ACTIVE VS. PASSIVE DECISIONS AND CROWD-OUT IN RETIREMENT SAVINGS ACCOUNTS: EVIDENCE FROM DENMARK*

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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 savings for the population of Denmark. We find that a policy's impact on total savings depends critically 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 savings automatically even if individuals take no action – such as employer-provided pensions or automatic 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 savings of passive individuals who do not reoptimize. We estimate that 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. These individuals also offset changes in automatic contributions and have higher wealth-income ratios. We conclude that automatic contributions are more effective at increasing total retirement savings 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.

Our results connect to the literature in macroeconomics showing that consumption is excessively sensitive to current income. One well known explanation for excess sensitivity is a model of "spenders" and "savers," 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 inform this work in three ways. First, they confirm that the marginal propensity to consume out of disposable income is high and that most individuals are rule-of-thumb spenders. In particular, the finding that the tax-financed savings mandate raised total savings directly implies that tax increases

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lower current consumption. Second, our findings suggest that heterogeneity in excess sensitivity across households may be partly driven by financial sophistication and planning. Finally, we find much greater heterogeneity in the degree of active response by wealth than by income, suggesting that fiscal stimulus targeted toward lower wealth households may have greater impacts on aggregate consumption.

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 a lack of suitable 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 pension savings 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]

We structure our empirical analysis around the comparative static predictions of the model. The model predicts that automatic contributions should have no impact on total savings for active savers: they fully offset the automatic contribution by reducing voluntary pension contributions provided that they are not at a corner. In contrast, the impact of automatic contributions on total savings 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 savings 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 savings. 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. Importantly, price subsidies only affect the behavior of active savers, who pay attention to the price subsidy. Because such attentive individuals are likely to optimize asset allocation across their accounts, there is substantial scope for crowd-out of

[1]Such passive behavior can be micro-founded in several ways, including fixed costs of attention or present-biased preferences (Carroll et al., 2009). Since our results do not depend on the specific source of passive behavior, we do not take a stance on the microfoundation for such behavior here.

the increased retirement savings induced by price subsidies.

We analyze the impacts of price subsidies and automatic contributions on savings behavior empirically using Danish income tax records.[2] 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 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 sample of individuals that is much larger than those used in recent studies (e.g., Gelber, 2011), which have been constrained by small survey datasets that typically have less than a thousand observations in their analysis samples. Second, there were a series of sharp reforms in Denmark that provide quasi-experimental research designs to analyze the impacts of retirement savings policies on savings behavior.

We divide our empirical analysis into three sections. First, we analyze the impacts of employer-provided pensions and government mandates, both of which are "automatic contributions" in the sense that they affect savings levels even if individuals take no action. Using event studies of individuals who switch firms, we find that individuals' total savings rates rise immediately by 90 cents when they move to a firm with $1 larger employer-provided pension contributions even if they could have fully offset the increased contribution by the firm.[3] Most individuals do not change voluntary pension contributions or savings in taxable accounts at all when they switch firms, consistent with passive behavior. The degree of pass-through remains similar 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 degree of pass-through also remains equally high for relatively large changes in employer-provided pensions (such as increases or decreases of 5% of earnings) and the changes in savings behavior persist for ten years after the firm switch, ultimately resulting 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 savings in 1998 and sharp reductions in total savings in 2004. On

[2]Although the variation in our data is in the form of employer-provided pensions and a mandatory government savings plan, we show that the essential feature of these policies is that they increase savings without requiring any action by individuals. Hence, we expect our results to translate to other policies such as defaults and automatic enrollment that share this feature.

[3]Throughout our empirical analysis, we remove mechanical effects due to corners by focusing either on individuals who were already saving more than the change in employer-provided pensions or using statistics such as the fraction contributing above thresholds that are not affected by corners.

average, a $1 increase in the MSP raises aggregate savings roughly 1-for-1. The MSP raised total savings 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 crowdout and increase total wealth accumulation significantly, suggesting that many individuals are passive savers who reduce consumption when their disposable income is reduced.

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 out as a lump sum and taxed at a flat rate upon retirement and annuity pensions that are paid out as annuities and taxed as income. In 1999, the government reduced the subsidy for contributing to capital pension accounts by 14 cents per 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 the other type of pension account (annuity pensions) was unchanged.[4] Using difference-in-difference 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. Most individuals 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 result again supports the view that the majority of individuals are passive savers.

Next, we investigate the extent to which the changes in pension contributions among the 17% of active savers led to changes in total wealth accumulation. We estimate two crowdout 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 60 cents of each $1 withdrawn from capital pension accounts is shifted to annuity pension accounts, leading to a net reduction in total pension contributions of 40 cents. Second, focusing on the reduction in total pension contributions, we estimate that 99 cents of each $1 withdrawn from pension accounts is deposited in a taxable savings account. This latter crowdout parameter is directly relevant for determining the overall

[4]The capital pension retained a tax advantage even after the 1999 reform – and hence the frontier of the budget set shifted inward for most individuals. As an analogy to the U.S., suppose that 401(k)'s start with a significant tax advantage relative to IRA's. In this setting, the reform we study effectively makes the 401(k) less attractive but leaves the tax treatment of the IRA unchanged.

impact of retirement savings subsidies on total savings and can be compared to estimates in the prior literature (e.g., Engen, Gale and Scholz, 1996; Poterba, Venti and Wise, 1996). We conclude that the few active savers who respond to the price subsidy do so primarily by shifting assets across accounts rather than reducing consumption. As a result, tax subsidies have much smaller impacts on total wealth accumulation than automatic contributions.

In the third part of our empirical analysis, we investigate heterogeneity in responses across individuals to test if automatic contributions and subsidies have different effects because of active vs. passive choice. 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 shows that individuals are more responsive to incentives when they are currently making active choices, consistent with prior evidence on inertial behavior in other domains (Samuelson and Zeckhauser, 1988; Ericson, 2012). We also find substantial heterogeneity across individuals in the degree of active response. Individuals who change their pension contributions more frequently in other years are also more likely to reduce capital pension contributions when the subsidy for capital pensions was reduced in 1999. These active optimizers are also more likely to offset changes in automatic contributions by changing voluntary pension contributions. Most interestingly, active savers – those who respond more to price subsidies and undo the effects of automatic contributions to a greater degree – are better prepared for retirement to begin with, in the sense that they have significantly higher wealth/income ratios. We also find that older individuals are more likely to be active savers. In sum, the patterns of heterogeneity closely match what a model of active vs. passive choice predicts: individuals who make active savings choices are more responsive to price subsidies *and* less responsive to changes in automatic contributions. These results support the view that the degree of active vs. passive choice is a key mediator of the impacts of retirement savings policies on wealth accumulation.

Overall, our analysis suggests that tax subsidies may be less effective in increasing retirement savings than policies such as defaults or automatic enrollment in employer-provided pensions for three reasons. First, subsidies are largely an infra-marginal transfer to agents already saving for retirement because few agents actively change retirement contributions because of a subsidy. Second, the active savers who do increase retirement contributions are likely to optimize by shifting assets across accounts, leading to relatively small impacts on total savings. Because of these two factors, we estimate that a $1 of expenditure by the government on subsidies for retirement savings raises total savings by less than 1 cent on average, with an upper bound on the 95% confidence interval

of 19 cents. Third, individuals who actively respond to subsidies tend to be those who are already planning and saving for retirement. Hence, subsidies do not target those with little retirement savings, for whom improving retirement security is perhaps most important. In contrast, policies that influence the behavior of passive savers have little infra-marginal cost, generate relatively little crowd-out, and have the largest impacts of individuals who are paying the least attention to saving for retirement.

Our analysis contributes to and builds on two large literatures: research in public finance analyzing crowd-out in retirement savings accounts and research in behavioral economics comparing the impacts of defaults, matches and other policy tools on savings within retirement accounts. While these literatures have developed independently, our results show that there is a deep connection between these two 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 savings. 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 due to company policies may induce little active decision making and generate minimal crowdout, while changes in tax incentives or programs that require voluntary participation could induce more substitution.[5] Indeed, some of these results were foreshadowed in early work by Cagan (1965) and Green (1981), who argued using cross-sectional survey data that many individuals were unaware of the details of their employer pension policies and that increases in employer pensions resulted in 1-1 increases in total savings.

The behavioral economics literature has shown that defaults and, to a lesser extent, salient price subsidies significantly increase savings *within* retirement accounts (e.g., Madrian and Shea, 2001; Duflo et al., 2006; Card and Ransom, 2011). Importantly, however, this prior work has not investigated whether defaults raise *total* savings. As we show in our stylized model, the impacts of these policies on total savings depend fundamentally on how consumers adjust their budgets when they recognize that they have less disposable income. There is no theoretical reason to expect

[5]In addition, Gelber (2011) analyzes an anticipated change in eligibility for tax-deferred savings accounts, which could generate different behavioral responses than the permanent changes we study here.

that the adjustment occurs by reducing consumption instead of non-retirement saving. Our finding that policies that change savings passively do raise total savings thus significantly strengthens the argument for policies such as automatic enrollment and defaults (e.g., Carroll et al., 2009; Madrian, 2012).

Our results also connect to the literature in macroeconomics showing that consumption is excessively sensitive to current income (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 inform this work in three ways. First, they confirm that the marginal propensity to consume out of disposable income is high and that most individuals are rule-of-thumb spenders. In particular, the finding that the tax-financed savings mandate raised total savings directly implies that tax increases lower current consumption. Second, our findings suggest that heterogeneity in excess sensitivity across households may be partly driven by financial sophistication and planning. Finally, we find much greater heterogeneity in the degree of active response by wealth than by income, suggesting that fiscal stimulus targeted toward lower wealth households may have greater impacts on aggregate consumption.

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 that we use to test the model's predictions. 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.

II Conceptual Framework

In this section, we present a stylized two-period model of savings behavior to structure our empirical analysis. We first set up the model and then analyze its comparative statics with respect to changes in government policies.

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 pension account or a taxable savings account. Let r denote the net-of-tax interest rate that individuals earn in their

taxable savings account. The government implements two policies with the goal of increasing total retirement savings: an automatic (mandated) contribution to the pension account of M and a price subsidy for saving in a retirement savings accounts that increases the return in those accounts to $r + \theta$. For simplicity, we assume that the price subsidy is financed by a tax on future generations or other agents outside the model.[6]

Let P denote voluntary contributions to the retirement account, M the automatic (mandatory) contribution to the retirement account, and S taxable savings. To eliminate mechanical effects of changes in M that force indivdiuals to save more, we abstract from corners and assume that S and P can be negative.[7] Consumption in the two periods is given by

$$c_1(S, P) = W - S - M - P \tag{1}$$

$$c_2(S, P) = (1 + r)\, S + (1 + r + \theta)(M + P).$$

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 maintain some savings outside retirement accounts despite their tax disadvantage. To model the cost of illiquidity, we assume that there is a concave benefit $\phi(S)$ of saving in the non-retirement account; for instance, one may think of this benefit as the reduced-form value of having liquid capital in the event of a negative shock.[8] Accounting for this benefit, individuals have utility

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

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.[9] 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

[6] 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.

[7] In our empirical analysis, we always focus on cases where an individual is not at a corner.

[8] 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 despite the tax disadvantage of doing so. Our model can be loosely interpreted as a reduced-form of the Gale and Scholz model.

[9] We define "active" and "passive" savers based upon how they respond to incentives rather than exogenous characteristics. Correspondingly, in our empirical analysis, we identify the two types of savers based on behavioral responses to policy changes and then correlate various observable characteristics with these measures of response to understand which types of individuals are active and passive savers.

model. Passive savers set retirement contributions at an exogenous level $P = \bar{P}_i$ that does not vary with M and θ. Following recent work in behavioral public finance (Bernheim and Rangel, 2009; Chetty, Looney and Kroft, 2009), we not specify a positive model of passive choice, as the questions we seek to answer – understanding the impacts of changes in government policies on total savings – do not depend on how $\bar{P}_i$ is set. There are several models in the literature for why individuals' retirement savings plans are insensitive to incentives, including fixed costs of adjustment that generate inertia, hyperbolic discounting that leads to procrastination in updating plans (Carroll et al., 2009), and a lack of information or inattention. The results that follow do not depend upon which of these micro-foundations drives passive behavior.

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 savings 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 and instead characterize how the impacts of government policy depend upon how c_1 and S adjust in response to changes in government policy.

II.B Comparative Statics

Table 1 summarizes the comparative static predictions of our simple framework. The first row of the table considers active savers, while the second considers passive savers. The columns consider the impacts of changes in the automatic contribution or price subsidy on two outcomes: contributions to retirement accounts $M + P$ and total savings $M + P + S$.

We consider the impacts of changes in the automatic contribution in Columns 1 and 2 of Table 1. Because voluntary pension contributions are a perfect substitute for automatic contributions, active savers undo changes in M 1-for-1 by reducing P if they are not at a corner. Hence, automatic contributions have no effects on total retirement contributions $M + P$ or total savings $M + P + S$. In contrast, passive savers leave P fixed by definition and hence their total retirement contributions $M + P$ rise. Hence, in our two-type framework, 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 savings of passive savers is uncertain. It depends on whether they

cut consumption c_1 or non-retirement savings S to meet the budget constraint in (3). When M rises, disposable income falls and passive savers must balance their budget by reducing consumption or savings. Two cases span the potential responses one may observe. At one extreme, an individual might absorb the reduction in disposable income purely by running down his bank balance S. If an individual has a fixed consumption plan c_1 and does not pay attention to retirement savings, he will automatically end up with a smaller bank balance at the end of the year and thus a change in M will have *no impact* on total savings. At the other extreme, an individual might absorb the reduction in disposable income purely by reducing consumption c_1. If an individual has a fixed target for his bank balance (e.g., he does not want it to drop below a given threshold that is invariant to M), then a \$1 increase in M will increase total savings by \$1. Between these two extremes, we may observe impacts of M ranging from 0 to 1 depending upon the positive model that passive savers follow.[10] The key lesson is that the impact of automatic contributions on total savings depends on how budgets are adjusted even if individuals are totally inattentive to their pensions. Existing evidence that automatic contributions increase savings within retirement accounts should not necessarily make us expect that such policies will raise total savings.

Columns 3 and 4 of Table 1 turn to the impacts of price subsidies. 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.[11] The impacts of an increase in the price subsidy in neoclassical models 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 effects: (1) by reducing the price of P relative to S, leading to subsitution 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 intertemporal elasticity of 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 price effects. Hence, we can obtain another estimate of the fraction of passive savers $(1 - \alpha)$ from the

[10] 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.

[11] Price subsidies do increase total retirement wealth for passive savers; our point here is simply that they do not affect ex-ante savings choices P and S.

fraction of individuals whose pension contributions are unresponsive to the subsidy.

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 $M + P$ by inducing individuals to raise retirement account contributions by shifting assets from taxable accounts or raising total savings. Prior work on the impacts of match subsidies (e.g., Duflo et al., 2006; Engelhardt and Kumar, 2007) consistently finds positive effects of subsidies on retirement contributions, suggesting that this is the relevant case in practice. However, the effects of price subsidies on total savings $M + P + S$ are unclear. If the EIS is small and the cross-account substitution elasticity is large, the increase in$P$ induced by a price subsidy could come largely from shifting assets across accounts, with little or no impact on total savings. If the EIS is sufficiently large, an increase in θ could increase total savings. Again, there is no *a priori* reason to expect price subsidies to raise total savings, and prior evidence documenting clear positive impacts within retirement accounts ($dP/d\theta > 0$) does not provide guidance on this question.

