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Does Mandatory Saving Crowd out Voluntary Saving? Evidence from a Pension Reform*

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Abstract

Recently, mandatory pension contributions in Iceland were increased substantially in the private sector while remaining unchanged in the public sector. Taking this as a large natural experiment, this paper studies the effects of this change on households' voluntary saving using comprehensive third-party reported information on tax-payers' income, assets and debt for all taxpayers. Using difference-in-differences, we find that households do not reduce voluntary saving when faced with a rise in mandatory saving. Our results are confirmed by an event study of workers switching from the private sector to the public sector. Survey evidence suggests widespread ignorance about the pension system.

Keywords: Pension reform, occupational pensions, saving, retirement

JEL Codes: E21, E24

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1 Introduction

Almost one hundred years have passed since Frank Ramsey published his seminal paper on optimal saving behavior (Ramsey, 1928). Ramsey's perfectly rational generations had no problem maximizing their own and future generations' lifetime utility through optimal saving. They possessed what Phelps and Pollak (1968) called *perfect altruism* in that their preference for their own consumption relative to the next generation's consumption was no different from their own preference for any future generations' consumption relative to the succeeding generation. In the world of Ramsey, individuals did not suffer from what has become known as *present bias*, overvaluing immediate rewards either by putting less weight on the next generation's utility compared to one's own or by not saving adequately to fund retirement.[1]

The lack of pension savings is a widespread phenomenon and, as large cohorts reach retirement age, a significant share of households do not possess adequate savings (Poterba, 2014). Furthermore, almost 20% of people over age 64 in the EU are at risk of poverty or social exclusion, and in some countries the share reaches 50% (European Commission, 2021). The process of population ageing is widely expected to intensify further in coming decades, putting public finances under greater pressure. Therefore, to increase households' retirement savings, governments have sought to reform pension systems.

In the Nordic countries of Denmark, Iceland and Sweden mandatory occupational defined-contribution pension schemes were intended to address the problem of under-saving, both within generations and between generations. In Denmark, higher household saving contributed to the emergence of persistent current account surpluses following pension reforms in 1987. These involved a funded occupational pension system agreed upon by the social partners (Hougaard Jensen et al., 2019). Pension reform in Sweden in 1993 also contributed to higher household saving in that country. In Iceland, occupational, defined-contribution pension funds were set up in 1970. Further legislation in 1974 and 1980 made participation mandatory for both wage-earners and the self-employed. However, while household saving increased significantly with the establishment of the occupational pension funds, the funds did not become viable sources of income in old age until financial repression ceased in the 1980s and the real rate of return became positive.

Key elements of such reforms have included introducing longevity adjustment of retirement age or increasing the reliance on defined contribution (DC) pension plans. As with other reforms where the costs appear before the gains, raising the retirement age has been widely

[1] See Laibson (1997) on how one particular cause of present bias, hyperbolic discounting, induces dynamically inconsistent preferences, which reduce saving when assets are liquid.

met with resistance and some countries have later reversed their initial reforms (OECD, 2019). However, DC pension plans have increasingly become an integral part of pension systems in many countries. Therefore, understanding the implications of higher mandatory pension contributions for households' aggregate saving is crucial to gauging their success in raising total saving and thereby relieving pressure on tax-financed social security systems.

It is theoretically possible that mandatory occupational pension savings may crowd out voluntary saving, raising total household saving only slightly if at all. This paper seeks to contribute to the literature on the effects of mandatory saving on voluntary saving along several dimensions. First, we study the effect of a major 2016-2018 reform to the Icelandic pension system, which involved a large exogenous variation to mandatory pension saving for all private-sector workers, leaving public sector workers' mandatory pension saving unchanged. Second, together with a large natural experiment in mandatory saving, we use accurate third-party-reported information from administrative data in Iceland. In fact, we improve on previous research in that our measure of household saving is more accurate, as we have information on mortgage debt, the value of housing and net wealth, and we correct for various sources of capital gains.[2] Third, we gauge the substitution effect between mandatory and voluntary saving within the pension system and test whether the reform led to debt expansion. Fourth, we check the robustness of our results by analyzing the saving behavior of households whose mandatory saving rates change as they switch jobs from the private sector, which has a relatively low mandatory saving rate, to the public sector, which has a relatively high mandatory saving rate. Fifth, we extend our analysis beyond the administrative dataset and conduct a survey to explore not only how individuals responded to the reform but also why they responded as they did.

Our findings suggest that raising mandatory saving led to higher total household saving, as households did not respond by cutting voluntary saving. As the reform took place in the fully funded occupational pension system rather than in the pay-as-you-go (PAYG) social security system, the rise in household saving translates into a higher saving rate for the economy as a whole. Moreover, our survey results do not indicate that workers are rational and forward-looking in their saving behavior. Instead, the results are consistent with descriptions of consumer behavior characterized by present bias, such as hyperbolic discounting, implying lack of self-control or by a lack of knowledge of pension savings. Thus, the survey results highlight a general lack of awareness of pension affairs. They also point to a potential role for saving motives in explaining households' responses to the pension reform, as a small share of households who report retirement saving as a main saving motive adjust

[2]Natural experiments from Iceland have already attracted attention in the literature. An example is Bianchi et al. (2001) who gauged the labor supply response to the tax-free year in Iceland in 1987.

their voluntary saving due to the reform.

The rest of the paper is organized as follows. Section 2 reviews the literature on the effects of mandatory saving on households' voluntary saving and highlights the key contributions of the paper. Section 3 discusses the institutional setting of pension saving in Iceland, including the 2016-2018 reform on which the paper focuses. We discuss the data and definition of key variables in Section 4 and outline the empirical specification and results in Section 5. In Section 6, we analyze households' saving responses to job switches from the private to the public sector and in Section 7 we discuss the findings from a survey, generating insights into the results found in previous sections. Finally, we discuss the results and conclude the paper in Section 8.

2 Literature

From a theoretical point of view, raising mandatory contributions would lower voluntary savings. In fact, in the standard life-cycle (or permanent income) model, households will completely offset the effects of higher mandatory saving by reducing voluntary saving, thereby leaving aggregate saving unchanged.[3] However, this approach ignores several aspects of reality that might break the perfect substitutability between mandatory and voluntary savings. First, mandatory savings, which tend to be mandatory pension savings, are illiquid and cannot in general be used as collateral, making such funds an ineffective buffer against unexpected shocks; second, a certain share of households is likely liquidity-constrained and unable to reduce voluntary saving; third, the average return on mandatory saving might differ from that on voluntary saving – owing, for example, to the relatively long investment horizon of pension funds and expertise in making financial investments; fourth, financial illiteracy or inattention to one's own pension affairs might reduce the substitutability between mandatory savings and voluntary savings; and fifth, households may have hyperbolic preferences. Overall, the extent to which mandatory pension schemes crowd out households' voluntary saving is inherently an empirical question.

This issue has already attracted attention in a growing literature. For example, Cagan (1965) in an early study of the effect of pension wealth on saving studied employer-linked pension schemes and found that pension plans increase aggregate personal saving by more than the sum of the contribution of the employer and the employee. Thus, there was an indication that pension saving increases other voluntary saving. In contrast, in an early contribution, Feldstein (1974) and Feldstein (2199) used an extended life-cycle model to

[3]Mandatory pension saving is the only form of mandatory saving in Iceland. Therefore, for simplification we use the term *mandatory saving* to denote *mandatory pension saving*.

show how a PAYG pension scheme can reduce voluntary saving. Using aggregate time series data on social security wealth in the US, Feldstein found that saving was approximately half of what it would have been in the absence of the social security program. Gale (1998) used data from the 1983 Survey of Consumer Finances, a cross-section of 3,824 US households and a supplemental survey of 438 high-income households and found a crowding-out effect of 39-82%. Notably, Gale found a large crowding-out effect for private pensions compared to that for social security, and these effects were found to be larger for highly educated households than for those with less education. More recently, Attanasio and Rohwedder (2003) used time series of cross-sectional household surveys comprising roughly 7,000 households per year and spanning from 1974 to 1987 together with pension reforms in the UK as natural experiments and found a crowding-out effect that varies with age and reaches a maximum of 65-75% for 43 to64-year-olds. Engelhardt and Kumar (2011) employ an instrumental-variable identification using one wave of the Health and Retirement Study in the US on pensions and lifetime earnings for older workers in 1992 to find a crowding-out effect of 53-67%. Furthermore, they find that most of the effect is concentrated in the upper quantiles of the wealth distribution. Other studies find a much lower crowding-out effect. For example, using an administrative data set and a quasi-natural experiment based on the differential impact of the global financial crisis on various pension funds in the Netherlands, Li et al. (2016) find a crowding-out effect of 33% in their favorite specification.

We are not the first to study the effect of pension systems on household saving in the Nordic countries. However, some of these study the effectiveness of tax incentives in raising savings for retirement instead of the effect of changes in expected pension income upon retirement. We start with those measuring the effect of changes in pension wealth on voluntary saving. Arnberg and Barslund (2014) study the effect of the introduction of a mandatory defined-contribution pension system in Denmark in the 1990s using administrative tax records. Their econometric identification is based on differences in the timing of implementation across different occupational groups. The results suggest that mandatory pension contributions have a crowding out effect much smaller than full crowding out, which implies that mandatory pensions add to national private savings. The crowding-out effect of mandatory pensions on private savings is always less than 30 percent in their estimates, smaller for the younger workers and larger for the older workers. The treatment of the value of housing is a weakness in the Danish administrative data, the official assessment trailing actual buying and selling prices by roughly two years. Therefore, Arnberg and Barslund (2014) confine their analysis to individuals who are renters, do not own real estate and are not living with other individuals who own real estate. Another paper studying the effect of pension wealth on private savings in Denmark is that of Chetty et al. (2014). They also use administrative

data but utilise the variation that comes from individuals changing jobs between occupations that differ in employers' pension contributions and expected pension income. The results show that only around 15% of individuals respond to changes in the contribution rates, the rest being heavily influenced by automatic pension contributions made on their behalf. On average, the individuals who respond are to be more financially sophisticated individuals who plan for retirement. The authors also study the impact of a government mandated Mandatory Savings Plan that required Danish citizens above a certain income threshold to contribute 1 percent of their earning to a retirement saving account starting in 1998. The mandatory savings plan raised total saving by an average of almost 1 percent of earnings. Overall, at least 85 percent of individuals responded passively to changes in automatic contributions. In contrast, subsidies for retirement savings are found to have only a limited effect on the volume of savings. Thus policies that rely on action taken by individuals to increase saving have a small impact on total saving, in contrast to policies that increase it automatically without requiring any individual action.

Two other recent papers study the effect of changes in the Danish tax system on retirement savings. Andersen (2018) analyses the effect of changes in tax credits on pension contributions for high-income earners on debt repayments and non-retirement savings. Taking advantage of an unanticipated reduction in tax credits on pension savings in 2010, he finds that individuals tend to make extraordinary repayments on their debt when saving in retirement accounts becomes less attractive. The findings show that for each DDK 1 reduction in private pension contributions, resulting from a reduced tax subsidy for annuity pensions, 19 øre were used for increased repayment of mortgage debt and 61 øre were saved in taxable accounts. Christensen and Ellegaard (2023) study the effect of a 2018 change that reduced the tax subsidy for retirement saving for those contributing to a public pension scheme, called the "Age Pension Scheme" (*Aldersopsparing* in Danish). The change primarily affected individuals in the middle of the income distribution. This age pension scheme is popular and provides the largest tax benefits for middle-income workers. The reform consisted of a reduction in the annual contribution limit, above which contributions are subject to a tax penalty. The authors define treatment and control groups based on contributions prior to the reform and then compare changes in private saving between the two groups. The reduced tax subsidies are found to cause individuals to lower their pension contributions, total retirement saving and total saving. The authors find only 20 percent substitution within the pension system and a total crowding out of 64 percent. The finding of less than full crowding out implies that the lower contribution limit led to a reduction in total saving for the workers.

Our analysis has several strengths compared to these previous studies using Danish data.

First, there is the design of the natural experiment: reform affecting all private sector workers (including the self-employed) leaving public sector workers as a control group. Second, there is the size of the change affecting the private sector workers who experienced a 29% increase in their mandatory pension contribution (from 12% to 15.5%). Third, the administrative data we used to calculate private savings are better than the Danish data in that the value of housing is updated annually, allowing us to include individuals who live in their own housing. Fourth, we complement the analysis by testing whether the saving behavior of those who switched from from the private sector to the public sector before the reform – when employers' contribution was lower in the private sector – changed. Finally, we conducted a survey to explore why people responded to the reform in the way they did.

In other northern European countries, recent papers on the savings effect of changes in pension wealth are those of Lindeboom and Montizaan (2020) and Lachowska and Myck (2018). Lindeboom and Montizaan (2020) study the effect on older workers of a 2006 pension reform in the Netherlands that reduced the public pension wealth of workers born in 1950 or later using linked administrative and survey data. The results show that the reform increased the labor supply for the lower income workers, who postponed retirement, while higher income workers increased their private savings to fully counter the impact of the drop in public pension wealth. Lachowska and Myck (2018) study a similar episode in Poland in 1999 using survey data. They compare household saving and expenditure across time and between cohorts affected and unaffected by the reform. An identification of the effect of pension wealth on private saving shows limited crowding out in household saving with a larger effect found for middle-aged cohorts – especially the highly-educated – and a smaller effect found for the younger cohorts.

From this brief review, it is clear that the empirical literature is inconclusive. Indeed, the estimates of the crowding-out effect span the spectrum from 0 (no effect) to 1 (full crowding-out). Two factors likely play a role in explaining the wide range of estimates. First, estimating the crowding-out effect of mandatory saving on voluntary saving at the household level ideally requires a long and representative panel of households containing comprehensive and accurate information on consumption, income, net wealth, and pension saving, along with various covariates. Such information is hard to establish. As a result, the literature has largely relied on surveys of consumption and savings, which tend to have a number of shortcomings (Browning et al., 2014). Also, relying on such information to study pension savings might be particularly problematic, as individuals, especially young people, do not have accurate knowledge of their future pension entitlements (Amilon, 2008; Lusardi and Mitchelli, 2007). Second, pension saving does not exhibit much exogenous variation, especially within periods covered by micro data sets. Hence, differences in pension saving

across individuals might reflect differences in preferences for saving (Attanasio and Rohwedder, 2003). To identify the effects of changes in mandatory pension saving on household wealth, Chetty et al. (2014) use changes in workers' saving rates when they switch jobs, as mandatory pension saving rates in Denmark differ across firms and sectors. However, job switches could potentially be endogenous in this setting, as job switches might be driven by preferences for saving. Our study overcomes both weaknesses by using reliable administrative data that allows us to calculate saving rates for each taxpayer in Iceland and then explore the effect of a large exogenous increase in mandatory saving.

3 Institutional setting

3.1 A brief look at the Icelandic pension system

The Icelandic pension system follows a 3-pillar model for the provision of income during retirement: First, a tax-financed pillar with means-tested pension entitlements; second, a fully funded pillar, based on occupational pension schemes, with mandatory contributions made by employees and employers, and with risk against accidents, illnesses, disability, and longevity shared among fund members; third, a fully private pillar, with individual, flexible, and voluntary saving accounts provided by pension funds, banks, and insurance companies. These are fully inheritable but without risk-sharing among fund members.

The foundations of the Icelandic pension system can be traced back to collective bargaining agreements in the private sector in 1969, which resulted in the establishment of fully funded occupational pension funds. Membership in an occupational pension fund is compulsory for both wage earners and self-employed workers. Reforms to the pension system have typically been negotiated in collective bargaining agreements, which later tend to provide the basis for legislative amendments to the system.

As of 2019, the effective mandatory contribution rate to pension funds amounts to 15.5% for the vast majority of workers (Table 1), albeit with a legislative minimum contribution rate of 12%.

Table 1: A stylized overview of the contribution rates to pension funds and pension accounts in the Icelandic pension system

	Employee contribution	Employer contribution	**Total contribution**
2nd pillar	4%	11.5%	**15.5%**
3rd pillar	4%	2%	**6%**
Total	**8%**	**13.5%**	**21.5%**

Notes: Table 1 shows mandatory contribution rates out of pre-tax labor income to occupational pension funds (2nd pillar) and the maximum tax-deductible contribution rate out of pre-tax labor income to private voluntary pension saving accounts (3rd pillar). The first pillar of the Icelandic pension system consists of means-tested tax-financed public pensions.

Employees are allowed to deduct from their taxable income a third-pillar pension contribution of up to 4.0%, which employers are obliged to match up to 2.0%. Thus, the combined second- and third-pillar pension contributions frequently total up to 21.5% of taxable income. Due to the long history of high mandatory pension saving in fully funded occupational pension funds, the Icelandic pension system is large in international comparison. Total assets in retirement savings plans surpass 200% of GDP and are higher only in Denmark among OECD countries (OECD, 2023).

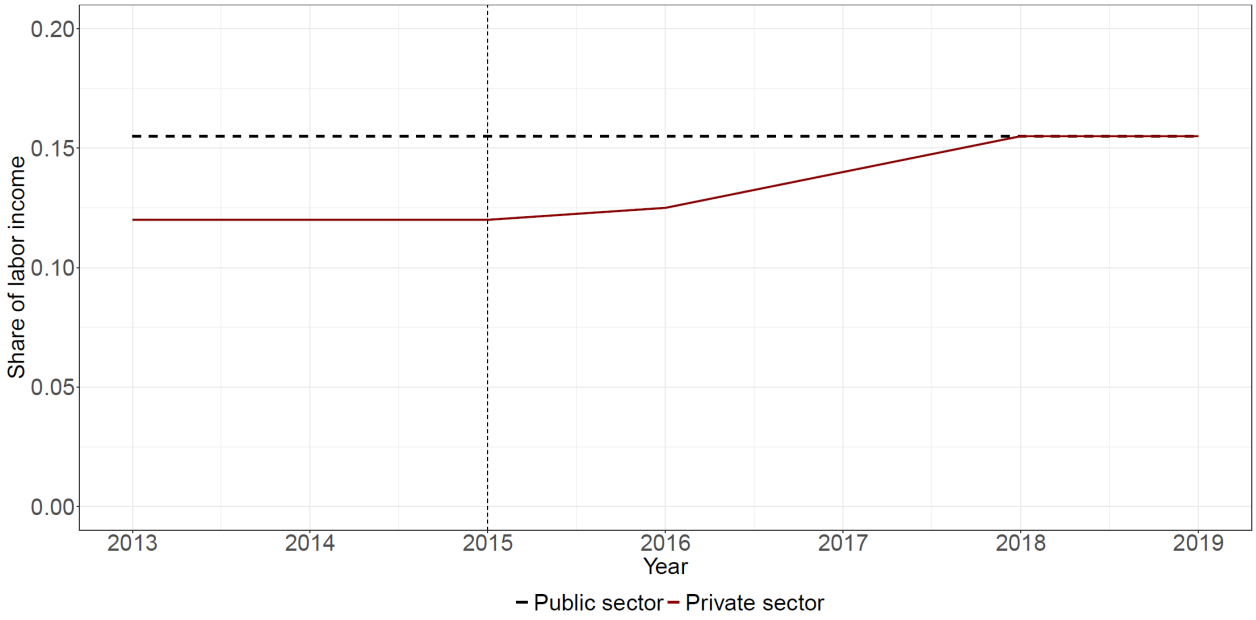
3.2 The 2016-2018 reform

At the turn of the century, the mandatory contribution rate in the private sector labor market was roughly half that of the public sector. Since then, the mandatory contribution rate has been raised in two steps – in 2006-2007 and 2016-2018 – through collective bargaining agreements, while that for public sector employers has remained constant. The objective of these changes was to equalize pension benefits between the private sector and the public sector. Historically wages were lower in the public sector while pension rights were more generous. The changes were intended to equalize both wages and pensions between the public and the private sectors so that wages would go up in the public sector relative to the private sector and contribution rates would increase in the private sector and become equal to those in the public sector.

