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

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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 currency unit reduction in retirement savings 31 cents goes to debt repayments. Taking into account all types of savings, we find full crowd-out. Consistent with previous findings, we document that the effect is driven by a minority, about 23 percent, who actively rebalance their savings.

Keywords: Crowding-out, Pension Savings, Household Debt

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 higer interests than savings, outstanding debt should be repaid before saving in non-retirement accounts. A growing interest in the development of household

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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-optimise 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 that 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 II presents the empirical identification strategy, estimated substitution effects and the robustness of the empirical results. In section III, the share of active savers is estimated, showing heterogeneity on both observables and policy responses, while section IV concludes.

1. Institutional Setting and Policy Reform

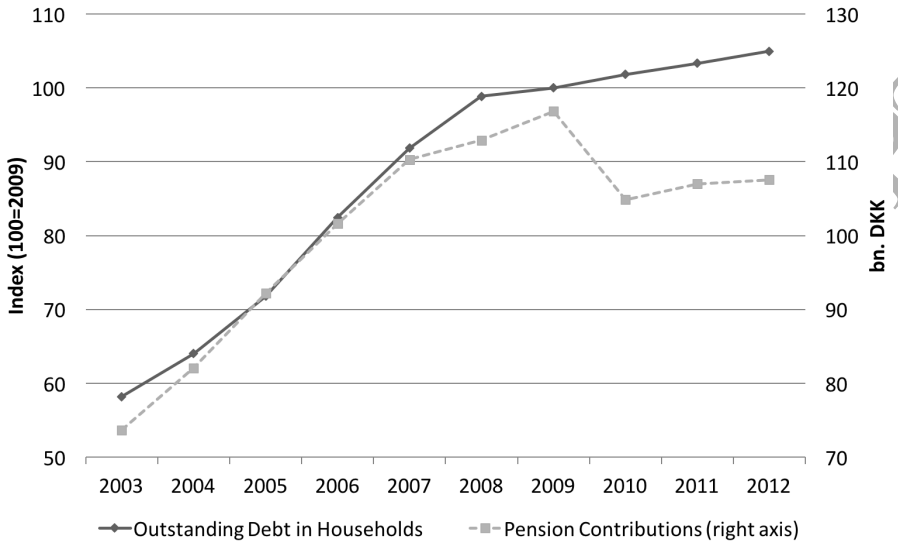
This section provides an overview of the Danish pension and mortgage system followed by an explanation of the policy reform that provides exogenous variation to savings behaviour in the research setup.

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?

1.1. Pension Tax Reform

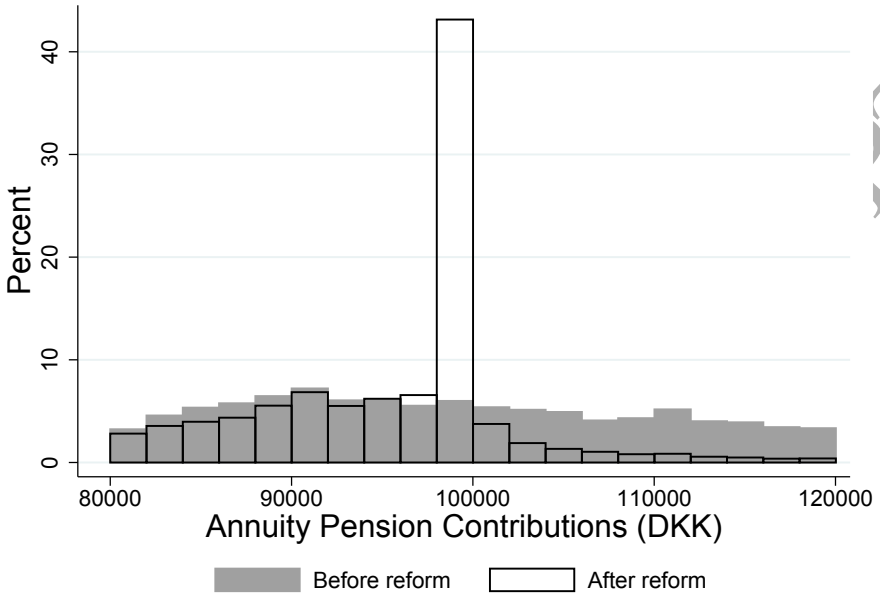
A Danish 2010 tax reform introduced a tax subsidy limit on contributions to annuity pension schemes. This reform implied that the sum of employer-paid and individual contributions to annuity pension schemes was tax-deductible only up to DKK 100,000 (US $15,000). This sharp change in taxation rules on 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, nation-wide 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.

