CAGE

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

CAGE working paper no. 645

November 2022

David Burgherr

WARWICK
THE UNIVERSITY OF WARWICK

UKRI Economic and Social Research Council

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

David Burgherr†

November 2022

Abstract

A straightforward policy tool to boost retirement savings is requiring workers to contribute some fraction of their earnings to a pension account. Drawing on detailed administrative tax data on income, wealth, and savings, I study the savings response to the occupational pension savings mandate in Switzerland using regression discontinuity and difference-in-differences designs. I find that employees respond to being obliged to contribute to an occupational pension account by raising voluntary forms of retirement savings such as preferentially taxed private pension savings and occupational pension buy-ins. The crowding-in effect on private pension savings is driven by reduced information frictions and increased salience of retirement savings and facilitated by having another earner in the household. The additional retirement savings appear to be funded by reduced private savings rather than lower current consumption, leaving total savings unaffected by the mandate, although this is imprecisely estimated.

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

Keywords: retirement savings, pension, savings mandate, crowding-in

*I am indebted to Maximilian von Ehrlich for his guidance and support. For helpful comments and suggestions, I thank Arun Advani, Pavel Brendler, Helen Hughson, Wouter van der Wielen, as well as participants at the ZEW Public Finance Conference, IIPF Annual Congress, and EEA-ESEM. I am grateful to the tax administration of the canton of Bern, in particular Hans Frauchiger, for providing access to de-identified administrative microdata for the purpose of this research, and Jonas Meier for helping with requesting and processing the tax data.

†London School of Economics, International Inequalities Institute. Email: d.m.burgherr@lse.ac.uk.

1 Introduction

Across the developed world, 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). This trend is one of the major structural challenges facing our societies and requires a fundamental modernization of current pension systems (Blanchard and Tirole, 2021). 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 and widely adopted instrument is requiring workers by law to contribute some fraction of their earnings to a pension account that they can only access upon entering retirement. This *pension savings mandate* is based on the premise that individuals are myopic and do not save optimally for retirement in absence of the policy, in which case the pension contributions directly add to total savings. But if individuals 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, employees 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. Another possibility is that insufficient retirement savings are driven by a lack of salience or information, in which case the mandate could crowd in additional pension savings. This is particularly likely to be prevalent for less financially literate households (Lusardi and Mitchell, 2011*a*). Understanding individual savings behavior and responses to pension savings policies is key to evaluating and improving the design of retirement systems to make them fit for present-day challenges.

Although a vast and long-standing literature studies the effects of retirement policies on savings, the empirical evidence is mixed. The impact of financial incentives for pension savings on other types of savings is ambiguous, with some papers finding 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 are often found to boost retirement savings (Madrian and Shea, 2001; Thaler and Benartzi, 2004; Chetty

et al., 2014) – although recent work has challenged the notion that the positive effect on total savings is sustained in the long run (Choukhmane, 2021) – but these papers are usually unable to consider impacts on other savings vehicles due to data limitations. To my knowledge, only two papers, both using Danish data, specifically study a pension savings mandate (Arnberg and Barslund, 2014; Chetty et al., 2014) – presumably due to a lack of identifying variation and high-quality data. They both find limited crowding-out effects and substantial increases in total savings.

A key challenge in this literature is disentangling the causal effect of the policy from unobserved heterogeneity in savings preferences. This issue is difficult to address with cross-sectional data and selection-on-observables approaches commonly used in older work. While more recent research employs more elaborate empirical strategies, it still frequently has to rely on survey data suffering from measurement error and small sample sizes or panel datasets that only cover a relatively short time period. These data limitations impede the use of research designs that are able to identify the causal effect of pension savings policies since these often require focusing on specific subpopulations affected by the policy or following individuals over time. Finally, many studies do not have access to data on all components of wealth and savings but just a subset such as financial wealth, which prevents researchers from characterizing the full savings response to pension policies.

This paper tests the effects of the occupational pension savings mandate in Switzerland on other forms of pension savings, private savings, and total savings, and sheds light on the mechanisms that drive the overall behavioral response. The Swiss setting has two major advantages allowing me to overcome the data and identification problems outlined above. First, I draw on comprehensive administrative tax data providing detailed information on the income, wealth, and savings of the entire population in the canton of Bern from 2002 to 2017. This facilitates the use of empirical strategies that require zooming in on specific parts of the earnings distribution and following people over time. I can measure different forms of pension savings, private savings, and total savings at the individual level, enabling me to examine all components of the savings response to the mandate. The richness of the data also allow me to document effect heterogeneity along dimensions such as age, gender, marital status, household income, and wealth. Second, the institutional rules and reforms of the Swiss occupational pension system provide compelling identifying variation that can be exploited using quasi-experimental research designs to study the causal effects of the pension savings mandate.

The Swiss savings mandate requires employees whose earnings exceed a relatively low threshold (around the 30th percentile of the earnings distribution) to contribute to an occupational pension account. The mandate differs from automatic enrollment in that covered individuals cannot opt out. As a consequence, the mandate is guar-

anteed to increase occupational pension savings mechanically, but the effect on other forms of savings are unknown. Contributions are generally calculated by applying a legally defined contribution rate to the 'coordinated salary' which is equal to earnings minus a deduction. For individuals with earnings only marginally above the mandate threshold, there is a minimum coordinated salary that the contribution rate is applied to. This policy feature causes a discontinuity in the occupational pension savings rate at the cutoff of about 2 percentage points, meaning that otherwise similar individuals are required to contribute substantially different amounts to occupational pension accounts.

I identify and estimate the causal effects of contributing to an occupational pension fund using two complementary quasi-experimental research designs that exploit credibly exogenous variation in occupational pension savings. Using a regression discontinuity design, I leverage the aforementioned jump in occupational pension savings at the mandate threshold. To shed more light on the dynamics and mechanisms behind the behavioral response to becoming covered by the savings mandate, I conduct a difference-in-differences analysis of the reform of the occupational pension system that expanded the mandate's coverage in 2005. Overall, the results from the regression discontinuity and difference-in-differences analyses are highly consistent.

The three main findings can be summarized as follows: *First*, I find strong evidence of a 'crowding-in' effect of the occupational pension savings mandate on voluntary private pension savings in separate accounts that benefit from advantageous tax treatment. An increase in the occupational pension savings rate by 1 percentage point induced by the mandate raises the private pension savings rate by 0.3–0.4 percentage points. Furthermore, there is suggestive evidence of a crowding-in effect on occupational pension buy-ins of 0.2–0.3 percentage points per 1 percentage point increase in the occupational pension savings rate. 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 is surprising from the viewpoint of standard life-cycle models which predict that optimizing individuals cut back their private pension savings in response to a forced increase in occupational pension savings as both types of savings have similar characteristics and thus appear to be substitutes. But it is in line with the earlier finding in Gelber (2011) that becoming eligible for employer-sponsored 401(k) plans raises savings in Individual Retirement Accounts (IRAs) in the US.

Second, investigating the mechanisms behind the behavioral response to the savings mandate, I document the important role of information frictions and salience as well as household income in driving the crowding-in effect on private pension savings. I provide evidence that the savings mandate reduces information frictions and increases the salience of pension savings by demonstrating that the crowding-in ef-

fect is concentrated among individuals who have not contributed to private pension accounts before becoming subject to the mandate. This suggests that being enrolled into an occupational pension plan provides new information about the pension system and encouragement to have a careful look at the personal retirement situation to individuals who appear to have previously been less aware of the need to save for retirement and the tax advantages of pension savings. This result ties in with a growing literature demonstrating the positive impact of providing information and boosting salience on pension savings (Duflo and Saez, 2003; Duflo et al., 2006; Goda, Manchester and Sojourner, 2014; Dolls et al., 2018). Whereas these papers specifically study information and salience treatments, I document that being required to contribute to an occupational pension fund stimulates retirement planning on its own. Moreover, I find that only individuals with relatively high household income, who likely are secondary earners, increase private pension savings in response to becoming subject to the mandate, implying that having another (main) earner in the household facilitates shifting savings into preferentially taxed private pension accounts. Consistent with this mechanism, individuals with low household income respond to the mandate by reducing private pension savings as they are more likely to face liquidity constraints.

Third, 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. The null effect on total savings contrasts with the few papers that have previously studied the effects of a pension savings mandate and found a positive impact (Arnberg and Barslund, 2014; Chetty et al., 2014). 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 total savings rate is unaffected by the mandate but the composition of the savings portfolio shifts towards retirement savings.

I provide empirical support for the validity of the identifying assumptions of the research designs used. Specifically, I document that the regression discontinuity results are unlikely to be biased by endogenous sorting around the threshold and that pre-treatment trends in the difference-in-differences analysis are flat and insignificant for all savings outcomes. Further, I conduct extensive sensitivity analyses confirming the robustness of the results to different choices regarding the model specifications and their operationalization.

The remainder of this paper is organized as follows. In Section 2, I provide information on the institutional background by way of explaining the relevant parts of the Swiss old-age provision system. Section 3 describes the administrative tax data from the canton of Bern and the data preparation for the empirical analyses. In Section 4, I present evidence on the behavioral response to the pension savings man-

date following a regression discontinuity approach. Section 5 provides evidence from the difference-in-differences analysis, confirming the regression discontinuity results and exploring the underlying mechanisms. Section 6 concludes by summarizing the main findings, discussing policy implications, and providing suggestions for future research.

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] In this section, I briefly introduce 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 their annual gross earnings in the main job exceed the legally defined threshold. The pension savings mandate applies to women between 25 and 64 years of age as well as men between 25 and 65 years of age. The mandate is binding: employees satisfying the legal 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 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.

[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.

[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.

The 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 who are not covered by the mandate but enrolled voluntarily. I cannot observe that these individuals 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 employees below the threshold make zero occupational pension contributions. Moreover, individuals below the threshold contributing to an occupational pension account are not 'treated' by the mandate as such 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.[4] The statutory age-specific contribution rates are 7% for age group 25–34 years, 10% for age group 35–44 years, 15% for age group 45–54 years, and 18% for age group 55–64 years among women and 55–65 years among men. By law, 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. Accordingly, 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 source of 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.[5]

In addition to regular contributions, many occupational pension funds allow lump-sum 'buy-ins' which can similarly be deducted from taxable income.[6] 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

[4]The deduction is CHF 24,675 and serves a similar purpose as the mandate threshold.

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

[6]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).

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.[7]

2.2 Private Pension Savings

In addition to contributing to the pay-as-you-go system and making occupational pension savings, 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. In return, 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. For the remainder of this paper, I refer to this savings vehicle as 'private pension savings.' Total private pension savings in designated accounts in Switzerland equal roughly CHF 10 billion per year, representing 1.5% of GDP (Schüpbach and Müller, 2019).

There are strong tax incentives for private pension savings, in particular for relatively well-off individuals, 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 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.

3 Data

Assessing the effect of the occupational pension savings mandate on savings behavior requires measuring various forms of savings including private savings at the individual level. In general, this is challenging because high-quality microdata on private wealth are scarce. Switzerland is a particularly well-suited context to study savings behavior and wealth accumulation due to the availability of administrative tax data

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

on wealth.[8] However, the Swiss Federal Tax Administration does not have individual-level data on wealth because the wealth tax in Switzerland is only collected at the cantonal and municipal level and not at the national level. Therefore, cantonal tax data are needed.[9]

I draw on administrative tax data from the canton of Bern, providing detailed and comprehensive information on income, wealth, and savings.[10] The dataset is a large panel covering all taxpayers in the canton in the time period between 2002 and 2017. It is based on the tax returns that virtually all adult residents in Switzerland must file. This dataset has numerous merits for studying savings behavior (see also Brunner, Meier and Näf, 2020): *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 Sections 4 and 5 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. For all income and wealth components reported only at the household level, I equally assign half to each partner (following Fagereng et al., 2020). However, 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.

My key focus is the effect of the pension savings mandate on a set of savings outcomes. Since the mandate applies to gross earnings before deductions, I compute

[8]Only two OECD countries other than Switzerland still levy a wealth tax: Norway and Spain (OECD, 2018*c*). France imposed a wealth tax ('impôt de solidarité sur la fortune,' ISF) until 2018 when President Macron replaced it with a tax on real estate not used for business activities ('impôt sur la fortune immobilière,' IFI) (for more information, see Dupas, 2020; Tirard, 2020).

[9]Martínez (2020) provides a helpful discussion of Swiss cantonal tax data and presents evidence on the composition and joint distribution of income and wealth in Switzerland, drawing on a large new dataset compiled based on tax data from eight Swiss cantons that cover about half the population of Switzerland.

[10]The same dataset has been used by Brunner, Meier and Näf (2020) to study heterogeneity in returns to wealth. Bern is the second-largest Swiss canton by population, with 1.03 million residents in 2017. Population statistics by canton are available on the website of the Federal Statistical Office: https://www.bfs.admin.ch/bfs/en/home/statistics/population.html [accessed on 22 October 2021].

gross earnings from net earnings reported by employees in the tax return using the year-specific social 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 tax data, 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 of *gross savings* includes both 'active savings' and 'passive savings' from changes in asset prices (see Fagereng et al., 2021), implying that it is highly variable due to the fluctuations in accrued capital gains which makes precise estimation of the effect on private savings more difficult. Total savings equal the sum of all four savings components discussed above. I generally 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 by removing unreliable observations and excluding individuals below 25 years and above 60 years of age since the savings mandate only applies to working-age individuals.[11] 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.

[11] 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 genders and time and to exclude people going into early retirement which is relatively widespread in Switzerland (Dorn and Sousa-Poza, 2005).

4 Evidence from a Discontinuity in Mandate Coverage

Identifying and estimating the causal effect of contributing to an occupational pension account on savings choices is challenging because individuals generally decide simultaneously the level and allocation of their savings into different vehicles.[12] Even with variation in retirement savings induced by differences in pension plan features, this problem usually persists due to selection as workers with a strong preference for pension savings may sort into firms with generous employer-sponsored pension funds. Thus, selection-on-observable approaches are unlikely to recover unbiased estimates of the causal effect of occupational pension contributions on other types of savings due to confounding of unobserved characteristics.

To overcome this challenge, I use two complementary quasi-experimental research designs that exploit credibly exogenous variation in occupational pension savings. In this section, I employ a regression discontinuity design taking advantage of 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 (as part of which the aforementioned threshold was lowered substantially) using a difference-in-differences design, allowing me to investigate the dynamics and mechanisms of the savings response to becoming covered by the mandate.

