Learning About the Stock Market: Asset Allocation Spillovers from Defined Contribution Pension Plan Access*

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Abstract

This paper examines how Defined Contribution (DC) pension plan enrollment affects individuals' non-pension investment behavior. I exploit exogenous changes in Swedish occupational pension rules to address selection concerns. Using Swedish Tax Registry data, I find that DC enrollment significantly increases stock market entry probability—by 1.34 percentage points initially and over 10 percentage points cumulatively within six years—even after controlling for changes in financial wealth. For existing stockholders, DC exposure increases the risky asset share in non-retirement portfolios. The impact on diversification is nuanced: while DC enrollment raises mutual fund ownership and funds' share of total financial wealth, it reduces funds' share within risky assets. Nevertheless, overall portfolio efficiency improves, as measured by a reduction in Sharpe ratio loss, and investors shift toward lower-cost funds. These results highlight the importance of informational participation costs in explaining limited stock market participation, suggesting that DC plans enhance financial market engagement by lowering barriers to entry.

Keywords: Pension-plan Design, Limited Stock Market Participation, Portfolio Choice, Financial Literacy, Diversification

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1 Introduction

The rapid expansion of Defined Contribution (DC) pension plans worldwide, driven in part by the funding issues faced by traditional Defined Benefit (DB) plans, is one of the most profound transformations in retirement savings in the last decades. This change has transferred the responsibility of investment decisions from institutions to individuals, bringing millions into direct contact with financial markets. For a large number of households this is their very first exposure to equity investments since stock market participation rates outside retirement plans is limited.[1]

In this paper I study the impact of DC plan access on the investment behavior of individuals in their non-retirement accounts. This is a particularly important question given the expansion of DC pension plans worldwide, and its potential to re-shape household portfolio decisions. This is a difficult question to answer because DC plan participation is typically not randomly assigned, and thus may correlate with individual saving and investment preferences, potentially biasing the results. This concern has therefore limited previous research to provide descriptive and correlational evidence on this specific topic.[2]

I establish a causal relationship between DC plan exposure and changes in non-pension investment behavior by leveraging exogenous variations in DC enrollment rules across cohorts and time in Sweden between 2000 and 2007. Specifically, I exploit both the differences in the DC enrollment age rules between different occupations and staggered rule changes in Sweden's occupational pension system to isolate the causal effect of DC plan enrollment on investment behavior in non-retirement accounts. I find that DC plan access leads to an initial 1.3pp (6.7% relative to unconditional mean of entry probability in the sample) increase in stock market entry decisions, with a cumulative rise of 10pp (about 56.5% relative to unconditional cumulative entry probability in the sample) over the next five years, consistent with a gradual learning process and inertia in household portfolio decisions. Furthermore, I show that exposure to DC plans also influences the risk-taking behavior of existing stockholders, who increase the risky share in their non-retirement accounts by 10pp (a 22.7% increase relative to the baseline mean of 44%) over 6 years.[3] I also find that DC enrollment modestly reduces the likelihood of exiting the stock market, reinforcing the participation effects. Collectively, these results highlight the potential of DC plans to shape broader financial behavior beyond retirement savings only.

The increase in stock market entry rates has important implications for theories of limited stock market participation. The existing literature has studied the role of participation costs (both monetary and infor-

[1] Only 30% of the US population participate in the stock market through non-retirement accounts based on the 2022 Survey of Consumer Finances data. Furthermore, in the subsample of people without any retirement savings, stock market participation is only 8%. Guiso, Sapienza, and Zingales (2008), Christelis, Georgarakos, and Haliassos (2013) and Badarinza, Campbell, and Ramadorai (2016) document significantly lower numbers for most other countries.

[2] Weisbenner (2002), Karlsson et al. (2007), Dahlquist, Setty and Vestman (2018).

[3] Following a common practice in the literature this is measured using the active change in risky share, to control for movements in asset prices and thus isolate pure rebalancing behavior.

mational) and first-order risk aversion in preferences for explaining the stock market participation puzzle.[4] First-order risk aversion would not predict a change in participation since households' preferences remain unchanged. Monetary participation costs (e.g. account fees) would also predict no effect once we control for changes in wealth, and a negative effect unconditionally, if households respond to the DC plan entry by reducing their non-retirement savings. Therefore, my results are strong evidence in support of informational participation costs, arising for example from limited financial literacy (Calvet, Campbell, and Sodini (2007), Van Rooij, Lusardi, and Alessie (2011)) or lack of trust in financial institutions (Guiso et al. (2008), Georgarakos and Pasini (2011)).

It is important to clarify that my results do not rule out first-order risk aversion or monetary participation costs as important factors contributing to limited stock market participation. They are just not potential explanations for my findings, which therefore highlight the role of informational costs and learning about stock market investments through the DC exposure. In fact, consistent with the importance of monetary participation costs as well, I find that the effect is much weaker once I do not control for changes in wealth, while further tests show that DC enrollment decreases private savings. This wealth effect implies a reduction in stock market entry by making those costs more relevant, and therefore counteracts the learning channel that I am isolating in the baseline regressions with all the controls.

I investigate this further by inspecting in more detail the changes in investments of previous stockholders. More precisely I study whether they are indeed moving to a more diversified portfolio as suggested by the learning channel. Here the result is mixed. On one hand, DC plan enrollment increases the probability of investing in mutual funds, and individuals do allocate a larger share of their financial wealth to mutual funds. However, the share of mutual funds as a fraction of risk asset holdings, stocks plus mutual funds, actually decreases. This suggests a potential (naive) substitution effect with investors viewing their mutual fund investments within DC plans as fulfilling their diversification needs. Beyond these composition effects, I find that DC enrollment improves overall portfolio efficiency: the Sharpe ratio loss (a welfare-relevant measure of diversification quality) declines significantly, and investors shift toward lower-cost mutual funds. This nuanced finding highlights the complexity of investor behavior and the interplay between learning, preferences, and investment choices.

To further probe the learning mechanism, I conduct several additional tests. First, I examine sibling spillovers by studying whether individuals who are not directly treated but have a sibling who enrolls in a DC plan also change their investment behavior. The absence of spillover effects suggests that learning operates through direct exposure and "learning-by-doing" rather than through second-hand information sharing. Second, I control for cumulative stock market returns since enrollment to rule out the possibility that results are driven by coincidental exposure to favorable market conditions. Third, I assess sensitivity to potential violations of parallel trends using the approach of Rambachan and Roth (2023), finding that

[4]See Gomes, Haliassos, and Ramadorai (2021) for a survey of this literature.

short-run effects are robust to sizable departures from the identifying assumption. Finally, I complement the baseline two-way fixed effects estimates with alternative difference-in-differences estimators designed for staggered adoption, including the extended TWFE approach of Wooldridge (2025) and the doubly-robust estimator of Callaway and Sant'Anna (2021), confirming the main findings.

My identification exploits the specifics of occupational DC plans in Sweden. The occupational pension system in Sweden is as close as a national-wide and covers 90% of the workforce through four trade unions, covering different occupational groups. The enrollment to the DC plan is automatic once a person reaches the eligibility age.[5] Once enrolled in an occupational DC, employees get an individual account where they can make their own investment choices, with employer contributions of around 4.5% of income, depending on the collective agreement. A narrow group of union representatives decides on the pension collective agreements, so the changes in rules are exogenous to the preferences of individual employees. For instance, blue-collar workers experienced a drop in enrollment age from 28 to 22 in 2000, then down to 21 by 2002. For local government workers, the change happened in 2004 and decreased the enrollment age from 28 to 21. I further leverage these changes as a natural experiment to study how individuals, affected at different age and at different points in time, altered their investment behavior. Importantly, the enrollment to a DC plan is equivalent to the enrollment to the occupational pension for the first time for the particular subsample of cohorts studied in this research.[6]

To rigorously assess investment behavior, I focus on several key financial measures: stock market participation (whether an individual holds any stocks or equity funds outside of pension accounts), the change in risky asset allocation (the proportion of non-pension financial wealth invested in risky assets), and the active change in risky assets (which accounts for inertia and represents deliberate rebalancing decisions). To further examine asset composition, I consider diversification metrics, including the probability of buying equity funds (whether an individual's brokerage account shows new inflows toward new or existing funds compared to the previous year), the share of funds within risky asset holdings, and the share of funds in total financial wealth. These measures capture both the extensive and intensive margins of investment behavior, offering a comprehensive view of how exposure to equities through DC pension plans may influence individuals' non-pension investment decisions.

To approach the question, I use a dynamic difference-in-differences (DiD) approach, where the treatment indicator is enrollment in a DC account. This method takes advantage of the staggered introduction of DC plans across different sectors and age groups in Sweden. By comparing individuals who got a DC account to those who have not enrolled yet, I can isolate the effect of DC plan exposure on investment behavior.

[5]Hence, the setting avoids the selection issue of eligibility for a DC plan versus enrollment, the common barrier for the identification.

[6]The only group in the sample who got the DC account after already having a DB pension is the central government employees aged 29 and older in 2000. These specifics are discussed in more details in Appendix A. This particular group is dropped at the sample selection stage described in Section 2.3.

In contrast to the static DiD approach, which estimates the average treatment effect for the treated (ATT) by comparing the average change in outcomes experienced by the treated group to the average change in outcomes experienced by the control group, my dynamic specification utilizes a set of relative-time indicators, capturing the time periods leading up to the enrollment to a DC account (leads) and the second summation captures the time periods following treatment (lags). This allows me to track changes in investment behavior inside non-retirement accounts over time and understand both short- and long-term effects.

Additionally, it is important to consider that the control group might also be indirectly affected. Individuals who have not yet enrolled in a DC plan might be influenced through social networks and anticipation effects or even may have exposure to other (e.g. DB, public pension DC, or private) pension accounts. These spillover effects could lead the control group to exhibit changes similar to the treated group, thereby reducing the observed differences between them. Consequently, the estimated effect of DC plan exposure on investment behavior would be biased downward, suggesting that my findings likely represent conservative estimates of the true impact.

The data for the analysis come from the Swedish Wealth and Tax Registry, a comprehensive dataset covering detailed information on finances, demographics, and employment for the whole population from 1999 to 2007. The sample selection criteria for this study include individuals aged between 18 and 34. This age range captures the variation in eligibility for the enrollment in a pension plan, and ensures that most individuals can be tracked back for at least 3 periods before and up to 7 years after treatment.[7] Furthermore, I apply several filters to ensure the robustness of the data. First, only those who are employed and have a positive labor income are included, focusing on economically active individuals. Second, the sample is restricted to those working within one of the four union group occupations, excluding military personnel. A financial wealth (FW) threshold is set, requiring individuals to have more than 500 (3,000) SEK (about 50 (300) USD) in pre-treatment years to emphasize relevant financial activities. Finally, only individuals who did not change their broad occupational group (one of the previous four categories) during the sample period and have complete occupational data are considered. The resulting dataset comprises around 4.31 million observations for approximately 672,500 unique individuals, covering the years from 1999 to 2007.

In my dataset I do not observe the actual investments in the DC account. This is not a direct concern since my object of study is the asset allocation decision in the non-retirement account. However, it will create some measurement error in the classification of treated individuals since some of them might not actually invest in equities in their DC accounts. Importantly this particular "fuzzy treatment" will only lead to a downward bias in my results since it is a one-sided mis-classification of non-treated as treated, but not the opposite.

[7] To make sure all individuals in the sample can be observed for some time before the treatment, I filter out those who were older than 28 in year 1999, as this is their first year in a DC plan, and corresponds to the first available year in the data.

I first estimate the dynamic treatment effects in a two-way fixed effects (TWFE) setting.[8] Recent work suggests that dynamic effect estimates from event-study estimators are problematic. Sun and Abraham (2021) show that, in the presence of staggered treatment timing and treatment effect heterogeneity, TWFE dynamic effect estimates for one relative-time period are contaminated by the causal effects of other relative-time periods in the estimation sample. I use the approach proposed by Wooldridge (2025) to correct the estimations for this potential bias using extended two-way fixed effect approach and Callaway and Sant'Anna (2021). I find that the results are robust to this alternative estimation procedure.

My research makes several important contributions. First, it provides strong evidence on the causal impact of DC plan exposure on non-retirement investments by using a natural experiment. The policy changes in Sweden that altered enrollment age for occupational DC plans provide a unique opportunity to study this effect without the usual biases. Second, the study explores whether DC plans can act as a form of financial education, increasing people's understanding and confidence in managing investments. It thus highlights the importance of financial literacy and learning for explaining investment decisions of households, and limited stock market participation in particular. Third, it contributes to discussions on retirement policy by showing how the design of pension plans can influence overall financial behavior.

This paper adds to the literature on portfolio choice in the presence of retirement accounts. The shift towards DC plans, where employees bear the investment risk, has led to significant research on how individuals manage their investments within these accounts, both empirically and theoretically (e.g., Madrian and Shea (2001), Choi, Laibson, Madrian, and Metrick (2002), Dammon, Spatt, and Zhang (2004), Gomes, Michaelides, and Polkovnichenko (2009)). Studies have also explored the impact of institutional settings on investment behavior, highlighting the significance of pension plan design and financial education (see Bernheim and Garrett (2003), Chetty, Friedman, Leth-Petersen, Nielsen, and Olsen (2014), Choukhmane (2025), Lindholm, Peters, and Sandström (2022)). However, these works do not fully address how DC plan exposure affects non-pension investments. Research examining the link between retirement plan design and the composition of savings indicates a positive relationship between the presence of DC plans and stock market participation (e.g., Weisbenner (2002), Karlsson, Massa, and Simonov (2007), Dahlquist, Setty, and Vestman (2018)). Many of these studies, however, face challenges in establishing causal interpretations due to potential correlations with unobserved individual characteristics. Nielsson, Press, Rangvid, and Setty (2025) studies choice-based switches within a pension fund using matching, while my design leverages exogenous cohort-by-sector shifts in DC eligibility. I confirm their central mechanism -— learning that lowers participation costs, yet identify it over policy timing rather than product choice. Relatedly, Bartscher and

[8] In contrast to a 'standard' TWFE DiD estimation, where the fixed effects are unit and time-specific, I estimate the regression in differences and control for time and age fixed effects. This is important since there are life-cycle patterns inherent to the participation and risky share, and it is impossible to separately identify age, time and cohort effects in levels, since they are perfectly collinear. (Poterba and Samwick (1997), Ameriks and Zeldes (2004), Fagereng, Gottlieb, and Guiso (2017), Catherine (2022), Gomes and Smirnova (2022)).

Mann (2025) document two-way spillovers between pension and non-pension investing in Denmark, they find that early adopters of the 2019 tax-favored accounts become more likely to switch pension risk, and a 2017 pension risk-choice reform increases non-pension stock market participation consistent with learning and salience mechanisms. There is also extensive literature on factors contributing to limited stock market participation, focusing on household preferences, participation costs, risks, and peer influences (e.g., Haliassos and Bertaut (1995), Vissing-Jorgensen (2002), Guiso et al. (2008), Calvet et al. (2007), Gomes and Michaelides (2003), Duraj, Grunow, Haliassos, Laudenbach, and Siegel (2024)). This paper adds to this literature by emphasizing the importance of informational participation costs in stock market entry decisions and asset allocation.

In summary, despite the significant implications of DC plan expansion, there is a notable gap in empirical research on the causal relationship between DC plan exposure and non-pension investment behavior. This study addresses this gap by leveraging a natural experiment in Sweden, where policy changes created exogenous variation in DC plan enrollment.

The remainder of this paper is organized as follows. Section 2 describes the data, the institutional setting of the Swedish pension system and the policy changes that create the natural experiment. Section 3 discusses the empirical strategy, including the identification strategy and testable theoretical predictions. Section 4 presents the main findings, including the impact of DC plan exposure on stock market participation and portfolio risk composition. It also discusses the mechanisms underlying these effects. Section 5 provides a series of additional mechanism tests, including analyses of diversification, portfolio efficiency, wealth effects, sibling spillovers, and controls for stock market returns. Section 6 presents robustness checks using alternative difference-in-differences estimators. Section 7 discusses external validity and policy-related interpretation. Finally, Section 8 concludes the paper by summarizing the key contributions.

2 Data, Sample Selection and Institutional Setting

In this section, I provide a detailed description of the data sources, the sample selection process, and the institutional context relevant to my analysis.

2.1 Data

I use the tax registry data supplied by Statistics Sweden spanning eight years (1999-2007). The data covers the whole population in Sweden and provides detailed information on demographic characteristics, balance sheets, family links and more.[9] Since Swedish tax law prohibits joint filing, all tax returns are filed individually.

[9] Applications of the same data include, but are not limited to Calvet et al. (2007), Calvet, Campbell, and Sodini (2009a), Calvet, Campbell, and Sodini (2009b), Girshina (2019), and Bach, Calvet, and Sodini (2020).

Demographics Demographic information includes age, gender, marital status, education, and place of residence, as well as household composition and identification number. The household identification number allows us to group residents by living units and thus investigate finances at the family level.

Occupation and Pension Scheme Workers in Sweden are enrolled in one of four main pension agreements depending on their occupation: blue-collar workers, white-collar workers, central government employees, and local government employees. These pension agreements are negotiated with the respective union confederations, which is why the occupational pension plans are standardized at this level of aggregation. I discuss this in more detail in the following subsection 2.2 on institutional setting and identification, and additionally in Appendix A.

Unfortunately, these agreements cannot be explicitly matched to individual workers or firms in my data. Instead, I proxy for this based on data on workers' occupations and industry codes. To determine whether individuals are in blue-collar or white-collar occupations, I use the 3-digit occupational code from the Standard for Swedish Classification of Occupations (SSYK). All occupations up to code 415 are considered blue-collar (including missing values), whereas codes above 415 are classified as white-collar.[10] Finally, there is an indicator for public sector workers, which distinguishes between local and central government employees.

Income Income information is available both in total and by source of income. For capital income, the dataset provides, for each individual bank account or security, the income earned (such as interest and dividends) as well as all sales transactions, including the quantity and price of the securities sold throughout the year. However, the exact sale dates and details of asset purchases are not reported (since they are not taxed).

Labor income data includes gross labor income as well as information about the business sector. In addition, the dataset provides records of public and private pension income, military service compensation, and welfare transfers. Welfare transfers include unemployment benefits, workers' compensation, paternity and maternity allowances, student allowances, housing allowances, and disability support.

The sum of all income sources, including labor, capital, pensions, and welfare transfers, constitutes an individual's disposable income. This income reflects the total resources available for consumption, savings, and investment, accounting for all revenue streams reported in the dataset.

Wealth To construct individual wealth, I utilize data from the Swedish Wealth Registry and tax records, which compile information from tax returns as well as financial institutions and banks. This dataset provides

[10]The detailed data on occupational code SSYK3 is available starting in 2001, so the information is backdated for the years 1999 and 2000. If an individual has a working status in all three years, they are assigned the same occupational code as in 2001.

comprehensive details on all financial and real estate assets, as well as formal loans for every Swedish resident from 1999 to 2007, recorded annually on December 31.

Financial Wealth (FW_t) is defined as the total value of non-pension liquid assets. It includes both *risky assets* (such as mutual funds excluding money market funds, and directly held stocks)[11] and *risk-free assets* (such as money market funds and bank account balances). Stocks refer to direct holdings only, while risky mutual funds are classified into bond funds or equity funds. Equity funds broadly encompass any fund that invests partially in stocks, meaning balanced funds are counted as equity funds. Bank account balances are reported if interest earned exceeds 100 Swedish kronor (for the period from 1999 to 2005) or if the year-end balance surpasses 10,000 kronor (in 2006 and 2007). I impute unreported cash balances using the method developed by Calvet et al. (2007). Financial holdings other than bank accounts are constructed using information at the individual security level, identified by International Security Identification Number (ISIN), with pricing data sourced from FINBAS, Datastream, and Morningstar for Nordic stocks and mutual funds.

Real Estate Wealth (HW_t) includes both residential and commercial properties owned by individuals. The value assigned to real estate reflects its market value. For apartments, market values are based on actual transaction prices and adjusted by local price indices between transactions. For other real estate, Statistics Sweden estimates the market value using tax assessments, property characteristics, hedonic pricing models, and local price indices derived from transaction data.

On the liabilities side, I define *debt* (D_t) as the sum of outstanding student loans (termed as student debt) and other liabilities, including mortgages and other types of loans to financial institutions (collectively, non-student debt).

Liquid Wealth (LW_t) is defined as the sum of financial wealth and real estate wealth. *Total Wealth* (TW_t) is calculated as liquid wealth minus total debt. Throughout this paper, all values are reported in Swedish kronor (SEK) and adjusted for inflation to 2010 price levels.

A limitation of the data is that it lacks comprehensive information on pension wealth, since it is tax exempt. Specifically, I do not observe the complete details regarding state or occupational pension accounts, neither their current value nor the investment options within them. While some data is available on private pension contributions, I use proxies for PPM (premium pension) wealth and occupational pensions based on collective agreement rules.

[11] The dataset also contains holdings of bonds, derivatives, and structured financial products; however, participation in these assets is low, and they form only a minor portion of household portfolios. The data also provides the total value of capital insurance products but does not specify their underlying asset allocation. Following Calvet et al. (2007) and Calvet et al. (2009a), these assets are excluded from the analysis for simplicity.

2.1.1 Indicators of Stock Market Activity

I characterize individuals' stock market behavior along three dimensions: (i) participation dynamics (entry), (ii) portfolio risk-taking (the risky share and its active component), and (iii) the composition of risky holdings (mutual funds versus directly held stocks). In addition, I construct an active change in wealth measure that nets out contemporaneous wealth movements mechanically induced by portfolio rebalancing.

Participation and entry Stock market participation is an indicator equal to one if individual j holds a strictly positive amount of risky financial assets in non-retirement accounts at the end of year t, and zero otherwise. Entry is defined as a transition from non-participation to participation between $t-1$ and t (i.e., $0 \to 1$).

Risky share and changes in risk-taking Let A_{jt} denote the market value of risky assets and C_{jt} the market value of riskless (cash and cash-like) assets in non-retirement accounts at the end of period t. The risky share of financial wealth is

$$\omega_{jt} = \frac{A_{jt}}{A_{jt} + C_{jt}}. \tag{1}$$

I compute risky wealth as the sum of position values across all risky securities,

$$A_{jt} = \sum_{i \in \mathcal{R}} Q^i_{jt} P^i_t, \tag{2}$$

where Q^i_{jt} is the number of units held and P^i_t is the period-t price. The change in risky share is

$$\Delta\omega_{jt} = \omega_{jt} - \omega_{j,t-1}. \tag{3}$$

A limitation of $\Delta\omega_{jt}$ is that it conflates deliberate rebalancing with passive valuation effects: even in the absence of trading, differential returns on risky and safe assets mechanically change portfolio shares. To isolate the component attributable to trading, I follow Calvet et al. (2009a) and construct the active change in risky share. I first compute counterfactual (passive) holdings under the assumption of no trades between $t-1$ and t:

$$A^p_{jt} = \sum_{i \in \mathcal{R}} Q^i_{j,t-1} P^i_t, \tag{4}$$

$$C^p_{jt} = \sum_{k \in \mathcal{C}} Q^k_{j,t-1} P^k_t, \tag{5}$$

and define the passive risky share as

$$\omega^{passive}_{jt} = \frac{A^p_{jt}}{A^p_{jt} + C^p_{jt}}. \tag{6}$$

The active change in risky share is then

$$\Delta\omega_{jt}^{active} = \omega_{jt} - \omega_{jt}^{passive}, \tag{7}$$

which captures deviations from the no-trade counterfactual and is therefore driven by active rebalancing rather than passive returns.[12]

Active wealth changes Following Di Maggio, Kermani, and Majlesi (2020), I decompose observed wealth changes into a mechanically implied component driven by changes in risky exposure and a residual active component. Specifically, I compute the wealth change that would arise solely from changing the risky share from $\omega_{j,t-1}$ to ω_{jt} while holding initial wealth $W_{j,t-1}$ fixed and applying period-t risky and risk-free returns:

$$\widetilde{\Delta W}_{jt} = W_{j,t-1}\Big(\omega_{jt}R_t + (1-\omega_{jt})R_t^f\Big) - W_{j,t-1}\Big(\omega_{j,t-1}R_t + (1-\omega_{j,t-1})R_t^f\Big). \tag{9}$$

I make the conservative assumption that any portfolio reallocation is implemented at the beginning of period t; if instead reallocation occurred at the end of period t, contemporaneous wealth changes would be unaffected by the change in ω during period t. I then define the active change in wealth as

$$\Delta W_{jt}^{Active} = \Delta W_{jt} - \widetilde{\Delta W}_{jt}, \tag{10}$$

where $\Delta W_{jt} = W_{jt} - W_{j,t-1}$ is the observed change in wealth. Intuitively, ΔW_{jt}^{Active} captures wealth changes driven by savings and withdrawals rather than by contemporaneous changes in risky exposure.

