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**ESSAYS ON DEBT AND PENSIONS**

**Henrik Yde Andersen**

# **ESSAYS ON DEBT AND PENSIONS**

Doctoral School of Economics and Management

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HANDELSHØJSKOLEN

PhD Series 18-2018

# Essays on Debt and Pensions

Henrik Yde Andersen

Supervisor: Svend Erik Hougaard Jensen

PhD School in Economics and Management  
Copenhagen Business School

Henrik Yde Andersen  
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# Preface

This dissertation is the result of my doctoral studies at the Department of Economics, Copenhagen Business School. I express my gratitude to Danmarks Nationalbank for funding, and I thank both institutions for providing excellent research environments.

There are many people I wish to thank. First, my supervisors, Svend Erik Hougaard Jensen, Søren Leth-Petersen and Torben M. Andersen, for being huge inspirations and taking their time to guide me through the world of academia. My wholehearted thanks to Søren for encouraging me to pursue a PhD and inviting me to work on a joint paper, which constitutes the second chapter in this dissertation. Working closely with you and your contagious curiosity have been truly rewarding. Thanks to Svend for giving me the opportunity to be part of PeRCent and for always being enthusiastic about my research. My best interests were always in safe hands with you. Thanks to Torben for our talks about connecting the dots between theory and empirics and for outstanding guidance along the way.

I wish to thank my colleagues at Danmarks Nationalbank for helpful discussions about my work; in particular, members of the research unit who invested time and effort in getting me started in 2014. Researchers at the Institute for Fiscal Studies in London also deserve a big thank you for their hospitality during my visit in 2017. Special thanks to Cormac O’dea and Orazio Attanasio for arranging the visit. On that note, I express my appreciation for funding from Oticon Fonden, Otto Mønstedts Fond, Christian og Ottilia Brorsons Rejselegat, GTN Fonden and Tranes Fond.

I was privileged enough to shuffle between two workplaces, one at Copenhagen Business School and one at Danmarks Nationalbank. I appreciate all the chats and laughs with fellow PhD students and department members at both institutions. Special thanks to my office mates for being great company throughout the years. I am grateful to the Department of Economics at University of Copenhagen for inviting me for seminars and workshops, especially thanks to Asger Lau Andersen and CEBI/CAM members.

My friends and family deserve my deepest appreciation for their encouragements over the past years. In particular my sister and parents who never failed to inspire me to do my best—even before my studies. I am deeply indebted to Nina for her unconditional support and for bearing with my absent-minded presence from time to time. For her genuine interest and challenging questions over the actual importance of my work and finally, for reminding me that there is a lot of other great stuff going on outside economics.

*Henrik Yde Andersen*



# Abstract

Money is a scarce resource for most people. For that reason, the decision whether to spend more today and less in the future or vice versa is a recurrent question to many of us. Pension systems provide incentives for saving for future consumption and mortgage markets allow us to increase consumption immediately by giving up future spending opportunities accordingly. For this reason, pension and mortgage systems play a key role to individual savings decisions. This dissertation is comprised by three self-contained chapters concerned with how individual savings behavior depends on the design of certain pension and mortgage features.

Chapter one, "*Do Tax Incentives for Saving in Pension Accounts Cause Debt Accumulation?*", applies a quasi-experimental research design on a Danish 2010 policy that reduced tax incentives for saving in annuity pension schemes to show significant substitution of savings from retirement accounts to gross debt repayments. We find that for every 1 Danish krone reduction in retirement savings 31 cents goes to debt repayments. Taking into account all types of savings, we find full crowding out. Consistent with previous findings, we document that the effect is driven by a minority, about 23 percent, who actively rebalance their savings.

Chapter two, "*Housing Wealth Effects and Mortgage Borrowing*", examines whether home price changes drive mortgage based equity extraction. To do this we use longitudinal survey data with subjective information about current and expected future house prices to calculate unanticipated house price changes. We link this information at the person level to high quality administrative records with information about mortgage borrowing as well as savings in various financial instruments. We find a marginal propensity to extract out of unanticipated housing wealth gains to be 3-5 percent. We find no adjustment to other components of the portfolio, and we find that mortgage extraction leads to an increase in spending. We find no evidence that the effect is driven by collateral constraints. Instead, the effect is driven by about 11 percent of the observations where the respondent is recorded having actively taken out a new mortgage. Three out of four among these refinance an existing fixed rate mortgage loan and exploit that the old loan can be prepaid and a new loan established to lock in a lower market rate. The propensity to extract equity is higher for the group that has an incentive to refinance following a drop in the market interest rate

and at the same time experience an unanticipated housing wealth gain. These results point to the existence of a housing wealth effect that is intimately connected to the functioning of the mortgage market, and this suggests that monetary policy plays an important role in transforming unanticipated housing wealth gains into spending by affecting interest rates on mortgage loans.

Chapter three, "*The Effects of Tax Penalties on Early Withdrawals from Pension Accounts*", use variation from a natural experiment that is plausibly exogenous to other savings decisions to show that reducing the tax penalty rate by 1 percent increases the propensity to withdraw pension assets early by 0.1 percentage points on average. Access to detailed administrative records allows us to show that the effect is two to three times larger for consumers who are likely to be affected by liquidity constraints and individuals who lose their job or divorce. This is consistent with the idea that consumers finance spending with pension assets only when other less costly ways to access liquidity have been exhausted. Conditional on withdrawing pension assets early the amount withdrawn increases by about 3 percent for each one percent reduction in the tax penalty. Only 1/3 of the withdrawals are rolled over to non-retirement savings accounts or used to repay debt. We find that those who cash out pension wealth in the year of a job loss reduce overall savings rates for at least six years after the withdrawal. Ultimately, our evidence suggests that tax penalties are efficient to increase long-term savings.



# Resumé (in Danish)

For de fleste er penge en knap ressource. Derfor er det en tilbagevendende overvejelse for mange, om de ønsker at øge forbruget i dag på bekostning af forbrug i fremtiden – eller omvendt. Pensionssystemer giver forbrugerne et incitament til at spare mere op, mens realkreditinstitutter stiller kredit til rådighed, så folk kan øge forbruget med det samme og dermed sænke forbrugsmulighederne i fremtiden. Pensionsselskaberne og realkreditinstitutterne spiller derfor en central rolle for den enkeltes opsparings- og forbrugsadfærd. Denne afhandling består af tre selvstændige artikler, der handler om, hvordan individual opsparingsadfærd påvirkes af særlige elementer i pensionssystemet og markedet for realkreditlån.

Kapitel et, "*Do Tax Incentives for Saving in Pension Accounts Cause Debt Accumulation?*", anvender et kvasi-eksperimentielt undersøgelsesdesign på en dansk politikændring i 2010, som reducerede skatteincitamentet ved at spare op i ratepensionskonti. Resultaterne viser en signifikant substitutionseffekt mellem opsparing i pensionskonti og en reduktion af bruttogæld. Vi finder, at for hver 1 dansk kroners reduktion i pensionsopsparingen går 31 øre til afdrag på gæld. Når alle opsparingskomponenter medregnes, finder vi fuld fortrængning. I lighed med tidligere resultater i litteraturen dokumenterer vi, at effekten er drevet af omkring 23 procent af opsparerne, som aktivt re-allokerer deres opsparede midler.

Kapitel to, "*Housing Wealth Effects and Mortgage Borrowing*", undersøger, om ændringer i huspriserne kan forklare udtræk af friværdi i boligen. Til dette anvender vi survey med subjektive informationer om nuværende og forventede fremtidige huspriser til at beregne uventede stød til boligprisen. Denne information kombineres med administrative registre på individniveau, som indeholder informationer om realkreditlån samt opsparing i en række andre finansielle instrumenter. Vi finder en marginal tilbøjelighed til at udtrække friværdi på 3–5 procent ud af den uventede boligprisstigning. Der er ingen justeringer på de øvrige opsparingskomponenter, og vi finder, at den øgede realkreditgæld fører til øget forbrug. Der er ingen tegn på, at effekten er drevet af kreditbegrænsninger. Den samlede effekt er drevet af de omkring 11 procent af observationerne, hvor respondenterne aktivt optager et nyt lån. 3/4 af disse refinansierer deres eksisterende, fastforrentede realkreditlån og udnytter, at det eksisterende lån kan blive indfriet ved optag af et nyt lån med lavere effektiv rente. Tilbøjeligheden til at udtrække friværdien er større for gruppen af respondenter, der har et incitament til at omlægge deres eksisterende lån, og som samtidig oplever en uventet stigning i boligpri-

sen. Disse resultater indikerer, at formueeffekten på boligmarkedet er tæt forbundet til den måde, som markedet for realkreditlån fungerer på. Det antyder, at monetær politik, der påvirker renten på realkreditlån, kan spille en vigtig rolle for transformationen af boligformue til forbrug.

Kapitel tre, "*The Effects of Tax Penalties on Early Withdrawals from Pension Accounts*", anvender potentielt eksogen variation fra et naturligt eksperiment til at vise, at tilbøjeligheden til at udtrække pensionsmidler i utide øges med 0,1 procentpoint for hver 1 procent reduktion i strafskatten, der pålægges førtidige udtræk. Adgang til detaljerede administrative data gør det muligt at vise, at effekten er to til tre gange større for individer, der er likviditetsbegrænsede eller oplever jobtab eller skilsmisse. Det er konsistent med ideen om, at folk finansierer deres forbrug med pensionsmidler, hvis de har udtømt øvrige finansieringsmuligheder forinden, fx likvid opsparing, kredit og friværdis. Eksistensen af en genkøbsklausul i pensionsordningen tillader dermed opsparerne i højere grad at håndtere negative indkomst- eller efterspørgselsstød via adgang til likviditet. Vi finder, at opsparingsraten forbliver lav i en årrække efter, at opsparerne udtrækker pensionsmidler i forbindelse med et jobtab – også selvom de hurtigt kommer i beskæftigelse igen.

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# Introduction

This dissertation consists of three self-contained chapters about how individual savings decisions are affected by pension taxation, house price changes and mortgage markets. Each chapter can be read independently but the topics of the papers are indeed overlapping.

Chapter one is concerned with the effect of tax incentives for saving in retirement accounts. There is a large literature in public economics that attempts to sort out whether such tax incentives increase overall individual savings or whether the tax benefits cause savers to shift savings from non-retirement accounts to tax-deferred pension accounts. For example, Bernheim (2002) provides a thorough review of this matter. Theoretically, it is impossible to determine whether the *substitution effect* or *income effect* dominate. The former implies that savers substitute savings from non-retirement accounts to tax-favored pension accounts in order to reap the tax benefit, while their overall savings rates are unchanged. The income effect implies that the tax subsidy enables savers to consume more both today and in the future. This would cause them to reduce overall savings rates. In addition, there is a third channel—an *intertemporal substitution effect*. This implies that savers consider present spending to be more expensive relative to future consumption because the tax subsidy obtained through saving in retirement accounts generates future spending opportunities only. This would cause savers to increase savings rates. Ultimately, whether tax incentives for saving in pension accounts increase total individual savings or whether savings in non-retirement accounts are crowded out by savings in pension accounts is an empirical question. A range of studies have attempted to answer this question using various identification strategies and data sources (see, e.g., Engen *et al.* , 1996; Poterba *et al.* , 1996). Recently, Chetty *et al.* (2014) used population-wide data records and quasi-experimental variation from Denmark to show that tax incentives are ineffective in boosting individual savings rates. Chapter one in this dissertation breaks new ground by decomposing this crowding out effect. The chapter's contribution to the literature is the finding that about 1/3 of the change in pension contributions that was caused by variation in the associated tax benefits is used to repay debt. This implies that tax rules within the pension system might affect accumulation of debt at the individual level.

Chapter two examines the well-known correlation between house prices and consumption. The sizable literature on this topic can be boiled down to three main hypotheses. The

*housing wealth hypothesis*, stating that unanticipated shocks to house prices cause home owners to increase spending. This explanation is based on the notion that consumers seek to smooth consumption over their life time and only reconsider their spending plan when new information arrives, e.g., an unanticipated increase in the price of their home (Campbell & Cocco, 2007; Skinner, 1996; Muellbauer *et al.*, 1990). There is also the *collateral channel hypothesis*, stating that house price increases do not affect spending directly, but improved access to mortgage borrowing relaxes otherwise binding credit constraints that consumers might face (Aladangady, 2017; Browning & Leth-Petersen, 2003; Cooper, 2013; Leth-Petersen, 2010). Finally, there is a *common factor hypothesis*, stating that house prices and consumption are driven by some third factor, e.g. expected income changes (Windsor *et al.*, 2015; Attanasio & Weber, 1994; Attanasio *et al.*, 2009). Using a novel combination of survey data on subjective expectations and public administrative records about individual savings outcomes, chapter two tests the three competing hypotheses against each other. The results show that unanticipated house price increases lead home owners to take up more mortgage debt and increase spending corresponding to 3–5 percent of the gain in the value of the home. The unique features of the data allow us to control for expected income changes. Indicators of binding credit constraints are found not to predict this spending response. However, the findings imply that house price gains translate into increased private spending when the necessary mortgage market conditions are in place. Specifically, when house prices increase unexpectedly, home owners can extract equity by exploiting that market interest rates are lower than when they took out their existing loan. This suggests that monetary policy might affect private spending through the mortgage markets when house prices increase unexpectedly. The contribution to the literature of the chapter is the detailed information about how the wealth effects interact with financial market conditions. Our findings are related to that of Bhutta & Keys (2016), but to the best of our knowledge, this chapter provides a more direct test of the housing wealth and spending relationship unprecedented in the literature.

Chapter three is concerned with disincentives to withdraw pension assets prior to retirement age. A range of papers have sought to clarify the extent to which early withdrawals take place Poterba *et al.* (1998); Poterba & Venti (2001); Engelhardt (2002), while others have shown that pre-retirement withdrawals correlate with liquidity constraint indicators and adverse demographic and labor market shocks (Hurd & Panis, 2006; Amromin & Smith, 2003). Another branch of studies within this topic examines the effects of tax penalties on early withdrawal behavior (Burman *et al.*, 2012, 1999; Chang, 1996), showing that increased tax rates on early withdrawals reduce the propensity to cash out. Chapter three attempts to embrace the predictions made by all these papers. Specifically, we examine whether individuals who suffer from liquidity constraints or adverse life events, e.g., job loss or divorce, respond more strongly to changes in the tax penalties. To test this prediction we utilize

public administrative records and quasi-experimental variation in the tax penalty rate. The empirical design allows us to show that those in financial hardship are more likely to cash out pension assets early when the tax penalty rate is reduced unexpectedly. Moreover, the chapter shows that, conditional on cashing out, withdrawn amounts are significantly larger when the tax price of doing so is reduced. Disregarding the change in tax penalties, 2/3 of the withdrawals were used for immediate consumption. The chapter contributes to our understanding of whether pension systems should include buy back clauses that allow pension owners to cope with unanticipated income or demands shocks.

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# Chapter 1

## Do Tax Incentives for Saving in Pension Accounts Cause Debt Accumulation?

A version of this chapter has been accepted for publication in *European Economic Review*.



# Do Tax Incentives for Saving in Pension Accounts Cause Debt Accumulation? Evidence from Danish Register Data

Henrik Yde Andersen\*

## Abstract

This paper applies a quasi-experimental research design on a Danish 2010 policy that reduced tax incentives for saving in annuity pension schemes to show significant substitution of savings from retirement accounts to gross debt repayments. We find that for every 1 Danish krone reduction in retirement savings 31 cents goes to debt repayments. Taking into account all types of savings, we find full crowding out. Consistent with previous findings, we document that the effect is driven by a minority, about 23 percent, who actively rebalance their savings.

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## 1 Introduction

Tax-favoured pension accounts have attracted attention over the years because of their importance to individual savings behaviour. Many developed countries use tax subsidies to affect individual saving rates and economists strive to determine the outcomes of such policies. Recent empirical work suggests that savings in tax-favoured retirement accounts are fully crowded out (Chetty et al., 2014). It is less clear, however, whether savings in pension accounts are crowded out by savings in non-retirement accounts or debt repayments. Imagine that tax incentives for saving in pension schemes are reduced from one day to the next and taxpayers respond by shifting savings from retirement accounts to the now-highest after-tax-return account. Given that debt usually carries higher interests than savings, outstanding debt should be repaid before saving in non-retirement accounts. A growing interest in the development of household debt calls for evidence-based insights into the link between retirement savings and gross debt accumulation. Studies based on household-level data have shown that highly leveraged households tend to cut spending more than their less leveraged peers during economic downturns (Mian and Sufi, 2010; Mian et al., 2013). Also, economic growth and macroeconomic stability seem to be negatively correlated with household debt (Cecchetti et al., 2011; Eggertsson and Krugman, 2012; Jorda et al., 2013). Bank debt and mortgages in the household sector might play an important role both in macroeconomic outcomes and when estimating crowd-out in retirement savings.

This paper revisits crowd-out in tax-favoured retirement savings but uses novel population-wide data to split the crowd-out effects between individual savings and debt accounts. Access to longitudinal information from Danish tax authorities and mortgage institutions makes it possible to cover the full financial balance sheet at the individual level. Mortgages comprise the largest financial liability in households and, to our knowledge, this is the first contribution to the crowd-out literature to include all household debt accounts in a panel dimension. Combined with public administration registers, the data have the advantage of providing many observations and objective information about individual wealth and personal characteristics. A Danish 2010 tax reform provides exogenous variation to the tax incentive for saving in pension schemes as it introduced a deductions threshold for contributions to annuity pension schemes. Using the introduced tax deduction threshold as the cutoff, a difference-in-differences estimator is applied in a quasi-experimental research design.

Increased availability of longitudinal information on individual saving accounts have made it possible to show that total net savings at the individual level are likely to be unaffected when reducing tax incentives for saving in pension accounts. This is demonstrated in Chetty et al. (2014) who use a large panel with third-party reported information to show that individuals simply shift savings from tax deductible retirement accounts to taxable saving accounts. The importance of the panel dimension is addressed in Gelber (2011) as

individuals might have unobserved preferences for saving, which is possibly confounded by the savings response that the econometrician wants to identify. Individuals with higher unobserved tastes for saving might more often choose tax-favoured, illiquid retirement accounts simply because of their strong preferences for saving and not due to the tax incentive itself. The two aforementioned studies have contributed to a large literature in public economics that for decades has sought to determine the effects of tax subsidies on savings. Bernheim (2002) thoroughly reviews the ambiguous findings in this literature, e.g. Skinner and Feenberg (1990); Venti and Wise (1990) who use consumer and expenditure surveys to show that savings in tax-favoured pension accounts represent new savings. This implies that individuals reduce consumption and increase savings because of the tax incentive. Similar results are supported by Hubbard (1984); Poterba et al. (1995, 1996); Hubbard and Skinner (1996); İmrohoroğlu et al. (1998). Contrary to this, Gale and Scholz (1994) use a different set of econometric assumptions to show that increased savings in retirement accounts are crowded out by decreased savings in non-retirement accounts. Their findings are supported by Engen et al. (1996); Gale (1998); Attanasio and DeLeire (2002); Attanasio and Rohwedder (2003); Benjamin (2003); Engelhardt and Kumar (2007). Most recently, Chetty et al. (2014) attempt to explain the ambiguities in the literature by identifying two very different types of economic agents, namely active and passive savers. Active savers respond to changes in taxation rules and re-optimize consumption and saving decisions according to the lifecycle model. Passive savers do not respond to incentives but tend to make consumption choices based only on their disposable income. The distinction between active and passive savers becomes essential when measuring outcomes of retirement policies. Tax credits for saving in pension schemes would have no effect on passive savers because they require individuals to make active decisions and adjust their savings. Unlike this, automatic enrolment policies would increase retirement savings for passive savers, while active savers might manually opt out (Madrian and Shea, 2001; Choi et al., 2009). All the studies mentioned have made important contributions to our understanding of tax incentives and their effect on individual savings. However, it is well-known that our knowledge is limited when it comes to the interplay between crowd-out in retirement savings and debt accumulation.

Standard lifecycle models predict that reduced tax incentives for saving in pension accounts affect pension contributions through both a price and a wealth channel. The price channel implies that retirement savings decrease when reducing tax incentives for saving in pension schemes because returns on pension savings decrease relative to returns on savings in non-retirement accounts. Also, individuals prefer to substitute consumption intertemporally, i.e. people prefer to consume more today than tomorrow. The wealth channel works in the opposite direction. Individuals perceive themselves less wealthy when tax incentives for saving are reduced. In order to smooth consumption over their lifetime they increase retirement savings. It is broadly acknowledged that the price channel dominates the wealth

channel (Duflo et al., 2006; Engelhardt and Kumar, 2007). This means that the price channel—the substitution effect—can be estimated by comparing two types of individuals with similar saving preferences but only one of them is affected by reduced tax incentives for saving. This paper does exactly this by identifying reduced pension contributions as tax incentives for saving in retirement schemes are reduced by the government. The main outcomes of interest are whether the reform increased other types of savings and in particular whether the reform increased debt repayments. Debt carries a higher interest rate than savings, which implies that any outstanding debt should be repaid before non-retirement savings are accumulated. Moreover, the most expensive debt should be repaid first. The availability of debt repayment information makes this analysis particularly valuable as we can test these predictions.

This study stands out for two reasons. First, it documents that, when reducing tax incentives for saving in retirement accounts, gross debt is reduced by 31 cents for every 1 unit of Danish currency, Danish Krone (DKK), that retirement savings decrease. This represents by far repayments of expensive debt in banks and to a lesser extent repayment of debt in mortgage institutions. Knowing that taxpayers actually do manipulate their debt when tax incentives for saving in pension schemes are changed is essential when assessing the overall outcomes of such policies. The second contribution is to confirm the results of the recent empirical literature by utilising exogenous variation from a new tax policy on comprehensive individual level data. By using a Danish 2010 tax reform this paper documents full crowd-out in retirement savings and find that only 23 percent of individuals respond actively to tax incentives. Chetty et al. (2014) use a Danish 1999 tax reform in a very different quasi-experimental setting to show almost identical results. The fact that similar results can be produced by two different research designs, using two very different tax reforms that were implemented more than a decade apart, underlines the robustness of the empirical evidence. Analysing a policy change, which targeted only a part of the population—those relatively high in the income distribution, implies that our findings cannot necessarily be extrapolated directly to the broader population, a limitation applicable to any empirical paper estimating causal impacts using quasi-experimental methods. Mean gross income for the Danish population is about DKK 300,000. We find full crowd-out for a subgroup of individuals with mean gross income of about DKK 670,000. Chetty et. al (2014) find similar results for a different policy targeting people at a lower level of income (around DKK 308,000). This is suggestive evidence that savings in pension schemes are fully crowded out for individuals in the upper half of the income distribution. Our analysis on heterogeneity indicates that those who responded actively to the reform are well educated and less exposed to unemployment compared to those who did not react to the rule change.

The next section introduces the Danish institutional setting and carefully explains the policy reform and data. Section 3 presents the empirical identification strategy, estimated



substitution effects and the robustness of the empirical results. In section 4, the share of active savers is estimated, showing heterogeneity on both observables and policy responses, while section 5 concludes.

## 2 Institutional Setting and Policy Reform

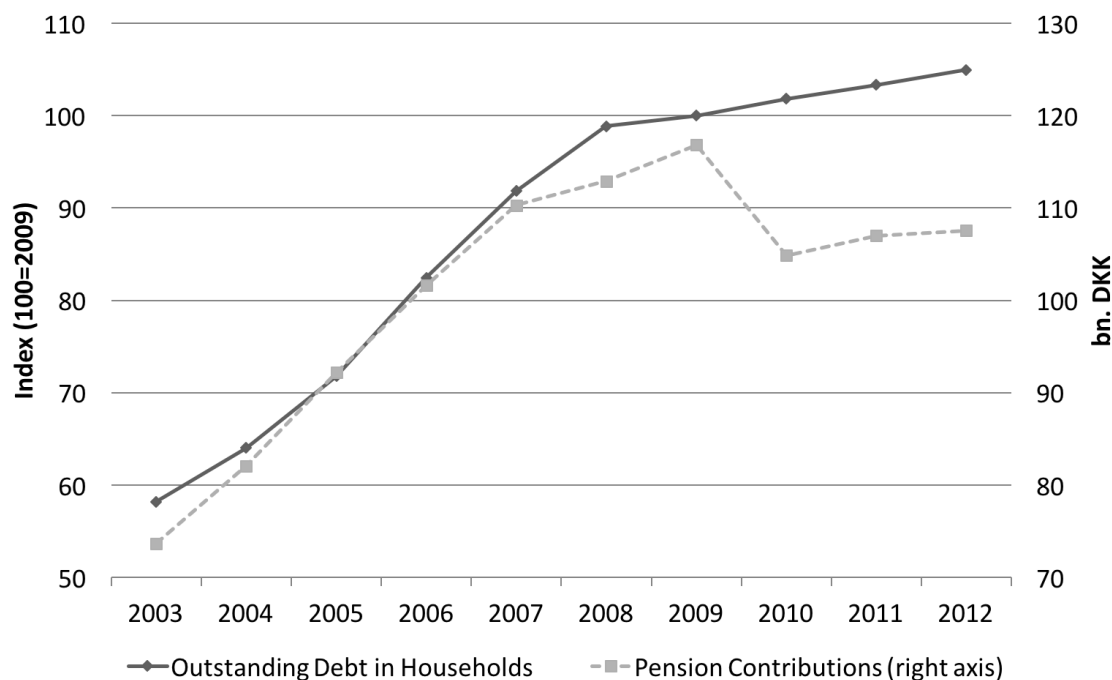
This section provides an overview of the Danish pension and mortgage system followed by an explanation of the policy reform that provides exogenous variation to savings behaviour in the research setup.

The Danish pension system is comparable to most retirement systems in developed countries. It has three pillars consisting of a state-provided defined benefit scheme (DB), occupational defined contribution schemes (DC) and voluntary pension savings accounts (DC). This setup is analogous to the US retirement savings system, reflecting Social Security, 401(k)s and IRAs, respectively. Within the second and third pillar, the Danish retirement system offers three types of DC pension schemes: annuity, capital and life-long schemes. Contributions for all schemes are tax deductible, but they differ in pay-out profile and taxation. The annuity scheme is paid out in annuities during a final time span of 10-25 years and payments are taxed as regular income. The capital scheme is paid out as a lump-sum and taxed at 40 percent. The life-long scheme is paid out in annuities and taxed as regular income, but pay-out continues until the owner dies. Second pillar contributions are generally set through collective bargaining agreements between employers' associations and workers' unions. Employers contribute to all three types of schemes, constituting more than 90 percent of total pension contributions in 2009. Second pillar contributions are mandatory but the employees do, however, have some decision power over the exact amount. This implies that the employees can ask the employer to increase or decrease occupational contributions to a certain extent. Third pillar contributions are completely voluntary. The sum of employer-paid and individual contributions to capital pension schemes is tax deductible up to a certain limit. This limit increases over time and amounted to DKK 46,000 (US \$7,000) in 2009. At that time, which is prior to the reform investigated in this paper, no subsidy thresholds existed for annuity and life-long schemes.

The dotted line in Figure 1 plots total pension contributions in nominal terms across years. Clearly, overall contributions in the economy declined in 2010—the year of the policy change that this paper examines. Before that, contributions had increased by a constant rate apart from a smaller reduction around the outbreak of the financial crisis. The 2010 decline is likely to be caused by the reform but other factors could also play a part, e.g. economic cycles and heterogenous responses to the post-recession recovery. One takeaway from Figure 1 is that the majority of taxpayers are likely to have reduced pension contributions. This paper attempts to identify the effects of one particular element of the reform, namely an introduction of a contribution limit up until tax deductions are granted, effectively reducing

tax-incentives for saving in pension accounts. This is explained in detail in the next section.

Figure 1: Total Household Debt and Pension Contributions



Note: Outstanding debt in households covers all debt in banks and mortgage institutions. Pension contributions are aggregate contributions recorded in each year.

Source: Danmarks Nationalbank and Danish Insurance Association.

The Danish mortgage system is funded using covered bonds like in most continental European countries. However, similar to the US system, Danish mortgages offer long-term fixed-rate mortgages without prepayment penalties. This ensures a flexible market for borrowers, who can always exit their loan by buying back the underlying bonds at face or market value, depending on which price is lower. Andersen et al. (2015) provide a detailed description of the Danish mortgage market and point out that borrowers have minimal barriers to refinance existing loans, even if they have negative home equity. Refinancing the loan is preferable if borrowers wish to adjust annual repayments or maturities or benefit from a decline in market yields. Most importantly in the context of this paper, such refinancing does not require a review of the borrower's credit quality. Once the mortgage loan is granted the borrower has room to adjust the loan characteristics. Collateralized mortgage loans carry a lower interest rate than credit in banks, but interest payments on both debt types are tax deductible by approximately one-third of the payments. The solid line in Figure 1 plots an index of total household debt across years. Up until 2008 household debt had increased by a constant rate, which was reduced dramatically around the years of the 2008 recession. The

interesting question is to what extent debt accumulation was affected by the sharp change in pension contributions. Had household debt increased more than was the case if pension contributions had not declined in 2010?

## 2.1 Pension Tax Reform

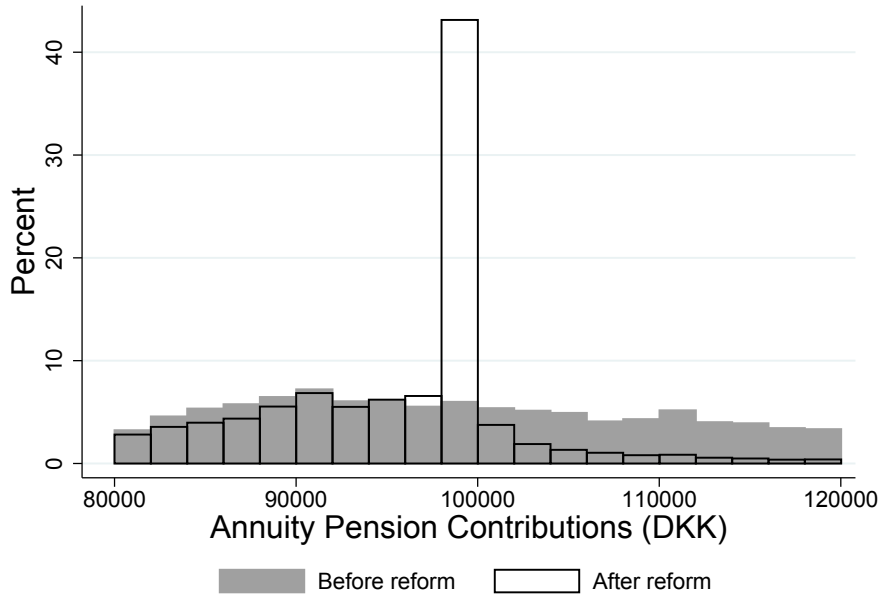
A Danish 2010 tax reform introduced a tax subsidy limit on contributions to annuity pension schemes. This reform implied that the sum of employer-paid and individual contributions to annuity pension schemes was tax-deductible only up to DKK 100,000 (US \$15,000). This sharp change in taxation rules on pension savings provides exogenous variation to annuity pension contributions and is ideal for a quasi-experimental research design. Individuals who intended to save more than DKK 100,000 in annuity pension accounts in 2010 experienced a reduction in tax incentives for saving in retirement accounts. Given that they paid more than this amount in the years up to the reform and given that they had no intention of changing their contribution rates, they experienced a reduced tax deduction from 2010 and onwards. Conditional on this fact and conditional on year and individual fixed effects, variation in annuity pension contributions is considered exogenous.

Measuring the reform effect relies on the fact that the public was aware of the rule change. We provide two sources of evidence that attention to pension-related information increased after the announcement of the reform. First, web searches of the word "pension" increased three to four times in the reform announcement year, 2009, compared to previous years—particularly in March, which is when the majority of the members of parliament agreed to the reform, and May, which is when the bill was proposed formally. Second, nationwide newspapers published more than three times more articles on pension matters in the announcement year compared to preceding years. Figures on web searches and newspaper articles can be found in the appendix. The change in tax incentives was passed by parliament as a permanent rule change and the public had no reason to believe otherwise.

Using the introduced subsidy threshold, a subsample for further analysis is drawn. This subsample includes individuals who contributed close to DKK 100,000 in annuity pension accounts in 2008—that is two years prior to implementation of the reform and one year prior to the announcement of the reform. Individuals with DKK 80,000-150,000 are included in the sample and the robustness section shows that variations to this assignment window does not change the results significantly. Individuals above the DKK 100,000-threshold are assigned for treatment, while those below are assigned as non-treated. Figure 2 is a histogram of annuity pension contributions close to the DKK 100,000-threshold for two different years. The darker bars show the distribution in the year right before implementation of the reform, while the lighter bars illustrate that of 2010. The darker bars have a smooth distribution around the DKK 100,000-threshold in the pre-reform period, while bunching close to the threshold is clearly observed after the reform was implemented. This suggests that the

sample did not anticipate the reform and paid no particular attention to contributions of DKK 100,000 prior to the reform. In the empirical part of the paper we show that other changes to taxation in the reform did not seem to drive our findings.

Figure 2: Histogram of Annuity Pension Contributions



Note: Individuals are grouped in equal sized bins by annuity pension contributions. The darker bars represent the distribution immediately before the reform was implemented, while the lighter bars represent the distribution of pension contributions in 2010.

Source: Own calculations based on administrative data from Statistics Denmark.

Standard lifecycle models predict that individuals allocate savings wherever the after-tax return is higher. The theory predicts that taxpayers respond to changes to the after-tax return by re-allocating their saving portfolio. We test this proposition directly by measuring substitution between available saving accounts when the after-tax return on pension savings declines. Assuming that debt carries a higher after-tax interest than savings, debt should be repaid before taxpayers accumulated non-retirement savings. We lack information on the exact after-tax return on every asset and liability but we do have information on how much each account type is changed. The saver would not avoid the contribution subsidy ceiling by substituting savings between second and third pillar pension schemes because the ceiling applies to the sum of contributions to employer and private accounts. Substitution between scheme types would, on the other hand, allow the saver to avoid the tax ceiling. The following section provides more details on the data available.

## 2.2 Data

Panel data from Statistics Denmark and the Danish mortgage institutions are merged using anonymised personal identifiers that cover everyone residing in Denmark. The time period

is 2003–2013 for the majority of the variables. Data on mortgage information covers 2009–2013 only. The estimation sample consists of individuals with annuity pension contributions of DKK 80,000–150,000 in 2008 as described in the previous section. The self-employed including spouse are excluded because they were not fully subject to the changed tax rules that this paper investigates. As we show in the appendix, however, including pre-reform self-employed individuals does not change our results significantly. Individuals aged 60 or above are excluded because they are eligible for early retirement schemes. Finally, people not fully liable to taxation in Denmark are excluded. Changes in non-retirement saving and debt accounts are censored at the 1st and 99th percentile in order to reduce noise from extreme observations.