Standard lifecycle models predict that individuals allocate savings wherever the after-tax return is higher. The theory predicts that taxpayers respond to

Figure 2: Kernel Density 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.

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.

1.2. 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.

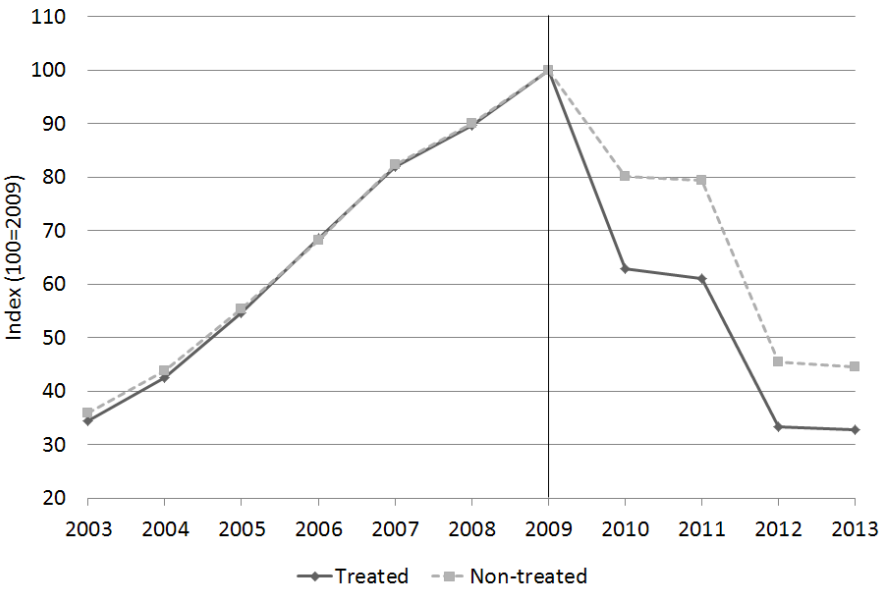
The estimation sample is not representative for the full Danish population. We include individuals in the estimation sample who contribute about DKK 100,000 to annuity pension schemes each year (see Table A.6). The full sample pension contribution average is about DKK 30,000. Similarly, income also differs 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 online appendix. 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.

2. 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 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

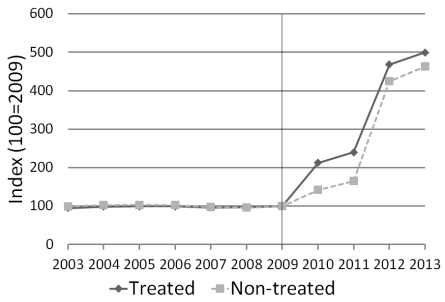
Figure 3: Annuity Pension Contributions



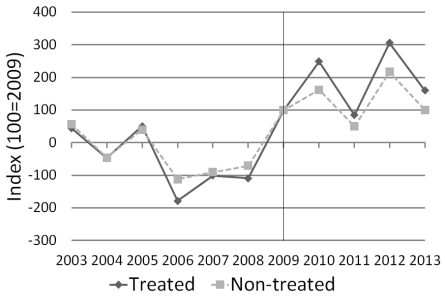
Note: Average contributions for annuity schemes are calculated within each year for the treated and non-treated groups. For each group, contributions are then indexed to 100 in 2009. Treatment individuals contributed DKK 100,001–150,000 to annuity schemes two years prior to the 2010 tax reform, while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

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

Figure 4: Alternative Saving Accounts



(a) Life-long Pensions



(b) Bank Debt Repayments (net)

Note: See Figure 3.
Source: Own calculations based on administrative data from Statistics Denmark.

contributions in Figure 4a show 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 B.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 α_i captures individual time-invariant effects. This includes individual tastes for savings as explained in Gelber (2011). Year fixed effects are captured by Ω_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 A.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 δ 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 A.4. The point estimate of δ 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.