Composition of risky holdings To capture how investors allocate risky wealth across diversified funds versus directly held stocks, I compute the mutual-fund share of risky assets. Let S_{jt} denote the market value of directly held stocks and F_{jt} the market value of mutual funds (or other diversified equity funds) at the end of period t:

$$S_{jt} = \sum_{i\in\mathcal{S}} Q_{jt}^i P_t^i, \tag{11}$$

$$F_{jt} = \sum_{i\in\mathcal{F}} Q_{jt}^i P_t^i. \tag{12}$$

[12] Acknowledging that survey-based data often lack security-level returns needed to construct passive rebalancing, I also compute an alternative passive risky share and corresponding alternative active change, $\Delta\omega_{it}^{active_{alt}}$, using aggregate returns on risky assets R_t (U.S.-based in this case) and the risk-free return R_t^f:

$$\omega_{it}^{passive_{alt}} = \frac{\omega_{i,t-1}R_t}{\omega_{i,t-1}R_t + (1-\omega_{i,t-1})R_t^f}\,. \tag{8}$$

I consider two fund-share measures:

$$F_{jt}^{\%Risky} = \frac{F_{jt}}{F_{jt} + S_{jt}}, \tag{13}$$

$$F_{jt}^{\%FW} = \frac{F_{jt}}{FW_{jt}}, \tag{14}$$

where FW_{jt} denotes total non-retirement financial wealth. I define changes in fund shares as

$$\Delta F_{jt}^{\%Risky} = F_{jt}^{\%Risky} - F_{j,t-1}^{\%Risky}, \tag{15}$$

$$\Delta F_{jt}^{\%FW} = F_{jt}^{\%FW} - F_{j,t-1}^{\%FW}, \tag{16}$$

computed conditional on the denominators being defined in both periods.

2.2 Occupational Pension in Sweden and Identification Strategy

Sweden's pension system is composed of two main pillars: a universal public pension system[13] and a mandatory occupational pension system for workers whose employers participate in an occupational pension plan.

Occupational pensions are an important part of the pension system, accounting for an estimated 24% of total pension savings. Workers are covered by four central collective agreements between the confederations of unions on the Swedish labor market: two for the private sector (ITP for white-collar workers and Avtalspension SAF-LO for blue-collar workers), and two for the public sector (PA03 for central government workers and KAP-KL for local government workers).[14] Approximately 90% of the Swedish workforce is entitled to occupational pensions (Pensionsåldersutredningen, 2012).[15] This high and growing number is due to the fact that, firstly, almost every company in Sweden is part of one of the unions, and secondly, employees are enrolled in occupational pensions regardless of their contract terms and whether they are union members or not.[16] Contributions to occupational pensions are typically around 4.5% of an individual's income up to the income cap in the public pension system, with higher contribution rates applied to income that exceeds the cap.[17] Importantly these contributions are made by the employer, not the employee, but they are placed

[13]The total contribution rate for public pension is 18.5% of earnings, with 16% going to the notional account and 2.5% allocated to the The Premium Pension Scheme. See Palme and Svensson (2004), Sjögren Lindquist and Wadensjö (2006), Glans (2009), Lindquist and Wadensjö (2009), Anderson (2015) for additional institutional details.

[14]The approximate decomposition of occupational groups is as follows: white-collar workers comprised 23%, blue-collar workers 36%, local government employees 33%, and central government employees 9% of the total workforce covered by collective agreements in 2007.

[15]Self-employed individuals, entrepreneurs, and business owners are not covered by occupational pensions under collective agreements, and they must actively choose to save for retirement, often through pension insurance or other retirement savings options.

[16]Occupational schemes regulated by collective agreements also cover sickness, work-related injury, shortage of work, death, and parental leave.

[17]Note that wages are tightly regulated in the collective agreement, leaving limited scope for employers to offset

into designated individual DC accounts. The individuals themselves then decide how to invest it across a pre-specified menu of investment funds.[18]

In my analysis I exploit both cross-sectional and time-series exogenous variation in enrollment in the DC plan. Cross-sectional variation arises because workers are only enrolled in the occupational DC plan once they reach a pre-determined age threshold, and this threshold varies across occupation groups. In addition, over my sample period there were changes in these age cutoffs, with a decrease in the DC enrollment age across all occupational groups. One reason for this decrease is the intended increase in the number of contribution accrual years to benefit employees. Although the direction of reform has been the same across sectors, the timing has differed. The timing of policy changes across occupations and cohorts provides exogenous variation in DC plan eligibility.

Figure 1 shows the differences in eligibility ages for the different occupation groups over time.

[INSERT FIGURE 1 HERE]

Figure 2 maps this variation into treatment status for each birth cohort–calendar year pair in the estimation sample, illustrating the staggered pattern of DC enrollment across panels.

[INSERT FIGURE 2 HERE]

To illustrate these differences and changes, consider the following cases. First, take two individuals of the same age but in different occupations. Person A is 25 years old in 2005 and works as a blue-collar worker. Person B is also 25 years old in 2005 but works as a white-collar worker. Following Figures 1 and 2, Person A, born in 1980, became eligible for the DC plan at age 22 in 2002 and has been enrolled for three years by 2005. On the other hand, the enrollment age for white-collar workers remained at 28 until 2007. It was only reduced to 25 in 2007 for those born in 1983 and later. Consequently, Person B, born in 1980, is not yet eligible for the DC plan in 2005 and will not be until turning 27 in 2007.

Similarly, consider two individuals in the same occupation and of the same age but from different cohorts. Person C is a local government worker aged 25 in 2003. Person D is a local government worker aged 25 in 2005. For local government workers, the enrollment eligibility age between 1998 and 2003 was at least 28 years old, and after 2004, the age was reduced to 21. Therefore, in 2003, Person C is not eligible for the DC plan at age 25 because the eligibility age is still 28. However, in 2005, Person D is eligible for the DC plan at age 25 since the eligibility age was reduced to 21 in 2004. Thus, although both are local government workers and of the same age, Person D benefits from the policy change and becomes eligible earlier than Person C did.

mandatory occupational pension contributions by unilaterally lowering wages.

[18] The same way as in 401(k) plan in the US.

As these collective agreements are universal in the sense that they are equally applied to all workers within a given occupational group (and cover approximately 90% of workplaces), these exogenous changes across occupations and cohorts provide a natural experiment for identifying the impact of pension plan type on investments outside of the retirement account. In particular this avoids endogenous selection concerns arising from individual saving preferences and employer selection effects.

2.3 Sample Selection

The dataset is filtered to align with the identification strategy. I focus on individuals aged 18–34[19], an age range that delivers meaningful variation in eligibility for enrollment in an occupational defined contribution (DC) plan and ensures that most individuals can be observed for at least one year prior to treatment and up to seven years after treatment within the sample window.

To guarantee the availability of pre-treatment observations, I exclude individuals who were age 28 or older in 1999. These individuals either enter the panel already treated or receive DC coverage at the very beginning of the panel, leaving little or no pre-treatment history. This restriction also helps exclude central government employees who were age 29 or older in 2000, because an additional policy change introduced a DC component for this group.[20]

I drop individuals classified as military personnel. I also exclude individuals with very low pre-treatment financial wealth—below 500 SEK (approximately 50 USD) for the participation/entry analysis and below 3,000 SEK (approximately 300 USD) for the portfolio-allocation analysis—as these observations contain little economically meaningful variation in portfolio choice measures.

To focus on active labor-market participants, I retain only individuals with positive labor income and restrict the sample to those employed in one of the four occupational groups covered by the collective agreements in my setting. I exclude individuals who switch occupational groups during the sample period to avoid conflating exposure to different pension agreements. Finally, I remove observations with missing or incomplete occupation information to ensure consistent assignment to treatment cohorts and reliable construction of key variables.

Although the panel begins in 1999, the regressions are estimated in first differences of participation, risky share, and wealth, which requires consecutive annual observations. Consequently, the earliest cohort that can be followed in the event-study framework is the 2001 cohort, since it is the first cohort for which I can compute changes from 2000 to 2001 while also observing at least one pre-treatment year. The hatched cells in panel (a) of Figure 2 show the excluded blue-collar cohorts first treated in 2000.

After applying these restrictions, the analysis sample contains approximately 4.31 million person-year

[19] I limit to 20–34 for stock market participants sample.

[20] All other collective agreements had switched to a DC design earlier, either fully or for at least half of the contribution amount, as described in Appendix A.

observations for about 672,500 individuals observed between 2000 and 2007.

2.4 Descriptive Statistics

The descriptive statistics provide an overview of the full analysis sample and the subset of stock market participants.

Panel (a) of Table 1 reports summary statistics for the full sample ($N \approx 4.31$ million person-year observations). Individuals are relatively young and are not married, but a half of them have children. Education is measured in discrete categories, with the median corresponding to completed high school, and about one quarter having completed a three-year general or vocational university program. Disposable income averages 169,635 SEK (SD 68,443). Just over half of observations correspond to stock market participants. Stock market entry is at 7% (measured for individuals observed in consecutive years). Mean financial wealth is 70,077 SEK with substantial dispersion (SD 121,987). Wealth dynamics are volatile: the average change in the wealth-to-income ratio is 0.05, the mean log change in financial wealth is -0.04 (with an active log-change measure of -0.15), and the mean growth measure $\Delta_g FW$ is 0.63.

Panel (b) restricts the sample to individuals who participate in the stock market in both $t-1$ and t ($N \approx 2.06$ million), and reports portfolio-allocation variables. The average risky share is 44%, while average changes in risky share are close to zero (mean $\Delta w = -0.01$, mean $\Delta w^{active} = -0.01$, and mean $\Delta\omega^{active_{alt}} = 0.01$). Participants allocate a large fraction of risky assets to mutual funds (mean $F^{\%Risky}$ is 76%) and about one third of total financial wealth to funds (mean $F^{\%FW}$ is 32%). The probability of buying mutual funds in a given year is 63%. Fund cost information is available for a subset of fund holders ($N \approx 0.64$ million), with an average annual fund cost of 1.45% (SD 0.27). Overall, the percentiles indicate substantial heterogeneity in wealth and portfolio behavior within the participant sample.

[INSERT TABLE 1 HERE]

3 Empirical Approach and Testable Predictions

Identifying the causal effect of retirement plan type on stock market activity outside retirement accounts requires exogenous variation in individuals' eligibility for DC plans. The ideal experiment would randomly assign access to a DC plan; differences in subsequent stock market activity could then be interpreted as causal effects of plan type. In the U.S., while 401(k) eligibility varies substantially across households at a point in time, eligibility is often correlated with unobserved saving preferences. As emphasized by Bernheim (1997) and Engen, Gale, Scholz, Bernheim, and Slemrod (1994), individuals with stronger tastes for saving may sort into employers offering more generous retirement benefits, and employers may introduce plans in

response to employee demand. As a result, most observed changes in 401(k) eligibility are either broad-based or plausibly endogenous to determinants of household saving behavior, complicating causal inference.

My empirical strategy instead exploits quasi-experimental variation from institutional changes in Sweden. Between 2000 and 2007, several collective pension agreements lowered the age of enrollment into occupational DC plans at different times across sectors (Figure 1). For example, for blue-collar workers the enrollment age fell from 28 to 22 in 2000 and then to 21 by 2002, while for local government workers the corresponding reform occurred in 2004, lowering the enrollment age from 28 to 21. These staggered, sector-specific shifts generate plausibly exogenous variation in the timing of DC enrollment across cohorts within occupation groups. I use this variation as a natural experiment to study how exposure to a DC plan affects investment behavior in non-retirement accounts.

I focus on outcomes that capture stock market activity outside retirement accounts, including stock market participation (entry) and changes in risky asset allocation (including active changes in risky share). I estimate dynamic difference-in-differences event-study specifications where treatment is enrollment into an occupational DC account. The dynamic design replaces a single post-treatment indicator with a set of relative-time indicators (leads and lags), allowing me to trace both pre-trends and the evolution of treatment effects over time.

In my baseline analysis, I estimate these dynamic effects in a two-way fixed-effects framework.[21] I slightly deviate from the canonical unit-and-time FE specification by estimating the regression in first differences and controlling flexibly for age and time. This approach is motivated by the strong life-cycle patterns in participation and portfolio choice and by the age–period–cohort collinearity problem in levels (Ameriks and Zeldes, 2004, Catherine, 2022, Fagereng et al., 2017, Poterba and Samwick, 1997). Differencing addresses this identification issue for participation and risky share and absorbs time-invariant unobserved heterogeneity, including stable individual preferences (Gomes and Smirnova, 2022). In this setting, adding individual fixed effects provides limited additional identification while substantially reducing precision.

Because identifying variation comes from discrete, occupation-specific changes in enrollment rules at particular points in time, I cluster standard errors at the treatment-time-by-occupation level, following the general recommendation to cluster at the level of treatment assignment in DiD designs (Abadie, Athey, Imbens, and Wooldridge, 2023, Bertrand, Duflo, and Mullainathan, 2004). Since the number of such clusters is small (fewer than 40), I report p-values based on the wild cluster bootstrap, which performs better than conventional cluster-robust inference with few clusters (Cameron, Gelbach, and Miller, 2008, MacKinnon and Webb, 2017).

Finally, in Appendix D.2 I assess robustness to deviations from parallel trends using Rambachan and Roth (2023) sensitivity intervals, which quantify how large pre-trend violations would have to be to overturn

[21] Section 6 complements the analysis with extended two-way fixed effects and Appendix D.3 estimators developed for staggered adoption, which both show similar results.

the baseline conclusions.

3.1 Stock market participation

Equation 17 formalizes the event-study specification used to estimate how stock market entry varies with time relative to occupational DC enrollment:

$$P(Entry_{it}) = \alpha + \sum_{j=-2}^{-1} \beta_j I[\hat{\tau}^{DC}=j] + \beta_1 I[\hat{\tau}^{DC}=1] + \sum_{j=2}^{6} \beta_j I[\hat{\tau}^{DC}=j] + \theta X_{it} + I[a_{it}] + I[\text{occupation} \times y_t] + I[y_t] + \varepsilon_{it}. \quad (17)$$

The outcome $Entry_{it}$ is an indicator equal to one if individual i enters the stock market between $t-1$ and t, i.e., $Partic_{i,t-1} = 0$ and $Partic_{it} = 1$. Event time is measured by $\hat{\tau}^{DC}$, the number of years relative to the year before DC enrollment, i.e. $\hat{\tau}^{DC} = 0$ omitted as the reference period. The coefficients β_j therefore trace out the dynamic effect of DC enrollment on the probability of stock market entry at horizon j relative to the reference period, after netting out common time effects and differences absorbed by the fixed effects.

X_{it} is a vector of time-varying controls. The specification includes occupation-by-year fixed effects $I[\text{occupation} \times y_t]$, age dummies $I[a_{it}]$, and year fixed effects $I[y_t]$. Standard errors are clustered at the treatment-time-by-occupation level, and ε_{it} denotes the regression residual.

I estimate treatment effects on stock market entry using a linear probability model (LPM). The LPM delivers coefficients that are directly interpretable as percentage-point changes in entry probabilities and accommodates the high-dimensional fixed effects required by the design (calendar-time effects and occupation-specific time effects, and in some specifications individual fixed effects). Nonlinear panel models such as fixed-effect logit/probit are less attractive in this setting subject to incidental-parameters problems, and their marginal effects are nonlinear functions of the estimated parameters, complicating event-study interpretation.

3.2 Risky share

I next consider changes in risky asset allocation among stock market participants, restricting the sample to individuals who hold risky assets in both $t-1$ and t ($Partic_{i,t-1} = 1$ and $Partic_{it} = 1$). In addition to the raw change in risky share, I also study the *active* change in risky share, which adjusts for mechanical movements in portfolio weights induced by differential asset returns. This decomposition isolates deliberate rebalancing decisions by separating active trades from passive changes due to market fluctuations (see Eqs. 6–7). The objective is to assess whether exposure to an occupational DC plan (which typically increases equity exposure inside retirement accounts) is associated with changes in investors' risk-taking in their

non-retirement portfolios.

I estimate the following event-study specification:

$$\Delta\omega_{it}^{(a)} = \alpha + \sum_{j=-2}^{-1} \beta_j I[\hat{\tau}^{DC}=j] + \beta_1 I[\hat{\tau}^{DC}=1] + \sum_{j=2}^{6} \beta_j I[\hat{\tau}^{DC}=j] + \theta X_{it} + I[a_{it}] + I[\text{occupation} \times y_t] + I[y_t] + \varepsilon_{it}. \quad (18)$$

Here, $\Delta\omega_{it}^{(a)}$ denotes the (active) change in individual i's risky share between $t-1$ and t. Event time is measured by $\hat{\tau}^{DC} \equiv age_{it} - age_t^{rule} + 1$, so that $I[\hat{\tau}^{DC} = j]$ indicates that the observation is j years relative to the year before DC enrollment. The coefficients β_j trace out the dynamic effect of DC enrollment on changes in risky share at horizon j, relative to the omitted reference period, comparing treated and not-yet-treated observations while absorbing common shocks and life-cycle patterns.

X_{it} is a vector of time-varying controls, and the specification includes age dummies $I[a_{it}]$, occupation-by-year fixed effects $I[\text{occupation} \times y_t]$, and year fixed effects $I[y_t]$. Standard errors are clustered at the treatment-time-by-occupation level, and ε_{it} denotes the regression residual.

3.3 Testable Predictions

In this section I describe the specific mechanisms that I will be considering when testing the asset allocation response of non-retirement investments to the DC enrollment event. Appendix B presents a simple two-period model that nests the main mechanisms discussed below and Table A1 summarizes the directional predictions of each channel.

3.3.1 Participation Decision

A large household-finance literature studies, both theoretically and empirically, why many households do not participate in the stock market.[22] A leading class of explanations emphasizes *participation costs*. These include explicit monetary costs (e.g., brokerage fees), the opportunity cost of time (monitoring, account management, taxes), and informational frictions such as lack of knowledge about how to invest or about the benefits of diversification. Closely related, stock market participation is lower when trust in financial markets is weak (Giannetti and Wang, 2016, Guiso et al., 2008) and when financial literacy is limited (Calvet et al., 2007, Van Rooij et al., 2011), and it rises with education (Black, Devereux, Lundborg, and Majlesi, 2018) and cognitive ability (Grinblatt, Keloharju, and Linnainmaa, 2011).

In my setting, enrolling in an occupational DC plan does not mechanically change the *monetary* participation costs (MPC) of investing in *non-retirement* accounts. Holding fixed the benefits of participation, the

[22] See, for example, Vissing-Jorgensen (2002), Haliassos and Michaelides (2003), Gomes and Michaelides (2005), Bonaparte, Korniotis, and Kumar (2014), Gomes and Smirnova (2022), Choukhmane and de Silva (2024).

MPC channel therefore predicts a zero effect on stock market entry outside retirement accounts. Moreover, to the extent that DC enrollment crowds out liquid wealth accumulation (e.g., by reducing private saving in taxable accounts), it can reduce the incentives to pay a fixed participation cost, implying a *negative* effect when wealth is not controlled for.[23] Conversely, if DC enrollment increases total resources and relaxes fixed-cost thresholds (e.g., by raising financial slack), it could generate a *positive* effect on participation. Thus, even within a cost-based framework, the sign can depend on how DC enrollment affects the level and composition of taxable wealth.

By contrast, informational participation costs (IPC) naturally generate a *positive* prediction. Exposure to the DC plan—its investment menu, communications, and the experience of holding equity within a retirement account—can increase financial knowledge, reduce perceived complexity, and build trust in markets. It may also raise attention to investing and facilitate learning through workplace interactions, which can lower subjective participation barriers and increase the likelihood of entering the stock market outside the plan.

A second class of explanations is preference-based, including first-order risk aversion, ambiguity aversion, or other non-standard preferences that make stockholding unattractive.[24] If preferences are fixed, this *preference channel* predicts no effect of DC enrollment on non-retirement participation. However, some preference-based mechanisms can still imply changes if DC enrollment alters *beliefs* rather than tastes: by providing experience with equity investing, it may reduce ambiguity or perceived downside risk, raising participation. Similarly, theories emphasizing hedging motives related to the correlation between labor income and stock returns would predict little change, since DC enrollment does not directly alter that correlation (Benzoni, Collin-Dufresne, and Goldstein, 2007, Catherine, 2022).

Finally, two additional mechanisms can work against increases in non-retirement participation. First, if households manage risk at the level of the consolidated balance sheet (taxable plus retirement wealth), then equity exposure inside the DC account can substitute for stockholding outside the plan, reducing the marginal benefit of entering the stock market in taxable accounts (a *substitution/crowd-out* effect). Second, because DC wealth is relatively illiquid, enrollment may increase the value of holding liquid, safe assets outside the plan for precautionary reasons, dampening stock market entry in taxable accounts (a *liquidity/precautionary* channel). Overall, the net effect of DC enrollment on non-retirement participation is therefore theoretically ambiguous, with positive predictions under learning and information spillovers and negative predictions under substitution or liquidity-based motives.

[23] With fixed costs of participation, lower taxable financial wealth can make stock market entry suboptimal.

[24] See, for example, Ang, Bekaert, and Liu (2005), Cao, Wang, and Zhang (2005), Gomes (2005), Barberis, Huang, and Thaler (2006), Chapman and Polkovnichenko (2009), Campanale (2011), Dimmock, Kouwenberg, Mitchell, and Peijnenburg (2016), Pagel (2018), Peijnenburg (2018), and Calvet, Celerier, Sodini, and Vallee (2023b).

3.3.2 Asset Allocation of Previous Stockholders

DC enrollment can affect the risky share of non-retirement portfolios through several channels. First, a learning mechanism implies that exposure to the DC plan menu (and to equity investing more generally) may increase financial knowledge, lower perceived information and participation costs, and raise confidence in equity markets. Consistent with this channel, DC enrollment may also increase attention to investing and improve access to low-cost products through employer communication, peer effects, or greater familiarity with indexed mutual funds. Together, these mechanisms predict an increase in the risky share of non-retirement wealth, as investors become more willing to hold risky assets and better understand diversification, and may also shift the composition of risky holdings toward lower-cost vehicles.

Second, tax incentives may change asset location across accounts. Because returns inside DC accounts are tax-deferred, households may reallocate assets between taxable and tax-advantaged accounts to improve after-tax returns. In particular, Dammon et al. (2004) show that investors can benefit from holding relatively tax-efficient equities in taxable accounts while placing taxable bonds in tax-deferred accounts. This tax-optimization channel therefore also predicts a positive effect on the risky share held outside retirement accounts.

Third, if households exhibit decreasing relative risk aversion (DRRA), optimal portfolio risk-taking rises with wealth. In this setting, the effect of DC enrollment depends on the reference point for risk-taking: if households choose risk exposure with respect to total wealth (including retirement wealth), additional DC saving may increase desired risk-taking; if instead households engage in mental accounting and primarily evaluate risk relative to non-retirement wealth, DC enrollment may crowd out liquid saving and mechanically lower the risky share measured within taxable wealth.

Additional mechanisms can work in the opposite direction. If households target a desired level of overall risk across taxable and retirement accounts, increased equity exposure inside the DC plan can induce offsetting rebalancing in taxable accounts, lowering the risky share of non-retirement wealth. Moreover, since DC wealth is relatively illiquid, enrollment may reduce liquid buffers and increase precautionary saving motives, leading households to tilt taxable portfolios toward safer and more liquid assets. At the same time, if greater retirement saving increases perceived financial security and lowers background risk, the liquidity channel may be attenuated or even reversed.

Importantly, any increases in non-retirement risky share are likely to be concentrated among investors with sufficient slack to adjust their taxable portfolios—i.e., those who are not already near a corner solution with very high pre-treatment risky share. For individuals with initially high risky shares, there is limited scope to further increase risk-taking outside retirement accounts, so estimated effects may be attenuated and could even turn negative if equity exposure is reallocated toward the DC account.