An obvious but important implication of our framework is that automatic contributions can affect the pension contributions of all individuals, whereas price subsidies can 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. The predictions are helpful in evaluating whether active vs. passive choice is the key mediator of the impacts of retirement savings policies on savings behavior. 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 pay attention to automatic contributions and undo their effects to a greater degree. Third, standard micro-foundations for active vs. passive choice predict that active savers have lower discount rates (higher δ_i) and thus save more for retirement to begin with (i.e., have higher levels of $P + S$). For instance, in Carroll et al.'s (2009) model, active vs. passive choice is directly determined by δ_i: individuals with high discount rates postpone retirement planning because the NPV gains from making plans do not outweigh the fixed up-front costs of doing

so. More generally, the characteristics that make some individuals actively optimize with respect to retirement savings incentives may also make these individuals plan carefully for retirement to begin with. Regardless of its source, such a correlation would imply that price subsidies target individuals who are already planning for retirement to some degree, whereas automatic contributions can change the behavior of those with low δ_i, who are less prepared for retirement.

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

III Data and Institutional Background

Institutional Background. 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.). 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 Ministry of Taxation(2002) for a description of the income tax system. Note that over the period we study, the exchange rate was approximately 6.5 DKr per US $1.

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.[12] 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.

For many wage earners, employer pension contributions provide an important source of retirement savings. 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. Typically, two-thirds of employer contributions are made by employer, while individuals are required to contribute one-third of the amount out of their own earnings. There is substantial

[12]This figure combines a base pension of DKr 48,024 for those with broad 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 (USD 3,300) as well as support for heating and rental expenses.

variation across employers in the size of retirement contributions, which we exploit for identification: 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. Importantly for our analysis, 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.[13] These two accounts have different payout 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.[14] Contributions to both types of accounts are tax deductible at the time of contribution. Income tax rates are 59% above incomes of DKR 258,400 in 1999 (roughly the 80th percentile of our sample). Hence, many individuals pay significantly less tax on retirement account contributions than taxable earnings. The sum of employer and individual contributions 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.

In addition to the variation described above from differences in employer-provided pensions, we exploit two changes in government policy to identify the impacts of automatic contributions and price subsidies on savings behavior. The first policy change we analyze is a Mandatory Savings Plan that was implemented from 1998-2003. In 1998, the Danish government introduced a Mandatory Savings Plan (MSP). All Danish residents under the age of 67 automatically had 1% of their earnings withheld by their employer and deposited in an independent pension fund whose board was appointed by the Danish employers' and Danish employees' organizations. In 1998, those with

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

[14] 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.

income below DKr 34,500 were exempted from the MSP, an eligibility cutoff that we exploit in our empirical analysis. In 1999, this eligibility restriction was removed. The MSP accounts functioned in a manner equivalent to individual pension accounts. 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 plan 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. The MSP was terminated at the end of 2003.

The second policy change we analyze is a reduction in the subsidy for capital pensions in 1999. Prior to 1999, both annuity and capital pension contributions were fully deductible from income up to the contribution limit. The marginal tax rate on income was approximately 59% for those in the top income tax bracket and 45% for those in the "middle" bracket, just below the top tax cutoff. As a result, the subsidy for both annuity and capital pension contributions was 45 cents for middle-bracket taxpayers and 59 cents per DKr contributed for top-bracket taxpayers. Starting in 1999, the tax deduction for capital pensions was reduced to 45 cents, the same level given to middle-bracket taxpayers. The tax treatment of annuity pensions was unchanged.

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 register data is 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.[15] Note that we observe flows into pension accounts but do not have data on balances 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

[15]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.

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. We are able to divide total net worth into various broad components, such as housing, bonds, and stocks, but do not observe greater detail on portfolio allocations. Liabilities consist of all unsecured debt (such as credit cards) as well as home mortgage debt. Because home mortgage debt is marked-to-market in Denmark, its value fluctuates significantly across years independent of individual repayment. We therefore use mortgage interest payments as a proxy to measure outstanding mortgage debt and account for potential changes in payments to home equity.

We analyze income and savings at the individual (rather than household) level because the Danish tax system is essentially 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 also show that we obtain similar results for the subsample of individuals without partners.[16]

We define total pension contributions ($M + P$) as the sum of mandatory contributions (from 1998-2003), employer contributions made on the individual's behalf, and individual pension contributions. We define total savings ($M + P + S$) as total pension contributions plus changes in the individual's taxable asset holdings.[17] This definition suffers from three limitations. First, because we do not directly observe home equity wealth, we miss investments in home improvements. We assess whether this is a significant source of bias by replicating our analysis on the subsample of renters. Second, our definition of total savings also does not account for other investments in durables, such as cars or appliances. To address this concern, we examine effects of the policies we study over several years and show that the impacts are stable over time, suggesting that our findings are not driven by intertemporal substitution of expenditure on durables. Third, the wealth data exclude some assets such as cash holdings outside bank accounts or 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

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

[17]Because we define our measure of taxable savings (S) based on changes in asset holdings, we measure savings with error due to fluctuations in asset values and durable goods purchases. 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.

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.

Sample Definition and Summary Statistics. Starting from the population dataset, we impose two restrictions to arrive at our primary analysis sample. First, we only include individuals aged 18-60, at which point various early retirement schemes begin. Second, we exclude self-employed individuals because income and own-business wealth are not measured precisely for this group and because a major break in the definition of the wealth variables for this group occured in 1997. This leaves us with a sample consisting of about 45 million observations for 4.2 million unique individuals. For our analysis of price subsidies, we focus on a subset of taxpayers whose incomes are 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 kink sample. To eliminate outliers, we drop observations with pension contributions above the 99.9th percentile of the full sample. We analogously 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 exceed capital income for most individuals. Mean non-retirement assets (excluding home equity wealth and pension wealth) is DKr 51,602 (US $9,000). On average, households have DKr 81,177 (US$ 14,000) of liabilities, most of which comes from mortgage debt. Median assets and liabilities are DKr 13,550 and DKr 33,406, respectively. The top-bracket analysis sample has higher income as expected.

Employer administered 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 potentially choose to offset automatic contributions.

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 sudden, sharp changes in employer pension contributions.[18] To account for the fact that increases in employer pensions also increase total compensation, we conduct analogous event studies of wage changes due to job switches and compare the impacts of changes in earnings and employer pensions on total savings. 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 describing our results.

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 obtains wage earnings from two distinct firms in year t and $t-1$.[19]

[18] 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 crowdout.

[19] 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 individuals who have only one job in each year.

In order 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.[20] 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 their pension than their previous firm.[21] 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-provided pensions for this set of 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 almost 1-for-1 with the increase in employer contributions, consistent with the hypothesis that most individuals are passive savers.

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 ($P \geq 0$). In this case, $M+P$ would rise mechanically even for neoclassical optimizers. This is potentially a serious problem because 63.9% of individuals in Figure 1a make zero individual pension contributions in year 0. Throughout the paper, we account for the potential mechanical bias due to corners in two ways: conditioning on positive lagged contributions and studying thresholds insteads 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 the subset of individuals who make positive individual pension contributions in year -1 before the firm switch ($P_{-1} > 0$). Conditioning on lagged contributions substantially reduces the potential for corners to have a mechanical impact. Only 11.9% of individuals make zero contributions in

[20] 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.

[21] In order to hold the sample fixed, we include only individuals with at least 5 years of data both before and after the switch.

year 0 after the firm switch, but the rate of pass-through remains at $4.86/5.65 = 86\%$. Under the neoclassical model, pass-through should be at most 11.9 cents per DKr, since only individuals who hit the corner should be forced to raise total pension contributions. Hence, it is clear that most individuals who are able to offset the increase in employer pensions do not 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 bias due to corners; the drawback is that estimating crowdout 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 switch to a firm with at least a 3% increase in employer pensions. The dashed line plots the increase in the fraction at the corner that would occur if all individuals were to fully offset the increase in employer pension contributions at year 0 relative to -1. The observed increase in the fraction making 0 individual contributions is very small relative to the predicted increase with full offset, confirming that very little of the increase in employer pensions is offset by reducing individual contributions.

To obtain a crowdout estimate from this threshold analysis, we assume that the underlying model of response is binary: individuals either respond by undoing the employer contribution fully or do not change behavior 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 simply 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, which is $1 - (1.4/29.8) = 95.3\%$. The neoclassical null hypothesis that individuals fully offset changes in employer contributions if they are able to do so is rejected with a t-statistic exceeding 200.

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 begin saving 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 quite 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 of income to retirement savings. Moreover, because individual and employer contributions have identical tax benefits, there is no reason to switch firms in order to save more rather than just raising one's own contributions to the same retirement accounts, which would involve much lower transaction costs. 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.[22] We present further evidence supporting the key identification assumption below by replicating the analysis for the subset who switch firms due to mass layoffs and studying the pattern of wage changes.

Impacts on Total Savings. Next, we turn to the question posed in Column 2 of Table 1: how do automatic contributions affect total savings, 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. It is therefore critical to examine whether savings in non-retirement accounts change to understand impacts on total wealth accumulation. The series in squares in Figure 1a plots total savings ($M + P + S$). Total savings rates also jump immediately when individuals switch to a firm with higher employer contributions. The jump is 4.92% – smaller than the jump in total pensions – implying that 12% of the increase in pension contributions is offset in non-retirement accounts. Employer contributions increase total wealth accumulation significantly because most individuals do not start to consume more when their pension balances rise.

[22]Pension contributions to individual accounts do not need to be updated when an individual switches firms. The lack of active choice partly explains why so few people change their individual pension contributions when they switch firms. We show in Section VI that new entrants who are making pension choices for the first time are more sensitive to incentives.

Figures 1b and 1c show that we obtain similar results when we account for corners. The series in squares in Figure 1b shows total savings 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 savings 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 savings. 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 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 and show that our results are robust to alternative definitions since very few individuals change taxable savings.[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 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 pensions and taxable savings 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 period -1 to period 0 vs. changes in employer-provided pensions. To construct this figure, we divide the x axis into twenty equal-sized bins (vingtiles) and plot mean changes in total pension contributions 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 slope of the relationship is 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. In our two-type framework, this implies that only $\alpha = 5\%$ of individuals are active savers. Increases and decreases in employer contributions have similar impacts on total retirement contributions. We reject the null hypothesis that the pass-through rate is 0 with a t-statistic of approximately 500, with standard errors clustered at the firm by two-digit occupation level to account for the correlation in employer pensions across workers in the same firm and occupation. The 95% confidence interval for the

[23] Samwick (Patience, Pensions, and Saving 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.

degree of pass-through to pensions is $(94.3, 95.1)$.

Figure 2b replicates Figure 2a for total savings, 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. As above, we find lower pass-through of employer pensions to total savings, as one would expect given that some individuals offset the changes in employer pensions in taxable savings. We estimate that 90 cents per DKr of employer pension contributions passes through to total savings 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 affect savings even for neoclassical optimizers through an income effect by raising an individual'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 savings by 10 cents, far less than the 90 cent increase observed for employer pensions.[24] Increasing compensation in the form of retirement savings 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 savings by $90 - 10 \cdot \frac{2}{3} = 83$ cents.[26] This implies that the vast majority of passive savers reduce consumption when employers automatically deposit a larger fraction of their pay in a retirement account. The finding that individuals' marginal propensity to save out of earnings is an order of magnitude smaller than their marginal propensity to save out of automatic employer contributions underscores the main lesson of this section: most individuals save much more when the saving is done automatically for them than when they have to make the savings decisions themselves.

Robustness. In Table 3, we assess the robustness of these results by estimating regression models

[24] This calculation overstates individual's true marginal propensity to save, insofar as the increase in employer pension contributions when earnings rise lead to greater savings because of passive behavior rather than active choice.

[25] Figure 1d also provides strong evidence that income effects are unlikely to be responsible for 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.

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 pensions or total savings, measured as a percentage of current 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 the destination firm to account for correlated changes in employer pensions across employees of the same firm.

We begin in Column 1 by analyzing impacts on total pension contributions. We estimate three variants of this regression. In Panel A, we replicate the specification in Figure 2a (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 "high savings" indicator as having private pension contributions P greater than 1.1% of income, the mean in the firm switcher sample. We then define the "predicted change" in this high savings indicator as the change in the fraction of individuals who would have high savings (from year -1 to 0) if they offset the change in employer pensions one-for-one. Finally, we regress the actual change in the high savings indicator on the predicted change. One minus the resulting regression coefficient is an estimate of pass-through under the same binary response assumption made to calculate crowdout in Figure 1c. Intuitively, this coefficient measures the fraction of agents whose savings change as one would predict if they did not offset the employer pension at all, which is equivalent to pass-through in the binary model. The resulting estimate is 85 cents of pass-through per DKr 1 of employer contributions.

Column 2 of Table 3 replicates the same triplet of specifications for changes in total savings instead of total pension contributions. Again, we find high rates of pass-through to total savings both when conditioning on having lagged wealth of more than 10% of income and when using the threshold approach, with the threshold defined as saving more than 7.7% of income.

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 these specifications while controlling for the change in the wage rate 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. By this measure, 15.1% of the firm-switches occur because of mass layoffs. In this sample, we estimate pass-through of employer contribution changes to total savings of 91.4%, 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 view that our research design identifies the causal effect of employer pensions on workers' savings.

Long-Term Impacts. One concern with the preceding analysis is that small changes in employer pensions may have significant impacts on total savings, but larger changes may draw individuals' attention and be offset by changes in consumption behavior 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 savings and M 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 in Column 2, restricting the sample to changes in employer pensions of more than 5% in magnitude. The pass-through coefficient to total savings remains at 90%. Hence, it is not merely because of adjustment frictions that individuals do not respond to changes in employer contributions.

Second, we directly investigate the persistence of the increases in savings. In Figure 3a, we replicate the regression specification in Column 2 of Table 3 at various horizons. For computational simplicity, we only include to 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 before the switch (-1) to year t. The

first point, $\beta_0 = 0.832$, corresponds to the one-year pass-through estimate shown in Column 6 of Table 3. The remaining points show that pass-through remains very stable, with no discernible trend over the subsequent 10 years. Column 7 of Table 3 evaluates the robustness of this result to alternative specifications by replicating the specification in Column 6 using changes in savings rates and employer pensions over a five-year horizon instead of a one-year horizon. The estimated pass-through remains stable across the specifications.

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 before age 55 and who reach age 60 within our sample frame. We calculate total retirement savings from the date of the switch to the most common retirement age in Denmark (age 60) by summing savings in each year (as a fraction of income in that year). Figure 3b then plots this change in total wealth, as a fraction of income, against 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 than average end up having additional wealth equivalent to more than 25% of income when they reach the retirement age.[27] Column 8 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 matter 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.[28] As described in Section III, the Danish government introduced a Mandatory Savings Plan (MSP) in 1998 with the goal of reducing consumption to lower the risk of an "overheating" economy (Green-Pedersen, 2007).[29] The MSP took 1% of individuals' pre-tax earnings and automatically allocated it to a retirement savings account. Individuals with incomes below DKr 34,500 (US $6,000) were excluded

[27] Although the average individual is age 51 at the point of the firm switch, the increase in retirement balances is smaller than $(60 - 51) \cdot 5\%$· (as one might expect based on the pass-through estimates of 80% in Figure 3a) because not all individuals stay at the same firm after the initial switch. The actual increase in employer contribution rates fades over time.

