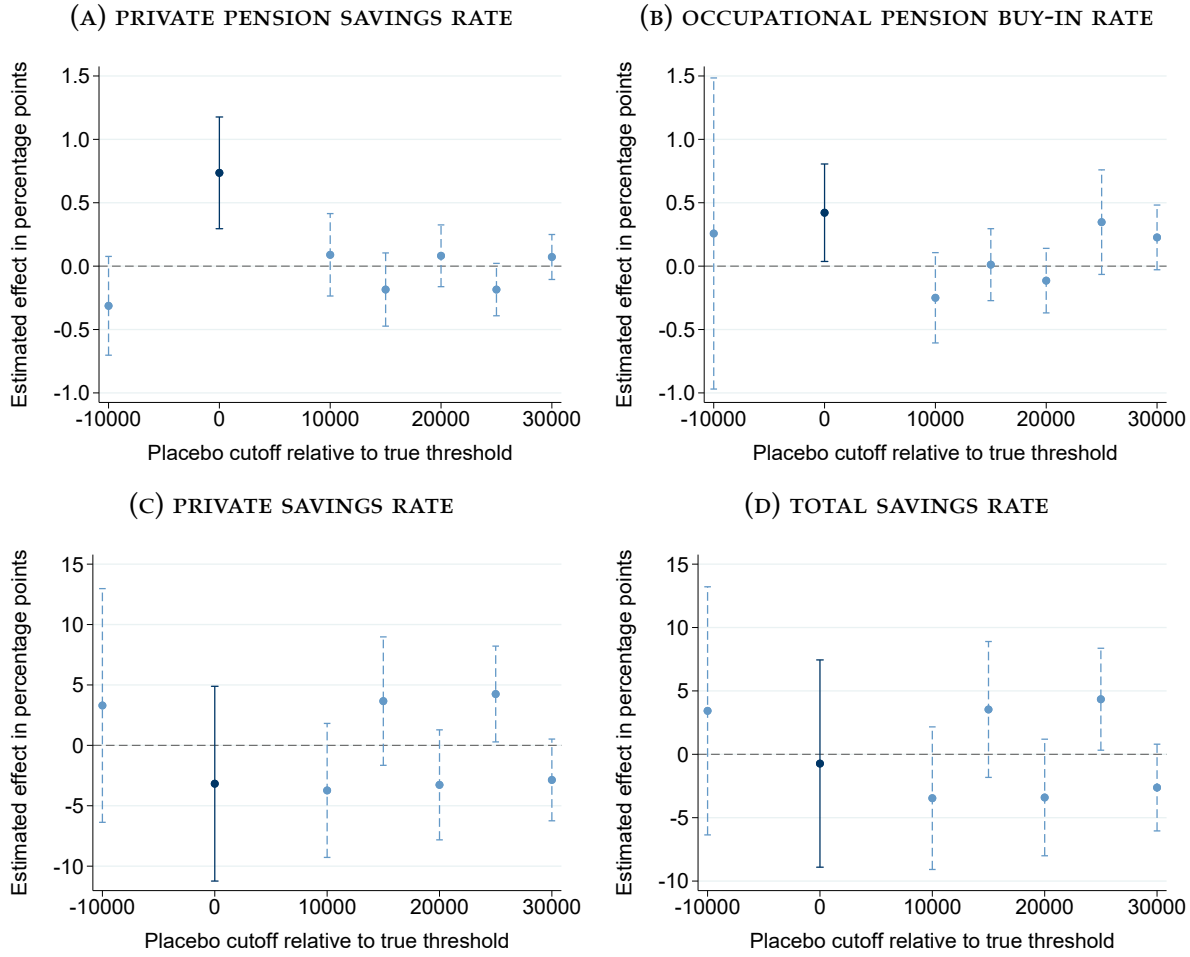
[22] 59% of women and 18% of men report to be working part-time in Switzerland in 2020, see the website of the Federal Statistical Office: https://www.bfs.admin.ch/bfs/en/home/statistics/work-income/employment-working-hours/labour-force-characteristics/full-time-part-time.html [accessed on 24 October 2021].

20,000, 25,000, and 30,000 above the true cutoff. Note that this range is asymmetric because I cannot go lower than CHF 10,000 under the threshold as the lower bound of the estimation window would otherwise go below zero.

The coefficients and 95% confidence intervals for the placebo cutoffs as well as the treatment effect estimates for the true threshold are plotted in Figure D.4. For private pension savings and occupational pension buy-ins, the only significant estimates are those at the true cutoff which clearly stand out from the results at the placebo thresholds in terms of both magnitude and significance. Regarding both private savings and total savings, for which the effect estimates at the true cutoff are not significantly different from zero in the first place, there is one narrowly significant discontinuity at CHF 25,000 above the threshold. However, given the variability of the RD estimates over the range of cutoffs considered, this seems to be driven by the high variance of the private savings measure. Since I do not find a significant treatment effect for those outcomes, I do not consider this one marginally significant placebo estimate a particular cause for concern.

In sum, the results of the validity tests suggest that the RD assumptions introduced in Section 4.2 hold. This conclusion is supported by the fact that the RD and DD estimates presented in this paper are quantitatively and qualitatively similar.

Figure D.4: Placebo Test



Notes: The figure displays regression discontinuity estimates and 95% confidence intervals for the effect of the occupational pension savings mandate on a set of savings outcomes in 2017, depending on using the true threshold or a range of placebo cutoffs defined relative to the true threshold. The dark marker represents the main result; the dashed light markers represent placebo estimates. Estimates are obtained by separately running the linear regression discontinuity model in Equation (2) using a triangular kernel for a range of cutoffs. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.

E Robustness of the RD Results

In this appendix, I extensively investigate the sensitivity of the RD results to different choices regarding the model specification in Equation (2) and its operationalization. I consider varying the bandwidth, kernel function, size of the donut hole, order of the local polynomial, and control variables, as well as conducting robust bias correction in turn. Further, I present separate effect estimates for each year in the data (2003–2017) and compare them to the headline results for 2017.

E.1 Bandwidth

Figure E.1 plots point estimates and 95% confidence intervals for the effect of the mandate on the savings outcomes of interest, showing the sensitivity of the estimates to varying the bandwidth between CHF 2,000 and CHF 20,000. Panels A through D document that using different bandwidths does not alter any of the findings obtained using the main bandwidth of CHF 9,000. The point estimates of the statistically significant positive effects on private pension savings (Panel A) and occupational pension buy-ins (Panel B) are remarkably robust to the size of the bandwidth and, although the confidence intervals naturally widen as the bandwidth shrinks, almost all estimates remain significant at the 5% level. The results for private savings and total savings are similarly robust. Panel C demonstrates that the estimated effects on private savings are all negative but insignificant. Panel D shows that all point estimates for total savings, except for very small bandwidths, are close to zero.