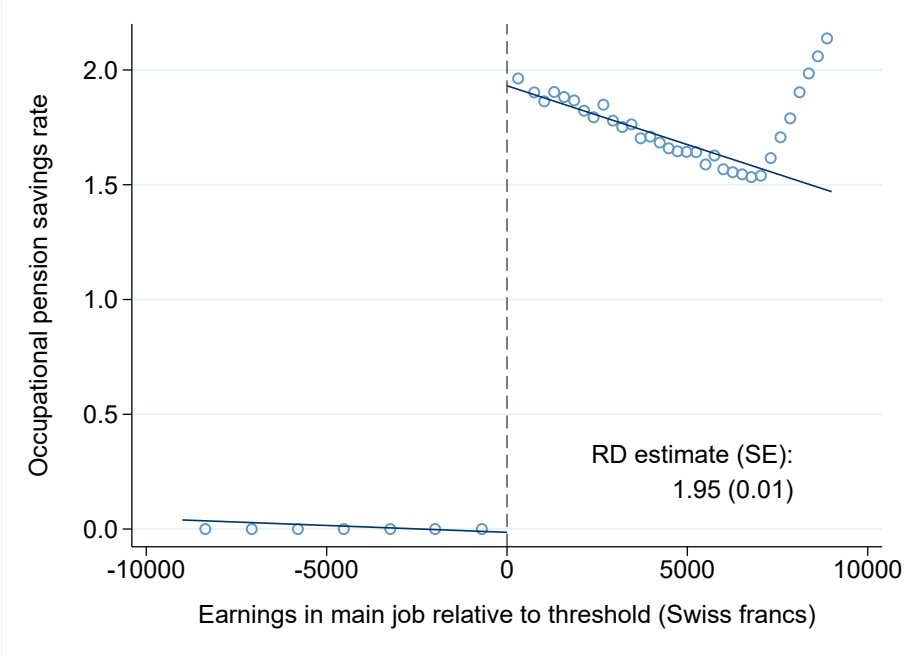
4.1 Discontinuity in Occupational Pension Savings

Employees with earnings above the mandate threshold automatically contribute a certain fraction of their earnings to an occupational pension account, while those below are not required to make those savings (see Section 2.1 for more detail). Importantly, because contribution rates are applied to the 'minimum coordinated salary' for those marginally above the threshold, occupational pension contributions are discontinuous at the cutoff. Given the minimum coordinated salary of CHF 3,525 in 2017, annual occupational pension savings jump from zero to roughly CHF 250–630 at the threshold, depending on the age-specific statutory contribution rate. Figure 1 depicts the relationship between contributions to occupational pension accounts and earnings in the main job, i. e. the treatment assignment.[13] There is a discontinuity in

[12] Randomized controlled trials that randomly assign different pension plans to individuals constitute a promising way of addressing this challenge. However, field experiments studying retirement savings policies are often not feasible due to a combination of financial, legal, and logistical constraints. A prominent exception is Duflo et al. (2006), although they do not observe non-retirement savings and thus cannot examine effects on total savings. I aim to approximate the ideal experiment by exploiting variation that is 'as good as random' using regression discontinuity and difference-in-differences designs.

[13] As occupational pension savings are not reported in tax forms and thus missing from the data (except for occupational pension buy-ins), pension fund contributions are predicted by applying the

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 4.3 for more detail on sample restrictions and estimation.

Source: Computations based on administrative tax data from the canton of Bern.

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 driven by the mandate.

I leverage this discontinuity in occupational pension savings using a sharp regression discontinuity (RD) design to identify and estimate the effect of the mandate on savings behavior.[14] In the following sections, I consider identification and interpretation, estimation and inference, validity tests, main results, robustness checks, and effect heterogeneity in turn.

4.2 Identification and Interpretation

Assuming that workers cannot sort perfectly below and above the threshold by manipulating their earnings, this policy feature generates variation in treatment assign-

statutory contribution rate to gross earnings according to Equation (7) in Appendix Section C.4.

[14]For reviews of the methodological literature on RD designs, see Imbens and Lemieux (2008); Lee and Lemieux (2010); Melly and Lalive (2020); Cattaneo and Titiunik (2021). Cattaneo, Idrobo and Titiunik (2020, 2021) provide comprehensive guides to practice.

ment that is orthogonal to other individual characteristics. Hence, it can be used to assess the causal effect of the pension savings mandate on savings outcomes. The fundamental idea is that individuals just below the threshold can be used as a valid counterfactual for those just above the threshold 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). This crucial identifying assumption – sometimes called continuity restriction or smoothness condition – 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:

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

The smoothness condition rules out endogenous sorting of individuals around the threshold (Lee, 2008; McCrary, 2008). It would be violated if individuals were able to manipulate their earnings strategically and precisely to impact their 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 extensively in Section 4.4.

The average treatment effect $\tau_{RD}(c)$ is specific to the group of employees in proximity of the threshold c. This subpopulation is an interesting group to study for at least three reasons. First, the marginal workers affected by the policy have low earnings. The 2017 threshold of CHF 21,150 is equivalent to roughly the 27th percentile of the distribution of main job earnings among working-age individuals in the canton of Bern. Low-income individuals may benefit particularly from improving their financial preparedness for retirement. Second, increasing (pension) savings of individuals with low earnings may promote their wealth accumulation. Through this channel, pension savings policies potentially affect the long-run evolution of wealth inequality which has increased in many countries over recent decades (Zucman, 2019), although not by much in Switzerland if pension wealth is included (Foellmi and Martínez, 2017). Third, from a policy perspective, the effect on individuals at the threshold is of particular interest because they are the ones affected by a potential change in the location of the threshold. After lowering the cutoff in the 2005 reform described in Section 5.1, policymakers are currently considering whether to decrease it further.[15]

[15]Concurrent to the writing of this paper, the Swiss parliament is debating a new reform of the occupational pension system. Among other changes, the government proposes to reduce

4.3 Estimation and Inference

Implementing the RD design calls for nonparametric estimation of the conditional expectation functions at the cutoff, i. e. at the boundary of the support of the running variable (Hahn, Todd and Van der Klaauw, 2001). Because global estimators implicitly place a high weight on observations far from the cutoff and are sensitive to the functional form of the polynomial approximation, local estimators using a low-dimensional model are generally to be preferred (Melly and Lalive, 2020). 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.[16] 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 (see Calonico et al., 2019).[17] The outcome variable Y_i represents various types of savings, including the private pension savings rate, occupational pension buy-in 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.

Local linear estimation requires choosing the bandwidth and weighting function. For ease of interpretation and to keep the effective estimation sample consistent, I use the same bandwidth for all savings outcomes in my main analysis. I choose the width of the estimation window in a data-driven and transparent way by computing for each of the savings outcomes of interest 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.[18] Based

the threshold. More information is available on the website of the Federal Social Insurance Office: www.bsv.admin.ch/bsv/de/home/sozialversicherungen/bv/reformen-und-revisionen.html [accessed on 19 October 2021].

[16] In Appendix Section E.4, I demonstrate that the estimates are not sensitive to the order of the local polynomial.

[17] Appendix Section E.5 documents that the effect estimates are robust to excluding control variables.

[18] The estimation results are similar when using a range of alternative bandwidths including the

on that approach, I restrict the sample to individuals within CHF 9,000 of the earnings threshold in my main analysis.[19] Note that I exclude individuals within CHF 350 of the threshold in my main analysis because their gross earnings cannot be imputed unambiguously (see Appendix Section C.2).[20] As the weighting function, I use the triangular kernel because it is MSE-optimal for point estimation when combined with an MSE-optimal bandwidth (Cattaneo and Titiunik, 2021).[21]

The main results are estimated using data from 2017 which is the most recent year in my dataset.[22] Besides focusing on working-age employees, individuals not observed in the preceding year are removed from the sample because private savings cannot be measured for them (4% of the sample). This results in the effective sample of 44,369 individuals within the estimation bandwidth.[23] 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. For inference, I use standard errors constructed 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 may be more robust in finite samples than conventional Huber-Eicker-White heteroskedasticity-robust standard errors (Calonico, Cattaneo and Titiunik, 2014*a*).[24]

4.4 Validation of the Empirical Approach

The main 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

outcome-specific optimal bandwidths, as documented in robustness checks presented in Appendix Sections E.1 and E.6.

[19]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 that employees within CHF 350 of the threshold are excluded.

[20]In Appendix Section E.3, I demonstrate that my estimates are not sensitive to the size of the 'donut hole' that is removed.

[21]In practice, the choice of the kernel function rarely leads to a significant change in the results (Lee and Lemieux, 2010; Melly and Lalive, 2020). Indeed, my results are robust to using different weighting functions such as the uniform kernel and the Epanechnikov kernel as I confirm in Appendix Section E.2.

[22]Appendix Section E.7 presents separate estimation results for each year between 2003 and 2017 using the year-specific cutoff values. The findings are broadly similar to the main results for 2017.

[23]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.

[24]In Appendix Section E.6, I verify the robustness of my results to using the robust bias correction approach proposed by Calonico, Cattaneo and Titiunik (2014*a*).

empirical approach. Appendix D presents and discusses the results of these validity tests in detail. In the following, I highlight the main findings.

The key threat to identification stems from employees manipulating their earnings strategically to sort around the threshold, in which case potential discontinuities in savings could be driven by differences in individual characteristics rather than the mandate. To affect their treatment status, employees would need to adjust their working hours in their current position or switch into a new job (or demand a change in the wage rate). On the other hand, employers might try to manipulate the earnings of their employees to be below the threshold, so they can avoid paying the employer share of the occupational pension contributions.

Empirically, I test for the presence of endogenous sorting by investigating the continuity of the density of earnings and the smoothness of predetermined characteristics across the threshold. In the density test, there appears to be some excess mass below but close to the cutoff (see Figure D.1). However, it is unlikely to be indicative of widespread earnings manipulation that would bias the RD results. When I repeat the RD analysis without the potential bunchers using a 'donut hole' approach (Bajari et al., 2011; Barreca et al., 2011), I find estimates similar to my main results (see Appendix Section E.3). Moreover, the RD results are very close to the difference-in-differences estimates (see Section 5.4) which are highly unlikely to be affected by strategic behavior since treatment and comparison groups are defined based on pre-reform earnings.

Assessing the continuity of covariates across the threshold, I do not find discontinuities in gender, marital status, number of children, and net wealth in the previous year (see Figure D.3). There is a slight discontinuity in age implying that individuals just above the threshold are (on average) half a year younger than those narrowly below. Again, to address concerns that this could reflect sorting, I re-run the main analysis using a range of donut hole sizes and generally find similar effect estimates (see Appendix Section E.3). Overall, individuals with earnings marginally above and marginally below the threshold have on average similar characteristics. This is additional evidence that the threat from manipulation of the running variable may be limited as it would likely affect the distribution of covariates correlated with savings outcomes around the threshold as well.

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. I find that the significant treatment effect estimates for private pension savings and occupational pension buy-ins (documented in Section 4.5) clearly stand out from the estimates at the placebo thresholds which are all insignificant and close to zero.

Given that the identifying assumptions appear valid, the RD approach allows

me to identify and estimate the causal effect of the savings mandate on the savings outcomes of interest. The results of the RD analysis are presented in the next section.

4.5 Main Results

The occupational pension savings mandate forces individuals just above the earnings threshold to increase their occupational pension savings rate by around 2 percentage points. In the following, I present my estimates of the causal effect of the mandate on the private pension savings rate, occupational pension buy-in rate, private savings rate, and total savings rate, obtained from estimating the RD model in Equation (2).[25]

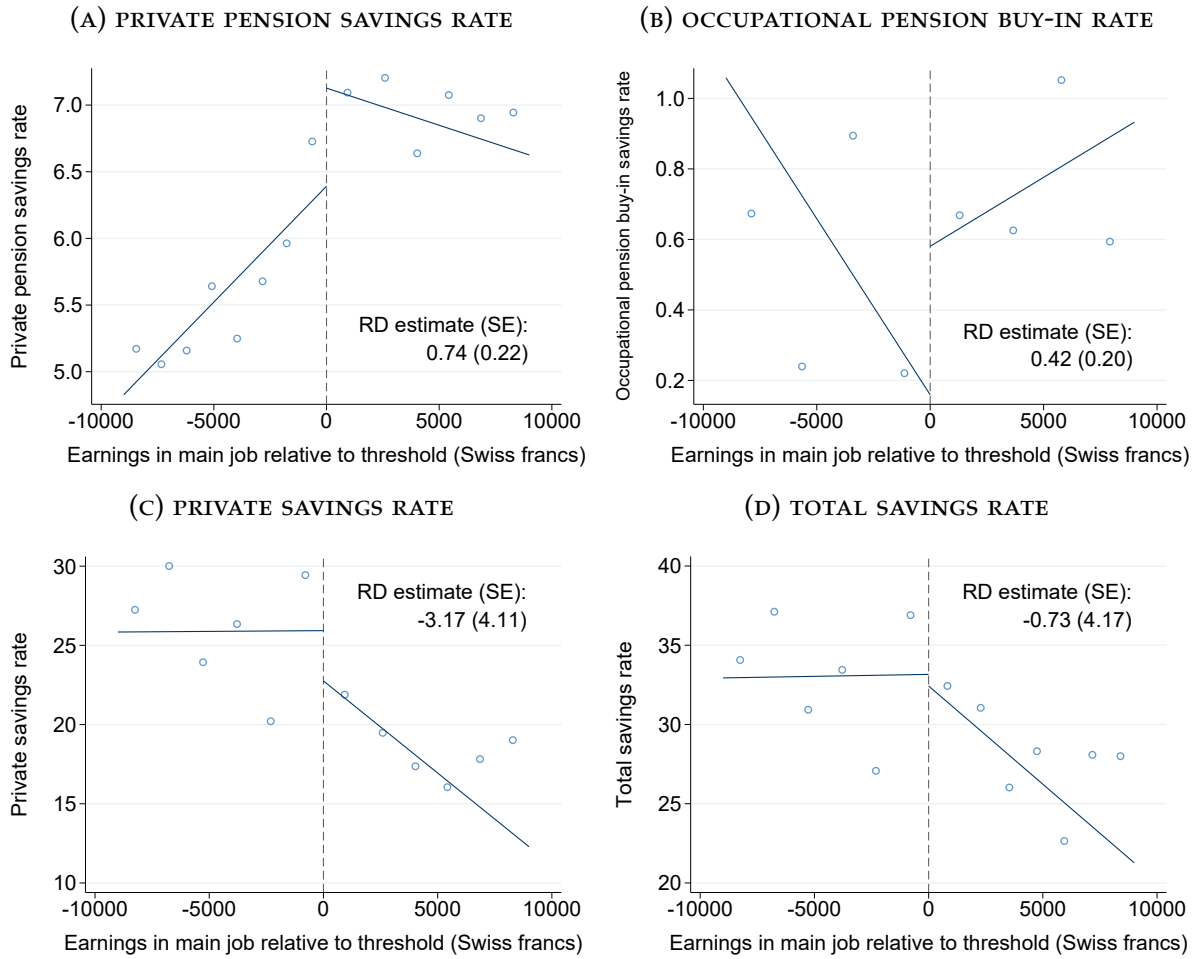
Figure 2 illustrates the main results by plotting the savings outcomes against the running variable. In lieu of a separate results table, it also reports the point estimates and standard errors for the treatment effect. The dots represent local sample means using non-overlapping quantile-spaced bins. 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). The lines represent linear regression fits separately estimated on each side of the threshold using a triangular kernel.