We focus solely on the 2016-2018 reform for two reasons. First, the time surrounding the 2006-2007 pension reform, when the private employers' contribution rate was raised by 2pp, is characterized by remarkable economic turbulence and turmoil in Iceland due to the financial bubble that preceded the collapse of the country's banking system in 2008, severely complicating plausible identification of the effect of the reform. Second, the quality of our data used for computing consumption and saving improved after 2010.

The 2016-2018 reform, which raised private employers' contribution rate by 44% (from 8.0% to 11.5%) was implemented in three stages; the contribution rate was raised by 0.5pp in mid-2016, by 1.5pp in mid-2017, and by 1.5pp in mid-2018. Therefore, the reform raised the private market's total mandatory pension contribution rate from 12.0% to 15.5%, as is seen in Figure 1

Figure 1: The mandatory pension contribution rate out of labor income.



Notes: Figure 1 shows mandatory contribution rates to occupational pension funds out of pre-tax labor income in 2013-2019 for the public sector labor market (black horizontal dashed line) and the private sector labor market (solid red line). We study the effects of the increase in private sector employers' mandatory contribution rate to occupational pension funds in 2016-2018. The last year before the reform is implemented is marked with a black vertical dashed line.

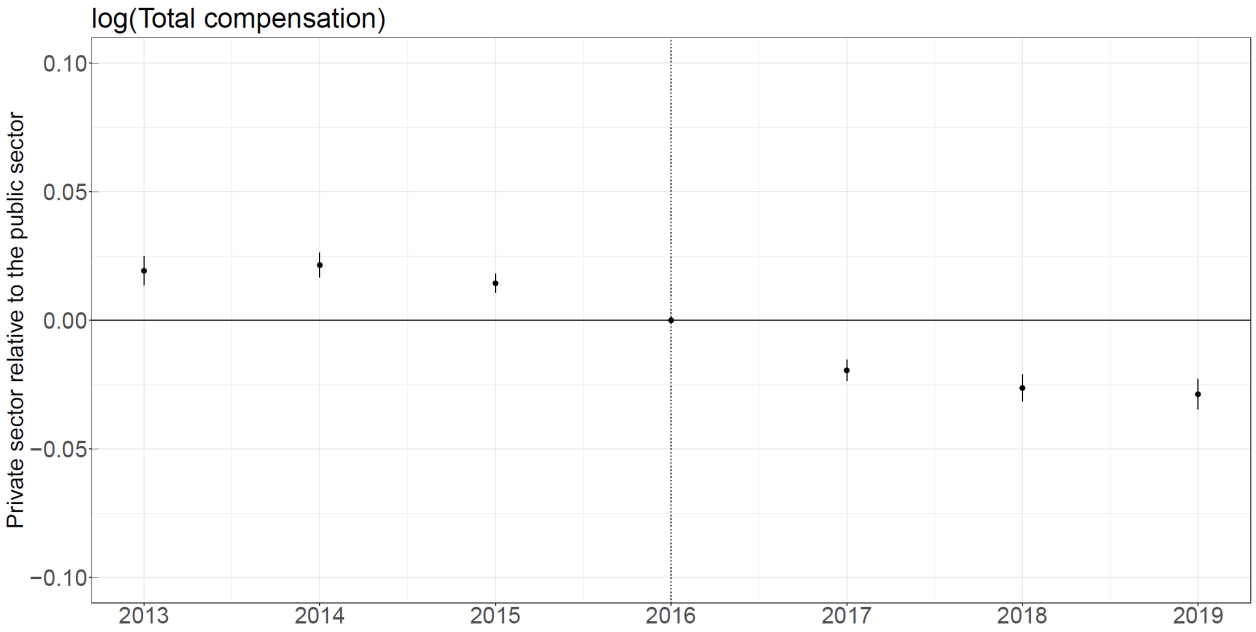
In the ideal experiment, the increase in the mandatory saving rate would be randomly assigned to a subgroup of the population, and not tied to the wage negotiations of a particular sector of the labor market. One concern which, potentially, could contaminate our experiment is that the increase in the employer's mandatory contribution rates constitutes a pay raise for private sector workers relative to public sector workers whose mandatory saving rate was unchanged. Since higher income households tend to save a larger share of their income than lower income households (Dynan et al., 2004), this relative wage increase could push up the saving rate of private sector households. This could lead to an underestimation of the crowding-out effect, which measures the extent to which households responded to the increase in the mandatory saving rate by reducing their voluntary saving rate.

In this regard, Figure 2 shows the average change in firms' total compensation in the

private sector over and above the average change in the same year for the public sector.[4] Total compensation is defined as households' wages plus the employer's mandatory contribution to Pillar 2 pension funds. The figure illustrates that while total compensation grew faster in the private sector before the reform, those roles were reversed during the post-reform period. In particular, there are no indications that total compensation in the private sector rose relative to the public sector in the post-reform period, thereby alleviating concerns of underestimation of the crowding-out effect.

Although the reform provides a rare opportunity to identify the crowding-out effect of mandatory saving on voluntary saving, it does deviate from the ideal experiment described above. Therefore, in Section 6, we complement our analysis of the 2016-2018 pension reform by studying the saving behavior of workers who switch from the low-contribution private sector to the high-contribution public sector.

Figure 2: Change in the total compensation of private sector households relative to public sector households



Notes: Figure 2 plots the estimated γ_t from the equation $tc_{it} = \alpha_g + \alpha_t + \gamma_t \alpha_g \times \alpha_t + x_{i2015}\beta + \varepsilon_{it}$. The dotted vertical line in 2016 shows when the first stage of the reform was implemented. tc_{it} is the log of total compensation, defined as the sum of households' pre-tax wage income and employers' mandatory contributions to Pillar 2 pension funds. α_g denotes group fixed effects, with the treatment and control groups defined as in section 4.3. α_t denotes year fixed effects. The controls are dummies for age (which, despite the notation, is the only characteristic not fixed at the 2015 value), marital status, gender, urban, region of residence, number of children, labor income ventiles, net wealth ventiles, and education. 95% confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines.

[4]Our treatment and control groups, here referred to as private sector households and public sector households, respectively, are defined in section 4.3.

4 Data

4.1 Sources and restrictions

We use a database comprising administrative tax records of all Icelandic taxpayers, aged sixteen and older, from 1981 to 2019. However, because our objective is to analyze households' responses to the 2016-2018 pension reform that raised the mandatory pension contribution out of labor income, we only use information on households of working age, defined as those aged 25-64 in 2013-2019.[5] In order to exclude students and others who have particularly low wages – for example, due to being employed part-time – we restrict our analysis to households active in the labor market and, hence, only include households whose labor income is above 50% of the median in a given year.[6] Furthermore, to reduce noise from extreme observations we only consider households with a saving rate out of income, computed and discussed in Section 4.2, between -1 and 1.

The data include comprehensive third-party reported information on all sources of taxable income except bequests (and, for obvious reasons, informal income not reported on tax returns). As such, they include information on various assets and liabilities, including bank deposits, the value of real estate, assets in mutual funds, mortgage debt, and total debt, along with individuals' and employers' contribution to pension funds. The data are merged with other administrative data and therefore include various socio-demographic information such as age, gender, education, marital status, occupation, and so forth. The data are collected by Statistics Iceland and Iceland Revenue and Customs.

4.2 Accounting identity for consumption and saving

We construct household measures of consumption and saving using the individual tax records by aggregating information across household members using unique household identifiers, thereby ignoring intrahousehold inequality. Each household comprises at most two individuals in the case of jointly taxed couples, as children over age 15 and young adults in the household are taxed individually.

There are two main reasons for computing consumption and saving at the household level rather than the individual level. First, we believe most jointly taxed couples make financial decisions based on their total income and wealth, rather than each household member making independent decisions based solely on their own income and wealth. Second, some variables in the tax records are defined at the household level rather than the individual level, such as

[5] Starting at age 25, we omit a large share of individuals living with parents.

[6] In the appendix, we show that our results are robust to this choice.

jointly taxed couples' total assets and liabilities.[7] Following Eika et al. (2020) and Kolsrud et al. (2020), who rely on a methodology first proposed by Browning and Leth-Petersen (2003), we construct measures of consumption and saving using an accounting identity that a household's consumption plus its change in net wealth equals the sum of the household's disposable income and its capital gains. The basic idea behind measuring consumption using this identity is that income earned is either saved, thus contributing to increasing net wealth, or consumed. However, unrealized capital gains/losses inflate/deflate households' net wealth without constituting income, and thus need to be accounted for specifically,

$$\underbrace{(e_{i,t} - \tau_{i,t})}_{\text{Disposable income}} + \underbrace{\sum_k (p_{k,t} - p_{k,t-1}) A_{i,k,t-1}}_{\text{Unrealized capital gains}} = c_{i,t} + \underbrace{\sum_k (W_{i,k.t} - W_{i,k,t-1})}_{\text{Change in net wealth}} \tag{1}$$

where c_{it} denotes household i' s expenditure in year t and $e_{i,t}$ is total income (the sum of labor and capital income), from which it pays taxes $\tau_{i,t}$. We include imputed rent for owner-occupied housing services in income to make consumption and saving comparable across renters and homeowners.[8] Furthermore, A_{ikt} denotes household i's amount of assets k in year t, with a unit price of $p_{k,t}$ and $W_{i,k,t}$ denotes net wealth invested in asset k, which, like income, is measured without pension assets on the tax records. Equation (1) can be rewritten to measure consumption in terms of disposable income plus capital gains minus the change in wealth over the period,

$$c_{i,t} = \underbrace{(e_{i,t} - \tau_{i,t})}_{\text{Disposable income}} - \underbrace{\sum_k \Delta W_{i,k,t}}_{\text{Change in net wealth}} + \underbrace{\sum_k \Delta p_{k,t} A_{i,k,t-1}}_{\text{Unrealized capital gains}} \tag{2}$$

Finally, our measure of household consumption adds the depreciation of vehicle assets to the consumption computed in from (2). As such, purchases of vehicle assets, the only durables we have information on aside from housing, do not result in consumption in the year of purchase. Instead, consumption of durables is considered to be a flow of services over their lifetime.[9,10]

[7] Although income, assets, and liabilities are made equal across household members, we allow other background information, such as age and education, to vary across household members. Therefore, even though we aggregate information across household members, each household member is treated as one observation.

[8] Imputed rent for each household is computed by dividing each household's value of real estate by the total value of real estate and multiplying the outcome by the aggregate value of imputed rent from national accounts.

[9] As is discussed in Appendix A1, vehicles depreciate by 10% each year according to Icelandic tax laws. This depreciation is viewed as consumption of vehicle assets.

[10] To account fully for the effect of capital gains/losses on household wealth accumulation – and hence

Having derived consumption, one measure of saving of individual i in year t would be the part of disposable income that is not consumed, $s_{i,t}$,

$$s_{i,t} = \underbrace{(e_{i,t} - \tau_{i,t})}_{\text{Disposable income}} - c_{i,t} \tag{3}$$

However, this measure of voluntary saving is incomplete, as income and net wealth are measured without voluntary pension saving, defined as contributions to third-pillar pension funds net of withdrawals, and pension assets. Thus, we arrive at our measure of voluntary saving $vs_{i,t}$ by adding voluntary third-pillar pension saving $ps_{i,t}^{3rd}$ to the measure of saving in equation (3),

$$vs_{i,t} = s_{i,t} + ps_{i,t}^{3rd} \tag{4}$$

Total saving is then the sum of voluntary saving and mandatory saving, $ms_{i,t}$, which is defined as the total mandatory contribution to occupational pension funds.

$$ts_{i,t} = vs_{i,t} + ms_{i,t} \tag{5}$$

Finally, we define three ratios, the voluntary saving ratio ($vs_{i,t}^r$), the mandatory saving ratio ($ms_{i,t}^r$), and the total saving ratio ($ts_{i,t}^r$), which are the ratio of voluntary saving, mandatory saving, and total saving, respectively, to wage income.

4.3 Summary statistics

We focus on the natural experiment of the 2016-2018 pension reform (see section 3.2). The treatment group is identified as households whose total mandatory contribution rate to occupational pension funds was below 13.75% (the mid-point between the 12.0% private sector rate and the 15.5% public sector rate) in 2015. Others are assigned to the control group. We show, in the appendix, that our results are robust to alternative definitions of the treatment and control groups. There, we define the treatment and control groups based on information on the individuals' sector of work. This definition of the groups is potentially problematic as the sector of work does not accurately identify public servants, which we believe information on the mandatory saving rate does. For example, workers in health services or education might be either private sector workers or public servants. Assuming

consumption – we need either information on the price and quantity of each asset on the household's balance sheet or information on asset transactions for all of its assets. Naturally, such data are extremely rare. However, while we do not have such information, the data still allow us to create a comprehensive identity for consumption and saving. This is discussed further in Appendix A1.

workers in those sectors belong to the public sector might lead us to incorrectly assigning private sector workers to the control group. As such, we believe defining the groups based on contribution rates to occupational pension funds results is more appropriate for our main analysis.

Although the vast majority of the private sector labor market is covered by collective wage agreements, there is a small minority of private sector workers, mainly fishermen and self-employed workers, who were not affected by the reform. We omit those observations by dropping households whose contribution rate to occupational pension fund was still below 13.75% in 2018. This, together with other sample restrictions outlined in Section 4.1, leaves us with 520,064 observations: 156,772 (30.1%) in the control group and 363,292 (69.9%) in the treatment group.

Table 2 shows summary statistics for both groups in 2015 (prior to the reform). Most importantly, the mandatory saving rate of the treatment group prior to the reform is significantly lower than that of the control group. On average, the treatment group also has a slightly lower voluntary saving rate, and therefore, the total saving rate of the treatment group is lower than that of the control group. The share of women in the treatment group is substantially smaller compared to the control group, reflecting the gender ratio in the public sector (more women than men), and on average, the treatment group is less likely to hold a university degree. This reflects the larger proportion of women and university-educated workers in the public sector. However, the groups are similar across most characteristics. They are close in age, have similar wages, hold similar amounts of wealth and debt, their voluntary pension saving rates are almost identical, and they have the same number of children.

5 Empirical framework and results

5.1 Empirical framework

The main aim of the empirical analysis is to estimate the extent to which the increase in mandatory saving was offset by households adjusting voluntary saving and thereby reducing the pass-through to total savings. Consider the following data-generating process for voluntary saving of individual i in year t,

$$vs_{it}^{r} = \mu_i + \mu_t + \rho ms_{it}^{r} + \boldsymbol{X}_{it}\boldsymbol{\beta} + \varepsilon_{it} \tag{6}$$

where μ_i is individual fixed effects, to account for individual-specific preferences for saving; μ_t is time fixed effects, to account for the business cycle, which may affect saving behavior;

Table 2: Summary statistics for the treatment and control groups in 2015.

	Control group	Treatment group
Voluntary saving rate	0.055	0.044
	(0.265)	(0.264)
Mandatory saving rate	0.152	0.122
	(0.009)	(0.009)
Total saving rate	0.207	0.166
	(0.266)	(0.264)
Voluntary saving	5,414	4,655
	(25,933)	(28,534)
Mandatory saving	13,166	11,357
	(7,417)	(6,366)
Total saving	18,580	16,012
	(28,091)	(30,053)
Wages	87,240	92,587
	(49,534)	(51,390)
Debt	146,311	147,846
	(131,679)	(206,417)
Net wealth	143,810	151,707
	(264,616)	(335,971)
Voluntary pension saving rate	0.024	0.023
	(0.031)	(0.031)
Age	45.064	43.578
	(11.138)	(11.271)
Gender	0.415	0.537
	(0.493)	(0.499)
Spouse	0.623	0.680
	(0.485)	(0.467)
University	0.494	0.308
	(0.500)	(0.462)
Children	0.875	0.881
	(1.080)	(1.089)
Urban	0.658	0.645
	(0.474)	(0.479)
Number of observations (N)	28,072	64,745

Notes: Table 2 reports sample means and the corresponding standard deviations in parentheses for our treatment and control groups in 2015, the year before the first stage of the reform was implemented. Measures of saving, disposable income, and net wealth are deflated using yearly averages of the Icelandic CPI and converted to USD using the average 2019 exchange rate. Gender equals 1 for males and 0 for females. Spouse is 1 if the individual has a spouse. A household is considered university-educated if a household member holds a university degree. Children denotes the number of children under age 16 in the household. Urban is 1 for those living in urban areas and zero for those living in rural areas.

$ms^r_{i,t}$ is the mandatory saving rate out of labor income; $vs^r_{i,t}$ is the voluntary saving rate out of labor income (see section 4.2); X_{it} is a set of observable controls that affect saving; and ε_{it} is an error term. The parameter of interest is the effect of the mandatory saving rate on the voluntary saving rate, ρ. If $\rho = -1$, then workers fully offset an increase in mandatory saving by reducing voluntary saving, whereas if $\rho = 0$, mandatory saving has no effect on voluntary saving.

Now, if we estimate ρ in the cross-section by regressing voluntary saving on mandatory saving, we can obtain a biased estimator because individual preferences for saving, μ_i, may correlate with the propensity to work in the private sector. In other words, individual preferences for saving may correlate with the mandatory saving rate, $ms^r_{i,t}$. We overcome this selection bias by exploiting the pension reform. More precisely, in a difference-in-differences (DID) framework, we compare the saving of households whose employer's mandatory pension contribution was increased by the reform (private sector households) to those whose mandatory pension contribution remained unchanged (public sector households). In the DID framework, the individual preferences for saving are cancelled out and therefore do not bias the estimation of ρ. This is formally done by estimating the following specification,

$$vs^r_{it} = \alpha_1 post_t + \alpha_2 treated_i + \rho ms^r_{it} + \boldsymbol{X}_{it}\boldsymbol{\beta} + \varepsilon_{it} \tag{7}$$

where $post_t = 1(t > 2015)_t$ is a post-reform dummy and $treated_i$ is a treatment group dummy, which takes the value 1 for individuals whose mandatory contribution rate was below 13.75% in 2015; that is, prior to the reform. Finally, the mandatory saving rate is instrumented with the interaction of the post-reform dummy and the treatment group dummy.