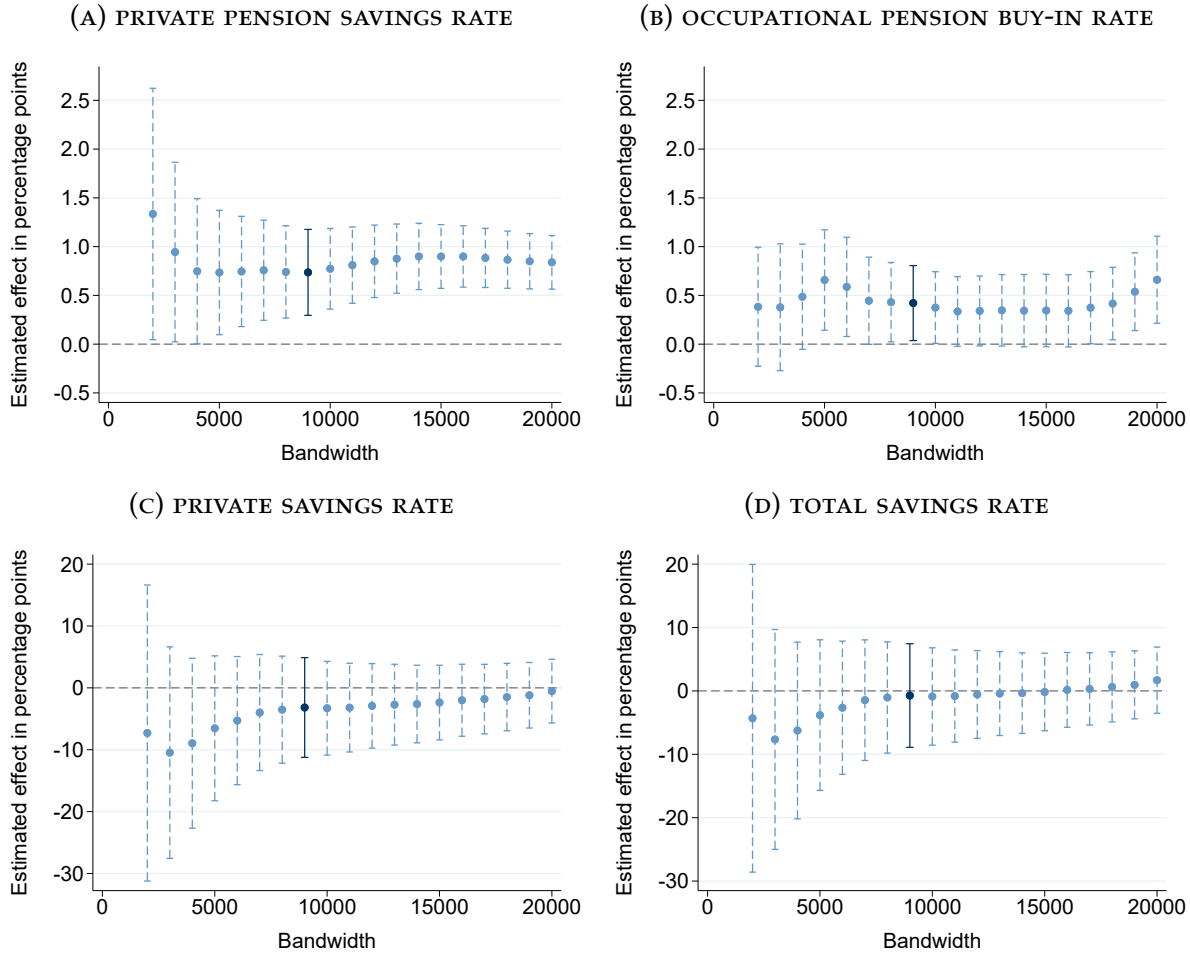
E.2 Kernel Function

Figure E.2 plots effect estimates using various weighting schemes. The main results obtained using a triangular kernel are robust to using a uniform or Epanechnikov kernel – the point estimates and confidence intervals hardly change at all. In addition, Figure E.2 provides a sense for the magnitude and precision of the estimates for the different savings outcomes.

E.3 Size of the Donut Hole

In Figure E.3, I examine the sensitivity of the estimates to removing the observations closest to the cutoff following a 'donut hole' approach (Bajari et al., 2011; Barreca et al., 2011). The intuition behind this check is that endogenous sorting through manipulation of the running variable is most likely to occur near the threshold. I find that the main results obtained after removing employees within CHF 350 of the cutoff, for whom earnings cannot be imputed unambiguously (see Section C.4), are

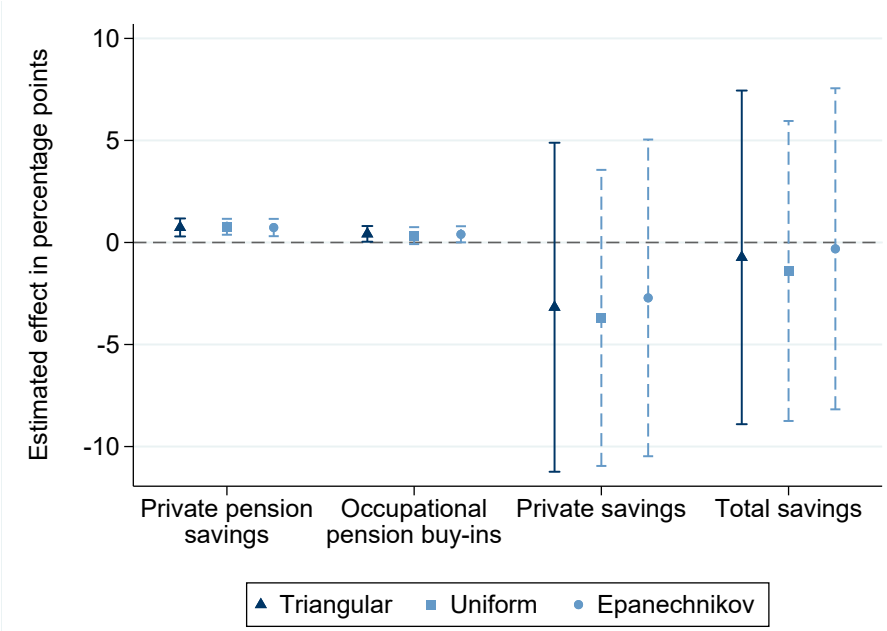
Figure E.1: Sensitivity of RD Estimates to Bandwidth Choice



Notes: The figure displays regression discontinuity estimates and 95% confidence intervals for the effect of the occupational pension savings mandate on a set of savings outcomes in 2017, depending on the bandwidth choice. The dark marker represents the main result using a bandwidth of CHF 9,000; the dashed light markers represent estimates using alternative bandwidths. Estimates are obtained by separately running the linear regression discontinuity model in Equation (2) using a triangular kernel for a range of bandwidths. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.