The estimation sample is not representative for the full Danish population. We include individuals in the estimation sample who contribute about DKK 100,000 to annuity pension schemes each year (see Table 6). The full sample pension contribution average is about DKK 30,000. Similarly, income also differs between the two samples, implying that our results confine to savers in the upper part of the income distribution as noted in the introduction. Further details on characteristics within the estimation sample can be found in the appendix Table 7. The sample used in our estimations covers 56,372 individuals over the period 2003–2013, providing an unbalanced panel of 599,744 observations. The Danish tax authorities provide information on saving accounts, pension contributions and income. This information is based on annual reports from financial intermediaries, which ensures a low risk of measurement error and no risk of self-report bias. Individual saving and debt information are reported each year by third parties, i.e. banks and mortgage institutions, to the Danish tax authorities. This reporting is made compulsory by Danish financial regulation law, leaving no space for selection into or out of the data sample. Mortgage loan information is provided directly from mortgage institutions. Noise in the data can still arise given that flow variables are calculated as year-on-year changes in stock variables. By using this approach annual variations in price and quantity measures cannot be separated. This paper attempts to identify quantity changes because these reflect actual saving decisions made actively by individuals, i.e. shifts of savings from one account to another. Price changes—e.g. returns from financial assets—constitute the noise that is filtered out in the empirical model. This challenge is, however, evident to any researcher that analyses savings behaviour empirically. Normalised by last year’s income, the mean savings rate in the estimation sample is 8.5 percent in 2009 with a standard deviation of 39.6 percent. When including only individuals with zero stock of financial assets one year earlier, the standard deviation is reduced to 35 percent. This indicates that the price channel accounts for only a minor part of the between-person variation in savings rates and should not be a major concern in this study. However, this is addressed further in the following section on quantifying the effects. It is essential to the analysis that the treated and non-treated groups had

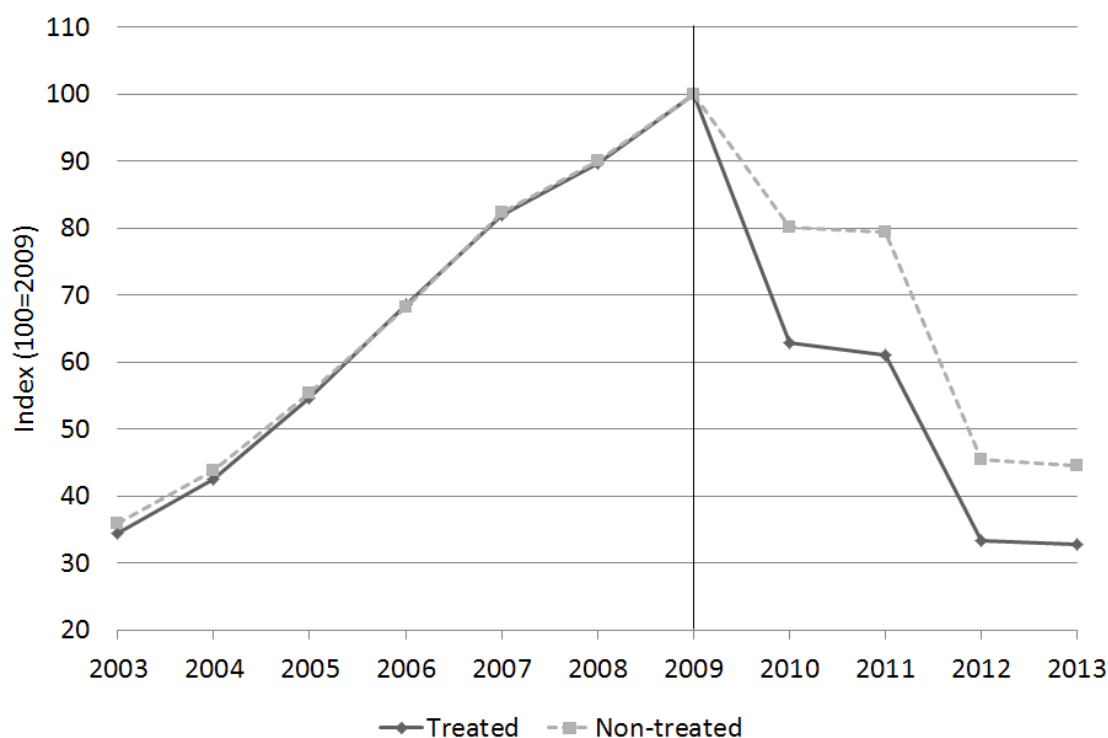
common savings behaviour prior to the reform. This matter is addressed thoroughly in the following section.

### 3 Measuring Substitution Effects

The empirical challenge is to quantify the reform impact on individual saving outcomes. Using the shock to saving decisions caused by the policy reform, a difference-in-differences estimator is set up to capture substitution of savings between saving and debt accounts.

Saving cashflows of the treatment group who were expected to change behaviour because of the pension tax reform are compared to cashflow of individuals in the assigned control group who were not expected to change behaviour. The treated and the non-treated groups are assigned one year prior to the reform announcement, which ensures no self-selection bias. The crucial assumption is that the treated and non-treated groups exhibited common trends in annuity pension contributions prior to the reform being implemented. Figure 3

Figure 3: Annuity Pension Contributions

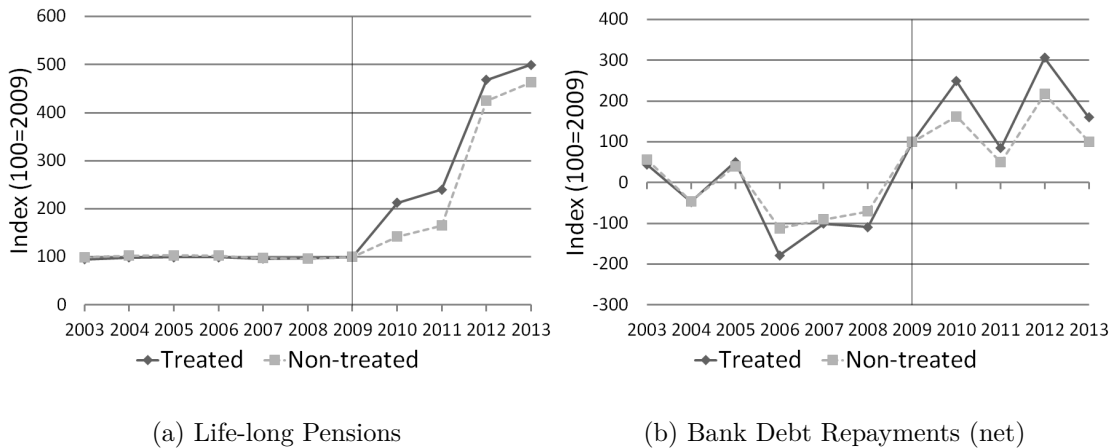


Note: Average contributions for annuity schemes are calculated within each year for the treated and non-treated groups. For each group, contributions are then indexed to 100 in 2009. Treatment individuals contributed DKK 100,001–150,000 to annuity schemes two years prior to the 2010 tax reform, while the non-treated individuals contributed DKK 80,000–100,000. Source: Own calculations based on administrative data from Statistics Denmark.

illustrates that annuity pension contributions were almost identical for the two groups in the pre-reform period. Therefore, by graphical inspection, we argue that the identifying

assumption is not violated. Specifically, the two groups are comparable and differ only in annual contributions for annuity pensions, while other saving preferences are alike. The sufficient identifying assumption is parallel pre-trends in the outcome variables, implying that changes in savings outcomes are similar for the treatment and control groups had they not been treated. We do not assume complete quasi-random assignment into the groups, which would be a stronger assumption than necessary in our design. For the same reasons we emphasize the importance of common pre-trends. The reform was implemented in 2010 and both the treated and non-treated groups reduced annuity pension contributions instantly. This observation is consistent with an overall decline in pension contributions that was observed for the whole population (see Figure 1). However, the treated group reduced contributions for annuity schemes much more than the non-treated group, indicating that the empirical design does in fact capture the reform effects. There exist no natural allocation of individuals into treatment and control groups. Our allocation could very well generate the decline in annuity pension contributions by the control group after reform implementation. We elaborate on this and test the implications for our results in the robustness section. Similar graphical inspection of pre-trends is performed in all saving and debt accounts that are examined. Life-long pension contributions in Figure 4a show

Figure 4: Alternative Saving Accounts



Note: See Figure 3.

Source: Own calculations based on administrative data from Statistics Denmark.

very similar trends prior to the reform, while the treated group increased savings in this account more than the non-treated group after the reform was implemented. The same applies to bank debt repayments (net of bank deposits) in Figure 4b despite being much more volatile than changes in retirement accounts. Graphical inspection of developments in capital pension schemes and financial assets is omitted because of very low savings and almost no substitution effects in these accounts. Mortgage institutions provide information

on annual repayments from 2009 only. With only one pre-reform year, inspection of pre-trends cannot be performed in this variable. However, interest payments on mortgages are collected by the tax authorities for the full pre-reform period. Figure 10b shows that the treated and non-treated had almost identical trends in mortgage interest payments prior to the reform, which is a good indication that their use of mortgage loans was also identical.

A standard difference-in-differences setup is developed to estimate shifts between saving accounts. The estimation is performed in two steps. The first step identifies the reform impact on annuity pension contributions. The second step measures substitution from annuity pension accounts to alternative saving and debt accounts.

$$P_{i,t} = \alpha_i + \Omega_t + Treat_i + \delta Treat_i \times Post_{i,t} + X_{i,t-2} + \varepsilon_{i,t} \quad (1)$$

In this first step,  $P_{i,t}$  is annual contributions for annuity pension schemes. On the right-hand side  $\alpha_i$  captures individual time-invariant effects. This includes individual tastes for savings as explained in Gelber (2011). Year fixed effects are captured by  $\Omega_t$ , which include macroeconomic developments that are common to all individuals in the sample, e.g. returns from financial markets.  $X_{i,t-2}$  is a vector of lagged values of control variables. The vector includes income, age, work tenure, marital status, a dummy for being divorced within 1 year, a dummy for being divorced within 2 years and years since individual  $i$  last changed address. Finally, housing wealth is controlled for. Lagged housing wealth could be correlated with the borrower's future mortgage payment profile, which would lead to housing wealth being endogenous. Omitting lagged housing wealth from the equation does not, however, change our results (see appendix Table 4).  $Treat_i$  is an indicator of individual  $i$  being in the treatment group, while  $Post_{i,t}$  is an indicator that takes the value 1 in all years after implementation of the reform. This allows the policy response to be measured over all post-reform years. In the robustness section, it is shown, however, that individuals tend to respond immediately in 2010. The parameter of interest is  $\delta$  as it measures the nominal change in annuity pension contributions for the treated relative to the non-treated group in the post-reform period. The identifying assumption is that  $Treat_i \times Post_{i,t}$  is not correlated with the idiosyncratic error term,  $\varepsilon_{i,t}$ . It follows from the graphical inspection of pre-reform annuity pension contributions that this assumption is not violated as the treated and non-treated groups showed common pre-reform trends. Following Bertrand et al. (2004), standard errors are clustered on the individual level. Serial correlation is a potential threat in our specification because savings outcomes are unlikely to be independent across time for each person. Clustering the observations reduces the risk of inconsistent standard errors following from autocorrelated errors. Further, as a robustness check, we collapse the pre and post reform years. The estimates do not change significantly. This is reported in the appendix Table 4. The point estimate of  $\delta$  is presented in Table 2 column 1 and shown to



be statistically significant with  $p < .001$ . In the second stage a regression almost identical to the one just presented is set up.

$$Z_{i,t} = \alpha_i + \Omega_t + Treat_i + \gamma \hat{P}_{i,t} + X_{i,t-2} + r_{i,t} \quad (2)$$

The dependent variable,  $Z_{i,t}$ , is either life-long or capital pension contributions, while the explanatory variable,  $\hat{P}_{i,t}$ , is annuity pension contributions. Other specifications are similar to equation (1). The obvious endogeneity problem in equation (2) is that the size of annuity, life-long and capital pension contributions are decided simultaneously by individual  $i$ , meaning that  $\gamma$  cannot be estimated consistently. To overcome this problem,  $Treat_i \times Post_{i,t}$  is used as an instrument for  $P_{i,t}$ . The first stage showed that this instrument is strongly correlated with the regressor and the graphical inspections of pre-trends showed that the instrument is not correlated with some common third factor. Based on this, substitutions from annuity pension schemes to life-long or capital pension schemes are estimated consistently in  $\gamma$ . Estimates are retrieved using a 2SLS-approach in order to obtain correct standard errors that take account of the generated regressors problem. This allows us to do inference. Retirement savings are measured before taxes, while non-retirement savings are measured after taxes are paid. To take account for this a mean tax rate  $\tau_i$  is calculated for each individual  $i$ . Provided with information on total taxes paid and taxable income from the tax authorities we proxy  $\tau_i$  by dividing these two numbers. The after-tax measure of pension contributions is simply  $P_{i,t}(1 - \tau_i)$ , where  $\tau_i$  is fixed to the 2008-level.

$$S_{i,t} = \alpha_i + \Omega_t + Treat_i + \gamma \hat{P}_{i,t}(1 - \tau_i) + X_{i,t-2} + r_{i,t} \quad (3)$$

Shifts of savings from annuity pension schemes to savings in non-retirement accounts, including debt repayments, are estimated in equation (3).  $S_{i,t}$  is either mortgage debt repayments, bank debt repayments, bank deposits or savings in financial assets.  $\hat{P}_{i,t}(1 - \tau_i)$  is annuity pension contributions measured after taxes and  $\gamma$  is estimated consistently with  $Treat_i \times Post_{i,t}$  as an instrument in a 2SLS model. Other specifications follow those explained above.

All substitution estimates captured by  $\gamma$  are reported in Table 1. For a 1 unit reduction in annuity pension contributions—the units being DKK—the table shows changes in alternative saving accounts caused by the pension tax reform. When reducing annuity pension contributions by DKK 1 almost 57 cents is shifted to life-long pension accounts, while less than 1 cent is substituted for the capital pension scheme. This implies that the life-long scheme was considered the closest substitute for the annuity scheme, while  $1 - (57 + 1) = 42$  cents exited the pension system completely. Of these 42 cents, just above 2 cents went to repayment of mortgage debt, while 29 cents was used to repay gross debt in banks. Based on these two estimates,  $2 + 29 = 31$  cents of each DKK 1 reduction in annuity pension

contributions was used for gross debt reduction. Finally, 15 cents was shifted to bank deposits and 4 cents was shifted to financial assets. Of all these estimates, only the latter is statistically insignificant. The sum of all substitution estimates is DKK 1.08, reflecting the total increase in alternative financial accounts for each DKK 1 reduction in annuity pension contributions. By omitting substitution for financial assets, which is estimated imprecisely, the total crowd-out effect is DKK 1, i.e. full crowd-out. This evidence suggests that reducing tax incentives for saving in retirement accounts made the affected individuals shift savings from pension accounts to non-retirement accounts and debt repayments. The substitution pattern does not change significantly when normalising outcome variables using lagged income (see appendix Table 4). This is supported by the fact that income develops similarly for the treatment and control groups across the reform period, which is also shown in the appendix.

To be certain that other factors do not drive the estimates, the power of the panel data is used to control for observable differences between the treated and non-treated groups. First, geographical region of residence is interacted with year indicators. This allows for different time trends in the five Danish geographical regions, capturing potential diverging housing market or labour market developments. Table 1 column 2 shows only marginal changes in the main findings. Second, changes in the progressive nature of the Danish income taxation that were introduced at the same point in time as the DKK 100,000-threshold, that we analyse, is addressed. Prior to the reform, two progressive tax brackets existed, namely the middle tax bracket and the top tax bracket. The middle tax bracket was removed and the top tax bracket was increased in 2010, which potentially could affect our measurements. Income tax brackets can be relevant for incentives to save in tax-favoured pension accounts because taxable income is reduced when pension contributions are increased. This reform element is expected to be less important in our setup because this paper analyses individuals high in the income distribution. To test whether the change in income tax brackets affects the results, a set of indicator variables is included. An indicator that takes the value 1 for individuals who, prior to the reform, had income just below the middle income tax brackets is generated. Next, this indicator is interacted with year dummies. This allows individuals with less than middle bracket income to have their own trend in the outcome that we attempt to measure after implementation of the reform. A similar indicator-interaction term is included for the top tax bracket. Also, educational level indicators are included as proxies for financial literacy. The educational level measures divide individuals into 6 groups based on their maximum level of completed educational training, including primary school, secondary school, vocational training and finally, 2-3, 3-4<sup>1/2</sup> or 5-6 years of tertiary education. This observable characteristic is expected to correlate with financial literacy, implying that individuals with more educational training are more likely to optimise their financial situation (Lusardi and Mitchell, 2014; Lusardi and Tufano, 2015), i.e. to respond

to changes in income tax brackets. Educational indicators are also interacted with year dummies. Table 1 columns 3-4 report our main results including indicator-interaction terms, showing almost identical results. We have also estimated equations (1)–(3) by OLS. The results (not reported) were very similar, and this suggests that the policy quasi-randomises in the vicinity of the cut-off. This claim hinges on the assumption that inherent savings propensities are approximately constant over the observation period.

Table 1: Crowd-out when Reducing Annuity Pensions by 1 Unit

Dep. var.:	Expl. var.: Annuity Pensions			
	(1)	(2)	(3)	(4)
Life-long Pensions	.567*** (.016)	.566*** (.016)	.560*** (.016)	.559*** (.016)
Capital Pensions	.007** (.003)	.007** (.003)	.006* (.003)	.006* (.003)
Mortgage Repayments	.024*** (.006)	.023*** (.006)	.021*** (.006)	.020*** (.006)
Bank Debt Repayments	.294*** (.058)	.291*** (.058)	.303*** (.058)	.302*** (.058)
Bank Deposits	.150*** (.050)	.146*** (.050)	.137*** (.050)	.137*** (.050)
Financial Assets	.042 (.036)	.041 (.036)	.031 (.036)	.030 (.036)
Total Crowd-out	1.084	1.074	1.058	1.054
Geographical Region $\times$ Year	-	Yes	Yes	Yes
Educational Level $\times$ Year	-	-	Yes	Yes
Medium Tax Bracket $\times$ Year	-	-	-	Yes
Top Tax Bracket $\times$ Year	-	-	-	Yes

Note: Significance levels 1%, 5% and 10% are reported as \*\*\*, \*\* and \*, respectively. All columns include lagged control variables, individual fixed effects and year fixed effects for 599,744 observations. Standard errors in parentheses are clustered on the individual level. Total Crowd-out is the sum of point estimates in each column. Educational Level captures individual  $i$ 's educational level prior to reform announcement as discrete values 1-6 for primary and secondary school, vocational training, short, medium and higher education, respectively. Educational Level is interacted with year dummies, allowing for different post-reform trends for each educational type. Medium Tax Bracket and Top Tax Bracket are dummies taking value 1 for individuals who had taxable income prior to the reform corresponding to not paying medium and top taxes, respectively. Each dummy is interacted with year dummies, allowing for different post-reform trends.

Source: Own calculations based on administrative data from Statistics Denmark.

Gale (1998) provides a review of empirical evidence and places prior results in three groups; (1) no offset at all (Cagan, 1965; Katona, 1966; Kotlikoff, 1979; Venti and Wise, 1990), (2) offsets of 20 percent (Diamond and Hausman, 1984; Hubbard, 1986) and (3) substantial offsets of 50–60 percent (Munnell, 1976; Dicks-Mireaux and King, 1984). Gale (1998) finds that pension savings offset 77 percent of savings in non-retirement accounts—an estimate not significantly different from 100 percent, however. Together with our study this

is supported by a more recent paper by Chetty et al. (2014), who provide empirical evidence of 99 percent offset. The administrative data that we use has a number of benefits. First, they hold many more observations compared to recent studies where surveys constitute the data source. Second, our data are third-party reported as opposed to surveys in which the information is self-reported. Third, we exploit the panel dimension, whereas earlier studies mainly rely on cross-sections, and finally, we have information on the full financial portfolio (except for cash and luxury items, e.g. art and yachts) and are able to split net wealth into bank credit, mortgage debt, savings and pension accounts. The research design developed for this paper is based on measuring the effects of introducing a tax subsidy ceiling on pension contributions. This implies that our findings are specific to this type of policy change. The results do not necessarily provide information on how savers respond to the removal of such a tax subsidy ceiling or an increase in tax incentives.

### 3.1 Robustness

This section provides a series of robustness tests, covering potential mean reversion, sample selection, housing wealth and income developments. Historic contributions to annuity pension accounts are used when forming the treatment and control groups. This raises a central concern in the empirical strategy—that contributions across years could be mean reverting. Mean reversion implies that individuals who increase contributions exceptionally in one specific year could have smaller contributions in later years. In this setup the findings could reflect a mechanical effect of individuals who reduce annuity pension contributions in the reform year because they contributed exceptionally large amounts in the year in which they are assigned into treatment. In this case, the estimated reform effects would have nothing to do with the reform itself. It is tested whether mean reversion is a problem in this paper by applying a well-known test in the empirical literature, namely the placebo test approach. Specifically, the empirical model is estimated in years with no reform, i.e. placebo reforms. Should any of these placebo reforms show significant substitution it is likely that the measured effects in our true model are not uniquely identifying the policy effect. Recall that individuals are assigned into treatment or control in 2008, while their reform response is measured after implementation of the reform—that is in 2010 and onwards. In the placebo test this setup is shifted backwards in time such that saving responses are measured in 2008, 2007, 2006 and 2005, i.e. years completely unaffected by the reform. The model in equation (1) is run for all these placebo-reform years and the results are presented in Table 2. Column 1 shows an estimated reduction in annuity pension contributions of DKK 21,038 in the actual reform year. In columns 2-5, we show estimates of placebo-reforms in 2008, 2007, 2006 and 2005, respectively. Estimates of the placebo reforms are close to zero except for the 2005 parameter, which is significant with DKK 1,280. However, these estimates are strong evidence that mean reversion is not driving our findings as the reduction in annuity

scheme contributions is unique to the reform year. The 2005 parameter could be statistically significant only because some individuals by coincidence are on the wrong side of the cutoff in the assignment year compared to their usual contribution level. By applying the so-called donut-hole around the DKK 100,000-threshold, meaning that we exclude individuals in the very vicinity of the threshold in 2008, we obtain the estimates in the second row of Table 2. Specifically, individuals with annuity pension contributions of DKK 90,000-110,000 in 2008 are excluded. This robustness test makes it possible to abstract from the fact that some individuals usually are very close to the threshold, and by coincidence could be just above or just below the threshold. This latter test makes the significant estimate in 2005 disappear, indicating that individuals very close to the threshold accounted for the measured effect in that year. The take-away from the placebo test is that the reduction in annuity pension contributions is large and statistically significant in 2010 only, implying that the empirical setup captures the effects from the policy change rather than mean reversion effects.

Table 2: Placebo Reform Impact on Annuity Pension Contributions

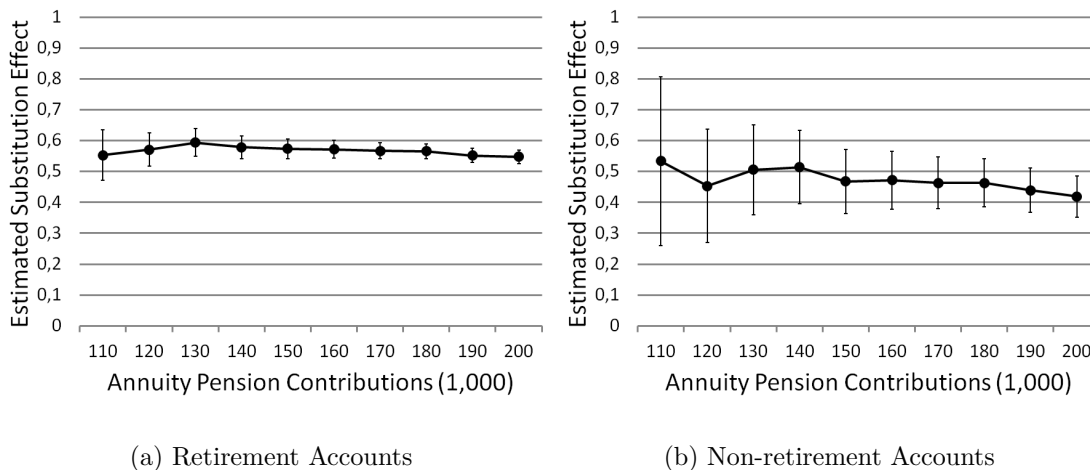
Dep. var.:	Expl. var.: $Treat \times Post$				
	2010 (1)	2008 (2)	2007 (3)	2006 (4)	2005 (5)
Annuity Pensions (Baseline)	-21,038*** (394)	49 (295)	103 (349)	-474 (387)	1,280*** (463)
Annuity Pensions (Donut-hole)	-28,657*** (493)	419 (378)	135 (444)	-710 (494)	884 (589)

Note: Significance levels 1%, 5% and 10% are reported as \*\*\*, \*\* and \*, respectively. All columns include lagged control variables, individual fixed effects, year fixed effects, education level fixed effects and geographical region fixed effects. Standard errors in parentheses are clustered on the individual level. The first row shows estimates of equation 1 applying different reform years. 2010 was the year of the actual reform and column 1, row 1 corresponds to the first stage in our empirical strategy. Columns 2-5 report placebo estimates of non-reform years. The second row excludes individuals very close to the introduced DKK 100,000-threshold, namely individuals within DKK 10,000 on each side of the threshold. Source: Own calculations based on administrative data from Statistics Denmark.

Robustness for changes in window size around the DKK 100,000-threshold is tested in the following. By varying the interval above the threshold when assigning individuals into treatment, while keeping the interval for assignment of non-treated individuals constant, sensitivity to the sample selection can be tested. The figures 5a and 5b show the estimated substitution from annuity pension accounts to alternative retirement accounts and non-retirement accounts, respectively, for different choices of window size above the DKK 100,000-threshold. Retirement accounts include substitution to life-long and capital pension schemes, while non-retirement accounts include repayments of debt in mortgage institutions and banks, bank deposits and financial assets. The dots reflect point estimates and the bars are two standard errors clustered on the individual level, indicating the 95%-confidence

band. Point estimates are based on equations (2) and (3). On the vertical axis, the value 1 indicates full crowd-out, while 0 reflects zero crowd-out when reducing annuity pension contributions by 1 unit. Figure 5a shows that for all windows, a little more than 0.5 units are shifted to alternative pension accounts and figure 5b shows that a little less than 0.5 units are shifted to non-retirement saving and debt accounts when reducing annuity pension contributions by 1 unit. These estimates are consistent with the main findings reported in table 1. The figure clearly shows that the full crowd-out result is robust to changes in the applied quasi-experimental design. Robust standard errors do, however, increase for more narrow windows than for wider windows. This is simply a consequence of having fewer observations in the former compared to the latter. Based on this, selection of individuals into treatment and control groups do not seem to be a determinant of the findings.

Figure 5: Treatment Window around DKK 100,000-threshold



Note: The estimated substitution effect is the estimates from equations (2) and (3), showing how many cents were shifted for a 1 unit reduction in annuity pension contributions. The effects are estimated for increasing window size when assigning individuals into treatment. For instance, the value 110 implies that individuals who had annuity pension contributions in 2008 of DKK 100,000-110,000 are assigned as treated. The bars are two standard errors clustered on the individual level, indicating the 95%-confidence band.

Source: Own calculations based on administrative data from Statistics Denmark.

Our allocation of individuals into treatment and control groups could generate the observed post-reform decline in annuity pension contributions by the control group. No natural allocation exist and such misallocation is likely to happen if some individual with *true* treatment behaviour is allocated into the control group. Imagine a saver who usually contributes more than DKK 100,000 to annuity pension schemes, but for random reasons contributes less (and below the cut-off) in the assignment year. This person would be placed in the control group but her behaviour reflects that of an individual in the treatment group as she reduces annuity pension contributions in the reform year. To test this explanation we return to the assignment process. Mean annuity pension contributions declined 11 percent for the control group from the assignment year 2008 to the reform year 2010, but when

conditioning on having control-group size annuity pension contributions two consecutive years (2007–2008) the decline in contributions is reduced to 7 percent from 2008 to 2010. Imposing more conditions, i.e. three consecutive years (2006–2008), four consecutive years (2005–2008), five consecutive years (2004–2008) and six consecutive years (2003–2008), the control group’s decline in annuity pension contributions is reduced to 6, 5, 4 and 3 percent, respectively. The numbers are shown in Table 5, which also includes similar calculations for the treatment group. Annuity pension contributions in the treatment group declined from 30 to 28 percent when conditioning on one pre-reform year (2008) to six consecutive years (2003–2008). Conditioning on having six consecutive years of either treatment or control behaviour reduces the analysis sample considerably (by more than 96 percent), providing us with very large standard errors. However, when estimating equation (3), the substitution effects are not significantly different from our baseline results where life-long pension schemes and debt repayments are the closest substitutes to annuity pension schemes (see appendix).

To check that the results are not driven by individuals who are forced to move out of their homes, e.g. as a consequence of the financial turmoil in the years that followed the global recession, the empirical model is run on a subsample in which individuals are excluded if they changed their address after 2010. This ensures that estimated debt repayments are not reflecting second-order effects of buying or selling property. The results are almost identical to the findings in the main sample as shown in Table 8. To further ensure that housing wealth is not interfering with the identification of the reform effect, housing wealth developments are plotted across time for both the treated and non-treated groups. Figure 11a shows almost identical trends for the two groups, both pre- and post-reform. This is a strong indication that real assets—the stock of housing—were not affected differently in the treatment and control groups in the reform year. Similarly, graphical inspection of income developments over the sample period in Figure 11b shows that income trends for the treated and non-treated groups were identical both before and after implementation of the reform. This is suggestive evidence that the two following concerns in our setup can be rejected. First, neither of the two groups seemed to be more exposed to unemployment. Second, the treated group did not seem to change labour supply relative to the non-treated group as a consequence of the reform. By comparing developments in unemployment benefits and unemployment rates between the treatment and control groups, we find evidence supporting that the individuals’ exposures to unemployment were not significantly different. Appendix Figures 12a and 12b show the differences between the treatment and control groups in the two outcomes across the sample period. Robust standard errors show that the differences are statistically insignificant. Lastly, it is shown that individuals respond immediately to the policy change in 2010. Table 8 shows the main findings estimated in 2010 (column 2), 2010-2011 (column 3) and 2010-2012 (column 4). All these estimates are comparable in size

and statistical significance, indicating that individuals mainly responded in the reform year.

## 4 Active Saving Decisions

Recent findings in the empirical literature suggest that only a minor share of the population respond to tax incentives (Chetty et al., 2014). In the following it is analysed how large is the share of individuals that substitute savings when tax incentives for saving in pension schemes are reduced.

### 4.1 Estimating the share of active savers

According to findings presented above, life-long pension contributions were increased by 56 cents for each unit reduction in annuity pension contributions because of the reform, indicating that life-long schemes tend to be the closest substitute for annuity schemes. An indicator,  $Comply_{i,t}$ , is constructed, which takes the value 1 in years where individuals reduce annuity scheme contributions and increase life-long scheme contributions. This is an indicator that captures a shift in savings between these two scheme types within the same year. By regressing  $Comply_{i,t}$  on a difference-in-differences specification, the share of active savers is estimated in a linear probability model.

$$Comply_{i,t} = \alpha_i + \Omega_t + Treat_i + \beta_t Treat_i \times Year_t + X_{i,t-2} + \varepsilon_{i,t} \quad (4)$$

Using the specification in equation (4), coefficients  $\beta_t$  capture the policy effect on compliance in each year  $t$ . Other specifications are equal to that of equation (1). Table 3 shows no significant response in the years prior to the reform but a significant increase by 23 percentage points in 2010 with  $p < .000$ . This suggests that the tax reform explains 23 percent of the shift of savings from annuity to life-long pension schemes. Our estimate is close to that of Chetty et al. (2014), who find that about 15 percent of savers shift to the closest substitute. Smaller adjustments were made in the year 2011 and 2012, but these effects tend to cancel out in size. This is consistent with the findings in Table 8, which showed that the estimated substitution occurred mainly in the reform year.



Table 3: Share of Individuals who Increase Annuity and Decrease Life-long Pensions

	Coef.	Std.Err.	p-value
2004	.001	.003	.661
2005	−.001	.003	.806
2006	.001	.003	.880
2007	.000	.003	.943
2008	.002	.003	.499
2009	.002	.003	.614
2010	.231***	.003	.000
2011	.028***	.003	.000
2012	−.021***	.003	.000
2013	−.004	.003	.215

Note: Significance levels 1%, 5% and 10% are reported as \*\*\*, \*\* and \*, respectively. 599,774 observations were used for estimating  $\delta$  in column 4, covering the treated and non-treated groups for the time period 2004-20013. Values in column 4 are  $\delta$  estimates using the specification in equation (1) with an indicator as dependent variable, taking the value 1 for individuals who decrease annuity pension contributions and increase life-long pension contributions in the same year. Source: Own calculations based on administrative data from Statistics Denmark.

## 4.2 Observable Heterogeneity

The estimated crowd-out in retirement savings seems to be conducted by 23 percent of all individuals. In order to sort out whether these individuals differ from passive savers, who did not respond to the tax reform, the compliance indicator is regressed on a set of personal characteristics. Included on the right-hand side is a range of personal information such as gender, age, educational attainment and dummies of labour market status, i.e. employed in a top management position or whether individuals have been unemployed more than three months within the last year. Also, a set of mortgage information dummies is included. These cover whether the individuals had interest only loans, adjustable rate mortgages, fixed rate mortgages or whether they had any loan at all prior to the reform. Finally, logs of income and financial assets and geographical region dummies are included to account for differences in the housing and labour markets across the country. The dependent variable is the indicator described earlier in this section, which takes the value 1 for individuals who shift savings from annuity pension accounts to life-long pension accounts in 2010. The regression is run on a cross section of 2008 values. Only treated individuals are included in the regression and the coefficients are presented in Table 10. Regressing the compliance indicator on a set of pre-reform observables gives us consistent parameter estimates that can be interpreted as the partial effect on the probability of being an active saver. Column 1 in Table 10 presents heterogeneity by a range of personal characteristics. Heterogeneity by mortgage

loan characteristics is presented in column 2. In column 3, financial variables are added and column 4 includes all available observables in the regression, including dummies for geographical region of residence. Column 5 presents marginal effects in a probit model, using the same specification as column 4. The probit model estimates very similar results as the linear probability model except for the unemployment indicator. However, in the following, heterogeneity based on column 4 is described, which includes all available information.

The likelihood of complying with the change in taxation increases with age and educational attainment. For each one year increase in age, individuals were 2.8 percentage points more likely to respond. By completing a college degree of more than 3<sup>1</sup>/<sub>2</sub> years, the likelihood of an active response increased by 3.5 percentage points. This increases to 6.3 percentage points if the degree covers more than 5 years of study. The estimates are, however, not statistically different from each other but both are statistically different from primary school, which is the baseline. Individuals with vocational training as their highest level of educational attainment were, on the contrary, 1.9 percentage points less likely to respond to the changed tax incentives.

Labour market status seems to play a significant role on tax compliance. Compared to the average wage earner, top managers in private or public institutions were 3.4 percentage points more responsive to the changed taxation rules. Contrary to this and even more striking is the result that individuals who had experienced more than three months of unemployment within the last year were 26.9 percentage points less responsive to the changed tax subsidy. The marginal effect is even larger in a probit model. Here, the effect of being unemployed on compliance is -38.3 percentage points.

Mortgage and financial characteristics also play a significant role. Individuals with mortgages were 4.4 percentage points more responsive to the change in tax incentives for saving in pension schemes. Whether the mortgage loan interest rate was adjustable or fixed does not seem to play a significant role. Individuals with interest-only mortgages were, however, 4.9 percentage points less responsive. Finally, log of annual income and log of financial assets are both significantly correlated with the compliance indicator. Compared to the average, individuals with a 1 percent increase in gross income were 14.6 percentage points more likely to respond to the change in tax incentives for saving in pension schemes. Regarding the stock of financial assets, the correlation is 0.4 percentage points.

Correlations between pre-reform observable characteristics and the compliance indicator provide a picture of the active saver who actually responded when the tax deduction threshold was introduced. However, the correlations do not provide information on the policy response itself. The following section attempts to measure policy responses for credit constrained individuals, specifically.

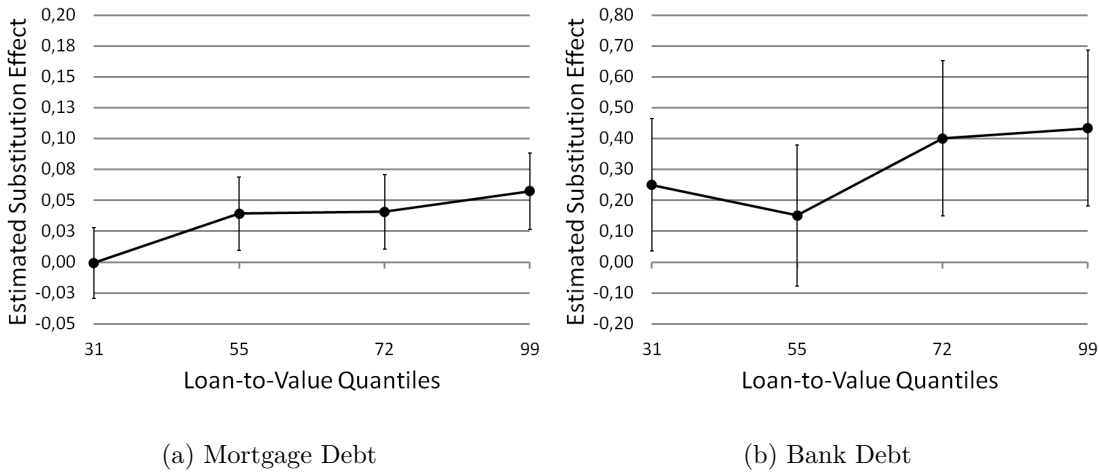
### 4.3 Policy Response and Credit Constraints

The final part of the empirical analysis tests the hypothesis that more credit constrained individuals utilise the policy change to reduce gross debt relatively more than their less leveraged peers. This hypothesis relies on the permanent income hypothesis, meaning that consumers prefer to smooth consumption over their lifetime and possibly need to borrow and save financial assets during different phases of their lives. Highly indebted or younger individuals might find it more difficult to obtain credit in banks. Empirical studies have found that debt levels and age tend to play significant roles in credit constraints (Attanasio and Weber, 1994; Leth-Petersen, 2010) and this section explores the policy response of these two groups.