The first stage of the instrumental variable estimation is mandatory saving rates regressed on a post-reform dummy, a treatment group dummy, the interaction of these two, and a vector of characteristics,

$$ms^r_{it} = \mu_{11} post_t + \mu_{12} treated_i + \pi_1 post_t \times treated_i + \boldsymbol{X}_{it}\boldsymbol{\beta} + \varepsilon_{1it} \tag{8}$$

and the second stage is voluntary saving rates regressed on a post-reform dummy, a treatment group dummy, the predicted mandatory saving rates, and a vector of characteristics,

$$vs_{it} = \mu_{21} post_t + \mu_{22} treated_i + \rho \widehat{ms}^r_{it} + \boldsymbol{X}_{it}\boldsymbol{\beta} + \varepsilon_{2it} \tag{9}$$

Therefore, the estimated crowding-out effect is the DID (between private and public sector workers, before and after the pension reform) in voluntary saving divided by the DID in

mandatory saving,

$$\hat{\rho} = \frac{DID_{vs}}{DID_{ms}} \tag{10}$$

where $DID(.)$ is the DID operator.[11,12]

5.2 Graphical evidence

We first present preliminary findings from a simple comparison of average saving of the two groups. Panel (a) of Figure 3 shows the average yearly voluntary pension saving rate of each group in 2013-2019. Voluntary saving of both groups rose as the economy recovered from the financial crisis but declined in 2018-2019 as growth slowed. More importantly, the simple comparison suggests that the saving rate of both groups moved in tandem both before and after the reform, with no visual indication of a structural break after the pension reform.

Panel (b) of Figure 3 shows the average yearly mandatory pension saving rate out of labor income for each group. The contribution rate of the control group is stable over the period, while that of the treatment group evidently rises in the post-reform period. As the simple comparison does not appear to show a clear effect of the reform on households' voluntary saving behavior, we would expect some differences to arise in total saving, which adds the mandatory saving rate to the voluntary saving rate. This is confirmed in panel (c) of Figure 3, which shows the narrowing gap in total saving rates between the two groups as the mandatory saving rate of the treatment group rises.

5.3 Parallel trends analysis

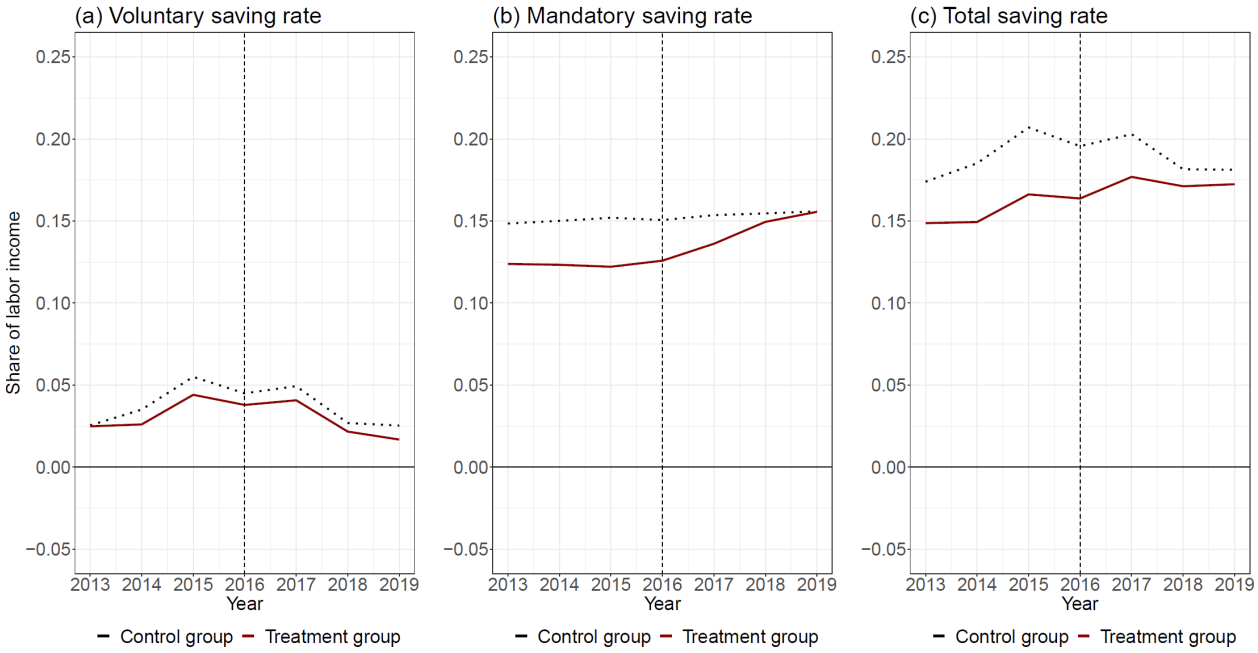
Before estimating the crowding-out effect of the reform, we test whether voluntary saving behavior of the two groups followed a similar trend prior to the reform. We do this to strengthen the case for our identifying assumption that unobservable household characteristics affecting voluntary saving are uncorrelated with the treatment, and therefore that the saving rate of our treatment and control groups would potentially move in tandem in the

[11] That is,

$$DIDq = (E[q_{it} \mid \text{ private}_i = 1, post_t = 1, \boldsymbol{X}_{it}] - E[q_{it} \mid \text{ private}_i = 1, post_t = 0, \boldsymbol{X}_{it}])$$
$$- (E[q_{it} \mid \text{ private}_i = 0, post_t = 1, \boldsymbol{X}_{it}] - E[q_{it} \mid \text{ private}_i = 0, post_t = 0, \boldsymbol{X}_{it}]).$$

[12] Appendix Table A3-1 shows the robustness of our results to including individual fixed effects rather than treatment group dummies.

Figure 3: Average voluntary, mandatory, and total saving rates



Notes: Figure 3 shows the average voluntary (panel (a)), mandatory (panel (b)), and total (panel (c)) saving rates out of household wages for the control group (dotted black line) and the treatment group (solid red line) as measured by fitted values from three regressions where each of the aforementioned variables is regressed on year fixed effects, group fixed effects, and the interaction between the two. The dotted vertical line in 2016 shows when the first stage of the reform was implemented.

absence of the reform. We implement this by estimating the following specification,

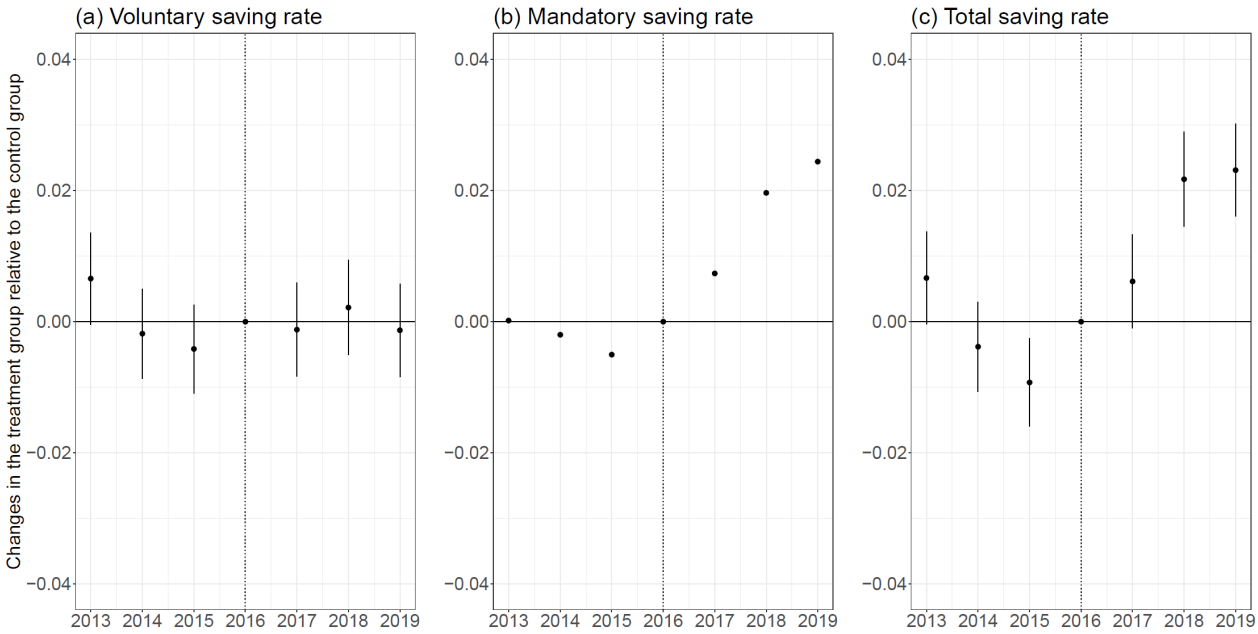
$$vs_{it}^{r} = \alpha_g + \alpha_t + \gamma_t \alpha_g \times \alpha_t + X_{i2015}\beta + \varepsilon_{it} \tag{11}$$

where α_g and α_t are group and year fixed effects. The vector γ_t contains the main coefficients of interest. It measures the average change in the voluntary contribution rate in each year relative to 2016 for private sector households, over and above the average change in the same period for public sector households. If the saving behavior of the two groups followed similar trends in the pre-reform period, the estimates of γ_t should be small and not statistically different from zero for $t < 2016$. Finally, X_{i2015} is a vector of household characteristics in 2015, prior to the reform. We restrict X_i to 2015 values to avoid controlling for responses to the reform. As an example, workers might respond to the reform by switching sectors.

Figure 4 reports changes in the treatment group's saving rates relative to that of the control group both before and after the reform is implemented.

Specifically, panel (a) shows the γ_t estimates from equation (11). It illustrates that the coefficients for the period prior to the implementation of the reform are small and statistically

Figure 4: Changes in the treatment group's saving rates relative to the control group



Notes: Panel (a) of Figure 4 plots the estimated γ_t from equation (11). In panels (b) and (c), the dependent variable has been replaced by the mandatory saving rate and the total saving rate, respectively. The dotted vertical line in 2016 shows when the first stage of the reform was implemented. The controls are dummies for age (which, despite the notation, is the only characteristic not fixed at the 2015 value), marital status, gender, urban, region of residence, number of children, labor income ventiles, net wealth ventiles, and education. 95% confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines.

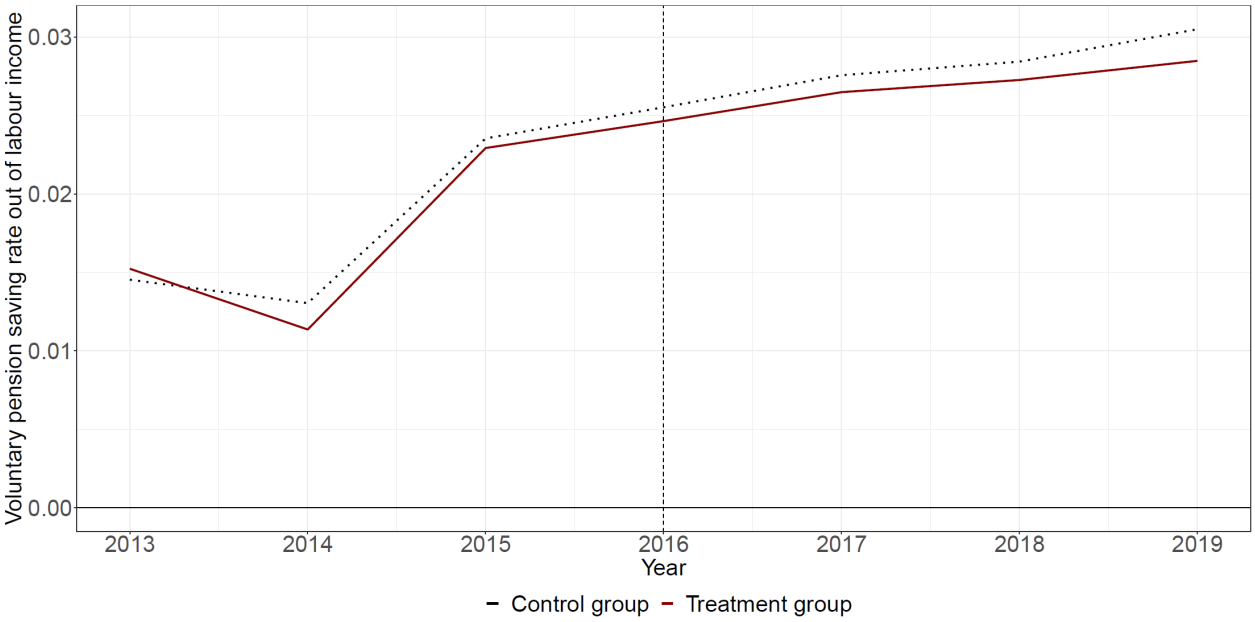
insignificant leading up to the reform. Hence, we conclude that the unobserved characteristics affecting saving behavior are roughly balanced across the two groups, and therefore, that potential differences arising between the groups in the post-reform period are plausibly due to the pension reform. Furthermore, we see the first evidence suggesting that private sector households did not, on average, respond to the reform by reducing their voluntary saving rate, as the coefficient estimates remain small and statistically insignificant after the reform was implemented. Panel (b) illustrates the relative increase in mandatory saving rates of the treatment group brought on by the reform, which, due to the non-responsiveness of the treatment group in terms of the voluntary saving rate, translates to a rising total saving rate of the treatment group relative to the control group, as is observed in panel (c).

5.4 Beyond voluntary saving

5.4.1 Substitution within the pension system

Increasing mandatory pension saving might be offset by substitution within the pension system. In particular, increasing the mandatory second-pillar pension saving rate could result in reductions in voluntary third-pillar pension saving rates. Voluntary pension saving is included in our voluntary saving variable above. Nevertheless, it is of interest to gauge specifically the substitution of mandatory and voluntary pension saving. Figure 5 shows the average net contribution to third-pillar pension funds of each group in 2013-2019. The two groups appear to follow a common trend both before and after the reform is implemented. Notice that the average voluntary third-pillar pension saving rate (net of withdrawals) rises sharply in 2015. This can likely be attributed to the fact that from mid-2014 onwards, individuals have been allowed to make tax-free withdrawals of up to ISK 500,000 (approximately USD 4,100 at the average 2019 exchange rate) from their voluntary third-pillar pension accounts and use the funds as payments towards mortgages.

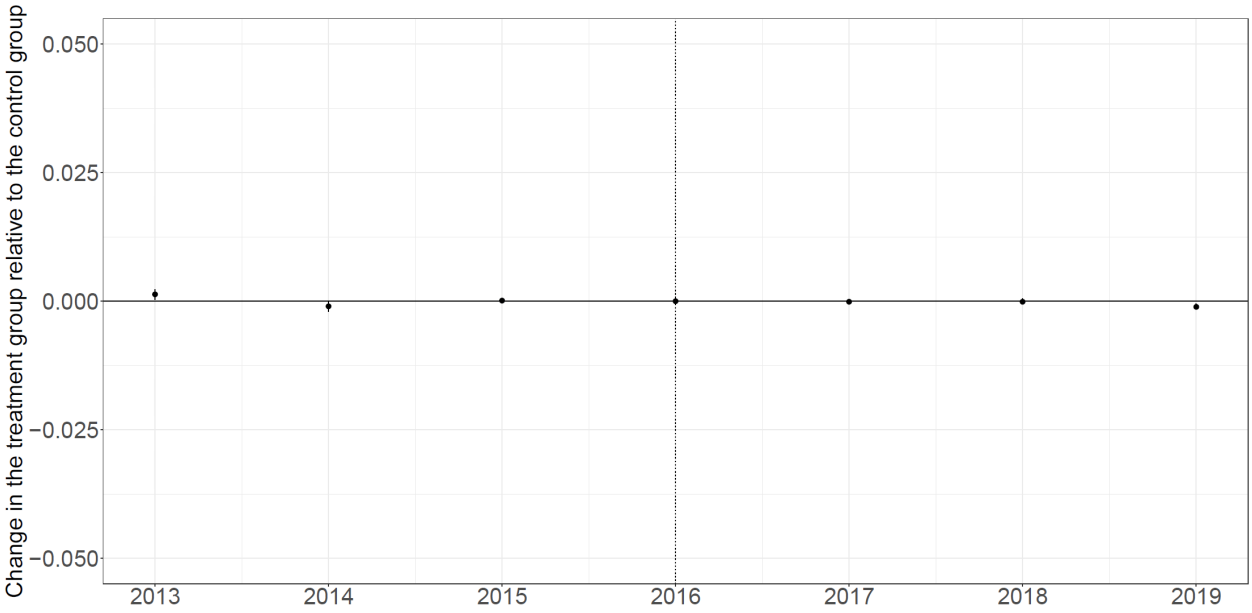
Figure 5: Changes in the treatment group's saving rates relative to the control group



Notes: Figure 5 shows average household net contribution to third-pillar pension accounts for the control group (dotted black line) and the treatment group (solid red line) as measured by fitted values from four regressions where each of the aforementioned variables is regressed on year fixed effects, group fixed effects, and the interaction between the two. Net contribution to third-pillar pension accounts shows the sum of employer and employee contributions to private pension accounts, net of any withdrawals made by individuals. All figures are presented at 2019 prices using the CPI and converted into USD using the 2019 exchange rate. The dotted vertical line in 2016 shows when the first stage of the reform was implemented.

Figure 6 shows the parallel trends analysis for the voluntary third pillar pension saving rate from equation (11). The coefficients are very precisely estimated and virtually the same both in the pre-reform and post-reform periods.

Figure 6: Changes in the treatment group's saving rates relative to the control group



Notes: Figure 6 plots the estimated γ_t from equation (11), with the voluntary pension saving rate, measured by contributions to voluntary third-pillar pension accounts net of withdrawals, as the dependent variable. The dotted vertical line in 2016 shows when the first stage of the reform was implemented. The controls are dummies for age (which, despite the notation, is the only characteristic not fixed at the 2015 value), marital status, gender, urban, region of residence, number of children, labor income ventiles, net wealth ventiles, and education. 95% confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines.

