Essays on Household Saving and Pension Reform

Olafsson, Sigurdur P.

Document Version
Final published version

DOI:
10.22439/phd.37.2024

Publication date:
2024

License
Unspecified

Citation for published version (APA):
Olafsson, S. P. (2024). *Essays on Household Saving and Pension Reform*. Copenhagen Business School [Phd]. PhD Series No. 37.2024 https://doi.org/10.22439/phd.37.2024

COPENHAGEN BUSINESS SCHOOL

Solbjerg Plads 3
DK-2000 Frederiksberg
Danmark

www.cbs.dk

ISSN 0906-6934

Print ISBN: 978-87-7568-305-5
Online ISBN: 978-87-7568-306-2

ESSAYS ON HOUSEHOLD SAVING AND PENSION REFORM

PhD Series 37-2024

CBS PhD School
Department of Economics

PhD Series 37.2024

SIGURÐUR PÁLL ÓLAFSSON

ESSAYS ON HOUSEHOLD SAVING AND PENSION REFORM

CBS

Doctoral Thesis in Economics

Essays on Household Saving and Pension Reform

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Essays on Household Saving and Pension Reform

First edition 2024
Ph.D. Serie 37.2024

ISSN 0906-6934

Print ISBN: 978-87-7568-305-5
Online ISBN: 978-87-7568-306-2

DOI: https://doi.org/10.22439/phd.37.2024

Acknowledgements

This dissertation marks the end of my PhD studies at the Department of Economics at Copenhagen Business School (CBS) as a member of the Pension Research Centre (PeRCent). As I am applying the finishing touches to the thesis, I'm reminded of the people that made it possible for me to pursue a PhD degree. This thesis is the fruit of inspiring conversations, valuable guidance, and endless support.

First, I owe immense gratitude to my supervisors and co-authors. Svend E. Hougaard Jensen, my primary supervisor, has been pivotal through his mentorship. Our discussions not only shaped my research agenda but also prepared me for presenting at numerous workshops and conferences. Svend's ability to always find time, often on a short notice within a busy schedule, to discuss solutions to the various challenges I have faced during my PhD studies has set a standard I aspire to emulate. None of this would have happened without Gylfi Zoëga who informed me of the possibility of going to Copenhagen Business School for my PhD studies. Gylfi has proven to be an excellent supervisor. His profound interest in my work, encouragement and ability to break down complex problems are unmatched. Herdís Steingrímsdóttir has proved invaluable, especially at the latter stages of my PhD studies. Conversations with her about my papers have been immensely helpful. My other two coauthors, Thorsteinn Sigurdur Sveinsson and Arnaldur Smári Stefánsson, from both of whom I have learnt a lot, also deserve a lot of credit.

Second, I would like to express my gratitude to my coworkers at the Economics Department at the Ministry of Finance and Economic Affairs. Countless conversations with them on economic affairs have been an endless source of ideas and motivation. They have also provided important insights and feedback through the course of my studies.

Third, during my time at CBS and at the University of Iceland, I was privileged to be part of a group of excellent PhD students. Conversations with them about economics and research over a glass of water were important. Conversations with them about anything else over a glass of something a bit stronger, even more so. Moreover, I am thankful for the Pension Research Centre (PeRCent) and its members for creating a fruitful research environment and the Icelandic Research Fund (Rannís) for funding and making this project possible.

Finally, and most importantly, I would like to thank my family for unwavering support. Both my parents and my wife Steinunn have taken on various additional responsibilities to support my studies. There would have been no way of doing this without them.

Summary

This thesis consists of three chapters on topics related to household consumption and saving. Underlying all papers is the computation of household-level consumption and saving using a detailed third-party reported administrative dataset from Icelandic tax records. The first chapter of the thesis studies households' consumption response to past experiences of economic hardship. The second chapter estimates a life-cycle consumption profile for three education groups and proceeds to estimate their consumption response out of transitory income shock over a period of dramatic economic fluctuations. Finally, the third chapter asks whether raising mandatory pension saving leads households to reduce their voluntary saving as predicted by canonical economic models.

Chapter 1

The first chapter is titled *Do Past Experiences of Economic Troubles Reduce Household Consumption?*. It is motivated by the sharp increase in national saving and a persistent improvement in Iceland's current account following the global financial crisis and the collapse of the country's banking system. Against this background, I develop a simple model framework to explain how seemingly transitory shocks can persistently affect households' consumption and saving behavior. The idea is that households' consumption and saving behavior are a function of the risk they face. However, past experiences might influence how households perceive those risks. Large negative shocks, in particular, might be salient enough to affect households' beliefs for years to come. I then use administrative panel microdata of Icelandic taxpayers to show that households experiencing unemployment spells subsequently reduce their consumption. What is more, households that have merely observed and lived through periods of heightened job-loss risk, without actual job loss, subsequently reduce consumption to a similar extent to those who experience job loss. The persistent reduction in consumption following periods of heightened job-loss risk leads to an accumulation of wealth, a higher national saving rate, and, thus, an improved current account balance. The magnitude of the results suggest that merely the risk of job loss experienced during the GFC/banking collapse, when average job-loss risk rose by approximately 10%, provided an economically meaningful drag on demand during the subsequent economic recovery.

Chapter 2

The second chapter is titled *Education and consumption smoothing* and is co-authored with Svend E. Hougaard Jensen, Thorsteinn Sigurdur Sveinsson, and Gylfi Zoëga. In this chapter, we study the marginal propensity to consume (MPC) of three groups of households with different levels of educational attainment - primary, secondary, and tertiary (university-level) education. It is motivated by recent research on the MPC and fiscal multipliers over the economic cycle, generally

finding a relatively high multiplier during economic downturns compared to when the economy operates close to or above capacity. Our data allows for a deeper dive into how the MPC differs across several socioeconomic characteristics compared to many other studies. Furthermore, our sample period covers a dramatic boom-bust-recovery cycle, ideal for studying how the MPC varies over the cycle. Our contribution stems from merging the detailed data with the unique sample period and a survey on consumption behavior, to study how the MPC varies over the economic cycle and for various subgroups. We find that higher education is associated with a lower marginal propensity to consume (MPC) due to higher disposable income and more liquidity. Moreover, the MPC out of positive idiosyncratic income shocks is higher than that out of negative shocks. Finally, we find a hump-shaped consumption profile, which is most prominent for higher educated households, as consumption follows income over the life cycle.

Chapter 3

The third chapter is titled *Does Mandatory Saving Crowd out Voluntary Saving? Evidence from a Pension Reform* and is co-authored with Svend E. Hougaard Jensen, Arnaldur Smari Stefansson, Thorsteinn Sigurdur Sveinsson, and Gylfi Zoëga. It is motivated by the rapid falls in both death rates and birth rates, together resulting in an unprecedented aging of Western populations. This has led governments to implement various pension reforms to ensure both fiscal sustainability and adequate incomes during retirement as the old age dependency ratio increases. In the chapter, we study an interesting such reform in Iceland, although its objective was to equalize pension rights and not raise retirement savings. The reform raised the mandatory saving rate of private sector workers in 2016-2018 while that of public sector workers remained unchanged. This serves as a quasi-natural experiment in mandatory saving, with private sector workers serving as a natural treatment group and public sector workers as a natural control group, thus providing a unique setting for studying crowding-out of mandatory saving. Our results, supported also by an event study of workers switching jobs from the low-mandatory-saving private sector to the high-mandatory-saving public sector, suggest minimal crowding-out. Finally, survey evidence suggest widespread ignorance about pension savings and the pension system, more generally.

Resumé

Denne afhandling består af tre kapitler om emner i tilknytning til husholdningers forbrug og opsparing. Et gennemgående tema i alle kapitlerne er forbrug og opsparing på husholdningsniveau ved hjælp af et detaljerede mikrodata baseret på islandske skatteregistre. Det første kapitel i afhandlingen undersøger, hvordan husholdningers forbrugsefterspørgsel reagerer på tidligere økonomiske tilbageslag. Det andet kapitel estimerer først en livscyklus forbrugsprofil for husholdninger med forskellig uddannelsesmæssig baggrund og dernæst deres forbrugsreaktion på et forbigående stød til deres indkomst over en periode med dramatiske økonomiske udsving. Endelig adresserer det tredje kapitel spørgsmålet, om en forhøjelse af den obligatoriske pensionsopsparing fører til, at husholdninger reducerer deres frivillige opsparing. I det følgende præsenteres et lidt mere detaljeret sammendrag af de tre kapitler.

Kapitel 1

Det første kapitel har titlen *"Do Past Experiences of Economic Hardship Reduce Household Consumption?"* (*"Fører en tidligere økonomisk nedtur til et fald i husholdningernes forbrug?"*). Det er motiveret af den skarpe stigning i den nationale opsparing og en vedvarende forbedring af Islands betalingsbalance efter den globale finanskrise og sammenbruddet af landets banksystem. På baggrund af dette udvikler jeg en simpel model til at forklare, hvordan tilsyneladende forbigående chok kan påvirke husholdningernes forbrugs- og opsparingsadfærd vedvarende. Idéen er, at husholdningers forbrugs- og opsparingsadfærd er en funktion af den risiko, de står overfor. Tidligere begivenheder kan dog påvirke, hvordan husholdninger opfatter disse risici. Især kan store negative chok være markante nok til at påvirke husholdningers perception i mange år fremover. Jeg anvender derefter administrative panel-mikrodata fra islandske skatteydere til at vise, at husholdninger, der oplever perioder med arbejdsløshed, efterfølgende reducerer deres forbrug. Derudover finder jeg, at husholdninger, der blot har observeret og levet igennem perioder med øget risiko for arbejdsløshed, men uden faktisk at have oplevet arbejdsløshed, ligeledes reducerer forbruget i en lignende grad som dem, der oplever arbejdsløshed. Den vedvarende reduktion i forbruget efter perioder med øget risiko for arbejdsløshed fører til en akkumulering af formue, en højere national opsparingsrate og dermed en stærkere betalingsbalance. Resultaterne tyder på, at blot risikoen for arbejdsløshed oplevet under banksammenbruddet i Island, da den gennemsnitlige risiko for jobtab steg med ca. 10%, har afstedkommet et betydeligt fald i efterspørgslen under det efterfølgende økonomiske opsving.

Kapitel 2

Det andet kapitel med titlen *"Education and Consumption Smoothing"* (*"Uddannelse og forbrugsudjævning"*) er skrevet sammen med Svend E. Hougaard Jensen, Thorsteinn Sigurdur Sveinsson og Gylfi Zoega. I dette kapitel undersøger vi den marginale forbrugstilbøjelighed

(MPC) hos tre grupper af husholdninger med forskellige uddannelsesniveauer - primær, sekundær og tertiær (universitetsniveau). Det er motiveret af nyere forskning om MPC og finanspolitiske multiplikatorer på tværs af økonomiske konjunkturer. Typisk finder forskningen en relativt høj multiplikator under økonomiske nedgangstider sammenlignet med, når økonomien fungerer tæt på eller over fuld kapacitetsudnyttelse. Vores data muliggør et dybere studie i, hvordan MPC varierer på tværs af flere socioøkonomiske karakteristika sammenlignet med mange andre studier. Desuden dækker vores sample-periode betydelige op- og nedture i økonomien, hvilket er ideelt til at studere, hvordan MPC varierer over konjunkturen. Vores bidrag bygger på en sammenfletning af de detaljerede data med den unikke sample-periode og et survey af forbrugeradfærd, for at studere hvordan MPC varierer over den økonomiske konjunktur og for forskellige undergrupper. Vi finder, at højere uddannelse er forbundet med en lavere marginal forbrugstilbøjelighed (MPC) på grund af højere disponibel indkomst og mere likviditet. Desuden er MPC ud af positive idiosynkratiske indkomstchok højere end, hvad vi finder for negative chok. Endelig finder vi en knækformet forbrugsprofil, som er mest fremtrædende for højtuddannede, da forbruget følger indkomsten over livsforløbet.

Kapitel 3

Det tredje kapitel har titlen *"Does Mandatory Saving Crowd out Voluntary Saving? Evidence from a Pension Reform"*(*"Fortrænger obligatorisk opsparing frivillig opsparing? Evidens fra en pensionsreform"*) og er skrevet i samarbejde med Svend E. Hougaard Jensen, Arnaldur Smari Stefansson, Thorsteinn Sigurdur Sveinsson og Gylfi Zoega. Det er motiveret af de hurtige fald i både døds- og fødselsrater, som samlet resulterer i en hidtil uset aldring af vestlige befolkninger. Dette har fået regeringer til at gennemføre forskellige pensionsreformer for at sikre både finansiel bæredygtighed og tilstrækkelige indkomster under pensionering, da den såkaldte demografiske forsørgerbyrde stiger. I kapitlet undersøger vi en reform i Island, selvom formålet med denne var at udligne pensionsrettigheder og ikke at øge pensionsopsparingen. Reformen hævede den obligatoriske opsparingsrate for arbejdere i den private sektor i 2016-2018, mens den forblev uændret for offentlige sektorarbejdere. Dette fungerer som et kvasi-naturligt eksperiment med hensyn til obligatorisk opsparing, med private sektorarbejdere som en naturlig treatment gruppe og offentlige sektorarbejdere som en naturlig kontrolgruppe, hvilket giver en unik ramme for at studere fortrængning af obligatorisk opsparing. Vores resultater bliver også understøttet af en event-studie af personer, der skifter job fra den private sektor med lav obligatorisk opsparing til den offentlige sektor med høj obligatorisk opsparing. Endelig tyder survey evidens på udbredt uvidenhed om pensionsopsparing og pensionssystemet generelt.

Contents

Acknowledgements **i**

Summary **iii**

Resumé **vi**

Contents **ix**

Introduction **1**

Chapter 1: Do Past Experiences of Economic Hardship Reduce Household Consumption? **7**

1. Introduction 9
2. Literature 13
3. A Conceptual Framework for Precautionary Saving 15
4. Data 19
5. The Persistent Effect of Uncertainty 27
6. Discussion & Conclusion 43

References 47

Appendix 49

Chapter 2: Education and Consumption Smoothing **56**

1. Introduction 58
2. The Context of the Paper 60
3. Data 63
4. Life-cycle Patterns of Consumption 66
5. Consumption Smoothing by Education 72
6. Further Perspectives based on Survey Evidence 79
7. Concluding Remarks 80

References 83

Appendix 85

Chapter 3: Does Mandatory Saving Crowd out Voluntary Saving? Evidence from a Pension Reform **104**

1. Introduction 106
2. Literature 107
3. Institutional Setting 111
4. Data 112

5. Empirical Framework and Results 117
6. Away from the Reform - Evidence from Job Switchers 130
7. From Hard Data to Soft Statistics 135
8. Concluding Remarks 139
References 141

Introduction

This dissertation consists of three self-contained chapters on household consumption and saving. All chapters deal with household consumption and saving behavior once faced with some kinds of shocks. They are also all based on the same methodology, first proposed by Browning and Leth-Petersen (2003) and later implemented by for example Eika et al. (2020) and Kolsrud et al. (2020), for computing household-level consumption and saving using third-party reported administrative data from Icelandic tax records, they can each be read independently.

The first chapter asks if households who have witnessed economic turbulence and lived through periods of economic hardship in which the probability of losing ones job is high, subsequently and persistently raise their precautionary savings. I am motivated by two strands of literature. First, severe financial and banking crises tend to be both long-lasting and followed by a sustained improvement in current account balances. Zoega (2021) has traced this to an increase in national saving following crisis episodes. Second, there is a literature on experience effects showing that salient events can influence households' beliefs and behavior for years. Examples are Malmendier and Shen (2024) who use survey data to illustrate that households who have experienced a high local unemployment rate spend significantly less than others and Malmendier and Nagel (2011) who find that individuals who lived through the Great Depression are less likely than others to take financial risks and participate in the stock market later in life.

This resonates well with Iceland's experience after the global financial which hit the country particularly hard. Iceland's current account, which had been in large deficits for years, turned into persistent surpluses. This was driven by a persistent change in household consumption behavior, characterized by greater prudence and a stronger propensity to save. Central Bank of Iceland (2018) traces this to "deep and long-lasting behavioral changes prompted by the traumatic experience of the financial crisis."

The key contribution of the chapter stems from identifying experience effects, that is the effect of past job-loss risk on current consumption using a detailed administrative dataset and within the unique context of an advanced economy that has experienced wild fluctuations over the sample period of 2004-2019. I find that households that have merely observed and lived through periods of heightened job-loss risk, without actual job loss, subsequently reduce consumption. The experience effects are statistically significant and economically meaningful. As such, merely the risk of job loss experienced during the GFC was a drag on demand during the subsequent economic recovery.

Chapter 2, which is coauthored with Svend E. Hougaard Jensen, Thorsteinn Sigurdur Sveinsson and Gylfi Zoega, studies consumption behavior by education group in Iceland in 2005-2019, a turbulent period with dramatic fluctuations in factor incomes before, during and after the collapse of the country's banking system. The chapter is motivated by recent papers showing that low-income, low-education and hand-to-mouth households demonstrate the largest response to income changes induced by monetary and fiscal innovations, see for example

Ampudia et al. (2018) and Guo et al. (2023). It combines a detailed dataset with various information on household characteristics with a sample period which covers a dramatic boom-bust-recovery cycle, ideal for studying how the MPC varies over the cycle. This allows us to study how the MPC varies over the economic cycle and for various subgroups.

Overall, we find that higher education is associated with a lower marginal propensity to consume (MPC) due to higher disposable income and more liquidity. However, for all education groups, the MPC is much higher than the close-to-zero MPC predicted by canonical theories of consumption and saving over the life cycle (Friedman, 1957; Ando and Modigliani, 1963). This is a common finding in more recent contribution, which has been attributed to liquidity constraints (Zeldes, 1989; Gross and Souleles, 2002) and buffer-stock saving behavior (Carroll, 2001).

As opposed to many other studies that estimate MPCs out of windfall income (see for example Fagereng et al. (2021)), we are able to identify both positive and negative income shocks. Studying the asymmetry in households' consumption response to income shocks is crucial to learn about how households respond to different shocks, which can have implications for how the MPC evolves over the economic cycle and the effectiveness of monetary and fiscal policy to influence demand. We find that the MPC out of negative idiosyncratic income shocks is higher than that out of positive shocks Furthermore, studying the time variation in the MPC, we show that the MPC rose markedly across education groups in the aftermath of the global financial crisis before normalizing again as the economic recovery took hold. This is driven by a particularly strong consumption response to negative income shock, with households tending to cut their consumption approximately one-to-one. The MPC out of positive income shocks also rose during the crisis albeit to a more limited extent. Therefore, households raised their consumption slightly more when faced with a positive shock during the crisis compared to when the economy operated closer to equilibrium or during economic expansions.

We also find a hump-shaped consumption profile, which is most prominent for higher educated households, as consumption follows income over the life cycle. In this regard, our findings are in line with those of Gourinchas and Parker (2002) and Fernández-Villaverde and Krueger (2007) who find a more prominent hump in the life-cycle consumption profiles of workers with high education compared to their less-educated counterparts.

Finally, the last chapter, which is coauthored with Svend E. Hougaard Jensen, Arnaldur Smari Stefansson, Thorsteinn Sigurdur Sveinsson and Gylfi Zoega asks whether mandatory saving crowds out voluntary saving and, if so, to what extent. While the literature on crowding-out of retirement savings dates back to Feldstein (1974), it is still inconclusive. Recent contributions from the Nordics include Chetty et al. (2014) who find that only around 15 percent of individuals respond to changes to pension contribution rates, Arnberg and Barslund (2014) whi find a crowding-out effect of mandatory saving on private saving ranging from 0-30 percent, and Christensen and Ellegaard (2023) who finds a crowding-out effect of 64 percent for middle-income households.

The chapter is motivated by the fact that as populations age and large cohorts are entering retirement age, a significant share of households do not possess adequate savings to support consumption during retirement (Poterba, 2014). Governments have responded by reforming pension systems to ensure adequate retirement incomes within the context of fiscal sustainability. We contribute to the literature by studying a 2016-2018 pension reform in Iceland which raised mandatory pension saving rates for private sector households while that of public sector households remained unchanged using a detailed administrative dataset. This serves as a quasi-natural experiment in mandatory saving, with private sector workers serving as a natural treatment group and public sector workers as a natural control group, thus, providing a unique setting for studying crowding-out of mandatory saving. Furthermore, we deepen our analysis by utilizing job switches from the low-mandatory-saving private sector to the high-mandatory-saving public sector and by gauging households' awareness of the pension reform and the pension system more generally using a survey. The results suggest minimal crowding-out and widespread ignorance about pension savings.

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Chapter 1

Do Past Experiences of Economic Troubles Reduce Household Consumption?

Do Past Experiences of Economic Hardship Reduce Household Consumption?*

Sigurdur P. Olafsson †

June 28, 2024

Abstract

Using administrative panel microdata of Icelandic taxpayers, I show that households experiencing unemployment spells subsequently reduce their consumption. What is more, to a similar extent to those who experience job loss. Since these households do not suffer an income loss, their reduced consumption leads to an accumulation of wealth following periods of heightened job-loss risk. The sample period, which includes remarkable variation in incomes, unemployment rates and other economic outcomes, along with the comprehensive dataset, offers a unique opportunity to study the effects of past experiences on current consumption. Using information on individuals' characteristics, I estimate individual-level job-loss risk and show how experiences of such risk persistently affects consumption. Specifically, a one percentage point increase in past exposure to job-loss risk reduces current consumption by 0.09-0.16%. In comparison, the average job-loss risk rose by approximately 10% in Iceland in the wake of the GFC. As such, merely the risk of job loss experienced during the GFC was an economically meaningful drag on demand during the subsequent economic recovery.

Keywords: Precautionary Saving, Job-Loss Risk, Experience Effects, Administrative Microdata

*I would like to thank Svend Erik Hougaard Jensen, Arnaldur Smári Stefánsson, Þorsteinn Sigurður Sveinsson, Gylfi Zoega, participants at the Workshop for Consumption and Saving over the Life Cycle, and seminar participants at the University of Iceland for helpful comments. This study was supported by the Icelandic Research Fund (Rannís), grant number: 207213-051, and PeRCent, which receives base funding from the Danish pension funds and Copenhagen Business School. The views expressed herein are those of the author and do not necessarily reflect the views or policies of any entities I represent.

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1 Introduction

Severe financial and banking crises tend to be both long-lasting and followed by a sustained improvement in current account balances. The latter has been traced to an increase in national saving following crisis episodes (Zoega, 2021). This was certainly the case in Iceland after the global financial crisis (GFC) and economic mismanagement at home had brought the country's entire banking system to its knees in 2008. Iceland's current account, which had been in large deficits for years, turned into persistent surpluses. This was driven by a persistent change in household consumption behavior. The share of private consumption in GDP had been approximately 57% in the years leading up to the GFC/banking collapse. The same ratio hovered around 50% in the years leading up to the COVID-19 pandemic. The Central Bank of Iceland (2018) explains this by greater prudence and a stronger propensity to save due to "deep and long-lasting behavioral changes prompted by the traumatic experience of the financial crisis." Such dynamics in the wake of economic crises are by no means unique to Iceland. Examples can be found in the Nordics following the banking crisis in the early 1990s, in Southeast Asia following the 1997 Asian Financial Crisis, and in Iceland, the Baltic countries, Spain and Portugal following the GFC, to name a few.

Those past experiences of many countries following economic hardship motivates the research question: Do households who have witnessed economic turbulence and lived through periods of elevated risk subsequently and persistently raise their precautionary savings? In this paper, I argue that consumption is a function of households' beliefs about macroeconomic risk, of which job-loss risk is arguably most important for most households. Having lived through the Great Moderation for years, households, perhaps particularly those cohorts who have limited or no memories of previous economic fluctuations, might make their consumption and saving decision on the premise that they faced relatively limited risk to their income and labor market outcomes. Once those same households have experienced large shocks such as the GFC, and in Iceland the collapse of the country's entire banking system, they may persistently, or even permanently, have changed their beliefs about the risk they face. Obviously, the COVID-19 pandemic is potentially another such drastic event. I argue that households who have in the past experienced times of elevated job-loss risk, even without necessarily becoming unemployed, persistently change their beliefs about future economic outcomes. Those changed beliefs lead households to raise their precautionary savings and, thus, reduce consumption and accumulate more buffer-stock wealth as insurance against those possible outcomes.

I use administrative third-party reported microdata from tax records of all taxpayers in Iceland, combined with other administrative data on various socio-economic factors to construct yearly individual-level job-loss probabilities using several background characteristics. I then relate past values of this job-loss risk measure to current consumption. In a similar vein, I also relate past experiences of actual job-loss to current consumption.

Before proceeding further, it is useful to address some terminology. Experience effects refer

to the effect of past experiences on current consumption. This paper deals with two types of experience effects. First, risk experience effects, or simply risk effects, which refer to the effects of past job-loss risk on current consumption. Second, personal unemployment experience effects, personal experience effects, or unemployment effects refer to the effects of past unemployment spells on current consumption.

As discussed further in Section 2, this is not the first paper to highlight the relationship between consumption dynamics and past economic experiences. In particular, a recent paper by Malmendier and Shen (2024) finds that households who have experienced high unemployment in their area of residence, a proxy for labor market risk, or experienced unemployment first hand in the past, spend significantly less than others.

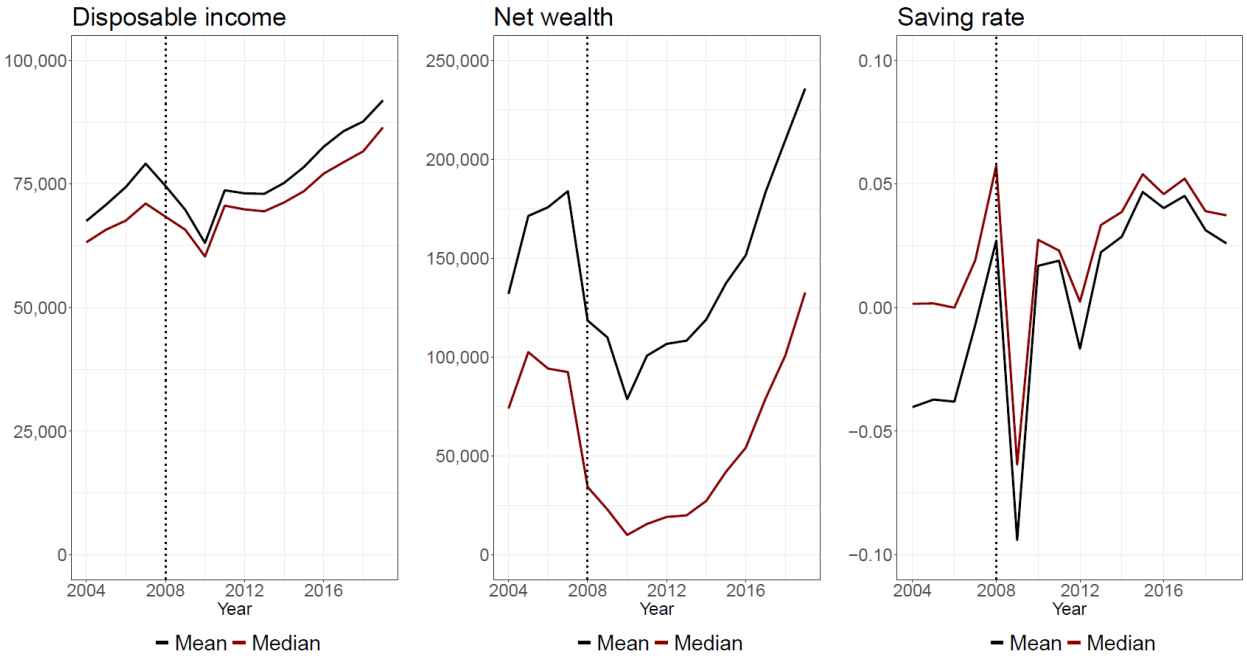
However, this paper contributes to the existing literature in several significant ways. To my knowledge, I am the first to use third-party reported administrative data to quantify the effects of past experiences with job-loss risk on current consumption, thereby emphasizing the persistent effects of such risk. I use administrative tax records to compute both household-level consumption and saving and estimate individual-level job-loss probabilities each year for all Icelandic taxpayers using third-party reported information. As such, I do not need to rely on surveys of consumption and saving, which tend to have several shortcomings (Browning et al., 2014). Second, I quantify the experience effects within the unique context of an advanced economy that has experienced wild fluctuations over the sample period of 2004-2019. The authors of the previously mentioned study emphasized the importance of analyzing experience effects in countries that have experienced drastic and volatile macroeconomic effects. Among advanced economies, few episodes of macroeconomic turbulence compare to those experienced by Iceland within the sample period. It covers the rapid economic expansion in the mid-2000s leading up to the GFC, the collapse of Iceland's entire banking system in the wake of the crisis, and, finally, its post-crisis recovery which was slow for the first 3-4 years but subsequently gained steam and turned into a long and robust growth phase. The large fluctuations in income and the unemployment rate, and the dramatic uncertainty and risk facing Icelandic households post-GFC provide a unique opportunity to analyze households' precautionary saving and consumption behavior. The Icelandic currency collapsed, which led to a sharp increase in inflation, which peaked at above 18% in January 2009 as import prices skyrocketed. Meanwhile, unemployment rose steeply, and house prices plummeted. To make matter worse, the vast majority of household debt, with households being heavily indebted at the time, was either linked to the CPI or to a foreign currency.

The impact of the GFC/banking collapse on household finances is clearly seen in Figure 1. Real disposable income fell sharply and persistently as high inflation, following the collapse of the exchange rate, eroded purchasing power and rising unemployment reduced households' income.[1]

[1]Note that disposable income is quite high since this is the same measure of disposable income as used in the empirical analysis. To obtain a comparable measure of consumption for homeowners and renters, I need to impute rent for homeowners. This imputed rent is added to capital income and, thus, shows up in disposable income although it is obviously not disposable. The issue is discussed further in Section 4.

Household wealth took a dramatic hit as asset prices collapsed while the value of their liabilities, frequently indexed to either the CPI or the exchange rate of a foreign currency, rose sharply. Although private consumption plunged, the sharp drop in income meant that households' saving ratios collapsed in the wake of the crisis before quickly recovering.

Figure 1: Household Finances During the Financial Crisis



Notes: Panel (a) of Figure 1 shows the mean (black) and median (red) of disposable income in Iceland in 2004-2019. As discussed further in the main text and in footnote 1, disposable income includes imputed rent for homeowners to ensure compatibility of imputed consumption across homeowners and renters. Panel (b) shows the mean (black) and median (red) of net wealth. Panel (c) shows the mean (black) and median (red) of the saving rate out of disposable income. The year of the GFC, 2008, is indicated by the black dotted vertical. All series are computed using the administrative tax records of the Icelandic population. Disposable income and net wealth are deflated using the CPI and converted to USD using the 2019 exchange rate.

The long panel dimension of the data allows for the identification of experience effects exclusively from time variation in the within-household consumption and past exposure to both job-loss and risk thereof, of which there is plenty within the sample period. The data set is rich of information on individuals' background and socioeconomic information. This allows me to construct individual-level job-loss probabilities each year using several background characteristics, rather than simply observing local unemployment rates. It also allows for exploring heterogeneities in the relationship between consumption and past experiences.

Summarizing the results, I find that past exposure to periods of heightened macro-economic unemployment, proxied by estimated individual job-loss probabilities, and past personal experiences with unemployment, reduce current consumption. A one standard deviation increase in both types of experience measures is associated with approximately 1.0% reduction in consumption. A heterogeneity analysis suggests the results are driven by younger, higher-income and university educated households. The results suggest that households who have merely been

exposed to increased risk of becoming unemployed, without actual job loss, subsequently reduce consumption by a similar amount as those who have actually suffered unemployment spells in the past. With private consumption accounting for approximately 50% of GDP in Iceland, this is equivalent to an economically meaningful 0.5% of GDP drop in consumption after a one standard-deviation in past exposure to unemployment. As such, merely the risk of job loss, which increased by approximately 10% in the wake of the GFC/banking collapse, was an economically meaningful drag on demand during the subsequent economic recovery.

Furthermore, and in line with the experience hypothesis that individuals who have lived through periods of high risk subsequently reduce consumption, I show that past exposure to unemployment is associated with higher future wealth accumulation. This only applies to those who do not become unemployed despite heightened past job-loss probabilities. In contrast, becoming unemployed is associated with lower wealth in the future. Furthermore, I show that lower future income after elevated risk experiences does not explain the subsequent reduction in consumption.

In economics, hysteresis refers to the idea that seemingly transitory economic shocks can have long-lasting effects on macroeconomic outcomes. Typically, this is attributed to the persistent loss of human capital associated with temporary increases in unemployment. While this paper does not refute such explanations for slow recovery and persistently depressed demand in many countries after the GFC, it offers a new explanation for hysteresis.

Specifically, the loss of income associated with the loss of human capital is not necessarily consistent with higher saving rates and healthier current account balances. Wealthy and higher-income households save a higher fraction of their income compared to middle-class and lower-income households. Thus, a higher unemployment rate and widespread loss of human capital might, other things equal, be expected to persistently lower national saving rather than increase it (Dynan et al., 2004). Instead, precautionary saving appears to play a vital role. Persistent changes in beliefs about future economic outcomes after the GFC may have raised precautionary savings among a significant share of Icelandic households. This could simultaneously explain persistently depressed demand, higher national saving, and stronger current account balances following the crisis.

The rest of the paper is organized as follows. Section 2 reviews the literature on precautionary savings. Section 3 presents a simple model of precautionary savings to help clarify how households' consumption and saving decisions are affected by uncertainty. Section 4 introduces the data and shows how the Icelandic tax registries are used to compute household-level consumption and saving for all households in Iceland. Section 5 analyzes the persistent effect of unemployment experiences on household consumption. Finally, Section 6 concludes the paper.

2 Literature

Starting with the seminal contributions of Friedman (1957) and Ando and Modigliani (1963), the economics profession began studying the consumption and saving decisions of individuals as depending on their permanent or lifetime income instead of present income. According to Friedman, consumers have a view of their permanent income, which is the average income they can expect to earn per year in the future and consume a fraction of this income each year. In Ando and Modigliani (1963), consumers assess their lifetime income profile and borrow when young, pay back debt and accumulate savings in middle age and run down their savings during retirement.

This paper draws on two strands of literature, both born out of the life-cycle-permanent-income hypothesis (LC-PIH) framework mentioned above. First, the one on precautionary saving where households optimally choose their level of savings based on the income risk they face. Second, there is a growing literature on hysteresis and the scarring effects of recessions, meaning that seemingly transitory negative shocks to the economy can have long-lasting effects.

Starting with the former, although still a workhorse model to analyze households' consumption and saving decisions, numerous studies find evidence contradicting the canonical LC-PIH model's predictions. One example is the comovement of income and consumption over the life cycle documented for example by Carroll and Summers (1991), Carroll (1994), and Andolfatto et al. (2000). Such excess sensitivity to changes in income can be explained by the buffer-stock model of Deaton (1991), Deaton (1992), Carroll et al. (1992), and Carroll (1997) which states that in the presence of income risk, risk averse and impatient households will optimally target a wealth-to-permanent-income ratio to guard against shocks to income. This framework implies a higher marginal propensity to consume out of transitory income compared to the standard LC-PIH model, as households adjust their consumption in response to deviations in wealth from their target caused by income fluctuations. Consequently, this leads to the comovement of income and consumption.

Second, the literature on hysteresis and scarring effects shows how business cycles and negative shocks can have persistent effects on economic outcomes. Fatas (2000) shows that in a model where aggregate demand and growth rates are positively correlated, exogenous cyclical shocks can generate persistent fluctuations. The growth rate of the economy is governed by the amount of research by firms. Optimal research, in turn, is a function of expected profitability of innovations, which depends on aggregate demand. Exogenous negative shocks to demand thus impact the incentives to innovate, which explains why the economy never reverts to the pre-shock trend. He also provides empirical evidence showing that growth-related variables such as capital accumulation and R&D expenditures are procyclical and that there is a correlation between how persistent fluctuations are and countries' long-term GDP growth rates.

In Kozlowski et al. (2020), crises can persistently change households' perceived probability

of an extreme, negative shock occurring in the future. The persistently changed beliefs, in turn, lead to scarring, that is large long-run costs from short-run losses in output. Malmendier and Nagel (2011) and Malmendier and Shen (2024) find empirical evidence for such belief-scarring. The former find that individuals who lived through the Great Depression and, therefore, experienced particularly low stock market returns are less likely to take financial risks and more likely to stay away from the stock market. The latter find that households who have experienced high local unemployment, a proxy for labor market uncertainty, or gone through unemployment spells themselves, subsequently reduce their consumption.

Several studies have tried to quantify the consumption or saving response to risk or uncertainty. Malmendier and Shen (2024) find that a one standard-deviation increase in macroeconomic experience, defined as having lived through times of high local and national unemployment, lowers consumption given income by 1.60-1.85%. Berg and Halvorsen (2018) use Norwegian register data to construct individual-level time variation in job loss risk and find that a one percentage point increase in the job loss probability leads to an increase in saving of 9%. The sudden collapse of oil prices in 2014, which led to an exogenous increase in job loss risk for certain occupations and regions, provided a novel natural experiment for Juelsrud and Wold (2019) who find that a one percentage point increase in job loss risk increases liquid savings by 1.3-1.7%. They conclude that job loss risk is an important driver of changes in households' saving during recessions as their estimated saving response explains roughly 80% of the observed increase in liquid saving rates during the 2014 oil price collapse. Lusardi (1998), in an early contribution, regresses income risk on households' net-wealth-to-permanent income ratio and concludes that the extent of precautionary accumulation ranges from 1% to 3.5%. Ceritoğlu (2013) uses the Household Budget Surveys from 2003 to 2009 prepared by the Turkish Statistical Institute to estimate the extent of precautionary saving in Turkey. He finds that a 10% increase in labor income risk raises household saving by 2-4%.

This paper adds to the growing literature, using Icelandic third-party reported administrative data over a sample period of remarkable macroeconomic volatility in Iceland, which provides a unique opportunity to quantify the relation between past exposure to unemployment and current consumption.

3 A Conceptual Framework for Precautionary Saving

To provide clarity on how job-loss risk might affect households' consumption behavior, I present a simple theoretical framework of precautionary saving. The model is motivated by that of Mody et al. (2012) but extended with stochastic unemployment risk and belief-persistent agents following Malmendier and Nagel (2011).

There are two types of agents who both share the same constant relative risk aversion (CRRA) utility function and they both live for T periods. The agents both face a stochastic probability of becoming unemployed each period but they differ in how they perceive that probability. The

rational agent knows the true distribution of the economy's unemployment rate and, thus, her perceived probability of becoming unemployed is the same every period and equals the long-run average unemployment rate. The belief persistent agent forms beliefs about the probability of becoming unemployed based on the recent history of the observed unemployment rates of the economy. In particular, she weighs the unemployment rate in the past k periods, giving a relatively high weight to recent unemployment rates compared to those in the more distant past.

More explicitly, the perceived unemployment probability of the belief persistent agent is computed as follows:

$$\tilde{u}^R_{t,BP}(n,k,\lambda,u_t) = \sum_{k=1}^{K} w(n,k,\lambda) \times P(u_t) \tag{1}$$

where $\tilde{u}^R_{t,BP}$ is the perceived exposure to risk of job-loss at time t of a belief persistent agent. The R superscript is to distinguish from actual unemployment spells further discussed in the empirical analysis below. $P(u_t)$ is the true aggregate probability of becoming unemployed at time t, which is the same for both types of agents, and w is the weighting function which assigns weights to the true unemployment rate of the economy observed k periods prior to the current period. $\tilde{u}^R_{t,BP}$ is also a function of n, the number of periods the agent considers when forming their beliefs about the probability of unemployment in the current period, and λ, the weighting parameter which governs the slope of the weighting function. I parametrize the weighting function as follows:

$$w(n,k,\lambda) = \frac{(n-k+1)^\lambda}{\sum_{j=1}^{n}(n-j+1)^\lambda} \tag{2}$$

$\lambda > 0$ ensures that past observations have a lower weight relative to more recent ones. In the model, I choose $\lambda = 1$ which entails a linearly declining weight. This implies that when agents form beliefs about the probability of becoming unemployed in the current period, they give more weight to recent experiences compared to those of the relatively more distant past.

Both agents solve a dynamic optimization problem which has the following recursive representation:

$$\begin{aligned} & V\left(M_t, \tilde{u}^R_{t,\theta}\right) \max_{c_t \leq W_t} \left\{u\left(C_t\right) + \beta E_t\left[V\left(M_{t+1}, \tilde{u}^R_{t+1,\theta}\right]\right\}\right. \\ & \text{s.t.} \\ & S_t = M_t - C_t \\ & M_{t+1} = S_t + Y_{t+1} \end{aligned} \tag{3}$$

where M_t is the stock of wealth and β is the intertemporal discount factor.[2] $\tilde{u}_{t,\theta}$ is the perceived probability of an individual of type $\theta \in \{Ra, BP\}$, where Ra denotes rational agents and BP denotes belief-persistent agents becoming unemployed at time t.

At the beginning of each period, the agents observe their level of wealth and form beliefs

[2] I impose the constraint that wealth cannot be negative.

about the probability of becoming unemployed in the current period. They then make their consumption and savings decisions. Their level of wealth next period is governed by those decisions and their stochastic labor income, which depends on whether or not they become unemployed. Labor income evolves as follows:

$$Y_{t+1} = \begin{cases} Y^{High} = Y\mu \text{ with probability } 1 - u_t \\ Y^{Low} = Y\zeta \text{ with probability } u_t \end{cases} \tag{4}$$

where Y is the deterministic level of permanent income around which current income fluctuates, u_t is the true objective probability of becoming unemployed at time t, and ζ is the unemployment insurance replacement rate. During the pandemic, this is the parameter that was indirectly raised in many countries through various fiscal measures, effectively insuring households against otherwise skyrocketing unemployment. Finally, μ is an adjustment factor which ensures that the expected value of Y_{t+1} equals Y regardless of the level of unemployment.[3] This is necessary to ensure that a change in the level of unemployment only affects the saving rate through the precautionary savings channel, i.e. due to increased perceived job-loss risk, and not through a reduction in permanent income associated with unemployment spells.

The true unemployment rate of the economy, u_t, which also equals the probability of each agent becoming unemployed in period t, evolves as follows:

$$u_t = \begin{cases} u^{High} \text{ with probability } p \\ u^{Low} \text{ with probability } (1 - p) \end{cases} \tag{5}$$

The calibration of the model is standard. The intertemporal discount factor β equals 0.97, the coefficient of relative risk aversion is set to 2, and the unemployment replacement rate is assumed to be 0.5. Both agents live for 80 periods. In principle, one could expect belief persistent agents to form beliefs about job-loss probabilities by considering the whole unemployment history of the economy. However, for consistency with the empirical analysis below, I assume that agents form their beliefs based on the previous five periods of the economy. The economy is in a high-unemployment state of 10% with a probability of $p = 0.95$ and in a low-unemployment state of 2% with a probability of $p = 0.05$.

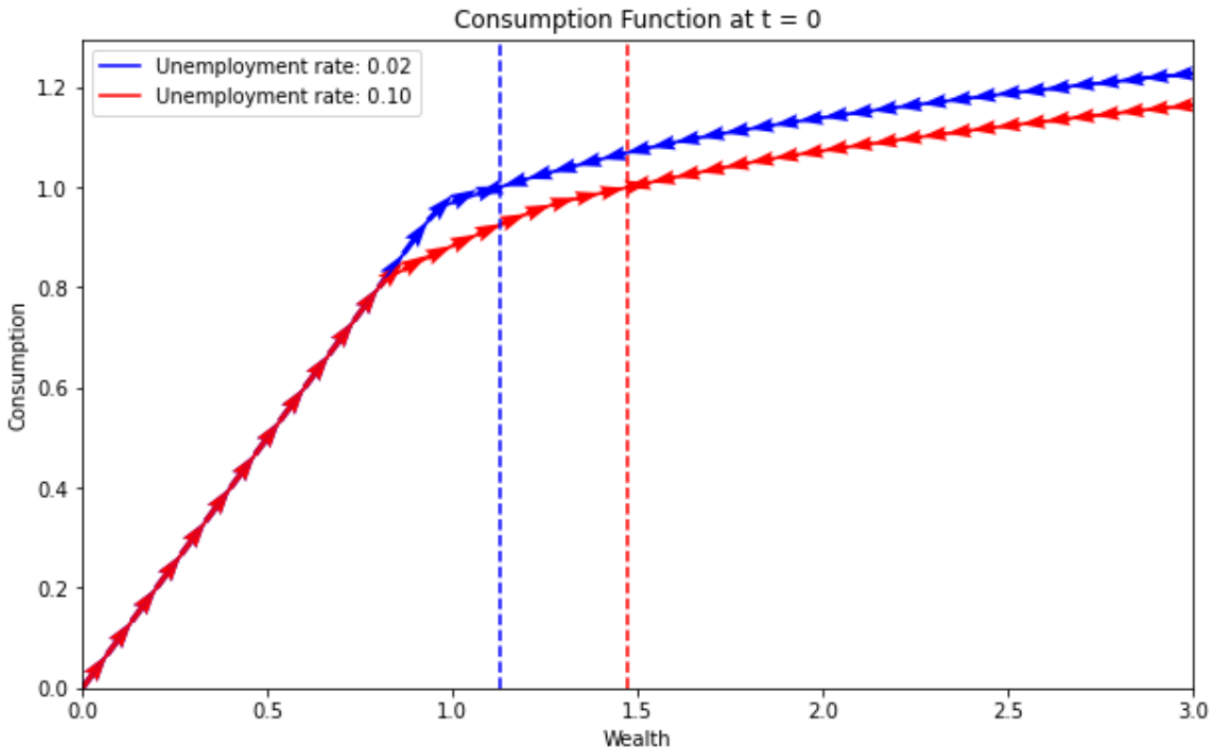
The model has no closed-form solution for households' optimal consumption decision in each period and I, therefore, resort to numerical optimization methods, namely backward induction. That is, I solve for the agents' consumption and saving decisions by solving the Bellman equation (3), starting at period T when the agents consume all their wealth and moving backwards in time.

The model generates a buffer-stock target savings behavior where the agents, who are both impatient and risk averse, target a certain age-dependent wealth-to-permanent-income ratio. When wealth is below the target, risk aversion dominates impatience and the agent saves and accumulates wealth to optimally guard against shocks to income, and vice versa if wealth is above the target.

[3] In particular $\mu = \frac{1-u\zeta}{1-u}$

To illustrate the consumption response to changes in the unemployment rate, I first consider two versions of the rational agent. One lives strictly in the low 2% unemployment state, while the other is stuck in the high unemployment state with a 10% unemployment rate. The former is close to the unemployment rate in Iceland prior to the GFC/banking crisis while the latter is close to the peak in its aftermath. Figure 2 shows the consumption function associated with each unemployment rate at time $t = 0$. It shows that while consumption is strictly increasing in wealth, beyond a certain amount of wealth the level of consumption is lower in the high unemployment state than it is in the lower unemployment state. Note that μ ensures that the permanent income of the two agents is the same. The differences between the consumption functions thus only reflect precautionary saving behaviour due to the higher risk associated with the high unemployment state. The optimal wealth-to-income ratio is 1.13 in the low unemployment state and 1.47 in the high unemployment state as the agent responds to increased job-loss risk by raising their precautionary savings. At those wealth levels, the agent consumes all their permanent income, thus leaving expected wealth-to-permanent income unchanged.

Figure 2: The Consumption Function



Notes: Figure 2 shows consumption as a function of wealth, both normalized by permanent income, for two different unemployment rates (u), 2% and 10%. The blue vertical line shows the optimal buffer-stock wealth-to-income target, that is the wealth-to-income ratio at which agents consume all their income, thus, keeping their wealth-to-income ratio unchanged, for the low unemployment state (2%). The orange vertical line shows the optimal buffer-stock wealth-to-income target for the high unemployment state (8%).

Having shown how the macroeconomic environment, captured by the risk of job-loss each

period, affects the consumption decisions of a rational agent, I simulate the model for $1,000,000$ agents of each type.[4] Figure 3 shows average consumption in periods 20-60, separately for rational agents and belief-persistent agents. Importantly, it only considers the consumption of agents who never become unemployed, and discards those who do. This is to isolate the consumption response to perceived income risk from the consumption response to actually becoming unemployed. The dashed vertical lines indicate a one-time transitory shift of the economy from the low-unemployment state to a high-unemployment state.

The blue line shows that rational agents, whose perceived probability of becoming unemployed in the current period is fixed over time and equals the average of the true unemployment process in (5), do not respond to the economy's shift from the low-unemployment state to the high-unemployment state. This is because the agents form their beliefs about the state of the economy and make their consumption and savings decisions before observing the actual state of the economy and whether or not they are employed in the current period. Since the state of the economy is time independent, the beliefs of the rational agents are fixed over time.

The orange curve shows how consumption of never-unemployed belief persistent agents evolves over time. Following a one-time transitory shift of the economy from the low-unemployment state to the high-unemployment state, the belief persistent agents reduce their consumption before gradually raising their consumption as they adjust to the low-unemployment state. The belief persistence thus leads to depressed consumption for multiple period after the shift of the economy to the high-unemployment state, both relative to the consumption of rational agents and relative to the consumption of belief persistent agents prior to the shift.

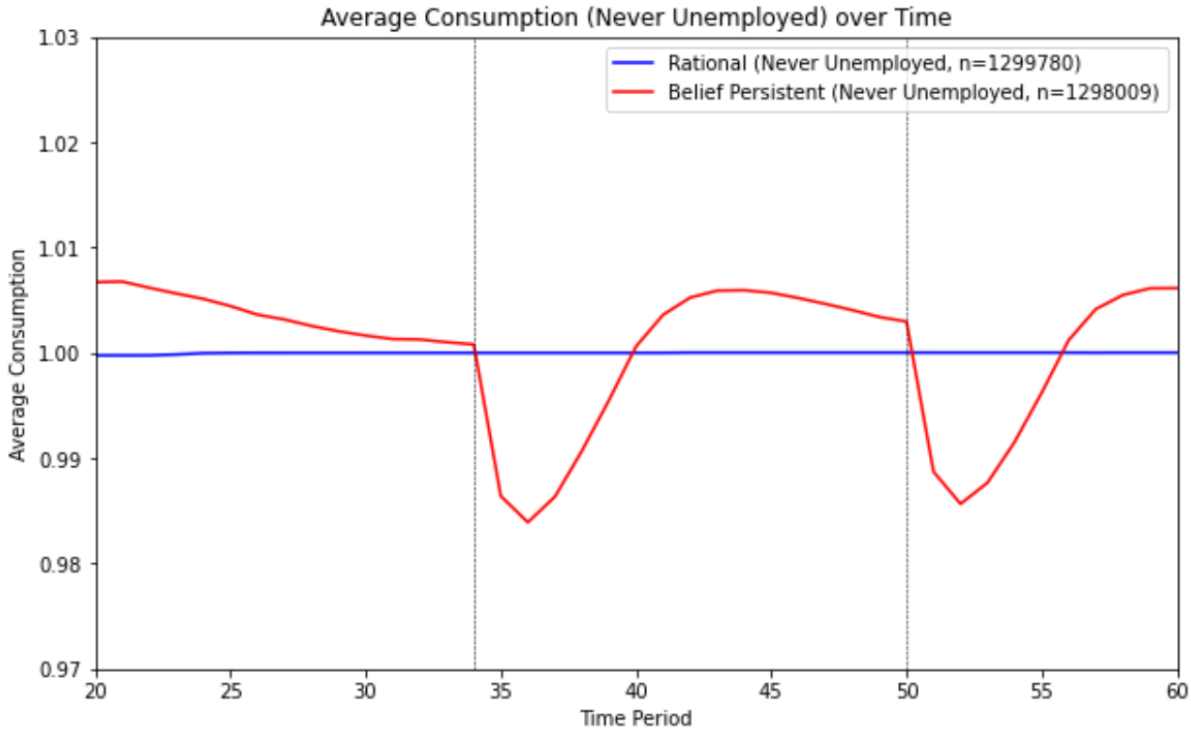
The simple model introduced in this section illustrates how heightened job-loss risk raises households' precautionary saving. It also shows how belief persistence can lead to persistently depressed consumption following a macroeconomic shocks. While in the calibration above the agents consider the unemployment rate over the past five years when forming beliefs about job-loss risk, in principle they could consider a longer time horizon. As a result, tail events such as the GFC/banking crisis, which occurred after years of relative macroeconomic stability, or the COVID-pandemic might lead households to persistently update their beliefs about the distribution of shocks. In turn, this would optimally and persistently alter those households' appetite for saving to insure against heightened perceived risk. This is how temporarily heightened job-loss risk can leave long-term scars on the economy as aggregate demand takes time to fully recover.