On average individuals repay gross debt by 31 cents for each 1 DKK that annuity pension accounts were reduced. It is expected, however, that substitution is stronger for more liquidity constrained individuals. To test this hypothesis, substitution effects are estimated for groups with different loan-to-value ratios and over different age intervals. The idea is that individuals with higher loan-to-value ratios and younger individuals use a larger share for deleveraging, conditional on responding to the tax reform.

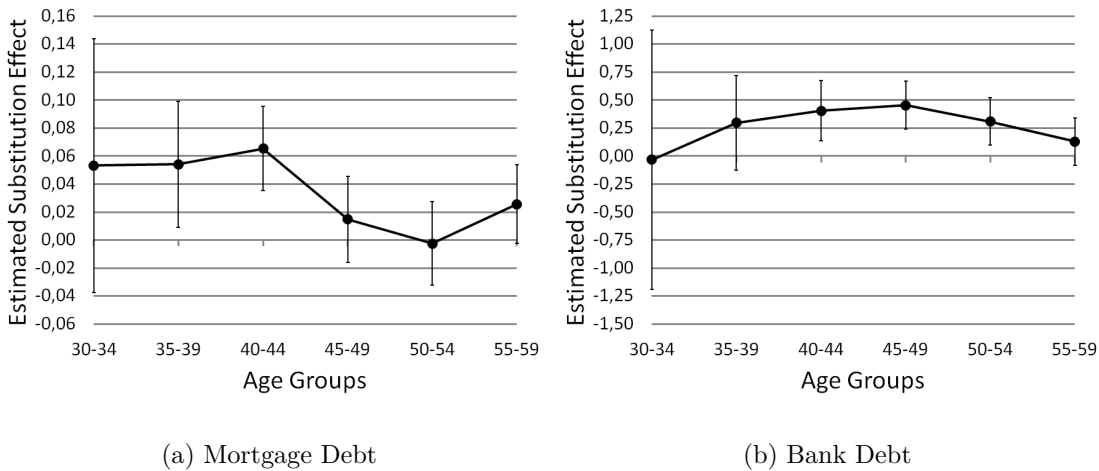
Equation (3) is used to test whether more leveraged individuals reduced debt more intensively than individuals with less debt relative to their real assets when the reform was introduced. First, individuals are divided into quantiles based on the pre-reform loan-to-value ratio. The ratio is total outstanding mortgage debt as a share of the property value and the property value is assessed by the mortgage institution. Substitution to mortgage debt repayments and bank debt repayments are estimated separately for each of the four groups. The results are illustrated in Figures 6a and 6b, both showing that debt reduction estimates are stronger for highly leveraged individuals. Standard errors are clustered on the individual level and illustrated by vertical bars. Substitution of savings between annuity pension schemes and mortgage debt repayments are statistically significant for individuals with loan-to-value ratios of 55 or above. The point estimates increase for larger ratios but are not statistically different from each other. Bank debt repayments are statistically larger than zero for individuals with ratios of 72 or above. The lowest quantile—a loan-to-value of 32—also return a significant response. This is probably because bank debt is more volatile than mortgage debt, which is also supported by the larger standard errors on bank debt estimates. The overall picture shows, however, that more indebted individuals account for the measured deleveraging. To test the robustness of these results, we construct similar figures but use loan-to-income ratios instead. Figures 13a and 13b illustrate almost identical patterns, namely that substitution of savings from pension accounts to debt repayments are prevalent for more credit constrained individuals. Here the credit constraints are proxied by outstanding debt as a share of annual income.

Figure 6: Debt Repayments across Loan-to-Value



Note: Loan-to-value is outstanding mortgage debt divided by the mortgage institutions' assessment of property values. The estimated substitution effect is the estimates from equations (2) and (3), showing how many cents were shifted for a 1 unit reduction in annuity pension contributions. The effects are estimated on subgroups that represent loan-to-value quantiles. The bars are two standard errors clustered on the individual level.  
 Source: Own calculations based on administrative data from Statistics Denmark.

Figure 7: Debt Repayments across 5-year Age Groups



Note: The estimated substitution effect is the estimates from equations (2) and (3), showing how many cents were shifted for a 1 unit reduction in annuity pension contributions. The effects are estimated on subgroups that represent 5-year age intervals. The bars are two standard errors clustered on the individual level.  
 Source: Own calculations based on administrative data from Statistics Denmark.

Credit constrained individuals are presumably younger than unconstrained individuals. By estimating mortgage debt repayments across age intervals a clear picture prevails, namely that younger individuals tend to substitute pension savings for debt reductions in the reform year, while older ones did not. Figure 7a shows that 30–44 year-old individuals reduced mortgage debt by around 6 cents for each DKK reduction in annuity pension contributions. Significant substitution to mortgage debt repayments for the 45–59 year-olds

is not detected. This finding supports the hypothesis that the reform was used by credit constrained individuals—or individuals who expect to be constrained in the near future—to bring down debt accounts. Figure 7b shows repayments in bank debt accounts across age intervals. This figure illustrates a hump shaped pattern, where the very young and the oldest did not substitute pension savings for bank debt reductions. However, individuals in the middle of their lifecycle—the 40–54 year-olds—did substitute 30-45 cents to bank debt repayments for each DKK reduction in annuity pension accounts. In our sample, 71 percent of younger individuals (below 35) are mortgage borrowers. For the middle-aged (35–45) and older individuals (above 45) the numbers are 82 percent and 76 percent, respectively. Estimating equation (1)-(3) on each subgroup shows that substitution to life-long pension schemes is similar for the three age groups, while substitution to mortgage debt repayments is driven by the middle-aged and bank debt repayments are driven by both middle-aged and older borrowers (see appendix).

## 5 Conclusion

Recent studies show that tax-favoured pension accounts have no effect on overall individual savings because taxpayers simply shift savings between accounts when tax incentives change. This paper offers a decomposition of this effect to test whether debt repayments account for a substantial part of the crowd-out. The basic story tested here is whether a reduction in the tax incentive for saving in retirement accounts prompts people to substitute savings to alternative accounts with the now-highest after-tax return. Debt usually carries a higher interest than savings, implying that repayment of debt should be warranted before accumulating non-retirement savings.

We show that savings are shifted from tax-favoured retirement accounts to gross debt repayments when tax incentives for saving in pension schemes are reduced. For each unit of DKK that retirement savings are reduced about 31 cents is used for deleveraging. The remaining 69 cents is shifted to alternative saving accounts, implying full crowd-out. These effects are identified by exploiting variation from a policy change that exactly did reduce tax incentives for saving in retirement accounts. Moreover, the paper documents that only 23 percent of individuals rebalance their saving accounts when tax rules change. Observable heterogeneity is documented on a range of personal characteristics when comparing active and passive savers.

A key feature in this paper is the comprehensive panel data coverage of financial balances on the individual level. Unlike former studies in the crowd-out literature, this paper benefits from access to data on both bank and mortgage debt. Mortgages comprise a major share of financial liabilities in the household sector and the ability to include mortgages in the analysis is an important innovation compared to recent studies. Without knowing whether taxpayers manipulate the liability side, one would simply not be able to assess the overall

consumption and savings response to tax incentive policies.

The findings suggest that pension savings and debt accumulation is positively correlated. Gross debt seem to change by almost a one-third of the change in pension savings, implying that gross debt accumulation could increase because of policies that induce people to save in pension schemes.

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## A Tables

Table 4: Changes in Savings when Reducing Annuity Pensions by 1 Unit

Dep. var.:	Expl. var.: Annuity Pensions			
	Collapse Years (1)	Incl. Self-emp (2)	Normalised using Income (3)	Excl. Housing Wealth (4)
Life-long Pensions	.567*** (.016)	.550*** (.018)	.441*** (.017)	.561*** (.016)
Capital Pensions	.007** (.003)	.004 (.003)	.022*** (.004)	.006* (.003)
Mortgage Repayments	.024*** (.006)	.025*** (.006)	.011 (.007)	.021*** (.006)
Bank Debt Repayments	.294*** (.058)	.419*** (.072)	.277*** (.069)	.304*** (.058)
Bank Deposits	.155*** (.050)	.123** (.058)	.112** (.057)	.141*** (.050)
Financial Assets	.051 (.036)	.065 (.042)	.022 (.046)	.031 (.036)
Observations	599,774	701,299	599,774	599,774

Note: Significance levels 1%, 5% and 10% are reported as \*\*\*, \*\* and \*, respectively. All columns include lagged control variables, individual fixed effects. Standard errors in parentheses are clustered on the individual level. Total Crowd-out is the sum of point estimates in each column.

Source: Own calculations based on administrative data from Statistics Denmark.

Table 5: Annuity pension contribution decline (%) in 2010 from various pre-reform periods

	Treatment group	Control group	Observations
2008	29.9	11.0	599,774
2007–2008	27.9	7.4	271,842
2006–2008	27.5	5.8	136,318
2005–2008	27.6	5.2	68,943
2004–2008	27.5	4.3	35,078
2003–2008	28.2	3.4	18,380

Note: The percent decline in annuity pension contributions is calculated for each group from the given time period to 2010.  
Source: Own calculations based on administrative data from Statistics Denmark.

Table 6: Summary Statistics

	Full sample		Estimation sample	
	Mean	SD	Mean	SD
	– (DKK 1,000) –			
Gross Income	305.2	174.2	668.5	279.3
Pension Contributions	27.2	66.0	96.5	89.9
Interest payments	14.5	20.3	33.2	30.0
Bank Debt	115.4	176.9	187.1	328.2
Bank Deposits	55.0	110.2	137.7	226.7
Financial Assets	22.7	119.4	89.1	261.5
Housing Wealth	530.6	957.8	1,328.2	1,289.4
Housing Equity	69.0	498.9	206.7	689.8
Age	37.5	10.7	43.3	7.5
Work Tenure	13.2	10.2	20.4	8.7
Years on Address	7.4	7.9	9.2	7.8
Female (%)	51.6	50.0	25.0	43.4
Married (%)	44.8	49.7	69.4	46.1
Secondary Education (%)	7.8	26.7	7.7	26.7
Vocational Training (%)	28.2	45.0	28.2	45.0
Short Tertiary (%)	8.6	28.1	8.7	28.1
Middle-long Tertiary (%)	23.8	42.6	23.8	42.6
Long Tertiary (%)	25.3	43.5	25.4	43.5
	– Quantities –			
Mortgage Loans	.60	.81	1.17	.98
–Fixed Rate	.30	.58	.53	.72
–Adjustable Rate	.30	.61	.65	.85
Interest Only Loans	.22	.50	.43	.70
Individuals	2,377,988		56,372	

Note: The statistics are based on all pre-reform years, i.e. from 2003 to 2009. The full sample excludes self-employed and their spouse and individuals older than 59 years of age. Gross income includes all pension contributions. The estimation sample includes the treated and control group individual, who contributed DKK 100,000-150,000 and DKK 80,000-100,000, respectively, for annuity pension schemes in 2008.

Source: Own calculations based on administrative data from Statistics Denmark.

Table 7: Summary Statistics

	Treated		Non-treated		Diff.	t-value
	Mean	SD	Mean	SD		
	<i>– (DKK 1,000) –</i>					
Gross Income	710.5	307.0	627.5	242.4	83.0	93.9
Pension Contributions	109.2	106.1	84.2	68.3	25.0	87.8
Interest payments	33.9	31.1	32.5	29.1	1.4	14.8
Bank Debt	195.5	347.9	179.0	307.6	16.4	15.7
Bank Deposits	149.5	238.4	126.2	213.9	23.3	32.2
Financial Assets	102.8	287.3	75.8	232.8	27.0	32.3
Housing Wealth	1,377.1	1,271.3	1,280.5	1,305.1	96.6	8.9
Housing Equity	221.5	594.4	192.2	771.2	29.3	5.0
Age	43.8	7.4	42.7	7.6	1.1	46.8
Work Tenure	20.8	8.7	19.9	8.7	0.9	31.0
Years on Address	9.5	7.9	8.9	8.7	0.7	26.7
Female (%)	23.7	42.5	26.4	44.1	2.7	19.2
Married (%)	70.9	45.4	68.9	46.7	2.9	19.9
Secondary Education (%)	7.8	26.8	7.7	26.6	0.1	–1.2
Vocational Training (%)	27.2	44.5	29.1	45.4	–1.9	13.3
Short Tertiary (%)	8.2	27.4	9.1	28.8	–0.9	10.6
Middle-long Tertiary (%)	24.2	42.9	23.5	42.4	0.7	–4.6
Long Tertiary (%)	26.6	44.2	24.2	42.8	2.4	–17.4
	<i>– Quantities –</i>					
Mortgage Loans	1.19	1.00	1.16	.95	.02	2.7
–Fixed Rate	.52	.73	.53	.72	.01	1.3
–Adjustable Rate	.66	.87	.63	.83	.03	4.3
Interest Only Loans	.46	.72	.41	.68	.05	7.8
Individuals	27,591		28,781			

Note: The statistics are based on all pre-reform years, i.e. from 2003 to 2009. The population means exclude self-employed and their spouse and individuals older than 59 years of age. Gross income includes all pension contributions. The treated contributed DKK 100,000-150,000 for annuity pension schemes in 2008, while the non-treated contributed DKK 80,000-100,000. The column of t-values measures significance of different means for each variable in the years prior to 2008.

Source: Own calculations based on administrative data from Statistics Denmark.

Table 8: Changes in Savings when Reducing Annuity Pensions by 1 Unit

Dep. var.:	Expl. var.: Annuity Pensions			
	Non-movers (1)	2010 (2)	2010-2011 (3)	2010-2012 (4)
Life-long Pensions	.567*** (.016)	.579*** (.019)	.580*** (.018)	.571*** (.016)
Capital Pensions	.007** (.003)	.023*** (.004)	.018*** (.004)	.013** (.004)
Mortgage Repayments	.024*** (.006)	.012** (.006)	.017*** (.006)	.020*** (.006)
Bank Debt Repayments	.294*** (.058)	.372*** (.129)	.300*** (.083)	.300*** (.066)
Bank Deposits	.150*** (.050)	.292** (.122)	.197*** (.075)	.174*** (.058)
Financial Assets	.042 (.036)	.356*** (.073)	.010 (.049)	.094** (.045)
Controls	Yes	Yes	Yes	Yes
Individual FE	Yes	-	Yes	Yes
Year FE	Yes	-	Yes	Yes

Note: Significance levels 1%, 5% and 10% are reported as \*\*\*, \*\* and \*, respectively. All columns include lagged control variables, individual fixed effects and year fixed effects. Standard errors in parentheses are clustered on the individual level. Total Crowd-out is the sum of point estimates in each column.

Source: Own calculations based on administrative data from Statistics Denmark.

Table 9: Changes in Savings when Reducing Annuity Pensions by 1 Unit

Dep. var.:	Expl. var.: Annuity Pensions			
	Six pre-reform years (1)	Age 18–34 (2)	Age 35–45 (3)	Age 46–59 (4)
Life-long Pensions	.594*** (.051)	.471*** (.115)	.513*** (.027)	.523*** (.021)
Capital Pensions	.015 (.011)	.018 (.030)	.012*** (.005)	.004 (.004)
Mortgage Repayments	.024*** (.025)	.032 (.046)	.045*** (.011)	.017** (.008)
Bank Debt Repayments	.595*** (.208)	.996 (.795)	.260*** (.119)	.213*** (.073)
Bank Deposits	.178 (.221)	.196 (.625)	.168* (.100)	.099 (.067)
Financial Assets	.097 (.0185)	.119 (.340)	.065 (.062)	.062 (.051)
Observations	18,380	60,101	247,681	291,963

Note: Significance levels 1%, 5% and 10% are reported as \*\*\*, \*\* and \*, respectively. Standard errors in parentheses are clustered on the individual level. Column (1) presents crowd-out estimates on a subsample, including savers with six consecutive years of either treatment- or control-type annuity pension contributions. Columns (2)–(4) are subsamples, dividing savers in age groups.

Source: Own calculations based on administrative data from Statistics Denmark.



Table 10: Observable Heterogeneity in Active vs. Passive Savers

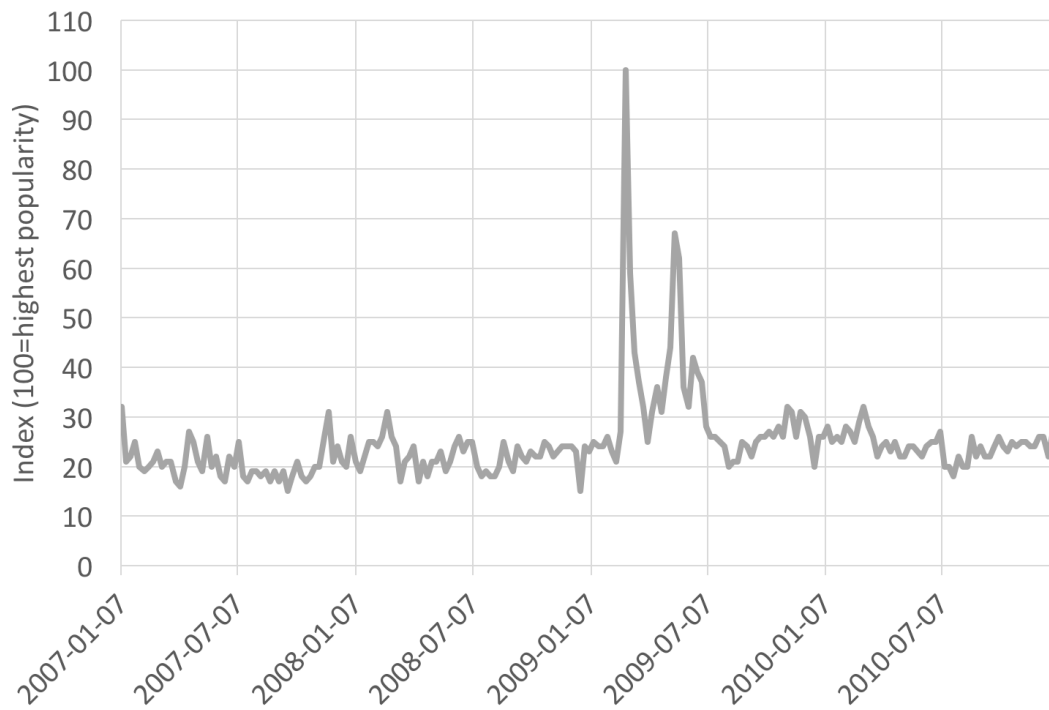
Expl. var.:	Dep. var.: $Comply_i$				
	(1)	(2)	(3)	(4)	(5)
Female	-.010 (.007)		.019*** (.007)	.009 (.007)	.010 (.007)
Age	.049*** (.005)		.029*** (.005)	.028*** (.005)	.029*** (.005)
Top Management	.061*** (.008)		.027*** (.008)	.034*** (.008)	.031*** (.008)
Unemp. within last year	-.275*** (.100)		-.272*** (.100)	-.269*** (.100)	-.383*** (.140)
Vocational Training	-.032*** (.010)		-.026*** (.010)	-.019** (.010)	-.018** (.010)
Short Tertiary	-.013 (.013)		-.008 (.013)	-.002 (.013)	-.004 (.013)
Medium-long Tertiary	.047*** (.010)		.032*** (.010)	.035*** (.010)	.035*** (.010)
Long Tertiary	.118*** (.010)		.032*** (.010)	.063*** (.010)	.061*** (.010)
Mortgage Borrower		.040*** (.013)	.040*** (.013)	.044*** (.013)	.045*** (.013)
Interest Only		-.047*** (.007)	-.042*** (.007)	-.049*** (.007)	-.047*** (.007)
Adjustable Rate		.016 (.010)	-.006 (.010)	-.006 (.010)	-.008 (.010)
Fixed Rate		.042*** (.010)	.016* (.010)	.011 (.010)	.010 (.010)
log(Income)			.165*** (.009)	.146*** (.009)	.153*** (.009)
log(Financial Assets)			.004*** (.001)	.004*** (.001)	.003*** (.001)
log(Pension Contr.)	-	-	Yes	Yes	Yes
Geo. Region Dummies	-	-	-	Yes	Yes
N	27,591	27,591	27,591	27,591	27,591
$R^2$	0.021	0.005	0.056	0.062	0.050

Note: Significance levels 1%, 5% and 10% are reported as \*\*\*, \*\* and \*, respectively. Explanatory variables are 2008-numbers. The dependent variable takes the value 1 if individuals reduce annuity pension contributions and increase life-long pension contributions from 2009 to 2010. Age squared is included in every column. Columns 1-4 are linear probability model estimates and column 5 is marginal effects of a probit model evaluated at sample averages.

Source: Own calculations based on administrative data from Statistics Denmark.

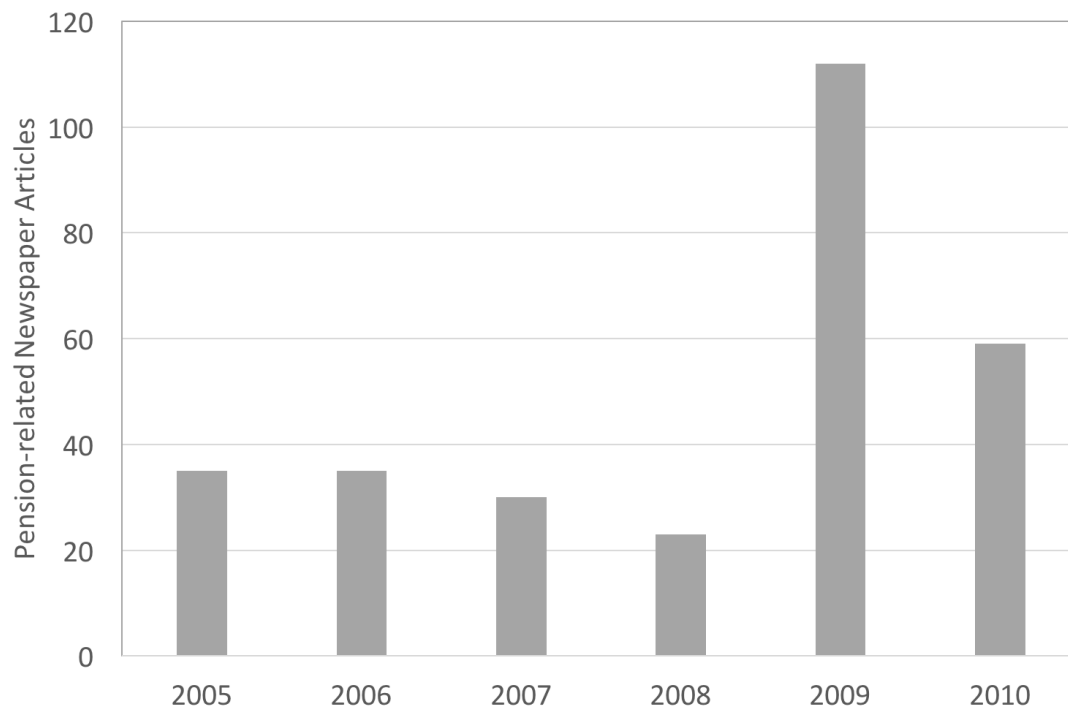
## B Figures

Figure 8: Pension-related web searches in Denmark (Google Trends)



Note: The numbers represent the search interest in relation to the highest point in the chart for that area and the time. A value of 100 is the greatest popularity of the term, a value of 50 means that the term is half as popular, and a result of 0 means that the term was less than 1% as popular as the result of 100. The search term used was "pension".  
Source: Google Trends

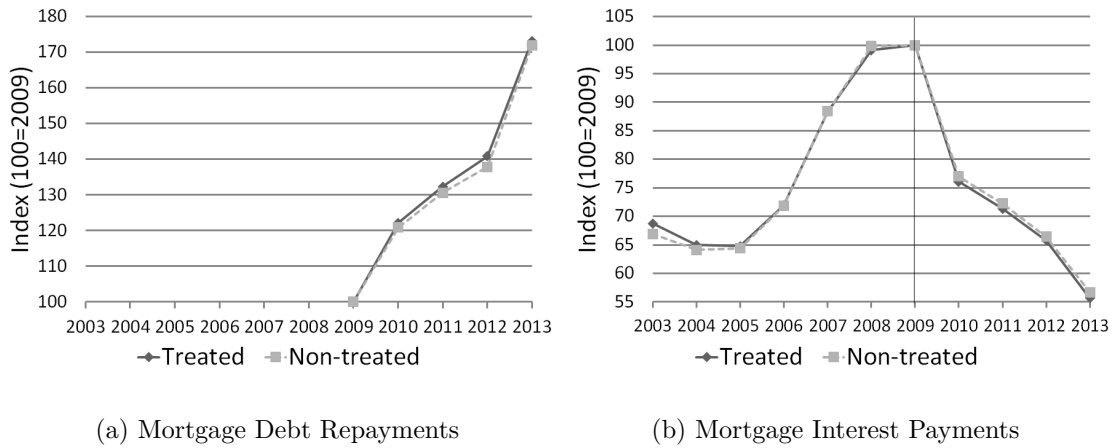
Figure 9: Pension-related newspaper articles in Denmark



Note: The number of articles is based on a search of nation-wide newspapers in Denmark using the phrases "ratepension", which is the Danish word for annuity pension and "fradrag", which is Danish for tax-deduction.

Source: Infomedia

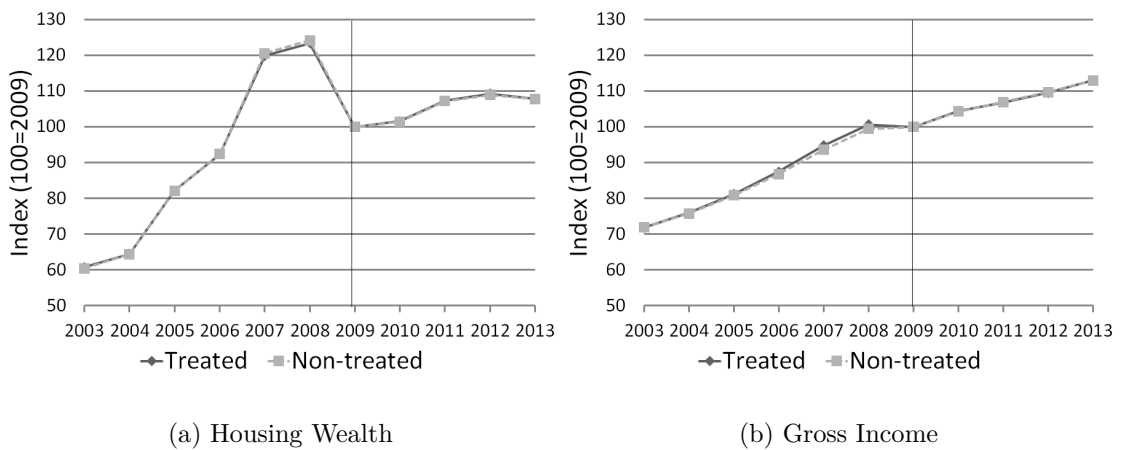
Figure 10: Mortgage Debt



Note: See Figure 3.

Source: Own calculations based on administrative data from Statistics Denmark.

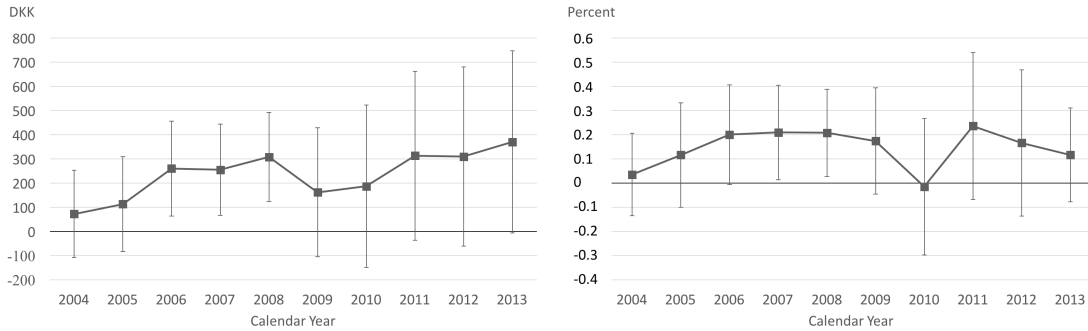
Figure 11: Housing Wealth and Income



Note: See Figure 3.

Source: Own calculations based on administrative data from Statistics Denmark.

Figure 12: Labour markets outcomes



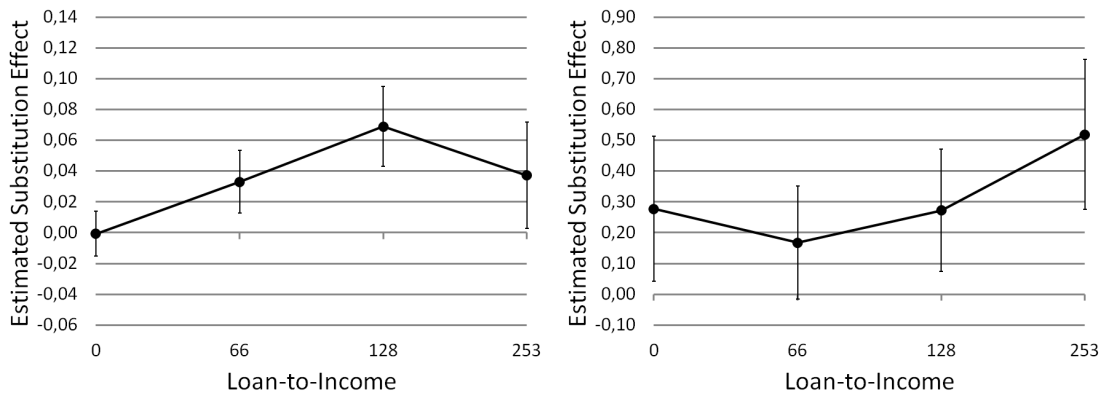
(a) Unemployment benefits

(b) Unemployment rate

Note: Figure 12a indicates the difference in unemployment benefits between the treatment and control groups. The difference is measured in DKK and the error bars are two standard errors clustered on the individual level. Figure 12b indicates the difference in the unemployment rate between the treatment and control groups. The difference is measured in percent and the error bars are two standard errors clustered on the individual level.

Source: Own calculations based on administrative data from Statistics Denmark.

Figure 13: Debt Repayments across Loan-to-Income



(a) Mortgage Debt

(b) Bank Debt

Note: The estimated substitution effect is the estimates from equations (2) and (3), showing how many cents that were shifted for 1 unit reduction in annuity pension contributions. The effects are estimated on subgroups that represent loan-to-income quantiles. The bars are two standard errors clustered on the individual level.

Source: Own calculations based on administrative data from Statistics Denmark.

## Chapter 2

# Housing Wealth Effects and Mortgage Borrowing





# Housing Wealth Effects and Mortgage Borrowing

## The Effect of Subjective Unanticipated Home Price Changes on Home Equity Extraction\*

Henrik Yde Andersen<sup>†</sup> and Søren Leth-Petersen<sup>‡</sup>

### Abstract

In this paper we examine whether home price changes drive mortgage based equity extraction. To do this we use longitudinal survey data with subjective information about current and expected future house prices to calculate unanticipated house price changes. We link this information at the person level to high quality administrative records with information about mortgage borrowing as well as savings in various financial instruments. We find a marginal propensity to extract out of unanticipated housing wealth gains to be 3-5 percent. We find no adjustment to other components of the portfolio, and we find that mortgage extraction leads to an increase in spending. We find no evidence that the effect is driven by collateral constraints. Instead, the effect is driven by about 11 percent of the observations where the respondent is recorded having actively taken out a new mortgage. Three out of four among these refinance an existing fixed rate mortgage loan and exploit that the old loan can be prepaid and a new loan established to lock in a lower market rate. The propensity to extract equity is higher for the group who have an incentive to refinance following a drop in the market interest rate and at the same time experience an unanticipated housing wealth gain. These results point to the existence of a housing wealth effect that is intimately connected to the functioning of the mortgage market, and this suggests that monetary policy plays an important role in transforming unanticipated housing wealth gains into spending by affecting interest rates on mortgage loans.

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# 1 Introduction

The recent financial crisis made it clear that the mortgage market and home equity extraction plays a critical role in creating a link between housing wealth and spending. However, the evidence about the mechanism through which house prices drive equity extraction and spending is limited. In this paper we examine whether there is a housing wealth effect, i.e. whether unanticipated housing wealth changes are associated with spending adjustments, and what role the mortgage market plays in facilitating such a link.

The starting point for our investigation is a sizable literature that has debated over the relevance of three different explanations for the association between house prices and spending. One explanation for the correlation is *the housing wealth hypothesis*, which is motivated by the life cycle framework and is based on the notion that agents smooth spending (marginal utility) and adjust spending only when *new* information about the value of housing wealth arrives. The housing wealth hypothesis is supported by Cambell and Cocco (2007), Muelbauer (1990), Skinner (1996), among many others, but these studies do not consider the role of the mortgage market.<sup>1</sup> An alternative hypothesis is that house prices generate additional collateral which households can borrow against. This channel would potentially allow people to smooth spending when current income (or cash-on-hand) is below the long term level. According to the *collateral channel hypothesis* innovations to house prices do not cause spending adjustments directly but the effect operates through the improved access to mortgage borrowing and we therefore denote this channel the collateral effect. Aron and Muellbauer (2013), Aladangady (2017), Browning et al. (2013), Cooper (2013), Disney and Gathergood (2011), and Leth-Petersen (2010) find evidence in support of this hypothesis. A third hypothesis, *the common factor hypothesis*, postulates that both house prices and spending are driven by a third variable

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<sup>1</sup>The evidence is mixed, however. Engelhardt (1996), using the PSID, does not find any effect of capital gains on consumption. Hoynes and McFadden (1997) are not able to find any link between saving and capital gains on housing. Juster et al. (2006) find no evidence that capital gains in housing influence savings decisions.

causing both house prices and spending. According to this idea, expected income changes or a general easing of credit availability could drive demand which then drive both house prices and spending. This idea was proposed by King (1990) and Paganao (1990) as a response to the findings of Muellbauer (1990). Attansio and Weber (1994), Attanasio et al (2009), and Windsor et al. (2015) find evidence in support of this hypothesis.<sup>2</sup>

Recently, this debate has gained new momentum in the context of trying to understand the causes and consequences of the US housing collapse and mortgage crisis. Mian and Sufi (2011) and Bhutta and Keys (2016) document how home values drive home equity extraction among younger house owners and is associated with a subsequent increase in loan defaults indicating that home equity was extracted for spending rather than kept for bad times. Mian, Rao, Sufi (2013) argue that the wealth loss associated with the housing collapse following the recent financial crisis is responsible for the significant coinciding spending decline, and that credit conditions play a critical role because the house price fall has limited the access to collateral for people who were already highly leveraged. These findings do, however, not stand uncontested. Davidoff (2016) argues that local demand factors are responsible for the house price cycle severity. Adelino et al. (2016) show that mortgage originations increased for borrowers across all income and FICO score levels and that the relation between mortgage growth and income growth at the individual level remained positive during the boom consistent with a general expansion of mortgage credit rather than an expansion driven by people who are likely to be constrained. Foote et al. (2016) show that mortgage debt growth in the early 2000s and subsequent defaults happened throughout the income distribution. They conclude that this is not consistent with borrowing constraints but rather with the wealth hypothesis where the causality runs from house prices, or house-price expectations, to the accumulation of mortgage debt. In other words, the US crisis literature effectively debates over the importance

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<sup>2</sup>There is a literature estimating propensities to spend out of housing wealth based on aggregate data, e.g. Slacalek (2009), Carroll et al (2011) Case et al. (2005). However, discriminating between the underlying hypotheses requires micro level data that can accurately describe the heterogeneity of expectations and credit access and other consumer characteristics.

of the same three hypotheses.