4 Results

In this section, I present the empirical findings on the impact of DC pension plan enrollment on individuals' non-retirement investment behavior. The analysis focuses on two key aspects: stock market participation and portfolio allocation decisions. Specifically, I examine how exposure to DC plans influences the probability of entering the stock market for non-participants and affects the risky share of portfolios for existing stockholders.

4.1 Participation

Table 2 reports estimates of Eq. (17) under progressively richer sets of controls, adding covariates such as occupation, education, marital status, and home ownership to assess the robustness of the effect of occupational DC eligibility/enrollment on stock market entry. The baseline specification controls for the active component of wealth growth (Eq. 10), which excludes contemporaneous wealth movements mechanically induced by portfolio rebalancing. I revisit alternative treatments of wealth (including omitting wealth controls and using total wealth growth) in the robustness exercises and in the mechanism discussion below. In Table 2, I report only the first-year treatment effect (the coefficient β_1 on $I[\hat{\tau}^{DC} = 1]$), while Figure 3 plots the full event-time profile of entry responses before and after DC enrollment.

[INSERT TABLE 2 AND FIGURE 3 HERE]

Effect in the first year of treatment Table 2 shows that DC eligibility has an immediate and robust positive effect on stock market entry. The coefficient on the first post-eligibility indicator, $\hat{\tau}^{DC} = 1$, is positive and highly statistically significant in all three specifications. In the preferred specification (Column (3)), the estimated short-run effect is a 1.34 pp increase in the one-year probability of entry.

Economically, this effect is sizable. Relative to the unconditional one-year entry rate among baseline nonparticipants in the sample (6.68%), the Column (3) estimate of 0.0134 implies an increase of about 20% in the probability of entering the stock market over the next year.

Importantly, since the exact date of birth is not observed in my data, this is the effect on the first year of treatment, not after 1 year of treatment. Hence, while $\hat{\tau}^{DC}$ equals one year, the real τ^{DC} might be (way) less than this. In the extreme case, it could be just one day with a DC account, if the birthday falls on 31 December.

The table also highlights the role of resources. The coefficient on active wealth growth, $\Delta \log FW_t^a$, is positive and precisely estimated across specifications (around 0.014). This pattern is consistent with monetary participation costs: when liquid resources rise, entry becomes more likely. Income growth, $\Delta \log Y_t$,

is also positive and statistically significant in all specifications (around 0.005), consistent with higher current resources and/or improved expected lifetime resources facilitating entry.[25]

Finally, observable household characteristics are strongly predictive of entry. Homeowners are more likely to enter the stock market, while having children is associated with a lower entry propensity. Relative to the omitted marital-status category (not married), both married and divorced individuals exhibit significantly lower entry rates, whereas widowed individuals display higher entry propensities (though it should be a much smaller group for the young sample considered in the analysis). Education is also a powerful predictor: relative to the omitted category (no high school), all education groups have higher entry propensities, with effects increasing monotonically up to college and remaining large for post-graduate education.

Multi-year treatment effect As noted above, β_1 captures the effect in the first treatment year, which for many individuals corresponds to substantially less than a full year of post-treatment exposure. Given inertia in household portfolio choice and the gradual nature of learning, entry responses are therefore expected to build up over the first years after DC enrollment.

Panel (a) of Figure 3 plots the event-study coefficients β_j, which measure the incremental effect of DC enrollment on the one-year probability of stock market entry between $t-1$ and t among those who are nonparticipants at $t-1$. The estimates show no evidence of differential pre-trends: coefficients in the two years prior to enrollment ($\hat{\tau}^{DC} = -2$ and $\hat{\tau}^{DC} = -1$) are small and statistically indistinguishable from zero.[26]

Panel (b) reports the cumulative sum of yearly effects $\sum_{k=1}^{j} \beta_k$.[27] The cumulative effect rises over the first years after enrollment, reaching about 6.25 pp by $j = 4$ and about 10.0 pp by $j = 6$. At longer horizons the coefficients are less precisely estimated, so the figure is most informative about the timing and gradual accumulation of the entry response rather than about the exact long-run magnitude.

[INSERT FIGURE 4 HERE]

To place these magnitudes in context, Figure 4 compares the cumulative sum of annual entry effects to the unconditional probability that a baseline nonparticipant has entered the stock market at least once by horizon τ. Panel (a) plots these unconditional cumulative ever entered probabilities, which are 7.06% by $\tau = 1$, 10.71% by $\tau = 2$, 13.26% by $\tau = 3$, 15.15% by $\tau = 4$, 16.63% by $\tau = 5$, and 17.75% by $\tau = 6$. These probabilities are computed by fixing a cohort of nonparticipants in a given baseline year and tracking

[25] Income is often used as a proxy for human capital; an alternative approach would be to model the present value of future labor income directly as in Gomes and Smirnova (2022).

[26] The limited number of pre-treatment coefficients reflects the short time dimension of the panel and the fact that, further from the reform date, there is insufficient within-occupation variation in treatment timing for the TWFE event-study to be reliably identified (Callaway and Sant'Anna, 2021, Sun and Abraham, 2021).

[27] Because individuals who enter and subsequently exit can re-enter the sample as nonparticipants this sum shows changes in annual entry probabilities across post-treatment years, i.e., an accumulated effect on entry flows.

subsequent participation, where entry equals one if the individual participates at least once within the next τ years. I compute this probability separately by baseline-year cohort and then take an unweighted average across cohorts, accounting for shorter follow-up for later baseline years. Panel (b) repeats the cumulative sum of the post-treatment event-study coefficients, $\sum_{k=1}^{j} \beta_k$. Panel (c) provides a heuristic comparison by scaling the cumulative sum of entry effects by the unconditional ever-entry probability at each horizon. By $\tau = 5$, this ratio reaches 56.5%. However, because the entry coefficients are estimated on the selected sample of non-participants at each $t-1$, this ratio should be interpreted cautiously, as it provides an upper bound on the proportional increase in ever-entry probability, with the true effect likely somewhat smaller due to the shrinking non-participant population over time.

4.1.1 Wealth Controls and Identification

A key concern when controlling for contemporaneous wealth growth in entry regressions is endogeneity. Changes in financial wealth can mechanically reflect portfolio choices made within the period—including stock market entry and rebalancing—which creates reverse causality if wealth growth is included as a right-hand-side control. My benchmark specification therefore uses the active change in wealth, which subtracts the mechanically implied component driven by contemporaneous changes in risky exposure (see Eq. (10)).

A related issue in difference-in-differences settings is bad-control bias. Even when wealth changes are clean of mechanical feedback from portfolio reallocation, they may still respond to treatment. In this setting, DC eligibility can affect saving behavior and wealth accumulation (see Section 5.2), so conditioning on contemporaneous wealth growth can partially absorb treatment-induced changes that are themselves part of the causal pathway. Conversely, omitting wealth controls changes the estimand in the opposite direction: if monetary participation costs depend on liquid resources, treatment-induced wealth movements can affect entry mechanically. In that case, specifications without wealth controls capture a reduced-form response that bundles learning effects together with any wealth-driven entry effects operating through participation costs, whereas specifications that condition on wealth are closer to a direct effect net of the wealth channel.

[INSERT TABLE 3 HERE]

Table 3 assesses the sensitivity of the estimated event-time entry effects to alternative treatments of wealth. Column (1) reports the benchmark specification with the active wealth-growth control. Column (2) replaces this control with a more conventional contemporaneous wealth-growth measure, which is more tightly linked to within-period portfolio adjustments and therefore more likely to behave as an endogenous control. Column (3) flexibly absorbs baseline wealth differences through pre-wealth×year controls, and Column (4) omits wealth controls altogether.

Two patterns emerge. First, the pre-treatment coefficients are small across specifications, consistent with limited differential pre-trends, though their magnitudes vary somewhat with the wealth specification. Second,

the post-treatment coefficients are sensitive to how wealth is controlled for. Relative to the benchmark in Column (1), using a conventional contemporaneous wealth-growth control (Column (2)) tends to attenuate the estimated post-treatment entry effects, consistent with wealth growth partly reflecting portfolio choices made in response to treatment. In contrast, dropping wealth controls (Column (4)) yields smaller post-treatment coefficients than the benchmark, consistent with a setting in which treatment reduces taxable financial wealth and where lower wealth increases monetary participation costs, mechanically lowering entry and thereby offsetting part of the positive learning/information effect in reduced form.

Finally, the pre-wealth×year specification in Column (3) produces particularly small and imprecise post-treatment coefficients and is estimated on a substantially smaller effective sample. This attenuation is consistent with aggressive wealth conditioning absorbing meaningful treatment-related variation in resources and with the tighter sample requirements induced by these controls.

Overall, the results indicate that the entry response to DC enrollment is positive across specifications, but its measured magnitude depends on whether wealth is treated as a control or allowed to adjust endogenously. This sensitivity is informative: together with the negative post-treatment wealth effects documented below, it suggests that DC enrolment operates through at least two opposing channels—a positive information/learning effect that increases entry and a negative wealth/participation-cost channel that dampens entry when taxable financial wealth falls which I further discuss in Section 5.2 below.

4.1.2 Peer Spillovers and Anticipation Effects

A potential threat to identification is that entry among not-yet-treated individuals may increase over time because they learn indirectly from treated peers. Such spillovers would mechanically compress treated–control differences and could contribute to the apparent leveling-off of the estimated post-treatment effects.

To probe this channel, I construct an indicator for exposed controls. I partition the sample into fine peer cells defined by parish (a highly granular geographic unit with about 2,500 unique parishes in the estimation sample) and occupational group, under the assumption that individuals who live and work in close proximity are more likely to interact and exchange information. For each parish–occupation cell and year, I compute the share of individuals who are already treated (i.e., enrolled in a DC plan). I then flag not-yet-treated individuals as exposed controls if, in their parish–occupation cell, at least 75% of peers are already treated.

[INSERT TABLE 4 HERE]

Table 4 compares the baseline event-study estimates to a specification that excludes these exposed controls. Column (1) reports the baseline event-time coefficients, while Column (2) re-estimates the same specification after dropping not-yet-treated individuals with high exposure to treated peers. The coefficients remain very similar across columns, but the estimates in Column (2) are, if anything, slightly larger

and more precisely estimated: several post-treatment coefficients increase modestly in magnitude and gain statistical significance (e.g., the effects at $\hat{\tau}^{DC} = 2$ and $\hat{\tau}^{DC} = 4$ become marginally and statistically significant, respectively). Pre-treatment coefficients remain close to zero and statistically insignificant. Overall, this pattern suggests that spillovers into the not-yet-treated group are not driving the results; if anything, including highly exposed controls slightly attenuates the estimated treatment effects, consistent with some information diffusion among control group.

4.1.3 Effect on Exits

Equation (17) studies how DC eligibility affects the probability that a *nonparticipant* at $t-1$ enters the stock market between $t-1$ and t. While this flow-based outcome is informative about the extensive margin, changes in the stock of participation also depend on exits and on the possibility of re-entry after exiting. Moreover, the learning and salience mechanisms emphasized in this paper have clear implications for entry but are less unambiguously mapped into predictions for exits: greater sophistication may reduce disengagement, but it may also increase active re-optimization and hence strategic exits.

To study persistence directly, I estimate an analogous event-study for stock market exit, defined as

$$Exit_{it} \equiv 1\{Partic_{i,t-1} = 1,\ Partic_{it} = 0\}.$$

I estimate the specification on the sample of participants at $t-1$ (i.e., conditional on $Partic_{i,t-1} = 1$), using the same event-time indicators, fixed effects, controls (including the active wealth-change measure), and inference procedure as in the entry analysis.

[INSERT TABLE 5 HERE]

Table 5 shows that exit coefficients are close to zero in the pre-treatment periods, consistent with no differential pre-trends. Post-treatment, the coefficients are negative and economically small—on the order of a 0.2–0.7 percentage point reduction in the one-year exit probability at intermediate horizons, with statistical significance emerging only for some post-treatment years. This pattern implies that DC eligibility does not generate excess attrition, if anything, it modestly reduces the likelihood that prior participants leave the stock market.[28]

These results refine the interpretation of the entry effects. Because individuals may exit and later re-enter, cumulating annual entry coefficients does not correspond mechanically to the probability of ever entering by a given horizon. However, the evidence of weakly negative exit responses indicates that the entry

[28] In specifications without wealth controls, the exit coefficients move toward zero and in some cases become positive, consistent with the idea that treatment-induced wealth changes can affect exit through monetary participation costs.

effects are not offset by higher subsequent exit rates. Instead, the policy appears to operate through both a participation inflow margin and a modest persistence margin. More broadly, changes in participation can be decomposed into entry and exit components:

$$\Delta Partic_{it} \approx (1 - Partic_{i,t-1}) \cdot Entry_{it} \; - \; Partic_{i,t-1} \cdot Exit_{it}.$$

A positive treatment effect on entry combined with a negative treatment effect on exit, therefore, both contribute to higher overall participation. In this sense, the estimates in Table 2 and Figures 3–4 capture effects on the inflow into participation, while the exit analysis shows that the policy also modestly strengthens the persistence of participation once households enter.

4.1.4 Aggregate Participation Impact

The analysis shows that DC enrollment increases the cumulative sum of annual entry effects by roughly 10 pp by event time $\hat{\tau}^{DC} = 6$, while the corresponding cumulative exit response is close to zero. Translating these flow effects into changes in the participation stock implies an increase in overall stock market participation of about 5 pp when scaled by the baseline share of nonparticipants (49%). To provide a more direct estimate of the participation effect, Appendix D.1 estimates the event-study using participation in levels, $Partic_{it}$, on the full sample. As shown in Figure A2 the level effect is hump-shaped, peaking at $\hat{\tau}^{DC} = 3$ (5.44 pp) and slightly declining after.

To benchmark the magnitude of the estimated DC effects, I compare them to other interventions that plausibly operate through reductions in informational participation costs. Using Danish registry data, Nielsson and Press (2025) show that holding employer stock is associated with a 3.6–4.7 pp higher probability of non-employer stock market participation (from 24.4% to 28.0%). Nielsson et al. (2025) document that an exogenous increase in pension equity exposure (switching from a low-equity guaranteed product to a high-equity product) raises non-pension equity ownership by about 7 pp among prior nonparticipants.

Taken together, the medium-run participation effects are of similar order of magnitude to other exposure-based interventions, and consistent with these policies operating, at least partly, through reductions in informational participation costs.

4.2 Risky share

The regression results for Equation (18) with different indicators of changes in risky share are reported in Table 6. These estimates quantify how DC plan enrollment affects changes in risky asset exposure among individuals who were already stockholders prior to treatment (i.e., participants in both $t-1$ and t).

[INSERT TABLE 6 HERE]

Table 6 shows a positive and statistically significant impact effect of DC enrollment on risky share adjustments. The impact coefficient $I[\hat{\tau}^{DC} = 1]$ implies an increase of about 1.15 percentage points in the overall change in risky share, Δw_t (Column (3)), and about 1.22 percentage points in the active change, Δw_t^{active} (Column (1)). Relative to the unconditional mean risky share of 44% in the first year, these magnitudes correspond to an economically meaningful shift in risk taking on the order of roughly 2.6–2.8% of the baseline risky share level.

Δw_t captures the total change in risky share between $t-1$ and t, combining both active rebalancing and passive valuation effects (e.g., due to equity price movements). By contrast, Δw_t^{active} isolates intentional reallocation decisions. The fact that the enrollment effect is positive for Δw_t^{active} indicates that the increase in risk exposure is not purely mechanical; treated individuals actively rebalance toward riskier portfolios following DC enrollment.

The coefficient on lagged financial wealth is positive and precisely estimated: $\log FW_{t-1}$ is associated with larger increases in risky share. This pattern is consistent with standard household finance predictions under decreasing relative risk aversion and with empirical evidence that wealthier households hold more equity and adjust equity exposure more actively (e.g., Bach et al., 2020). Overall, the results are consistent with DC enrollment lowering informational or behavioral participation frictions and/or increasing investors' effective willingness to bear risk, potentially through learning and increased familiarity with equity investing. The findings are also compatible with tax-optimization mechanisms in which households reallocate risk across account types (e.g., Dammon et al., 2004).

Learning and Portfolio Adjustment Slack If the informational participation cost (IPC) channel operates through learning and increased confidence, effects on non-retirement risky share should be strongest among investors with sufficient "slack" to adjust taxable portfolios—i.e., those not already near a corner solution with extremely high pre-treatment risky shares. When pre-treatment risky share is close to one, there is limited scope to further increase risk outside retirement accounts, and any re-optimization may instead involve shifting equity exposure toward the DC account.

[INSERT TABLE 7 HERE]

To test this implication, Table 7 re-estimates the model excluding individuals with pre-treatment risky share above 90% (approximately 2% of the sample). The impact effect becomes substantially larger: $I[\hat{\tau}^{DC} = 1]$ rises to about 2.61 percentage points for Δw_t (Column (3)) and about 2.70 percentage points for Δw_t^{active} (Column (1)). This more-than-twofold increase supports the slack mechanism and is consistent with the learning/IPC channel being attenuated in the full sample by individuals who are already near a high-risk corner solution.

Given the evidence above, I take Δw_t^{active} as the preferred intensive-margin outcome and restrict the estimation sample to individuals with pre-treatment risky shares below 90%. This restriction removes the mechanical attenuation from investors already near the upper bound of risk-taking, allowing the learning channel to be identified more cleanly in the event-study profiles that follow.

Multi-year treatment effect for risky share Consistent with inertia and gradual portfolio adjustment, the response strengthens over time as shown in Figure 5.

[INSERT FIGURE 5 HERE]

The cumulative increase in active risky share reaches approximately 10 percentage points by six years after enrollment. Interpreted relative to the unconditional mean risky share of 44%, this corresponds to a sizable medium-run increase in risky asset exposure (22.7%).

Taken together, the event-study evidence indicates that DC plan enrollment leads incumbent stockholders to actively rebalance toward riskier portfolios, with effects that build progressively over several years.

5 Inspecting the Mechanism

Having found a positive effect of DC account enrollment on stock market entry and risky share, in this section I conduct additional tests that allow for a further understanding of the different channels at work. More precisely, I study the impact of the treatment on both portfolio diversification and savings/wealth accumulation.

5.1 Diversification

I assess changes in portfolio diversification by examining how individuals allocate risky financial assets between diversified mutual funds and directly held individual stocks. If DC plan exposure leads agents to internalize not only the benefits of stock market participation but also basic diversification principles, then—conditional on already holding risky assets—they should tilt away from concentrated direct stock positions and toward mutual funds.[29]

I use the same event-time framework around the first year of DC enrollment. I consider two continuous outcomes: (i) the mutual-fund share within risky assets (Eq. 13) and (ii) the mutual-fund share within total financial wealth (Eq. 14).

[29] Low-fee exchange-traded funds (ETFs) were essentially absent during the sample period.

I estimate the following event-study specification, which mirrors the baseline entry regressions:

$$\begin{aligned}\Delta F_{it}^{\%Risky} &= \alpha + \sum_{j\in\mathcal{J},\, j\neq 0} \beta_j \, I[_{\hat{\tau}_{it}^{DC}=j}] + \theta X_{it} + I[a_{it}] + I[y_t] + I[\text{occ} \times y_t] + \varepsilon_{it}, \\ \Delta F_{it}^{\%FW} &= \alpha + \sum_{j\in\mathcal{J},\, j\neq 0} \beta_j \, I[_{\hat{\tau}_{it}^{DC}=j}] + \theta X_{it} + I[a_{it}] + I[y_t] + I[\text{occ} \times y_t] + \varepsilon_{it}.\end{aligned} \tag{19}$$

Event time $\hat{\tau}_{it}^{DC}$ is measured relative to the year before DC enrollment, with $\hat{\tau}^{DC} = 0$ omitted as the reference period. As in the entry analysis, X_{it} collects time-varying controls, and I include age fixed effects $I[a_{it}]$, year fixed effects $I[y_t]$, and occupation-by-year fixed effects $I[\text{occ} \times y_t]$. I estimate these regressions conditional on stock market participation in both $t-1$ and t, so the interpretation is about reallocation within participants' risky portfolios rather than extensive-margin participation.

A positive β_j implies that, j years relative to enrollment, individuals increase the mutual-fund share of risky assets (or financial wealth), consistent with an informational mechanism in which DC exposure promotes diversification knowledge and lowers informational frictions to implementing diversified strategies outside the pension account. A negative β_j would instead suggest substitution or mental accounting: because DC plans are typically diversified by construction, households may perceive pension holdings as providing sufficient diversification and therefore concentrate taxable risky investments more heavily in directly held stocks.

To complement the share-based outcomes with a direct measure of active choice, I also examine whether DC exposure affects the probability of purchasing mutual funds. To maintain full consistency with the entry specification, I estimate this probability using a linear probability model (LPM) with the same event-time structure and fixed effects:

$$Buy_{it}^{F} = \alpha + \sum_{j\in\mathcal{J},\, j\neq 0} \beta_j \, I[_{\hat{\tau}_{it}^{DC}=j}] + \theta X_{it} + I[a_{it}] + I[y_t] + I[\text{occ} \times y_t] + \varepsilon_{it}, \tag{20}$$

where $Buy_{it}^{F} \in \{0,1\}$ indicates whether individual i purchases mutual funds at time t. A positive β_j indicates that DC enrollment increases the likelihood that new risky investment flows are directed into diversified mutual funds rather than into direct stock positions, consistent with improved diversification behavior following DC exposure.

Multi-year treatment effect for portfolio composition outcomes. Figure 6 plots event-study coefficients from Eqs. (19)–(20) for three mutual-fund outcomes: the LPM probability of purchasing mutual funds, $P(Buy_t^F)$, the change in the mutual-fund share of total financial wealth, $\Delta F_t^{\%FW}$, and the change in the mutual-fund share within risky assets, $\Delta F_t^{\%Risky}$.

[INSERT FIGURE 6 HERE]

The dynamics reveal a nuanced pattern. In the impact year ($\hat{\tau}^{DC} = 1$), DC enrollment increases the probability of purchasing mutual funds by 1.57 pp (about 2.5% relative to an unconditional mean purchase probability of 63%). The effect remains positive in year 2 (1.51 pp) and is close to zero thereafter, suggesting that the incremental fund-buying response is concentrated in the first years after enrollment. Consistent with this short-run increase in mutual-fund activity, the mutual-fund share of total financial wealth rises by 1.75 pp at $\hat{\tau}^{DC} = 1$ (about 5.5% relative to an unconditional mean of 32%) and remains positive at intermediate horizons (e.g., 1.38 pp at $\hat{\tau}^{DC} = 2$ and 1.39 pp at $\hat{\tau}^{DC} = 3$), with weaker precision at longer horizons.

At the same time, the composition of risky taxable holdings shifts slightly away from funds. The mutual-fund share within risky assets, $\Delta F_t^{\%Risky}$, declines by 0.30 pp in the impact year (relative to an unconditional mean of 76%) and remains modestly negative in the first few post-enrollment years (e.g., -0.24 pp at $\hat{\tau}^{DC} = 3$), before becoming statistically indistinguishable from zero at longer horizons. Thus, DC exposure appears to raise the use of mutual funds in taxable portfolios (higher buying probability and higher fund share in total financial wealth), but it does not translate into a sustained increase in the share of risky assets held via funds.

This combination of findings aligns with the broader portfolio responses documented in this analysis. I find that active rebalancing increases the risky share following DC enrollment, while non-retirement financial wealth declines, implying higher risk intensity in a shrinking taxable portfolio. In that setting, an increase in $F^{\%FW}$ can arise through a level expansion of fund holdings or through reallocating a larger fraction of remaining taxable wealth into mutual funds. However, within the risky bucket, households may still express a preference for directly held stocks (e.g., stock-picking motives, familiarity, perceived control, or entertainment), so that $F^{\%Risky}$ does not rise and can even decline slightly. One interpretation is that pension menus provide "default" diversification inside the DC account, while taxable risky investing remains relatively stock-oriented. Overall, the evidence is consistent with learning or reduced informational frictions about mutual funds that increases fund adoption, alongside persistent non-pension preferences for individual stocks in the composition of taxable risky holdings, with the strongest effects occurring in the first years after enrollment.