[28] Estimates obtained by directly studying changes in government policy could in principle differ from those obtained from variation in employer pensions for several reasons. For instance, individuals might update consumption habits and commitments when they switch jobs but maintain fixed consumption paths at other times. This would lead to greater crowdout of automatic contributions for the average person in the population than for the job switchers we analyzed above.

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

from the program in 1998; starting in 1999, all individuals were included in the program. The MSP was terminated in 2004. The variation in the MSP generates two quasi-experimental designs. The first is 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 resposnes 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 found no evidence of discontinuities in these variables (not reported). These tests support the identification assumption underlying the RD design that individuals to the left and right of the cutoff are comparable.

We now use the RD design to estimate the impacts of the MSP on total pension contributions and savings. We restrict the sample to 1998 and estimate OLS regression models of the following form:

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

where y_i denotes individual i's income and $f(y_i)$ 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. 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.

In Column 2, we analyze mean individual savings rates (i.e., excluding employer pension contributions) around the eligibility cutoff. 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 rates 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 savings: 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, which is DKr 1,952. This indicator variable is more stable than mean savings rates, which are heavily influenced by outliers. Figure 4c plots the fraction of individuals saving more than the mean in their retirement and non-retirement accounts. 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 saving more than this threshold 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 1,952 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 crowdout, 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 1-1 into total savings. 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 mechanical 1-1 pass-through. As a result, we obtain a point estimate of pass-through that is not significantly different from 1. Column 3 of Table 4 replicates the analysis in Column 2 using total savings (including employer pensions) and shows that we obtain very similar estimates when employer pensions are taken into account. We conclude that few if any individuals offset the MSP by reducing savings in other accounts.

Difference-in-Differences Design. The RD estimates apply to individuals with incomes around DKr 34,500. To test if the impacts of the MSP are similar for higher income individuals, we turn to the DD estimator. To illustrate the DD approach, 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.[30] To estimate pass-through to total retirement savings, we first divide the 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.[31] 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 pension contributions ($M + P$) exceed 1.5% of income, which is 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

[30] 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 perhaps not surprising given that individuals do not appear to pay attention to even the changes in the level of contributions in 1998 and 2004.

[31] 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).

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 savings, including savings in taxable accounts. Unfortunately, when we replicate the analysis in Figures 5b and 5c for total savings, 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 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 estimates of impacts on taxable savings that rely on comparisons across individuals in different income groups; the same problem arises in our analysis of price subsidies in Section V. 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 savings.

While we cannot make inferences about the impacts of the MSP on total savings throughout the distribution, the similarity of the DD and RD estimates within pension accounts suggests that the RD estimates of total savings impacts may be representative of impacts at higher income levels. Combining the results from the two designs, we conclude that the MSP raises total pension contributions and total savings significantly, corroborating the results of our analysis of employer pensions.

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 and taxed at a flat 40% rate, while annuity pensions pay out as annuities and are taxed as regular income. In 1999, the deduction for capital pensions was reduced by 14 cents per DKr (30%) in the top income tax bracket but left unchanged for those in lower tax brackets, as shown in Appendix Figure 3. The tax treatment of annuity pension contributions was

unchanged.[32] Because most retired individuals face marginal income tax rates above 40%, capital pensions were a more tax advantaged savings vehicle for most individuals prior to 1999. After the 1999 reform, annuity pensions became roughly equivalent to capital pensions for most individuals whose income placed them in the top tax bracket while working. The 1999 reform can therefore be interpreted as making the most tax-favored savings account (capital pensions) equivalent to the second-best alternative (annuity pensions).

We divide our analysis of the impacts of price subsidies into two parts. First, we analyze the impacts of the reform on contributions to capital pension accounts to quantify the amount of active response to the change in price subsidies. Second, we investigate crowdout: 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?

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? We identify the impacts of the capital pension subsidy reduction using two estimators. The first is a difference-in-differences (DD) design that compares the evolution of savings around 1999 for those with incomes above and below the top tax cutoff, which was DKr 251,000 $\simeq$ US 43,100 in 1999. The second is a regression-kink (RK) design that exploits the sharp change in the subsidy at the top tax cutoff.

We begin by illustrating the DD estimator in Figure 6a. For each year between 1995 and 2005, we plot mean capital pension contributions (measured as a percentage of current income) for two groups: those with current incomes between 25,000 to 75,000 DKr *below* the top tax bracket cutoff and those with incomes between 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.[33] Prior to the reform, capital pension contributions trended very similarly for the treatment and control groups. Capital pension contributions fall sharply for the treated group relative to the control group in 1999, immediately

[32]The reform did not change any other aspect of the tax treatment of these two pension accounts and thus had no effect on the value of existing balances.

[33]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 qualitative results are quite robust to varying this DKr 25,000 exclusion threshold, as can be seen from Figure 6b.

after the subsidy was reduced. Contributions in the treated group then continue to decline steadily relative to the control.

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

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

where P_{it} denotes a measure of pension contributions (calculated as a percentage of income), post_{it} denotes an indicator for the years after 1999, 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 elimination of the capital pension subsidy reduced capital pension contributions by $\beta_3 = -1.7\%$ of income relative to a pre-reform mean of 3.6% for treated individuals with incomes DKr 25,000-75,000 above the top tax cutoff. This estimate 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.

Figure 6b illustrates the RK estimator by plotting mean capital pension contributions for workers whose incomes place them within DKr 75,000 of the top income tax cutoff, demarcated by the dashed vertical line. To construct this figure, we first group individuals into DKr 5,000 income bins based on their current income relative to the threshold. 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. Starting in 1999, individuals above the top tax cutoff – for whom the capital pension subsidy was reduced – cut back on capital pension contributions significantly. In contrast, contributions by those below the kink do not change significantly in 1999.

Figure 6c shows the impact of the reform on the marginal propensity to save more directly. To construct this figure, we first compute mean capital pension contributions for individuals in each income bin in the three years prior to the reform (1996-1998) and the three years after the reform (1999-2001). We then plot the difference between these two series, subtracting the post-reform means from the pre-reform means.[34] It is clear that the marginal propensity to contribute to capital pensions rises sharply at the top tax cutoff prior to 1999 relative to after 1999. Since

[34]We depict the difference as the pre minus the post because this allows us to interpret the increase in slope at the kink as the increase in the marginal propensity to save in capital pensions caused by the subsidy, which is more intuitive expositionally.

there was no other change in incentives at the kink between these years, we attribute this change to the larger capital pension subsidy prior to 1999.

The RK estimates are identified by the change in slope at the top tax cutoff in Figure 6c.[35] We quantify the magnitude of the change in slopes 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:

$$P_{it} = \alpha + \beta_1 \text{post}_{it} + \beta_2[y_{it} > 0] + \beta_3 \text{post}_{it} \cdot [y_{it} > 0] + \alpha^s y_{it} +$$

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

In this equation, β_3^s is the change in the marginal propensity to save in capital pensions dP_{it}/dy_{it} when the capital pension subsidy is removed for individuals in the top bracket. We implement this specification in Column 3 of Table 5. The coefficient of $\beta_3 = -0.044$ implies that a DKr 1,000 increase in income led to DKr 44 of additional saving in capital pensions when the additional 13 cent subsidy was in place prior to 1998, as shown in Figure 6b. Again, adding controls does not affect this estimate (Column 4 of Table 5).

Figure 6 shows that changes in tax incentives induce sharp changes in aggregate contributions to the affected account and appears to be consistent with the neoclassical model. However, this aggregate change 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.[36] 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,

[35] The slope change actually appears to begin slightly to the left of the top tax cutoff, a generic 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.

[36] 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 subsidy.

supporting the view that the difference between the distributions in Figure 7a are caused by the change in subsidy.

Figure 7 yields two lessons about the heterogeneity of response. First, a large group of individuals do not change pension contributions across years. While the degree of non-response falls in 1999, many prior contributors 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.[37]

We begin by estimating the impact of the subsidy on the fraction of individuals who do not change their capital pension contributions at all, i.e. those with $P_t = P_{t-1}$. 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 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.[38] 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

[37] Directly identifying individuals who actively respond to the change in price subsidy is challenging because we need a counterfactual for how each individual would have changed his pensions absent the subsidy change in 1999. We analyze the distribution of changes to identify α because 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.

[38] 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.

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 increase the rate of extensive margin (capital to annuity) substitution by 11.6% 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 substitutuion response – which we can easily detect – is proportional to the overall latent rate of active response to the subsidy, we can estimate the mean α as $\frac{10.2\%}{6.8/11.6}$. That is, the aggregate reduction in capital pension contributions from 1998 to 1999 shown in Figure 6a is accounted for by 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.[39]

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

[39]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).

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 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 is in contrast with the steady decline in aggregate capital pension contributions after 1999 in the treatment group in Figure 6a. We show in Section VI that the continued decline after 1999 in the treatment group is entirely accounted for by new entrants who start their pensions after 1999 and are more responsive to incentives when they make their initial allocations.[40]

V.B Crowdout 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 crowdout 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.

Crowdout 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.[41]

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 annuity contributions (measured as a percentage of income) for the treatment (income DKr 25K-75K above the top tax cutoff) and control (DKr 25-75K below the cutoff) groups as in Figure 6a. The pattern is the mirror image of that in Figure 6a. Annuity pension contributions remain roughly constant from 1996 to 1998,

[40] We also replicated the specification in Column 2 of Table 3 for the subset of individuals whose incomes are within DKr 75,000 of the top income tax cutoff, the analysis sample used in Section V below to study the impacts of price subsidies. The pass-through rates remain very similar for this subgroup, showing that the differences between the impacts of subsidies and automatic contributions are not driven by differences in sample characteristics.

[41] Policy changes in the U.S. often take this form because they increase the contribution limit for one of the account types, thus effectively changing the subsidy for one type of account relative to another on the margin.

the years before the reform, for both groups. In 1999, there is a sharp relative *increase* in annuity pension contributions for the treated group. After the reform, annuity contributions grow over time in the treated group relative to the control group.

Figure 8b plots total pension contributions (capital plus annuity) for these two groups. It shows that the growth in annuity pensions largely offsets the gradual decline of capital pension contributions in Figure 6a. The dashed black line shows the difference between capital pension contributions in the treatment and control groups in Figure 6a. There is a small and clearly discernible reduction in total pension contributions for the treated group relative to the control group in 1999, but this effect is considerably smaller than the drop in capital pension contributions, showing that there is considerable crowdout within pension accounts.

To quantify the degree of crowdout, we estimate IV regressions that use (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. We cluster standard errors by year x DKr 5,000 income bin to allow for correlated errors by income group over time. The coefficient μ_3 is the crowdout parameter of interest.

Panel A of Table 6 presents 2SLS estimates of (9). In Column 1 of Table 6, we use annuity pension contributions as the dependent variable and obtain an estimate of $\mu_3 = -0.62$. That is, 62 cents of each DKr withdrawn from capital pension accounts is shifted to annuity pension accounts. Note that this coefficient is equivalent to the DD reduced-form impact of the reform on annuity pensions divided by the DD impact on capital pensions reported in Column 1 of Table 5. In Column 2 of Table 6, we use total pensions as the dependent variable. This specification confirms that pass-through to total pensions is 38 per DKr withdrawn from capital pension accounts. 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. We first compute mean total pensions for individuals with incomes in DKr 5,000 bins in the three years prior to the reform (1996-1998) and the three years after the reform (1999-2001). We then plot the difference between these two series, subtracting the post-reform means from the

pre-reform means. The marginal propensity to contribute to total pensions increases sharply at the top tax cutoff prior to 1999 relative to after 1999. This result confirms that individuals contribute more to pensions when they cross into the top bracket prior to 1999, when capital pensions were subsidized more heavily.

To quantify the degree of pass-through, we again 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 estimate of crowdout can be equivalently obtained by dividing the estimated reduced-form change in slope at the cutoff in total pensions in Figure 8c 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 60 cents of each DKr withdrawn from capital pensions after 1999 was shifted to annuity pensions, and that total pensions fell by 40 cents per DKr of capital pensions as a result, consistent with the DD estimates.

Crowdout of Taxable Savings. Next, we analyze whether changes in pension contributions are offset by changes in savings in taxable (non-retirement) accounts, which has been the focus of the prior literature on crowdout (e.g., Engen, Gale and Scholz, 1996; Poterba, Venti and Wise, 1996). The degree of shifting between pensions and taxable savings (rather than *within* pension accounts) is of interest because it determines how changes in subsidies that apply to all tax-deferred accounts affect total wealth accumulation. To estimate crowdout of taxable savings, we ask whether the 40 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 crowdout in non-retirement accounts.

The literature has used two definitions of "crowdout" 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 savings. 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 savings. 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 savings over time for the treatment and control groups. Unfortunately, as in our analysis of the mandated savings plan in Section IV.A, DD estimates of changes in taxable savings are very imprecise and unstable. The reason is again the large differential fluctuations across the income groups over time in taxable savings. Recall that the reform reduced capital pension contribution rates by 1.7% of income, and 38% 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 savings accounts, one would observe an increase in taxable savings of $1.7\% \cdot 0.38 \cdot 0.4$ of income. Visually, it is apparent from Appendix Figure 4 that one cannot detect a differential change of 0.26% of income in taxable savings. Correspondingly, the DD estimate for crowdout in taxable savings using the specification in (9) – in which we instrument for total pensions using the interaction term in (7) – has a 95% confidence interval spanning $(-0.35, 2.14)$, 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. Figure 9a illustrates our RK estimate of the impact of the subsidy on savings in taxable (non-retirement) accounts.[42] This figure replicates Figure 8c, changing the dependent variable to taxable savings. The marginal propensity to save in taxable accounts falls at the top tax cutoff prior to 1999 relative to after 1999. This change is the mirror image of the relative *increase* in pension contributions at the top tax cutoff in Figure 8c, showing that part of the increase in pension contributions comes from reductions in taxable savings.

We quantify the degree of crowdout in taxable savings using the same RK specification described

[42]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 simple DD estimator compares changes in savings rates for individuals L and H. 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 relies on stability of the gradient (parallel trends) across years for identification, which is violated for taxable savings (Appendix Figure 4).

above, instrumenting for total pensions 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 crowdout parameter μ_2 defined above and report the resulting estimate in Column 4 of Panel B of Table 6. We estimate that savings in taxable accounts fall by $\rho_2 = 73$ cents per DKr deposited in pension accounts. To better understand this crowdout estimate, consider the impacts of a DKr 1,000 increase in pre-tax income. The estimate in Figure 8c implies that this leads to an extra contribution of DKr 20.07 to pensions due to the additional subsidy in the top tax bracket prior to 1999. Given a marginal tax rate of 60%, this additional pension contribution reduces disposable income by $0.405 \times 21.3 = 8.12$. Of this 8.12, DKr 5.91 comes from reductions in taxable savings, as shown by the estimate in Figure 9a. The remaining DKr 2.21 comes from reduced consumption, which leads to increased national savings. Hence, we estimate that $5.91/8.12 = 73\%$ of the amount deposited in pensions is crowded out by reduced taxable savings. That is, 27 cents of each additional dollar contributed by individuals to pensions passes through into an increase in total savings.[43]

While the point estimate of crowdout of 73 cents is significantly different from 0, the confidence interval spans (-1.41,-0.04). To improve precision, we analyze thresholds of changes in savings rather than mean savings rates, as we did with the MSP RD design. We define an indicator for having taxable savings above the mean for individuals within DKr 5,000 of the top tax cutoff. We then calculate the mean value of this indicator for the three years before and after the reform in each income bin and plot the difference in these means in Figure 9b. There is a clear change in the relationship between income and the probability of having high taxable savings at the top tax cutoff. We reject the hypothesis that there is no change in slope at the kink with a t-statistic of 6.09 ($p < 0.0001$).

