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ABSTRACT

Do retirement savings policies – such as tax subsidies or employer-provided pension plans – increase total saving for retirement or simply induce shifting across accounts? We revisit this classic question using 45 million observations on wealth for the population of Denmark. We find that a policy's impact on wealth accumulation depends on whether it changes savings rates by active or passive choice. Tax subsidies, which rely upon individuals to take an action to raise savings, have small impacts on total wealth. We estimate that each $1 of tax expenditure on subsidies increases total saving by 1 cent. In contrast, policies that raise retirement contributions if individuals take no action – such as automatic employer contributions to retirement accounts – increase wealth accumulation substantially. Price subsidies only affect the behavior of active savers who respond to incentives, whereas automatic contributions increase the savings of passive individuals who do not reoptimize. We estimate that approximately 85% of individuals are passive savers. The 15% of active savers who respond to price subsidies do so primarily by shifting assets across accounts rather than reducing consumption, implying that they have low elasticities of intertemporal substitution. These individuals are also more likely to offset changes in automatic contributions and have higher wealth-income ratios. We conclude that automatic contributions are more effective at increasing savings rates than price subsidies for three reasons: (1) subsidies induce relatively few individuals to respond, (2) they generate substantial crowdout conditional on response, and (3) they do not influence the savings behavior of passive individuals, who are least prepared for retirement.

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I Introduction

Do retirement savings policies – such as tax subsidies, employer-provided pensions, and savings mandates – raise total wealth accumulation or simply induce individuals to shift savings across accounts? This question is central for understanding the optimal design of retirement savings policies. Despite extensive research, the impacts of retirement savings policies on wealth accumulation remain unclear, largely due to limitations in data and research designs (Bernheim, 2002).

In this paper, we revisit this classic question using a panel dataset with 45 million observations on savings in both retirement and non-retirement accounts for the population of Denmark. We organize our empirical analysis using a stylized model in which the government implements two policies targeted at raising savings: a price subsidy and an automatic contribution that puts part of an individual's salary in a retirement account. We analyze the impacts of these policies on two types of agents: active savers and passive savers. Active savers make savings decisions by maximizing utility, taking into account the subsidies and automatic contributions. Passive savers make fixed pension contributions that are invariant to the automatic contribution and subsidy.[1]

The model predicts that automatic contributions should have no impact on total saving for active savers who are not at a corner, as they fully offset the automatic contribution by reducing voluntary pension contributions.[2] In contrast, the impact of automatic contributions on total saving is ambiguous for passive savers. If passive savers absorb the reduction in disposable income due to the automatic contribution by maintaining a fixed consumption plan and running down their bank balance, automatic contributions have no impact on total saving even though they increase savings within pension accounts. But if passive savers absorb the reduction in disposable income by reducing consumption and maintaining a fixed non-pension savings target, automatic contributions increase total saving. Price subsidies induce active savers to save more in pension accounts. But once again, the impacts on total wealth accumulation are ambiguous, as they depend upon the relative magnitude of price and wealth effects.

We analyze the impacts of price subsidies and automatic contributions empirically using Danish income tax records. These data provide administrative information on the value of assets and liabilities of all Danish citizens from 1994-2009. The Danish pension system – which has individual accounts, employer-provided pensions, and a government defined-benefit plan – is broadly similar

[1] Such passive behavior can be micro-founded in several ways, including fixed costs of attention or present-biased preferences (Carroll et al., 2009). Our results do not depend on the source of passive behavior.

[2] Throughout the paper, we use the term "total saving" to refer to the sum of annual flows to retirement and non-retirement accounts.

in structure to that in the U.S. and other developed countries. The Danish data and institutional environment have two primary benefits. First, they offer administrative information for a much larger number of individuals than recent studies based on survey data, which typically have fewer than 1000 observations in their analysis samples (e.g., Gelber, 2011). Second, there were a series of sharp reforms in Denmark that provide quasi-experimental research designs to identify the impacts of retirement savings policies.

We divide our empirical analysis into three sections. First, we analyze the impacts of defined-contribution employer pension plans and government mandates, both of which are "automatic contributions" in the sense that they raise retirement saving if individuals take no action. Using event studies of individuals who switch firms, we find that individuals' total saving rises immediately by more than 85 cents when they move to a firm that contributes DKr 1 more to their retirement account even if they could have fully offset the increase.[3] Most individuals do not change voluntary pension contributions, savings in taxable accounts, or liabilities at all when they switch firms, consistent with passive behavior. The savings impacts are equally large when we restrict attention to the subset of individuals who switch firms because of a mass-layoff at their prior firm, confirming that our estimates are not biased by endogenous sorting. The changes in savings behavior persist for ten years after the firm switch and ultimately result in higher wealth balances at the age of retirement.

We also analyze the impacts of a Mandatory Savings Plan (MSP) that required all Danish citizens to contribute 1% of their earnings to a retirement savings account from 1998 until 2003. We find sharp increases in total saving in 1998 and sharp reductions in total saving in 2004. We estimate that the MSP raised total saving by roughly 1% on average, i.e. there was little or no offset in other accounts. The MSP raised total saving even for individuals who were previously saving more than 1% of their earnings in voluntary retirement savings accounts, which are nearly a perfect substitute for MSP. We conclude that automatic contributions generate relatively little crowd-out and increase total wealth accumulation significantly, implying that many individuals are passive savers who reduce consumption when their disposable income falls.

In the second part of our empirical analysis, we study the impacts of subsidies for retirement savings. Denmark has two types of tax-deferred savings accounts – capital pensions that are paid

[3] Throughout our empirical analysis, we account for mechanical effects due to corners by focusing either on individuals who were already saving more than the change in employer pensions or using statistics such as the fraction contributing above thresholds that are not affected by corners. We also show that changes in pension contributions have no effect on debt, so impacts on gross and net saving are similar.

out as a lump sum upon retirement and annuity pensions that are paid out as annuities. In 1999, the government reduced the subsidy for contributing to capital pension accounts by 14 cents per Danish Kroner (DKr) for individuals in the top income tax bracket. Individuals below the top income tax bracket were unaffected by the reform and the tax treatment of annuity pension accounts was unchanged. Using difference-in-differences and regression kink designs around the top tax cutoff, we find that capital pension contributions fell sharply for individuals in the top income tax bracket but remained virtually unchanged for individuals just below that bracket. Importantly, the aggregate reduction in capital pension contributions is entirely accounted for by just 17% of prior contributors, most of whom stop making capital pension contributions in 1999. We estimate that 83% of prior contributors do not change their capital pension contributions at all even though utility maximization would call for some non-zero change in contributions when prices change at an interior optimum. This finding is consistent with our estimate that 85% of individuals also do not undo automatic contributions.

Next, we investigate whether the changes in pension contributions among the 17% of active savers led to changes in total wealth accumulation. We estimate two crowd-out parameters: the degree of shifting between different pension accounts and the degree of shifting from pension accounts to taxable savings accounts. First, we find that 55 cents of each DKr that would have been contributed to capital pensions is shifted to annuity pension accounts when the capital pension subsidy was reduced. Hence, total pension contributions fall by 45 cents for each DKr of reduction in capital pensions. Second, focusing on this reduction in total pension contributions, we estimate that 98 cents of each DKr contributed to retirement accounts comes from money that would have been saved in a taxable account. The small impact of the subsidy on total saving even for active savers implies that their elasticity of intertemporal substitution (EIS) is low. The 98 cent crowd-out estimate determines the overall impact of retirement savings subsidies on total saving and can be compared to estimates reported in the prior literature (e.g., Engen, Gale and Scholz, 1996; Poterba, Venti and Wise, 1996). Based on this estimate, we calculate that each DKr 1 of expenditure by the government on subsidies for retirement savings raises total saving by less than 1 cent on average. The upper bound on the 95% confidence interval for this estimate is 23 cents.

In the final part of our empirical analysis, we investigate heterogeneity in responses across individuals. We document three pieces of evidence which indicate that active vs. passive choice is the key reason that automatic contributions and subsidies have very different effects on total saving. First, we find that the 1999 subsidy reduction has much larger effects on individuals who are starting a new pension in that year relative to those already making pension contributions in previous years. This result confirms our model's prediction that individuals making active choices should be more responsive to incentives and is consistent with evidence on inertial behavior in other domains (Samuelson and Zeckhauser, 1988; Ericson, 2012). Second, we find that individuals who actively change their pension contributions more frequently in other years are more responsive to the price subsidy change and more likely to offset automatic contributions by changing their own individual pension contributions. Third, we find that individuals who have higher wealth/income ratios, are older, or have economics training are more responsive to price subsidies and more likely to offset automatic contributions. In sum, the people we would expect to be active savers – financially sophisticated individuals who plan for retirement and frequently reoptimize their pension choices – are more responsive to price subsidies and less influenced by automatic contributions.

We conclude that the impacts of retirement savings policies on wealth accumulation depend on whether they change behavior through active or passive choice. Policies that rely upon individuals to take an action to raise savings have significantly smaller impacts on total saving than policies that raise savings automatically even if individuals take no action. Furthermore, the low EIS of active savers combined with the lack of response by passive savers suggests that capital income taxes are likely to have small effects on savings rates.

Our results contribute to and build on three large literatures: research in public finance analyzing crowd-out in retirement savings accounts, research in behavioral economics on increasing retirement savings using non-traditional policies, and research in macroeconomics on consumption behavior. While these literatures have developed independently, our results show that there is a deep connection between these three strands of work.

In the public finance literature, Hubbard (1984), Venti and Wise (1986), Skinner and Feenberg (1990), Poterba, Venti and Wise (1994, 1995, 1996), Hubbard and Skinner (1996), and Gelber (2011) present evidence that increases in IRA or 401(k) savings represent increases in total saving. But Gale and Scholz (1994), Engen, Gale and Scholz (1994, 1996), Gale (1998), and Engelhardt and Kumar (2007) argue that much of the increase in 401(k) savings represents substitution from other accounts. Although some of the difference between the results of these studies likely stems from differences in econometric assumptions, the variation that drives changes in contributions to 401(k)'s could also explain the differences in results. For instance, increases in 401(k) contributions by employers may generate less crowd-out than tax incentives or programs that require active individual choice.[4]

The behavioral economics literature has shown that defaults and, to a lesser extent, salient price subsidies significantly increase saving *within* retirement accounts (e.g., Madrian and Shea, 2001; Duflo et al., 2006; Card and Ransom, 2011; Thaler and Benartzi, 2004). Importantly, however, this prior work has not investigated whether defaults raise *total* saving. As we show in our stylized model, the impacts of these policies on total saving depend fundamentally on how consumers adjust their budgets when their disposable income falls. There is no theoretical reason to expect that the adjustment occurs by reducing consumption instead of non-retirement saving. Our finding that policies that change saving passively do raise total saving thus significantly strengthens the argument for policies such as automatic enrollment and defaults if a policy maker's goal is to increase savings rates (e.g., Carroll et al., 2009; Madrian, 2012).

Finally, our results are consistent with the literature in macroeconomics showing that the marginal propensity to consume out of disposable income is much higher than predicted by frictionless lifecycle models (e.g., Johnson, Parker and Souleles, 2006). One well known explanation for excess sensitivity is a model of "spenders" and "savers" (Campbell and Mankiw, 1989; Mankiw, 2000), in which some agents follow a rule-of-thumb based on current disposable income and others optimize according to the life-cycle model. Our results support such a model and suggest that 85% of individuals are rule-of-thumb spenders. The heterogeneity in excess sensitivity across households appears to be partly driven by financial sophistication and planning rather than pure liquidity constraints. Finally, our results are consistent with small estimates of elasticities of intertemporal substitution (e.g. Hall, 1988), both because the "structural" EIS is small and because passive behavior further attenuates the observed elasticity.

The remainder of the paper is organized as follows. Section II presents a stylized model and characterizes its comparative statics. Section III describes the Danish data and institutional background. Sections IV, V, and VI present the empirical results on automatic contributions, price subsidies, and heterogeneity across individuals, respectively. We conclude in Section VII by discussing policy implications.

[4] This idea was foreshadowed in early work by Cagan (1965) and Green (1981), who argued that employer pensions led to 1-for-1 increases in total saving because many individuals were unaware of the details of their employer pension policies.

II Conceptual Framework

In this section, we set up a stylized model of savings behavior and analyze its comparative statics to structure our empirical analysis.

II.A Setup

Individuals, indexed by i, live for two periods. They earn a fixed amount W in period 1, which they can either consume or save in one of two risk-free accounts: a retirement account or a taxable savings account. Let r denote the net-of-tax interest rate that individuals earn in the taxable account. The government implements two policies with the goal of increasing total retirement savings: an automatic (mandated) contribution to the retirement account of M and a subsidy θ that increases the return to saving in the retirement account to $r + \theta$. To simplify notation, we abstract from income and capital gains taxes and let θ represent the net subsidy to retirement accounts taking all taxes into account. We assume that the subsidy is financed by a tax on future generations or other agents outside the model.[5]

Let P denote voluntary individual contributions to the retirement account, M the automatic (mandatory) contribution to the retirement account, and S the flow of taxable saving. To eliminate mechanical effects of changes in M that force individuals to save more, we abstract from corners and assume that S and P can be negative.[6] Consumption in the two periods is given by

$$\begin{aligned} c_1(S, P) &= W - S - M - P \qquad (1) \\ c_2(S, P) &= (1 + r)\, S + (1 + r + \theta)\, (M + P)\,. \end{aligned}$$

In this two period setting, saving in the retirement account strictly dominates saving in taxable accounts, and hence all individuals would optimally set $S = 0$. In practice, retirement accounts are illiquid and cannot be accessed prior to retirement, leading many individuals to save outside retirement accounts despite their tax disadvantage. We model the value of liquidity as a concave benefit $\phi(S)$ of saving in the non-retirement account.[7] Accounting for the value of liquidity, individuals

[5] We make this assumption because the variation in θ in our empirical application affects a small set of agents and is financed out of general revenues.

[6] In practice, individuals essentially face a constraint of $P \geq 0$ because there are significant penalties for early withdrawal in retirement accounts in Denmark. However, we always account for corners in our empirical analysis by restricting attention to individuals who are in the interior.

[7] Gale and Scholz (1994) develop a three period model in which individuals face uncertainty in the second period, motivating them to keep some assets in a liquid buffer stock. Our model can be loosely interpreted as a reduced-form of the Gale and Scholz model.

have utility

$$u(c_1) + \delta_i u(c_2) + \phi(S). \tag{2}$$

where $\delta_i < 1$ denotes individual i's discount factor.

Active vs. Passive Savers. There are two types of individuals in the economy – active and passive savers – who differ in the way they choose S and P.[8] Let α denote the fraction of active savers. Active savers choose S and P to maximize utility (2) given M and θ as in the neoclassical model. Passive savers set retirement contributions at an exogenous level $P = \bar{P}_i$ that does not vary with M and θ. There are several models in the literature for why individuals' retirement savings plans are insensitive to incentives, such as fixed costs of adjustment that generate inertia, hyperbolic discounting that leads to procrastination in updating plans (Carroll et al., 2009), or a lack of information. The results that follow do not depend upon which of these micro-foundations drives passive behavior, and we therefore do not specify a particular model of passive choice.

Regardless of how passive savers make choices, they must satisfy the budget constraint in (1), which can be rewritten as

$$c_1 + S = W - M - \bar{P}_i, \tag{3}$$

i.e. consumption plus taxable saving equals disposable income net of pension contributions. We assume that passive savers choose S (or, equivalently, c_1) as a function of disposable income $W - M - \bar{P}_i$, so that changes in retirement savings policies affect behavior in period 1 only if they affect retirement contributions. Again, we do not posit a specific model of how passive savers choose S. Instead, we show how the impacts of government policy depend upon the way in which passive savers adjust c_1 and S when disposable income changes.

II.B Comparative Statics

Table 1 summarizes the comparative static predictions of our model. We begin by characterizing the impacts of the automatic contribution in Columns 1 and 2 of the table. Because voluntary pension contributions are a perfect substitute for automatic contributions, active savers who are not at a corner undo changes in M 1-for-1 by reducing P. Hence, automatic contributions do not affect their total retirement contributions $M + P$ or total saving $M + P + S$. In contrast, passive savers leave P fixed by definition and hence their total retirement contributions $M + P$ rise with

[8] We define "active" and "passive" savers based upon how they respond to incentives rather than exogenous characteristics. In our model, active savers not only respond to changes in pension plans but do so as predicted by the neoclassical model. Correspondingly, in our empirical analysis, we identify the two types of savers based on their responses to policy changes and show that active responders' behavior aligns with the neoclassical predictions.

M. It follows that we can estimate the fraction of passive savers $(1-\alpha)$ as $d(M+P)/dM$ if we restrict attention to individuals who are not at a corner.

The impact of M on the total saving of passive savers depends on whether they cut consumption c_1 or non-retirement savings S to meet the budget constraint in (3). Two cases span the potential responses. At one extreme, if an individual has a fixed consumption plan $\bar{c}_1$ and does not pay attention to retirement savings, he will end up with a smaller bank balance S at the end of the year. In this case, changes in M will have *no impact* on total saving. At the other extreme, if an individual has a fixed target for his bank balance $\bar{S}$, he will absorb the reduction in disposable income by reducing consumption c_1. In this case, a \$1 increase in M will increase total saving by \$1. Between these two extremes, we may observe impacts of M ranging from 0 to 1 depending upon the way in which passive savers set their budgets.[9] The key point is that the impact of automatic contributions on total saving is unclear even if individuals are inattentive to retirement savings. Existing evidence that automatic contributions increase savings within retirement accounts should not necessarily make us expect that such policies will raise total saving.

Next, we turn to the impacts of price subsidies (Columns 3 and 4 of Table 1). By definition, price subsidies have no impact on the retirement contributions of passive savers. Since changes in price subsidies have no impact on disposable income in period 1 when individuals do not change P, they also do not affect $M+P+S$ for passive savers.[10] The impacts of an increase in the price subsidy on active savers have been characterized in prior work (e.g., Gale and Scholz, 1994; Bernheim, 2002). Increases in the subsidy θ affect S and P through three channels: (1) by reducing the price of P relative to S, leading to substitution across accounts; (2) by reducing the price of c_2 relative to c_1, leading to increased total saving; and (3) by increasing total lifetime wealth, which raises period 1 consumption c_1 and hence reduces saving. The magnitudes of these effects are controlled by two preference parameters: the elasticity of intertemporal substitution (EIS), which is determined by the curvature of utility $u(c)$, and the elasticity of substitution across retirement savings accounts and non-retirement accounts, which is determined by the curvature of the liquidity benefit $\phi(S)$. Note that $dP/d\theta = 0$ only in the knife-edge case where the wealth effect exactly offsets the two price effects. Hence, we can obtain another estimate of the fraction of passive savers $(1-\alpha)$ from the fraction of individuals for whom $dP/d\theta = 0$.

[9] If all individuals are either pure consumption or savings targeters, then we can estimate the fraction of savings targeters as $\frac{d(M+P+S)}{dM}/\frac{d(M+P)}{dM}$, i.e. the fraction of passive savers who do not change S when M is increased.

[10] Price subsidies do mechanically increase total retirement wealth for passive savers; the point here is that they do not affect period 1 choices P and S.

If either the EIS or the cross-account substitution elasticity is sufficiently large, the two price effects dominate the wealth effect within pension accounts. In this case, an increase in θ will increase total pension contributions $M + P$. Prior work consistently finds positive effects of subsidies on retirement contributions (e.g., Duflo et al., 2006; Engelhardt and Kumar, 2007), suggesting that this is the relevant case in practice. However, the effects of price subsidies on total saving $M+P+S$ are unclear. If the EIS is small, the increase in P induced by a price subsidy could come largely from shifting assets across accounts, with little impact on total saving. If the EIS is large, an increase in θ would increase total saving.

An obvious but important implication of our framework is that automatic contributions can affect the pension contributions of all individuals, whereas price subsidies only affect the behavior of active savers. Because active optimizers are also likely to be cognizant of all the accounts in which they might save, one may expect such individuals to reoptimize by shifting assets across accounts in response to an increase in a subsidy. As a result, the increased pension contributions induced by subsidies could potentially be crowded out more than policies that affect passive savers, who do not reshuffle assets because they are not paying attention to retirement account balances.

Finally, our framework makes a set of predictions about the heterogeneity of responses across individuals that are helpful for testing the model. First, individuals who are currently making active choices – e.g. those who are starting new pension accounts – should be more responsive to price subsidies. Second, the types of individuals who respond to price subsidies – and hence are active savers – should also be more likely to offset automatic contributions. Third, typical micro-foundations for active vs. passive choice predict that active savers should have higher levels of retirement savings $P + S$. For instance, in Carroll et al.'s (2009) model, individuals with low discount factors δ_i – who have low savings rates – tend to be passive savers. These individuals postpone retirement planning because the fixed up-front costs of planning for retirement outweigh the low NPV gains from planning. More generally, the same characteristics that make some individuals actively optimize with respect to retirement savings incentives, such as financial literacy or attentiveness, are also likely to make these individuals plan for retirement to begin with. The testable implication of this correlation is that automatic contributions should have greater impacts on the savings of low-wealth individuals, while price subsidies should generate larger responses among high-wealth individuals.

In the remainder of the paper, we (1) test the qualitative predictions in Table 1, (2) quantify the fraction of active vs. passive savers, and (3) analyze heterogeneity across individuals.

III Data and Institutional Background

Institutional Background. This section provides institutional background relevant for the research designs we implement below. See OECD (2009) or Bingley et al. (2007) for a comprehensive description of the Danish retirement system and Danish Ministry of Taxation (2002) for a description of the income tax system. Note that over the period we study, the exchange rate was approximately DKr 6.5 per US $1.

The Danish pension system consists of three components that are typical of retirement savings systems in developed countries: a state-provided defined benefit (DB) plan (analogous to Social Security in the U.S.), employer-provided defined contribution (DC) accounts (analogous to 401(k)'s in the U.S.), and individual retirement accounts (analogous to IRA's in the U.S.).

The defined-benefit pension in Denmark pays a fixed benefit subject to earnings tests. For example, in 1999 the DB pension paid a benefit of DKr 95,640 (US $14,700) for most single individuals over the age of 67.[11] Because our analysis focuses exclusively on DC accounts, we do not summarize the DB system further here. The structure of the DB pension system did not change in a way that affects our analysis of DC accounts over the period we study.

Most jobs in Denmark are covered by collective bargaining agreements between workers' unions and employer associations. These agreements set wage rates and often include a pension savings plan in which a fixed proportion of an individual's earnings is paid into a retirement account that is managed by an independent pension fund. Typically, two-thirds of contributions to employer-provided pensions are made by employer, while individuals are required to contribute one-third of the amount out of their own earnings. Retirement contributions vary substantially across employers: the standard deviation of employer contribution rates is 5.3% of income among those who have employer pensions.

Individual DC accounts are completely independent of employer accounts but have equivalent tax properties. Individual contributions do not need to be updated once they are set up, and in particular do not necessarily need to be changed as individuals change employers.