Portfolio Efficiency: Relative Sharpe Ratio Loss To quantify changes in the quality of diversification among stock market participants, I implement the Sharpe-ratio-loss test of Calvet et al. (2007), which provides a welfare-relevant measure of the efficiency cost of imperfect diversification. Calvet et al. (2007) define the Sharpe ratio of individual i's risky portfolio as

$$S_{i,t} = \frac{\mu_{i,t}}{\sigma_{i,t}}, \tag{21}$$

where $\mu_{i,t}$ and $\sigma_{i,t}$ are the mean and standard deviation of the excess return on the risky portfolio.[30] The relative Sharpe ratio loss (RSRL) is

$$RSRL_{i,t} = 1 - \frac{S_{i,t}}{S_B}, \tag{22}$$

where S_B is Sharpe ratio of a benchmark portfolio.[31] A decline in $RSRL_{i,t}$ indicates improved portfolio efficiency—higher expected excess return per unit of risk—consistent with learning that reduces informational and optimization frictions (e.g., improved diversification and lower idiosyncratic risk).

I use $RSRL_{i,t}$ as the outcome in the event-study DiD design from Section 3. Figure 7 shows that DC exposure is associated with economically meaningful improvements in portfolio efficiency. The pre-treatment coefficients at $\hat{\tau}^{DC} = -2$ and -1 are small and statistically indistinguishable from zero, supporting parallel trends. After enrollment, RSRL declines: in the first post-treatment year ($\hat{\tau}^{DC} = 1$), the estimate is -0.872 p.p., corresponding to an approximately 1.30 p.p. reduction relative to the unconditional mean loss of 0.67 (panel b). The reduction remains statistically significant through years 2–4, with point estimates between -0.91 and -0.98 percentage points (about 1.35–1.47 percentage points relative to the mean). By years 5–6, the coefficients attenuate and lose statistical significance, suggesting that efficiency gains are concentrated in the first few years after access.

[INSERT FIGURE 7 HERE]

Overall, the RSRL evidence is consistent with a learning channel that improves diversification quality shortly after DC exposure.

Fund costs To further investigate whether learning may operate through mutual funds, I estimate the baseline event-study design with mutual-fund fees as the outcome. Fund costs were substantially higher during my estimation period (about 1.45% on average), when low-cost index products and ETFs were not yet widespread. Descriptively, average fees decline over time in my data (from roughly 1.7% in 2001 to 1.4% in 2007). The key question is whether access to an occupational DC plan is associated with an additional shift toward cheaper funds, i.e., whether exposure to the DC menu moves investors into lower-cost products beyond the aggregate downward trend.[32]

To measure fund fees, I construct an individual-level mutual-fund cost measure by merging security-level fund holdings to Morningstar fund characteristics using ISIN and calendar year. For each year (1999–2007), I start from the register-based portfolio holdings file and keep mutual fund positions, excluding money-market

[30] Because cash earns the risk-free rate, the Sharpe ratio of the individual's complete portfolio coincides with that of the risky sub-portfolio, so the object can be computed using risky holdings alone.

[31] I provide implementation details in Appendix E.

[32] Historic public statistics on average management fees in Swedish occupational DC funds during 2000–2007 are limited. For ITP (white-collar) plans, Collectum, Svenskt Näringsliv, and PTK (2018) report an average fund fee of 1.32% in the pre-procurement period and 0.89% after the first procurement in 2007 (for comparison, the open-market average in 2007 in Sweden is reported as 1.38%).

funds. I then merge these holdings with Morningstar's annualized fund-fee measure. For each individual i in year t, I compute the value-weighted average fee across the mutual funds in the portfolio for which Morningstar fees are available:

$$Cost_{it}^{F} = \sum_{f \in \mathcal{F}_{it}} \left(\frac{Value_{ift}}{\sum_{k \in \mathcal{F}_{it}} Value_{ikt}} \right) \cdot CostXFee_{ft}^{ann},$$

where $\mathcal{F}_{it}$ is the set of mutual fund holdings with non-missing annualized fees and $CostXFee_{ft}^{ann}$ is Morningstar's annualized fund-fee measure. The resulting indicator captures the average annual fund cost (in percent terms) faced by the investor given the composition of her mutual-fund holdings; if an individual holds no mutual funds with matched fee data in a given year, $Cost_{it}^{F}$ is missing.

[INSERT FIGURE 8 HERE]

Figure 8 shows a gradual decline in fund costs following DC enrollment. Pre-treatment estimates at $\hat{\tau}^{DC} = -2$ and -1 are noisy and statistically indistinguishable from zero. Post-enrollment coefficients are negative and grow in magnitude over time. In the first post-treatment year, the point estimate is about -0.32 percentage points ($\hat{\tau}^{DC} = 1$), and the decline becomes larger at intermediate horizons, reaching roughly -1.05 percentage points by $\hat{\tau}^{DC} = 4$; estimates at $\hat{\tau}^{DC} = 4$ and 5 are statistically significant at conventional levels. Relative to the unconditional mean fund cost of 1.45%, these magnitudes correspond to economically meaningful fee reductions (Panel (b)). Overall, the dynamic pattern is consistent with DC exposure shifting investors toward lower-cost mutual funds beyond the aggregate secular decline in fees during the early 2000s, in line with learning and improved product selection in the fund market.

Taken together, the diversification evidence supports a learning mechanism: DC plan exposure increases familiarity with fund products, improves the efficiency of risky portfolios, and leads investors to hold cheaper mutual funds. However, the modest decline in the mutual-fund share within risky assets indicates that learning operates more through product adoption and quality (using funds and selecting lower-cost funds) than through a wholesale substitution away from taxable direct stockholding.

5.2 Wealth effect

Swedish occupational DC plans are fully funded by the employer. When individuals are enrolled in the DC plan, they start receiving an employee contribution into their account and do not make any investments themselves. Therefore, it is natural to expect that this could lead to the crowding-out of private savings. In this subsection, I first test for this channel and then repeat the stock market entry regressions without controlling for changes in wealth. If individuals are indeed reducing their non-retirement savings, the monetary participation cost channel would predict a reduction in stock market entry in regressions where

we do not control for changes in wealth. Such results can therefore shed further light on the underlying mechanisms and, in particular, on the role of MPC. I test whether DC enrollment decreases private savings in the following specification:

$$\Delta FW_{it} = \alpha + \sum_{j=-2}^{-1} \beta_j I[\hat{\tau}^{DC}=j] + \beta_1 I[\hat{\tau}^{DC}=1] + \sum_{j=2}^{6} \beta_j I[\hat{\tau}^{DC}=j] + \theta X_{it} + I[a_{it}] + I[\text{occupation} \times y_t] + I[y_t] + \varepsilon_{it}, \quad (23)$$

where ΔFW_{it} is measured by $\Delta FW/Y$, $\Delta_g FW_t$, $\Delta \log FW_t$, or $\Delta \log FW_t^a$.[33]

The regression results in Table 8 show the impact of DC plan enrollment on various financial wealth measures.

[INSERT TABLE 8 HERE]

The first column presents the effect of treatment on the log of the financial wealth to income ratio ($\Delta FW/Y$). In the first year after enrollment ($\hat{\tau}^{DC} = 1$), the estimate is negative but small and not statistically significant (-0.0052). The point estimates become more negative at longer horizons (e.g., $\hat{\tau}^{DC} = 5$), but the confidence intervals remain wide. Still, this outcome provides economically meaningful evidence of a short-run decline in FW/Y.

The second column focuses on wealth growth, $\Delta_g FW$. Here, the first post-treatment coefficient is large and statistically significant: at $\hat{\tau}^{DC} = 1$, the estimate is -0.179, and the negative effects persist in subsequent years (e.g., $\hat{\tau}^{DC} = 2$ and $\hat{\tau}^{DC} = 3$ are also significantly negative). The third column uses the change in log financial wealth, $\Delta \ln FW$. The first-year estimate is again negative (-0.168) but imprecise. The fourth column uses active wealth growth, $\Delta \log FW^a$, which is closest to the "active savings" margin. The first-year effect is negative and statistically significant: at $\hat{\tau}^{DC} = 1$, the estimate is -0.192, with additional negative post-treatment effects at $\hat{\tau}^{DC} = 2$ and $\hat{\tau}^{DC} = 3$. Taken together, Columns (2) and (4) provide the strongest evidence that DC enrollment crowds out non-retirement wealth accumulation along the active-wealth-growth margin.

Overall, the event-study results for wealth indicators point to a crowding-out effect from DC wealth toward retirement saving, but this effect is concentrated in outcomes that capture wealth growth more directly (especially $\Delta_g FW$ and $\Delta \log FW^a$), rather than showing as a clean, precisely estimated decline in FW/Y immediately after enrollment.

To gauge economic magnitude, I compute the average occupational DC contribution rate in the sample, which is 4.65% of income.[34] Comparing this to the first-year estimate for $\Delta(FW/Y)$ of about -0.52%, the

[33] All the other controls are similar to the baseline specifications for stock market entry and change in risky share.

[34] The data does not explicitly provide DC account values, but I impute occupational DC contributions using the

implied offset in liquid financial wealth is modest relative to the flow of new DC saving. Thus, even with some substitution away from non-retirement wealth, total saving in non-retirement and retirement wealth is likely to rise.

Finally, this crowding-out channel works against stock market entry in a model with monetary participation costs: holding information fixed, lower liquid wealth would tend to reduce participation. If this were the dominant force, one would expect lower entry (or a null effect once controlling for wealth changes). Instead, my baseline entry specifications explicitly control for wealth and income changes, thereby netting out this opposing wealth channel and isolating the role of informational participation costs generated by exposure to DC investing.

To assess the reduced-form entry response when wealth is allowed to adjust freely, I therefore re-estimated Eq. (17) excluding the wealth-growth control. If MPCs are quantitatively important, the omission of wealth controls should reduce the estimated post-treatment entry effects (and could even flip their sign if the wealth/MPC channel dominates the learning/IPC channel). Column 4 of Table 3 shows exactly this attenuation: the post-treatment coefficients are economically smaller and lose statistical significance relative to the baseline specification that conditions on wealth growth. Importantly, the estimates remain positive, consistent with IPC reductions being the dominant force on average. At the same time, the attenuation is informative: together with the negative post-treatment wealth responses documented above, it suggests that DC eligibility operates through two opposing channels—an information/learning channel that raises entry and a wealth/MPC channel that dampens entry when liquid financial wealth falls.

5.3 Sibling Spillovers

To test whether the learning channel operates through within-family information transmission, I construct a sample of individuals who are not directly treated during the observation window (though they may obtain an occupational DC account outside the sample period), but who have a sibling who enrolls due to the reform. The resulting sibling-linked panel contains roughly 1.3 million eligible person-year observations. I then estimate a staggered DiD event study around the sibling's treatment year, using the year before sibling enrollment ($t = -1$) as the omitted category, for (i) stock market entry, (ii) the change in active risky share, (iii) the probability of buying mutual funds, and (iv) the change in financial wealth relative to income.

This exercise helps distinguish between two versions of the "informational participation costs" mechanism: awareness spillovers versus direct exposure (learning-by-doing). If reduced participation costs operate mainly through awareness — for example, a treated sibling shares information, increases salience, or lowers perceived complexity —then sibling enrollment should raise stock market activity even among individuals who do not receive DC exposure themselves during the sample period. By contrast, if learning requires direct

institutional details in Appendix A, following Calvet, Campbell, Gomes, and Sodini (2023a) and the implementation in Calvet, Campbell, Gomes, and Sodini (2019).

interaction with the DC menu and personal experience managing the account, sibling enrollment should have little effect.

[INSERT FIGURE 9 HERE]

The results in Figure 9 point strongly toward the latter. Across event times, the estimated effects on stock market participation are small and statistically indistinguishable from zero (with coefficients ranging roughly from -0.0047 to 0.0027). On the intensive margin, the estimates for active risky-share changes are also generally close to zero; only one post-event coefficient is positive and statistically significant (about 0.0054, i.e., roughly half a percentage point), while the remaining post-event estimates are insignificant. In addition, I find no statistically robust effects on mutual-fund buying, changes in the financial-wealth-to-income measure, or the fund fees measure, where point estimates fluctuate but remain statistically insignificant.

Overall, the sibling-spillover analysis suggests that the main learning effects documented in the paper are unlikely to be driven by within-family diffusion of information that induces new entry into stock markets. If anything, indirect exposure through a sibling produces at most a small, isolated change in portfolio adjustment among existing participants. The absence of consistent effects on participation and on the fund fees measure is more in line with a mechanism that operates through direct exposure and learning-by-doing when managing one's own DC account, rather than through awareness or second-hand information about DC investing.

5.4 Stock Returns Since DC Enrollment

A potential concern is that the estimated post-enrollment dynamics may partly reflect heterogeneity in realized market performance rather than learning effects. For example, individuals who enroll in a DC plan in years with strong subsequent equity performance may become more likely to enter the stock market (or to raise their risky share) because they observe unusually favorable returns. To address this concern, I augment the baseline event-study specifications with a control for the cumulative stock market return realized since the individual's DC enrollment, measured relative to a global stock market index.

[INSERT TABLE 9 HERE]

Table 9 reports the resulting estimates for (i) stock market entry among nonparticipants and (ii) active rebalancing in the risky share among existing stock market participants. The coefficient on "return since entry" is small and statistically insignificant in both columns, and the event-time coefficients remain very close to the baseline path. In particular, the participation effects continue to emerge after enrollment and persist over time (e.g., estimates of about 1.3–1.8 pp in years $t = 1$–5), while the active risky-share response remains economically and statistically meaningful (roughly 1.0–2.7 pp in years $t = 1$–4). Overall, conditioning

on realized stock returns since DC enrollment does not materially change the timing or magnitude of the estimated spillovers, suggesting that the main results are not driven by coincidental exposure to favorable market returns following enrollment.

6 Robustness: Alternative Difference-in-Differences Estimators

Sun and Abraham (2021) (hereafter SA) show that, under staggered treatment timing and heterogeneous treatment effects, conventional TWFE event-study specifications can yield coefficients that are weighted averages of multiple cohort-by-time effects and may therefore mix "forbidden" comparisons across relative-time periods.[35]

To address these concerns, I complement the baseline dynamic DiD results with estimators developed for staggered adoption, including Wooldridge (2025) extended two-way fixed effects (hereafter ETWFE) and Callaway and Sant'Anna (2021) (hereafter CS). Both approaches accommodate heterogeneous and dynamic treatment effects when implemented in a sufficiently flexible cohort-by-time specification.

In this application, I report ETWFE as my primary robustness check for two practical reasons. First, ETWFE allows me to incorporate time-varying covariates flexibly by conditioning on contemporaneous covariate realizations in the outcome equation, which is useful given that several individual characteristics evolve over time and predict participation and portfolio choice. By contrast, CS implementations often emphasize covariates measured prior to treatment to avoid conditioning on post-treatment variables that may themselves respond to treatment. Second, ETWFE differs from CS in how it uses untreated observations over time. CS with a "not-yet-treated" comparison group constructs counterfactuals period by period using only units that are untreated in that period, whereas ETWFE leverages the full pre-treatment histories of later-treated cohorts, which can improve precision in settings without a never-treated group.[36]

Reassuringly, the main conclusions are unchanged under ETWFE, and the resulting event-time coefficients provide an additional perspective on how effects evolve with time since treatment. Appendix D.3 reports the corresponding CS estimates and confirms the findings from the baseline TWFE and ETWFE analyses.

[35] See Baker, Larcker, and Wang (2022) for a comprehensive discussion of biases in staggered difference-in-differences and approaches to address them.

[36] A closely related approach is the imputation estimator of Borusyak, Jaravel, and Spiess (2021) (BJS). In a simple non-staggered setting, CS can be viewed as forming DiD estimates relative to the last pre-treatment period, whereas BJS uses the average of pre-treatment periods to construct the counterfactual path. Averaging over multiple pre-periods can improve precision when outcomes are not too heteroskedastic or serially correlated, but it effectively requires parallel trends to hold over a longer pre-treatment window and may therefore be more sensitive to slowly drifting differential trends. Wooldridge (2025) further shows that, under standard DiD assumptions and in sufficiently flexible specifications, ETWFE can be interpreted as a regression-based implementation of an imputation-type estimator, offering similar identifying content while providing a convenient framework for incorporating covariates and producing familiar regression output.

ETWFE event-time patterns. Figure 10 summarizes the post-enrollment dynamics in the ETWFE specification. Participation rises in the first four years after DC enrollment, with point estimates between 0.70 and 1.53 percentage points, before attenuating and becoming statistically indistinguishable from zero by years five and six. Active rebalancing toward risky assets is positive and precisely estimated over the first four post years (about 0.71–1.40 percentage points), and is close to zero thereafter. Consistent with increased use of fund products, the mutual-fund share of financial wealth increases throughout the post period, peaking around year four (about 2.02 percentage points), with wider intervals at longer horizons. In contrast, the wealth-to-income measure is negative throughout years one through five and only turns close to zero by year six; the corresponding confidence intervals are wide, so this pattern should be interpreted cautiously.

A key advantage of ETWFE is that it delivers cohort-by-calendar-year effects, $\text{ATT}(g,t)$, which makes it easy to inspect heterogeneity across treatment cohorts and to verify whether the post-treatment dynamics are broadly aligned for groups that enter the DC system at different points in time. Table 10 reports these $\text{ATT}(g,t)$ estimates for the core outcomes over all post years.

[INSERT TABLE 10 HERE]

Several regularities stand out. First, participation effects in Panel A are positive and typically grow with event time for cohorts treated between 2001 and 2005. For the earliest cohort ($g = 2001$), participation increases from about 1 pp in 2001–2002 (0.96 and 0.88) to 2–3 pp by 2004–2005 (2.04 and 3.16), with the intermediate cohort-year cells also positive (e.g., 1.33 in 2003). Later cohorts show the same qualitative pattern: for example, $g = 2003$ rises from 0.59 in 2003 to 1.72 in 2004 and 2.76 in 2005, and $g = 2005$ shows a positive effect in 2005 (1.61). This consistency across cohorts supports the interpretation that DC enrollment is associated with a sustained increase in participation that builds over the first few post-treatment years, rather than being driven by a single cohort or a single calendar year.

Second, the intensive-margin responses in Panel B mirror the extensive-margin pattern in the early post years. For $g = 2001$, the active change in risky share is strongly positive throughout 2001–2005 (roughly 2.1–4.1 pp), and the $g = 2002$ and $g = 2003$ cohorts also display positive effects that tend to strengthen into 2005 (e.g., $g = 2002$: 1.33 in 2002 and 2.69 in 2005; $g = 2003$: 2.14 in 2005). Intuitively, this is the pattern one would expect if the DC plan primarily lowers informational frictions: newly exposed households are more likely to start participating, and conditional on already participating, they tilt their active portfolios toward higher risky shares as they update beliefs, learn about products, or become more comfortable holding equity risk.

Third, the mutual-fund channel in Panel C is also mostly positive, but with more heterogeneity across cohorts and calendar years. Early cohorts show increases in mutual-fund shares in several cells (e.g., $g = 2001$: 1.93 in 2001 and 3.19 in 2002; $g = 2003$: 2.30 in 2004 and 3.83 in 2006; $g = 2004$: 4.22 in 2005), consistent

with DC enrollment shifting households toward mutual funds as an entry vehicle into risky investing. The occasional negative cells (e.g., $g = 2001$ in 2003; $g = 2002$ in 2003) suggest that the composition of risky holdings may vary over time and across cohorts, even when overall participation rises.

Finally, Panel D indicates that financial wealth growth relative to income ($\Delta FW/Y$) is often negative in cohort-year cells where participation and risky-share responses are positive, especially for early cohorts in later post years (e.g., $g = 2001$ in 2004–2005: -9.69 and -10.20; $g = 2002$ in 2005: -7.83; $g = 2004$ in 2005: -8.89). This juxtaposition is informative: it is consistent with DC enrollment operating through two countervailing forces—a positive information/learning channel that increases stock market participation and risk taking, alongside a negative non-retirement wealth channel that can mechanically raise monetary participation costs by reducing taxable liquid resources.

Across panels, there is also a clear attenuation (and in some cases reversal) in the final calendar year shown, 2006: participation effects are close to zero or negative across cohorts (e.g., $g = 2001$: -1.24; $g = 2002$: -0.97; $g = 2003$: -0.01; $g = 2004$: -0.10; $g = 2006$: -0.81), and risky-share effects in Panel B similarly turn negative for several cohorts (notably $g = 2002$, $g = 2003$, $g = 2004$, and $g = 2005$). Taken together, the ATT(g, t) decomposition shows that the core post-treatment build-up in participation and active risky share is broadly replicated across treatment cohorts in the early post years, while the wealth-growth effects are frequently negative in the same cells, reinforcing the interpretation of offsetting wealth and learning channels.

7 External Validity and Sample Composition

A natural concern is whether estimates obtained from a sample of young workers extend to broader populations. In my setting, the identifying variation in DC access is concentrated among ages 20–34, so the estimates should be interpreted as a local average treatment effect for early-career workers at the margin of first-time DC exposure. That said, several considerations speak to the broader relevance of the mechanism and suggest that the magnitudes documented in this setting are unlikely to be overstated.

First, the core mechanism—learning about equity markets through repeated exposure to pension investing—is not inherently age-specific. Informational participation costs arise whenever households lack familiarity with equity investing and the associated decision technology. While the level of baseline sophistication may vary across age groups, the learning channel predicts effects with the same sign whenever individuals are exposed to equity investing for the first time or face salient increases in equity exposure.

Second, complementary evidence is consistent with spillovers beyond young cohorts. Nielsson et al. (2025) document that exogenous increases in pension equity exposure in Denmark raise non-pension stock market participation in a setting that covers a wider age distribution. In addition, my replication exercise using U.S. Health and Retirement Study data (Appendix F) which focuses on individuals aged 50 and above and relies on within-job transitions in retirement plan type rather than exogenous plan assignment yields

qualitatively similar patterns. While the U.S. results should be interpreted as suggestive correlations, taken together they support the view that the learning channel can operate beyond the specific Swedish early-career context.

Third, there are reasons to view the estimates for young Swedish workers as conservative. Baseline financial sophistication among this group is relatively high, leaving less scope for detectable learning effects. Moreover, the not-yet-treated control group may be partially exposed through anticipation and social spillovers, mechanically attenuating treatment–control differences. If these forces are weaker in populations with lower baseline sophistication or less cross-group exposure, the effect of DC-induced equity exposure could be larger than what is measured here.

Finally, the focus on young workers is policy-relevant. Expansions of defined contribution coverage primarily affect new labor-market entrants and early-career workers, and changes in participation and risky asset allocation at the start of the investing lifecycle can compound over long horizons. Even if per-period responses are modest, early-career effects can be welfare-relevant because they shape financial behavior over subsequent decades.

8 Conclusion

This paper examines the impact of Defined Contribution (DC) pension plan enrollment on individuals' non-pension investment behavior by exploiting exogenous changes in Swedish occupational pension rules. Utilizing detailed data from the Swedish Tax Registry, the study addresses selection concerns and finds that DC enrollment significantly boosts stock market entry probability, with an initial increase of 1.34 pp and a cumulative effect of 10pp over the first six years. These results hold even after controlling for various observables, including changes in financial wealth, highlighting the importance of information costs in explaining the limited stock market participation observed in the data.

For individuals who were already stockholders, exposure to DC plans raises the share of risky assets in their liquid portfolios. This finding is consistent with a learning channel effect, decreasing relative risk aversion preferences, or tax optimization strategies. Interestingly, the impact on diversification is mixed: while DC enrollment increases the likelihood of buying mutual funds and raises their share relative to financial wealth, individuals decrease their share of funds over risky assets.

Overall, these findings demonstrate that DC plans positively influence stock market engagement outside retirement accounts by enhancing financial literacy, manifested in reduced stock market participation costs. The results underscore the potential of DC plans to shape broader financial behavior beyond retirement savings and highlight the importance of designing pension plans that promote financial literacy and market participation.