Starting with Panel A of Figure 2, I estimate that the savings mandate *increases* the private pension savings rate by 0.7 percentage points. This effect is statistically highly significant (p-value = 0.001). Relative to the mean within the estimation window, this is equivalent to an increase in the private pension savings rate of about 12%. The graphical evidence shows that while private pension savings increase with earnings below the cutoff, there is a clear jump at the threshold. Above the cutoff, the relationship is flat or even slightly decreasing. At first, the 'crowding-in' effect on private pension savings seems surprising. The standard neoclassical life-cycle model predicts that individuals reduce their private pension savings in response to a forced increase in occupational pension savings (Chetty et al., 2014), given that these savings vehicles have similar characteristics. A potential behavioral explanation is that being subject to the mandate increases the salience of the need to save for retirement and provides information about how to do so, which leads to positive spillovers to other forms of pension savings. I return to this explanation in Section 5.6 when I investigate potential mechanisms in more detail.

I also find a statistically significant positive effect (p-value = 0.032) on occupational pension buy-ins in Panel B of Figure 2. The point estimate is 0.4 percentage points. This corresponds to a 66% increase relative to the mean in the estimation sample. This result is less surprising than the positive effect on private pension savings be-

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

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. 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. See Section 3 for more information on data preparation and variable construction as well as Section 4.3 for more detail on sample restrictions and estimation.

Source: Computations based on administrative tax data from the canton of Bern.

cause employees subject to the savings mandate must be enrolled in an occupational pension fund which facilitates occupational pension buy-ins in the first place. Note that these buy-ins 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 of Figure 2 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). Unfortunately, my RD approach is underpowered to detect statistically significant effects on private savings.

How does the savings mandate affect the total amount of savings that individuals set aside? Panel D of Figure 2 depicts the impact of the mandate on total savings rates. There is no discontinuity at the cutoff which suggests that there is no discernible treatment effect. 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 even above and beyond the direct effect on occupational pension savings, through raising contributions to separate private pension accounts and occupational pension buy-ins. At the same time, I do not find evidence that total savings respond to the mandate. These findings imply that requiring occupational pension contributions does not crowd out but rather *crowds in* other forms of pension savings. The increase in retirement savings appears to be funded by an equally sized reduction in private savings, rather than lower current consumption, although the evidence for this is suggestive at best. Overall, the total savings rate seems to remain unaffected by the mandate but the composition shifts towards retirement savings.

In Appendix E, I examine the sensitivity of these results to different choices regarding the bandwidth, kernel weighting function, size of the donut hole, order of the local polynomial, and control variables. I also present effect estimates obtained by employing robust bias correction and using an MSE-optimal bandwidth following Calonico, Cattaneo and Titiunik (2014*a*) as well as effects estimated separately for each year in the data. The main findings are robust to varying the model specification and operationalization of the RD approach.

4.6 Effect Heterogeneity

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, I explore heterogeneity in 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

Table 1: Regression Discontinuity Estimates by Net Wealth

		Lagged net wealth in Swiss francs		
	All	Wealth < 0	Wealth $\in [0, 35k]$	Wealth $> 35k$
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 4.3 for more detail on sample restrictions and estimation. Stars indicate significance according to * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Computations based on administrative tax data from the canton of Bern.

as the cutoff value.[26] Table 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.

[26] 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.

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 the median age of 43 years as the cutoff. Table 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 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. Appendix Table A.1 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 Appendix Table A.2, I report effect estimates by marital status. The impact on private pension savings is identical for unmarried and married individuals, while married individuals appear to increase their occupational pension buy-ins more. The effects on private and total savings are too noisy to warrant further discussion.

Table 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 4.3 for more detail on sample restrictions and estimation. Stars indicate significance according to * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Computations based on administrative tax data from the canton of Bern.

5 Evidence from a Reform Extending Mandate Coverage

To pin down the dynamics of the effect of the occupational pension savings mandate and explore the mechanisms that drive the behavioral response, I proceed by studying the reform of the Swiss occupational pension system that expanded mandate coverage in 2005. I analyze how individuals newly covered by the savings mandate respond to being forced to contribute around 2% of their earnings to occupational pension accounts using a difference-in-differences approach. In the following, I describe the 2005 reform before considering identification and interpretation, estimation and inference, main results, robustness checks, effect heterogeneity, and potential

mechanisms in turn.

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.[27] 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. In the 2005 reform, the threshold was lowered to 3/4 of the maximum benefit level. As a consequence, the threshold fell from CHF 25,320 in 2004 to CHF 19,350 in 2005.

Appendix Figure A.1 depicts the relationship between occupational pension savings and earnings in the years immediately before and after the reform. The vertical difference between the lines illustrates the impact of the reform at each level of earnings. The jump in contributions in the earnings range between the 2004 threshold and the 2005 threshold is clearly visible. The graph also shows that occupational pension savings increased almost across the whole earnings distribution above the mandate threshold because, in addition to reducing the cutoff, the reform lowered the deduction from CHF 25,320 to CHF 22,575.[28] The decrease of the threshold and deduction aimed to offset some of the decline in benefit levels caused by the reduction of the conversion rate implemented in the same reform and improve pension benefit levels for employees with modest earnings.

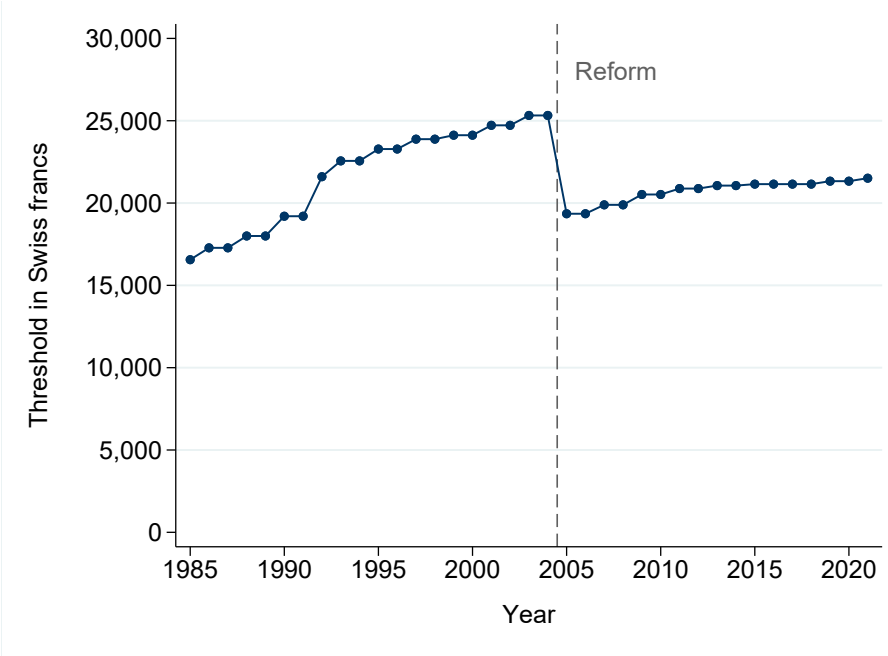
Around 140,000 employees in Switzerland who otherwise would not have been subject to the savings mandate became covered as a consequence of the reform (Ecoplan, 2010). The number of employees subject to the mandate increased by roughly 4%, bringing the total share of employees covered to 81%. The majority of newly affected employees were female, working part-time, and had low hourly wages below CHF 25 (Ecoplan, 2010). These groups of workers were the target population of the reform, suggesting that the policy achieved its objectives in terms of mandate coverage.

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. In addition to the changes already described, the conversion rate was lowered from 7.2% to 6.8%

[27] Detailed information on the reform of the occupational pension system between 2004 and 2006 ('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].

[28] In absence of the policy change, both the threshold and the deduction would have been equal to the maximum benefit from the pay-as-you-go system which was CHF 25,800 in 2005.

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.

over a transition period of ten years, women's retirement age in the occupational pension system was increased to 64 years in line with the rules in the pay-as-you-go system, and the contribution rate schedule for women was aligned with that of men. These changes are very unlikely to confound the difference-in-differences estimates because neither the treatment nor the comparison group was contributing to occupational pension accounts before the reform.

5.2 Identification and Interpretation

The 2005 reform induces exogenous variation in the coverage of the savings mandate that can be leveraged to estimate the causal effect of the mandate on individuals' savings behavior. I analyze the behavioral response to the policy change using a difference-in-differences (DD) design. Identification in the DD design relies on the parallel trend assumption. The fundamental idea is that savings outcomes of individuals subject to the mandate would have followed the same trend on average in absence of the policy as savings of individuals not covered by the mandate. If this key identifying assumption holds, the evolution of savings outcomes of individuals not covered by the mandate (comparison group) can be used as a counterfactual trend

for individuals that become subject to the mandate (treatment group).

I construct treatment and comparison groups based on earnings in 2004, the year directly preceding the reform. This approach prevents endogenous selection into treatment. The group definitions follow naturally from the provisions of the reform: the treatment group consists of individuals with earnings in 2004 that are below the 2004 threshold but above the 2005 threshold. The comparison group comprises individuals with 2004 earnings below but close to the 2005 cutoff.[29] 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. The comparison group is chosen to cover a similar earnings range as the treatment group. In the main analysis, it only includes individuals earning at least CHF 13,000 in 2004. Accordingly, the estimation sample includes all individuals with 2004 earnings in a bandwidth of approximately CHF 6,000 above and below the 2005 threshold.[30]

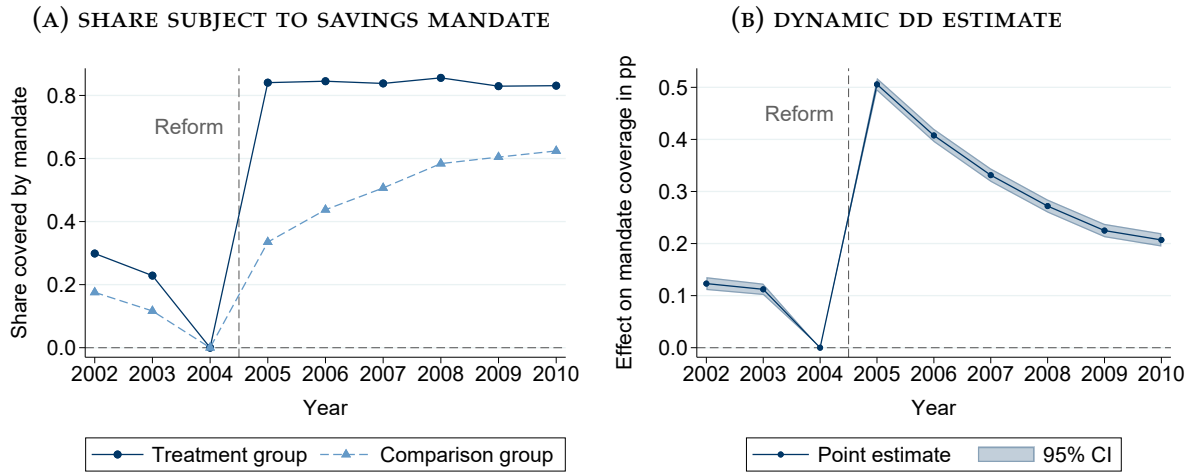
The parallel trend assumption would be violated if the savings of the treatment and comparison group were on different trends without the treatment occurring. Its validity can be assessed by checking the pre-treatment differences in trends (also called 'pre-trends'), assuming that pre-trends are informative for (counterfactual) post-trends (Rambachan and Roth, 2021). As documented in Section 5.4, I find no pre-trends for any of the savings outcomes. Contamination from simultaneous changes to other policies or the macroeconomic situation should not be of much concern either because these are unlikely to differentially affect the particular low-earning employees that the treatment and comparison groups comprise. As such, these potentially confounding impacts would be picked up by the year fixed effects in the DD models presented in Section 5.3. This holds for other types of year-specific shocks or common trends as well. Another reassuring fact is that RD estimates and DD estimates are similar, as mentioned earlier.

As the treatment and comparison groups are defined based on pre-reform earnings, there is an exogenously higher probability that individuals in the treatment group will be treated by the savings mandate relative to the comparison group, but the difference in the probabilities is not equal to one. Figure 4 illustrates the persistence of the treatment assignment. Panel A displays the evolution of the share of

[29] 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.

[30] I verify the robustness of the results to using smaller and larger comparison groups as well as donut hole approaches that remove individuals close to the cutoff in Appendix F.

Figure 4: Treatment Assignment



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 comparison groups 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 comparison groups. 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: Computations based on administrative tax data from the canton of Bern.

individuals in the treatment and comparison group who are subject to the savings mandate. By construction, nobody is covered by the mandate in 2004. This fraction jumps to about 85% in the treatment group and 35% in the comparison group after the reduction of the threshold in 2005. While the share stays about constant in the treatment group in later years, the fraction subject to the mandate in the comparison group slowly converges due to individuals' earnings growing over time and eventually rising above the cutoff. Panel B shows the corresponding dynamic effect on mandate coverage estimated from the dynamic DD model in Equation (4) using an indicator for contributing to occupational pension accounts as the dependent variable. This could be interpreted as the first stage in an instrumental-variable (IV) DD design. Appendix Figure A.2 displays the corresponding graphs for the occupational pension savings *rate* instead of the share subject to the mandate. It shows that the treatment causes a persistent increase in the occupational pension savings rate of about 1 percentage point in the treatment group relative to the comparison group.

Due to the 'fuzziness' of the treatment, the reduced-form results that I present later should be interpreted as *intention-to-treat* (ITT) effects. Following an IV DD approach, *treatment-on-the-treated* (TOT) effects can be obtained by scaling the (reduced-

form) ITT coefficients by the first-stage estimate.[31] The first-stage effect averaged over the post-reform period is 0.25 (SE = 0.0057) (see Table 3). Accordingly, the (static) ITT coefficients must be scaled by a factor of $1/0.25 = 4$ to be interpreted as TOT effects.

5.3 Estimation and Inference

To examine how the effect of the 2005 reform unfolds over time and to check the pre-trends for potential violations of the parallel trend 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 employee 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). As in the RD analysis, Y_{it} represents various kinds of savings rates, including the private pension savings rate, occupational pension buy-in rate, private savings rate, and total savings rate. The reduced-form parameters of interest are β_k for $k \geq 2005$. They capture the effect of the savings mandate in post-reform year k relative to pre-reform year 2004, provided that the parallel trend assumption holds. The coefficient for 2004 is normalized to zero by omitting it from the model.[32] The coefficients β_k for $k < 2004$ can be interpreted as pre-trends to assess the credibility of the parallel trend assumption.

To increase power and simplify interpretation, I aggregate the dynamic, year-specific DD effects into an average post-treatment effect by running additional 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).[33] The coefficient of interest is the static DD parameter β that captures the average effect of the savings mandate in the post-reform period, 2005 to 2010, relative to the year before the reform, 2004.

As the DD specifications include individual fixed effects, time-invariant con-

[31] For a thorough exposition of fuzzy DD designs and the underlying assumptions, see de Chaisemartin and D'Haultfœuille (2018).

[32] This normalization is necessary because the parameters β_k are only identified up to a constant due to the inclusion of individual fixed effects (Schmidheiny and Siegloch, 2022).