Figure E.2: Sensitivity of RD Estimates to Kernel Choice

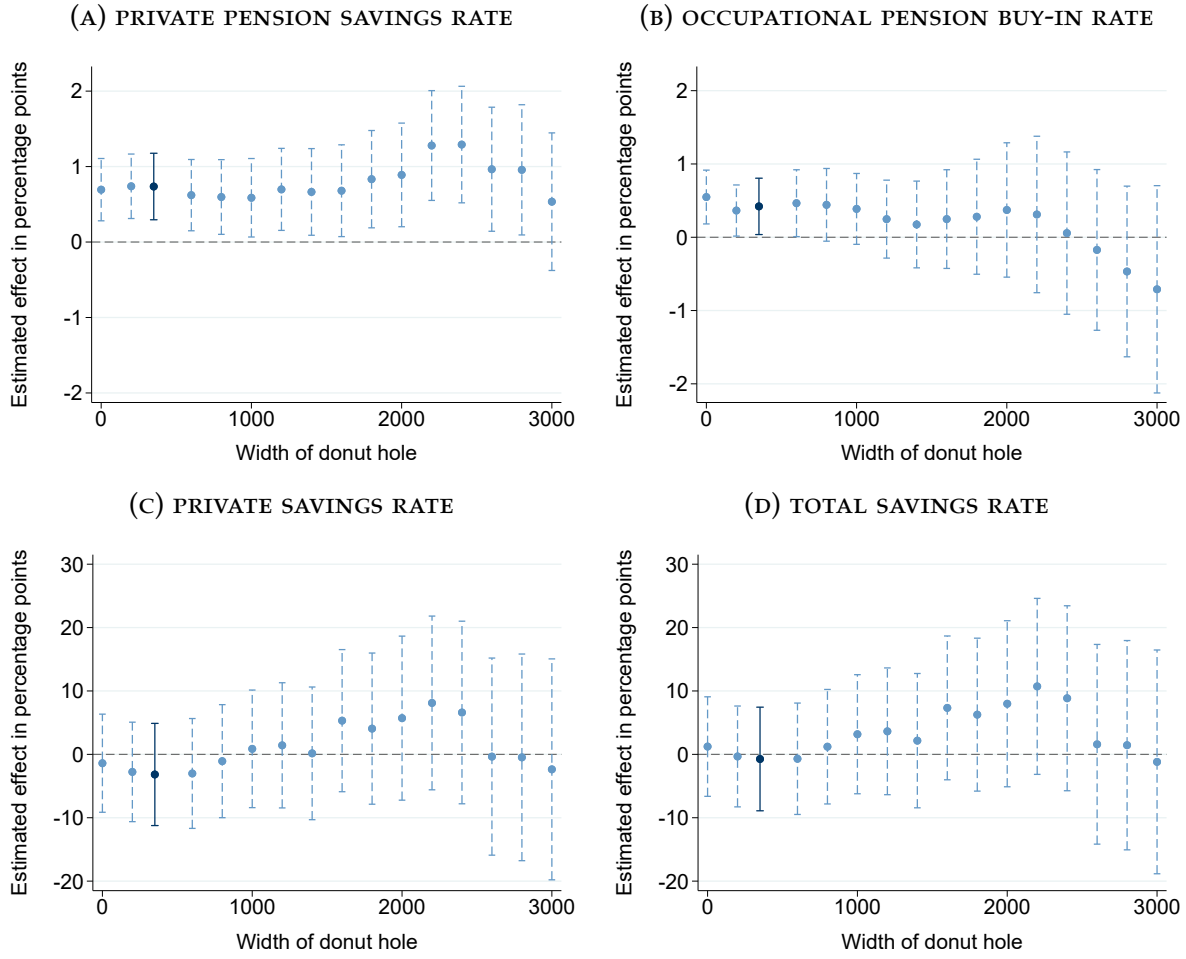


Notes: The figure displays regression discontinuity estimates and 95% confidence intervals for the effect of the occupational pension savings mandate on a set of savings rates in 2017, depending on the weighting function used. The dark marker represents the main result using a triangular kernel; the dashed light markers represent estimates using alternative kernels. Estimates are obtained by separately running the linear regression discontinuity model in Equation (2) using a triangular, uniform, or Epanechnikov kernel, respectively. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.

generally robust to varying the size of the donut hole, although the confidence intervals naturally widen when larger donut holes are used because the sample size shrinks. The effect on private pension savings in Panel A is particularly persistent – showing no big changes over the range of donut hole sizes considered. Estimated effects on occupational pension buy-ins in Panel B are also quite constant, except when excluding large donut holes of more than CHF 2,000. Effect estimates for private and total savings in Panels C and D start to change somewhat once a donut hole larger than roughly CHF 1,500 is removed. However, even when all employees within CHF 3,000 of the cutoff are excluded, the confidence interval contains the point estimate of my main results. These findings provide further evidence that there is a causal effect of the savings mandate on private pension savings and, to a lesser extent, on occupational pension buy-ins, rather than a selection effect from individuals with a preference for low retirement savings bunching below the threshold driving the results.

Figure E.3: Sensitivity of RD Estimates to Size of Donut Hole



Notes: The figure displays regression discontinuity estimates and 95% confidence intervals for the effect of the occupational pension savings mandate on a set of savings outcomes in 2017, depending on the size of the donut hole removed from the estimation sample. The dark marker represents the main result using a donut hole of CHF 350; the dashed light markers represent estimates using alternative donut hole sizes. Estimates are obtained by separately running the linear regression discontinuity model in Equation (2) using a triangular kernel for a range of donut hole sizes. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.

E.4 Order of Local Polynomial

In general, the methodological literature on RD designs recommends using local linear regression as done in the main analysis of this paper. However, in a recent paper, Pei et al. (2021) conduct Monte Carlo simulations showing that in some applications alternative polynomials perform better in terms of mean squared error, coverage rate of the 95% confidence interval, and size-adjusted confidence interval length. Thus, I investigate the robustness of the effect estimates to the degree of the local polynomial.

Figure E.4 plots the results allowing for constant, linear, quadratic, and cubic local polynomials of the supporting functions on each side of the cutoff. The plots show that using higher-order polynomials leads to noisier estimates, but the point estimates for private pension savings (Panel A) and occupational pension buy-ins (Panel B) remain roughly the same. Some of the confidence intervals at higher-order polynomials contain zero, but just because the standard errors become much larger, not due to an attenuation of the point estimate. The estimates for the private savings rate in Panel C and the total savings rate in Panel D are much lower when using a cubic rather than a linear polynomial, but the confidence intervals become so large as to render the results of this specification meaningless. In sum, the relevant findings from the main analysis remain unaltered using these alternative specifications.