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Figures

Figure 1: Policy Changes in Fixed Age Rule for DC Enrollment

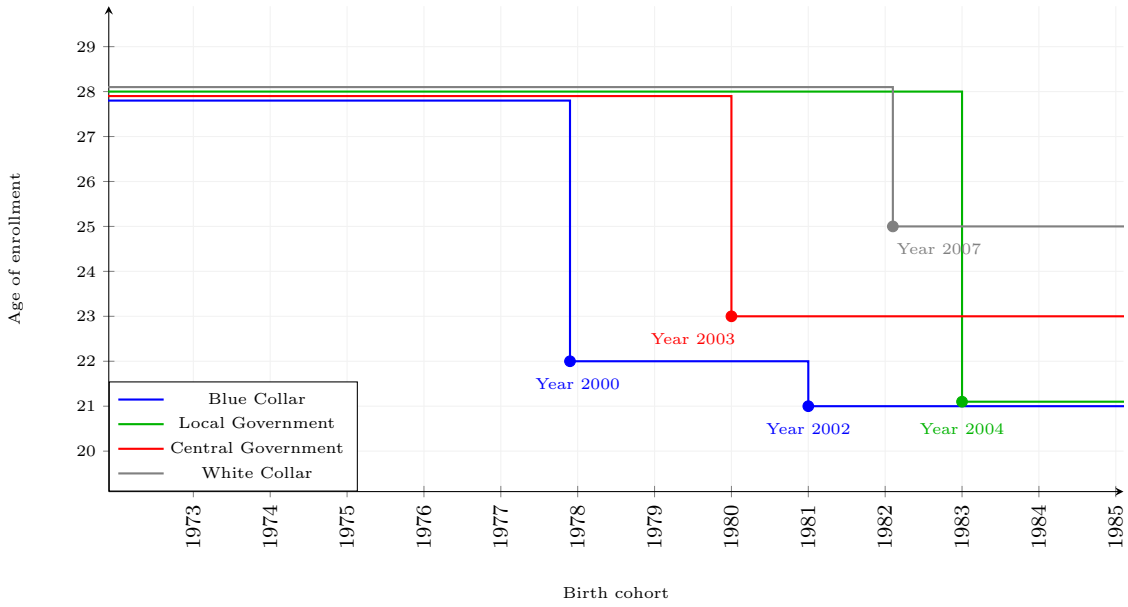
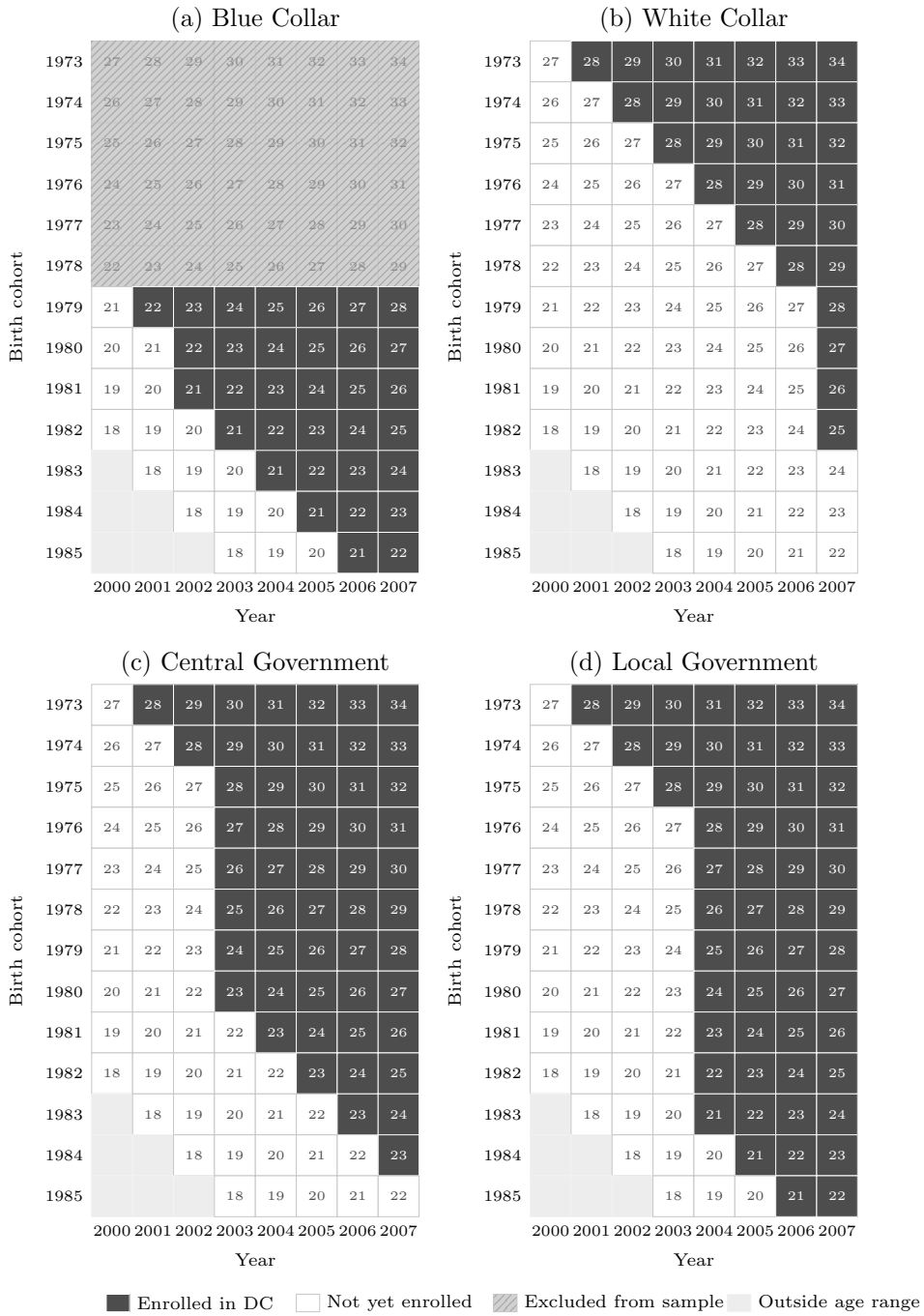


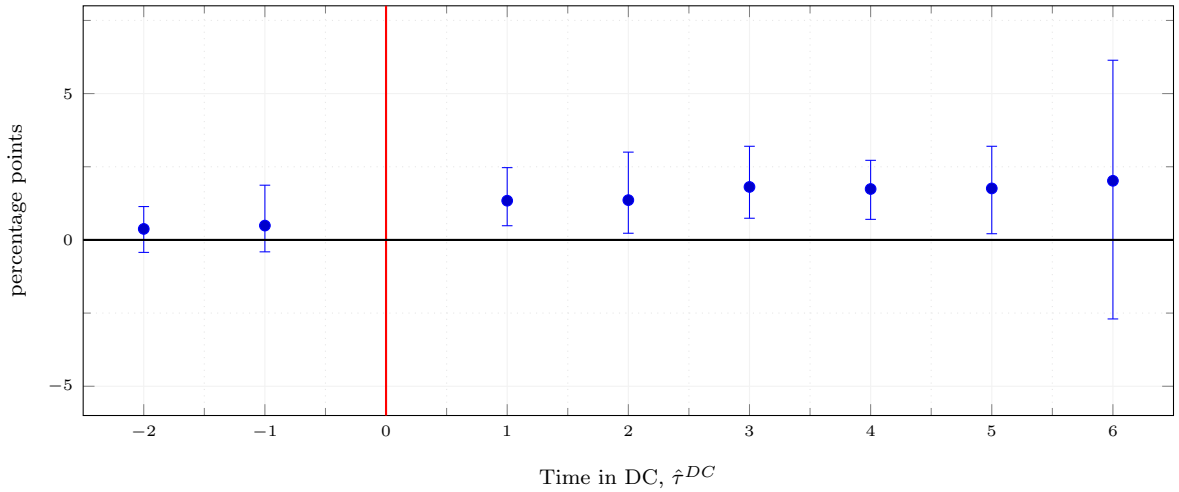
Figure 1 presents the enrollment eligibility age for defined contribution (DC) pension plans across different occupational groups and birth cohorts in Sweden, summarizing the policy changes detailed in Appendix A. Distinct colors represent Blue Collar, Local Government, Central Government, and White Collar workers. The x-axis indicates birth cohorts first affected by the policy change, while the y-axis displays the eligibility age for enrollment. Key policy changes are marked by dots, showing the years when eligibility ages were revised. For example, Blue Collar workers saw their eligibility age reduced to 22 in 2000 and further to 21 in 2002. The lines illustrate that the treatment—the change in eligibility age—varies by both birth cohort and occupational group.

Figure 2: Treatment Variation in DC Plan Enrollment across Occupations and Cohorts

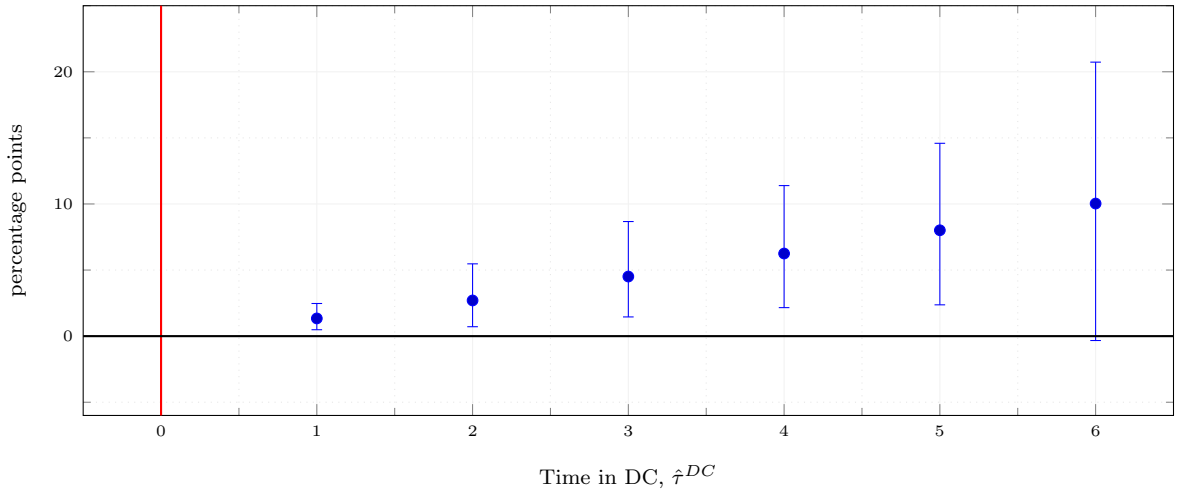


Each cell in Figure 2 corresponds to a birth cohort–calendar year pair; the number indicates the individual's age. Dark cells denote enrollment in an occupational DC pension plan. Hatched cells mark cohorts first treated in 2000, which are excluded from the estimation sample because the event-study design, estimated in first differences, requires at least one pre-treatment observation (earliest usable treatment cohort: 2001). The sample is restricted to ages 18–34.

Figure 3: Stock Market Entry by Time in DC: (a) Yearly effect; (b) Cumulative effect



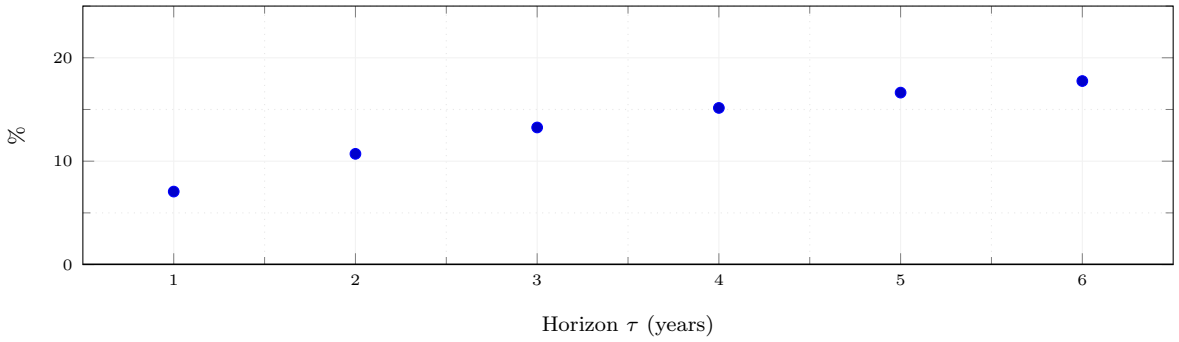
(a) Yearly effect



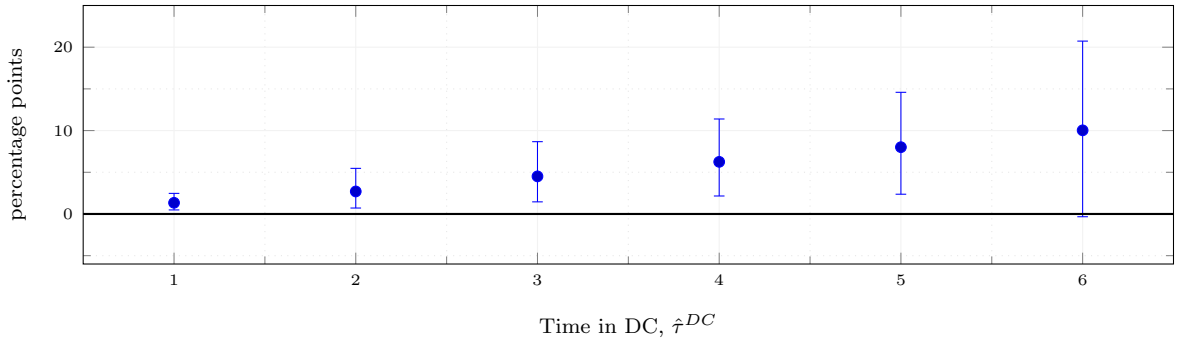
(b) Cumulative effect

Figure 3 plots event-study estimates of the effect of occupational DC enrollment on stock market entry from Eq. (17), using the Column (3) specification in Table 2. Panel (a) shows the event-time coefficients $100 \times \beta_j$ and Panel (b) shows their cumulative sum $100 \times \sum_{k=1}^{j} \beta_k$, so the vertical axis can be interpreted as percentage-point changes in the one-year entry probability. Error bars correspond to 95% wild cluster bootstrap confidence intervals, with standard errors clustered at the treatment-time-by-occupation level. The vertical red line at $\hat{\tau}^{DC} = 0$ denotes the year preceding DC enrollment.

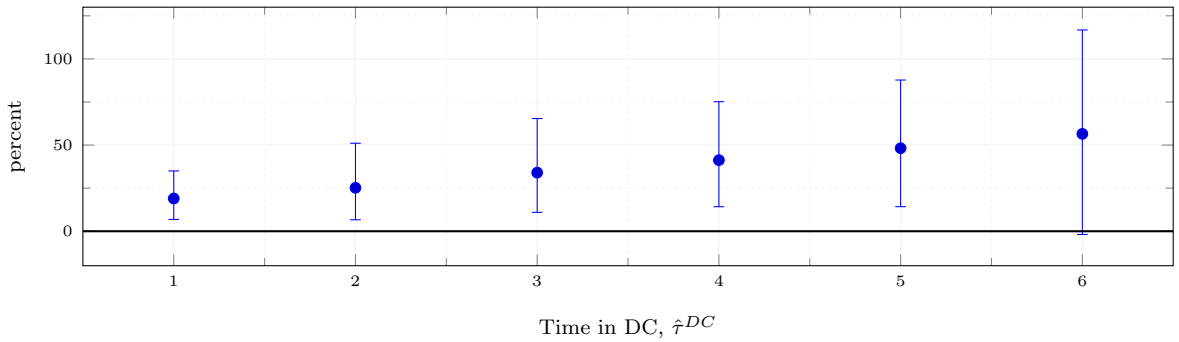
Figure 4: Stock Market Entry by Time in DC: (a) Unconditional cumulative entry; (b) Cumulative sum of event-study coefficients; (c) Cumulative effect relative to unconditional cumulative entry



(a) Unconditional cumulative entry



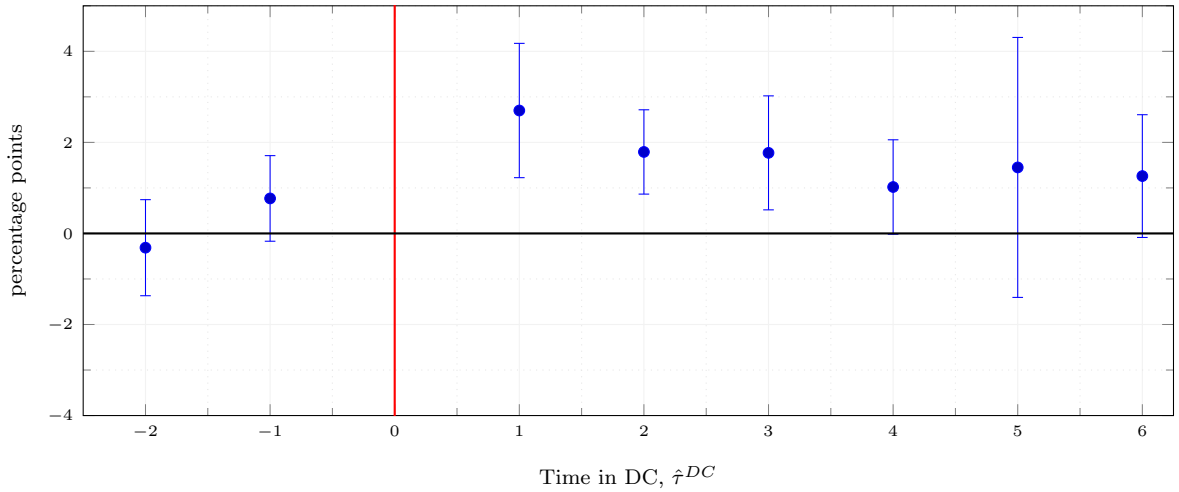
(b) Cumulative sum of event-study coefficients



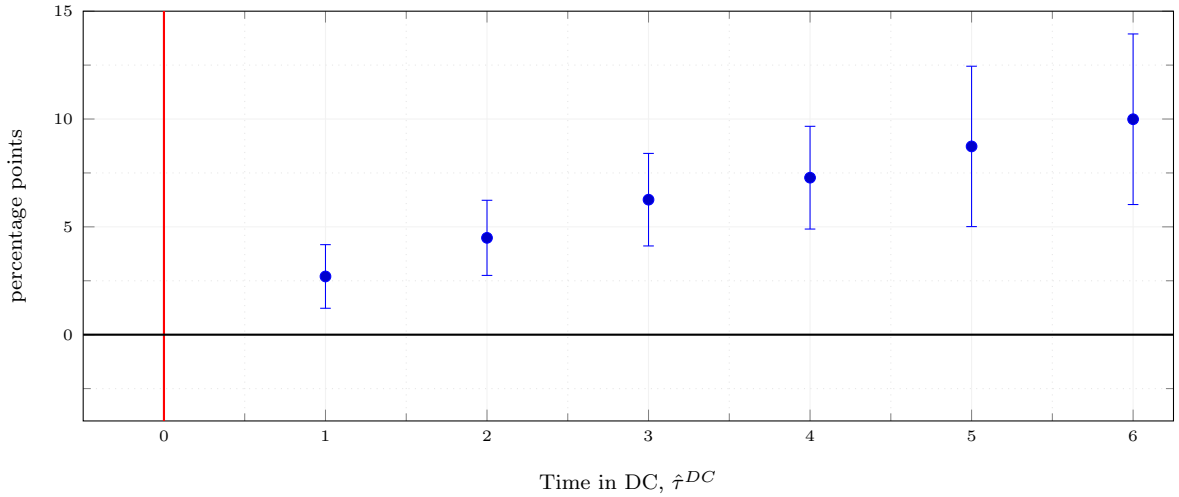
(c) Cumulative effect scaled by unconditional cumulative entry

Panel (a) plots the unconditional cumulative (ever entered) stock market entry probability p_τ for horizons $\tau = 1, \ldots, 6$. For each baseline-year cohort, I fix the sample of non-participants in the baseline year and define entry as participation at least once within the next τ years. I then compute p_τ within each cohort and take the unweighted average across cohorts, accounting for shorter follow-up for later baseline years. Panel (b) plots the cumulative sum of the post-treatment event-study coefficients, $\sum_{k=1}^{j} \beta_k$, from Eq. (17). Panel (c) rescales Panel (b) by p_j from Panel (a), i.e., $100 \times \left(\sum_{k=1}^{j} \beta_k\right) / p_j$. Error bars in Panels (b) and (c) correspond to 95% wild cluster bootstrap confidence intervals with standard errors clustered at the treatment-time-by-occupation level.

Figure 5: Active change in Risky Share by Time in DC: (a) Yearly effect; (b) Cumulative effect



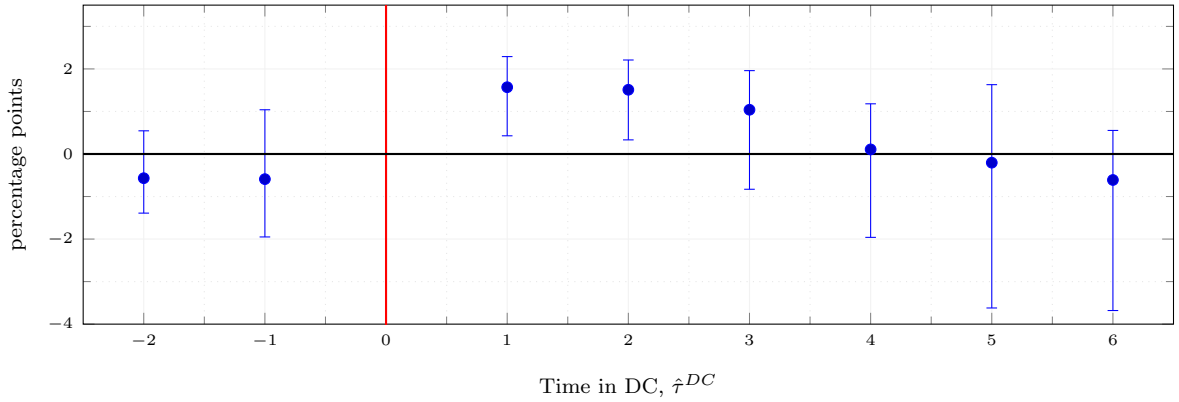
(a) Yearly effect



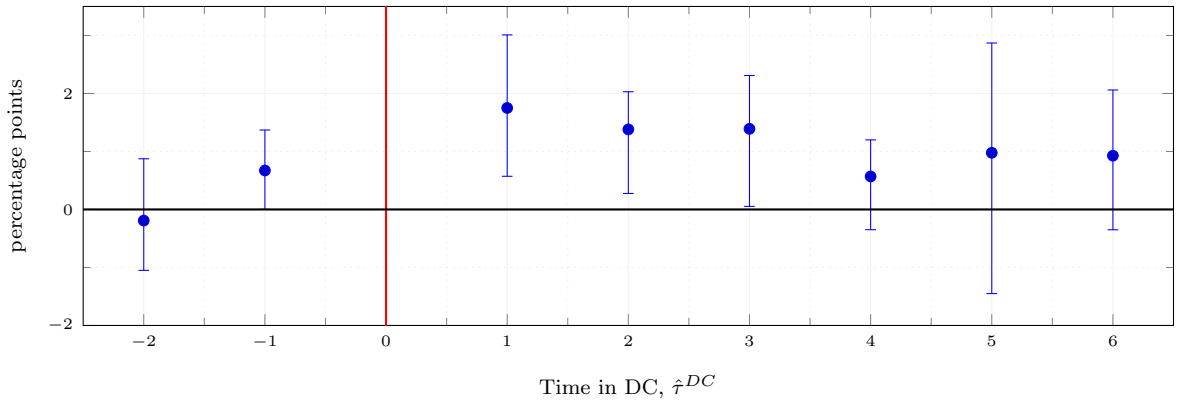
(b) Cumulative effect

Figure 5 presents event-study estimates of the dynamic effects from Eq. (18) for the active change in risky share, $\Delta\omega^a_{it}$. Panel (a) plots the event-time coefficients $100 \times \beta_j$ (percentage points) for $\hat{\tau}^{DC}$. Panel (b) plots the cumulative sum of post-treatment coefficients, $100 \times \sum_{k=1}^{j} \beta_k$, so the vertical axis can be interpreted as the accumulated percentage-point change in $\Delta\omega^a$ since enrollment. Both panels report 95% confidence intervals based on the wild cluster bootstrap. The vertical red line at $\hat{\tau}^{DC} = 0$ denotes the year preceding DC enrollment.