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

founders (such as employees' permanent earnings potential) are accounted for. Moreover, by restricting the sample to individuals in a narrow earnings range, I employ a local approach in which the treatment and comparison groups have quite similar characteristics. I do not include additional control variables in the main analysis because of the problems associated with controlling for time-varying covariates in DD models. As these controls may be affected by the treatment, they have the potential to be 'bad controls' inducing bias in the causal estimates. Recent econometric work points out that the inclusion of time-varying covariates in two-way fixed effect specifications requires invoking relatively strong assumptions to recover the average treatment effect (Sant'Anna and Zhao, 2020). In this case, the estimated effects are very similar when I do control for time-varying covariates such as marital status, number of children, earnings, and net wealth.

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) who argue that it should capture correlation of the treatment assignment within clusters, because the policy is implemented at the individual level.

I focus on individuals between 25 and 60 years of age and restrict the sample to individuals with some employment in each year considered.[34] 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, in turn, power. This is an innocuous restriction as it is not to be expected that it takes more than five years for the effects of the savings mandate to unfold, which is confirmed by the results presented in Section 5.4.[35] 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.3 displays summary statistics separately for the treatment and comparison group in 2004. There are 8,905 employees in the treatment group and 8,583 employees in the comparison group. Overall, the DD estimation sample includes 157,392 individual-year observations over the period 2002 to 2010. Comparing the groups' pre-reform characteristics, they are very similar with respect to the distribution of age, gender, and marital status. By construction, the treatment group has higher earnings in 2004 than the comparison group. Unsurprisingly, mean net wealth in the treatment group is also higher than in the comparison group, although

[34]Specifically, I only include employees with annual earnings in the main job of at least CHF 2,500 in every year. The precise value of the minimum cutoff is arbitrary but my estimates are not sensitive to the specific level.

[35]The effect estimates are somewhat noisier but qualitatively unchanged when extending the estimation window all the way to 2017.

the median is almost the same. The savings rates of the two groups before the reform are also very similar.

5.4 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. The pre-treatment trends in all panels are flat and insignificant, providing empirical support for the credibility of the parallel trend assumption. Regrettably, I can only estimate one pre-treatment coefficient for private and total savings 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.

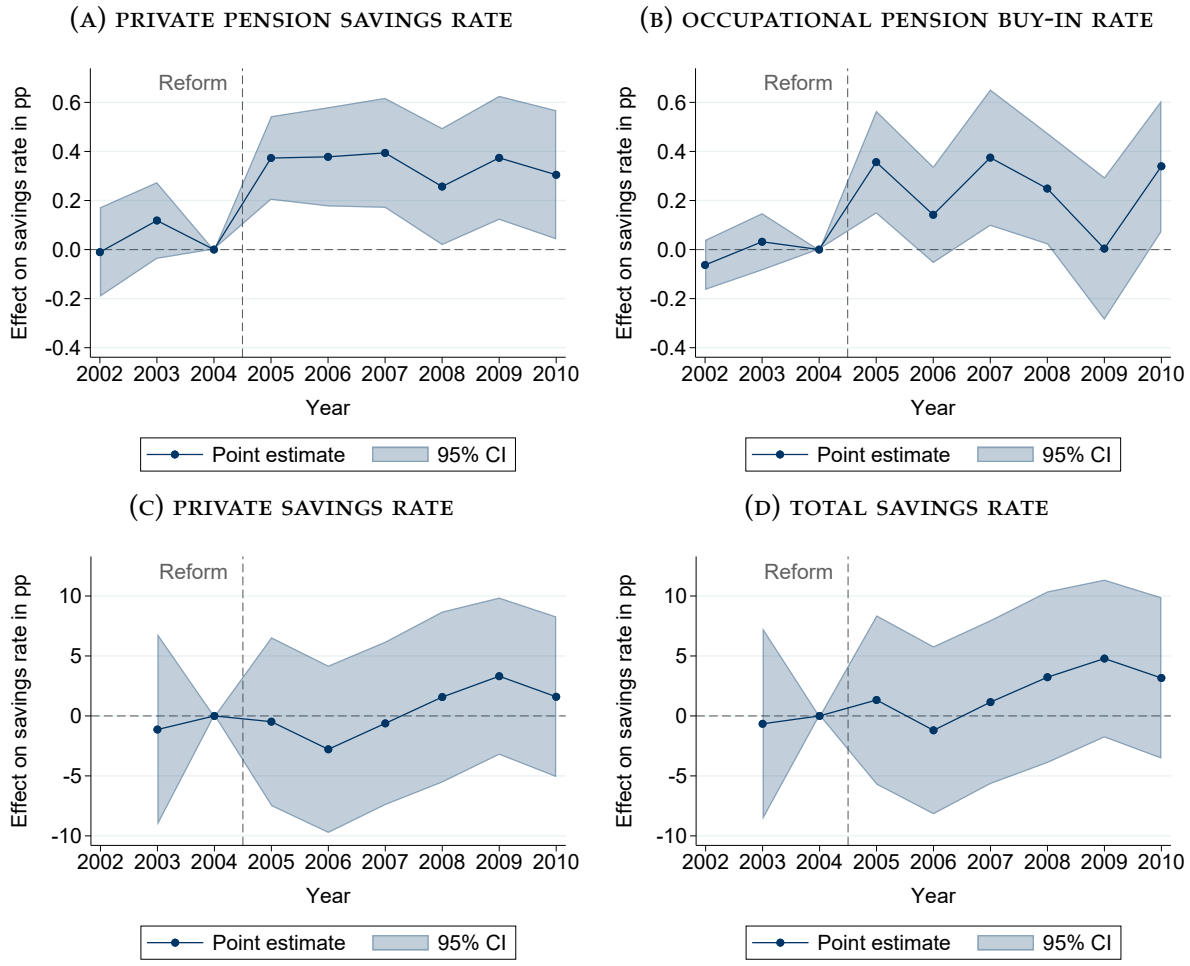
Panel A shows that private pension savings increase sharply in the treatment group relative to the comparison group after the reform in 2005. The results suggest that treated individuals respond to the reform, which on average raises their occupational pension savings rate by 1 percentage point (relative to the comparison group), by *increasing* the private pension savings rate by between 0.3 and 0.4 percentage points. The crowding-in effect arises instantaneously after the reform and is quite constant afterwards, lending additional credibility to the interpretation that this estimate reflects a causal effect of becoming subject to the savings mandate. The statistically significant positive DD estimate confirms the surprising result of the RD analysis. I shed more light on potential mechanisms driving this crowding-in effect in Section 5.6.

Occupational pension buy-ins, depicted in Panel B, also rise in response to the reform. The year-specific estimates vary between 0 and 0.4 percentage points, depending on the year, but they are quite noisy. As noted earlier, this is due to the inherent lumpiness of voluntary buy-ins.

Private savings rates in Panel C do not appear to respond to the policy change. There is a slight decrease in the two years following the reform, but given the imprecision of the estimates, this pattern does not provide conclusive evidence of a negative effect. Overall, the year-specific coefficients are close to zero and statistically insignificant.

Panel D displays the estimated effects on the total savings rate. Due to the wide confidence intervals of the estimates for private savings, I cannot draw any definite conclusions for total savings either. The coefficients hover around zero in the years directly after the reform and increase towards the end of the estimation window, but they are statistically insignificantly different from zero in each period. It seems implausible that these dynamics represent causal long-run effects of the reform rather

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 comparison groups 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: Computations based on administrative tax data from the canton of Bern.

than just noise from the high variability of private savings rates.

Table 3 summarizes the aggregated post-treatment effects estimated using the static DD specification. In addition to the impacts on savings outcomes, column (1) reports the effect on the fraction of individuals contributing to occupational pension accounts. This can be used as the first stage when moving from reduced-form ITT effects to TOT effects in an IV DD approach. For example, scaling the effect on the private pension savings rate by the first stage results in a TOT effect of $0.31/0.25 = 1.24$.

To compare the results from the RD and DD approaches, I set the respective effect estimates in relation to the change in savings rates that these designs exploit. The

Table 3: Static Difference-in-Differences Estimates

	First stage (1)	Private pension savings rate (2)	Occupational buy-in rate (3)	Private savings rate (4)	Total savings rate (5)
Static DD	0.25***	0.31***	0.25***	1.00	2.40
	(0.0057)	(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 comparison groups 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: Computations based on administrative tax data from the canton of Bern.

RD estimate for the change in the occupational pension savings rate is around 2 percentage points (see Figure 1); the change in the DD design is roughly 1 percentage point (see Appendix Figure A.2). Thus, after dividing the RD effects by two, both sets of estimates can be interpreted as the change in savings rates in response to a 1 percentage point increase in the occupational pension savings rate. Focusing on the statistically significant effects that I found in both the RD and DD analysis, the scaled coefficients are of remarkably similar magnitude: The positive effect on the private pension savings rate of a 1 percentage point increase in the occupational pension savings rate is 0.37 percentage points in the RD and 0.31 percentage points in the DD approach. The equivalent effect on the occupational pension buy-in rate is 0.21 percentage points in the RD and 0.25 percentage points in the DD approach. Overall, I conclude that the findings of the DD analysis are highly consistent with the RD results.

In Appendix F, I document that the DD results are robust to using alternative definitions of the treatment and comparison group.

5.5 Effect Heterogeneity

The crowding-in effect of the occupational pension savings mandate on private pension savings seems at odds with predictions of standard economic theory. To investigate the driving forces behind this behavioral response, I examine effect heterogeneity by individual characteristics in this section. Building on these findings, I shed more

Table 4: 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 comparison groups 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: Computations based on administrative tax data from the canton of Bern.

light on the specific mechanisms through which the behavioral response operates in the subsequent Section 5.6.

To estimate heterogeneous effects, I include separate treatment indicators for the subgroups considered in the static DD specification in Equation (5). Table 4 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. As in the RD analysis, the aggregate treatment effect is purely driven by older individuals. 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 of equal size. 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 significantly different from zero, except for the estimate for younger individuals which is a precisely estimated null effect. In short, the positive effect of the savings mandate on private pension savings appears to be mainly driven by older, female, and married individuals. Of course, these categories overlap which could explain some of the similarities in the subgroup-specific effect estimates – for example, men in the sample are much more likely to be unmarried than women.[36]

As married individuals with low earnings in the range considered here are usually secondary earners in their household, these findings suggest that the crowding-in effect may be driven by individuals with relatively high household income which facilitates shifting savings into preferentially taxed private pension accounts in response to becoming subject to the mandate. Consistent with this mechanism, unmarried individuals respond to being forced to contribute to occupational pension accounts by reducing their private pension savings because liquidity constraints are more likely to be binding for them as they are often the main earner in their household. This pattern could also (partially) reflect that high-income households have stronger financial incentives to contribute to private pension accounts because they face higher marginal tax rates. Potential explanations for the increase in private pension savings of married employees are a reduction in information frictions and an increase in salience regarding retirement savings. I test these hypotheses in the next section.

5.6 Mechanisms

To provide evidence on the conjecture that having another earner in the household facilitates shifting of savings towards private pension accounts in response to the reform, I allow for heterogeneous coefficients by household income. For this purpose, 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.[37] Column 2 of Table 5 reports the estimated effects. In line with my hypothesis, individuals with household income above the median strongly increase their private pension savings rate in response to the reform, by roughly 1 percentage

[36] 19% of women in the estimation sample are unmarried, whereas the unmarried fraction is as high as 62% among men.

[37] 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 financial income, self-employment income, or transfer income.

Table 5: Private Pension Savings: Mechanisms

	Private pension savings rate		
	(1)	(2)	(3)
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 × no private pension savings before reform			0.64***
			(0.091)
Static DD × positive private pension savings before reform			-0.46***
			(0.15)
Individual fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Observations	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 comparison groups 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: Computations based on administrative tax data from the canton of Bern.

point. On the other hand, individuals with below-median household income reduce their private pension savings rate by 0.3 percentage points. These findings provide evidence that sharing a household with another earner facilitates shifting of savings into preferentially taxed private pension accounts in response to the reform.

But why do individuals with relatively high household income increase their private pension savings in response to becoming covered by the mandate? A potential behavioral explanation is that becoming subject to the mandate and being enrolled in an occupational pension plan provides new information about the need to save for retirement and the pension system, including the tax advantages associated with both occupational and private pension savings. The attention that is drawn to these issues may encourage individuals to have a careful look at their retirement situation. In short, the mandate could help to reduce information frictions and increase salience of pension savings.[38] In this framework, individuals fail to fully optimize their sav-

[38] I cannot disentangle information effects that 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 specifically drawn to the subject (see Chetty, Looney and Kroft, 2009). For the interpretation of my results, this distinction is of secondary

ings portfolio in absence of the policy. This seems plausible as the prevalence of individuals who appear to have insufficient retirement savings is well documented in the literature (Skinner, 2007).[39]

I test for information and salience effects by estimating separate coefficients for individuals who have had positive private pension savings in at least one year in the three-year period before the 2005 reform and individuals who have not contributed to private pension accounts before the reform.[40] Arguably, information and salience effects should be larger for individuals who have not had positive private pension savings 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. If, on the contrary, their lack of private pension savings reflected their revealed preference for (or rather, aversion to) 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 3 of Table 5 displays the effect estimates by past savings behavior. The results show that the positive effect on private pension savings is concentrated among the individuals who have not contributed to private pension accounts in the three years leading up to the reform. They increase their private pension savings rate by 0.6 percentage points in response to the policy change. Conversely, individuals with positive private pension savings before the reform reduce their savings by 0.5 percentage points. Both estimates are statistically significant at the 1% level. This finding is even more striking considering that some individuals 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.

These results suggest that the crowding-in effect of the occupational pension savings mandate on private pension savings is driven by a reduction in information frictions and an increase in salience of the pension system. Information and salience effects are muted for individuals who were already aware of the benefits of contributing to private pension accounts and thus more likely to have optimized their savings portfolio already before the reform. Being required by the mandate to contribute to occupational pension funds crowds out private pension savings for those individuals, suggesting that these savings vehicles are substitutes for them. Overall, these findings highlight the importance of providing information about and increasing the salience of pension savings when policymakers aim to improve financial prepared-

importance.

[39]People saving too little for retirement due to myopia is also one of the main rationales for the occupational pension savings mandate in Switzerland. The other major argument is that there is a market failure due to moral hazard because individuals are aware that they would receive supplementary benefits covering their minimum living costs, even if they did not save for retirement at all.

[40]For reference, 70% of individuals in the treatment group have not made any private pension savings in the pre-reform period between 2002 and 2004.

ness for retirement. The presented evidence of information and salience effects 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 Mitchelli, 2007; Lusardi and Mitchell, 2011*a*,*b*).

6 Conclusion

Against the backdrop of demographic change, a wide range of pension policies aiming to boost retirement savings and ensure adequate living standards in old age have been implemented around the globe. In this paper, I draw on rich administrative tax data on income, wealth, and savings to study the impact of the occupational pension savings mandate on savings behavior in Switzerland. Using regression discontinuity and difference-in-differences designs that exploit credibly exogenous variation in occupational pension savings, I document three main findings:

First, being required to contribute to an occupational pension account *crowds in* other types of retirement savings such as preferentially taxed private pension savings in separate accounts and occupational pension buy-ins. The results suggest that a 1 percentage point increase in the occupational pension savings rate induced by the mandate increases the private pension savings rate by 0.3–0.4 percentage points and occupational pension buy-ins by 0.2–0.3 percentage points. This finding is surprising as standard life-cycle models predict the opposite effect, given that these different forms of pension savings have similar characteristics (Chetty et al., 2014). Yet, it is consistent with the positive impact of 401(k) eligibility on IRA assets in the US documented in Gelber (2011).