4 Data

For the empirical analysis, I use a database containing administrative tax records of all Icelandic taxpayers from 1996 to 2019, although the sample period in the estimations ranges from 2004 to 2019. The sample period spans a period of vast macroeconomic fluctuations and uncertainty,

[4] Both agents start with wealth $M = 1$

Figure 3: The Consumption Response to a 50% negative wealth shock



Notes: Figure 3 illustrates the savings response to an exogenous negative wealth shocks (orange) at times $t = 34$ and $t = 50$ compared to a baseline specification of no shock (blue). Wealth is normalized by permanent income.

including rapid growth rates leading up to the GFC and the collapse of Iceland's banking system, and the subsequent recovery period. The data includes detailed third-party reported information on various income sources, assets, and liabilities, such as bank deposits, real estate values, some financial assets, mortgage debt, and other liabilities. In addition, the data is integrated with other administrative sources, providing socio-economic information including education, occupation, loan repayments and interest payments. The data are collected by Statistics Iceland and Iceland Revenue and Customs.

I use the tax records to construct measures of each household's saving and consumption. First, I construct household measures of income, assets, and liabilities by aggregating information across household members using unique household identifiers.[5] Therefore, I disregard potential intra-household inequality and assume that couples with joint finances make financial decisions based on their combined income and wealth.

I compute consumption for each household using the accounting identity that a household's consumption equals its disposable income minus changes in net wealth plus unrealized capital gains (see for example Browning et al. (2014), Eika et al. (2020), and Fagereng and Halvorsen (2017))

[5]Household members consist of at most two individuals in the case of married or jointly taxed couples. Children over the age of sixteen are treated as a unique household even though they might live with their parents.

$$C_{it} \equiv Y_{it} - \Delta W_{it} + \sum_{k} \Delta p_{kt} A_{ikt-1} \tag{6}$$

where Y_{it} is the after-tax income of household i at time t, ΔW_{it} is the change in net wealth from the previous year and $\sum_k \Delta p_{kt} A_{ikt-1}$ denotes unrealized capital gains on asset A_k.

Having derived consumption, saving is the part of disposable income which is not consumed.

$$S_{it} \equiv Y_{it} - C_{it} \tag{7}$$

where S_{it} denotes household's i saving at time t.

The idea is that income is either saved, thus leading to increasing net wealth, or consumed. However, net wealth is also governed by unrealized capital gains/losses. To derive consumption from equation (6) it is, therefore, 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 saving some of its income, which in turn reduces current consumption. To avoid misattributing changes in market prices as saving out of income, one needs to undo the contribution of unrealized capital gains/losses to net wealth.

To see this, consider a household whose only asset is the house in which they live. The household earns an income with which it either consumes or pays down its zero-interest-rate mortgage. In the absence of price changes, the household's consumption is accurately identified as the share of its income not devoted to paying down the mortgage. However, if the price of their home increases, then the wealth of the household is not only influenced by its income and consumption decisions but also by the unrealized capital gain in their real estate asset. Unless accounted for, the increase in net wealth due to rising home prices would lead to an underestimation of the household's consumption.

Fortunately, the Icelandic tax records include estimates of each household's real estate market value. This would ensure the precise measure of consumption in the above example. However, the example points to two key challenges in measuring consumption and saving using accounting identities (6) and (7). First, the method requires accurate information on all sources of households' income and their level of assets and liabilities. Such data are scarce, but third-party reported information from tax registries which cover labor income, capital income, various government transfers, and tax payments are tough to beat. Second, to account for the effect of a households' asset portfolio on its consumption we either need information on the price and quantity of each asset on its balance sheet or information on asset transactions for all its assets. Such comprehensive data is extremely rare, but I now turn to outlining in detail the information on income, unrealized capital gains, assets, and liabilities available in the Icelandic tax registries.

4.1 Income

The measure for post-tax household income includes labor income, capital income, income from pension funds, government transfers and other income, such as due to lottery winnings. Naturally, the tax registries do not include information on undeclared income. However, the consumption measure does capture informal sector consumption assuming the income used for financing such consumption is declared. Moreover, I lack information on inheritance income.

Another income source not reported in the tax registries is homeowners' income from owner-occupied housing services. If a household purchases a home they had previously rented, its consumption expenditure would drop by the amount of the rent they no longer pay, while their consumption of housing services would be unchanged. Therefore, to make the consumption measure comparable between homeowners and renters, I add imputed rent for homeowners to their capital income and, consequently, to their consumption.

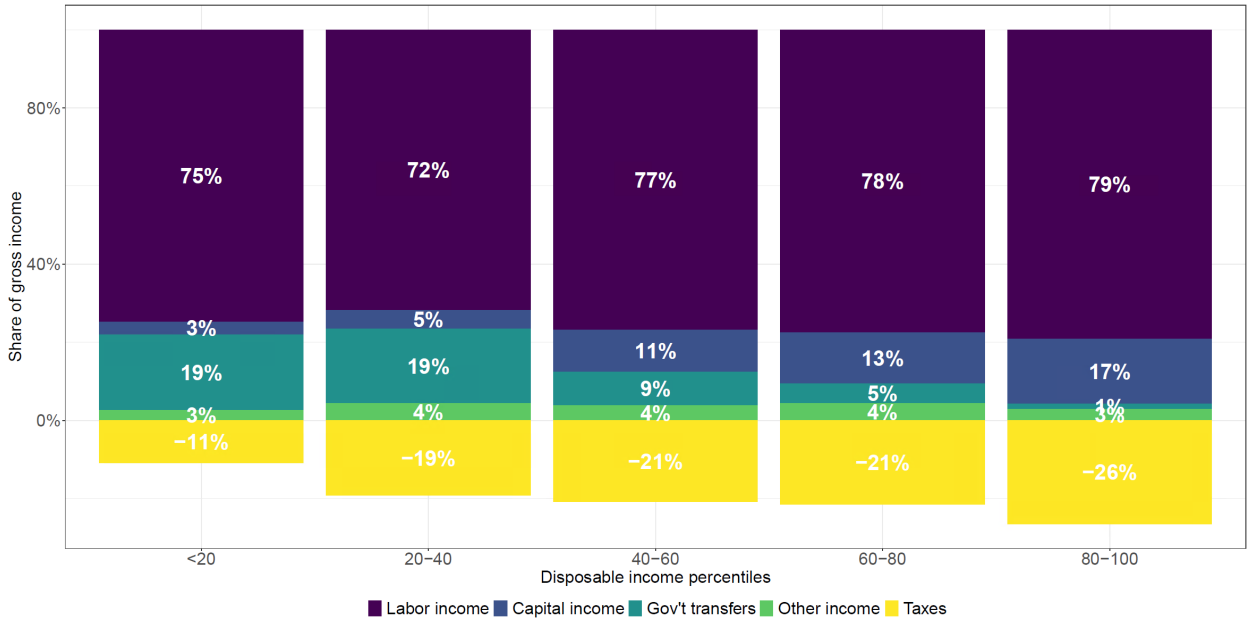
Statistics Iceland uses a simple user cost method to estimate the value of owner occupied housing services (see Iceland (2019)). I compute imputed rent at the household level by distributing the aggregate value of owner occupied housing services found in Iceland's national accounts among households according to their share of the total residential real estate value. I add this measure of imputed rent to capital income. Finally, I compute disposable income as all labor income, capital income, and other income, consisting i.a. of government transfers, pension income, grants and lottery winnings net of total tax payments.

Figure 4 shows the share of the various income components by disposable income quintiles in 2019. On average, labor income makes up 76% of gross income and capital income accounts for 10%, 80% of which is imputed rent. Government transfers and other income account for 11% and 4% of gross income, respectively. As expected, the bottom two quintiles rely more on government transfers than the top quintiles and the share of capital income is increasing in disposable income. Furthermore, we see evidence of the progressivity of the Icelandic tax system as higher income quintiles pay a larger fraction of their gross income in taxes.

4.2 Net Wealth

The tax records provide information on each household's assets and liabilities. The data on assets include ownership of real estate and vehicles, savings account balances, and savings in stocks and bonds through equity funds, bond funds or mixed funds. A notable feature of Icelandic tax records is that direct ownership of stocks is not recorded at market value. Consequently, the level of households' assets is unaffected by changes in the market value of stocks, unless the ownership of stocks is indirect through funds, in which case it is registered at market value. Although direct ownership of stocks is not a significant share of most households' assets, this obviously means that I lack the complete picture of households' assets. Generally, this would be a drawback, but since capital gains/losses in directly owned stocks are not observed, it simplifies accurate identification of households' consumption and saving as long as no stock transactions occur within the year. In

Figure 4: Income components by disposable income in 2019



Notes: Figure 4 shows the relative size of different income components by disposable income quintiles in 2019. Each income component is summed over all households within each disposable income quintile and then divided by the total gross income of the quintile.

this case, no assumptions about the capital gains/losses of those assets are required to accurately compute consumption. On the other hand, the fact that stock assets are registered at nominal value gives rise to complications if households do engage in stock transactions in a given year, potentially leading to biased measurements of consumption and saving in those cases. This is discussed further in the following section.

4.3 Unrealized Capital Gains/Losses

To derive consumption from equation (6), it is necessary to differentiate between changes in net wealth due to unrealized capital gains, which do not affect consumption but do influence net wealth, and changes in net wealth due to households saving, which reduces consumption. This is a key challenge in using tax records to determine household consumption and saving, as they typically provide end-of-year asset values but lack detailed information on unrealized capital gains or asset transactions.

A key difference between the Icelandic administrative data and that of other Nordic countries stems from the fact that direct stock ownership is not recorded at market values. This lack of market value information creates complications, as changes in household portfolio allocation through stock transactions can lead to changes in measured net wealth, when no such changes really occur. Conversely, net wealth is unaffected by unrealized capital gains/losses in stocks, simplifying accurate measurement of consumption and saving if no stock transactions occur.

Assumptions are needed for ownership of financial assets through funds, which are registered at end-of-year market value. Following Eika et al. (2020), I measure unrealized capital gains/losses assuming no within-year transactions, allowing for heterogeneous returns. However, I assume unrealized capital gains in such financial assets are zero for those who have such assets in a given year but did not in the previous year. In such cases, a within-year transaction clearly took place and likely drives the change in the value of such assets from one year to the next.

4.4 Real Estate Prices

For most households, the largest source of unrealized capital gains/losses arises from real estate price changes. The dataset includes each households' real estate market value as estimated by Register Iceland (RI), which annually assesses the market value of all Icelandic properties. RI valuations form the basis for property charges and inheritance taxes, making yearly changes in RI's property valuation a natural measure of real estate capital gains/losses.

While this real estate price measure is accurate for most households, challenges arise for households engaging in real estate transactions within a given year. Such transactions do not imply net wealth changes but merely asset portfolio rebalancing. Lacking transaction data, I need to resort to assumptions. Specifically, I assume no within-year transactions for the vast majority of households. However, I assume transactions took place if the change in real estate assets is either at the top 5% or the bottom 5% of the distribution of changes in real estate in a given year. In such cases, I assume the transaction occurred in the middle of the year and that the price of the new real estate asset followed Statistics Iceland's property price index.

4.5 Loan Indexation

A large share of household debt in Iceland is CPI-indexed, meaning loans principals are tied to the CPI index. This affects consumption and saving measurements, as indexation represents unrealized capital gains, typically losses though as inflation is usually in positive territory, thereby raising the value of household liabilities. Applying Equation (7), if not accounted for, unrealized capital losses would be misattributed to consumption out of income. As such, the consumption measure is biased upwards if the indexation of liabilities is not subtracted from the change in net wealth. I compute the indexation by summing a household's repayment of a loan and the change in the loan's principal. For non-indexed loans, repayments would explain all changes to a loan's principal but for indexed loans repayments are typically lower than the change in a loan's principal.

4.6 Mortgage Relief

In 2014-2016 a government mortgage relief lowered household debt by the equivalent of 3.5% of GDP. The relief constitutes an unrealized capital gain for households that benefitted from it as it

raised their net wealth without affecting their disposable income, and, thus, needs to be accounted for.

4.7 Durable Consumption

Durable goods generate expenditure at purchase while providing consumption services until replacement or disposal. Although the tax records lack detailed data on types of consumption, they include household vehicle values, which is arguably the largest source of durable consumption for the average household after housing services, which I have already accounted for. According to Icelandic tax laws, vehicles are depreciated by 10% per year. I assume that the consumption flow from vehicles equals their depreciation value.

4.8 Consumption and Saving

Finally, I measure consumption in the Icelandic administrative tax records using a version of Equation (6) which, considering the discussion above, takes the following form:

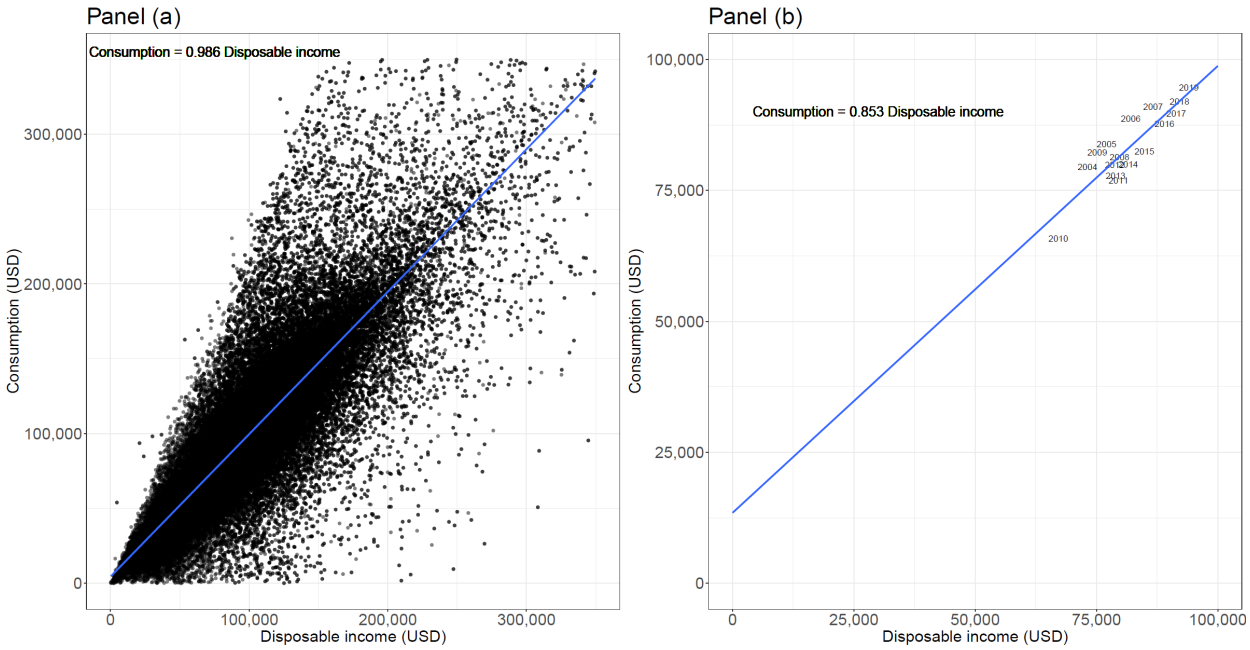
$$C_{it} \equiv Y_{it} - \Delta W_{it} + \Delta HP_{it} - \text{Index}_{it} + DC_{it} + MR_{it} + \Delta SP_{it} \tag{8}$$

where ΔHP_{it} denotes the change in real estate prices for household i in year t, $Index_{it}$ denotes loan indexation, DC_{it} is durable consumption in vehicles, MR_{it} captures unrealized capital gains from the 2014-2016 government mortgage relief, and ΔSP_{it} is the unrealized capital gains in stocks and funds.

Panel (a) of Figure 5 shows the relationship between consumption and post-tax income for a cross-section in 2019. Evidently, and as expected, the two are very positively correlated, with a regression coefficient of 0.986. On average, therefore, higher income households also have a higher level of consumption and vice versa. Panel (b) shows a time series version of panel (a). Specifically, it shows the average consumption against average post-tax income for each year. The slope of the regression line is now 0.853 which, although significantly less than what is found in the cross-section, is still high and close to 1. In comparison, Parkin (2022) found a slope of 0.90 for the US using data from the Bureau of Economic Analysis.

Previous literature has found that rich and high-income households save a larger fraction of their income than lower income households. Dynan et al. (2004) find a strong positive association between permanent income and saving rates in the U.S. Mian et al. (2021) show that the top 10% income group in the U.S. has a substantially higher saving rate than the next 40% and the bottom 50%. Panel (a) of Figure 6 plots the average saving rates by disposable income groups in 2019. The income groups are formed within each birth cohort, defined as an individual's year of birth, to ensure saving rates across income groups are not driven by life-cycle effects. Panel (a) confirms the aforementioned trends in the literature using information from Icelandic tax registries. In particular, the saving rate, as defined above, is 4.6% of disposable income for the top 10% of

Figure 5: Consumption and disposable income in 2019



Notes: Panel (a) of Figure 5 shows the relationship between disposable income in 2019 on the x-axis and imputed consumption in 2019 on the y-axis. Panel (b) shows the consumption function in 2004-2019. Each point represents average consumption and average disposable income for a particular year. In both panels, variables are converted to USD using the average exchange rate of the ISK against the USD in 2019. The blue lines represent an OLS regression line obtained by regressing consumption on disposable income.

the income distribution but between 1.4% and 1.9% for the bottom 50% and the mid 40% of the distribution, respectively. Furthermore, panel (b) of Figure 6 shows that, in accordance with the life cycle model of consumption and saving, the saving rates are low for young workers and rise as they approach middle age and retirement.[6]

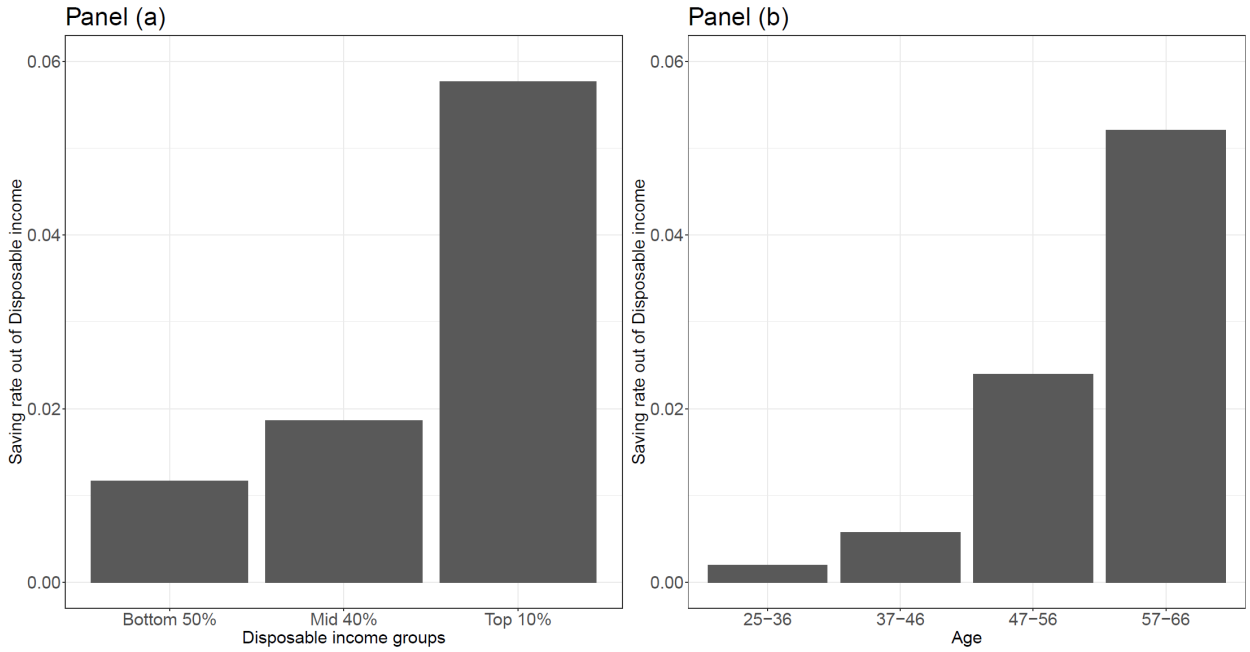
In what follows I restrict the sample to individuals aged 25-66 and I also omit households whose disposable income is below 50% of the median. The rationale is to exclude those that have limited or no ties to the labor market. Finally, to reduce noise from outliers, I only consider households with a saving rate out of income between -1 and 1.

5 The Persistent Effect of Uncertainty

A key prediction of the simple model in Section 3 is that when faced with higher aggregate income risk, households will optimally save more. There are several ways in which the increase in saving can become persistent. First, a one-time permanent increase in true income risk will raise households' optimal buffer-stock target savings. Accumulating those savings takes time as households are also impatient and prefer smoothing consumption. Households' saving will be

[6]The life cycle model predicts that saving rates drop post-retirement, however the sample only includes individuals aged up to 66 years old.

Figure 6: Households' saving rate out of disposable income in 2019



Notes: Panel (a) of Figure 6 shows households' saving rate out of disposable income for three disposable income groups. The income groups are formed within each birth cohort, defined as the year of birth, to ensure saving rates across income groups are not driven by life-cycle effects. Panel (b) shows the saving rate by age groups. The saving rate is computed as the average saving rate within each group.

elevated until the higher savings target is reached. Second, one can hypothesize that over time households learn about the true distribution of possible macroeconomic outcomes, and therefore continually update their beliefs about income risk and job-loss probabilities. Thus, a period of heightened risk may result in households persistently raising their beliefs about job-loss risk, or macroeconomic risk more broadly. This would lead to an optimally persistent increase in saving as households adjust to higher optimal buffer-stock target savings following a large and negative macroeconomic shock. Finally, one can take the view that households' consumption and saving behavior is a function of their subjective beliefs about income risk, rather than the true risk which is not known in practice. Households' recent experiences might heavily influence those subjective beliefs. A dramatic event such as the GFC/banking collapse might be salient enough to affect those beliefs for some time. A period of heightened job-loss risk could, thus, persistently raise the households' optimal buffer-stock target savings, leading to persistently higher saving.

In this section, I examine the relationship between consumption and past exposure to unemployment while controlling for a wide range of characteristics. My primary focus is on the consumption response to past exposure to individual-level job-loss risk. Additionally, I explore how personal experiences with job-loss influence subsequent consumption. I focus on consumption rather than saving, since the latter is frequently negative, while consumption is strictly positive and, thus, allows me to take logarithms. Since I am interested in the consumption response to past experiences of heightened risk episodes, I need to track consumption behavior of

households over time. As such, I only include households with at least six years of tax records in Iceland. The sample period of 2004-2019 covers the rapid expansion of the mid-2000s and the financial crisis of 2008 which hit Icelandic households particularly hard, substantially raising the uncertainty and risk surrounding the economic outlook and households' future incomes. It also includes the slow post-crisis recovery in 2009-2013 and the subsequent rapid growth phase in 2013-2019.

Table 1 shows summary statistics for the sample over the whole sample period. After implementing the sample restrictions from Section 4.8 and after omitting individuals for which I am unable to construct the experience measure, I am left with 1,514,196 observations on 139,461 different individuals. Average consumption amounts to approximately $89,000 and average disposable income amounts to approximately $87,000 dollars. Bear in mind that the consumption measure includes imputed consumption for homeowners and those who own vehicles. Average net wealth is close to $176,000 over the sample period. All the variables are converted to 2019 prices, using the Icelandic CPI, and converted to USD using the average 2019 exchange rates.

Risk experiences, which are defined further below but represent weighted average of past individual-level job-loss probabilities, measure households' perceived probabilities of losing their job in a given year, given both personal characteristics and the macroeconomic environment. It is 6.3% on average over the sample period both when using linear weights ($\lambda = 1$) in Equation (2) and when increasing the steepness of the weighting function to $\lambda = 3$, giving recent experiences a higher weight. The measure for personal unemployment experiences, which measures individuals' past experiences of becoming unemployed, is 5.7% for both $\lambda = 1$ and $\lambda = 3$.

5.1 Descriptive Results

Before estimating a formal model, I present preliminary descriptive findings. Younger and less-educated individuals were disproportionately affected by the GFC. They were more likely to lose their jobs compared to their older and more educated counterparts (see Figure 8 below). With the model from Section 3 in mind, one could hypothesize that the young and less-educated individuals who kept their jobs, but faced increased job insecurity, responded to the elevated income risk by cutting their consumption and raising their saving rates, thereby accumulating more wealth given their level of income. However, liquidity constraints, which make it difficult for individuals to raise their saving, might have the opposite effect if such constraints are more binding for those groups relative to others. Figure 7 plots the saving rates by income groups, education groups and age groups. As before, I define income groups based on the distribution of after-tax income within each birth cohort in a given year to ensure the saving rates are not driven by life-cycle effects.

Panel (a) shows that while the saving rate of top earners is relatively high and stable over the sample period, it rises markedly after the financial crisis and during Iceland's economic recovery for both the bottom 50% and the mid-40% of the after-tax income distribution. This is consistent with lower- and middle-income individuals facing more job insecurity in the wake of the crisis

Table 1: Summary statistics

	Mean	Standard deviation
Consumption	88,523	64,170
Disposable income	87,282	57,893
Net wealth	175,803	287,186
Gender	0.494	0.500
Age	45.363	10.793
Spouse	0.721	0.449
Children	0.922	1.091
University	0.432	0.495
Urban	0.636	0.481
Macroeconomic experience ($\lambda = 1$)	0.063	0.078
Macroeconomic experience ($\lambda = 3$)	0.063	0.081
Personal experience ($\lambda = 1$)	0.057	0.186
Personal experience ($\lambda = 3$)	0.057	0.186
Number of observations (N)	1,514,196	
Number of different individuals (n)	139,461	

Notes: Table 1 reports means and standard deviations of the key variables over the sample period of 2004-2019. Consumption, 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.

compared to top-earners. It is also evident that this increase in the saving rate was persistent, at least through the sample period. This potentially points to a role of experience effects. The saving rate appears to be largely independent of the level of education as seen in Panel (b). Panel (c) shows that while the post-crisis saving rate is higher than the pre-crisis saving rate for all age groups, the increase seems most pronounced in the youngest age groups. Hence, in line with the hypothesis above, young individuals appear to raise their saving rate persistently after the GFC.

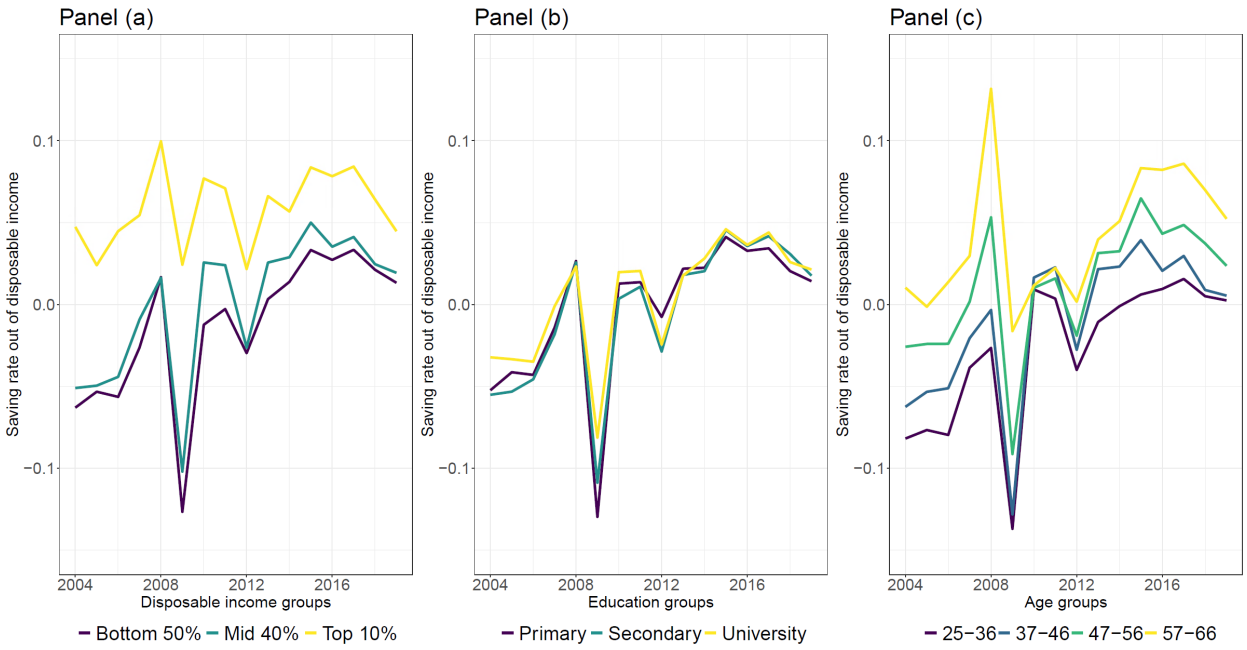
5.2 Empirical Framework

The objective of the empirical analysis is to analyze the relationship between consumption and past economic experiences given households' level of permanent income and other characteristics. In particular, I estimate

$$\log\left(C_{it}\right) = \alpha_i + \alpha_t + \rho u^R(\lambda)_{it} + \gamma u^P(\lambda)_{it} + \sum_{g=i}^{20} \delta_g \, Y^P_{git} + X_{it-1}\beta + \varepsilon_{it} \tag{9}$$

where C_{it} is consumption of household i in year t as computed by Equation 8. α_i and α_t are individual and time fixed effects. $u^R(\lambda)_{it}$ and $u^P(\lambda)_{it}$ are measures for the exposure to job-loss risk outlined by Equations (1) and (2) in Section 3 and personal experiences with job-loss,

Figure 7: Households' average saving rates in 2004-2019



Notes: Figure 7 shows households' average saving rate in 2004-2019 by disposable income, in Panel (a), education, in Panel (b), and age, in Panel (c). The income groups are formed within each birth cohort, defined as the year of birth, to ensure saving rates across income groups are not driven by life-cycle effects. Primary education refers to households where no household member has a higher level of education than corresponding to lower secondary education. Secondary school refers to households where the higher educated household member has an upper secondary education, post-secondary non-tertiary education, or a short-cycle tertiary education without a diploma. University education refers to households where the higher educated household member has a bachelor's degree, a master's degree, or a doctoral degree.

respectively. In constructing the former, I first identify an unemployed individual as someone who receives any unemployment benefits in a given year. I then regress the resulting unemployment dummy variable on a number of controls using a probit model to obtain individual-specific probabilities of becoming unemployed, given a set of characteristics.[7] Those probabilities are then weighted according to the weighting function in Equation (2) using $n = 5$ previous periods. The probability of becoming unemployed in period $t-1$ has the highest weight and the weights then fall linearly for probabilities prior to period $t-1$. Instead of using the unemployment probability in year $t-5$ as the earliest period that governs $u^R(\lambda)_{it}$ and $u^P(\lambda)_{it}$, I take the average of all estimated unemployment probabilities prior to $t-5$. As such, I utilize information on each individual going back to 1996, assuming they are old enough to have tax records at the time. $u^P(\lambda)_{it}$ is constructed using the same weighting scheme to a variable that equals one in years in which the individual receives any unemployment benefits and zero otherwise. As such, the difference between $u^M(\lambda)_{it}$ and $u^P(\lambda)_{it}$ is that the former is a measure of past job-loss risk while the latter is a measure of past experiences with actual job-loss.

[7]The controls are dummy variables for year, age, education, gender, marital status, number of children, sector of work, region of residence, urbanization and the number of past unemployment spells.

Appendix Table A4-1 shows the results using the unemployment probability in year $t-5$ rather than an average unemployment probability up to that point. The results are similar to the ones reported below in Section 5.3.

Figure 8 shows the estimated unemployment probabilities over time for different education and age groups. It shows how the probability of unemployment shot up in 2008-2009 at the peak of the financial crisis, and then gradually recovered although it never reached its pre-crisis rates. There are also indications that the crisis disproportionately affected younger and less educated individuals.

Guided by the theoretical framework above, I control for permanent income, Y_{git}^{P}, using dummy variables for each permanent income ventile. I obtain permanent income by regressing log real disposable income on individual and year fixed effects and a number of controls.[8] Finally, X_{it-1} denotes controls using dummy variables for age, education, marital status, gender, number of children, net wealth ventiles, sector of work, region of residence, urbanization, and the number of past unemployment spells. As such, I do not impose any functional form on the data. By lagging the control variables, I seek to avoid endogeneity issues.

The experience effects are identified exclusively from time variation in the within-household consumption and unemployment experiences, both risk experiences and personal experiences with unemployment. The average job-loss probability of a household over the economic cycle is likely endogenous as it involves, for example, the household's decisions on where to live, what to study and for how long. However, it is less clear that households can, in the short term, influence the variation in job-loss probabilities, which is the relevant identifying assumption underlying the analysis.

Example of such variations include macroeconomic fluctuations which can impact job-loss risk differently depending on household characteristics such as age, education, and region of residence. Furthermore, such exogenous variation in job-loss risk can come from changes in the demand for certain types of education or if certain sectors of the economy suffer from a recession, which can affect workers with skills specific to those sectors. This identifying assumption would not hold if households can influence their job-loss probabilities in the short-term by changing their observed characteristics, for example by moving from a high job-loss probability region to a low job-loss probability region. I attempt to alleviate those concerns by using lagged values of household characteristics. Moreover, Appendix Figure A2-1 shows the rate at which households move between regions between 1982 and 2019. Overall, the rate of moves between regions in Iceland does not appear particularly cyclical and is around 5-7% in most years. The two exceptions are the strong growth periods leading up to the GFC/banking collapse and the pandemic. Notably, however, cyclical downturns, prolonged periods of sluggish growth rates such as in the early 1990s, and economic crises do not seem to be characterized by low or high moving rates.

[8] The controls are dummy variables for age, education, gender, marital status, number of children, sector of work, region of residence, and urbanization.

Furthermore, I assume a constant association between consumption and observable characteristics. This implies that changes in saving behavior are attributed to changes in past job-loss risk or past personal experiences with unemployment rather than changes in the relationship between consumption and the characteristics. I also assume that households with similar observable characteristics and, thus, similar job-loss risk, consume similarly. This assumption entails that observed differences in consumption behavior across households are attributed to differences in past job-loss risk or past personal experiences, and not due to other unobserved factors.

Under the assumptions above, and assuming conventional life-cycle channels for consumption have been controlled for, ρ and γ, the key coefficients of interest, measure the consumption response to households' past experiences with unemployment and their exposure to periods of elevated job-loss risk, that is high unemployment among households with similar characteristics. Individual fixed effects control for time-invariant individual characteristics and ensure that experience effects are identified from time variation in the comovement of consumption and unemployment histories within the household. Year effects control for variation in consumption common to all households. Age fixed effects, included in X_{it-1}, capture life cycle patterns. As do income, wealth, marital status, and number of children which are also characteristics associated with consumption that are important to control for.

I use information from at least the previous five tax returns to compute the household-specific experience measures in the current period. As such, I omit households with fewer than six tax returns during the sample period. There is a trade-off between losing observations and allowing belief formation to be based on a longer period. Since the variables I need for identifying consumption and saving according to Equation (6) are only available from the early 2000s and I want to include the period surrounding the GFC, I restrict belief formation to be formed over the past 5 years. Like in Section 3, I choose a weighting parameter of $\lambda = 1$ for the weighting function in Equation (2). For robustness, I also show the key results using a weighting parameter of $\lambda = 3$, which entails that households assign a higher weight to more recent experiences. Appendix Table A3-1 also reports results for $\lambda = 0$, which assumes households place an equal weight on past experiences. The key results are robust to those changes in the weighting parameter, although interestingly the estimated experience effects are still somewhat stronger using the constant weight.

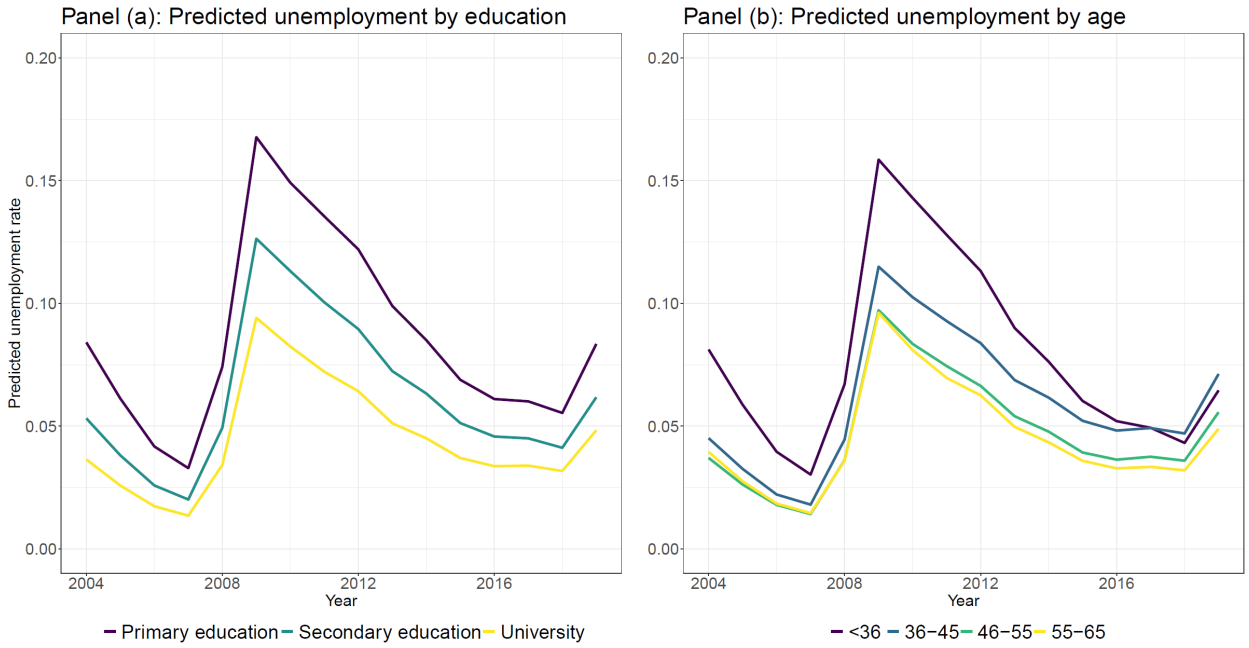
5.3 Results

5.3.1 The Persistent Effect of Experience on Consumption

Table 2 shows the key estimation results from equation (9). The first three columns show estimates based on a linearly declining weight ($\lambda = 1$). The last three columns show estimates based on a weight that puts more emphasis on recent experiences ($\lambda = 3$).

Overall, the results suggest that both past risk experiences and past personal unemployment

Figure 8: Average predicted unemployment rates over time by group



Notes: Figure 8 shows average predicted unemployment rates by education (Panel (a)) and age (Panel (c)). It represents results from estimating a probit model with an indicator variable for unemployment as the dependent variable. The independent variables are dummy variables for age, education, marital status, gender, number of children, net wealth ventiles, sector of work, region of residence, urbanization, and the number of past unemployment spells.

experiences reduce current consumption. Focusing first on risk experience, I find, controlling for permanent income and other characteristics, that individuals who observed and lived through periods of elevated job-loss risk subsequently reduce their consumption. The effect is highly statistically significant in all six specifications. Given the identifying assumptions, the results suggest past macroeconomic unemployment experiences have a persistent negative effect on consumption given permanent income and other characteristics. This is in line with the behavior of belief-persistent agents in the theoretical framework of Section 3.

Moreover, the effects are economically meaningful. In particular, a one percentage point increase in exposure to job-loss risk reduces consumption by 0.09-0.16%. This is equivalent to a one standard deviation increase in macroeconomic unemployment experiences reducing consumption by 0.8-1.2%. The estimates are within the range found in the literature mentioned above. Since private consumption equals approximately 50% of GDP in Iceland, such an increase in past risk experiences for all individuals might subsequently suppress consumption, given income and other characteristics, by around 0.4-0.6% of GDP. In the appendix, I reproduce the results for a sample containing only those who never become unemployed. This is potentially important since although one might expect those who are merely exposed to variation in job-loss risk to respond by reducing their consumption, consumption smoothing behavior of those who actually become unemployed and suffer a loss of income should result in higher consumption

and lower saving given income. This, in turn, might bias the risk experience estimates toward zero. However, Table A4-1 shows that the results reported below are robust to the exclusion of households that suffer unemployment spells within the sample period.

Turning to personal unemployment experiences, the results suggest past experiences with job-loss subsequently lowers household consumption. The coefficient is very robust to the specifications shown in Table 2. All specifications suggest that a one percentage point increase in past personal unemployment experiences reduces consumption by 0.06-0.07%. This is equivalent to a one standard deviation in personal unemployment experiences reducing consumption by 1.0-1.2%.

Notice that the magnitude of the coefficients, once put in the context of a one standard deviation increase in each experience measure, is very similar. This means that individuals who simply observe or live through periods of high unemployment among others with similar characteristics, without becoming unemployed themselves, subsequently reduce consumption to a similar extent as those who become unemployed. This is controlling for income and other characteristics with a lag.

Table 2: Experience Effects and Consumption

	(1)	(2)	(3)	(4)	(5)	(6)
Risk experience	-0.160***		-0.140***	-0.139***		-0.094***
	(0.023)		(0.022)	(0.021)		(0.020)
Personal experience		-0.067***	-0.065***		-0.067***	-0.064***
		(0.004)	(0.004)		(0.004)	(0.004)
n	139,461	139,461	139,461	139,461	139,461	139,461
N	1,424,862	1,424,862	1,424,862	1,424,862	1,424,862	1,424,862
R^2	0.539	0.539	0.539	0.539	0.539	0.539
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	$\lambda = 1$	$\lambda = 1$	$\lambda = 1$	$\lambda = 3$	$\lambda = 3$	$\lambda = 3$

*Notes: Table 2 shows the estimated experience effects, both risk experience ($\hat{\rho}$) and personal unemployment experience ($\hat{\gamma}$) estimated using Equation (9). The first three columns show estimates based on a linearly declining weight ($\lambda = 1$). The last three columns show estimates based on a weight that puts more emphasis on recent experiences ($\lambda = 3$). The dependent variable is the logarithm of consumption computed using Equation (2), converted to 2019 prices using Statistics Iceland's CPI. The model is estimated with individual fixed effects (FE), year FE and various controls. The controls are dummy variables for age, education, marital status, gender, number of children, net wealth ventiles, sector of work, region of residence, and urbanization. N is the total number of observations and n is the number of different individuals. R^2 is the adjusted coefficient of determination. Standard errors, clustered at the cohort-level, are in parentheses. *** denotes significance at the 1% significance level, ** denotes significance at the 5% level, and * denotes significance at the 10% level.*

5.3.2 The Persistent Effect of Experience on Income

Not only have I found evidence of a negative relation between past personal experiences with unemployment and future consumption, the results in the previous section suggest that merely a history of increased job-loss probabilities, that is past experiences of living through periods of elevated job-loss risk, predicts lower future consumption, irrespective of whether an individual actually becomes unemployed. The fact that elevated probabilities of job loss predict lower future consumption suggests that hysteresis, that is the persistence of unemployment and the protracted effects of shocks on unemployment due to the loss of workers' skills and their inability to maintain and upgrade their skills when out of work, might not be the sole explanation of persistently depressed demand following such large negative economic shocks as the GFC. I now turn to analyzing whether the experience effects reported in Table 2 possibly stem for lower future income after experiencing either increased job-loss risk or actually becoming unemployed.

The loss of human capital following economic shocks, central to the hysteresis argument, predicts persistently lower income for those who lose their jobs. Consequently, the sustained decrease in income leads to persistently lower consumption. Furthermore, it could also be the case that individuals who are not unemployed themselves, but share characteristics with others who frequently are unemployed, suffer from lower income as well. This could for example be the case if demand for certain types of education persistently decreases or if a certain sector of the economy suffers from a recession and workers in the sector are forced to take lower paid jobs elsewhere. Both persistently lower incomes following unemployment spells and potentially lower income associated with high unemployment among individuals with similar characteristics are rational explanations for lowering one's consumption even without the existence of experience effects.

While this is one reason for controlling for income and wealth in the regression results reported in Table 2, I also estimate equation (9) with disposable income as the dependent variable. This is to assess whether risk experiences and personal unemployment experiences predict lower income in the future. Table 3 shows the results. The top panel shows the association between past experiences and current disposable income, the middle panel shows the same relationship for disposable income four years ahead, and the bottom panel shows it for disposable income eight years in the future.

Starting with the top panel, past exposure to periods of elevated job-loss risk does not seem to predict current disposable income. However, past personal experiences with unemployment do predict lower current disposable income. What is more, the magnitude of the coefficient is similar to the ones found in Table 2. Importantly, this is not the case for macroeconomic unemployment experiences.

Looking at results for disposable income four years ahead, the association between personal unemployment experiences and future income has vanished. However, past macroeconomic experiences appear to predict lower disposable income four years into the future. Importantly,

however, the coefficients are substantially and significantly lower than those found in Table 2. Furthermore, the association between disposable income and both personal unemployment experiences and risk experiences has disappeared looking eight years into the future.

Overall, while past personal unemployment experiences predict lower income at the same horizons as reported in Table 2, the same is not true for past risk experiences. Although I find some evidence that past exposure to job-loss risk reduces future disposable income, those effects are only found for disposable income four years into the future. Furthermore, they are not large enough to explain the findings reported in Table 2.

Note also that the results in the bottom two panels, suggesting past personal experiences with unemployment do not predict lower disposable income four and eight years into the future, do not necessarily refute the hysteresis argument for long-lasting scars from seemingly transitory negative shocks. First, the top panel does suggest past unemployment spells are indeed associated with lower income. Second, and more importantly, I control for income and other characteristics in the regressions. As such, the channels hysteresis works through have, at least to an extent, already been accounted for.

Although there is limited evidence of reduced disposable income following past exposure to macroeconomic unemployment, such experiences could potentially lead to greater future income volatility. As such, precautionary saving motives could lead individuals to rationally raise saving and reduce consumption given their level of income if that income is subject to more fluctuations than before.

To test this I estimate equation (9) with income volatility as the dependent variable. In defining income volatility, I adopt the approach of Malmendier and Shen (2024), and follow Meghir and Pistaferri (2004) and Jensen and Shore (2015). I separate transitory and permanent income volatility. Transitory volatility is the squared two-year change in excess log disposable income. Excess log disposable income is the difference between actual disposable income and permanent income, estimated as outlined in Section 5.2. Permanent income volatility is the product of two-year changes in excess log labor earnings (between years t and $t-2$) and the six-year changes that span them (between years $t+2$ and $t-4$).

The results for current disposable income volatility are shown in Table 4. The top part of the table shows results for transitory income volatility while the bottom part shows results for permanent income volatility. The weighting scheme is $\lambda = 1$ in columns (1)-(3) and $\lambda = 3$ in columns (4)-(6). The results do not suggest a relation between past exposure to job-loss and higher current income volatility. On the contrary, past exposure to unemployment is associated with significantly lower income volatility in six out of eight specifications, including risk experiences. However, past personal unemployment experiences are associated with increased income volatility. Appendix Table A5-1 shows that looking at income volatility four years into the future those effects have mostly vanished. Out of the twelve specifications, only two are significant at the 10% significance level, suggesting some correlation between macroeconomic experiences and transitory income volatility four years later. In the other ten specifications, the

Table 3: Experience Effects and Future Income

	Income $_t$	Income $_t$	Income $_t$
Risk experience	-0.008		0.011
	(0.016)		(0.015)
Personal experience		-0.062***	-0.062***
		(0.003)	(0.003)
n	139 461	139 461	139 461
N	1 424 862	1 424 862	1 424 862
R^2	0.832	0.832	0.832
	Income $_{t+4}$	Income $_{t+4}$	Income $_{t+4}$
Risk experience	-0.072***		-0.074***
	(0.017)		(0.017)
Personal experience		0.007***	0.008***
		(0.002)	(0.002)
n	136 566	136 566	136 566
N	870 476	870 476	870 476
R^2	0.855	0.855	0.855
	Income $_{t+8}$	Income $_{t+8}$	Income $_{t+8}$
Risk experience	-0.010		-0.010
	(0.019)		(0.019)
Personal experience		0.005	0.005
		(0.003)	(0.003)
n	90 118	90 118	90 118
N	395 144	395 144	395 144
R^2	0.898	0.898	0.878
Income controls	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes
Experience weighting	$\lambda = 1$	$\lambda = 1$	$\lambda = 1$

*Notes: Table 3 shows results from estimating Equation (9) with disposable income at time t, $t+4$, and $t+8$ as the dependent variable. The estimates are based on a linearly declining weight ($\lambda = 1$). The model is estimated with individual fixed effects (FE), year FE and various controls. The controls are dummy variables for age, education, marital status, gender, number of children, net wealth ventiles, sector of work, region of residence, and urbanization. N is the total number of observations and n is the number of different individuals. R^2 is the adjusted coefficient of determination. Standard errors, clustered at the cohort-level, are in parentheses. *** denotes significance at the 1% significance level, ** denotes significance at the 5% level, and * denotes significance at the 10% level.*

experience coefficients are economically small and statistically insignificant at conventional significance levels.

Overall, increased future income volatility does not play an important role in the consumption

Table 4: Experience Effects and Income Volatility in t

	(1)	(2)	(3)	(4)	(5)	(6)
Transitory						
Risk experience	-0.053**		-0.066***	-0.058***		-0.089***
	(0.024)		(0.025)	(0.021)		(0.021)
Personal experience		0.040***	0.041***		0.040***	0.043***
		(0.006)	(0.006)		(0.006)	(0.006)
n	139,461	139,461	139,461	139,461	139,461	139,461
N	1,350,218	1,350,218	1,350,218	1,350,218	1,350,218	1,350,218
R^2	0.107	0.107	0.107	0.107	0107	0.107
Permanent						
Risk experience	-0.005		-0.007	-0.022*		-0.030**
	(0.012)		(0.012)	(0.012)		(0.012)
Personal experience		0.011***	0.011***		0.011***	0.012***
		(0.003)	(0.003)		(0.003)	(0.003)
n	136,834	136,834	136,834	136,834	136,834	136,834
N	928,518	928,518	928,518	928,518	928,518	928,518
R^2	0.112	0.112	0.112	0.112	0.112	0.112
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	$\lambda = 1$	$\lambda = 1$	$\lambda = 1$	$\lambda = 3$	$\lambda = 3$	$\lambda = 3$

*Notes: Table 4 shows results from estimating Equation (9) with transitory income volatility and permanent income volatility as the dependent variable. Transitory volatility is the squared two-year change in excess log disposable income. Excess log disposable income is the difference between actual disposable income and permanent income, estimated as outlined in Section 5.2. Permanent income volatility is the product of two-year changes in excess log labor earnings (between years t and $t-2$) and the six-year changes that span them (between years $t+2$ and $t-4$). The estimates are based on a linearly declining weight ($\lambda = 1$). The model is estimated with individual fixed effects (FE), year FE and various controls. The controls are dummy variables for age, education, marital status, gender, number of children, net wealth ventiles, sector of work, region of residence, and urbanization. N is the total number of observations and n is the number of different individuals. R^2 is the adjusted coefficient of determination. Standard errors, clustered at the cohort-level, are in parentheses. *** denotes significance at the 1% significance level, ** denotes significance at the 5% level, and * denotes significance at the 10% level.*

response to past experiences of job-loss risk. However, there are some indications it might do so in the short run following actual job-loss.

5.3.3 The Persistent Effect of Experience on Wealth

Thus far, I have found evidence of a persistent consumption response to both risk experiences and personal experiences. Furthermore, I have shown this response is at most only partially

explained by lower income following negative experiences. Increased income volatility following such experiences also does not seem to play an important role.

This is consistent with the experience hypothesis, that households who have experienced periods of heightened risk or gone through personal hardship tend to be more careful in their consumption and saving decisions going forward, relative to the counterfactual that they had not had those experiences. As such, and as opposed to the labor market scarring mechanism where job-loss causes the loss of human capital and, thus, lowers consumption through depressed income, the experience hypothesis suggests going through economic hardship causes households to save more and, hence, accumulate more wealth than they would have otherwise.