We translate this reduced-form impact to an estimate of crowdout 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-average savings by 0.82%. Next, based on the density of the savings distribution around the mean, we calculate that if the DKr 1,000

[43]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 crowdout calculation because the 84% of passive savers who do not respond to the reform also do not change taxable savings when their disposable income changes, as shown by the results in Section IV. The 16% of active savers who respond to the subsidy change do so 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 crowdout estimate is driven purely by the behavior of the active savers: the passive savers affect neither the numerator (change in taxable savings), 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 crowdout, as the increase in taxable savings after 1999 would have been even larger absent this income effect.

increase in pensions were entirely financed by reducing taxable savings, the fraction with above mean savings would have fallen by 0.83%. The ratio of these two estimates measures the degree of crowdout 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 0.991 and a confidence interval of (0.61, 1.37). Column 6 shows that this estimate does not change significantly when we include the standard controls.[44]

The precision of the RK estimates in Panel B of Table 6 relative to the DD estimates in Panel A 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. Instead, it is critical to develop research designs that effectively compare individuals at similar income levels.

Using these estimates of crowdout, we can calculate the net savings impact of each DKr of government expenditures on subsidies for retirement accounts as follows. First, note that mean per capita contributions to capital pensions fell from DKr 12,074 in 1998 to DKr 7,272 in 1999. Based on the estimate in Column 2 of Table 6, this change resulted in a DKr 1,906 reduction in total pension contributions, which results in an increase of DKr 772 in post-tax disposable income. The estimate in Column 5 of Table 6 implies that taxable savings increase by DKr 765 as a result, so that the net reduction in post-tax savings due to the subsidy change was DKr 7. The subsidy change reduced the NPV of the average individual tax benefit for capital pension contributions by DKr 1,745.[45] The policy change therefore reduced total savings by less than 1 cent per DKr reduction in tax expenditures. At the upper bound of the 95% confidence interval for the crowdout estimate in Column 5, the resulting estimate is 19 cents per DKr of tax expenditure on the subsidy.

We conclude that the few individuals who respond to the price subsidy also optimize asset allocation across all their accounts, both by shifting assets across different types of pension accounts and by offsetting changes in pension contributions with changes in taxable savings. As a result, pension contributions induced by tax subsidies generate much more crowdout and have much smaller impacts on total wealth accumulation than those induced by automatic contributions.[46]

[44]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-mean 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.

[45]The DKr 1,745 reduction in government expenditure includes both the mechanical gain from the subsidy reduction and the savings from the behavioral response of reduced pension contributions.

[46]To ensure that the difference in crowdout is not due to differences in the analysis samples used to the study the

VI Heterogeneity: Identifying Active and Passive Savers

The results in Sections IV and V have shown that increases in automatic contributions M generate larger increases in total savings than increases in price subsidies θ. While this finding is consistent with our model of active vs. passive choice, these policies might have different impacts for other reasons. In this section, we test whether active vs. passive choice is responsible for the policies' differential impacts by studying the heterogeneity of responses across individuals. We organize our analysis around the three testable predictions on heterogeneity described at the end of 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 pension 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%, implying that new contributors are roughly 1.5 times as responsive to the change in subsidy as prior contributors. These estimates are not sensitive to the inclusion of controls, as shown in Column 2 of Table 7.

The higher responsiveness of new contributors explains the gradual shift from capital pensions to annuities from 1999 to 2008 in Figures 6a and 8a. The DD estimate of the impact of the subsidy change on total capital pension contributions in Figure 6a grows from an immediate impact of 1.4% in 1999 to 2.4% by 2008, a 72% increase. 52% of the individuals contributing to capital pensions in 2008 began contributing to pensions (i.e., transitioned from zero contributions to positive contributions) at some point after the 1999 reform. Given that new contributors are 1.5 times as responsive

price subsidy and automatic contributions, we replicated our analysis of employer pensions in Table 3 for the subset of individuals whose incomes are 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 savings remains high in this subgroup: for example, the coefficient in Column 2 of Panel A is 0.87, compared with 0.90 in the full sample.

as prior contributors, we would expect the impact of the reform to be approximately 75% larger in 2008 relative to 1999, very similar to the actual change.

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* indiviudals 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 proxy for 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.[47] 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 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 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 pensions in other years.[48] We then estimate the degree of pass-through from employer

[47] In Appendix Figure 5, we replicate this figure for the control group of individuals whose incomes are DKr 25,000 to 75,000 below the top bracket cutoff. As expected, there is little relationship between the degree of response in prior years and changes in capital pension contributions in 1999 for the control group.

[48] There are fewer than 20 groups in the figure because of mass-points in the distribution of the x-axis variable.

pensions to total pensions by estimating the regression model in Column 1 of Table 3, Panel A, for each of these subgroups separately. Figure 10b plots the coefficients of these regressions vs. the average frequency of 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.[49] Column 6 verifies that this result is robust to the inclusion of controls. Together, 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 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. As predicted, changes in employer pensions have much smaller impacts on total savings for individuals with higher wealth/income ratios. The correlations in Figure 11 support Carroll et

[49]There are two explanations for why pass-through rates are 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 crowdout 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 crowdout of automatic contributions is that those who respond actively to subsidy changes may still be passive with respect to employer pensions. For instance, tax advisors frequently advertise subsidies for retirement contributions but information about employer pension contributions may be less salient.

al. (2009)'s prediction that active savers have lower discount rates, as wealth/income ratios are a natural proxy for discount rates.

Figure 11c analyzes heterogeneity by age. We plot the fraction of individuals exiting capital pensions and increasing annuity contributions in 1999 by their age group in 1999. We also plot 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 manage their financial decisions more actively (Agarwal et al., 2009).[50]

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 contributors who were in the treatment group in 1999, as in Figure 11a. 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 savings. 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. In this column, we use information on whether the individual took courses related to economics and finance in high school or college to construct an indicator for having economics-related training. We find that individuals with economics or finance training are 7.2 percentage points (55%) more likely to respond to the subsidy change. While we cannot determine whether this large interaction effect is due to a causal effect of obtaining economics training or

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

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). However, we find little relationship between pass-through rates from employer pensions and education, 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

The main lesson of our analysis is that the impacts of retirement savings policies on wealth accumulation depend fundamentally on whether they change behavior through active or passive choice. Policies such as tax subsidies that rely upon individuals to take an action to raise savings have relatively small impacts on total wealth. In contrast, policies that raise savings automatically even if individuals take no action – such as employer-provided pensions or mandated savings contributions – increase wealth accumulation significantly.

We conclude that price subsidies are less effective than automatic contributions in stimulating retirement savings 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 saving for retirement already. Hence, price subsidies are not very effective in increasing savings amongst those who are least prepared 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, the structure of the retirement savings system in Denmark – with a state-provided defined-benefit plan, employer-provided defined contribution accounts, and individual

defined contribution accounts – is very similar to that of 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 contributions raise total pension balances (Madrian and Shea, 2001), employer pension contributions are not offset within pension accounts (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 respond to price subsidies and undo automatic pension contributions (Engelhardt and Kumar, 2007; Beshears et al., 2012). Given the close parallels between the U.S. and Denmark in behavior within retirement accounts, we would expect the responses 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 commonly used retirement savings policies on total savings, 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. While increasing retirement savings may be one part of a policy maker's objective function, it is unlikely to be the sole objective. For instance, some individuals who have temporary shocks to income (e.g., due to unemployment) may be forced to consume less than the optimal amount when their savings rates are increased through automatic contributions. Using the empirical estimates here to assess the optimal combination of automatic contributions, price subsidies, and other retirement savings policies would be a very valuable direction for further work.

Beyond retirement savings, a broader implication of our findings is that changing quantities directly through defaults or regulation may be more effective than providing incentives to change behaviors of interest, such as 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 most like to change.[51] Comparing quantity-based and price-based methods in models where agents make optimization errors is an interesting direction for future research.

Finally, from a policy perspective, the findings reported here call into question whether subsidies

[51] 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.

are the best policy tool to increase retirement savings. 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). While further research using data from the U.S. is needed to definitively understand the impacts of these policies, our findings strengthen recent arguments for using "nudges" such as automatic payroll deductions or savings defaults to stimulate retirement savings instead of 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 Savings M+P+S?	Raises Pension Contributions M+P?	Raises Total Savings M+P+S?
Active Savers (Neoclassical)	No	No	Yes	Uncertain
Passive Savers	Yes	Uncertain	No	No

TABLE 2

Summary Statistics

Variable	Full Sample		Top Tax Threshold Sample	
	Mean (1)	Median (2)	Mean (3)	Median (4)
Gross Labor Income	199,565	191,447	256,618	253,190
Gross Taxable Income	217,474	208,985	282,607	279,335
Net Capital Income	-14,549	-5,870	-20,541	-17,042
Non-Pension Assets (not incl. home equity)	51,602	13,550	60,495	19,478
Non-Pension Assets> 10% of Gross Labor Inc.	0.47		0.42	
Non-Pension Assets/Gross Labor Income Ratio	0.37	0.09	0.22	0.07
Total Savings	23,904	12,438	32,752	25,191
Saving Rate	18.92%	10.92%	15.32%	10.86%
Liabilities	81,177	33,406	101,072	62,906
Pension Contributions				
Fraction with Individual Pension Contribs.	0.27		0.36	
Individual Pension Contribution	3,143	0	4,081	0
Individual Pension Contribution Rate	1.18%	0.00%	1.25%	0.00%
Individual Capital Pension Contribution	1,859	0	2,643	0
Individual Annuity Pension Contribution	1,284	0	1,438	0
Fraction with Employer Pension Contribs.	0.59		0.83	
Employer Pension Contribution	15,542	5,281	21,717	19,705
Employer Pension Contribution Rate	5.67%	5.29%	6.98%	7.51%
Fraction with Any Pension Contribution	0.66		0.90	
Demographics				
Age	38.70	38.00	41.36	41.00
Female	0.52		0.44	
Married	0.48		0.58	
Has Partner	0.62		0.73	
Homeowner	0.51		0.68	
College Degree	0.41		0.59	
Some Economics Training	0.04		0.04	
Number of Observations	45,428,846	45,428,846	17,712,370	17,712,370

Notes: This table presents means and medians of key variables in the entire sample, as well as for those with gross taxable income within DKr 75,000 of the top tax threshold. We trim all pension contribution variables at the 99.9th percentile. We trim all non-pension variables that are strictly non-negative at the 99th percentile. We trim all other variables at the 1st and 99th percentiles. Gross labor income is measured before pension contributions. Gross table income includes non-labor income. 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 savings is the change in non-pension assets plus pension contributions in each year. Saving rate is the ratio of total savings to gross labor income. Liabilities include mortgage debt, other secured debt, and unsecured debt. Individual pension contributions are the sum of individual contributions to capital and annuity pensions. The individual pension contribution rate is the ratio of individual pension contributions to gross labor income. Employer pension contributions and the employer pension contribution rate are analogously defined. Age is measured at the end of the calendar year. We define that an individual has a partner if they are married or cohabitate with any non-family member of the opposite gender that is within fifteen years of age. An individual is a home-owner if either he or his partner has positive home equity. Economics training includes specialized vocational training programs, college degrees, and post-graduate degrees in economics-related fields such as economics, accounting, and finance.

TABLE 3

Employer Pensions: Pass-Through Estimates

Sample:	Firm Switcher	Firm Switcher	Firm Switcher	Mass Layoff Switcher	Big Change Switcher	First-Time Switcher	Firm Switcher	Switcher Ages 45-55
Dep. Var.:	Δ Pension Rate	Δ Savings Rate	Δ Savings Rate	Δ Savings Rate	Δ Savings Rate	Δ Savings Rate	Δ Savings Rate, 5 yrs	Δ Retirement Balance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Lagged Savings >0								
Δ Employer Pension Rate	0.947	0.900	0.888	0.914	0.897	0.832	0.894	5.128
	(0.002)	(0.009)	(0.009)	(0.045)	(0.011)	(0.018)	(0.019)	(0.273)
Δ Wage Rate			0.043					
			(0.001)					
Observations	910,866	2,078,612	2,078,612	345,494	216,613	716,273	387,090	55,608
Panel B: Lagged Savings >0 with Controls								
Δ Employer Pension Rate	0.946	0.908	0.897	0.929	0.904	0.868	0.928	5.155
	(0.002)	(0.009)	(0.009)	(0.045)	(0.011)	(0.018)	(0.018)	(0.274)
Δ Wage Rate			0.048					
			(0.001)					
Observations	910,866	2,078,612	2,078,612	345,494	216,613	716,273	387,090	55,608
Panel C: Threshold Approach								
Δ Employer Pension Rate	0.847	0.965	0.960	0.931	1.072	0.932	0.838	
	(0.003)	(0.004)	(0.004)	(0.013)	(0.014)	(0.007)	(0.027)	
Δ Wage Rate			0.048					
			(0.003)					
Observations	3,958,920	3,367,175	3,367,175	521,038	387,125	889,261	69,880	

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. See notes to Figure 2 for more details on the definition of firm switch. In Panel A, the key 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). 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). Column 3 includes the change 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. 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. Column 7 repeats Column 6, instead calculating the changes in the dependent and independent variable between event year $t = -1$ and $t = 5$. The dependent variable in Column 8 is the cumulative change in savings rates between the time of the first firm switch (when the switch occurs between ages 45 and 55) and age 60, and we include only individuals with either positive pension contributions or lagged wealth greater than 10% of current gross labor income. Panel B replicates Panel A controlling for age, marital status, gender, college attendance, and dummy variables for occupation (at the 2-digit DISCO level). Panel C replicates Panel A using a threshold approach to calculate pass-through. In Column 1, we regress the change in a dummy variable for an individual 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 a dummy variable for an individual being close enough to that threshold such that they would cross it given 100% pass-through. This latter variable takes a value of 1 if the individual would pass from below to above the threshold following an increase in the employer contribution rate, and a value a -1 if the individual would pass from above to below the threshold following a decrease in the employer contribution rate. We then instrument for the independent variable with the change in the employer contribution rate, and include a dummy variable for a positive change in the employer contribution rate. The remaining columns in Panel C repeat this procedure using the relevant dependent variable. We cluster all standard errors by destination firm.

TABLE 4

Regression Discontinuity for Government Mandated Savings Plan: Pass-Through Estimates

Dep. Var.:	Δ Total Pensions	Δ Total Ind. Savings	Δ Total Savings
	(1)	(2)	(3)
Panel A: Lagged Savings >0			
Pass-Through Estimate	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 Estimate	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 Estimate	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 from a regression-discontinuity in eligibility for the Mandated Savings Program (MSP) in 1998. In Panel A, we estimate a separate linear fit to the data on each side of the threshold and report the discontinuity estimate at the threshold divided by DKr 345 (the increase in mandated savings at the threshold) as the pass-through estimate. The dependent variables are the change in total pension contributions (Column 1), total non-employer savings (Column 2), and total savings (Column 3) from 1997 to 1998. Panel B replicates Panel A controlling for age, marital status, gender, college attendance, and dummy variables for occupation (at the 2-digit DISCO level). 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). See notes to Table 3 for the definition of corners. Panel C uses a threshold approach to estimate pass-through; see notes to Figure 4 for the details of this method. We cluster all standard errors by DKr 1,000 income bin.