Within both the employer and individual DC pensions, there are two types of accounts: "capital pension" accounts and "annuity pension" accounts.[12] These two accounts have different payout

[11] This figure combines a base pension of DKr 48,024 for those with income under DKr 204,000 and an additional pension of DKR 47,616 for those with labor income under approximately DKr 45,000. For every DKr 1,000 of income that individuals have above these thresholds, the relevant pension payout is reduced by DKr 300. For individuals with low liquid wealth, there is a separate supplemental pension of DKr 21,468 in 1999 (US $3,300) as well as support for heating and rental expenses.

[12] Annuity pensions can be further broken into two sub-categories labeled "rate" and "annuity" pensions, which

profiles and tax consequences. Balances accrued in capital pension accounts are paid out as a lump sum and taxed at 40% on payout. Balances accrued in annuity pension accounts are paid out over several years – e.g., as a 10-year annuity or a lifetime annuity – and are taxed as regular income.[13] Balances in capital pension accounts can be converted to annuity pensions, but the reverse is not possible. Contributions to both types of accounts are tax deductible at the time of contribution. Capital gains in both capital and annuity retirement accounts are taxed at 15%, compared with an average of approximately 29% for assets in taxable accounts. Withdrawals prior to retirement from either account incur a tax of 60% plus administrative fees, making early withdrawals quite rare.

Employers set the amount that they contribute to capital and annuity pension accounts for their workers. The sum of employer and individual contributions to each type of account is capped at limits that have gradually increased over time. For instance, in 1999, total contributions to capital pension accounts were limited to DKr 34,000 (US $5,000); in 2009, the cap was DKr 46,000. The cap binds for relatively few individuals: in all years of our sample, less than 5% of individuals are at the limit for capital pensions. The cap for annuity pensions is in principle the same as for capital pensions, but special provisions typically make this cap softer and essentially non-binding in practice.

Data. We merge data from several Danish administrative registers, which include annual information for the Danish population (approximately 5 million people) for the period 1994-2009. We obtain information on income and wealth from the Income Tax Register. We merge this database with the Danish Integrated Database for Labor Market Research (IDA) to link employees with their employer and obtain educational and occupational information. Finally, we use population register data for demographic information such as age and gender. Again, we focus here on the variables most relevant for our empirical analysis. See Chetty et al. (2011) for more information on the income data and Leth-Petersen (2010) for information on the wealth data.

The income-tax data are collected by the tax authorities using information from several sources. Earnings and pension contributions are reported directly by employers and pension funds to the tax authority.[14] Note that we observe flows into pension accounts but do not have data on balances

pay out over a different number of years. We use the term "annuity pension" for simplicity here to refer to both of these accounts because the difference between these sub-categories does not matter for our empirical analysis.

[13]Because annuity payouts are treated as income, they lead to a clawback of the means-tested DB pension benefit. We estimate that the average clawback of DB pension benefits is less than 5 cents per DKr of annuity pension contributions.

[14]Kleven et al. (2011) conducted a randomized tax audit in collaboration with the Danish tax authorities and found that tax evasion is negligible among wage earners. Their finding suggests that the third-party reported information we use here are of high quality and accurately capture real economic behavior.

within pension accounts. End-of-year assets and liabilities are reported directly by banks and financial institutions. These wealth data are collected because Denmark levied a wealth tax until 1997; data collection continued after that point and the tax authorities use the wealth data to cross check if reported income is consistent with the level of asset accumulation from one year to the next. Our measure of liabilities covers all forms of secured and unsecured debt (such as credit cards) except home mortgages. Because home mortgage debt is marked-to-market in Denmark, its value fluctuates significantly across years and cannot be used to measure repayment of mortgage debt with accuracy.

We define gross taxable saving as the change in an individual's taxable asset holdings.[15] We define net savings as gross savings minus the change in liabilities (excluding home mortgages). Our measures of taxable saving suffers from three limitations. First, because we do not directly observe home equity wealth and mortgage debt, we miss investments in home improvements and payments to home equity. We assess whether this is a significant source of bias by replicating our analysis on the subsample of renters. Second, our definition of savings also does not account for other investments in durables, such as cars or appliances. To test for intertemporal substitution in durable goods purchases, we analyze policy impacts on savings behavior over several years. Third, the wealth data exclude some assets such as cash holdings outside bank accounts and exotic assets such as yachts. Such assets likely account for a small fraction of total wealth and are unlikely to be the main substitutes for savings in retirement accounts. Moreover, because wealth was not directly taxed during the period we study, there was no incentive to hold assets in the form of cash rather than financial assets for wage earners. We therefore believe that our measure of savings is comprehensive for the vast majority of households in our sample.

We analyze income and savings at the individual (rather than household) level because Denmark effectively has an individual tax system and thus the key incentives operate at the individual level. The tax authority divides balances held in joint accounts equally among the account's owners to obtain measures of individual capital income for tax purposes, and we use these individual-specific measures to compute savings in our analysis. To ensure that our results are not biased by resource pooling within couples, we directly test for offset in the partner's account and analyze the subsample of individuals without partners.[16]

[15] Fluctuations in asset values and durable goods purchases generate noise in this measure of savings. This problem is inherent to any study of savings behavior based on wealth data. We account for this problem using research designs that exploit variation that is orthogonal to these sources of measurement error.

[16] Our definition of partners includes cohabitation, which is common in Denmark. The administrative records identify partners as individuals who (1) are married, (2) live together and have one or more child together, or (3) live

Sample Definition and Summary Statistics. Starting from the population dataset, we impose two restrictions to obtain our primary analysis sample. First, we exclude observations in which individuals are below the age of 18 or over 60, at which point early retirement schemes begin. Second, we exclude observations with self-employment income because business wealth and income are not measured precisely for the self-employed. This leaves us with an (unbalanced) panel of approximately 45 million observations for 4.2 million unique individuals. For our analysis of price subsidies, we focus on a subset of observations with income within DKr 75,000 (US $11,500) of the top tax bracket cutoff, which we refer to as the "top tax threshold" sample.

Table 2 presents summary statistics for the full sample and top tax threshold sample. To eliminate outliers, we drop observations with pension contributions above the 99.9th percentile of the full sample. We trim savings variables and all rates (pension contributions or savings as a percentage of income) at the 1st and 99th percentiles. The mean individual labor (non-capital) income in the full population is DKr 199,565 (US $34,000). Note that we always measure labor income prior to pension contributions. Mean net capital income is negative because mortgage interest payments are included in capital income, but imputed rental income from owner-occupied housing is not. Mean non-retirement assets (excluding home equity wealth and pension wealth) is DKr 51,602 (US $9,000). On average, households have DKr 76,539 (US$ 13,500) of non-mortgage liabilities. The top tax threshold sample has higher income and wealth as expected.

Employer contributions to retirement accounts are significantly larger than individual contributions. 58.7% of individuals have an employer contribution to either capital or annuity pensions. 26.8% of individuals make voluntary contributions to retirement accounts themselves, and thus have some capacity to offset changes in employer pensions or the government mandated savings plan. The remaining individuals are at a corner within pension accounts, but typically have significant non-retirement savings with which they could offset automatic contributions.

The asset positions of individuals in our sample are fairly similar to those of individuals in the U.S. on average. The median savings rate (including pension contributions) in our sample is 11%. The median savings rate for households in the U.S. Panel Study of Income Dynamics is approximately 14% (Dynan et al., 2004). 17.6% of individuals own stock in non-retirement accounts and 79.6% held non-mortgage debt in Denmark; the corresponding fractions in the 2001 U.S. Survey of Consumer Finances were 21.3% and 75.1% (Aizcorbe et al., 2003).

together, are of opposite gender, differ in age by less than 15 years, and are not blood relatives.

IV Impacts of Automatic Contributions

The ideal experiment to analyze the impacts of automatic contributions on savings would be to randomize automatic contributions holding fixed total compensation. For example, we would set up automatic pension contributions for a random subset of individuals of say DKr 1,000 and reduce their take-home pay by DKr 1,000 so that total compensation is held fixed. We approximate this ideal experiment using two quasi-experimental research designs: (1) changes in employer provided pensions and (2) the introduction of a mandated government savings plan (MSP). Both of these changes are "automatic contributions" in the sense that they increase individuals' pension savings if they take no action. The employer pension variation provides much more precise estimates because it generates idiosyncratic variation at the individual level, while the MSP is a purer approximation of the ideal policy experiment. We discuss each of these research designs in turn, organizing our analysis around the predictions in Columns 1 and 2 of Table 1.

IV.A Employer Provided Pensions

We obtain quasi-experimental variation in employer pension contributions using event studies of individuals who switch jobs. Because pension benefits vary significantly across firms, job changes often lead to sharp changes in employer pension contributions.[17] To account for the fact that increases in employer pensions also increase total compensation, we conduct analogous event studies of earnings changes due to job switches and compare the impacts of changes in earnings and employer pensions on total saving. Although firm switches are endogenous, the high-frequency variation in employer pensions around job switches is plausibly orthogonal to tastes for savings, which presumably evolves more smoothly over time. We evaluate this identification assumption in detail below after presenting a set of baseline results, e.g. by analyzing individuals who switched firms because of a mass layoff.

Impacts on Pension Contributions. The analysis in this subsection focuses on the subgroup of individuals who switch between firms at some point in our sample. We define an individual as switching firms in year t if he has earnings from two distinct firms in year t and $t-1$.[18] In order

[17] An alternative source of variation is changes in firm pension policies over time. Unfortunately, such changes were very gradual and relatively small on an annual basis during the period we study, making it difficult to disentangle the causal impacts of changes in firms' policies from other confounding factors that trend over time. Arnberg and Barslund (2012) correlate changes in savings rates with changes in employer pensions and, consistent with our results, find little evidence of crowd-out.

[18] The firm identifiers were changed in 2003, 2005, and 2007; we therefore define the firm switch variable as missing for observations in these years. For individuals who hold multiple jobs within a single year, we define a firm switch as having a different "primary job" in the next year. We also confirm that our results hold for the subsample of

to limit the sample to individuals switching between full-time jobs rather than entering or exiting the labor force, we drop observations in which earnings either fell by more than half or more than doubled, which account for approximately 15% of the switches in our sample.[19] This leaves us with 4.57 million job-switches in the data.

Figure 1a illustrates our research design using an event study of individuals who move to a firm that contributes at least 3 percentage points more of income to retirement accounts than their previous firm.[20] Let year 0 denote the year in the sample that an individual switches firms and define all years relative to that year (e.g., if the individual switches firms in 2001, year 1998 is -3 and year 2003 is +2). The series in circles in Figure 1a plots employer contributions (to capital plus annuity accounts) for these individuals. By construction, employer pensions jump in year 0, by an average of 5.58% for individuals in this sample.

How does this jump in employer pensions affect individual pension contributions? Because employer and individual retirement savings accounts are perfect substitutes, the neoclassical model predicts that agents should undo increases in automatic contributions by employers (M) by reducing their own contributions (P). The series in triangles in Figure 1a tests this hypothesis by plotting the sum of employer and individual pension contributions ($M + P$) around the firm switch. Total pension contributions increase by 5.23%, which is 93% of the increase in employer contributions.

An immediate concern with the analysis in Figure 1a is that many individuals are unable to offset the increase in employer pensions because they hit the constraint of making zero pension contributions ($P \geq 0$). 63.9% of individuals in Figure 1a make zero individual pension contributions in year 0. Under the neoclassical model, the impact of a DKr 1 increase in automatic contributions on total pension contributions, which we term the rate of "pass-through," should therefore be at most 63.9 cents.[21] The actual impact of 93% implies that at least some individuals who are able to offset the increase in employer pensions must not do so, rejecting the neoclassical model. To estimate the rate of pass-through for those who are not constrained by corners, we use two methods: conditioning on positive lagged contributions and studying thresholds instead of levels. Both methods yield similar results in all cases.

We implement the first method in Figure 1b. This figure replicates Figure 1a, restricting to

individuals who have only one job in each year.

[19]Our results are insensitive to this restriction provided that we exclude the 1% of individuals who experience earnings changes exceeding 250% or below -80%, which are most likely outliers driven by entry or exit from the labor force.

[20]In this figure, we include only individuals with at least 5 years of data both before and after the switch to hold the sample composition fixed across years.

[21]Pass-through is one minus the rate of offset or crowd-out.

the subset of individuals who make positive individual pension contributions in the year before the firm switch ($P_{-1} > 0$). In this sample, only 11.9% of individuals make zero contributions in year 0 after the firm switch, but the rate of pass-through remains at $4.86/5.65 = 86\%$. This result shows that most individuals do not offset the increase in employer pensions even if they are able to do so.

A second method of accounting for corners is to directly analyze the fraction of individuals who hit the corner when the employer pension is increased. The advantage of this approach is that it completely eliminates any mechanical impact of corners; the drawback is that estimating crowd-out using this approach requires stronger assumptions. We implement this approach in Figure 1c. The series in triangles plots the fraction of individuals making 0 individual pension contributions around the firm switch. With full pass-through, the predicted increase in the fraction at the corner is the fraction of individuals whose individual pension contribution rate at $t = -1$ is less than or equal to the increase in the employer contribution rate at $t = 0$. The observed increase in the fraction making 0 individual contributions is very small relative to the predicted increase, which is shown by the dashed line in Figure 1c. This confirms that very little of the increase in employer pensions is offset by reducing individual contributions.

To estimate crowd-out from this threshold analysis, we assume that the underlying model of response is binary: individuals either offset the employer contribution fully (up to the corner) or do not respond at all. We show below that this binary model is supported by the data. Under this model of response, the degree of pass-through is one minus the ratio of the change in the actual fraction of individuals at the corner (from year -1 to 0) to the predicted change. This yields a pass-through estimate of $1 - (1.4/29.8) = 95.3\%$.

As noted above, the identification assumption underlying our research design is that an individual's preferences for saving would not have jumped sharply in year 0 in the absence of the change in firm policies. This assumption might be violated for two reasons: sorting and omitted variables. Sorting is a problem if individuals switch to firms that provide more generous pension plans at times when they themselves wanted to save more for retirement. One example of an omitted variables problem is that individuals may get paid higher salaries when they switch to firms with more generous pensions. In both cases, the pattern in Figure 1 would be driven by changing tastes rather than the causal effect of employer pensions.

Two pieces of evidence suggest that the identification assumption is likely to be satisfied and hence that employer contributions actually have a causal effect. First, there is no evidence of a trend toward higher individual pension contributions prior to year 0 in Figure 1a, as one would expect if individuals' tastes are changing around the job switch. Second, and more importantly, more than 40% of individuals leave their individual pension contributions literally unchanged at the time of the job switch. This is illustrated in Figure 1d, which plots a histogram of changes in individual contributions from the year before the firm switch to the year of the firm switch for prior contributors (the sample in Figure 1b). It follows that for more than 40% of individuals, total pension contributions change by exactly the same amount as the change in employer contributions. Given switching costs and search frictions, it is unlikely that individuals who want to save say 3.3% more of their income in a given year manage to switch to firms that contribute exactly 3.3% more to retirement savings. Moreover, because individual and employer contributions have identical tax benefits, there is no reason to switch firms to save more; it would be much easier to simply raise one's own contributions to the same retirement accounts. Hence, Figure 1d strongly suggests that the jumps in pension contributions in Figures 1a-1c are driven by the causal effect of employer pensions rather than jumps in other factors. Figure 1d also provides direct evidence that many individuals are passive savers, as they do not update their pension contributions at all even when employer contributions change. This lack of response is consistent with direct survey evidence that knowledge about employer pension plans is quite low (Mitchell, 1988; Gustman and Steinmeier, 1999).

Impacts on Total Saving. Next, we turn to the question posed in Column 2 of Table 1: how do automatic contributions affect total saving, including savings in taxable non-retirement accounts? As discussed above, individuals may reduce savings in non-retirement accounts even if they are passive savers who do not pay attention to pension contributions. The series in squares in Figure 1a plots total gross savings ($M + P + S$).[22] Total savings rates also jump immediately when individuals switch to a firm with higher employer contributions. Figures 1b and 1c show that we obtain similar results when we account for corners. The series in squares in Figure 1b shows total saving for the same sample as the other series, i.e. those making positive lagged pension contributions. Despite the fact that only 11.9% of these individuals hit the corner within pension accounts, total saving rises by 78.5 cents per DKr increase in employer pensions in this sample. To implement the threshold approach, we must define the corner in taxable saving. In a model with credit constraints and no uncertainty, the corner would be zero wealth. However, because early withdrawal penalties make retirement savings illiquid, individuals may seek to maintain a

[22]We exclude changes in debt from our baseline measure of savings to maximize precision, but show below that defining savings net of liabilities yields similar estimates.

buffer stock in non-retirement financial wealth as precautionary savings in an environment with uncertainty (Carroll, 1997). Because there is no exogenously defined wealth constraint in a buffer-stock model, we use 10% of income as a baseline definition of the threshold for liquid wealth.[23] The series in squares in Figure 1c plots the fraction of individuals who are at the corner in both retirement and non-retirement accounts, i.e. those with gross non-retirement financial wealth below 10% of income and zero private pension contributions. With this definition, 38.3% of individuals are at the corner with respect to total savings in the year prior to the firm switch. This fraction changes very little after the switch relative to what would occur if pension contributions and taxable saving were reduced as much as possible to offset the increase in employer pensions.

Figure 2 generalizes the event studies in Figure 1 to include changes in employer pension contributions of varying sizes. Figure 2a is a binned scatter plot of changes in total pension contributions ($M + P$) from year -1 to year 0 vs. changes in employer pension contributions. To construct this figure, we divide the x axis into twenty equal-sized bins (vingtiles) and plot the means of the x and y variable within each bin. We restrict the sample to individuals making positive individual pension contributions in year -1 to reduce the influence of corners as in Figure 1b. The intercept of the best-fit line is approximately 0, confirming that total pension contributions do not change significantly for individuals who switch to firms whose pension plans are similar to their prior firm. The slope of the best-fit line provides an estimate of the degree of pass-through. On average, a DKr 1 increase in employer contributions M raises total pension contributions $M + P$ by 95 cents.We reject the null hypothesis that the pass-through rate is 0 with a t-statistic of approximately 500, with standard errors clustered by destination firm to account for the correlation in employer pensions across workers in the same firm. The 95% confidence interval for the degree of pass-through to pensions is $(94.3, 95.1)$.

Figure 2b replicates Figure 2a for total saving, restricting the sample to those with either positive pension contributions or liquid (non-retirement) wealth in the year before the switch of more than 10% of income. We estimate that 90 cents per DKr of employer pension contributions passes through to total saving and reject the null of zero impact with a t-statistic of 100.

As we described at the beginning of this section, an increase in employer pension contributions could raise savings even for neoclassical optimizers through an income effect by raising an indi-

[23]We choose 10% because Samwick (2003, Table 5b) calibrates a life-cycle model and shows that individuals with high discount rates maintain approximately 10% of income in non-retirement financial wealth as precautionary savings when they have access to tax-deferred retirement accounts. Our results are robust to alternative definitions of the threshold because very few individuals change taxable saving.

vidual's total compensation. To quantify income effects, we analyze the impacts of increases in earnings when individuals switch firms. Figure 2c plots the relationship between changes in savings rates and changes in earnings during firm switches. The x variable in this figure is the percentage change in earnings at the new job relative to the old job ($\frac{E_t}{E_{t-1}} - 1$). To estimate the marginal propensity to save out of earnings changes, we measure savings (the y variable) as a percentage of prior earnings ($\frac{M_t+P_t+S_t}{E_{t-1}}$). We estimate that a DKr 1 increase in earnings increases total saving by 10 cents, far less than the 90 cent increase observed for employer pensions.[24] Increasing compensation in the form of automatic retirement contributions has much larger effects on wealth accumulation than paying people an equivalent amount of money.[25]

Together, our estimates imply that raising employer pensions by DKr 1 while reducing earnings by DKr 1 – the ideal experiment described at the start of this section – would raise total saving by $90 - 10 \cdot \frac{2}{3} = 83$ cents.[26] This implies that the vast majority of individuals reduce consumption when employers automatically deposit a larger fraction of their pay in a retirement account. Conversely, individuals marginal propensity to consume out of disposable income is an order of magnitude larger than their MPC out of pension balances.

Robustness. We assess the robustness of these results by estimating regression models of the impact of changes in employer pensions on savings. We restrict the sample to individuals switching firms and use only data from the year before and the year after the firm switch (years -1 and 0), as in Figure 2. We estimate variants of the following regression specification:

$$\Delta Z_i = \alpha + \beta \Delta M_i + \gamma X_i + \varepsilon_i \tag{4}$$

where ΔZ_i denotes the change in a savings measure (total pension contributions or total saving, measured as a percentage of income) from the year before to the year after the firm switch, ΔM_i denotes the change in the employer pension contribution rate, and X_i denotes a vector of covariates. Standard errors are clustered by destination firm to account for the correlation in employer pensions across employees of the same firm.

We begin in Column 1 of Table 3 by analyzing impacts on total pension contributions. We estimate three variants of this regression. In Panel A, we replicate the specification in Figure 2a

[24] Of this 10 cent increase, 4 cents comes from increased employer contributions. Hence, this calculation overstates individual's true marginal propensity to save, insofar as the increase in employer pensions raises savings primarily because of passive behavior rather than active choice.

[25] Figure 1d also provides strong evidence that income effects are unlikely to drive the observed increases in savings, as a large fraction of individuals would need to have a marginal propensity to save of exactly 1 to explain this pattern.

[26] Recall that two-thirds of the employer pension is paid by the employer and one-third does come directly out of the individual's wage earnings.

(no controls), restricting the sample to those making individual pension contributions prior to the firm switch. In Panel B, we add the following vector of covariates: age, gender, marital status, an indicator for attending college, and two-digit occupation indicators. Not surprisingly, the coefficient is virtually unchanged, as the sharp change in employer pensions at the time of the job switch is essentially orthogonal to these covariates.

In Panel C, we use the full sample of all firm switchers and use a threshold-based approach to account for corners. To implement this approach, we first define a "positive savings" indicator as having individual pension contributions P greater than zero, which is the median individual pension contribution in the firm switcher sample. We then define "predicted positive savings" as an indicator for whether an individual would have positive savings in year 0 if they offset the change in employer pensions one-for-one starting from his year -1 contribution level (up to the corner). Finally, we regress the actual change in the positive savings indicator (from year -1 to 0) on the predicted change, instrumenting for the predicted change with the change in employer pension rates. The resulting 2SLS coefficient is an estimate of pass-through under the same binary response assumption made to calculate crowd-out in Figure 1c. Intuitively, this coefficient is one minus the ratio of the fraction of agents who actually move away from the corner to the fraction who would move away from the corner if they offset the employer pension fully. This is equivalent to the fraction of agents who undo the change in employer pensions in the binary model. The resulting estimate is 97 cents of pass-through to total pensions per DKr 1 of employer contributions.

Column 2 of Table 3 replicates the same triplet of specifications for changes in total savings rates instead of total pension contributions. In Panels A and B, we condition on having liquid wealth of more than 10% of income or having positive individual pension contributions in the year before the firm switch. In Panel C, we use the threshold approach, with the threshold defined as having a total savings rate above 6.4% of income, the median savings rate in the estimation sample. Again, we find high rates of pass-through to total saving in all specifications.