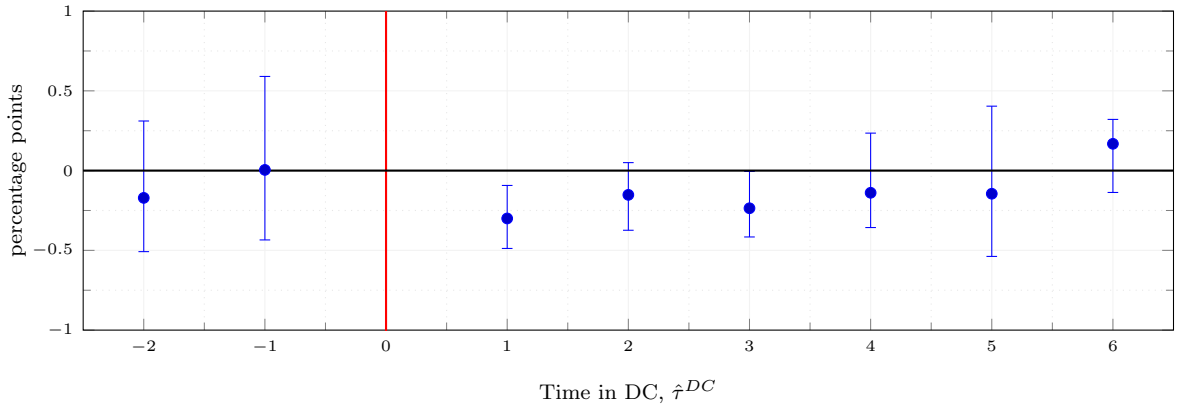
Figure 6: Portfolio Composition Outcomes by Time in DC: (a) $P(Buy_t^F)$; (b) $\Delta F_t^{\%FW}$; (c) $\Delta F_t^{\%Risky}$



(a) $P(Buy_t^F)$



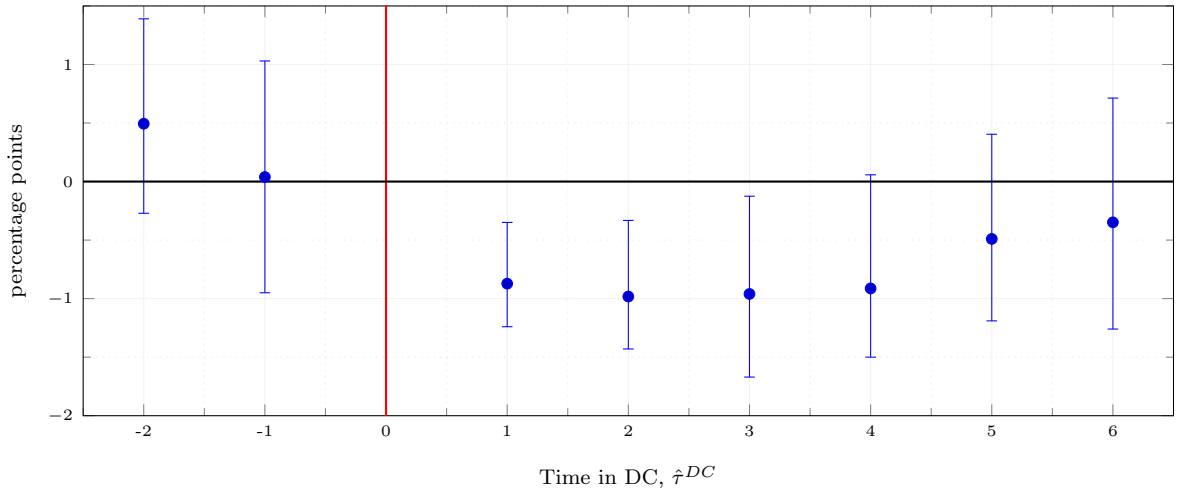
(b) $\Delta F_t^{\%FW}$



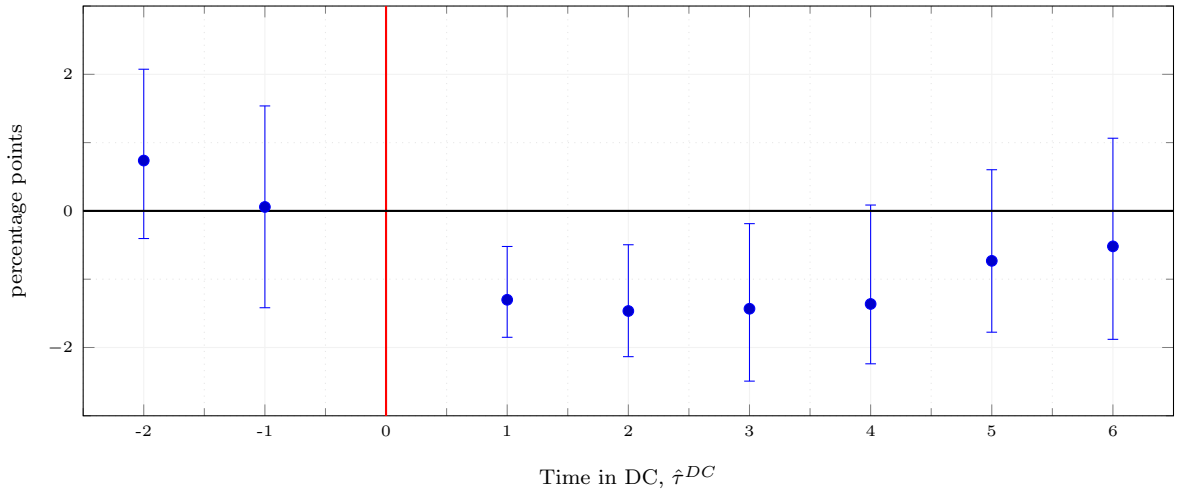
(c) $\Delta F_t^{\%Risky}$

Figure 6 plots event-study coefficients for diversification outcomes from Eqs. (19)–(20), scaled by 100 so the vertical axis is in percentage points. Event time is measured relative to the year before DC enrollment, with $\hat{\tau}^{DC} = 0$ omitted as the reference period. Panel (a) reports estimates for the linear probability model (LPM) outcome $Buy_{it}^F \in \{0, 1\}$. Panels (b) and (c) report estimates for the change in the mutual-fund share of total financial wealth, $\Delta F_{it}^{\%FW}$, and the change in the mutual-fund share within risky assets, $\Delta F_{it}^{\%Risky}$. Error bars are 95% confidence intervals based on the wild cluster bootstrap with clustering at the treatment-time-by-occupation level.

Figure 7: Sharpe ratio loss by time in DC: (a) Regression results; (b) Effects relative to the unconditional mean SR loss



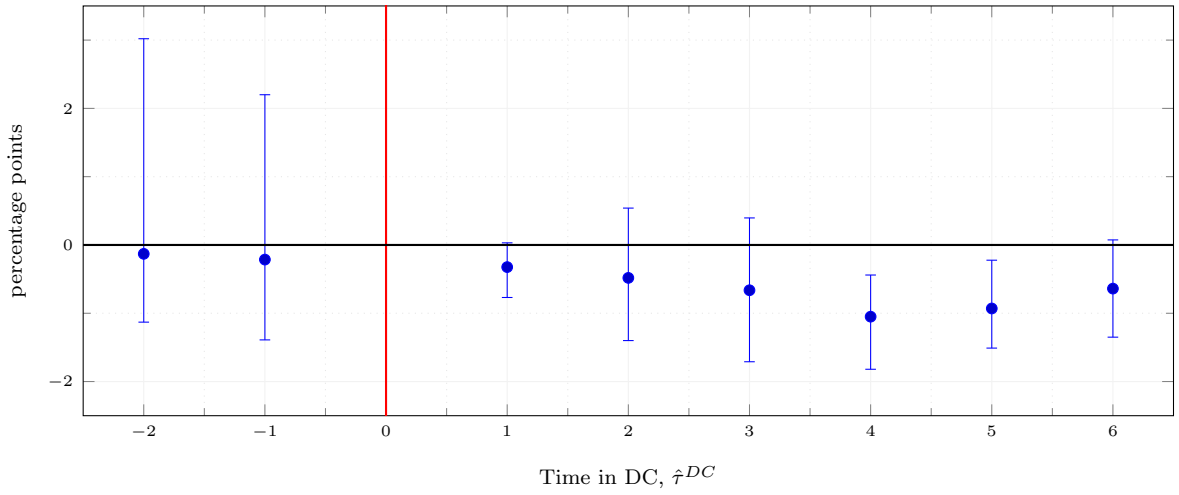
(a) Regression results



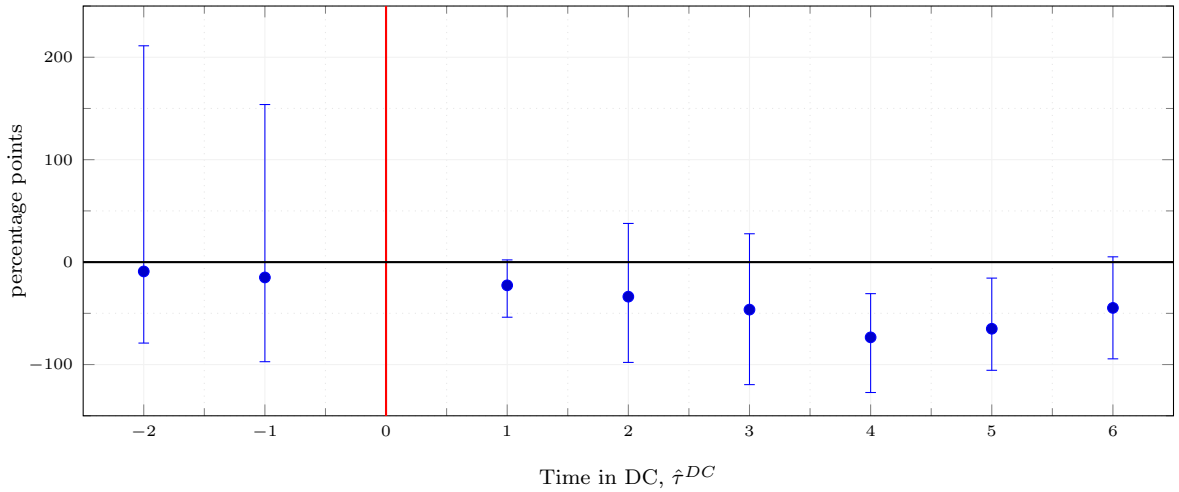
(b) Effects relative to the unconditional mean

Figure 7 plots event-study estimates for the Sharpe-ratio-loss outcome. Panel (a) reports percentage point changes in *RSRL* from the event-study specification (analogous to the entry and risky-share regressions), with 95% confidence intervals based on the wild cluster bootstrap. Panel (b) rescales these effects by the unconditional mean Sharpe ratio loss in the estimation sample (67%), expressed in percentage points.

Figure 8: Fund costs by time in DC: (a) Regression results; (b) Effects relative to the unconditional mean fund cost



(a) Regression results



(b) Effects relative to the unconditional mean

Figure 8 plots event-study estimates for fund costs. Panel (a) reports percentage-point changes in fund costs ($100 \times \beta_j$) from the event-study specification (analogous to the entry and risky-share regressions), with 95% confidence intervals based on the wild cluster bootstrap. Panel (b) rescales these effects by the unconditional mean fund cost in the estimation sample (1.45%), expressed in percentage points.

Figure 9: Sibling spillovers around sibling DC enrollment: event-time effects

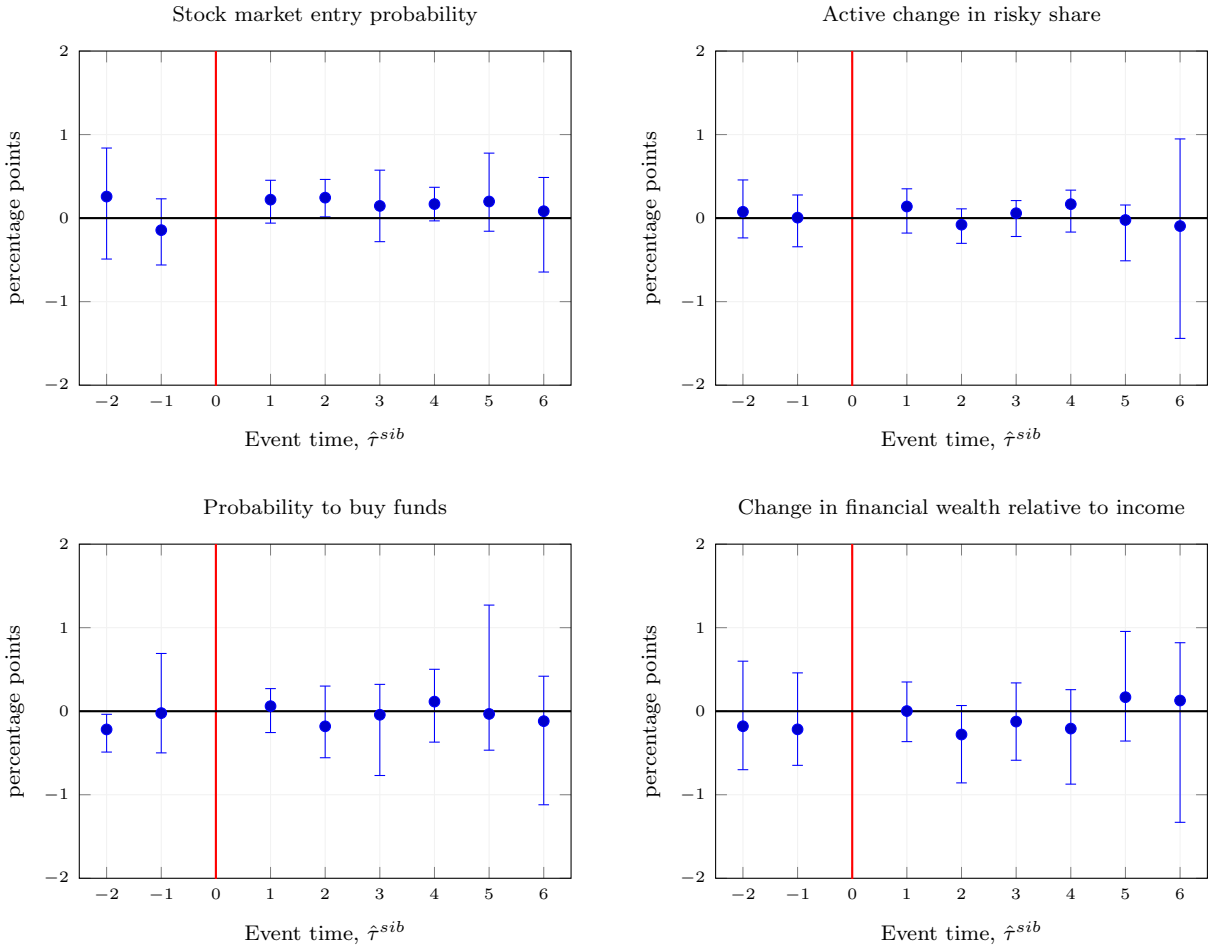
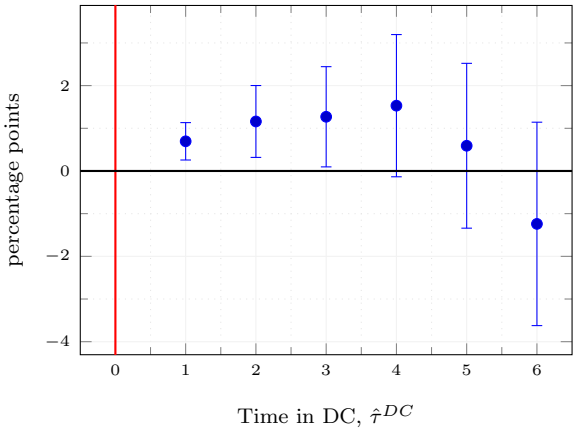
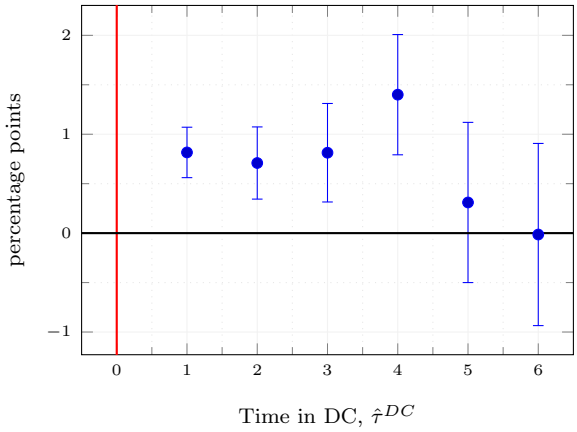


Figure 9 plots staggered DiD event-time coefficients for four outcomes around sibling DC enrollment, with $\hat{\tau}^{sib} = 0$ omitted. Points show percentage-point effects ($100 \times \beta_j$). Error bars are 95% wild cluster bootstrap confidence intervals with clustering at the treatment-time-by-occupation level.

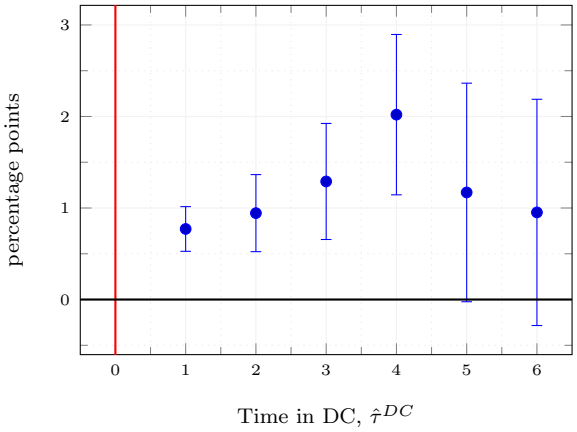
Figure 10: ETWFE event-study effects by time in DC (post periods): participation, risky-share rebalancing, funds, and wealth



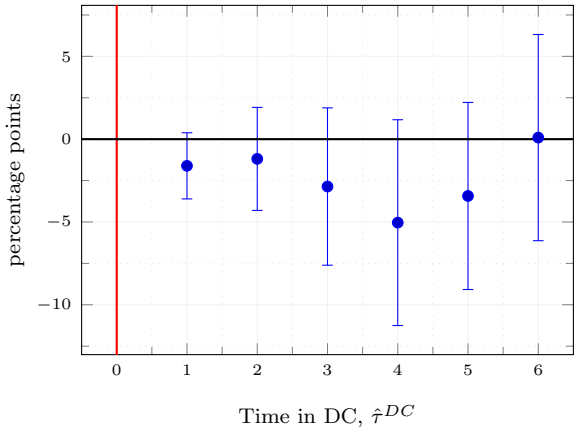
(a) Stock market entry probability



(b) Active change in risky share



(c) Mutual-fund share of financial wealth



(d) Change in financial wealth relative to income

Figure 10 plots post-enrollment event-time coefficients from the ETWFE model for four outcomes. The vertical axis reports percentage-point effects ($100 \times \beta_j$). Error bars are 95% confidence intervals with standard errors clustered at the treatment-time-by-occupation level. The omitted reference period is $\hat{\tau}^{DC} = 0$; event times correspond to the first through sixth years after DC enrollment.

Tables

Table 1: Descriptive Statistics for Sweden

(a) Full Sample

	(1) Kids	(2) Marital	(3) Edu	(4) Y	(5) Partic	(6) Entry	(7) FW	(8) $\Delta FW/Y$	(9) $\Delta \log FW$	(10) $\Delta^a \log FW$	(11) $\Delta_g FW$
N	4.31m	4.31m	4.3m	4.31m	4.31m	2.24m	4.31m	4.30m	4.31m	4.2m	4.2m
Mean	0.45	0.18	1.42	169,635	0.51	0.07	70,077	0.05	-0.04	-0.15	0.63
SD	0.49	0.41	0.86	68,443	0.50	0.25	121,987	0.48	2.08	1.86	2.54
p25	0.00	0.00	1.00	125,260	0.00	0.00	12,301	-0.05	-0.23	-0.25	-0.22
p50	0.00	0.00	1.00	166,991	1.00	0.00	23,248	0.02	0.15	0.15	0.16
p75	1.00	0.00	2.00	209,935	1.00	0.00	69,131	0.08	0.45	0.44	0.55

(b) Stock Market Participants ($t-1$ and t)

	(1) w	(2) Δw	(3) Δw^{active}	(4) $\Delta \omega^{active_{alt}}$	(5) $F^{\%Risky}$	(6) $F^{\%FW}$	(7) $P(Buy_t^F)$	(8) $Cost^F$
N	2.06m	2.06m	2.06m	2.06m	2.06m	2.06m	2.06m	0.64m
Mean	0.44	-0.01	-0.01	0.01	0.76	0.32	0.63	1.45
SD	0.29	0.22	0.22	0.21	0.37	0.28	0.48	0.27
p25	0.17	-0.10	-0.10	-0.07	0.62	0.06	0.00	1.41
p50	0.41	-0.00	-0.01	0.00	1.00	0.27	1.00	1.43
p75	0.68	0.08	0.09	0.09	1.00	0.53	1.00	1.46

Table 1 presents descriptive statistics for the Swedish sample of individuals aged 18–34 used in the study. Panel (a) reports statistics for the full sample: number of children (*Kids*), marital status (*Marital*), education level (*Edu*), income (Y), a stock market participation indicator (*Partic*), stock market entry (*Entry*), financial wealth (FW), the change in the financial-wealth-to-income ratio ($\Delta FW/Y$), the change in (log) financial wealth ($\Delta \log FW$), the active (log) change in wealth ($\Delta^a \log FW$), and the growth rate of financial wealth ($\Delta_g FW$), shown in Columns (1)–(11). Panel (b) restricts the sample to individuals who participate in the stock market in both $t-1$ and t, and reports the risky share of financial wealth (w), the change in risky share (Δw), active changes in risky share (Δw^{active} and $\Delta \omega^{active_{alt}}$), the share of risky assets held in mutual funds ($F^{\%Risky}$), the share of financial wealth held in mutual funds ($F^{\%FW}$), the probability of buying mutual funds ($P(Buy_t^F)$), and mutual-fund cost (in percent) measures ($Cost^F$), shown in Columns (1)–(8). The table reports the number of observations (N), mean, standard deviation (SD), and the 25th (p25), 50th (p50), and 75th (p75) percentiles. Monetary values are in 2010 Swedish Krona (SEK).

Table 2: Stock Market Entry and DC Plan Eligibility

	(1) Baseline	(2) + Family & Housing	(3) + Education & Region
$\hat{\tau}^{DC} = 1$	0.0139***	0.0141***	0.0134***
	[0.00497,0.0261]	[0.00531,0.0259]	[0.00485,0.0247]
$\Delta \log FW_t^a$	0.0142***	0.0139***	0.0136***
	[0.0129,0.0155]	[0.0127,0.0151]	[0.0124,0.0148]
$\Delta \log Y_t$	0.00551***	0.00542***	0.00516***
	[0.00324,0.00779]	[0.00323,0.00761]	[0.00319,0.00712]
		Marriage status (Base is not married)	
Married		-0.00933***	-0.0102***
		[-0.0117,-0.00696]	[-0.0116,-0.00876]
Divorced		-0.00913***	-0.00449***
		[-0.0115,-0.00679]	[-0.00699,-0.00198]
Widowed		0.0455***	0.0489***
		[0.0152,0.0759]	[0.0199,0.0780]
Have kids		-0.0126***	-0.00649***
		[-0.0160,-0.00928]	[-0.00938,-0.00359]
Homeowner		0.00510***	0.00449***
		[0.00256,0.00763]	[0.00213,0.00685]
		Education (Base is no high school)	
High school			0.0129***
			[0.0101,0.0157]
Some college			0.0235***
			[0.0191,0.0280]
College			0.0467***
			[0.0374,0.0560]
Post-grad			0.0434***
			[0.0291,0.0577]
Year and Age FE	Yes	Yes	Yes
Region FE	No	No	Yes
Occupation×Year FE	Yes	Yes	Yes
N	2,139,142	2,139,142	2,132,709
Adj. R^2	0.0578	0.0588	0.0614

Standard errors are clustered at the treatment-time-by-occupation level. Brackets report 95% confidence intervals based on the wild cluster bootstrap; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 reports estimates from Eq. (17) for stock market entry between $t-1$ and t, estimated on the sample of non-participants at $t-1$ (i.e., conditional on $Partic_{i,t-1} = 0$). The key regressor is the event-time indicator for the first treatment year, $I[\hat{\tau}^{DC} = 1]$, with $\hat{\tau}^{DC} = 0$ omitted as the reference period. All specifications include age dummies $I[a_{it}]$, year fixed effects $I[y_t]$, and occupation-by-year fixed effects $I[\text{occupation} \times y_t]$. The baseline controls include active wealth growth $\Delta \log FW_t^a$ (Eq. 10) and income growth $\Delta \log Y_t$, Columns (2) and (3) add family/housing and education/region controls respectively. Standard errors are clustered at the treatment-time-by-occupation level, confidence intervals and significance are based on the wild cluster bootstrap.