Second, the crowding-in effect on private pension savings is driven by reduced information frictions and increased salience of pension savings and facilitated by having another (main) earner in the household. The evidence of information and salience effects is consistent with a growing literature finding that information provision related to the retirement system raises pension savings (Duflo and Saez, 2003; Duflo et al., 2006; Goda, Manchester and Sojourner, 2014; Dolls et al., 2018).

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. Taken together, they suggest that the pension savings mandate does not necessarily increase total savings but shifts the composition of the savings portfolio towards accounts earmarked for retirement. Thus, even if the mandate does not increase total savings, it can improve financial preparedness for retirement by raising the share of assets that are only accessible in old age. In light of the documented information and salience effects, it is important to provide comprehensible and transparent information about the pension system and encourage people to consider their prospective pension situation for financial incentives such as preferentially taxed private pension accounts to effectively boost retirement savings. Such efforts may have persistent effects as temporary policies to increase pension savings can change employees' attitudes and interest in savings (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).

There is plenty of room for future research on the impacts of pension savings mandates. I highlight three areas in the following. First, to establish precise results for private and total savings, it would be valuable to disentangle active savings from accrued capital gains using data at the level of single assets or transactions (see Fagereng et al., 2021). Unfortunately, data as granular as this are very difficult to obtain. Second, I leverage local variation in occupational pension savings which provides high internal validity but may limit external validity as the effects could be specific to the low-earning individuals near the mandate threshold. Future work assessing the effect of similar policies on savings of average- or high-earners can shed light on whether the impact of the mandate varies across the earnings distribution. Third, formalizing a model of savings behavior that explains the savings response to the mandate and pins down the underlying mechanisms would help to refine our understanding, result in more robust policy implications, and facilitate welfare analysis. Nevertheless, following the 'pragmatic approach' advocated by Chetty (2015), the empirical evidence presented in this paper has practical value even in the absence of a fully developed theory of savings behavior by helping policymakers aiming to increase retirement savings to find the most effective policy tools to achieve their objective.

References

Abadie, Alberto, Susan Athey, Guido W. Imbens, and Jeffrey Wooldridge. 2022. "When Should You Adjust Standard Errors for Clustering?" *The Quarterly Journal of Economics*, forthcoming.

Annaheim, Melanie, and Lukas Heim. 2021. "Household Wealth in Switzerland: Concepts and Trends in an International Comparison." available on the website of the Swiss National Bank: https://data.snb.ch/en/topics/texts#!/doc/focus_20210429 [accessed on 25 October 2021].

Arnberg, Søren, and Mikkel Barslund. 2014. "The Crowding-Out Effect of Mandatory Labour Market Pension Schemes on Private Savings." CEPS Working Document No. 390.

Attanasio, Orazio P., and Thomas DeLeire. 2002. "The Effect of Individual Retirement Accounts on Household Consumption and National Saving." *The Economic Journal*, 112(481): 504–38.

Bach, Laurent, Laurent E. Calvet, and Paolo Sodini. 2020. "Rich Pickings? Risk, Return, and Skill in Household Wealth." *American Economic Review*, 110(9): 2703–47.

Bajari, Patrick, Han Hong, Minjung Park, and Robert Town. 2011. "Regression Discontinuity Designs with an Endogenous Forcing Variable and an Application to Contracting in Health Care." NBER Working Paper No. 17643.

Barreca, Alan I., Melanie Guldi, Jason M. Lindo, and Glen R. Waddell. 2011. "Saving Babies? Revisiting the Effect of Very Low Birth Weight Classification." *The Quarterly Journal of Economics*, 126(4): 2117–23.

Benartzi, Shlomo, and Richard H. Thaler. 2013. "Behavioral Economics and the Retirement Savings Crisis." *Science*, 339(6124): 1152–53.

Benjamin, Daniel J. 2003. "Does 401(k) Eligibility Increase Saving?: Evidence from Propensity Score Subclassification." *Journal of Public Economics*, 87(5-6): 1259–90.

Bernheim, B. Douglas. 2002. "Taxation and Saving." In *Handbook of Public Economics, Vol. 3.* , ed. Alan J. Auerbach and Martin Feldstein, 1173–1249. Elsevier.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-in-Differences Estimates?" *The Quarterly Journal of Economics*, 119(1): 249–75.

Blanchard, Olivier, and Jean Tirole. 2021. *Major Future Economic Challenges.* Report to President Macron. France Stratégie, Paris.

Blumenstock, Joshua, Michael Callen, and Tarek Ghani. 2018. "Why Do Defaults Affect Behavior? Experimental Evidence from Afghanistan." *American Economic Review*, 108(10): 2868–2901.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess. 2021. "Revisiting Event Study Designs: Robust and Efficient Estimation." Working Paper.

Bozio, Antoine, Thomas Breda, and Julien Grenet. 2020. "Does Tax-Benefit Linkage Matter for the Incidence of Social Security Contributions?" PSE Working Paper No. 43.

Börsch-Supan, Axel H., Michela Coppola, and Anette Reil-Held. 2012. "Riester Pensions in Germany: Design, Dynamics, Targetting Success and Crowding-In." NBER Working Paper No. 18014.

Brunner, Marc, Jonas Meier, and Armando Näf. 2020. "Heterogeneity in Returns to Wealth: Evidence from Swiss Administrative Data." Working Paper.

Bütler, Monika. 2009. "Switzerland: High Replacement Rates and Generous Subsistence as a Barrier to Work in Old Age." *The Geneva Papers on Risk and Insurance - Issues and Practice*, 34(4): 561–77.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik. 2014*a*. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica*, 82(6): 2295–2326.

Calonico, Sebastian, Matias D. Cattaneo, and Rocío Titiunik. 2014*b*. "Robust Data-Driven Inference in the Regression-Discontinuity Design." *The Stata Journal*, 14(4): 909–46.

Calonico, Sebastian, Matias D. Cattaneo, and Rocío Titiunik. 2015. "Optimal Data-Driven Regression Discontinuity Plots." *Journal of the American Statistical Association*, 110(512): 1753–69.

Calonico, Sebastian, Matias D. Cattaneo, Max H. Farrell, and Rocío Titiunik. 2017. "rdrobust: Software for Regression-Discontinuity Designs." *The Stata Journal*, 17(2): 372–404.

Calonico, Sebastian, Matias D. Cattaneo, Max H. Farrell, and Rocío Titiunik. 2019. "Regression Discontinuity Designs Using Covariates." *The Review of Economics and Statistics*, 101(3): 442–51.

Cattaneo, Matias D., and Rocio Titiunik. 2021. "Regression Discontinuity Designs." prepared for the *Annual Review of Economics*.

Cattaneo, Matias D., Michael Jansson, and Xinwei Ma. 2018. "Manipulation Testing Based on Density Discontinuity." *The Stata Journal*, 18(1): 234–61.

Cattaneo, Matias D., Michael Jansson, and Xinwei Ma. 2020. "Simple Local Polynomial Density Estimators." *Journal of the American Statistical Association*, 115(531): 1449–55.

Cattaneo, Matias D., Nicolás Idrobo, and Rocío Titiunik. 2020. *A Practical Introduction to Regression Discontinuity Designs: Foundations. Elements in Quantitative and Computational Methods for the Social Sciences*, Cambridge University Press.

Cattaneo, Matias D., Nicolás Idrobo, and Rocío Titiunik. 2021. *A Practical Introduction to Regression Discontinuity Designs: Extensions. Elements in Quantitative and Computational Methods for the Social Sciences*, Cambridge University Press (to appear).

Chetty, Raj. 2015. "Behavioral Economics and Public Policy: A Pragmatic Perspective." *American Economic Review*, 105(5): 1–33.

Chetty, Raj, Adam Looney, and Kory Kroft. 2009. "Salience and Taxation: Theory and Evidence." *American Economic Review*, 99(4): 1145–77.

Chetty, Raj, John N Friedman, Søren Leth-Petersen, Torben Heien Nielsen, and Tore Olsen. 2014. "Active vs. Passive Decisions and Crowd-Out in Retirement Savings Accounts: Evidence from Denmark." *The Quarterly Journal of Economics*, 129(3): 1141–1219.

Choukhmane, Taha. 2021. "Default Options and Retirement Saving Dynamics." Working Paper.

de Chaisemartin, Clément, and Xavier D'Haultfœuille. 2018. "Fuzzy Differences-in-Differences." *The Review of Economic Studies*, 85(2): 999–1028.

Dolls, Mathias, Philipp Doerrenberg, Andreas Peichl, and Holger Stichnoth. 2018. "Do Retirement Savings Increase in Response to Information About Retirement and Expected Pensions?" *Journal of Public Economics*, 158: 168–79.

Dorn, David, and Alfonso Sousa-Poza. 2003. "Why Is the Employment Rate of Older Swiss so High? An Analysis of the Social Security System." *The Geneva Papers on Risk and Insurance - Issues and Practice*, 28(4): 652–72.

Dorn, David, and Alfonso Sousa-Poza. 2005. "The Determinants of Early Retirement in Switzerland." *Swiss Journal of Economics and Statistics*, 141(2): 247–83.

Duflo, Esther, and Emmanuel Saez. 2003. "The Role of Information and Social Interactions in Retirement Plan Decisions: Evidence from a Randomized Experiment." *The Quarterly Journal of Economics*, 118(3): 815–42.

Duflo, Esther, William Gale, Jeffrey Liebman, Peter Orszag, and Emmanuel Saez. 2006. "Saving Incentives for Low- and Middle-Income Families: Evidence from a Field Experiment with H&R Block." *The Quarterly Journal of Economics*, 121(4): 1311–46.

Dupas, Marine. 2020. "Wealth Tax: France." Wealth Tax Commission Background Paper 134.

Ecoplan. 2010. "Herabsetzung der Eintrittsschwelle in der 1. BVG-Revision." Beiträge zur sozialen Sicherheit. Forschungsbericht Nr. 8/10, Bundesamt für Sozialversicherungen, Bern.

Engen, Eric M., William G. Gale, and John Karl Scholz. 1996. "The Illusory Effects of Saving Incentives on Saving." *Journal of Economic Perspectives*, 10(4): 113–38.

Fagereng, Andreas, Luigi Guiso, Davide Malacrino, and Luigi Pistaferri. 2020. "Heterogeneity and Persistence in Returns to Wealth." *Econometrica*, 88(1): 115–70.

Fagereng, Andreas, Martin Blomhoff Holm, Benjamin Moll, and Gisle Natvik. 2021. "Saving Behavior Across the Wealth Distribution: The Importance of Capital Gains." Working Paper.

Federal Statistical Office. 2019. "Pensionskassenstatistik 2017." Bundesamt für Statistik, Neuchâtel.

Foellmi, Reto, and Isabel Z. Martínez. 2017. "Volatile Top Income Shares in Switzerland? Reassessing the Evolution between 1981 and 2010." *The Review of Economics and Statistics*, 99(5): 793–809.

Gelber, Alexander M. 2011. "How Do 401(k)s Affect Saving? Evidence from Changes in 401(k) Eligibility." *American Economic Journal: Economic Policy*, 3(4): 103–22.

Gelman, Andrew, and Guido Imbens. 2019. "Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs." *Journal of Business & Economic Statistics*, 37(3): 447–56.

Goda, Gopi Shah, Colleen Flaherty Manchester, and Aaron J. Sojourner. 2014. "What Will My Account Really Be Worth? Experimental Evidence on How Retirement Income Projections Affect Saving." *Journal of Public Economics*, 119: 80–92.

Hahn, Jinyong, Petra Todd, and Wilbert Van der Klaauw. 2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." *Econometrica*, 69(1): 201–209.

Hubbard, R. Glenn, and Jonathan S. Skinner. 1996. "Assessing the Effectiveness of Saving Incentives." *Journal of Economic Perspectives*, 10(4): 73–90.

Imbens, Guido W., and Thomas Lemieux. 2008. "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics*, 142(2): 615–35.

Krapf, Matthias. 2019. "Does Income Risk Affect the Wealth Distribution?" Working Paper.

Kuhn, Ursina. 2020. "Augmented Wealth in Switzerland: The Influence of Pension Wealth on Wealth Inequality." *Swiss Journal of Economics and Statistics*, 156(19).

Lee, David S. 2008. "Randomized Experiments from Non-random Selection in U.S. House Elections." *Journal of Econometrics*, 142(2): 675–97.

Lee, David S., and Thomas Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature*, 48(2): 281–355.

Lusardi, Annamaria, and Olivia S. Mitchell. 2011*a*. "Financial Literacy and Planning: Implications for Retirement Wellbeing." NBER Working Paper No. 17078.

Lusardi, Annamaria, and Olivia S. Mitchell. 2011*b*. "Financial Literacy Around the World: An Overview." *Journal of Pension Economics and Finance*, 10(4): 497–508.

Lusardi, Annamaria, and Olivia S Mitchelli. 2007. "Financial Literacy and Retirement Preparedness: Evidence and Implications for Financial Education." *Business Economics*, 42(1): 35–44.

Madrian, Brigitte C., and Dennis F. Shea. 2001. "The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior." *The Quarterly Journal of Economics*, 116(4): 1149–87.

Martínez, Isabel Z. 2020. "Evidence from Unique Swiss Tax Data on the Composition and Joint Distribution of Income and Wealth." Working Paper.

McCrary, Justin. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics*, 142(2): 698–714.

Melly, Blaise, and Rafael Lalive. 2020. "Estimation, Inference, and Interpretation in the Regression Discontinuity Design." Working Paper No. 20-05, Department of Economics, University of Bern.

OECD. 2018*a*. *Financial Incentives and Retirement Savings.* OECD Publishing, Paris.

OECD. 2018*b*. *OECD Pensions Outlook 2018.* OECD Publishing, Paris.

OECD. 2018*c*. *The Role and Design of Net Wealth Taxes in the OECD.* OECD Tax Policy Studies No. 26, OECD Publishing, Paris.

Pei, Zhuan, David S. Lee, David Card, and Andrea Weber. 2021. "Local Polynomial Order in Regression Discontinuity Designs." *Journal of Business & Economic Statistics*.

Poterba, James M. 2014. "Retirement Security in an Aging Population." *American Economic Review*, 104(5): 1–30.

Poterba, James M., Steven F. Venti, and David A. Wise. 1996. "How Retirement Saving Programs Increase Saving." *Journal of Economic Perspectives*, 10(4): 91–112.

Rambachan, Ashesh, and Jonathan Roth. 2021. "An Honest Approach to Parallel Trends." Working Paper.

Sant'Anna, Pedro H.C., and Jun Zhao. 2020. "Doubly Robust Difference-in-Differences Estimators." *Journal of Econometrics*, 219(1): 101–22.