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

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 γ 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 his 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 γ. 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 τ_i is calculated for each individual i. Provided with information on total taxes paid and taxable income from the tax authorities we proxy τ_i by dividing these two numbers. The after-tax measure of pension contributions is simply $P_{i,t}(1-\tau_i)$, where τ_i is fixed to the 2008-level.

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

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 γ 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 γ 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 A.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, 3-4½ 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

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

2.1. 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

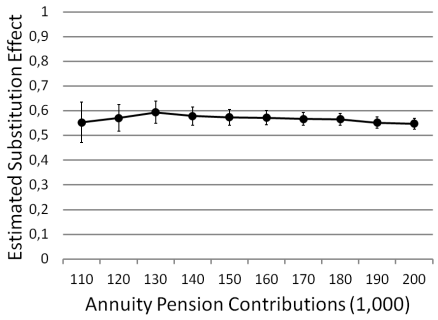
	Expl. var.: $Treat \times Post$				
	2010	2008	2007	2006	2005
Dep. var.:	(1)	(2)	(3)	(4)	(5)
Annuity Pensions	-21,038***	49	103	-474	1,280***
(Baseline)	(394)	(295)	(349)	(387)	(463)
Annuity Pensions	-28,657***	419	135	-710	884
(Donut-hole)	(493)	(378)	(444)	(494)	(589)

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.

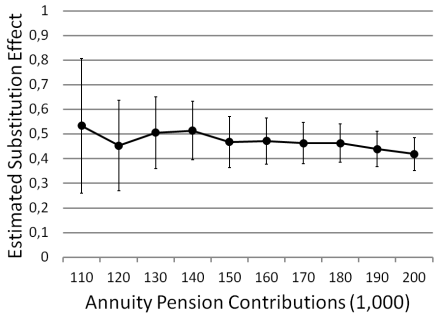
Robustness for changes in window size around the DKK 100,000-threshold is tested in the following. By varying the interval above the threshold when assigning individuals into treatment, while keeping the interval for assignment of non-treated individuals constant, sensitivity to the sample selection 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



(a) Retirement Accounts



(b) Non-retirement Accounts

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 A.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 online 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 A.7. 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 B.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 B.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 B.12a and B.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 A.7 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.

3. 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.

3.1. 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.

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

Using the specification in equation (4), coefficients β_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 A.7, 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 δ in column 4, covering the treated and non-treated groups for the time period 2004-20013. Values in column 4 are δ 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.

3.2. 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 A.8. 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 A.8 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 3½ 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.

3.3. 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

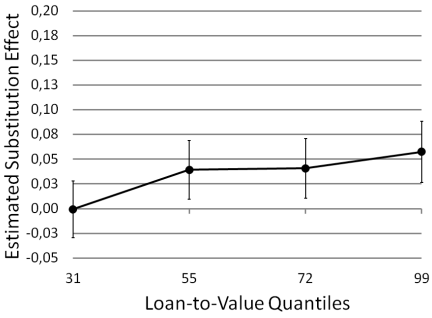
different phases of their lives. Highly indebted or younger individuals might find it more difficult to obtain credit in banks. Empirical studies have found that debt levels and age tend to play significant roles in credit constraints (Attanasio and Weber, 1994; Leth-Petersen, 2010) and this section 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.

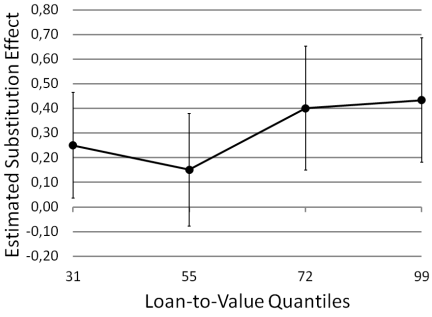
Equation (3) is used to test whether more leveraged individuals reduced debt more intensively than individuals with less debt relative to their real assets when the reform was introduced. First, individuals are divided into quantiles based on the pre-reform loan-to-value ratio. The ratio is total outstanding 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 B.13a and B.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.