TABLE 5

Capital Pensions: First-Stage Estimates

Dep. Var.:	Total Capital Pension Contrib. Rate		Total Capital Pension Contributions		No Response	Sharp Response	Sharp Response, Lagged No-Responders	Exit Capital Pension
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Above Cutoff × Post	-0.017 (0.0004)	-0.016 (0.0003)			-0.030 (0.009)	0.116 (0.004)	0.068 (0.004)	0.161 (0.006)
Income×Above Cutoff×Post			-0.044 (0.003)	-0.044 (0.003)				
Controls		X		X				
Empirical Design	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	4,610,328	4,610,328	6,876,590	6,876,590	516,857	516,857	192,301	516,857

Notes: This table characterizes the response to the 1999 reduction in the capital pension subsidy on savings behavior within capital pensions. Column 1 presents a difference-in-difference estimate of the effect of the 1999 reform on average capital pension contribution rates. 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 differenced-regression-kink estimate of the effect of the 1999 reform on capital pension contributions. We estimate separate linear fits to the data above and below the threshold (for individuals with taxable income within DKr 75,000 of the threshold), both before and after the reform. The reported coefficient is the change in regression-kink coefficient from before to after the reform. Columns 2 and 4 replicate Columns 1 and 3 controlling for age, marital status, gender, college attendance, and dummy variables for occupation (at the 2-digit DISCO level). In Columns 5-8, we report difference-in-difference estimates on various individual dummy-variables for response, using only data from 1998-1999 and restricting to individuals with positive lagged capital pension contributions. In Column 5, the dependent variable is a dummy variable for exactly no change in capital pension contributions. In Column 6, the dependent variable is a measure for sharp response, defined as an individual decreasing capital pension contributions to zero while increasing annuity pension contributions. Column 7 replicates Column 6, restricting to individuals with exactly no lagged change in capital pension contributions. In Column 8, the dependent variable is a dummy variable for decreasing capital pension contributions to zero. In Columns 1-2 and 5-8, we cluster standard errors at the DKr 5,000-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 Design						
Dep. Var.:	Total Annuity Contrib. Rate	Total Pension Contrib. Rate	Total Pension Contrib. Rate	Total Taxable Savings Rate	Taxable Savings Threshold	Taxable Savings Threshold
Capital Pension Contrib. Rate	-0.620 (0.057)	0.380 (0.057)	0.310 (0.052)			
Total Pension Contrib. Rate				0.494 (0.839)	0.335 (0.725)	0.234 (0.938)
Controls			X			X
Observations	4,610,328	4,610,328	4,610,328	4,610,328	4,610,328	4,610,328
Panel B: Regression-Kink Design						
Dep. Var.:	Total Annuity Contrib.	Total Pension Contrib.	Total Pension Contrib.	Total Taxable Savings	Taxable Savings Threshold	Taxable Savings Threshold
Capital Pension Contrib.	-0.603 (0.065)	0.397 (0.065)	0.345 (0.062)			
Total Pension Contrib.				-0.727 (0.348)	-0.991 (0.194)	-1.159 (0.225)
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-difference IV approach to estimate crowd-out and pass-through, where we instrument for the key independent variable with the D-D interaction term. See notes to Table 5 for more details on the D-D design. In Columns 1-3, the independent variable is the capital pension contribution rate; in Columns 4-6, the independent variable is the total pension contribution rate (i.e., capital and annuity pensions). The dependent variables are the annuity pension contribution rate (Column 1), the total pension contribution rate (Columns 2-3), the taxable saving rate, defined as the change in non-pension assets as a fraction of gross labor income (Column 4), and a dummy variable for having a taxable saving rate above 4.4%, which is the mean taxable savings rate for those within DKr 5,000 of the top tax threshold (Columns 5-6). Panel B uses a differenced-regression-kink IV approach to estimate crowd-out and pass-through, where we instrument for the key independent variable with the D-RK triple interaction term. See notes to Table 5 for more details on the D-RK design. In each column, the dependent and key independent variables replicate those in the same column in Panel A, but in levels rather than in rates. The threshold for taxable savings in Columns 5 and 6 is DKr 1,962, which is the mean taxable savings for those within DKr 5,000 of the top tax threshold. In both panels, Columns 3 and 6 replicate Columns 2 and 5 controlling for age, marital status, gender, college attendance, and dummy variables for occupation (at the 2-digit DISCO level). In Panel A, we cluster standard errors at the DKr 5,000-income-bin-by-year level. In Panel B, we cluster standard errors by DKr 5,000 income bin.

TABLE 7

Hetereogeneity in Response to Capital Pension Subsidy Reduction and Employer Pensions

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

Notes: Column 1 presents a difference-in-difference estimate of the fraction of contributors to any pension that contribute to capital pensions. The new saver variable is an indicator for zero lagged individual pension contributions. (The regression also includes this dummy variable without an interaction, though this coefficient is omitted from the table). We use data from 1998 and 1999 and individuals with taxable income between DKr 25,000 and DKr 75,000 in this regression. Column 3 regresses a dummy variable for sharp response to the 1999 capital pension reform (see notes to Table 5 for more detail on this variable) on the fraction of other years in which an individual adjusts pension contributions (see notes to Figure 10 for more detail on this variable). We use only data from 1999 and individuals with taxable income between DKr 25,000 and DKr 75,000 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 in which an individual adjusts pension contributions. Columns 2, 4, and 6 replicate Columns 1, 3, and 5 controlling for age, marital status, gender, college attendance, and dummy variables for occupation (at the 2-digit DISCO level). In Columns 1-4, we cluster standard errors at the DKr 5,000-income-bin-by-year level; in Columns 5-6, we cluster by destination firm.

TABLE 8

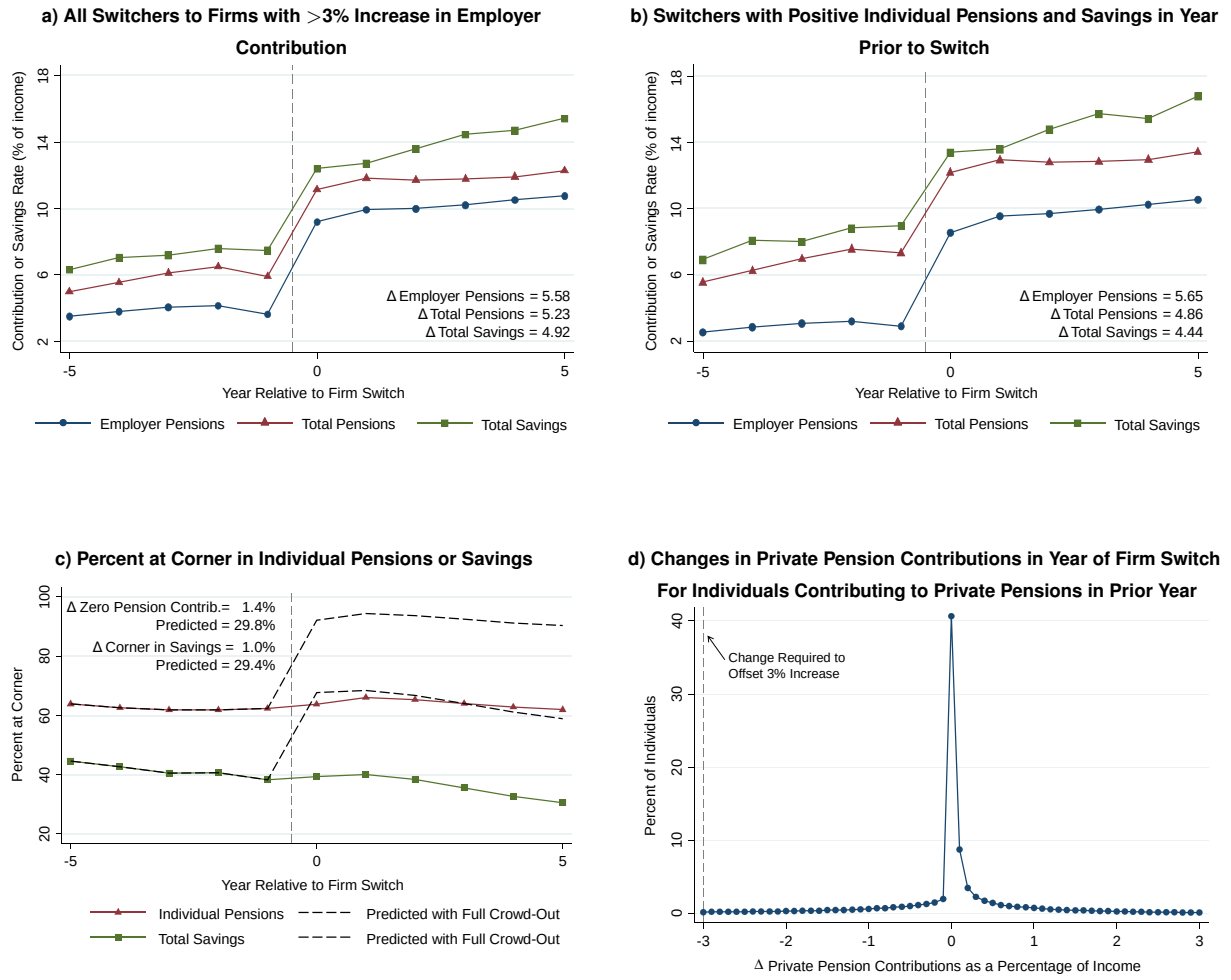
Correlated Heterogeneity in Response to Capital Pension Subsidy and Employer Pensions

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Capital Pension Subsidy Reduction						
Dep. Var.:	Exit Capital Pensions in 1999?					
Wealth/Inc. Ratio	0.071		0.062	0.060	0.057	0.053
	(0.004)		(0.004)	(0.004)	(0.004)	(0.004)
Age		0.002	0.002	0.002	0.002	0.002
		(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.001)
College				0.030	0.027	0.015
				(0.004)	(0.003)	(0.004)
Economics Education					0.072	0.055
					(0.007)	(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 Savings					
Δ Employer Pension Rate	0.890	0.913	0.892	0.901	0.903	0.895
	(0.010)	(0.009)	(0.010)	(0.015)	(0.015)	(0.015)
Δ Emp. Pen. Rate×Wealth/Inc.	-0.208		-0.199	-0.200	-0.199	-0.196
	(0.041)		(0.041)	(0.041)	(0.041)	(0.041)
Δ Emp. Pen. Rate×Age		-0.006	-0.007	-0.007	-0.007	-0.006
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Δ Emp. Pen. Rate ×College				0.007	0.005	0.009
				(0.022)	(0.023)	(0.023)
Δ Emp. Pen. Rate ×Econ. Ed.					-0.002	-0.011
					(0.036)	(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 a dummy variable for sharp response to the 1999 capital pension reform (see notes to Table 5 for more detail on this variable) on individual characteristics, using only data from 1999 and individuals with taxable income between DKr 25,000 and DKr 75,000. 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. Column 3 combines the two. Column 4 adds a dummy variable for college attendance. Column 5 adds a dummy variable for having training in economics. Column 6 replicates Column 5 adding controls for marital status and gender, and dummy variables for occupation (at the 2-digit DISCO level). Panel B replicates Column 2 of Table 3A, including an interaction of the change in the employer pension contribution rate with the lagged ratio of non-pension assets to gross labor income (Column 1), age (Column 2), both age and the lagged ratio of non-pension assets to gross labor income (Column 3). Column 4 adds an interaction with college attendance; Column 5 adds an interaction with economics training. Column 6 replicates Columns 5 controlling for marital status and gender, and dummy variables for occupation (at the 2-digit DISCO level). All regressions include the level for any interacted variable. All interacted characteristics (that are not dummy variables) have been 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 these variables and dummy variables equal to zero. We cluster all standard errors in Panel A at the DKr 5,000-income-bin-by-year level; we cluster all standard errors in Panel B by destination firm.

FIGURE 1

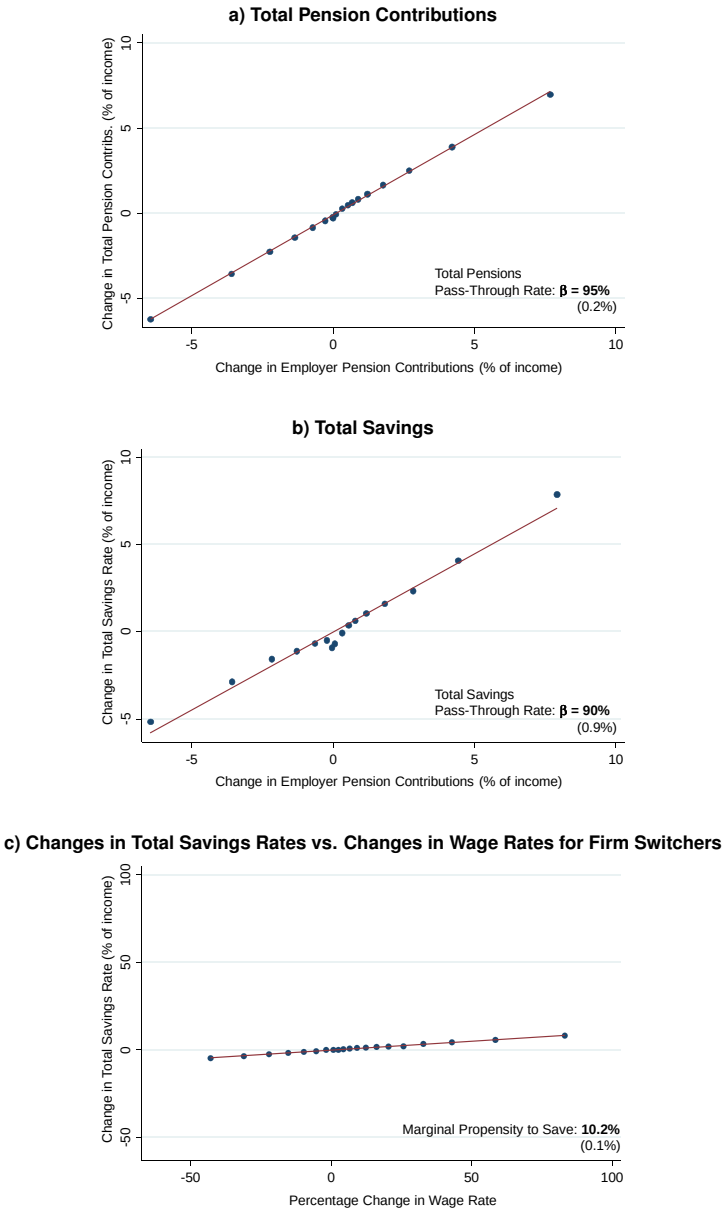
Employer Provided Pensions: Event Studies Around Switches to Firm with >3% Increase in Employer Pension Contribution



Notes: These figures display event-studies of pension contribution rates when workers switch firms. We include only the first firm switch for individuals in our data, and so we define as $t = 0$ 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 fraction of wage income. Total savings rate is defined as the change in assets plus total pension contributions, as a fraction of income. We include only workers experiencing at least a 3 percentage point increase the in employer pension contribution rate at $t = 0$, as well as 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; 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 represents this constant fraction added to the solid series in each event year after the firm switch. 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