I test this prediction by estimating Equation (9) with log future assets, deflated by the CPI, as the dependent variable. The idea is to assess whether unemployment experiences, whether risk experiences or personal, predict future wealth accumulation. I use assets as a proxy for wealth as net wealth itself is frequently negative over the sample period, particularly in the aftermath of the financial crisis, thus preventing me from taking logs. Appendix Table A6-1 shows the results using the logarithm of net wealth as the dependent variable. The results are robust, and suggest even somewhat stronger wealth accumulation following exposure to job-loss risk, although at the loss of approximately 15% of the observations.

Furthermore, note that unlike other results in the paper, this analysis does not rely on the imputation of consumption or saving. The level of assets is third-party reported directly into the administrative data. As such, it alleviates potential reservations about the quality of the imputation. However as discussed in Section 4, a caveat is that this variable does not include the market value of stocks and bonds, unless it is held indirectly through equity funds, bond funds, or mixed funds.

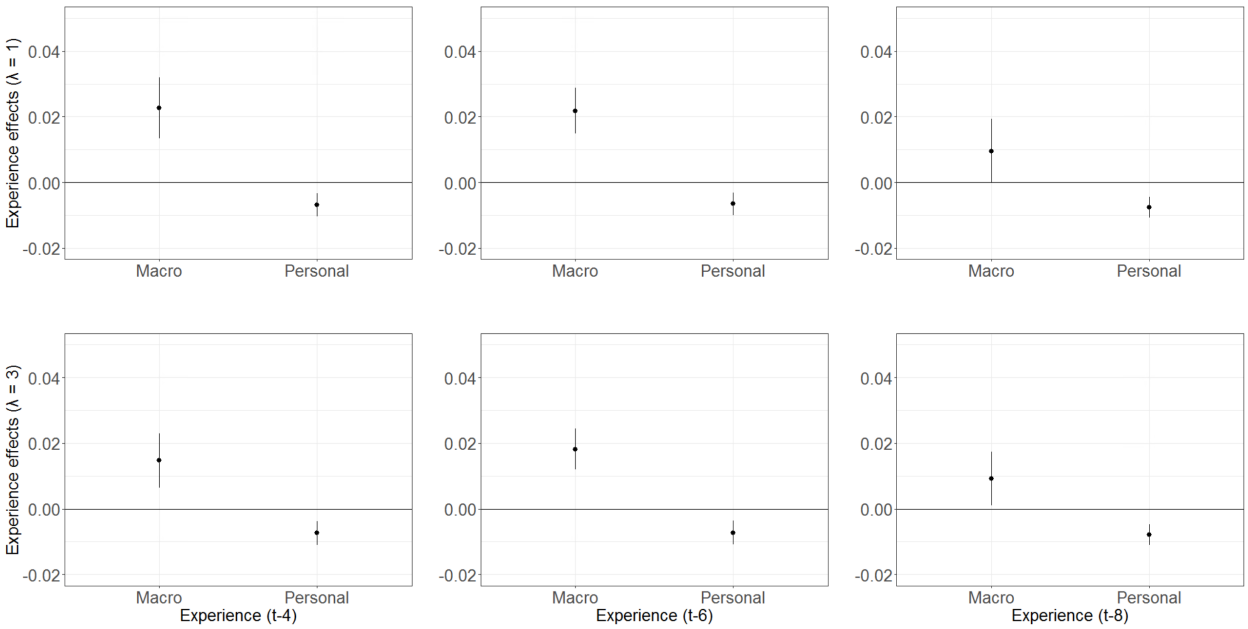
Figure 9 illustrates the results. The top panels show results with an experience weight of $\lambda = 1$ while the bottom panels show results with an experience weight of $\lambda = 3$. Going from left to right, the first column shows results from regressions with four lags of the experience measures serving as the key independent variable, the second column shows results for six lags of the experience measures, and the third column shows results for eight lags. As the accumulation of wealth through saving takes time, I use four, six, and eight lags of the experience measures to obtain the results. This allows me to look at longer horizons while keeping as many observations as possible in the sample.

As can be seen, higher individual-level job-loss risk predicts wealth buildup in the future. The effect is statistically significant and economically meaningful in all specifications. A one standard deviation increase in perceived job loss probabilities predicts additional wealth buildup of approximately 2% four and six years later. The effect on wealth eight years into the future is somewhat smaller, but still around 1% and significant at the 5% significance level.

I do not find evidence of wealth buildup among those who not only experience times of elevated job-loss risk, but actually do become unemployed. On the contrary, the estimates suggest a one standard deviation in personal unemployment experience predicts lower level of

wealth four, six, and eight years into the future. The estimates are statistically significant at the 1% significance level.

Figure 9: Effects of a One Standard Deviation Increase in Unemployment Experience on Future Assets



Notes: Figure 9 shows the effect of a one standard deviation increase in past exposure to personal experiences and risk experiences on households' assets. It is found by estimating Equation (9) using the log of assets, deflated by the CPI, as the dependent variable. The top panels show estimates assuming linearly declining weights for past unemployment experiences ($\lambda = 1$). The bottom panels show estimates assuming households put less weight on recent past unemployment experiences ($\lambda = 3$). Going from left to right, the Figure shows estimates of unemployment experiences on assets four, six and eight years into the future. The results for assets four year into the future are based on 870,341 observations of 136,552 individuals. The results for assets six years into the future are based on 609,255 observations of 112,889 individuals. The results for assets eight years into the future are based on 395,114 observations of 90,110 individuals. The black dots represent the point estimates and the black vertical lines represent 95% confidence intervals based on standard errors clustered at the cohort level.

Overall, the results are consistent with the experience hypothesis, that households who observe and live through rough economic times, with an elevated risk of job-loss for households with similar characteristics, tend to subsequently consume less. The results are not fully explained by lower income or an increased volatility in income. Moreover, those households tend to subsequently accumulate wealth which is consistent with the experience hypothesis, in that economic crises and downturns persistently impact households' beliefs about the risk and uncertainty they face. As such, they optimally and persistently reduce their consumption and raise their target wealth. This wealth accumulation effect is, however, somewhat at odds with other explanations for depressed consumption following periods of macroeconomic turbulence.

5.4 Heterogeneity

The analysis in Section 5.3 confirms that, under the identification assumptions, households reduce their consumption and, thus, raise saving after facing a period of heightened job-loss risk. Below, I examine whether the consumption response to past experiences differs across subgroups. In doing so, I estimate equation (9) with the relevant subgroup interacted with the measures for past exposure to job-loss risk. As such, the regressions in this section focus solely on past job-loss risk and do not include measures of past experience with unemployment.

First, I explore the role of permanent income. Lower income households might be more likely to face a binding liquidity constraint than higher income households, thus having limited capacity to reduce consumption when faced with elevated risk. One might, therefore, expect a more muted consumption response to elevated risk among low-income households compared to high-income households. To test this, I construct a dummy variable for households in the top 20% of the permanent income distribution. The variable is based on the distribution of estimated permanent income within each birth cohort. Although the estimated permanent income should take the individuals' position in the life cycle into account, this helps to make sure that the differences in consumption responses across income groups are not driven by life-cycle effects.

I then turn to testing the role of education. Figure 8 showed that unemployment rose more for lower educated households compared to higher educated households in the wake of the financial crisis. Furthermore, Rubinstein and Tsiddon (1999) find that higher educated individuals experience less cyclical unemployment and wages. They conclude that the less educated individuals with less educated parents bear the heaviest burden of recessions as both their wages and employment are highly procyclical. Similarly, Shi et al. (2022) find that education moderates the strength of unemployment scarring. Therefore, there might be some heterogeneity in the consumption response to risk across education groups. Specifically, the literature suggests lower educated households might optimally raise their precautionary savings more than higher educated households in the wake of increased macroeconomic uncertainty. I, therefore, construct three dummy variables for education: primary education, secondary education and university education. Finally, I gauge whether the consumption response to risk is age-dependent. This could be the case if, as Panel (b) of Figure 8 suggests, younger individuals' labor market status is more affected by the business cycle than that of older households. This might lead younger households to optimally raise their savings more when faced with elevated income risk.

Figure 10 shows the results from the above heterogeneity analysis. The dots represent the coefficient estimates and the vertical lines represent 95% confidence intervals. Panel (a) shows that the experience effects of the top permanent income quintile are larger than for lower income households. The coefficient estimate for the top permanent income quintile is -0.204 compared to -0.155 for lower quintiles. The difference is statistically significant at the 10% significance level. Economically, the coefficients are similar. One potential reason for this is that while higher-income households appear to have more labor market security compared to lower income

households and, thus, should not need the same buffer-stock wealth to ensure against shocks other things being equal, lower-income households might not have the same resources to adjust their level of consumption following economic fluctuations as do higher-income households.

The results shown in Panel (b) suggest that, while small, the experience effects are significant at 5% and 1% significance levels for households with primary and secondary education, respectively. The coefficient estimate is -0.073 for primary educated households and -0.149 for secondary educated households and the difference between the two is significant at the 1% significance level, However, it is much larger (-0.303) for university educated households. The results are difficult to square with the fact that less educated households' wages and labor market status are generally more affected by the business cycle and times of elevated uncertainty and risk. However, they are consistent with the ones found in Panel (a) which shows that the experience effect is rising, in absolute terms, in permanent income. This is in line with the fact that higher permanent income is positively associated with higher education, in fact education is often used as a proxy for permanent income.

Finally, Panel (c) of Figure 10 shows that the coefficient estimate for younger households (-0.221) is larger, in absolute terms, than that for middle-aged households (-0.033). The latter is not significantly different from zero at conventional significance levels. However, the difference between the two is statistically significant at the 1% significance level. This is in line with the evidence from Figure 8 suggesting economic crises are associated with a larger increase in job-loss risk for young households compared to their older counterparts.

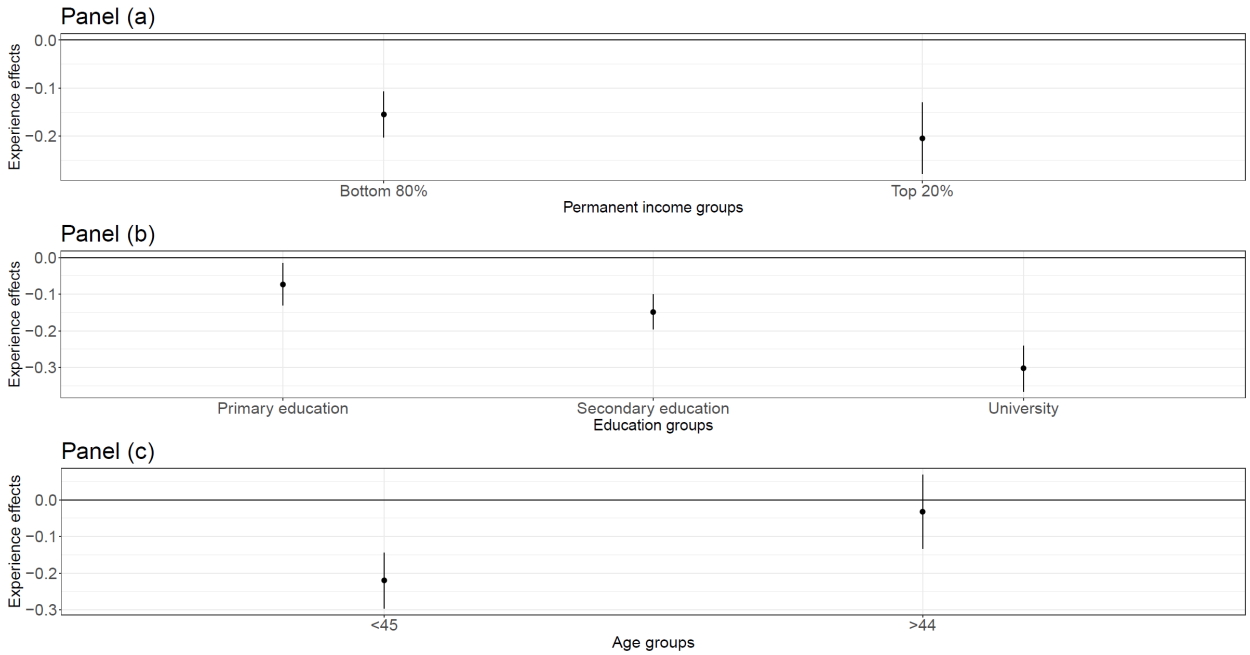
Taken together, the results of the heterogeneity analysis suggest that the experience effects from exposure to elevated job-loss risk are particularly pronounced for younger, higher-income and more educated households. Age, permanent income and education, which is a common proxy for permanent income, thus seem to be important factors for how past exposure to job-loss risk predicts lower consumption.

6 Discussion & Conclusion

In this paper, I argue that periods of uncertainty and risk can persistently affect households' beliefs about future economic outcomes, thus, leading to an increase in precautionary savings. Economic crises are typically associated with a sharp increase in uncertainty and risk for a significant share of households. The resulting increase in precautionary saving implies a persistent and economically meaningful drag on demand. Furthermore, it could also explain the increased national saving and stronger current account balances experienced by many countries following a crisis.

More specifically, using Icelandic administrative third-party reported tax records and a sample period that includes wild macroeconomic fluctuations, providing a unique opportunity to study households' precautionary saving behavior, I find evidence that households who have lost their jobs in the past tend to subsequently reduce their consumption. Not only that, I use information on background characteristics in the tax records to estimate individual-level job-loss risk and find

Figure 10: Heterogeneity in the consumption response to risk



Notes: Figure 10 plots the uncertainty effect on consumption, estimated using equation (9) with various subgroups interacted with the uncertainty variable. Panel (a) shows the uncertainty effect separately for the top 20% of the permanent income distribution within each cohort, defined as an individual's birth year, and the bottom 80% of the same distribution. Panel (b) shows the uncertainty effect by education. Primary education refers to households where no household member has a higher level of education than corresponding to lower secondary education. Secondary school refers to households where the higher educated household member has an upper secondary education, post-secondary non-tertiary education, or a short-cycle tertiary education without a diploma. University education refers to households where the higher educated household member has a bachelor's degree, a master's degree, or a doctoral degree. Panel (c) shows the uncertainty effect by age groups. The black dots represent the point estimates and the black vertical lines represent 95% confidence intervals based on standard errors clustered at the cohort level.

that households who have merely been exposed to elevated risk of job-loss, without actually losing their job, also respond to such risk by subsequently reducing their consumption. This behavior is only to a minor extent explained by lower future income following episodes of high job-loss risk or actual job-loss. Furthermore, I show that, as opposed to those who actually lose their jobs, those merely exposed to increased risk of job-loss subsequently accumulate higher wealth. A heterogeneity analysis reveals that younger, higher-income and more educated households, in particular, sharply reduce their consumption when faced with elevated uncertainty.

This is consistent with what has been called the experience effect, the idea that large economic events, such as crises and recessions, tend to persistently affect households' beliefs about future outcomes. As households update their beliefs and assume an increased risk of future shocks, precautionary saving motives trigger persistently lower consumption, higher saving and wealth accumulation among those households. At the macroeconomic level, this can reduce a country's current account deficits or raise their surpluses. This has been the experience of many countries following large shocks, such as the Nordics after the early 1990s banking crisis, many Southeast Asian countries following the 1997 Asian financial crisis, and Iceland, but also the Baltics, Spain

and Portugal, following the 2008 GFC/banking collapse.

More recently, most countries were hit by the COVID-19 pandemic. Both the pandemic itself and authorities' containment and mitigation strategies led to a sharp contraction in economic activity. However, a swift and forceful economic policy response shielded households against the economic consequences of the pandemic and acted as an insurance against elevated economic uncertainty due to the virus. In the conceptual framework of Section 3, this is equivalent to raising the replacement rate, ζ. In fact, Ganong et al. (2020) find that replacement rates in the U.S. were often above 100%, meaning that benefits exceeded lost wages. Although the risk of job-loss clearly rose, US households were on average more than completely insured against the economic effects of the pandemic. While the US economic policy response was particularly forceful in the wake of the pandemic, monetary and fiscal authorities in most advanced economies responded aggressively. As such, the theoretical framework does not imply long-lasting scars to consumption.

At the time the rhetoric among policymakers was that the cost of overdoing stimulus measures was lower than the cost of not doing enough. Indeed, in an October 2020 speech, Federal Reserve chair Jerome Powell warned that "too little support would lead to a weak recovery, creating unnecessary hardship for households and businesses", and that "by contrast, the risk of overdoing it seems, for now, to be smaller"

The paper sheds light on what policymakers were potentially afraid of. A period of dramatically increased job-loss risk, especially if coupled with a low replacement rate for those who actually lose their jobs, can cast long shadows on the economy by persistently affecting households' beliefs and scarring demand. As such, inherently transitory shocks can potentially have long-lasting and negative demand effects even if they do not affect workers' skills.

The experience in Iceland following the GFC/banking collapse episode seems to support this. Greater prudence and a stronger propensity to save compared to the years prior to the crisis seems to characterize Icelandic households. Figure 7 shows that households' saving rates have increased for all age and education groups and all but the top income group. The country's current account has turned into persistent surpluses after years of large deficits and the consumption share of GDP has decreased by some 6-7% of GDP. After sluggish growth in the wake of the GFC/banking collapse, the recovery gained substantial steam from 2013.

The results point to an important role for economic policy, be it automatic stabilizers or discretionary monetary or fiscal measures, to support households through large negative economic shocks if long-lasting depressed consumer demand is to be avoided. Future research could better zoom in on the crisis episode and analyze its direct impact, not merely of job-loss risk estimated over the crisis period, on subsequent households consumption. Furthermore, merging administrative tax records with information from consumer confidence surveys could shed better light on how job loss risk affects households' beliefs.

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Appendix

A1. Experience Effects Using only Data for the Past Five Years

In the main specifications, I compute the experience measure using the past four years of either exposure to job-loss risk or to actually becoming unemployed and the average experience from all periods up to period $t-5$. Here, I show the robustness of the results by only considering the past five years of experience. As such, the experience up to period $t-5$ is replaced by the experience in period $t-5$ but everything else stays the same. The results, reported in Table A1-1 are similar to the ones of Table 2 in the main text.

Table A1-1: Experience Effects and Consumption

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Risk)	-0.152***		-0.093***	-0.139***		-0.094***
	(0.023)		(0.021)	(0.021)		(0.020)
Experience (Personal)		-0.079***	-0.075***		-0.067***	-0.064***
		(0.005)	(0.005)		(0.004)	(0.004)
n	139,461	139,461	139,461	139,461	139,461	139,461
N	1,424,862	1,424,862	1,424,862	1,424,862	1,424,862	1,424,862
R^2	0.539	0.539	0.539	0.539	0.539	0.539
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	$\lambda = 1$	$\lambda = 1$	$\lambda = 1$	$\lambda = 3$	$\lambda = 3$	$\lambda = 3$

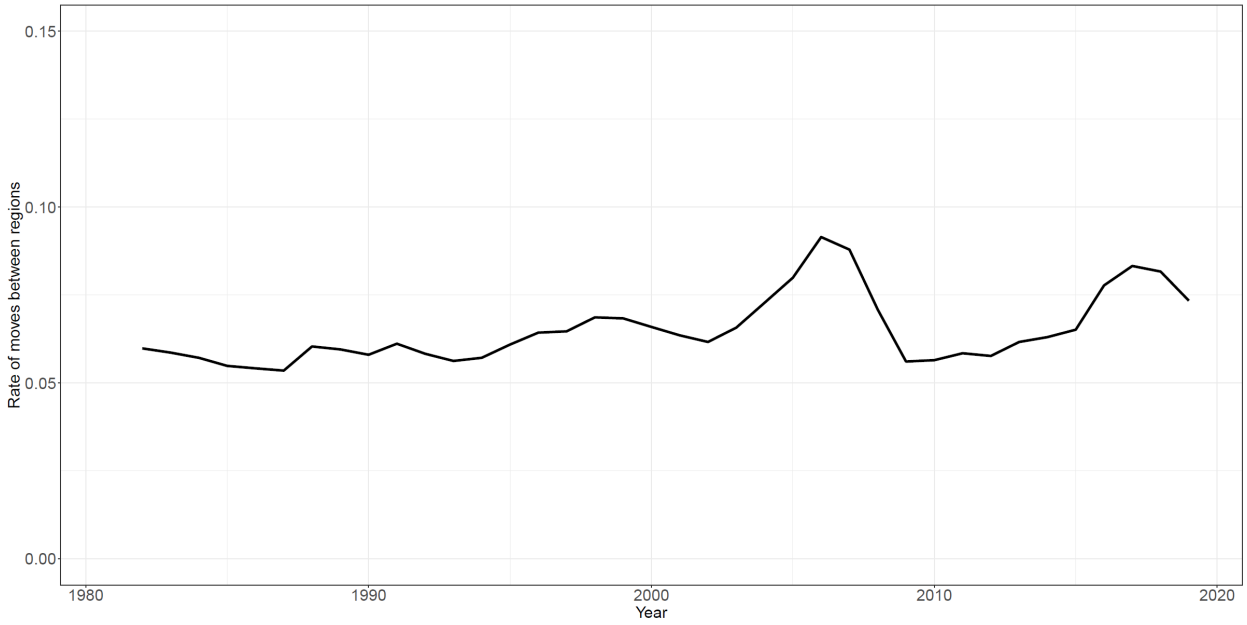
*Notes: Table A1-1 shows the estimated experience effects, both risk experiences ($\hat{\rho}$) and personal experiences ($\hat{\gamma}$) estimated using Equation (9). The first three columns show estimates based on a linearly declining weight ($\lambda = 1$). The last three columns show estimates based on a weight that puts more emphasis on recent experiences ($\lambda = 3$). The dependent variable is the logarithm of consumption computed using Equation (8), converted to 2019 prices using Statistics Iceland's CPI. The model is estimated with individual fixed effects (FE), year FE and various controls. The controls are dummy variables for age, education, marital status, gender, number of children, net wealth ventiles, sector of work, region of residence, and urbanization. N is the total number of observations and n is the number of different individuals. R^2 is the adjusted coefficient of determination. Standard errors, clustered at the cohort-level, are in parentheses. *** denotes significance at the 1% significance level, ** denotes significance at the 5% level, and * denotes significance at the 10% level.*

A2. The Rate of Moves Between Regions

The empirical analysis assumes that households have limited ways of affecting the probability of job-loss they face. One of the ways in which they arguably can do that is by moving between

regions, particularly from plausible high-unemployment regions to plausible low-unemployment regions. Figure A2-1 shows the rate of moves between regions in Iceland. For the most part it appears relatively constant with two exceptions, both characterized by rapid economic growth. First is the growth phase in the early 2000s preceding the GFC/banking collapse. Second is the growth period leading up to the pandemic. However, the rate of moves does not appear low or high during economic downturns. This applies, for example, to the long period of contraction and sluggish growth in the early 1990s and the period in the wake of the dramatic GFC/banking collapse.

Figure A2-1: Rate of moves between regions in Iceland



Notes: Figure A2-1 shows the rate at which people move between regions in Iceland.

A3. Robustness Check Using Constant Weights

The results from the baseline specification, for example those reported in Table 2, are obtained using declining weights. This implies that when households form beliefs about future outcomes, they give a higher weight to recent past experiences compared to those in the relatively more distant past. The baseline specification uses linearly declining weights ($\lambda = 1$ in Equation 2) and weights where a relatively higher weight is put on more recent experiences ($\lambda = 3$ in Equation 2). In this section, I show results using constant weights, implying that experiences from the distant past are equally important to the household as recent experiences. Table X reports the results. As can be seen by comparing Table 2 and Appendix Table A3-1, the results do not materially differ. However, it is interesting that using constant, instead of assuming households put a higher weight on recent past experiences compared to more distant past experiences, yields somewhat higher estimates of experience effects.

Table A3-1: Experience Effects and Consumption Using Constant Weights ($\lambda = 0$)

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Macro)	-0.209***		-0.132***			
	(0.032)		(0.029)			
Experience (Personal)		-0.103***	-0.096***			
		(0.009)	(0.008)			
n	139,461	139,461	139,461			
N	1,424,862	1,424,862	1,424,862			
R^2	0.539	0.539	0.539	0.539	0.539	0.539
Demographic controls	Yes	Yes	Yes			
Income controls	Yes	Yes	Yes			
Wealth controls	Yes	Yes	Yes			
Age fixed effects	Yes	Yes	Yes			
State fixed effects	Yes	Yes	Yes			
Year fixed effects	Yes	Yes	Yes			
Individual fixed effects	Yes	Yes				
Experience weighting	$\lambda = 0$	$\lambda = 0$	$\lambda = 0$			

*Notes: Table A3-1 shows the estimated experience effects, both risk experience effects (ρ) and personal experience effects (γ) estimated using Equation (9). The estimates are based on a constant weight ($\lambda = 0$) in Equation (2). The dependent variable is the logarithm of consumption computed using Equation (8), converted to 2019 prices using Statistics Iceland's CPI. The model is estimated with individual fixed effects (FE), year FE and various controls. The controls are dummy variables for age, education, marital status, gender, number of children, net wealth ventiles, sector of work, region of residence, and urbanization. N is the total number of observations and n is the number of different individuals. R^2 is the adjusted coefficient of determination. Standard errors, clustered at the cohort-level, are in parentheses. *** denotes significance at the 1% significance level, ** denotes significance at the 5% level, and * denotes significance at the 10% level.*

Table A4-1: Experience Effects and Consumption

	(1)	(2)
Experience (Risk)	-0.175***	-0.139***
	(0.040)	(0.035)
n	99,961	99,961
N	1,036,888	1,036,888
R^2	0.526	0.526
Demographic controls	Yes	Yes
Income controls	Yes	Yes
Wealth controls	Yes	Yes
Age fixed effects	Yes	Yes
State fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Individual fixed effects	Yes	Yes
Experience weighting	$\lambda = 1$	$\lambda = 3$

*Notes: Table A4-1 shows the estimated experience effects, both risk experience effects ($\hat{\rho}$) and personal experience effects ($\hat{\gamma}$) estimated using Equation (9) and a sample that excludes households which have had unemployment spells within the sample period. The first column shows estimates based on a linearly declining weight ($\lambda = 1$). The second column shows estimates based on a weight that puts more emphasis on recent experiences ($\lambda = 3$). The dependent variable is the logarithm of consumption computed using Equation (8), converted to 2019 prices using Statistics Iceland's CPI. The model is estimated with individual fixed effects (FE), year FE and various controls. The controls are dummy variables for age, education, marital status, gender, number of children, net wealth ventiles, sector of work, region of residence, and urbanization. N is the total number of observations and n is the number of different individuals. R^2 is the adjusted coefficient of determination. Standard errors, clustered at the cohort-level, are in parentheses. *** denotes significance at the 1% significance level, ** denotes significance at the 5% level, and * denotes significance at the 10% level.*

A4. Risk Experiences Without Those who Have Been Unemployed

Since I am interested in the consumption response to both past experiences of job-loss risk and to past unemployment spells, the sample in the baseline specifications includes both households that experience unemployment spells and those that do not. One could argue that in estimating the risk experience effects, the sample should only include households that do not actually become unemployed, but merely are faced with variation in job-loss risk. While I do expect households exposed to elevated job-loss risk to reduce their consumption and increase their savings to insure against the higher perceived risk, I expect those who actually experience the shock of losing their job to smooth consumption and reduce their saving given income. As such, the inclusion of households who experience unemployment spells within the sample period could bias the risk experience estimates towards zero. The results reported in Table A4-1 alleviate those concerns. They are obtained using a sample of only households who at no point during the sample period have experienced unemployment. The results are very close to those reported in Table 2 of the main text.

A5. Future Income Volatility

The results in Section 5.3.2 show that while past personal experiences predict elevated current income volatility, past risk experiences are not associated with increased current income volatility. In this section, I repeat the regression of Table 4 but with income volatility four years into the future as the dependent variable. The results, reported in Table A5-1, suggest that four years into the future, those effects have mostly vanished. Past personal unemployment experiences are no longer associated with increased income volatility. However, two out of eight specifications suggest a statistically significant, at a 10% significance level, relationship between past risk experiences and increased income volatility four years into the future. Overall however, I conclude that increased volatility of income does not play an important role in explaining the consumption response to experiences found in the main analysis.

A6. Wealth Accumulation Using Net Wealth Instead of Assets

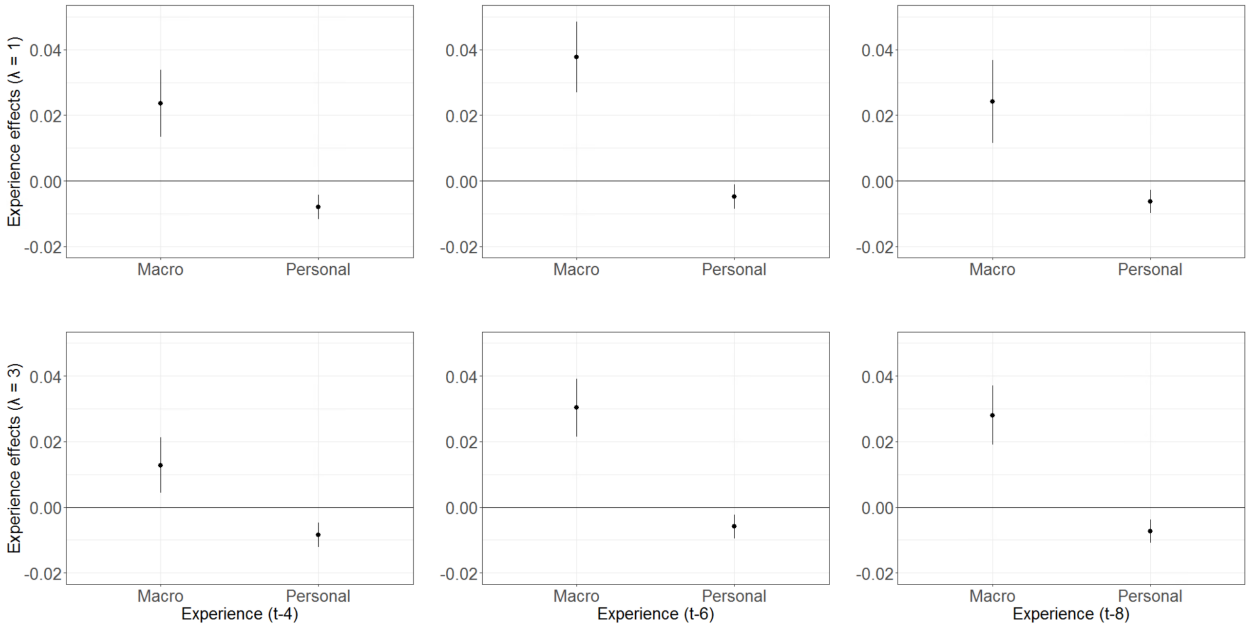
The experience hypothesis predicts that as households who have faced elevated job-loss risk in the past reduce their consumption, they accumulate wealth to insure against the higher perceived risk they face. The empirical evidence presented in Section 5.3.3 support this hypothesis as it suggests that past exposure to job-loss risk predicts wealth accumulation. The results are obtained by estimating Equation (9) using the logarithm of assets as the dependent variable. Theoretically, it is net wealth, that is the difference between assets and liabilities, that matters. However, assets are used instead of net wealth as the former is non-negative, thus allowing me to take logs. Figure A6-1 shows the results using net wealth. The results are robust, and suggest even a mildly stronger wealth accumulation following exposure to job-loss risk. However, as net wealth is frequently negative, these results are obtained at the loss of approximately 15% of the observations.

Table A5-1: Experience Effects and Income Volatility in $t + 4$

	(1)	(2)	(3)	(4)	(5)	(6)
Transitory						
Experience (Risk)	0.070		0.073	0.074*		0.082*
	(0.049)		(0.049)	(0.042)		(0.043)
Experience (Personal)		-0.009	-0.010		-0.009	-0.012
		(0.010)	(0.010)		(0.010)	(0.010)
n	137,102	137,102	137,102	137,102	137,102	137,102
N	932,010	932,010	932,010	932,010	932,010	932,010
R^2	0.207	0.207	0.207	0.0.207	0.207	0.207
Permanent						
Experience (Risk)	0.038		0.008	0.031		0.007
	(0.047)		(0.018)	(0.044)		(0.016)
Experience (Personal)		-0.002	-0.002		-0.002	-0.002
		(0.005)	(0.004)		(0.005)	(0.004)
n	88,990	88,990	88,990	88,990	88,990	88,990
N	503,417	503,417	503,417	503,417	503,417	503,417
R^2	0.292	0.269	0.269	0.292	0.269	0.269
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	$\lambda = 1$	$\lambda = 1$	$\lambda = 1$	$\lambda = 3$	$\lambda = 3$	$\lambda = 3$

*Notes: Table 4 shows results from estimating Equation (9) with transitory income volatility and permanent income volatility four years into the future as the dependent variable. Transitory volatility is the squared two-year change in excess log disposable income. Excess log disposable income is the difference between actual disposable income and permanent income, estimated as outlined in Section 5.2. Permanent income volatility is the product of two-year changes in excess log labor earnings (between years t and $t2$) and the six-year changes that span them (between years $t + 2$ and $t4$). The estimates are based on a linearly declining weight ($\lambda = 1$). The model is estimated with individual fixed effects (FE), year FE and various controls. The controls are dummy variables for age, education, marital status, gender, number of children, net wealth ventiles, sector of work, region of residence, and urbanization. N is the total number of observations and n is the number of different individuals. R^2 is the adjusted coefficient of determination. Standard errors, clustered at the cohort-level, are in parentheses. *** denotes significance at the 1% significance level, ** denotes significance at the 5% level, and * denotes significance at the 10% level.*

Figure A6-1: Effects of a One Standard Deviation Increase in Unemployment Experience on Future Net Wealth



Notes: Figure 9 shows the effect of a one standard deviation increase in past exposure to personal unemployment experiences and macroeconomic unemployment experiences on households' assets. It is found by estimating Equation (9) using the log of assets, deflated by the CPI, as the dependent variable. The top panels show estimates assuming linearly declining weights for past unemployment experiences ($\lambda = 1$). The bottom panels show estimates assuming households put less weight on recent past unemployment experiences ($\lambda = 3$). Going from left to right, the Figure shows estimates of unemployment experiences on assets four, six and eight years into the future. The results for assets four years into the future are based on 705,589 observations of 125,036 individuals. The results for assets six years into the future are based on 519,301 observations of 105,004 individuals. The results for assets eight years into the future are based on 353,685 observations of 85,187 individuals. The black dots represent the point estimates and the black vertical lines represent 95% confidence intervals based on standard errors clustered at the cohort level.

Chapter 2

Education and Consumption Smoothing

Education and Consumption Smoothing*

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June 28, 2024

Abstract

We study consumption and saving behavior by education group in Iceland in 2005-2019, a turbulent period with dramatic fluctuations in factor incomes before, during and after the collapse of the country's banking system. Using microdata containing the tax returns of all Icelandic taxpayers, we find that higher education is associated with a lower marginal propensity to consume (MPC) due to higher disposable income and more liquidity. Moreover, the MPC out of negative idiosyncratic income shocks is higher than that out of positive shocks. Finally, we find a hump-shaped consumption profile, which is most prominent for higher educated households, as consumption follows income over the life cycle.

Keywords: Education, consumption, inequality, macroeconomic stability.

JEL Codes: E21, E24

*This research was supported by the Icelandic Research Fund (Rannís), grant number: 207213-051. We are grateful to Yunus Aksoy, Daniel Borowczyk-Martins, Lúdvík Elíasson, Ron Smith, and Andrea Tafuro as well as seminar participants at several workshops and universities for their helpful comments on earlier drafts of this paper. The views expressed in this paper are those of the authors and do not necessarily reflect the views or policies of any entities we represent.

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1 Introduction

We study consumption smoothing by education over a turbulent period in Iceland, during which disposable income fluctuated dramatically due to a financial boom and bust and the subsequent recovery of the country's economy. The sample period is unique for an advanced economy as the country's entire banking system collapsed, resulting in an large exogenous shock to household income. Education may matter for the ability of individuals to smooth consumption through economic recessions. Not only is a high level of education typically associated with a relatively high wage level, but it may also increase an individual's ability to make financial plans into the future, thereby improving households' capacity to smooth consumption over the life cycle.

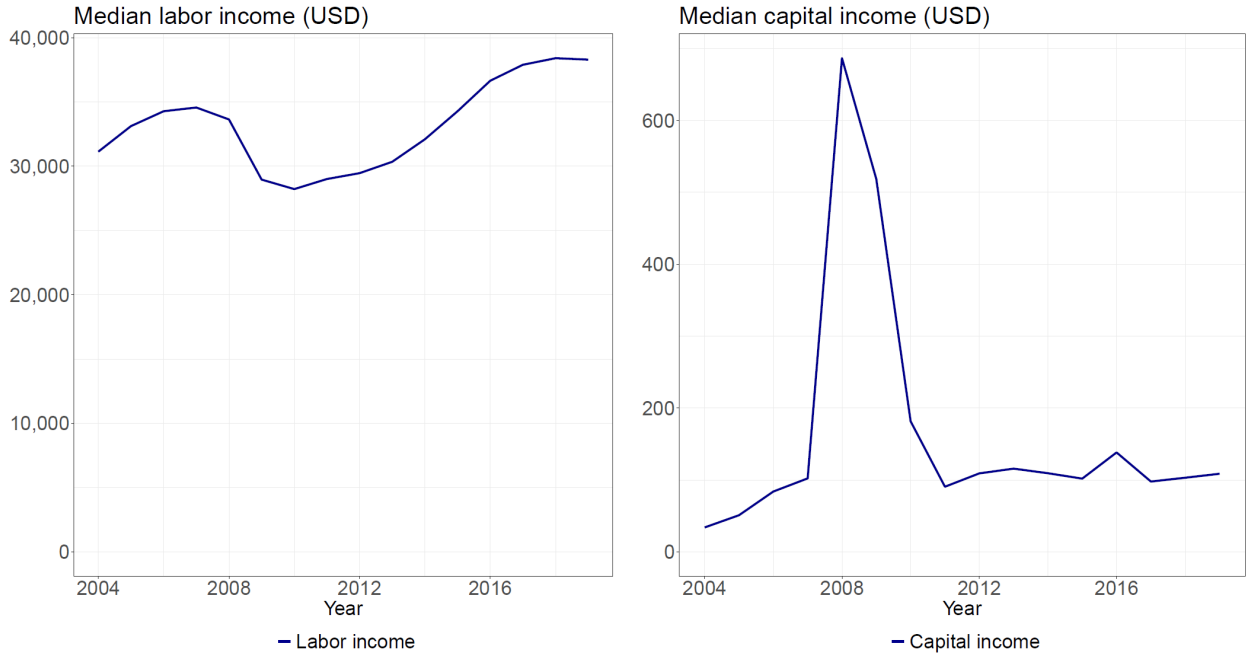
We take advantage of a rich administrative panel dataset consisting of tax records for all Icelandic taxpayers from 2005 to 2019 combined with various background characteristics. We use the data to impute consumption for every household in Iceland and then examine life-cycle consumption behavior. In so doing, we estimate the life-cycle profile of consumption for three groups of households with different levels of educational attainment – primary, secondary, and tertiary (university-level) education. We then use the panel dimension of our data together with information on household characteristics to estimate households' consumption response to idiosyncratic transitory income shocks. First, we obtain idiosyncratic income and consumption shocks based on the within-household relationship between consumption and household characteristics on the one hand and income and household characteristics on the other. Second, we use a 2SLS estimation to estimate the consumption response to the idiosyncratic transitory income shocks. Furthermore, we test for heterogeneity in the consumption response to income shocks across both education groups and several other characteristics, such as income, wealth, and liquidity.

Our sample period of 2005-2019 includes major changes in disposable income in Iceland. These changes were unexpected because the collapse of the bubble economy came as a surprise to most people, providing a unique opportunity for us to estimate the level of consumption smoothing by education group and analyze time variation in the MPC over this turbulent period. Furthermore, as opposed to many other studies that estimate MPCs out of windfall income, we are able to identify both positive and negative income shocks. Studying the asymmetry in households' consumption response to income shocks is crucial to learn about how households respond to different shocks, which can have implications for how the MPC evolves over the economic cycle and the effectiveness of monetary and fiscal policy to influence demand. Average disposable income rose during the financial boom from 2003 to 2008 and then fell after the banking collapse in 2008 before rising again during the recovery phase prior to the Covid-19 pandemic.

Figure 1 shows median wage income and median capital income (before taxes) between 2003 and 2019. Wage income peaks in 2007, the year before the collapse of the country's banking system, then falls until 2010 and rises thereafter. It reaches its earlier peak in 2016 and increases

thereafter, hitting a new peak in 2018. The fall from the peak in 2007 to the trough in 2010 is 22%. The fluctuations in capital income were even greater. Capital income rose rapidly in the bubble economy, increasing around fifteen-fold between 2003 and 2007, and then collapsing in 2009 and 2010. These fluctuations in average earnings and income were unanticipated and exogenous to each household's planned expenditures and saving decisions. Moreover, we have data on the education of each individual: their age, marital status, and number of children, as well as a measure of their wealth. This enables us to estimate the propensity to consume for the different education groups over this turbulent period.

Figure 1: Median annual labor and capital income in US dollars, 2004-2019



Notes: Figure 1 shows median annual labor income (left panel) and median annual capital income (right panel) in 2004-2019 for individuals aged 31-80 in USD at 2019 prices. Both series are converted to 2019 prices using Statistic Iceland's CPI index and then converted to USD using mid-2019 exchange rates.

Summarizing our key results, we find that consumption and income move together over the life cycle. The life-cycle consumption profile is hump-shaped for both university educated households and households with secondary education. However, it is almost strictly increasing in age for primary educated households. Moreover, university-educated households smooth consumption to a larger extent than others as measured by the marginal propensity to consume (MPC) out of transitory income shocks.

We also find that there is an asymmetry in the effect of negative and positive shocks in that households' consumption response to shocks is stronger for negative income shocks than for positive income shocks. Furthermore, there is heterogeneity in the MPC across the distributions of income (the MPC varies with income) and liquidity (the MPC varies by liquidity and is higher for liquidity-constrained households). Higher income and more liquidity, thus, explain the lower

MPC for university-educated households.

Finally, during the global financial crisis and following the collapse of Iceland's banking system, the MPC increased markedly before normalizing again once the economic recovery took hold. While this is true across education groups, the fluctuations in the MPC is particularly pronounced among university-educated households. Studying the time variation in the MPC separately for positive and negative income shocks reveals that the surge during the crisis was largely driven by a particularly strong consumption response to negative income shock, with households tending to cut their consumption approximately one-to-one. The MPC out of positive income shocks also rose during the crisis albeit to a more limited extent. Therefore, households raised their consumption slightly more when faced with a positive shock during the crisis compared to when the economy operated closer to equilibrium or during economic expansions.

From here the paper proceeds as follows. Section 2 reviews parts of the literature relevant to our study, and Section 3 presents our data. Section 4 shows patterns of life-cycle household consumption, and Section 5 attempts to estimate the effect of education on household consumption behavior. Section 6 offers some further perspectives based on survey evidence. Finally, we conclude in Section 7.

2 The Context of the Paper

Our period of study covers the financial crisis in 2008, which hit Iceland extremely hard. We are not the first to study consumption smoothing during a financial crisis, but our study has the advantage of linking data taken from tax returns to data on the educational level of each taxpayer as well as exploring the reasons for our results using survey data on saving motivation.

Canbary and Grant (2019) use the Family Expenditure Survey in the UK to find the relationship between the MPC and socioeconomic status and find that households with higher socioeconomic status have lower MPCs. However, they do not have access to data on households' level of education. These findings are consistent with those of Carroll (2001), who found that the MPC is declining in income so that wealthy people spend a smaller proportion of a transitory income shock than poor people do, a result that is consistent with our findings in this paper. It follows from this finding that the average MPC should increase when income falls in a crisis. Krueger et al. (2016) use data from the Panel Study of Income Dynamics and document the patterns of household income, consumption, and wealth inequality before and during the Great Recession. They find that wealth inequality can significantly amplify the impact of an aggregate shock when the distribution features a large fraction of households with little net worth, which sharply increase their saving when the recession hits.

Our study contributes to both the literature on heterogeneity in the marginal propensity to consume (MPC) out of unanticipated income shocks and the literature on the life-cycle profile of consumption.[1] Let us start with the former. The literature typically finds more excess sensitivity

[1] For a survey, see Jappelli and Pistaferri (2010)

to transitory changes in income than is predicted by canonical theories of consumption and saving over the life cycle (Friedman, 1957; Ando and Modigliani, 1963). This has been attributed to the presence of liquidity constraints and uncertainty surrounding income generating target saving behavior. Carroll (2001) found that buffer-stock saving generates consumption behavior which resembles the effect of liquidity constraints. We contribute to this literature by measuring the excess sensitivity to transitory changes in income by education over a period when both wage income and capital income fluctuated wildly in a way that could not be anticipated.

It is not straightforward to establish which households are liquidity-constrained but we have data from tax returns on liquid assets, total assets, net worth and income, which enables us to measure liquidity constraints for each taxpayer. Hayashi (1985) uses savings as an indicator of such liquidity constraints, while Zeldes (1989) uses the assets-to-income ratio. Lusardi (1996) uses data from the Consumer Expenditure Survey and the PSID and finds that consumption is excessively sensitive to predictable income growth. Fagereng et al. (2021) study the response of lottery winners in Norway and find that spending peaks in the year of winning and that the MPC for low-liquidity winners is close to one. Parker et al. (2013) use the Consumer Expenditure Survey to calculate the propensity of households in the US to spend out of the economic stimulus payments made in 2008 and find that households spent 50-90% of the payment on non-durable and durable goods over the three-month period during which they received the payments.

Our data allow us to plot the life-cycle path of consumption for the different education groups. We contribute to the literature both by estimating the path for different education groups using administrative data for all taxpayers in a country as well as by supporting our results with survey evidence on saving motives. Gourinchas and Parker (2002) emphasize the importance of the expected growth rate of income for consumption as individuals age and find that both income and consumption profiles are more hump-shaped for the higher-education group (college and some graduate school). Fernández-Villaverde and Krueger (2007) find a smaller hump for workers with low education and a larger hump for workers with high education. These findings have been attributed to liquidity constraints, as in Zeldes (1989) and Gross and Souleles (2002). This pattern is also at least partly due to variation in the size of the household over the lifespan, as is shown by Attanasio and Weber (1995) and Attanasio et al. (1999). Another explanation of the tendency of consumption to track income over the life cycle is the computational difficulty of maximizing utility over time, as is pointed out by Tversky and Kahneman (1974). Carroll (2001) shows that precautionary saving can also explain the observation that growth in consumption is closely related to the predictable growth rate of permanent income.

Others have looked at the relationship between education or financial literacy on consumption and saving behavior. Cooper and Zhu (2016) discuss how education may matter because of differences in income processes, patience, participation in financial markets, and financial literacy. They find that education affects household finance mainly through increased average income, leaving some role for a lower discount rate. Lusardi (2008) shows that financial illiteracy is widespread among the U.S. population and very acute among those with low

education, women, African Americans, and Hispanics.

The level of consumption smoothing and differences in the MPC across education groups affect the macroeconomic expenditure multiplier and the transmission of monetary and fiscal policy. Ampudia et al. (2018) study the effect of monetary policy on consumption in the major euro area countries. They find that low-income, low-education households demonstrate the largest consumption response to income changes induced by monetary innovations. Therefore, the main effects of monetary policy on consumption are channeled through low-income households that have a larger MPC. Fisher et al. (2020) use the Panel Study of Income Dynamics from 1999–2013 to examine how the MPC differs across the wealth distribution. They find that the MPC is lower at higher wealth quintiles, which indicates that poorer households cannot smooth consumption as much as other households. It follows that increased wealth inequality reduces aggregate consumption. Guo et al. (2023) employ data from 20 European countries and find that a higher ratio of hand-to-mouth households raises the value of fiscal multipliers.

We contribute to the literature by using an administrative dataset that includes all taxpayers in Iceland, classified by educational level, to estimate consumption smoothing for three distinct education groups over a period that includes very large fluctuations in real disposable income, namely (a) the large increase in disposable income before the financial crisis of 2008, (b) the collapse in 2008-2009, and (c) the subsequent recovery. The choice of the sample period enables us to better identify the different education groups' propensity to consume out of disposable income.

As opposed to much of the literature, the sample period together with the long panel data allows us to identify both positive and negative income shocks. This distinction is crucial since we find that the MPC out of negative income shocks is significantly higher than that out of positive income shocks for all education groups. Furthermore, we document time variation in the MPC, showing that it increased markedly at the onset of the crisis and the collapse of Iceland's banking system. Again, this is driven by negative shocks to income as households, across education groups, responded to such shocks by cutting their consumption. Moreover, we use survey evidence to find differences in the motivation for saving between education groups and find differences in both the level of saving and consumption smoothing across education groups.

3 Data

We use a dataset of annual administrative tax records from all Icelandic taxpayers from 2005 to 2019. The data are collected by Statistics Iceland and Iceland Revenue and Customs and include third-party reported information on multiple sources of income and various assets and liabilities.[2]

[2]The data include information on income, taxes, assets, and liabilities. Income includes labor income, capital income, income from pension funds, government transfers, and other income such as lottery winnings and grants. Furthermore, we add imputed rent as income for homeowners. Liabilities include student loans, mortgages, credit card debt, and other forms of debt. Assets include the market value of real estate and cars, stocks and bonds in mutual funds, and money in savings accounts.

The data are linked with other administrative data and therefore include socio-demographic factors as well.[3]

By aggregating information across household members each year using unique household identifiers, we construct household-level measures of income and other characteristics. Each household consists of at most two adults in the case of jointly taxed couples. We treat intra-household inequality by assigning each household member an equal share of the household's income, assets, and liabilities, while allowing background information such as age to vary across household members. This approach assumes that financial decisions within a household are made jointly based on the household's total income and wealth, rather than individually for each household member based on individual income and wealth.

We avoid complications caused by young people living with their parents and ensure enough observations within each age group by restricting the sample to individuals aged 31 to 80 years and those born between 1935 and 1979. To further alleviate the effects of such complications, we also exclude students and individuals who live abroad for part of the year or have an abnormally low disposable income for other reasons by omitting households whose equivalized disposable income is below 40% of the median.[4] We also omit deficient tax records and individuals who have negative income.[5] Deficient tax records include those who have income from abroad or have failed to file their taxes. Those require tax authorities to either estimate or manually calculate taxes. In some of those instances, it is possible we do not have accurate information on household income and wealth. Therefore, those observations are omitted. Similarly, we discard the top 1 percentile in consumption to alleviate biases from potentially misattributing wealth declines to consumption when they might stem from unrealized capital losses or stock transactions not observed in the data. Finally, we remove from the sample individuals with saving-to-disposable income ratio lower than -1.[6] This leaves us with 1,754,611 observations from 167,838 individuals for our analysis.

We compute consumption for each household using the accounting identity that a household's consumption equals its disposable income minus changes in net wealth plus unrealized capital gains (Browning and Leth-Petersen, 2003; Eika et al., 2020). Implying that any income is either saved or consumed

[3]This includes information on age, gender, education, marital status, occupation, education, and number of children.

[4]In equivalizing household disposable income, we use a version of the OECD modified equivalence scale. The OECD scale assigns a value of 1 to the household head, 0.5 to each household member aged 14 and over, and 0.3 to each child aged under 14. We use the same values, but instead of using 14 years of age as a cut-off value we use 7 years, as we only have information on whether a child is above or below the age of 7.

[5]Although we account for several sources of unrealized capital gains, measured consumption is negative for some households, which can be due to misattributing wealth increases to saving out of income when they are due to unrealized capital gains or to income not observed in the tax records, such as income from inheritances or gifts, or tax evasion (Kolsrud et al., 2020).

[6]Timing issues occasionally result in income or changes in assets or liabilities which occur in year t being registered in year t+1 leading to extreme values for calculated consumption and savings in each year, while still producing a sensible average across the two years. By removing negative consumption values and highly negative saving ratios, we seek to omit those observations.

$$C_{i,t} \equiv Y_{i,t} - \Delta W_{i,t} + \sum_{k} \Delta p_{k,t} A_{i,k,t-1} \tag{1}$$

where $Y_{i,t}$ is disposable income (annual income in local currency) for individual i at time t. $\Delta W_{i,t}$ is the change in net wealth between periods t and $t-1$, and $\Delta p_{k,t} A_{i,k,t-1}$ is unrealized capital gains on asset k. Note that $Y_{i,t}$, $W_{i,t}$, and $A_{i,t}$ are defined as the intra-household averages of disposable income, net wealth and assets, respectively.