As noted above, one natural concern with our design is that wage rates may also jump when individuals switch to firms with more generous pensions. Column 3 replicates the specifications in Column 2 while controlling for the change in wage earnings that the individual experiences during the firm switch. Including this control has little impact on the estimated pass-through of employer pensions. The reason is that the variation in employer pensions is essentially orthogonal to changes in earnings: the correlation between changes in employer pensions and earnings for individuals who switch firms is 0.03.

In Column 4, we address potential concerns about endogenous sorting by limiting the sample to individuals who left their old firm in a mass layoff, which we define as more than 90% of workers leaving a firm that had at least 50 employees.[27] By this measure, 1.75% of the firm-switches occur because of mass layoffs. In this sample, we estimate pass-through of employer contribution changes to total saving of 86.5%, similar to the estimate in the full sample. Since those who lost their jobs in a mass layoff are unlikely to be switching firms purely because of their pension plans, this result supports the validity of our research design.

Lastly, we assess the robustness of our findings to alternative measures of total saving to address potential concerns with our baseline savings measure. First, our baseline measure of savings does not account for changes in liabilities. Column 1 of Appendix Table 1A replicates the baseline specification in Column 2 of Table 3B with savings net of liabilities as the dependent variable. The pass-through estimate is virtually unchanged, showing that changes in employer pensions do not have significant effects on debt. Second, as noted above, we do not measure mortgage debt. In Column 2 of Appendix Table 1, we replicate the baseline specification for the subsample of renters. Pass-through rates again remain similar, indicating that changes in the rate of home mortgage repayment do not drive our findings. Third, our baseline measure of savings is defined at the individual level; if individuals respond to employer pensions by changing savings in their partner's account, we would understate crowd-out. In Column 3, we define total saving at the household level (summing individual savings for partners). In Column 4, we replicate the baseline specification but restrict the sample to individuals who do not have a partner. In both columns, the pass-through estimates remain close to 0.9, allaying the concern that resource pooling in couples leads us to understate crowd-out.

Long-Term Impacts. One may be concerned that small changes in employer pensions are passed through to total saving, but larger changes draw individuals' attention and lead to larger changes in consumption (Cochrane, 1991; Chetty, 2012; Browning and Crossley, 2001). A related hypothesis is that individuals might react to the change in employer pensions over time, so that long-term impacts are much smaller than short-run changes. We now investigate these two issues directly.

First, Figures 2a and 2b show that the relationship between total saving and employer pensions is approximately linear. Large changes (e.g. +/-5% of earnings) continue to have significant impacts on savings behavior. We confirm this result in Column 5 of Table 3, by replicating the specification

[27] To ensure that such mass layoffs are not simply a relabeling of the firm ID, e.g. due to a change in ownership, we also restrict to firm closures in which no more than 50% of workers from the old firm end up at the same new firm in the next year.

in Column 2, restricting the sample to changes in employer pensions of more than 5% in magnitude. The pass-through coefficient to total saving remains at 90% for these large changes.

Second, we investigate the persistence of the increases in savings. In Figure 3a, we replicate the regression specification in Column 2 of Table 3A at various horizons. For computational simplicity, we only include the first firm switch for each individual in the sample. Each point in this figure is the regression coefficient β_t from a regression of the form in (4), where ΔM_i is measured as the change in employer pensions from the year -1 before the switch to year t. The first point, $\beta_0 = 0.832$, corresponds to the one-year pass-through estimate shown in Column 6 of Table 3A. The remaining points show that there is no discernible trend in pass-through over the subsequent 10 years.

Figure 3b shows the consequence of this persistent change in savings behavior on wealth balances in retirement. This figure restricts attention to the subset of individuals whose first firm-switch occurs between ages 46-54 and who reach age 60 within our sample frame. We define total wealth accrued from the date of the switch up to age 60, the most common retirement age in Denmark, as the cumulative sum of savings in retirement and non-retirement accounts. Figure 3b plots total accrued wealth vs. the change in employer pensions at the time of the switch. Individuals who happened to switch to firms that had employer pension contribution rates that were 5 percent higher end up having additional wealth equivalent to more than 25% of income when they reach the retirement age.[28] Column 7 of Table 3 replicates this specification and shows that it is robust to controlling for the standard vector of covariates. This is perhaps the most direct evidence that automatic employer contributions raise total saving in the long run: they substantially increase the amount of wealth with which individuals enter retirement.

IV.B Government Mandatory Savings Plan

We complement our analysis of employer pensions by studying a government policy that directly implemented automatic pension contributions by reducing individuals' earnings.[29] In 1998, the Danish government introduced a Mandatory Savings Plan (MSP) with the goal of reducing con-

[28] Although the average individual is age 51 at the point of the firm switch, the increase in retirement balances is smaller than what one would predict based on the 90% pass-through rates in Figure 3a: $(60-51)\cdot 5\% \cdot 90 = 41\%$. This is because not all individuals stay at the same firm after the initial switch, and thus the actual increase in employer contribution rates shrinks on average over time.

[29] Changes in government policy could in principle have different impacts than variation in employer pensions for several reasons. For instance, individuals might update consumption habits and commitments primarily when they switch jobs. This would lead to greater crowd-out of automatic contributions for the average person in the population than for the job switchers analyzed above.

sumption to lower the risk of an "overheating" economy (Green-Pedersen, 2007).[30] The MSP took 1% of individuals' pre-tax earnings and automatically allocated it to a separate retirement savings account managed by an independent pension fund. Individuals with incomes below DKr 34,500 (US $6,000) were exempted from the program in 1998; starting in 1999, all individuals were included in the program. The MSP was terminated at the end of 2003.[31] The MSP accounts were distinct from other retirement accounts, but functioned like individual capital pension accounts when they were set up. Individuals received annual notifications of the balances in their MSP accounts, as they did for other retirement pension accounts; see Appendix Figure 1 for an example.

The variation in the MSP generates two quasi-experimental designs. We begin with a regression-discontinuity design around the DKr 34,500 eligibility cutoff. This approach yields precise estimates, but estimates a local average treatment effect for very low income individuals. We analyze responses at higher income levels using a difference-in-differences design that compares changes in savings around the introduction and termination of the MSP for individuals with different income levels.

Regression Discontinuity Design. Figure 4a illustrates the regression discontinuity design by plotting MSP contributions vs. income in 1998 in DKr 1,000 income bins. Individuals who earn just below DKr 34,500 make no contribution to the MSP; individuals just above DKr 34,500 are forced to make a contribution of DKr 345. The size of the contribution then increases linearly (with a slope of 1%) with income.

Figure 4b plots a histogram of the income distribution around the eligibility cutoff. There is no evidence of manipulation of income around the cutoff. This is not surprising given prior evidence that individuals are typically unable to sharply manipulate their earnings to take advantage of even much larger tax incentives (Chetty et al., 2011, 2012). Following standard practice, we also test whether several observable characteristics – such as age, gender, etc. – are smooth around the cutoff and find no evidence of discontinuities in these variables (not reported). These tests support the identification assumption underlying the RD design.

We estimate the impacts of the MSP on savings by restricting the sample to 1998 and estimating OLS regressions of the following form:

$$\Delta Z_i = \alpha + \beta \cdot 345 \cdot [y_i > 34,500] + f(y_i) + \gamma X_i + \varepsilon_i \tag{5}$$

[30] The government's intention of reducing consumption is consistent with our empirical findings and suggests that policymakers implicitly viewed most individuals as passive savings-targeters.

[31] The MSP was originally announced as a provisional plan for 1998, but was made permanent the following year. In 1998, 2002, and 2003, each individual's full contribution was deposited directly into his own account. Between 1999 and 2001, every full time employee got the same amount deposited in his account irrespective of his own contribution, in order to increase redistribution.

where ΔZ_i denotes the change in total pension contributions or savings, y_i denotes individual i's income, $f(y_i)$ is a linear function interacted with the indicator for being above the eligibility cutoff, and the other variables are defined as in (4). In this equation, β is an estimate of pass-through that is identified from the discontinuous jump in MSP contributions of DKr 345 at the cutoff. We cluster standard errors by DKr 1,000 income bins to account for specification error (Card and Lee, 2008).

Column 1 of Table 4 estimates this specification with total pension contributions as the dependent variable. As in Table 3, Panels A conditions on having positive lagged total pension contributions to avoid the impact of corners. We estimate pass-through of mandated savings to total pensions of 94.6% among prior contributors, very similar to the estimates obtained from the variation in employer pensions. Panel B shows that including the standard controls does not change this estimate significantly. In Panel C, we implement a threshold approach by using the change in an indicator for contributing more than 1.5% of income to individual pensions as the dependent variable in (4). We divide the estimated impact by the predicted impact based on the density of individuals with pension contributions between 0.5% and 1.5% of income to estimate pass-through. We obtain a pass-through estimate of 86.2% using this approach.

In Column 2, we analyze the impacts of the MSP on mean individual savings (excluding employer pension contributions). We exclude employer contributions from this figure because individuals may not have the ability to change their employer contributions in response to the MSP. Unfortunately, because there are relatively few individuals with incomes around the cutoff, mean savings are extremely volatile around the cutoff, as shown in Appendix Figure 2a. As a result, we obtain very imprecise estimates of the impacts of the MSP on total saving: the standard error on the pass-through estimate exceeds 14 in Panels A and B of Column 2.

To increase precision, we turn to the threshold approach and study the fraction of individuals saving more than the mean level of individual savings for those within DKr 5,000 of the eligibility cutoff in 1998, which is DKr 1,962. This indicator variable is more stable than mean savings, which are heavily influenced by outliers. Figure 4c plots the fraction of individuals with above-average savings around the MSP eligibility cutoff. Again, we exclude employer pensions to ensure that individuals are not forced over the threshold by employer contributions outside their control. The fraction of individuals with above-average savings rises from 47% to 50% for individuals who are forced to participate in the MSP. Note that because the MSP contribution was only 1% of income – which is DKr 345 at the cutoff – this result cannot be driven by corners, as any individual saving more than DKr 1,962 would have been able to fully offset the increase in the MSP by reducing other savings. To translate this impact into a measure of the degree of crowd-out, we mechanically add 1% of income to observed savings on the left side of the discontinuity to estimate what the level of savings would be if the MSP were passed through 1-for-1 into total saving. We then re-estimate the linear control function and calculate the size of the jump that would be predicted at the cutoff with no offset. The observed increase is very similar in magnitude to the predicted increase with 1-for-1 pass-through. As a result, we obtain a point estimate of pass-through that is not significantly different from 1, as shown in Panel C of Column 2. Column 3 of Table 4 replicates the analysis in Column 2 using total saving (including employer pensions) and shows that we obtain similar estimates when employer pensions are taken into account.

We assess the robustness of these results to alternative measures of total saving in Panel B of Appendix Table 1. We replicate the specification in Column 3 of Table 4C using the same four variants described above in the analysis of employer pensions. Across all the specifications, we find estimates of pass-through that are significantly above 0 and not statistically distinguishable from 100%. We conclude that few if any individuals offset the MSP by reducing savings in other accounts.

Difference-in-Differences Design. To test if the impacts of the MSP are similar for higher income individuals, we turn to a DD design. To illustrate the design, we divide the population into three terciles based on their current individual income, as defined for calculating the MSP contribution. Figure 5a plots the levels of MSP contributions from 1995 to 2005 for these three groups. Individuals in the top tercile (incomes above DKr 273,000) were forced to contribute approximately DKr 3,760 on average between 1998 and 2003 to the MSP. Individuals in the middle tercile were forced to contribute DKr 2,250 on average, while individuals in the bottom tercile were forced to contribute only DKr 825 on average.

Figure 5b plots individual retirement savings – including contributions to the MSP and individual pension contributions – for the same three income terciles. We again exclude employer contributions, as in the RD analysis. The introduction and termination of the MSP have sharp effects on total contributions to retirement accounts that correspond to the magnitudes of the changes in the MSP.[32] To quantify pass-through to total retirement savings, we first divide the

[32] From 1999-2001, the MSP had a redistributive element, so that MSP balances were fixed even though contribution amounts still varied with income as shown in Figure 4a. The fact that the series in Figure 4b show no breaks around 1999 and 2001 implies that individuals' pension contributions are unaffected by MSP balances even though they should change in a neoclassical model. This is not surprising given that individuals do not appear to pay attention to even the changes in the level of contributions in 1998 and 2004.

sample into cells of DKr 25,000 income groups for each year and calculate mean mandated ($\bar{M}_{gt}$) and total individual pension contributions ($\bar{Z}_{gt}$) in each group. We then estimate the following regression specification, weighting by the number of observations in each cell:

$$\Delta\bar{Z}_{gt} = \alpha_t + \beta \cdot \Delta\bar{M}_{gt} + \varepsilon_{gt} \tag{6}$$

where $\Delta\bar{Z}_{gt}$ denotes the change in mean total individual contributions from year $t-1$ to year t in each cell, $\Delta\bar{M}_{gt}$ is defined analogously, and α_t is a year fixed effect. We limit the sample to $t = 1998$ and $t = 2004$, the years in which the MSP was introduced and terminated.[33] We obtain a pass-through estimate of $\beta = 91.1\%$, as shown in Figure 5b.

Figure 5c uses a threshold approach to confirm that these increases in pension contributions are not driven by individuals who make zero individual contributions and are unable to offset the MSP. It plots the fraction of individuals whose total individual (non-employer) pension contributions exceed 1.5% of income, the mean individual pension saving rate in the sample. Because the MSP was only 1% of income, any changes in this indicator must be driven by individuals who are not at the corner. The MSP again clearly increased the fraction of individuals saving more than 1.5% of their income in pension accounts between 1998 and 2003. To estimate pass-through, we repeat the regression in (6) with the dependent variable defined as change in the fraction of individuals whose total individual pension contributions exceed 1.5% of income. To calculate pass-through, we divide this coefficient by the change one would have obtained by mechanically adding the changes in the MSP to prior-year individual pension contributions. The resulting estimate, shown in Figure 5c, is almost exactly 100%.

Finally, we examine total saving, including savings in taxable accounts. Unfortunately, when we replicate the analysis in Figures 5b and 5c for total saving, we obtain very noisy and unstable results, as shown in Appendix Figures 2b and 2c. The fundamental source of the problem is large differential shocks to savings by income group. These differential shocks arise because we measure savings in non-retirement accounts as the difference in wealth across years. Because higher income individuals invest more in stocks and have different patterns of durable good purchases, their wealth trajectories are not well correlated with those of lower income individuals. Visually, it is clear that we would be unable to distinguish differential changes of DKr 2,900 – the difference in the MSP

[33] This synthetic cohort approach isolates variation in MSP due to the law changes in 1998 and 2004; changes in MSP at the individual level confound variation driven by changes in income and changes in the law. An alternative approach is to instrument for the changes at the individual level by simulated changes in MSP due to the law. We find that this approach yields much less stable estimates because the results are sensitive to the control function used to capture mean reversion at the individual level, a well known problem in the literature on estimating taxable income elasticities (Saez, Slemrod and Giertz, 2012).

treatment between the top and bottom terciles – from the year-to-year income-specific shocks to savings in Appendix Figure 2b. This is a generic problem with estimating impacts on taxable saving using comparisons across individuals in different income groups; the same problem arises in our analysis of price subsidies in Section V below. Increasing precision requires an estimator that compares individuals with similar income levels, which is why the RD design yields more precise estimates of impacts on total saving.

The smallest estimate of pass-through across the specifications is 83.2% in Table 3 and 86.2% in Table 4. Our analysis of automatic contributions therefore implies that at most $\alpha = 15\%$ of individuals are active savers in our two-type model.

V Price Subsidies

We now turn to the impacts of price subsidies on savings. As described in Section III, there are two types of retirement savings accounts in Denmark: capital pensions and annuity pensions. Capital pensions are paid out as a lump sum, while annuity pensions are paid out as annuities. Prior to 1999, both annuity and capital pension contributions were fully deductible from taxable income. The marginal tax rate on income was approximately 59% for those in the top income tax bracket and 45% for those in the bracket below the top tax cutoff. The top tax cutoff was DKr 251,200 (US $38,600) in 1998, roughly the 80th percentile of the income distribution. Starting in 1999, the deduction for capital pensions was reduced from 59 cents per DKr to 45 cents per DKR for individuals in the top income tax bracket.[34] The deduction was left unchanged for those in lower tax brackets. The tax treatment of annuity pension contributions was also unchanged.[35]

We divide our analysis of price subsidies into two parts. First, we analyze the impacts of the 1999 reform on contributions to capital pension accounts to quantify the amount of active response to the change in price subsidies. Second, we investigate crowd-out: how much of the change in capital pension contributions was offset by changes in contributions to other pension accounts and savings in non-retirement accounts?

[34] Starting in 1999, individuals in the top income tax bracket faced a marginal income tax rate of 59 cents but had to pay 13.6 cents of tax per DKr contributed to capital pensions.

[35] Capital pensions were a more tax advantaged savings vehicle for most individuals prior to 1999 because individuals paid lower taxes on payouts from capital pensions; after 1999, the two accounts became roughly equivalent in tax treatment. The reform did not change the tax treatment of payouts or capital gains within either account and thus had no effect on the value of existing balances.

V.A Effect of Subsidies on Capital Pension Contributions

In this section, we assess the predictions in Column 3 of Table 1: how do changes in price subsidies affect contributions to the subsidized retirement account? Figure 6a illustrates the impact of the 1999 top-bracket subsidy reduction by plotting mean capital pension contributions for workers whose incomes place them within DKr 75,000 of the top income tax cutoff. To construct this figure, we first group individuals into DKr 5,000 income bins based on their current income relative to the top tax cutoff, demarcated by the dashed vertical line. We then plot the mean capital pension contribution in each bin in each year from 1996 to 2001. The relationship between income and capital pension contributions is stable from 1996 to 1998, the years before the reform. In 1999, capital pension contributions fall sharply for individuals above the top tax cutoff, who face a reduced subsidy.

Figure 6a shows the impact of the reform on total capital pension contributions, including both employer and individual contributions. The subsidy reduction induced many employers to switch their pension plans from capital to annuity pensions. Because our primary goal is to characterize individuals' savings behavior, we focus exclusively on individual pension contributions for the remainder of the paper.[36]

We quantify the impacts of the capital pension subsidy reduction on individual pension contributions using two estimators: a difference-in-differences (DD) design and a regression-kink (RK) design. The DD estimator is simpler and facilitates analysis of heterogeneity, but the RK estimator yields much more precise estimates of crowd-out as shown below. Figure 6b illustrates the DD estimator. For each year between 1995 and 2005, we plot mean individual capital pension contributions for two groups: those with current incomes between DKr 25,000 to 75,000 *below* the top tax bracket cutoff and those with incomes between DKr 25,000 to 75,000 *above* the top tax bracket cutoff. The first group constitutes a "control group" in that their incentives to contribute to capital pensions remained unchanged around the 1999 reform. The second is the "treatment" group, whose incentives to contribute to capital pensions fell sharply in 1999.[37] Capital pension

[36]As one might expect, we find much less evidence of passive behavior among employers: most employers change pension contributions in response to the reform. Subsidies targeted to employer-provided pensions could have larger impacts on total saving because larger employer contributions would raise total saving for passive savers as demonstrated above.

[37]Note that the set of individuals in these two groups varies across years due to income fluctuations. We exclude individuals whose incomes are within DKr 25,000 of the top bracket from both the control and treatment groups because these individuals are partially treated by the reform, insofar as they may have faced some uncertainty when making pension contributions about which bracket they would end up in at the end of the year. Our results are robust to varying this DKr 25,000 exclusion threshold, as can be seen from Figure 6a.

contributions fall sharply for the treated group relative to the control group in 1999. Contributions by treated individuals then continue to decline steadily relative to the control.

We quantify the DD estimates by estimating regressions of the form:

$$P_{it} = \alpha + \beta_1 \text{post}_{it} + \beta_2 \text{treat}_{it} + \beta_3 \text{post}_{it} \cdot \text{treat}_{it} + \gamma X_{it} + \varepsilon_{it} \quad (7)$$

where P_{it} denotes capital pension contributions, post_{it} denotes an indicator for the years after 1999, and treat_{it} is an indicator for having income in the treatment group as defined in Figure 6a. The other variables are defined as in (4). We restrict the sample to the years 1998 and 1999 to measure the immediate impact of the reform. Column 1 of Table 5 implements this regression without controls for capital pension contributions. We estimate that the reduction of the capital pension subsidy reduced capital pension contributions by $\beta_3 = 2,439$ relative to a pre-reform mean of DKr 5,174 for individuals with incomes DKr 25,000-75,000 above the top tax cutoff. This 47% reduction is significantly different from 0 with $p < 0.001$. Column 2 of Table 5 shows that adding the standard vector of controls to this specification does not change the estimate.

Our second estimator is a regression-kink (RK) design that exploits the sharp change in the subsidy at the top tax cutoff. This estimator is illustrated in Figure 6c. To construct this figure, we divide individuals into DKr 5,000 income bins around the top tax cutoff and plot mean individual capital pension contributions in each bin in the three years after the reform (1999-2001) minus the three years before the reform (1996-1998). The marginal propensity to contribute to capital pensions falls sharply at the top tax cutoff when the subsidy is reduced in 1999.[38] We quantify the magnitude of the slope change at the cutoff by fully interacting the specification in (7) with income relative to the top bracket cutoff, which we denote by y_{it}. In particular, we estimate OLS regressions of the following form, including all individuals with income within DKr 75,000 of the top tax cutoff:

$$\begin{aligned} P_{it} = {} & \alpha + \beta_1 \text{post}_{it} + \beta_2 [y_{it} > 0] + \beta_3 \text{post}_{it} \cdot [y_{it} > 0] + \alpha^s y_{it} + \\ & \beta_1^s \text{post}_{it} \cdot y_{it} + \beta_2^s [y_{it} > 0] \cdot y_{it} + \beta_3^s \text{post}_{it} \cdot [y_{it} > 0] \cdot y_{it} + \gamma X_{it} + \varepsilon_{it}. \quad (8) \end{aligned}$$

[38] The slope change actually appears to begin slightly to the left of the top tax cutoff, a pattern that we observe for all savings measures below. This is likely explained by uncertainty about year-end income. Since individuals cannot forecast their incomes perfectly, some who make capital pension contributions while expecting to be in the top bracket end up with incomes slightly below the cutoff at the end of the year. The fuzziness induced by this uncertainty is problematic for non-parametric RK estimators that identify the change in slope purely at the kink. To address this problem, we estimate the change in slope over a broader window using linear control functions on the left and right of the cutoff. Our research design is therefore not a non-parametric RK as defined by Card, Lee and Pei (2009), but rather a parametric estimate of the change in slopes around the top tax cutoff; we use the RK terminology here for expositional convenience.

In this equation, β_3^s is the change in the marginal propensity to save (MPS) in capital pensions dP_{it}/dy_{it} for individuals in the top bracket relative to those below the top bracket when the capital pension subsidy is removed in 1999. We implement this specification in Column 3 of Table 5. The coefficient of $\beta_3 = -0.021$ implies that a DKr 1,000 increase in income led to DKr 21 of additional saving in capital pensions when the additional 13.6 cent subsidy was in place prior to 1998. Again, adding controls does not affect this estimate (Column 4 of Table 5).