The objective of this study is to provide a clean test of the housing wealth hypothesis, i.e. testing whether individual agents respond to subjective *unanticipated* home value changes by adjusting spending in the same direction, and to learn about the importance of mortgage borrowing in transforming housing wealth adjustments into spending adjustments. To do this we use Danish longitudinal data with subjective information about current and expected future house prices and income using the probabilistic survey questions as proposed by Manski (2004). Using this information it is possible to calculate unanticipated house price and income changes which do not rely on parametric assumptions about the formation of expectations. In this sense our data documents exactly what home owners believe about their wealth and not what the econometrician believes. At the same time subjective assessments are able to capture very local housing markets dynamics which is arguably an important type of variation. The subjective information about house prices is linked to high quality third party reported administrative records with information about savings in bank accounts, savings in financial assets such as stocks and bonds as well as information about bank and credit card debt. Finally, we link up to data obtained directly from mortgage banks with very detailed information about mortgage debt and the timing of refinancing decisions. These data makes it possible to regress spending as well as savings in different types of assets and liabilities on direct measures of anticipated and unanticipated innovations to house prices while controlling for the importance of credit constraints as well as anticipated and unanticipated income changes. This setup enables us to design a test of the housing wealth hypothesis which is close in spirit to the notion of a wealth effect as it derives from the life cycle framework in a setting where we can explicitly control for the two main competing alternative hypotheses while learning about the importance of the mortgage market in facilitating the link between changes in housing wealth and adjustments to spending.

We find that an unanticipated gain in housing wealth leads people to take up more mortgage

debt and we find no effect on other components of the portfolio. Overall, an unanticipated increase in housing wealth leads to an increase in mortgage extraction and spending of 3-5 percent of the unanticipated home value gain. We find no effect of negative shocks, i.e. the effect is asymmetrically related to positive and negative shocks. We test for the importance of credit constraints by splitting the sample according to the level of *ex ante* loan-to-value (LTV) ratio as well as by splitting the sample into groups with *ex ante* high and low levels of liquid assets, and we find that none of these indicators predict the spending response. We find that it is important to control for anticipated income losses implying that the common factor hypothesis is a potentially important confounder. The overall response to unanticipated gains in housing wealth is driven by about 11 percent of the observations where the respondent is recorded to have actively taken out a new mortgage. When we zoom in on this group we find that the majority are refinancers. As in the US, fixed rate mortgages (FRM) are important in Denmark and the mortgage system enable borrowers to refinance to lock in lower market rates. 3 out of 4 refinancers in the data take advantage of this opportunity and those who experience unanticipated price gains extract equity at the same time. These findings suggest that the unanticipated house price increases are translated into spending and that this effect is amplified by borrowers being able to lock in lower market interest rates at the same time. The findings resonate with the findings of Bhutta and Keys (2016), but we are, to the best of our knowledge, the first to show in detail how the mortgage market plays together with unanticipated house price gains in causing spending adjustments even for house owners who are not likely to be affected by severe credit market constraints. This result suggests that monetary policy can affect private spending when the mortgage system make it possible for FRM borrowers to actively lock in lower market interest rates *and* extract equity when housing wealth increases unexpectedly.

The contribution is facilitated by several unique features of our data. First, our new data allows us to consider both mortgage extraction as well as adjustments to the balance sheet and

spending. In particular, we exploit detailed longitudinal mortgage records to document the key role of the mortgage market, and because the data cover the entire budget constraint, it is also possible to measure the effect of unanticipated shocks on other parts of the portfolio as well as on total spending. This is in contrast to most studies who typically only observe mortgage debt (e.g. Bhutta and Keys, 2016) or spending (e.g. Campbell and Cocco, 2007). Second, the availability of subjective expectations data about house prices and income make it possible to perform a clean test of the housing wealth hypothesis while controlling for the productivity hypothesis without making parametric assumptions about how expectations are formed. In this way we provide a test that is close to the spirit of the theory. A few other papers have attempted to separate anticipated and unanticipated gains in housing wealth, but these studies typically have to make strong assumptions about the formation of expectations. Some studies estimate statistical models for house prices as in Campbell and Cocco (2007), Disney et al. (2010), and Browning, Gørtz and Leth-Petersen (2013), but identification in these models essentially hinges on the the parametric assumptions in the specification of the price proces, and these are typically strong.<sup>3</sup> One exception is Paiella and Pistaferri (2017) who use subjective asset price expectations, including house price expectations, to derive unanticipated asset price innovations for a sample of households in the Italian SHIW. However, the focus of their study is on measuring the effect of shocks to different asset prices and not on the role of the mortgage based home equity extraction. Further, an important feature of our data is that it holds well-measured indicators of credit constraints, including the loan-to-house value ratio and the holdings of liquid assets, and this allows us to provide different tests for the importance of credit/liquidity constraints. We are thus able to provide a strong test of the housing wealth hypothesis while controlling for the two leading alternative explanations, the collateral hypothesis and the common factor hypothesis, as an explanation for the existence of the correlation

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<sup>3</sup>That parametric specifications have important implications for the outcome is emphasized in an important paper Cristini and Sevilla Sanz (2014). They replicate the studies by Campbell and Cocco (2007) and Attanasio et al. (2009), who use the same data but reach different conclusions, and find that the two studies reach different conclusions because they use different specifications.

between house prices, mortgage extraction, and spending while imposing minimal parametric assumptions. Another advantage of combining data from survey and administrative sources is that it ensures that idiosyncratic response biases are not systematically driving both the information about shocks and about savings behaviour. Finally, the combined data set is longitudinal in nature, i.e. it includes repeated unanticipated house price and income changes as well as savings and spending data for the individuals in our sample. This allows us to examine the dynamic response to unanticipated home value gains and losses, and we exploit this to show that the spending effect is likely concentrated on durable spending. Furthermore, the longitudinal dimension permits us to examine whether unanticipated changes and the associated responses are tied to particular types of individuals. This turns out not to be the case suggesting that personal traits, such as preferences, personality, or other stable characteristics, are not driving the response to the unanticipated house wealth gains. A final advantage is that our data set is relatively large compared to other data sets that include subjective information. This allows us to document the effects nonparametrically and to illustrate graphically how unanticipated price changes factor into spending and savings decisions thus documenting the responses with a high level of transparency.

The next section presents the institutional context. After that the empirical model is outlined and the data presented. In section 5 results are presented. We start out presenting graphical bivariate evidence that unanticipated house price shocks drive debt accumulation and spending. We then move on to the multivariate analysis and estimate the housing wealth effect while controlling for competing explanations. Finally, we explore the importance of mortgage refinancing to lock in lower interest rates. The final section sums up and concludes.

## **2 Institutional context**

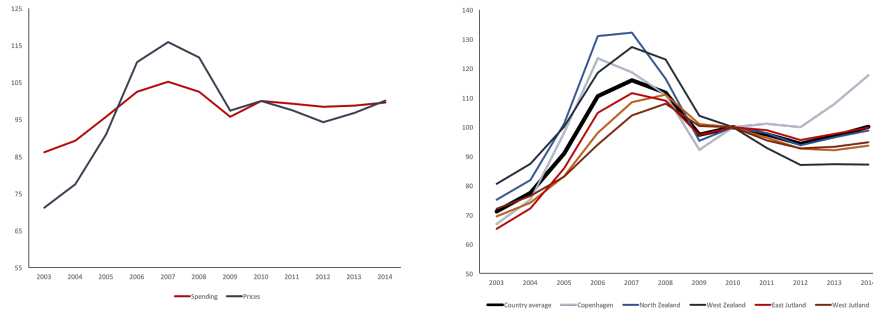
As in many developed economies house owners in Denmark experienced dramatic changes in

house prices in the 2000s. Prices increased by more than 60% on average during the run-up from 2003-2007 and then plummeted in 2008-2010 after which the overall price level has remained stable. This pattern is shown in Figure 1, left panel, which also pictures aggregate spending from the national account statistics. The figure documents the well established fact that house prices and aggregate spending move together. Figure 1, right panel, displays indices for prices for six selected regions in Denmark and it shows that prices developed very differently across the country over the period. In this study we are going to analyze data collected over the period 2011-2014 and even in this period, where the overall price level has been quite stable, prices have developed quite differently across the regions shown in the graph illustrating that there is in fact a lot of heterogeneity in how prices have developed across the country. Later, we shall make use of individual level assessments of home values which allows for very local price dynamics, increasing the potential for heterogenous price dynamics even further.

More than half of the adult population in Denmark are home owners at a given point in time, and many more are house owners at some point during their life time. Only a relatively small fraction hold financial assets such as bonds and stocks, and even for owners of such financial assets, the value of these assets constitutes a relatively small fraction of total assets. For most home owners the housing asset and the mortgage make up the two dominant portfolio components. Housing is financed primarily through mortgage banks, which are financial intermediaries specialized in the provision of mortgage loans. When granting a mortgage loan for a home in Denmark, the mortgage bank issues bonds that directly match the repayment profile and maturity of the loan granted. The bonds are sold on the stock exchange to investors and the proceeds from the sale is paid out to the borrower. A basic principle underlying the design of the Danish mortgage market is the balance-principle whereby total payments from the borrower and total payments from mortgage banks to mortgage bond holders must be in balance. This principle ensures that the mortgage banks face no funding risk and it also prevents them from charging any risk premium. Once the bank has screened potential borrowers based on the



Figure 1: House Prices and Spending



Notes: The left panel shows the evolution of house prices and household sector spending. The right panel shows the evolution of house prices in selected regions in Denmark. All series are indexed (2010=100). Sources: The Association of Danish Mortgage Banks and Statistics Denmark.

valuation of their property at the time of the loan origination and on their ability to service the loan, i.e. their income, all borrowers who are granted a loan of a given type at a given point in time face the same interest rate which is determined by the market.

Mortgage banks offer both fixed rate and adjustable rate loans. Loans can be of different maturity up to 30 years, and they can be issued up to a legally defined threshold of 80% of the house value at loan origination. A significant fraction of mortgage loans are fixed rate, and this also the case in the sample analyzed here. Fixed rate loans can be prepaid without penalties at face value at any time prior to maturity. In this sense the Danish mortgage market is similar to the US market, where long term fixed rate loans are also common and refinancing is also possible (Andersen et al., 2015). The possibility to prepay the loan at face value enables FRM borrowers to exploit changes in the market rate of interest in order to reduce the costs of funding. If the interest rate falls, an FRM borrower may prepay his loan and raise a new mortgage loan at the lower coupon rate and this is possible to do also for borrowers who have a LTV ratio that exceeds 80% if the balance is not increased as a result of refinancing. This implies that refinancing activity can be quite high when the market interest rate is declining. Refinancing with cash-out, i.e. where the principal is increased, is also possible as long as the

new loan is within 80% of the house value. For more details about the Danish mortgage system, we refer to Andersen et al. (2015) and Campbell (2012).

### 3 The empirical model

In order to test for the existence of a housing wealth effect we estimate a reduced form equation linking spending growth to unanticipated gains in home values while controlling for variables related to the alternative hypotheses. This approach is inspired by the life cycle framework positing that agents smooth marginal utility and make consumption and savings decisions to achieve this. Inherent to this framework is that agents distribute their known life time resources consisting of human, financial and housing wealth over time to smooth the marginal utility of consumption. In reality there is uncertainty about future resources. The individual forms *subjective* expectations about these and revise the spending and savings plan when new information about the level of future resources arrive, i.e. when wealth or income changes unexpectedly relative to his subjective expectations. For example, if at some point in time an agent learns that his wealth has increased more than he expected then that would lead him to increase consumption going forward. When credit markets are frictionless anticipated changes should have no impact on spending growth. The key is that the agent responds when the *information* about the windfall arrives rather than when the windfall itself arrives. For example, if the agent learns that house prices have increased more than he thought then this unanticipated gain yields an increase in life time resources that can be translated into increased spending. This is what we term the wealth effect. Critically for the wealth effect hypothesis is that the spending decision is not necessarily linked to the time at which the gain is realized which, in the case of housing, is when the house is sold. Of course, the ability to transform new information about a gain in wealth into consumption before the gain has actually materialized hinges critically on an assumption that asset markets work without frictions. In practice people

are likely to face many such frictions. For example borrowing rates may differ from lending rates, and households may even face hard borrowing limits such as in the Danish mortgage market, where it is only possible to mortgage up to 80% of the house value, and this is the collateral constraint. In this way an increase in the home value will make the collateral constraint less binding, and if this is the case then we would expect the spending response to be starkest among agents who are closer to the collateral constraint. In practice, house price increases and house price falls have asymmetric effects on the collateral constraint. What matters for the collateral constraint is the home value at the time where the loan is originated. Consequently, a house price increase adds collateral value whereas a house price drop does not entail that the lender requires the loan to be paid back at a higher pace than was originally planned. This could give rise to an asymmetric response to house price increases and falls. The common factor hypothesis claims that there is a factor driving both spending and house prices. One leading example is when (local) demand, and hence income, increases and causes both house prices and spending to increase. If this mechanism is at play then house price innovations could appear to be driving spending growth even if this is not a causal relationship. A general credit easing could also represent a common factor driving both spending and house prices. This effect is common to all households, but potentially operating with different intensity at different locations.

To capture these three effects we consider an empirical model relating the change in spending to expected and unexpected changes in house prices and incomes

$$\Delta c_{it} = \pi_0 + \pi_1 \theta_{it}^p + \pi_2 E_{it-1}[\Delta p_{it}] + \pi_3 \theta_{it}^y + \pi_4 E_{it-1}[\Delta y_{it}] + \mu_i + \lambda_t + \nu_{it} \quad (1)$$

$\pi_1, \dots, \pi_4$  are parameters to estimate.  $E_{it-1}[\cdot]$  is the expectation operator indicating individual  $i$ 's expectation as of period  $t - 1$ .  $p_{it}$  is the house value and  $y_{it}$  the income of individual  $i$  at time  $t$ .  $\theta_{it}^p = \Delta p_{it} - E_{it-1}[\Delta p_{it}]$  is the unanticipated change in the home value and  $E_{it-1}[\Delta p_{it}]$

is the anticipated change. Similarly,  $\theta_{it}^y = \Delta y_{it} - E_{it-1}[\Delta y_{it}]$  is the unanticipated income change and  $E_{it-1}[\Delta y_{it}]$  is the anticipated income change. Since the expectation is measured as of  $t - 1$  the expected value of the change in house prices and income can be re-stated as  $E_{it-1}[\Delta p_{it}] = E_{it-1}[p_{it}] - p_{it-1}$  and  $E_{it-1}[\Delta y_{it}] = E_{it-1}[y_{it}] - y_{it}$ .  $\mu_i$  is an individual level fixed effect, which is potentially correlated with the observed regressors. This allows fixed unobserved factors, such as preference parameters, to be determinants of the spending response to a house price change, even if we do not observe these factors.  $\lambda_t$  is a year fixed effect, which can be common across the sample or be specific to the municipality where the house owner live.  $\nu_{it}$  is a random error term.

Equation (1) splits price changes into expected price changes and unexpected price changes. Dividing innovations into expected and unexpected innovations increases the focus on the theory consistent notion that household consumption should respond only to unanticipated innovations. Hence if there is a housing wealth effect we would expect to see that  $\pi_1$  is significant. If consumers are not affected by constraints in the credit market and able to plan freely then we would expect that anticipated changes to house prices would have no impact on spending, i.e.  $\pi_2 = 0$ . However, if they are affected by constraints then anticipated increases in housing wealth could potentially be driving spending. However, this may not be a very powerful test for collateral constraints. For lifting the collateral constraint it is not important whether the increase in home value is anticipated or unanticipated. In order to provide a more powerful test of the collateral hypothesis we will also characterize the individuals in our sample in terms of *ex ante* LTV and availability of financial assets and estimate equation (1) for different subgroups defined according to these indicators of availability of credit and liquidity. Finally, equation (1) includes anticipated and unanticipated income growth. If (local) demand factors drive income which in turn drive both spending and house prices, then (un)anticipated income gains would be potential confounding factors which could bias the estimated effect of the unanticipated house price change. The income terms potentially also capture mortgage extraction that is re-

lated to using housing equity as insurance against adverse income shocks (Leth-Petersen, 2010; Hurst and Stafford, 2004). Including year fixed effects, which may be specific to the municipal level, also helps to control for common factors to the extent that these summarise shocks and revisions to expectations that are common to households in a particular municipality in a particular year.

The primary outcome is mortgage debt growth, but we will also apply (1) to learn about wealth accumulation through other portfolio components. This enables us to pinpoint what types of assets and liabilities are adjusted and thereby to learn how households manage their balance sheet following the arrival of unanticipated changes to their housing wealth. We will also take advantage of the fact that our data includes information about both income and total wealth to impute total spending as proposed by Browning and Leth-Petersen (2003). Finally, we will consider administrative records from the tax authorities which documents tangible spending related to house maintenance as well as the purchase of cars. More details about these outcome variables are presented in section 4.

In order to be able to identify the causal effect of unanticipated house price changes on spending it is required that unobserved components,  $\mu_i$  and  $\nu_{it}$  are uncorrelated with the explanatory variables in Equation (1).  $\mu_i$  could, for example, be correlated with  $\theta_{it}^p$  if the magnitude of the unanticipated home value gain is systematically related to unobserved characteristics, say, preference parameters. In a robustness check we estimate the equation by standard fixed effects methods and verify that this does not appear to be the case. Consequently, the effective identifying assumption is that  $\nu_{it}$  is uncorrelated with the observed variables. This assumption could, for example, be violated if  $\theta_{it}^p$  is driven by sentiments such that individuals who are generally confident in the overall development of the economy tend to have more optimistic expectations about the development of the value of their home and consequently have a lower unanticipated gain. In the survey we ask respondents about such sentiments and we will include these the regressions.

## 4 Data

The data used for estimating equation (1) are constructed by combining data from many different sources. The core is a longitudinal survey data set where respondents are asked about subjective expectations concerning their home value and income. The survey data are combined at the individual level with third-party reported administrative register data from mortgage banks and from the Danish Tax Agency (SKAT) with information about all assets and liabilities for the interviewed person, as well as a host of other administrative data providing background information about the respondent. Combining such high quality data sources, made possible by our ability to link individuals across modes of data collection using the Central Person Registry number, is, to our knowledge, unique and offers several advantages.

### 4.1 The Survey Data

To collect the subjective data about price and income expectations we commissioned the survey agency Epinion A/S to conduct a telephone survey in week 4-7 in the years 2011-2015. Each interview lasted 10-12 minutes and covered about 40 questions including the questions about subjective expectations to home values, income, and a range of other topics. The questions about expectations were placed in the beginning of the questionnaire following questions about the respondents' financial situation. We asked about expectations to the home value using probabilistic questions inspired by the work of Manski (2004). Specifically, we asked:

- *What is the maximum price you could get for your house one year from now?*
- *What is the minimum price you could get for your house one year from now?*

We denote the answer to the first question  $E_{it-1}[p_{it}^{max}]$  and the answer to the second question

$E_{it-1}[p_{it}^{min}]$ . Based on the answers we calculated the midpoint  $p_{it}^{mid} = \frac{(E_{it-1}[p_{it}^{max}] - E_{it-1}[p_{it}^{min}])}{2}$  and then asked

- *What is the chance that your house will be worth less than  $p_{it}^{mid}$ ?*

The answer to this question is denoted  $p_{it}^{mid}$ .

In order to quantify the subjective probability distribution over the home value 12 months ahead we interpret  $p_{it}^{min}, p_{it}^{mid}, p_{it}^{max}$  as points on the support of a normal distribution and assume that  $F_{it}^1 = \Phi(p_{it}^{min}) = 0.01$ ,  $F_{it}^2 = \Phi(p_{it}^{mid}) = F_{it}^{mid}$ , and  $F_{it}^3 = \Phi(p_{it}^{max}) = 0.99$ .<sup>4</sup> Following Dominitz (1997) and Manski (2004), for each observation we then estimate the mean and standard deviation of the subjective probability distribution by solving the least squares problem  $\min_{\mu_{it}^p, \sigma_{it}^p} \sum_{k=1}^3 \left[ F_{it}^k - \Phi\left(\frac{p_{it}^k - \mu_{it}}{\sigma_{it}}\right) \right]^2$ . The expected price one year ahead is then  $E_{it-1}[p_{it}] = \mu_{it}$ .

In the same survey we also ask

- *How much could you sell your house for today?*

Denoting the answer to this question  $p_{it-1}$  we can now calculate the expected price change  $E_{it-1}[\Delta p_{it}] = E_{it-1}[p_{it}] - p_{it-1}$ , which is one of the terms in equation (1). In the survey wave issued in the following year we then return to the same respondent and ask the same questions including  $p_{it}$ . With this information we can calculate the unanticipated change in the home value,  $\theta_{it}^p = \Delta p_{it} - E_{it-1}[\Delta p_{it}]$ . We also ask corresponding questions about the respondents annual income, and equipped with this information we are able to construct all the terms pertaining to anticipated and unanticipated changes in housing wealth and income on the right hand side of equation (1).

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<sup>4</sup>We also experimented with alternative assumptions ( $\Phi(p_{it}^{min}) = 0.005, \Phi(p_{it}^{max}) = 0.995$ ), and ( $\Phi(p_{it}^{min}) = 0.05, \Phi(p_{it}^{max}) = 0.95$ ) but that did not change the results in any important way.

The survey population is based on a random sample from the population of Danes who are active in the labor market. In this analysis we use survey data collected each January in the period 2011-2015.<sup>5</sup> Each year respondents who participated in the previous year were contacted and reinterviewed. The reinterview rate is about 75%, and in each round the sample is refreshed with new randomly selected subjects.

## 4.2 The Administrative Data

We use register data made available by Statistics Denmark from three different sources. First, we use a standard battery of merged administrative register data compiled by Statistics Denmark. These data include standard demographic information such as age, sex, education, household composition, address and moving date, and data about income and wealth collected through income-tax returns. The latter gives information about disposable income during the year and about wealth, which can be broken into a number of subcategories. This information allows us to construct asset classes such as net bank assets, including deposits as well as bank loans and any other type of loan not secured with real estate, and financial assets including the market value of bonds and stocks. Information is provided only about the market value of these financial assets, and we are therefore not able to trace whether movements in the total value of financial assets are related to active trading or passive movements related to capital gains. The wealth data are measured by their market value at the last day of the year. Because these data are collected annually for the entire Danish population they are longitudinal by nature; for this study we make use of data covering the period 2008-2014. The tax return data are known to be of high quality (Kleven et al, 2011) and have been used extensively in previous

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<sup>5</sup>The survey waves used in this paper are a continuation of a survey that was originally issued in January 2010 and based on random sample from the the Danish population of people active in the labor market. The first round of the survey did, however, not include questions about expectations to the home value. See Kreiner et al. (2016) for details about the original survey.



studies of savings behavior, see for example Browning et al. (2013), Leth-Petersen (2010) and Chetty et al. (2014).

The second type of register data includes detailed information about mortgage loans. These data cover the period 2009-2014 and include information about the terms of the mortgage, ie. the principal, the size of the outstanding debt, the coupon rate and the issue date. The data are collected by the Association of Danish Mortgage Banks (Realkreditrådet) and the Danish Mortgage Banks' Federation (Realkreditforeningen). They cover the five largest mortgage banks representing a total market share of 94.2% (Andersen et al., 2015). In combination with the income-tax return data we then have an almost complete picture of the balance sheet for all individuals in the Danish population.<sup>6</sup>

Spending is not recorded in administrative data, but we construct a measure of total spending,  $c_{it}$ , by subtracting from disposable income,  $y_{it}$ , the value of net savings and pension contributions, i.e.  $c_{it} = y_{it} - S_{it} - \Delta W_{it}$ , where  $S_{it}$  are pension contributions and  $\Delta W_{it}$  is the change in net wealth from period  $t - 1$  to  $t$ . The main challenge is that the imputation counts capital gains on stocks and bonds as savings and this can potentially misrepresent actual spending decisions. We document in a robustness check that this is not important in the current analysis. The imputation was proposed by Browning and Leth-Petersen (2003) who showed that it, while noisy, performs well in terms of matching the individual level expenditures in the Danish Expenditure Survey<sup>7</sup>, and it has been applied by Browning et al. (2013) and Leth-Petersen (2010) among others.

Besides the data described above, we have obtained data about two subcomponents of

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<sup>6</sup>We do not have information about informal borrowing and transfers outside the formal banking system, and we do not have information about high value items such as paintings and boats.

<sup>7</sup>Browning and Leth-Petersen (2003) examine the quality of the imputation using data drawn from the Danish Family Expenditure Survey (DES) for the years 1994–6. The DES gives diary and interview-based information on expenditure on all goods and services, which can then be aggregated to give total expenditure in a subperiod within the calendar year for each household in the survey. The households in the DES can be linked to their administrative income/wealth tax records for the years around their survey year, making it possible to directly check the reliability of the imputation against the self-reported total expenditure measure at the household level.

spending, and in both cases the data are collected from the tax authorities. One includes information about tax deductions for home maintenance and improvements. Since 2011 it has been possible to deduct expenditures related to home maintenance and improvements as well as expenditures for cleaning and housing services. The scheme only covers expenditure related to the labor input (not materials), and it is possible to deduct up to 15,000 DKK (1USD $\simeq$ 7DKK) per year. To get the deduction, receipts with information about the identity of the provider should be uploaded to the tax authorities through a dedicated home page, and it is the data collected here that we have gained access to directly from the tax authorities. Because these data provide the basis for actual tax subsidies they are audited and therefore very reliable. We will use these data to document spending on home improvements. The other documents the purchase of cars, and it is based on the official car register, which is used for collecting car taxes. Together these data complement the data with information about total spending by documenting two specific types of tangible spending.

### 4.3 The Combined Data

Estimation of (1) relies critically on data where the timing of the measurement is accurate. The administrative register data are summarized by the end of the year and the survey data are collected in January. The survey period was chosen to match the timing of the measurement in the administrative data as closely as possible. For example, the unanticipated change in the home value recorded for a given respondent in 2012 is  $\theta_{i2012}^p = p_{i2012} - p_{i2011} - (E_{i2011}[p_{i2012}] - p_{i2011})$ .  $p_{i2012}$  is collected in the survey in January 2013,  $p_{i2011}$  in January 2012, and  $E_{i2011}[p_{i2012}]$  in January 2011.  $\theta_{i2012}^p$  thus pertains to the end of 2012 which corresponds almost exactly to the timing of  $\Delta c_{i2012} = c_{i2012} - c_{i2011}$  where  $c_{i2012}$  is summarized by the end of 2012. Since the survey has been issued in 2011-2015 we are able to construct at most four consecutive terms summarizing the unanticipated home value change for an individual who participated in all survey rounds.

An advantage of the combined administrative and survey data is that they do not suffer from the same types of measurement error. We use the subjective data to construct the terms on the right hand side of equation (1) and the objective third party reported data to construct the outcomes, i.e. the left hand side of equation (1). In this way we are sure that idiosyncratic measurement errors related to the survey are not systematically driving both the left and the right hand side of the equation, a point formalized by Kreiner et al. (2015).

For doing the analyses we make a few sample selections. First, we include only observations for which we can identify the right hand side variables in equation (1), i.e. cases where we have answers from at least two consecutive survey waves. Second, we use only observations for house owners. This is because we only have subjective information about home values for home owners. Third, we drop people who are self employed. This is because the administrative wealth information does not separate business wealth and private wealth. Fourth, we drop observations for 216 individuals who move during the sample period. We do this because the change in the home value now includes adjustment to the home value that is the result of an active choice thus obstructing the identification of passive movements in the home value. As a result we are left with 12,788 observations for 5,207 individuals.

Table 1 presents summary statistics for the sample. The sample includes people aged 21 to 73 and on average the respondent is middle aged. The respondents are all home owners and the level of pre-tax income is about 400k DKK, which is above the average of the population at large, but matches the average among home owners in the population.<sup>8</sup> The average respondent holds a simple portfolio, which is dominated by the house and the associated mortgage. Typically, the respondent holds a very small amount of money in deposit accounts, and a limited amount in financial assets. In fact 27 percent has liquid wealth corresponding to less than one months

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<sup>8</sup>We also know the identity of the the non-respondents. In the Appendix we compare the characteristics of the respondents and the non-respondents based on information available in the administrative records. Non-respondents tend to be slightly younger, have slightly more expensive houses and more mortgage debt. However, in terms of demographics and income the differences are small, and overall, the respondents look quite similar to the non-respondents.

Table 1: Summary Statistics

	Mean	SD
Female	47.9	50.0
Age	51.9	10.8
Single	10.2	30.2
Gross Income	393.1	183.1
Bank Deposits (net)	5.2	309.0
Has Low Liquid Wealth (%)	27.2	44.5
Financial Wealth	69.1	204.3
Has No Financial Wealth (%)	60.5	48.9
Housing Wealth	1,046.9	962.2
Mortgage Loan to House value, LTV (%)	38.2	36.6
Have mortgage	73.1	44.4
Have FRM (if have mortgage) (%)	36.6	44.4
Number of observations	12,788	

Notes: Monetary variables are reported in 1,000 DKK. 'Low liquid wealth' is a dummy variable taking the value 1 when the respondent starts the period with liquid wealth worth less than one month's worth of disposable income. 'No financial wealth' is a dummy variable taking the value one if the respondent does not hold stocks or bonds.

disposable income. 60 percent holds no financial wealth, i.e. stocks and bonds, at all, and conditional on having financial wealth about half hold financial wealth worth less than 10 percent of annual income. 73 percent has a mortgage, and average LTV is about 40 percent, and among mortgage holders 36 percent has a FRM.

Respondents have different income and wealth levels. In order to get measures that are relative to the scale of each individuals' economy we normalize variables on both the left and the right hand-side of (1) by the average of the individuals income as measured in the administrative data over the period 2008-2010. To reduce the influence of outliers we censor all non-categorical variables at the 2nd and the 98th percentile of their distributions year-by-year. In our reference setup we analyze how individual level outcomes are related to unanticipated shocks. We do this because the survey is administered to individuals and the survey questions literally asks the respondent to state his home value and income. However, many of the decision arguably relate to household level decisions, and we will therefore return to this and present robustness analyses that consider outcomes calculated at the household level.

## 5 Results

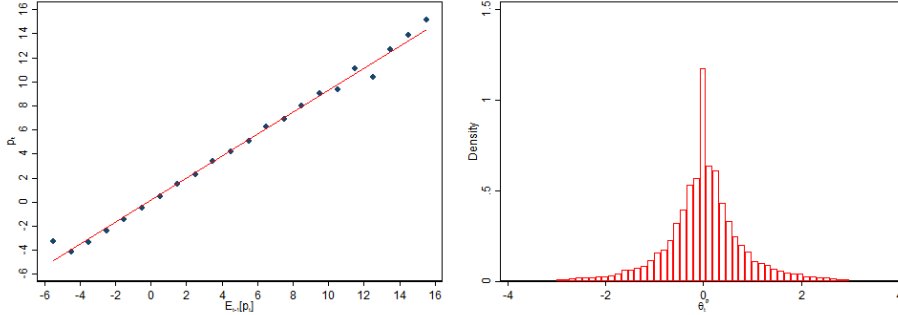
In this section we present results from estimating the response to unanticipated changes in home values on mortgage extraction, savings and spending decisions. We start out by presenting graphical evidence characterizing the anticipated and unanticipated home value changes and how they are correlated with the main outcome variables. After that, the multivariate analyses are presented.

### 5.1 Descriptive analysis

Individual expectations to future home values are very heterogenous. This could reflect that prices develop very differently across locations or that respondents have little sense of how house prices in their area will develop. In order to examine whether respondents' expectations to their future home value has any relation to how prices have actually developed when we ask them about this one year later, we present in the left panel of Figure 2 a binned scatterplot of stated actual home value changes,  $p_{it}$ , against expected home value changes,  $E_{t-1}[p_{it}]$ . The picture shows that respondents expectations accurately capture actual home values realized one year later. The right panel of Figure 2 presents a histogram of the unanticipated house price change,  $\theta_{it}^P = p_{it} - E_{t-1}[p_{it}]$ , and it show that at the individual level prices expectations stated in  $t - 1$  do not align perfectly with actual prices as perceived one year later. In terms of testing the wealth effect hypothesis the theory posits that individuals make spending and savings decisions according to their subjective expectations and the associated unanticipated home value changes.

We now turn to describe how unanticipated changes in home values are related to mortgage borrowing as well as other savings and spending decisions. In Figure 3 we investigate how unanticipated house price changes is related to the accumulation of mortgage debt. The figure has unanticipated home value growth on the horisontal axis, and mortgage debt growth on

Figure 2: The Relationship between Expected and Actual Home Value, and the Distribution of Unanticipated Price Changes.

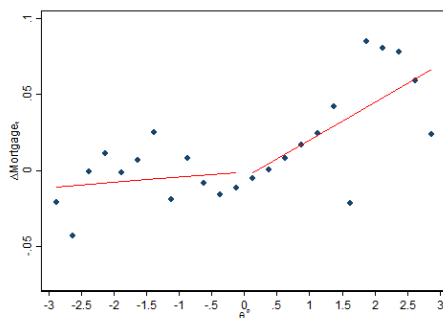


Notes: The left hand side panel shows the relationship between stated actual home value in period  $t$ ,  $p_{it}$  and expected home value changes as of  $t-1$ ,  $E_{it-1}[p_{it}]$ . Before constructing the graph  $p_{it}$  and  $E_{it-1}[p_{it}]$  are regressed on year dummies, and it is the residuals from these regressions that enter the plot. The panel shows a binned scatterplot (blue dots) where the bins are defined over equal intervals of  $E_{it-1}[p_{it}]$ . A regression lines (red) is overlaid. The right hand side panel shows a histogram of unanticipated house price changes. All variables are normalized on average income during 2008-2010 and censored at the 2nd and 98th percentile of their distribution in each calendar year.

the vertical axis. The relationship is shown as a binned scatterplot with a regression lines fitted separately for positive and negative values of the unanticipated home value growth,  $\theta_{it}^p$ . The panel shows a compelling relationship between the unanticipated home value growth and the growth of mortgage debt where positive values of the unanticipated home value growth are associated with mortgage debt growth whereas there is no systematic relationship with mortgage debt growth for negative values of  $\theta_{it}^p$ .

In Figure 4 we investigate how other components of the balance sheet are adjusted. Again, the unanticipated home value growth is on the horizontal axis, and on the vertical axis is net bank asset growth (left), and the growth in financial assets (right). Negative unanticipated gains in the home value,  $\theta_{it}^p < 0$ , appear to stimulate bank asset accumulation, but for positive values of,  $\theta_{it}^p > 0$ , the relationship is not systematically increasing with the size of  $\theta_{it}^p$ . The right panel of Figure 4 shows no evidence that unanticipated home value growth is systematically related to the growth in the value of the stock of financial assets.

Figure 3: Unanticipated Home Value Growth and Mortgage Debt Growth



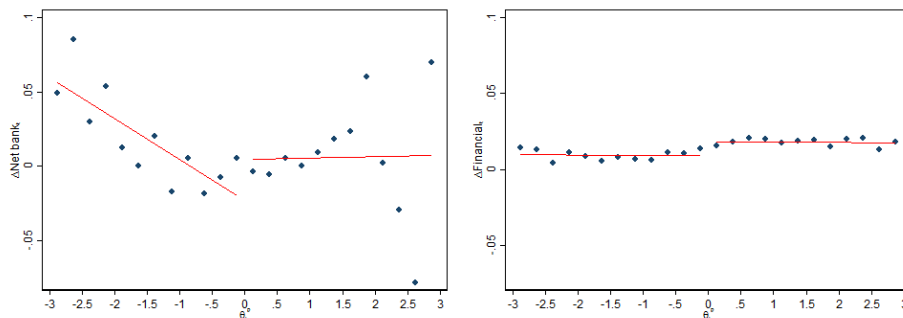
Notes: on the horizontal axis is unanticipated home value growth,  $\theta_{it}^p$ . On the vertical axis is mortgage debt growth. The dependent variable first regressed on year dummies and it is the residual from this regression that is used for constructing the panel. Mortgage debt growth is derived directly from records reported by mortgage banks. The panel shows a binned scatterplots (blue dots) where the bins are defined over equal intervals of  $\theta_{it}^p$ . Regression lines estimated separately for  $\theta_{it}^p \leq 0$  (red) are overlaid. All variables are normalized on average income during 2008-2010 and censored at the 2nd and 98th percentile of their distribution in each calendar year.