The idea underlying the identity in Equation (1) is that income is either spent, thereby contributing to consumption, or saved, thereby leading to increasing net wealth. However, net wealth is also influenced by factors other than income, namely unrealized capital gains, which are caused by changes in market prices of households' assets and liabilities, and do not change current consumption. Unrealized capital gains include changes in house prices, investment funds' asset prices, the effects of CPI-indexation of household debt, and a 2014-2016 mortgage relief program, all of which we correct for.

The dataset contains a natural measure of capital gains/losses in real estate, as it includes information on the market value of real estate for each household as estimated by Registers Iceland.[7] The public institution estimates the market value of all real properties in Iceland each year, and its valuations form the basis for property tax and inheritance tax. We have information on the year-end market value of financial assets in investment and savings funds. Following Eika et al. (2020), we assume no intra-year transactions, and therefore allow for heterogeneous portfolio returns across households in computing the capital gains/losses from such assets. However, direct ownership of stocks is not registered at market values. In fact, it does not change from year to year unless households engage in transactions with individual stocks. Therefore, as long as households do not engage in such transactions in a given year, capital gains/losses on individual stocks do not influence our measure of households' consumption and saving. Furthermore, a large share of household debt in Iceland is CPI-indexed, meaning that the loan principal is linked to the consumer price index. We have information on the principal, installments, and interest payments on each loan. This allows us to identify indexed loans and compute the indexation, which is effectively an unrealized capital loss. Our dataset also contains information on a 2014-2016 government mortgage relief program that lowered household debt by the equivalent of 3.5% of GDP, thus constituting a large unrealized capital gain for households that benefited from it.

Finally, we have information on the value of motor vehicles, and we compute each household's imputed rent for homeowners. These two are arguably the most significant components of durable consumption for most households. We assume that consumption derived from vehicles each year is equal to its depreciation, which is 10% according to Icelandic tax law, and that consumption of own housing equals imputed rent.[8]

[7] Register Iceland is a public institution responsible for maintaining a property register and national register in Iceland.

[8] We compute imputed rent for each household by dividing the value of each household's real estate by the total

In the absence of unreported income and transactions in individual stocks and given the assumption of no within-year transactions in funds, Equation 1 should give an accurate estimate of consumption. Net wealth includes information on all types of assets, albeit with the caveats discussed above. Additionally, we have year-end data on the principal of all liabilities, including mortgages, consumer loans, auto loans, credit card debt, and student loans. Furthermore, the largest share of unrealized capital gains come from changes in valuation of real estate assets, an estimate of which is third-party reported by Register Iceland. However, there is risk of measurement errors when households engage in transactions in financial assets or real estate. Also, in the event of separations, we assume couples equally divide their assets. If this assumption does not hold, it may lead to incorrect attribution of one former spouse's assets to the other. As discussed in Fagereng and Halvorsen (2017), measurement errors and instances of negative consumption can arise from intra-year sales and purchases of houses or financial assets, or if there are changes in a household's composition. For example, a household might pay for a house before a year's end but only become the registered owner afterward, or vice versa. Such timing issues, discussed in footnote 6, can lead to measurement errors and, in some cases, negative imputed consumption. Following Fagereng and Halvorsen (2017), we exclude households with negative imputed consumption.

Data from consumption surveys are not available to us. Therefore, we are not able to validate our imputations for consumption by comparing to survey data. However, we are able to replicate some stylized facts from the literature. Panel (a) of Figure 2 shows the median saving rate out of after-tax income by income groups in 2019. The income groups are constructed using the distribution of after-tax income within each birth cohort. This ensures that the saving rate in the figure is not driven by households' position in the life cycle. As can be seen, the median saving rate in the bottom 50% of the income distribution is smaller than the next 40%. We look at the top 10% of the distribution, specifically, to highlight the group's high median saving rate compared to the rest of the sample. In particular, it is roughly three times higher than that of percentiles 50 to 90. This is consistent with findings in the literature that rich and higher income households save more than others (see for example: Dynan et al. (2004) and Mian et al. (2020)).

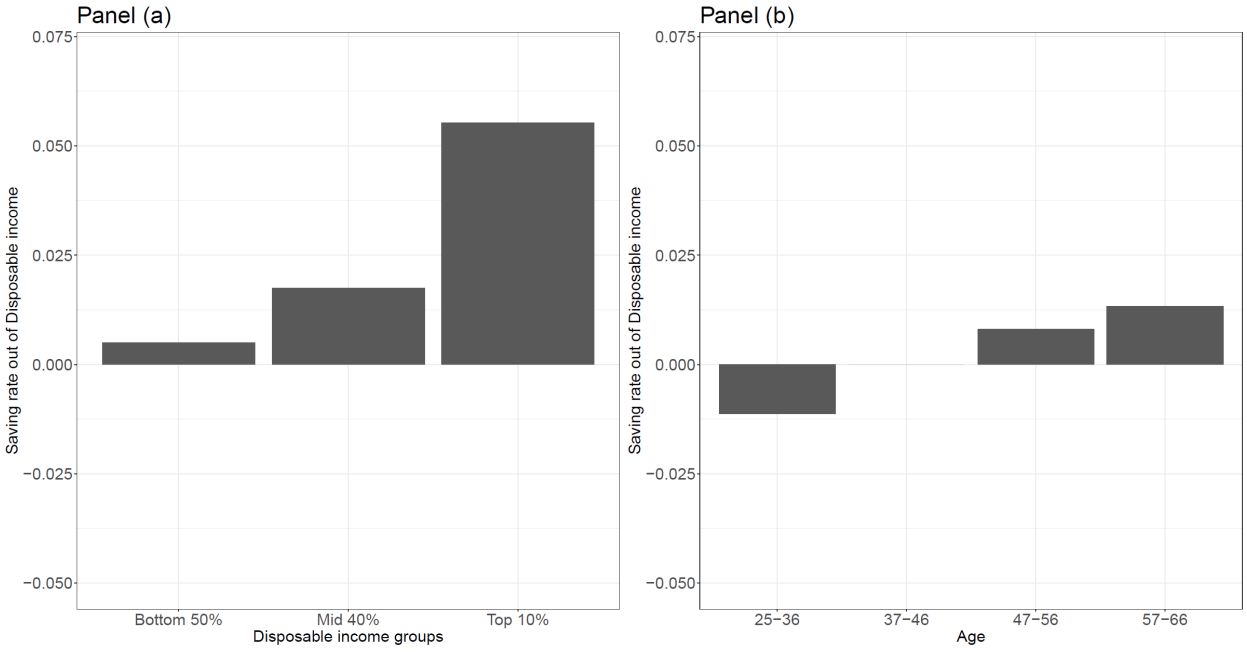
Panel (b) of Figure 2 shows the saving rate by age groups for the working age population over the whole sample period of 2005-2019. It shows that, in line with the predictions of a canonical life cycle model of consumption and saving, the saving rate is rising in age for the working age population. Income is relatively low for workers soon after they enter the labor market. As they gain skills and experience, their income increases, as illustrated in Figure 4, and so does their saving rate. The saving rate peaks in the years just before retirement.

Appendix Figure A2-1 shows the coefficient estimates from regressing our imputed consumption, deflated using Statistic Iceland's CPI index, on the interaction of real after-tax income and year dummy variables, while controlling for individual fixed effects. By interacting income with year fixed effects we obtain a time-varying association between consumption and

value of real estate and multiply the outcome by the aggregate value of imputed rent from national accounts.

income. The inclusion of individual fixed effects allows us to estimate the relationship using the co-movement of consumption and income within each individual or household. Figure A2-1 shows that there is a sizeable change in the relationship between income and consumption following the economic crisis in 2008. The coefficient estimates, which had fluctuated around 0.25 prior to the crisis, declined to approximately 0.10-0.15 for years after the crisis, suggesting a weaker association between the two variables.

Figure 2: The saving rate out of disposable income across income and age groups



Notes: Figure 2 shows the median saving rate out of after-tax income. Consumption is computed using Equation (1) and saving is the share of income not consumed. Note that both consumption and after-tax income include imputed values for homeowners' imputed rent and consumption includes imputed values for vehicle depreciation. Panel (a) shows the saving rate by income groups in 2019. The income groups are formed within each cohort so as to ensure the saving rates are not influenced by households' position in the life cycle. Panel (b) shows the saving rate by age groups for the whole sample period.

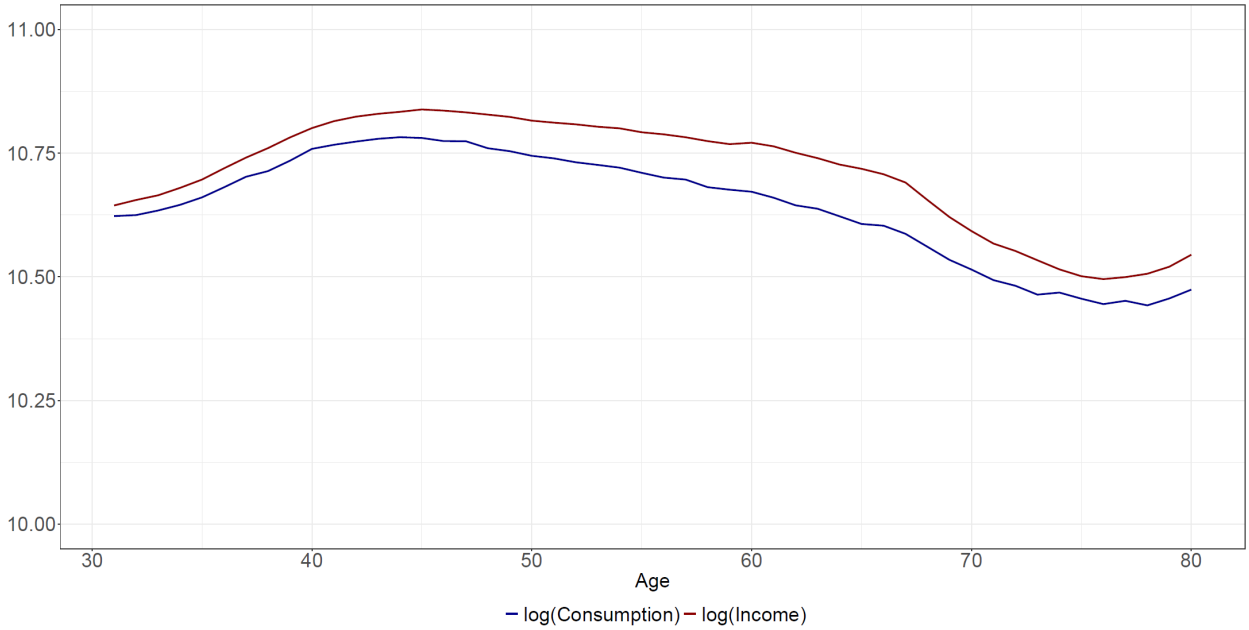
4 Life-cycle Patterns of Consumption

4.1 Consumption and Income over the Life Cycle

We apply equation (1) to compute the life-cycle profile of consumption and income. Figure 3 shows average consumption and income in logs over the life cycle for the entire sample in 2005-2019.

Consumption and disposable income are virtually parallel over the life cycle. As income rises early in the life cycle, consumption also increases, but to a lesser extent. After both variables peak in the mid-40s, consumption falls at a faster rate than income, reflecting increased saving

Figure 3: Average disposable income and consumption by age



Notes: Figure 3 shows average log consumption and average log income by age for the full sample (1,754,611 observations) of individuals aged 31 to 80 in 2005-2019. Before taking logs, both series are converted to 2019 prices using the CPI and then converted to USD using mid-2019 exchange rates.

for retirement. The drop in disposable income at retirement is more pronounced than the drop in consumption, reflecting consumption smoothing. Moreover, there is no discrete drop in consumption around age 70. Still, somewhat at odds with standard theories of consumption and saving over the life cycle, households continue saving deep into retirement.

Before introducing a model, we split households into three groups, based on the highest educational attainment within the household. The three groups are primary school, secondary school, and university.[9] We do this to obtain life-cycle consumption paths for each group. Table 1 contains summary statistics for the variables used in the analysis, by level of education. It shows that educational attainment is increasing with each cohort in the last decades, causing the university-educated to be, on average, five to six years younger than those without a university education. A larger proportion of those with secondary or tertiary education lives with a spouse compared to those with primary education, and a larger proportion live in urban areas. The university-educated have more children under age 18 than other education groups do, which could be caused by the gap between the average ages of the groups. The university-educated have higher income, consumption, and net wealth than those with a secondary school education, who in turn have higher income, consumption, and net wealth than those with a primary school education. University-educated households are more likely to own their place of residence

[9]Primary education refers to households where the higher-educated individual has primary or lower secondary education. Secondary school refers to households where the higher-educated individual has an upper secondary education, post-secondary non-tertiary education, or short-cycle tertiary education without a diploma. University education refers to households where the higher-educated individual has a bachelor's, master's, or doctoral degree.

compared to those with a secondary school education; the latter are more likely to own than those with only primary school education. This trend likely reflects the income disparities among these groups.

We then come to measures of liquidity constraints. Interestingly, the ratios of net wealth to disposable income, on the one hand, and liquid assets (proxied by bank deposits and financial assets in investment and savings funds) to disposable income, on the other, are lower for university graduates than for secondary school graduates. The latter measure is even lower compared to that of primary school-educated households. The lower average age of those with a tertiary education likely explains this finding, as younger households typically have accumulated less wealth relative to income than older households have. Comparing the groups in terms of an absolute liquidity constraint, proxied by liquid assets below USD 8,000 for couples and USD 4,000 for singles, shows more binding constraints for primary-educated households, 53% of whom are liquidity-constrained by this measure, than for university graduates, 37% of whom are liquidity-constrained.

Figure 4 shows consumption and income by age for the average household in each education group. Consumption follows income over the life cycle in all groups. Predictably, income and consumption clearly rise with education. Moreover, the income profile from 31 years of age onward becomes steeper as educational attainment rises, peaking at 27 log points above their level at 31 years of age for university graduates, compared to 18 log points for secondary school graduates and 6 log points for primary-educated households. This is consistent with the results of Ampudia et al. (2018) for eurozone countries.

The steep consumption profile for the university-educated can be explained by liquidity constraints and also by buffer-stock saving; see Carroll (2001). In the presence of buffer-stock saving, consumption follows income growth because, if it grew more slowly, the stock of saving would continue to grow. Below, we gauge the role of liquidity constraints in households' consumption smoothing and using the survey evidence presented in Section 6 discuss the motives for households' saving behavior.

4.2 Life-Cycle Paths of Consumption

In this section, we estimate a life-cycle consumption profile for the three education group. We do this by regressing our measure of consumption on various household characteristics and obtain differential life-cycle consumption profiles across educational groups by interacting age effects with the level of education.

$$c_{i,t} = \alpha_i + \tilde{\alpha}_t + \beta_a D_{a,i,t} \times E_{i,t} + \mathbf{Z}_{i,t}\phi + \varepsilon_{i,t} \tag{2}$$

where $c_{i,t}$ is the log of real consumption deflated by Statistic Iceland's CPI index. α_i is individual fixed effects which control for time-invariant household characteristics, such as attitudes and preferences. They also serve as a proxy for permanent income. Furthermore, they allow us to identify the life-cycle profile of consumption based on how consumption varies with age within

Table 1: Summary statistics

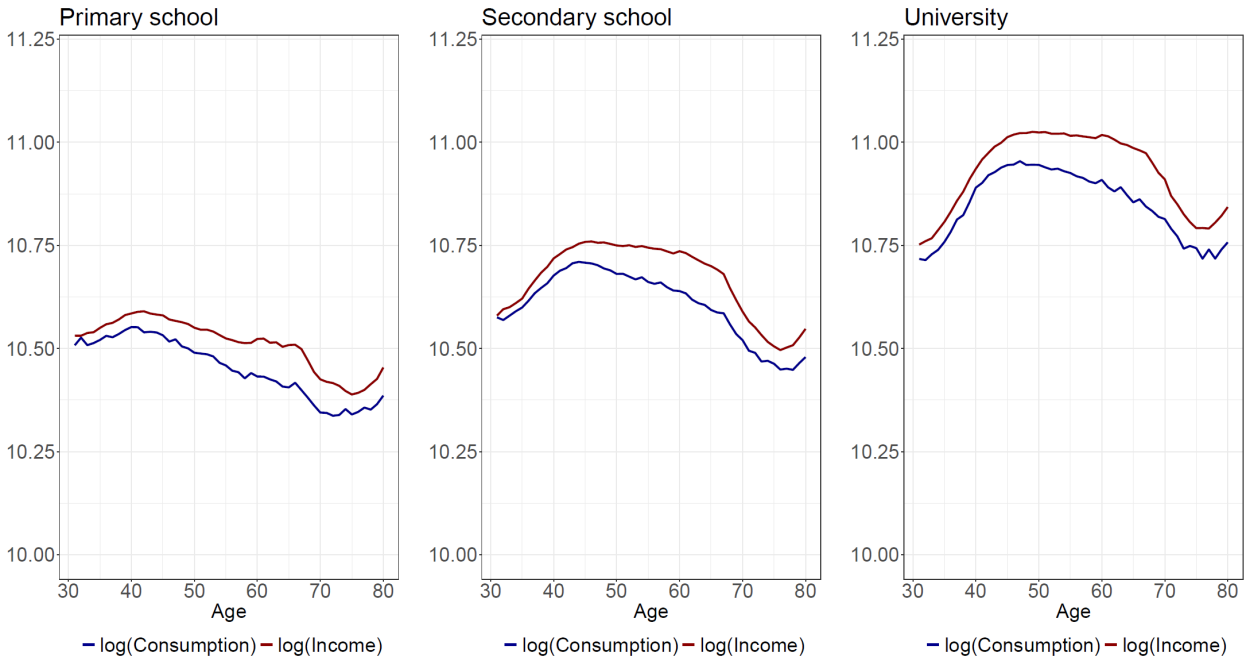
	Primary (391,419 obs.)	Secondary (739,941 obs.)	Tertiary (623,251 obs.)
Age	55.29	54.16	49.16
	(12.59)	(11.70)	(10.67)
Spouse	0.44	0.74	0.81
	(0.50)	(0.44)	(0.39)
No. of children	0.35	0.54	1.03
	(0.80)	(0.92)	(1.14)
Urban	0.50	0.60	0.75
	(0.50)	(0.49)	(0.43)
Disposable income	40,161	48,463	63,625
	(26,637)	(36,062)	(47,190)
Consumption	39,657	47,556	61,497
	(21,565)	(26,195)	(33,995)
Net wealth	76,869	112,542	136,154
	(143,117)	(172,433)	(234,030)
Real estate	115,730	167,496	213,985
	(119,909)	(126,668)	(143,448)
Net wealth-to-income	2.23	2.72	2.31
	(3.29)	(3.35)	(3.34)
Liquid asset-to-income	0.81	0.98	0.98
	(2.11)	(2.50)	(2.83)
Liquidity-constrained	0.53	0.44	0.37
	(0.50)	(0.50)	(0.48)

Notes: Table 1 reports variable means for the full sample period 2005-2019. Standard deviations are shown in parentheses. Disposable income, consumption, net wealth, and real estate values are reported in USD. Nominal variables are converted to 2019 prices using Statistic Iceland's CPI index and then converted to USD using mid-2019 exchange rates. A household is considered liquidity-constrained if liquid assets are less than USD 8,000 for couples and USD 4,000 for singles. Urban is 1 for those living in urban areas and zero for those living in rural areas.

the individual or households. $\tilde{\alpha}_t$ are year fixed effects that capture common variation in consumption across all households – for example, due to changes in the macroeconomic environment – at a given time. They are represented by year dummies that are orthogonal to a time trend and normalized to sum to zero (Deaton and Paxson, 1994).[10] This is to enable the simultaneous inclusion of controls for age and time effects within the individual fixed effects framework while avoiding collinearity issues. Other controls include dummy variables for gender, marital status, number of children, the interaction between marital status and number of

[10]We do this by defining the time dummies as $\tilde{D}_t = D_t + (1-t)D_2 + (t-2)D_1$, where D_t is the conventional time indicator variable, which equals 1 in year t and zero otherwise. Due to the construction of D_t, the first two-time dummies are dropped in the estimation. These effects can be recovered using the fact that all year effects sum to zero and are orthogonal to a time trend.

Figure 4: Average disposable income and consumption by age for three education groups



Notes: Figure 4 shows average log consumption and average log income by age for households aged 31 to 80 in 2005-2019. The left panel shows 391,419 observations of 42,116 primary-educated households, the mid panel shows 739,941 observations of 79,169 of secondary school-educated households, and the right panel shows 623,251 observations of 64,291 of university-educated households.

children, education, sector of work and the degree of urbanisation. Furthermore, we control for the log of real estate assets.

β_a is a vector containing the main coefficients of of interests. It shows the age effects interacted with the level of education, $E_{i,t}$, while controlling for the variables discussed above. Figure 5 plots the age effects (β_a) from Equation (2). Age 31 serves as a benchmark for each group; therefore, the age effect is by definition zero for 31-year-olds, with the rest of the life-cycle path defined in relation to that benchmark point.

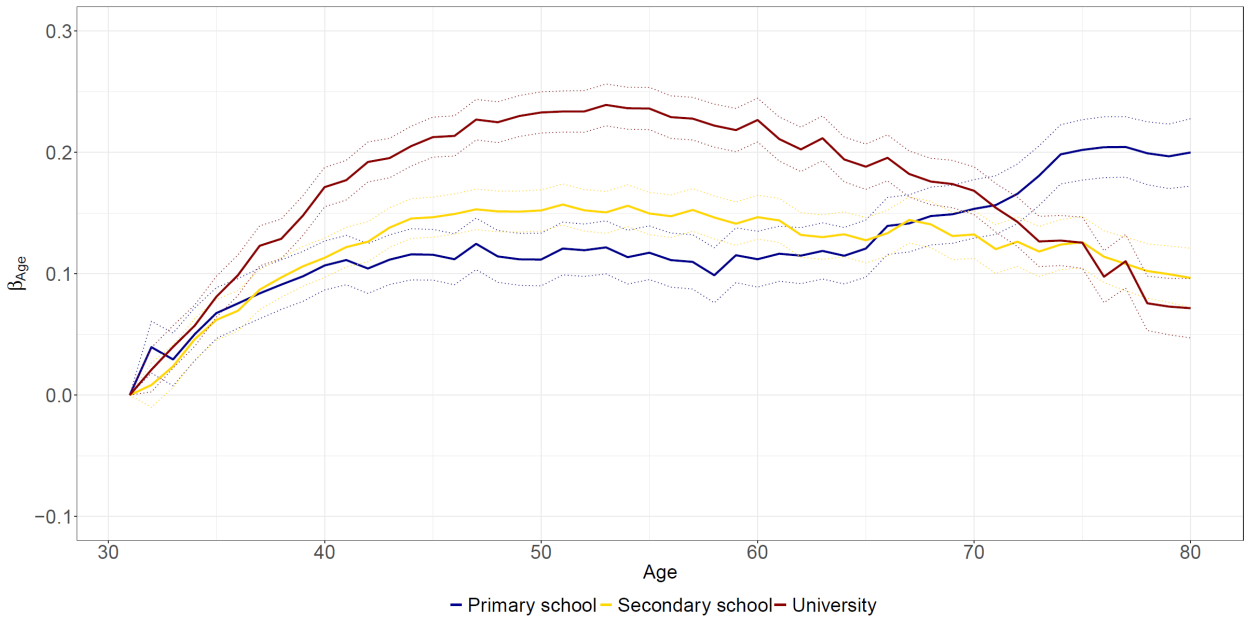
The consumption paths for the three education groups have shapes similar to those in Figure 4 in that between ages 31 and 40, consumption rises more steeply for university-educated households than for those without a university degree. This could be because those with a university degree rationally expect higher future income but are liquidity-constrained. We do not observe the same rise for the other groups, who, as is observed in Figure 4, do not experience the same increase in disposable income in their 30s and early 40s. As such, they may not face the same liquidity constraints as the educated young who would like to bring forward a share of their high future income in order to smooth consumption over the life cycle but cannot due to imperfect capital markets.

For all groups, the age effect of consumption drops around middle age, suggesting increased preferences for retirement saving. Finally, we see that after retirement, consumption rises again for primary educated households because these households reduce their saving once they reach

retirement. For university-educated households, however, the age effect declines in retirement, which may indicate a bequest motive (De Nardi, 2004).[11]

Appendix Figure A2-2 shows a version of Figure 5 with additional controls for the log of current disposable income. While there is a substantial and statistically significant hump in the life-cycle profiles reported in Figure 5, most notably for university-educated and secondary-educated households, the inclusion of controls for current income results in relatively flat consumption profiles. This suggests that current income, rather than household characteristics, explains the lion's share of the hump in life-cycle consumption paths.

Figure 5: Life-cycle profile for consumption



Notes: Figure 5 plots the age effects (β_{age}) from Equation (2). Age 31 serves as a benchmark for each education group; therefore, the age effect is zero by definition for 31-year-olds, with the rest of the life-cycle profile defined in relation to that benchmark point. In addition to age effects, the regression includes controls for individual and year fixed effects and dummy variables for gender, marital status, number of children, the interaction between marital status and number of children, education, sector of work and the degree of urbanisation. Furthermore, we control for the log of real estate assets. The dotted lines show the 95% confidence interval based on standard errors clustered at the individual level.

5 Consumption Smoothing by Education

5.1 Consumption Response to Transitory Income Shocks

Next we estimate the MPC out of transitory income shocks to gauge consumption smoothing behavior by educational attainment. We follow the methodology proposed by Blundell et al. (2008) and further examined by Kaplan and Violante (2010). First, we regress log income and log

[11]The effective retirement age in Iceland in 2015 was 69.4 for males and 68 for females (Ólafsson, 2017).

consumption on individual fixed effects, dummies for age, year, gender, education, marital status, number of children, the interaction between marital status and the number of children, residence, net wealth deciles, and the log of real estate assets. We proceed by obtaining the first differenced residuals of log consumption, $\Delta\tilde{c}_{i,t}$, and log income, $\Delta\tilde{y}_{i,t}$. As in Blundell et al. (2008), the income process for each household is decomposed into a permanent component, P, and a mean-reverting transitory component, ν. Hence, income growth is given by:

$$\Delta\tilde{y}_{i,t} = P_{i,t} + \Delta v_{i,t} \tag{3}$$

Finally, we obtain the MPC out of transitory income shocks using an 2SLS regression of $\Delta\tilde{c}_{i,t}$ on $\Delta\tilde{y}_{i,t}$, which is instrumented by $\Delta\tilde{y}_{i,t+1}$ as it is correlated with the transitory shock at time t, but not with the permanent one. Specifically, we estimate:

$$\begin{aligned} \Delta\tilde{y}_{i,t} &= \alpha_0 + \beta_{0,E}\Delta\tilde{y}_{i,t+1} \times E_{i,t} + \epsilon_{0,i,t} \\ \Delta\tilde{c}_{i,t} &= \alpha_1 + \beta_{1,E}\Delta\hat{\tilde{y}}_{i,t} \times E_{i,t} + \epsilon_{1,i,t} \end{aligned} \tag{4}$$

where $E_{i,t}$ again is an indicator for the education groups, with primary education serving as a benchmark.

The β_1 estimates are reported in Table 2. The first column has the full sample of individuals aged 31- 80. In columns (2) and (3), we report estimates of the interaction of the instrument and dummy variables for working age individuals (31-66 years) and retirees (67-80) to explore heterogeneities in the MPC by age groups. The MPCs for working-age individuals is shown in column (2) and that for retirees is shown in column (3). Starting with working-age households, we find that the MPC out of transitory income shocks is lower for university-educated households than for the other two groups. The difference between the MPC of university educated households and secondary educated households is statistically significant at the 1% significance level while the difference between university educated households and primary educated households is statistically significant at the 5% level. The difference between the university-educated and the secondary school-educated remains statistically significant at the 1% level in the full sample, but the difference between university-educated and primary school-educated is not significant at conventional significance levels. As is shown in that last column, this is because the MPC for primary school-educated retirees is significantly smaller than that for the university-educated retirees, the opposite of what is found for working-age individuals. We conclude that working-age university-educated households smooth consumption out of transitory income shocks to a larger extent than households in the other two education groups.

Appendix Tables A1-3 and A1-4 show the robustness of the above results to alternative standard error clustering. The former shows standard errors at the individual and year level while the latter shows standard errors at the cohort and year level. All results are robust to alternative clustering methods.

Table 2: MPC by education

	(1) 31-80 years	(2) 31-66 years	(3) 67-80 years
$\hat{\tilde{y}}_{i,t} \times primary$	0.551^{***} (0.020)	0.576^{***} (0.021)	0.422^{***} (0.056)
$\hat{\tilde{y}}_{i,t} \times secondary$	0.604^{***} (0.016)	0.621^{***} (0.017)	0.484^{***} (0.041)
$\hat{\tilde{y}}_{i,t} \times university$	0.526^{***} (0.017)	0.514^{***} (0.018)	0.679^{***} (0.062)
$R^2 = 0.033$ N = 1,423,797			

Notes: Table 2 presents 2SLS estimates from Equation (4). The estimates are based on households aged 31 to 80 in 2005-2019. Standard errors clustered at the individual level are in parentheses. *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. N is the number of observations.

Our results do not necessarily imply that education causes consumption smoothing. On average, university-educated households have higher income (see Table 1), which could be associated with a lower MPC out of transitory income shocks. This is consistent with Carroll (2001) buffer-stock model, which yields a strongly concave consumption function. As such, wealthy households optimally spend a smaller proportion of transitory income shocks than do poor households. This is also consistent with empirical estimates showing that higher-income households save a larger share of their income than lower-income households (Dynan et al., 2004; Mian et al., 2021). Thus, it could be the level of income, and not educational attainment itself, that explains the relatively lower average MPC out of transitory income shocks among university-educated households. The university-educated may also be less liquidity-constrained, as is shown in Table 1.

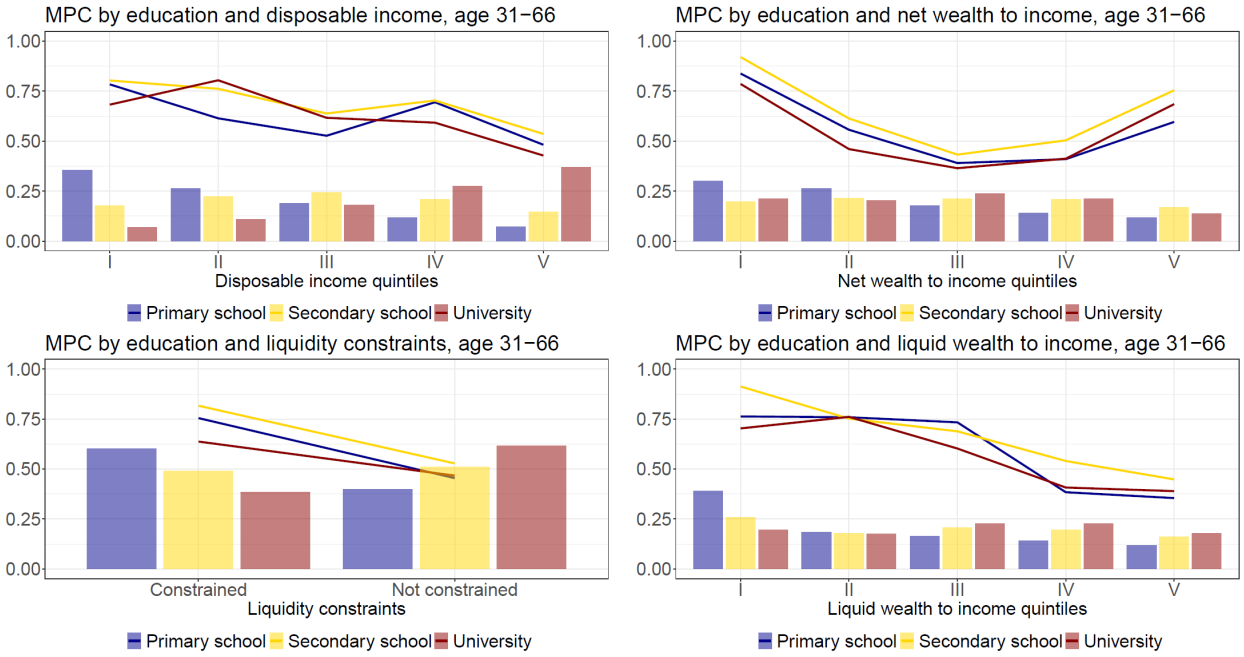
We study this by exploring the heterogeneities in the MPC out of transitory income across the distribution of income and liquidity, which are proxied by net wealth-to-disposable income, liquid wealth-to-disposable income, and a direct measure of liquidity constraints. For the first three variables, we estimate the MPC for each subgroup, constructed using quintiles of the distributions. Regarding the direct measure of liquidity constraints, a household is considered liquidity-constrained if liquid wealth, defined as the sum of bank deposits and assets in investment funds, is below USD 8,000 for couples and USD 4,000 for singles, as in Section 3.

Now we have more endogenous variables – namely, the transitory income shock for each education group interacted with the relevant subgroup. Hence we need additional instruments. As before, these are $\Delta\tilde{y}_{i,t+1}$, interacted with the education groups but now interacted with the relevant

subgroup as well.

The lines in Figure 6 show the coefficient estimate of the MPC for each education group and each subgroup, while the columns represent the share of each education group within the relevant subgroup.[12] Since the consumption behavior might differ between the working age population and retirees, the figure also focuses on a sample of 31-66 year olds. However, Appendix Figures A2-3 and A2-4 then show the MPCs for the whole sample of 31-80 year olds and retirees aged 67-80, respectively.

Figure 6: MPC by income, wealth and liquidity



Notes: Figure 6 shows the MPC estimates for the working-age population (aged 31-66) from Equation (4) with additional interactions with disposable income quintiles (upper left panel), net wealth-to-income quintiles (upper right panel), absolute liquidity constraint (lower left panel), and liquid wealth-to-income quintiles (lower right panel). Liquid wealth is defined as the sum of bank deposits and assets in investment and savings funds. A household is considered liquidity-constrained in the lower left panel if liquid wealth is below USD 4,000 for singles and USD 8,000 for couples. The lines show estimates of the marginal propensity to consume for subgroup or quintile. The columns represent the share of each education group within the respective subgroup or quintile. The estimates are based on 1,423,797 households aged 31 to 80 in 2005-2019, but with interaction terms for the working-age population (aged 31-66) and retirees (age 67 and above).

The MPC falls in income and liquidity constraints, as in Gelman (2021), but is U-shaped for the net wealth-to-income ratio. The U-shaped pattern indicates wealthy consumers who live hand-to-mouth due to liquidity shortages, as is suggested by Kaplan et al. (2014). However, there is no clear difference across education groups. Therefore, the relatively lower MPC of university-educated households found in Table 2 is to an extent explained by the fact that relatively many lower-educated households are in lower income quintiles and thus have a high MPC, while relatively few lower-educated households are in higher quintiles. The opposite is true for higher-

[12] See also Tables A2-1 to A2-4 in the Appendix for more detailed regression results.

educated households. This can be seen from the columns in Figure 6, and the pattern is clearest for the income quintiles. The share of the primary-educated is highest for the lowest income quintile and then falls monotonically over the higher quintiles. The share of the university-educated is lowest for the lowest quintile and then rises monotonically to the highest quintile. There is also a clear pattern in the bottom left figure, where a larger share of the primary-educated are liquidity-constrained and a larger share of the university-educated are not liquidity-constrained.

Appendix Figure A2-3 shows results for full sample of households aged 31-80. The results are similar to Figure 6, but the MPC estimates for the university-educated are somewhat higher relative to the other education groups compared to our results for the working age population. As is shown in Table 2 and Appendix Figure A2-3, this is driven by the fact that the MPC for university-educated retirees is higher than that for other education groups. This is the opposite of what is found for the working-age sample and the sample as a whole.

5.2 Asymmetry in the MPC out of Transitory Income Shocks

Next we allow for asymmetry in the MPC, depending on whether households face a positive or negative income shock. Our motivation is that responding to a negative transitory income shock might be more challenging from a practical standpoint than responding to a positive shock; for example, due to liquidity constraints or imperfections in the capital markets. The specification is the same as before, except that we separate those with positive transitory income shocks from those with negative shocks using indicator variables. Specifically, we estimate the following modification of Equation (4):

$$\begin{aligned} \Delta y_{i,t} &= \alpha_0 + \beta_{0,E,-}\Delta y_{i,t+1} \times E_{i,t} \times I^- + \beta_{0,E,+}\Delta y_{i,t+1} \times E_{i,t} \times I^+ + \epsilon_{0,i,t} \\ \Delta c_{i,t} &= \alpha_1 + \beta_{1,E,-}\Delta \widehat{y_{i,t}} \times E_{i,t} \times I^- + \beta_{1,E,+}\Delta \widehat{y_{i,t}} \times E_{i,t} \times I^+ + \epsilon_{1,i,t} \end{aligned} \tag{5}$$

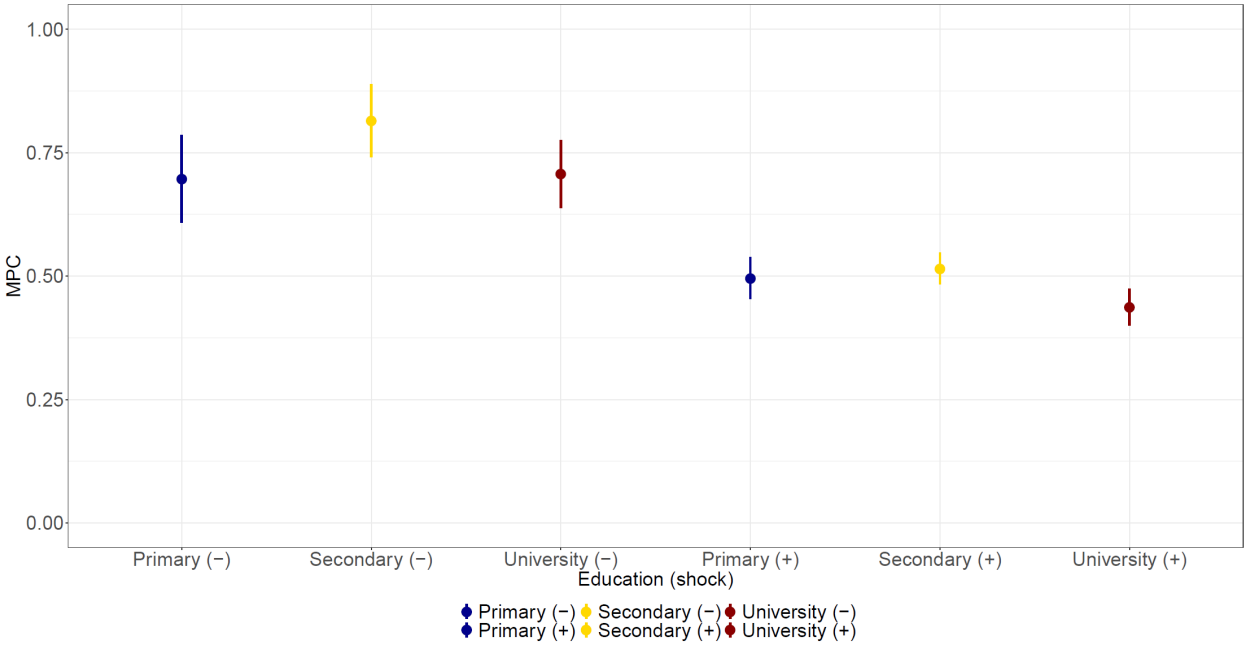
where I^- and I^+ are indicator variables for negative transitory income shocks and positive transitory income shocks, respectively. The results are reported in Figure 7.

Two lessons emerge from this. First, all education groups smooth positive income shocks to a much larger extent than negative income shocks, as the MPC for positive shocks is significantly lower than that for negative shocks.[13] Second, university-educated households smooth consumption out of positive income shocks to a significantly larger extent than other households do. They also smooth more than secondary-educated households for negative income shocks.

Appendix Figures A2-6 to A2-9 explore heterogeneities in the MPC out of the positive and negative transitory income shocks defined in Equation (5). Similar to Figure 7, we find limited evidence of systematic differences across education groups. In general, the MPCs out of negative shocks are higher and less precisely estimated than that out of positive shocks. The MPCs out of negative shocks are generally high for both liquidity-constrained and non-constrained

[13] This is consistent with the findings of Christelis et al. (2019) who studies responses of a representative sample of Dutch households to survey questions.

Figure 7: MPC out of negative and positive transitory income shocks



Notes: Figure 7 shows estimates for the MPC separately for positive and negative transitory income shocks from equation (5). Primary (-) on the x-axis refers to primary-educated households with negative transitory income shocks, and Primary (+) refers to primary-educated households with positive transitory income shocks. The other education groups are represented analogously. The estimates are based on 1,423,797 households aged 31 to 80 in 2005-2019. The points refer to the MPC coefficient estimates, and 95% confidence intervals based on standard errors clustered at the individual level are represented by the vertical lines.

households, at close to 0.75. The MPC out of positive shocks is similar among liquidity-constrained households, but significantly lower – close to 0.4 – among non-constrained households. The MPC out of positive shocks is also particularly low for those who have a high liquid asset-to-income ratio.

5.3 Time Variation in the Marginal Propensity to Consume

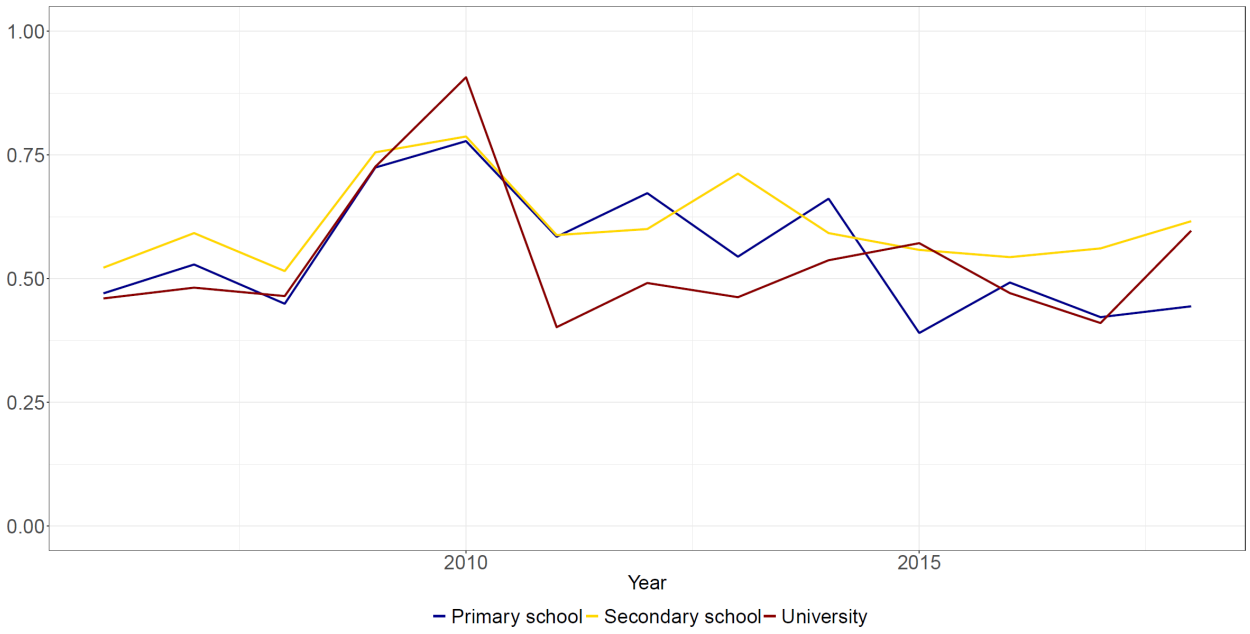
In this section, we illustrate the time variation in the MPC estimated in Section 5.1. The analysis is motivated by the degree of asymmetry found in Section 5.2, where we show that the MPC is significantly lower for positive transitory income shocks than it is for negative transitory income shocks. This holds for all education groups. The findings suggest that the MPC might be higher during economic downturns and crises than it is when the economy operating near equilibrium or experiencing expansion.

The empirical strategy is the same as the one underlying Figure 7; however, it replaces the interaction of the relevant subgroup with year fixed effects. As such, year fixed effects are added to the interaction between the instrument and the education group in Equation (4).

Figure 8 presents the results, highlighting the significant increase in the MPC following the global financial crisis and the collapse of Iceland's banking system in 2008. While the increase

is broadly similar across all education groups, it is most pronounced among university-educated households. As the economy began to recover in 2011, the MPC of all groups declined; however, this reduction was milder for households with primary and secondary education compared to those with university education. Therefore, although all education groups experienced a notable increase in the MPC during the crisis and a subsequent decline once the recovery took hold, the fluctuations were most pronounced for university educated households.

Figure 8: MPC by year



Notes: Figure 8 shows how the estimated MPCs vary over our sample period. It is estimated with a slight variation of Equation 5. In particular, year fixed effects are added to the interactions in the 2SLS specification in Equation 5. The regression includes controls for individual and year fixed effects and dummy variables for gender, marital status, number of children, the interaction between marital status and number of children, education, sector of work and the degree of urbanisation. Furthermore, we control for the log of real estate assets.

Appendix Figure A2-10 shows the time variation of the MPC separately for negative and positive income shocks. It shows that the increase in the MPC during the crisis, as reported in Figure 8, is driven by a strong consumption to negative income shocks. In fact, during the crisis households appear to have cut consumption when faced with a negative income shock approximately one-to-one. Once the economic recovery took hold as of 2011, the MPC out of negative income shocks declined, suggesting households increased consumption smoothing. The MPC out of positive income shocks also rose during the crisis but not to the same extent. Households raised their consumption slightly more when faced with a positive shock during the crisis compared to when the economy operated closer to equilibrium or during economic expansions. As such, households also appear to smooth consumption to a lesser extent during the crisis than outside the crisis.

6 Further Perspectives based on Survey Evidence

We have demonstrated three ways in which the education groups differ in consumption behavior. First, the life-cycle consumption profile of the university-educated features a more prominent hump-shape than the profiles of the lower-education groups. Second, the MPC out of transitory income shocks is lower for the university-educated than the other education groups, implying a higher degree of consumption smoothing.

We conducted a survey to better understand households' saving motives and behavior, and to gather insights on these results. The survey covered 946 individuals. Of these, 33.6% work in the private sector, 27.3% work in the public sector, 22.9% are not employed, 11.5% are self-employed, and the rest work for private institutions and voluntary associations.[14] The first question is:

Which of the following choices best describes your main motivation for saving? (multiple answers not permitted).

(1) I don't save; (2) I save for retirement; (3) I save to be able to react to unanticipated expenditures or drop in income; (4) I save for specific future expenditures such as housing and vehicles; (5) I save for future expenditures such as hobbies or vacations; (6) I save to finance future consumption; (7) I save out of habit; (8) I save to provide bequests; or (9) Other.

The responses are presented in Figure 9 below. Primary-educated households are more motivated by buffer savings, labelled precautionary, while young, whereas the buffer savings motivation seems to peak later in life for university-educated households. The university-educated are more motivated to save for future consumption while young compared to their less educated counterparts.

Retirement seems to play less of a role in motivating saving behavior among the university-educated.A larger proportion of the non-university-educated report that they do not save, which is in line with decreasing MPC in income.

The second question is:

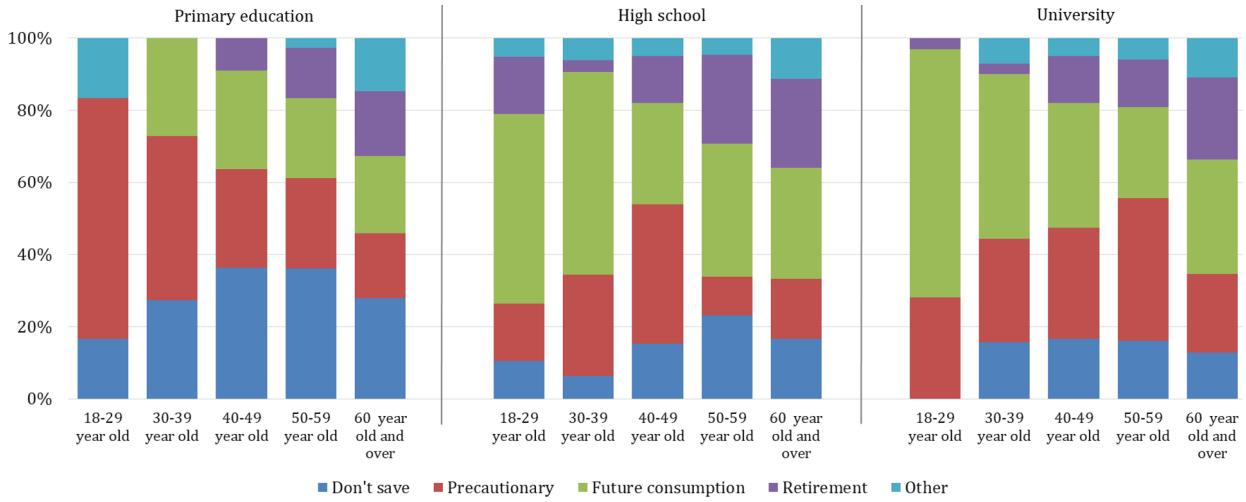
Which of the following options best describes how you save, apart from pension savings?

(1) I don't save apart from pension savings; (2) I have specific expenditures each month, and I save if my income is higher, (3) I save a fixed proportion of my income each month, (4) I save a fixed sum each month, (5) I set myself a specific goal for savings over a period and organize my saving accordingly, (6) Other. Again, multiple answers are not permitted.

Figure 10 decomposes saving behavior by education and age. It turns out that individuals without a university degree are more prone to save only through pension savings. This implies that any changes in disposable income would directly lead to a change in current consumption. On the other hand, consumption smoothing behavior, captured by saving when income is higher than planned expenditures, increases with education. This corresponds to our findings that those with a higher level of education have

[14]The survey was conducted by the firm Maskina for the purpose of this study between 27 September and 7 October 2021.

Figure 9: Main motivation for saving, by education and age



Notes: Figure 9 shows results from question 1, by education and age. Choices (4), (5), and (6) are grouped together and labelled as future consumption while choices (7), (8), and (9) are grouped together and labelled as other. Based on 785 observations, including 136 individuals whose highest educational attainment is primary education, 269 whose highest educational attainment is secondary school education, and 380 who are university-educated.

a lower propensity to consume out of current income, which is somewhat consistent with the estimation results in Table 2 and Figure 6 suggesting university-educated households have the lowest MPC out of transitory income shocks.

7 Concluding Remarks

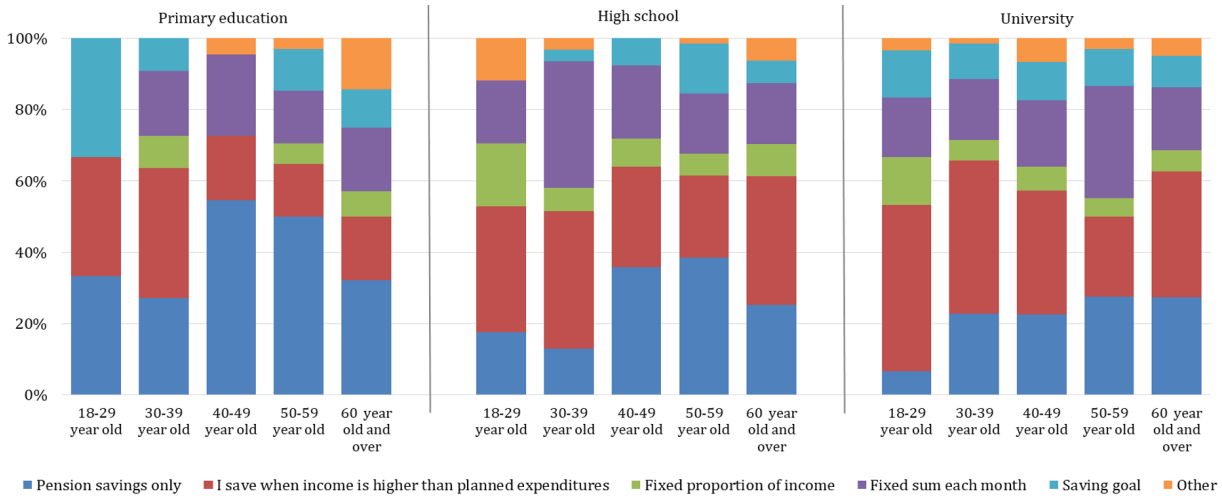
Previous research suggests education matters for a number of outcomes related to household behavior. For example, a higher level of educational attainment may increase saving rates and have a positive impact on financial market participation. Also, compared to workers with lower education, there is a finding of a larger hump in consumption for highly educated workers, who have also been found to have relatively steeper income and consumption profiles in the first half of their working life.