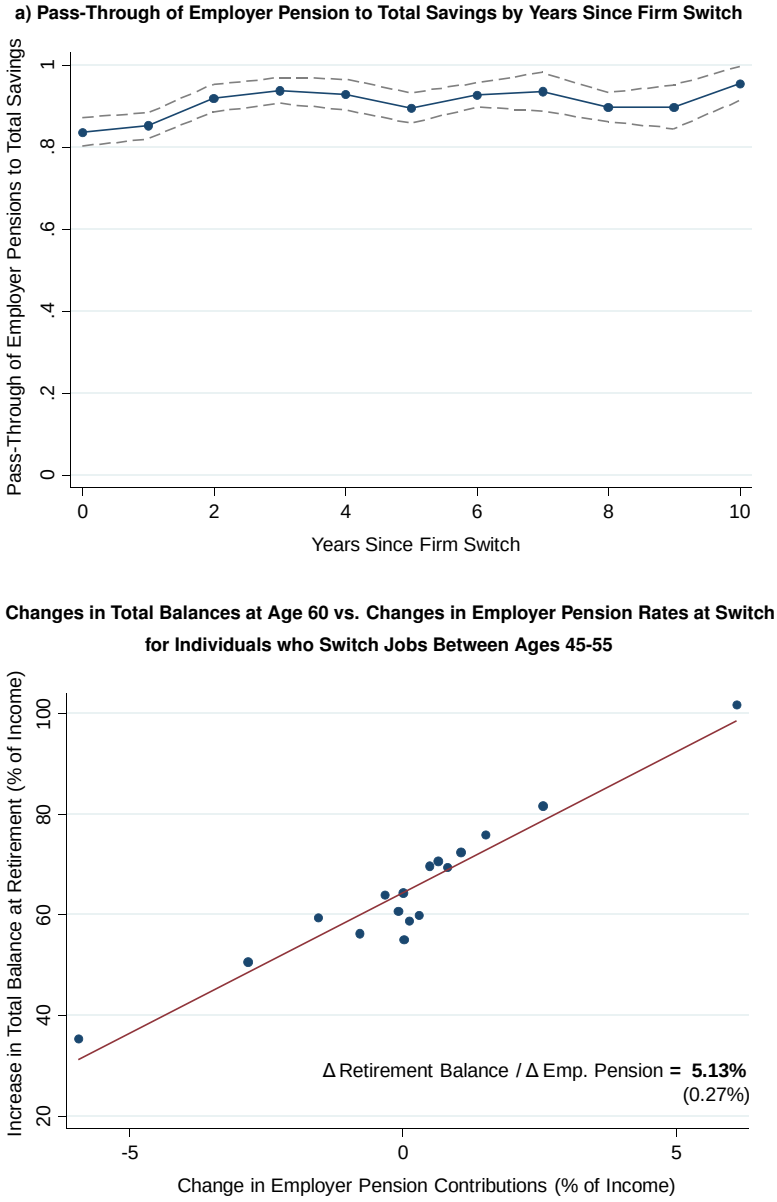
Change in Savings vs. Change in Employer Pensions for Firm Switchers Making Individual Contributions Prior to Switch



Notes: These figures display binned scatterplots that characterize the change in savings behavior at the time of a firm switch. For these figures we 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 pre-switch year. 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 wage as a fraction of lagged wage. The coefficient reported can be interpreted as the marginal propensity to save out of wage increases at the time of a firm switch. See the notes to Figure 1 for details on the definitions of corners. In order to generate the binned scatterplot, we group the data into 20 even-sized bins in 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.

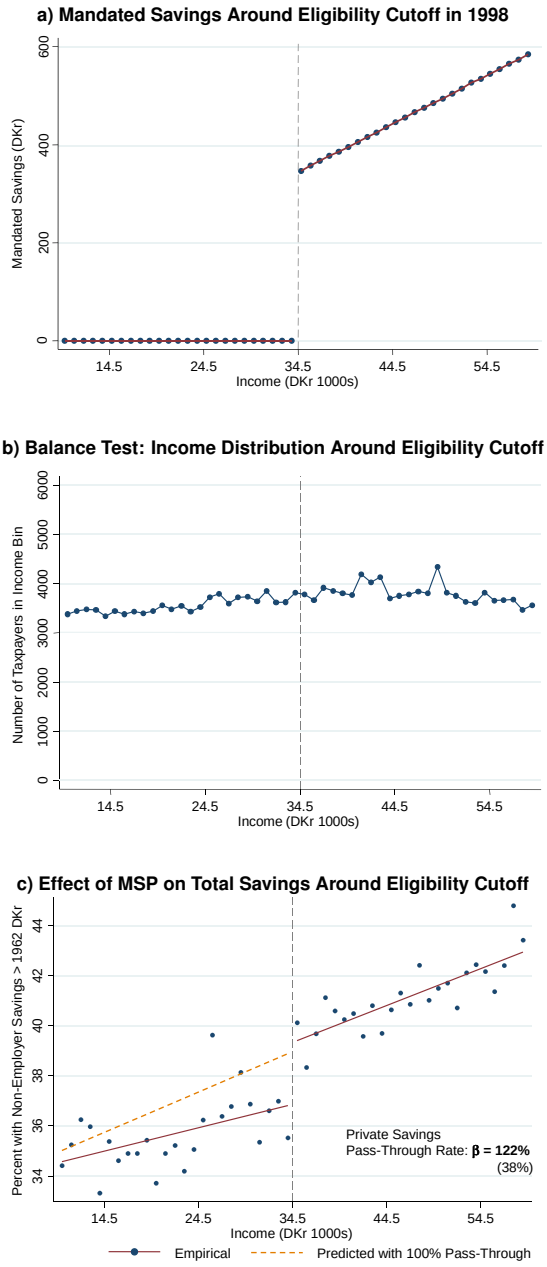
FIGURE 3

Long Run Impacts of Employer Pensions on Wealth Accumulation



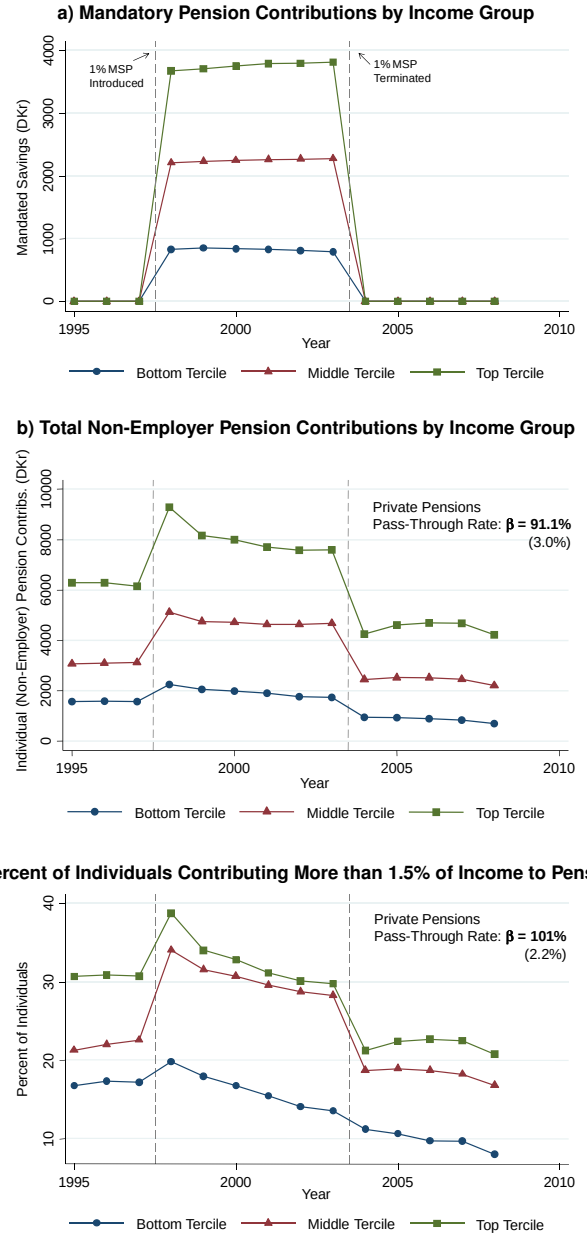
Notes: These figures present the long-term impacts of changes in employer pension contribution rates at the time of firm switches. Panel A plots the pass-through coefficients of changes in employer pension contribution rates to total savings at different horizons. We restrict to the first firm switch 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. The dots at $t = 0$ and $t = 5$ match exactly the coefficients in Table 3A, Columns 6 and 7. The dashed lines represent the boundaries of the 95% confidence interval, using standard errors estimated from a regression that clusters on firm ID. Panel B plots the relationship between the change in employer contribution rates and the accrual of total retirement wealth between the firm switch and age 60, for individuals whose first firm switch occurs between ages 45 and 55. To calculate the accrual of total retirement wealth for each individual, we add the total savings rate across all 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 firm ID.

FIGURE 4
Mandated Savings Plan: Regression Discontinuity Design



Notes: These figures present a regression discontinuity for 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 presents 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 savings) 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 and re-estimate the regression discontinuity, a counterfactual regression discontinuity represented by the dashed best-fit lines. If β_1 is the estimated coefficient for size of the actual discontinuity, andβ_2 is the estimated coefficient for the counterfactual discontinuity, then we calculate pass-through as $\beta_1/(\beta_1 - \beta_2)$. We calculate the standard error from a regression on the micro-data, clustered by DKr 1,000 income bin.

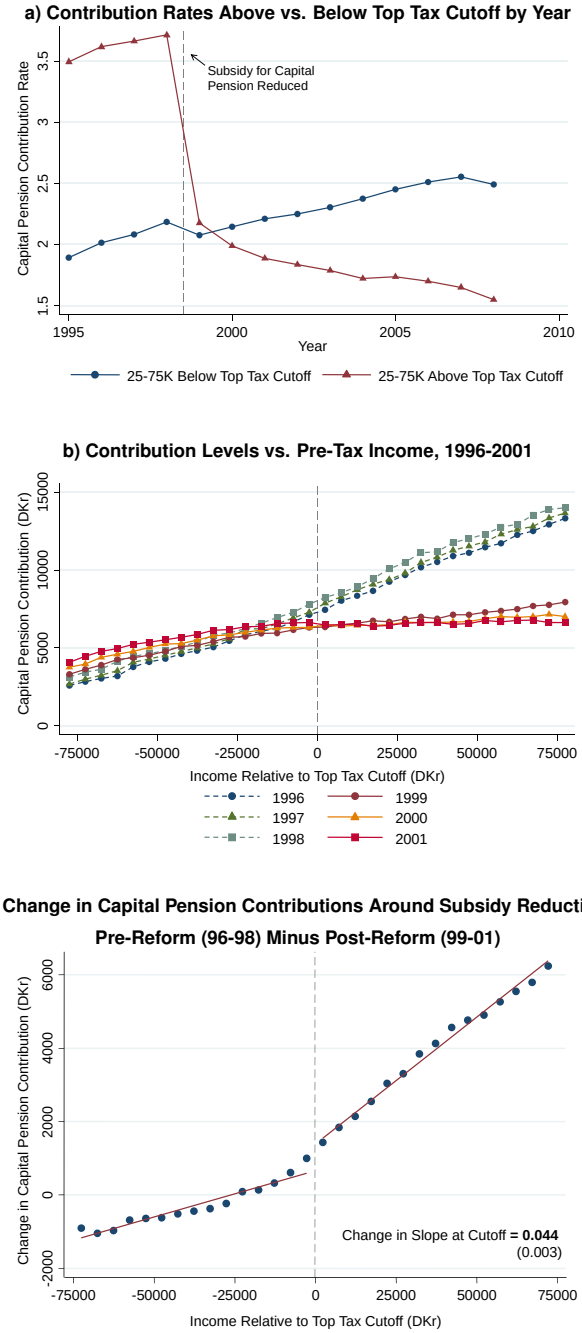
FIGURE 5
Impacts of Mandated Savings Plan: Difference-in-Differences Design



Notes: These figures present the effect of the Mandated Savings Plan (MSP) on non-employer pension contributions. In all three panels, we split the data into three even-sized bins based on income in each year (so that individuals can switch groups from year to year). In any given year, we include only individuals with positive income. Panel A then 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 population 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 from a regression of the change in average non-employer pension contributions on the change in MSP contributions in a repeated cross-section sample, grouping the data into DKr 25,000 bins. We calculate the coefficient in Panel C from a 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). In Panel C we use the same repeated cross-section structure as in Panel B but only examine the changes between 1997-1998 and 2003-2004, and we instrument for the predicted fraction with the average change in MSP contributions in those years.

FIGURE 6

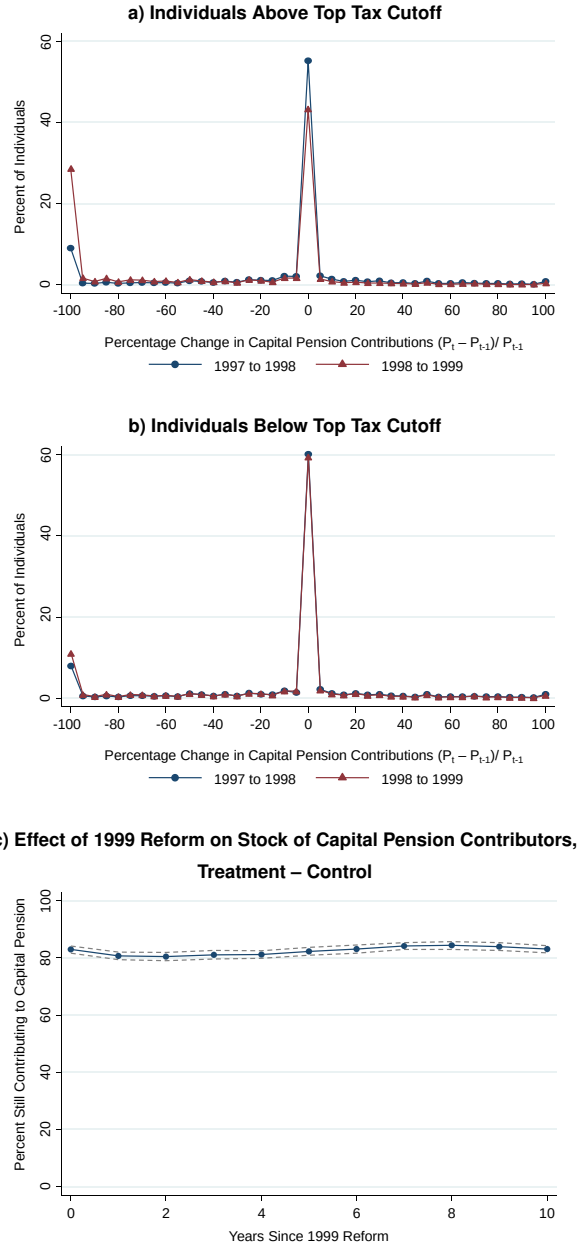
Impact of Subsidy Reduction On Capital Pension Contributions



Notes: These figures present the impact of the 1999 capital pension subsidy reduction on capital pension contributions. Panel A plots average capital pension contribution rates (including both individual and employer 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 B plots average 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 C plots the difference between average capital pension contributions in the pre-reform years (1996-1998) and post-reform years (1999-2001) in each income bin in Panel B. 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, as well as the best-fit lines, are estimated from a differenced-regression-kink regression on the micro data where we allow an independent slope and intercept above and below the kink, before and after the reform. 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

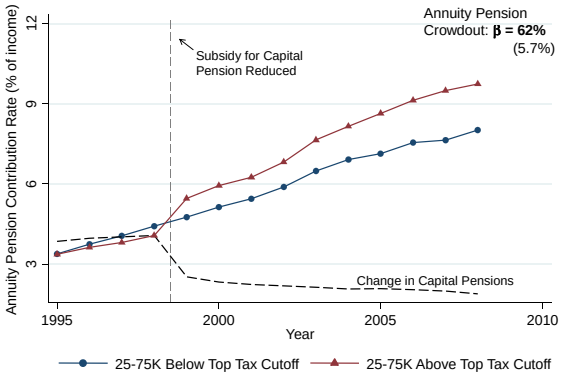


Notes: Panels A and B plot the distribution of changes to individual capital pension contributions, as a fraction of lagged individual pension contributions, for individuals in the treatment and control groups from Figure 6A in 1998 and 1999. Panel A plots the distribution of changes for individuals in the treatment group above the top tax threshold in 1998 and 1999. Panel B replicates Panel A for individuals in the control group below the top tax threshold. 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 presents estimates of the long-term dynamics of response to the 1999 reform. To do so, we calculate in each year the fraction of the stock of individuals with positive individual capital pension contributions in 1998 that are still contributing in each post-reform year. In order to estimate the causal effect of the reform, as distinguished from the natural pattern of decay, we plot the difference between this fraction in the treatment group and that in the control group, plus 1. For instance, the dot at 0.83 in year 0 implies that 17 percentage points less of the stock of contributors in 1998 were still contributing in 1999, in the treatment group relative to the control group. The dashed lines represent the boundaries of the 95% confidence interval, using standard errors estimated from a regression clustered at the DKr 5,000-income-bin-by-year level.