The aggregate reduction in individual capital pension contributions masks substantial heterogeneity in responses across households. Figure 7a plots the distribution of changes to individual capital pension contributions (as a fraction of lagged contributions) for those in the treatment group in Figure 6b who were contributing to capital pensions in the prior year.[39] We plot the distribution of changes in contributions from 1998 to 1999, the year of the treatment, as well as from 1997 to 1998 as a counterfactual. Figure 7b replicates Figure 7a for the control group defined in Figure 6b. The distributions of changes are virtually identical in 1998 and 1999 for individuals who were unaffected by the 1999 tax reform, supporting the view that the difference between the distributions in Figure 7a is caused by the change in subsidy.

Figure 7a yields two lessons about the heterogeneity of response. First, many people do not change their capital pension contributions even when the subsidy is reduced. As discussed in Section II, every active saver should cut capital pension contributions by some non-zero amount at an interior optimum. Hence, it is evident that many individuals are passive savers. Second, the reform induces a substantial fraction of individuals to exit capital pensions completely, i.e. reduce contributions by 100%. We now estimate the fraction of individuals who actively respond to the subsidy (α) from these two patterns.[40]

We begin by estimating the impact of the subsidy on the fraction of individuals who leave their capital pension contributions unchanged, i.e. set $P_t = P_{t-1}$. To do so, we use the DD estimating equation in (7) with the dependent variable as an indicator for having $P_t = P_{t-1}$. We restrict the sample to individuals contributing to capital pensions in the prior year and only include data from 1998 and 1999, the years shown in Figure 7. Column 5 of Table 5 shows that the fraction of individuals who leave their capital pension contributions unchanged falls by 3.0 percentage points

[39] We do not include employer capital pension contributions in this figure because our goal is to measure the fraction of households that actively reoptimize in response to the change in the subsidy.

[40] Directly identifying which individuals actively respond to the change in price subsidy requires a counterfactual for how each individual would have changed his pension contributions absent the subsidy change in 1999. Unfortunately, we lack such a counterfactual; in particular, behavior in other years is not a good counterfactual because many individuals change pensions even when their incentives do not change for other idiosyncratic reasons, as shown in Figure 7. We therefore identify α by comparing the distribution of changes in pension contributions across years.

when the subsidy is reduced in 1999. In 1998, 29.3% of individuals in the treatment group did not change their capital pensions at all.[41] It follows that an additional 3.0/29.3 = 10.2% respond actively to the subsidy change among those who would not have changed their pensions at all absent the reform.

The 10.2% figure is an estimate of α among individuals who do not actively change their pensions for non-tax reasons. One might expect that the fraction who respond to the subsidy will be larger among those who reoptimize their portfolios for other reasons, a conjecture that we confirm empirically in Section VI below. To estimate the mean value of α in the full sample, we must measure the rate of active response among the average individual relative to those who did not change their pension contributions in 1998. To do so, we first identify a set of individuals who are almost certainly responding to the reform: those who exit capital pensions *and* raise annuity pension contributions (the closest substitute) at the same time. Only 1% of individuals change pensions in this way in the control group in 1999 and the treatment group in 1998. In contrast, in the treatment group after the reform in 1999, 13% of individuals exit capital pensions and raise annuities at the same time. Hence, this measure of "extensive margin substitution" identifies active responders with a very low Type-I (false positive) error rate. However, this measure may have a large Type-II error rate, as individuals can respond without exiting capital pensions entirely or raising annuities.

We exploit the low Type-I error rate in this proxy to identify the relative rate of response in the full sample compared with those who do not adjust their pensions in prior years. Column 6 of Table 5 implements the same DD specification as in Column 5 and shows that the subsidy change increased the rate of extensive margin (capital to annuity) substitution by 11.6 percentage points on average among treated individuals. Column 7 of Table 5 replicates this specification for the subset of individuals who did not change their pensions in the prior year ($P_t = P_{t-1}$). As predicted, the degree of active response is smaller in this subgroup: 6.8% of prior non-responders exit capital pensions and raise annuities in 1999. Finally, making the assumption that the rate of extensive margin substitution response – which we can easily detect – is proportional to the overall latent

[41] This figure is lower than the 55.2% at 0 in the histogram in Figure 7a because the 0 bin in the histogram includes those with changes between 0% and 5%. There are mass points of individuals with small positive increases – for instance, 9% of individuals in the treatment group have an increase of exactly DKr 900 in 1998. These small increases may be driven automatic inflation adjustments in pension contributions and could reflect passive behavior. Nevertheless, to be conservative, we use a strict definition of having a pension change of exactly DKr 0 to calculate the fraction of active savers. If we repeat the calculations below with a broader definition that includes all those with changes between 0 to 5% as "non-responders," we obtain an estimate of $\alpha = 23.1\%$ in the full population, very similar to the estimate of 17.4% obtained below.

rate of active response to the subsidy, we can estimate the mean α as $10.2 \times \frac{11.6}{6.8}$. That is, the aggregate reduction in individual capital pension contributions from 1998 to 1999 shown in Figure 6b is accounted for by just 17.4% of individuals who actively reoptimize in response to the subsidy reduction.

The vast majority of these 17.4% of individuals respond by completely exiting capital pensions. Using the standard DD specification, in Column 8 of Table 5 we estimate that 16.1% of individuals exit capital pensions because of the reform. Hence, only a small portion of the response occurs on the intensive margin: most individuals either recognize the subsidy change and stop contributing to capital pensions entirely or do nothing at all.[42]

One natural question is whether the fraction of individuals who respond to the subsidy rises over time, which would imply that the on-impact estimate of α above is biased downward. We study the dynamics of response at the individual level in Figure 7c, which plots the fraction of individuals contributing to capital pensions by year for those who were contributing in 1998, the year before the reform. To construct this figure, we first compute the difference in the fraction of individuals contributing to capital pensions in the treatment (above top tax cutoff) and control (below top tax cutoff) groups in order to remove secular trends due to mean reversion when selecting on contribution in 1998. We then plot this difference (plus 1) to show the causal impact of the reform over time. As with the response to changes in employer pensions in Figure 3a, there is no evidence of gradual adjustment over time: 16% of individuals exit immediately but the rest do not respond at all, even 10 years later. This may appear to be inconsistent with the decline in aggregate capital pension contributions after 1999 in the treatment group in Figure 6b. We show in Section VI that new entrants who begin making pension contributions after 1999 are more responsive to incentives when they make their initial allocations, explaining why capital pension contributions decline after 1999.

V.B Crowd-out in Retirement and Taxable Savings Accounts

We now turn to the question posed in the last column of Table 1. When active savers reduce capital pension contributions following the 1999 reform, what do they do with this money? We estimate two crowd-out parameters, each of which is relevant for different policy questions: the degree of shifting between different types of retirement accounts and the degree of shifting from retirement accounts to taxable accounts. Again, we restrict attention to the impacts of changes in individual

[42] The response to price subsidies may be concentrated on the extensive margin because gains from reoptimization are second-order (i.e., small) on the intensive margin but first-order (large) on the extensive margin (Chetty, 2012).

pension contributions rather than employer contributions.[43]

Crowd-out Within Pension Accounts. We first estimate the extent to which individuals shift assets from capital pensions to annuity pensions when the subsidy for capital pensions was reduced in 1999. This parameter is relevant for assessing the impacts of changes in the tax treatment of one type of retirement account – such as increasing 401(k) subsidies – while leaving the treatment of other retirement accounts (such as IRA's) unchanged.

We begin by using the DD estimator to identify the impacts of the 1999 reform on contributions to annuity pension accounts. Figure 8a plots the time series of individual annuity contributions for the treatment (income DKr 25K-75K above the top tax cutoff) and control (DKr 25-75K below the cutoff) groups as in Figure 6b. The pattern is the mirror image of that in Figure 6b. In 1999, there is a sharp relative *increase* in annuity pension contributions for the treated group.

Figure 8b plots total individual voluntary pension contributions (capital plus annuity) for these two groups. Total pension contributions fall sharply in the treated group relative to the control group in 1999. However, the change is considerably smaller than the relative reduction in capital pension contributions, shown by the dashed black line. This implies that there is considerable crowd-out within pension accounts.

To quantify the degree of crowd-out, we estimate IV regressions that use the DD equation in (7) as a first stage. The second stage is specified as:

$$Z_{it} = \alpha + \mu_1 \text{post}_{it} + \mu_2 \text{treat}_{it} + \mu_3 \text{cappen}_{it} + \gamma X_{it} + \varepsilon_{it} \tag{9}$$

where cappen_{it} is individual i's contribution to the capital pension in year t. We instrument for cappen_{it} using the interaction $\text{post}_{it} \cdot \text{treat}_{it}$ to isolate changes in capital pension contributions that are induced by the subsidy change. The coefficient μ_3 is the crowd-out parameter of interest. We cluster standard errors by year and DKr 10,000 income bin to allow for correlated errors by income group over time.

Panel A of Table 6 presents 2SLS estimates of (9). In Column 1 of Table 6, we use individual annuity pension contributions as the dependent variable and obtain an estimate of $\mu_3 = -0.56$. That is, individuals shift 56 cents of each DKr they would have contributed to capital pension accounts to annuity pensions instead. In Column 2 of Table 6, we use total pensions as the

[43]We are able to ignore employer contributions when estimating crowd-out of individual pensions because the reduction in employer capital pension contributions is fully offset by increases in employer annuity pension contributions, leaving total employer contributions unchanged. We also exclude the mandated pension contribution from 1998-2003 from the definition of total individual pensions in this section in order to isolate the impacts of the subsidy on voluntary pension contributions.

dependent variable. This specification confirms that pass-through to total pensions is 44 cents per DKr of capital pension contributions. Column 3 shows that the inclusion of the standard vector of controls does not change this estimate significantly.

Next, we turn to the regression kink design. Figure 8c shows the impact of the 1999 reform on total pension contributions using the RK design. We construct this figure following the method used in Figure 6c, plotting the change in total pension contributions from the three years before the reform (1996-1998) to the three years after the reform (1999-2001). Total pension contributions fall sharply at the top tax cutoff in 1999 once the capital pension subsidy is reduced. To quantify the degree of pass-through to total pensions, we use a two-stage least squares specification corresponding to the first-stage in (8). In particular, the second stage equation replaces the triple interaction $post_{it} \cdot [y_{it} > 0] \cdot y_{it}$ in (8) with cappen_{it}, and we instrument for the capital pension contributions with the triple interaction. The resulting 2SLS estimate of pass-through equals the reduced-form change in slope at the cutoff in total pensions in Figure 8c divided by the change in slope in capital pensions in Figure 6c. Panel B of Table 6 reports RK estimates for the same dependent variables used in Panel A. We estimate that each DKr 1 reduction in capital pension contributions leads to a 48 cent reduction in total pension contributions, similar to the DD estimate.

Crowd-out of Taxable Saving. Next, we analyze whether the changes in pension contributions documented above are offset by changes in saving in taxable (non-retirement) accounts, which has been the focus of the prior literature on crowd-out (e.g., Engen, Gale and Scholz, 1996; Poterba, Venti and Wise, 1996). The degree of shifting between pensions and taxable saving (rather than *within* pension accounts) is of interest because it determines how subsidies that apply to all tax-deferred accounts affect total wealth accumulation. To estimate crowd-out of taxable saving, we ask whether the 48 cent reduction in total pensions identified above went into additional consumption or led to greater savings in taxable accounts. That is, we use the change in the capital pension subsidy as an instrument for *total* pension contributions to analyze crowd-out in non-retirement accounts.

The literature has used two definitions of "crowd-out" in taxable accounts, which differ in the way they account for the tax subsidy for retirement savings. One definition, used e.g. by Poterba, Venti and Wise (1996), is $\rho_1 = \frac{\Delta S}{\Delta P}$, the fraction of retirement account balances that come from reduced taxable saving. This definition includes the subsidy from the government to the individual in the denominator and is bounded in magnitude between 0 and 1-MTR if individuals do not offset \$1 of post-tax pension contributions by more than \$1. The second definition, used e.g. by Engen, Gale and Scholz (1996), is $\rho_2 = \frac{\Delta S}{\Delta P \times (1-MTR)}$, the fraction of retirement account balances net of the government subsidy that come from taxable saving. The first definition is the relevant concept for determining what fraction of retirement balances are "new" savings from the *individual's* perspective. The latter definition is the relevant concept for determining the increase in total national savings, as the subsidy itself is a transfer from the government to individuals that does not affect total national savings. We report estimates using the latter definition (ρ_2) here; given the MTR of 60% in the top bracket, one can calculate ρ_1 by multiplying these estimates by 0.4.

Once again, we begin with the difference-in-differences estimator, comparing the evolution of taxable saving over time for the treatment and control groups. Unfortunately, as in our analysis of the mandated savings plan in Section IV.B, DD estimates of changes in taxable saving are very imprecise and unstable. The reason is again the large differential fluctuations across the income groups over time in taxable saving. The DD estimates above imply that the 1999 reform reduced capital pension contributions by DKr 2439, and 44% of this reduction was passed through to total pensions. Given a marginal tax rate of 60%, if this reduction in pension contributions were directly deposited into taxable accounts, one would observe an increase in taxable saving of DKr 435. It is apparent from Appendix Figure 3 that one cannot detect a differential change of DKr 435, shown by the dashed black line, in taxable saving. Correspondingly, the DD estimate for crowd-out in taxable saving using the specification in (9) – in which we instrument for total pensions using the interaction term in (7) – has a 95% confidence interval spanning $(-3.94, 5.67)$, as shown in Column 4 of Table 6.

As in our analysis of the mandated savings plan, we obtain more precision by comparing individuals at similar income levels using the RK estimator.[44] We illustrate the design in Figure 9a. This figure plots RK estimates of the change in the marginal propensity to save in retirement accounts and taxable accounts in the three years before and after the 1999 subsidy reduction. We

[44]The RK estimator is unaffected by differential shocks to wealth across income groups because it compares wealth changes for individuals with similar incomes just below and above the top tax cutoff. Thus, smooth changes in the wealth-income gradient across years affect the DD estimator but not the RK estimator. As noted above, in practice we implement the RK estimator parametrically using linear control functions. To see why this parametric estimator still yields a more precise estimate than the DD, let L denote an individual with income below the top tax cutoff, M denote an individual with income at the cutoff, and H denote an individual with income above the cutoff. With this notation, the parametric estimate is a triple-difference: it asks whether the difference in savings rates between individuals H and M is larger than the difference between M and L after the 1999 reform relative to before the reform. The triple difference nets out changes in the savings-income gradient across years by comparing the gradient above and below the kink. In contrast, the DD estimator simply compares changes in savings rates for individuals L and H. The DD estimator relies on stability of the gradient (parallel trends) across years for identification, which is violated for taxable saving as shown in Appendix Figure 3.

construct the series for total pensions by estimating models of the form

$$P_{it} = \alpha + \beta_{1t}[y_{it} > 0] + \beta_{2t}y_{it} + \beta_{3t}[y_{it} > 0]\cdot y_{it} + \varepsilon_{it} \tag{10}$$

for total pension contributions in each year t from 1996 to 2001 separately. We then plot β_{3t}, the change in the marginal propensity to save in pensions at the top tax cutoff in year t. We construct the series for taxable saving in the same way, using taxable saving as the dependent variable in (10). Consistent with the result in Figure 8c, the marginal propensity to save in retirement accounts falls sharply for individuals in the top bracket after the subsidy for capital pensions is reduced in 1999. The marginal propensity to save in taxable accounts rises sharply after 1999 for those in the top bracket, showing that at least part of the increased savings in retirement accounts prior to 1999 came from reduced savings in taxable accounts.

We quantify the degree of crowd-out in taxable saving using the same RK specification as in Panel B of Table 6, now instrumenting for *total* pension contributions with the triple interaction term in (8). Because pension contributions are measured in pre-tax dollars, we divide the IV coefficient by 0.4 to obtain the crowd-out parameter ρ_2 defined above. We illustrate the crowd-out calculation in Figure 9b. This figure replicates Figure 8c, changing the dependent variable to taxable saving. The marginal propensity to save in taxable accounts rises by 5.9 cents at the top tax cutoff when the subsidy is reduced in 1999. This change is the mirror image of the relative *decrease* in pension contributions of 9.9 cents per DKr of income at the top tax cutoff in Figure 8c. This 9.9 cent decrease in pension contributions led to an increase of 3.9 cents in disposable income. The ratio of the reduced-form coefficient in Figure 9b and the first-stage coefficient in Figure 8c yields a crowd-out estimate of $5.9/(0.4 \times -9.9) = -1.47$. That is, each extra DKr 1 deposited in pension accounts because of the subsidy is offset by a DKr 1.47 reduction in taxable saving, as shown in Column 4 of Table 6.[45] While we can reject the hypothesis of zero crowd-out, the confidence interval for the crowd-out estimate is very wide, spanning $(-2.77, -0.17)$.[46]

[45] Crowd-out can in principle exceed 100% in neoclassical models because of wealth effects (Gale, 1998); intuitively, individuals may choose to save less when offered a pension subsidy if they are targeting a fixed level of wealth in retirement.

[46] The increased taxation of capital pension contributions after the 1999 reform reduces disposable income for those who continued to make capital pension contributions after the reform. This change in disposable income has no impact on our crowd-out calculation because the 83% of passive savers who do not respond to the reform also do not change taxable saving significantly when their disposable income changes, as shown by the results in Section IV. The 17% of active savers who respond to the subsidy change do so primarily by exiting capital pensions entirely, and thus their tax liabilities are unaffected by the change in the tax rate on capital pensions. As a result, our crowd-out estimate is driven purely by the behavior of the active savers: the passive savers affect neither the numerator (change in taxable saving), nor the denominator (change in pension contributions). If a small fraction of passive savers do reduce consumption when their disposable income falls after 1999, we would understate the degree of crowd-out, as the increase in taxable saving after 1999 would have been even larger absent this income effect.

To improve precision, we again analyze thresholds rather than mean savings rates. Figure 9c plots the change (from the three years before the reform to after the reform) in the fraction of individuals who have taxable saving above DKr 688, which is the median level of taxable saving for individuals within DKr 5,000 of the top tax cutoff. There is a clear change in the relationship between income and the probability of having high taxable saving at the top tax cutoff after the subsidy is reduced in 1999. We reject the hypothesis that there is no change in slope at the kink with a t-statistic of 4.6 ($p < 0.001$).

We translate this reduced-form impact to an estimate of crowd-out as follows. First, using an RK specification analogous to that in Column 4, we estimate that a DKr 1,000 increase in pension contributions reduces the number of individuals with above-median taxable savings by 0.019%. Next, based on the density of the savings distribution around the mean, we calculate that if the DKr 1,000 increase in pensions were entirely financed by reducing taxable saving, the fraction with above-median savings would have fallen by 0.0194%. The ratio of these two estimates measures the degree of crowd-out induced by the increase in pensions under a binary response model. We report this threshold-based RK estimate in Column 5 of Panel B of Table 6. We obtain a point estimate of -0.98 and a confidence interval of (-1.41, -0.55). Because this 2SLS estimate is identified from the behavior of individuals who reduced their pension contributions when the subsidy fell, the small impact on total saving implies that the EIS is low for active savers. Column 6 shows that the point estimate remains similar when we include the standard controls.[47]

The precision of the RK estimates of crowd-out relative to the DD estimates underscores a general methodological lesson of our analysis. Even with large samples, one cannot obtain precise estimates of how policies affect savings in taxable accounts using estimators that compare individuals at different income levels – as is commonly done in the literature on savings – because individuals with different incomes face very different shocks to wealth. It is critical to use a research design that compares individuals at similar income levels.

We assess the robustness of the crowd-out estimates to alternative definitions of taxable saving in Panel C of Appendix Table 1. In Column 1, we replicate the baseline specification in Column 6 of Table 6B using taxable saving net of changes in liabilities. We again define an indicator for being above the median in this net taxable saving measure and replicate our calculation of crowd-

[47] Columns 5 and 6 of Panel A report DD estimates using the threshold approach. These columns estimate the impact of total pensions on the fraction with above-median savings using the specification in (9). We then rescale the coefficients by the same predicted mechanical impact used to rescale the RK coefficients. The DD estimator does not yield precise estimates even when we use thresholds instead of the level of savings as the dependent variable.

out as above. Column 2 replicates the baseline specification for renters. Column 3 defines taxable saving at the household level and Column 4 restricts the sample to single individuals. Across all specifications, we find estimates of crowd-out that are significantly different from 0 but not from the baseline estimates of crowd-out close to 100%. We conclude the pension contributions induced by tax subsidies have much smaller impacts on total wealth accumulation than those induced by automatic contributions.[48]

We use these crowd-out estimates to calculate the savings impact of each DKr of government expenditure on subsidies for retirement savings. This "bang-for-the-buck" calculation can be compared to the marginal cost of public funds or the benefits of other government expenditures for policy evaluation. First, note that mean per capita contributions to capital pensions fell from DKr 5,174 in 1998 to DKr 2,661 in 1999 in the treatment group. Based on the estimate in Column 2 of Table 6B, this change resulted in a DKr 1,194 reduction in total pension contributions, which results in an increase of DKr 483 in post-tax disposable income. The estimate in Column 5 of Table 6 implies that taxable saving increases by DKr 474 as a result, so that the net reduction in post-tax savings due to the subsidy change was DKr 9. We estimate that the subsidy change reduced the NPV of the individual tax benefit for capital pension contributions by DKr 887 on average across individuals.[49] The capital pension subsidy reduction therefore reduced total saving by 9/887, or roughly 1 cent per DKr reduction in tax expenditure on the subsidy. At the upper bound of the 95% confidence interval for the crowd-out estimate in Column 5, we obtain an estimate of 24.6 cents of savings per DKr of tax expenditure on the subsidy.[50] The subsidy has small impacts on total saving both because the EIS is low for active savers and because it is an infra-marginal transfer that has no impact on the behavior of passive savers.

[48] Among individuals within DKr 75,000 of the top income tax cutoff, the sample used in Table 6, the pass-through rate of employer pensions to total saving (estimated using the specification in Column 2 of Table 3A) is 0.87, compared with 0.90 in the full sample. Hence, the difference between the impacts of price subsidies and automatic contributions is not due to differences in sample composition. We also find that pass-through rates of employer pensions to total saving remain similar before and after the 1999 subsidy reduction, indicating that automatic pension contributions significantly increase total saving irrespective of the subsidy rate (not reported).

[49] The mechanical subsidy reduction of 13.6 cents per DKr yields a fiscal gain of DKr $5,174 \times 0.136 = 704$. Calculating the added fiscal gain from reduced pension contributions requires an estimate of the net subsidy to capital pensions. We estimate the value of reduced capital gains taxation in the retirement account by assuming that the agents invests at age 40, earns a 5% nominal return per year, and realizes 40% of that return each year until liquidating his account at age 60. After accounting for the tax deductions on contributions, taxes on payout, and reduced capital gains taxation, we estimate that the net subsidy to capital pension contributions was 15.4 cents per DKr after the 1999 reform. With this subsidy, the DKr 1,194 reduction in total pension contributions led to further fiscal savings of DKr 183. For simplicity, we ignore the additional fiscal gain due to shifting of assets from capital pension to annuity pensions in 1999.

[50] If we include the fiscal cost of the subsidy for employer pensions, which as we noted above have no effect on total employer pension contributions, the point estimate falls below 1 cent per DKr of tax expenditure and the upper bound of the 95% confidence interval is 8.3 cents.