In this paper, we have further explored the role of education in life-cycle patterns of consumption. Using administrative tax records from Iceland over the period 2005-2019, a period that saw large fluctuations in real disposable income due to the 2008 financial crisis, we have compared consumption behavior across households with different levels of educational attainment: primary, secondary, and tertiary.

We find evidence for a hump-shaped life-cycle consumption profile for secondary and university-educated households. The hump is more pronounced for university-educated households. The life-cycle consumption profile for primary educated households, however, is almost strictly increasing in age. The steeper consumption profile for the university-educated in the first half of their working life mirrors their steeper income path, suggesting the presence of liquidity constraints or buffer stock savings.

Our results suggest that consumption smoothing rises with education, as university-educated households respond less to unexpected changes in transitory income than do lower-educated households. Allowing for heterogeneity, we find that the relatively low MPC of university-educated households is

Figure 10: Saving behavior, by education and age



Notes: Figure 10 shows results from question 2, by education and age. Based on 767 observations, including 129 individuals whose highest educational attainment is primary education, 263 whose highest educational attainment is secondary school education, and 375 who are university-educated.

driven to an extent by the fact that they are overrepresented in the upper part of the income distribution, where the MPC is generally lower, and they are less likely to be liquidity-constrained. Furthermore, we find that households smooth consumption out of positive transitory income shocks to a much larger extent than out of negative shocks. This result is driven solely by non-liquidity-constrained households. While the MPC out of both positive and negative transitory income shocks for constrained households and the MPC out of negative income shocks is high, close to 0.75, the consumption response of non-constrained households out of positive income shocks is markedly more muted, with an MPC of approximately 0.4.

We also explore time variation in the MPC estimates over the sample period and find that it increased markedly during the aftermath of the global financial crisis and the failure of Iceland's banking system. Interestingly, this was driven by a sharp increase in the MPC out of negative transitory income shocks across all education groups, which households responded to by cutting consumption. This is potentially explained by buffer-stock saving behavior as for households whose wealth collapsed, risk aversion, which encourages households to save and get back to their target wealth, might outweigh their impatience and willingness to consume today.

These results are complemented with survey evidence, which suggests that university-educated households are more likely to report consumption smoothing behavior and that financing future consumption is their main motive for saving. On the other hand, lower-educated households are more likely to report behavior consistent with a high propensity to consume out of transitory income, such as saving only through mandatory pension contributions.

Our results suggests the stylized fact that inequality of consumption should be lower than inequality of disposable income. To illustrate this, we compute the Gini coefficient for both disposable income and consumption for households aged 31 to 80 over the sample period (2005-2019); see Figure A2-5. There is a rise in inequality during the financial bubble from 2005 to 2007, when higher-income individuals' capital income rose. Then inequality receded after 2008, when the stock market collapsed and capital income

fell. Thereafter, inequality increased gradually. As expected, inequality of consumption is smaller than inequality of income. It also increased during the bubble, but by less. The fall after 2008 was smaller as well, as is the recent increase.

The finding that the business cycle has a stronger impact on the consumption of the least educated due to their lower income and greater likelihood of being liquidity constrained and the weakest impact on the university-educated has a direct implication for the topical debate about inequality because if increased equality lowers the fluctuations of consumption over the business cycle. Our results are mainly indicative on the effect of education on the MPC but to the extent that higher education improves income it follows that better education may shield the population from the consumption effects of the business cycle and reduce the size of the cycle by lowering the Keynesian multiplier.

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Appendix

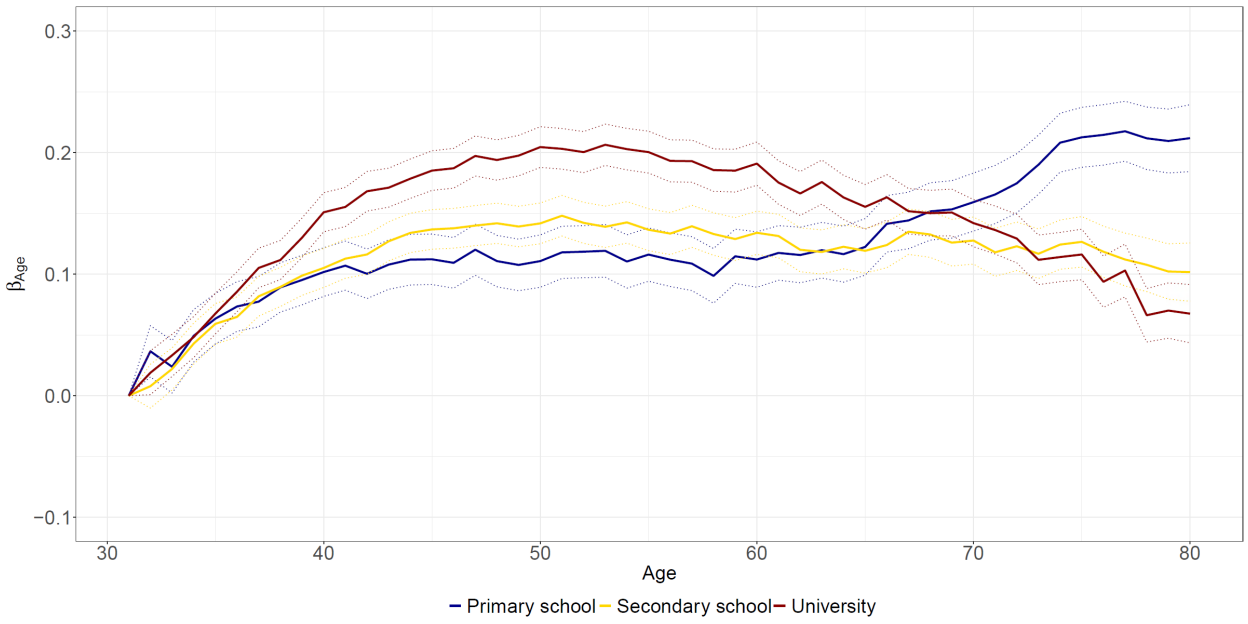
A1. Robustness Checks

A1.1 Robustness to Sample Restrictions

In our main estimation, we omit the top 1 percentile in imputed consumption to alleviate biases from potentially misattributing wealth declines to consumption when they might stem from unrealized capital losses or stock transactions not observed in the data. Here, we test the robustness to this by omitting the top 5 percentiles instead. Figure A1-1 plots the resulting life-cycle profiles for consumption. The results are similar to those reported in Figure 5 of the main text.

Furthermore, Table A1-1 reports the MPCs for this sample. The MPCs are somewhat lower from those reported in Table 2 of the main text. This is to be expected given the omission of more observations at the top of the consumption distribution. However, in both cases university-educated households have a significantly lower MPC out of transitory income shocks than primary school and secondary school-educated households. The difference is statistically significant at the 1% significance level, while in the main text the difference between the MPC of university-educated households and primary school-educated households was significant at the 5% significance level.

Figure A1-1: Life-cycle profile for consumption



Notes: Figure A1-1 plots the age effects (β_{age}) from Equation (2). While the top 1 percentile of the consumption distribution is omitted from the sample in the main analysis, here we omit the top 5 percentiles. Age 31 serves as a benchmark for each education group; therefore, the age effect is zero by definition for 31-year-olds, with the rest of the life-cycle profile defined in relation to that benchmark point. In addition to age effects, the regression includes controls for individual and year fixed effects and dummy variables for gender, marital status, number of children, the interaction between marital status and number of children, education, sector of work and the degree of urbanisation. Furthermore, we control for the log of real estate assets. The dotted lines show the 95% confidence interval based on standard errors clustered at the individual level.

Table A1-1: MPC by education

	(1) 31-80 years	(2) 31-66 years	(3) 67-80 years
$\hat{\hat{y}}_{i,t} \times primary$	0.469^{***} (0.021)	0.493^{***} (0.022)	0.347^{***} (0.058)
$\hat{\hat{y}}_{i,t} \times secondary$	0.485^{***} (0.016)	0.504^{***} (0.017)	0.349^{***} (0.045)
$\hat{\hat{y}}_{i,t} \times university$	0.380^{***} (0.018)	0.379^{***} (0.019)	0.391^{***} (0.068)
$R^2 = 0.021$ N = 1,388,160			

Notes: Table A1-1 presents 2SLS estimates from Equation (4) with the top 5 percentiles in consumption omitted, instead of the top 1 percentile in the main analysis. The estimates are based on households aged 31 to 80 in 2005-2019. Standard errors clustered at the individual level are in parentheses. *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. N is the number of observations.

A1.2 Alternative Education Classification

In our main specification, we define the household's level of education as the educational attainment of the more educated member of the household. However, some households with mixed educational attainment might have more in common with households with the same education as the lower-educated spouse. To ensure that our main results are robust to our education classification, we redo our analysis for three alternative education groups; households in which all members have primary education (this is the same sample as in our main specification), households in which all members have a secondary school education, and households in which all members have a university degree. Households with mixed educational attainment are, thus, omitted.

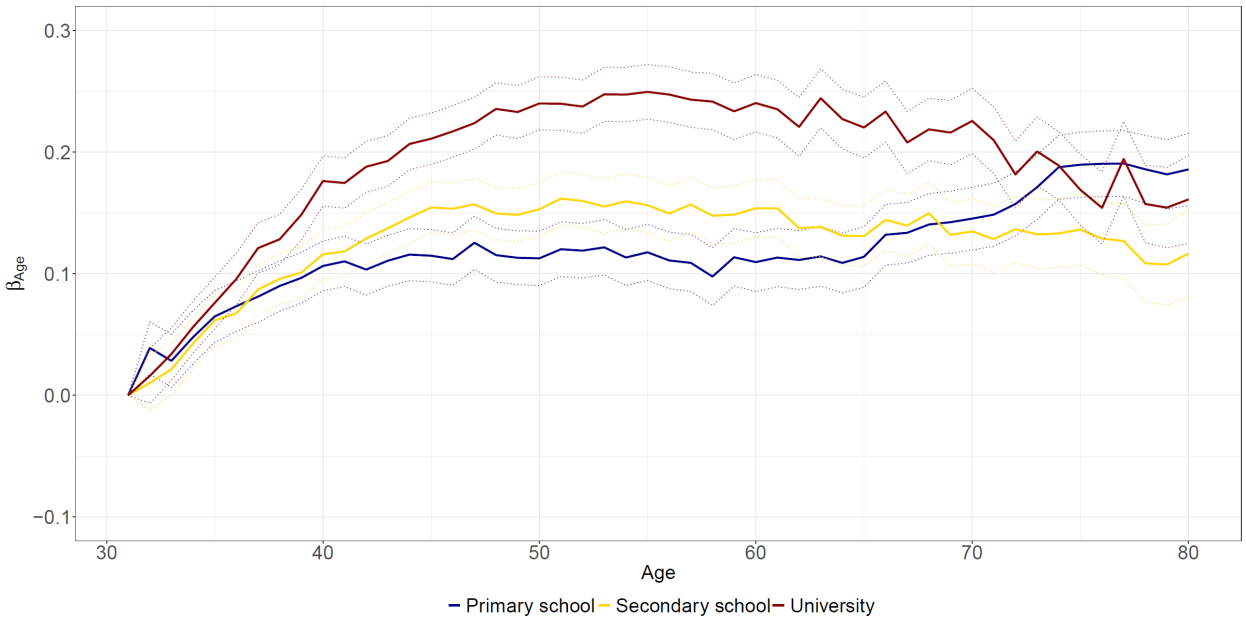
Figure A2-1 shows the resulting life-cycle profiles of consumption for each education group. The results are broadly similar to those depicted in Figure 5. However, as the sample of the top two education groups is smaller than in our main specification, especially as individuals age, the profile is less precisely estimated.

Table A1-2 shows the resulting MPC estimates. They are broadly similar to the ones reported in Table 2 of the main text.

A1.3 Robustness to Alternative Standard Error Clustering

Tables A1-3 to A1-5 illustrate the robustness of our MPC estimates to alternative standard error clustering.

Figure A2-1: Life-cycle profile for consumption



Notes: Figure A2-1 plots the age effects (β_{age}) from Equation (2) using a definition of education that differs from the baseline, presented in Table 2, in that the household is assigned to an education group if, and only if, all members within the household belong to that education group. This differs from the baseline, where the education group of the household is determined by the household's highest educational attainment. Age 31 serves as a benchmark for each education group; therefore, the age effect is zero by definition for 31-year-olds, with the rest of the life-cycle profile defined in relation to that benchmark point. In addition to age effects, the regression includes controls for individual and year fixed effects and dummy variables for gender, marital status, number of children, the interaction between marital status and number of children, education, sector of work and the degree of urbanisation. Furthermore, we control for the log of real estate assets. The dotted lines show the 95% confidence interval based on standard errors clustered at the individual level.

A2. Supplementary Figures and Tables

Figure A2-1 shows the coefficient estimates from regressing our imputed consumption, deflated using Statistic Iceland's CPI index, on the interaction of real consumption and year dummy variables, while controlling for individual fixed effects.

Appendix Tables A2-1 to A2-4 reports the estimates and standard errors underlying Figure 6.

Figure A2-2 shows the life-cycle profiles of consumption as estimated by equation (2), with an additional control for the logarithm of current income. Controlling for income, the hump in the life-cycle profiles reported in Figure 2 virtually disappear.

Figure A2-4 shows MPCs by income, wealth and liquidity estimated with equation (4) with additional interactions for age groups 31-66 (working age) and 67-80 (retirees). Specifically, the figure shows the results for retirees. Interestingly, the results for retirees are different from those obtained for the working age population. In particular, the MPC of university educated retirees tend to be higher than for the other retirees. Thus, since we find a lower MPC for university graduates compared to other education groups in our main specification, excluding retirees from the sample would yield a larger difference in MPCs across education groups

Table A1-2: MPC by education

	(1) 31-80 years	(2) 31-66 years	(3) 67-80 years
$\hat{\hat{y}}_{i,t} \times primary$	0.553^{***} (0.021)	0.577^{***} (0.022)	0.427^{***} (0.057)
$\hat{\hat{y}}_{i,t} \times secondary$	0.611^{***} (0.022)	0.623^{***} (0.023)	0.494^{***} (0.068)
$\hat{\hat{y}}_{i,t} \times university$	0.520^{***} (0.024)	0.514^{***} (0.025)	0.614^{***} (0.108)
$R^2 = 0.036$ N = 891,171			

Notes: Table A1-2 presents 2SLS estimates from Equation (4) using a definition of education that differs from the baseline, presented in Table 2, in that the household is assigned to an education group if, and only if, all members within the household belong to that education group. This differs from the baseline, where the education group of the household is determined by the household's highest educational attainment. The estimates are based on households aged 31 to 80 in 2005-2019. Standard errors clustered at the individual level are in parentheses and the probability that the estimated parameters are equal in the last column. *** denotes significance at the 1% level. N is the number of observations.

Table A1-3: MPC by education

	(1) 31-80 years	(2) 31-66 years	(3) 67-80 years
$\hat{\hat{y}}_{i,t} \times primary$	0.551^{***} (0.033)	0.576^{***} (0.033)	0.422^{***} (0.056)
$\hat{\hat{y}}_{i,t} \times secondary$	0.604^{***} (0.025)	0.621^{***} (0.027)	0.484^{***} (0.050)
$\hat{\hat{y}}_{i,t} \times university$	0.526^{***} (0.034)	0.514^{***} (0.031)	0.679^{***} (0.098)
$R^2 = 0.033$ N = 1,423,797			

Notes: Table A1-3 presents 2SLS estimates from Equation (4). As opposed to Table 2 in the main text which reports standard errors clustered at the individual level, Table A1-3 reports standard errors clustered at the individual and year level. The estimates are based on households aged 31 to 80 in 2005-2019. Standard errors clustered at the individual level are in parentheses. *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. N is the number of observations.

Figure A2-5 is referred to in the conclusion section of the main paper. It illustrates the rise in income inequality during the financial bubble of 2005-2007 and the reduction in inequality as the stock market

Table A1-4: MPC by education

	(1) 31-80 years	(2) 31-66 years	(3) 67-80 years
$\hat{\hat{y}}_{i,t} \times primary$	0.551^{***} (0.037)	0.576^{***} (0.036)	0.422^{***} (0.054)
$\hat{\hat{y}}_{i,t} \times secondary$	0.604^{***} (0.026)	0.621^{***} (0.028)	0.484^{***} (0.043)
$\hat{\hat{y}}_{i,t} \times university$	0.526^{***} (0.032)	0.514^{***} (0.028)	0.679^{***} (0.111)
R^2 = 0.033 N = 1,423,797			

Notes: Table A1-4 presents 2SLS estimates from Equation (4). As opposed to Table 2 in the main text which reports standard errors clustered at the individual level, Table A1-4 reports standard errors clustered at the cohort and year level. The estimates are based on households aged 31 to 80 in 2005-2019. Standard errors clustered at the individual level are in parentheses. *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. N is the number of observations.

Table A1-5: MPC by education

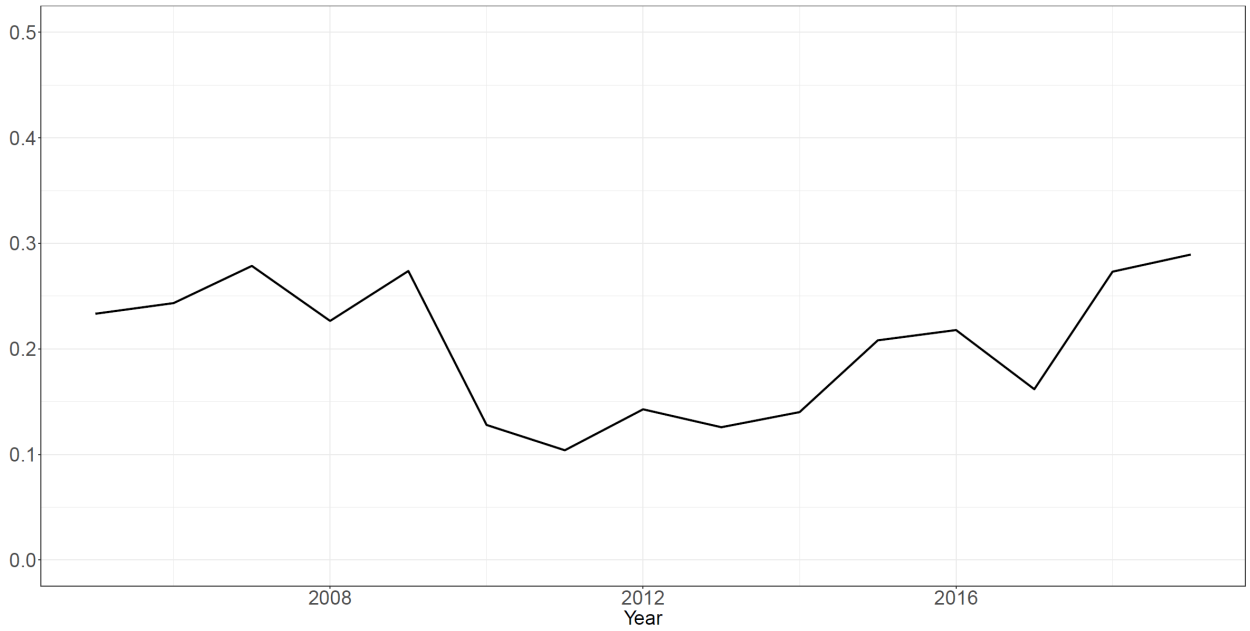
	(1) 31-80 years	(2) 31-66 years	(3) 67-80 years
$\hat{\hat{y}}_{i,t} \times primary$	0.551^{***} (0.046)	0.576^{***} (0.056)	0.422^{***} (0.023)
$\hat{\hat{y}}_{i,t} \times secondary$	0.604^{***} (0.014)	0.621^{***} (0.024)	0.484^{***} (0.065)
$\hat{\hat{y}}_{i,t} \times university$	0.526^{***} (0.022)	0.514^{***} (0.016)	0.679^{***} (0.065)
R^2 = 0.033 N = 1,423,797			

Notes: Table A1-5 presents 2SLS estimates from Equation (4). As opposed to Table 2 in the main text which reports standard errors clustered at the individual level, Table A1-5 reports standard errors clustered at the degree of urbanization, which serves as a proxy for region of residence, and year level. The estimates are based on households aged 31 to 80 in 2005-2019. Standard errors clustered at the individual level are in parentheses. *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. N is the number of observations.

collapsed and capital income fell. As expected, inequality of consumption is smaller than that of income.

Figures A2-6 to A2-9 show the MPC out of positive and negative transitory income shocks by wealth,

Figure A2-1: The relationship between consumption and income by year



Notes: Figure A2-1 shows the coefficient estimates from regressing real consumption, deflated using Statistic Iceland's CPI index, on the interaction of real consumption and year dummy variables, while controlling for individual fixed effects.

Table A2-1: MPC by disposable income quintiles

	Primary	Secondary	University
$\Delta\hat{y}_{i,t} \times Q1$	0.733^{***}	0.765^{***}	0.693^{***}
	(0.042)	(0.036)	(0.039)
$\Delta\hat{y}_{i,t} \times Q2$	0.611^{***}	0.773^{***}	0.817^{***}
	(0.072)	(0.055)	(0.055)
$\Delta\hat{y}_{i,t} \times Q3$	0.458^{***}	0.650^{***}	0.649^{***}
	(0.080)	(0.066)	(0.065)
$\Delta\hat{y}_{i,t} \times Q4$	0.690^{***}	0.667^{***}	0.610^{***}
	(0.065)	(0.053)	(0.058)
$\Delta\hat{y}_{i,t} \times Q5$	0.466^{***}	0.521^{***}	0.440^{***}
	(0.024)	(0.017)	(0.020)
$N = 1,423,797$			

Notes: Table A2-1 presents the MPC estimates, shown in Figure A2-3, obtained from Equation (4) with additional interactions with disposable income quintiles. The estimates are based on households aged 31 to 80 in 2005-2019. Standard errors clustered at the individual level are in parentheses. Q1 is the lowest quintile and Q5 is the highest. *** denotes significance at the 1% level, and ** denotes significance at the 5% level. N is the number of observations.

income and liquidity across education groups.

Figure A2-10 shows the time variation in the MPC out of negative income shocks in panel (a) and

Table A2-2: MPC by net-wealth-to-income quintiles

	Primary	Secondary	University
$\Delta\hat{y}_{i,t} \times Q1$	0.845^{***}	0.912^{***}	0.789^{***}
	(0.037)	(0.037)	(0.039)
$\Delta\hat{y}_{i,t} \times Q2$	0.548^{***}	0.614^{***}	0.470^{***}
	(0.027)	(0.023)	(0.026)
$\Delta\hat{y}_{i,t} \times Q3$	0.374^{***}	0.420^{***}	0.370^{***}
	(0.038)	(0.023)	(0.027)
$\Delta\hat{y}_{i,t} \times Q4$	0.424^{***}	0.485^{***}	0.414^{***}
	(0.072)	(0.046)	(0.048)
$\Delta\hat{y}_{i,t} \times Q5$	0.518^{***}	0.707^{***}	0.719^{***}
	(0.080)	(0.048)	(0.049)
$N = 1,423,797$			

Notes: Table A2-2 presents the MPC estimates, shown in Figure A2-3, obtained from Equation (4) with additional interactions with net-wealth-to-income quintiles. The estimates are based on households aged 31 to 80 in 2005-2019. Standard errors clustered at the individual level are in parentheses. Q1 is the lowest quintile and Q5 is the highest. *** denotes significance at the 1% level, and ** denotes significance at the 5% level. N is the number of observations.

Table A2-3: MPC by liquidity

	Primary	Secondary	University
$\Delta\hat{y}_{i,t} \times Q1$	0.752^{***}	0.909^{***}	0.715^{***}
	(0.033)	(0.030)	(0.040)
$\Delta\hat{y}_{i,t} \times Q2$	0.722^{***}	0.746^{***}	0.764^{***}
	(0.049)	(0.040)	(0.044)
$\Delta\hat{y}_{i,t} \times Q3$	0.730^{***}	0.697^{***}	0.609^{***}
	(0.045)	(0.035)	(0.036)
$\Delta\hat{y}_{i,t} \times Q4$	0.409^{***}	0.535^{***}	0.420^{***}
	(0.037)	(0.027)	(0.029)
$\Delta\hat{y}_{i,t} \times Q5$	0.346^{***}	0.437^{***}	0.425^{***}
	(0.042)	(0.028)	(0.030)
$N = 1,423,797$			

Notes: Table A2-3 presents the MPC estimates, shown in Figure A2-3, obtained from Equation (4) with additional interactions with liquid-assets-to-income quintiles. The estimates are based on households aged 31 to 80 in 2005-2019. Standard errors clustered at the individual level are in parentheses. Q1 is the lowest quintile and Q5 is the highest. *** denotes significance at the 1% level, and ** denotes significance at the 5% level. N is the number of observations.

positive income shocks in panel (b).

Figures A2-11 to A2-13 show the share of observations with negative consumption by age groups, disposable income groups, and permanent income groups, respectively. Overall, approximately 6% of

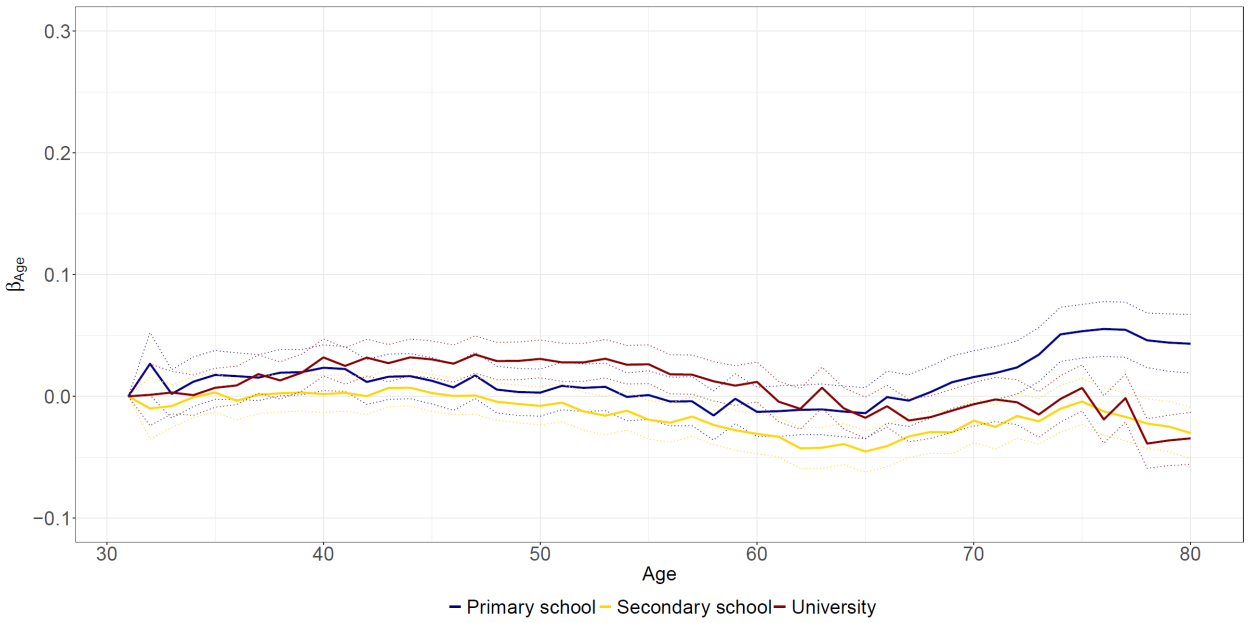
Table A2-4: MPC by liquidity constraint

	Primary	Secondary	University
$\Delta\hat{y}_{i,t} \times NC$	0.448^{***}	0.516^{***}	0.485^{***}
	(0.026)	(0.019)	(0.020)
$\Delta\hat{y}_{i,t} \times LC$	0.732^{***}	0.815^{***}	0.649^{***}
	(0.029)	(0.026)	(0.032)
$N = 1,423,797$			

Notes: Table A2-4 presents the MPC estimates, shown in Figure A2-3, obtained from Equation (4) with additional interactions with a liquidity constraint. A household is considered liquidity constrained if liquid assets are less than USD 8,000 for couples and USD 4,000 fir singles. The estimates are based on households aged 31 to 80 in 2005-2019. Standard errors clustered at the individual level are in parentheses. NC denotes not constrained and LC denotes liquidity constrained. *** denotes significance at the 1% level, and ** denotes significance at the 5% level. N is the number of observations.

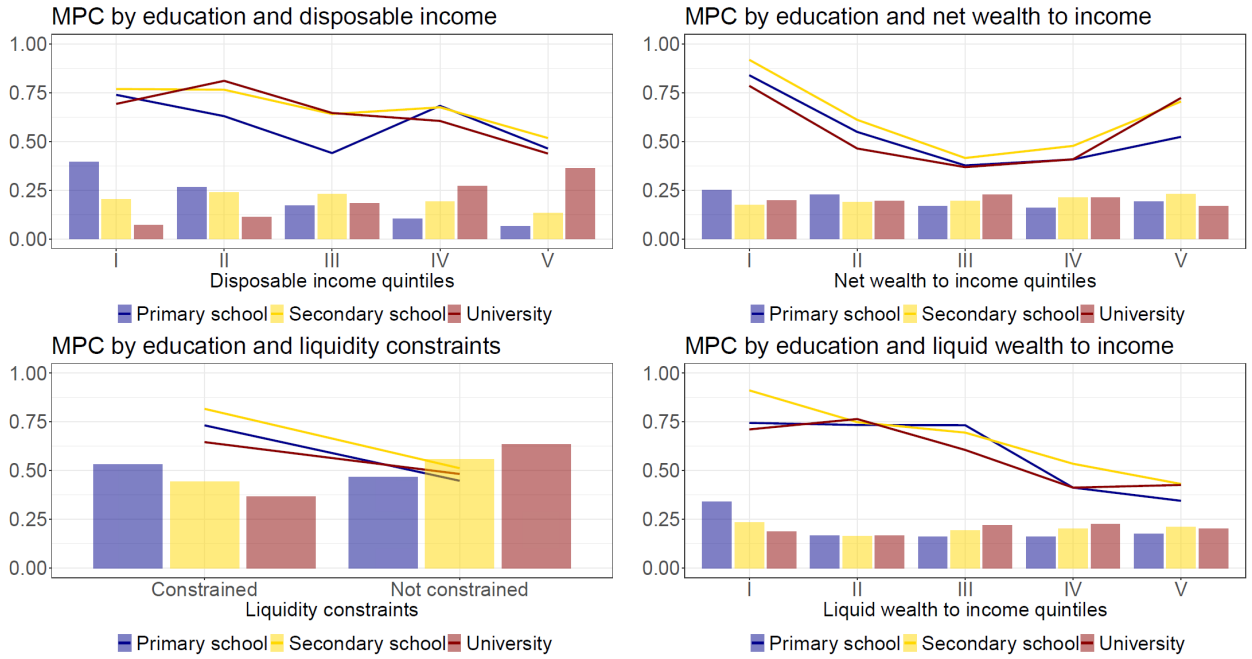
observations have negative imputed values for consumption. Those observations are omitted in the main analysis. The figures show that there is little evidence of particularly high rates of negative imputed consumption among certain types of households. The frequency of negative consumption is approximately constant over the life cycle, although it is relatively low for the eldest age group. Both the bottom and top quintile of the disposable income distribution have a slightly higher frequency of negative consumption compared to the middle quintiles. The share of observations with negative consumption is highest for the top quintile in the permanent income distribution. As discussed in the Section 3, negative imputed consumption likely stems from households with intra-year transactions in real estate or financial assets and from changes in household composition.

Figure A2-2: Life-cycle profile for consumption



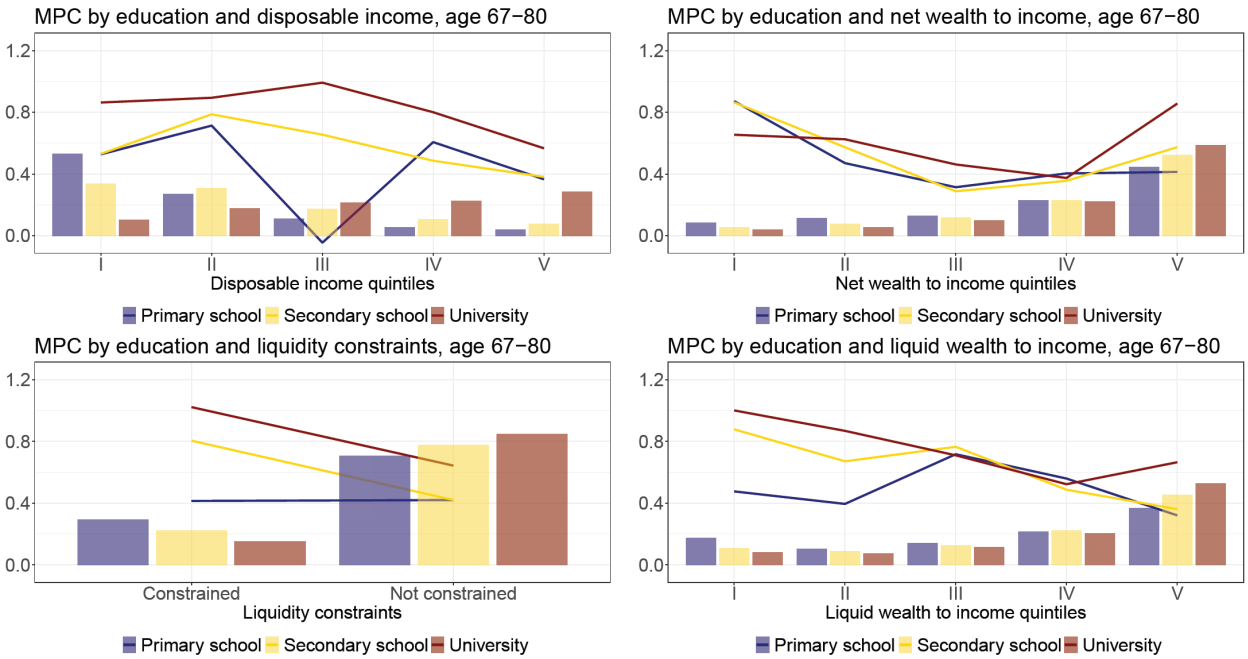
Notes: Figure A2-2 plots the age effects (β_{age}) from Equation (2) with an additional control for the logarithm of current income. Age 31 serves as a benchmark for each education group; therefore, the age effect is zero by definition for 31-year-olds, with the rest of the life-cycle profile defined in relation to that benchmark point. In addition to age effects, the regression includes controls for individual and year fixed effects and dummy variables for gender, marital status, number of children, the interaction between marital status and number of children, education, sector of work and the degree of urbanisation. Furthermore, we control for the log of real estate assets. The dotted lines show the 95% confidence interval based on standard errors clustered at the individual level.

Figure A2-3: MPC by income, wealth and liquidity



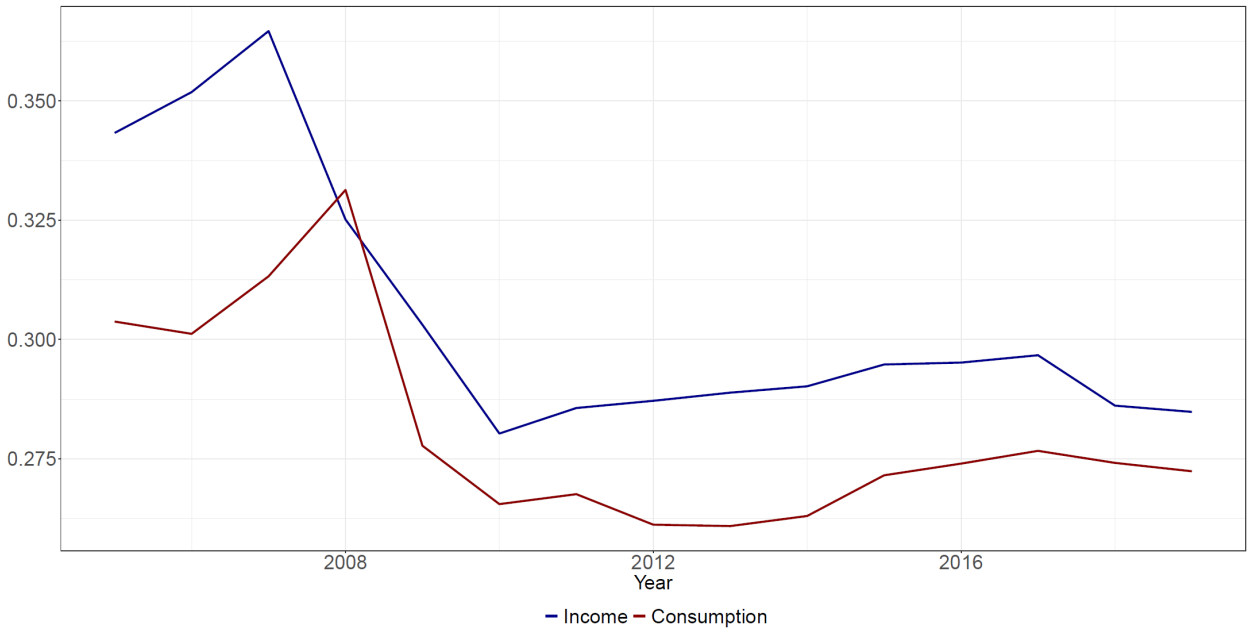
Notes: Figure A2-3 shows the MPC estimates for the whole sample aged 31-80 from Equation (4) with additional interactions with disposable income quintiles (upper left panel), net wealth-to-income quintiles (upper right panel), absolute liquidity constraint (lower left panel), and liquid wealth-to-income quintiles (lower right panel). Liquid wealth is defined as the sum of bank deposits and assets in investment and savings funds. A household is considered liquidity-constrained in the lower left panel if liquid wealth is below USD 4,000 for singles and USD 8,000 for couples. The lines show estimates of the marginal propensity to consume for subgroup or quintile. The columns represent the share of each education group within the respective subgroup or quintile. The estimates are based on 1,423,797 households aged 31 to 80 in 2005-2019, but with interaction terms for the working-age population (aged 31-66) and retirees (age 67 and above).

Figure A2-4: MPC by income, wealth and liquidity



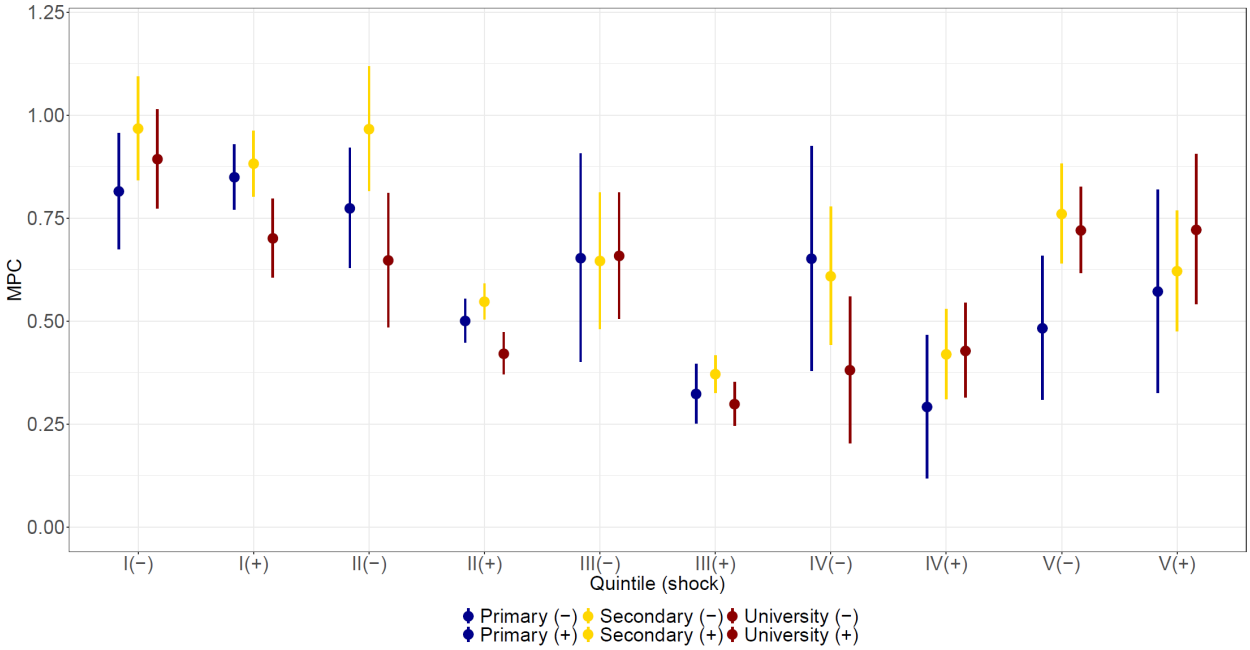
Notes: Figure A2-4 show the MPC estimates for the population of retirees (aged 67-80) from equation (4) with additional interactions with disposable income quintiles (upper left panel), net wealth-to-income quintiles (upper right panel), absolute liquidity constraint (lower left panel), and liquid wealth-to-income quintiles (lower right panel). Liquid wealth is defined as the sum of bank deposits and assets in investment and savings funds. A household is considered liquidity-constrained in the lower left panel if liquid wealth is below USD 4,000 for singles and USD 8,000 for couples. The lines show estimates of the marginal propensity to consume for subgroup or quintile. The columns represent the share of each education group within the respective subgroup or quintile. The estimates are based on 1,423,797 households aged 31 to 80 in 2005-2019, but with interaction terms for the working-age population (aged 31-66) and retirees (age 67 and above).

Figure A2-5: Income and consumption inequality



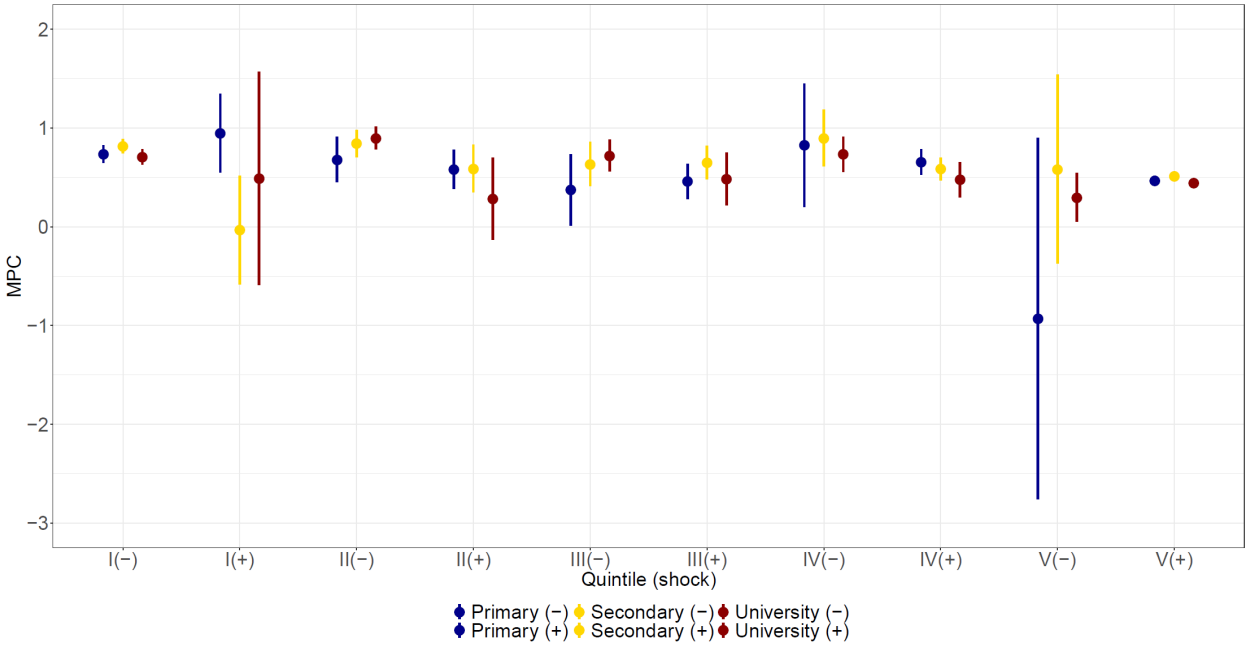
Notes: Figure A2-5 shows the intra-year Gini coefficient for consumption and disposable income. The estimates are based on households aged 31 to 80 in 2005-2019.

Figure A2-6: MPC by education and net wealth to income



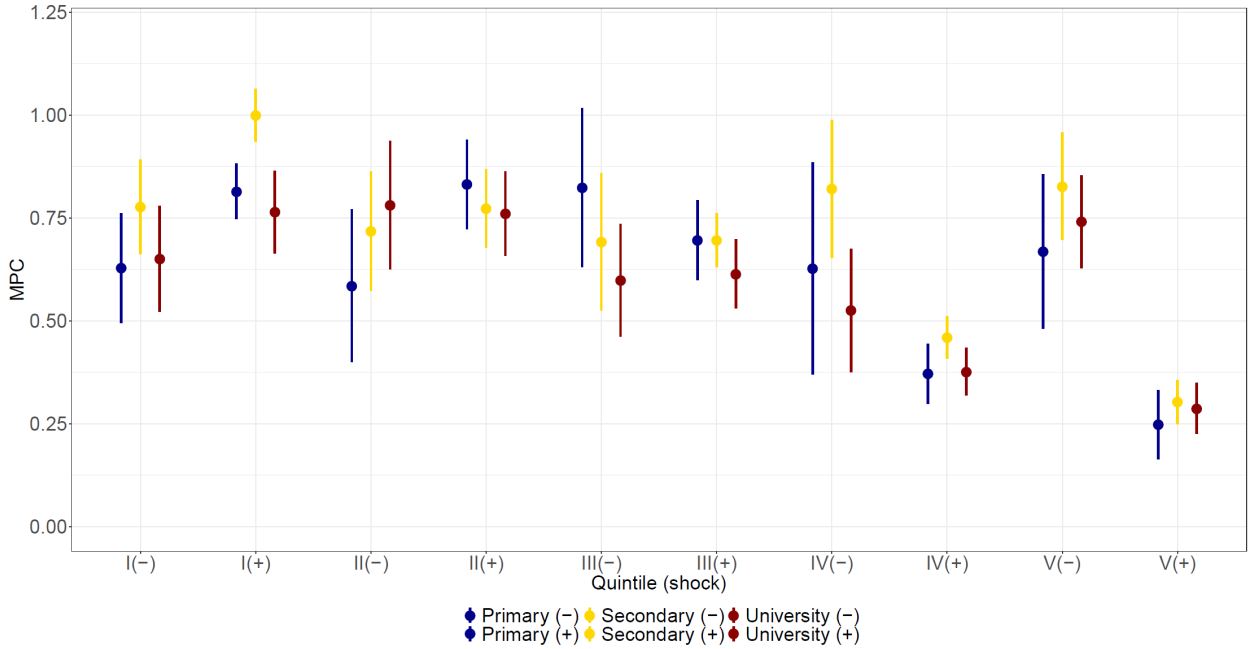
Notes: Figure A2-6 shows the MPC estimates from Equation (5) with added interactions for net-wealth-to-disposable-income quintiles. The points refer to the MPC coefficient estimates, and 95% confidence intervals based on standard errors clustered at the individual level are represented by the vertical lines. Primary-educated households are shown in blue, secondary-educated households in yellow, and university-educated households in red. I(-) on the x-axis refers to the first quintile of the distribution and a negative transitory income shock. I(+) refers to the first quintile of the distribution and a positive transitory income shock. The other quintiles are represented analogously.

Figure A2-7: MPC by education and disposable income



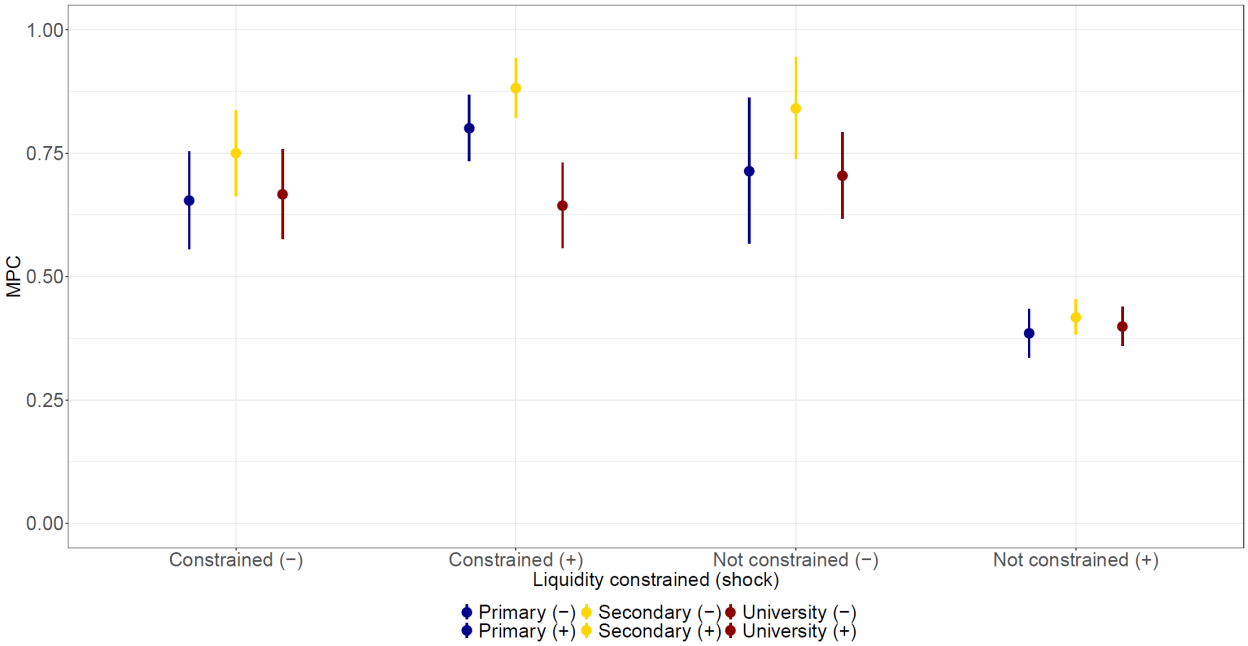
Notes: Figure A2-7 shows the MPC estimates from Equation (5) with added interactions for disposable income quintiles. The points refer to the MPC coefficient estimates, and 95% confidence intervals based on standard errors clustered at the individual level are represented by the vertical lines. Primary-educated households are shown in blue, secondary-educated households in yellow, and university-educated households in red. I(-) on the x-axis refers to the first quintile of the distribution and a negative transitory income shock. I(+) refers to the first quintile of the distribution and a positive transitory income shock. The other quintiles are represented analogously.