5.4.2 Debt accumulation

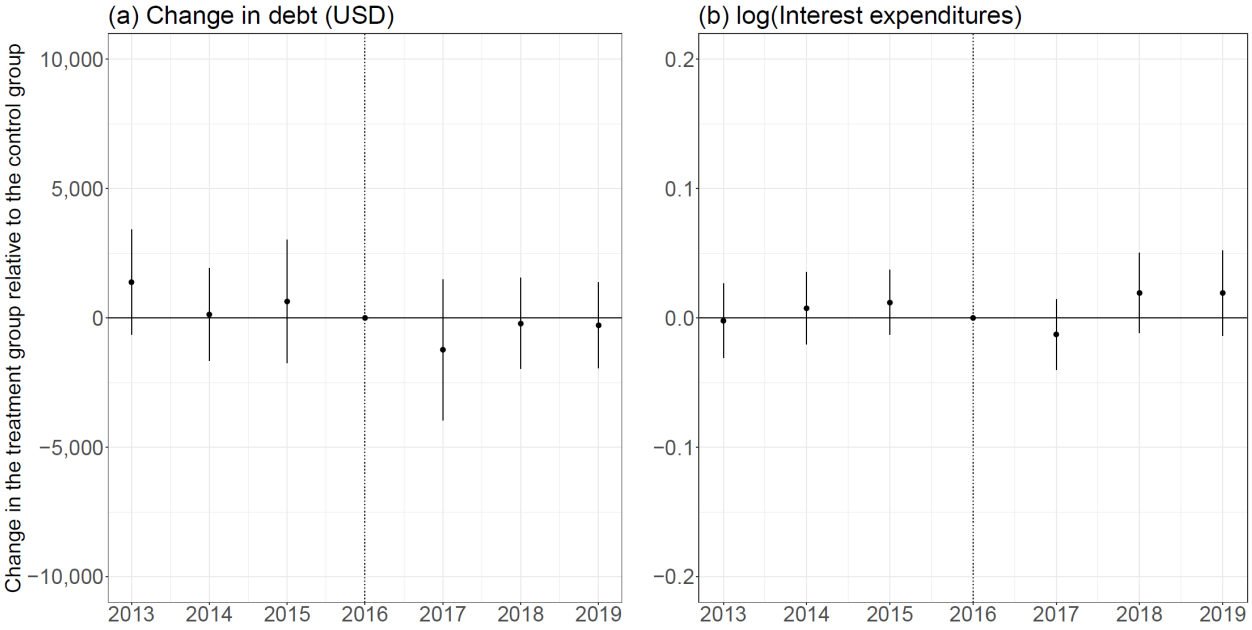
Next, we check whether the reform caused debt accumulation in the treatment group. Since households are unable to draw on their 2nd pillar pension savings, for example to smooth consumption once faced with shocks, they might offset their higher mandatory retirement savings by incurring more debt. This is a topic of important policy relevance as Mian and Sufi (2014, 2015) find that excessively indebted households sharply reduced their consumption in the wake of the GFC, thereby sustaining high levels of unemployment after the crisis. Prior evidence suggests mandatory saving might cause debt expansion. For example, Andersen (2018) finds that a 1 Danish currency unit reduction in retirement savings reduces household debt by 31 cents and Andersen et al. (2022) find that a 1-dollar increase in pension wealth leads to a 26-cent rise in total debt.

We formally explore the relative development of the change in debt by estimating equation (11) with the first difference of debt as the dependent variable rather than the voluntary saving. Other variables in the regression are unchanged. Since the first difference of debt is a volatile series with large outliers, we expect our estimates to be rather imprecise. Therefore, we also test the parallel trends for the log of interest expenditures as a proxy for debt. If the pension reform raised indebtedness in the treatment group relative to the control group, we would also expect the former's interest bill to have risen. Hence, we also estimate equation (11) with the log of interest expenditures as the dependent variable. Figure 7 shows that, controlling for household characteristics, both the first difference of debt (Panel (a)) and log of interest expenditures (Panel (b)) move closely together both before and after the pension reform for the two groups. Under the identification assumption that the two variables would have moved in tandem for the groups in the absence of the reform, we are unable to conclude that the reform caused a change in the indebtedness of households in the treatment group. However, while we do not find evidence of debt accumulation in the wake of the reform, it is possible such effects might emerge in the longer run. We, therefore, leave it to future research to examine the ins and outs of the potential medium to long-run effects of the reform on debt accumulation, and focus the rest of the paper on its effects on voluntary saving.

5.5 Panel regression evidence

The results from the first-stage regression in Equation (8), shown in Table 3, confirm that the pension reform had a clear effect on the mandatory saving rate of the treatment group. In particular, it raised the post-reform average mandatory saving rate of the treatment group by 1.5pp relative to that of the control group for the whole sample. The effect is very precisely estimated and robust to the inclusion of controls. Moreover, a weak instrument is clearly ruled out by a high F-statistic.

Figure 7: Changes in the treatment group's saving rates relative to the control group



Notes: Figure 7 plots the estimated γ_t from equation (11), with the change in debt (panel (a)) and the log of interest expenditures (panel (b)) as the dependent variable. Both variables are deflated using the CPI and converted to USD using average 2019 exchange rates. The dotted vertical line in 2016 shows when the first stage of the reform was implemented. The controls are dummies for age (which, despite the notation, is the only characteristic not fixed at the 2015 value), marital status, gender, urban, region of residence, number of children, labor income ventiles, net wealth ventiles, and education. 95% confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines.

Table 3: First stage results

	(1)	(2)	(3)	(4)
	Whole sample		Single households	
$\hat{\pi}_1$	0.015^{***}	0.015^{***}	0.017^{***}	0.017^{***}
	(<0.001)	(<0.001)	(<0.001)	(<0.001)
Controls	No	Yes	No	Yes
F-statistic for weak instrument	42,975	42,567	22,066	21,616
R^2	0.473	0.480	0.566	0.572
N	520,064	520,064	161,129	161,129

Notes: Table 3 reports OLS estimates of the effects of the instrument, that is the interaction of a post-reform dummy and the treatment group dummy on the mandatory saving rate (see equation (8)). Columns (1) and (2) report estimates for the whole sample, whereas columns (3) and (4) report estimates for single households only. Estimates are shown without controls (odd columns) and with controls (even columns). The controls are dummy variables for marital status, gender, age, urban, region of residence, number of children in the household, labor income ventiles, and net wealth ventiles. Standard errors, clustered at the household level, are in parentheses.

Next, we estimate the extent to which the effects of the pension reform were offset by changes in households' voluntary saving behavior. Table 4 shows the estimates obtained from our second-stage regression using (10). We also formally evaluate whether the reform led to substitution out of private pension saving.

The 2SLS analysis yields a crowding-out effect of 0.020, suggesting that raising the mandatory saving rate by a percentage point is met by a 0.020pp increase in households' voluntary saving rate. The coefficient is not statistically different from zero. Since we have a strong instrument which explains a substantial share of the variation in mandatory saving rates, we also report the reduced-form estimates of the crowding-out effect. Again, the results are not statistically different from zero although they are much more precisely estimated. Table A3-1 shows the robustness of our results to including individual fixed effects instead of treatment group dummies. The crowding-out coefficient is close to zero and similar to that of the baseline specification for the whole sample, although it is somewhat higher for single households (0.13-0.16) compared to the baseline (0.06-0.09).

Table 4: Crowding-out results

	2SLS		Reduced-form		2SLS		Reduced-form	
	Whole sample				*Single households*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Crowding-out	-0.000	0.020	-0.000	0.000	-0.058	-0.087	-0.001	-0.002
	(0.134)	(0.134)	(0.002)	(0.002)	(0.155)	(0.155)	(0.003)	(0.003)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	0.001	0.018	0.001	0.018	0.001	0.016	0.001	0.016
N	520,064	520,064	520,064	520,064	161,129	161,129	161,129	161,129

Notes: Table 4 shows the crowding-out effects ($\hat{\rho}$) estimated using equation (10). Columns (1) and (2) report the findings from a 2SLS estimation for the whole sample. Columns (3) and (4) report reduced-form results for the whole sample. Columns (5) and (6) report findings from a 2SLS estimation for single households only. Columns (7) and (8) report reduced-form results for single households only. The estimates are shown without controls (odd columns) and with controls (even columns). The controls are dummy variables for age (which, despite the notation, is the only characteristic not fixed at the 2015 value), marital status, gender, urban, region of residence, number of children, labor income ventiles, net wealth ventiles, and education. Standard errors, clustered at the household level, are in parentheses.

Furthermore, since we group households, rather than individuals, into treatment and control groups, some households in the treatment group include individuals who work in the public sector and, thus, belong to the control group and vice versa. We therefore repeat our analysis using only information on individuals who do not have a spouse, labelled single

households. [13]

Figures A5-1 and A5-2 show the average saving rates and the parallel trends analysis, respectively for the single households. The mandatory saving rate, which assigns households into treatment and control groups, is therefore not a weighted average across couples, but rather the individuals' mandatory saving rate directly observed in the data. The average mandatory saving rate is flat both before and after the reform for the control group, whereas it rises for the treatment group as the reform is implemented. The voluntary saving rate of the two groups moved together both before and after the reform. This supports our identifying assumption that unobservable characteristics affecting the voluntary saving rate are uncorrelated with the treatment, so that the voluntary saving rate of the two groups would have moved together in the absence of the reform.

The first stage regression shows that the average mandatory saving rate of the treatment group rose by 1.7pp relative to the control group in the post-reform period, compared to 1.5pp for the whole sample. Although restricting the sample to single households allows for a cleaner definition of the control and treatment groups, it comes at the cost of fewer observations. However, the estimates of the crowding-out effect only change marginally. In sum, we do not find evidence suggesting households offset the increase in mandatory saving rates by reducing their voluntary saving rate.

Table 5 presents the estimates obtained from equation (10) using the rate of voluntary private pension saving out of labor income as dependent variable. In line with previous results, we find a limited crowding-out effect. For the whole sample, we find a statistically insignificant crowding-out of 0.026pp. For single households, we find a crowding-out of 0.046, significant at 5% significance levels. Hence, the reform succeeded in increasing total retirement savings.

5.6 Heterogeneity analysis

As shown above, we find a limited crowding-out effect of mandatory saving rates on voluntary saving rates. Below, we explore whether the estimated crowding-out effect is driven by particular subgroups. First, we explore the role of liquidity constraints. Households with binding liquidity constraints, by definition, have a limited capacity to respond to the reform by lowering their voluntary saving rates. Therefore, we would expect their crowding-out effect to be lower compared to households not subject to such constraints. A household is considered liquidity-constrained if the sum of the household's bank deposits and its assets in mutual funds are worth less than three months of the household's disposable income.

[13] We use strictly single households, rather than single households and couples who work in the same sector, as the pre-trends are more convincing if we limit the sample to single households.

Table 5: Crowding-out results for voluntary pension saving

	2SLS			
	Whole sample		*Single households*	
	(1)	(2)	(3)	(4)
Crowding-out	-0.048**	-0.026	-0.061***	-0.046**
	(0.020)	(0.020)	(0.023)	(0.023)
Controls	No	Yes	No	Yes
	0.018	0.044	0.020	0.032
N	520,064	520,064	161,129	161,129

Notes: Table 5 shows the crowding-out effect ($\hat{\rho}$) estimated using equation (10), where the rate of voluntary pension saving out of labor income have replaced the voluntary saving rate as the dependent variable. The estimates are shown without controls (odd columns) and with controls (even columns). The controls are dummy variables for age (which, despite the notation, is the only characteristic not fixed at the 2015 value), marital status, gender, urban, region of residence, number of children, labor income ventiles, net wealth ventiles, and education. Standard errors, clustered at the household level, are in parentheses.

According to this definition, two-thirds of households in our sample have a binding liquidity constraint. Second, we test whether household's responses to the reform depend on their place in the income distribution. The hypothesis is that households at the top of the income distribution might be better able to adjust their voluntary saving when faced with higher mandatory saving. We, therefore, split the sample into households in the top 25% of the wage income distribution and those in the bottom 75%. To ensure that differences in the saving response to the reform are not driven by life-cycle effects, the income distribution is defined within each birth cohort. Third, we gauge whether the crowding-out effect is age-dependent. This could be the case, for example, if older individuals, proxied by those aged 45 and over, are more aware and informed about their pension affairs and changes therein than younger individuals, proxied by those aged 44 and under. Finally, we consider whether households' responses differed depending on their level of education, as education might serve as a proxy for financial literacy and awareness of the reform. Specifically, we compare the responses of university-educated households to those of other households. A household is considered university-educated if any household member holds a university degree.

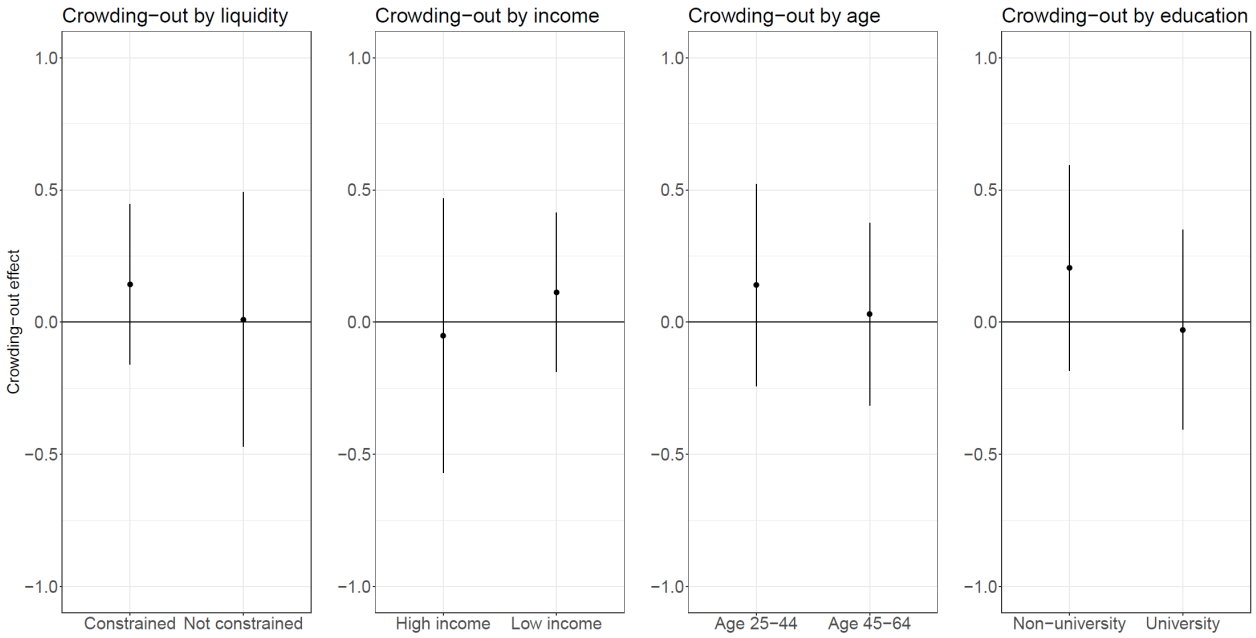
Specifically, we estimate the crowding-out effect for various subgroups following the same 2SLS procedure as above, albeit using a slightly different specification,

$$vs_{it}^{r} = \alpha_1^k post_t + \alpha_2^k treated_g + \sum_j \alpha^{jk} G_i^{jk} + \alpha_3^k treated \times G_i^{jk} + \alpha_4^k post \times G_i^{jk} \qquad (12)$$
$$+ \rho^k \widehat{ms}_{it}^{r} + \rho^{jk}(G_i^{jk} \times ms_{it}^{r}) + \boldsymbol{X}_{it}\boldsymbol{\beta} + \varepsilon_{it}$$

where G_i^{jk} is a dummy that takes the value 1 if individual i belongs to subcategory k of category j and otherwise takes the value zero. Now we have more than one endogenous variable – namely, the mandatory saving rate, $ms_{i,t}^r$, and the interaction of mandatory saving with the relevant subcategory dummies, $G_i^{jk} \times ms_{it}$ – and therefore need more than one instrument. As before, these are the interaction of the post-reform dummy and the treatment group dummy, but also the interaction of the treatment group dummy and the subcategory dummy, the post-reform dummy and the subcategory dummy, and finally, the interaction among all three variables.

Figure 8 plots the crowding-out effect for the four subcategories. Overall, we do not find evidence of much heterogeneity in the crowding-out effect. However, in line with our predictions, the point estimate of the crowding-out effect is higher; i.e., the coefficient is lower, for households that do not face a binding liquidity constraint compared to those that do.

Figure 8: Crowding-out by liquidity, income, age, and education



Notes: Figure 8 plots the estimated crowding-out effect for various subcategories (ρ^k and $\rho^k+\rho^{jk}$) from equation (12). The subcategories are liquidity-constrained vs. non-liquidity-constrained households (left), higher-income vs. lower-income households (mid-left), younger vs. older households (mid-right), and university-educated vs. non-university-educated households (right). Households with deposits and assets in mutual funds of less than three months of disposable income are considered liquidity-constrained. A household is considered a high-income household if its labor income per household member is in the top quartile; otherwise, it is considered a low-income household. The household's age corresponds to the age of the oldest household member. A household is considered university-educated if any household member holds a university degree. The controls are dummies for age (which, despite the notation, is the only characteristic not fixed at the 2015 value), marital status, gender, urban, region of residence, number of children, labor income ventiles, net wealth ventiles, and education. The black dots represent point estimates, and 95% confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines.

In a similar vein, the crowding-out effect appears to be marginally lower for higher-income households than lower-income households, although again the crowding-out effect is close to zero for both groups and the two coefficients do not differ significantly from one another. Furthermore, and again in line with our predictions, we find a higher point estimate for the crowding-out effect for older households compared to younger households, although the difference is insignificant. Finally, households with a university degree seem to be more likely than other households to reduce their voluntary saving when faced with a policy raising their mandatory saving although, again, the difference is insignificant at standard significance levels. The findings suggest that while liquidity constraints and financial awareness do contribute to a muted crowding-out effect, they do not explain why it is far from the full crowding-out effect predicted by theory. As such, increasing mandatory saving does not rely on frequently binding liquidity constraints and lack of financial literacy to increase total

saving.

6 Away from the reform - Evidence from job switchers

The 2016-2018 reform of the Icelandic pension system offers a large exogenous variation to mandatory pension saving of a subset of the labor market and, thus, provides a rare opportunity to study the crowding-out effect of mandatory saving on voluntary saving. However, as pointed out in section 3.2, it does deviate from the experiment one would ideally design to estimate the crowding-out effect, in that the increase in the mandatory saving rate was negotiated as a part of a collective bargaining agreement. As such, the reform affected a large subgroup of the population, i.e. private sector workers, which is not drawn at random and might differ from the subgroup of the population that was not affected by the reform, i.e. public sector workers.