Table 3: Stock Market Entry: Event-Time Coefficients Under Alternative Wealth Controls

	(1) Baseline: $\Delta \log FW_t^a$	(2) $\Delta \log FW_t$	(3) Pre-wealth×year	(4) No wealth controls
$\hat{\tau}^{DC} = -2$	0.00376	0.00250	0.00623*	0.00168
	[-0.00426, 0.0114]	[-0.00698, 0.00988]	[-0.000624, 0.0154]	[-0.00790, 0.00956]
$\hat{\tau}^{DC} = -1$	0.00490	0.00325	0.00340	0.00257
	[-0.00407, 0.0187]	[-0.00466, 0.0129]	[-0.0101, 0.0169]	[-0.00460, 0.0123]
$\hat{\tau}^{DC} = 1$	0.0134***	0.00957***	0.00153	0.00566***
	[0.00485, 0.0247]	[0.00266, 0.0175]	[-0.00183, 0.00601]	[0.00182, 0.0141]
$\hat{\tau}^{DC} = 2$	0.0136**	0.00590	0.000713	0.00182
	[0.00228, 0.0300]	[-0.00454, 0.0154]	[-0.00427, 0.00902]	[-0.00517, 0.0128]
$\hat{\tau}^{DC} = 3$	0.0181***	0.00882***	0.00154	0.00406*
	[0.00741, 0.0320]	[0.00410, 0.0183]	[-0.00858, 0.00971]	[-0.000457, 0.0146]
$\hat{\tau}^{DC} = 4$	0.0174***	0.00691*	0.000109	0.00354*
	[0.00703, 0.0272]	[0.000287, 0.0160]	[-0.00570, 0.00619]	[-0.000352, 0.0120]
$\hat{\tau}^{DC} = 5$	0.0176**	0.00719	-0.00273	0.00278**
	[0.00212, 0.0320]	[-0.00286, 0.0134]	[-0.0105, 0.00535]	[0.000165, 0.0102]
$\hat{\tau}^{DC} = 6$	0.0202	0.00834	-0.00670*	0.00524
	[-0.0270, 0.0614]	[-0.0245, 0.0326]	[-0.0178, 0.000324]	[-0.0133, 0.0180]
Year and Age FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Occupation×Year FE	Yes	Yes	Yes	Yes
N	2,132,709	2,233,659	1,410,478	2,233,659
Adj. R^2	0.0614	0.0597	0.0488	0.0474

Standard errors are clustered at the treatment-time-by-occupation level. Brackets report 95% confidence intervals based on the wild cluster bootstrap; significance levels are based on the corresponding wild-bootstrap p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 reports event-time coefficients from Eq. (17) for stock market entry between $t-1$ and t, estimated on the sample of non-participants at $t-1$ (i.e., conditional on $Partic_{i,t-1} = 0$). Event time is measured relative to the year before DC enrollment, with $\hat{\tau}^{DC} = 0$ omitted as the reference period. Column (1) is the baseline specification using active wealth growth, $\Delta \log FW_t^a$. Columns (2)–(4) modify the wealth controls as indicated in the column headers, holding the remaining controls and fixed effects constant. Standard errors are clustered at the treatment-time-by-occupation level, confidence intervals and significance are based on the wild cluster bootstrap.

Table 4: Stock Market Entry: Event-Time Coefficients (Baseline vs. Excluding Exposed Controls)

	(1) Baseline	(2) Exclude exposed controls
$\hat{\tau}^{DC} = -2$	0.00376	0.00490
	[-0.00426, 0.0114]	[-0.00376, 0.0132]
$\hat{\tau}^{DC} = -1$	0.00490	0.00615
	[-0.00407, 0.0187]	[-0.00279, 0.0190]
$\hat{\tau}^{DC} = 1$	0.0134***	0.0136***
	[0.00485, 0.0247]	[0.00531, 0.0252]
$\hat{\tau}^{DC} = 2$	0.0136**	0.0140***
	[0.00228, 0.0300]	[0.00333, 0.0310]
$\hat{\tau}^{DC} = 3$	0.0181***	0.0191***
	[0.00741, 0.0320]	[0.00974, 0.0312]
$\hat{\tau}^{DC} = 4$	0.0174***	0.0187***
	[0.00703, 0.0272]	[0.00900, 0.0282]
$\hat{\tau}^{DC} = 5$	0.0176**	0.0188**
	[0.00212, 0.0320]	[0.00419, 0.0326]
$\hat{\tau}^{DC} = 6$	0.0202	0.0208
	[-0.0270, 0.0614]	[-0.0244, 0.0602]
Year and Age FE	Yes	Yes
Region FE	Yes	Yes
Occupation×Year FE	Yes	Yes
N	2,132,709	2,029,895
Adj. R^2	0.0614	0.0614

Standard errors are clustered at the treatment-time-by-occupation level. Brackets report 95% confidence intervals, that are based on the wild cluster bootstrap along with significance levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 reports event-time coefficients from Eq. (17) for stock market entry between $t-1$ and t, estimated on the sample of non-participants at $t-1$. The key regressors are indicators for event time relative to the year before DC enrollment, $I[\hat{\tau}^{DC} = j]$, with $\hat{\tau}^{DC} = 0$ omitted as the reference period. All specifications include age dummies $I[a_{it}]$, year fixed effects $I[y_t]$, and occupation-by-year fixed effects $I[\text{occupation} \times y_t]$, and control for time-varying wealth and income growth ($\Delta \log FW_t^a$ (Eq. 10) and $\Delta \log Y_t$) as well as family status, housing, and education controls. Column (1) reports the baseline specification estimated on the full sample. Column (2) re-estimates the same specification after excluding not-yet-treated individuals classified as exposed controls (defined as members of parish×occupation cells in which at least 75% of peers are already treated). Standard errors are clustered at the treatment-time-by-occupation level, confidence intervals and significance are based on the wild cluster bootstrap.

Table 5: Stock Market Exit and DC Plan Eligibility (With vs. Without Active Wealth Growth)

	(1) Baseline: $\Delta \log FW_t^a$	(2) No active-wealth control
$\hat{\tau}^{DC} = -2$	0.0000646	0.00160*
	[-0.00456, 0.00427]	[-0.0000980, 0.00294]
$\hat{\tau}^{DC} = -1$	-0.00205	-0.000293
	[-0.00726, 0.00317]	[-0.00205, 0.00165]
$\hat{\tau}^{DC} = 1$	-0.00281	0.00107
	[-0.00808, 0.00181]	[-0.00125, 0.00329]
$\hat{\tau}^{DC} = 2$	-0.00293	0.00276**
	[-0.00672, 0.00146]	[0.000655, 0.00496]
$\hat{\tau}^{DC} = 3$	-0.00529***	0.00150**
	[-0.00832, -0.00150]	[0.000266, 0.00347]
$\hat{\tau}^{DC} = 4$	-0.00569**	0.000884
	[-0.0101, -0.000661]	[-0.00120, 0.00270]
$\hat{\tau}^{DC} = 5$	-0.00731	0.00169
	[-0.0157, 0.00196]	[-0.00117, 0.00481]
$\hat{\tau}^{DC} = 6$	-0.00736	0.000534
	[-0.0305, 0.0133]	[-0.00311, 0.00418]
$\Delta \log FW_t^a$	-0.0813***	
	[-0.0871, -0.0754]	
$\Delta \log Y_t$	0.00938***	-0.00266***
	[0.00780, 0.0110]	[-0.00360, -0.00173]
	Marriage status (Base is not married)	
Married	0.00663***	0.00897***
	[0.00508, 0.00819]	[0.00728, 0.0107]
Divorced	0.0424***	0.0470***
	[0.0374, 0.0473]	[0.0403, 0.0537]
Widowed	-0.00662	-0.0224***
	[-0.0239, 0.0106]	[-0.0367, -0.00818]
Have kids	0.00250**	0.00811***
	[0.000449, 0.00455]	[0.00584, 0.0104]
Homeowner	-0.00578**	-0.00610**
	[-0.0109, -0.000633]	[-0.0118, -0.000381]
	Education (Base is no high school)	
High school	-0.0323***	-0.0389***
	[-0.0381, -0.0264]	[-0.0488, -0.0289]
Some college	-0.0469***	-0.0546***
	[-0.0569, -0.0370]	[-0.0705, -0.0387]
College	-0.0555***	-0.0650***
	[-0.0634, -0.0476]	[-0.0778, -0.0522]
Post-grad	-0.0533***	-0.0633***
	[-0.0631, -0.0435]	[-0.0773, -0.0493]
Year and Age FE	Yes	Yes
Region FE	Yes	Yes
Occupation×Year FE	Yes	Yes
N	2,270,525	2,270,525
Adj. R^2	0.113	0.00955

Standard errors are clustered at the treatment-time-by-occupation level. Brackets report 95% confidence intervals based on the wild cluster bootstrap; significance levels are based on the corresponding wild-bootstrap p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 reports estimates from the exit analogue of Eq. (17), where the outcome equals one if an individual exits the stock market between $t-1$ and t (i.e., $Partic_{i,t-1} = 1$ and $Partic_{it} = 0$), estimated on the sample of participants at $t-1$. The key regressors are the event-time indicators, $I[\hat{\tau}^{DC} = j]$, with $\hat{\tau}^{DC} = 0$ omitted as the reference period. Both columns include age dummies $I[a_{it}]$, year fixed effects $I[y_t]$, occupation-by-year fixed effects $I[\text{occupation} \times y_t]$, and the same set of covariates as in the entry specification. Column (1) additionally controls for active wealth growth, $\Delta \log FW_t^a$; Column (2) omits this control.

Table 6: Risky Share Adjustments and DC Plan Enrollment

	(1) Δw_t^{active}	(2) $\Delta \omega^{active_{alt}}$	(3) Δw_t
$\hat{\tau}^{DC} = 1$	0.0122**	0.0110**	0.0115**
	[0.00256,0.0219]	[0.00121,0.0209]	[0.00159,0.0215]
$\log FW_{t-1}$	0.0190***	0.0201***	0.0190***
	[0.0179,0.0200]	[0.0194,0.0209]	[0.0183,0.0197]
	Marriage status (Base is not married)		
Married	0.00131***	0.000476	0.000513
	[0.000359,0.00226]	[-0.000248,0.00120]	[-0.000237,0.00126]
Divorced	0.00713**	0.00552**	0.00536*
	[0.00116,0.0131]	[0.000148,0.0109]	[-0.0000950,0.0108]
Widowed	-0.0106	-0.0120	-0.0122
	[-0.0425,0.0213]	[-0.0375,0.0135]	[-0.0370,0.0127]
Have kids	0.0131***	0.0122***	0.0124***
	[0.0103,0.0158]	[0.00913,0.0153]	[0.00937,0.0154]
Homeowner	-0.00564***	-0.00686***	-0.00692***
	[-0.00766,-0.00363]	[-0.00912,-0.00460]	[-0.00920,-0.00464]
	Education (Base is no high school)		
High school	-0.00656***	-0.00707***	-0.00762***
	[-0.00879,-0.00433]	[-0.0101,-0.00400]	[-0.0104,-0.00488]
Some college	-0.00760***	-0.0120***	-0.0130***
	[-0.0108,-0.00445]	[-0.0152,-0.00886]	[-0.0158,-0.0102]
College	-0.0168***	-0.0207***	-0.0223***
	[-0.0196,-0.0140]	[-0.0239,-0.0176]	[-0.0251,-0.0195]
Post-grad	-0.0162***	-0.0221***	-0.0236***
	[-0.0200,-0.0123]	[-0.0260,-0.0181]	[-0.0273,-0.0198]
Year and Age FE	Yes	Yes	Yes
Region FE	Yes	Yes	Yes
Occupation×Year FE	Yes	Yes	Yes
N	2,065,719	2,066,014	2,066,014
Adj. R^2	0.0232	0.0714	0.0518

Standard errors are clustered at the treatment-time-by-occupation level. Brackets report 95% confidence intervals based on the wild cluster bootstrap; stars use the corresponding wild-bootstrap p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6 reports estimates from Eq. (18) on the full subsample of stock market participants in both $t-1$ and t. Event time is measured relative to the year before DC enrollment; $\hat{\tau}^{DC} = 0$ is omitted. The table reports the impact coefficient ($\hat{\tau}^{DC} = 1$) and key covariates.

Table 7: Risky Share Adjustments and DC Plan Enrollment (Excl. high pre-treatment ω)

	(1) Δw_t^{active}	(2) $\Delta \omega^{active_{alt}}$	(3) Δw_t
$\hat{\tau}^{DC} = 1$	0.0270***	0.0254***	0.0261***
	[0.0126,0.0421]	[0.0106,0.0403]	[0.0113,0.0414]
$\log FW_{t-1}$	0.0217***	0.0220***	0.0210***
	[0.0197,0.0237]	[0.0208,0.0231]	[0.0199,0.0221]
	Marriage status (Base is not married)		
Married	0.00149***	0.000768**	0.000819**
	[0.000560,0.00241]	[0.0000647,0.00147]	[0.0000894,0.00155]
Divorced	0.00600**	0.00483*	0.00480*
	[0.000288,0.0117]	[-0.0000664,0.00974]	[-0.000119,0.00973]
Widowed	-0.0133	-0.0151	-0.0158
	[-0.0479,0.0214]	[-0.0438,0.0136]	[-0.0441,0.0125]
Have kids	0.0128***	0.0119***	0.0121***
	[0.0101,0.0155]	[0.00889,0.0148]	[0.00920,0.0150]
Homeowner	-0.00561***	-0.00685***	-0.00699***
	[-0.00758,-0.00364]	[-0.00905,-0.00464]	[-0.00921,-0.00477]
	Education (Base is no high school)		
High school	-0.00694***	-0.00753***	-0.00813***
	[-0.00956,-0.00431]	[-0.0111,-0.00393]	[-0.0115,-0.00480]
Some college	-0.00750***	-0.0119***	-0.0129***
	[-0.0113,-0.00368]	[-0.0158,-0.00798]	[-0.0165,-0.00937]
College	-0.0172***	-0.0212***	-0.0230***
	[-0.0205,-0.0140]	[-0.0249,-0.0175]	[-0.0265,-0.0196]
Post-grad	-0.0180***	-0.0236***	-0.0254***
	[-0.0219,-0.0140]	[-0.0280,-0.0192]	[-0.0297,-0.0211]
Year and Age FE	Yes	Yes	Yes
Region FE	Yes	Yes	Yes
Occupation×Year FE	Yes	Yes	Yes
N	1,908,554	1,908,813	1,908,813
Adj. R^2	0.0268	0.0750	0.0552

Brackets report 95% confidence intervals based on the wild bootstrap; stars use the corresponding wild-bootstrap p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7 reports estimates from Eq. (18) on the subsample of stock market participants in both $t-1$ and t conditional on their pre-treatment risky share does not exceed 90%. Event time is measured relative to the year before DC enrollment; $\hat{\tau}^{DC} = 0$ is omitted. All specifications include age dummies, year fixed effects, region fixed effects, and occupation-by-year fixed effects. Brackets report 95% confidence intervals based on the wild cluster bootstrap; statistical significance is based on the corresponding wild-bootstrap p-values. The full set of dynamic effects is shown in Figure 5.

Table 8: Dynamic Effects of DC Enrollment on Wealth Outcomes

	(1) $\Delta FW/Y$	(2) $\Delta_g FW$	(3) $\Delta \ln FW$	(4) $\Delta \log FW^a$
$\hat{\tau}^{DC} = -2$	0.000916	-0.0225	-0.0195	-0.0247
	[-0.0100, 0.0119]	[-0.192, 0.0892]	[-0.218, 0.154]	[-0.248, 0.149]
$\hat{\tau}^{DC} = -1$	-0.0105	0.0238	-0.0213	-0.0359
	[-0.0414, 0.0153]	[-0.200, 0.223]	[-0.233, 0.107]	[-0.290, 0.107]
$\hat{\tau}^{DC} = 1$	-0.00520	-0.179***	-0.168	-0.192**
	[-0.0199, 0.0104]	[-0.286, -0.0516]	[-0.378, 0.0706]	[-0.388, -0.0155]
$\hat{\tau}^{DC} = 2$	-0.0119	-0.209**	-0.183*	-0.245**
	[-0.0295, 0.00571]	[-0.385, -0.0271]	[-0.313, 0.00467]	[-0.411, -0.0403]
$\hat{\tau}^{DC} = 3$	-0.0118	-0.197**	-0.241***	-0.324***
	[-0.0334, 0.00530]	[-0.368, -0.0221]	[-0.422, -0.0306]	[-0.514, -0.116]
$\hat{\tau}^{DC} = 4$	-0.00205	-0.138	-0.149	-0.264**
	[-0.0323, 0.0231]	[-0.257, 0.0219]	[-0.298, 0.0783]	[-0.431, -0.0386]
$\hat{\tau}^{DC} = 5$	-0.0247*	-0.169	-0.230	-0.344
	[-0.0606, 0.00346]	[-0.359, 0.105]	[-0.440, 0.134]	[-0.661, 0.0703]
$\hat{\tau}^{DC} = 6$	-0.0331	-0.0969	-0.159	-0.302
	[-0.125, 0.0156]	[-0.492, 0.542]	[-0.383, 0.120]	[-0.727, 0.527]
$\Delta \log Y_t$		0.306***	0.262***	0.161***
		[0.258, 0.354]	[0.217, 0.306]	[0.143, 0.179]
	Marriage status (Base is not married)			
Have kids	-0.0193***	-0.0593***	-0.168***	-0.217***
	[-0.0231, -0.0155]	[-0.0849, -0.0337]	[-0.217, -0.120]	[-0.277, -0.157]
Married	-0.00356*	0.0586***	-0.0359***	-0.0662***
	[-0.00738, 0.000248]	[0.0380, 0.0791]	[-0.0486, -0.0231]	[-0.0867, -0.0457]
Divorced	-0.00646**	0.161***	-0.0636***	-0.130***
	[-0.0115, -0.00146]	[0.103, 0.220]	[-0.0979, -0.0293]	[-0.177, -0.0826]
Widowed	0.215***	1.707***	0.288***	0.250***
	[0.172, 0.257]	[1.372, 2.043]	[0.204, 0.373]	[0.125, 0.374]
Homeowner	-0.0184***	0.0119	0.186***	0.240***
	[-0.0287, -0.00802]	[-0.0142, 0.0379]	[0.134, 0.237]	[0.176, 0.303]
	Education (Base is no high school)			
High school	0.0144**	-0.0109	0.180***	0.238***
	[0.00315, 0.0256]	[-0.0300, 0.00813]	[0.0740, 0.287]	[0.102, 0.375]
Some college	0.0320**	-0.0689***	0.239***	0.321***
	[0.00625, 0.0577]	[-0.0983, -0.0394]	[0.0921, 0.385]	[0.123, 0.519]
College	0.0380***	-0.117***	0.276***	0.380***
	[0.0199, 0.0562]	[-0.154, -0.0799]	[0.159, 0.392]	[0.226, 0.533]
Post-grad	0.0930***	0.00903	0.299***	0.374***
	[0.0773, 0.109]	[-0.0401, 0.0582]	[0.195, 0.404]	[0.234, 0.514]
Stock market entry	0.149***	2.128***	1.130***	0.868***
	[0.127, 0.170]	[2.002, 2.255]	[0.952, 1.307]	[0.815, 0.922]
Year and Age FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Occupation×Year FE	Yes	Yes	Yes	Yes
N	4,297,935	4,189,882	4,296,347	4,195,397
Adj. R^2	0.0174	0.0440	0.147	0.167

Brackets report 95% confidence intervals based on the wild bootstrap; stars use the corresponding wild-bootstrap p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8 reports estimates from the wealth specification in Eq. (23). The outcomes are: $\Delta FW/Y$, group-normalized wealth growth $\Delta_g FW$, log wealth growth $\Delta \ln FW$, and active wealth growth $\Delta \log FW^a$. All columns include age dummies, year fixed effects, region fixed effects, and occupation-by-year fixed effects. Brackets report 95% confidence intervals based on the wild cluster bootstrap; statistical significance is based on the corresponding wild-bootstrap p-values.

Table 9: Event-study with return since entry: participation and active risky-share rebalancing

	(1) Participation	(2) $\Delta\omega^a$
$\hat{\tau}^{DC} = -2$	0.00371 [-0.00455, 0.0116]	-0.00326 [-0.0140, 0.00705]
$\hat{\tau}^{DC} = -1$	0.00496 [-0.00377, 0.0184]	0.00778* [-0.00137, 0.0173]
$\hat{\tau}^{DC} = 1$	0.0133*** [0.00363, 0.0251]	0.0269*** [0.0124, 0.0421]
$\hat{\tau}^{DC} = 2$	0.0135** [0.00122, 0.0321]	0.0178** [0.00614, 0.0247]
$\hat{\tau}^{DC} = 3$	0.0181*** [0.00772, 0.0320]	0.0177** [0.00257, 0.0279]
$\hat{\tau}^{DC} = 4$	0.0176*** [0.00818, 0.0257]	0.0105* [-0.00198, 0.0192]
$\hat{\tau}^{DC} = 5$	0.0181** [0.00488, 0.0271]	0.0154 [-0.0133, 0.0349]
$\hat{\tau}^{DC} = 6$	0.0206 [-0.0189, 0.0549]	0.0135* [-0.00465, 0.0292]
Wealth control	0.0136*** [0.0124, 0.0148]	0.0941*** [0.0892, 0.0989]
Income control	0.00516*** [0.00319, 0.00712]	8.66e-08*** [5.39e-08, 1.19e-07]
Return since entry	0.00155 [-0.00599, 0.00910]	0.00426 [-0.00645, 0.0150]
Other controls	Yes	Yes
Year and Age FE	Yes	Yes
Region FE	Yes	Yes
Occupation×Year FE	Yes	Yes
N	2,132,709	1,877,764
Adj. R^2	0.0614	0.00148

Brackets report 95% confidence intervals based on the wild bootstrap; stars use the corresponding wild-bootstrap p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9 reports estimates from the baseline specifications augmented with stock returns since DC enrollment. Column (1) reports coefficients from Eq. (17) for stock market entry between $t-1$ and t, estimated on the sample of non-participants at $t-1$ (i.e., conditional on $Partic_{i,t-1} = 0$). Column (2) reports estimates from Eq. (18) for the active change in the risky share, $\Delta\omega^a_{it}$, estimated on the subsample of stock market participants in both $t-1$ and t and additionally restricting to individuals whose pre-treatment risky share does not exceed 90%. In both columns, event time is measured relative to the year before DC enrollment; $\hat{\tau}^{DC} = 0$ is omitted. Both specifications additionally control for cumulative stock returns since DC enrollment. "Wealth control" is $\Delta \log FW^a_t$ in Column (1) and lagged log wealth in Column (2). "Income control" is $\Delta \log Y_t$ in Column (1) and log income in Column (2). Brackets report 95% confidence intervals based on the wild cluster bootstrap; stars use the corresponding wild-bootstrap p-values.