Schöchli, Hansueli. 2021. "Die geplante Rentenreform ist für Angestellte mit tiefen Löhnen ein zweischneidiges Schwert." *Neue Zürcher Zeitung*. available at: https://www.nzz.ch/wirtschaft/die-geplante-rentenreform-drueckt-nettoloehne-und-kostet-stellen-ld.1641932 [accessed on 15 October 2021].

Schmidheiny, Kurt, and Sebastian Siegloch. 2022. "On Event Studies and Distributed-Lags in Two-Way Fixed Effects Models: Identification, Equivalence, and Generalization." Working Paper.

Schüpbach, Salome, and Stefan Müller. 2019. "Wie viel Kapital steckt in der Säule 3a?" *Soziale Sicherheit CHSS*, 4.

Skinner, Jonathan. 2007. "Are You Sure You're Saving Enough for Retirement?" *Journal of Economic Perspectives*, 21(3): 59–80.

Thaler, Richard H., and Shlomo Benartzi. 2004. "Save More Tomorrow™: Using Behavioral Economics to Increase Employee Saving." *Journal of Political Economy*, 112(S1): 164–87.

Tirard, Jean-Marc. 2020. "Wealth Taxes in France." Wealth Tax Commission Background Paper 135.

Zucman, Gabriel. 2019. "Global Wealth Inequality." *Annual Review of Economics*, 11(1): 109–38.

Online Appendix

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

David Burgherr[1]

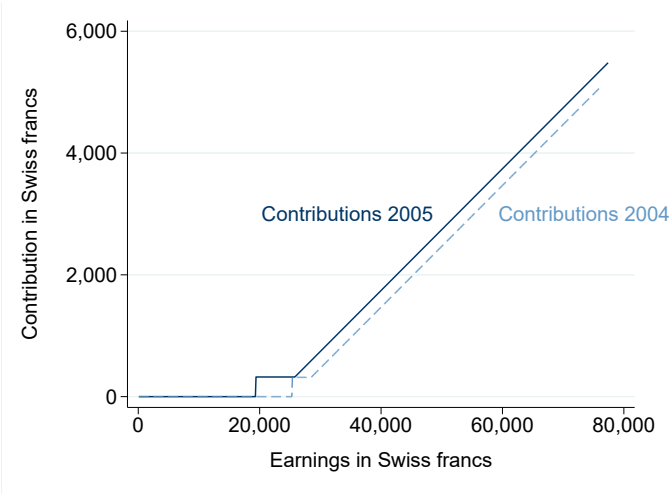
Contents

[1]London School of Economics, International Inequalities Institute. Email: d.m.burgherr@lse.ac.uk.

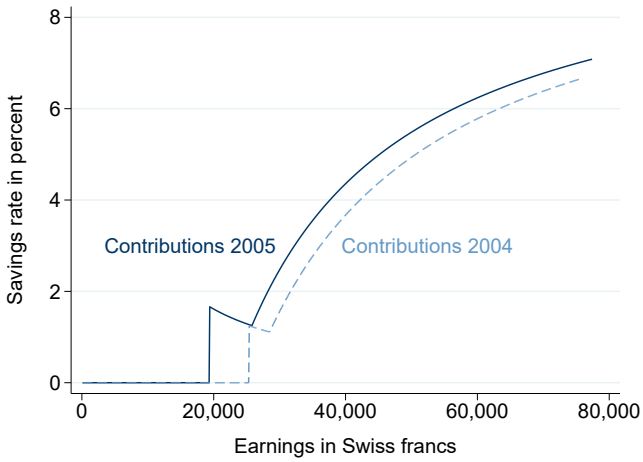
A Appendix Figures and Tables

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

(a) contribution levels in swiss francs

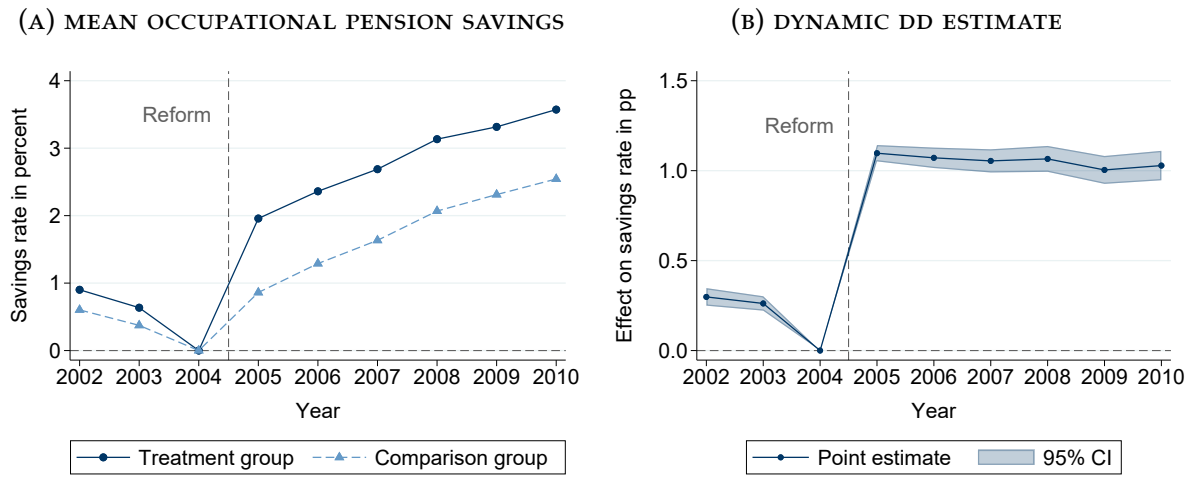


(b) savings rates as a percentage of earnings



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



Notes: The figure displays the effect of the 2005 reform on occupational pension savings rates. Individuals are sorted into treatment and comparison groups 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 comparison groups. 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: Computations based on administrative tax data from the canton of Bern.

Table A.1: 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 4.3 for more detail on sample restrictions and estimation. Stars indicate significance according to * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Computations based on administrative tax data from the canton of Bern.

Table A.2: 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 4.3 for more detail on sample restrictions and estimation. Stars indicate significance according to * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Computations based on administrative tax data from the canton of Bern.

Table A.3: Summary Statistics of DD Treatment and Comparison Groups 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		
	Comparison 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 comparison groups 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: Computations 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

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

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: Computations 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 smoothness condition. 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 the smoothness of 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.

[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 4.3 for more detail on sample restrictions.

Source: Computations based on administrative tax data from the canton of Bern.

D.1 Density Test

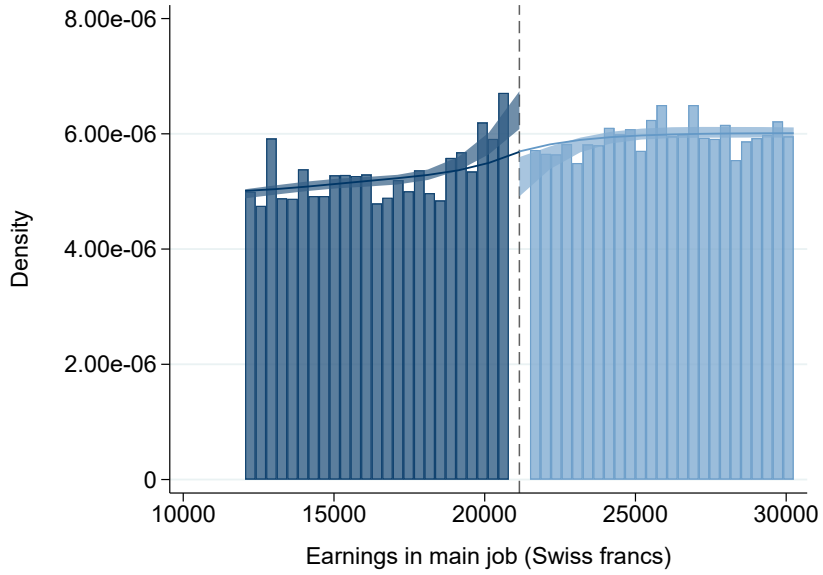
I test for a discontinuity in the density of earnings at the mandate threshold following the idea of manipulation testing introduced by McCrary (2008). Figure D.1 shows the frequency distribution of annual earnings in the main job within CHF 9,000 of the cutoff in 2017. There is some missing mass around 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.

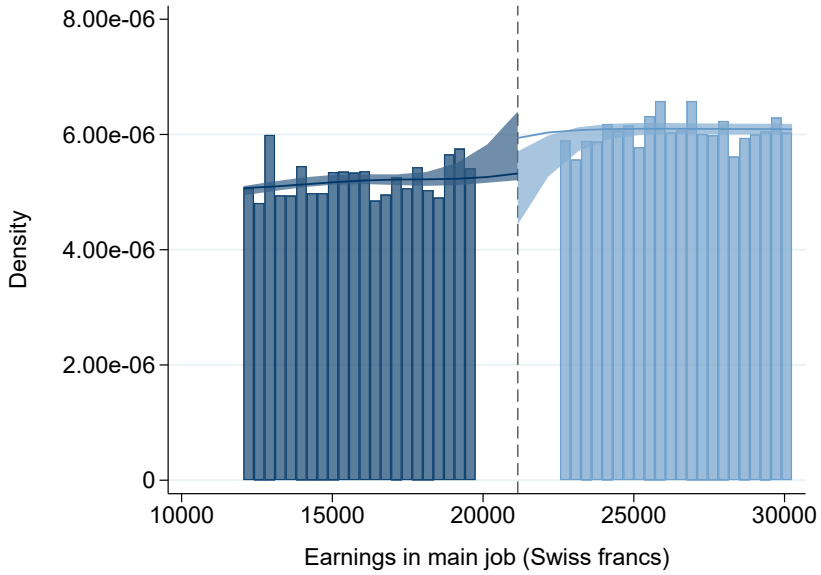
I investigate the possibility of earnings manipulation in numerous ways, concluding that it is unlikely to bias the RD results. *First*, the excess mass is modest com-

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 4.3 for more detail on sample restrictions.

Source: Computations based on administrative tax data from the canton of Bern.

pared 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 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 5.4. 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 comparison groups, 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.

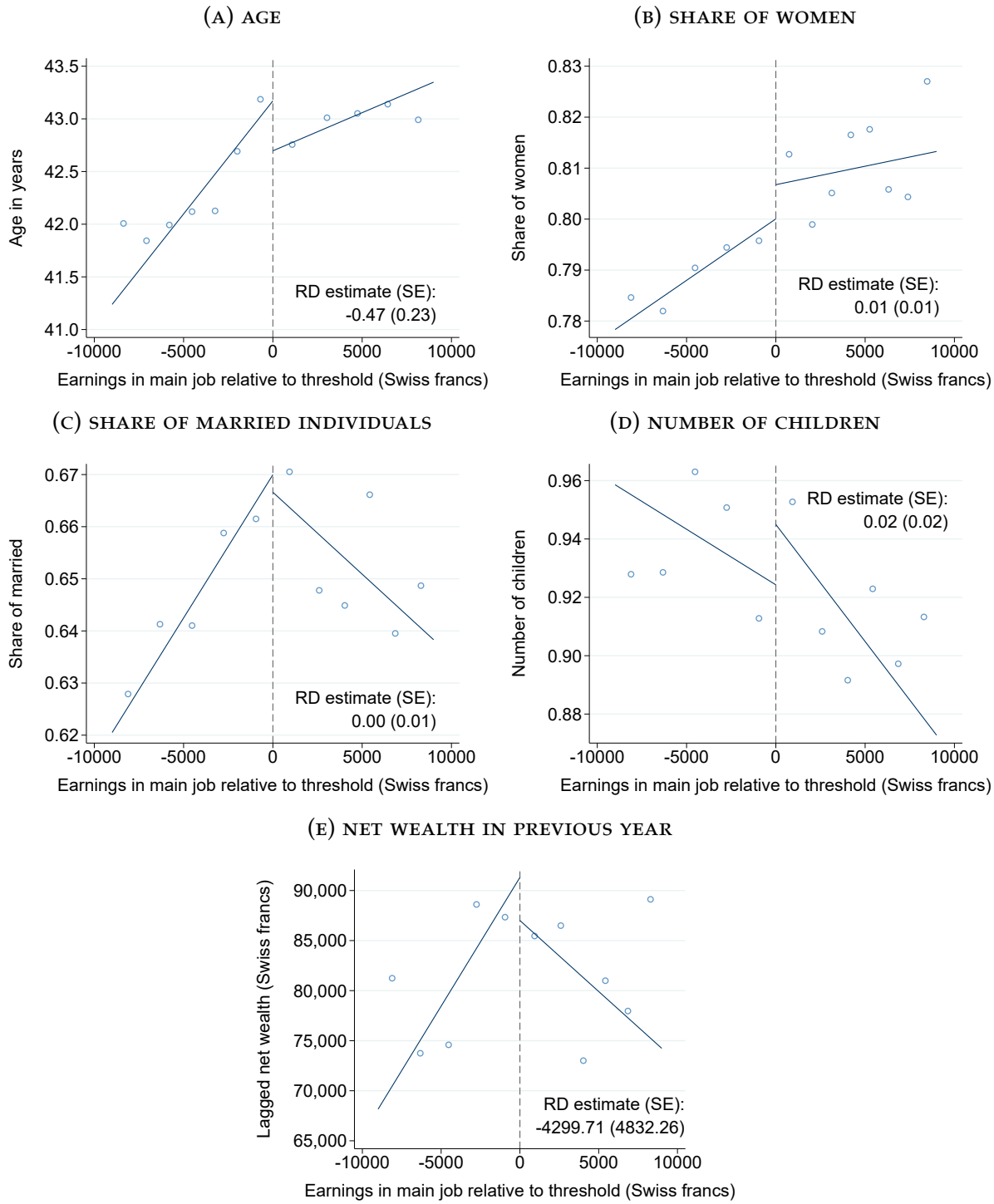
D.2 Continuity of Predetermined Covariates

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,

[21] Fagereng et al. (2021) find that net savings rates (excluding capital gains) are flat across most of

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. 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 4.3 for more detail on sample restrictions and estimation.

Source: Computations based on administrative tax data from the canton of Bern.

in order for net wealth to be a *predetermined* covariate not affected by the treatment, I take the lag by one year.

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 4.6.

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.

the wealth distribution, while gross savings rates (including capital gains) increase considerably with wealth.