In Figure 5 we consider how two spending outcomes relate to unanticipated home value changes. The binned scatterplot illustrates that the spending growth variable is quite noisy, but the regression lines suggest that there is a positive association between of the unanticipated home value change and spending growth. Based on the slope of regression line the marginal propensity to spend out of an unanticipated home value gains is about 2-3 percent. The spending increase for positive values of  $\theta_{it}^p$  is consistent with the pattern of extraction of mortgage debt, and the spending drop for negative values of  $\theta_{it}^p$  can potentially be reconciled with the fact that negative values of  $\theta_{it}^p$  are also associated with accumulation of deposits. In the middle panel the outcome is the amount that has been reported to the tax authorities for home improvements. The scale is different (about 1/10) here than in the other panel, and this reflects the fact that the tax deduction only concerns a specific sub-component of total spending. There appears to be a positive association between unanticipated home value increases and the reporting of tax deductions for maintenance.<sup>9</sup> In the right panel another subcomponent of

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Figure 4: Unanticipated Home Value Growth and the Accumulation of Deposits and Financial Wealth



Notes: Both panels have on the horizontal axis unanticipated home value growth,  $\theta_{it}^p$ . On the vertical axis is net bank asset growth (left panel), and financial asset growth (right panel). In both cases is the dependent variable first regressed on year dummies and it is the residual from this regression that is used for constructing the panel. Net bank assets include all assets held in banks less any type of non-mortgage debt, Financial assets include the market value of stocks and bonds. Both panels show binned scatterplots (blue dots) where the bins are defined over equal intervals of  $\theta_{it}^p$ . Regression lines estimated separately for  $\theta_{it}^p \leq 0$  (red) are overlaid. All variables are normalized on average income during 2008-2010 and censored at the 2nd and 98th percentile of their distribution in each calendar year.

spending, car purchase, is considered. The dependent variable is a dummy variable taking the value one if the household has bought a new car. Also here, the bivariate relationship suggests that positive unanticipated home value increases are associated with car purchase.

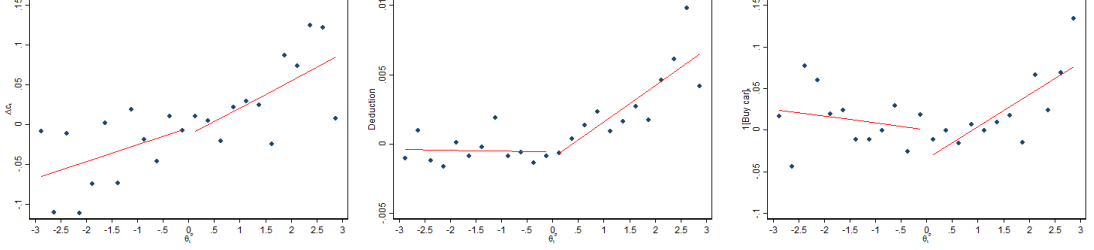
Overall, Figures 3-5 show evidence that unanticipated home value gains drive the accumulation of mortgage debt and to a lesser extent deposits, but there is no evidence that house price gains drive the accumulation of financial assets. The graphical analysis also suggests that unanticipated house price changes drive spending. The bivariate graphical analysis does, however, not take into account all the potential confounding explanations that we listed in the introduction, including expected future adjustments to income. To address this we now turn to

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One potential caveat associated with the association shown in the middle panel is that respondents who have undertaken maintenance or improvements of their home may subsequently report a higher value of their home,  $p_t$ , and this will reverse the causality. In the robustness section we will perform a check that this is not the driving force behind the results.



Figure 5: Unanticipated Home Value Growth and Spending, Tax deduction, and Car Purchase



Notes: Both panels have on the horizontal axis unanticipated home value growth,  $\theta_{it}^p$ . On the vertical axis is total spending growth (left), tax deduction for home renovation (middle), and dummy for car purchase in the household (right). In all cases the dependent variable is first regressed on year dummies and it is the residual from this regression that is used for constructing the panel. Total spending is imputed from income and wealth data as described in section 4.2. The value of tax deductions are obtained from the Danish tax authorities. Both panels show binned scatterplots (blue dots) where the bins are defined over equal intervals of  $\theta_{it}^p$ . Regression lines estimated separately for  $\theta_{it}^p \leq 0$  (red) are overlayed. Spending and the tax deduction are normalized on average income during 2008-2010 and censored at the 2nd and 98th percentile of their distribution in each calendar year.

the multivariate analysis where we can simultaneously take all the three channels into account.

## 5.2 Multivariate analysis and test of hypotheses

The multivariate analysis is based on estimating equation (1). Because the descriptive analysis clearly suggested that responses to positive and negative unanticipated changes in home values are asymmetric we allow for this in the multivariate analysis by estimating separate parameters for positive and negative values of  $\theta_{it}^p$ ,  $E[\Delta p_{it}]$ ,  $\theta_{it}^y$ , and  $E_{it-1}[\Delta y_{it}]$ .

The baseline estimates of equation (1) estimated by OLS are presented in Table 2. Each column in Table 2 shows estimation results from estimating independent OLS regressions with different dependent variables. In all regressions we control for year fixed effects as well as municipality fixed effects, variances of the subjective home value and income distributions. As discussed in section 4 one threat to identification could be that sentiments are correlated with subjective projections. To address this we include two dummy variables for positive and

negative sentiments.<sup>10</sup> Standard errors are clustered at the municipal level.<sup>11</sup> The first three columns focus on balance sheet adjustments and columns 4-6 consider spending outcomes.

The dependent variable in column 1 is mortgage debt growth. The estimated parameters for unanticipated home value increases and losses, which are the parameters of main interest, are presented in rows (a) and (b).<sup>12</sup> Rows (c)-(d) contain parameter estimates for expected price increases and drops. Rows (e)-(h) present the estimated parameters for unanticipated as well as anticipated income changes that are positive and negative in direction. Concentrating on the effect of unanticipated home value changes we find that an unanticipated price increase leads to accelerated mortgage debt growth and the effect is about 3 percent. The fact that there appears to be a significant effect only for positive unanticipated house price changes is consistent with the graphical evidence and the magnitude is also similar to the unconditional graphical analysis. Expected house price increases, row (c), are borderline significant. Interestingly, the results show that expected income declines, row (h), lead to deleveraging. This suggest that it is important to control for the expected income growth, cf. the common factor hypothesis. The variance of the subjective price and income distribution enter significantly. We have also estimated the model without including these variables (not reported), and the parameters of interest are not affected in any important way by their inclusion. Finally, the sentiment dummies are generally not significant and thus do not appear to have any important impact on the estimated price dummies. In column 2 and 3 we look into the balance sheet and consider adjustments of net bank assets, i.e. bank deposits less all non-mortgage debt, and financial

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<sup>10</sup>In each round of the survey we ask respondents: *Thinking about the Danish economy, how do you think it will develop this year?* Respondents are given the option to answer: *improve, no change, deteriorate*. The dummy variable for negative sentiments take the value 1 if the respondent answer *deteriorate* and the dummy variable for positive sentiments takes the value one if the respondent answers *improve*.

<sup>11</sup>House prices arguably vary at some local level. Our analyses suggest that this level is more local than the level of the municipality. Clustering standard errors at the municipal level is therefore conservative in terms of rejecting the null-hypothesis of no wealth effects.

<sup>12</sup>

Negative home value changes are coded as positive values, such that a positive parameter estimate is to be interpreted as an increase.

Table 2: Effect of Unanticipated Price Changes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$\Delta Mortgage$	$\Delta Netbank$	$\Delta Financial$	$\Delta Spending$	$\Delta Spending_{t+1}$	Deductions	Car purchase
(a)	$\theta^p > 0$	0.025*** (0.005)	-0.001 (0.003)	0.002 (0.002)	0.036*** (0.009)	-0.035*** (0.010)	0.001*** (0.000)
(b)	$\theta^p < 0$	-0.003 (0.004)	0.003 (0.005)	-0.002 (0.002)	-0.006 (0.008)	-0.010 (0.010)	0.001 (0.005)
(c)	$E[\Delta p] > 0$	0.023* (0.008)	-0.022 (0.016)	-0.000 (0.007)	-0.020 (0.032)	-0.018 (0.027)	0.025 (0.016)
(d)	$E[\Delta p] < 0$	-0.017 (0.010)	0.009 (0.010)	0.005 (0.005)	-0.032 (0.022)	0.034** (0.019)	-0.001 (0.010)
(e)	$\theta^y > 0$	-0.013 (0.016)	0.073*** (0.020)	-0.010 (0.012)	-0.012 (0.044)	0.000 (0.051)	0.005 (0.020)
(f)	$\theta^y < 0$	-0.013 (0.010)	-0.007 (0.011)	-0.012* (0.006)	-0.013 (0.024)	-0.041* (0.025)	0.003 (0.011)
(g)	$E[\Delta y] > 0$	0.002 (0.012)	0.032** (0.012)	0.016 (0.011)	-0.002 (0.032)	-0.004 (0.032)	0.028 (0.020)
(h)	$E[\Delta y] < 0$	-0.044** (0.021)	-0.005 (0.022)	0.029** (0.014)	-0.181*** (0.064)	-0.031 (0.073)	-0.019 (0.025)
(i)	$\sigma^p$	0.000*** (0.000)	0.000* (0.000)	-0.000 (0.000)	0.000** (0.000)	0.000*** (0.000)	0.000 (0.000)
(j)	$\sigma^y$	-0.001*** (0.000)	-0.001** (0.000)	-0.000 (0.000)	-0.001* (0.000)	-0.000 (0.000)	0.000 (0.000)
(k)	DK Economy +	0.004 (0.005)	-0.007 (0.006)	-0.003 (0.003)	0.003 (0.014)	-0.021* (0.013)	0.001 (0.008)
(l)	DK Economy -	-0.001 (0.004)	0.004 (0.006)	-0.001 (0.004)	-0.010 (0.013)	-0.024 (0.015)	-0.009 (0.009)
	Year FE	Yes	Yes	Yes	Yes	Yes	Yes
	Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
	Number of obs.	12,778	7,579	12,778	12,778	12,778	12,778

Notes: (a)  $\theta^p$  interacted with a dummy variable for  $\theta^p > 0$ . (b)  $\theta^p$  interacted with a dummy variable for  $\theta^p < 0$  and multiplied by -1, so that unanticipated house price falls enter the regression analysis with positive values. (c)  $E[\Delta p]$  interacted with a dummy variable for  $E[\Delta p] > 0$ . (d)  $E[\Delta p]$  interacted with a dummy variable for  $E[\Delta p] < 0$  and multiplied by -1, so that anticipated house price falls enter the regression analysis with positive values. (e)  $\theta^y$  interacted with a dummy variable for  $\theta^y > 0$ . (f)  $\theta^y$  interacted with a dummy variable for  $\theta^y < 0$ , and multiplied by -1, so that unanticipated income falls enter the regression analysis with positive values. (g)  $E[\Delta y]$  interacted with a dummy variable for  $E[\Delta y] > 0$ . (h)  $E[\Delta y]$  interacted with a dummy variable for  $E[\Delta y] < 0$ , and multiplied by -1, so that anticipated income falls enter the regression analysis with positive values. Standard errors are clustered at the municipal level. \* significant at the 10% level, \*\* significant at the 5% level, \*\*\* significant at the 1% level.

assets. The results indicate no adjustments related to unanticipated home value gains or losses.

The dependent variable in column 4 is spending growth. There is a significant effect of unanticipated house price increases, and it is of the same magnitude as the effect estimated for mortgage debt growth. None of the other parameters are clearly significant, except the parameter for anticipated income losses indicating that spending is reduced when an expected adverse income change arrives. In column 5 the outcome is the spending adjustment in the following year. Here, the parameter on positive unanticipated house price gains is significant. The estimated parameter is negative indicating that spending spikes up in the year where the unanticipated home value increase arrives but then reverts back in the following year. This points to that unanticipated housing wealth gains are transformed into spending on goods that are only purchased infrequently, such as durable goods. In column 6, the outcome is the amount spent on housing maintenance that is reported to the tax authorities in order to get a the tax subsidy. This is significant only for unanticipated house value gains. The effect is much smaller than for total spending, but this is natural as home improvements constitutes only one component of total spending. In column 7 the dependent variable is a dummy variable for car purchase. Here the results indicate that an unanticipated house price gains is related to car purchase, although the estimated parameter is only significant at the 10 percent level. The findings in column 6 and 7 confirm that some of the unanticipated home value gain is spent on durable goods. Overall, the findings for total spending mimics those of mortgage debt accumulation suggesting that spending increases are financed through housing equity extraction.

### **5.2.1 Robustness**

The results presented are potentially sensitive to some aspects of the design of the analysis. To confirm that the effects found are robust, a number of consistency checks are carried out. First, imputed spending is potentially sensitive to capital gains on bonds and stocks, and this could have influenced the results if house price increases are correlated with capital gains on

these assets. Second, we have used anticipated and unanticipated income growth as proxies for demand factors in order to control for the common factor channel. However, these may not capture all relevant demand factors, and we examine whether our results are sensitive to including municipal specific year dummies. Third, since home equity in some cases have been extracted to improve the value of the home this may have led respondents to report higher actual home values after the home improvement has been carried out. If this happens then the causality does not go from measured unanticipated home value increases to spending, but rather the other way. We will perform a test for whether this drives the results. Fourth, in the analysis we have used individual measurements. However, spending decisions may have been taken at the household level, and we will investigate whether this influences the results. In Table 3 we present the results from a series of robustness checks attempting at addressing these issues. For each of the robustness checks only the estimated parameters pertaining to the unanticipated changes in the home value are included, but the results are based on estimations including the full set of covariates also included the estimations reported in Table 2.

We start out by considering the importance of controlling for individual fixed effects. An interesting and unique feature of the data is the longitudinal dimension which makes it possible to control for fixed unobserved factors. Fixed unobserved factors could, for example, include preference parameters or other fixed factors. If such factors are determinants of the propensity to spend out of unanticipated home value gains, then they could bias the estimated effects. The results are presented in Table 3, row (a), They show that controlling for fixed unobserved effects does not change the parameter estimates pertaining to the unanticipated housing wealth gain significantly, although in some cases the significance level changes. The results suggests that the response to such unanticipated housing wealth gains is not biased by fixed idiosyncratic factors.

One important potential confounding factor is that movements in the value of housing assets might be correlated with movements in the prices of bonds and stocks. Also, the imputation

Table 3: Robustness Analyses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$\Delta Mortgage$	$\Delta Netbank$	$\Delta Financial$	$\Delta Spending$	$\Delta Spending_{t+1}$	$Deductions$	$Car purchase$
$\theta^p > 0$	0.038*** (0.008)	-0.003 (0.007)	0.002 (0.003)	0.053*** (0.015)	-0.055*** (0.020)	0.001** (0.000)	0.006 (0.005)
(a) $\theta^p < 0$	0.014*** (0.007)	0.004 (0.007)	-0.003 (0.003)	0.006 (0.013)	-0.023 (0.019)	0.000 (0.000)	0.006 (0.007)
$\theta^p > 0$	0.027*** (0.007)	0.002 (0.005)	-0.000 (0.001)	0.033*** (0.009)	-0.046*** (0.012)	0.001*** (0.000)	0.007 (0.005)
(b) $\theta^p < 0$	0.005 (0.006)	0.002 (0.005)	0.003** (0.001)	-0.013 (0.011)	-0.002 (0.011)	-0.000 (0.000)	-0.001 (0.006)
$\theta^p > 0$	0.024*** (0.005)	-0.001 (0.003)	0.001 (0.002)	0.033*** (0.009)	-0.031*** (0.010)	0.001*** (0.000)	0.005 (0.004)
(c) $\theta^p < 0$	-0.003 (0.004)	0.003 (0.005)	-0.002 (0.002)	0.005 (0.009)	-0.007 (0.010)	0.000 (0.000)	0.001 (0.005)
$\theta^p > 0$	0.017*** (0.005)	-0.001 (0.004)	0.003 (0.002)	0.022*** (0.010)	-0.022* (0.012)	-	0.006 (0.004)
(d) $\theta^p < 0$	0.004 (0.004)	-0.002 (0.005)	-0.003 (0.002)	0.008 (0.010)	-0.006 (0.010)	-	-0.003 (0.005)
$\theta^p > 0$	0.023* (0.012)	-0.003 (0.011)	0.002 (0.006)	0.016 (0.025)	-0.026 (0.026)	0.002* (0.001)	0.016 (0.019)
(e) $\theta^p < 0$	-0.009 (0.010)	0.003 (0.011)	0.002 (0.006)	-0.032 (0.025)	0.004 (0.024)	0.000 (0.001)	0.022 (0.019)
$\theta^p > 0$	0.070*** (0.013)	-0.003 (0.011)	0.004 (0.005)	0.096*** (0.023)	-0.088*** (0.028)	0.003*** (0.001)	0.006 (0.006)
(f) $\theta^p < 0$	-0.008 (0.011)	0.007 (0.017)	-0.009 (0.008)	0.006 (0.023)	-0.011 (0.032)	0.001 (0.001)	-0.003 (0.008)

Notes: The table reports results from a series of robustness checks. Each row reports estimates from estimation of a model that involves all the covariates used in Table 1, but where only of  $\theta^p > 0$  and  $\theta^p < 0$  are reported. (For each of the robustness checks the full set of estimates are tabulated in the Appendix). Similarly to the estimation results in Table 2 each (column, row) cell is estimated by independent OLS regressions.

(a) repeats the estimations in Table 1 while controlling for individual fixed effects. (b) repeats the estimations in Table 1 but is based on a sample of observations where no stock or bond holdings is observed at the beginning of the period. (c) repeats the estimation reported in Table 1 but includes municipality $\times$ year fixed effects. (d) is based on a sample excluding all observations where deductions for house improvements are recorded. (e) is based on estimation by two stage least squares where the unanticipated price increase/decrease is instrumented with the municipal level house price growth obtained from a house price index published by the association of Danish mortgage banks, Finance Denmark (f) repeats the estimation in Table 1, but where the outcome is calculated at the household level. Standard errors are clustered at the municipal level. \* significant at the 10% level, \*\* significant at the 5% level, \*\*\* significant at the 1% level.

may count capital gains on bonds and stocks as savings when it is in reality merely passive movements in asset prices. In order to assess whether this drives our results, we have reproduced the result from Table 2 for a subsample that does not hold bonds or stocks, and the results are reported in Table 3, row (b). The omission of bonds/stock holders does not change the result, although the parameter estimate for car purchases become insignificant due to larger standard errors.

One of the hypotheses states that there may be a third factor driving both spending and house prices. The previous analysis found that anticipated as well as unanticipated income growth did not confound the estimated effect of unanticipated home value growth on mortgage and spending growth. However, an alternative third factor could be a general local expansion of credit that stimulates local demand. This is difficult to measure directly, but we try to add municipality times year fixed effects. This will pick up municipality specific time varying factors including such local aggregate effects. The results are reported in Table 3, row (c). The results from this analysis are for all practical purposes identical to the original analysis confirming that the common factor hypothesis is unlikely to play an important role in our data set and it also points out that much of the identifying variation in the analysis comes from an even more local level, either because there are indeed very local markets within municipalities, or because the expectations of the respondents really reflect subjective expectation about how prices will develop.

The analysis is based on the assumption that the difference between actual and anticipated price changes identify a truly unanticipated price change, i.e.  $\theta_{it}^p = p_{it} - p_{it-1} - (E_{it-1}[p_{it}] - p_{it-1})$ , and that this surprise is uncorrelated with the decision to spend in period  $t$ . However, we have found that there is significant mortgage based extraction of housing equity and that at least some people use this to renovate or maintain their house. A potential threat to our approach to identifying the effect of unanticipated home value gains is that house owners may improve their home and subsequently report a higher value of  $p_{it}$  because improve-

ments have been made during the year. For this to bias the results, the decision to make home improvements must not have been taken before  $E_{it-1}[p_{it}]$  was reported, because the value of the improvements would then have already been included in the expectation. To check for the possibility that the reporting of  $p_{it}$  is endogenous, we make a robustness check where we re-estimate the basic specification but omit observations who have claimed deductions for home improvements. This reduces the sample size by some 20 percent, arguably also removing some relevant variation. These results are shown in Table 3, row (d), and they show that there is still a significantly positive effect of an unanticipated increase in the home value on mortgage debt growth and on spending. The point estimates are slightly smaller than the reference estimates in Table 2, but they are not significantly different. This confirms that our main estimates are not purely the result of endogenous home value reporting. This analysis does not take into account that people may have improved their home without having claimed the tax deduction. In order to address that concern with also take another approach where the model is estimated by two stage least squares and unanticipated price gain is instrumented with the aggregate municipal level growth in the prices of traded houses. This is based on a municipal level house price index published by the association of Danish mortgage banks, Finance Denmark. This approach effectively exploits variation that is not collected at the individual level and hence cannot be the result of endogenous reporting. The findings are reported in Table 3, row (e). Two stage least squares is notoriously inefficient and the standard errors more than double in size compare to the standard errors reported in Table 2<sup>13</sup>, and this generally reduces the level of significance. However, mortgage debt growth is still significant and of the same order of magnitude as the baseline estimates, and all the remaining point estimates are generally similar to and lie within two standard errors from the estimates reported in Table 2. These findings suggests that endogenous reporting did not cause the estimated parameters to be biased.

The analysis has so far been based on individual level information, but some of the spending

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<sup>13</sup>The t-statistics in the first stage regression of  $\theta_{it}^p$  on the instrument is 35 for the regression of positive unanticipated and 39 for the regression of unanticipated negative changes



decisions could have obviously have been made at the household level. In Table 3, row (f) we have implemented the analysis where all outcome variables are measured at the household level. This analysis confirms the previous findings that unanticipated increases in home values drive mortgage debt accumulation and expenditure growth.

Overall, the results presented in Table 3 confirms that mortgage debt accumulation result is robust to a list of potentially important confounding factors. All the alternative specification tested are more data demanding than the specification used in Table 1 and are therefore generally associated with larger standard errors. However, mortgage debt growth remain significantly related to unanticipated home value increases, and the point estimates for the other outcomes remain similar even if they are not estimated as precisely as in the references specification presented in Table 2.

### 5.2.2 Credit constraints

One of the main hypotheses for the underlying correlation between house prices and mortgage extraction is the collateral or credit channel. In order to better understand whether the spending response is in fact related to collateral constraints we split the sample at the median value in the LTV distribution, where LTV is summarized at the beginning of the period, ie. before potential wealth gain is realized, and estimate the model separately on the two sub-samples. Results from doing this are reported in Table 4, rows (a) and (b). We only include the estimated parameters pertaining to the unanticipated changes in the home value in Table 4 and report the full set of estimates in the Appendix. Comparing the estimates in row (a) with those in rows (b) the propensity to spend and the propensity to extract housing equity appears to be similar across the two groups.<sup>14</sup> In rows (c) and (d) we attempt an alternative indicator for constraints and split the sample according to whether the respondent starts out period  $t$  with

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<sup>14</sup>We also attempted at splitting the sample into three LTV groups,  $LTV=0$ ,  $0<LTV<50$  and  $LTV>50$ , in order to separate out those who are closer to the borrowing limit more accurately. However this split asks even more of the data, and differences were not statistically significantly different. These results are not reported.

liquid assets with less and more than two months disposable income, a measure of constraints often used in previous studies (e.g. Zeldes, 1989, Leth-Petersen, 2010). The results show no significant difference between the two groups and they also suggest that the response is not driven by constraints. Nonetheless, the results strongly indicate that unanticipated home value increases generate extraction of housing equity and associated spending increases. In the next section we will look into the pattern of equity extraction in more detail.

### 5.2.3 Refinancing

The results presented so far relate unanticipated home value gains to home equity extraction. In order to establish this more precisely we identify observations where an active mortgage transaction is recorded. By an active mortgage transaction we mean that a new mortgage loan has been taken out. 1,474 observations fall into this category. Figure 6 presents a binned scatter plot of the propensity to actively take out a new mortgage against the unanticipated home value growth. The picture shows very clearly that the propensity to actively take out a new mortgage is increasing with the size of the unanticipated home value growth when this is positive. For negative values of the unanticipated home value growth there is no systematic relationship. This confirms the suspicion that the spending effect documented above is driven by people who actively take out a new mortgage. This claim is further backed by the fact that the spending response documented in the previous section disappears (not reported) when holding these observations out of the data set.

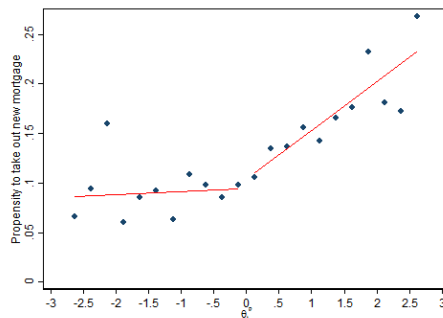
24 percent of those who actively take out a new mortgage do so without closing another, but 76 percent take out a new mortgage while at the same time ending another mortgage loan, i.e. they are refinancing. In the latter group 86 percent terminate a fixed rate mortgage (FRM). People holding FRMs can potentially gain from refinancing when the market rate has changed sufficiently since the loan was established. In the period considered, the market interest rate has

Table 4: Effect of Unanticipated Price Changes by level of LTV

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$\Delta Mortgage$	$\Delta Net\ bank$	$\Delta Financial$	$\Delta Spending$	$\Delta Spending_{t+1}$	<i>Deductions</i>	<i>Car purchase</i>
$\theta^p > 0$	0.029*** (0.009)	-0.004 (0.005)	0.001 (0.003)	0.035*** (0.014)	-0.018*** (0.015)	0.001*** (0.000)	0.006 (0.007)
$\theta^p < 0$	0.000 (0.005)	-0.001 (0.007)	-0.004 (0.004)	0.003 (0.014)	0.001 (0.016)	0.000 (0.000)	0.002 (0.007)
$\theta^p > 0$	0.019*** (0.006)	-0.000 (0.005)	0.002 (0.002)	0.031*** (0.011)	-0.046 (0.012)	0.001*** (0.000)	0.007* (0.004)
$\theta^p < 0$	-0.007 (0.005)	-0.004 (0.006)	-0.002 (0.003)	0.012 (0.013)	-0.016 (0.011)	-0.000 (0.000)	0.003 (0.006)
$\theta^p > 0$	0.018** (0.007)	-0.009 (0.006)	0.004* (0.002)	0.022** (0.010)	-0.038** (0.015)	0.001** (0.000)	0.004 (0.006)
$\theta^p < 0$	-0.006 (0.007)	-0.001 (0.006)	-0.003 (0.002)	0.006 (0.013)	-0.007 (0.012)	-0.000 (0.000)	0.002 (0.008)
$\theta^p > 0$	0.030*** (0.007)	-0.008 (0.005)	-0.001 (0.003)	0.042** (0.013)	-0.030** (0.014)	0.001*** (0.000)	0.009** (0.004)
$\theta^p < 0$	-0.001 (0.005)	0.005 (0.007)	-0.002 (0.004)	0.015 (0.012)	-0.009 (0.014)	0.000 (0.000)	0.001 (0.006)

Notes: The table reports results from a series of robustness checks. Each row reports estimates from estimation of a model that involves all the covariates used in Table 1, but where only of  $\theta^p > 0$  and  $\theta^p < 0$  are reported. (The full set of results are tabulated in the Appendix). Similarly to the estimation results in Table 1 each (column, row) cell is estimated by independent OLS regressions. (a) repeat the estimations in Table 1 but is based on a sub sample of observations where the respondent has a LTV ratio above the median by the beginning of the period. (b) repeat the estimations in Table 1 but is based on a sub sample of observations where the respondent has a LTV ratio below the median by the beginning of the period. (c) repeat the estimations in Table 1 but is based on a sub sample of observations where the respondent has a liquid asset worth less than two months of disposable income by the beginning of the period. (d) repeat the estimations in Table 1 but is based on a sub sample of observations where the respondent has a liquid asset worth more than two months of disposable income by the beginning of the period. Standard errors are clustered at the municipal level. \* significant at the 10% level, \*\* significant at the 5% level, \*\*\* significant at the 1% level.

Figure 6: Propensity to actively take out new mortgage



Notes: The panel has unanticipated home value growth,  $\theta_{it}^p$ , on the horizontal axis and a dummy variable indicating whether a loan has been refinanced on the vertical axis. The graph shows a binned scatterplot (blue dots) where the bins are defined over equal intervals of  $\theta_{it}^p$ . Regression lines estimated separately for  $\theta_{it}^p \leq 0$  (red) are overlaid. The unanticipated home value growth is normalized on average income during 2008-2010 and censored at the 2nd and 98th percentile of their distribution in each calendar year.

declined and refinancing is relevant in order to lock in a lower market rate. It is advantageous to refinance a FRM when the market rate has dropped significantly and when the loan has a certain volume and when there is sufficient time until maturity. Agarwal et al (2013) have developed a formula describing when it is optimal to refinance. According to this, optimal refinancing depends, among other things, on the size of the mortgage, transaction costs and the mortgage interest gain from refinancing. Andersen et al. (2015) show that few borrowers refinance optimally. Mortgage banks and financial advisors apply rules-of-thumb when advising their customers about the potential profitability of refinancing FRMs. The exact rules-of-thumb vary slightly across mortgage banks and financial advisors, but a typical rule is that it would potentially be profitable to refinance if the volume of the loan is at least 500k DKK, time until maturity is at least 10 years, and if the market interest rate is 1.5 percentage points lower than the coupon rate on the existing loan.<sup>15</sup> Based on the mortgage data we are able to apply this rule-of-thumb in order to identify whether it is potentially profitable to refinance an existing

<sup>15</sup>see for example, <https://www.rd.dk/da-dk/privat/Omlaegning-af-laan/Guides/Pages/Guides.aspx> and <https://www.brf.dk/omlaeg-laan/vaerd-at-vide/hvornaar-skal-du-omlaegge>.

Table 5: Rule-of-Thumb Refinancing

	(1)	(2)	(3)	(4)
	<i>New Loan, No Refinance</i>	<i>Refinance</i>	$\Delta$ <i>Mortgage</i>	$\Delta$ <i>Debt Service</i>
$\theta^p > 0$	0.017** (0.07)	0.022** (0.007)	0.022* (0.09)	-0.014 (0.019)
$\theta^p < 0$	-0.004 (0.005)	-0.003 (0.005)	0.006 (0.007)	0.000 (0.014)
$[\theta^p > 0] \times D$	-0.024 (0.012)	0.056 (0.040)	0.067** (0.033)	0.049 (0.033)
$[\theta^p < 0] \times D$	-0.009 (0.007)	-0.043 (0.044)	-0.006 (0.035)	-0.030 (0.027)
$D$	-0.008* (0.005)	0.313*** (0.030)	-0.032* (0.018)	-0.420*** (0.025)

Notes: The table reports results from regressions corresponding to regression (1), except that  $D$  is added as a regressor as well as its interactions with  $\theta^p \leq 0$ . The regressions reported in the table are based on a subset consisting of 8,664 observations selected from the original sample by including only observations where the respondent is recorded with a mortgage loan in the beginning of the period and where  $LTV_{t-1} < 60$ . Finally, the sample is also restricting by omitting extreme values of the unanticipated price change, i.e. including  $-2.5 < \theta^p < 2.5$ . The dependent variable in column (1) is a summary variable taking the value 1 if the respondent has taken out a new mortgage without closing another. in column (2) the dependent variable is a dummy variable taking the value one if the respondent is recorded as having refinanced, ie. both established a new mortgage and closed another. The dependent variable in column (3) is the mortgage debt growth (normalized on average income calculated over the period 2008-2010). This is similar to the outcome modelled in column 6 in Table 1-3. The dependent variable in column (4) is the debt service growth rate from  $t - 1$  to  $t$ . Standard errors are clustered at the municipal level. \* significant at the 10% level, \*\* significant at the 5% level, \*\*\* significant at the 1% level.

FRM for all the observations in our data set, and we find that in 1,227 cases the rule-of-thumb suggest that it is potentially profitable to refinance. In order to examine whether equity extraction takes places when it is profitable to refinance we re-estimate equation (1) adding  $D_{it}$  as a regressor as well as interactions between unanticipated house price changes and  $D_{it}$ . To the extent that the development in the market interest rates is unpredictable,  $D_{it}$  identifies the subgroup in the sample who will potentially profit from refinancing based on plausible exogenous variation. For the regressions we focus on a subsample consisting of 8,664 observations selected from the original sample by including only observations where the respondent is recorded with a mortgage loan in the beginning of the period and where  $LTV_{t-1} < 60$ . The latter restriction is imposed in order consider only potential refinancers who have the capacity to extract equity.

The results are shown in Table 5. In column 1 the dependent variable is a dummy variable taking the value 1 if the respondent establishes a new mortgage without closing another, i.e. the respondent extracts equity but not in connection with a refinance. The results indicate the propensity to extract without refinancing is positively related to unanticipated price increases, but negatively related to having an incentive to refinance. In column 2 the dependent variable is a dummy variable taking the value 1 if the respondent has refinanced, and in column 3 the dependent variable is mortgage debt growth (also analyzed in col. 1 in Table 2-4). Turning to the results in column (2) the parameter estimates show that irrespective of whether the respondent has experienced an unanticipated house price gain,  $D_{it}$  significantly predicts that the respondent will refinance. This is as expected because it is profitable to refinance also without extracting housing equity in order to lock in the lower market interest rate. The propensity to refinance appears to also increase with unanticipated house price gains suggesting that house owners with positive unanticipated price increases are more likely to refinance irrespective of whether the rule-of-thumb indicates that it would be profitable for them to do so. This could reflect that these house owners refinance even when it is not profitable or that the rule-of-thumb is too crude to capture all the circumstances that make it advantageous for borrowers to refinance. In column (3) the outcome is the change in size of the mortgage. Interestingly, the mortgage debt growth is not driven by the incentive to refinance as witnessed by the fact that the parameter on  $D_{it}$  is negative and borderline insignificant. Comparing with results from column (1) this suggest that a significant fraction of refinancing takes place without extracting additional equity. Instead, mortgage debt growth is significantly positively correlated with the existence of positive unanticipated price innovations, and the effect is three times as big for the subgroup who, according to the rule-of-thumb, has an incentive to refinance their mortgage to lock in a lower market interest rate and at the same time experiences an unanticipated price gain. In column (4) the dependent variable is the relative debt service adjustment. For refinancers not taking any equity we would expect debt service to become smaller, and for

refinancers who extract equity the debt service will be smaller because they now have to service a bigger loan, albeit at a lower interest rate. The results presented in column (4) shows exactly that. Debt service is significantly reduced for people who have an incentive to refinance but did not experience an unanticipated price change. In summary, the results presented in this section shows that home equity extraction is amplified when borrowers have an incentive to refinance to lock in a lower market interest rate.

#### **5.2.4 Link to previous studies**

We estimate a marginal propensity to extract equity of unanticipated housing wealth of 3 percent, and the effect is higher for house owners who refinance. The effect is precisely estimated and broadly in line with most of the existing evidence finding a marginal propensity to consume that is no bigger than 10%. Methodologically, our study is most directly linked to the studies by Browning et al. (2013), Campbell and Cocco (2007), Disney et al. (2010), and to Paiella and Pistafferri (2017) who also attempt to identify the effect of unanticipated house price changes. Using Danish data for the early 1990s Browning et al. (2013) find that an overall MPC of about 3% and document that it is driven by young liquidity constrained home owners. Campbell and Cocco (2007) use UK household budget data and find an MPC of 10%. They find that the effect is biggest among the most mature individuals in the sample supporting the notion of a true wealth effect. Disney et al. (2010) use the British Household Panel Survey 1994–2003 and find a marginal propensity to consume out of surprise innovations to housing wealth of maximally 1%. Paiella and Pistafferri (2016) use data from the Italian SHIW. They find an MPC of 3%, and the effect is related to expected price changes suggesting that the effect is driven by home owners who are liquidity constrained. Common to these studies is that the effects are quite imprecisely estimated. Given this and the fact that they are based on different types of data covering different periods it is not surprising that conclusions about the underlying driving factors differ. Unlike any of these previous studies we are able to tie the spending decision

to a subset of households engaging in mortgage based housing equity extraction, and to show that the response is more intense among house owners who have an incentive to refinance an existing FRM loan to lock in a lower market interest rate while at the same time extracting equity. This transparently shows that the mortgage market plays a key role in understanding the effects of unanticipated home value gains on spending even for house owners who are not affected by severe constraints. These findings are broadly consistent with those of Bhutta and Keys (2016).

## 6 Conclusion

By implication of the life cycle hypothesis unanticipated gains in home values potentially cause spending to grow. This study investigates the empirical relationship between unanticipated home value gains, mortgage extraction, and spending decisions by using longitudinal survey data with subjective information about current and expected future house prices to calculate unanticipated house price changes. The subjective information is linked at the person level to high quality administrative records with information about savings in various financial instruments to provide a test of the housing wealth hypothesis that is close in spirit to the theoretically motivated hypothesis.