Figure A2-8: MPC by education and liquidity constraints, age 31-80



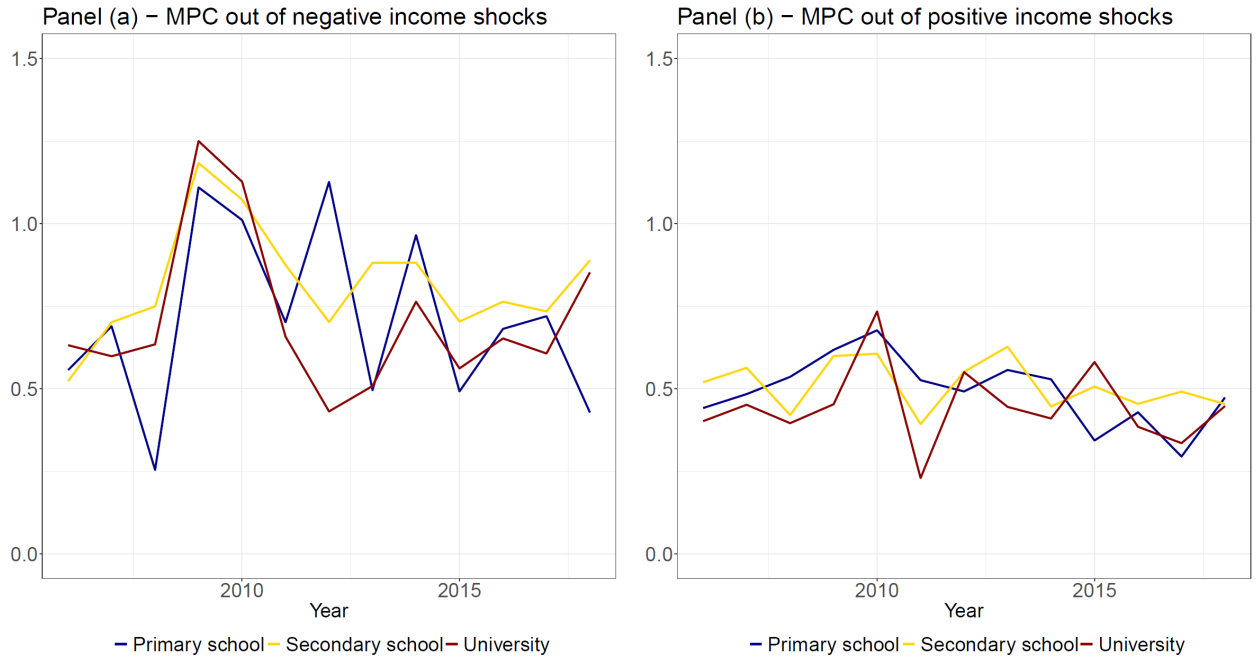
Notes: Figure A2-8 shows the MPC estimates from Equation (5) with added interactions for liquidity constraints. A household is considered liquidity-constrained if the sum of bank deposits and assets in investment and savings funds is below USD 4,000 for singles and USD 8,000 for couples. The points refer to the MPC coefficient estimates, and 95% confidence intervals based on standard errors clustered at the individual level are represented by the vertical lines. Primary-educated households are shown in blue, secondary-educated households in yellow, and university-educated households in red. Constrained (-) on the x-axis refers to liquidity-constrained households and a negative transitory income shock. Constrained (+) refers to liquidity-constrained households and a positive transitory income shock. Non-constrained households are represented analogously.

Figure A2-9: MPC by education and liquid wealth to income, age 31-66



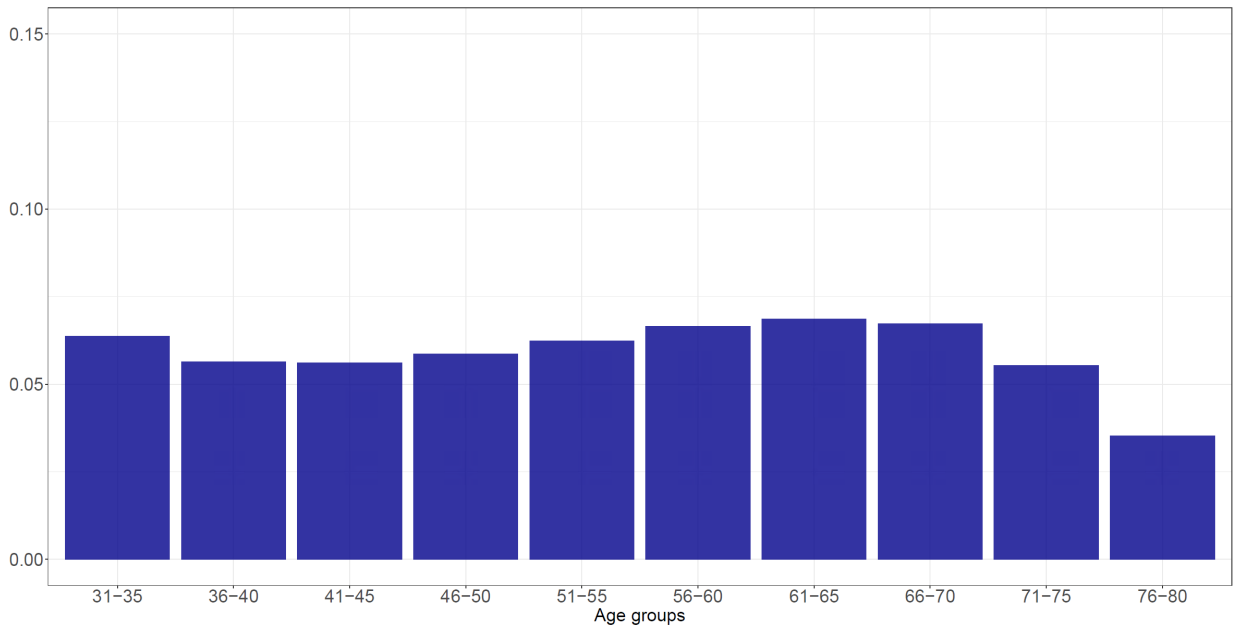
Notes: Figure A2-9 shows the MPC estimates from Equation (5) with added interactions for liquid asset-to-disposable-income quintiles. The points refer to the MPC coefficient estimates, and 95% confidence intervals based on standard errors clustered at the individual level are represented by the vertical lines. Primary-educated households are shown in blue, secondary-educated households in yellow, and university-educated households in red. I(-) on the x-axis refers to the first quintile of the distribution and a negative transitory income shock. I(+) refers to the first quintile of the distribution and a positive transitory income shock. The other quintiles are represented analogously.

Figure A2-10: MPC out of positive and negative income shocks by year



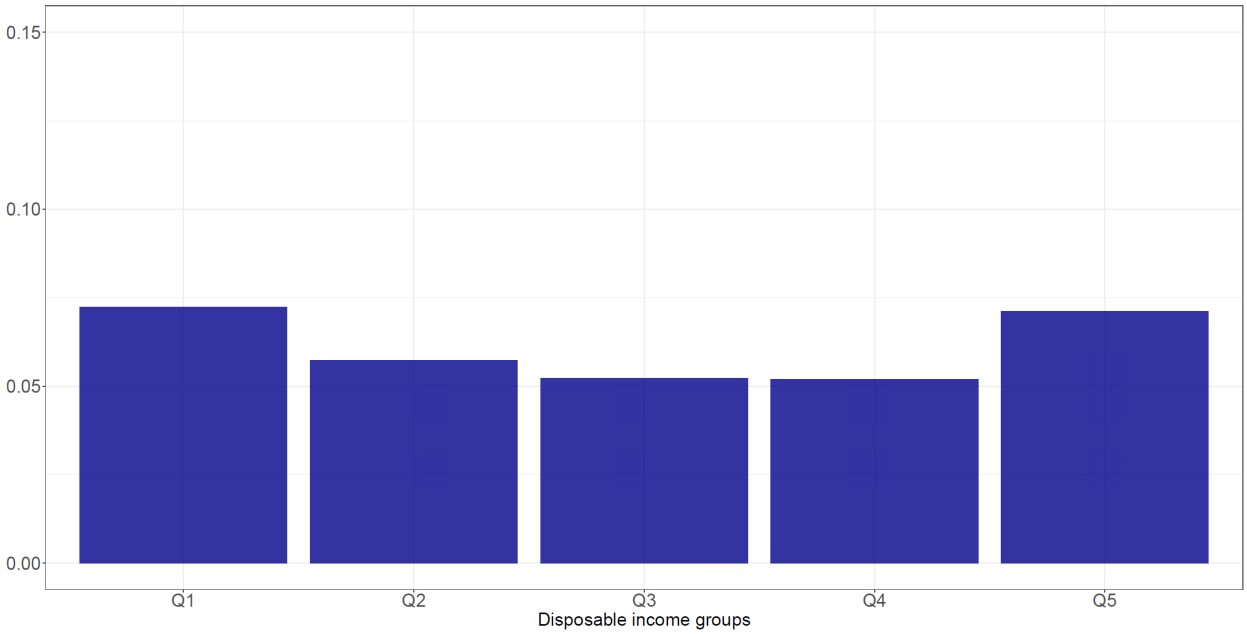
Notes: Figure A2-10 plots the time variation in the MPC separately for positive and negative income shocks. It is estimated with a slight variation of Equation 5. Age effects are omitted to allow for the inclusion of traditional year fixed effects in the regression of log income and consumption on various household characteristics to identify the consumption and income shocks. The regression includes controls for individual and year fixed effects and dummy variables for gender, marital status, number of children, the interaction between marital status and number of children, education, sector of work and the degree of urbanisation. Furthermore, we control for the log of real estate assets.

Figure A2-11: Share of observations with negative consumption



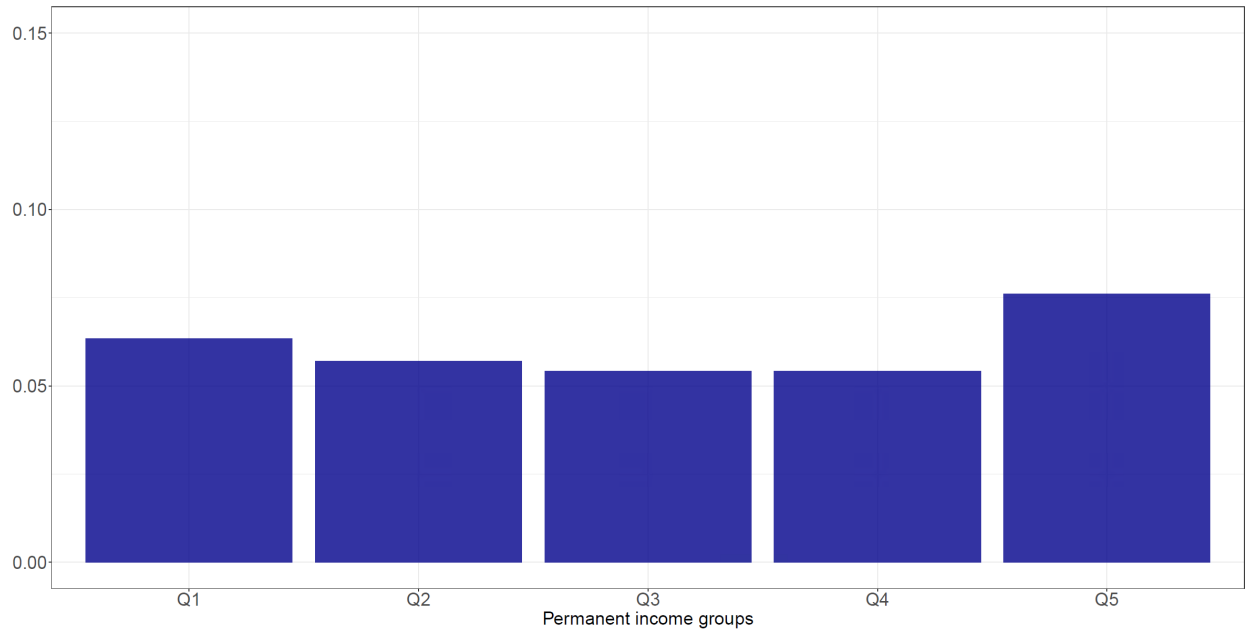
Notes: Figure A2-11 plots the share of observations with negative consumption by age groups.

Figure A2-12: Share of observations with negative consumption



Notes: Figure A2-12 plots the share of observations with negative consumption by disposable income quintiles. Q1 refers to the bottom quintile of the disposable income distribution and Q5 refers to the top quintile.

Figure A2-13: Share of observations with negative consumption



Notes: Figure A2-13 plots the share of observations with negative consumption by disposable income quintiles. Q1 refers to the bottom quintile of the disposable income distribution and Q5 refers to the top quintile.

Chapter 3

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

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

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June 28, 2024

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 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 supported 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

*We would like to thank John Friedman, Henrik Yde Andersen, Camilla Skovbo Christensen, Andrea Tafuro, Daniel Borowczyk-Martins, Yunus Aksoy, seminar participants at the Central Bank of Iceland, participants at the annual International Institute of Public Finance conference held in Reykjavik in August 2022, and seminar participants at the University of Arizona in October 2022. Any remaining errors are our own responsibility. This study was supported by the Icelandic Research Fund (Rannís), grant number: 207213-051, and PeRCent, which receives base funding from the Danish pension funds and Copenhagen Business School. The views expressed herein are those of the authors and do not necessarily reflect the views or policies of any entities we represent.

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1 Introduction

We take advantage of a natural experiment in pension reform in Iceland to study whether mandatory occupational pension savings crowd out voluntary saving. The reform equalized pension benefits between the private and the public sector, the latter having had more generous benefits. Specifically, the reform increased the contribution rate for private employers by 44 percent, elevating it from 8 percent to 11.5 percent, while maintaining a constant contribution rate in the public sector.

We contribute to the literature 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 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.[1] Third, we gauge the substitution effect between mandatory and voluntary saving within the pillars of 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 did not lead to lower voluntary private saving and, therefore, increased total household saving, as households did not respond by cutting voluntary saving. Moreover, we do not find evidence the reform led to substitution within the pension system as households did not respond to the increase in the mandatory pension saving rate by reducing their voluntary third-pillar pension saving rate. Also, our results do not suggest the reform led to higher debt or higher interest expenditures. In spite of significant media coverage of the reform, our survey results suggest widespread lack of awareness of 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 before the reform and in Section 7 we discuss the findings from a survey, generating insights into

[1] 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.

the results found in previous sections. Finally, we discuss the results and conclude the paper in Section 8.

2 Literature

We contribute to an empirical literature on the effects of mandatory pension contributions on voluntary saving. The issue of crowding-out of mandatory saving is not a new one. The literature started with early contributions from Feldstein (1974) who used an extended life-cycle model to 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. However, despite being 50 years old, the literature is inconclusive, with estimates of the crowding-out effect spanning the spectrum from no effect to full crowding-out. Two factors likely play a role in explaining the wide range of estimates, both of which our study can overcome. 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 Mitchell, 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). 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.

We are not the first to study the effect of pension systems on household saving in the Nordic countries. 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. 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 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, which is a limitation that our data does not have. Therefore, these authors 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 percent of individuals respond to these changes in the contribution rates. On average, the individuals who respond are 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 earnings 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.

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 1 Danish krona reduction in private pension contributions, resulting from a reduced tax subsidy for annuity pensions, 19 øre were used for repayment of mortgage debt and 61 øre were saved in taxable accounts.[2] Christensen and Ellegaard (2023) study the effect of a 2018 change in Denmark 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 cause individuals to lower their pension contributions, total retirement saving and total saving.

There is also a large literature from other countries. The key contributions are listed in Table 1. Engelhardt and Kumar (2011) use 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 pension wealth of 53-67 percent, with most of the effect 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 percent in their favorite specification. 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

[2]The unit below krona, equivalent to a European or American cent although different in value, is called øre.

or later using linked administrative and survey data. The reform increased the labor supply for lower-income workers, who postponed retirement, while higher-income workers increased their private savings to 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.

Our analysis has several strengths compared to the above studies using Danish data. First, there is the design of the natural experiment: a reform affecting all private sector workers (including the self-employed) leaving public sector workers as a control group. Second, there is the large size of the change affecting the private sector workers who experienced a 44 percent increase in their mandatory pension contribution paid by employers (from 8 percent to 11.5 percent). Third, we complement the analysis by testing whether the saving behavior of those who switched from the private sector to the public sector before the reform – when employers' contribution was lower in the private sector – changed. Finally, we conduct a survey to explore why people responded to the reform in the way they did.

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 both 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 percent for the vast majority of workers (Table 2). In 2022, the legislative minimum contribution rate was also raised from of 12 percent to 15.5 percent. However, due to the reform negotiated in 2016 and discussed in Section 3.2, the legislative changes in 2022 did not bind for the vast majority of

Table 1: Summary of recent literature on crowding-out effect on private saving

Study	Main results	Data & Identification strategy
Feldstein (1974)	Social security depresses personal saving by 30-50 percent.	Regresses consumption expenditure on permanent income and social security wealth using OLS and maximum likelihood.
Engelhardt and Kumar (2011)	Estimates a crowding-out effect of 53-67%. No crowding out for lower-wealth households, but significant crowding out for higher-wealth households.	Utilized data on pensions and lifetime earnings from the 1992 Health and Retirement Study (US), linked to administrative data for lifetime earnings. Employed OLS and IV regressions, with the dependent variable being non-pension household wealth and one of the explanatory variables being pension wealth.
Arnberg and Barslund (2014)	Find a crowding-out effect of mandatory saving on private saving ranging from 0-30%.	Utilized administrative data from Denmark spanning the period 1998-2005. Identified the crowding-out effect by exploiting differences in the timing of the implementation of a pension reform.
Chetty et al (2014)	Approximately 15% of individuals respond to changes in contribution rates, indicating a low crowding-out effect.	Utilized administrative data from Denmark spanning the period 1994-2009. Exploited variation arising from individuals changing jobs between occupations with differing employers' pension contributions and expected pension income.
Li et al. (2016)	Estimates a crowding-out effect of mandatory occupational pension saving on private wealth ranging from 33-80%.	Utilized administrative data from the Netherlands spanning the period 2007-2010. Utilized various identification strategies, including IV regressions, propensity score matching, and difference-in-difference analysis, leveraging a quasi-natural experiment during the Global Financial Crisis (GFC)
Andersen (2018)	Find that reduced tax incentives for private pensions lead to a decline in mortgage debt.	Utilized administrative data from Denmark spanning 2003-2013. Analyzed the effect of changes in tax credits for pension contributions for high-income earners on debt repayments and savings in non-retirement accounts.
Lachowska and Myck (2018)	Find a crowding-out effect of pension saving on other household saving to be 24%. The estimates of crowding out increase with age and education levels.	Utilized data from a household budget survey conducted in Poland from 1997-2003. Analyzed the effects of the pension reform in 1999 in Poland, which affected individuals differently based on age, by examining variations in household saving and expenditures over time.
Lindeboom and Montizaan (2020)	Find a crowding-in effect of 38% between public and private wealth. Reduced pension wealth led lower-income workers to postpone retirement due to inadequate savings, while higher-income workers increased private savings one-to-one.	Utilized linked administrative and survey data from the Netherlands. Analyzed the effects of a pension reform in the Netherlands in 2006, which reduced the public pension wealth for those born in 1950 or later, coupled with tax incentives for private saving. The analysis used cohorts of men born in 1950 and 1949 as treatment and control groups, respectively.
Christensen and Ellegaard (2023)	Finds a 20% substitution within the pension system and a total crowding out of 64%.	Utilized administrative data from Denmark spanning 2013-2019. Analyzed the effect of a 2018 change that reduced the tax subsidy for retirement saving for those contributing to a public pension scheme, affecting saving incentives for those in the middle of the income distribution.
Beshears et al. (2024)	Additional automatic retirement savings result in increased debt and a higher likelihood of having a mortgage.	Utilized data from a pension provider in the UK. Leveraged a natural experiment in the form of a randomized roll-out of automatic enrollment to a pension plan.

workers.

Table 2: A stylized overview of the contribution rates to pension funds and pension accounts in the Icelandic pension system post-2018

	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 2 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 can deduct from their taxable income a third-pillar pension contribution of up to 4.0 percent, with employers required to match these contributions one-for-one up to 2.0 percent. These contributions are deducted before taxes but are subject to taxation upon withdrawal from the third pillar. Thus, the combined second- and third-pillar pension contributions frequently total up to 21.5 percent 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 percent 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, and not to increase the country's saving rate as the size of the total pension assets were already quite significant at the time.[3] 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.

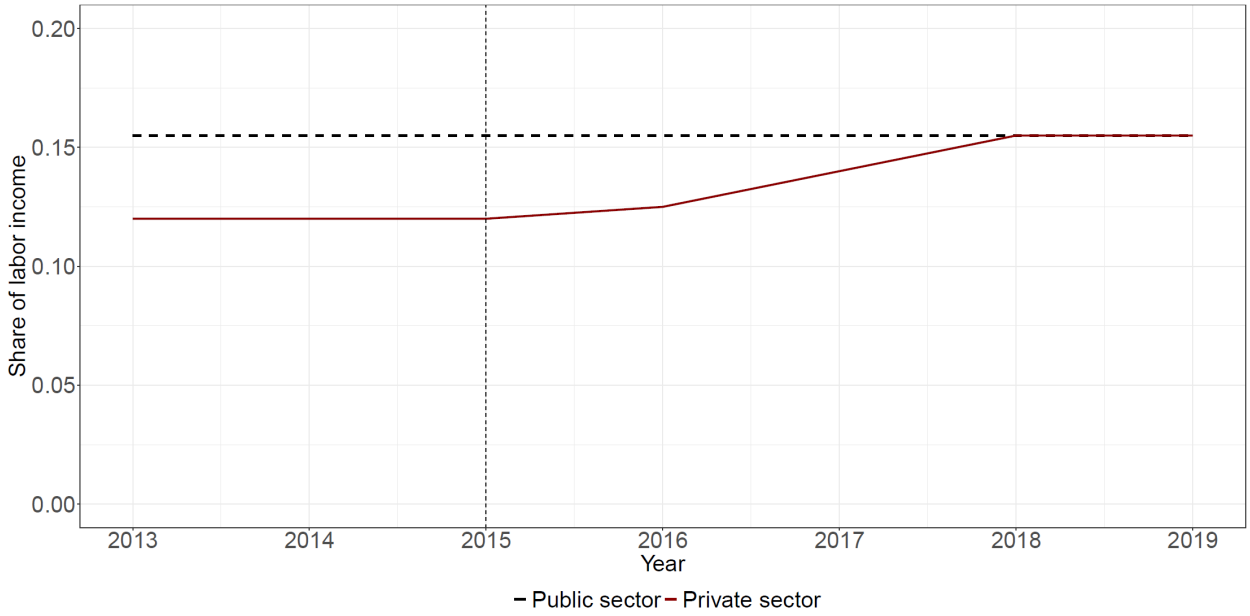
We focus solely on the 2016-2018 reform for two reasons. First, the time surrounding the 2006-2007 pension reform, when the private employers' contribution rate was raised by 2pp, is characterized by remarkable economic turbulence and turmoil in Iceland due to the financial

[3]The assets of pension funds alone, thus excluding other custodians of voluntary pension savings, amounted to approximately 140 percent of GDP in 2016. In 2023 it stood at 170 percent of GDP.

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 percent (from 8.0 percent to 11.5 percent) 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 percent to 15.5 percent, as is seen in Figure 1.

Figure 1: The mandatory pension contribution rate out of labor income in 2013-2019.



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.

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, we apply several sample restrictions to carry out our analysis. Since 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.[4] We omit

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

observations with incomplete tax returns and missing values and impose that imputed consumption must be nonnegative. 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 percent of the median in a given year.[5] Furthermore, to reduce noise from extreme observations we only consider households with a voluntary saving rate out of income, computed and discussed in Section 4.2, between -1 and 1. We also omit extreme observations in the mandatory saving rate. Appendix Table A3-1 shows the share of observations omitted for each restriction.

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 a household-level measure 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 jointly taxed couples' total assets and liabilities.[6] 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,

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

[6] 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.

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 $\tau_{i,t}$. Furthermore, $A_{i,k,t}$ denotes household i's amount of asset 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 in the tax records.

Rearranging Equation (1) gives,

$$s_{i,t} = \underbrace{\sum_k \Delta W_{i,k,t}}_{\text{Change in net wealth}} - \underbrace{\sum_k \Delta p_{k,t} A_{i,k,t-1}}_{\text{Unrealized capital gains}} \tag{2}$$

Where $s_{i,t}$ is defined as the difference between disposable income and consumption. Finally, our measure of household saving subtracts the depreciation of vehicle assets to imputed consumption. 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.[7,8,9]

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 (2),

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

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 – second-pillar pension savings.

$$ts_{i,t} = vs_{i,t} + ms_{i,t} \tag{4}$$

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

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

[9]In Appendix Section 8, we check the robustness of our results to the inclusion of durable consumption proxied by the depreciation of vehicle assets. The results, shown in Table A4-1, are very close to those reported in the main text.

Finally, we define three ratios, the voluntary saving ratio ($vs^r_{i,t}$), the mandatory saving ratio ($ms^r_{i,t}$), and the total saving ratio ($ts^r_{i,t}$), which are the ratio of voluntary saving, mandatory saving, and total saving, respectively, to wage income.[10]

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 percent (the mid-point between the 12.0 percent private sector rate and the 15.5 percent public sector rate) in 2015. Others are assigned to the control group. Since our saving measure is defined at the household level, we define the treatment and control groups at the household level. In practice this means that we define the control group as those households where the primary earner was in the public sector in 2015, and the treatment groups as those households where the primary earner was in the private sector in 2015. Since there will be some treated individuals in our control group, it may lead to biased estimates of crowding-out if treatment effects are heterogeneous (see for example De Chaisemartin and d'Haultfoeuille (2020)). We address this issue by studying only single households. We could have defined our control group as households in which all earners were in the public sector in 2015. However, the parallel trends are slightly less convincing for this sample and, therefore we emphasize single households in the main text (see Appendix Figures A5-1 and A5-2). Nevertheless, estimating the crowding-out effect using this definition of control and treatment groups, gives estimates of the same magnitude as in our main specifications (see Appendix Table A4-12). Therefore, we believe that heterogeneous treatment effects are not a concern for our findings.

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 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 percent in 2018. This, together with other sample restrictions outlined in Section 4.1, leaves us

[10]The validity of the results are discussed in Appendix section A7

with 520,682 observations: 156,843 (30.1%) in the control group and 363,839 (69.9%) in the treatment group. The sample is not a balanced panel since individuals move to and from the country within the sample period and due to the sample restrictions discussed above.

Table 3 shows summary statistics for both groups in 2015, prior to the reform. There are 28,084 observations in the control group and 64,855 observations in 2015. Most importantly, the mandatory saving rate of the treatment group prior to the reform is significantly lower than that of the control group.[11] 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 Graphical Evidence

We first present preliminary findings from a simple comparison of average saving of the two groups. Panel (a) of Figure 2 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. Figure 2 points to systematically different saving preferences between the private sector treatment group and the public sector control group, with the former seemingly saving a somewhat smaller share of their income than the latter. However, our empirical strategy relies on the assumption that in the absence of the reform, there would have been co-movement in the voluntary saving rate of the two groups. As such, as long as the difference in saving preferences between the group is constant over time, our identifying assumption holds. The simple comparison of Figure 2 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. This is tested formally in the Section 5.2.

Panel (b) of Figure 2 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

[11] Appendix Figures A2-1 and A2-2 show the distributions of mandatory saving rates in 2015 and 2019 for the whole sample and single households, respectively.

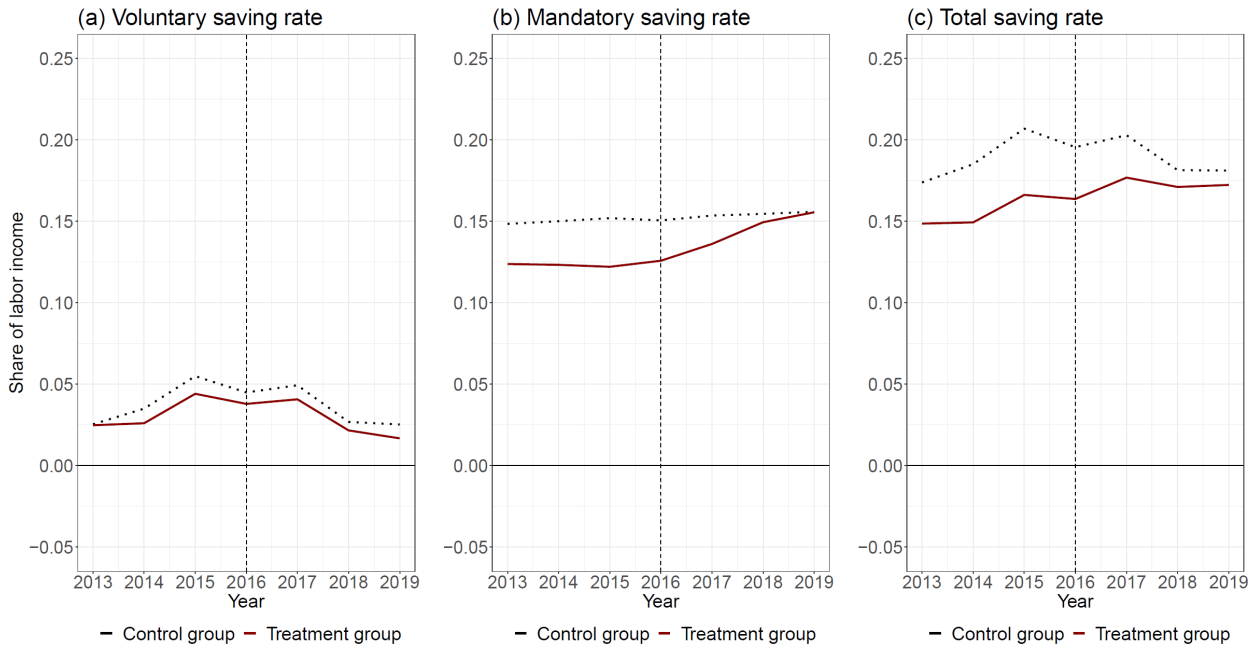
Table 3: Summary statistics for the treatment and control groups in 2015.

	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	5,410	4,651
	(25,928)	(28,515)
Mandatory saving	13,162	11,345
	(7,417)	(6,366)
Total saving	18,572	15,997
	(28,088)	(30,034)
Wages	87,217	92,495
	(49,537)	(51,388)
Debt	146,261	147,618
	(131,680)	(206,321)
Net wealth	143,750	151,440
	(264,576)	(335,714)
Voluntary pension saving rate	0.024	0.023
	(0.031)	(0.031)
Age	45.055	43.548
	(11.143)	(11.287)
Gender	0.415	0.537
	(0.493)	(0.499)
Spouse	0.623	0.679
	(0.485)	(0.467)
University	0.494	0.308
	(0.500)	(0.462)
Children	0.875	0.880
	(1.080)	(1.089)
Urban	0.659	0.644
	(0.474)	(0.479)
Number of observations (N)	28,084	64,855

Notes: Table 3 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.

the voluntary saving rate. This is confirmed in panel (c), which shows the narrowing gap in total saving rates between the two groups as the mandatory saving rate of the treatment group rises.

Figure 2: Average voluntary, mandatory, and total saving rates in 2013-2019



Notes: Figure 2 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.

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

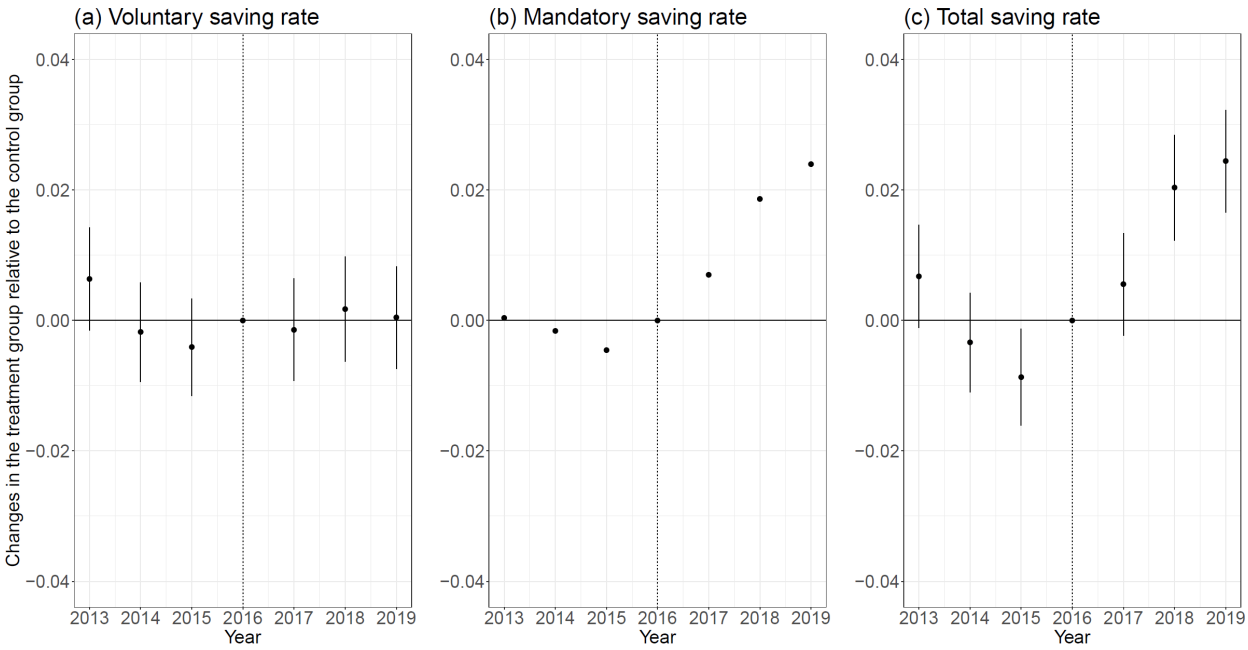
$$vs^r_{i,t} = \alpha_i + \alpha_t + \gamma_t \alpha_g \times \alpha_t + \beta age_{i,t} + \varepsilon_{i,t} \tag{5}$$

where α_i, α_g, and α_t are individual fixed effects, group fixed effects, and year fixed effects, respectively. $age_{i,t}$ denotes controls for age 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 in the pre-reform period $t < 2016$.

Figure 3 reports changes in the treatment group's saving rates relative to that of the control

group both before and after the reform is implemented.

Figure 3: Changes in the treatment group's saving rates relative to the control group



Notes: Panel (a) of Figure 3 plots the estimated γ_t from equation (5). 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, marital status, gender, urban, region of residence, number of children, labor income ventiles, net wealth ventiles, and education. All controls, except age, are fixed at their 2015 values to avoid endogeneity issues. 95 percent confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines. Note that those are small and barely visible in panel (b).

Specifically, panel (a) shows the γ_t estimates from Equation (5). It illustrates that the coefficients for the period prior to the implementation of the reform are small and statistically 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).[12]

[12]In Figure 3, there is a small downward trend in the mandatory saving prior to the reform. There is no institutional reason for why this should be the case. However, it could relate to the fact that the treatment and control groups are defined based on the mandatory saving rates in 2015. In Appendix Section 8, we assign into treatment and control groups using households' average mandatory saving rates in 2014-2015. This alleviates the downward pre-trent in

5.3 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,

$$vs^r_{i,t} = \mu_i + \mu_t + \rho ms^r_{i,t} + \beta age_{i,t} + \varepsilon_{i,t} \tag{6}$$

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; $ms^r_{i,t}$ 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); $age_{i,t}$ is age fixed effects which is controlled for to account for potential life-cycle effects; and $\varepsilon_{i,t}$ 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,

$$vs^r_{it} = \mu_i + \alpha_1 post_t + \rho ms^r_{it} + \beta age_{it} + \varepsilon_{it} \tag{7}$$

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 percent 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 individual fixed-effects, a post-reform dummy, the interaction of these two, and a vector of

2013-2015. As can be seen in Figures A4-1 and A4-2 and Table A4-5, our key results are robust to this alternative definition of the two groups.

characteristics,

$$ms^r_{i,t} = \mu^1_i + \mu^1 post_t + \pi post_t \times treated_i + \beta^1 age_{i,t} + \varepsilon^1_{i,t} \tag{8}$$

and the second stage is voluntary saving rates regressed on individual fixed-effects, a post-reform dummy, the predicted mandatory saving rates, and age fixed effects,[13]

$$vs_{i,t} = \mu^2_i + \mu^2 post_t + \rho \widehat{ms}^r_{i,t} + \beta^2 age_{i,t} + \varepsilon^2_{i,t} \tag{9}$$

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.

5.4 Panel Regression Evidence

Table 4 shows the results from the first stage of the 2SLS outlined in Equations (8) and (9). It confirms 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 1.4pp relative to that of the control group for the whole sample and 1.6pp for single households. The effect is very precisely estimated. Moreover, a weak instrument is clearly ruled out by a high F-statistic. Finally, the instrument explains a large share of the variation in the mandatory saving rate out of labor income as noted by an R^2 of roughly 0.7.

Table 4: First stage: The effect of the reform on mandatory saving rates

	Whole sample	Single households
$\hat{\pi}_1$	0.014^{***}	0.016^{***}
	(<0.001)	(<0.001)
F-statistic (instrument)	54,040	27,602
R^2	0.701	0.725
n	92,939	36,409
N	520,681	161,518

Notes: Table 4 reports regression results from the first stage estimation using Equation (8). It reports the coefficient on the instrument, that is an interaction of a post-reform dummy variable and a treatment group dummy variable, in a regression of mandatory saving rates on individual fixed effects, a post-reform dummy variable, the aforementioned interaction, and controlling for age fixed effects. The first column reports estimates for the whole sample and the second column reports results for single households only. 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 5 shows the 2SLS estimates obtained from Equations (8) and (9).

[13] Superscripts are used to distinguish between coefficients in the first stage and the second stage.

The 2SLS analysis yields a negative crowding-out effect of 0.045, suggesting that raising the mandatory saving rate by a percentage point is met by a 0.045pp 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 5: Crowding-out: The effect of mandatory saving rates on voluntary saving rates

	Whole sample		*Single households*	
	2SLS	Reduced-form	2SLS	Reduced-form
	(1)	(2)	(3)	(4)
Crowding-out	0.045	-0.001	-0.172	-0.002
	(0.163)	(0.002)	(0.176)	(0.003)
R^2	0.062	0.062	0.104	0.103
n	92,939	92,939	36,409	36,409
N	520,681	520,681	161,518	161,518

Notes: Table 5 shows estimation of the crowding-out effects ($\hat{\rho}$) using Equations (8) and (9). Columns (1) and (2) report the findings from a 2SLS estimation and reduced-form results for the whole sample, respectively. Columns (3) and (4) report findings from a 2SLS estimation and reduced-form results for single households only. The estimates are shown with controls for age fixed effects. 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.[14]

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 1.6pp relative to the control group in the post-reform period, compared to 1.4pp 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. The crowding-out estimates obtained for the sample of single households is somewhat larger than for

[14] We use strictly single households, rather than single households and couples who work in the same sector, as the pre-trends are more convincing for the former group.

the whole sample. For a one percentage point increase in the mandatory saving rate, single households reduce voluntary saving by 0.171 percent. The results are statistically insignificant at all conventional significance level and still suggest very limited crowding-out of mandatory saving.

5.5 Heterogeneity in Crowding-Out

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. 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. We construct a measure of liquidity by adding bank deposits to assets in mutual funds within each household. We then split the resulting liquid-assets distribution into terciles. 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. 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.

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

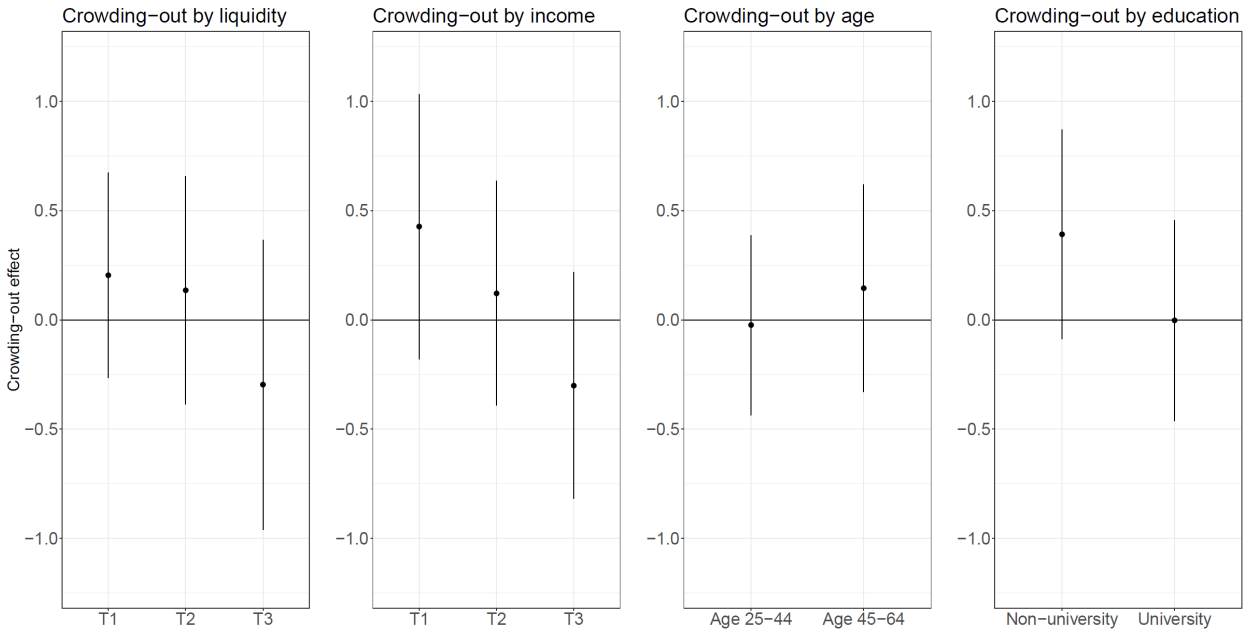
$$\begin{aligned} vs^r_{i,t} = {} & \alpha^k_1 post_t + \alpha^k_2 treated_g + \sum_j \alpha^{jk} G^{jk}_i + \alpha^k_3 treated \times G^{jk}_i + \alpha^k_4 post \times G^{jk}_i \\ & + \rho^k \widehat{ms}^r_{it} + \rho^{jk}(G^{jk}_i \times ms^r_{it}) + \beta age_{it} + \varepsilon_{it} \end{aligned} \tag{10}$$

where G^{jk}_i 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, $ms^r_{i,t}$, and the interaction of mandatory saving with the relevant subcategory dummies, $G^{jk}_i \times ms_{it}$ – 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 4 plots the crowding-out effect for the four subcategories. In line with our predictions, the point estimate of the crowding-out effect is increasing; i.e., the coefficient is lower, in liquid assets. While the estimated crowding-out effect is only marginally lower for the second tercile than for the first (+0.13 vs. +0.20), the crowding-out effect is higher for the top tercile of the liquid-assets distribution (-0.30).

Figure 4: Crowding-out by liquidity, income, age, and education



Notes: Figure 4 plots the estimated crowding-out effect for various subcategories (ρ^k and $\rho^k + \rho^{jk}$) from Equation (10). The subcategories are terciles in the liquid assets distribution (left), terciles in the labor income distribution (mid-left), younger vs. older households (mid-right), and university-educated vs. non-university-educated households (right). Liquid assets are defined as bank deposits and assets in mutual funds. 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 estimates are shown with controls for age fixed effects. The black dots represent point estimates, and 95 percent 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 is also increasing in income. There are no signs of crowding-out for the first and second terciles of the income distribution, with an estimated crowding-out effect is +0.42 and +0.12, respectively. However, there are indications of crowding-out effects for the top tercile, with an estimated effect of -0.30. Furthermore, we find a higher point estimate for the crowding-out effect for younger households compared to older households, although the difference is small. 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. The findings suggest that while liquidity constraints and low income 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.

5.6 Third-Pillar Pension Saving and Debt Accumulation

5.6.1 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. Appendix Figure A6-1 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. 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 shows the parallel trends analysis from Equation (5) using voluntary third-pillar pension saving rate as the dependent variable. The coefficients are very precisely estimated and virtually the same both in the pre-reform and post-reform periods.

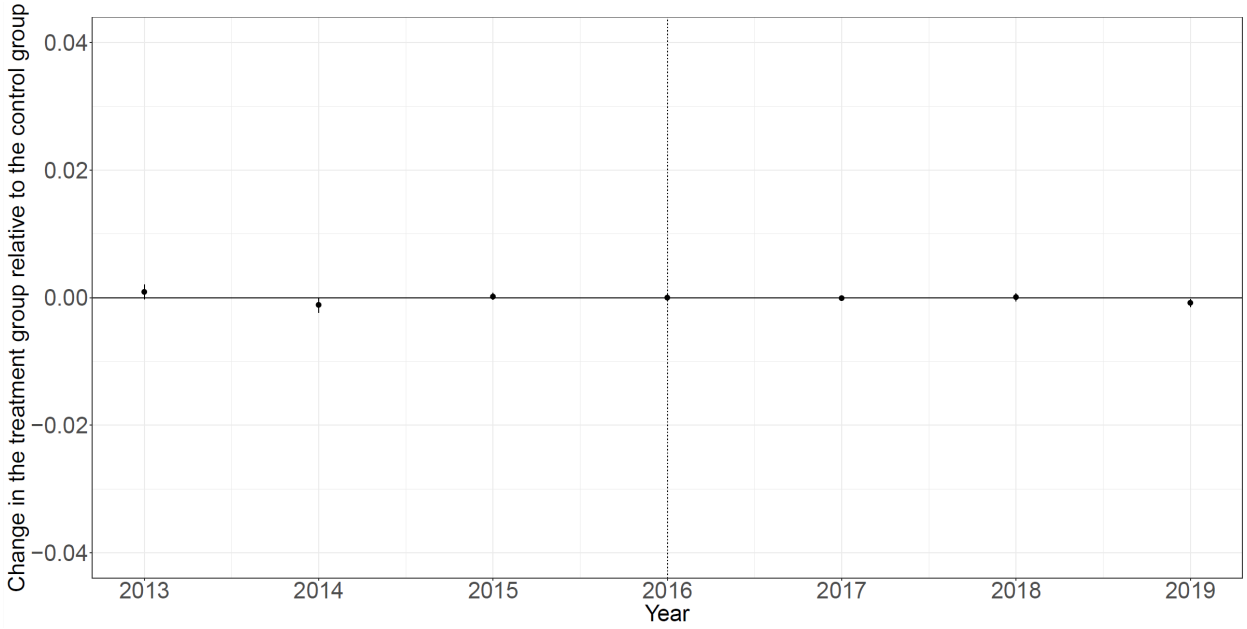
Individuals may be reluctant to reduce their voluntary (third-pillar) pension saving due to the one-to-one matching by employers up to two percent. To assess whether this influences the lack of response to the reform, we estimate the crowding-out coefficient using voluntary saving outside the pension system as the dependent variable and for a sample including only individuals who have never saved in the voluntary third-pillar and, thus have no such savings.[15]

Columns (1) and (2) of Table 6 present the estimates derived from Equations (8) and (9), with the rate of voluntary private pension savings out of labor income serving as the dependent variable in the second stage. Columns (3) and (4) provide analogous estimates using voluntary savings outside the pension system as the dependent variable. Finally, Columns (5) and (6) display the results for total voluntary savings, based on the subsample of individuals without any voluntary pension savings.

Consistent with prior findings, our results indicate a limited crowding-out effect across all cases. Specifically, for the entire sample, we observe a crowding-out effect of voluntary pension savings of 0.009 percentage points (pp), while for single households, the effect is 0.034 pp. Regarding voluntary saving outside the pension system, the crowding-out estimates are +0.054 pp for the whole sample and -0.138 pp for single households. For individuals without voluntary pension savings, the estimates are +0.121 pp and +0.362 pp, respectively. Our analysis consistently demonstrates that the crowding-out effect is minimal and not statistically significant at conventional levels. Therefore, the reform led to increased total retirement savings, although

[15]The parallel trends analyses for the two exercises can be found in Appendix Figures A6-2 and A6-3

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



Notes: Figure 5 plots the estimated γ_t from Equation (5), 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 estimates are shown with controls for age fixed effects. 95 percent confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines. Note that those are small and barely visible.

that was not one of its stated objectives.

Table 6: Crowding-out results for voluntary (third pillar) pension saving, 2SLS

	Voluntary pension saving		Non-pension voluntary saving		Voluntary saving: subsample	
	Whole sample	*Single household*	*Whole sample*	*Single household*	*Whole sample*	*Single household*
	(1)	(2)	(3)	(4)	(5)	(6)
Crowding-out	-0.009	-0.034	0.054	-0.138	0.121	0.362
	(0.024)	(0.026)	(0.162)	(0.174)	(0.525)	(0.453)
R^2	0.272	0.288	0.054	0.099	0.095	0.145
n	92,939	36,409	92,939	36,409	8,218	7,240
N	520,681	161,518	520,681	161,511	36,254	25,748

Notes: Table 6 shows the estimated crowding-out effect ($\hat{\rho}$). The estimates are shown for the whole sample and single households. In Columns (1) and (2) the rate of voluntary 3rd pillar pension saving out of labor income have replaced the voluntary saving rate as the dependent variable. In Columns (3) and (4) the rate of non-pension voluntary saving out of labor income have replaced the voluntary saving rate as the dependent variable. In Columns (5) and (6) the estimates for total voluntary saving are shown for the subsample of the population that has no voluntary (third pillar) pension savings. The estimates are shown with controls for age fixed effects. Standard errors, clustered at the household level, are in parentheses.

5.6.2 Debt Accumulation

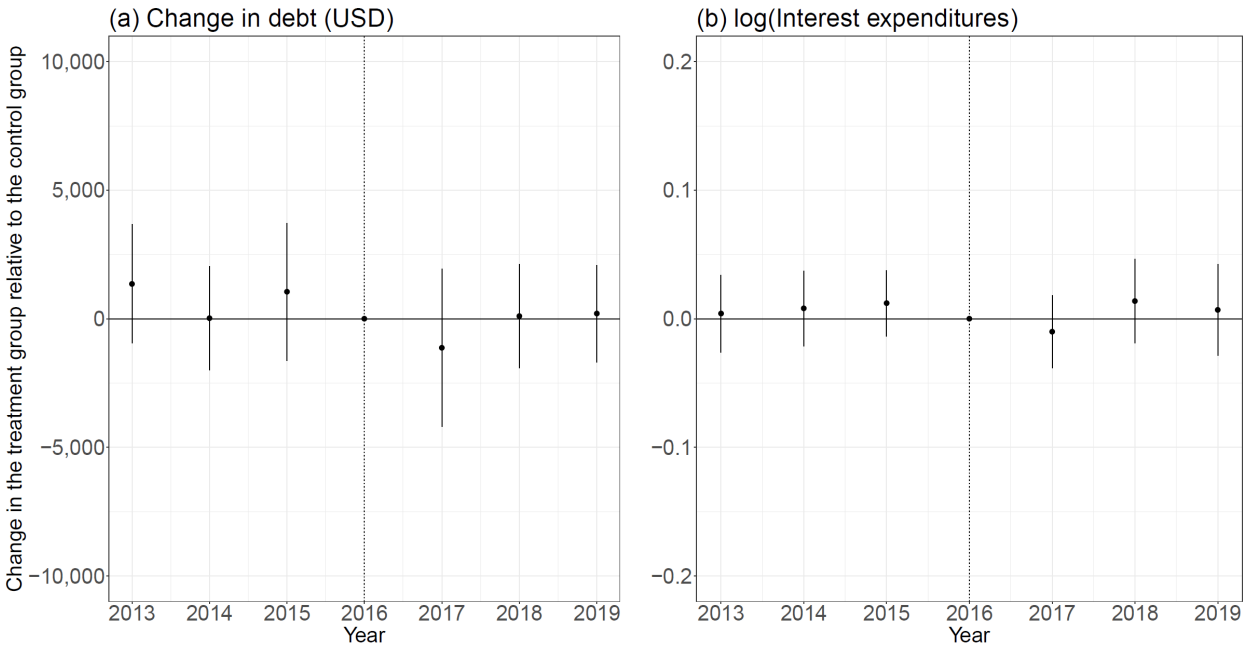
Next, we check whether the reform caused debt accumulation in the treatment group. Since households are unable to draw on their 2nd pillar pension savings, for example to smooth consumption once faced with shocks, they might offset their higher mandatory retirement savings by incurring more debt. This 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 Global Financial Crisis (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 lowering retirement savings by one Danish currency unit (krona) 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 (5) 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.[16] 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 (5) with the log of interest expenditures as the dependent variable. Figure 6 shows that 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. Furthermore, we estimate Equations (8) and (9) with the amortization rate out of wages as the dependent variable in the second stage to see whether households responded to the increase in mandatory saving rates by changing their amortization rates. Appendix Table A4-13 reports a small and statistically insignificant crowding-out effect. As such, we find no evidence for households responding to the pension reform by adjusting their amortization rates.