VI Heterogeneity: Identifying Active and Passive Savers

In this section, we test whether the differences between the impacts of automatic contributions and subsidies are driven by active vs. passive choice by studying the heterogeneity of responses across individuals. We organize our analysis around the three testable predictions on heterogeneity described in Section II.B.

We begin by testing whether individuals who are currently making active choices are more responsive to price subsidies. We proxy for active choice by focusing on individuals who are starting a new pension account. Define "new contributors" in year t as those who contribute to either individual annuity or capital pensions in year t but did not contribute to either account in year $t-1$. Conversely, define prior contributors as individuals who were already contributing to an individual pension account in year $t-1$. Are new contributors more sensitive to the change in the relative subsidy for capital vs. annuity pensions in 1999? To answer this question, we regress an indicator for contributing to capital pensions on an indicator for the 1999 reform, an indicator for being a new pension contributor, and the interaction of the two indicators. We limit the sample to individuals whose incomes are between DKr 25,000 and 75,000 above the top tax cutoff, the treatment group in Figure 6a, and use data from 1998 and 1999. The estimates are reported in Column 1 of Table 7. The reduction in the subsidy for capital pensions reduces the probability of contributing to the capital pension by 15% for prior contributors. For new contributors, the impact is an additional 23%. These estimates are not sensitive to the inclusion of controls, as shown in Column 2 of Table 7.

The preceding result shows that behavioral responses are state-dependent in the sense that individuals are more responsive to price subsidies when they happen to be reoptimizing their portfolios for other reasons. This result suggests that attention varies *within* individuals over time. Next, we test for variation in attention *across* individuals by correlating the response to the 1999 subsidy reduction with the frequency of changes in pension contributions in other years. Are individuals who actively reoptimize their portfolios in other years more likely to respond to the change in incentives in 1999?

We identify individuals who responded to the 1999 subsidy change using the sharp indicator of response developed in Section V.A: exiting capital pensions *and* increasing annuity contributions. Recall from Table 5 that 11.6% of individuals who were contributing to capital pensions in 1998 respond to the subsidy reduction in this way. Figure 10a plots this indicator of extensive margin substitution in 1999 against the percentage of other years in which individuals changed the level of either their capital or annuity pension contributions. We include only individuals in the treatment group (income between DKr 25,000 and 75,000 above the top tax cutoff) who were previously contributing to capital pensions in this figure. The figure shows that frequent reoptimizers in other years are more likely to exit capital pensions in 1999. More than 20% of individuals who adjust their pensions in every year respond to the 1999 reform by exiting capital pensions and raising annuities, compared with less than 5% of individuals who never adjusted their pensions in other years. Columns 3 and 4 of Table 7 report estimates from OLS regressions corresponding to Figure 10a. We regress the indicator for extensive margin substitution in 1999 on the fraction of other years in which individuals change pension contributions. Consistent with the figure, we find a highly significant negative relationship that is robust to the inclusion of controls.

We now turn to the second prediction: active savers should not only be more responsive to the price subsidy but should also be more likely to undo automatic contributions by reducing individual pension contributions. We evaluate this prediction in Figure 10b. To construct this figure, we first divide individuals in our firm switchers sample into vingtiles based on the frequency with which they change individual pension contributions in other years. We then estimate the degree of pass-through from employer pensions to total pensions by running the regression in Column 1 of Table 3A for each of these subgroups separately. Figure 10b plots the coefficients from these regressions vs. the average frequency of pension changes in other years within each bin. Changes in employer pensions have significantly lower impacts on total pension contributions for individuals who reoptimize their portfolios more actively. Column 5 of Table 7 replicates the specification in Column 1 of Table 3, Panel A, interacting the change in employer pensions during the firm switch with the fraction of other years in which individuals change pension contributions. Consistent with Figure 10b, we find a highly significant interaction effect: individuals who reoptimize their portfolios in all other years have pass-through rates that are 8% lower than those who never change their portfolios in other years.[51] Column 6 verifies that this result is robust to the inclusion of controls. Together,

[51] There are two explanations for why pass-through rates remain relatively high even for individuals who reoptimize very frequently. First, the frequency of changes in pension contributions is a noisy proxy for active response. Figure 10a shows that even among those who change contributions in every other year, the rate of active response to the subsidy change is only about 20%. This suggests that the degree of crowd-out among active savers could be up to 40 percentage points larger than passive savers. Unfortunately, we do not have adequate precision to directly verify that the specific individuals who responded to the 1999 reform (by exiting capital pensions and raising annuities) also offset employer contributions, as there are only 4,647 individuals in our sample who both responded to the 1999 reform and switched employers in another year. A second potential explanation for imperfect crowd-out of automatic contributions is that those who respond actively to subsidy changes may still be passive with respect to employer pensions. For instance, tax advisers frequently advertise subsidies for retirement contributions but information about

the findings in Figure 10 suggest that the key reason automatic contributions have larger effects on total wealth than price subsidies is because they change the behavior of passive rather than active savers.

Finally, we study the observable characteristics of active and passive savers. In Figure 11a, we plot the same indicator of response to the price subsidy used in Figure 10a – exiting the capital pension in 1999 and raising annuity contributions – against individuals' wealth/income ratios, defined as total financial assets in non-retirement accounts in 1998 divided by gross labor income in 1998. We limit the sample to prior contributors in the treatment group in 1999, as in Figure 10a. The probability of response to the subsidy change is twice as large for those who have accumulated liquid assets of twice their annual income relative to individuals with little or no wealth. Conversely, in Figure 11b, we test whether individuals with high wealth/income ratios are also more likely to undo automatic contributions in other savings accounts. We construct this figure in the same manner as Figure 10b, first dividing the observations in the firm switchers sample into twenty equal-sized bins (vingtiles) based on their wealth/income ratios, and then estimating pass-through within each bin. Changes in employer pensions have much smaller impacts on total saving for individuals with higher wealth/income ratios. The correlations in Figure 11 support the prediction that the individuals who respond to retirement savings incentives tend to be those who are planning and saving for retirement to begin with.

Figure 11c analyzes heterogeneity by age. The solid bars plot the fraction of individuals who respond to the subsidy reduction by exiting capital pensions and increasing annuity contributions by their age group in 1999. The hollow bars show the pass-through of employer pensions to total pensions, estimated as in Figure 11b, after grouping individuals into age deciles at the time they switch employers. Individuals in their 50's are almost twice as likely to respond to the change in price subsidies than those in their 20's. Pass-through rates of employer pensions to total pensions are also about 20 percentage points smaller for those in their 50's. These patterns are consistent with recent evidence that individuals in their 50's tend to make financial decisions more actively (Agarwal et al., 2009).[52]

We evaluate the robustness of these patterns and analyze heterogeneity along other dimensions in Table 8. In Panel A, we regress the indicator for exiting capital pensions and raising annuities in 1999 on various observable characteristics. In this panel, we limit the sample to prior capital pension

employer pension contributions may be less salient.

[52]While these patterns are suggestive of heterogeneity by age, note that we cannot distinguish cohort effects from age effects in our relatively short sample.

contributors who were in the treatment group in 1999. In Panel B, we include all individuals in our firm switchers sample who were not at a corner prior to the switch (as in Table 3a) and regress the change in total savings rate on the change in employer pensions at the time of the firm switch interacted with observable characteristics. Columns 1 and 2 replicate the results in Figure 11 and confirm that wealthier and older individuals are more responsive to the subsidy change and have lower pass-through rates of employer pensions to total saving. Column 3 of Table 8 shows that the age and wealth interaction effects remain similar when they are jointly included in the regression specification, suggesting that responses are heterogeneous along both dimensions.

In columns 4 and 5, we analyze heterogeneity by education. Column 4 shows that individuals with a college education are 3 percentage points more responsive to the change in price subsidies, relative to the sample of mean of 13%. Column 5 shows that the type of education one obtains matters even more. The Danish registers record the subject in which the individual majored in his terminal (highest) degree. Individuals who majored in economics, accounting, or finance in their terminal degree are 7.2 percentage points (55%) more likely to respond to the subsidy change. While we cannot determine whether this large interaction effect is caused by learning economics or the sorting of active savers to such courses, the correlation supports the view that active response to financial incentives is correlated with financial sophistication and literacy (Lusardi and Mitchell, 2007; Gale, 1998; Bernheim and Scholz, 1993). However, we find little relationship between education and pass-through rates from employer pensions to total savings, suggesting that even well informed individuals may not be attentive to automatic changes in pension contributions.

Finally, in Column 6, we replicate Column 5 and include gender, marital status, and two-digit occupation indicators. The heterogeneity of treatment effects remains similar when we include these additional controls. Overall, the results in Table 8 indicate that price subsidies tend to target individuals who are planning for retirement, while automatic contributions raise savings more amongst those who are less prepared for retirement.

VII Conclusion

Our analysis shows that price subsidies are less effective than automatic contributions in increasing savings rates for three reasons. First, approximately 85% of individuals are passive individuals who save more when induced to do so by an automatic contribution but do not respond at all to price subsidies. As a result, much of the subsidy is an inframarginal transfer to pension contributors that induces little change in behavior at the margin. Second, individuals who respond do so primarily by shifting savings across accounts rather than raising the total amount they save. Third, the active savers who respond to price subsidies tend to be those who are planning and saving for retirement already. Hence, price subsidies are not effective in increasing savings amongst those who are least prepared for retirement. In contrast, policies that influence the behavior of passive savers have lower fiscal costs, generate relatively little crowd-out, and have the largest impacts on individuals who are paying the least attention to saving for retirement.

It is natural to ask whether these conclusions apply to other economies, such as the United States. While there is no substitute for directly studying the economy of interest empirically, we believe that the qualitative lessons from the Danish data are likely to generalize to other economies for two reasons. First, aggregate savings rates and the structure of the retirement savings system in Denmark are similar to those in the U.S. and other developed economies. Second, and more importantly, our findings on behavior *within* pension accounts closely match the results of prior research using data from the U.S., which has much higher quality data on pension contributions than non-retirement savings. In particular, studies using U.S. data have also found that automatic employer contributions raise total pension balances (Madrian and Shea, 2001; Card and Ransom, 2011), subsidies induce relatively few individuals to contribute to retirement accounts (Duflo et al., 2006; Engelhardt and Kumar, 2007), and higher socio-economic status households are more likely to change pension defaults (Beshears et al., 2012). Given the similarity of behavior within retirement accounts between the U.S. and Denmark, we expect the new responses that we document in taxable non-retirement accounts to be similar as well.

Our results raise several questions for further research. We have provided a positive analysis of the impacts of retirement savings policies on total saving, but have not compared the welfare consequences of these policies. Such a normative analysis would be a natural next step in understanding the optimal design of retirement savings policies. Beyond retirement savings, a broader implication of our empirical results is that changing quantities directly through defaults or regulation may be more effective than providing incentives to change behaviors of interest, such as the consumption of "sin" goods or the use of preventive healthcare. Because incentives require active reoptimization, they may be less cost-effective and, moreover, may end up missing the least attentive individuals whose behavior one might want to change.[53] Comparing quantity-based and price-based methods in models where agents make optimization errors is an interesting direction for future research.

[53] One recent example is New York City's ban on large sodas in 2012. Although neoclassical models would suggest that a tax might be the best way to reduce consumption of sodas, the quantity restriction that was imposed might be more effective if present-biased individuals who purchase large sodas also tend to be inattentive to taxes.

Finally, from a policy perspective, our findings call into question whether subsidies and reductions in capital income taxation are the best way to increase savings rates. For instance, the U.S. will spend more than $100 billion in tax expenditures on subsidies for 401(k)'s, IRA's, and related accounts in 2013 (Joint Committee on Taxation, 2012). Our findings strengthen recent arguments for using "nudges" such as automatic payroll deductions or savings defaults to increase savings instead of such subsidies (e.g. Thaler and Sunstein, 2008; Madrian, 2012).

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TABLE 1

Impacts of Government Policies on Savings for Active vs. Passive Savers

	Automatic Contribution		**Price Subsidy**	
	Raises Pension Contributions M+P?	Raises Total Saving M+P+S?	Raises Pension Contributions M+P?	Raises Total Saving M+P+S?
Active Savers (Neoclassical)	No	No	Yes	Uncertain
Passive Savers	Yes	Uncertain	No	No

Notes: This table summarizes the predictions of the stylized model in Section II. Active savers follow the neoclassical lifecycle model when choosing pension contributions (P) and taxable saving (S). Passive savers set P at a fixed level irrespective of government policies.

TABLE 2

Summary Statistics

Variable	Full Sample			Top Tax Threshold Sample		
	Mean (1)	SD (2)	Median (3)	Mean (4)	SD (5)	Median (6)
Gross Labor Income	199,565	108,265	191,447	256,618	51,768	253,190
Gross Taxable Income	217,474	121,378	208,985	282,607	57,575	279,335
Net Capital Income	-14,549	19,512	-5,870	-20,541	20,257	-17,042
Non-Pension Assets (not incl. home equity)	51,602	104,664	13,550	60,495	109,046	19,478
Non-Pension Assets > 10% of Gross Labor Inc.	47%			42%		
Non-Pension Assets/Gross Labor Inc. Ratio	0.37	0.95	0.09	0.22	0.39	0.07
Total Saving	23,904	51,740	12,438	32,752	53,192	25,191
Savings Rate	18.92%	31.37%	10.92%	15.32%	17.43%	10.86%
Liabilities (not incl. home mortgage)	76,539	111,070	31,022	95,374	116,157	59,538
Change in Liabilities	5,681	47,170	0	5,529	54,157	0
Net Savings Rate	4.06%	58.83%	8.44%	9.68%	30.09%	10.22%
Pension Contributions						
Fraction with Individual Pension Contribs.	27%			36%		
Individual Pension Contribution	3,143	9,952	0	4,081	10,243	0
Individual Pension Contribution Rate	1.18%	3.07%	0.00%	1.25%	2.80%	0.00%
Individual Capital Pension Contribution	1,859	5,908	0	2,643	6,824	0
Individual Annuity Pension Contribution	1,284	7,096	0	1,438	6,595	0
Fraction with Employer Pension Contribs.	59%			83%		
Employer Pension Contribution	15,542	28,477	5,281	21,717	24,390	19,705
Employer Pension Contribution Rate	5.67%	5.27%	5.29%	6.98%	5.02%	7.51%
Fraction with Any Pension Contribution	66%			90%		
Demographics						
Age	38.70	12.09	38.00	41.36	10.21	41.00
Female	52%			44%		
Married	48%			58%		
Has Partner	62%			73%		
Homeowner	51%			68%		
College Degree	41%			59%		
Economics Major in Terminal Degree	4%			4%		
Number of Observations		45,428,846			17,712,370	

Notes: This table presents means, standard deviations, and medians of key variables in the full sample and for those with gross taxable income within DKr 75,000 of the top tax threshold. We trim all pension contribution levels at the 99.9th percentile. We trim savings measures and all pension contribution rates (measured as a percentage of income) at the 1st and 99th percentiles. Gross labor income is total wage earnings before pension contributions. Gross taxable income adds non-labor income to this measure. Net capital income includes capital income minus mortgage interest payments. Non-pension assets are measured at the end of each calendar year and exclude home equity. Total saving is the change in non-pension assets plus pension contributions in each year. All rates are calculated relative to a base of gross labor income. Liabilities measures total non-mortgage debt, including other secured debt and unsecured debt. Net saving rate is defined as total saving minus the change in liabilities divided by gross labor income. Age is measured at the end of the calendar year. An individual has a partner if he/she is married or cohabitates with any non-blood relative of the opposite gender that is within fifteen years of age. An individual is a home-owner if he or his partner has positive home equity. Economics major is an indicator for majoring in economics, accounting, or finance in one's terminal degree.

TABLE 3
Employer Pensions: Pass-Through Estimates

Sample:	All Firm Switches	All Firm Switches	All Firm Switches	Mass Layoffs	Large Changes	First Switches	Switches Age 46-54
Dep. Var.:	Δ Pension Rate	Δ Savings Rate	Δ Savings Rate	Δ Savings Rate	Δ Savings Rate	Δ Savings Rate	Δ Retirement Balance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Lagged Savings >0							
Δ Employer Pension Rate	0.947 (0.002)	0.900 (0.009)	0.888 (0.009)	0.865 (0.110)	0.897 (0.011)	0.832 (0.018)	5.806 (0.380)
Δ Earnings			0.043 (0.001)				
Observations	910,866	2,078,612	2,078,612	36,659	216,613	716,273	55,608
Panel B: Lagged Savings >0 with Controls							
Δ Employer Pension Rate	0.946 (0.002)	0.908 (0.009)	0.897 (0.009)	0.892 (0.107)	0.904 (0.011)	0.868 (0.018)	5.933 (0.378)
Δ Earnings			0.048 (0.001)				
Observations	910,866	2,078,612	2,078,612	36,659	216,613	716,273	55,608
Panel C: Threshold Approach							
Δ Employer Pension Rate	0.966 (0.001)	0.961 (0.004)	0.955 (0.004)	0.915 (0.025)	1.067 (0.017)	0.929 (0.006)	
Δ Earnings			0.063 (0.003)				
Observations	3,958,920	3,367,175	3,367,175	60,284	387,125	889,261	

Notes: This table presents pass-through estimates of the impact of changes to employer pension contribution rates on savings at the time of a firm switch. Standard errors, clustered by the firm to which the individual switches, are reported in parentheses. See notes to Figure 2 for more details on the definition of firm switch. In Panel A, the independent variable is the change in the employer contribution rate from the year before to the year of the switch. In Column 1, the dependent variable is the change in the total pension contribution rate over the same period, and we include only individuals not at a corner in individual pensions (defined as positive lagged individual pension contributions) at $t = -1$. In Columns 2-6, the dependent variable is the change in savings rate over the same period, and we include only individuals not at a corner in individual savings (defined as either positive lagged individual pension contributions or lagged wealth greater than 10% of current gross labor income) at $t = -1$. Column 3 includes the change in gross labor income, as a fraction of lagged gross labor income, as an additional control. Column 4 repeats Column 2, restricting to the sample of workers whose firm switch is classified as coming from a mass layoff. We define mass layoffs as more than 90% of workers leaving a firm for firms larger than 50 employees, and no more than 50% of the original employees ending up at the same new firm. Column 5 repeats Column 2, restricting to the sample of workers experiencing a change in the employer pension contribution rate greater than 5 percentage points in absolute value. Column 6 repeats Column 2, restricting to the first firm switch for each individual. The dependent variable in Column 7 is the cumulative change in total saving between the time of the first firm switch and age 60 for those workers switching before age 55 and reaching 60 in our data. We present the change in savings as a ratio of gross labor income at $t = -1$ and include only those not at a corner in individual savings at $t = -1$. Panel B replicates Panel A controlling for age, marital status, gender, college attendance, and two-digit occupation indicators. Panel C replicates Panel A using a threshold approach to calculate pass-through. In Column 1, we regress the change in an indicator for having a total pension contribution rate above the threshold (defined as the regression-sample mean of the total pension contribution rate) on the change in an indicator for crossing the same threshold if the pass-through rate were 100% and savings in other accounts stayed at their year $t - 1$ level. We instrument for the independent variable with the change in the employer contribution rate and include an indicator for having a positive change in the employer contribution rate as a regressor. The remaining columns in Panel C repeat this procedure using the relevant dependent variable, defining the threshold as the regression-sample mean of that variable.

TABLE 4

Government Mandated Savings Plan: Pass-Through Estimates

Dep. Var.:	Δ Total Pensions	Δ Total Ind. Saving	Δ Total Saving
	(1)	(2)	(3)
Panel A: Lagged Savings >0			
Pass-Through RD	0.946	-2.248	2.771
	(0.251)	(14.692)	(1.744)
Observations	37,616	92,872	92,186
Panel B: Lagged Savings >0 with Controls			
Pass-Through RD	1.126	-1.948	2.766
	(0.246)	(15.077)	(1.765)
Observations	37,616	92,872	92,186
Panel C: Threshold Approach			
Pass-Through RD	0.862	1.172	1.149
	(0.172)	(0.271)	(0.290)
Observations	183,001	156,157	156,157

Notes: This table presents pass-through estimates using an RD design based on the eligibility cutoff for the Mandated Savings Program (MSP) in 1998 at an income of DKr 34,500. Standard errors, clustered by DKr 1,000 income bin, are reported in parentheses. All cells report estimates from regression specifications with separate linear control functions above and below the threshold. The estimates reported are for the discontinuity at the threshold divided by DKr 345 (the increase in mandated saving at the threshold). The dependent variables are the change in total pension contributions (Column 1), total saving excluding employer pensions (Column 2), and total saving (Column 3) from 1997 to 1998. Panel A estimates the specification with no controls. Panel B replicates Panel A controlling for age, marital status, gender, college attendance, and two-digit occupation indicators. In Panels A and B, we restrict the sample to individuals who are not at a corner in individual pensions (Column 1) or individual savings (Columns 2 and 3) in 1997. See notes to Table 3 for the definition of corners. Panel C uses a threshold approach to estimate pass-through, comparing the actual change in the fraction above the sample mean with the change that would be predicted with 100% pass-through. See notes to Figure 4 for further details.

TABLE 5

Impact of Capital Pension Subsidies on Capital Pension Contributions

Dep. Var.:	Individual Capital Pension Contributions				No Indiv. Response	Ext. Margin Substitution	Ext. Margin Substitution, Prior Non-Responders	Indiv. Exits Capital Pension
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Above Cutoff ×Post	-2439.2 (97.65)	-2334.3 (94.33)			-0.028 (0.016)	0.116 (0.007)	0.068 (0.006)	0.161 (0.011)
Income×Above Cutoff×Post			-0.021 (0.002)	-0.022 (0.002)				
Controls		X		X				
Estimator	D-D	D-D	R-K	R-K	D-D	D-D	D-D	D-D
Years Included	1996-2001	1996-2001	1996-2001	1996-2001	1998-1999	1998-1999	1998-1999	1998-1999
Observations	4,697,656	4,697,656	7,011,068	7,011,068	509,679	509,679	191,372	509,679

Notes: This table characterizes the response to the 1999 reduction in the capital pension subsidy on savings behavior within capital pensions. In Columns 1-4, the dependent variable is individual capital pension contribution. Columns 5-8 each use different indicators of response. Column 1 presents a difference-in-differences estimate of the effect of the 1999 reform on average capital pension contributions using the specification in equation (7). The treatment group includes individuals with taxable income between DKr 25,000 and DKr 75,000 above the top tax threshold; the control group includes individuals with taxable income between DKr 75,000 and DKr 25,000 below the threshold. Column 3 presents a regression-kink estimate of the effect of the 1999 reform on capital pension contributions using all individuals with taxable income within DKr 75,000 of the threshold. We estimate the specification in equation (8), which includes separate linear control functions above and below the threshold, both before and after the reform. The reported coefficient is the triple interaction, which measures the change in the slope at the kink due to the reform. Columns 2 and 4 replicate Columns 1 and 3 controlling for age, marital status, gender, college attendance, and two-digit occupation indicators. In Columns 5-8, we report difference-in-differences estimates on various indicators for individual response to the reform, using only data from 1998-1999 and restricting to individuals with positive lagged capital pension contributions. In Column 5, the dependent variable is an indicator for leaving capital pension contributions unchanged relative to the previous year. In Column 6, the dependent variable is "extensive margin substitution," defined as decreasing capital pension contributions to zero while increasing annuity pension contributions. Column 7 replicates Column 6, restricting to individuals who did not change their capital pension contributions in the previous year. In Column 8, the dependent variable is an indicator for decreasing capital pension contributions to zero. In Columns 1-2 and 5-8, we cluster standard errors at the DKr 10,000-income-bin-by-year level. In Columns 3 and 4, we cluster standard errors by DKr 5,000 income bin.