We estimate the average marginal propensity to extract housing equity and to spend out of unanticipated housing wealth gains to be 3%. This estimate conceals important heterogeneity in the responses. Unlike any of the existing studies, our study highlights the importance of being able to consider both spending effects and adjustments of the balance sheet. We do not find evidence that unanticipated house price drops have any effect on the spending or any important effects on the balance sheet. However, we show that unanticipated house price gains factor into spending through the extraction of housing equity. The estimated effect is robust to controlling



for expected future income changes, and we find no sign that the effect is driven by individuals who are likely to be affected by severe collateral or liquidity constraints. Moreover, and unique to this study, we have longitudinal data on unanticipated home value gains, balance sheets, and spending and we use this to show that the spending increase associated with an unanticipated house price gain is accompanied by a corresponding drop in spending in the following period. This finding suggests that spending is concentrated on durable goods. We are thus able to paint a very detailed and precise picture of how unanticipated housing wealth gains factor into the household budget and spending decisions. In particular, we document that the effect is driven by about 11% of the respondents who are recorded having actively taken out a new mortgage loan. Among these households, we show that those who have a strong motive to lock in lower market rates by refinancing an existing mortgage, do so, and at the same time extract equity when they have experienced an unanticipated gain in housing wealth, and this happens even when they are not affected by severe collateral constraints. Interest rate declines thus boost the effect of unanticipated house price increases on mortgage extraction and spending. Specifically, this happens when the mortgage system makes it possible for FRM borrowers to actively lock in lower market interest rates *and* extract equity when housing wealth increases unexpectedly. These results point to the existence of a housing wealth effect that is intimately connected to the functioning of the mortgage market, suggesting that monetary policy can play a role in amplifying the effect of housing wealth gains on spending by affecting interest rates on mortgage loans.

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Appendix  
to

Housing Wealth Effects and Mortgage Borrowing  
The Effect of Subjective Unanticipated Home Price Changes on Home Equity  
Extraction

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Table A1: Summary Statistics

	Sample=0		Sample=1		Difference	t-value
	Mean	SD	Mean	SD		
Female	45.6	49.8	47.9	50.0	-2.3	-3.7
Age	49.5	11.0	51.9	10.8	-2.4	-17.2
Single	13.5	34.1	10.2	30.2	3.3	8.1
LTV	47.9	36.3	38.2	36.6	9.7	21.2
Gross Income	399.8	211.2	393.0	183.1	6.8	2.7
Financial Wealth	61.1	198.8	69.1	204.3	-8.0	-3.2
Bank Deposits (net)	-41.2	354.3	5.2	309.0	-46.4	-11.2
Mortgage Debt	819.0	768.3	592.7	612.6	226.2	25.8
Housing Wealth	1,389.7	1,222.6	1,046.9	962.1	342.7	24.7
N	12425		12788			

Notes: Monetary variables are reported in 1,000.

Table A2: Robustness Analysis (a)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta$ Spending	$\Delta$ Spending,t+1	Deductions	$\Delta$ Net Bank	$\Delta$ Financial	$\Delta$ Mortgage
	b/se	b/se	b/se	b/se	b/se	b/se
$\theta^p > 0$	0.053*** (0.015)	-0.055*** (0.020)	0.001** (0.000)	-0.002 (0.007)	0.001 (0.003)	0.038*** (0.008)
$\theta^p < 0$	0.006 (0.013)	-0.023 (0.019)	0.000 (0.000)	0.004 (0.007)	-0.004 (0.003)	0.014** (0.007)
$E[\Delta p] > 0$	0.022 (0.049)	-0.008 (0.049)	-0.002 (0.001)	-0.037* (0.021)	0.009 (0.012)	-0.000 (0.016)
$E[\Delta p] < 0$	-0.036 (0.031)	0.046 (0.039)	-0.001 (0.001)	0.002 (0.015)	0.002 (0.008)	-0.012 (0.013)
$\theta^y > 0$	0.085 (0.054)	0.029 (0.077)	0.003 (0.002)	0.027 (0.022)	-0.024 (0.018)	0.006 (0.022)
$\theta^y < 0$	0.069* (0.036)	-0.010 (0.047)	0.001 (0.001)	-0.028* (0.015)	-0.016* (0.009)	0.001 (0.015)
$E[\Delta y] > 0$	0.044 (0.049)	0.033 (0.047)	0.000 (0.001)	0.000 (0.019)	0.011 (0.014)	0.005 (0.021)
$E[\Delta y] < 0$	-0.125 (0.084)	0.126 (0.105)	-0.001 (0.002)	-0.053* (0.027)	0.011 (0.022)	-0.051** (0.025)
$\sigma^p$	0.001*** (0.000)	-0.021 (0.015)	0.000 (0.000)	-0.000*** (0.000)	0.000*** (0.000)	-0.000 (0.000)
$\sigma^y$	-0.003*** (0.000)	-0.009 (0.008)	-0.000 (0.000)	0.001*** (0.000)	-0.000*** (0.000)	-0.000** (0.000)
DK Econ +	0.012 (0.020)	-0.042 (0.026)	-0.000 (0.001)	-0.009 (0.008)	-0.005 (0.004)	0.003 (0.007)
DK Econ -	0.027 (0.022)	-0.041 (0.028)	-0.001* (0.001)	-0.002 (0.009)	-0.003 (0.007)	0.008 (0.008)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
N	12788	7579	12788	12788	12788	12788

Notes: The table repeats the estimations in Table 1 while controlling for individual fixed effects. Standard errors are clustered at the municipal level. \* significant at the 10% level, \*\* significant at the 5% level, \*\*\* significant at the 1% level. See notes in Table 1 for variable explanations.

Table A3: Robustness Analysis (b)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta$ Spending	$\Delta$ Spending,t+1	Deductions	$\Delta$ Net Bank	$\Delta$ Financial	$\Delta$ Mortgage
	b/se	b/se	b/se	b/se	b/se	b/se
$\theta^p > 0$	0.032*** (0.009)	-0.046*** (0.012)	0.001*** (0.000)	0.003 (0.005)	-0.000 (0.001)	0.027*** (0.008)
$\theta^p < 0$	-0.014 (0.011)	-0.001 (0.011)	-0.000 (0.000)	0.003 (0.005)	0.003** (0.001)	-0.005 (0.006)
$E[\Delta p] > 0$	0.004 (0.038)	-0.017 (0.040)	0.003* (0.002)	-0.030* (0.018)	-0.005 (0.003)	0.020 (0.015)
$E[\Delta p] < 0$	-0.009 (0.029)	0.017 (0.023)	-0.000 (0.001)	0.003 (0.011)	0.002 (0.003)	-0.022 (0.014)
$\theta^y > 0$	-0.046 (0.053)	0.083 (0.051)	0.003* (0.002)	0.054*** (0.020)	-0.001 (0.005)	-0.005 (0.022)
$\theta^y < 0$	-0.036 (0.028)	-0.016 (0.029)	0.000 (0.001)	-0.005 (0.013)	-0.007*** (0.002)	-0.018 (0.012)
$E[\Delta y] > 0$	0.048 (0.037)	-0.032 (0.043)	0.000 (0.001)	0.023 (0.016)	0.005 (0.004)	0.023 (0.017)
$E[\Delta y] < 0$	-0.196** (0.090)	-0.039 (0.084)	-0.001 (0.002)	0.009 (0.033)	0.021** (0.009)	0.010 (0.032)
$\sigma^p$	0.000** (0.000)	0.000 (0.000)	-0.000 (0.000)	0.000** (0.000)	-0.000*** (0.000)	0.000*** (0.000)
$\sigma^y$	-0.001* (0.000)	0.000** (0.000)	-0.000 (0.000)	-0.001** (0.000)	0.000 (0.000)	-0.001*** (0.000)
DK Econ +	-0.003 (0.015)	-0.004 (0.015)	0.001 (0.001)	-0.008 (0.007)	-0.001 (0.002)	0.001 (0.006)
DK Econ -	0.021 (0.016)	-0.011 (0.018)	-0.001** (0.001)	-0.007 (0.009)	-0.000 (0.002)	-0.002 (0.007)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
N	7754	4522	7754	7754	7754	7754

Notes: The table repeats the estimations in Table 1 but is based on a sample of observations where no stock or bond holdings is observed at the beginning of the period. Standard errors are clustered at the municipal level. \* significant at the 10% level, \*\* significant at the 5% level, \*\*\* significant at the 1% level. See notes in Table 1 for variable explanations.



Table A4: Robustness Analysis (c)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta$ Spending b/se	$\Delta$ Spending,t+1 b/se	Deductions b/se	$\Delta$ Net Bank b/se	$\Delta$ Financial b/se	$\Delta$ Mortgage b/se
$\theta^p > 0$	0.034*** (0.009)	-0.031*** (0.010)	0.001*** (0.000)	-0.001 (0.003)	0.001 (0.002)	0.024*** (0.005)
$\theta^p < 0$	-0.006 (0.009)	-0.007 (0.010)	0.000 (0.000)	0.004 (0.005)	-0.002 (0.002)	-0.004 (0.004)
$E[\Delta p] > 0$	-0.020 (0.033)	-0.015 (0.028)	0.001 (0.001)	-0.024 (0.016)	-0.001 (0.007)	0.021* (0.012)
$E[\Delta p] < 0$	-0.030 (0.021)	0.031 (0.019)	-0.001 (0.001)	0.010 (0.010)	0.004 (0.005)	-0.018* (0.011)
$\theta^y > 0$	-0.005 (0.044)	-0.010 (0.051)	0.004** (0.002)	0.071*** (0.020)	-0.010 (0.012)	-0.013 (0.017)
$\theta^y < 0$	-0.015 (0.024)	-0.042 (0.026)	0.001 (0.001)	-0.008 (0.012)	-0.012* (0.006)	-0.013 (0.010)
$E[\Delta y] > 0$	-0.007 (0.033)	-0.003 (0.033)	-0.000 (0.001)	0.035*** (0.013)	0.017 (0.012)	-0.000 (0.012)
$E[\Delta y] < 0$	-0.196*** (0.066)	-0.014 (0.075)	-0.001 (0.002)	0.001 (0.023)	0.033** (0.015)	-0.042* (0.022)
$\sigma^p$	0.000** (0.000)	0.000*** (0.000)	-0.000 (0.000)	0.000* (0.000)	-0.000 (0.000)	0.000*** (0.000)
$\sigma^y$	-0.001* (0.000)	-0.000*** (0.000)	-0.000 (0.000)	-0.001* (0.000)	-0.000 (0.000)	-0.001*** (0.000)
DK Econ +	0.002 (0.014)	-0.017 (0.013)	0.001 (0.001)	-0.006 (0.006)	-0.003 (0.003)	0.004 (0.005)
DK Econ -	0.014 (0.013)	-0.026* (0.015)	-0.001** (0.001)	0.004 (0.006)	-0.002 (0.004)	0.004 (0.005)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
N	12788	7579	12788	12788	12788	12788

Notes: The table repeats the estimation reported in Table 1 but includes municipality times years fixed effects. Standard errors are clustered at the municipal level. \* significant at the 10% level, \*\* significant at the 5% level, \*\*\* significant at the 1% level. See notes in Table 1 for variable explanations.

Table A5: Robustness Analysis (d)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta$ Spending	$\Delta$ Spending,t+1	Deductions	$\Delta$ Net Bank	$\Delta$ Financial	$\Delta$ Mortgage
	b/se	b/se	b/se	b/se	b/se	b/se
$\theta^p > 0$	0.023** (0.010)	-0.023* (0.012)	0.000 (.)	-0.001 (0.004)	0.003 (0.002)	0.017*** (0.005)
$\theta^p < 0$	-0.008 (0.010)	-0.006 (0.010)	0.000 (.)	-0.002 (0.005)	-0.003 (0.003)	-0.004 (0.004)
$E[\Delta p] > 0$	-0.023 (0.038)	-0.039 (0.031)	0.000 (.)	-0.011 (0.017)	-0.002 (0.007)	0.018 (0.016)
$E[\Delta p] < 0$	-0.018 (0.023)	0.013 (0.022)	0.000 (.)	0.010 (0.011)	-0.000 (0.006)	-0.014 (0.010)
$\theta^y > 0$	-0.003 (0.052)	0.013 (0.054)	0.000 (.)	0.076*** (0.024)	-0.014 (0.012)	-0.010 (0.019)
$\theta^y < 0$	-0.007 (0.027)	-0.047* (0.025)	0.000 (.)	-0.008 (0.014)	-0.011* (0.006)	-0.008 (0.010)
$E[\Delta y] > 0$	-0.014 (0.035)	0.033 (0.032)	0.000 (.)	0.027* (0.014)	0.012 (0.013)	-0.011 (0.011)
$E[\Delta y] < 0$	-0.176** (0.073)	-0.018 (0.072)	0.000 (.)	0.005 (0.022)	0.020 (0.014)	-0.026 (0.024)
$\sigma^p$	0.000** (0.000)	0.000*** (0.000)	0.000 (.)	0.000** (0.000)	-0.000 (0.000)	0.000*** (0.000)
$\sigma^y$	-0.001* (0.000)	-0.000** (0.000)	0.000 (.)	-0.001** (0.000)	-0.000 (0.000)	-0.001*** (0.000)
DK Econ +	0.011 (0.016)	-0.020 (0.013)	0.000 (.)	-0.009 (0.006)	-0.000 (0.003)	0.000 (0.006)
DK Econ -	0.017 (0.017)	-0.033** (0.016)	0.000 (.)	0.005 (0.006)	-0.000 (0.005)	0.003 (0.007)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
N	9601	5743	9601	9601	9601	9601

Notes: The table is based on a sample excluding all observations where deductions for house improvements are recorded. Standard errors are clustered at the municipal level. \* significant at the 10% level, \*\* significant at the 5% level, \*\*\* significant at the 1% level. See notes in Table 1 for variable explanations.

Table A6: Robustness Analysis (e)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta$ Spending	$\Delta$ Spending,t+1	Deductions	$\Delta$ Net Bank	$\Delta$ Financial	$\Delta$ Mortgage
	b/se	b/se	b/se	b/se	b/se	b/se
$\theta^p > 0$	0.017 (0.027)	-0.024 (0.027)	0.002* (0.001)	-0.001 (0.012)	0.001 (0.007)	0.023* (0.012)
$\theta^p < 0$	-0.026 (0.028)	0.005 (0.027)	-0.000 (0.001)	0.002 (0.012)	0.001 (0.007)	-0.009 (0.011)
$E[\Delta p] > 0$	-0.003 (0.039)	-0.032 (0.039)	0.001 (0.001)	-0.021 (0.019)	-0.002 (0.009)	0.025* (0.015)
$E[\Delta p] < 0$	-0.019 (0.029)	0.023 (0.028)	-0.001 (0.001)	0.010 (0.012)	0.004 (0.008)	-0.016 (0.013)
$\theta^y > 0$	0.007 (0.050)	-0.003 (0.049)	0.004** (0.002)	0.075*** (0.019)	-0.011 (0.013)	-0.010 (0.018)
$\theta^y < 0$	-0.005 (0.026)	-0.043* (0.025)	0.001 (0.001)	-0.005 (0.012)	-0.012* (0.007)	-0.011 (0.011)
$E[\Delta y] > 0$	0.001 (0.033)	-0.006 (0.032)	-0.001 (0.001)	0.036*** (0.013)	0.017 (0.011)	0.004 (0.012)
$E[\Delta y] < 0$	-0.176*** (0.063)	-0.032 (0.071)	-0.000 (0.002)	-0.002 (0.022)	0.029** (0.014)	-0.040* (0.021)
$\sigma^p$	0.000** (0.000)	0.000** (0.000)	-0.000 (0.000)	0.000** (0.000)	-0.000 (0.000)	0.000*** (0.000)
$\sigma^y$	-0.001* (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.001** (0.000)	-0.000 (0.000)	-0.001*** (0.000)
DK Econ +	0.003 (0.013)	-0.020 (0.013)	0.001 (0.000)	-0.006 (0.006)	-0.003 (0.003)	0.004 (0.005)
DK Econ -	0.010 (0.013)	-0.023* (0.014)	-0.001** (0.001)	0.005 (0.006)	-0.002 (0.004)	0.004 (0.005)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	12788	7579	12788	12788	12788	12788

Notes: The table is based on estimation by two stage least squares where the unanticipated price increase/decrease is instrumented with the municipal level house price growth obtained from a house price index published by the association of Danish mortgage banks, Finance Denmark. Standard errors are clustered at the municipal level. \* significant at the 10% level, \*\* significant at the 5% level, \*\*\* significant at the 1% level. See notes in Table 1 for variable explanations.

Table A7: Robustness Analysis (f)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta$ Spending	$\Delta$ Spending,t+1	Deductions	$\Delta$ Net Bank	$\Delta$ Financial	$\Delta$ Mortgage
	b/se	b/se	b/se	b/se	b/se	b/se
$\theta^p > 0$	0.095*** (0.022)	-0.088*** (0.028)	0.003*** (0.001)	-0.003 (0.011)	0.004 (0.005)	0.070*** (0.013)
$\theta^p < 0$	-0.006 (0.023)	-0.011 (0.032)	0.001 (0.001)	0.007 (0.017)	-0.009 (0.007)	-0.007 (0.011)
$E[\Delta p] > 0$	-0.048 (0.098)	-0.033 (0.098)	0.001 (0.002)	-0.123** (0.060)	-0.016 (0.023)	0.082** (0.036)
$E[\Delta p] < 0$	-0.045 (0.065)	0.128* (0.072)	-0.002 (0.002)	-0.006 (0.046)	0.001 (0.019)	-0.039 (0.031)
$\theta^y > 0$	-0.006 (0.127)	-0.038 (0.138)	0.007** (0.003)	0.183*** (0.054)	-0.028 (0.031)	-0.017 (0.041)
$\theta^y < 0$	-0.033 (0.073)	-0.109 (0.076)	0.003** (0.002)	-0.052 (0.036)	-0.035* (0.021)	-0.046* (0.026)
$E[\Delta y] > 0$	-0.002 (0.099)	-0.004 (0.103)	-0.001 (0.002)	0.061 (0.043)	0.033 (0.032)	0.003 (0.033)
$E[\Delta y] < 0$	-0.655*** (0.189)	-0.004 (0.219)	-0.001 (0.003)	-0.054 (0.076)	0.079 (0.049)	-0.118* (0.060)
$\sigma^p$	-0.115 (0.147)	0.050 (0.180)	0.011*** (0.004)	0.130 (0.090)	0.053 (0.040)	-0.041 (0.060)
$\sigma^y$	0.875 (0.673)	-0.604 (0.692)	-0.036** (0.018)	0.356 (0.405)	-0.097 (0.164)	0.293 (0.304)
DK Econ +	0.005 (0.021)	-0.030 (0.022)	0.000 (0.001)	-0.009 (0.012)	-0.004 (0.005)	0.001 (0.009)
DK Econ -	0.005 (0.019)	-0.033 (0.025)	-0.001* (0.001)	0.015 (0.011)	-0.001 (0.007)	0.000 (0.007)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
N	12788	7540	12788	12788	12788	12788

Notes: The table repeats the estimation in Table 1, but where the outcome is calculated at the household level. Standard errors are clustered at the municipal level. \* significant at the 10% level, \*\* significant at the 5% level, \*\*\* significant at the 1% level. See notes in Table 1 for variable explanations.

Table A8: Robustness Analysis (g)

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta$ Spending	$\Delta$ Spending,t+1	Deductions	$\Delta$ Net Bank	$\Delta$ Financial	$\Delta$ Mortgage
	b/se	b/se	b/se	b/se	b/se	b/se
$\theta^p > 0$	0.030*** (0.006)	-0.190*** (0.061)	0.000 (0.000)	-0.006 (0.004)	-0.001 (0.001)	0.183*** (0.030)
$\theta^p < 0$	-0.009 (0.009)	0.010 (0.055)	0.000 (0.000)	0.002 (0.004)	-0.001 (0.001)	0.017 (0.022)
$E[\Delta p] > 0$	0.012 (0.039)	-0.037 (0.215)	-0.000 (0.000)	-0.036 (0.023)	-0.008* (0.004)	0.182** (0.081)
$E[\Delta p] < 0$	0.012 (0.021)	0.179 (0.138)	0.000 (0.000)	0.002 (0.011)	-0.005 (0.003)	-0.093 (0.068)
$\theta^y > 0$	0.006 (0.031)	0.004 (0.227)	0.000 (0.000)	0.058*** (0.020)	-0.004 (0.005)	-0.064 (0.071)
$\theta^y < 0$	-0.035 (0.024)	-0.156 (0.128)	-0.000 (0.000)	-0.001 (0.011)	-0.008** (0.004)	-0.142*** (0.043)
$E[\Delta y] > 0$	0.012 (0.034)	0.000 (0.220)	0.000 (0.000)	0.015 (0.018)	0.006 (0.007)	0.080 (0.078)
$E[\Delta y] < 0$	-0.150*** (0.057)	-0.107 (0.394)	0.001 (0.001)	-0.005 (0.029)	0.012 (0.007)	-0.151 (0.148)
$\sigma^p$	-0.135*** (0.048)	0.179 (0.337)	-0.000 (0.001)	0.021 (0.026)	0.012* (0.006)	-0.100 (0.129)
$\sigma^y$	-0.303 (0.197)	-1.052 (1.266)	0.000 (0.002)	0.109 (0.108)	-0.032 (0.033)	0.777 (0.588)
DK Econ +	0.000 (0.003)	-0.032 (0.022)	0.000 (0.000)	-0.002 (0.002)	-0.000 (0.000)	0.001 (0.009)
DK Econ -	0.002 (0.003)	-0.032 (0.025)	-0.000 (0.000)	0.001 (0.001)	-0.000 (0.001)	-0.000 (0.007)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
N	12788	7540	12788	12788	12788	12788

Notes: The table repeats the estimation in Table 1, but normalizes on average cash-on-hand over the period 2008-2010 instead of gross income averaged over the period 2008-2010. Standard errors are clustered at the municipal level. \* significant at the 10% level, \*\* significant at the 5% level, \*\*\* significant at the 1% level. See notes in Table 1 for variable explanations.



## Chapter 3

# The Effects of Tax Penalties on Early Withdrawals from Pension Accounts





# The Effects of Tax Penalties on Early Withdrawals from Pension Accounts

## Evidence from Danish Register Data

Henrik Yde Andersen\*

### Abstract

Using variation from a natural experiment that is plausibly exogenous to other savings decisions, we show that reducing the tax penalty rate by 1 percent increases the propensity to withdraw pension assets early by 0.1 percentage points on average. Access to detailed administrative records allows us to show that the effect is two to three times larger for consumers who are likely to be affected by liquidity constraints and individuals who lose their job or divorce. This is consistent with the idea that consumers finance spending with pension assets only when other less costly ways to access liquidity have been exhausted. Conditional on withdrawing pension assets early the amount withdrawn increases by about 3 percent for each one percent reduction in the tax penalty rate. Only 1/3 of the withdrawals are rolled over to non-retirement savings accounts or used to repay debt. We find that those who cash out pension wealth in the year of a job loss reduce overall savings rates for at least six years after the withdrawal. Ultimately, our evidence suggests that tax penalties are efficient to increase long-term savings.

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## 1 Introduction

In many developed countries, people have access to save in tax-favored retirement accounts. This type of savings account has two advantages over non-retirement accounts. First, they provide a tax benefit and second, they work as a commitment device to prevent present biased consumers from over-spending.<sup>1</sup> The downside is that wealth held in such accounts is less liquid, i.e. working-aged consumers can access their retirement savings only by paying a tax penalty. An important question is, however, whether penalizing early withdrawals prevents consumers from smoothing consumption when facing adverse shocks, e.g., a job loss or divorce. The access to funds in tax-favored retirement savings accounts is typically associated with a tax penalty and this means that the consumer faces a trade-off between current and future consumption based on the relative price dictated by the tax penalty. The objective of this study is to examine how consumers make this tradeoff. To do this we examine the change in the propensity to make early withdrawals from retirement accounts when the tax penalty is reduced.

Pension savers know at the point of contributing that the pension scheme contains a buy back clause, determining the price of which the assets can be withdrawn early. This implies that they should only update their savings behavior when new information arrives about how costly it is to access their pension wealth prior to retirement age. An unanticipated reduction in the tax penalty produces two predictions. One is that consumption should increase for individuals characterized by a high discount rate who want to front-load spending. The second is that consumption increases for individuals who are otherwise constrained from doing so because of adverse employment or demographic shocks. We stratify individuals on their ex ante liquid wealth holdings and test whether savers affected by such shocks engage more in early withdrawal behavior when the tax penalty rate is reduced relative to those unaffected by such shocks. This would be consistent with the idea that consumers exhaust the least expensive sources of liquidity such as bank savings, home equity and credit card debt, to finance expenditure before turning to early withdrawals from pension plans.

We use the case of Denmark as it provides quasi-experimental variation in the tax penalty rate as well as population-wide data about savings in both retirement and non-retirement accounts at the individual level. The tax penalty rate on early withdrawals was fixed at 60 percent for all Danish pension schemes until 2013. From then on, balances in one particular pension scheme could be cashed out by paying a tax penalty rate of about 50 percent, while taxation on other schemes was left unchanged. A key feature of our paper is access to the Danish administrative records that have the advantage that they hold objective information—not self-reported—about a range of important characteristics. This allows

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<sup>1</sup>Diamond (1977) and Poterba (2014) propose the rationale for some degree of forced pre-retirement savings to overcome the problem that a share of the population saves too little for retirement.

us to identify the year of, e.g., a job loss or divorce. In addition, the data covers non-retirement wealth at the individual level and unlike former studies, we are able to test the correlation between liquid assets holdings and early withdrawals. The ability to combine plausibly exogenous variation in the tax penalty rate and individual-level information about non-retirement wealth and life events is, to our knowledge, unprecedented in the literature.

We construct two quasi-experimental research designs to quantify both extensive and intensive margin responses to the policy change. To measure the extensive margin response, we use a difference in differences setup to capture the change in early withdrawal propensities at the time the tax penalty rate was reduced. The design exploits that only one of two types of pension schemes was affected by the policy change, such that savers in the scheme that was unaffected are used as counterfactual. Particularly, both groups of savers were unaware of the tax penalty reduction at the time of contributing to the pension scheme. This allows us to interpret the results as the causal effect of the tax penalty reduction on the propensity to withdraw. To measure the intensive margin response we construct an event study. Here, savings and consumption behavior of savers who withdraw pension assets in year  $t$  are compared to that of savers who cash out pension assets early in year  $t+3$ . This allows us to quantify the immediate effect of the withdrawal on a range of savings outcomes conditional on withdrawing early.

Our paper offers three contributions. First, we provide quasi-experimental evidence on how much early withdrawal propensities increases when the tax penalty rate is substantially reduced. The average effect is in line with previous papers, but we break new ground on the heterogeneity in the response. Savers who are affected by liquidity constraints, job loss or divorce tend to increase probabilities up to three times the average effect—consistent with consumption smoothing behavior. Second, we show that the amounts withdrawn prematurely increase by about 3 percent for each one percent reduction in the tax penalty rate. On average,  $2/3$  of the withdrawal is consumed immediately, while the rest is saved in non-retirement accounts or used to repay debt. Finally, we find that those who cash out pension assets in the same year as a job loss reduce their overall savings rates for at least six years after the shock. This implies that reducing the price at which pension wealth can be accessed prematurely allows consumers to smooth consumption but they do not increase savings afterwards to compensate for the early withdrawal. Consequently, savings rates of individuals who engage in early withdrawals decline when reducing the tax penalty rate.

The rest of the paper is organized as follows. Section 2 provides a review of relevant literature. Section 3 explains the Danish institutional setting and 2013 policy reform. In section 4, the sampling process is explained and statistics on withdrawals across a range of individual characteristics are shown. The empirical model and heterogeneous responses are presented in sections 5 and 6, respectively, while overall savings effects are quantified in section 7. Finally, section 8 concludes.

## 2 Literature Review

The literature on early withdrawals is divided in two branches. The one aims for understanding whether tax penalties affect the decision on whether or not to cash out of a pension account before retirement age. The other examines the purpose for which savers engage in early withdrawal behavior. Our paper attempts to combine both strands of literature. This provides a more complete picture of how buy back clauses in retirement accounts affect individual savings and consumption. Variation from a policy change provides the necessary change in the tax penalty rate allowing us to address the importance of this particular institutional feature. Detailed individual information from public administrative and income-tax records provide information that allows us to characterize those who cash out and how they spend or save the proceeds.

To our knowledge, only few papers use variation in the tax penalty rate caused by a tax reform in a natural experiment setting to quantify the effect of the penalties on early withdrawal behavior. Chang (1996); Burman et al. (1999, 2012) exploit that a 1986 US Tax Act introduced a fixed tax penalty of 10 percentage points to be paid on top of the savers' individual income tax rate should they decide to cash out pension savings before 55 years of age.<sup>2</sup> Savers above the age cut-off could extract retirement savings by paying only their marginal income tax rate. Using this cut-off in a quasi-experimental design both studies conclude that the group of younger taxpayers reduced early withdrawals significantly compared to the group of older taxpayers when the tax penalty was introduced. However, both papers leave a set of important questions unanswered because of limited data availability and due to the type of variation caused by the policy change. First, their data sources are the Current Population Survey and and Health and Retirement Study, which do not include any non-retirement wealth information. This implies that they are unable to test the liquidity constraint hypothesis directly. Second, the exact change in the tax penalty rate at the individual level is unknown to the researchers and the quantitative effects of the tax changes are consequently difficult to interpret. This is due to the fact that total taxes paid on early withdrawals varies with individual marginal tax rates in the US—a measure not provided by the survey sources. Third, using survey information entails potential self-reporting bias, as taxpayers are asked to report their history of past years' early withdrawals. Finally, it is unknown if overall savings rates of consumers, who, e.g., lose their job and cash out pension assets, are affected negatively.

A range of other studies have used the same survey sources and individual tax return data to examine the characteristics of savers who withdraw savings from pension accounts early. Poterba et al. (1998a) and Poterba and Venti (2001) show that individuals who cash

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<sup>2</sup>Pre-retirement withdrawals were penalised by 10 percentage points up to the age of  $59\frac{1}{2}$  if the early withdrawal was unrelated to a job switch or termination.

out large amounts tend to reinvest the proceeds in other savings accounts, while smaller proceeds were not rolled over. Younger consumers were more likely to spend early withdrawals immediately, while older consumers reinvested withdrawals such that their overall savings were unchanged. This implied that liquidity constraints mattered for withdrawal behavior. Similar findings are found in Sabelhaus and Weiner (1999) who document that early withdrawals are prevalent among younger individuals and low-income groups. In a more recent paper, Hurd and Panis (2006) present evidence that pre-retirement cash outs correlate with the timing of divorce and widowhood. Other studies have documented that early withdrawals correlate with job loss (Amromin and Smith, 2003) and economic business cycles (Argento et al., 2015). It seems common to all these studies that liquidity constraints and adverse life events tend to correlate with early withdrawal behavior.

### 3 Institutional Setting and Pension Reform

The Danish pension system constituted two account types prior to 2013, namely Annuity Pension schemes that pay out as yearly payments and Capital Pension schemes that pay out as a lump-sum. Contributions for both schemes could be deducted from the taxpayer's income before taxes, and pre-retirement withdrawals were subject to a 60 percent tax penalty rate. Besides the tax deduction at the time of pay-in accrued capital gains in retirement accounts are taxed at lower rates than capital gains in non-retirement accounts. In January 2013 the Capital Pension scheme was closed for further contributions. Savings in this scheme could be transferred to annuity schemes or they could be transferred to a new pension type, the so-called Age Pension scheme. The payout profile of the Capital Pension scheme and the Age Pension schemes were similar, i.e. a lump-sum, but contributions to the Age Pension scheme were not tax deductible at the time the contribution was made but subject to a 40 percent tax rate.<sup>3</sup> In other words, the Danish lump-sum pension scheme was changed from being back-loaded to being front-loaded regarding taxation. Most importantly, however, early withdrawals from the new front-loaded Age Pension were subject to a 20 percent tax penalty. Together with the front-loaded 40 percent tax rate, the change implied that existing balances in the old lump-sum scheme could be transferred to the new scheme upon payment of the 40 percent tax rate and subsequently become accessible to the consumer by paying the 20 percent penalty. Overall taxation on the pre-retirement withdrawal would correspond to  $40 + (100 - 40)/100 \times 20 = 52$  percent. In addition to the scheme type change, the government offered an extra tax rebate of 2.7 percentage points when transferring the balance in 2013, such that the 2013 tax penalty rate was  $37.3 + (100 - 37.3)/100 \times 20 = 49.84$  percent. During 2013, the government decided to extend the tax rebate period by one additional year. This was repeated in 2014, such that the 49.84 percent rate covered 2013–2015. Table 1 presents the statutory tax rates that are levied on pre-retirement withdrawals from balances in lump-

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<sup>3</sup>Instead, no taxes are to be collected when the funds are paid out in retirement.

sum and annuity pension schemes across years.<sup>4</sup>

Table 1: Tax Penalty Rates on Pre-retirement Withdrawals (%)

	Prior to 2013	2013–2015	After 2015
Lump-sum	60	49.84	52
Annuity	60	60	60

The announcement of the reform could be important for understanding the results. The rebate was levied on both early withdrawals and transfers of capital pension balances to other pension accounts. In this paper we consider early withdrawals only and discard transfers between accounts completely. The government explicitly promoted the pension reform as an opportunity to transfer balances from the Capital Pension scheme to the Age Pension scheme. Something that would provide a 2.7 percentage point (6.75 percent) tax deduction to the saver and strengthen government finances immediately. The tax penalty rate reduction on early withdrawals was more of a second order consequence of the policy change. Nonetheless, national newspapers wrote about the decline in the tax penalty rate in 2012–2014, explaining in a simple fashion that pension assets could be withdrawn at a lower tax rate and consumed immediately or saved in other accounts.<sup>5</sup> The reform was presented in May and adopted in August 2012. Keep in mind, however, that at the first announcement of the reform in 2012, the 2.7 percentage point tax rebate was set to cover 2013 only. Later this rebate window was extended. Nonetheless, as we will show, most of the response to the policy change takes place within 2013.

#### 4 Data and Univariate Analysis

Danish administrative registers provide population-wide information on income, wealth, pension contributions and withdrawals alongside personal characteristics such as gender, age, employment status, marital status and educational attainment. We construct a sample of individuals between 18 and 59 years of age over the time period 1999–2016. From age 60 pension payments are considered retirement income and taxed at individual marginal tax rates or at a 40 percent fixed rate. The sample includes savers who contributed voluntarily to a pension plan at least once during the period before the 2013 pension tax reform. This

<sup>4</sup>There are exceptions to the reported tax rates in Table 1. If the consumer suffers from severe illness or is unable to work because of illness, early withdrawals are taxed at 40 percent. Also, balances earned before 31 December 1979 are taxed at rates of 23.31 and 25 percent in 2013 and 2014, respectively. These exceptions imply that the tax rates can vary from 23.31 to 60 percent. Individuals paying tax penalty rates of less than 49.84% are, however, excluded in this analysis (less than 2,000 observations).

<sup>5</sup>See e.g. *"Stor rabat hvis kapitalpensioner hæves i utide"*, Berlingske Business, 22 August 2012; *"Hæv pension i utide med skatterabat"*, Finans, 14 January 2013; *"På trods af en solid strafgift trækker rekordmange danskere deres kapitalpensioner ud til forbrug eller anden opsparing"*, Berlingske Business, 2 April 2014.

ensures that savers in the sample do in fact have pension assets to cash out in the post-reform years if they are willing to do so.<sup>6</sup> We balance the sample such that individual characteristics are available for all individuals, which provide a sample of almost 9.7 million observations.

#### 4.1 Information on Pensions

The tax penalty rate is calculated as the amount paid in tax penalties divided by the amount withdrawn early for each taxpayer in each year. This means that the data provide the actual tax penalty rate paid by the owner upon cashing out pension assets. This is an important innovation compared to Chang (1996) and Burman et al. (1999, 2012) who use variation in the statutory income-tax rates around the 1986 US Tax Act. Their studies struggle to measure the change in actual tax penalty rates because the penalties depend on individual income-tax rates in the US pension system. Tax penalty rates in the Danish pension system are fixed independently of income taxation. However, the recorded penalty rate for each person per year could reflect withdrawals from a set of different pension accounts, which are subject to different tax penalties in 2013 and thereafter. Consider a saver who withdraws annuity pension assets only and is taxed at a 60 percent tax penalty rate in 2013. Consider another saver who withdraws lump-sum pension assets only and is taxed at 49.84 percent in 2013. Now, consider a third saver who withdraws from both annuity and lump-sum pension schemes. Depending on how large withdrawals are from each scheme, he is taxed at a weighted average of the 60 and 49.84 percent rates. We illustrate and elaborate on this point below.

The data records provide annual pension contributions for the full sample period. We use this detailed information to group the savers into treatment and control groups. The treatment group holds individuals who contributed to a Capital Pension scheme prior to 2013, while individuals in the control group did not. To obtain as much balance on covariates between the groups, they are both allowed to have contributed to Annuity Pension schemes in the pre-reform years too. The only difference in the two groups is that the treated individuals are certain to own a lump-sum pension balance, while the individuals in the control group are likely to have annuity pension assets only. This allocation is an approximation to the true allocation. Clearly, the treatment group could have contributed to annuity pension schemes prior to 1999 where the data records begin and vice versa for the control group.