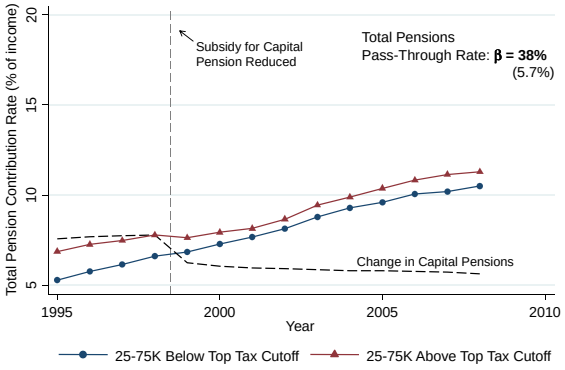
FIGURE 8

Crowdout Within Retirement Accounts Induced by Subsidy to Capital Pensions

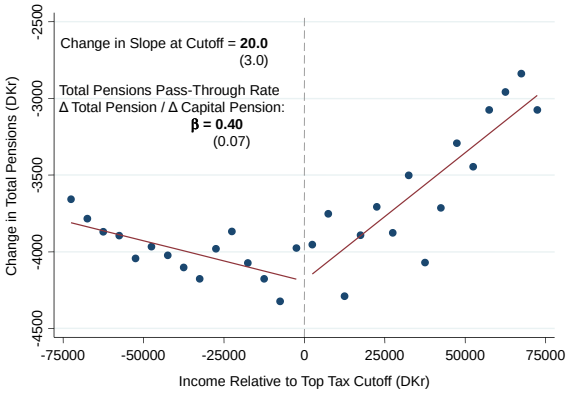
a) Annuity Pension Contribution Rates Above vs. Below Top Tax Cutoff by Year



b) Total Pension Contribution Rates Above vs. Below Top Tax Cutoff by Year



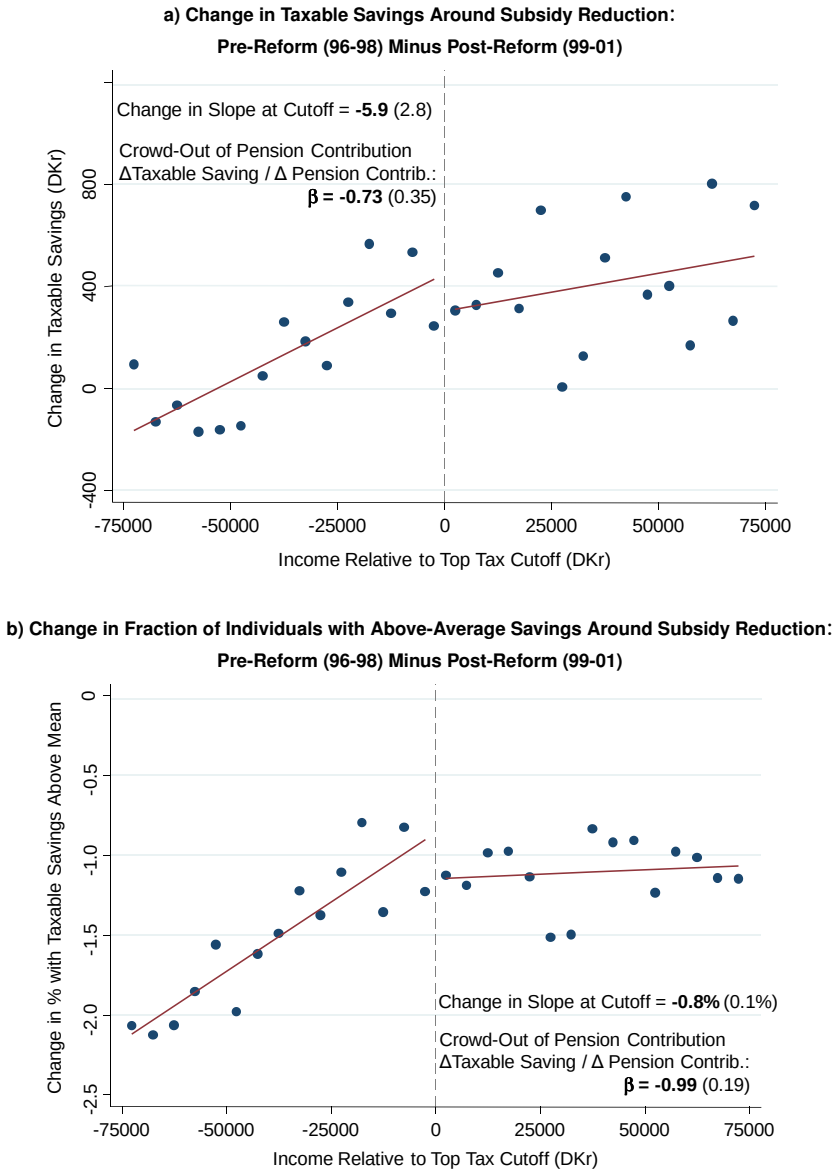
c) Change in Total Pensions Around Subsidy Reduction:
Pre-Reform (96-98) Minus Post-Reform (99-01)



Notes: Panel A replicates Figure 6A, instead plotting the average annuity pension contribution rate in the treatment and control groups by year. Panel B replicates Panel A, instead plotting the average total (i.e., capital and annuity) pension contribution rate. Panel C replicates the differenced-regression-kink in Figure 6C, instead plotting the difference in average total pension contribution in each income bin. See the notes to Figure 6 for details on the construction of these figures, and on the estimation of the change in slope coefficient in Panel C. We estimate the crowd-out coefficients in Panel A and B with a difference-in-differences IV regression of the y-axis variable on the capital pension contribution rate (using the D-D interaction as the instrument) on the micro-data. The standard errors are clustered at the DKr 5,000-income-bin-by-year level. We estimate the crowd-out coefficient in Panel C with a differenced-regression-kink IV regression of total pension contributions on capital pension contributions (using the D-RK triple interaction as the instrument) on the micro-data. The standard errors are clustered by DKr 5,000 income bin.

FIGURE 9

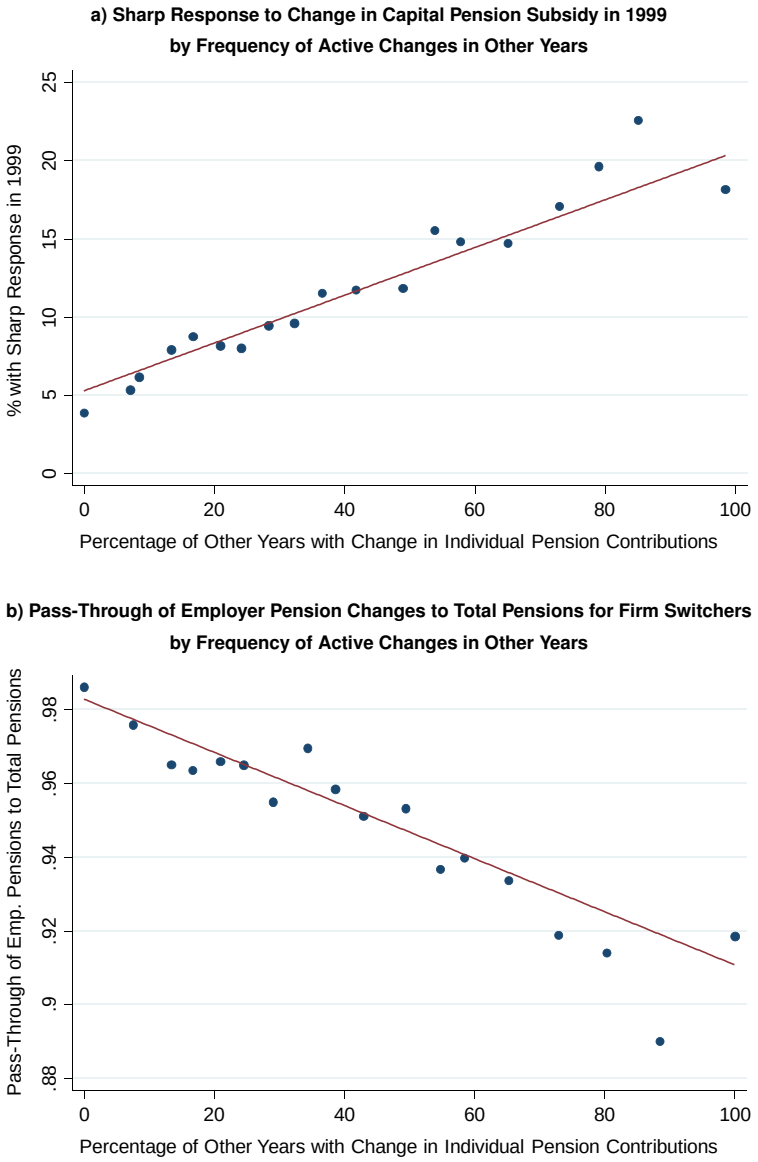
Crowdout of Taxable Savings Induced by Subsidy



Notes: This figure replicates the differenced-regression-kink in Figure 6C, instead plotting the difference in average taxable savings in each income bin (Panel A) and the difference in the fraction with taxable savings above DKr 6863 (mean taxable savings for those within DKr 5,000 of the threshold) in each income bin (Panel B). See the notes to Figure 6 for details on the construction of these figures and the estimation of the change in slope coefficients. We estimate the crowd-out coefficients with a differenced-regression-kink IV regression of the y-axis variable on total pension contributions (using the D-RK triple interaction as the instrument) on the micro-data. The reported coefficient in Panel A is the coefficient from this regression, divided by 0.405 (the net-of-tax rate for those affected by the reform). The reported coefficient in Panel B is the coefficient from this regression, divided by 0.405 times the fraction of individuals within DKr 500 of the threshold, times 1000 (the width of the window). The standard errors are clustered by DKr 5,000 income bin.

FIGURE 10

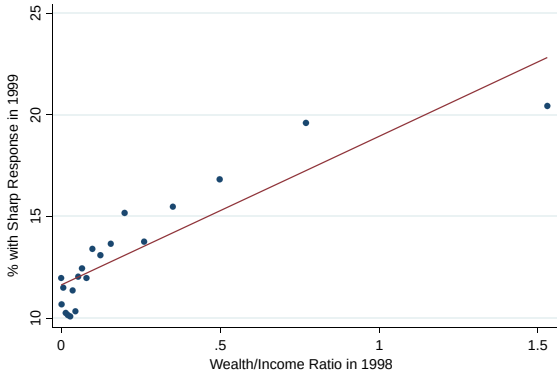
Heterogeneity in Responses to Subsidies and Employer Pensions



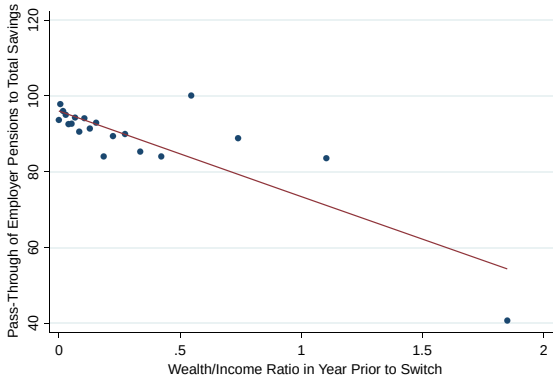
Notes: Panel A plots the fraction of workers exhibiting a sharp response to the 1999 reform (see notes to Table 5) against the fraction of other years in which the worker adjusted individual pension contributions (to either capital or annuity pensions) by more than DKr 500 (excluding years in which workers made no change and did not contribute). In order to generate the binned scatterplot, we group the data into 19 bins based on the x-axis variable that are as evenly sized as possible given the mass-points in the distribution. 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 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. We group the data into 18 bins based on the x-axis variable that are as evenly sized as possible given the mass-points in the distribution. We then replicate the regression in Table 3A, Column 1 within each bin and plot the estimated coefficient. The best-fit line is calculated from an interacted version of the regression in Table 3A, Column 1 (shown in Table 7, Column 5).