TABLE 6

Capital Pensions: Crowd-Out and Pass-Through Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Difference-in-Differences Estimates						
Dep. Var.:	Annuity Contrib.	Total Pension Contrib.	Total Pension Contrib.	Taxable Saving	Taxable Saving Threshold	Taxable Saving Threshold
Capital Pension Contrib.	-0.562 (0.047)	0.438 (0.047)	0.392 (0.047)			
Total Pension Contrib. Rate				0.867 (2.453)	-0.011 (0.743)	0.034 (0.869)
Controls			X			X
Observations	4,697,656	4,697,656	4,697,656	4,697,656	4,697,656	4,697,656
Panel B: Regression-Kink Estimates						
Dep. Var.:	Annuity Contrib.	Total Pension Contrib.	Total Pension Contrib.	Taxable Saving	Taxable Saving Threshold	Taxable Saving Threshold
Capital Pension Contrib.	-0.525 (0.049)	0.475 (0.049)	0.510 (0.039)			
Total Pension Contrib.				-1.471 (0.665)	-0.980 (0.222)	-0.918 (0.187)
Controls			X			X
Observations	7,011,068	7,011,068	7,011,068	7,011,068	7,011,068	7,011,068

Notes: This table presents crowd-out and pass-through estimates from the 1999 reduction in the capital pension subsidy. Panel A uses a difference-in-differences estimator to estimate crowd-out and pass-through, in which we instrument for the key independent variable with the DD interaction term shown in equation (7). See notes to Table 5 for more details on the DD design. Panel B uses a regression kink estimator to estimate crowd-out and pass-through, in which we instrument for the key independent variable with the triple interaction term shown in equation (8). See notes to Table 5 for more details on the RK design. In Columns 1-3, the independent variable is individual capital pension contributions. In Columns 4-6, the independent variable is the total individual pension contribution (including capital and annuity pension contributions). The dependent variables are individual annuity pension contributions (Column 1), total individual pension contributions (Columns 2-3), taxable saving, defined as the change in non-retirement assets (Column 4), and an indicator for having taxable saving above DKr 688, which is median taxable saving for those within DKr 5,000 of the top tax threshold (Columns 5-6). We convert the threshold estimate into an estimate of crowd-out by rescaling the IV coefficient by the predicted change in taxable saving with 100% crowd-out given the density of the taxable saving distribution around the threshold; see text for details. In both panels, Columns 3 and 6 replicate Columns 2 and 5 controlling for age, marital status, gender, college attendance, and two-digit occupation indicators. In Panel A, we cluster standard errors at the DKr 10,000-income-bin-by-year level. In Panel B, we cluster standard errors by DKr 5,000 income bin.

TABLE 7

Active vs. Passive Choice and Responses to Subsidies and Employer Pensions

Dep. Var.:	Contributes to Capital Pension		Extensive Margin Substitution in 1999		Δ Total Pension Contrib. Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Post 1999	-0.148 (0.009)	-0.149 (0.006)				
Post 1999 ×New Saver	-0.228 (0.006)	-0.227 (0.006)				
Δ Employer Pension					0.983 (0.002)	0.981 (0.002)
Fraction of Other Years with Change in Pension			0.153 (0.005)	0.136 (0.005)	-0.005 (0.0002)	-0.005 (0.0002)
Δ Employer Pension×Fraction of Other Years with Change in Pension					-0.076 (0.005)	-0.075 (0.005)
Controls		X		X		X
Observations	142,998	142,998	63,656	63,656	909,776	909,776

Notes: Column 1 regresses an indicator for making individual contributions to capital pensions on a post-1999 indicator, a new saver indicator, and the interaction of these two variables. The new saver variable is an indicator for making zero individual annuity and capital pension contributions in the prior year. We use data from 1998 and 1999 and individuals with taxable income between DKr 25,000 and DKr 75,000 above the top tax cutoff who are currently contributing to either capital or annuity pensions in this regression. Column 3 regresses an indicator for extensive margin substitution in response to the 1999 capital pension reform, defined as exiting capital pensions and raising annuity pension contributions, on the fraction of other years in which an individual changes individual capital or annuity pension contributions relative to the prior year. We use data from 1999 and individuals with taxable income between DKr 25,000 and DKr 75,000 above the top tax cutoff who made positive contributions to individual capital pensions in 1998 in this regression. Column 5 replicates Column 1 of Table 3a, including an interaction of the change in the employer pension contribution rate with the fraction of other years (excluding the year of the firm switch) in which an individual adjusted pension contributions. Columns 2, 4, and 6 replicate Columns 1, 3, and 5 controlling for age, marital status, gender, college attendance, and two-digit occupation indicators. In Columns 1-4, we cluster standard errors at the DKr 10,000-income-bin-by-year level; in Columns 5-6, we cluster by the firm to which the individual switches.

TABLE 8

Observable Heterogeneity in Responses to Subsidies and Employer Pensions

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Capital Pension Subsidy Reduction						
Dep. Var.:	Exit Capital Pensions and Raise Annuity Pensions in 1999?					
Wealth/Inc. Ratio	0.071 (0.004)		0.062 (0.004)	0.060 (0.004)	0.057 (0.004)	0.053 (0.004)
Age		0.002 (0.0001)	0.002 (0.0001)	0.002 (0.0001)	0.002 (0.0001)	0.002 (0.001)
College				0.030 (0.004)	0.027 (0.003)	0.015 (0.004)
Economics Education					0.072 (0.007)	0.055 (0.007)
Controls						X
Observations	62,641	62,641	62,641	62,641	62,641	62,641
Panel B: Employer Pensions						
Dep. Var.:	Δ Total Saving					
Δ Employer Pension Rate	0.890 (0.010)	0.913 (0.009)	0.892 (0.010)	0.901 (0.015)	0.903 (0.015)	0.895 (0.015)
Δ Emp. Pen. Rate $\times$ Wealth/Inc.	-0.208 (0.041)		-0.199 (0.041)	-0.200 (0.041)	-0.199 (0.041)	-0.196 (0.041)
Δ Emp. Pen. Rate $\times$ Age		-0.006 (0.001)	-0.007 (0.001)	-0.007 (0.001)	-0.007 (0.001)	-0.006 (0.001)
Δ Emp. Pen. Rate $\times$ College				0.007 (0.022)	0.005 (0.023)	0.009 (0.023)
Δ Emp. Pen. Rate $\times$ Econ. Ed.					-0.002 (0.036)	-0.011 (0.036)
Controls						X
Observations	2,040,244	2,040,244	2,040,244	2,040,244	2,040,244	2,040,244

Notes: Panel A regresses an indicator for extensive margin substitution in response to the 1999 capital pension reform, defined as exiting capital pensions and raising annuity pension contributions, on various individual characteristics. We use data from 1999 and individuals with taxable income between DKr 25,000 and DKr 75,000 above the top tax cutoff in Panel A. The lone independent variable in Column 1 is the lagged ratio of non-pension assets to gross labor income. The lone independent variable in Column 2 is age in 1999. Column 3 includes both of these variables together. Column 4 adds an indicator for having attended any college. Column 5 adds an indicator for having some training in economics, either in college or at lower levels if the individual did not attend college. Column 6 replicates Column 5 adding controls for marital status and gender and two-digit indicators for occupation. Panel B replicates Column 2 of Table 3a, including interactions of various individual characteristics with the change in employer pension contribution rate at the time of the firm switch and the direct effect of the same characteristics. The characteristics are the same as those in Panel A in Columns 1-5. Column 6 replicates Column 5 and includes controls for gender, marital status, and occupation, but does not interact these controls with the change in employer pensions. All interacted characteristics except indicators are demeaned, so that the raw effect of the change in the employer pension contribution rate can be interpreted as the pass-through rate for individuals with mean values of the continuous variables and all indicator variables equal to zero. We cluster all standard errors in Panel A at the DKr 10,000-income-bin-by-year level; we cluster all standard errors in Panel B by the firm to which the individual switches.

APPENDIX TABLE 1

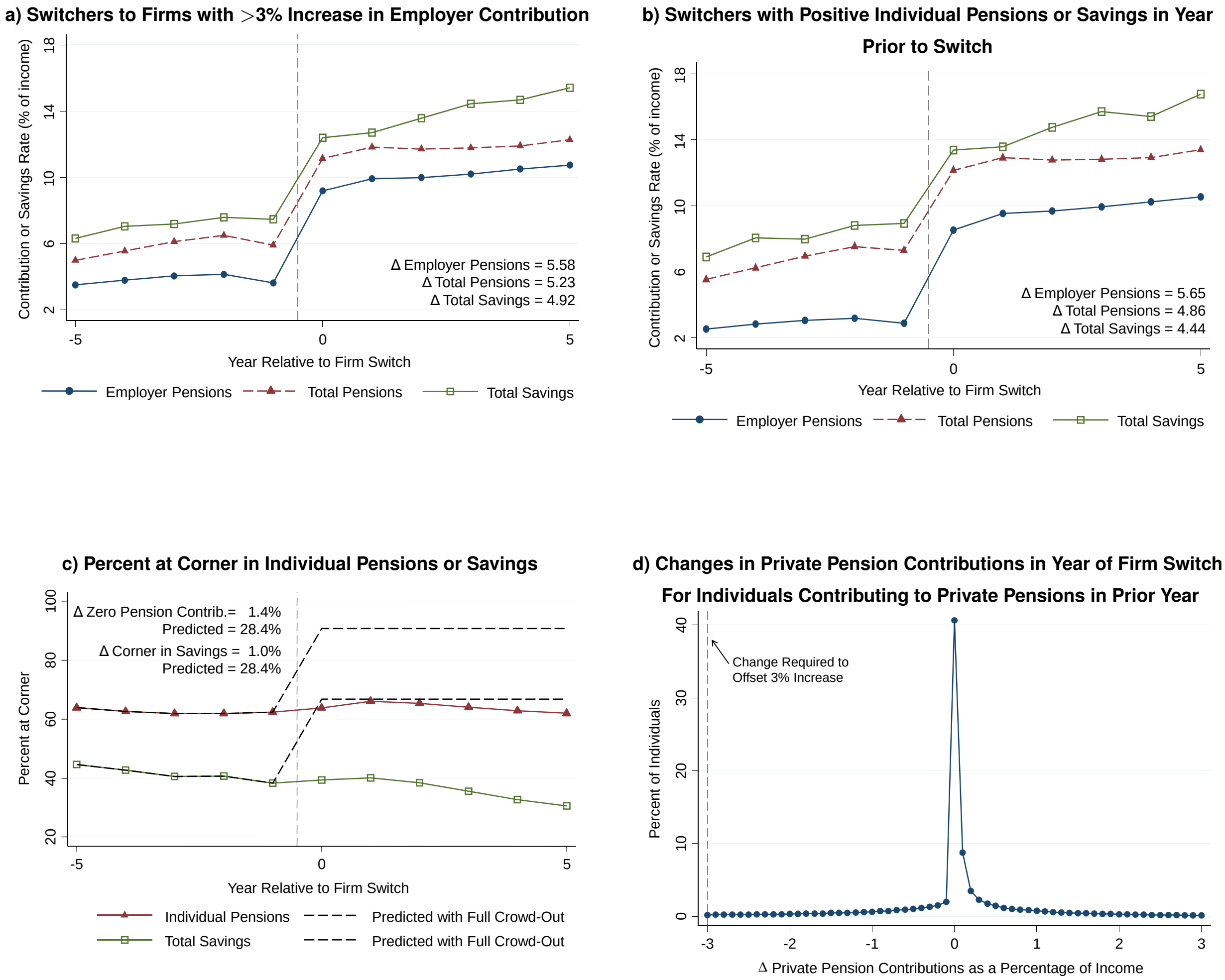
Robustness Checks

	Net Saving	Renters	Household Saving	Single Individuals
	(1)	(2)	(3)	(4)
Panel A: Effect of Employer Pensions on Total Saving (Table 3B, Column 2)				
Dep. Var.:	Δ Savings Rate			
Δ Employer Pension Rate	0.899	0.954	0.868	0.911
	(0.014)	(0.013)	(0.010)	(0.015)
Observations	1,858,297	941,450	2,024,950	793,188
Panel B: Effect of MSP on Total Saving (Table 4C, Column 3)				
Dep. Var.:	Total Saving Threshold			
Pass-Through RD	1.939	1.310	1.349	1.575
	(0.748)	(0.337)	(0.413)	(0.497)
Observations	155,013	124,107	156,157	97,273
Panel C: Subsidy Change and Crowd-out of Taxable Saving (Table 6B, Column 6)				
Dep. Var.:	Taxable Saving Threshold			
Total Pension Contrib.	-1.901	-1.175	-0.956	-1.468
	(0.400)	(0.468)	(0.340)	(0.300)
Observations	6,981,197	2,320,648	7,011,069	2,986,147

Notes: This table replicates three key specifications using alternative samples or dependent variables to evaluate the robustness of the results. Panel A replicates the specification in Table 3B, Column 2, measuring the effect of a change in the employer pension contribution rate on total saving at the time of a firm switch. Panel B replicates the specification in Table 4C, Column 3, measuring the effect of the Mandated Savings Plan on savings using a threshold indicator for having above-average saving. Panel C replicates the specification in Table 6B, Column 6, measuring the effect of the 1999 capital pension subsidy reform on taxable savings using a threshold indicator for having above-average savings in a regression-kink specification. See notes to the earlier tables for further details on each specification. In Column 1, we replicate each specification using net saving (i.e., change in total assets minus change in non-mortgage liabilities) to construct the dependent variable. The dependent variable in Panel A of Column 1 is the change in the net saving rate; in Panels B and C, the dependent variable is an indicator for having net saving above the relevant sample mean (Panel B) or median (Panel C). In Column 2, we replicate the three original specifications exactly, restricting to individuals who do not own homes (i.e., have zero home equity). In Column 3, we replicate each specification including partner's taxable assets in our savings measure where present. In Panel A of Column 3, the dependent variable is the change in the household saving rate (measured relative to original partner's income); in Panels B and C, the dependent variable is a dummy variable for having household saving (or household taxable saving) above the relevant sample mean or median. In Column 4, we replicate the three original specifications exactly, restricting to individuals who do not have a partner, defined as in the notes to Table 2.

FIGURE 1

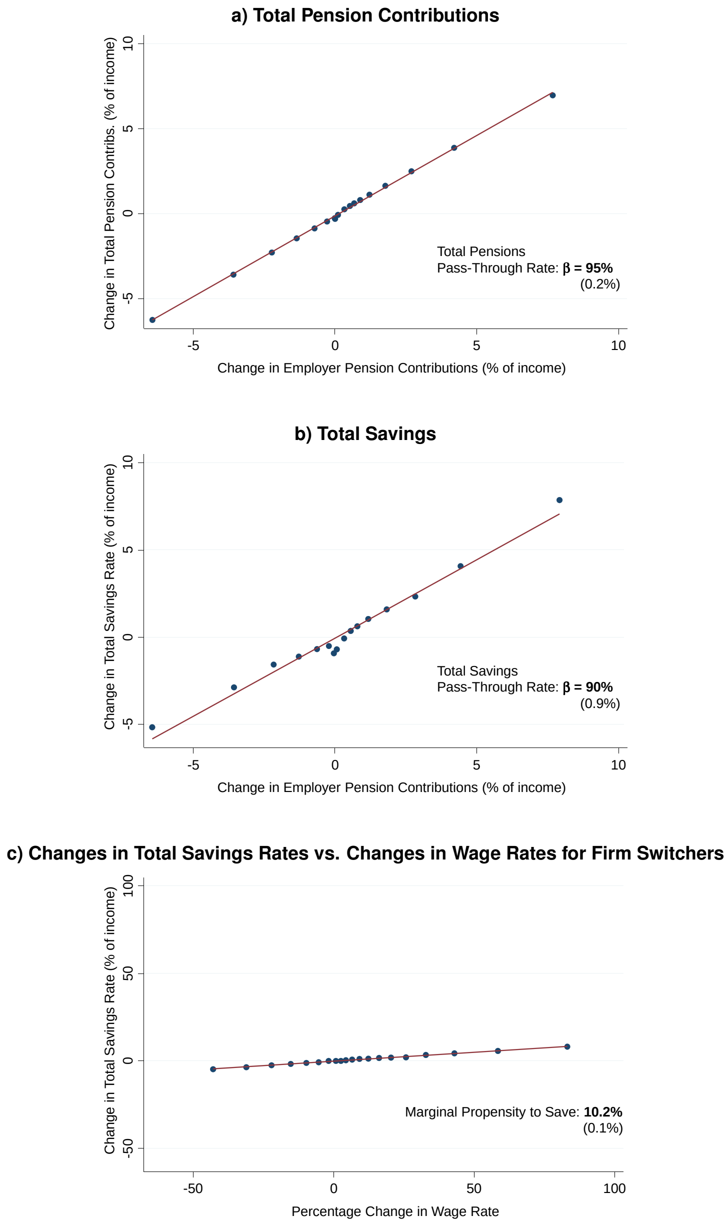
Employer Provided Pensions: Event Studies Around Switches to Firm with >3% Increase in Employer Pension Contribution



Notes: Figures 1a-1c are event studies of pension contribution rates when workers switch firms. We include only the first firm switch for individuals in our data in these figures; hence, $t = 0$ denotes the first year in which the primary firm ID in the data changes for an individual. Panel A plots the average employer contribution rate, total pension (i.e., employer + individual) contribution rate, and total savings rate, as a percentage of gross labor income. Total savings rate is defined as the change in assets plus total pension contributions, as a percentage of gross labor income. We include only workers experiencing at least a 3 percentage point increase in employer pension contribution rate at $t = 0$, and include only workers for whom data is available for event years [-5, +5] so that the sample is constant through the figure. Panel B replicates Panel A, further restricting to workers with positive individual pension contributions at $t = -1$. Panel C plots the fraction of individuals in Panel A that are at a corner in either pension contributions (defined as 0 individual pension contributions) or total savings (defined as assets less than 10% of income and at the corner in pension contributions) in each event year. Panel C also plots, in the dashed lines, the predicted increase in these fractions at $t = 0$ if individuals attempted to offset fully the changes to employer pensions in either individual pension contributions or total saving. We calculate this number for pensions as the fraction with an individual pension contribution rate at $t = -1$ less than or equal to the increase in the employer contribution rate at $t = 0$. We calculate this number for savings as the fraction with assets-to-income ratio less than or equal to 0.1 plus the increase in the employer contribution rate or with an individual pension contribution rate less than or equal to the increase in the employer contribution rate. The dashed line plots this constant fraction added to the level of the solid series at $t = -1$. Panel D plots a histogram of the change in individual pension contributions, as a fraction of lagged contributions, from $t = -1$ to $t = 0$, for individuals in Panel B.

FIGURE 2

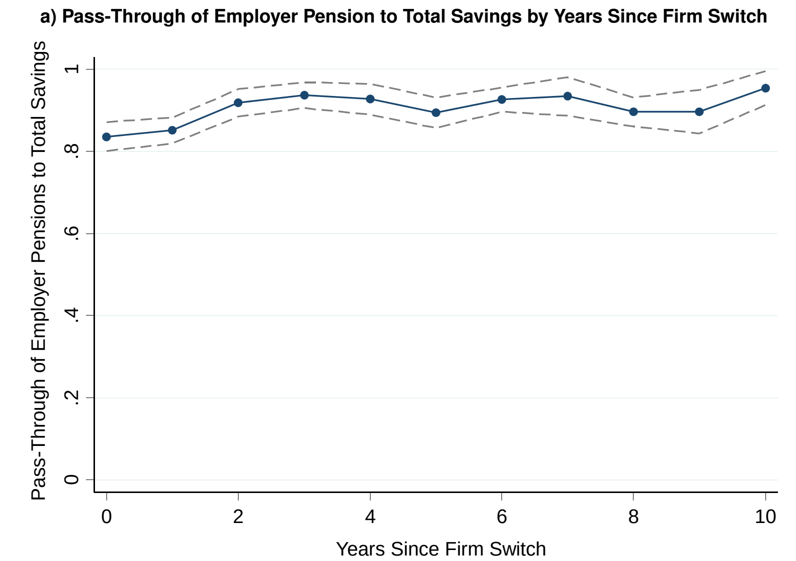
Changes in Saving vs. Changes in Employer Pension Contributions for Firm Switchers Making Individual Contributions Prior to Switch

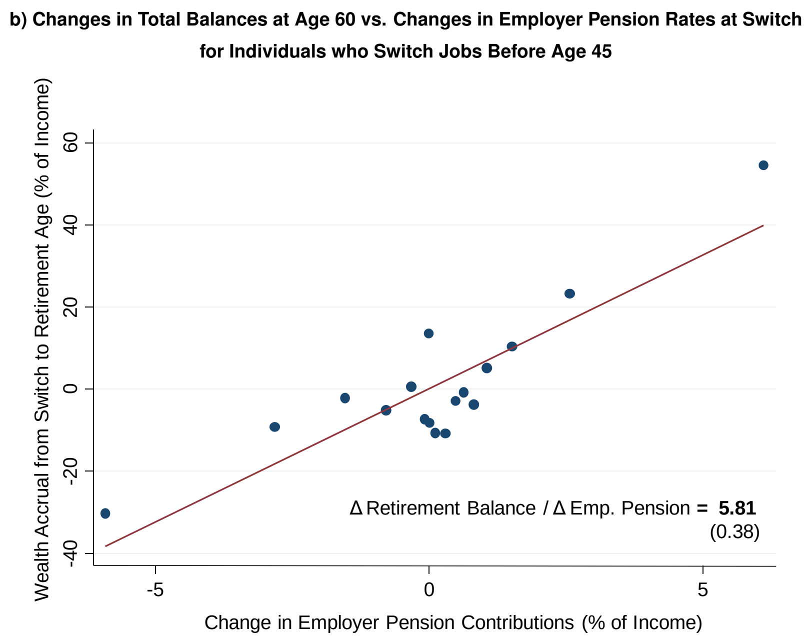


Notes: These figures display binned scatterplots that characterize the change in savings behavior at the time of a firm switch. To generate the binned scatterplot, we group the data into vingtiles (twenty equal-sized bins) on the x-axis variable. The dots represent the means of the y-axis and x-axis variables within each bin. The best-fit lines, as well as the coefficients and reported standard errors, are calculated from regressions on the micro-data, clustering standard errors by firm ID. These figures include all firm switches, defined as the primary firm ID changing from one year to the next for an individual. Panel A plots the change in the total pension contribution rate against the change in the employer pension contribution rate, for firm switches in which the individual was not at a corner in individual pension contributions in the pre-switch year. Panel B plots the change in total savings rate against the change in the employer pension contribution rate, for firm switches in which the individual was not at a corner in total savings in the year before the switch. See the notes to Figure 1 for details on the definitions of corners. The coefficients reported in Panels A and B can be interpreted as the pass-through rate of employer pension rate changes to total pension and savings. Panel C plots the change in total savings as a fraction of lagged income against the change in earnings as a fraction of earnings prior to the switch. The coefficient reported can be interpreted as the marginal propensity to save out of earnings.