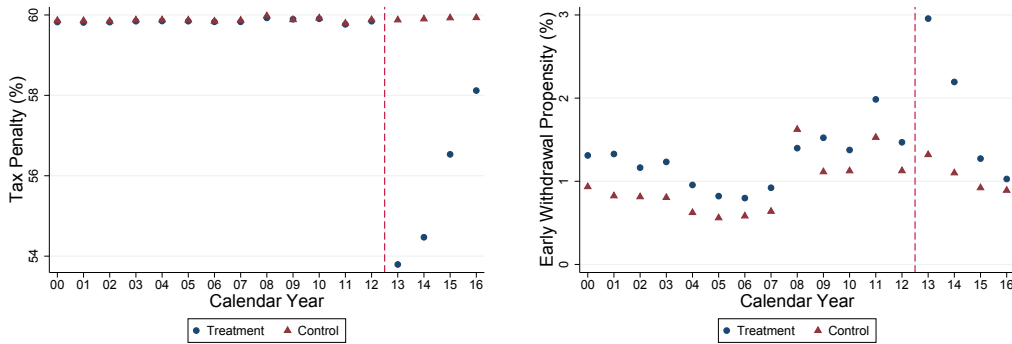
Figure 1a plots mean tax penalty rates in each year for the savers in the treatment and control groups. Clearly, the average tax penalty rate of the control group was unaffected by

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<sup>6</sup>Collective agreements between workers' unions and employers' associations mean that many employer-paid pension contributions cannot be withdrawn prior to retirement age. As a consequence, a large fraction of Danish pension assets are completely illiquid. On the contrary, voluntary pension contributions are usually not subject to such no-buy back clauses.

the 2013 tax reform, while the savers in the treatment group paid about 54 and 54.5 percent in 2013 and 2014, respectively, and about 56.5 and 58 percent in 2015 and 2016, respectively. The tax penalty rates do not match the statutory rates in Table 1, because the treatment group withdrew pension assets from both lump-sum and annuity pension plans, which were taxed equally at a 60 percent rate before 2013 and differentially after this point. The data allow us to show that, on average, the reform reduced the individual tax penalty rate by  $60 - 54 = 6$  percentage points in 2013 for the treated savers in the sample considered. This information becomes crucial when assessing the quantitative effects of this specific tax rule change. The decline in the tax rate should be  $60 - 49.84 \approx 10$  percentage points when savers withdraw strictly from lump-sum pension schemes. This is illustrated in Appendix Figure 9a, which includes savers in the treatment group only if they saved strictly in lump-sum pension plans in pre-reform years that we observe in the data.

Figure 1: Full Sample



(a) Tax Penalty Rate (%)

(b) Early Withdrawal Propensity (%)

*Note:* The figures are constructed by dividing the sample into treatment and control groups. The treatment group consists of savers who contributed to lump-sum pension schemes prior 2013 and savers in the control group did not. Both groups could have savings in annuity pension schemes too. Figure 1a shows the mean tax penalty rate in each year for both the treatment and control groups. Figure 1b shows the mean propensity to withdraw pension assets early in each year for both groups. The red dotted line represents the time of implementation of the policy change.

The propensity to withdraw pension assets early can be measured by constructing a dummy variable,  $E_{i,t}$ , that takes the value 1 for individual  $i$  when she withdraws pension assets in year  $t$ , otherwise zero. This is considered one of the main outcome variables of interest as it captures any extensive margin response to the 2013-policy change. Figure 1b plots the mean propensity to withdraw pension assets early in each year for the treatment and control group savers. Prior to the 2013 policy change, the two groups show similar trends in propensities of about 1 percent—whereas the propensities shift upwards from



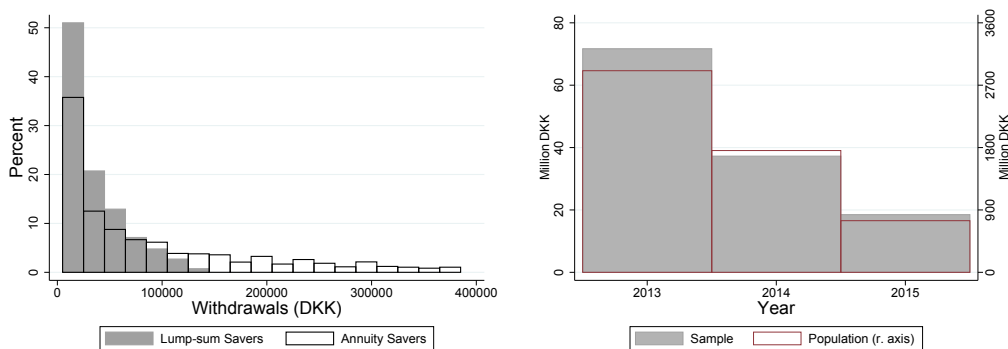
2008 to about 1.5 percent. In 2013 and 2014 the withdrawal propensities increase to about 3 and 2 percent, respectively, for the treatment group, while that of the control group remain at the pre-reform level. In 2015 and 2016 the propensities are about 1 percent for both groups. This nonparametric plot serves as empirical evidence that the propensity to cash out pension assets prior to the retirement age increased substantially when the tax penalty rate was reduced. Despite the relatively modest propensities, it is difficult to reject that the two groups responded differentially at the time of the reform, while pre-reform trends for the treatment and control groups follow each other closely. In 2008, the propensity to withdraw increased more for the control group relative to the treatment group, which is a contrast to all other pre-reform years. This might reflect effects from the global financial crisis that escalated in Denmark that particular year. Specifically, one particular institutional feature might cause the data to show this. Capital gains on pension accounts are subject to a wealth tax—a fixed 15.3 percent rate—which is levied each year on the balance and paid automatically to the government by the pension funds on behalf of the owners. Capital losses are also taxed at this rate. This implies that, upon a capital loss, the negative tax payment—a tax subsidy—is added to the pension balance. However, if the pension balance is relatively small and unlikely to produce a noteworthy gain in subsequent years, the negative tax payment is cashed out to the owner, automatically. Appendix Figure 10 illustrates this point. The figure plots propensities across years for savers, who cashed out below or above DKK 5,000 (US \$800). Consistent with the 2008 negative tax payment, we see a large spike in the early withdrawal propensity for savers who cashed out small amounts. These withdrawals are likely to be undertaken by the pension fund rather than active saving decisions by the owners. We discard individuals completely from the sample if they withdrew less than DKK 5,000 at any point in time. At this threshold, pension funds are likely to cash out balances automatically to the owners because the balance (including capital gains) is insufficient to pay the annual fees and the insurance premium if the plan includes an insurance policy.

Figure 2a presents an overlay of histograms showing the distribution of early withdrawn amounts for the treatment and control groups. Despite the tendency for savers in the treatment group to withdraw smaller amounts than savers in the control group, both groups show a similar withdrawal pattern in the lower part of the distribution. Note that about 80 percent of treatment group savers cashed out less than DKK 60,000. This figure is about 55 percent for the control group savers. For both groups, the top 10-percentiles are omitted for data confidentiality reasons as the bins contained too few individuals in the tails.

Whether an early withdrawal originates from a lump-sum or annuity pension balance is not observed in the data. Thus, to us as researchers, there exists no true allocation of savers into those who are affected by the 2013-policy change and who are not. As explained in the previous section we exploit information from past contributions to allocate savers

to treatment and control groups in order to proxy for whether they are affected or not. Aggregate numbers provided by the Danish government on annual totals of early withdrawals from lump-sum pension schemes is used to assess the validity of our allocation process. The government registered early withdrawals from lump-sum pension balances totaling DKK 2.9, 1.8 and 0.7 billion in 2013, 2014 and 2015, respectively. We take out a subgroup of our treatment group who contributed to lump-sum pension schemes only prior to the reform (comparable to the lump-sum savers in Appendix Figure 9a). Total withdrawals of this subgroup was DKK 72, 39 and 20 million in 2013, 2014 and 2015, respectively. All the numbers are shown in Figure 2b. The grey bars show the annual sums in the subsample, while the transparent bars show the population-wide sums. Clearly, this subgroup captures only a minor fraction of total withdrawals. However, the pattern over the years is consistent with the aggregate numbers, which indicates that using individual past pension contributions to allocate savers to treatment and control groups is not systematically wrong.

Figure 2: Pre-retirement Withdrawals 2013–2015



(a) Histogram

(b) Totals for Lump-sum Savers

*Note:* Figure 2a illustrates two overlapping histograms. One for the treatment group and one for the control group, both showing the distribution of early withdrawn amounts during 2013–2015 for each group. The top decile is omitted from both histograms for data confidentiality reasons. Figure 2b shows the total amount early withdrawn strictly from lump-sum schemes within our sample (left-hand axis) and for the full population (right-hand axis). The grey bars represent those of the treatment group who strictly saved in lump-sum pension schemes in 2000–2012.

## 4.2 Individual Characteristics

The administrative records provide a set of individual characteristics. The information are objective in the sense that they are reported by third-parties ensuring no self-reporting biases in the data. This section provides an overview of the characteristic features of treatment group savers who withdrew pension assets prior to retirement age. Moreover, we present a univariate analysis on whether withdrawal propensities differ across these characteristics.

Table 2: Descriptive Statistics for Treatment Group Savers 2000–2016

	(E=1) Mean	(E=0) Mean	Difference	Std. Err.
Age	42.8	42.5	0.3***	0.1
Female (%)	33.3	41.8	−8.5***	0.1
Income	505.9	427.2	78.7***	0.7
Liquid Assets	87.2	166.9	−79.7***	1.0
Debt	259.1	166.3	92.7***	0.8
Mortgage	597.2	519.6	77.6***	1.8
Housing Wealth	757.4	817.2	−59.8***	2.8
Observations	116,724	8,324,403		

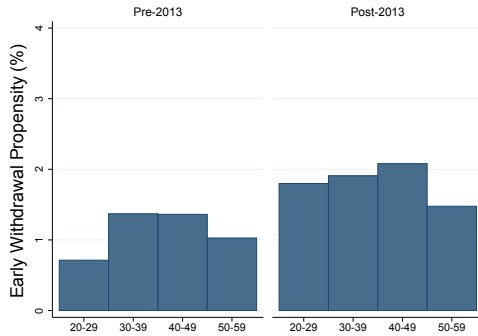
Notes: The dummy,  $E$ , indicates whether individual  $i$  cashed out pension assets during 2000–2016. The table includes the treatment group only. Stars reflect significance level from two-sample  $t$ -tests of equal means, where \*\*\*  $p < 0.01$ .

To learn more about which types of savers were more likely to withdraw pension assets after the tax penalty reduction, we investigate whether certain characteristics were more active in early pension assets withdrawal in the post-reform years compared to the pre-reform period.

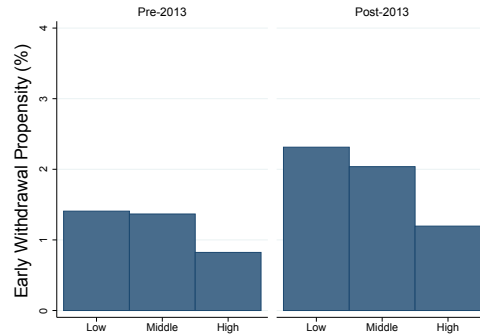
First, we do a two-sample  $t$ -test of equal means between lump-sum savers who withdrew pension assets early in the sample period and people who did not engage in early withdrawals. Table 2 presents means for the two groups, the difference and standard errors by age, gender and a set of monetary measures. During 2000–2016, 116,724 incidences of treatment group savers cashing out pension assets were recorded. About 8.3 million observations in the treatment group take the value zero in the early withdrawal indicator,  $E_{i,t}$ . Note that the savers who withdrew pension savings ( $E=1$ ) are included in the non-withdrawal group ( $E=0$ ) in years that they did not engage in early withdrawals. The two groups are about the same age, but with about 9 percent fewer females in the withdrawal group. Also, the early withdrawal-group earned about DKK 79,000 (about 19 percent) more each year compared to the non-cash out group. Savers who engaged in early withdrawal behavior had DKK 80,000 less in liquid assets (about 48 percent) and DKK 93,000 more debt in banks (about 56 percent). Their mortgages were DKK 78,000 larger (about 15 percent), while their housing wealth was DKK 127,000 smaller (about 7 percent). The latter implies that the group of pension asset withdrawers had significantly less equity than the non-withdrawers. All together the statistics draw a clear pattern that early withdrawal propensities are correlated with low liquid asset holdings, high debt and low equity—all of which characterize liquidity constrained consumers.

Second, we plot the early withdrawal propensity across a set of personal characteristics, such as age groups, educational attainment, socio-economic classification and marital status. All plots are divided into a left and right panel that split the measures into before (pre-

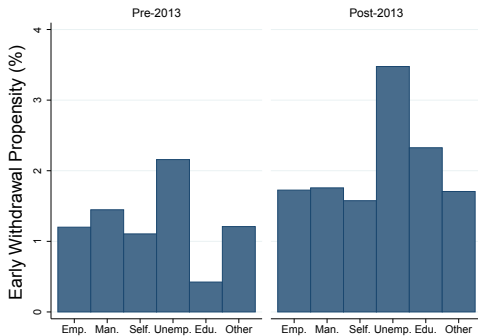
Figure 3: Early Withdrawal Propensity and Individual Characteristics



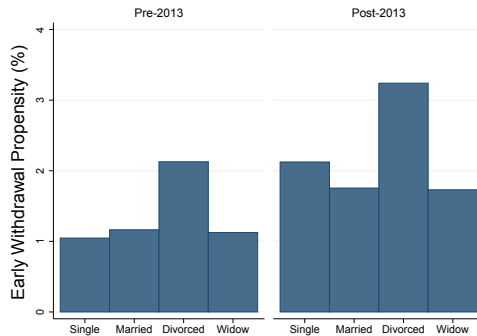
(a) Age Groups



(b) Educational Attainment



(c) Socio-economic Classification

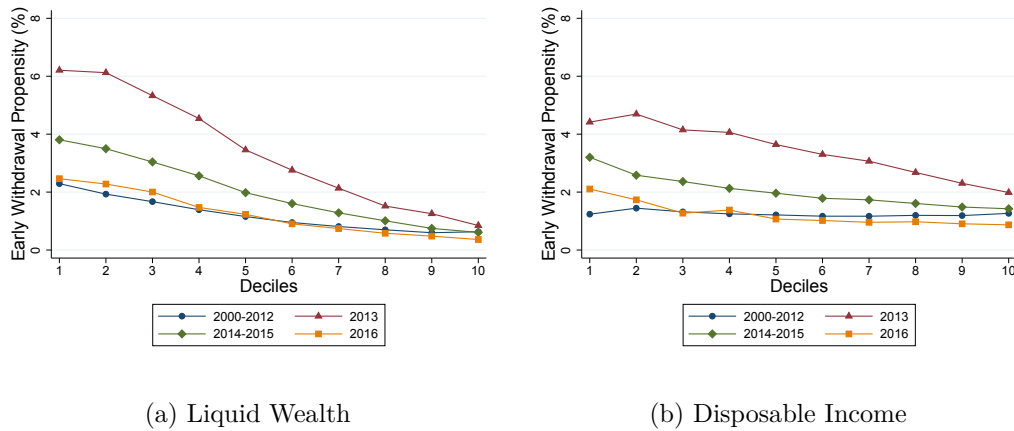


(d) Marital Status

*Note:* Figures 3a–3d illustrate the early withdrawal propensity, measured by the dummy,  $E_{i,t}$ , across age, education, socio-economic classification and marital status. All characteristics are measured by end of year. Educational attainment is divided in three groups; *Low*, which covers primary schooling, *Middle* covering secondary schooling and vocational training and *High*, which includes college, university or PhD degrees. Socio-economics classifications are grouped in Employment, Management, Self-employed, Unemployed, Education and Other. The latter mainly covers individuals receiving disability pension benefits. Marital status is grouped in Single, Married, Divorced and Widow by end of year.

2013) and after (post-2013) implementation of the 2013 tax reform. This provides an idea on whether certain characteristic features are more or less likely to be correlated with a response to the reduction in the tax penalty rate. Figure 3a plots the early withdrawal propensity for four age groups. The left panel shows a hump-shaped pattern where the 20–29 year-olds and 50–59 year-olds have a lower propensity than the two middle-aged groups. However, in the post-reform years, the propensity in the youngest age group increased to match the middle-aged ones. This implies that the very youngest could be responding substantially to the tax penalty rate reduction. Note, however, that all age groups increased their early withdrawal propensity after 2013. Figure 3b divides individuals in three groups that reflect their educational attainment. The group labelled *Low* has primary schooling only. The *Middle* group has secondary schooling or vocational training, while the *High* group has a college, university or PhD degree. In the pre-2013 years the *Low* and *Middle* groups are almost identical with an early withdrawal propensity of about 1.5 percent, while that of the *High* group is well below 1 percent. After 2013 in the right-most panel, the propensities increase for the *Low* and *Middle* groups to about 2–2.5 percent, while the *High* group increases to little more than 1 percent. This suggests that the reduction in tax penalties affects the behavior of individuals with relatively low educational attainment. In Figure 3c, savers are grouped according to their socio-economic classification—that is, their labor market status. Comparing the left and right panels, all groups increase their early withdrawal propensity. However, the unemployed and students seem to respond relatively more than the remaining ones. Finally, Figure 3d split the sample by marital status—single, married, divorced and widow. Before the policy change, divorced individuals seemed to stand out by having a higher early withdrawal propensity. After implementation of the 2013 tax reform, all groups have increased their propensities, but the singles and divorced seem to have responded the most. Going forward, the personal characteristics presented in this section will be used as control variables in regressions that aim to quantify the 2013 reform effects on the early withdrawal propensity. Later we will use details about the timing of, e.g., job loss and divorce to show how life events interact with early withdrawal behavior.

Figure 4: Early Withdrawal Propensity and Liquidity Indicators



*Note:* Figure 4a divides the sample in ten equally sized bins (deciles) and plots the mean early withdrawal propensities within each bin for four time periods. Figure 4b divides the sample in disposable income deciles and plot the mean early withdrawal propensities within each bin for the same four time periods.

According to the literature, early withdrawal propensities are predicted to be larger for consumers who suffer from liquidity constraints (Amromin and Smith, 2003). Danish administrative records provide individual wealth and income information, which we use to test this prediction. First, we construct ten equally sized bins (deciles) of liquid wealth and income. Then we calculate the mean early withdrawal propensity within each bin. Figures 4a and 4b plot early withdrawal propensities across liquid wealth and income deciles, respectively. Both Figures split the propensity in four time periods; the pre-reform period (2000–2012), the first year after the tax penalty rate reduction (2013), the two subsequent years in which the tax penalty rate remained extraordinarily low (2014–2015) and the year in which the rate was set to its new permanent level (2016). It is clear to see that withdrawal propensities stand out in 2013. Savers in the lowest decile of liquid wealth increases their withdrawal propensity from about 2 to 6 percent—a factor three—while the top decile do not change at all when comparing the pre-reform years to 2013. In 2014–2016 the propensities in the bottom decile tend to adjust downwards to the pre-reform level but is still about twice as large as for the pre-reform years. For all time periods illustrated in the Figure there is a monotonically and negative correlation between liquid wealth (measured by the beginning of the year) and withdrawal propensities (end of year). We test this finding more formally in an empirical model in the following section. For comparison, Amromin and Smith (2003) use US tax return data to show that the lowest quantile of financial wealth has an average early withdrawal propensity of 6.6 percent, which declines monotonically to 1 percent for

the top quantile—a pattern consistent with ours.<sup>7</sup> For the income deciles in Figure 4b, the negative correlation to withdrawal propensities is less prevailing and stand out only for 2013. This indicates that income might be a less useful instrument to sort liquidity constrained savers from the unconstrained ones.

The statistics draw the picture that early withdrawals are more likely to be undertaken by young and middle-aged consumers with relatively low educational attainment and who might be unemployed or divorced. Moreover, they hold little liquid financial wealth and is placed in the lower part of the income distribution. A range of studies suggest that overall savings rates within this group of people depend highly on savings in tax-deferred retirement accounts (Bernheim, 2002; Engen et al., 1996; Gale, 1998; Poterba et al., 1998b). These are the same types of savers that we find is more likely to withdraw pension assets early, which leads to the question how early withdrawals affect retirement income of this group. We return to this question in section 7.

## 5 Empirical Model

The empirical model tests whether the observed increase in early withdrawal propensity for the treatment group at the time of tax penalty reduction was statistically significant relative to the change in propensity for the control group. The identifying assumption relies on common pre-trends in propensities between the two groups. In order to test this, we regress the early withdrawal indicator in the following specification

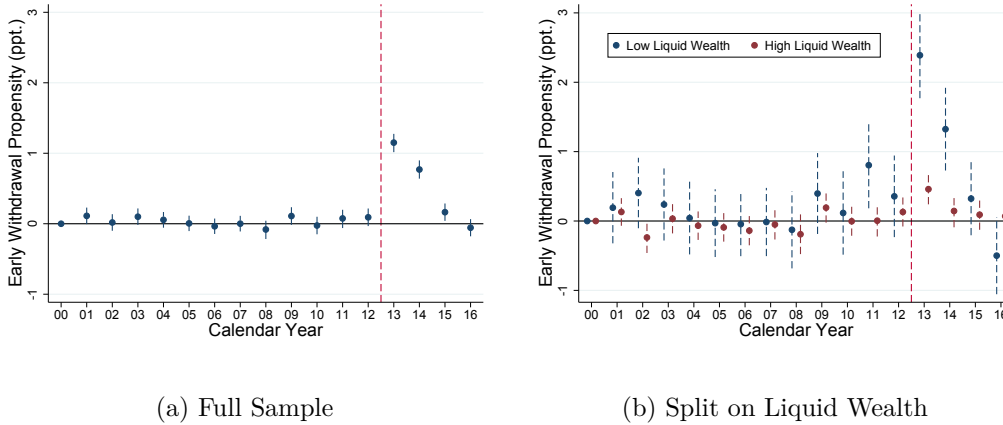
$$E_{i,t} = \alpha + \beta^1 \Omega_i + \beta^2 Treat_i + \beta_t^3 \Omega_t \times Treat_i + X_i + \varepsilon_{i,t}, \quad (1)$$

where the dependent variable,  $E_{i,t}$ , takes the value 1 for individual  $i$  if he cashed out pension assets in year  $t$ , otherwise zero.  $Treat_i$  takes the value 1 for individuals in the treatment group and zero for the those in the control group. This dummy is interacted with year dummies,  $\Omega_t$ , such that the coefficients in the vector  $\beta_t^3$  capture differences in propensities between the treated and non-treated savers in each year  $t$ . Idiosyncratic errors are contained in  $\varepsilon_{i,t}$  and  $X_i$  is the vector of individual control variables. Figure 5a is a plot of the  $\beta_t^3$ -coefficient vector, showing that, for all years prior to implementation of the reform, propensities between the treatment and control group are not statistically different at the 5-percent level. In 2013, the propensity increases for the treated savers by about 1.1 percentage points relative to the non-treated ones. The treatment effect reduced gradually towards zero in the following years. The takeaway from this Figure is that the assumption of parallel pre-trends is not violated. Even when we look specifically at those who ex ante had little access to liquid wealth versus those in the high end of the liquid wealth distribution the

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<sup>7</sup>The measure of financial wealth is imputed in Amromin and Smith (2003) by grossing up individual capital income and cross-country average dividend and interest rates. This measure serves only as a proxy for the true financial wealth at the individual level.

Figure 5: Estimated Early Withdrawal Propensities by Years



*Note:* Figure 5a plots the  $\beta_i^3$ -coefficient in equation (1), illustrating the difference in early withdrawal propensities across years between the treatment and control groups. Vertical bars indicate the 5-percent significance level. Figure 5b is a similar illustration but divides the sample in high and low liquid wealth. The former includes the top three deciles and the latter includes the bottom three deciles. The liquid wealth deciles covers stocks, bonds and cash in bank accounts in 2011.

common pre-trends persist. Figure 5b plots the treatment effect for the top and bottom three deciles in high and low liquid wealth groups, respectively. The low liquid wealth group increased the propensity to early withdraw pension assets significantly more than the high liquid wealth group consistent with the non-parametric evidence in Figure 4a. We proceed to identify the reform effect in the following specification

$$E_{i,t} = \alpha + \beta_1 Post_t + \beta_2 Treat_i + \beta_3 Post_t \times Treat_i + X_i + \Omega_t + \varepsilon_{i,t}, \quad (2)$$

where a post-reform indicator takes the value 1 for the years 2013–2016, otherwise zero.  $\Omega_i$  is included to control for year fixed effects, but the coefficient of interest is  $\beta_3$ , the difference-in-differences estimator. This interaction between  $Post_t$  and  $Treat_i$  identifies the reform effect on early withdrawal propensities assuming that the treatment and control groups are comparable groups—an assumption we could not reject in Figure 5a. Table 3 presents the coefficient of interest,  $\beta_3$ , from equation (2). Column one shows the regression that includes year fixed effects only. In column two, controls and age fixed effects are added, while columns three and four include municipal fixed effects and individual fixed effects, respectively. Municipal fixed effects set out to capture potential differences in the reform response across local labor and housing markets. There are 98 municipalities in Denmark and considerable differences in unemployment rates, house prices and population densities across the country. By including municipal fixed effect we ensure that such differences are not driving the measured treatment effect. Individual fixed effects might, for instance, capture time-invariant individual characteristics and deep parameter savings preferences. This, e.g.,



individual tastes for saving, which are unobservable and considered to be constant for adults throughout their lives. All columns present an estimated average treatment effect over all post-reform years of 0.5 percentage points. Columns 5–8 present the interaction parameter from an identical regression to equation (2) but here, the dependent variable is the tax penalty rate, corresponding to the illustration in Figure 1a. The average decline in tax penalties for the treatment group relative to the control group for all post-reform years is 4.5 percentage points in the baseline specification. Adding all controls, the average change in tax penalty is -3.6 percentage points. Relative to the pre-reform years, this corresponds to an  $3.6/60 = 6$  percent reduction in the tax penalty rate for the sample considered. These estimates allow us to calculate a semi-elasticity. The propensity to withdraw pension assets early increased, on average, by 0.1 percentage points when the tax penalty rate was reduced by one percent. The result is slightly lower but consistent with, e.g., Chang (1996) who estimates an increase in rollover rates of 0.2–0.4 percentage points when tax penalty rates increase by one percent, using an US 1986 tax reform but under different econometric assumptions. Here, an increased rollover rate—the rate of which savers roll over withdrawals to other savings accounts—compares to a decrease in the early withdrawal rate.

Table 3: Regression Table

	Early Withdrawal Propensity				Tax Penalty Rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat	0.003*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	- (.)	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	- (.)
Post × Treat	0.005*** (0.000)	0.005*** (0.000)	0.005*** (0.000)	0.005*** (0.000)	-0.045*** (0.001)	-0.042*** (0.001)	-0.042*** (0.001)	-0.036*** (0.001)
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Age FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Municipal FE	No	No	Yes	Yes	No	No	Yes	Yes
Individual FE	No	No	No	Yes	No	No	No	Yes
Observations	9666029	9666029	9666029	9666029	128595	128595	128595	128595
$R^2$	0.002	0.004	0.005	0.003	0.488	0.498	0.500	0.440

Notes: The Table present coefficients from equation (2), where columns 1–4 is estimated with the early withdrawal propensity as dependent variable, while columns 5–8 are estimated using the tax penalty rate as dependent variable. The  $Post_t$  coefficients are omitted because year fixed effects are included in all columns. Standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

The treatment effect is robust to the inclusion of a range of controls. To further test the robustness of our empirical model, Table 4 presents the interaction coefficient of equation (2) estimated on various subsamples. Column one shows that the treatment effect remains unchanged if we exclude individuals in the control group who cash out amounts larger than DKK 140,000. By doing this, we show that the treatment effect is likely not to be affected by the fact that the treatment group cashes out smaller amounts, on average, than the

control group (see Figure 2a). Changes to non-retirement savings are calculated as year-on-year changes in stock measures. This implies that the first differences include both annual price changes and quantitative changes in the accounts made actively by the owner. We are interested in the latter channel only. In order to control for the fact that the price-channel does not drive our findings, we exclude savers with stocks or bonds in their liquid asset portfolio in 2011—the year before the reform was announced. The estimated treatment effect do not change, which implies that the response is not driven by price fluctuations in the financial markets. The treatment effect on renters is similar to homeowners according to column three. Here, all homeowners have been removed from the sample but the estimate is unchanged. This implies that the decision to withdraw was likely not to be connected to the housing market. Columns four and five divide the sample into younger and older savers, splitting at age 35. The estimated response is similar for the two subsamples, implying that the overall treatment effect is not driven solely by the very young, as suggested in Figure 3a. The full sample includes individuals who contributed voluntarily to pension accounts in the pre-reform years. Savers who saved only in occupational pension schemes are not included. This restriction aims to ensure that the savers included in the sample were in fact eligible to withdraw pension assets prior to retirement age upon payment of the tax penalty. It is possible that some of the individuals in our sample were not eligible to withdraw pension assets prior to retirement age even if they were willing to pay the tax penalty. Their pension assets could be lawfully bound in the pension fund and hence, completely inaccessible to the owner until he retires. We use a new data source which holds information on buyback eligibility in 2014 only. Ideally, we would want to have information on eligibility for the years prior to and in the reform year. This information is not available. However, using the 2014-information in eligibility allows us to keep savers in they wither had a buy back clause in their retirement accounts by the end of that particular year or withdrew pension wealth in 2013–2014. Column six shows that the treatment effect remain almost unchanged by doing this. In column seven, a non-linear model is used to estimate the difference in differences specification. The marginal treatment effect in a probit model is exactly as that of the baseline linear model. For that reason, we proceed using the linear model because the linear model is simpler to apply in the following section, in which we examine heterogeneous treatment effects across different individual characteristics.

Table 4: Robustness Table

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Common support	No stocks and bonds	Renters	Age<35	Age>35	Eligible	Probit
Post $\times$ Treat	0.004*** (0.000)	0.005*** (0.000)	0.005*** (0.001)	0.004*** (0.001)	0.005*** (0.000)	0.004*** (0.000)	0.005*** (0.000)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	No
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	No
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes	No
Observations	9642491	5688604	2401693	1322985	8343044	2745411	9666029
$R^2$	0.004	0.005	0.007	0.008	0.004	0.005	0.004

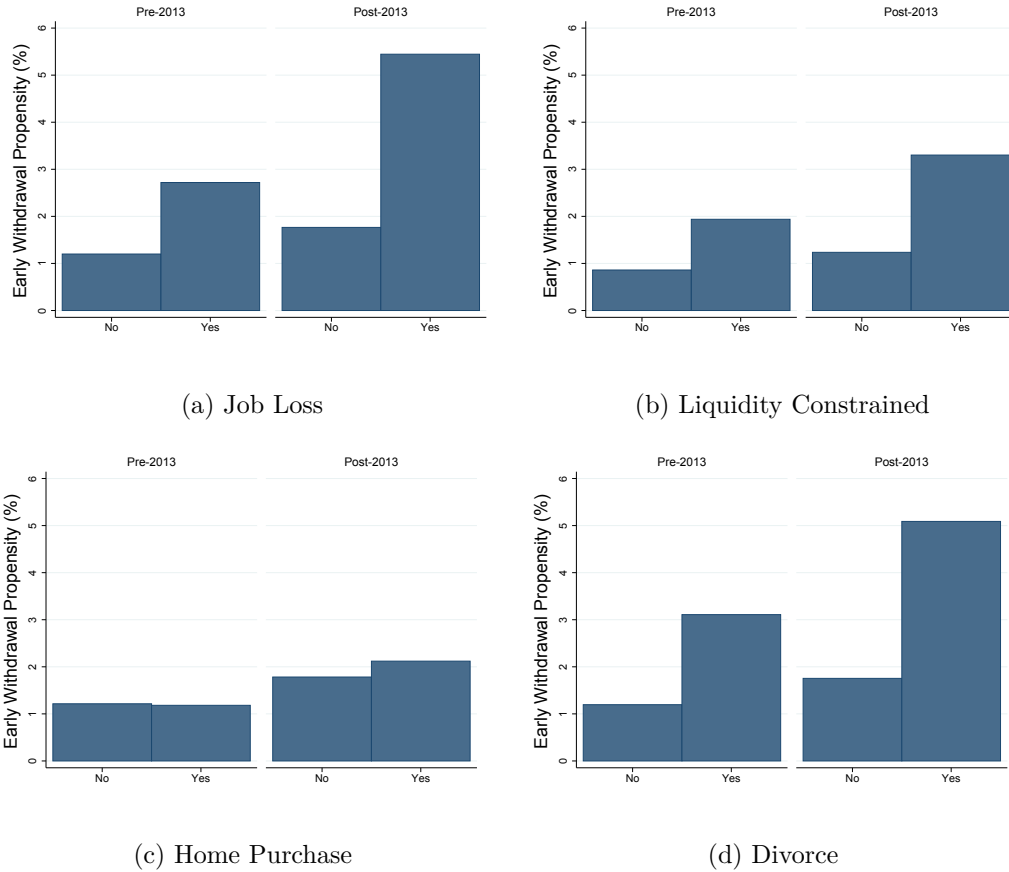
Notes: The Table present coefficients from equation (2) with the early withdrawal propensity as dependent variable. Each column represents a robustness check, in which the equation is regressed on a subsample. Column one excludes withdrawals from the control group that were larger than DKK 140,000. Column two excludes individuals with non-zero values of stock and bonds in their financial portfolio in 2011. Column three excludes savers who owned their own home in 2011. Column four and five divide the sample at age 35. Column six includes savers only with non-zero pension assets eligible for early withdrawal in 2014 if they did not withdraw assets prior to that year. This provides a subsample of savers who could have cashed out pension wealth in 2013 but refrained from doing so. Column seven show marginal effects of a probit regression using the specification in equation (2). Standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## 6 Heterogeneous Withdrawal Propensities

Studies have found that early pension assets withdrawal is more likely when certain life events occur. Based on US income-tax data, Amromin and Smith (2003) find that an involuntary job loss produces a 4 percent increased probability of withdrawing pension assets early. This figure is 8 percent for divorce and 9 percent for home purchases. In this section, we show similar patterns in the Danish administrative records, regarding job loss and divorce, while home purchases do not seem to interact significantly with early withdrawal decisions. Moreover, we extend the analysis of Amromin and Smith (2003) by testing whether consumers that are affected by such life events have a higher propensity to cash out pension assets after the reduction in the tax penalty rate. This would imply that the reduction in tax penalties allow consumers to smooth consumption and cope with such adverse life events. To our knowledge, this is the first paper to combine plausibly exogenous variation in the tax penalty rate and administrative information about adverse shocks. First, we do a univariate analysis on how early withdrawal propensities interact with life events before and after the tax penalty rate reduction. Later, we formally test the interactions in a regression model.

Using individual employment information, we define a job loss indicator,  $Job\ Loss_{i,t}$ , that takes the value 1 for individual  $i$ , who was recorded as employed at the beginning of year  $t$  but experienced at least six months unemployment during year  $t$ , otherwise zero.

Figure 6: Early Withdrawal Propensities



Note: Figures 6a–6d illustrate the early withdrawal propensity, measured by the dummy,  $E_{i,t}$ , across a set of shock indicators. All figures are divided in left and right panels, dividing the sample in pre-2013 and post-2013 period. The illustrations cover the treatment group only. *Job Loss* indicate that the individual was employed by the beginning of the year and unemployed by the end of year. *Liquidity Constraints* indicate that the individual had liquid assets available corresponding to one month's disposable income, measured by the beginning of the year. *Home Purchase* indicate whether individuals had non-zero housing wealth by the end of year and no housing wealth at the beginning of that same year. *Divorce* captures if individuals are married at the beginning of the year and divorced by the end of the year.