E.5 Exclusion of Control Variables

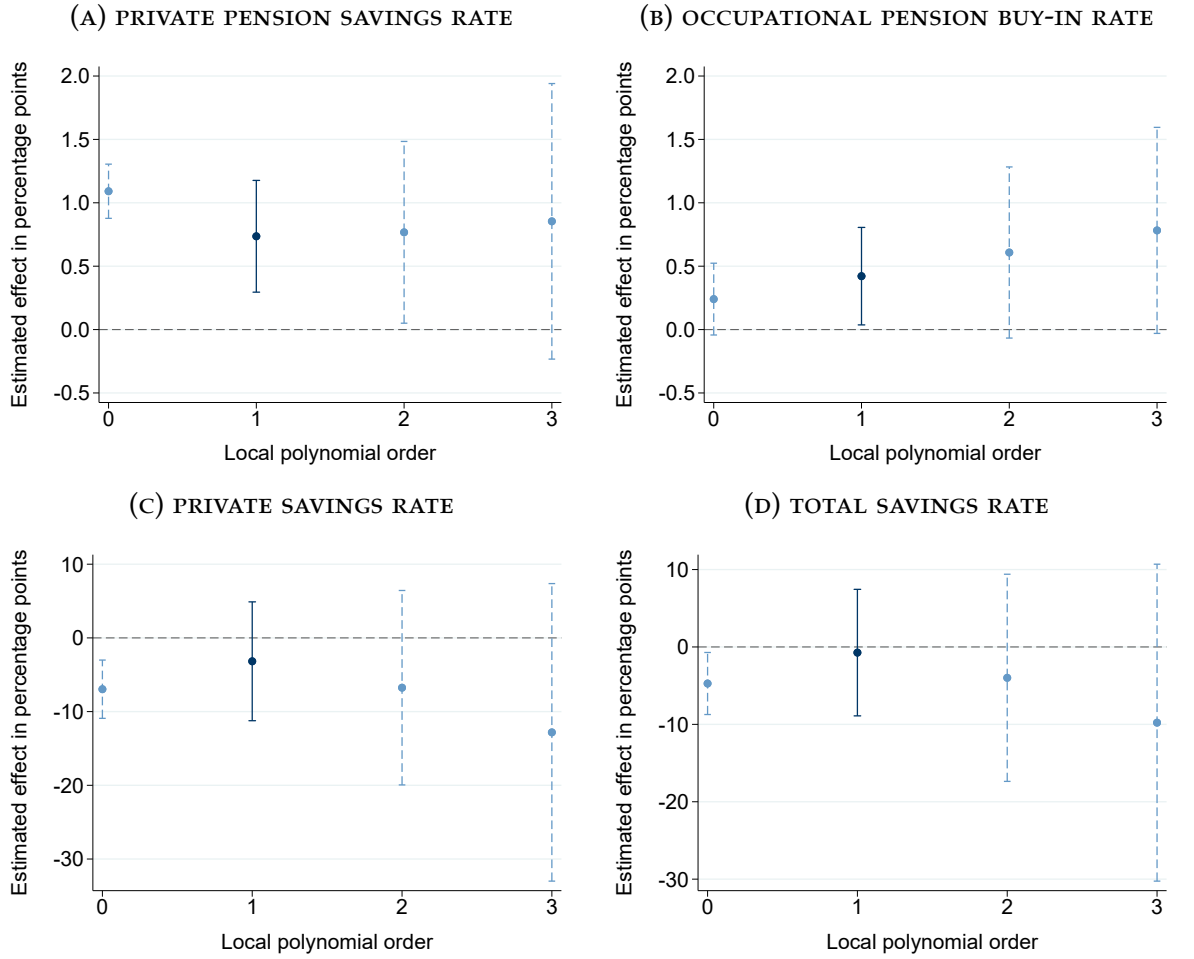
Under standard RD assumptions, the inclusion or exclusion of control variables should not affect the point estimates. If the effect sizes vary substantially depending on what controls are included, this could indicate a violation of the identifying assumptions, for example due to endogenous sorting around the threshold leading to a discontinuity in covariates (Lee and Lemieux, 2010). In Figure D.3, I document that there are no relevant discontinuities in predetermined characteristics at the cutoff, except for a small jump in age. In line with those findings, the estimated effects on all outcomes are unaffected by whether control variables – age, gender, marital status, and number of children – are included or excluded in the RD estimation, as Figure E.5 demonstrates.[23] This is another piece of suggestive evidence indicating that the RD assumptions hold in this application.

E.6 Robust Bias Correction

When an MSE-optimal bandwidth is used, the local linear RD estimator is asymptotically normally distributed (Melly and Lalive, 2020). But this distribution is not centered at zero because of the misspecification error (also called smoothing bias)

[23] The graph also shows that the confidence intervals do not shrink much when including controls.

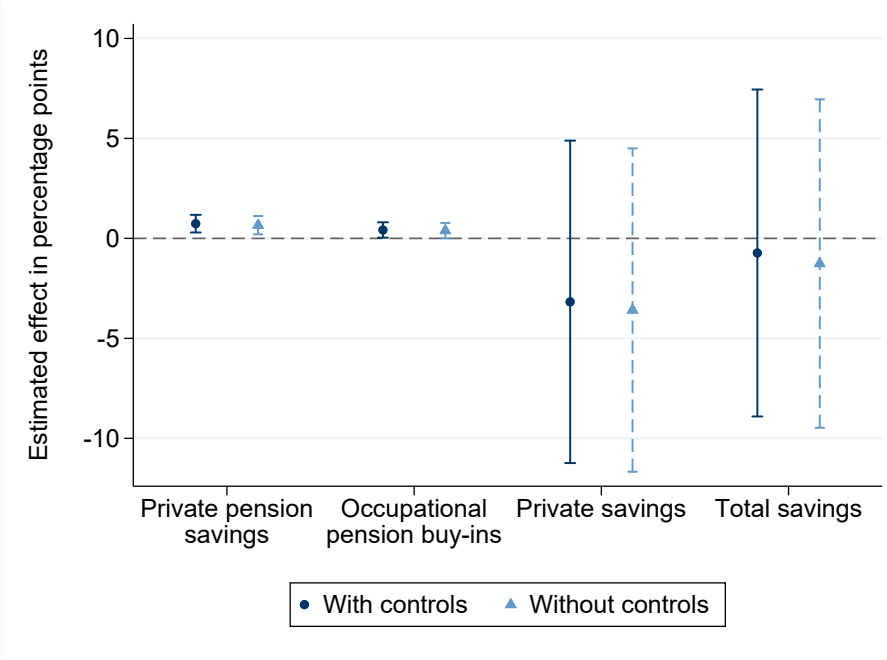
Figure E.4: Sensitivity of RD Estimates to Order of Local Polynomial



Notes: The figure displays regression discontinuity estimates and 95% confidence intervals for the effect of the occupational pension savings mandate on a set of savings outcomes in 2017, depending on the order of the local polynomial. The dark marker represents the main result using a local linear polynomial; the dashed light markers represent estimates using alternative polynomial orders. Estimates are obtained by separately running the regression discontinuity model in Equation (2) using a triangular kernel, varying the polynomial degree from local constant up to local cubic. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.

Figure E.5: Sensitivity of RD Estimates to Inclusion of Controls



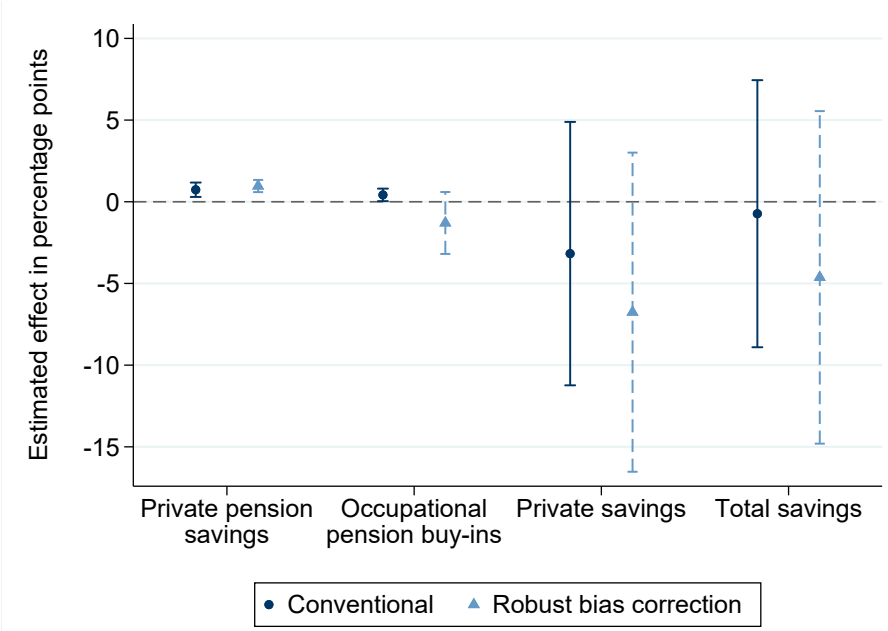
Notes: The figure displays regression discontinuity estimates and 95% confidence intervals for the effect of the occupational pension savings mandate on a set of savings rates in 2017, depending on the inclusion of control variables. The dark markers represent the main results estimated with controls; the dashed light markers represent coefficients estimated without controls. Estimates are obtained by separately running the linear regression discontinuity model in Equation (2) using a triangular kernel while controlling for age, gender, marital status, and number of children, or excluding control variables, respectively. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.

stemming from nonparametric estimation of the conditional expectation functions near the threshold (Cattaneo and Titiunik, 2021). This error must be taken into account for inference to be valid. Calonico, Cattaneo and Titiunik (2014*a*) propose a robust bias correction approach that involves estimating the asymptotic bias, subtracting it from the effect estimate, and using the bias-corrected statistic to conduct inference. The standard errors need to be adjusted to account for the fact that both the coefficient on the treatment effect and the bias term have been estimated from the data.