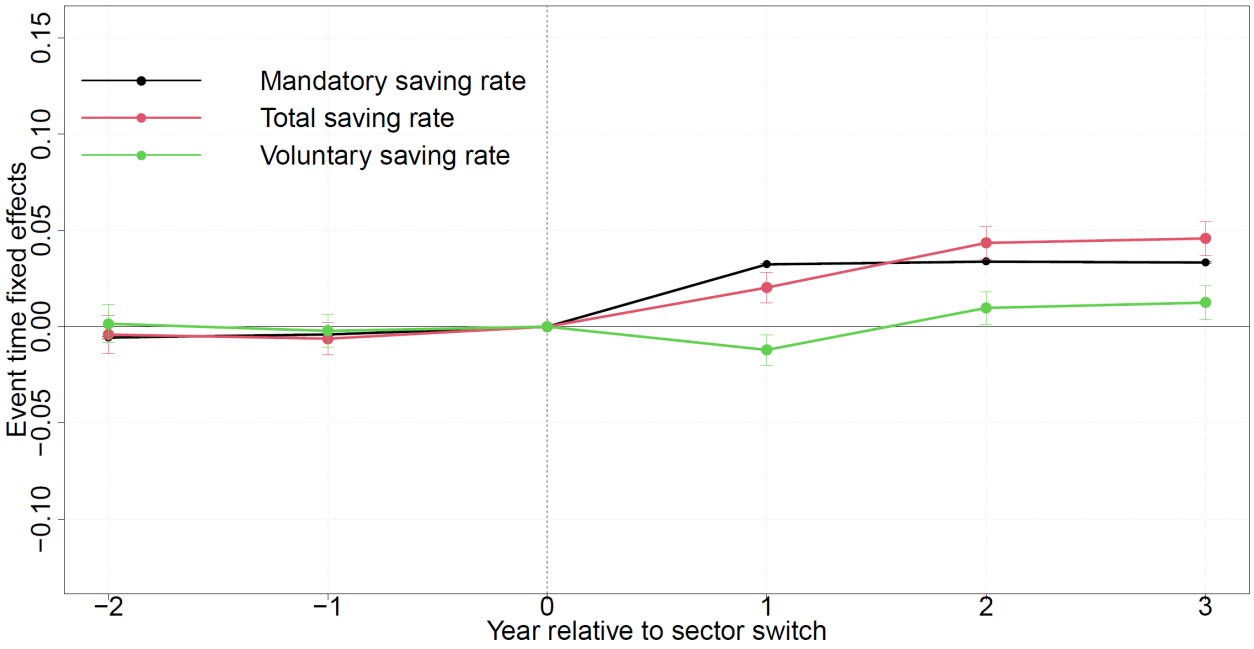
While some concerns about the reform have been addressed above, in this section we show that our findings are robust to other changes in the mandatory saving rate. Following Chetty et al. (2014), we study changes in households' saving rates when they switch jobs.

Figure 9 plots an event study of households who switch jobs from the private sector, which historically has had a relatively low mandatory pension contribution rate, to the public sector, which historically has had a relatively high mandatory pension contribution rate. Figure 10 shows the same for single households only. Under a full crowding-out of mandatory saving, the voluntary saving rate (green line) would decline in period 1 to offset the increase in the mandatory saving rate (black line), thereby leaving the total saving rate (red line) unaffected and close to zero after the job switch.

Our sample is the same as in the main analysis, but the sample period is now 2004-2016, thus ending as the first and smallest stage of the pension reform is being implemented. A switch is identified using information on mandatory pension contribution rates. In particular, a job switcher has a mandatory contribution rate below 13.75% in year $t-1$ and above 13.75% in year t and the difference between year's t and year's $t-1$ mandatory contribution rate is at least 0.5pp. Year 0 is defined as the last year before a job switch occurs. All other years are defined relative to that year and the sample includes three years prior to the switch and three years post-switch, inclusive. The black line plots the mandatory saving rate and the red line plots the total saving rate.

Under the identification assumption that the total saving rate would have remained unchanged between period -1 and period 0 in the absence of a job switch, we infer that a 1pp increase in the mandatory saving rate caused the total saving rate to increase by 0.64pp for the whole sample. For single households, which arguably provide a better identification of

Figure 9: Saving rates of job switchers in the whole sample.

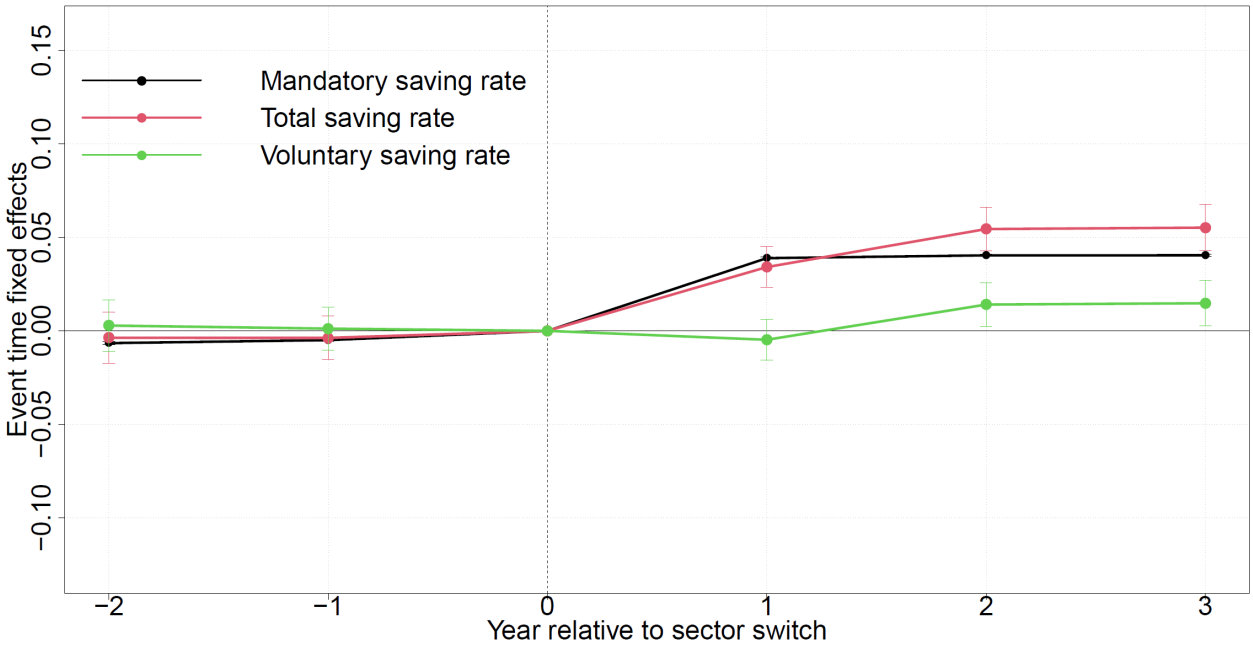


Notes: Figure 9 plots the estimated $\alpha_{et,i}$ from the equation $Y_{i,t} = \alpha + \alpha_{et,i} + \varepsilon_{i,t}$ for the whole sample, where $Y_{i,t}$ is either the total saving rate (red), the mandatory saving rate (black), or the voluntary saving rate (green), α is the intercept and $\alpha_{et,i}$ is event time fixed effects. The sample consists of 100,642 observations of 22,277 households that moved from the private sector to the public sector once and only once in 2004-2016. The point estimates for mandatory saving rates are represented by black dots. The point estimates for total saving rates, which is the sum of voluntary saving rates out of household wages and mandatory saving rates out of household wages, are represented by red dots. 95% confidence intervals, based on standard errors clustered at the household level, are represented by dotted vertical lines.

the crowding-out effect in this case, a 1pp in-crease in the mandatory saving rate caused the total saving rate to rise by 0.88. As such, the voluntary saving rate was adjusted downwards by 0.12-0.36pp. While the crowding-out effects estimated using job switches are somewhat higher compared to estimates from the 2016-2018 pension reform, they still point to limited crowding-out effects suggesting households' aggregate saving behavior is heavily influenced by automatic contributions made on their behalf.

To shed light on the underlying assumption in the job switchers analysis that switches are unrelated to occupational pensions, appendix Figure A6-1 shows that the rate of job switches to the public sector, as defined above, has been relatively stable post 2010, including in 2016 when the first step of the reform was implemented. Figure A6-2 shows the same for single households only. As our definition of a job switch relies on changes to the mandatory contribution rate, we are not able to observe the rate of switches in the post-reform period. We, therefore, proceed by defining the public sector as a) public ad-ministration, defense, and compulsory social security, b) education, c) human health services, d) residential care

Figure 10: Saving rates of single job switchers.



Notes: Figure 10 plots the estimated $\alpha_{et,i}$ from the equation $Y_{i,t} = \alpha + \alpha_{et,i} + \varepsilon_{i,t}$ for single job switchers only, where $Y_{i,t}$ is either the total saving rate (red), the mandatory saving rate (black), or the voluntary saving rate (green), α is the intercept and $\alpha_{et,i}$ is event time fixed effects. The sample consists of 30,734 observations of 9,264 single individuals that moved from the private sector to the public sector once and only once in 2004-2016. The point estimates for mandatory saving rates are represented by black dots. The point estimates for total saving rates, which is the sum of voluntary saving rates out of household wages and mandatory saving rates out of household wages, are represented by red dots. 95% confidence intervals, based on standard errors clustered at the household level, are represented by dotted vertical lines.

and social work activities, and e) libraries, archives, museums, and other cultural activities. Other sectors are classified as the private sector. Appendix Figure A6-3 shows the rate of switches to and from the public sector in 2009-2019, thus covering the post-pension reform period. The rate of switches to both sectors is relatively stable over time with no indication that the rate job switches to the private sector rose in the post-reform period. In sum, we find limited evidence of strategic switches from the public sector to the private sector as the private sector mandatory employer pension contribution rate rose.

Finally, to provide context on the nature of job switches, appendix Figures A6-4 and A6-5 report the development of surrounding the job switch. Wages seem to fall somewhat as individuals switch from the private sector to the public sector before recovering in the first post-switch year. This is consistent with the fact that average wages are lower in the public sector compared to the private sector, as seen in Table 2. However, the sample period might also play a role as it includes a large drop in real wages following the global financial crisis and the collapse of the Icelandic banking system, and the subsequent real wage recovery. A

short unpaid leave or a short unemployment spell between jobs might also explain the drop in wages at the year of the switch.

7 From hard data to soft statistics - Survey evidence

We have found that on average, private sector employees did not respond to the increase in the employer contribution to their pension funds. This is broadly in line with the findings of previous research using quasi experimental evidence. While the literature can provide insight into *how* individuals respond to pension reforms, the explanation of *why* there is inertia in pension saving behavior is largely absent.

Above, we sought to fill this gap in the literature by exploring heterogeneities in the crowding-out effect using comprehensive administrative data. However, such data lack information on households' awareness of the reform and their saving motives. We therefore conduct a survey to better understand the reasons for our results.[14] In particular, we test four hypotheses for lack of response:

1. Lack of awareness about the reform might explain why an individual's responses were muted.

2. Liquidity-constrained individuals might lack the means to respond by reducing their saving.

3. Saving methods might affect individuals' responses. For example, individuals may apply a rule of thumb, such as saving a fixed amount or fixed percentage of their income per month, and therefore are not affected by the reform.[15]

4. Saving motives might affect individuals' responses. For example, if an individual's main motive for saving is to finance an expense that will take place prior to retirement, such as a vacation or the purchase of a new car, then the reform will not influence saving.

The survey covered 946 individuals. Of these, in 2015, 35.4% worked in the private sector, 23.7% in the public sector, 13.3% were not employed, and 13.8% were self-employed, worked for non-governmental organizations (NGOs), or did not specify their sector (see Table 6). In this section, we focus on those aged 25-65 who worked in either the private or the public

[14]The survey was conducted by the firm Maskina for the purpose of this study between 27 September and October 2021.

[15]It is possible that an individual saves a fixed amount or a fixed percentage of her income per month and considers her employers' pension contributions when arriving at that percentage or amount. We do however assume that majority of individuals are referring to the amount they put aside themselves

Table 6: Survey summary statistics

		Sample used		
	Original	Full	Treatment group	Control group
Private	35.4	59.4	100	0
Public	23.7	40.6	0	100
Not employed	13.3	0	0	0
Self-employed	13.8	0	0	0
NGO	3.8	0	0	0
Missing	9.9	0	0	0
Age	46.1	45.3	43.9	47.4
Female	52.6	54.2	47.4	64.2
Married	65.5	76.4	77.7	74.3
Observations	946	461	274	187

Notes: The first column shows descriptive statistics for the full surveyed sample. The second to fourth columns show descriptive statistics for the sample used; individuals aged 25-65 who worked in the treatment or control groups in 2015. The second column shows both the treatment and control groups, while the third and fourth columns show the treatment and control groups, respectively.

sector in 2015. We refer to 2015 private sector workers as the *treatment* group and 2015 public sector workers as the *control* group. A majority of these – 71.2% of private sector workers and 71.1% of public sector workers – were in the same sector in 2021. On average, private sector workers are slightly younger, less likely to be female, and more likely to be married than public sector workers.

Because the variable of interest is voluntary saving, we begin by inquiring about changes in saving behavior between 2015 and 2021. In total, 40% say that they have increased their voluntary saving, or "other saving", since 2015; 33% say it is unchanged; 20% say they had no voluntary saving in 2015 nor 2021; and 7% say they reduced voluntary saving (see Figure 11). More people in the public sector say that they have increased voluntary saving, and more people in the private sector say that they have reduced it or that it is unchanged. To formally test this hypothesis, we create the variable Δvs, which equals - 1 if voluntary saving has decreased, 0 if voluntary saving is unchanged (which includes those who neither saved in 2015 nor in 2021), and 1 if voluntary saving has increased,

$$\Delta vs_i = \begin{cases} -1 & \text{if saving has reduced} \\ 0 & \text{if saving is unchanged} \\ 1 & \text{if saving has increased} \end{cases} \tag{13}$$

If the average of Δvs_i is positive then the share which has increased saving exceeds the share that decreased saving. Although not being entirely correct, we will when comparing two groups say that a larger share of individuals has decreased their saving in the group that has a smaller average of Δvs. To formally test our hypotheses, we run the following regression, one for each hypothesis,

$$\Delta vs_i = \alpha_0^j + \alpha_1^j Private_i + \alpha_2^j G_i^j + \alpha_3^j (Private_i \times G_i^j) + \boldsymbol{X}_i \boldsymbol{\beta}_j + \varepsilon_{ji} \tag{14}$$

where j indicates the hypothesis being tested, and G_i^j is an indicator for hypothesis j's group of interest, those who: i) are aware of the reform, ii) have liquidity, iii) have target saving, and iv) save for their pension (see further discussion below).

To gauge awareness of the reform, one question tests whether respondents know what the employer contribution is, and another tests whether they know if the employer contribution has changed in past years (see Figure 12a). Only around 26% of respondents answered correctly that the employer contribution was between 9% and 13% of wages. The proportion responding correctly is somewhat higher for the treatment group (29%) than for the control group (22%). When asked about changes in the employer contribution in the past six years, 36% of the treatment group responded - correctly - that the employer contribution had increased, while 25% of the control group responded - incorrectly in their case - that the contribution had increased. Only 34% of the control group answered correctly that the employer contribution had not changed. This implies that workers are to a large extent uninformed about their pension contribution and future pension income.[16]

For the first hypothesis - the indicator, $G_i^1 = \text{Aware}_i$, takes the value 1 for treatment group who believe that their employer's contribution rate has increased since 2015 and for control group members who believe the contribution rate is unchanged, but otherwise it is zero.

Hypothesis 1 ***(Awareness)*** *In response to the reform, treated workers that are aware of the reform decreased their saving more than others,* $\alpha_3^1 < 0$.

The second hypothesis is that households with *liquidity* did reduce their voluntary saving due to the reform. Figure 13 shows the responses to the question on households' finances. Around 62% say that expenses are generally lower than income, and the distribution of answers was very similar across sectors. For the second hypothesis - the indicator, $G_i^2 = \text{Liquidity}_i$, equals 1 if *expenses are generally lower than income*, but is zero otherwise.

[16]For these statistics on the contribution rate and how it has changed, we have excluded those who were not employed in 2021.

Hypothesis 2 ***(Liquidity)*** *In response to the reform, private sectors workers with liquidity decreased their saving more than others,* $\alpha_3^2 < 0$.

The third hypothesis is that the lack of response is driven by households' *saving methods*. To address this, we ask households which method best described how they handled their saving (see Figure 14). A striking 21% of the control group and 31% of the treatment group do not save in addition to their pension saving. Now, 7.5% and 12% of public and private sector workers, respectively, say that they *have a certain amount in mind that they want to have saved at a certain time, and that they plan their saving accordingly*. We call this a saving target. Finally, 5% either said that they decide their saving according to another method or did not respond. The only group we expect to respond to a pension reform are the 12% of private sector workers who have a saving target. First, we have no reason to believe that those who do not save in addition to their pension saving will respond. Second, since the reform is likely to affect neither income (as shown above) nor expenses, it should not affect the saving of those who save what is left when they have paid their expenses. Third, the reform should not affect the saving of those who save a fixed amount each month or a fixed percentage of their monthly income. Finally, only the saving of those with a saving target is likely to be affected by the reform. Therefore, for the third hypothesis - that in response to the reform, private sectors workers with a saving target decrease their saving - the indicator, $G_i^3 = \text{Target saving}_i$, takes the value 1 for those that *have a certain amount in mind that they want to have saved at a certain time and plan their saving accordingly*, but is zero otherwise.

Hypothesis 3 ***(Target saving)*** *In response to the reform, private sectors workers with a saving target decrease their saving more than others,* $\alpha_3^3 < 0$.

Finally, the fourth hypothesis is on saving motives. As mentioned above, if an individual is saving for an expense that will take place prior to retirement, then the pension reform will not influence saving. To address this, we asked individuals about their saving motives, with 15% reporting that their main saving motive was saving for retirement (see Figure 15). This can also serve as an alternative test for the role of awareness about the reform, as households whose main saving motive is saving for retirement can be assumed to be better informed about pension affairs than those with other saving motives. For the fourth hypothesis - that in response to the reform, the treatment group with pension saving motives decreases saving - the indicator, $G_i^4 = \text{Pension motives}_i$, takes the value 1 for those who claim that *the main motive of their saving is retirement saving*, but is zero otherwise.

Hypothesis 4 ***(Pension motives)*** *In response to the reform, private sector workers with pension saving motives decrease their saving more than others,* $\alpha_3^4 < 0$.

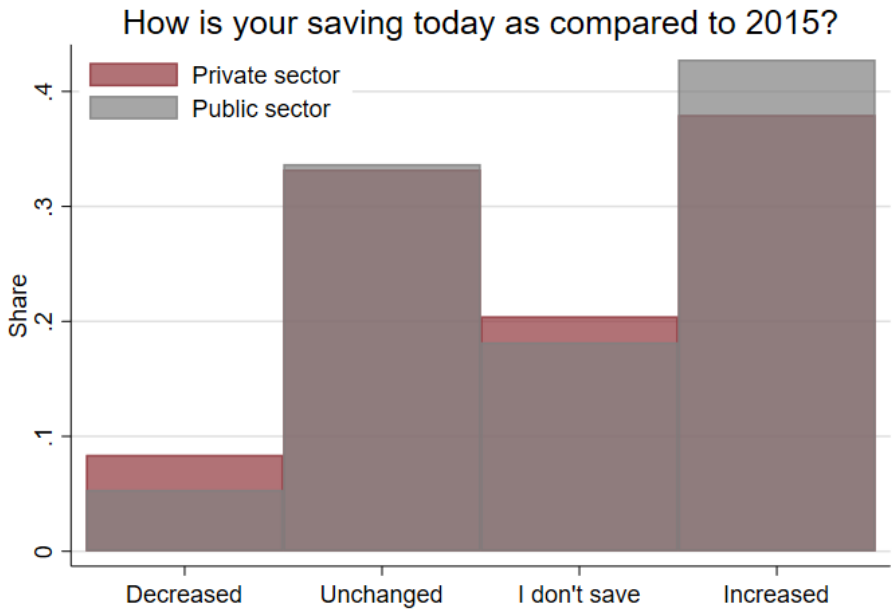
Before we consider our hypotheses, we start by estimating specification (14), with the treatment group and the subgroup dummies excluded (see column (1) of Table 7). Between 2015 and 2021, 40% of individuals increased their saving and 7% decreased their saving, a difference of 33 percentage points. To test our base hypothesis, that those who were affected by the reform reduced their voluntary saving, we can include the treatment group dummy (see column (2)). We obtain the (albeit statistically insignificant) result that, during this period, a higher share of the treatment group acted by decreasing their saving due to the reform. Column (3) shows the results from the same specification when, in addition, controlling for age, gender, marital status, education, income, and whether an individual's sector status has changed. This results in an even smaller, and still insignificant coefficient.