Figure 6a presents the early withdrawal propensity for savers who lose their job, indicated by a *Yes* or *No*. The Figure is split into two panels. The left-most panel includes only pre-reform years, i.e. 2000-2012, while the right-most panel covers 2013–2016, in which the tax penalty rate was reduced. The Figures include only individuals from the treatment group, who are all expected to be affected by the policy change. The group not affected by job loss increased the probability of cashing out from about 1 percent before 2013 to a little under 2 percent after 2013. Between the two time periods, the job loss-group increased the propensity from less than 3 percent to about 5.5 percent. The substantial increase in probability for the job loss-group indicates that this group exploits the tax penalty reduction. On average, we expect them to be more liquidity constrained than people who do not lose their job. To test directly how liquidity constraints might interact with the policy change, we define an indicator,  $Liq. Con_{i,t}$ , that takes the value 1 for savers who had liquid assets at the beginning of the year that corresponded to less than one month’s disposable income. Otherwise, the indicator takes the value zero. This might not be the perfect allocation of constrained and unconstrained consumers (See e.g. Jappelli, 1990) but it is sufficient for our purpose that those labelled *liquidity constrained* are more likely to face constraints compared to the *unconstrained* group. Figure 6b shows that for the unconstrained consumers, early withdrawal propensities are just below 1 percent before the reform and just above 1 percent after implementation of the tax penalty reduction. For the liquidity constrained consumers, the propensity increases from about 2 to little under 3 percent. Not only are propensities for the constrained ones at a higher level, but the propensity also seem to increase for this group specifically as the tax penalty declines. Figure 6c shows early withdrawal propensities for individuals who buy a new home compared to those who do not.<sup>8</sup> The propensities are almost identical for these two groups at about 1 percent. After implementation of the 2013 reform, the propensities seem to increase to about 2 percent whereas the group of home buyers increased their propensity marginally more than the non-buyers. Overall, the tax penalty reduction did not seem to affect withdrawal behavior at the timing of home purchases. Figure 6d show early withdrawal propensities for individuals who are recorded as married at the beginning of the year and divorced at the end of that same year. Savers in the divorce group increased their propensities to cash out from about 2 to 5 percent, while propensities of the non-divorce group increased marginally from little above 1 percent to little below 2 percent for the two time periods.

The next step is to incorporate the life event information presented above into a regression specification that allows us to test whether our findings are statistically significant.

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<sup>8</sup>The home purchase indicator is crude as it captures only first time buyers who is recorded to have no housing wealth at the beginning of the year and non-zero housing wealth at the end of the year. Also, we do not observe when the credit assessment of the home buyer takes place, which might correlate with the decision to withdraw pension assets early.

The following model departs from equation (2) and adds four additional terms.

$$E_{i,t} = \alpha + \beta_1 Post_t + \beta_2 Treat_i + \beta_3 Post_t \times Treat_i + \beta_4 S_{i,t} + \beta_5 Post_t \times S_{i,t} + \beta_6 Treat_i \times S_{i,t} + \beta_7 Post_t \times Treat_i \times S_{i,t} + X_i + \varepsilon_{i,t}, \quad (3)$$

where  $S_{i,t}$  is one of the four predefined indicators, either *Job Loss*, *Liq. Con.*, *Buy House* or *Divorce*. We estimate the model with each of the four indicators included separately, and run a full specification in which all indicators are included simultaneously.  $S_{i,t}$  is interacted with  $Post_t$  to filter out changes in propensities across the pre-reform and post-reform years for individuals characterized by that particular life event. Similarly,  $S_{i,t}$  is interacted with  $Treat_i$  to control for differences between the treatment and control groups. The triple interaction term,  $Post_t \times Treat_i \times S_{i,t}$ , captures the additional response in early withdrawal propensities for the treated savers who are characterized by the life events in  $S_{i,t}$  when the tax penalty rate is reduced in 2013. Appendix Table 7 presents all coefficients in the model, while Table 5 presents the coefficients of interest from equation (3). The average treatment effect is estimated to be 0.4–0.6 percentage points, consistent with that of Table 3. However, four additional effects are presented in Table 5. Column one shows that the liquidity constrained savers increased their early withdrawal propensity by an additional 0.5 percentage points compared to the unconstrained savers. Similarly, those in the treatment group suffering from a job loss increased their early withdrawal propensity by an additional 1.3 percentage points. This is an important empirical finding as it links the tax policy response to the timing of a job loss. This finding implies that the price of liquidity in pension accounts matters for consumers who lose their job. Upon the job loss this group is more likely to buy back pension assets if the price of doing so is reduced. Column three shows that early withdrawal propensities increase for house buyers by an additional 0.1 percentage points for when the tax penalty rate is reduced. The estimate is, however, not statistically significant. Individuals who undergo divorce respond to the tax reform by an additional 1.2 percentage points in column four. Column five includes all the four indicators, simultaneously. Almost all coefficients remain unchanged in this specification. Reassuringly, the pattern emerging from the empirical model verifies the nonparametric evidence in Figures 6a–6d.

The estimated treatment effects in Table 5 can be translated into semi-elasticities by dividing the percentage point change by the estimated 6 percent decline in the tax penalty rate caused by the tax reform. We show in Appendix Table 8 that the tax rate change is not statistically different to the job loss and divorce groups compared to the average effect presented in Table 3. Thus, consumers who lose their job increase their early withdrawal propensity by  $(.006 + .013)/(-.06) = 0.3$  percentage points for each one percent decrease in the tax penalty rate. For the divorce-group this figure is almost identical,  $(.006 + .012)/(-.06) = 0.3$  percentage points for each one percent decrease in the penalty

Table 5: Heterogeneous Reform Effects on Early Withdrawal Propensity

	(1)	(2)	(3)	(4)	(5)
Post $\times$ Treat	0.004*** (0.000)	0.006*** (0.000)	0.006*** (0.000)	0.006*** (0.000)	0.004*** (0.000)
Post $\times$ Treat $\times$ Liq. Con.	0.005*** (0.001)				0.005*** (0.001)
Post $\times$ Treat $\times$ Job Loss		0.013*** (0.004)			0.013*** (0.004)
Post $\times$ Treat $\times$ Buy House			0.002 (0.002)		0.002 (0.002)
Post $\times$ Treat $\times$ Divorce				0.012*** (0.003)	0.011*** (0.003)
Controls	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes
Observations	9609187	9609187	9609187	9609187	9609187
$R^2$	0.005	0.002	0.002	0.002	0.005

Notes: The Table present coefficients from equation (3) with the early withdrawal propensity as dependent variable. The triple interaction terms are interactions of the difference-in-differences estimator and each of the shock measures. See notes for Figures 6a–6d for explanations on the shocks. Coefficients of the full model are presented in Appendix Table 7. Standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

rate. For the group of savers who are characterized as being liquidity constrained the response to the reform is  $(.004 + .05)/(-.06) = 0.2$  percentage points for a one percent tax penalty rate reduction. All together, the estimated treatment effects are about 2-3 times larger for the liquidity constrained consumers and those experiencing adverse demographic or labor market shocks.

## 7 Effects on Overall Savings when Early Withdrawing

This section exploits variation in the timing of early withdrawals in the panel data to identify how much is withdrawn from tax-deferred retirement accounts conditional on cashing out. Moreover, we examine how early withdrawals that take place simultaneously as a job loss might affect subsequent savings rate differentially depending on whether or not the saver is liquidity constrained or not.

### 7.1 Intensive margin response

In order to measure how much income increases upon early withdrawing and whether this increase was spent immediately or saved in non-retirement accounts we design an event study. Here, we compare behavior of savers who withdraw pension assets early in year  $t$  to savers who cash out pension assets in year  $t + 3$ . The idea is to quantify the effects of the withdrawals on other savings components and consumption relative to the change in these same outcomes in the absence of the early withdrawal. By comparing these estimated effects to similar calculations in the pre-reform period we obtain a gauge on whether the tax penalty reduction affected the size of the withdrawals and how the withdrawals were spent.

The first step is to identify withdrawn amounts and how large they are relative to individual disposable income. We do this in the following regression

$$\Delta Y_{i,t} = \alpha + \beta_1 Post_t + \beta_2 E_1 + \beta_3 Post_t \times E_1 + \varepsilon_{i,t}, \quad (4)$$

where the change in disposable income,  $\Delta Y_{i,t}$ , is regressed on our reform indicator, an indicator that takes the value 1 for individuals who withdraw pension savings early in 2013 and 0 for individuals who cash out in 2016 and the interaction of the two. Disposable income is defined as income after taxes and interest payments. Pension contributions are excluded but early withdrawals measured after tax penalties are included in the variable. The idiosyncratic error is  $\varepsilon_{i,t}$ . The interaction term captures the effect on income when cashing out in 2013 relative to the counterfactual group, those who cashed out three years later.  $\beta_3$  is therefore the coefficient of interest. In order to interpret  $\beta_3$  as the effect of the early withdrawal on income changes, we need to assess whether the control group serves as a valid counterfactual. The estimated changes to disposable income for the 2013-withdrawal group and 2016-withdrawal group are plotted in Figure 7a. The Figure includes savers from the treatment group only and the plots are provided with standard errors in vertical



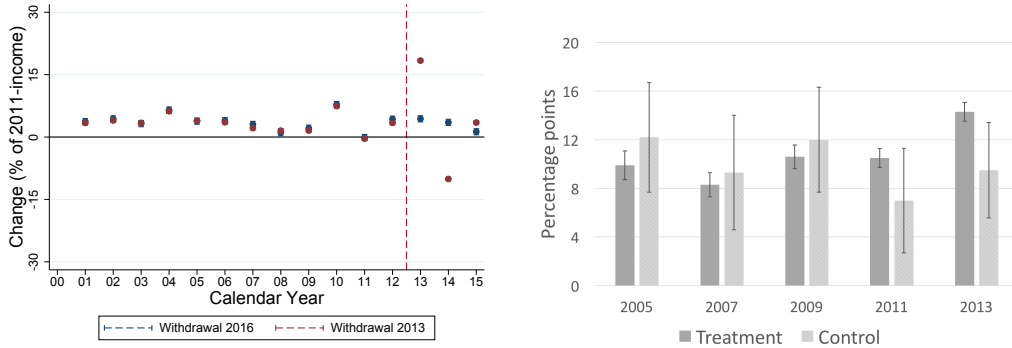
bars, testing for whether the trends in income changes are significantly different between the two groups.<sup>9</sup> Clearly, disposable income increases substantially in the withdrawal year relative to the counterfactual. In the year after the withdrawal, the disposable income decline substantially, reflecting that we consider annual changes in the graph. It is compelling to see that the 2016-withdrawal group had pre-trends similar to the 2013-withdrawal group. For this reason, we attribute the quantified differences in 2013 to be related to the withdrawals and not some third factor. Similar plots showing common pre-trends in all financial savings accounts can be found in Appendix Figures 11 and 12 for the treatment and control groups, respectively. Figure 7b summarizes the estimated effects of event studies in 2005, 2007, 2009, 2011 and 2013. In 2013, the bar representing the treatment group corresponds to the illustrated response in Figure 7a for that same year showing an increase in disposable income of 14.3 percentage points. Clearly, the estimated effect of the early withdrawal in 2013 on disposable income was significantly larger than in preceding years, in which early withdrawals increased disposable income of about 9.8 percentage points on average. For the control group, the estimated effects of early withdrawals on disposable income do not change significantly across years. For 2013, the point estimate is 9.5 percentage points. The average of the preceding years shown in Figure 7b is 10.1 percentage points. Using the 2013-estimates within each group relative to each of their pre-2013 averages we estimate that the 2013 tax penalty reduction increased the size of early withdrawals of  $(14.3/9.8)/(9.5/10.1) - 1 = 55$  percent.

The next step is to quantify how the money was spent or saved in non-retirement savings accounts. To define a consumption measure we follow the approach in Leth-Petersen and Browning (2003) by subtracting pension contributions and changes in non-retirement wealth from individual disposable income,  $C_{i,t} = Y_{i,t} - P_{i,t} - \Delta W_{i,t}$ . The imputed consumption measure has the caveat that price changes in stock and bonds, capital gains and losses are interpreted as actual saving and dissaving decisions by the owners. However, as shown in Table 4 column two, our main results do not change when removing individuals with stocks or bonds in their financial portfolio. The consumption measure is, therefore, less problematic to interpret in our specific setup.

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<sup>9</sup>The error bars are calculated using a regression model similar to equation (1)

Figure 7: The Effect of Early Withdrawals on Disposable Income



(a) Effect of Withdrawals on Disposable Income 2013

(b) Effect of Withdrawals on Disposable Income 2005–2013

Note: Figure 7a show the year-on-year change in disposable income for the treatment and control groups, respectively, measured as share of two year lagged disposable income. Each dot represent the mean within the specified calendar year for two groups. One withdraw pension assets early in 2013, while the other cashes out in 2016. The latter is used as counterfactual to quantify the change caused by the early withdrawal. The red dotted line represent marks the timing of the 2013-withdrawal. Vertical bars represent 5-percent significance levels estimated in a linear regression. Figure 7b summarizes estimated effects of event studies carried out in 2005, 2007, 2011 and 2013, for both the treatment and control groups.

Table 6: The Effect of Early Withdrawals on the Individual Financial Balance

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta$ Income (ex. Pension)	$\Delta$ Pension	$\Delta$ Assets	$\Delta$ Debt	$\Delta$ Mortgage	$\Delta$ Consumption
$Post \times Event_{2013}^T$	0.143*** (0.004)	-0.004*** (0.001)	0.030** (0.008)	-0.044*** (0.010)	0.025*** (0.015)	0.048** (0.021)
$Post \times Event_{2013}^C$	0.095*** (0.002)	-0.006 (0.005)	0.055* (0.031)	-0.001 (0.039)	-0.069 (0.051)	-0.071 (0.078)

Notes: The Table presents coefficients from equation (4) with various outcomes as dependent variables. Column one uses disposable income (including early withdrawals measured after taxes) as dependent variable. Column two uses total pension contributions as dependent variable. Columns three-five use financial assets, debt and mortgage, respectively, as left-hand side variables. Column six uses imputed consumption as dependent variable. All these variables are in first differences. Standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

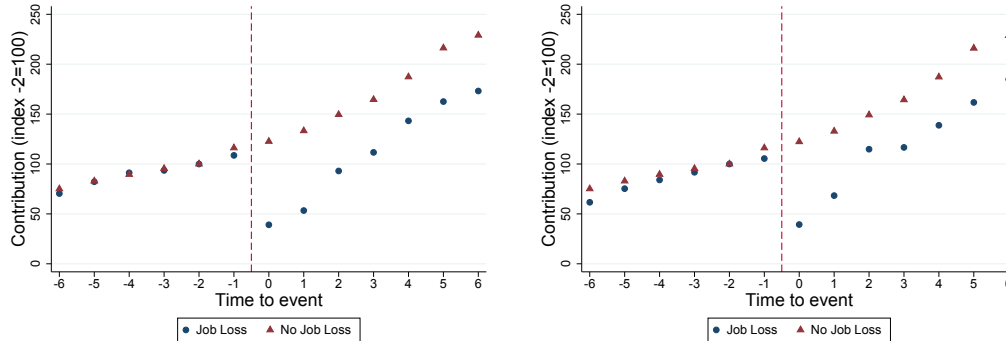
Table 6 presents the 2013-withdrawal effects on all savings components and consumption. The top row presents the effects of withdrawals in the treatment group, while the bottom row presents estimates from the control group. For the treatment group, the 2013-withdrawal lead to a 14.3 percentage point increase in disposable income relative to the counterfactual. Pension contributions were reduced marginally by 0.4 percentage points. Liquid assets were increased by 3 percentage points, debt was reduced by 4.4 percentage points and mortgages

were increased by 2.5 percentage points. Changes to the imputed consumption measure are increased by 4.8 percentage points. All estimates are statistical significant at the 5 percent-level. The substitution patterns indicate that  $4.8/14.3 = 34$  percent of the withdrawals were spent immediately, while  $(3 - (-4.4) - 2.5) = 34$  percent were rolled over to non-retirement savings accounts or used to repay debt. The estimated coefficients account for 2/3 of the withdrawn amount. However, the standard errors on the imputed consumption are large—about 2 percentage points. Therefore, within the 5-percent confidence band, the spending estimate could be  $4.8 + (2.1 * 1.96) = 8.9$  percentage points. This corresponds to a pass-through effect from early withdrawals to consumption of  $8.9/14.3 = 62$  percent. Hence, together with changes in savings and debt, this accounts fully for the increase in disposable income caused by the withdrawal. In the bottom row, the substitution effects of the control group is much less clear. This group consists of fewer individuals and hence, the estimated standard errors are larger compared to that of the treatment group. Nonetheless, we find a 9.5 percentage point increase in disposable income. At the 10 percent significance level liquid assets tend to increase by 5.5 percentage points, accounting for  $5.5/9.5 = 58$  percent of the withdrawals. The estimated coefficients does not, however, draw a clear picture of the substitution pattern of the control group.

## 7.2 Savings Rate after Job Loss and Early Withdrawals

If pre-retirement withdrawals are used for consumption smoothing when temporary, adverse shocks occur, we would expect to see savings rates rebound shortly after the timing of cashing out. To test this prediction, two comparable groups are defined out of the treated savers. One group who lose their job and cash out retirement savings, labelled *Job Loss*, and a group who cash out retirement savings without a job loss, labelled *No Job Loss*. Figure 8a plots the mean of pension contributions, including both voluntary and occupational contributions, for both groups indexed at two years before the timing of the early withdrawal. Individuals included in the illustration are restricted to be liquidity constrained at the beginning of time 0. Also, the Figure covers the pre-reform period only and all individuals are restricted to being included at least one year after the timing of an early withdrawal. Pension contributions decline substantially—about 50 percent—when the pre-retirement withdrawal takes place in the same year as a job loss. This is not surprising as pension contributions are closely connected to the total compensation when employed. When the employment ends the pension contributions are likely also to end. The more interesting part of the Figure is to observe how average contributions develop in the years following a job loss and early withdrawal. Pension contributions increase after few years but the job loss-group never catches up with the no-job loss group—at least not for the six following years. The pattern is similar if we consider contribution rates, where contributions are measured as a share of total earnings, rather than contribution levels (see Appendix Figures 13a).

Figure 8: Pension Contributions at Timing of Early Withdrawal (Liquidity Constrained)



(a) Effect of Job Loss

(b) Effect of Temporary Job Loss

*Note:* Figure 8a plot pension contributions in DKK across time, where time 0 represent the timing of an early withdrawal. The Figure includes treatment group savers only who cash out pension assets during 2000–2012. The savers are divided in two groups. One group lose their job in the same year as the early withdrawal, while the other group does not change labor market status. Pension contributions are indexed to time -2, two years prior to the withdrawal. Figure 8b is similar but here, those who are still unemployed in year 2 have been removed from the sample.

We do not know the the characteristics of the employment shock, e.g., whether it is short or long-term. However, by removing individuals who are still unemployed at time 2, the overall pattern remain unchanged. This is depicted in Figure 8b. This implies that it is not permanent employment shocks that explain how pension savings rates remain lower for the group that early withdraw pension assets simultaneously as experiencing a job loss. Another explanation for this pattern is that the job loss-group takes the opportunity to rebalance their financial portfolio and cash out pension assets for this purpose. Savings in non-retirement accounts do increase immediately as the job loss and early withdrawal take place (see Appendix Figure 13b), however, the job loss and no-job loss groups did not change non-retirement account savings rate differentially. This implies that the decline in pension contributions that we observe in Figure 8a is not counteracted by increased savings in non retirement accounts or increased debt repayments. The conclusion is that the overall savings rates decline when cashing out pension assets at the time of a job loss.

## 8 Conclusion

This paper examines who might benefit from a buy-back clause in retirement pension schemes. The clause entails that the owner of the pension account can withdraw assets early upon payment of a tax penalty. Using plausibly exogenous variation in the tax penalty rate caused by a Danish 2013 pension tax reform, we measure the response on both the extensive and intensive margins. Also, we assess the post-withdrawal effects on savings rates.

The empirical evidence point to that the propensity to withdraw pension assets early increases on average by 0.1 percentage points for one percent reduction in the tax penalty rate. The effect is three times larger for individuals who are affected by a job loss or divorce within the same year as the withdrawal. This is consistent with the idea that pension assets are used to mitigate effects of adverse life events and allow liquidity constrained savers to smooth shocks. Conditional on withdrawing pension assets early we find that withdrawals increase significantly when the tax penalty rate is reduced.

Two key features of this paper cause the empirical evidence to stand out from the existing literature. First, we exploit variation in the tax rates that are levied on pre-retirement withdrawals. The two existing retirement schemes in the Danish pension system were subject to the same tax penalty rates until 2013. After this point, the tax rate on the one scheme was reduced by 17 percent, while the tax rate remained unchanged for the other scheme. We design a quasi-experiment to quantify the response in early withdrawal propensities as the tax reform was implemented. The setup is compelling because we define a counterfactual with common pre-trends in withdrawal propensities as the group of savers who were likely to be affected by the policy change. The second key feature is that we use Danish administrative records to estimate heterogenous treatment effects across a range of personal characteristics. In particular, we exploit information about the timing of job loss and divorce to show that such shocks interact with the decision to engage in early withdrawal behavior when the costs of doing so decline. The findings imply that those affected by adverse shocks or liquidity constraints seem to utilize the tax reform to obtain access to needed liquidity.

Our results are consistent with the idea that people finance their spending with the least costly sources first and turn to early withdrawals only when access to liquid wealth is limited. Further, those who cash out from retirement accounts around a job loss continue with lower savings rates for at least 6 years after the withdrawal. Together, this implies that people who engage in early withdrawals are less well prepared for retirement. The tax penalty rate is effective to refrain these individuals from cashing out early and thereby increase their overall savings and retirement income.

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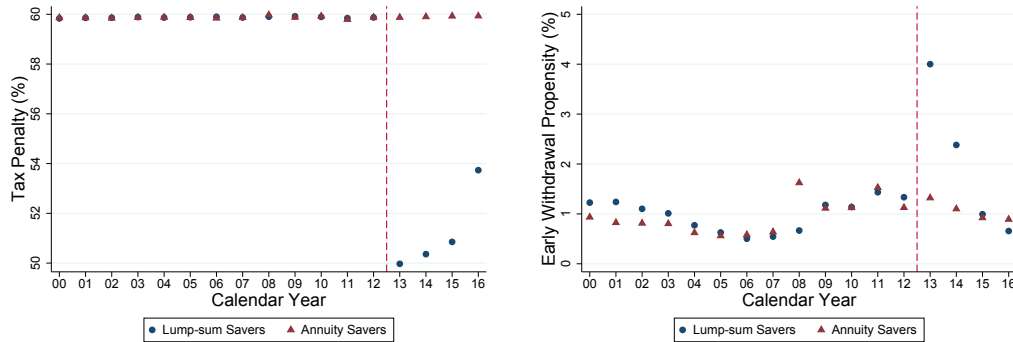
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# A Appendix

## A.1 Figures

Figure 9: Subsample of strictly lump-sum or annuity savers

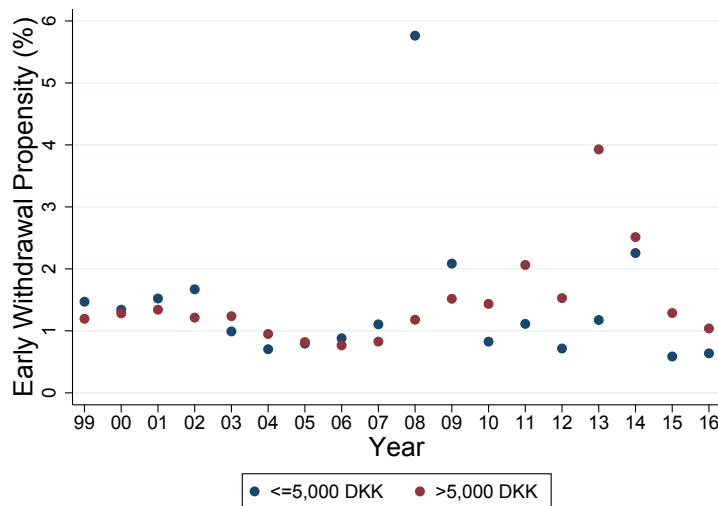


(a) Tax Penalty Rate (%)

(b) Early Withdrawal Propensity (%)

*Note:* The Figures divide the sample in two groups. One who saved in lump-sum pension schemes only during 1999–2012 and another who saved in annuity pension schemes only. Figure 9a shows the tax penalty rate for the two groups and 9b shows the propensity to withdraw pension assets early. The red dotted line represent the time of implementation of the tax penalty reduction.

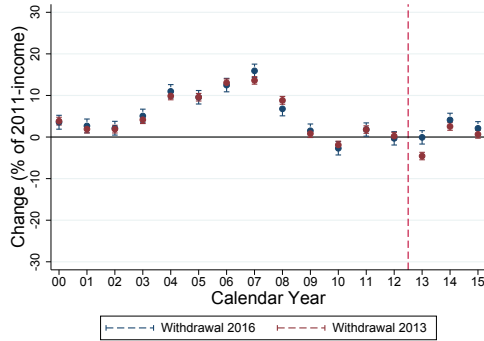
Figure 10: Early Withdrawal Propensity (%) Split in Small and Large Withdrawals



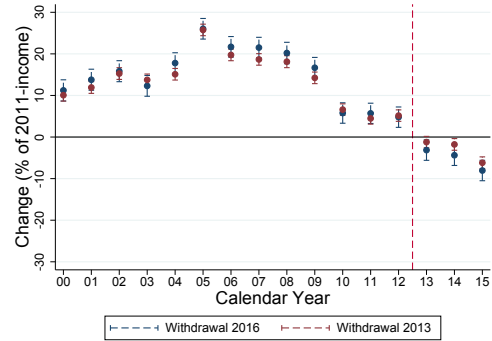
*Note:* The Figures shows the propensity to withdraw pension assets early within each calendar year. Withdrawals are divided in two groups. One that includes withdrawals below DKK 5,000 (US \$ 800) and one that includes withdrawals larger than this threshold. The DKK 5,000-threshold represents the value at which pension funds are likely to cash out the balance to the owner automatically. The paper considers withdrawals only when they exceed this limit.



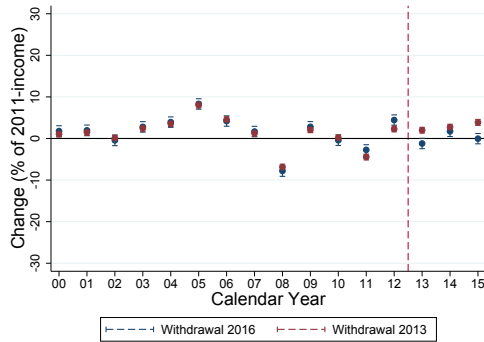
Figure 11: Effects of Early Withdrawals on Non-retirement Accounts (Treatment)



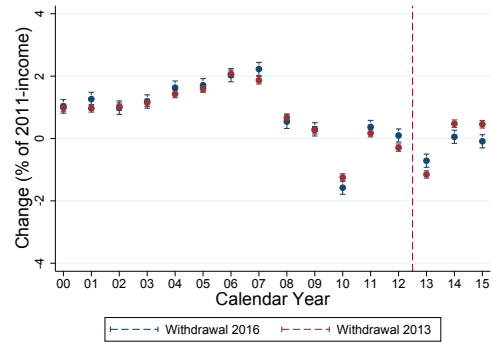
(a) Debt



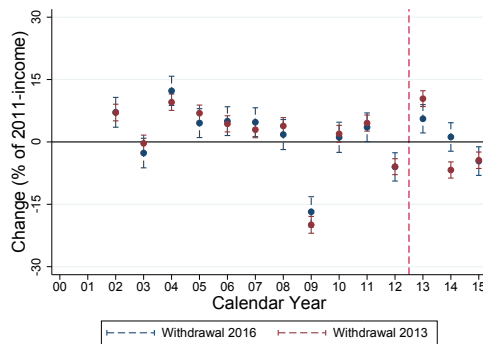
(b) Mortgages



(c) Assets



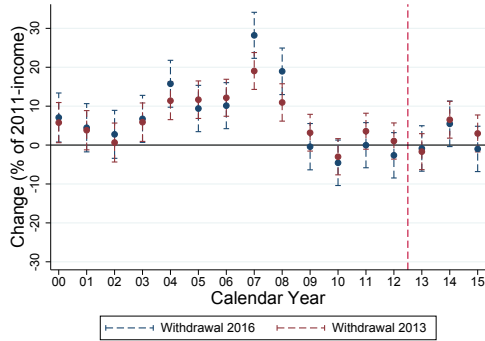
(d) Pensions



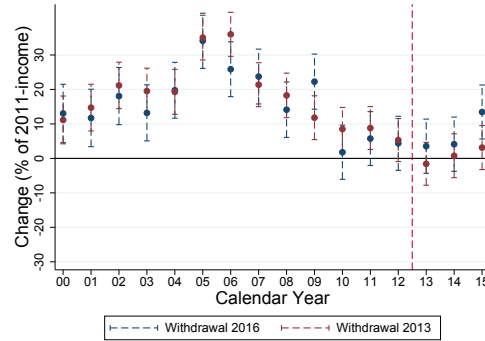
(e) Consumption

*Note:* The Figures present first differences to the change in savings and consumption components for the treatment group. They have all been normalized using 2011-values of disposable income. See notes in 7a.

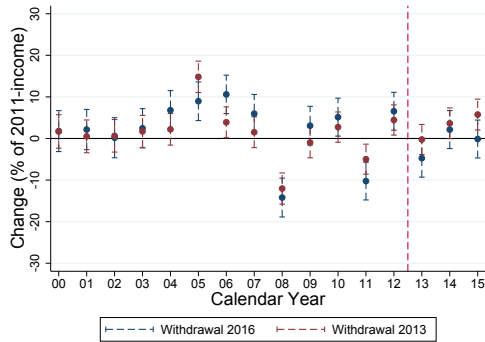
Figure 12: Effects of Early Withdrawals on Non-retirement Accounts (Control)



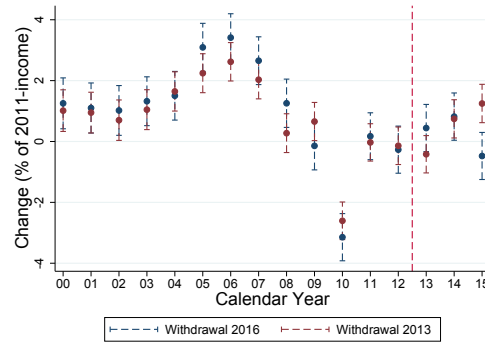
(a) Debt



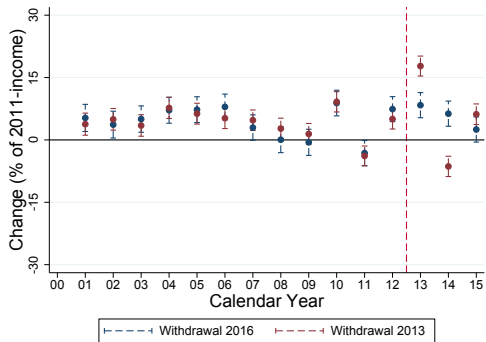
(b) Mortgages



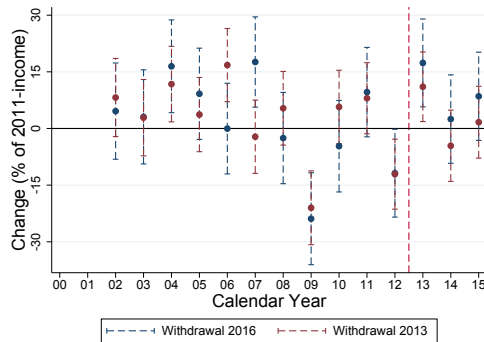
(c) Assets



(d) Pensions



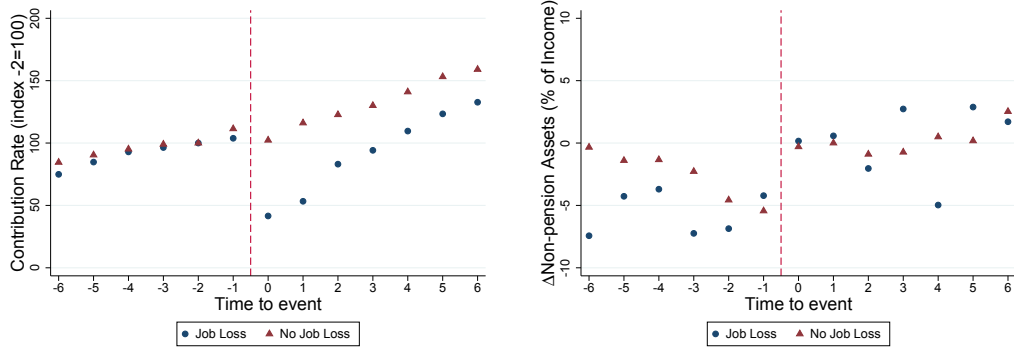
(e) Disposable Income



(f) Consumption

Note: The Figures present first differences to the change in savings and consumption components for the control group. They have all been normalized using 2011-values of disposable income. See notes in 7a.

Figure 13: Savings at Timing of Early Withdrawal and Job Loss (Liquidity Constrained)



(a) Pension Contribution Rate

(b) Savings in Non-retirement Accounts

*Note:* Figure 13a shows the pension contribution rate—that is the share of pension contributions relative to total compensation—indexed to 2 years prior to an early withdrawal. Figure 13b shows the first difference in changes to non-retirement wealth savings, including bank deposits, stocks, bonds and credit. This measure is normalized by disposable income 2 years prior to an early withdrawal. See notes in 8a.

## A.2 Tables

Table 7: Heterogeneous Reform Effects on Early Withdrawal Propensity

	(1)	(2)	(3)	(4)	(5)
Post	-0.000 (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)	-0.000 (0.000)
Treat	0.002*** (0.000)	0.003*** (0.000)	0.003*** (0.000)	0.003*** (0.000)	0.002*** (0.000)
Post × Treat	0.004*** (0.000)	0.006*** (0.000)	0.006*** (0.000)	0.006*** (0.000)	0.004*** (0.000)
Liq. Con.	0.008*** (0.000)				0.008*** (0.000)
Post × Liq. Con.	0.005*** (0.001)				0.005*** (0.001)
Treat × Liq. Con.	0.003*** (0.000)				0.003*** (0.000)
Post × Treat × Liq. Con.	0.005*** (0.001)				0.005*** (0.001)
Job Loss		0.009*** (0.001)			0.008*** (0.001)
Post × Job Loss		0.010*** (0.003)			0.009*** (0.003)
Treat × Job Loss		0.007*** (0.001)			0.007*** (0.001)
Post × Treat × Job Loss		0.013*** (0.004)			0.013*** (0.004)
Buy House			0.000 (0.001)		0.001 (0.001)
Post × Buy House			0.001 (0.002)		0.002 (0.002)
Treat × Buy House			-0.000 (0.001)		-0.000 (0.001)
Post × Treat × Buy House			0.002 (0.002)		0.002 (0.002)
Divorce				0.014*** (0.001)	0.014*** (0.001)
Post × Divorce				0.004 (0.003)	0.003 (0.003)
Treat × Divorce				0.004*** (0.001)	0.004*** (0.001)
Post × Treat × Divorce				0.012*** (0.003)	0.011*** (0.003)
Controls	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes
Observations	9609187	9609187	9609187	9609187	9609187
R <sup>2</sup>	0.005	0.002	0.002	0.002	0.005

Standard errors in parentheses. See notes in Table 5.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 8: Heterogeneous Reform Effects on Tax Penalty Rate

	(1)	(2)	(3)	(4)	(5)
Post	0.025*** (0.001)	0.025*** (0.001)	0.025*** (0.001)	0.025*** (0.001)	0.025*** (0.001)
Treat	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Post × Treat	-0.043*** (0.001)	-0.045*** (0.001)	-0.045*** (0.001)	-0.045*** (0.001)	-0.043*** (0.001)
Liq. Con.	0.000 (0.000)				0.000 (0.000)
Post × Liq. Con.	0.001 (0.001)				0.001 (0.001)
Treat × Liq. Con.	-0.000 (0.001)				-0.000 (0.001)
Post × Treat × Liq. Con.	-0.003*** (0.001)				-0.003*** (0.001)
Job Loss		0.000 (0.002)			0.000 (0.002)
Post × Job Loss		0.007* (0.004)			0.007 (0.004)
Treat × Job Loss		0.000 (0.002)			0.000 (0.002)
Post × Treat × Job Loss		-0.003 (0.004)			-0.002 (0.004)
Buy House			-0.000 (0.001)		0.000 (0.001)
Post × Buy House			-0.002 (0.003)		-0.002 (0.003)
Treat × Buy House			-0.000 (0.001)		-0.000 (0.001)
Post × Treat × Buy House			0.005 (0.003)		0.004 (0.003)
Divorce				0.000 (0.002)	0.000 (0.002)
Post × Divorce				0.006* (0.003)	0.006* (0.003)
Treat × Divorce				0.000 (0.002)	0.000 (0.002)
Post × Treat × Divorce				-0.005 (0.003)	-0.004 (0.003)
Controls	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes
Observations	128279	128279	128279	128279	128279
R <sup>2</sup>	0.492	0.492	0.492	0.492	0.492

Standard errors in parentheses. See notes in Table 5.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

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