Figure E.6 depicts the bias-corrected estimates and robust confidence intervals computed using an MSE-optimal bandwidth following Calonico, Cattaneo and Titiunik (2014*a*,*b*) and Calonico et al. (2017) alongside the conventional effect estimates and confidence intervals from the main analysis. The effect estimate for private pension savings is higher and more precise when employing robust bias correction, bolstering the conclusion that the savings mandate increases private pension savings. On the other hand, the effect on occupational pension buy-ins is not robust to using bias correction. The main reason is that the MSE-optimal bandwidth selector chooses a

Figure E.6: Estimation and Inference Using Robust Bias Correction



Notes: The figure displays regression discontinuity estimates and 95% confidence intervals for the effect of the occupational pension savings mandate on a set of savings rates in 2017. The dark markers represent the main results estimated following conventional approaches; the dashed light markers represent coefficients estimated with robust bias correction. Conventional estimates are obtained by running the linear regression discontinuity model in Equation (2) using a triangular kernel. Robust bias-corrected estimates are obtained using an MSE-optimal bandwidth and following the procedure proposed by Calonico, Cattaneo and Titiunik (2014*a*). The bandwidth used is CHF 13,364 for private pension savings, CHF 935 for occupational pension buy-ins, CHF 6,704 for private savings, and CHF 6,408 for total savings. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.

very small bandwidth of CHF 935. Given that employees within CHF 350 of the threshold are excluded, this leaves a limited number of individuals in the estimation bandwidth. This is particularly problematic because the overall share of individuals making non-zero buy-ins is just around 1%. This means that the effect on buy-ins estimated using robust bias correction is driven by only few individuals and correspondingly noisy. The coefficients for the private and total savings rate decline somewhat and standard errors increase when using bias correction, but the confidence intervals largely overlap with the main results.

E.7 Year-by-Year Effect Estimates

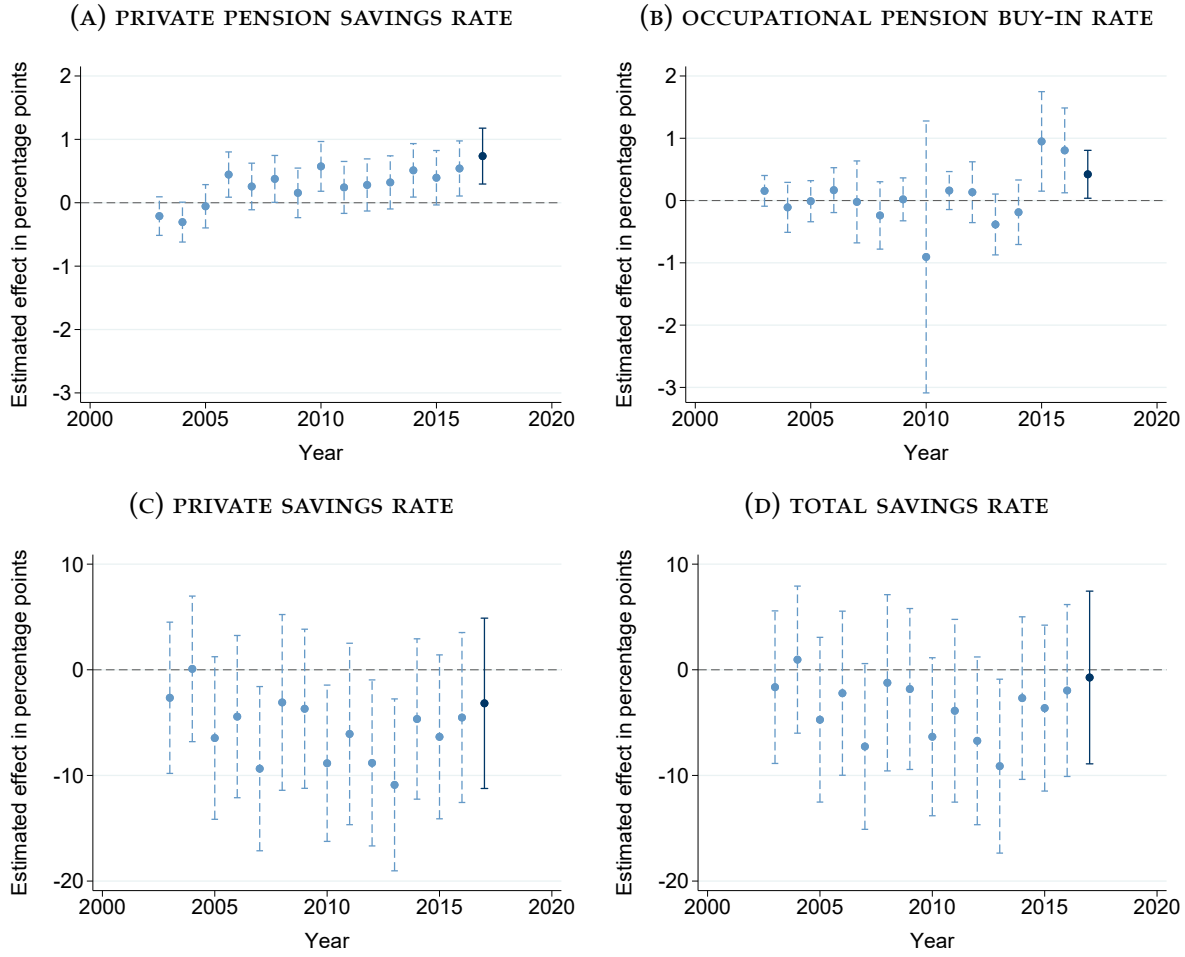
Figure E.7 plots treatment effects estimated separately for each year from 2003 to 2017 using the year-specific mandate thresholds.[24] The estimated effects on private pen-

[24]Because 2002 is the first year in my data, I cannot measure private savings which are defined as the difference in net wealth between the current and the previous year (see Section C.4). Thus, I do

sion savings shown in Panel A are broadly similar between 2006 and 2017, although some are not significant at the 5% level. The estimates for 2003 through 2005 are close to zero or even negative. Given that the threshold was lowered from CHF 25,320 to CHF 19,350 in 2005 and only gradually raised thereafter (to CHF 21,150 in 2017), one potential interpretation of the negative point estimates in 2003 and 2004 is that the average treatment effect varies with the running variable. The impact on occupational pension buy-ins depicted in Panel B seems rather unstable over time. This is likely due to the inherent lumpiness of occupational pension buy-ins. Pooled over all years from 2003 to 2017, only 0.8% of individuals in the estimation sample make any buy-ins in a given year. But, conditional on making a buy-in, the mean contribution is CHF 12,400. Although the effect is reliably positive in years 2015 through 2017, the dispersion of estimates over the full sample period suggests that the results from 2017 might not be fully generalizable. The estimated treatment effects on private savings in Panel C and total savings in Panel D shift around somewhat over the years, but the main findings remain unaffected. Virtually all the point estimates for private savings are negative, although most are not significant at the 5% level; all point estimates for total savings are close to zero or negative. As in the main analysis, the standard errors are too wide to draw strong conclusions. However, I do not find any evidence that total savings increase in response to the savings mandate in any year in the data.

not present effect estimates for 2002.