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.

[16] In Appendix Section 8, we winsorize both debt and interest payments at the 5th and 95th percentile. The objective is to test the robustness of our results to outliers in the distribution of debt and interest payments. The results, shown in Figure A6-4 are very close to those of Figure 6 of the main text.

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 (5), 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 estimates are shown with controls for age fixed effects. 95 percent confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines.

6 Away from the Reform - Evidence from Job Switchers

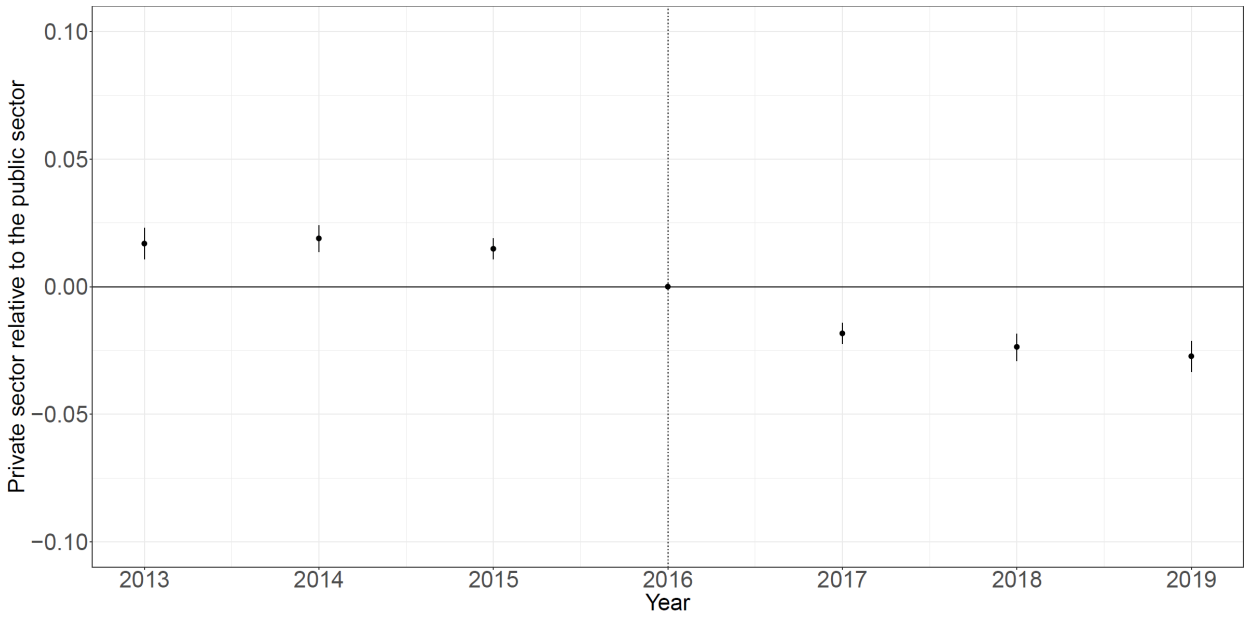
In the ideal experiment, the increase in the mandatory saving rate would be randomly assigned to a subgroup of the population and not tied to the wage negotiations of a particular sector of the labor market. One concern that could potentially contaminate our experiment is that the increase in the employer's 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.

Figure 7 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.[17] It plots the coefficient on the interaction between group fixed effects and year fixed effects in a regression of the logarithm of total consumption on individual fixed effects, year fixed effects, the aforementioned interaction

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

and controls for age fixed effects. Total compensation is defined as households' pre-tax wages plus the employer's mandatory contribution to pillar 2 pension funds. The figure illustrates that while total compensations were on a similar path prior to the reform, those of the public sector grew relative to the private sector in the post-reform period. In particular, there is no indication that total compensation in the private sector rose relative to the public sector in the post-reform period, thus alleviating concerns of underestimation of the crowding-out effect. In contrast, using the same argument as above, the relative wage decrease of private sector households could push down their saving rate, and thereby leading to an overestimation of the crowding-out effect.

Figure 7: Change in the total compensation of private sector households relative to public sector households



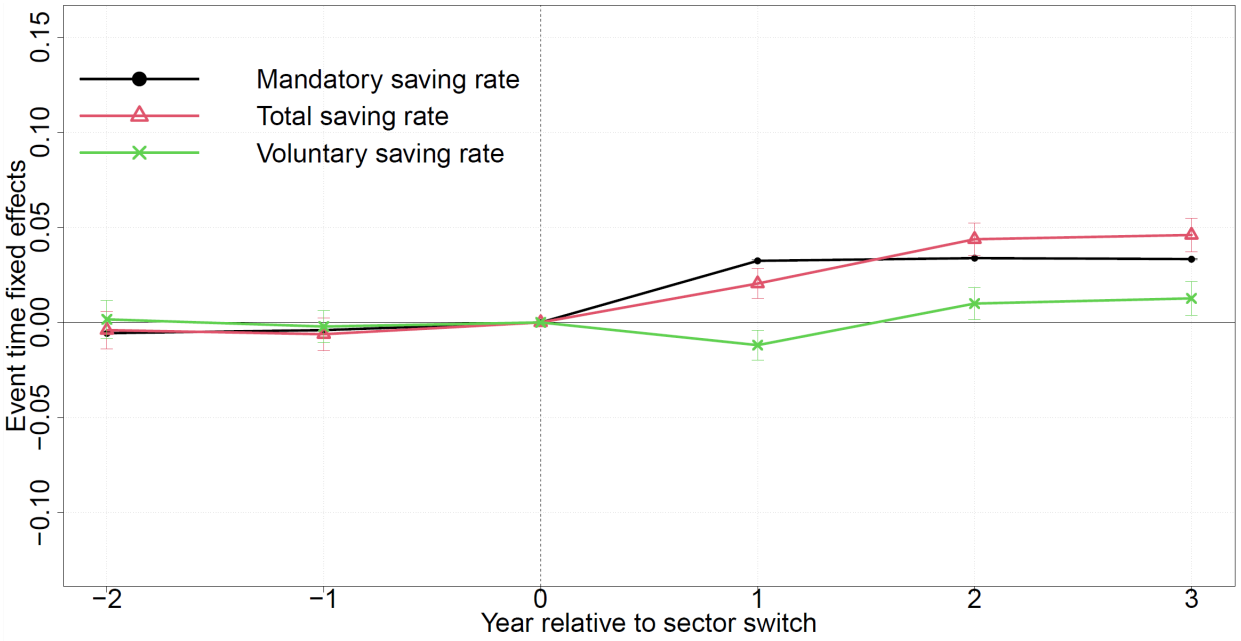
Notes: Figure 7 plots the estimated γ_t from the equation $tc_{it} = \alpha_g + \alpha_t + \gamma_t \alpha_g \times \alpha_t + \varepsilon_{it}$. The dotted vertical line in 2016 shows when the first stage of the reform was implemented. tc_{it} is the log of total compensation, defined as the sum of 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. 95 percent confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines.

Due to the concerns raised above about the reform, we complement our analysis by studying the saving behavior of workers who switched from the low-contribution private sector to the high-contribution public sector prior to the reform, similar to Chetty et al. (2014). We show that our findings of a minuscule crowding-out of saving in response to the pension reform are consistent with a minuscule crowding-out of saving when studying switchers.

Figure 8 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 9 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.

Figure 8: Saving rates of job switchers in the whole sample.

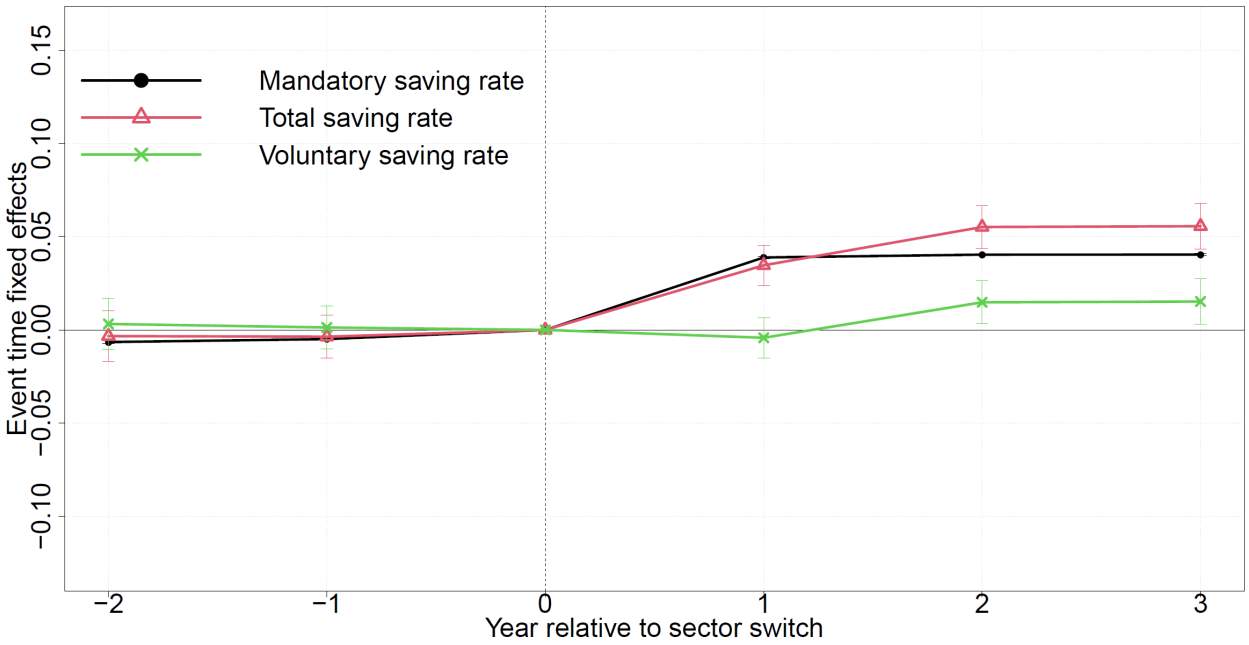


Notes: Figure 8 plots the estimated $\alpha_{et,i}$ from the equation $Y_{i,t} = \alpha + \alpha_{et,i} + \varepsilon_{i,t}$ for the whole sample, where $Y_{i,t}$ is either the total saving rate (red), the mandatory saving rate (black), or the voluntary saving rate (green), α is the intercept and $\alpha_{et,i}$ 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 percent confidence intervals, based on standard errors clustered at the household level, are represented by dotted vertical lines.

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 percent in year $t-1$ and above 13.75 percent 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 green line plots the voluntary saving rate, black line plots the mandatory saving rate, and the red line plots the sum of the two, which is 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 the crowding-out effect

Figure 9: Saving rates of single job switchers.



Notes: Figure 9 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 percent confidence intervals, based on standard errors clustered at the household level, are represented by dotted vertical lines.

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 A7-1 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 A7-2 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 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. Appendix Figure A7-3 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, Appendix Figures A7-4 and A7-5 report the development of wages 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 3. 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

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. To gauge the public's exposure to information on the pension reform we searched news archives for articles on the pension reform. In addition to this we conducted a survey to better understand the reasons for our results.[18] In particular, we tested whether lack of awareness about the reform might explain why an individual's responses were muted.

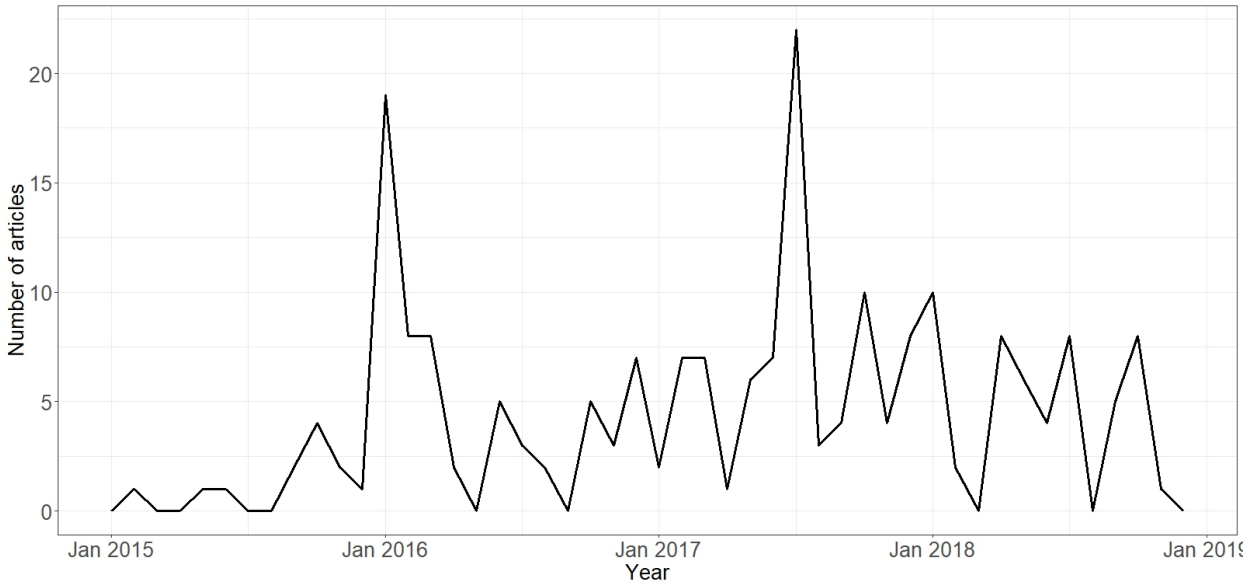
7.1 Media Coverage

Although not necessarily a prerequisite for responding to the reform, awareness of it certainly raises the probability of doing so. To this end, we conducted a comprehensive search across archives of all major news outlets in Iceland spanning the period surrounding the reform and its implementation. This archive includes printed newspapers, transcripts of radio and television broadcasts, as well as online articles. Several articles on the pension reform were identified, some prominently featured on the front pages of widely circulated newspapers. Typically, these articles outlined the timeline of the reform's roll-out, providing delineation of both the adjustments and the levels of employer contributions at each stage. Some articles covered the reform using accessible

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

language, an example of this is *8.0% → 11.5%: Pension-rights equalized*. Figure 10 illustrates the frequency of mentions of *employer contribution* in the Icelandic media. There are two noticeable spikes in the coverage: when signing of the collective bargaining agreement in January 2016 and around the second increase in employer's contributions in July 2017.[19]

Figure 10: Number of news reports featuring the phrase *employer contribution*



Notes: Figure 10 captures all mentions of the phrase *employer contribution* in published media, including newspapers, broadcast news on radio and television along with online articles. The spike in coverage in early 2016 coincides with the signing of the collective bargaining agreement. The spike in July 2017 coincides with the second increase in employer's pension contribution, see footnote 19.

Beyond media coverage, the labor union responsible for negotiating the pension reform initiated a public awareness campaign. This campaign entailed disseminating printed informational materials to the residences of all union members, totaling 76,000 copies.[20] Moreover, the labor union disseminated news announcements and purchased advertisements regarding the reform across various online and traditional media platforms. Information was made available in the Icelandic, English, and Polish languages.

7.2 Survey Evidence

In light of the media coverage, it is interesting to see if people were actually aware of the change. Our survey covered 946 individuals. Of these, in 2015, 35.4 percent worked in the private sector, 23.7 percent in the public sector, 13.3 percent were not employed, and 13.8 percent were self-employed, worked for non-governmental organizations (NGOs), or did not specify their sector

[19]The spike in July 2017 was partially due to public debate on the interpretation of the reform. The Financial Supervisory Authority concluded that individuals could opt for the employer's contribution being part of their third pillar pension savings.

[20]In early 2016 the working age population of Iceland was around 235,000 individuals

Table 7: 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

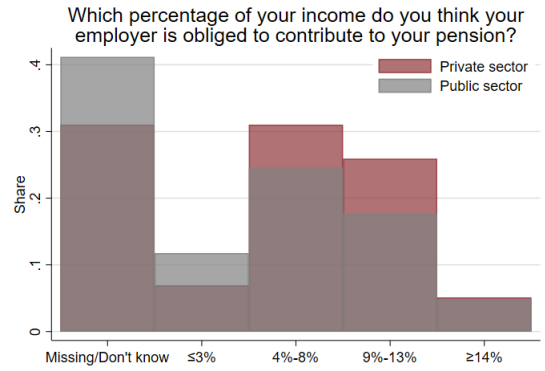
Notes: The first column shows descriptive statistics for the full surveyed sample. The second to fourth columns show descriptive statistics for the sample used; individuals aged 25-65 who worked in the treatment or control groups in 2015. The second column shows both the treatment and control groups,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

(see Table 7). In this section, we focus on those aged 25-65 who worked in either the private or the public sector in 2015. A majority – 71.2 percent of private sector workers and 71.1 percent 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. A large majority of public sector workers are female. To gauge awareness of the reform, one survey 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 11 below. Only around 26 percent of respondents answered correctly that the employer contribution was between 9 percent and 13 percent 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 percent of the treatment group responded - correctly - that the employer contribution had increased, while 25 percent of the control group responded - incorrectly in their case - that the contribution had increased. Only 34 percent 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.[21]

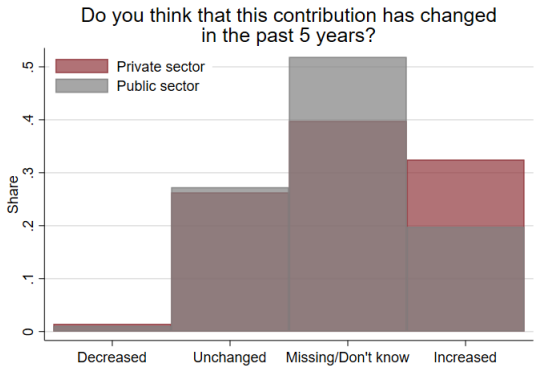
We next inquire about changes in saving behavior between 2015 and 2021. In total, 40 percent say that they have increased their voluntary saving, or "other saving", since 2015; 33 percent say it is unchanged; 20 percent say they had no voluntary saving in 2015 nor 2021; and 7 percent

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

Figure 11: 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). Observations 946.

say they reduced voluntary saving. 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,

$$\Delta vs_i = \begin{cases} -1 & \text{if saving has reduced} \\ 0 & \text{if saving is unchanged} \\ 1 & \text{if saving has increased} \end{cases} \tag{11}$$

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 the hypothesis that awareness of the reform leads to a decision to lower voluntary saving, we run the following regression,

$$\Delta vs_i = \alpha_0^j + \alpha_1^j Private_i + \alpha_2^j G_i^j + \alpha_3^j (Private_i \times G_i^j) + \boldsymbol{X}_i \boldsymbol{\beta}_j + \varepsilon_{ji} \tag{12}$$

where j indicates the hypothesis being tested, and G_i^j is an indicator for hypothesis j's group of interest who are aware of the reform (individuals belonging to the treatment group who responded that their employer's contribution had increased and individuals belonging to the control group

who responded that their employer's contribution had not increased). It takes the value 1 for those who are aware of the reform but otherwise it is zero.

The regression results from Equation (12), using the awareness can be seen in the four columns of Table 8 respectively. Table A8-1 reports results for several other subgroups. Only those who report that their primary motive for saving is retirement seem to somewhat lower their voluntary saving after the reform was implemented.

Table 8: Estimates of the effects of the reform on voluntary saving by subgroups

Constant	0.33***	0.37***	0.14	0.12
	(0.03)	(0.04)	(0.13)	(0.13)
Private		-0.08	-0.03	-0.05
		(0.06)	(0.06)	(0.06)
Aware				0.23
				(0.15)
Private × Aware				-0.04
				(0.17)
R^2	0.00	0.00	0.08	0.09
N	461	461	461	461

Notes: The results from regression (12) 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. $***p < 0.01$,$** p < 0.05$,$*p < 0.1$.

The estimate of the parameter of the awareness variable is statistically insignificant. The interaction between the variable and a dummy variable for the private sector also yields an insignificant coefficient.

We have found that the reform featured in the media, which should have alerted people working in the private sector to the changes in their future pension income. However, our survey does not find that workers were aware of the changes and those who seem to have been aware did not adjust their voluntary saving any differently than others.

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. We document the media coverage prior to the reform, which was not insignificant. The lack of awareness and the media coverage suggests that people do not pay much attention to their future retirement pension.

Our findings may have important implications for the design of pension policies. For example, to restore fiscal sustainability, several countries have either already implemented or announced cuts in public ("pillar 1") pension benefits. All else equal, this lowers the replacement rate of public pensions, so the question is how to dampen the fall in the total replacement rate. Recent policy changes in Portugal offer an interesting case in point. According to pension expenditure projections provided by the European Commission (2024), announced cuts in public pension benefits in Portugal will lead to a fall in the replacement rate of public pensions from 90.1% in 2040 to 38.5% in 2050. This is quite dramatic and would, if implemented, require increases in private savings to provide compensating income during retirement.

Indeed, without a "pillar 2" occupational pension scheme, workers would need to respond by raising their private "pillar 3" saving. Thereby, they would accumulate financial assets, the returns of which could (fully or partly) make up for lower public pensions. However, this is unlikely to happen in practice, not least in countries where the experience with saving for retirement is only weakly developed. In such a situation a better way out might be to increase mandatory pension contributions through pillar 2, ideally through an existing occupational pension scheme. Our findings strongly indicate that this would only affect voluntary saving to a small extent, thus leading to a significant increase in total saving.

More generally, 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 saving from Equation (1), 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.

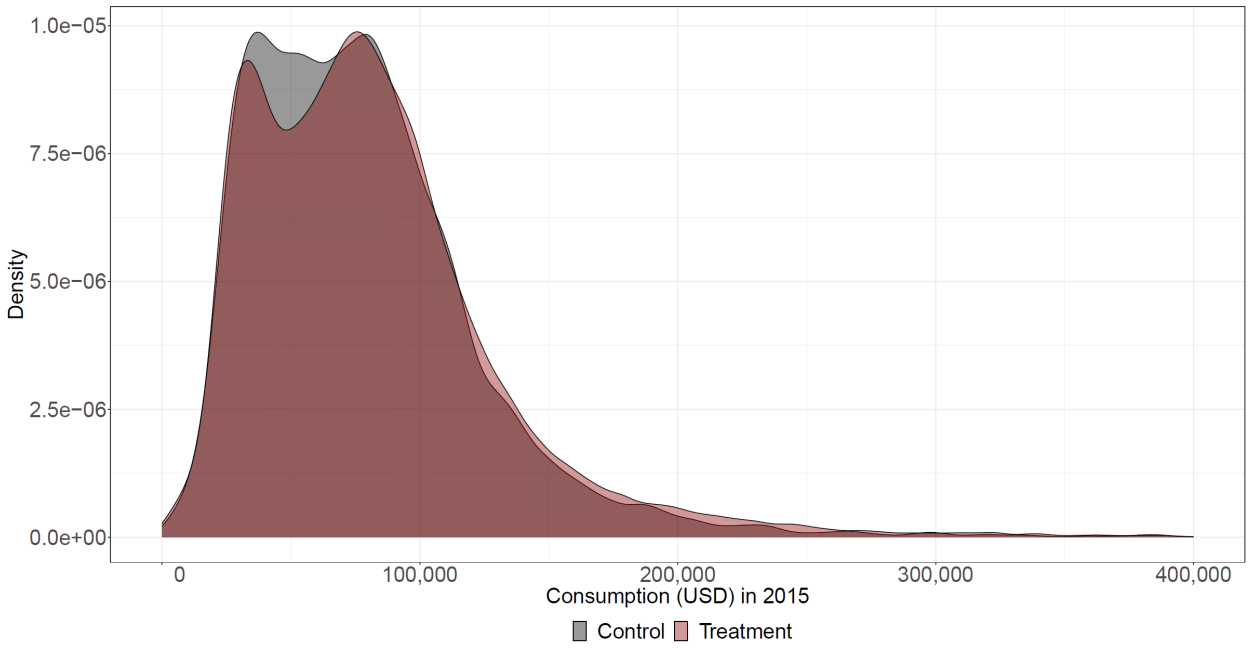
We account for changes in the price of real estate – arguably the largest source of unrealized capital gains/losses for most households – using information on the value of each property owned by the house-hold as estimated by Registers Iceland, an institution responsible for maintaining the property 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 percent 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.

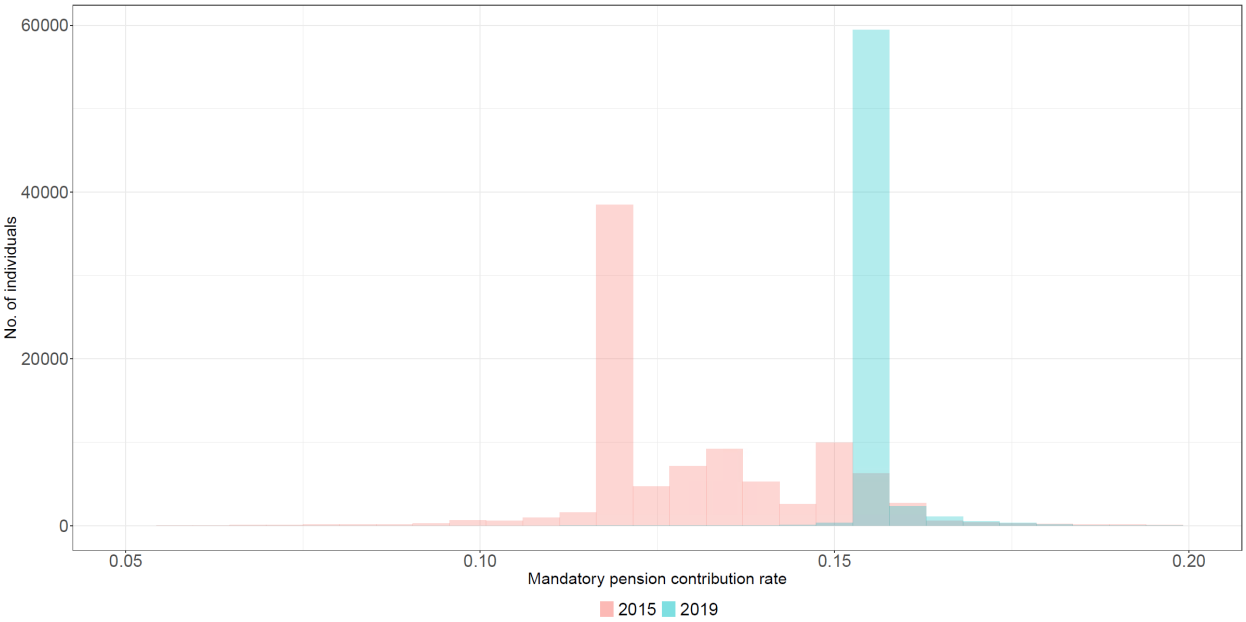
A2. Distribution of Mandatory Saving Rates

Figures A2-1 and A2-2 show the distributions of mandatory saving rates in 2015 and 2019 for the whole sample and single households, respectively. Starting with the latter, the distribution in 2015 is bimodal, with a mass of individuals around the 11.5 percent mandatory saving rate of the private sector at the time and 15.5 percent, the mandatory saving rate of the public sector. As can be seen, the mandatory saving rate in 2019 is exactly 15.5 percent for the vast majority of households. The former figure, showing the mandatory saving rates of the whole sample, is very similar except for the fact that the effective mandatory saving rates of households with one individual in the private sector and the other in the public sector is between 11.5 percent and 15.5 percent in 2015.

A3. Sample Restrictions

Appendix Table A3-1 shows the share of the original sample lost by applying the sample restrictions discussed in Section 4.1. The details of each restriction are discussed in the notes to the table. The table shows the share of the original sample lost rather than the number of observations omitted in each sample restriction. While our estimation period is 2013-2019, we use lags of various variables. As such, the data set used for the analysis spans 2012-2019. Since we omit 2012 after applying the sample restrictions, it is difficult to show exactly how the sample is reduced by the application of each restriction.

Figure A2-1: Mandatory contribution rate by year - Whole sample



Notes: Figure A2-1 shows a histogram of the mandatory contribution rate by year for the whole sample.

Table A3-1: Sample restrictions

Restriction	**Share of observations lost**
Original example	2,549,204 observations
Age 25-64	34%
Deficient observations	11%
Non-negative consumption	4%
Wage criteria	11%
Outliers in voluntary saving	4%
Outliers in mandatory saving	7%
Treatment group unaffected by the reform	4%

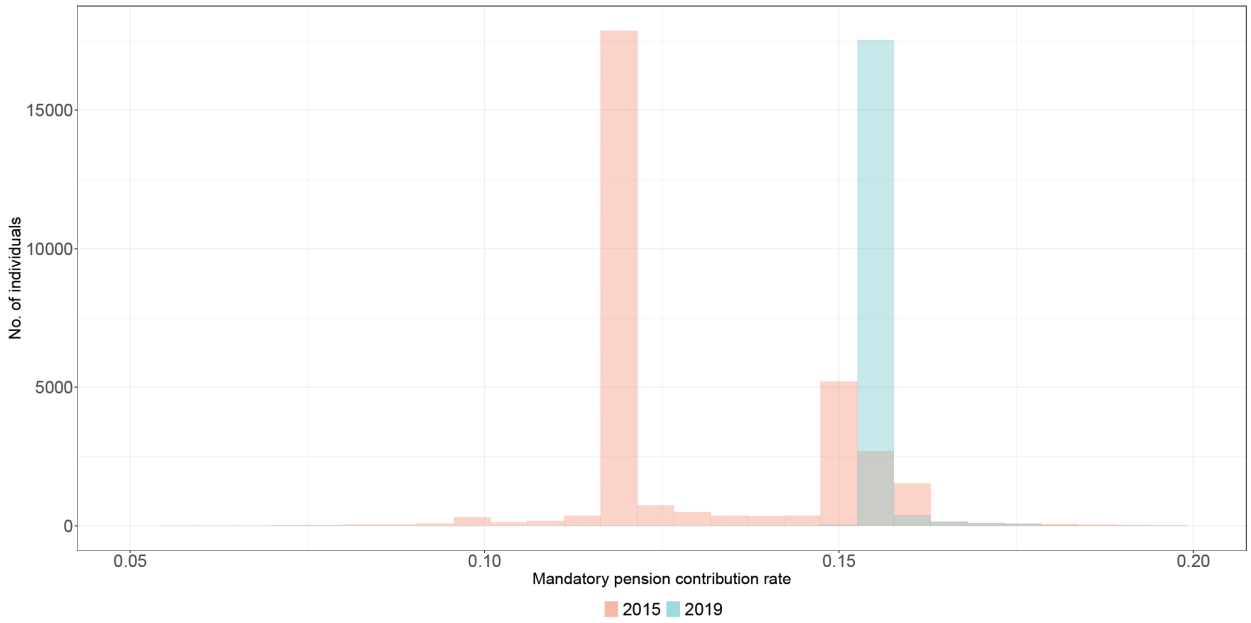
Notes: Table A3-1 shows the share of the original sample lost due to each restriction imposed on the sample. Age 25-64 refers to the fact that we focus the analysis on working age households and omit those under the age of 25 and over the age of 64. Deficient observations refer to those with incomplete tax returns or missing values. Non-negative consumption means that we exclude those with negative imputed consumption. Wage criteria refers to the restriction that those with wages below 50 percent of the median income each year. In 2019, this was equivalent to earning $2,100 a month, which is well below the negotiated minimum wage for a full-time job in Iceland. Outliers in voluntary saving refers to the restriction that the voluntary saving rate out of income must be between -1 and 1. Outliers in mandatory saving refers to the restriction that the mandatory saving rate is between 0 and 0.20. Prior to the reform, this ratio was exactly 0.12 or 0.155 for the vast majority of individuals. Finally, treatment group unaffected by the reform excludes those who despite the reform still have a low mandatory saving rate in 2018, after the reform was implemented.

A4. Robustness Checks

Saving measured without durable consumption

In our measure of saving, vehicle purchases do not subtract from saving in the year of purchase. Instead, it is interpreted as an asset transformation from liquid assets to vehicle assets. The 10%

Figure A2-2: Mandatory contribution rate by year - Single households



Notes: Figure A2-2 shows a histogram of the mandatory contribution rate by year for single households.

yearly depreciation of vehicle assets from Icelandic tax laws is interpreted as consumption and subtracts from saving each year. Table A4-1 reports the crowding-out results for a measure of voluntary saving that does not subtract vehicle depreciation from saving. Note that the number of observations is slightly higher due the sample restriction of not including observations with negative consumption. Our results in the main text are robust to this alternative definition of household saving.

Table A4-1: Crowding-out results

	Whole sample	Single households
	(1)	(2)
Crowding-out	0.051	-0.162
	(0.160)	(0.171)
R^2	0.068	0.110
n	93,096	36,475
N	522,009	162,039

Notes: Table A4-1 shows the crowding-out effects ($\hat{\rho}$) estimated using Equations (8) and (9). In contrast to the main results in Table 5, the measure of household saving used here does not subtract imputed rent for homeowners and vehicle depreciation. Column (1) reports the findings for the whole sample and columns (2) reports results for single households only. The estimates are shown with controls for age fixed effects. Standard errors, clustered at the household level, are in parentheses.

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 A4-2 reports results analogous to those reported in Table 4 except that stock-trading households are excluded. The exclusion of stock-trading households does not materially change our estimates.

Table A4-2: Crowding-out results.

	Whole sample	Single households
	(1)	(2)
Crowding-out	-0.031	-0.106
	(0.165)	(0.176)
R^2	0.075	0.109
n	91,865	35,887
N	472,579	154,252

Notes: Table A4-2 shows the crowding-out effect ($\hat{\rho}$) estimated using Equations (8) and (9), with households who engage in stock-trading excluded from the sample. Column (1) reports estimates for the whole sample and column (2) reports estimates for single households only. The estimates are shown with controls for age fixed effects. Standard errors, clustered at the household level, are in parentheses.

Alternative definitions of the treatment and control groups

Prior to the pension reform, the mandatory contribution rate was 12.0 percent in the private sector and 15.5 percent in the public sector. In our baseline specification, households with a mandatory saving rate below 13.75 percent, the mid-point between the two rates, are assigned to the treatment group and households with a mandatory saving rate above 13.75 percent are assigned to the control group. Here, we test the sensitivity of our results to this choice. Table A4-3 presents results with 13.5 percent as the critical value which assigns households into groups and Table A4-4 shows results for 14.0 percent serving as the critical value. The results are in line with those presented in our baseline specification.

Table A4-3: Crowding-out results.

	Whole sample	Single households
	(1)	(2)
Crowding-out	0.069	-0.242
	(0.165)	(0.176)
R^2	0.062	0.104
n	92,939	36,409
N	520,681	161,518

Notes: Table A4-3 shows the crowding-out coefficient ($\hat{\rho}$) estimated using Equations (8) and (9) with 13.5 percent serving as the cut-off value assigning households into treatment and control groups, rather than 13.75 percent as in the baseline specification. Column (1) reports estimates for the whole sample and column (2) reports estimates for single households only. The estimates are shown with controls for age fixed effects. Standard errors, clustered at the household level, are in parentheses.

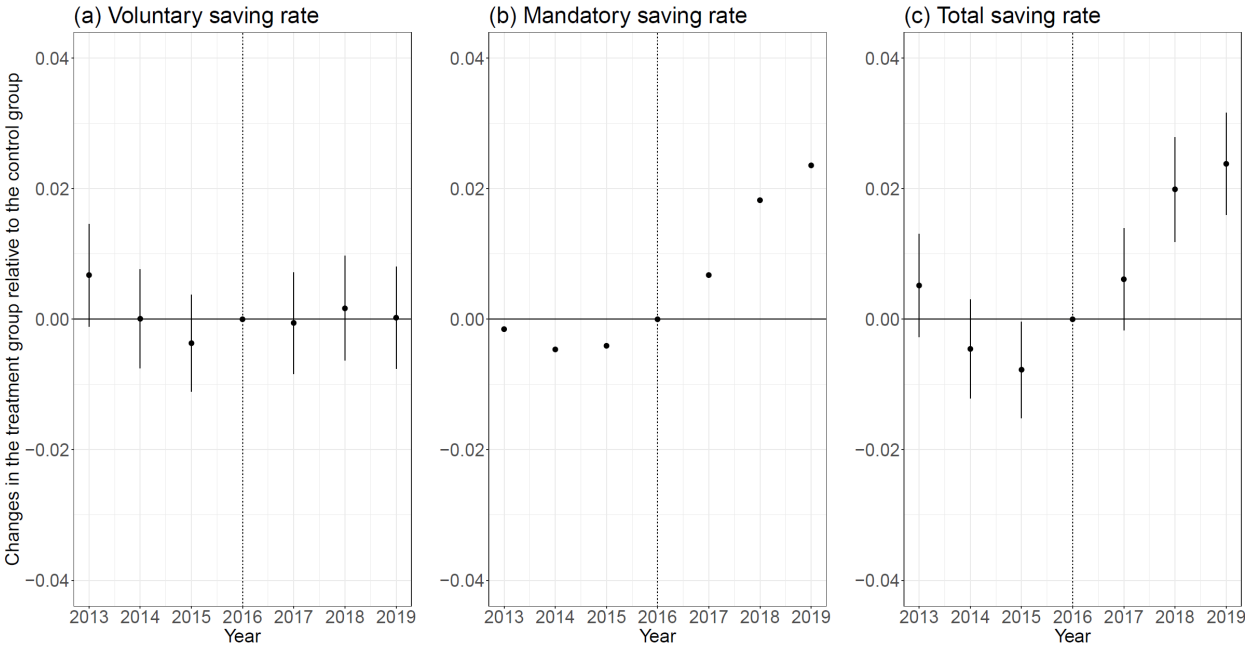
Table A4-4: Crowding-out results.

	Whole sample	Single households
	(1)	(2)
Crowding-out	-0.006	-0.146
	(0.163)	(0.176)
R^2	0.062	0.104
n	92,939	36,409
N	520,681	161,518

Notes: Table A4-4 shows the crowding-out coefficient ($\hat{\rho}$) estimated using Equations (8) and (9) with 14.0 percent serving as the cut-off value assigning households into treatment and control groups, rather than 13.75 percent as in the baseline specification. Column (1) reports estimates for the whole sample and column (2) reports estimates for single households only. The estimates are shown with controls for age fixed effects. Standard errors, clustered at the household level, are in parentheses.

In the main analysis, we assign into treatment and control groups based on households' mandatory saving rates in 2015. As discussed in the main text, Figure 3 shows a small downward trend in the mandatory saving rate before the first stage of the reform is implemented in 2015. While there is no institutional reason for why the mandatory saving rate of the treatment group should decline relative to that of the control group in 2013-2015, this could be due to the definition of the two groups. To test this, we now assign into treatment and control groups based on households' average mandatory saving rates in 2014-2015. As can be seen in Figures A4-1 and A4-2, this alleviates the downward pretrend in 2013-2015. Furthermore, as can be seen by comparing Table 5 to Table A4-5, the crowding-out estimates are robust to this alternative definition of the two groups.

Figure A4-1: Changes in the treatment group's saving rates relative to the control group



Notes: Figure A4-1 plots the estimated γ_t from equation (5), 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. TThe estimates are shown with controls for age fixed effects. 95 percent confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines.

Table A4-5: Crowding-out results - Treatment and control based on 2014-2015 averages

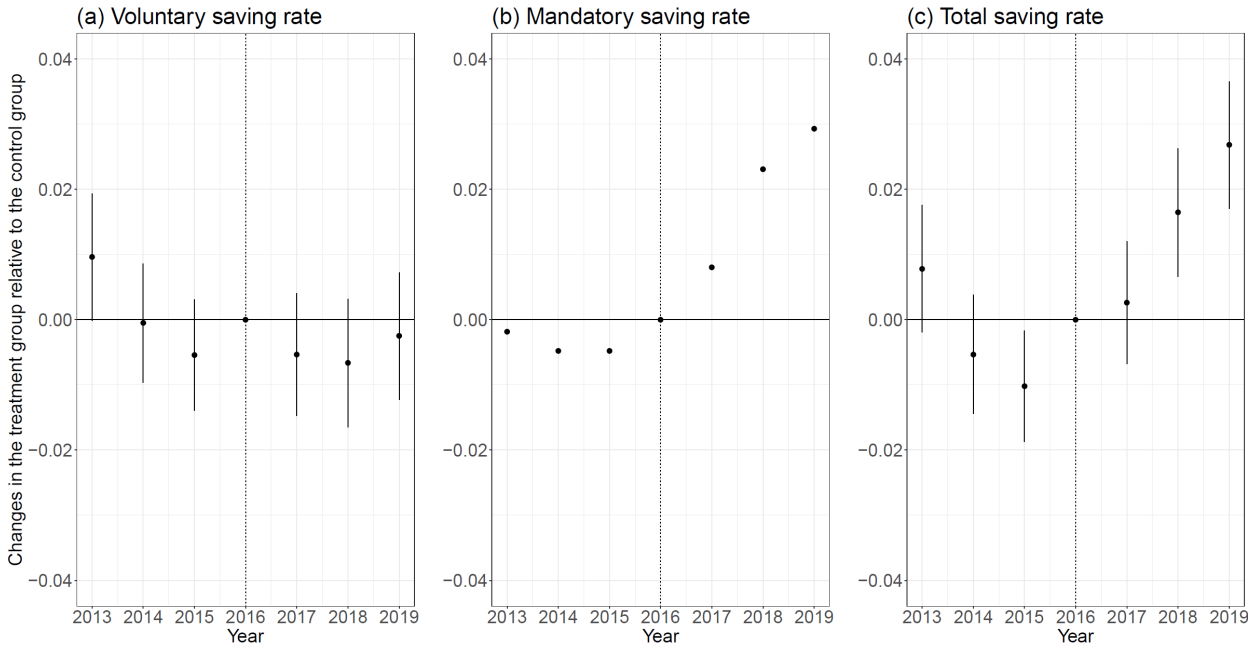
	Whole sample	Single households
	(1)	(2)
Crowding-out	-0.007	-0.206
	(0.151)	(0.168)
R^2	0.062	0.104
n	92,939	36,409
N	520,682	161,518

Notes: Table A4-5 shows estimation of the crowding-out effects ($\hat{\rho}$) using Equations (8) and (9). The results are based on a definition of the treatment and control groups that differ from the results in the main text. In the main text, households with a mandatory saving rate of above 13.75 percent in 2015 are assigned into the control group and others into the treatment group. Here, we use the same cut-off value but assign into groups based on the average mandatory contribution rate in 2014-2015. Columns (1) reports findings from a 2SLS estimation for the whole sample and column (2) reports findings for single households only. Standard errors, clustered at the household level, are in parentheses.

The full effect of the reform

The analysis in the main text is based on data from the entire post-reform period. However, the full effect of the reform was reached only in 2019. Therefore, it is informative for the size of the crowding-out effect to estimate it for 2019 only. As such, here we estimate the crowding-out effect

Figure A4-2: Changes in the treatment group's saving rates relative to the control group



Notes: Panel (a) of Figure A4-2 plots the estimated γ_t from Equation (5) for single households and couples in the same sector of the labor market. 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 estimates are shown with controls for age fixed effects. 95 perent confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines.

using 2019 as the only post-reform year. The sample period is, thus, 2013-2015 and 2019. Table A4-6 reports the first stage. As expected, the first stage effect is larger than in Table 4 since the effect of the reform on the mandatory saving rate in 2019 is larger than it is on average over the post-reform period of 2016-2019. However, the crowding-out estimates reported in Table A4-7 is both close to zero and close to the results reported in Table 5 in the main text.

Table A4-6: First stage results

	Whole sample (1)	Single households (2)
$\hat{\pi}_1$	0.026***	0.033***
	(<0.001)	(<0.001)
F-statistic (instrument)	120,411	2,020
R^2	0.829	0.857
n	92,939	35,810
N	305,822	95,797

Notes: Table A4-6 reports first stage results using Equation (8) using 2019 as the only post-reform year. It reports the coefficient on the instrument, that is the interaction of a post-reform dummy variable and the treatment group dummy variable, in a regression of mandatory saving rates on individual fixed effects, a post-reform dummy variable, the aforementioned interaction. The estimates are shown with controls for age fixed effects and separately for the whole sample and single households only. Standard errors, clustered at the household level, are in parentheses.

Table A4-7: Crowding-out results

	Whole sample (1)	Single households (2)
Crowding-out	0.057	-0.073
	(0.143)	(0.139)
R^2	0.061	0.108
n	92,939	35,810
N	305,822	95,797

Notes: Table A4-7 shows estimation of the crowding-out effects ($\hat{\rho}$) estimated using Equations (8) and (9) with 2019 as the only post-reform year. Column (1) reports estimates for the whole sample and column (2) reports estimates for single households only. The estimates are shown with controls for age fixed effects. 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 percent 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.[22]

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 percent 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 A4-8 shows that the estimated crowding-out effect using the alternative measure of pension saving is similar to the ones reported in section 5.4.

Table A4-8: Crowding-out results.

	Whole sample	Single households
	(1)	(2)
Crowding-out	0.038	-0.148
	(0.135)	(0.151)
R^2	0.062	0.104
n	92,939	36,409
N	520,681	161,518

Notes: Table A4-8 shows the crowding-out ($\hat{\rho}$) estimated using Equations (8) and (9) using an alternative measure for pension saving, which is a function of the household's age and survival probabilities. Column (1) reports estimates for the whole sample and column (2) reports estimates for single households only. The estimates are shown with controls for age fixed effects. Standard errors, clustered at the household level, are in parentheses.

[22]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.

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 A4-9 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 A4-9: Crowding-out results.

	Whole sample	Single households
	(1)	(2)
Crowding-out	-0.194	-0.283
	(0.154)	(0.166)
R^2	0.074	0.118
n	103,374	42,964
N	581,577	192,746

Notes: Table A4-9 shows the crowding-out effects ($\hat{\rho}$) estimated using Equations (8) and (9) including households whose labor income is below 50 percent of the median. Column (1) reports estimates for the whole sample and column (2) reports estimates for single households only. The estimates are shown with controls for age fixed effects. 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 A4-10 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 A4-11 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 A4-10: First-stage results.

	Whole sample	Single households
	(1)	(2)
	0.009***	0.014***
	(<0.001)	(<0.001)
F-statistic (instrument)	21,115	15,783
R^2	0.640	0.644
n	103,374	42,964
N	581,577	192,746

Notes: Table A4-10 reports first stage results using Equation (8) using 2019 as the only post-reform year with the treatment and control groups defined based on individuals' sector of work rather than cut-off values for the mandatory saving rate. It reports the coefficient on the instrument, that is the interaction of a post-reform dummy variable and the treatment group dummy variable, in a regression of mandatory saving rates on individual fixed effects, a post-reform dummy variable, the aforementioned interaction. The estimates are shown with controls for age fixed effects and separately for the whole sample and single households only. Standard errors, clustered at the household level, are in parentheses.

Table A4-11: Crowding-out results.

	Whole sample	Single households
	(1)	(2)
	-0.228	-0.459
	(0.188)	(0.214)
R^2	0.074	0.118
n	103,374	42,964
N	581,577	192,746

Notes: Table A4-11 shows the crowding-out coefficient ($\hat{\rho}$) estimated using Equations (8) and (9) and treatment and control groups defined based on individuals' sector of work rather than cut-off values for the mandatory saving rate as in the main analysis. Column (1) reports estimates for the whole sample and column (2) reports estimates for single households only. The estimates are shown with controls for age fixed effects. Standard errors, clustered at the household level, are in parentheses.

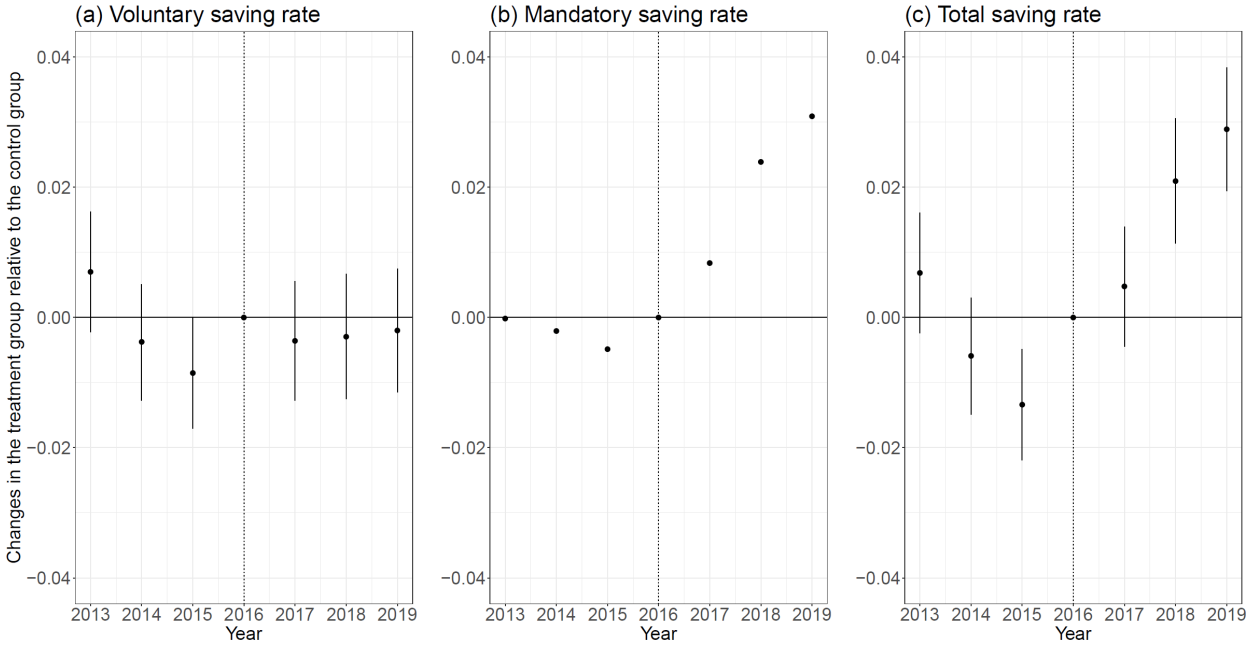
Single households and couples in the same sector of the labor market

In our main specifications, we estimate the crowding-out coefficient using both the full sample, which includes single households and couples, and single households only. We estimate the crowding-out effect using single households only since for the full sample, assignment into treatment and control groups is based on whether the pre-reform mandatory saving rate was below or above 13.75 percent, which is the mid-point between the 12.0 percent private sector rate and the 15.5 percent public sector rate. Effectively, this means that the primary earner of the household governs whether the household is assigned into the treatment or control group since

the household mandatory saving rate will be above the cutoff point if the primary earner is a public sector worker and below the cutoff if the primary earner is a private sector worker. For individuals without partners or with partners who work in the same sector, the mandatory saving rate should be exactly 12 percent or 15.5 percent unless they switched sectors within a given year. This could be problematic as some individuals of the control group, namely the secondary earner in a household, is potentially treated, although this does not seem to alter our results much. Yet another option would be to define the sample as single households and those couples who work in the same sector of the labor market, where sector refers to the private sector and public sector.

Figure A4-1 shows the parallel trends analysis for this sample, as estimated by equation (5). As can be seen by comparing Figures A4-1 and A5-2, the pre-trends are somewhat more convincing for the sample of single households, which is why we include it as a part of our main specification. Table A4-12 shows that our results are robust across all these samples.

Figure A4-1: Changes in the treatment group's saving rates relative to the control group



Notes: Panel (a) of Figure A4-1 plots the estimated γ_t from equation (5) for single households and couples in the same sector of the labor market. 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 estimates are shown with controls for age fixed effects. 95 percent confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines. Note that those are small and barely visible in panel (b).

Table A4-12: Crowding-out results.

	W/o households in different sectors
Crowding-out	0.036
	(0.160)
R^2	0.068
n	63,442
N	329,122

Notes: Table A4-12 shows the crowding-out coefficient ($\hat{\rho}$) estimated using Equations (8) and (9) with an alternative measure for pension saving, which is a function of the household's age and survival probabilities. Column (1) reports estimates for the whole sample and column (2) reports estimates for single households only. The estimates are shown with controls for age fixed effects. Standard errors, clustered at the household level, are in parentheses.