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

Figure 6: Debt Repayments across Loan-to-Value



(a) Mortgage Debt

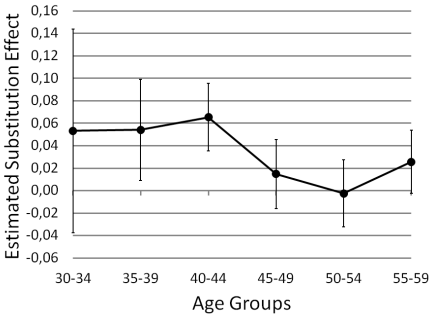


(b) Bank Debt

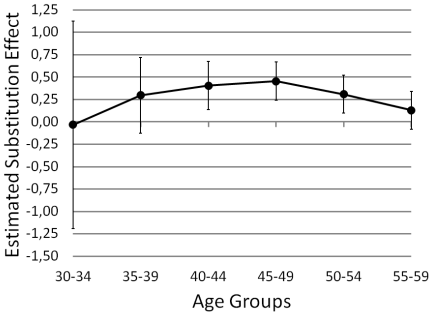
Note: Loan-to-value is outstanding mortgage debt divided by the mortgage institutions' assessment of property values. The estimated substitution effect is the estimates from equations (2) and (3), showing how many cents were shifted for a 1 unit reduction in annuity pension contributions. The effects 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.

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 online appendix).

Figure 7: Debt Repayments across 5-year Age Groups



(a) Mortgage Debt



(b) Bank Debt

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.

4. 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 crowding-out effect. 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 crowding-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 crowding-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 changes 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. Despite the fact that our findings are confined to the subgroup investigated they fit into a broader research agenda in the general pension policy debate, in particular in relation to the debate about whether policy makers should turn to mandates or tax incentives in order to increase individual savings. In order for tax incentives to increase savings rates, two conditions would have to be satisfied. One is that retirement savings should not create (full) crowding-out in other savings accounts. The other condition is that the majority of savers would have to respond actively to variation in after-tax prices on savings in tax-favoured retirement accounts. Our paper rejects that these conditions are fulfilled for high-income earners. Similar conclusions are drawn for mid-range income earners in other microdata based quasi-experiments. This supports the general view that savings mandates (or more broadly, default savings schemes) dominate tax incentives as the most effective policy tool to increase individual savings rates. How effective these types of savings policies are in the bottom income deciles remain an empirical question for further research. A second topic of interest in the pension policy debate is how to explain the observed balance sheet expansion that has taken place for households over the past decades, i.e. an increase in both assets and liabilities with no change in net balances. Such an expansion could pose a threat to macroeconomic stability when high gross debt levels enhance spending cuts during recessions as documented by a range of empirical studies. Our study suggests that a generous tax-treatment of savings in retirement accounts is connected directly to the accumulation of debt, potentially causing both savings and debt accounts to increase. To our best knowledge no other paper has been able to quantify the link between incentives for saving in pension accounts and accumulation of debt.

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Appendix A. Tables

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

	Expl. var.: Annuity Pensions			
	Collapse Years	Incl. Self-emp	Normalised using Income	Excl. Housing Wealth
Dep. var.:	(1)	(2)	(3)	(4)
Life-long Pensions	.567***	.550***	.441***	.561***
	(.016)	(.018)	(.017)	(.016)
Capital Pensions	.007**	.004	.022***	.006*
	(.003)	(.003)	(.004)	(.003)
Mortgage Repayments	.024***	.025***	.011	.021***
	(.006)	(.006)	(.007)	(.006)
Bank Debt Repayments	.294***	.419***	.277***	.304***
	(.058)	(.072)	(.069)	(.058)
Bank Deposits	.155***	.123**	.112**	.141***
	(.050)	(.058)	(.057)	(.050)
Financial Assets	.051	.065	.022	.031
	(.036)	(.042)	(.046)	(.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 A.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 A.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 A.7: Changes in Savings when Reducing Annuity Pensions by 1 Unit