Figure E.7: RD Estimates for Each Year in the Data



Notes: The figure displays regression discontinuity estimates and 95% confidence intervals for the effect of the occupational pension savings mandate on a set of savings outcomes, depending on the year in the data used for estimation. The dark marker represents the main result for 2017, the most recent data available; the dashed light markers represent estimates for the other years in the data. Estimates are obtained by separately running the linear regression discontinuity model in Equation (2) using a triangular kernel for each year from 2003 to 2017. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation.
Source: Author's calculations based on administrative tax data from the canton of Bern.

F Effect Heterogeneity in the RD Results

To improve our understanding of the aggregate effects of the savings mandate and assess how its impact may vary with the characteristics of the affected employees, this appendix explores heterogeneity in RD effect estimates by wealth, age, gender, and marital status.

A key goal of the Swiss savings mandate is to improve the preparedness of individuals with otherwise low wealth for retirement by raising their savings rate. To investigate whether the policy achieves this objective, I split up the estimation sample by net wealth in the previous year. I separate the subpopulation reporting negative net wealth in the tax records and split individuals with non-negative net wealth into two equally sized groups using the (approximate) median net wealth of CHF 35,000 as the cutoff value.[25] Table F.1 reports the effects on the savings outcomes estimated separately for these three wealth groups, alongside the overall estimates presented in Figure 2. I also report the group-specific mean for each outcome as well as the predicted increase in the occupational pension savings rate for each group. Before discussing the estimated effects, I note that, as one would expect, individuals with lower levels of net wealth – between CHF 0 and CHF 35,000 – generally have lower savings rates across all savings components. Their total savings rate is 10.8% which is only about one third of the mean total savings rate in the estimation sample.

Individuals with low but non-negative net wealth do not respond to the pension savings mandate by increasing their private pension savings or occupational buy-ins. At the same time, the effect on private savings also appears to be muted. Hence, the mandate's impact on the total savings rate is equal to the mechanical increase in the occupational pension savings rate. Note, however, that the effect on total savings is not significant at the 5% level. High-wealth individuals react to the mandate by shifting more of their savings into pension funds – both through private pension savings and occupational pension buy-ins. This is more than offset by a reduction in private savings, although the evidence on this is not conclusive due to the large standard errors. These findings suggest that the savings mandate succeeds in raising pension savings of individuals with limited assets, but that there is no crowding-in effect on other types of pension savings for them.

Another purpose of the savings mandate is to increase savings of young individuals that are potentially not yet as concerned about their retirement and thus do not save enough from the perspective of policymakers. I allow treatment effects to vary by age by splitting the estimation sample in two equally sized groups using

[25] I do not discuss the results for the group with negative net wealth as it is difficult to interpret what type of individuals have negative net worth in the tax data. Krapf (2019) points out that they are not necessarily poor as they report high incomes and gains in wealth and income over time.

Table F.1: Regression Discontinuity Estimates by Net Wealth

		Lagged net wealth in Swiss francs		
	All	Wealth < 0	Wealth $\in [0, 35\text{k}]$	Wealth $> 35\text{k}$
Private pension savings rate	0.74***	0.85*	0.24	1.47***
	(0.22)	(0.47)	(0.24)	(0.42)
Mean	6.32	5.51	2.73	10.4
Occupational pension buy-in rate	0.42**	0.32	0.026	0.89*
	(0.20)	(0.28)	(0.033)	(0.46)
Mean	0.63	0.15	0.037	1.47
Private savings rate	-3.17	2.33	-0.33	-7.89
	(4.11)	(8.69)	(3.14)	(8.76)
Mean	22.2	32.0	7.03	33.3
Total savings rate	-0.73	4.80	1.57	-4.58
	(4.17)	(8.80)	(3.19)	(8.86)
Mean	30.5	39.0	10.8	47.0
Increase in occ. pension savings rate	1.95	1.91	1.74	2.17
Observations	43,079	8,164	17,813	17,102

Notes: Regression discontinuity results for the effect of the occupational pension savings mandate on a set of savings outcomes by subgroup defined with respect to net wealth in the previous year. Estimates are obtained from separately estimating Equation (2) using a triangular kernel for each outcome and subgroup. Standard errors obtained from heteroskedasticity-robust nearest-neighbor variance estimators are reported in parentheses. Subgroup-specific outcome means, predicted increases in the occupational pension savings rate due to the savings mandate, and sample sizes are reported for reference. The sample is based on 2017 data and includes individuals between 25 and 60 years of age with earnings within CHF 9,000 of the pension savings mandate threshold at CHF 21,150. Individuals within CHF 350 of the cutoff and those with no observed measure of private savings are excluded. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation. Stars indicate significance according to * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Author's calculations based on administrative tax data from the canton of Bern.

the median age of 43 years as the cutoff. Table F.2 displays means and effect estimates separately for those two age groups. Unsurprisingly, individuals between 25 and 42 years of age have much lower savings rates for all the savings outcomes than older individuals. The mean total savings rate is 17.4% compared to the overall mean of 30.6% and the mean in the above-median age group of 43.0%. The predicted increase in the occupational pension savings rate due to the mandate is also lower for the below-median age group because the statutory age-specific contribution rate increases quite strongly with age (see Section 2.1).

Strikingly, the savings mandate only increases private pension savings and occupational pension buy-ins of older individuals. The corresponding effects for the younger age group are precisely estimated zeros. The estimates for private and total

Table F.2: Regression Discontinuity Estimates by Age

	All	Age $\in [25, 42]$	Age $\in [43, 60]$
Private pension savings rate	0.74***	0.052	1.38***
	(0.22)	(0.28)	(0.34)
Mean	6.32	4.73	7.83
Occupational pension buy-in rate	0.42**	0.0067	0.83**
	(0.20)	(0.12)	(0.37)
Mean	0.63	0.066	1.16
Private savings rate	-3.17	-5.62	-0.62
	(4.11)	(5.27)	(6.25)
Mean	22.2	11.5	32.3
Total savings rate	-0.73	-4.45	3.04
	(4.17)	(5.33)	(6.35)
Mean	30.5	17.4	43.0
Increase in occ. pension savings rate	1.95	1.35	2.51
Observations	43,079	21,006	22,073

Notes: Regression discontinuity results for the effect of the occupational pension savings mandate on a set of savings outcomes by subgroup defined with respect to age. Estimates are obtained from separately estimating Equation (2) using a triangular kernel for each outcome and subgroup. Standard errors obtained from heteroskedasticity-robust nearest-neighbor variance estimators are reported in parentheses. Subgroup-specific outcome means, predicted increases in the occupational pension savings rate due to the savings mandate, and sample sizes are reported for reference. The sample is based on 2017 data and includes individuals between 25 and 60 years of age with earnings within CHF 9,000 of the pension savings mandate threshold at CHF 21,150. Individuals within CHF 350 of the cutoff and those with no observed measure of private savings are excluded. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation. Stars indicate significance according to * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Author's calculations based on administrative tax data from the canton of Bern.

savings rates are considerably lower for the below-median age group as well but these are again imprecisely estimated. The variation in effects by age could explain part of the effect heterogeneity by wealth (or vice versa) as older individuals are generally wealthier than young people.