FIGURE 3

Long Run Impacts of Employer Pensions on Wealth Accumulation

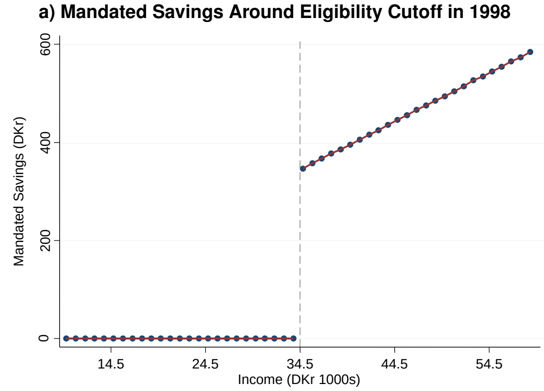


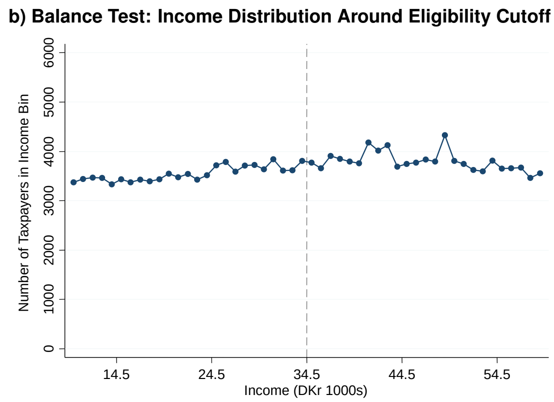


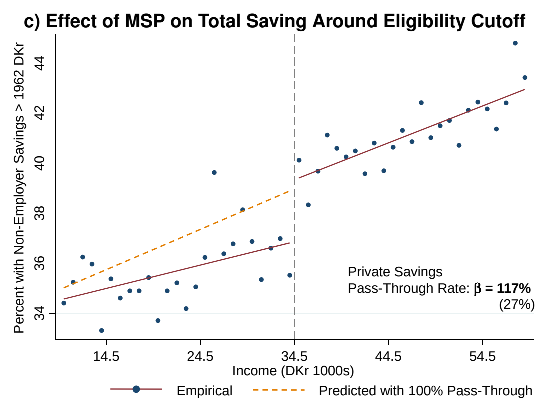
Notes: These figures present the long-term impacts of changes in employer pension contribution rates at the time of firm switches. Panel A plots the pass-through coefficients of changes in employer pension contribution rates to total savings at different horizons. We restrict to the first firm switch for each individual in our data. The dot at each event year t represents the coefficient from a regression of the change in total savings rates from event year -1 to event year t on the change in employer contribution rates over the same horizon, for individuals who are not at a corner in total savings in event year -1, using the same specification as in Columns 6 and 7 of Table 3A. The dots at $t = 0$ and $t = 5$ thus match the coefficients in those columns. The dashed lines represent the boundaries of the 95% confidence interval, using standard errors estimated from a regression that clusters on the firm to which the individual switches. Panel B is a binned scatter plot of the relationship between the change in employer contribution rates and total wealth accrued between the firm switch and age 60, for individuals whose first firm switch occurs between ages 45 and 55. Total wealth accrued is calculated by summing total savings over the years between the firm switch and age 60. The best-fit line, as well as the coefficient and reported standard error, is calculated from a regression on the micro-data, clustering standard errors by the firm to which the individual switches. See notes to Figure 2 for further details on construction of the binned scatter plot.

FIGURE 4

Impact of Mandated Savings Plan: Regression Discontinuity Design



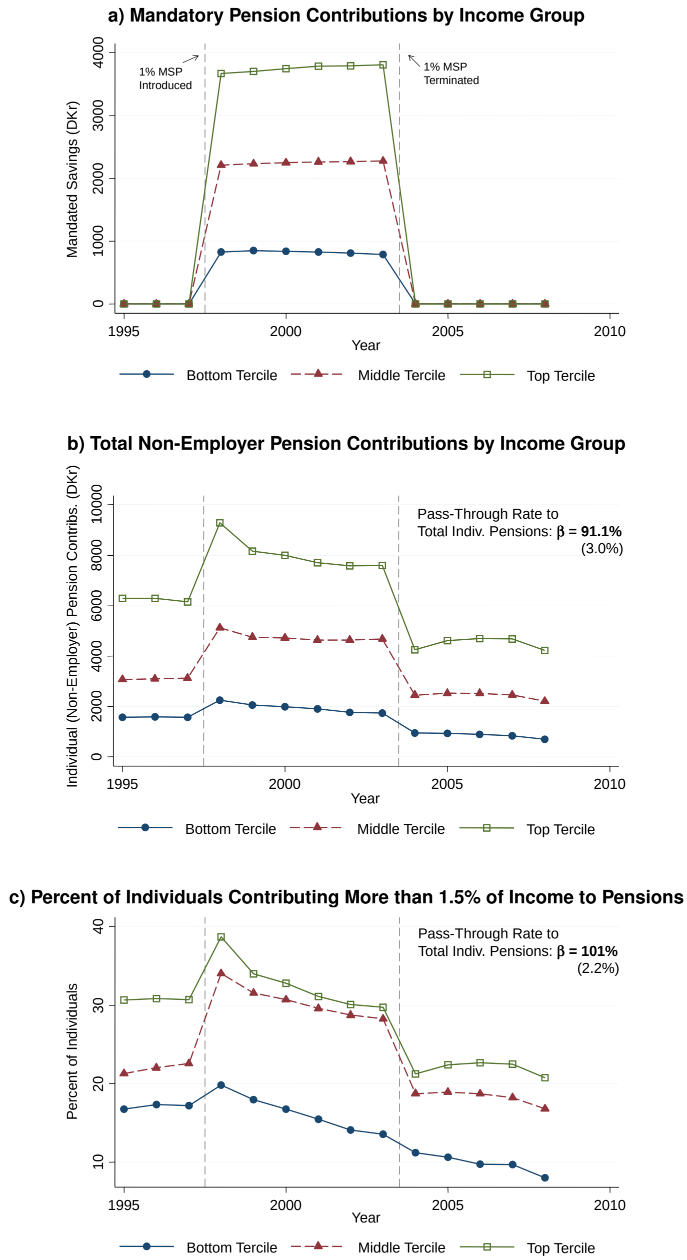




Notes: These figures present a regression discontinuity analysis of the impact of the Mandated Saving Program (MSP) on total savings in 1998. All panels present the data in DKr 1,000 income bins relative to the threshold, so that the dot at DKr -500 includes all individuals with income in the range [-1000,0). Panel A shows the contributions mandated by the program. Individuals with income below DKr 34,500 were not required to make any contributions; those earning more than this threshold were required to contribute 1% of income. Panel B plots the count of individuals in each bin around the threshold. Panel C plots the fraction of individuals in each bin with total individual savings (i.e., individual pension contributions + MSP + taxable saving) above DKr 1,962, which is the mean total individual savings for those within DKr 5,000 of the threshold. The solid lines plot the linear best-fit for those data above and below the threshold. In order to calculate the pass-through coefficient, we mechanically add 1% of income to total individual savings for observations below the threshold, obtaining the counterfactual shown by the dashed best-fit line. Letting β_1 denote the estimated coefficient for size of the actual discontinuity and β_2 the estimated coefficient for the discontinuity estimated using the counterfactual (dashed) line, we calculate pass-through as $\beta_1/(\beta_1 - \beta_2)$. We calculate the standard error from an analogous regression on the micro-data, clustered by DKr 1,000 income bin.

FIGURE 5

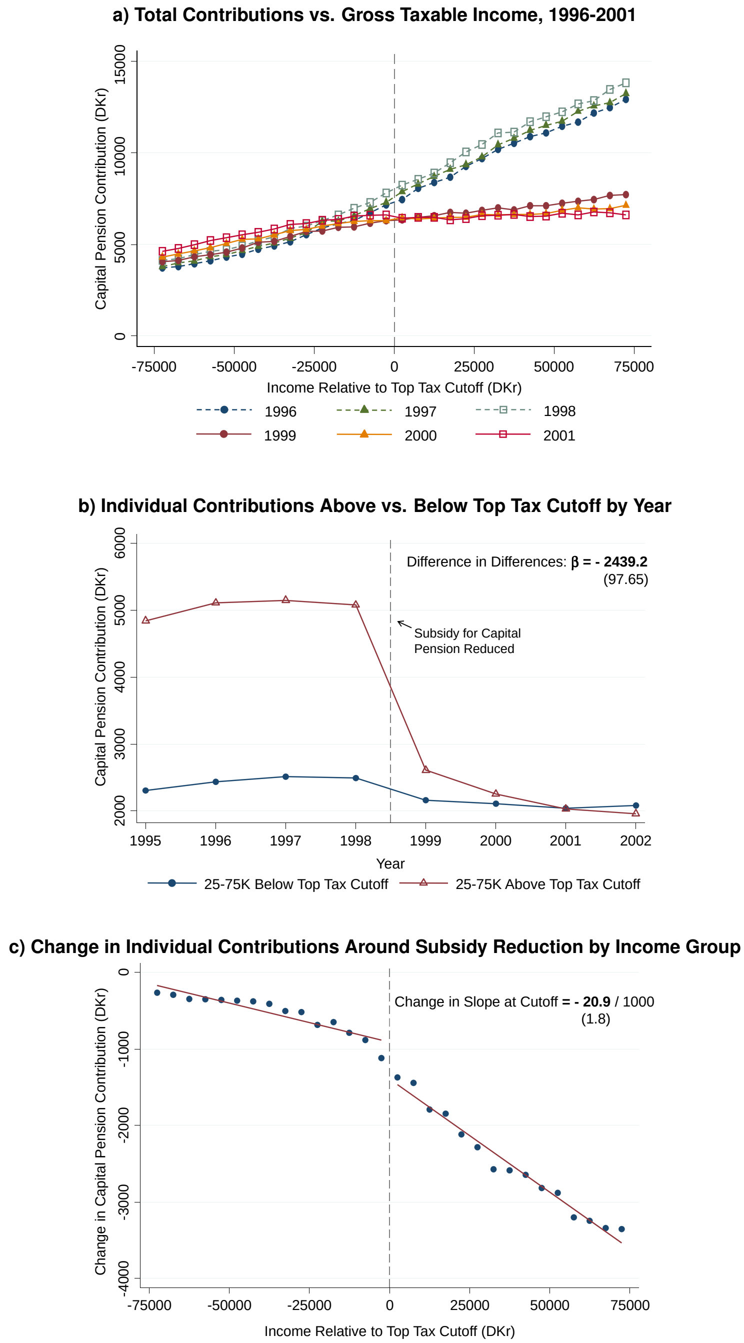
Impact of Mandated Savings Plan: Difference-in-Differences Design



Notes: These figures present the effect of the Mandated Savings Plan (MSP) on total non-employer pension contributions. In all three panels, we split the data into terciles based on income in each year. Individuals may therefore switch groups across years. We include only individuals with positive income in each year. Panel A plots the average contribution to the MSP in each year for these three groups. Panel B plots the average total non-employer pension contribution (MSP + individual pension contribution) in each year for the three groups. Panel C plots the fraction of individuals in each group with total non-employer pension contributions greater than 1.5% of income, which is the mean total non-employer contribution rate for the sample across all years. In Panels B and C, we present estimates of pass-through from MSP contributions to total non-employer pension contributions. We calculate the coefficient in Panel B by grouping the data into DKr 25,000 bins and regressing the mean change in average non-employer pension contributions on the mean change in MSP contributions in each bin using the specification in equation (6). We calculate the coefficient in Panel C from an analogous regression of the change in the fraction above the threshold on the fraction predicted to cross the threshold under full pass-through (calculated as the fraction with non-employer pension contributions between 0.5% and 1.5% of income in 1997 and between 1.5% and 2.5% of income in 2003). We instrument for the predicted fraction with the average change in MSP contributions in those years.

FIGURE 6

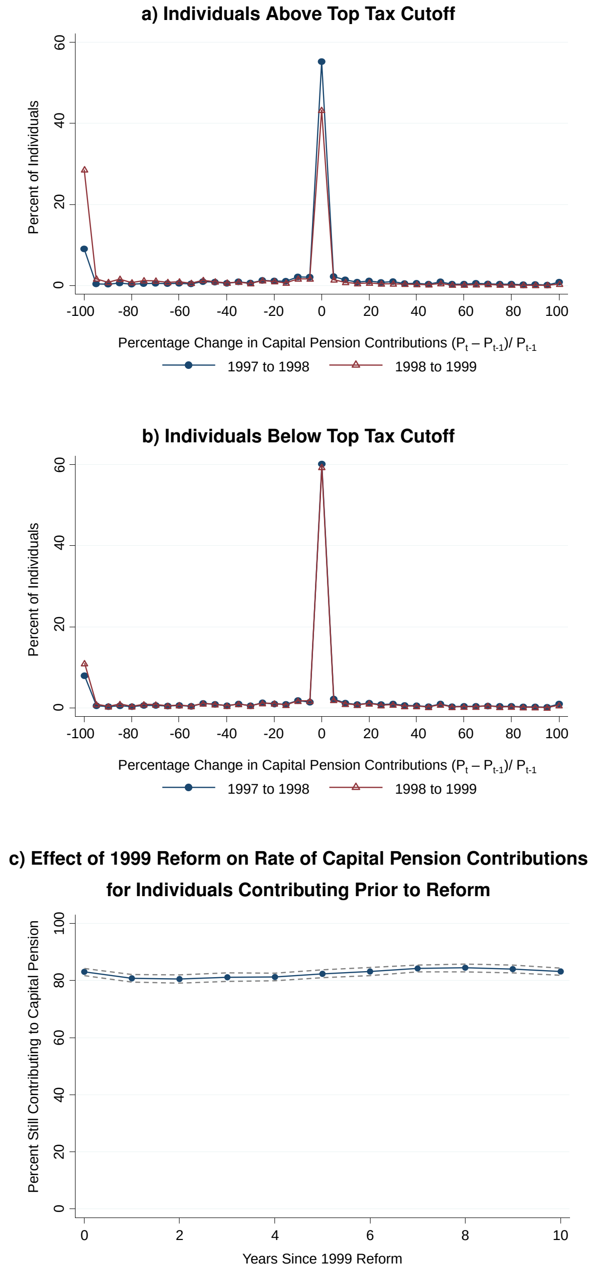
Impact of Subsidy Reduction On Capital Pension Contributions



Notes: These figures present the impact of the 1999 capital pension subsidy reduction on capital pension contributions. Panel A plots average total (individual plus employer) capital pension contributions for individuals with income in each DKr 5,000 income bin within DKr 75,000 of the top tax threshold, in each year 1996-2001. Panel B plots average individual capital pension contributions in each year for two income groups: those with income in the range DKr 75,000 to DKr 25,000 below the top tax threshold (control group), and in the range DKr 25,000 to DKr 75,000 above the top tax threshold (treatment group). Panel C plots average individual capital pension contributions in the post-reform years (1999-2001) minus pre-reform years (1996-1998) in each DKr 5,000 income bin. We then plot a linear fit to those data below the kink and above the kink. The coefficient for the change in slope at the threshold (shown per DKr 1000 of income), as well as the best-fit lines, are estimated using the regression kink specification in Column 3 of Table 5. The standard error for the slope change is clustered by DKr 5,000 income bin.

FIGURE 7

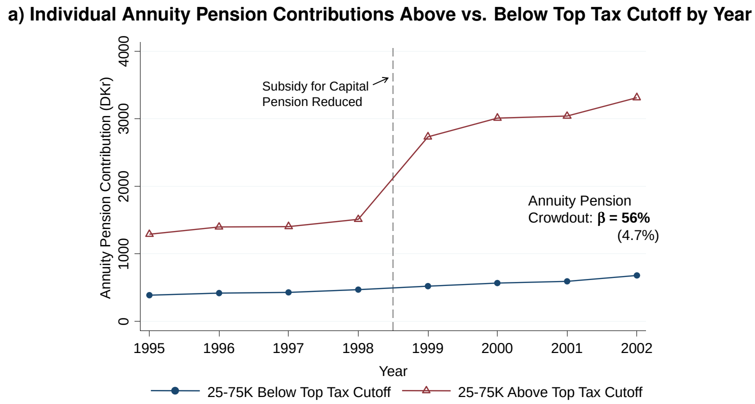
Impact of Subsidy Reduction on Distribution of Changes in Individual Capital Pension Contributions

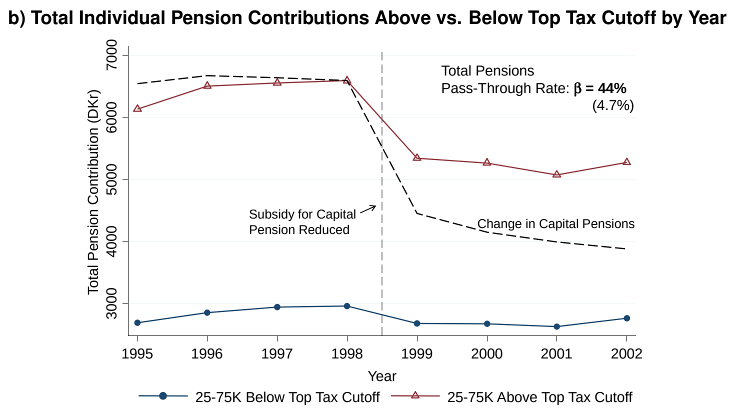


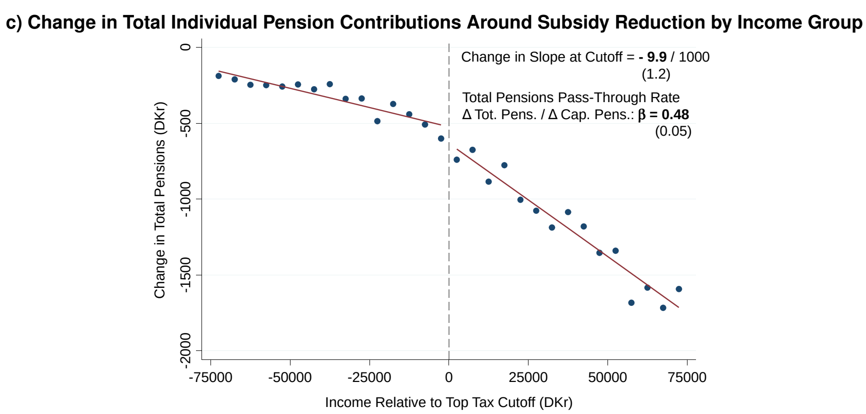
Notes: Panel A plots the distribution of changes to individual capital pension contributions, as a fraction of lagged individual pension contributions, for individuals who are above the top tax cutoff in 1998 and 1999 (the treatment group). Panel B replicates Panel A for the below the top tax cutoff (the control group). Both panels include only individuals with positive lagged individual pension contributions. The dots represent the floor of DKr 5,000 income bins, so that the dot at 0% represents individuals with changes in the range [0%, 5%). Panel C shows the long-term dynamics of response to the 1999 reform for those who were contributing to capital pensions in 1998. To construct this figure, we first calculate the fraction of individuals with positive individual capital pension contributions in each post-reform year in the treatment and control groups. We then plot the difference between this fraction in the treatment and control groups and add 1 to facilitate interpretation of the scale. For instance, the dot at 0.83 in year 0 implies that the reform induced 17 percent of those contributing in 1998 to stop contributing in 1999. The dashed lines represent the boundaries of the 95% confidence interval, estimated from a DD regression analogous (7) with standard errors clustered at the DKr 5,000-income-bin-by-year level.

FIGURE 8

Crowd-out Within Retirement Accounts Induced by Subsidy to Capital Pensions



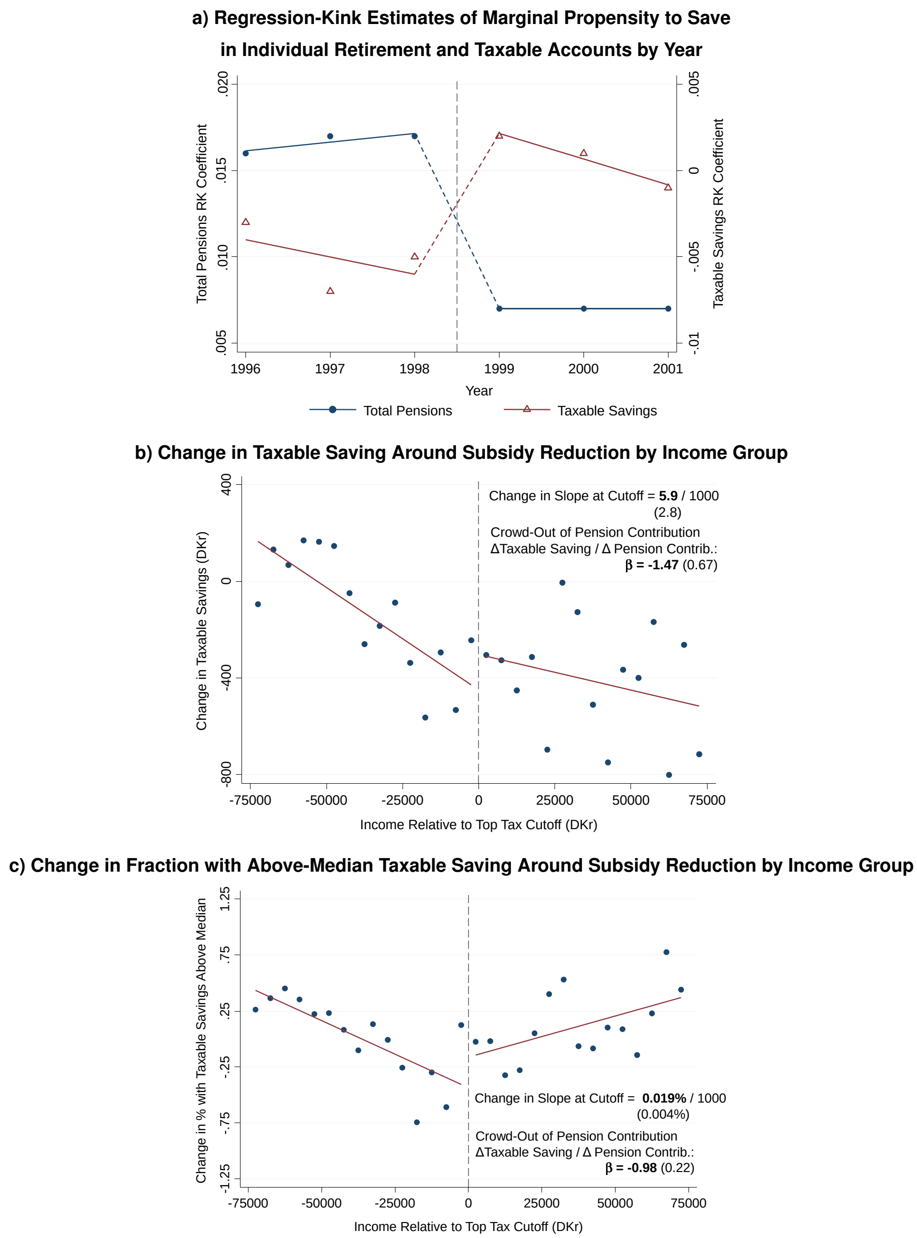




Notes: Panel A replicates Figure 6b, plotting mean individual annuity pension contributions in the treatment and control groups by year. Panel B replicates Panel A, instead plotting total voluntary individual pension contributions, i.e. capital plus annuity pensions excluding the MSP. Panel C replicates Figure 6c, plotting mean total voluntary individual pension contributions in the post-reform years (1999-2001) minus pre-reform years (1996-1998) in each DKr 5,000 income bin. See the notes to Figure 6 for details on the construction of these figures. We estimate the crowd-out coefficients in Panel A and B using the DD specifications in Columns 1 and 2 of Table 6A. The standard errors are clustered at the DKr 10,000-income-bin-by-year level. We estimate the crowd-out coefficient in Panel C using the RK specification in Column 2 of Table 6B. The standard errors are clustered by DKr 5,000 income bin.