The regression results from equation (14), using the awareness, liquidity, and target dummies, can be seen in columns (4), (5) and (6), respectively. The estimates of the parameters of interest are statistically insignificant. That is, although the survey confirms that individuals are generally unaware of the reform and of the design of the pension system in general, we are not able to link the lack of awareness to the limited households' responses to the reform reported above. Also, we fail to conclude that illiquidity is driving the lack of response we find, and that saving target is driving the lack of response we find.

The final hypothesis is that in response to the reform, treated individuals with pension saving motives decreases saving. The regression results for this hypothesis can be seen in column (7). Now, the coefficient on the interaction of the private sector and the pension motives dummies is significant at the 5 percent significance and large in absolute terms, -0.28. Therefore, private sector workers with pension motives were significantly more prone to offset their savings than private sector workers with other motives. To put the number in perspective, the estimate should, on average, have been 0 if private sector workers with and without pension motives were as likely to offset their saving. Now, assume that no private sector worker without pension motive responded to the reform, and that no private sector worker with pension motive increased saving due the reform. In this case the estimated coefficient will entirely capture private sector worker with pension motive that decreased their saving.

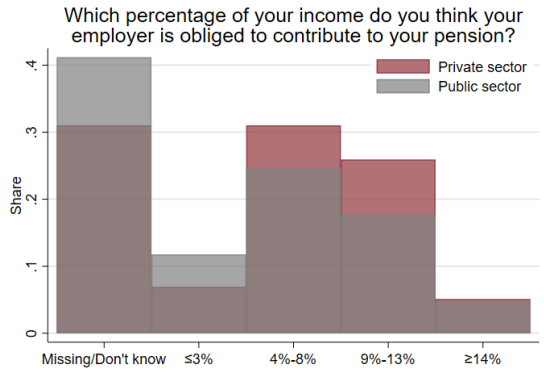
Since only 14% of the treatment group report pension saving as their main saving motive, we conclude that a part of the very limited response to the reform is likely to be driven by a few individuals with pension saving motives.

Figure 11: Changes in saving behavior between 2015 and 2021

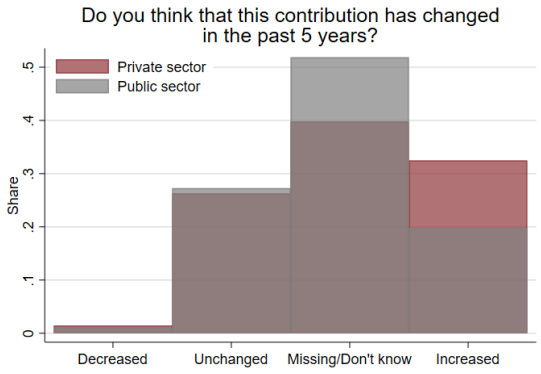


Notes: Which of the following best describes your "other saving" compared to that in 2015? i) I reduced "other saving" (Reduced), ii) My "other saving" is unchanged (Unchanged), iii) I do not save now nor did I save in 2015 (I don't save) , iv) I increased "other saving" (Increased).

Figure 12: Knowledge about pension saving and about the pension reform



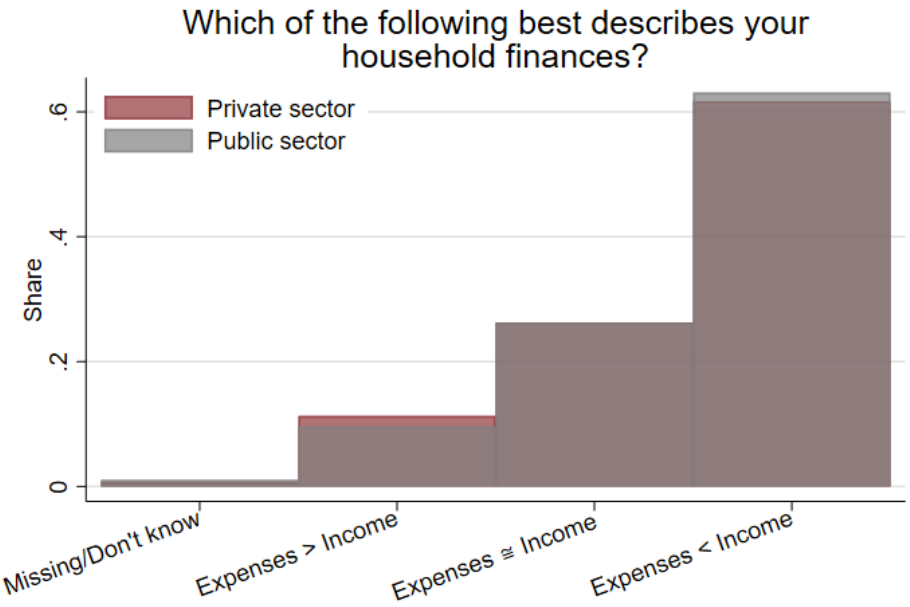
(a) Beliefs about contribution rate



(b) Beliefs about change in contribution rate

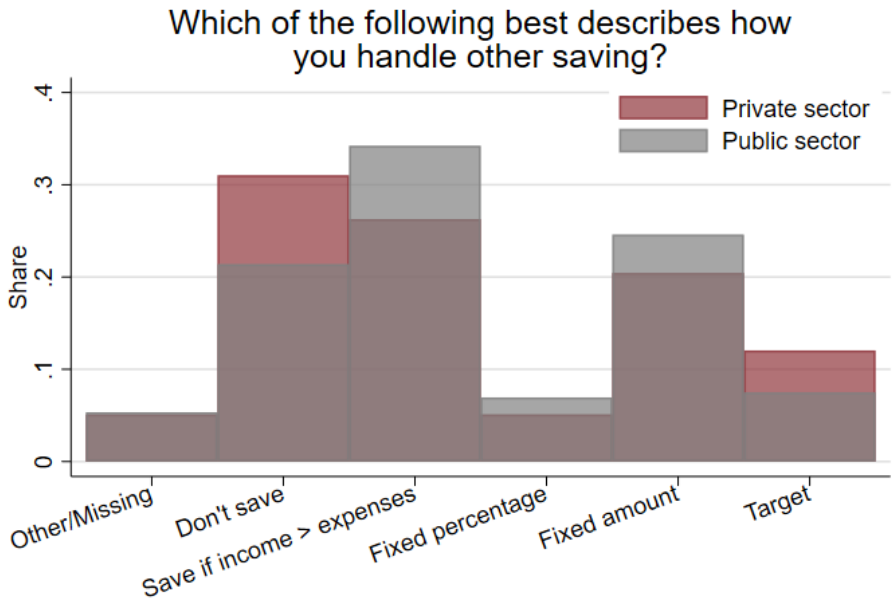
Notes: a) What is the proportion of your wage that your employer contributes to your pension fund? i) Less than 3%, ii) Between 4% and 8%, iii) Between 9% and 13%, iv) 14% or more. b) Do you think that the proportion your employer contributes to your pension fund has changed in the past five years? i) Yes, it has decreased (Decreased), ii) No, it has not changed (Unchanged), iii) Do not know (Don't know) , iv) Yes, it has increased (Increased).

Figure 13: Households' saving situation



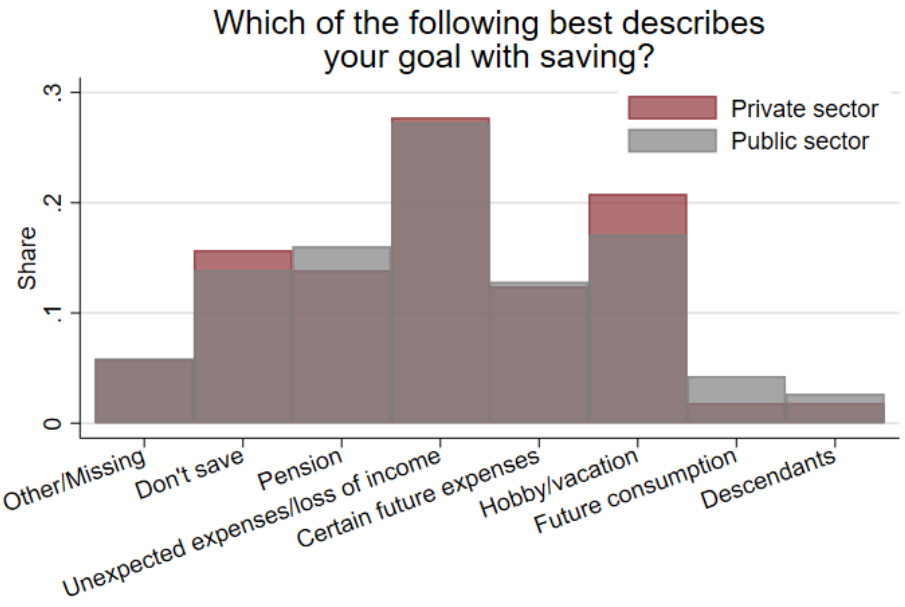
Notes: Which of the following best describes your household finances? i) Expenses are generally higher than income (Expenses > Income), ii) Expenses approximately equal income (Expenses ≈ income), iii) Expenses are generally lower than income (Expenses < Income).

Figure 14: Households' method of saving.



Notes: Which of the following best describes how you handle "other saving"? i) I do not save in addition to my pension savings (Do not save), ii) I have certain expenses each month and if my income (or my family income) is higher than the expenses, then I save (Save if income ¿ expenses), iii) I save a fixed percentage of my income (Fixed percentage), iv) I save a fixed amount every month (Fixed amount), v) I have a certain amount in mind that I want to have saved at a certain time, and I plan my savings accordingly. (Target), vi) Other, what? (Other).

Figure 15: Households' motive for saving



Notes: Which of the following best describes your saving objectives? i) I do not save in addition to my pension savings (Do not save), ii) I save for retirement (Pension), iii) I save to meet unexpected expenses or income losses (Unexpected expenses/loss of income), iv) I save for certain expected future expenses, such as buying an apartment or a car (Certain future expenses), v) I save to finance hobbies and vacations (Hobby/vacation), vi) I save to be able to increase my future consumption (Future consumption), i) I save in order to leave bequests for my descendants (Descendants) viii) Other, what? (Other).

Table 7: Estimates of the effects of the reform on voluntary saving by subgroups

	Δvs						
Constant	0.33***	0.37***	0.14	0.12	-0.04	0.15	0.10
	(0.03)	(0.04)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)
Private		-0.08	-0.03	-0.05	0.04	-0.06	0.01
		(0.06)	(0.06)	(0.06)	(0.09)	(0.06)	(0.06)
Aware				0.23			
				(0.15)			
Private × Aware				-0.04			
				(0.17)			
Liquidity					0.31***		
					(0.09)		
Private × Liquidity					-0.10		
					(0.11)		
Target						0.19	
						(0.17)	
Private × Target						0.14	
						(0.20)	
Pension motive (G^4)							0.37***
							(0.11)
Private × Pension motive							-0.28*
							(0.15)
R^2	0.00	0.00	0.08	0.09	0.12	0.10	0.10
N	461	461	461	461	461	461	461

Notes: The results from regression (14) using the survey sample restricted to individuals aged 25-65 who worked in the private or public sector in 2015. Controls used are age, gender, marital status, education, income, and sector switch dummies. For those who were public sector workers in 2015, a sector switch dummy indicates that they were not employed in 2021. For those who were private sector workers in 2015, a sector switch dummy indicates that they were either not employed or self-employed in 2021. $***p < 0.01$,$**p < 0.05$,$*p < 0.1$.

8 Concluding remarks

We study whether increasing mandatory pension saving rates leads households to reduce their voluntary saving or whether the increase is passed through to total saving. We use a large increase in the mandatory pension saving rate in the private labor market in Iceland as a natural experiment providing exogenous variation in pension saving. We do not find evidence suggesting that households responded to the pension reform by materially reducing their voluntary saving. Rather, the evidence suggests that the increased mandatory pension saving largely passed through to higher total saving, thereby succeeding in raising the overall saving rate of the economy.

We attribute the findings to lack of awareness about the reform and limited responsiveness to the reform, even among those who appear to have known that their mandatory saving rate had increased and those who aim to have a buffer stock of savings, and low-income, liquidity-constrained households. Although non-liquidity-constrained, older, university-educated and higher-income households seem to partly offset the effects of the reform by reducing voluntary saving, the crowding-out effect is close to zero – and well below the full crowding-out predicted by conventional theory.

Thus, our results do not indicate that workers are rational and forward-looking in their saving behavior. Instead, the results are consistent with descriptions of consumer behavior found in behavioral economics. Thaler (1990, 1999) assumes that people have a system of mental accounts. Among these mental accounts is one for current income, another for voluntary saving, and a third for retirement saving. They are not fungible, so that a helicopter drop of retirement saving does not affect current saving – the marginal propensity to consume out of retirement saving is very low.

There is also the notion of hyperbolic discounting, going back to Phelps and Pollak (1968) and Laibson (1997), that people are impatient in the short run. High short-run discount rates then create the problem of lack of self-control. People who realize the difficulty of self-control then enter into binding commitments such as joining a pension scheme. In Iceland, the second-pillar pension scheme is mandatory and solves the problem of self-control, whereas the third-pillar is optional but also helps solve the same problem by committing a worker to contribute to a pension account (4%) – a contribution partially matched by the employer (2%) – every month as a default option. Apart from their pension contributions, workers are impatient and have a high propensity to consume out of current income.

Our findings may also have important policy implications. It is now widely accepted that a well-designed pension system requires a combination of public (pay-as-you-go, defined benefits) and labor market (funded, defined contributions) pensions, see, e.g., World Bank

(1994). Indeed, occupational pensions are critical to avoid a sharp fall in living standards after retirement (consumption smoothing), which requires a high level of saving before retirement. Our findings bring comforting evidence that it is in fact possible to raise aggregate saving by expanding the second pillar of the pension system.

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Appendix

A1. Detailed information on the data

Disposable income

Our measure of household income is a comprehensive one. It includes labor income, capital income, income from pension funds, government transfers, and other income such as lottery winnings or grants. Also, to make income comparable across homeowners and tenants, we add imputed rent for owner-occupied housing services to capital income.

Changes in net wealth

The tax records include information on each household's assets and liabilities. The data on assets include the market value of real estate and cars, money in savings accounts, stocks and bonds, and equity funds or mixed funds. Debt comprises mortgages, credit card debt, student loans, and other forms of debt. The Icelandic tax registries differ substantially from those of the other Nordic countries in that direct ownership of stocks is registered at nominal value rather than market value. As such, the household's level of assets is not affected by changes in the market value of stocks unless ownership of stocks is indirect through funds, in which case it is registered at market value. This facilitates the identification of household consumption and saving in years when the household does not engage in stock transactions, but it raises issues in the years when they do. This is discussed further in the following session.

Capital gains/losses

In order to derive consumption from equation (2), it is necessary to distinguish between changes in net wealth due to unrealized capital gains, which do not change current consumption but do influence net wealth, and changes in net wealth due to a household's saving some of its income, which in turn reduces current consumption. To avoid misattributing changes in market prices as saving out of income, we need to undo the contribution of unrealized capital gains/losses to net wealth.

In the Icelandic tax records, direct ownership of stocks (as opposed to ownership through mutual funds) is registered at nominal value, which corresponds to the number of shares, rather than at year-end market value as is done in many other countries. As such, changes in stock assets only occur when households engage in stock transactions. Therefore, if households do not buy or sell a part of their stock assets in a given year, their unrealized capital gains/losses in stocks are not registered and therefore do not contribute to changes in

their asset position. In this case, we do not need to resort to any assumptions to accurately identify consumption and saving despite fluctuations in stock prices. On the other hand, the fact that stock assets are registered at nominal value gives rise to complications if households engage in stock transactions in a given year, potentially leading to biased measurements of consumption and saving in those cases. However, as long as the distribution of stock transactions in our treatment group and control group is similar, active stock traders should not drive our results. Nevertheless, we show that our findings are robust to the inclusion of those who engage in stock transactions within a given year.

In contrast with direct ownership of stocks, indirect ownership of stocks and bonds through funds is registered at market value. We follow Eika et al. (2020) in measuring unrealized capital gains/losses in such assets by assuming no intra-year transactions while allowing for heterogeneous returns. However, we assume that unrealized capital gains in bonds and funds are zero for those who have such assets in a given period but did not in the previous period. In such cases, an intra-year transaction clearly took place and likely drives the change in the value of assets in money market funds.

We account for changes in the price of real estate – arguably the largest source of unrealized capital gains/losses for most households – using information on the value of each property owned by the house-hold as estimated by Registers Iceland, an institution responsible for maintaining the property register and the national register in Iceland. Furthermore, a large share of household debt in Iceland is CPI-indexed, meaning that the principal of the loans follows the consumer price index. The indexation constitutes an unrealized capital loss for some homeowners, which we account for using detailed information on the principal, installments, and interest payments on households' loans. In particular, indexation is defined as the change in a given loan's principal not explained by installments. Moreover, a 2015 mortgage relief measure constituted an unrealized capital gain for some households, for which we account. This is important because consumption and saving are derived using nominal values and then deflated by the average yearly CPI.