I also examine effect heterogeneity by gender and marital status. Table F.3 breaks out the results by gender. Women, who make up 80% of the estimation sample, appear to increase their private pension savings more than men, while men seem to make larger occupational pension buy-ins in response to the mandate. In Table F.4, I report effect estimates by marital status. The impact on private pension savings is identical for unmarried and married individuals, while married individuals appear

Table F.3: RD Estimates by Gender

	All	Female	Male
Private pension savings rate	0.74***	0.83***	0.22
	(0.22)	(0.26)	(0.42)
Mean	6.32	6.84	4.23
Occupational pension buy-in rate	0.42**	0.30	0.87*
	(0.20)	(0.21)	(0.47)
Mean	0.63	0.57	0.85
Private savings rate	-3.17	-4.15	-0.024
	(4.11)	(4.79)	(7.61)
Mean	22.2	24.9	11.2
Total savings rate	-0.73	-1.65	2.04
	(4.17)	(4.86)	(7.68)
Mean	30.5	33.8	17.3
Increase in occ. pension savings rate	1.95	1.99	1.77
Observations	43,079	34,500	8,579

Notes: Regression discontinuity results for the effect of the occupational pension savings mandate on a set of savings outcomes by subgroup defined with respect to gender. Estimates are obtained from separately estimating Equation (2) using a triangular kernel for each outcome and subgroup. Standard errors obtained from heteroskedasticity-robust nearest-neighbor variance estimators are reported in parentheses. Subgroup-specific outcome means, predicted increases in the occupational pension savings rate due to the savings mandate, and sample sizes are reported for reference. The sample is based on 2017 data and includes individuals between 25 and 60 years of age with earnings within CHF 9,000 of the pension savings mandate threshold at CHF 21,150. Individuals within CHF 350 of the cutoff and those with no observed measure of private savings are excluded. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation. Stars indicate significance according to * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Author's calculations based on administrative tax data from the canton of Bern.

to increase their occupational pension buy-ins more. The effects on private and total savings are too noisy to warrant further discussion.

Table F.4: RD Estimates by Marital Status

	All	Unmarried	Married
Private pension savings rate	0.74***	0.69**	0.71**
	(0.22)	(0.31)	(0.30)
Mean	6.32	3.56	7.80
Occupational pension buy-in rate	0.42**	0.11	0.58**
	(0.20)	(0.23)	(0.27)
Mean	0.63	0.22	0.85
Private savings rate	-3.17	1.45	-5.57
	(4.11)	(5.61)	(5.51)
Mean	22.2	9.57	29.0
Total savings rate	-0.73	3.49	-2.97
	(4.17)	(5.67)	(5.59)
Mean	30.5	14.6	39.1
Increase in occ. pension savings rate	1.95	1.72	2.06
Observations	43,079	15,090	27,989

Notes: Regression discontinuity results for the effect of the occupational pension savings mandate on a set of savings outcomes by subgroup defined with respect to marital status. Estimates are obtained from separately estimating Equation (2) using a triangular kernel for each outcome and subgroup. Standard errors obtained from heteroskedasticity-robust nearest-neighbor variance estimators are reported in parentheses. Subgroup-specific outcome means, predicted increases in the occupational pension savings rate due to the savings mandate, and sample sizes are reported for reference. The sample is based on 2017 data and includes individuals between 25 and 60 years of age with earnings within CHF 9,000 of the pension savings mandate threshold at CHF 21,150. Individuals within CHF 350 of the cutoff and those with no observed measure of private savings are excluded. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 4.2 for more detail on sample restrictions and estimation. Stars indicate significance according to * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Author's calculations based on administrative tax data from the canton of Bern.

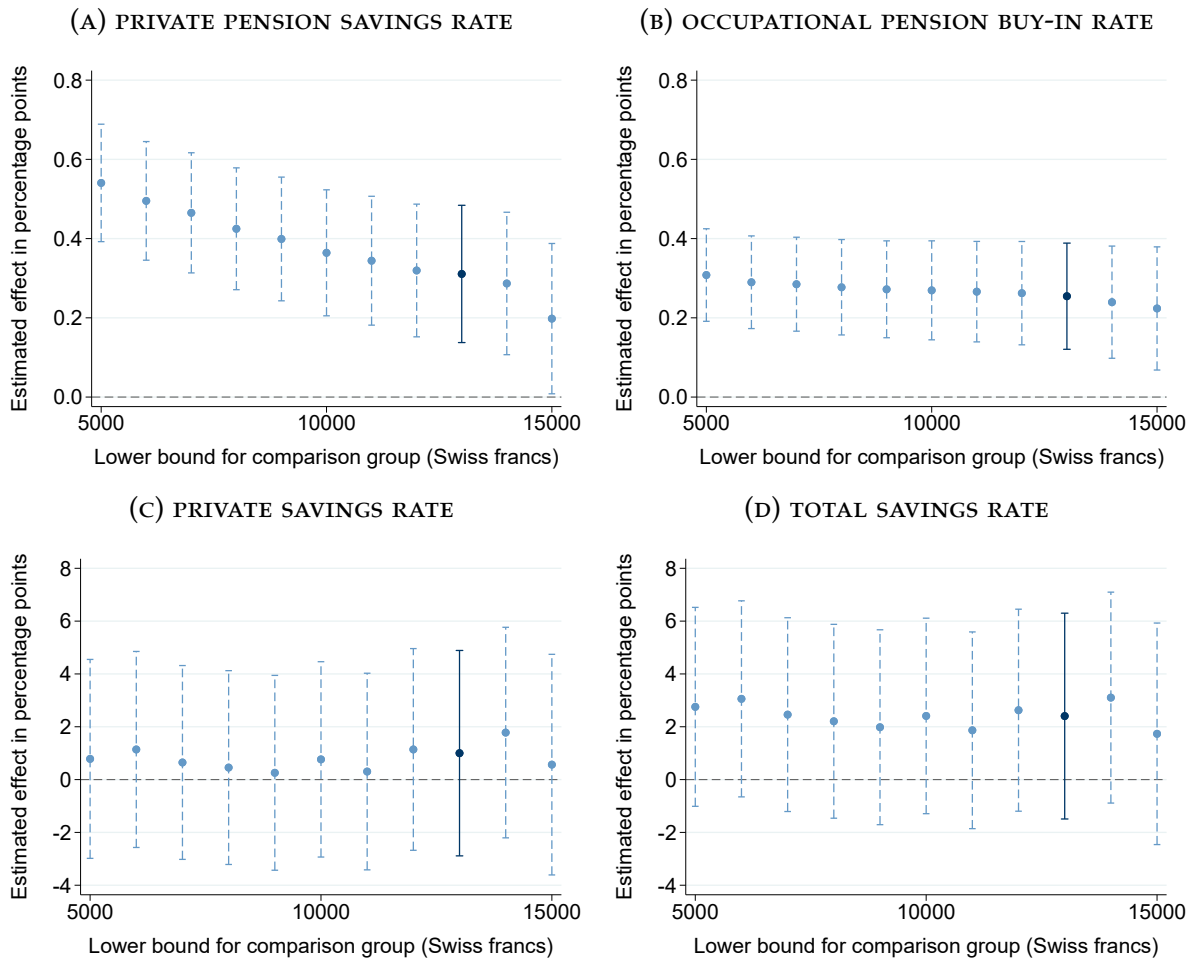
G Robustness of the DD Results

This appendix assesses the sensitivity of the main results from the DD analysis to choices regarding the definition of treatment and control group.