Crowding-out of amortizations

It is possible that a higher mandatory saving rate would reduce households' ability to pay down debt. Here, we estimate the extent to which the pension reform reduced amortizations. In doing so, we estimate Equations (8) and (8) with the amortization rate out of wages as the dependent variable in the second stage. Table A4-13 reports the results. The crowding-out effect is a small and statistically insignificant -0.049 for the whole sample and -0.028 for single households only. As such, there are no indications households responded to the increase in the mandatory saving rate by changing their amortization rates.

Table A4-13: Crowding-out results for the amortization rate out of wages

	Whole sample	*Single households*
	(1)	(2)
Crowding-out	-0.049	-0.028
	(0.099)	(0.122)
R^2	0.051	0.083
n	92,939	36,409
N	520,681	161,518

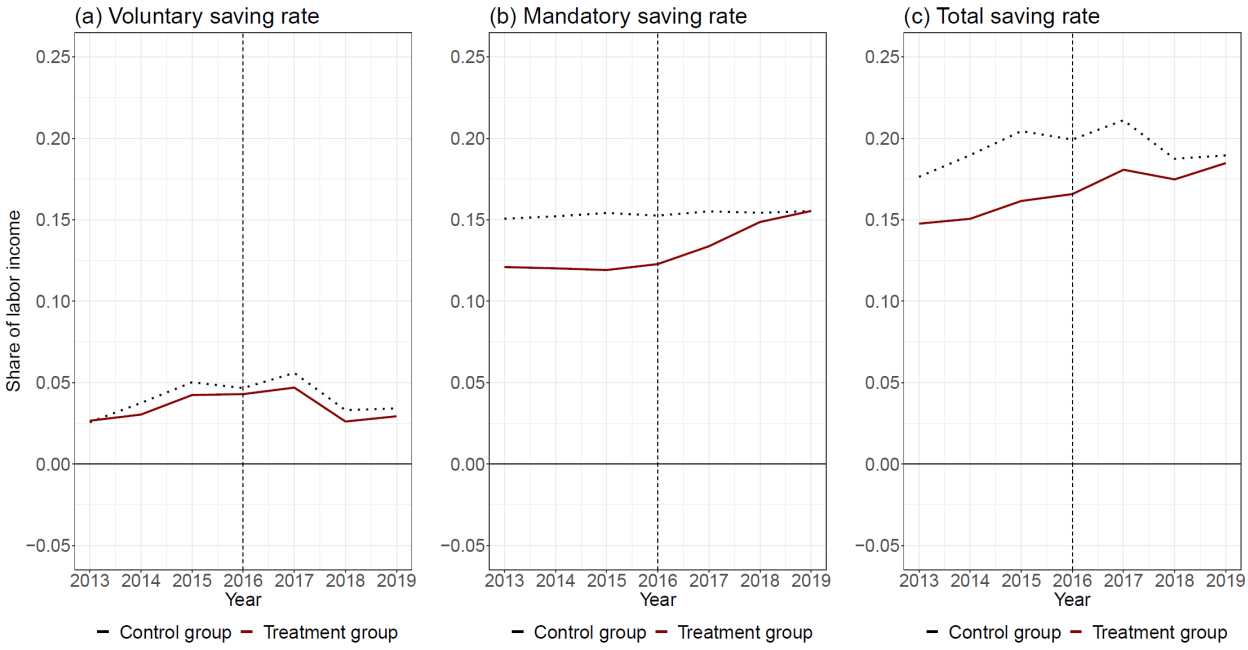
Notes: Table A4-13 shows the crowding-out effects ($\hat{\rho}$) estimated using Equations (8) and (9). In contrast to the main results in Table 5, the measure of household saving used here does not subtract imputed rent for homeowners and vehicle depreciation. Column (1) reports estimates for the whole sample and column (2) reports estimates for single households only. The estimates are shown with controls for age fixed effects. Standard errors, clustered at the household level, are in parentheses.

A5. Average Saving Rates and Parallel Trends for Single Households

Figures A5-1 and A5-2 show average saving rates and the parallel trends analysis for a sample of single households only. This is of interest since for single households, we do not have households with one individual in the high-mandatory-saving public sector and the other in the low-mandatory-saving private sector, resulting in effective mandatory saving rates between the institutional rates of 12.0 percent and 15.5 percent prior to the reform.

A sample of strictly single households, thus, might offer a cleaner natural experiment than the whole sample, although at the loss of 69 percent of the observations.

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

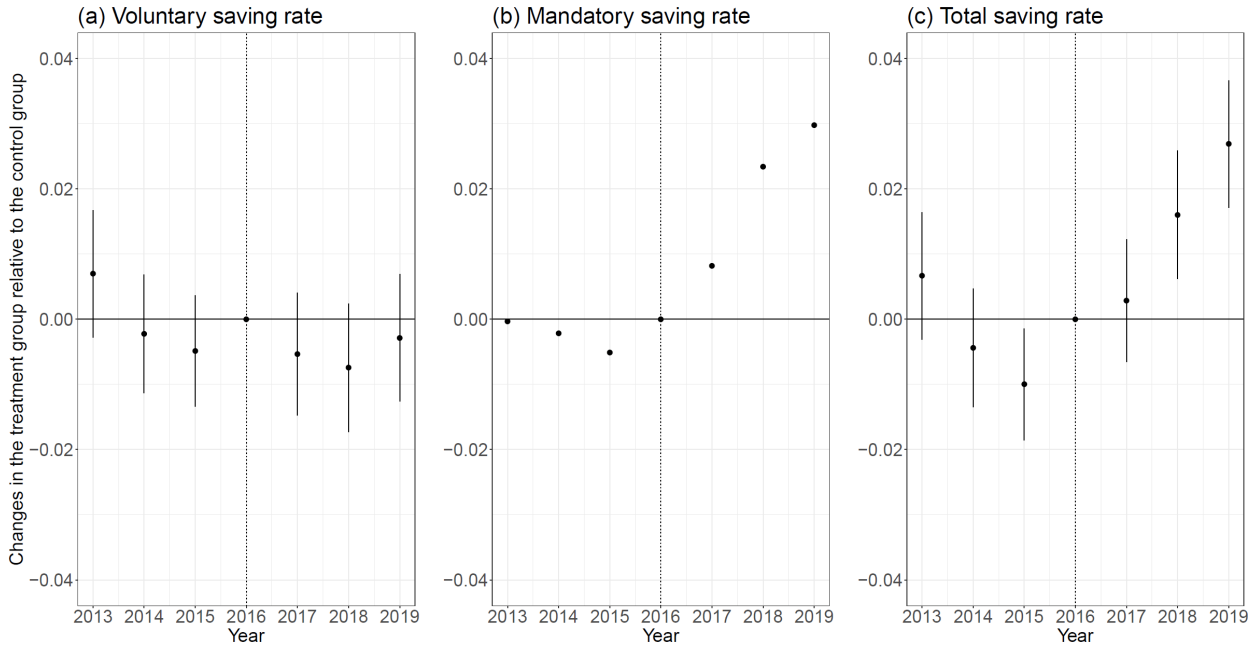


Notes: Figure A5-1 shows the average voluntary (panel a), mandatory (panel b), and total (panel c) saving rates out of household wages for the control group (dotted black line) and the treatment group (solid red line) as measured by fitted values from three regressions where each of the aforementioned 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.

A6. Substitution within the Pension System and Debt Accumulation

Figure A6-1 shows the average net contribution to third-pillar pension funds of each group in 2013-2019. Figure A6-2 shows the parallel trends analysis for a measure of voluntary saving that excludes voluntary third-pillar pension saving. Figure A6-3 shows the same for a sample of

Figure A5-2: Changes in the treatment group's saving rates relative to the control group



Notes: Panel (a) of Figure A5-2 plots the estimated γ_t from Equation (5) 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 estimates are shown with controls for age fixed effects. 95 percent confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines. Note that those are small and barely visible in panel (b).

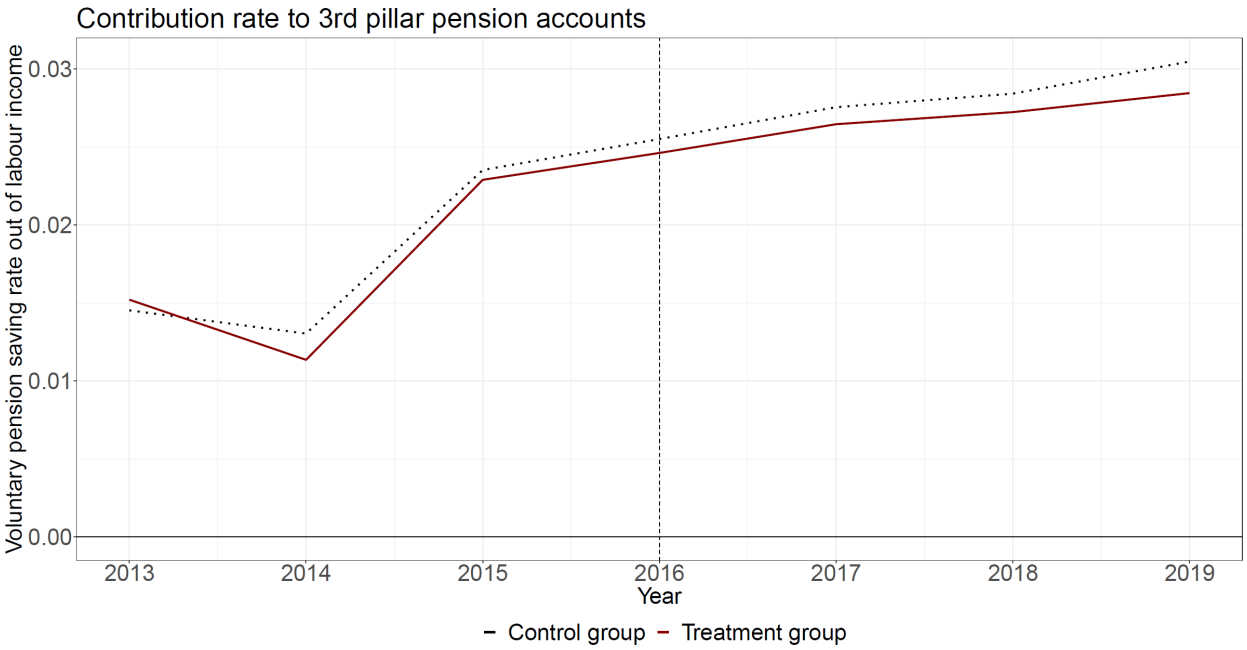
households who have never contributed to a voluntary third-pillar pension scheme. The corresponding crowding-out estimates are reported in Table 6 of the main text. Excluding voluntary third-pillar pension contributions from our measure of voluntary saving does not meaningfully change our results. Furthermore, studying a sample of only households who do not possess any voluntary third-pillar pension savings also does not suggest those household responded to the reform by adjusting their voluntary saving rate. However, due to the small sample size, the confidence intervals around the estimates are large.

Turning to debt, since both debt and the first difference thereof contain large outliers, we redo the analysis of Section 5.6 with both debt and interest payments winsorized at the 5th and 95th percentiles. Figure A6-4 shows the results, which are very similar to those of Figure 6 of the main text.

A7. Job Switches

Figures A7-1 to A7-3 show measures for the job switching rate from the private sector to the public sector. We show this to support the assumption underlying the job switchers analysis that switches are unrelated to the mandatory saving rate. As discussed further in the main text, there are no visible indications of strategic switches to the private sector before or during the pension reform.

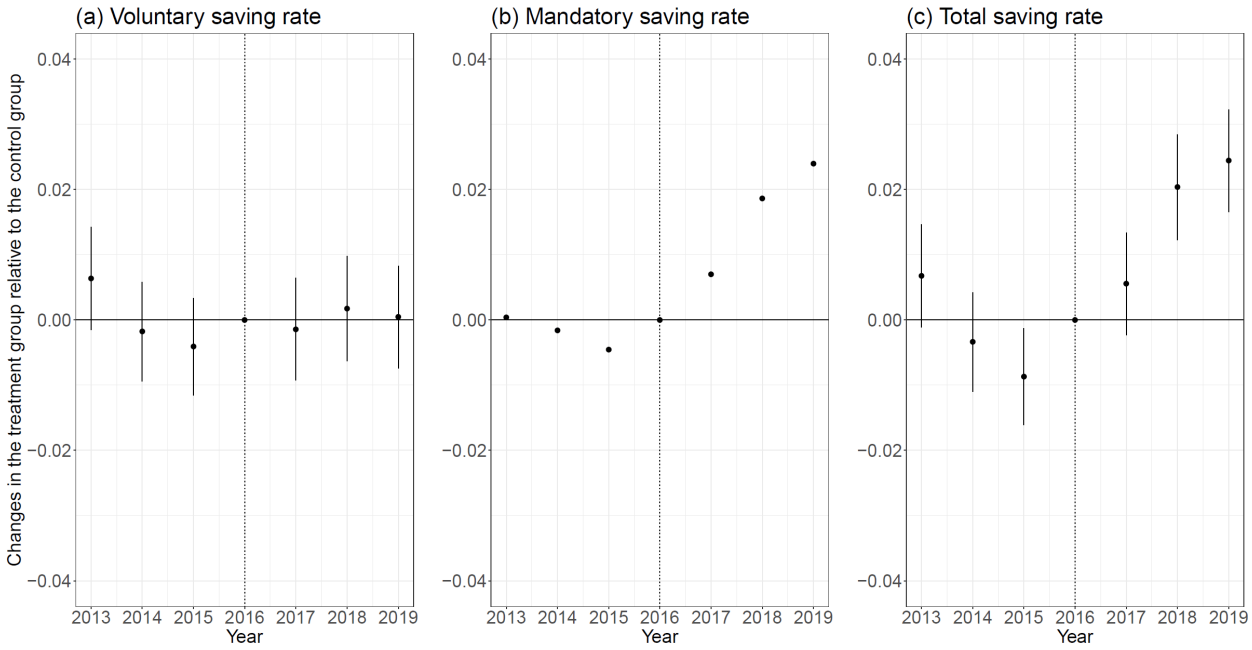
Figure A6-1: Saving rates for the treatment and control group



Notes: Figure A6-1 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.

Figures A7-4 and A7-5 show how wages develop around the time of the job switch. As can be seen, there are no indications of wages increasing markedly after a job switch. If anything, it appears as if they decreases slightly. This is consistent with the reduction in real wages within the sample period. As such, rising wages do not explain the increase in total saving following a job switch. Rather, it seems like the increase in the mandatory saving rate associated with the switch from the private sector to the public sector drives the increase in the total saving rate.

Figure A6-2: Changes in the treatment group's voluntary saving outside of third-pillar pension saving relative to the control group



Notes: Panel (a) of Figure A6-2 plots the estimated γ_t from Equation (5) for voluntary saving outside of third pillar pension saving. 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 estimates are shown with controls for age fixed effects. 95 percent confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines. Note that those are small and barely visible in panel (b).

A8. Additional Survey Information

A8.1. Additional Results

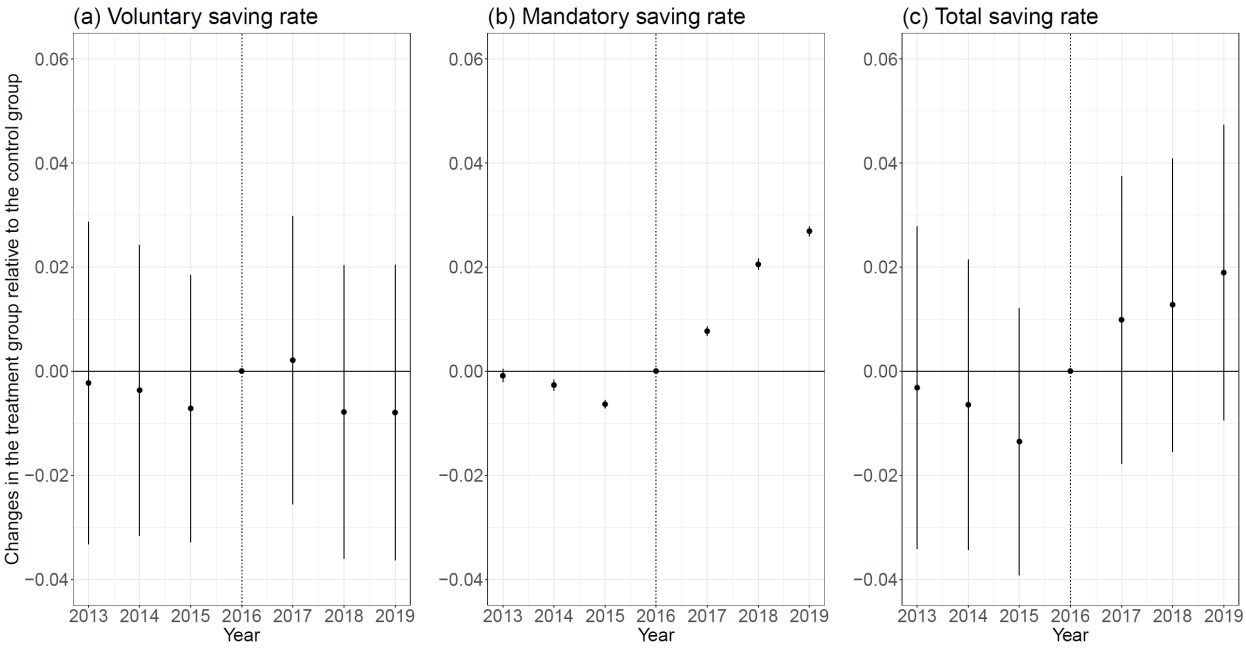
In this section, we estimate Equation (12) from Section 7.2 for several additional subgroups.

The first hypothesis is the one from the main text, which we repeat here for completeness. 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 A8-1 shows the responses to the question on households' finances. Around 62 percent 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.

Figure A6-3: Changes in the treatment group's voluntary saving relative to the control group for the sub sample of individuals that do not possess third pillar pension savings

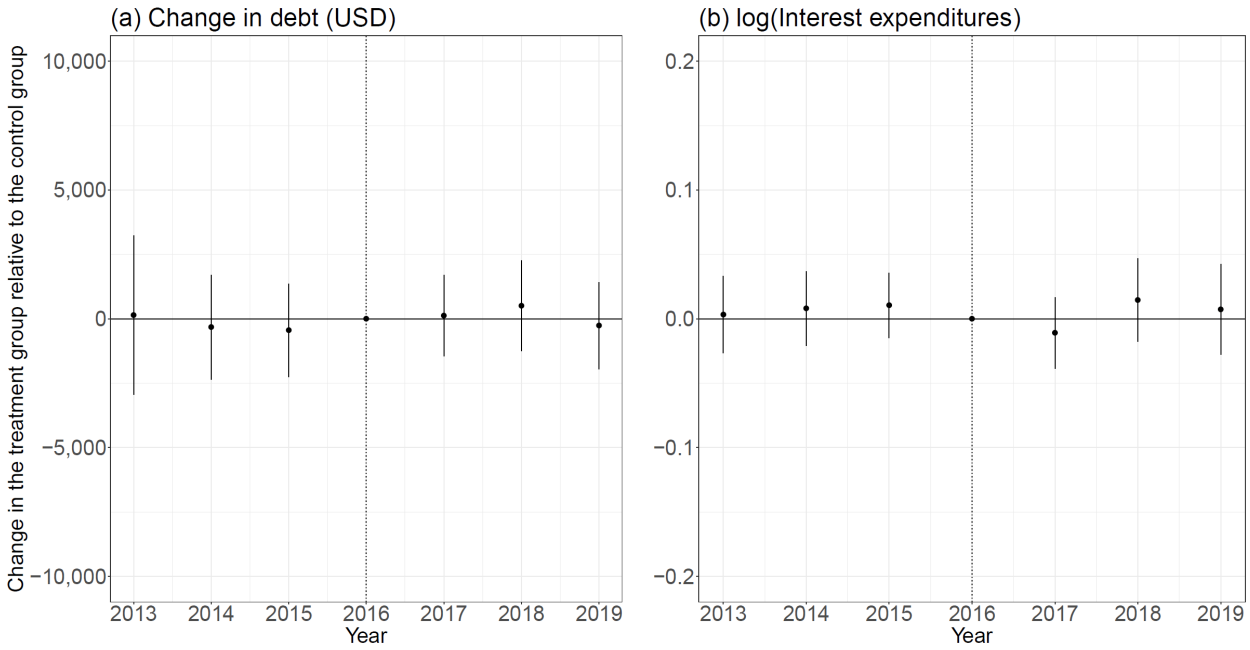


Notes: Panel (a) of Figure A6-3 plots the estimated γ_t from Equation (5) for voluntary saving for those that do not possess any third pillar pension savings. In panels (b) and (c), the dependent variable has been replaced by the mandatory saving rate and the total saving rate, respectively. The dotted vertical line in 2016 shows when the first stage of the reform was implemented. The estimates are shown with controls for age fixed effects. 95 percent confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines. Note that those are small and barely visible in panel (b).

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 A8-2). A striking 21 percent of the control group and 31 percent of the treatment group do not save in addition to their pension saving. Now, 7.5 percent and 12 percent 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 percent 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 percent 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

Figure A6-4: Changes in the treatment group's debt and interest expenditures relative to the control group



Notes: Figure A6-4 plots the estimated γ_t from Equation (5), with the voluntary pension saving rate, measured by contributions to voluntary third-pillar pension accounts net of withdrawals, as the dependent variable. Unlike in the main analysis, both debt and interest payments are winsorized at the 5th and 95th percentiles. The dotted vertical line in 2016 shows when the first stage of the reform was implemented. The estimates are shown with controls for age fixed effects. 95 percent confidence intervals, based on standard errors clustered at the household level, are represented by solid vertical lines.

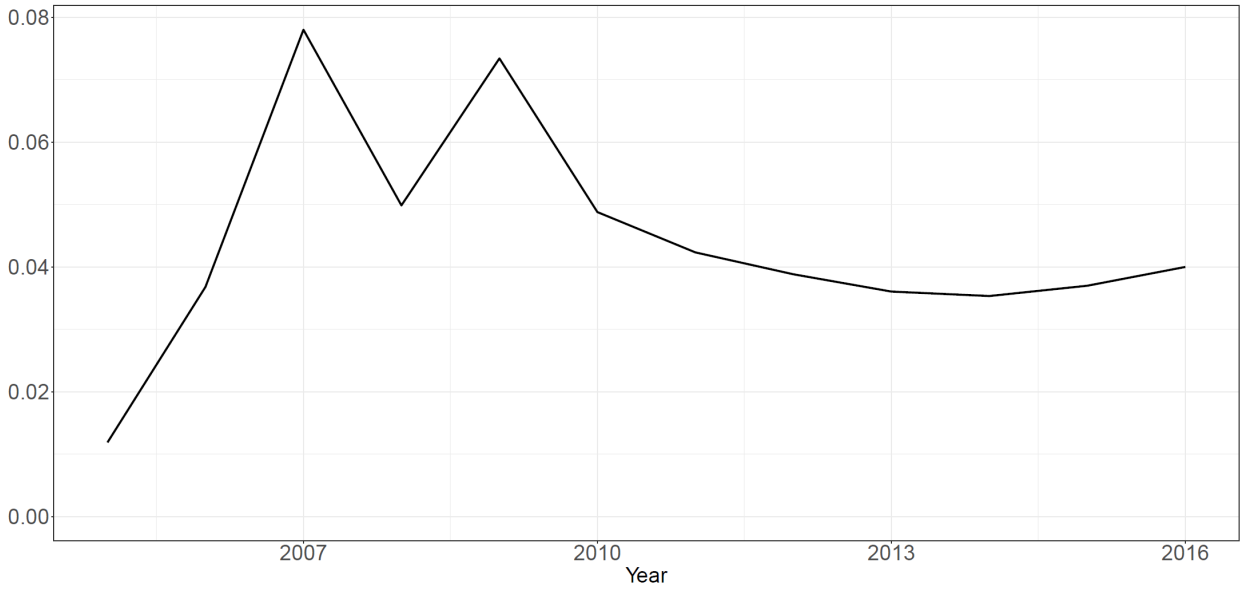
decrease their saving - the indicator, $G_i^3 = \text{Target saving}_i$, takes the value 1 for those that *have a certain amount in mind that they want to have saved at a certain time and plan their saving accordingly*, but is zero otherwise.

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 percent reporting that their main saving motive was saving for retirement (see Figure A8-3). 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.

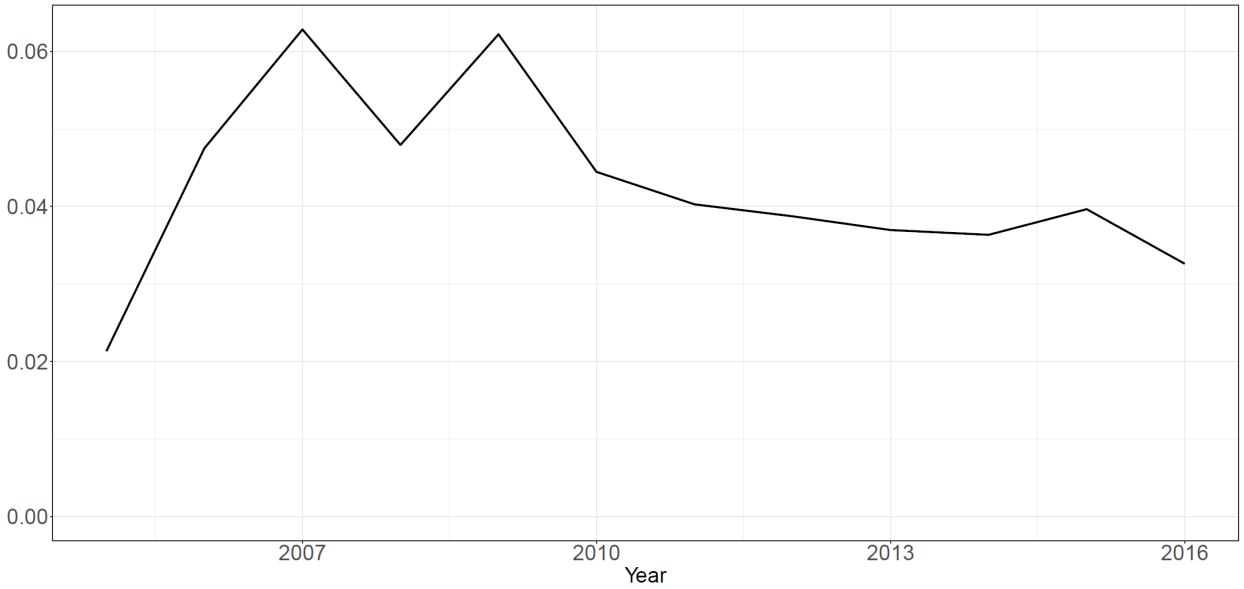
Hypothesis 4 ***(Pension motives)*** *In response to the reform, private sector workers with pension*

Figure A7-1: The rate of job switches from the private sector to the public sector.



Notes: Figure A7-1 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 percent in the current period but was below 13.75 percent in the previous period and that the jump in the contribution rate is at least 0.5pp.

Figure A7-2: The rate of job switches from the private sector to the public sector among single households.

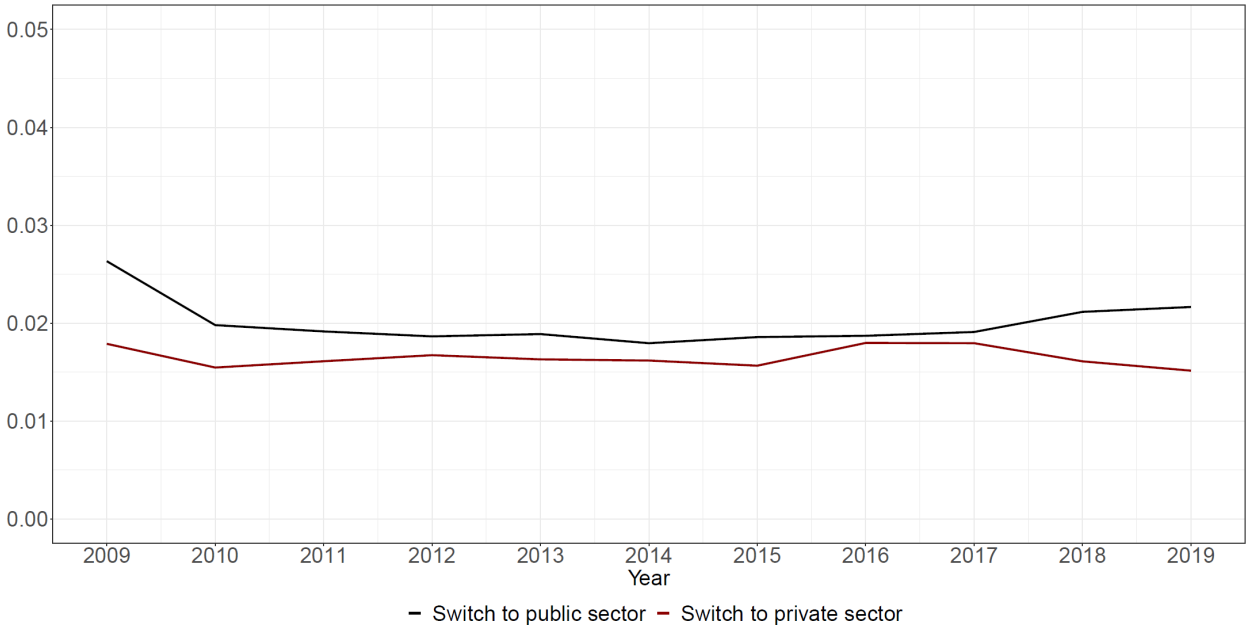


Notes: Figure A7-2 shows the rate of job switches from the private sector to the public sector in the whole sample. A switch is assumed to have taken place if the contribution rate is above 13.75 percent in the current period but was below 13.75 percent in the previous period and that the jump in the contribution rate is at least 0.5pp.

saving motives decrease their saving more than others, $\alpha_3^4 < 0$.

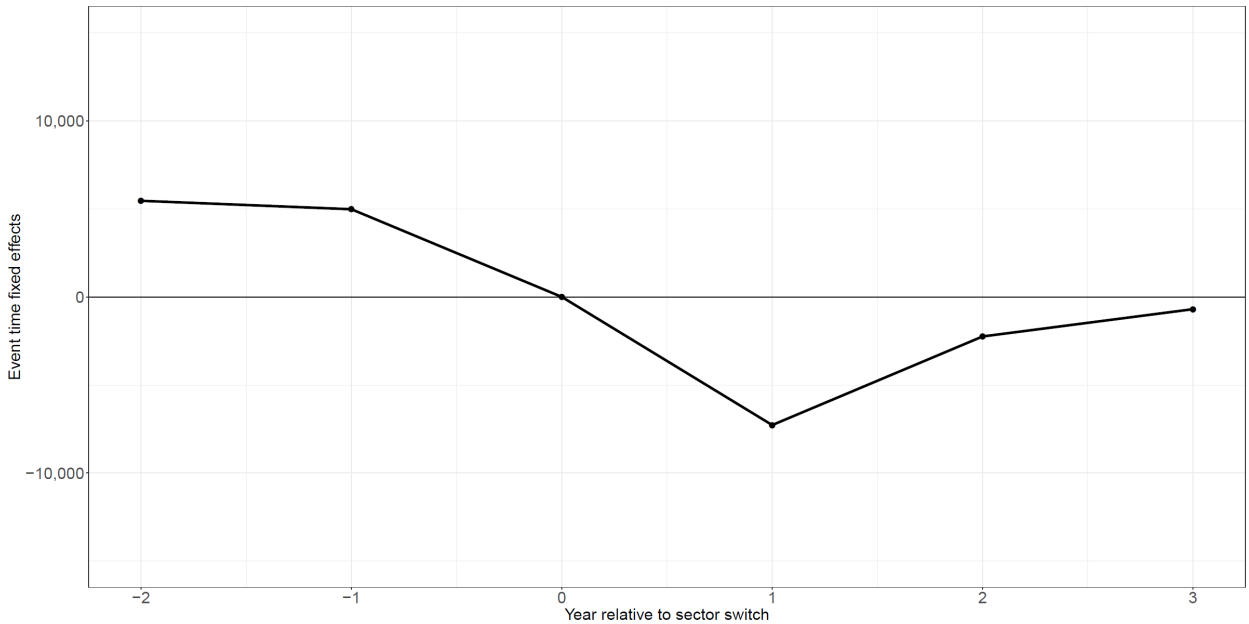
Before we consider our hypotheses, we start by estimating specification (12), with the treatment group and the subgroup dummies excluded (see column (1) of Table A8-1). Between 2015 and

Figure A7-3: The rate of job switches from the private sector to the public sector based on sector classification.



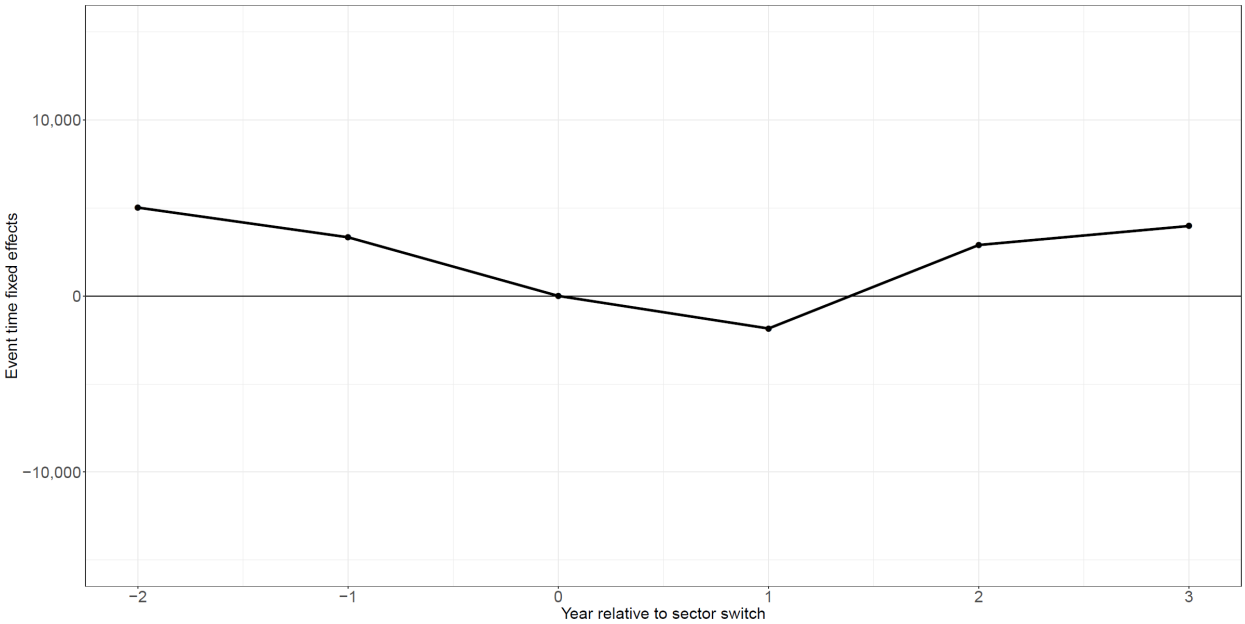
Notes: Figure A7-3 shows the rate of job switches from the private sector to the public sector in the whole sample. The public sector is defined as a) public administration, defense, and compulsory social security, b) education, c) human health services, d) residential care and social work activities, 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 A7-4: Wage development surrounding a job switch.



Notes: Figure A7-4 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 A7-5: Wage development surrounding a job switch.



Notes: Figure A7-5 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.

2021, 40 percent of individuals increased their saving and 7 percent 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.

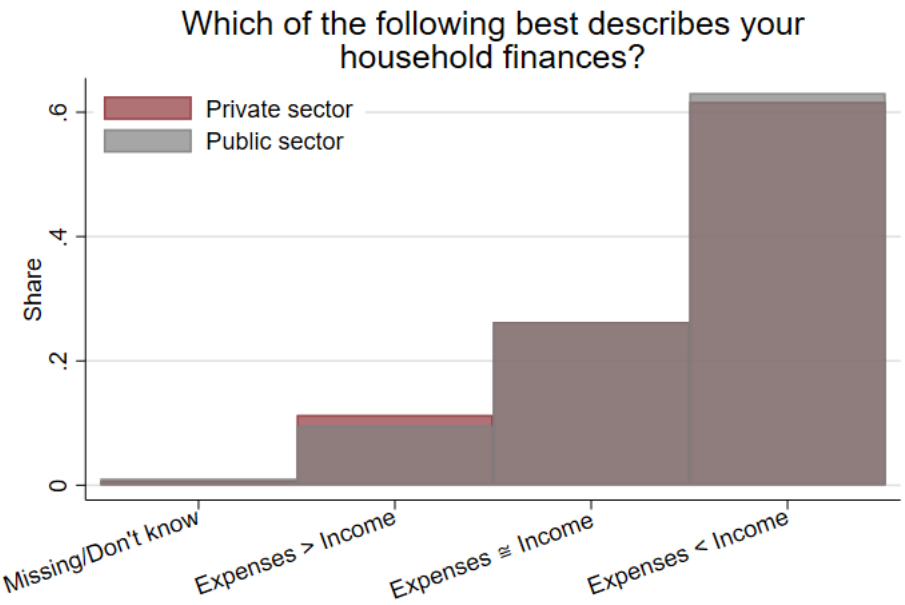
The regression results from Equation (12), using the awareness, liquidity, and target dummies, can be seen in columns (4), (5) and (6), respectively. The estimates of the parameters of interest are statistically insignificant. That is, although the survey confirms that individuals are generally unaware 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.

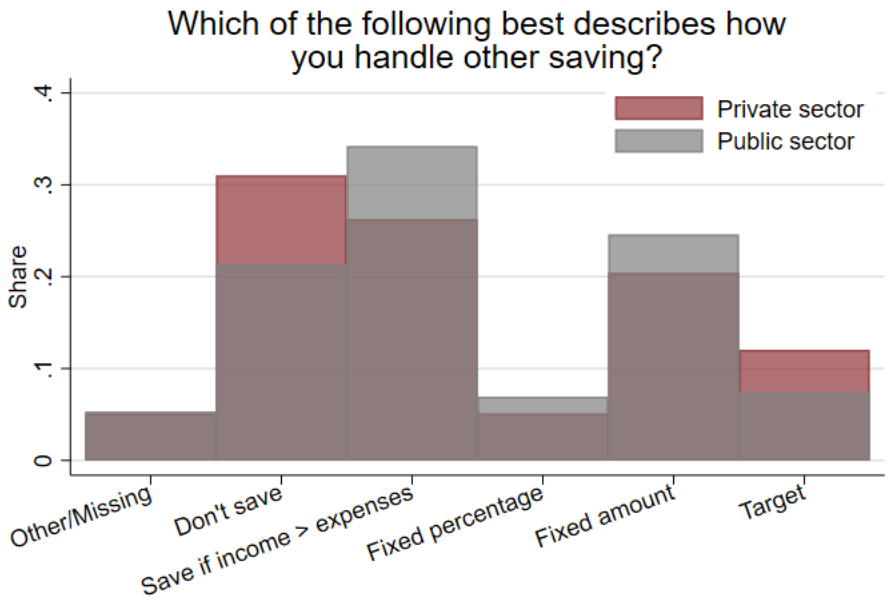
Since only 14 percent of the treatment group report pension saving as their main saving motive, we conclude that a part of the very limited response to the reform is likely to be driven by a few individuals with pension saving motives.

Figure A8-1: Households' saving situation



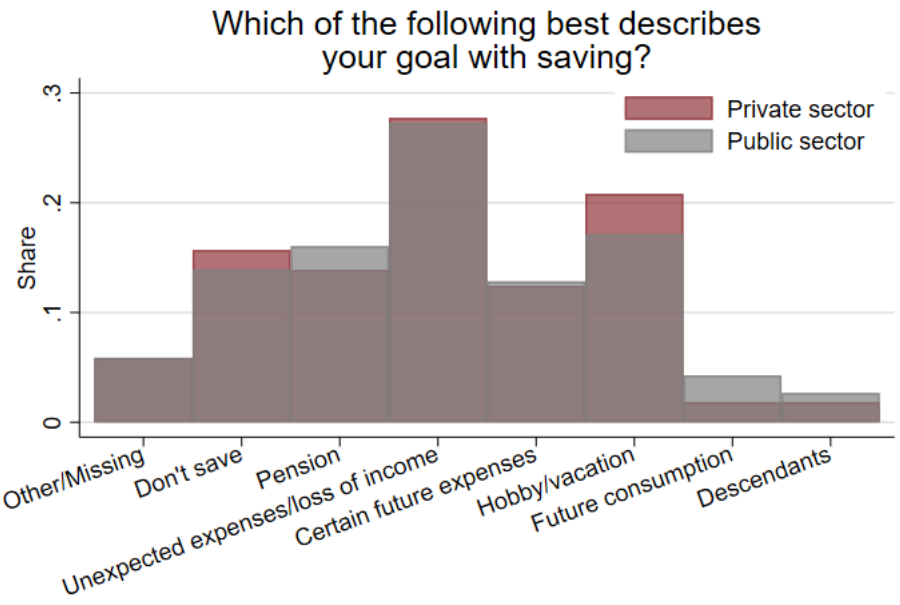
Notes: Which of the following best describes your household finances? i) Expenses are generally higher than income (Expenses $>$ Income), ii) Expenses approximately equal income (Expenses $\approx$ income), iii) Expenses are generally lower than income (Expenses $<$ Income).

Figure A8-2: 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 A8-3: 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 A8-1: Estimates of the effects of the reform on voluntary saving by subgroups

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							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 (12) 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. $***p < 0.01$,$** p < 0.05$,$*p < 0.1$.

A8.2 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 percent of responses, while the lowest-income people gave the correct answer only 23.3 percent 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 percent of responses, while the lowest-income people gave the correct answer in only 23.3 percent 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.

A9. Validity of Consumption and Saving Measures

While survey data on consumption and saving tend to be inaccurate and relying on imputed data from accurate third-party reported information from tax registries is a clear improvement, such imputed measures might have problems of their own. Although these types of methods to construct measures for consumption and saving has gained popularity in recent years, it remains important to illustrate the validity of such imputed values. Unfortunately, we were not able to access survey data from Statistics Iceland, which would have allowed us to compare survey results and the imputed values for consumption and saving for each household in the survey. Instead, we compare our measure of consumption with private consumption in the national accounts and our measure of the voluntary saving rate with stylized facts from the literature. When comparing with the national accounts, which includes the consumption of every individual in Iceland, we only omit deficient returns and outliers. Deficient returns stem from households that either do not file their taxes or for other reasons their tax returns must be filled manually. In those cases, we do not have all of the information necessary to impute consumption and saving. The outliers are defined as households with a voluntary saving rate out of disposable income below -1 and above 1, which is also a restriction we use in the main analysis. As such the sample size for this comparison is larger than in our regression analysis (1,140,110 vs. 520,064 observations). Importantly, this larger sample size is only used when comparing with private consumption in national accounts. Otherwise, we use the same sample as in our main regression analysis.

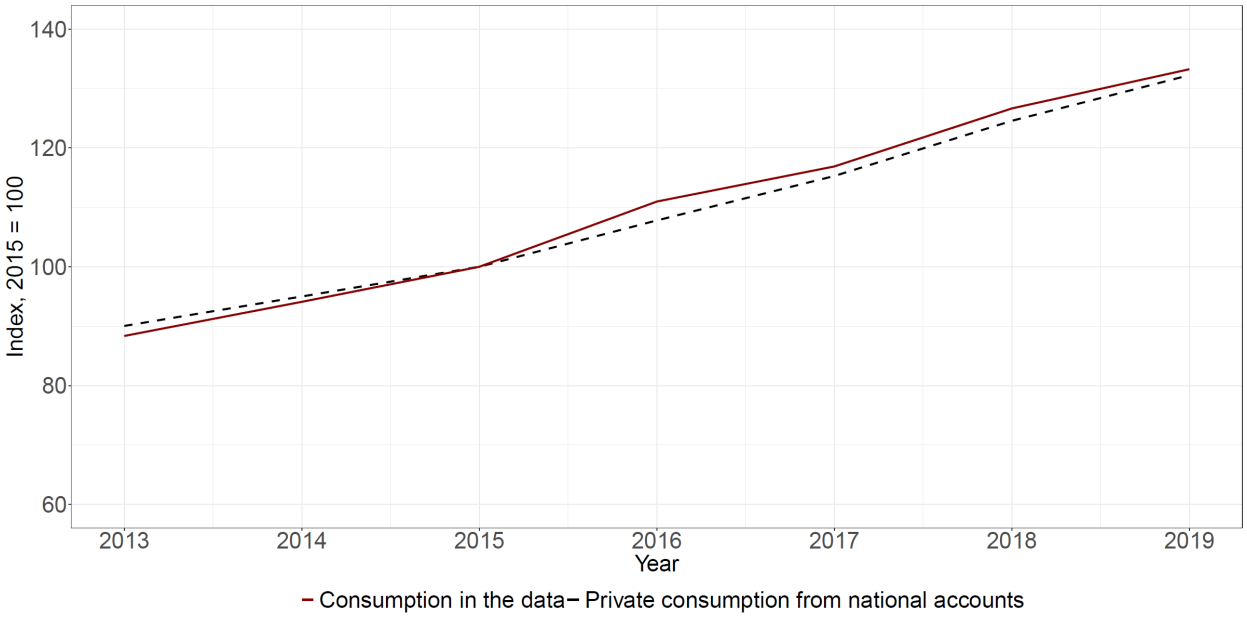
Figure A9-1 shows indices of total consumption according to our measure and private

consumption from national accounts in 2013-2019. The index is equal to 100 in 2015 by construction. The indices move broadly together, although some differences emerge in certain years.

Figure A9-2 shows the yearly growth rate in average consumption according to the two measures. We do this since the share of deficient tax records and outliers might fluctuate between years. The growth rates of the two are similar, again with the exception of 2016.

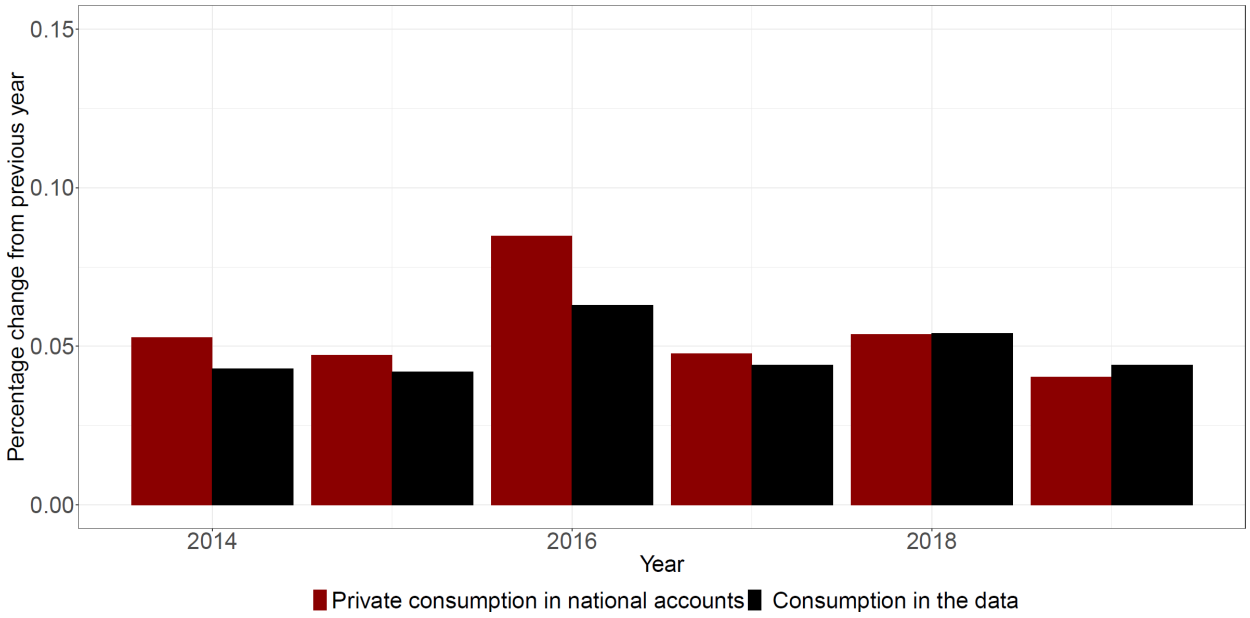
Finally, previous literature has found that rich and high-income households save a larger fraction of their income than lower income households. Dynan et al. (2004) find a strong positive association between permanent income and saving rates in the U.S. Mian et al. (2021) show that the top 10% income group in the U.S. has a substantially higher saving rate than the next 40% and the bottom 50%. Panel (a) of Figure A9-3 plots the average saving rates by disposable income groups in 2019. The income groups are formed within each birth cohort, defined as an individual's year of birth, to ensure saving rates across income groups are not driven by life-cycle effects. Panel (a) confirms the aforementioned trends in the literature using information from Icelandic tax registries. Furthermore, panel (b) of Figure A9-3 shows that, in accordance with the life cycle model of consumption and saving that the saving rates are low for young workers and rise as they approach middle age and retirement.

Figure A9-1: Consumption index, 2015 = 100 - our measure vs. national accounts



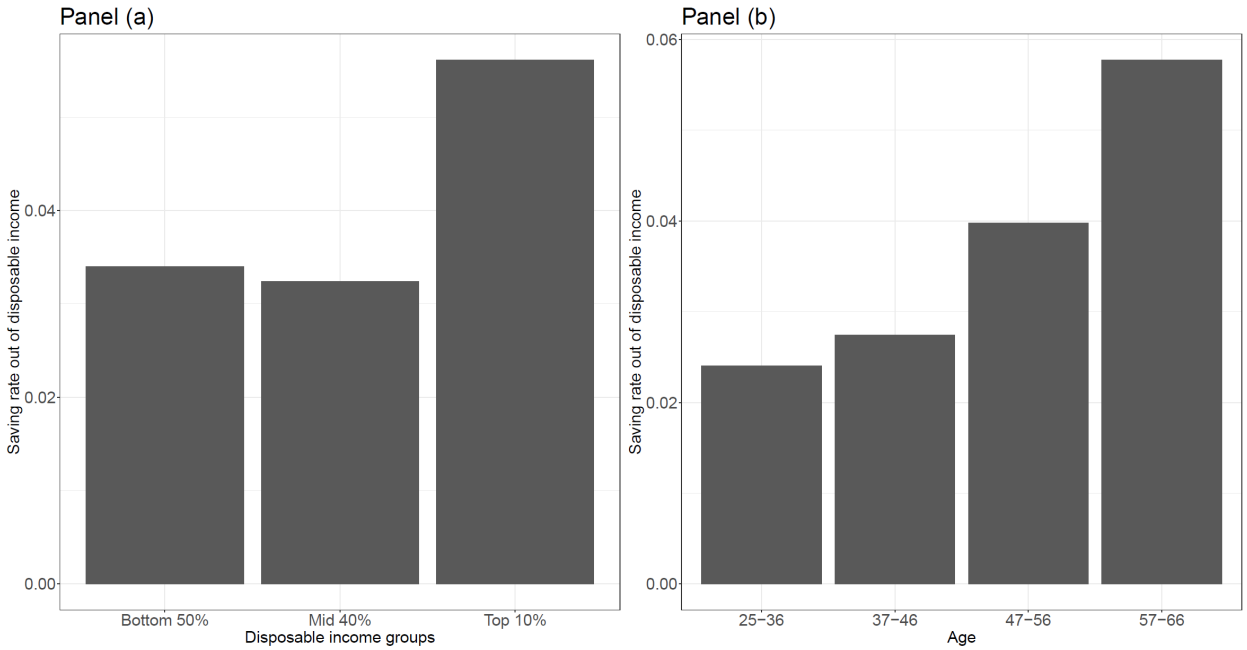
Notes: Figure A9-1 plots an index of total consumption in our data (red line) and national accounts (black line). The index is equal to 100 in 2015 by construction.

Figure A9-2: Yearly change in avg. consumption - our measure vs. national accounts



Notes: Figure A9-2 shows the yearly change in average consumption according to our consumption measure (red bars) and national accounts (black bars)

Figure A9-3: Households' saving rate out of disposable income in 2019



Notes: Panel (a) of Figure A9-3 shows households' saving rate out of disposable income for three disposable income groups. The income groups are formed within each birth cohort, defined as the year of birth, to ensure saving rates across income groups are not driven by life-cycle effects. Panel (b) shows the saving rate by age groups. The saving rate is computed as the average saving rate within each group.

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