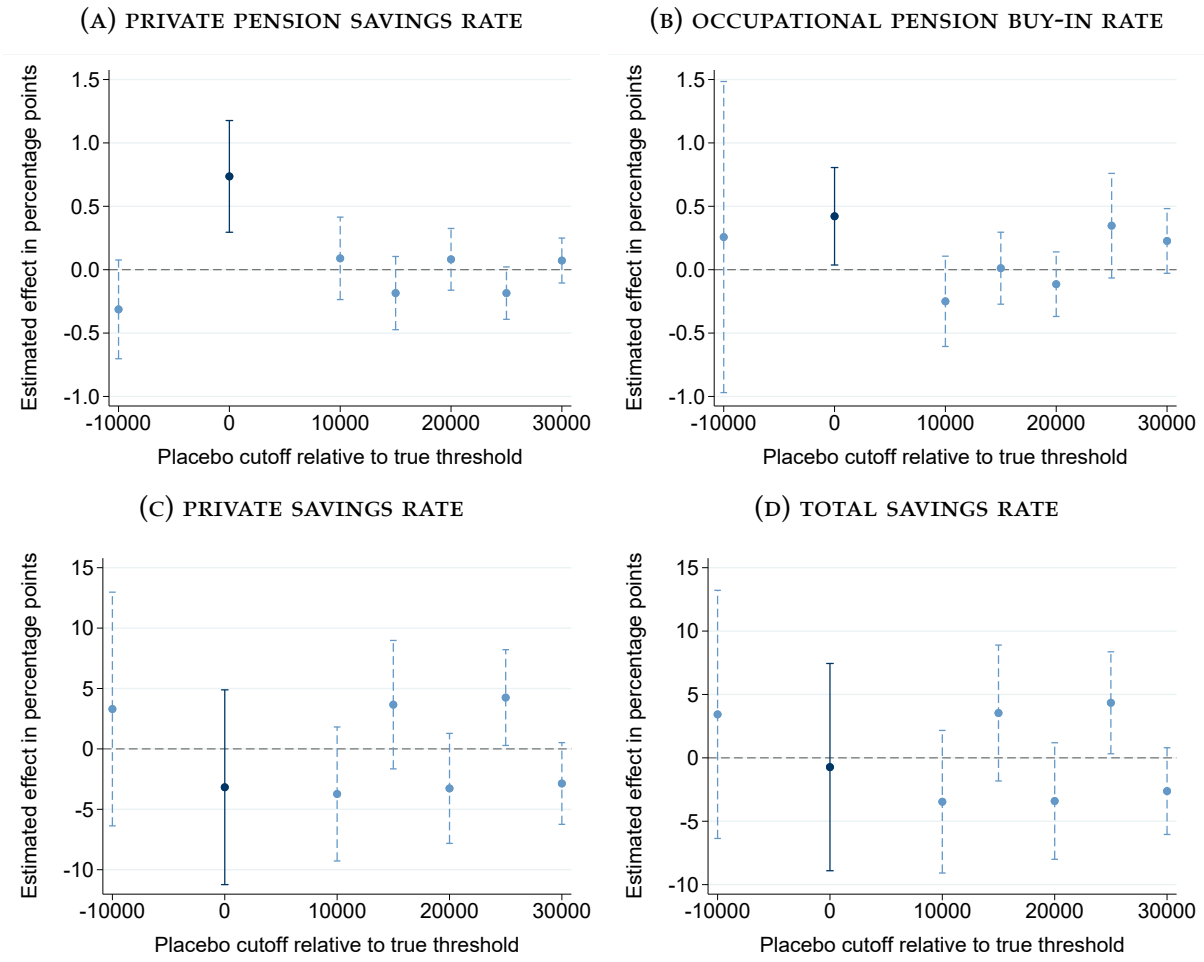
[22] 59% of women and 18% of men report to be working part-time in Switzerland in 2020, see the website of the Federal Statistical Office: https://www.bfs.admin.ch/bfs/en/home/statistics/work-income/employment-working-hours/labour-force-characteristics/full-time-part-time.html [accessed on 24 October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

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, 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 4.3 for more detail on sample restrictions and estimation.

Source: Computations 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 weighting 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.

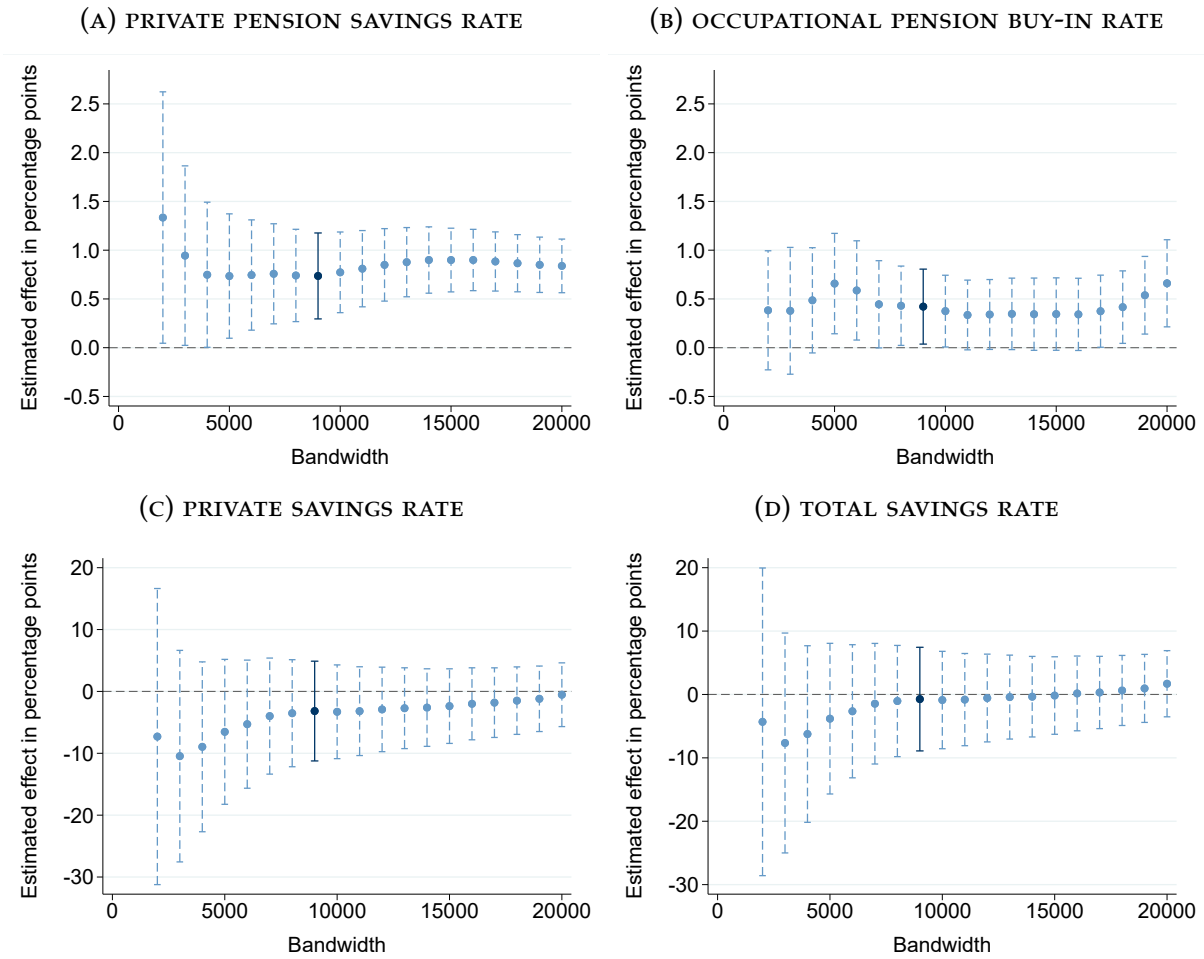
E.2 Kernel Weighting Function

Figure E.2 plots effect estimates using various weighting functions. 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

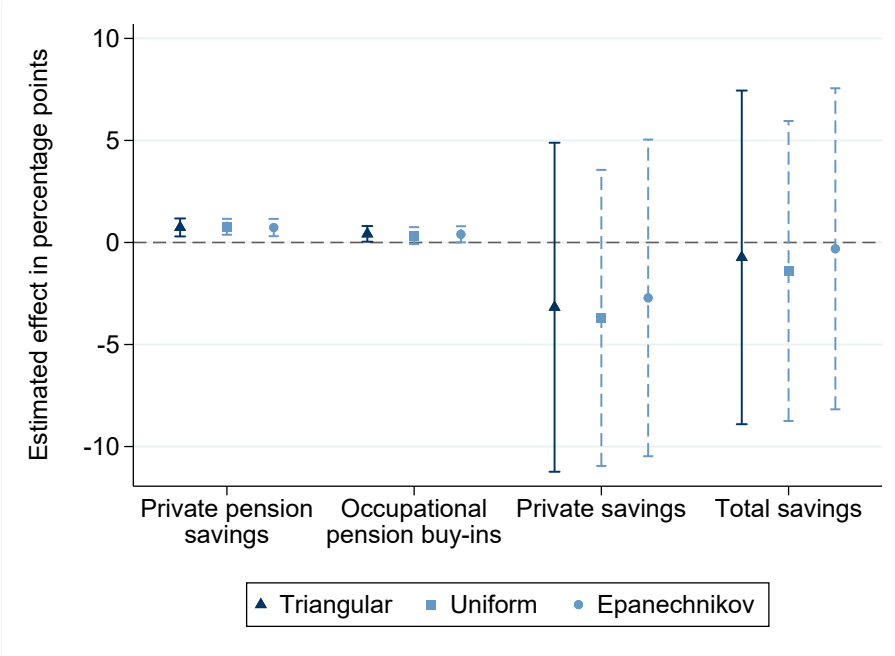
Figure E.1: Sensitivity of RD Estimates to Bandwidth Choice



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 4.3 for more detail on sample restrictions and estimation.

Source: Computations based on administrative tax data from the canton of Bern.

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

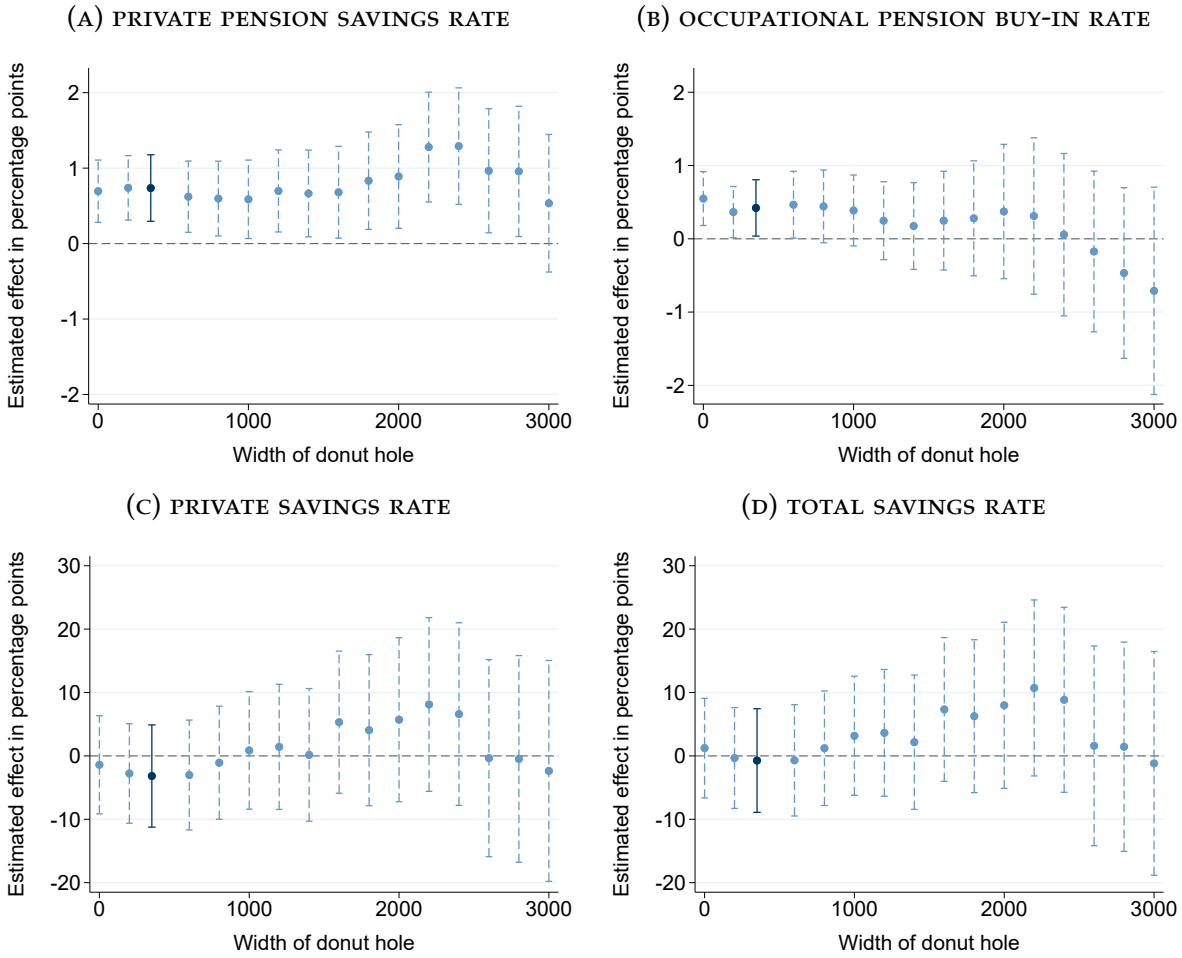


Notes: The figure displays regression discontinuity estimates and 95% confidence intervals for the effect of the occupational pension savings mandate on a set of savings rates in 2017, depending on the weighting function used. The dark marker represents the main result using a triangular kernel; the 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 4.3 for more detail on sample restrictions and estimation.

Source: Computations 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 4.3 for more detail on sample restrictions and estimation.