First, I vary the size of the control group. In my main specification, I choose the control group to cover a similar earnings range as the treatment group which results in a lower bound for inclusion in the control group of CHF 13,000. Thus, the bandwidth around the 2005 mandate cutoff for inclusion in the treatment or control group is approximately CHF 6,000. Figure G.1 shows the effect on each savings outcome estimated using the static DD model in Equation (5) as a function of the lower bound for inclusion in the control group. The results look very similar irrespective of the size of the control group, both in terms of effect size and statistical significance. The estimated effect on private pension savings decreases somewhat when a smaller control group is used, although it remains significant at the 5% level. This decline is driven by the fact that using a control group closer to the cutoff leads to a smaller first stage, i. e. a smaller difference in the share of individuals affected by the 2005 reform between treatment and control groups, resulting in lower reduced-form ITT effects. Note that I cannot increase the size of the treatment group because only individuals with earnings below the 2004 threshold should be included.

Second, I implement a donut hole approach that removes employees with 2004 earnings close to the 2005 cutoff to check the robustness of the results to potential measurement error and limited persistence of the assignment to treatment and control groups. Figure G.2 displays the static DD effect estimates using a range of donut hole sizes. The findings are very robust to the magnitude of the donut hole. Conversely to the impact of shrinking the control group described above, the effects on private pension savings and occupational pension buy-ins increase with the size of the donut hole. This is probably driven by an increase in the first stage when individuals close to the cutoff are removed from the estimation sample.

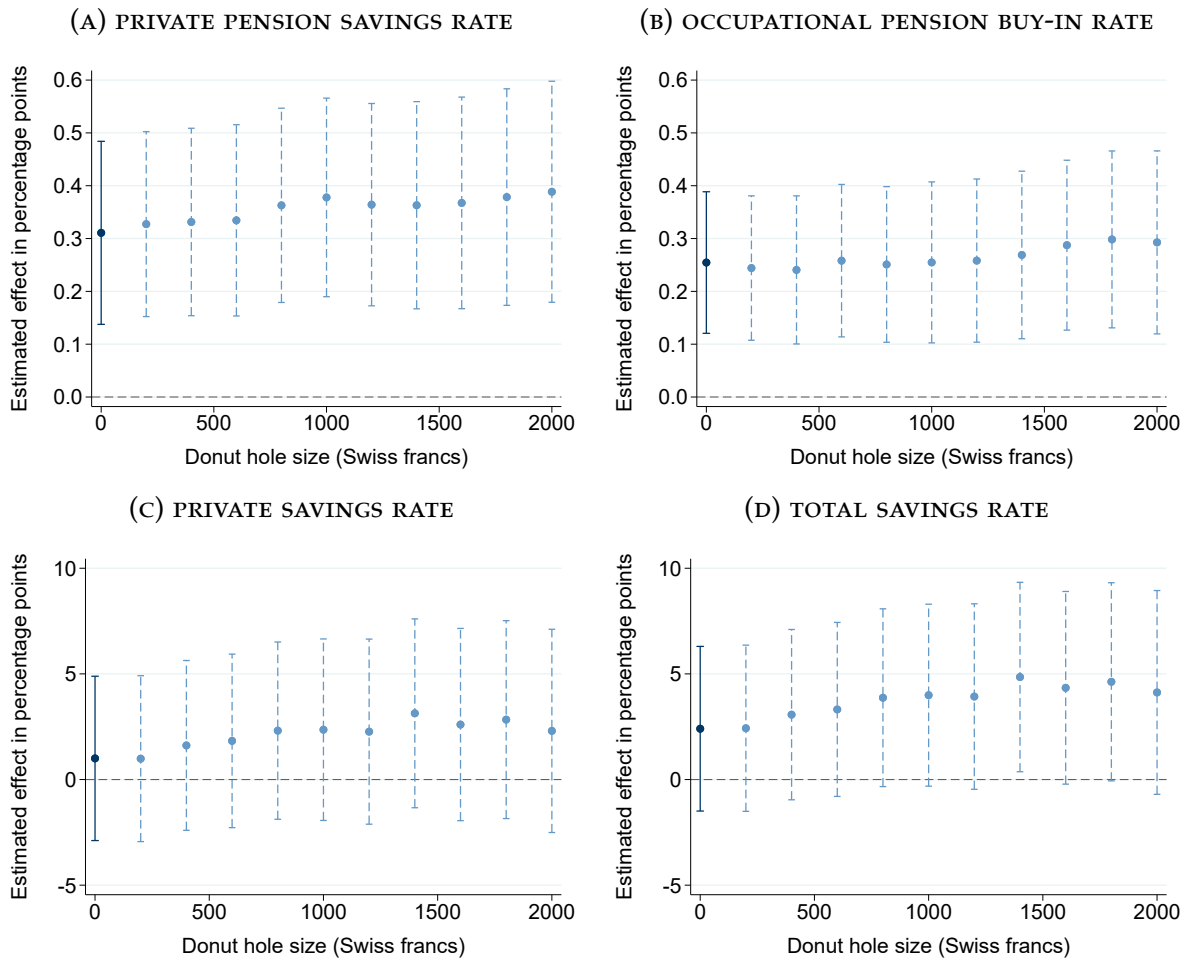
Figure G.1: Sensitivity of DD Estimates to Size of Control Group



Notes: The figure displays difference-in-differences estimates and 95% confidence intervals based on standard errors clustered at the individual level for the effect of the occupational pension savings mandate on a set of savings outcomes, depending on the lower bound used for inclusion in the control group. The dark marker represents the main result using a lower bound of CHF 13,000; the dashed light markers represent estimates using alternative lower bounds. Estimates are obtained by running the static DD model in Equation (5) separately for each control group definition using data from 2002 to 2010, exploiting the 2005 reform that expanded mandate coverage. Treatment and control group are defined according to Equation (3) and lower bounds for the control group plotted on the x-axis in the figure. All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 5 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.

Figure G.2: Sensitivity of DD Estimates to Size of Donut Hole



Notes: The figure displays difference-in-differences estimates and 95% confidence intervals based on standard errors clustered at the individual level for the effect of the occupational pension savings mandate on a set of savings outcomes, depending on the size of the donut hole removed from the estimation sample. The dark marker represents the main result estimated without excluding a donut hole; the dashed light markers represent estimates using a range of donut hole sizes. Estimates are obtained by running the static DD model in Equation (5) separately for each donut size using data from 2002 to 2010, exploiting the 2005 reform that expanded mandate coverage. Treatment and control group are defined according to Equation (3). All outcomes except occupational pension buy-ins are winsorized at percentiles 1 and 99. See Section 3 for more information on data preparation and variable construction as well as Section 5 for more detail on sample restrictions and estimation.

Source: Author's calculations based on administrative tax data from the canton of Bern.