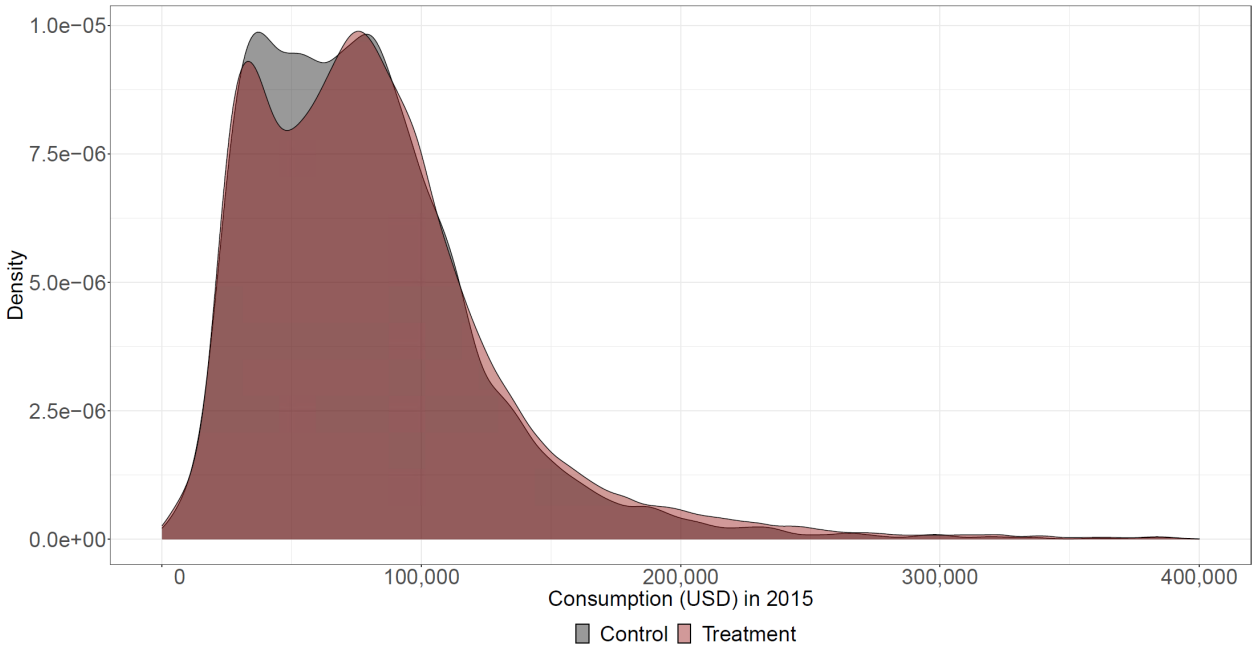
Consumption

We know from previous studies measuring consumption and saving using tax records that measured consumption is negative for some households. Although we account for several sources of unrealized capital gains, this can occur when wealth increases are misattributed to saving out of income, when they are in fact attributable to unrealized capital gains, inheritances, or gifts not observed in the tax records Kolsrud et al. (2020). To mitigate the influence of such measurement issues on our results, we condition our sample such that the saving rate is between -1 and 1. As such, negative consumption is ruled out, thus ruling

out zero or negative consumption. Finally, the purchase of real estate and vehicles does not constitute consumption according to our consumption measure but merely a portfolio rebalancing from, for example, bank deposits to real estate or vehicles. Instead, those durables are viewed as a flow of services over their lifetime, which is viewed as consumption, i.e., we view such durable assets as generating flows of consumption services until they are replaced or scrapped. The annual depreciation of vehicles is 10% according to Icelandic tax law, which we assume equals the consumption flow of vehicles.

Figure A1-1 shows the distribution of our consumption measure in 2015 for the treatment and control groups.

Figure A1-1: Density of consumption.



Notes: Figure A1-1 shows the density of consumption in 2015 for the treatment and control groups. Consumption is deflated using yearly averages of the CPI and converted to USD using the average 2019 exchange rate.

A2. Heterogeneity in the survey

Decomposing the results according to gender, age, education, and residence revealed no pattern. There was a pattern when it came to income. The higher-income respondents gave the correct answer in 40.2% of responses, while the lowest-income people gave the correct answer only 23.3% of cases, which implies that the higher the income, the more alert people are to their pension contribution.

More men than women responded that the employer contribution had increased. There is a monotonic positive relationship between age and saying the employer contribution has increased, but no systematic pattern across education groups and income groups.

The higher-income individuals appear to be more aware than the lower-income individuals. It is interesting that this applies to all education groups, i.e., university education appears not to make people more financially literate. The older groups have been more alert to the increase in recent years. More people have increased their saving than reduced it.

In the question on the employer contribution, decomposing the results according to gender, age, education, and residence revealed no pattern. There was a difference between income groups: the higher-income respondents gave the correct answer in 40.2% of responses, while the lowest-income people gave the correct answer in only 23.3% of cases, which implies that the higher the income, the more alert people are to their pension contribution. More men than women responded that the employer contribution had increased. There is a monotonic positive relationship between age and saying the employer contribution has increased, but no systematic pattern across education groups and income groups. For the third question, younger people are more prone to respond that they have increased voluntary saving, and the older groups more prone to respond that other saving is unchanged or nonexistent. Across education groups, there is a clear pattern that the higher the education category, the more people respond that they have increased other saving and the lower the proportion who answer that other saving is unchanged. There is also a clear relationship with income. The higher the income, the more people respond that they have increased their voluntary saving and the fewer who claim that they have no voluntary saving.

A3. Robustness checks

Individual fixed effects

Table A3-1 shows the robustness of our results to controlling for individual fixed effects, as opposed to including treatment group dummies as in our baseline specification. While the source of variation is at the group level, and hence the inclusion of treatment group dummies in our baseline specification, we are interested in knowing how savings changed for each worker at the onset of the treatment. The estimated crowding-out coefficients are close to zero and similar to the ones found in the baseline specification for the whole sample. The crowding-out coefficient of 0.13-0.16 is somewhat higher for single households, relative to our baseline results (0.06-0.09).

Table A3-1: Crowding-out results with individual fixed effects

	2SLS		Reduced-form		2SLS		Reduced-form	
	Whole sample				*Single households*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Crowding-	-0.034	0.053	-0.000	0.001	-0.130	-0.164	-0.002	-0.003
out	(0.163)	(0.163)	(0.002)	(0.002)	(0.176)	(0.176)	(0.003)	(0.003)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	0.062	0.063	0.062	0.063	0.001	0.104	0.104	0.104
N	520,064	520,064	520,064	520,064	161,129	161,129	161,129	161,129

Notes: Table 3-1 shows the crowding-out effects ($\hat{\rho}$) estimated using equation (10) but with controls for individual fixed effects substituted in for treatment group dummies. Columns (1) and (2) report the findings from a 2SLS estimation for the whole sample. Columns (3) and (4) report reduced-form results for the whole sample. Columns (5) and (6) report findings from a 2SLS estimation for single households only. Columns (7) and (8) report reduced-form results for single households only. The estimates are shown without controls (odd columns) and with controls (even columns) for age.

Excluding stock-trading households

We know that our measure of household consumption and saving is potentially problematic for households who engage in stock transactions within a given year. Table A3-2 reports results analogous to those reported in Table 3 except that stock-trading households are excluded. The exclusion of stock-trading households does not materially change our estimates.

Table A3-2: Crowding-out results.

	Whole sample		Single households	
	(1)	(2)	(3)	(4)
	-0.053	-0.050	0.031	-0.000
	(0.135)	(0.134)	(0.155)	(0.156)
Controls	No	Yes	No	Yes
	0.001	0.020	0.001	0.016
N	471,962	471,962	153,867	153,867

Notes: Table A3-2 shows the crowding-out effect ($\hat{\rho}$) estimated using 2SLS (see equation (10), with households who engage in stock-trading excluded from the sample. Columns (1) and (2) report estimates for the whole sample, whereas columns (3) and (4) report estimates for single households only. Estimates are shown without controls (odd columns) and with controls (even columns). The controls are dummy variables for marital status, gender, age, urban, region of residence, number of children in the household, labor income ventiles, and net wealth ventiles. Standard errors, clustered at the household level, are in parentheses.

Alternative definition of the treatment and control groups

Prior to the pension reform, the mandatory contribution rate was 12.0% in the private sector and 15.5% in the public sector. In our baseline specification, households with a mandatory saving rate below 13.75%, the mid-point between the two rates, are assigned to the treatment group and households with a mandatory saving rate above 13.75% are assigned to the control group. Here, we test the sensitivity of our results to this choice. Table A3-3 presents results with 13.5% as the critical value which assigns households into groups and Table A3-4 shows results for 14.0% serving as the critical value. The results are in line with those presented in our baseline specification.

Table A3-3: Crowding-out results.

	Whole sample		Single households	
	(1)	(2)	(3)	(4)
	0.012	0.029	-0.090	-0.148
	(0.135)	(0.135)	(0.156)	(0.156)
Controls	No	Yes	No	Yes
	0.001	0.018	0.001	0.016
N	520,064	520,064	161,129	161,129

Notes: Table A3-3 shows the crowding-out coefficient ($\hat{\rho}$) estimated using equation (10) with 13.5% serving as the critical value assigning households into treatment and control groups, rather than 13.75% as in the baseline specification. Columns (1) and (2) report estimates for the whole sample, whereas columns (3) and (4) report estimates for single households only. Estimates are shown without controls (odd columns) and with controls (even columns). The controls are dummy variables for marital status, gender, age, urban, region of residence, number of children in the household, labor income ventiles, and net wealth ventiles. Standard errors, clustered at the household level, are in parentheses.

Table A3-4: Crowding-out results.

	Whole sample		Single households	
	(1)	(2)	(3)	(4)
	0.012	0.029	-0.090	-0.148
	(0.135)	(0.135)	(0.156)	(0.156)
Controls	No	Yes	No	Yes
	0.001	0.018	0.001	0.016
N	520,064	520,064	161,129	161,129

Notes: Table A3-4 shows the crowding-out coefficient ($\hat{\rho}$) estimated using equation (10) with 14.0% serving as the critical value assigning households into treatment and control groups, rather than 13.75% as in the baseline specification. Columns (1) and (2) report estimates for the whole sample, whereas columns (3) and (4) report estimates for single households only. Estimates are shown without controls (odd columns) and with controls (even columns). The controls are dummy variables for marital status, gender, age, urban, region of residence, number of children in the household, labor income ventiles, and net wealth ventiles. Standard errors, clustered at the household level, are in parentheses.

Alternative measure of pension saving

While our main measure of mandatory pension saving has the advantage of being precisely measured, as contributions to pension funds are reported on tax returns by employers, it is somewhat simplistic because it does not take households' age and survival probabilities into account. Therefore, we define an alternative and more involved measure of pension saving that equals the change in the household's pension wealth between two consecutive periods,

$$PS_{it}^{W} = \Delta PW_{it} \tag{A3-1}$$

where PS_{it}^{W} is pension saving, defined as the yearly change in pension wealth (ΔPW_{it}).

We now turn to measuring pension wealth, which we define as the present value of the stream of income from second-pillar pension funds after retirement. Thus, it is a function of pension contributions, the age of the individual, and the individual's survival probability. Furthermore, what governs an individual's pension wealth is the stream of disposable income, and therefore we deduct tax payments on pension income, assuming an unchanged tax system as of 2020. To link pension contributions to the stream of pension income during retirement, we use entitlement tables published by each fund. The tables demonstrate the amount of pension income bought with an ISK 10,000 contribution made at a given age. Naturally, the pension income bought with a given pension contribution declines with age, as the contribution of older fund members earns returns for a shorter amount of time than that of younger fund members. In particular, since we do not have information on which

pension fund individuals and households pay into, we use the entitlement tables of Iceland's 12 largest pension which account for 97% of employees, to construct a combined entitlement table where each fund is weighted by the number of its fund members.

We then use the combined entitlement table to compute the pension income an individual is entitled to given their age and pension contribution. The yearly pension benefits individuals have already earned are then the sum of their entitled pension income to date,

$$PI^e_{it} = \sum_{\tau=1}^{t} PC_{i\tau} \frac{A_{age}}{10,000} \tag{A3-2}$$

where PI^e_{it} is the yearly pension income the individual has earned until time t, PC_{it} is the total amount of second-pillar pension contributions made on behalf of the individual in time τ , and A_{age} is the age-dependent coefficient from the combined entitlement table, which governs the rate at which an ISK 10,000 pension contribution results in pension income during retirement.

Abstracting away from uncertainty surrounding pension funds' returns and general ability to honor their obligations, the primary risk surrounding the value of pension income comes from mortality risk. Working-age individuals cannot know for how long, if at all, they will be able to claim their pension income. Therefore, we weigh the disposable pension income by age- and gender-specific survival probabilities for Iceland, which we compute using mortality rates from the Human Mortality Database.[17]

Specifically, pension wealth is computed as follows:

$$PW_{it} = \sum_{\tau=0}^{T} \frac{1}{(1+r)^t} (1-\tau_p) \times PI^e_{it} \times \sigma_{age,g,t} \tag{A3-3}$$

where T denotes the maximum lifespan, which we assume is 110 years, r is a constant discount rate assumed to equal 2% following Bönke et al. (2020), τ_p is the tax rate on pension income, and $\sigma_{age,g,t}$ is the age- and gender-specific survival probability. Finally, we use pension wealth to compute pension saving using equation (A3-1).

Table A3-5 shows that the estimated crowding-out effect using the alternative measure of pension saving is similar to the ones reported in section 5.5.

[17]The Human Mortality Human Mortality Database is a joint project of the Department of Demography at the University of California, Berkeley, and the Max Planck Institute.

Table A3-5: Crowding-out results.

	Whole sample		Single households	
	(1)	(2)	(3)	(4)
Crowding-out	-0.000	0.016	-0.052	-0.075
	(0.108)	(0.111)	(0.139)	(0.135)
Controls	No	Yes	No	Yes
	0.001	0.018	0.001	0.016
N	520,064	520,064	161,129	161,129

Notes: Table A3-5 shows the crowding-out ($\hat{\rho}$) estimated using equation (10) using an alternative measure for pension saving, which is a function of the household's age and survival probabilities. Columns (1) and (2) report estimates for the whole sample, whereas columns (3) and (4) report estimates for single households only. Estimates are shown without controls (odd columns) and with controls (even columns). The controls are dummy variables for marital status, gender, age, urban, region of residence, number of children in the household, labor income ventiles, and net wealth ventiles. Standard errors, clustered at the household level, are in parentheses.

Including households with limited ties to the labor market

As the pension reform changed the mandatory saving rate out of labor income, and thus is inherently a labor market reform, we focus our main analysis on households that are active in the labor market. We do this by omitting households whose labor income is below 50% of the median each year. Table A3-6 shows the robustness of our results to this sample definition. While the crowding-out effect is higher, approximately 0.17 for the whole sample and 0.14-0.22 for single households, it is still substantially below a full crowding-out effect.

Table A3-6: Crowding-out results.

	Whole sample		Single households	
	(1)	(2)	(3)	(4)
Crowding-out	-0.170	-0.165	-0.137	-0.217
	(0.127)	(0.127)	(0.149)	(0.149)
Controls	No	Yes	No	Yes
	0.001	0.020	0.001	0.021
N	580,825	580,825	192,214	192,214

Notes: Table A3-6 shows the crowding-out effects ($\hat{\rho}$) estimated using equation (10) including households whose labor income is below 50% of the median. Columns (1) and (2) report the findings from a 2SLS estimation for the whole sample and columns (3) and (4) report findings for single households only. The estimates are shown without controls (odd columns) and with controls (even columns). The controls are dummy variables for age (which, despite the notation, is the only characteristic not fixed at the 2015 value), marital status, gender, urban, region of residence, number of children, labor income ventiles, net wealth ventiles, and education. Standard errors, clustered at the household level, are in parentheses.

Defining treatment and control groups based on sectors

In our baseline model, we identify the treatment and control groups based on the household's mandatory saving rate out of labor income in 2015. We have information on the sectors in which the individuals work. Here we define the treatment and control groups based on this information. In particular, we assign individuals who work in a) public administration, defense, and compulsory social security, b) education, c) human health services, d) residential care and social work activities, and e) libraries, archives, museums, and other cultural activities, to the control group. The rest are assigned to the treatment group. This classification is somewhat problematic because, even though most workers in the above sectors are public sector workers, a non-negligible share of them actually work in the private sector. As such, they may be incorrectly assigned to the control group.

Table A3-7 shows the results from the first-stage regression of equation (8) for the above definition of the groups. While the mandatory saving rate of the treatment group rose significantly relative to the control group in the post-reform period, the increase is significantly lower than in our baseline specification. Table A3-8 shows the crowding-out results. Although it suggests a larger crowding-out effect than in our baseline specification, it is still well below a full crowding-out. Notably, the effect is also less precisely estimated than in our baseline specification.

Table A3-7: First-stage results.

	Whole sample		Single households	
	(1)	(2)	(3)	(4)
	0.008***	0.008***	0.013***	0.013***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)
Controls	No	Yes	No	Yes
F-statistic for weak instrument	8,112	8,388	6,911	6,976
	0.260	0.279	0.368	0.379
N	520,064	520,064	161,129	161,129

Notes: Table A3-7 reports OLS estimates of the effects of the instrument, that is the interaction of a post-reform dummy and the treatment group dummy on the mandatory saving rate (see equation (8)). Columns (1) and (2) report estimates for the whole sample, whereas columns (3) and (4) report estimates for single households only. Estimates are shown without controls (odd columns) and with controls (even columns). The controls are dummy variables for marital status, gender, age, urban, region of residence, number of children in the household, labor income ventiles, and net wealth ventiles. Standard errors, clustered at the household level, are in parentheses.

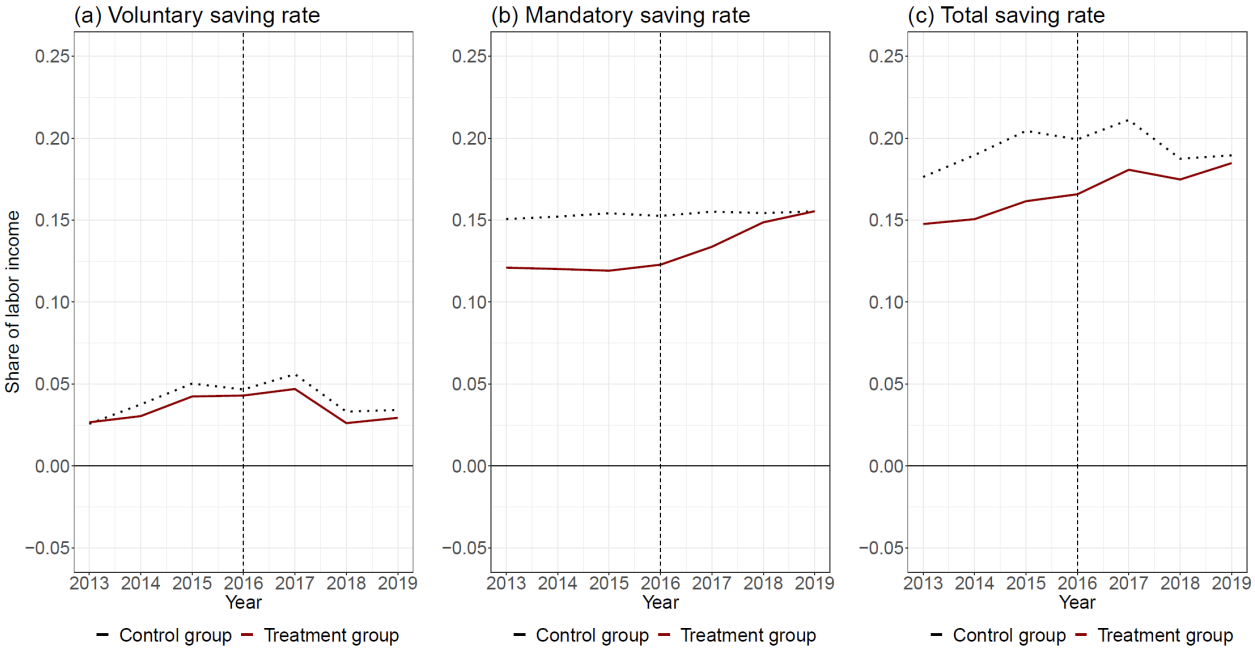
Table A3-8: Crowding-out results.