FIGURE 11
Heterogeneity in Responses by Observable Characteristics

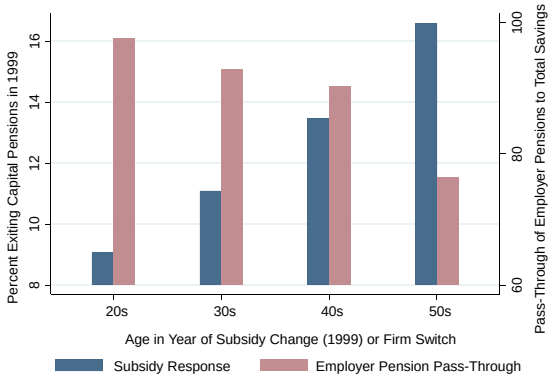
a) Heterogeneity in Sharp Response to Change in Capital Pension Subsidy in 1999 by Wealth/Income Ratio



b) Heterogeneity in Pass-Through of Employer Pensions by Wealth/Income Ratio, for Firm Switchers with Positive Savings Prior to Switch



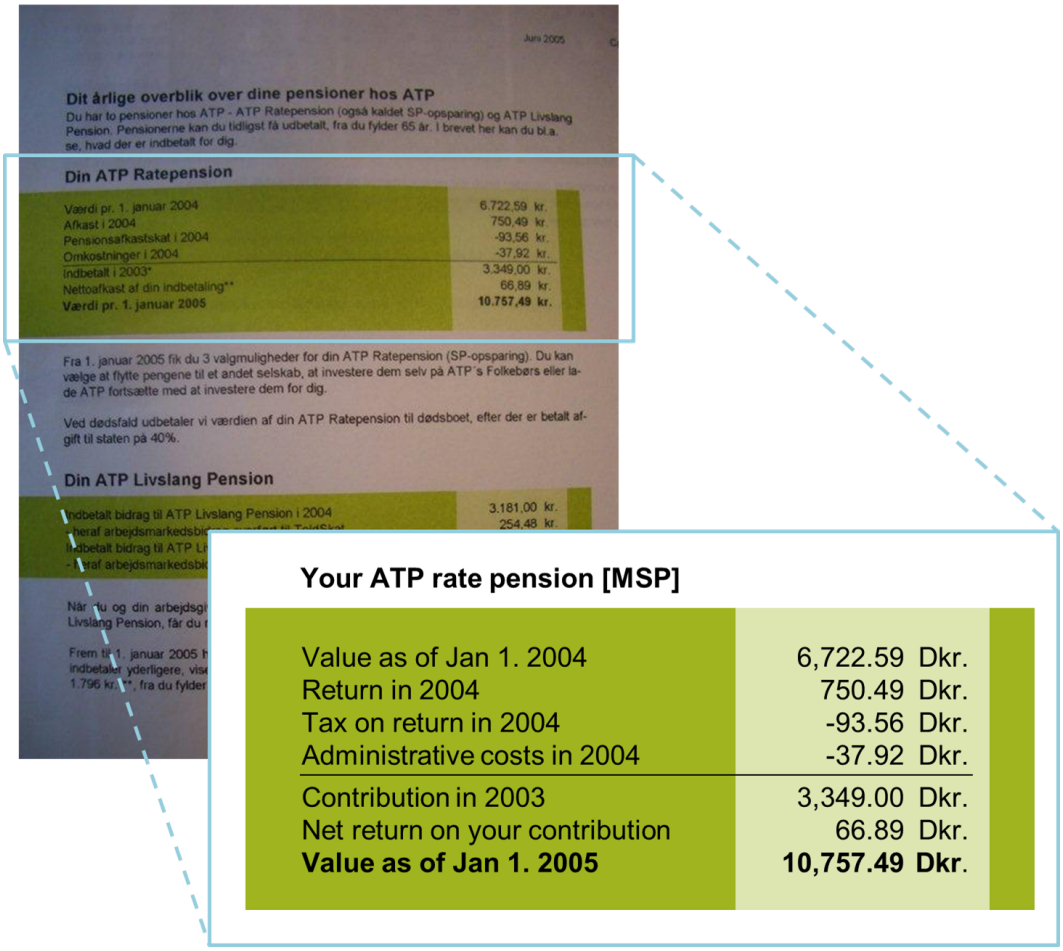
c) Heterogeneity in Responses to Subsidies and Employer Pensions by Age



Notes: These figures present heterogeneity in response to the 1999 capital pension reform and employer contribution switches. Panel A plots the fraction of workers exhibiting a sharp response to the 1999 reform (see notes to Table 5) against lagged wealth-to-income ratio (W/Y). In order to generate the binned scatterplot, we group the data into 20 even-sized bins 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 sharp response on lagged W/Y on 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 group the data into 20 even-sized bins based on lagged W/Y, and then replicate the regression in Table 3A, Column 2, within each bin and plot the estimated coefficient. The best-fit line is calculated from an interacted version of the regression in Table 3A, Column 2 (shown in Table 8B, Column 1). Panel C replicates Panels A and B, cutting on age binned into decades. The two sets of bars plot the fraction of workers exhibiting a sharp response to the 1999 reform (left set of bars, left y-axis) and 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 (right set of bars, right y-axis) within each decade.

APPENDIX FIGURE 1

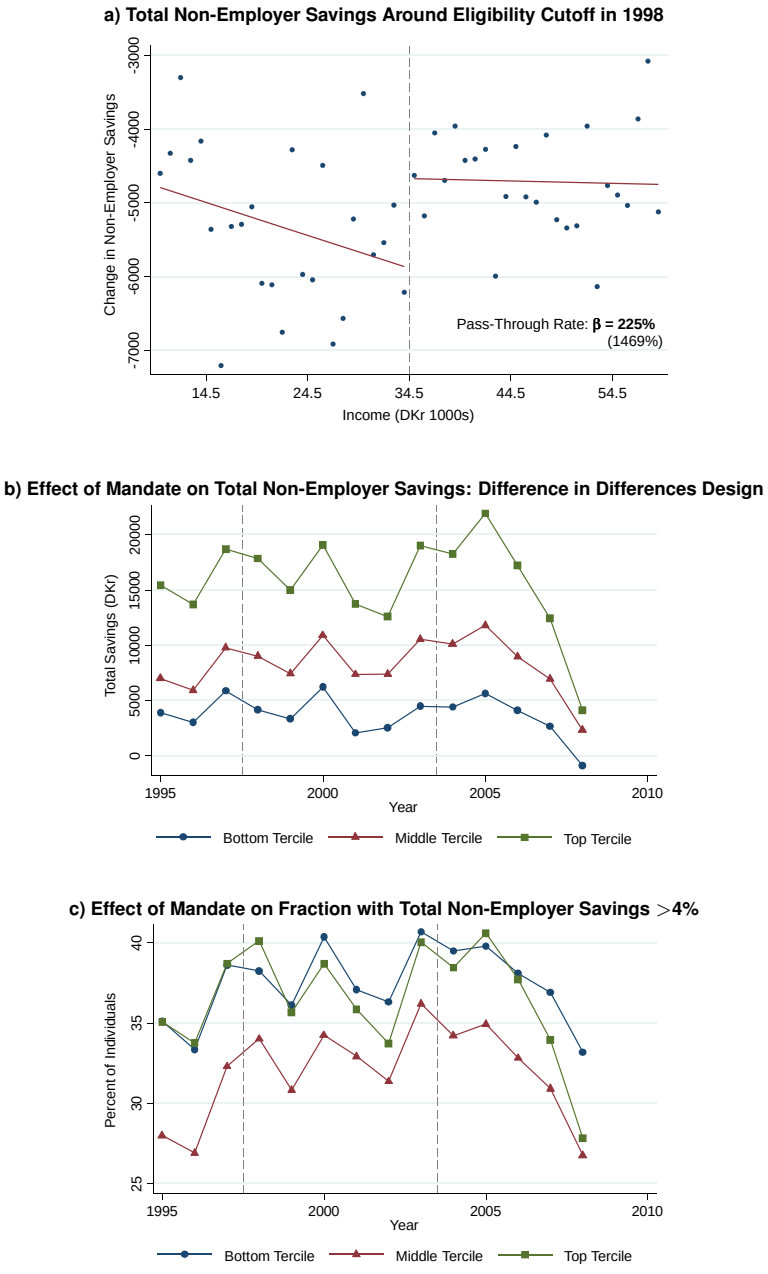
Mandated Savings Account Balance Notification Letter



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, alongside information about the balances in their other retirement pension accounts.

APPENDIX FIGURE 2

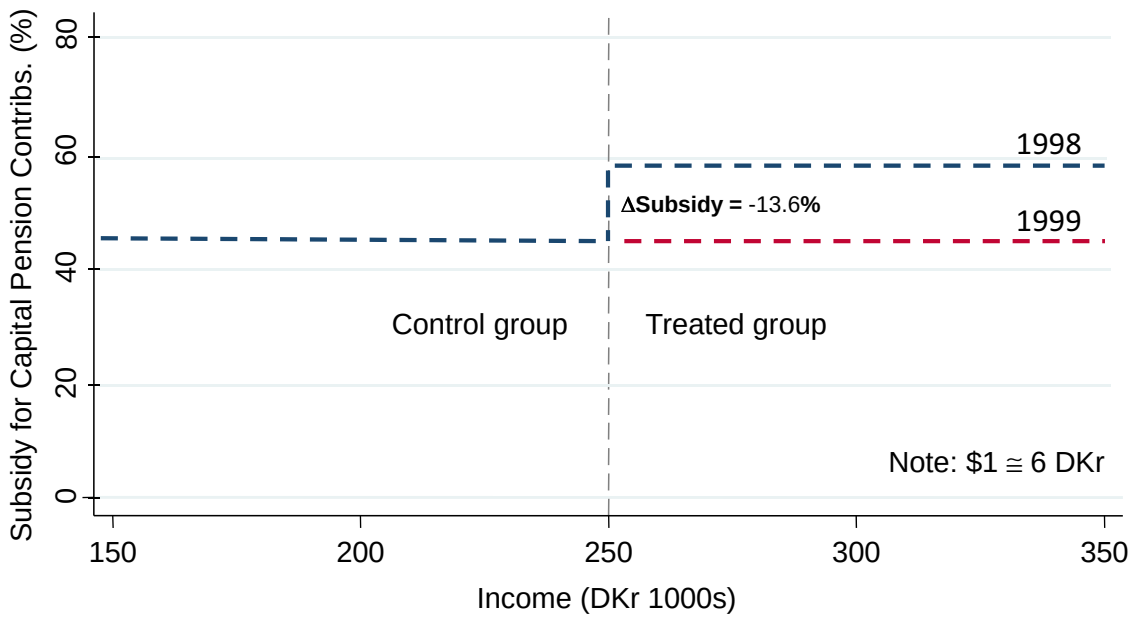
Mandated Savings Plan: Impacts on Total Non-Employer Savings



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 without using thresholds, plotting the mean change in total non-employer savings from 1997 to 1998 within DKR 1,000 income bins. Panel B replicates the differences-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 differences-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

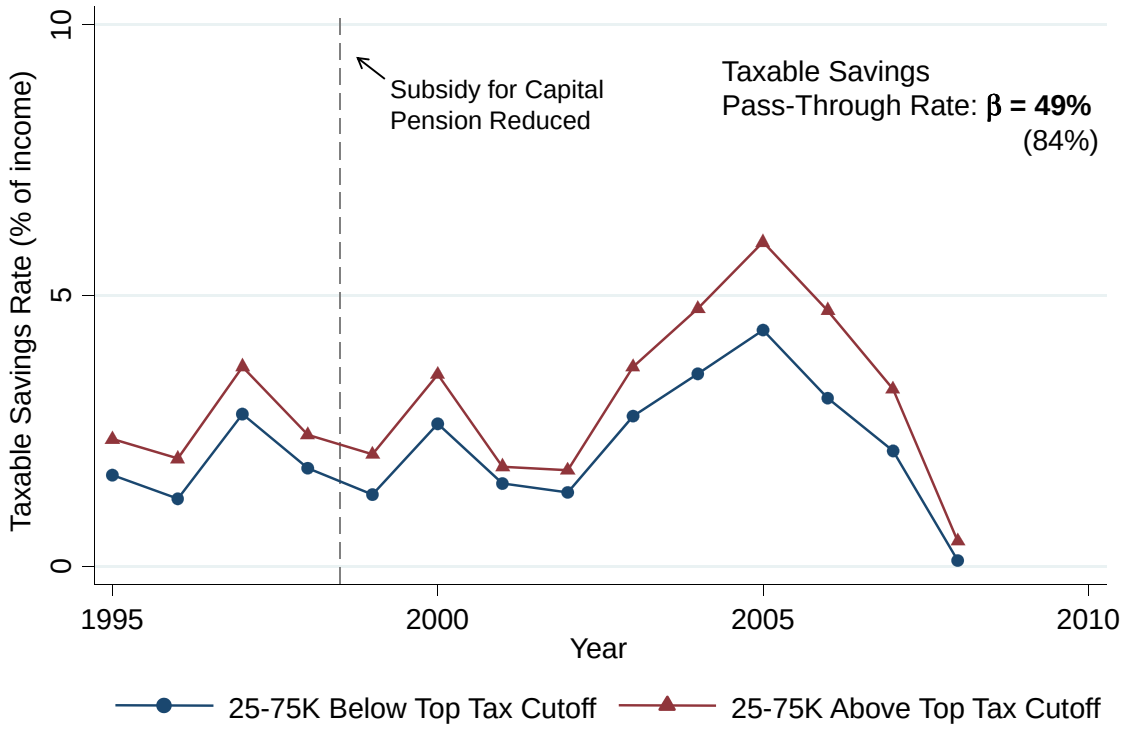
Change in Subsidy for Capital Pensions in 1999



Notes: This figure illustrates the tax subsidy rate for capital pension contributions across income levels, before and after the 1999 reform.

APPENDIX FIGURE 4

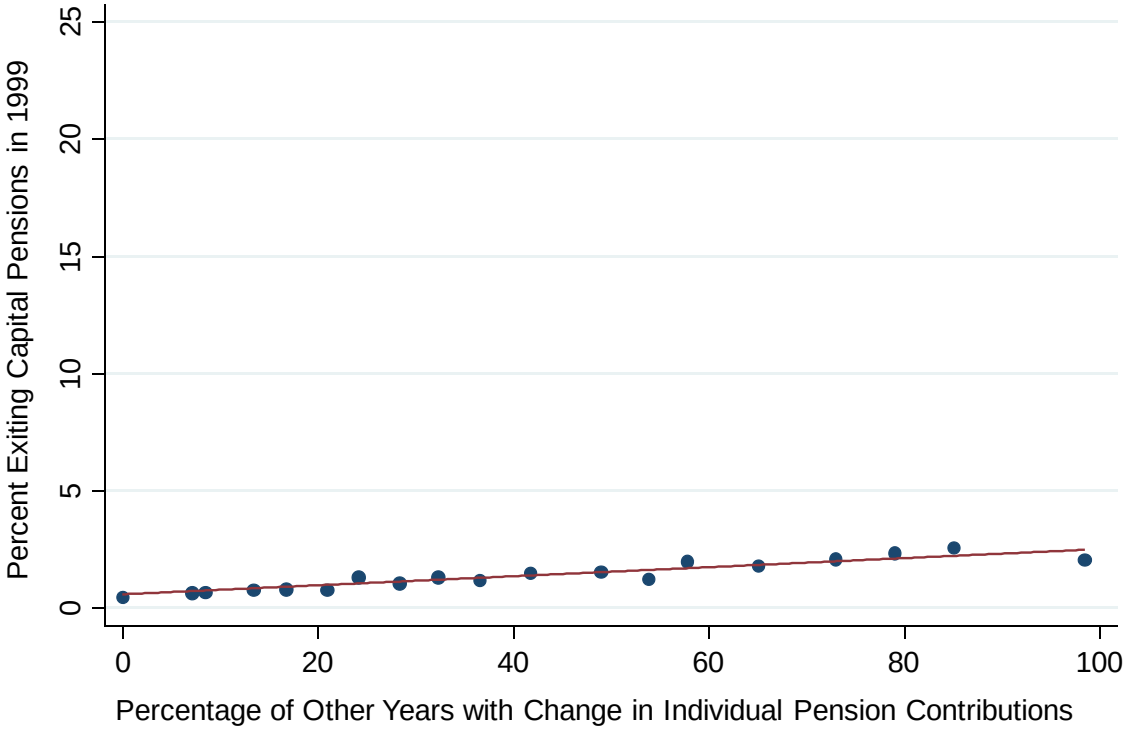
Impact of 1999 Subsidy Change on Taxable Savings Rate: Difference-in-Differences Design



Notes: This figure replicates Figure 6a, plotting the average taxable savings rate in the treatment and control groups by year.

APPENDIX FIGURE 5

Percent Responding to Capital Pension Subsidy Change in 1999 by Frequency of Active Changes in Other Years (Placebo)



Notes: This figure replicates Panel A of Figure 10, for individuals below the top tax cutoff instead of those above it. These individuals were unaffected by the 1999 change in the capital pension subsidy.