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

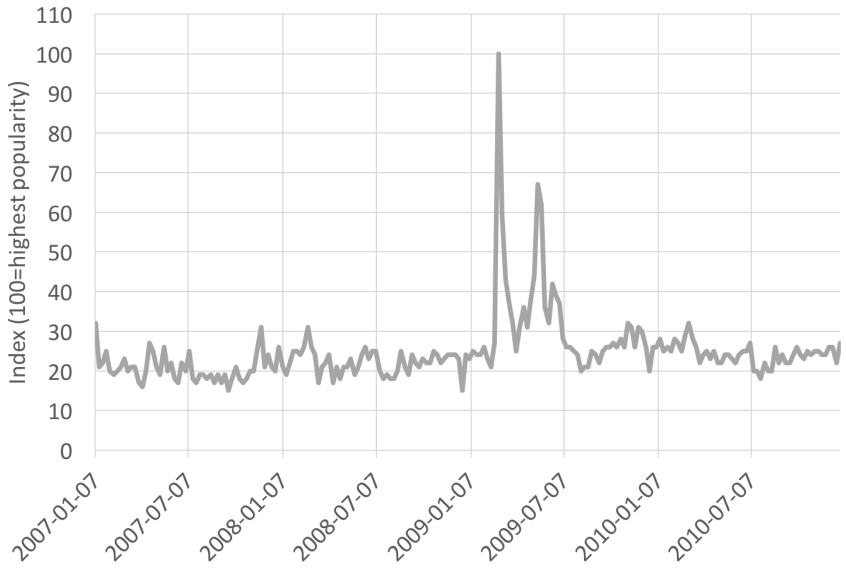
Table A.8: Observable Heterogeneity in Active vs. Passive Savers

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

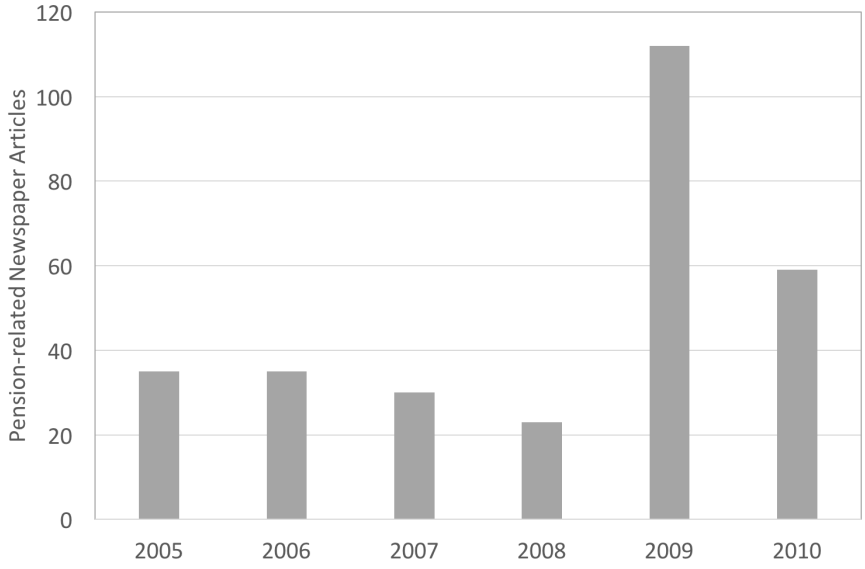
Appendix B. Figures

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



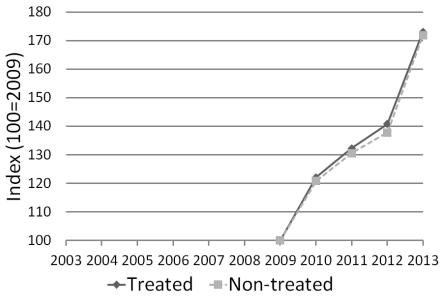
Note: The numbers represent the search interest in relation to the highest point in the chart for that area and the time. A value of 100 is the greatest popularity of the term, a value of 50 means that the term is half as popular, and a result of 0 means that the term was less than 1% as popular as the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