	Whole sample		Single households	
	(1)	(2)	(3)	(4)
	-0.132	-0.272	0.143	-0.032
	(0.194)	(0.194)	(0.225)	(0.224)
Controls	No	Yes	No	Yes
	0.001	0.018	0.001	0.016
N	520,064	520,064	161,129	161,129

Notes: Table A3-8 shows the crowding-out coefficient ($\hat{\rho}$) estimated using equation (10) using an alternative measure for pension saving, which is a function of the household's age and survival probabilities. Columns (1) and (2) report estimates for the whole sample, whereas columns (3) and (4) report estimates for single households only. Estimates are shown without controls (odd columns) and with controls (even columns). The controls are dummy variables for marital status, gender, age, urban, region of residence, number of children in the household, labor income ventiles, and net wealth ventiles. Standard errors, clustered at the household level, are in parentheses.

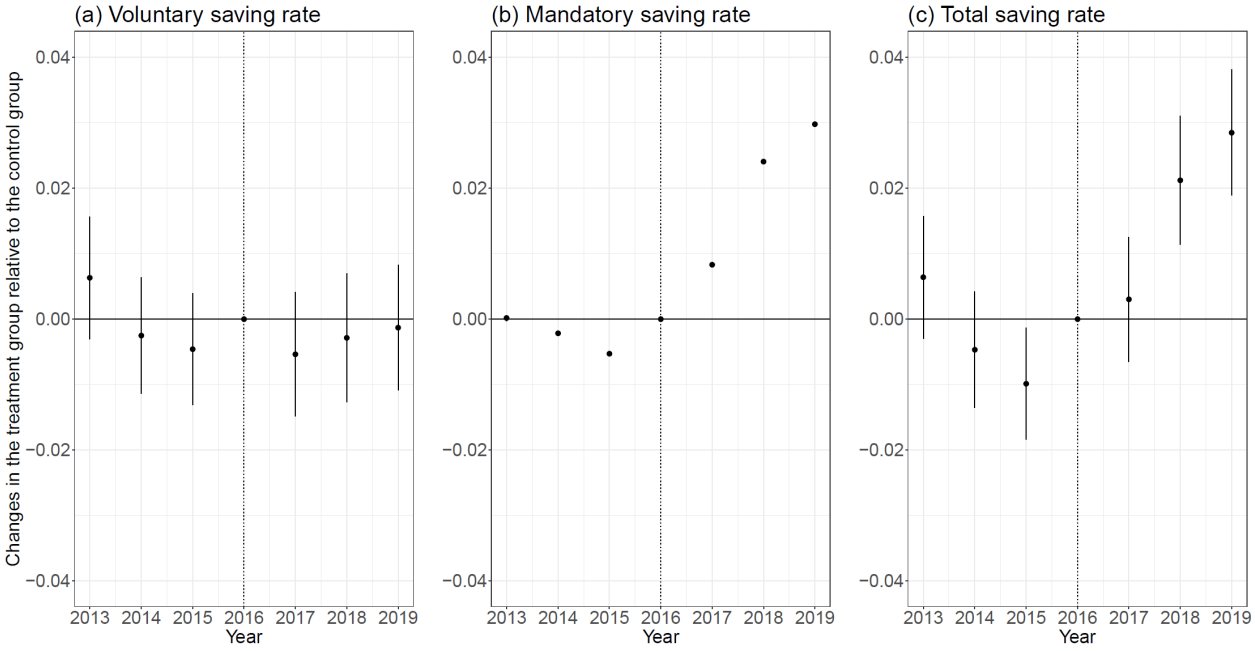
A4. Average saving rates and parallel trends for single households

Figure A5-1: Average saving rates for single households.



Notes: Figure A5-1 shows the average voluntary (panel a), mandatory (panel b), and total (panel c) saving rates out of household wages for the control group (dotted black line) and the treatment group (solid red line) as measured by fitted values from three regressions where each of the aforementioned variables is regressed on year fixed effects, group fixed effects, and the interaction between the two. The sample is restricted to single households, i.e. individuals who do not have a spouse. The dotted vertical line in 2016 shows when the first stage of the reform was implemented.

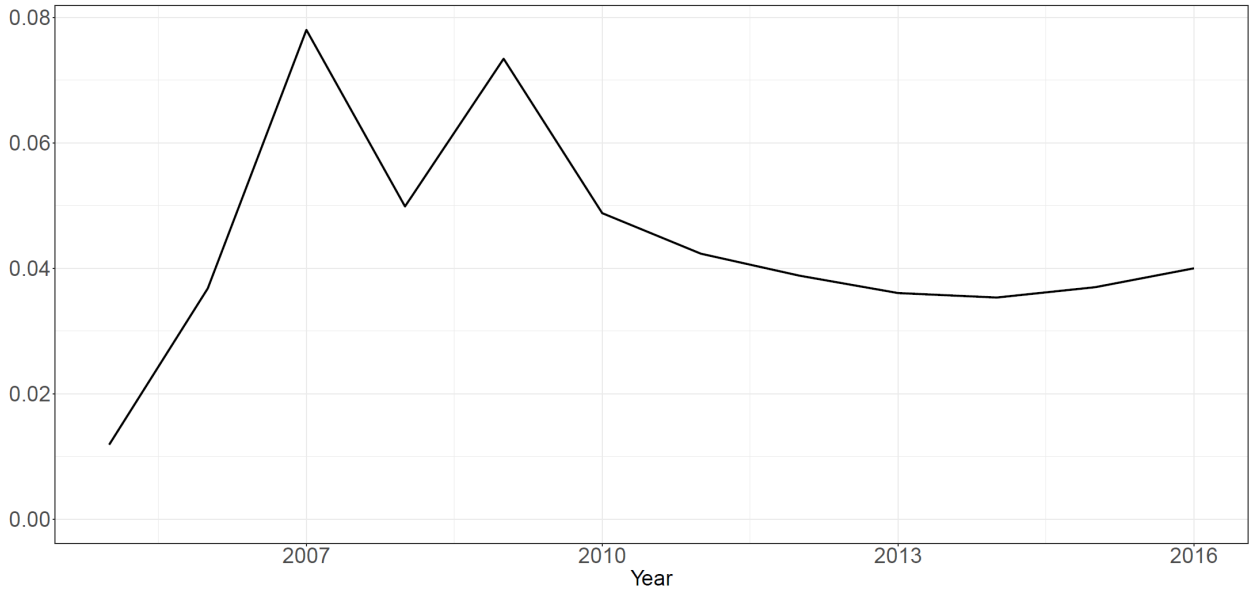
Figure A5-2: Average saving rates for single households.



Notes: Panel (a) of Figure A5-2 plots the estimated γ_t from equation (11) for single households only. In panels (b) and (c), the dependent variable has been replaced by the mandatory saving rate and the total saving rate, respectively. The dotted vertical line in 2016 shows when the first stage of the reform was implemented. The controls are dummies for age (which, despite the notation, is the only characteristic not fixed at the 2015 value), marital status, gender, urban, region of residence, number of children, labor income ventiles, net wealth ventiles, and education. 95% confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines.

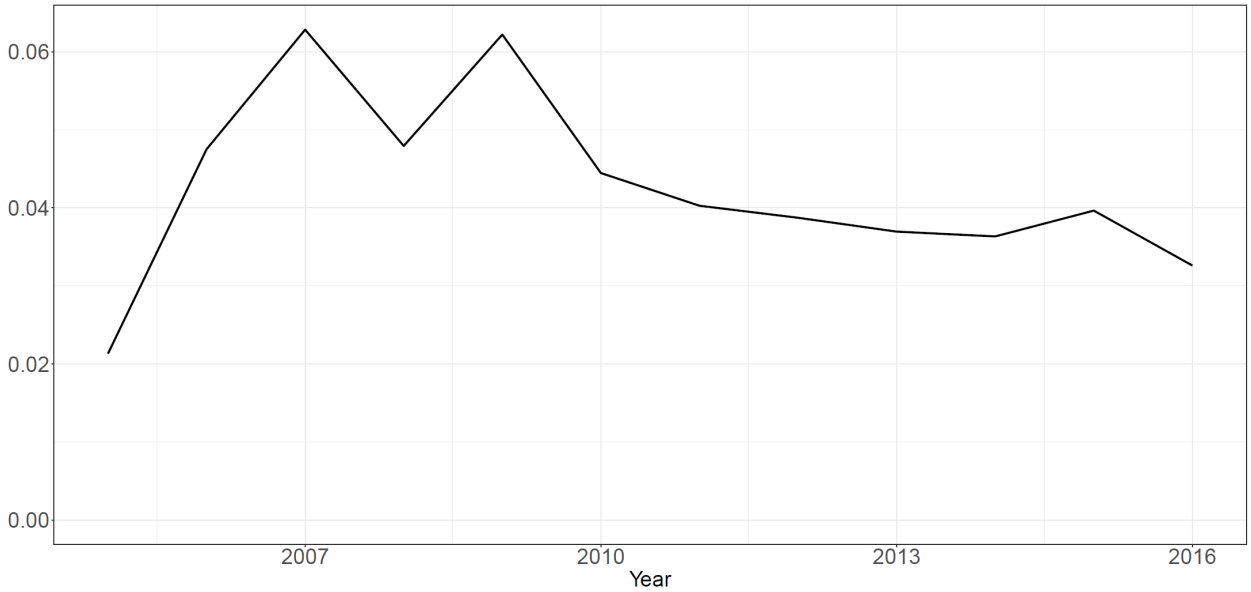
A5. The rate of job switches

Figure A6-1: The rate of job switches from the private sector to the public sector.



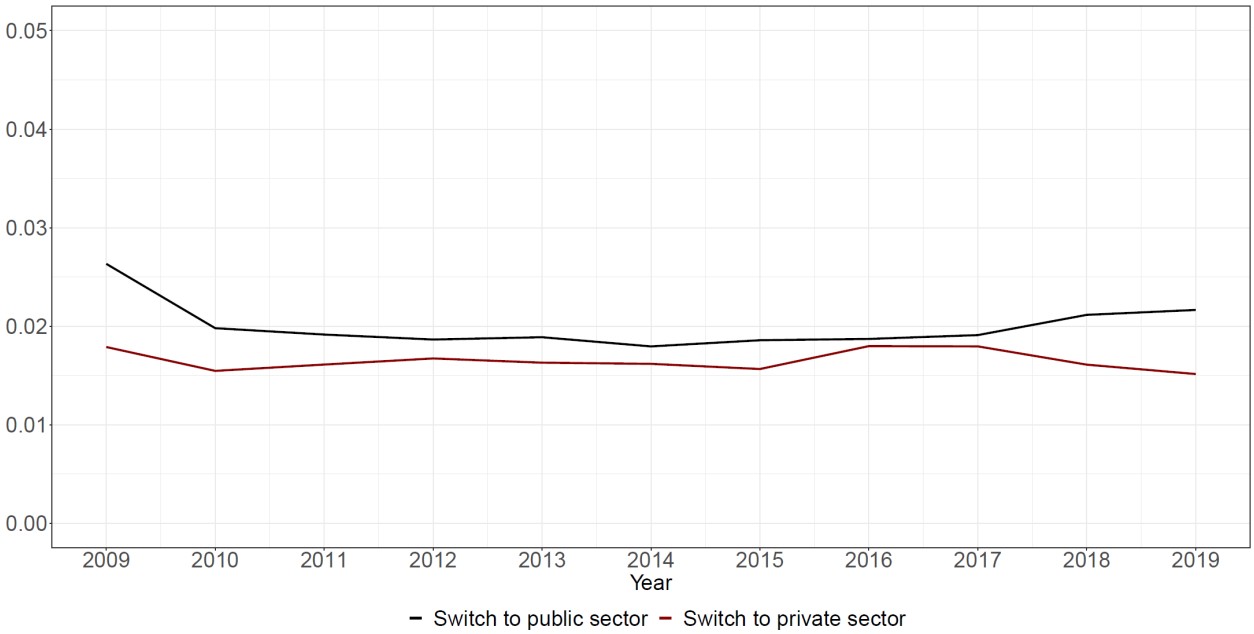
Notes: Figure A6-1 shows the rate of job switches from the private sector to the public sector in the whole sample. A switch is assumed to have taken place if the contribution rate is above 13.75% in the current period but was below 13.75% in the previous period and that the jump in the contribution rate is at least 0.5pp.

Figure A6-2: The rate of job switches from the private sector to the public sector among single households.



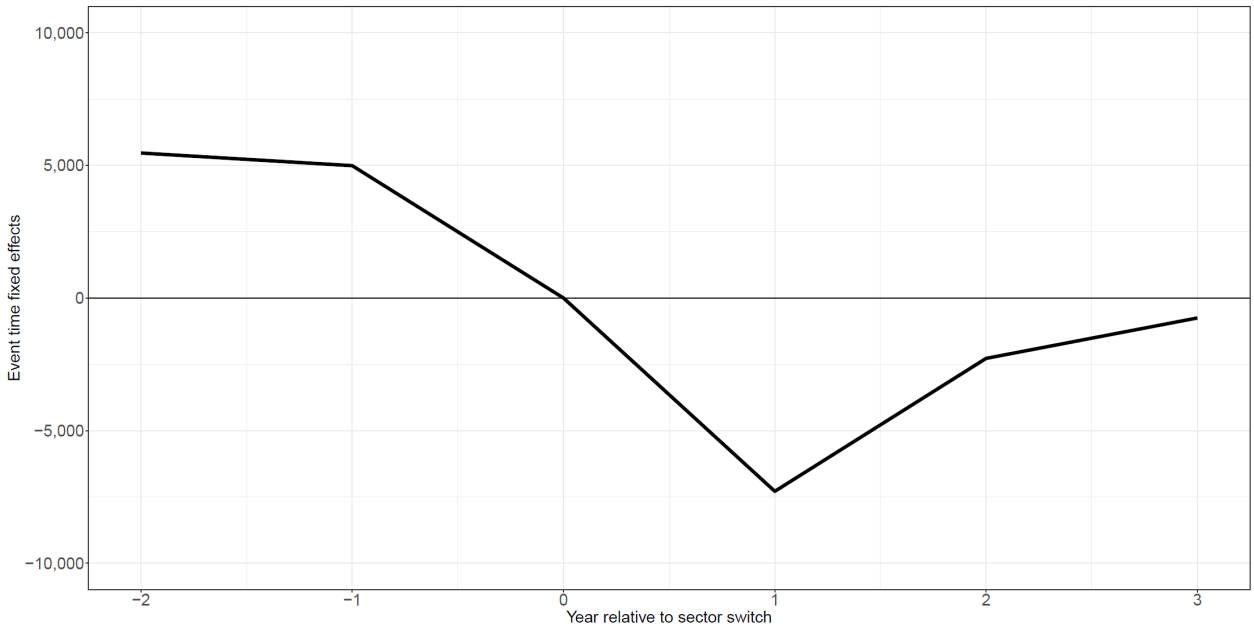
Notes: Figure A6-2 shows the rate of job switches from the private sector to the public sector in the whole sample. A switch is assumed to have taken place if the contribution rate is above 13.75% in the current period but was below 13.75% in the previous period and that the jump in the contribution rate is at least 0.5pp.

Figure A6-3: The rate of job switches from the private sector to the public sector based on sector classification.



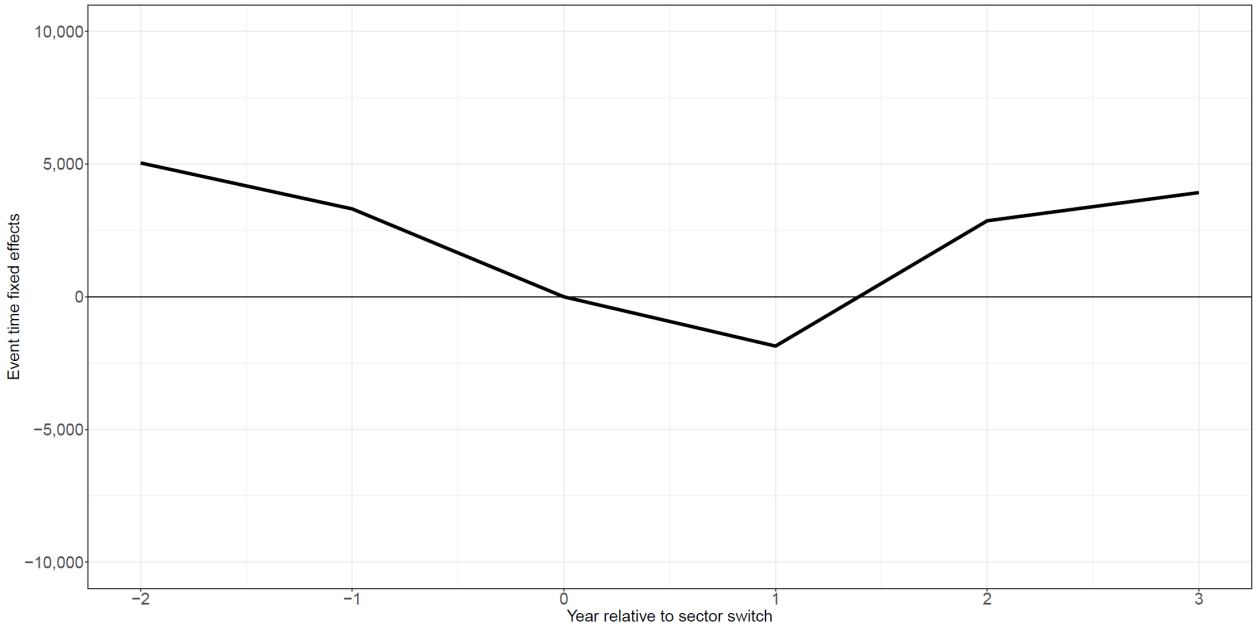
Notes: Figure A6-3 shows the rate of job switches from the private sector to the public sector in the whole sample. The public sector is defined as a) public administration, defense, and compulsory social security, b) education, c) human health services, d) residential care and social work activities, and e) libraries, archives, museums, and other cultural activities. Other sectors are classified as the private sector. A switch is assumed to have taken place if a worker is in one sector in the current period but was in the other sector in the previous period.

Figure A6-4: Wage development surrounding a job switch.



Notes: Figure A6-4 plots the estimated $\alpha_{et,i}$ from the equation $Y_{i,t} = \alpha + \alpha_{et,i} + \varepsilon_{i,t}$ for the whole sample, where $Y_{i,t}$ is wage income, deflated using the CPI, and $\alpha_{et,i}$ is event time fixed effects. The sample consists of 100,642 observations of 22,277 households that moved from the private sector to the public sector once and only once in 2004-2016.

Figure A6-5: Wage development surrounding a job switch.



Notes: Figure A6-5 plots the estimated $\alpha_{et,i}$ from the equation $Y_{i,t} = \alpha + \alpha_{et,i} + \varepsilon_{i,t}$ for single job switchers only, where $Y_{i,t}$ is wage income, deflated using the CPI, and $\alpha_{et,i}$ is event time fixed effects. The sample consists of 30,734 observations of 9,264 single individuals that moved from the private sector to the public sector once and only once in 2004-2016.

WORKING PAPER
2023|92