FIGURE 9

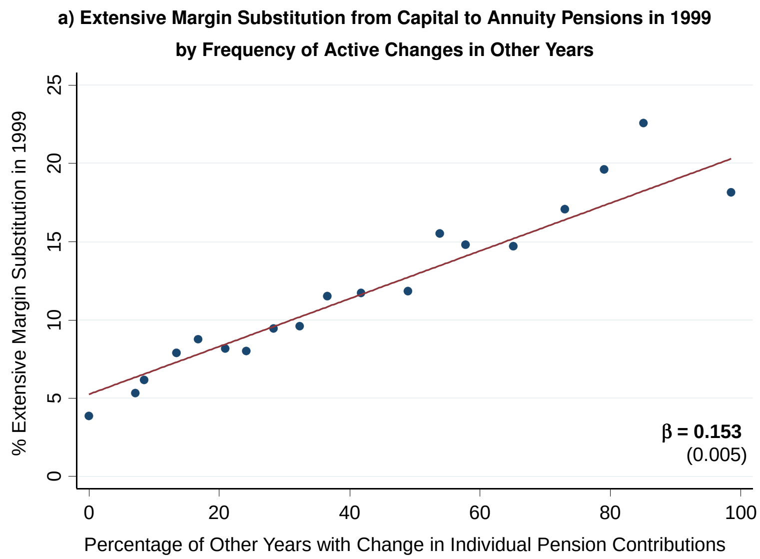
Crowd-out of Taxable Saving Induced by Subsidy

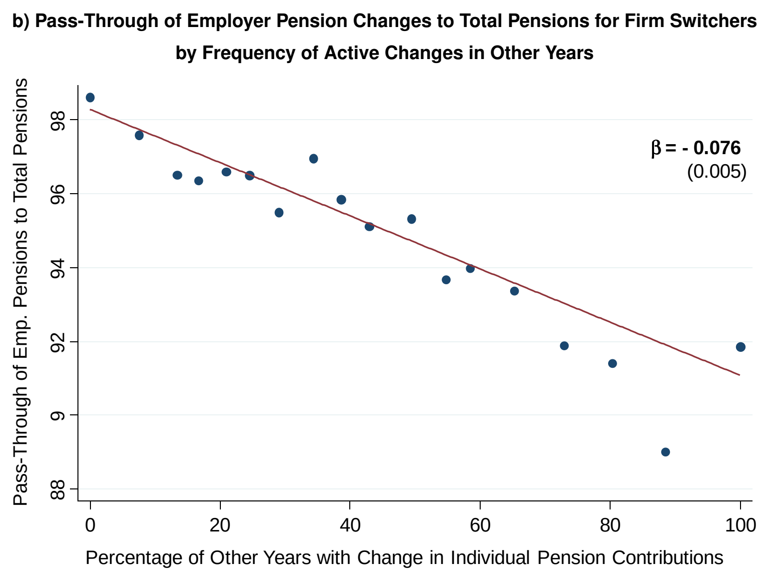


Notes: Panel A plots RK coefficients estimated using the specification in equation (10) separately for each year from 1996-2001. The coefficients can be interpreted as the change in the marginal propensity to save in retirement accounts (circles) and taxable accounts (triangles) at the top tax cutoff. The solid lines are best-fit lines for the coefficients before and after 1999. Panel B replicates Figure 6c, plotting mean saving in taxable accounts in the post-reform years (1999-2001) minus pre-reform years (1996-1998) in each DKr 5,000 income bin. Panel C plots the fraction who save more than DKr 688 in taxable accounts (median taxable saving for those within DKr 5,000 of the threshold) in the post-reform years (1999-2001) minus pre-reform years (1996-1998) in each income bin. See the notes to Figure 6 for details on the construction of these figures. The crowd-out coefficients are estimated using the specifications in Columns 4 and 5 of Table 6, Panel B. In particular, the estimate reported in Panel B is the coefficient from a 2SLS regression of taxable saving on total individual pensions, with total pensions instrumented by the triple interaction term in equation (8) divided by 0.4 (the net-of-tax rate for those affected by the reform). The estimate in Panel C is the coefficient from an analogous regression with an indicator for having above-median taxable saving. This coefficient is divided by 0.4 times the fraction of individuals with taxable saving between the median and DKr 500 above the median, times 500 (the width of the window). The standard errors are clustered by DKr 5,000 income bin.

FIGURE 10

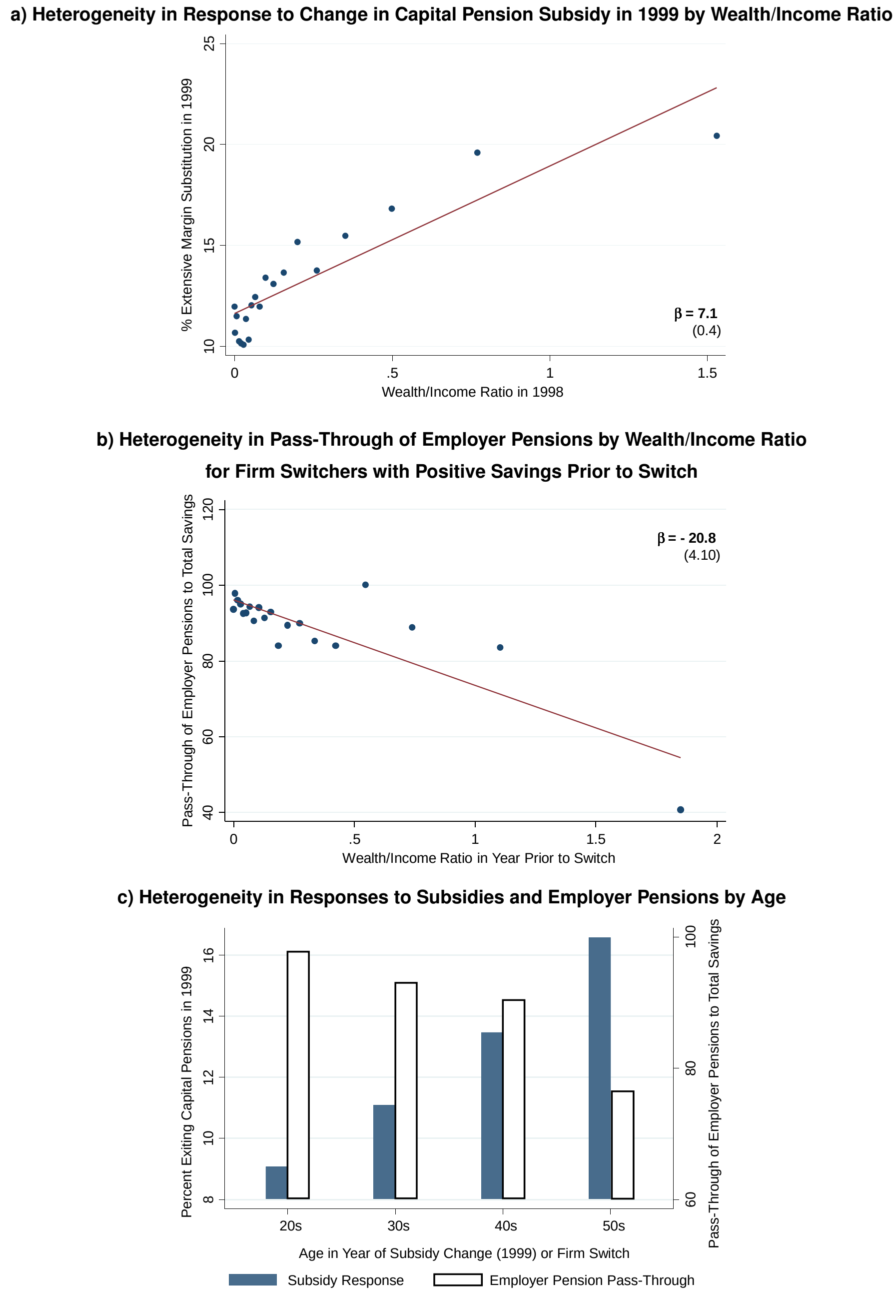
Active vs. Passive Choice and Responses to Subsidies and Employer Pensions





Notes: The y variable in Panel A is the fraction of individuals in the treatment group in 1999 who substitute on the extensive margin (i.e. exit capital pension contributions and raise annuity pension contributions) when the capital pension subsidy is reduced. The x variable is the fraction of other years in which contributors changed their contribution levels (to either capital or annuity accounts) by more than DKr 500. We exclude years in which individuals made zero contributions in both the prior year and current year when calculating this fraction. To construct Panel A, we first group the x axis into vingtiles. This yields 19 bins because of mass points in the distribution. The dots on the figure are the means of the y axis and x-axis variables within each bin. The best-fit line is calculated from a regression on the micro-data (shown in Table 7, Column 3). Panel B plots the pass-through of changes to the employer pension contribution rate to changes in the total pension contribution rate at the time of a firm switch against the same x axis variable as in Panel A. To construct this figure, we again begin by splitting the x axis variable into vingtiles, which yields 18 distinct bins in this sample. We then replicate the regression in Table 3A, Column 1 within each bin and plot the estimated coefficient. The best-fit line across these points is calculated using the specification shown in Column 5 of Table 7.

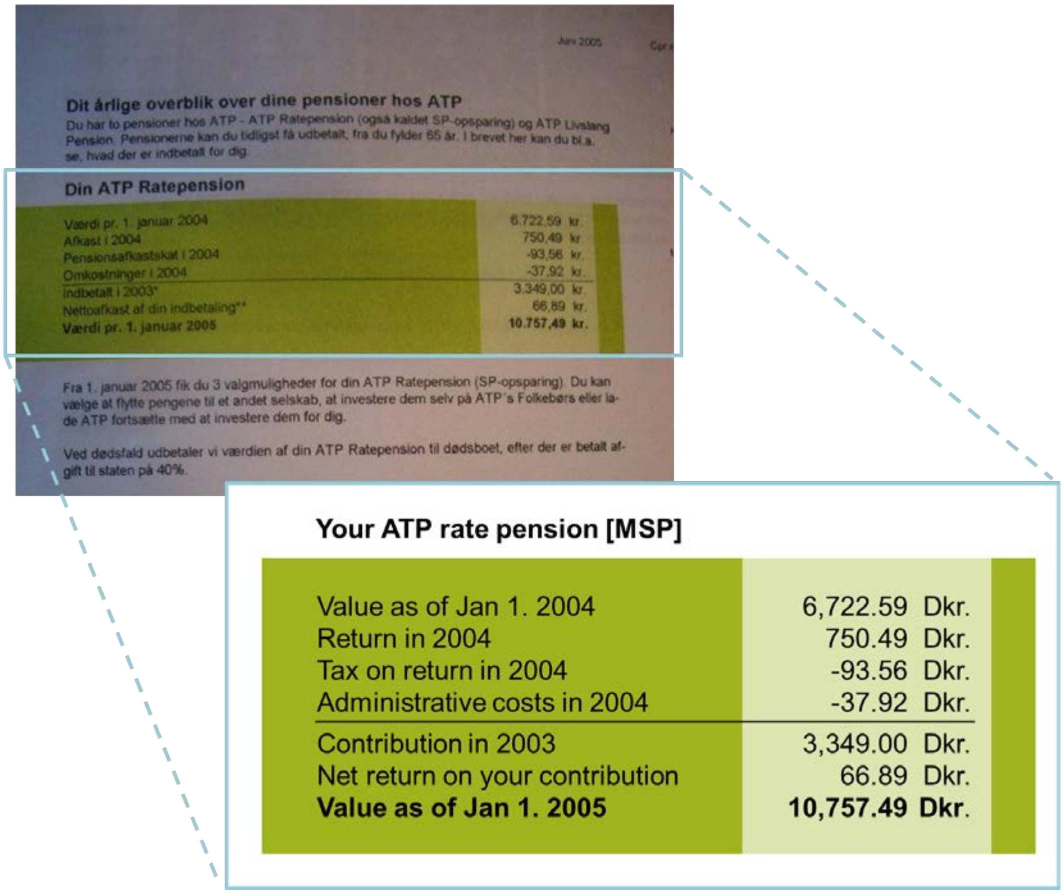
FIGURE 11
Heterogeneity in Responses by Observable Characteristics



Notes: These figures show heterogeneity in responses to the 1999 capital pension subsidy reduction and changes in employer pension contributions. Panel A plots the fraction of workers in the treatment group who substitute on the extensive margin, i.e. exit capital pensions and raise annuity pension contributions, against their wealth-to-income ratio in 1998. To construct the figure, we first group the data into vingtiles based on lagged wealth-to-income ratio. The dots represent the means of the y axis and x axis variables within each bin. The best-fit line is calculated from a regression of extensive margin substitution on lagged W/Y in the micro-data (shown in Table 8A, Column 1). Panel B plots the pass-through of changes to the employer pension contribution rate to changes in the total savings rate at the time of a firm switch against lagged W/Y. We first group the data into vingtiles based on W/Y in the year before the firm switch. We then replicate the regression in Table 3A, Column 2, within each bin and plot the estimated coefficients. The best-fit line is calculated using the specification in Column 1 of Table 8B. Panel C replicates Panels A and B, cutting on age deciles (in 1999 or the year before the firm switch). The two sets of bars plot the rate of extensive margin substitution in response to the 1999 reform (left bars, left y-axis) and the pass-through of changes to the employer pension contribution rate to changes in the total savings rate (right bars, right y-axis). Pass-through is estimated separately for each age bin using the specification in Column 2 of Table 3A.

APPENDIX FIGURE 1

Mandated Savings Account Balance Notification Letter



Juni 2005

Dit årlige overblik over dine pensioner hos ATP

Du har to pensioner hos ATP - ATP Ratepension (også kaldet SP-opsparing) og ATP Livslang Pension. Pensionerne kan du tidligst få udbetalt, fra du fylder 65 år. I brevet her kan du bl.a. se, hvad der er indbetalt for dig.

Din ATP Ratepension

Værdi pr. 1. januar 2004	6.722,59 kr.
Afkast i 2004	750,49 kr.
Pensionsafkastskat i 2004	-93,56 kr.
Omkostninger i 2004	-37,92 kr.
Indbetalt i 2003*	3.349,00 kr.
Nettoafkast af din indbetaling**	66,89 kr.
Værdi pr. 1. januar 2005	**10.757,49 kr.**

Fra 1. januar 2005 fik du 3 valgmuligheder for din ATP Ratepension (SP-opsparing). Du kan vælge at flytte pengene til et andet selskab, at investere dem selv på ATP's Folkebørs eller lade ATP fortsætte med at investere dem for dig.

Ved dødsfald udbetaler vi værdien af din ATP Ratepension til dødsboet, efter der er betalt afgift til staten på 40%.

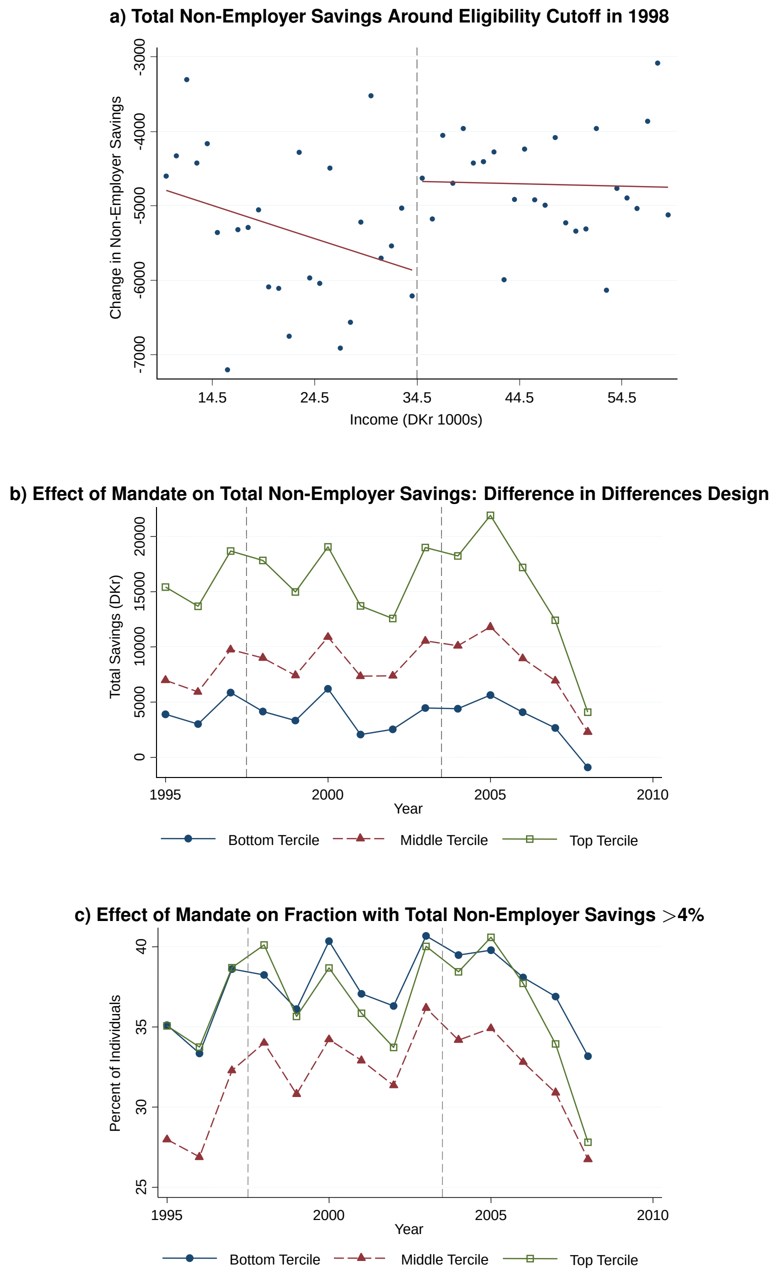
Your ATP rate pension [MSP]

Value as of Jan 1. 2004	6,722.59 Dkr.
Return in 2004	750.49 Dkr.
Tax on return in 2004	-93.56 Dkr.
Administrative costs in 2004	-37.92 Dkr.
Contribution in 2003	3,349.00 Dkr.
Net return on your contribution	66.89 Dkr.
Value as of Jan 1. 2005	**10,757.49 Dkr.**

Notes: This figure presents a pension balance notification letter sent to a Danish citizen in 2004. These letters were sent annually by ATP, Denmark's largest pension company, giving citizens information about the balance in their mandated savings account.

APPENDIX FIGURE 2

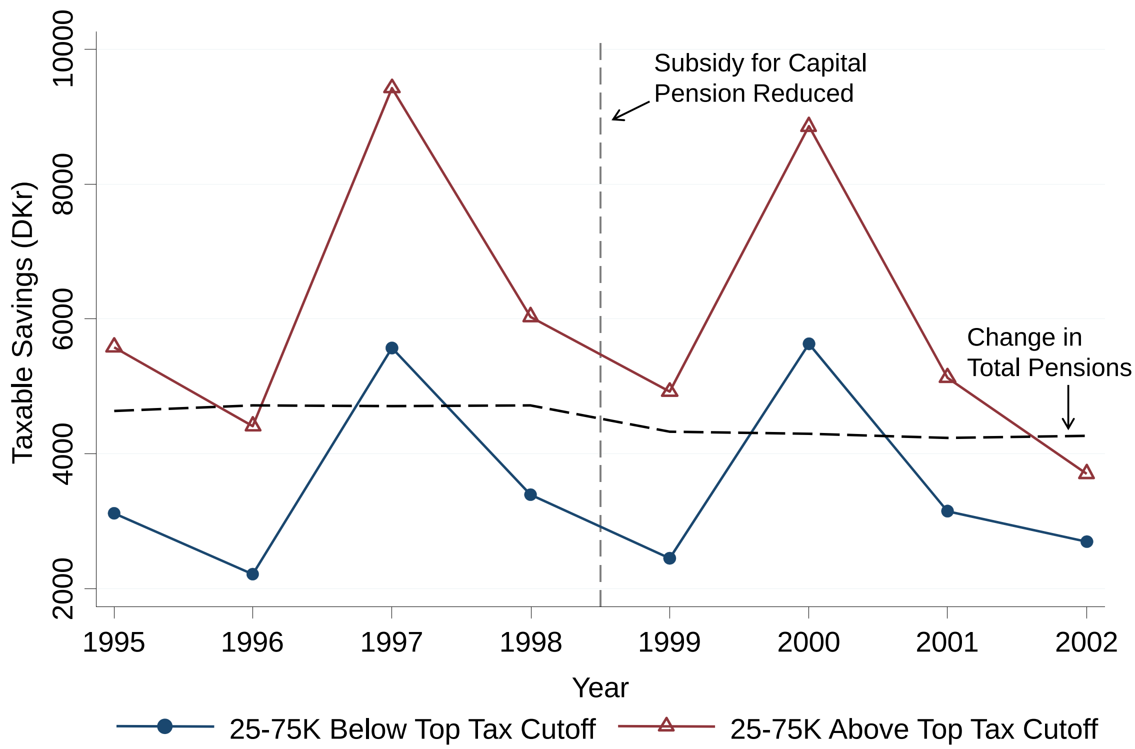
Mandated Savings Plan: Impacts on Total Savings



Notes: These figures present the impact of the Mandated Saving Program (MSP) on total non-employer savings in 1998. Panel A replicates the regression discontinuity in Figure 4c, plotting the mean change in total non-employer savings from 1997 to 1998 within DKr 1,000 income bins. Panel B replicates the difference-in-differences design in Figure 5b, plotting the average of total non-employer savings in each income tercile for each year. Panel C replicates the thresholds difference-in-differences design in Figure 5c, plotting the fraction of individuals in each income tercile with total non-employer savings greater than 4% of income, which is the mean total non-employer savings rate for the sample population across all years.

APPENDIX FIGURE 3

Impact of 1999 Subsidy Change on Taxable Saving: Difference-in-Differences Design



Notes: This figure replicates Figure 6b, plotting mean taxable saving levels in the treatment and control groups by year. As a reference, the dashed line shows the difference between the treatment and control group means of total pensions from Figure 8b multiplied by 0.4, the net-of-marginal-tax rate.