Source: Computations 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

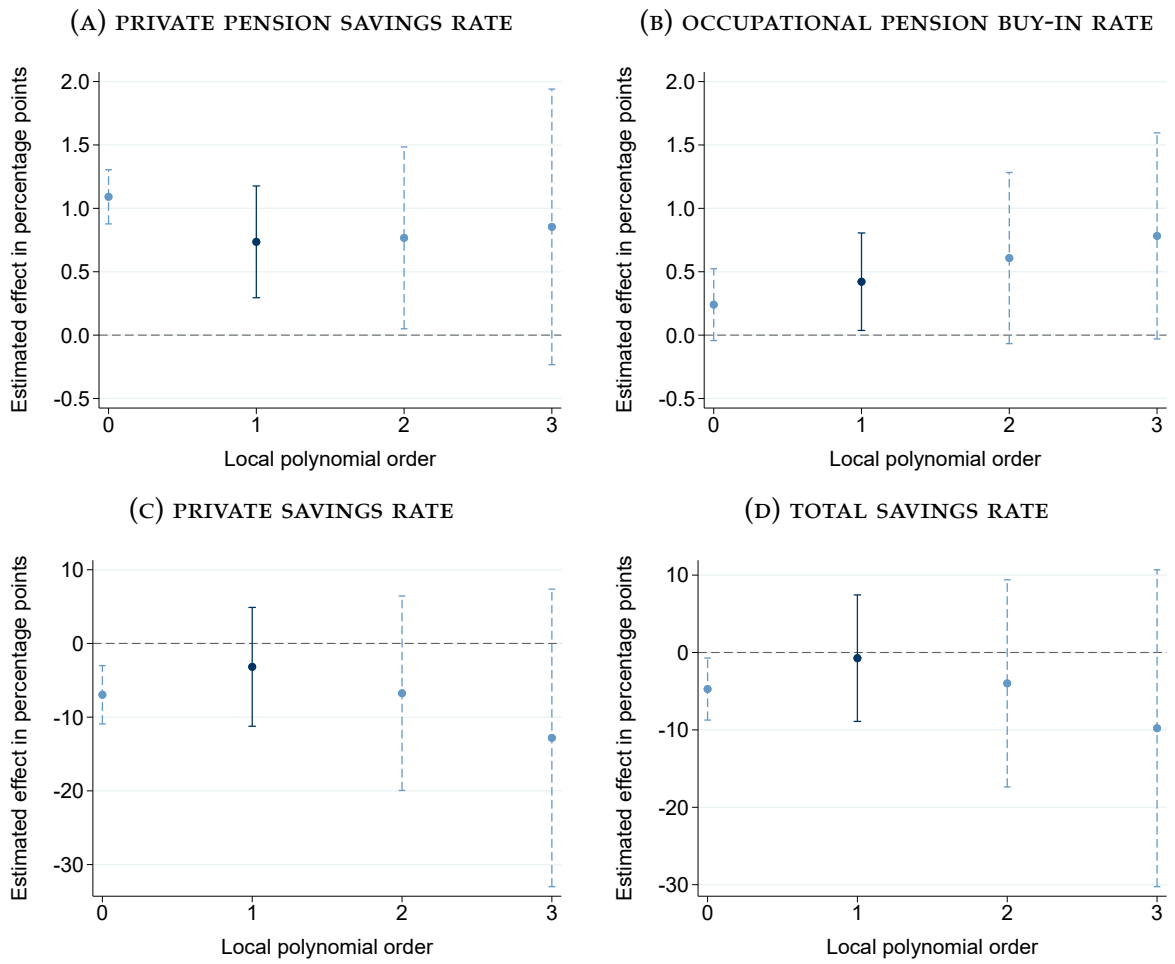
Under standard RD assumptions, the inclusion or exclusion of control variables should not affect the point estimates. If the effect sizes vary substantially depending on what controls are included, this could indicate a violation of the identifying assumptions, for example due to endogenous sorting around 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.

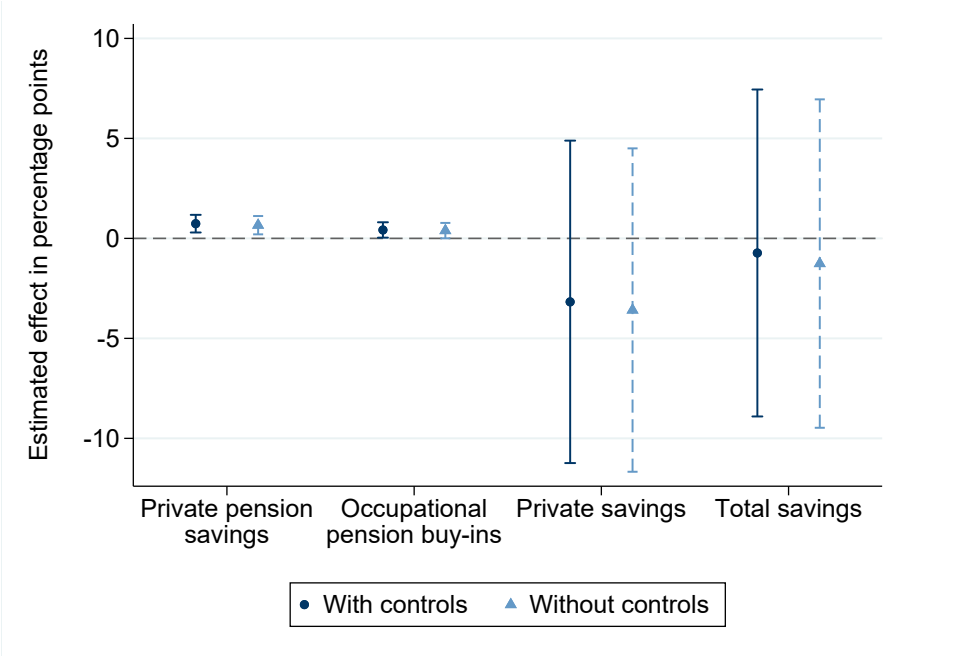
Figure E.4: Sensitivity of RD Estimates to Order of Local Polynomial



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 4.3 for more detail on sample restrictions and estimation.

Source: Computations based on administrative tax data from the canton of Bern.

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



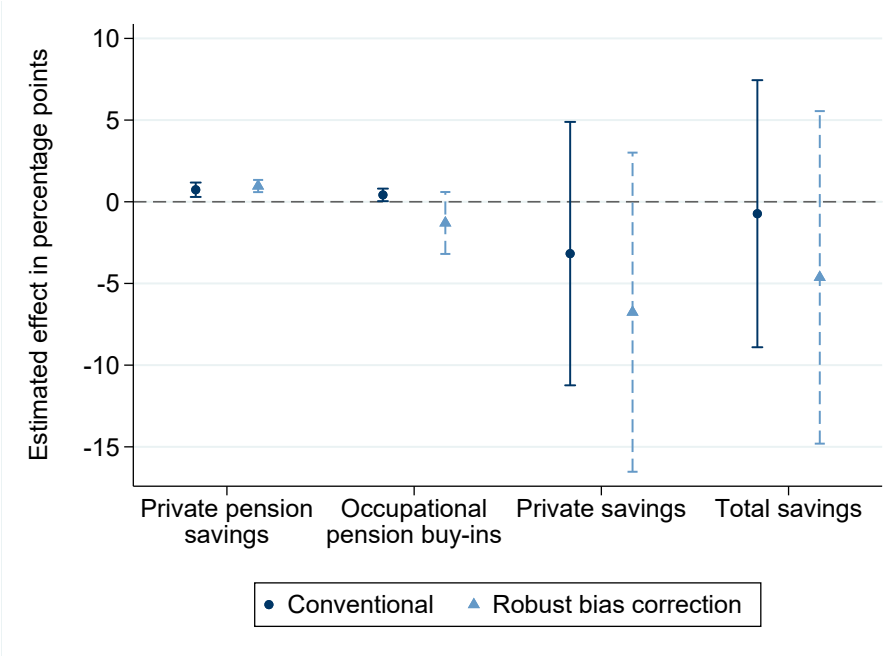
Notes: The figure displays regression discontinuity estimates and 95% confidence intervals for the effect of the occupational pension savings mandate on a set of savings rates in 2017, depending on the inclusion of control variables. The dark markers represent the main results estimated with controls; 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 4.3 for more detail on sample restrictions and estimation.

Source: Computations 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 4.3 for more detail on sample restrictions and estimation.

Source: Computations 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 4.3 for more detail on sample restrictions and estimation.

Source: Computations based on administrative tax data from the canton of Bern.

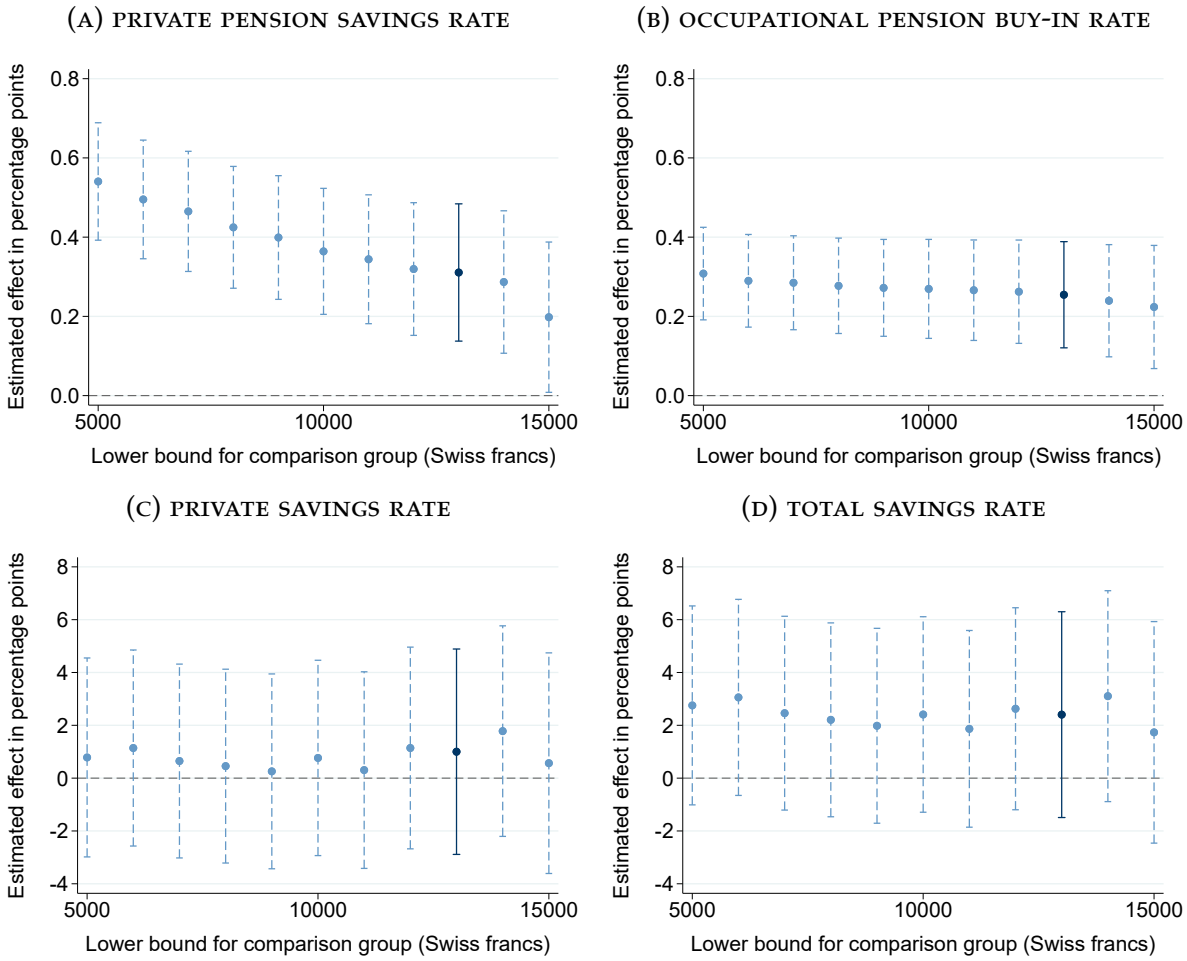
F Robustness of the DD Results

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

First, I vary the size of the comparison group. In my main specification, I choose the comparison group to cover a similar earnings range as the treatment group which results in a lower bound for inclusion in the comparison group of CHF 13,000. Thus, the bandwidth around the 2005 mandate cutoff for inclusion in the treatment or comparison group is approximately CHF 6,000. Figure F.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 comparison group. The results look very similar irrespective of the size of the comparison group, both in terms of effect size and statistical significance. The estimated effect on private pension savings decreases somewhat when a smaller comparison group is used, although it remains significant at the 5% level. This decline is driven by the fact that using a comparison 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 comparison 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 comparison groups. Figure F.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 comparison 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.

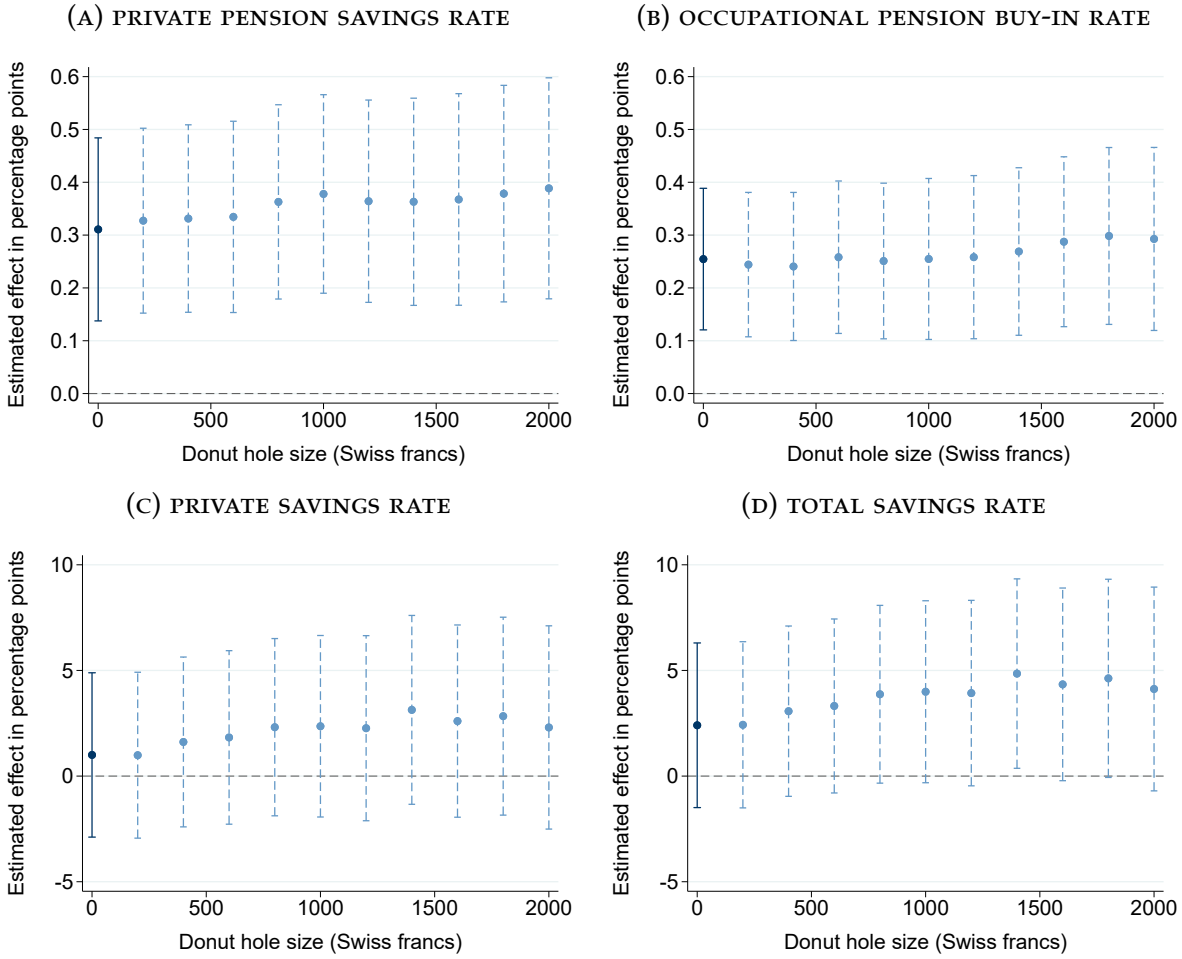
Figure F.1: Sensitivity of DD Estimates to Size of Comparison Group



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 comparison 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 comparison group definition using data from 2002 to 2010, exploiting the 2005 reform that expanded mandate coverage. Treatment and comparison groups are defined according to Equation (3) and lower bounds for the comparison 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: Computations based on administrative tax data from the canton of Bern.

Figure F.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 comparison groups 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: Computations based on administrative tax data from the canton of Bern.