Figure B.9: Pension-related newspaper articles in Denmark

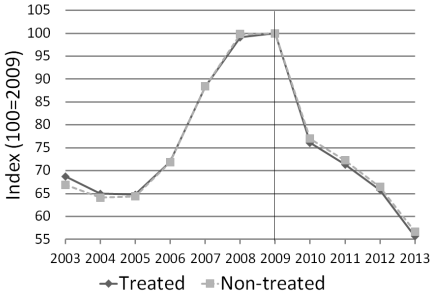


Note: The number of articles is based on a search of nation-wide newspapers in Denmark using the phrases "ratepension", which is the Danish word for annuity pension and "fradrag", which is Danish for tax-deduction.
Source: Infomedia.

Figure B.10: Mortgage Debt



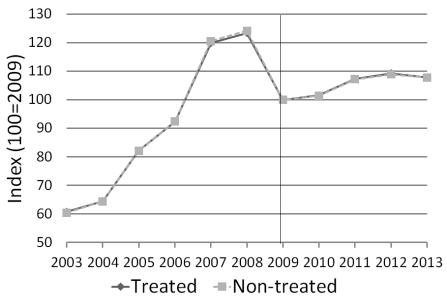
(a) Mortgage Debt Repayments



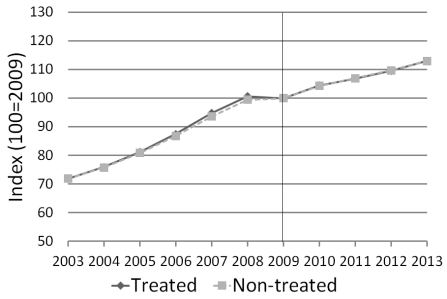
(b) Mortgage Interest Payments

Note: See Figure 3.
Source: Own calculations based on administrative data from Statistics Denmark.

Figure B.11: Housing Wealth and Income



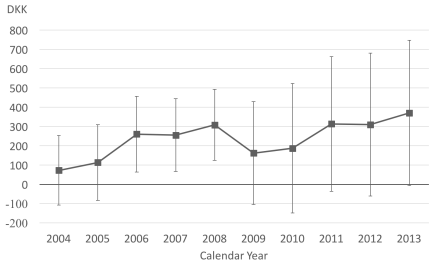
(a) Housing Wealth



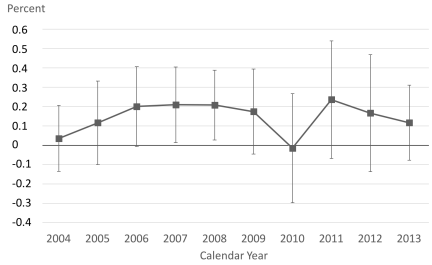
(b) Gross Income

Note: See Figure 3.
Source: Own calculations based on administrative data from Statistics Denmark.

Figure B.12: Labour markets outcomes



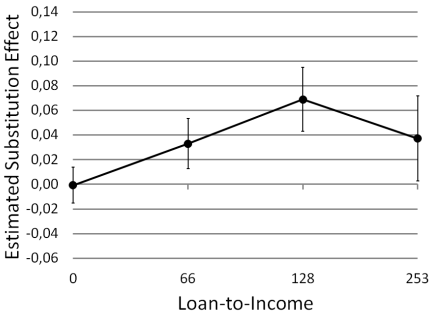
(a) Unemployment benefits



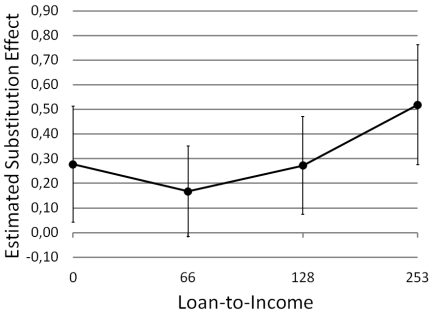
(b) Unemployment rate

Note: Figure B.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 B.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 B.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.