Table 10: ETWFE cohort-by-year ATT(g,t) for core outcomes (all post years)

	Calendar year t					
Cohort g	2001	2002	2003	2004	2005	2006
Panel A: Stock Market Entry, percentage points						
2001	0.96*** [0.33, 1.60]	0.88** [0.02, 1.74]	1.33** [0.24, 2.42]	2.04*** [0.50, 3.58]	3.16*** [1.31, 5.01]	-1.24 [-3.62, 1.14]
2002	–	0.60 [-0.14, 1.35]	0.51 [-0.60, 1.62]	1.58** [0.19, 2.97]	2.47*** [0.66, 4.28]	-0.97 [-3.00, 1.06]
2003	–	–	0.59* [-0.06, 1.24]	1.72*** [0.70, 2.74]	2.76*** [1.30, 4.22]	-0.01 [-1.19, 1.18]
2004	–	–	–	1.11*** [0.56, 1.66]	2.16*** [1.23, 3.09]	-0.10 [-1.20, 1.00]
2005	–	–	–	–	1.61*** [0.95, 2.27]	-0.25 [-1.12, 0.62]
2006	–	–	–	–	–	-0.81*** [-1.36, -0.26]
Panel B: Active change in risky share, percentage points						
2001	2.76*** [1.76, 3.76]	2.19*** [1.59, 2.79]	2.10*** [1.34, 2.86]	3.27*** [2.28, 4.26]	4.08*** [3.17, 4.99]	-0.01 [-0.94, 0.91]
2002	–	1.33*** [0.64, 2.02]	0.08 [-0.48, 0.65]	1.58*** [0.73, 2.43]	2.69*** [2.00, 3.38]	-1.94*** [-2.74, -1.14]
2003	–	–	0.51* [-0.09, 1.10]	1.16** [0.17, 2.15]	2.14*** [1.44, 2.84]	-1.06*** [-1.69, -0.43]
2004	–	–	–	0.21 [-0.74, 1.17]	1.00*** [0.42, 1.58]	-1.78*** [-2.27, -1.29]
2005	–	–	–	–	0.66 [-0.41, 1.74]	-0.89** [-1.69, -0.09]
2006	–	–	–	–	–	-0.06 [-0.72, 0.60]
Panel C: Mutual-fund share of financial wealth ($\Delta F^{\%FW}$), percentage points						
2001	1.93*** [1.08, 2.78]	3.19*** [2.22, 4.16]	-0.82 [-2.00, 0.37]	0.29 [-0.25, 0.83]	1.22*** [0.37, 2.07]	2.45*** [1.54, 3.36]
2002	–	1.54*** [0.96, 2.12]	-0.72** [-1.28, -0.16]	0.86* [-0.11, 1.83]	-0.32 [-0.92, 0.27]	0.15 [-0.97, 1.28]
2003	–	–	–	2.30*** [1.58, 3.02]	0.42 [-0.11, 0.96]	3.83*** [3.08, 4.58]
2004	–	–	–	–	4.22*** [3.21, 5.23]	1.87*** [0.94, 2.80]
2005	–	–	–	–	–	3.81*** [2.60, 5.02]
2006	–	–	–	–	–	–
Panel D: Financial wealth growth relative to income ($\Delta FW/Y$), percentage points						
2001	-1.93* [-4.03, 0.17]	-1.15 [-5.85, 3.55]	5.27 [-1.53, 12.07]	-9.69** [-19.09, -0.29]	-10.20*** [-17.24, -3.16]	0.10 [-6.13, 6.32]
2002	–	-0.31 [-5.81, 5.20]	7.61* [-0.28, 15.50]	-7.46 [-17.21, 2.29]	-7.83** [-14.47, -1.19]	1.05 [-4.83, 6.93]
2003	–	–	6.88* [-0.49, 14.25]	-5.90 [-15.96, 4.16]	-5.51 [-12.76, 1.74]	1.76 [-5.00, 8.52]
2004	–	–	–	-9.05 [-20.07, 1.97]	-8.89** [-16.88, -0.90]	-1.51 [-5.86, 2.84]
2005	–	–	–	–	0.60 [-8.40, 9.60]	4.75 [-2.10, 11.60]
2006	–	–	–	–	–	-4.46* [-9.73, 0.81]

Brackets report 95% confidence intervals based on the wild bootstrap; stars use the corresponding wild-bootstrap p-values. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 10 reports cohort-by-year average treatment effects on the treated, ATT(g,t), from an event-time weighted two-way fixed effects (ETWFE) specification for the core outcomes. Rows index the first treatment (DC enrollment) cohort g, while columns index calendar years t; each cell reports the estimated ATT(g,t) for that cohort-year pair, with 95% confidence intervals in brackets. Panel A reports effects on stock market entry. Panel B reports effects on the active change in risky share. Panel C reports effects on the mutual-fund share of financial wealth. Panel D reports effects on financial-wealth growth relative to income. All specifications include age dummies, year fixed effects, region fixed effects, and occupation-by-year fixed effects, and condition on the same set of time-varying covariates as in the corresponding baseline outcome regressions.

Appendix A Details on Occupational Pension Collective Agreements

Understanding the structure and specifics of Sweden's occupational pension collective agreements is crucial for analyzing their impact on investment behavior and financial outcomes. This section synthesizes key information gathered from policy reports, academic research, and official agreement documentation (Calvet et al. (2023a), Hagen (2013), Hagen (2017), Jansson, Ottosson, Murhem, and Magnusson (2018), Sjögren Lindquist and Wadensjö (2006), Anderson (2015), Lindquist and Wadensjö (2009), Palme and Svensson (2004), Fora AB, Collectum AB, KPA Pension, Kåpan Pensioner). By providing an overview of these agreements for different worker groups—blue-collar, white-collar, central government, and local government employees—this section outlines the evolution, contribution structures, and investment choices available within the Swedish pension system.

Appendix A.1 Blue collar workers

Summary: Contributions begin accruing at age 28 up until 1999, at age 22 in 2000 and 2001, and at age 21 from 2002 onward.

Introduced in 1996, the SAF-LO contract pension is a defined contribution system. The employer contributes 3.5% of the employee's salary each year. (Avtalspension (2024)) All earnings are pensionable from the age of 21, with the pensionable age reduced from 28 to 22 in 2000 and to 21 in 2002. Employees may choose between two types of pension plans: traditional pension insurance, which offers a guaranteed minimum return, or mutual fund insurance.

Members also select their fund manager. Six traditional pension insurance companies and five companies offering unit-linked annuities are available for selection. For non-choosers, there is a default fund. Strict limits are imposed on the risk level any fund may take. The default fund is a fixed annuity provider, and about 65% to 70% of members are enrolled in the default fund.

Appendix A.2 White collar workers

Summary: Before 2007, contributions began accruing at age 28; after July 2007, contributions started at age 25.

Before 2007. The supplementary pension for white-collar workers in industry and trade, ITP, was introduced in 1960. The ITP plan included all white-collar workers in companies covered by

the agreement, except for managing directors in limited companies, and it had a defined benefit (DB) structure. ITP years were earned from the age of 28.

A DC component, called ITPK (where 'K' stands for supplementary), was introduced in 1977. Contributions to ITPK amounted to approximately 2 percent of each worker's earnings up to 30 base amounts (BAs). Prior to 1990, the ITPK plan operated as if each payment generated a benefit, with ITPK payouts being part of the collective refund, combining elements of both DB and DC schemes. In 1990, this changed, and it became possible to invest ITPK contributions in guaranteed or unit-linked products provided by companies such as Collectum or Valcentralen (Palme and Svensson (2004)).

In 1990, high earners (those earning more than 10 income-related base amounts (IBB) per month) could waive guaranteed ITP benefits above 7.5 IBB and set up their own DC plan, known as *Alternative ITP*. In this arrangement, the employer subscribes to another pension solution, often a fixed-premium "ten-tag solution". The details of this ten-tag solution, such as the amount the employer contributes, are not specified in the collective agreement and are left to the employer's discretion. Both the employer and employee must agree to opt for the employer's offer instead of the collective agreement's defined benefit ITP. Collectum reported that around half of eligible high-earners switched to Alternative ITP.

After 2007. The general transition of the ITP plan to a defined contribution system began in 2006, with a new plan taking effect on 1 July 2007. The new ITP plan consists of two sections: ITP1, a defined contribution retirement pension for civil servants born in 1979 or later. Employers contribute 4.5% of income up to the statutory ceiling and 30% of income above it.

Half of the contribution is allocated to fixed annuity products, with members selecting from four fund providers. For the other half, members choose between five providers offering fixed or unit-linked annuity products (Pensionsåldersutredningen (2012)). Non-choosers' contributions are placed in a default option, which is a lifetime fixed annuity with repayment protection.

The second section, ITP2, is a defined benefit retirement pension for civil servants born in 1978 or earlier. ITP2 continues the old ITP plan with some modifications. At retirement age 65, the compensation levels are as follows: 10% of salary up to 7.5 IBB, 65% of salary between 7.5 and 20 IBB, and 32.5% of salary between 20 and 30 IBB. Employers who signed ITP agreements before 25 April 2006 have their employees insured under both sections of the ITP plan. Employers contribute 2 percent of the pension base to individual accounts, and participants can choose how the funds

are invested. Participants may select between fixed and variable annuity products.

Appendix A.3 Central Government Workers

Summary: To be enrolled for a DC, an individual had to be at least 28 years of age until 2002, and at least 23 years old after 2003.

Prior to 1992, the occupational pension scheme for employees in the central government provided a gross pension, meaning that it fully replaced the state pension for workers covered by the scheme. After 1992, PA-91 pension scheme was introduced. The major part of the PA-91 pension was a DB scheme, where the occupational pension was based on the average of the last five annual salaries before the year of retirement. Additionally, there was a supplementary old-age pension financed by employers under PA-91, where contributions were made under the *Kåpan Tjänste* (1.7% of the employee's salary since 1995) and *Kåpan Extra* (introduced in 1998).

As of January 1, 2003, PA-91 was replaced by PA-03 (PA-91 payments continued after the introduction of PA-03 for individuals born before 1943). From age 23, central government employers began paying the *individuell ålderspension* premium. This pension is a DC scheme, where the employer finances this part of the pension by paying a fee for pension insurance. The premium corresponds to approximately 2.5% of the pension base, and employees can choose between fixed and unit-linked annuity products. There are nine providers of fixed annuities and thirteen providers of variable annuities, with each company offering between four and thirty-one funds. For non-choosers, the default fund is a lifelong fixed annuity.

Starting at age 28, employers also pay the *Kåpan* premium, which corresponds to 2% of the employee's salary or pension base up to an annual salary of 30 income base amounts. Employers transfer the *Kåpan* premiums to the Insurance Association for the state sector, *Kåpan Pensioner Försäkringsförening*, and the employee does not have the option to choose the management of this coverage.

Appendix A.4 Local Government Workers

Summary: enrollment eligibility age between 1998 and 2003 was at least 28 years old, and after 2004, the age was reduced to 21.

The new pension scheme for local government employees came into effect in 1998, around the

same time the new public pension scheme was implemented, serving as an adaptation to the new public pension system.

Initially, the pension scheme consisted solely of a contribution-based component. However, in 2000, an individual component was introduced, allowing employees to invest their pension in either a traditional pension insurance or a unit-linked insurance. By 2003, the contribution-based component was completely removed, and since then, the pension has only consisted of an individual component. Employees now have the freedom to choose among various fund managers and select the level of risk for their investments (Glans (2009)).

The contribution, paid by the employer, has varied slightly over time and depending on the employer, period, and tenure, but generally centered around 3.4–3.5% for wage portions below the income ceiling and 1–1.1% for earnings above the ceiling.

From 1998 to 2003, employees could credit an annual pension contribution to the contribution base starting at age 28. In 2004, the eligibility age was lowered from 28 to 21 years for the majority of employees in the contract area.

Three companies offer fixed annuities, and twelve companies offer unit-linked annuities. Participants can choose from between four and forty funds, depending on how many are offered by the selected company Ministries of Finance and Social Affairs (2013). For non-choosers, the default fund is a fixed annuity fund.

Appendix B Predictions in a Simple Two-Period Model

This section presents a single, intentionally parsimonious two-period model that nests the main mechanisms discussed in the text—monetary and informational participation costs (MPC/IPC), learning, preference-based non-participation, income-hedging motives, tax-driven asset location, wealth-dependent risk taking (DRRA/mental accounting), and liquidity-driven precautionary motives. Each channel arises as a special case under simple parameter restrictions.

Timing and resources. A household lives for $t \in \{1, 2\}$. In period 1 the household receives disposable income Y and is automatically enrolled in an occupational DC plan with an employer contribution $m \geq 0$ (funded outside of Y). The household chooses (i) total desired retirement saving T, (ii) whether to participate in the stock market in the *taxable* (non-retirement) account, $d \in \{0, 1\}$, and (iii) conditional on participation, the risky share in the taxable account $\alpha \in [0, 1]$. The household also chooses the risky share in the DC account $\alpha^R \in [0, 1]$; the plan features a default $\bar{\alpha}^R \in [0, 1]$ (e.g., a target-date fund).

Total saving T is the sum of employer DC wealth m and privately chosen taxable saving $T - m$. Hence the period-1 budget is

$$c_1 + (T - m) = Y \qquad \Longrightarrow \qquad c_1 = Y + m - T. \tag{A1}$$

This is the "total saving" structure: for a given desired T, higher m mechanically reduces privately funded (taxable) saving $T - m$, generating a crowd-out channel that operates even absent any behavioral response.

Returns, beliefs, and income risk. Let R_f denote the gross risk-free return and R_e the gross equity return. I allow beliefs about equity returns to depend on DC exposure, capturing learning about risk and return:

$$R_e = R_f + \mu(m) + \varepsilon, \qquad \mathbb{E}[\varepsilon] = 0, \qquad \mathbb{V}[\varepsilon] = \sigma^2(m), \tag{A2}$$

where $\mu'(m) \geq 0$ and/or $\sigma'(m) \leq 0$ capture learning that improves perceived equity opportunities (e.g., higher perceived equity premium or lower perceived risk). In a richer dynamic setting,

these beliefs would evolve through Bayesian updating as tenure in the plan increases; the static formulation here captures the reduced-form implication that longer exposure improves beliefs.[37]

To nest the income-hedging channel, allow period-2 nonfinancial income y_2 to be risky and correlated with stock returns:

$$y_2 = \bar{y}_2 + \eta, \qquad \mathrm{Cov}(\eta, \varepsilon) \neq 0. \tag{A3}$$

Participation costs (MPC/IPC) and learning. Accessing taxable equity requires paying a fixed cost. I model this cost as reducing investable taxable wealth rather than period-1 consumption, capturing the notion that participation costs represent foregone investment returns rather than direct utility losses.[38] Let

$$K(m) \equiv \kappa_M + \kappa_I(m), \qquad \kappa_I(m) \equiv \max\{0, \bar{\kappa}_I - \theta m\}, \qquad \theta \geq 0, \tag{A4}$$

where κ_M captures monetary participation costs (brokerage fees, time costs) that are invariant to DC exposure, and $\kappa_I(m)$ captures informational participation costs (IPC) that fall with DC exposure when $\theta > 0$. The IPC component reflects reduced perceived complexity, improved financial literacy, and increased trust in markets that may result from experience with the DC plan.

If $d = 1$, the household must fund $K(m)$ from taxable saving. Thus investable taxable wealth is

$$W^T \equiv T - m - d\,K(m), \qquad \text{with feasibility } W^T \geq 0 \text{ (in particular, } T \geq m + K(m) \text{ if } d = 1). \tag{A5}$$

If $d = 0$, set $\alpha = 0$ (no taxable equity).

DC default and adjustability. To capture default-driven equity exposure in the DC plan, impose a quadratic adjustment cost around the default:

$$\text{Adjustment cost in period 1:} \qquad \frac{\varphi_R}{2}\,(\alpha^R - \bar{\alpha}^R)^2, \qquad \varphi_R \geq 0. \tag{A6}$$

[37] This reduced-form approach is consistent with the empirical finding that treatment effects grow over event time, suggesting gradual learning rather than an immediate discrete shift in beliefs.

[38] If costs were instead paid from consumption, the participation threshold would depend on the marginal utility of consumption rather than on investment returns. The current specification yields cleaner comparative statics and is consistent with interpreting participation costs as brokerage fees or time costs that reduce net investable wealth.

This cost can be interpreted as an attention or cognitive cost of deviating from the default allocation, rather than a monetary cost.[39] The limit $\varphi_R \to \infty$ pins $\alpha^R = \bar{\alpha}^R$ (pure default behavior), while $\varphi_R = 0$ corresponds to full active choice with no friction.

Liquidity and precautionary motives. DC wealth is relatively illiquid: early withdrawal typically incurs penalties and tax consequences. To capture this, let $\lambda \in [0, 1]$ denote the effective liquidity of DC wealth, where $\lambda = 1$ means DC wealth is as liquid as taxable wealth and $\lambda = 0$ means DC wealth is completely illiquid until retirement. The household's *liquid* wealth available for precautionary purposes is

$$W^{\text{liq}} \equiv W^T + \lambda\, m. \tag{A7}$$

When $\lambda < 1$, DC enrollment reduces liquid wealth relative to a counterfactual where the same resources were saved in taxable accounts, potentially increasing precautionary demand for safe, liquid assets outside the plan.

Period-2 consumption. Given (T, d, α, α^R),

$$c_2 = y_2 + W^T\Big((1-\alpha)R_f + \alpha R_e\Big) + m\Big((1-\alpha^R)R_f^R + \alpha^R R_e^R\Big), \tag{A8}$$

where (R_f^R, R_e^R) denote the effective DC returns, allowing for tax deferral and fee differences. The baseline case sets $(R_f^R, R_e^R) = (R_f, R_e)$. Tax advantages of DC accounts correspond to $R_f^R > R_f$ and/or $R_e^R > R_e$; importantly, the *relative* advantage may differ across asset classes, which is central to the tax-optimization channel discussed below.

Preferences (including a preference-based participation wedge). The household maximizes

$$\max_{T,d,\alpha,\alpha^R} \; u(c_1) + \beta\, \mathbb{E}[u(c_2)] \;-\; \psi\, d \;-\; \frac{\varphi_R}{2}(\alpha^R - \bar{\alpha}^R)^2, \tag{A9}$$

where $\psi \geq 0$ is a reduced-form preference wedge for holding equities in taxable accounts. This parameter captures first-order risk aversion, ambiguity aversion, or other non-standard preferences that create a utility cost of equity participation beyond what standard expected utility would imply.

[39] Empirically, most DC participants remain at or near plan defaults, consistent with substantial adjustment costs in this sense.

Importantly, in the "preference channel" interpretation, ψ is a stable preference parameter that does not change with DC enrollment.

Portfolio choice and DRRA. To connect risky-share predictions transparently, I use the standard mean–variance approximation for optimal risky exposure. Let W^{ref} denote the wealth measure that the household uses as the reference base for risk-taking:

$$W^{\text{ref}} \equiv W^T + \omega\, m, \qquad \omega \in [0,1], \tag{A10}$$

where $\omega = 1$ corresponds to integrated wealth (taxable and DC wealth treated symmetrically for risk-taking decisions) and $\omega = 0$ corresponds to mental accounting in which only taxable wealth is salient for risk-taking. Intermediate values $\omega \in (0,1)$ capture partial integration, where DC wealth is somewhat salient but discounted relative to liquid taxable wealth.

Let $\text{RRA}(W^{\text{ref}})$ denote the coefficient of relative risk aversion evaluated at the relevant wealth level. Under the mean–variance approximation, the (interior) optimal *total* risky share satisfies

$$\alpha^*(W^{\text{ref}}) \;\approx\; \frac{\mu(m)}{\sigma^2(m)} \cdot \frac{1}{\text{RRA}(W^{\text{ref}})}. \tag{A11}$$

CRRA benchmark. If $u(c) = c^{1-\gamma}/(1-\gamma)$, then $\text{RRA}(W^{\text{ref}}) = \gamma$ is constant and

$$\alpha^* \approx \frac{\mu(m)}{\gamma\, \sigma^2(m)}. \tag{A12}$$

Under CRRA, the risky share depends on beliefs (which may be affected by DC enrollment through learning) but not on the level of wealth.

DRRA (HARA) example. If preferences exhibit decreasing relative risk aversion via HARA utility

$$u(c) = \frac{(c-\underline{c})^{1-\gamma}}{1-\gamma}, \qquad c > \underline{c}, \tag{A13}$$

then $\text{RRA}(W^{\text{ref}}) = \gamma \frac{W^{\text{ref}}}{W^{\text{ref}}-\underline{c}}$ is *decreasing* in W^{ref}, and (A11) yields the familiar DRRA risky-share formula

$$\alpha^*(W^{\text{ref}}) \;\approx\; \frac{\mu(m)}{\gamma\, \sigma^2(m)} \left(1 - \frac{\underline{c}}{W^{\text{ref}}}\right), \qquad \frac{\partial \alpha^*}{\partial W^{\text{ref}}} > 0. \tag{A14}$$

Thus under DRRA, increases in the *relevant* wealth base W^{ref} raise optimal risk-taking.

Taxable risky share and rebalancing. Define total desired risky dollars $X^* \equiv \alpha^*(W^{\text{ref}})\, W^{\text{ref}}$. With DC risky dollars $X^R \equiv \alpha^R m$ (pinned to $\bar{\alpha}^R m$ if $\varphi_R \to \infty$), the implied taxable risky share is

$$\alpha^T = \min\left\{1,\ \max\left\{0,\ \frac{X^* - X^R}{W^T}\right\}\right\}, \tag{A15}$$

which highlights why taxable risky shares can fall when DC equity exposure rises (rebalancing), especially for households already near the corner $\alpha^T \approx 1$ pre-treatment.

An important implication of (A15) is that for households with high pre-treatment taxable risky shares (near the upper bound), the effect of DC enrollment on α^T is mechanically constrained. Specifically, if $\alpha^T \approx 1$ before enrollment and the DC default is equity-heavy, then any increase in X^R *must* reduce α^T under integrated portfolio management, even if desired total risk X^* remains unchanged. This generates heterogeneous treatment effects that depend on the pre-treatment portfolio position.

Mapping to mechanisms via parameter restrictions. The nested model above delivers the channels in the main text as special cases:

1. **Frictionless benchmark (100% participation).** Set $K(m) \equiv 0$, $\psi = 0$, $\varphi_R = 0$, $\lambda = 1$, and ignore (A3). With $\mu(m) > 0$, the household chooses $\alpha > 0$ and hence participates ($d = 1$) for all households with $T > m$. This benchmark clarifies that limited participation requires some friction or preference wedge.

2. **MPC channel (no direct effect; potentially negative via crowd-out).** Set $\theta = 0$ and $\bar{\kappa}_I = 0$ so that $K(m) = \kappa_M$ is constant. DC enrollment does not change MPC, but higher m reduces taxable saving $T - m$ and hence investable taxable wealth $W^T = T - m - d\kappa_M$, shrinking the set of households for which paying a fixed cost is optimal or feasible. This channel predicts a zero or negative effect on non-retirement participation.

3. **IPC / learning-about-costs channel (positive participation effect).** Set $\theta > 0$ (with $\bar{\kappa}_I > 0$). Then $K(m)$ decreases in m by (A4), relaxing the feasibility constraint (A5) and raising the value of participation ($d = 1$). This channel predicts a positive effect on non-retirement stock market entry.

4. **Learning-about-returns channel (higher risky share and/or entry).** Allow $\mu'(m) >$

0 and/or $\sigma'(m) < 0$ in (A2). Then (A11) implies higher optimal risky exposure as DC experience improves perceived risk-return tradeoffs. Combined with fixed costs, this increases the incentive to enter ($d = 1$) and can raise α^T among marginal participants. This channel predicts positive effects on both participation and risky share.

5. **Preference (first-order risk aversion / ambiguity) channel (zero effect).** Set $K(m) \equiv 0$ and $\psi > 0$ in (A9). If ψ is a stable preference parameter unaffected by DC enrollment, then the participation wedge does not shift with m, implying approximately no treatment effect through this channel. However, if DC exposure reduces ambiguity about equity returns (shifting ψ downward), this channel could generate positive effects similar to the learning channels.

6. **Income–stock return correlation channel (zero effect).** Activate (A3). The hedging component of optimal risky exposure depends on $\mathrm{Cov}(\eta, \varepsilon)$; if DC enrollment does not change this covariance—which depends on the correlation between labor income and stock returns—then this channel predicts no systematic treatment effect on participation or risky shares.

7. **Tax-optimization / asset location channel (Dammon et al., 2004).** Introduce asset-specific tax wedges in taxable accounts such that bond income is taxed more heavily than equity returns (e.g., due to favorable capital gains treatment or dividend tax rates). This implies that the effective taxable safe return is relatively more penalized than the taxable equity return. Meanwhile, DC returns are tax-deferred, so $R_f^R/R_f > R_e^R/R_e$ when the tax penalty on bonds exceeds that on equities.

   Example: Suppose taxable bonds face a 30% marginal tax rate while taxable equities face an effective 15% rate (due to capital gains deferral and qualified dividend treatment). If $R_f = 1.05$ and $R_e = 1.10$ pre-tax, then after-tax returns are approximately 1.035 for bonds and 1.085 for equities in taxable accounts, while DC accounts preserve the pre-tax returns. The optimal strategy places tax-inefficient bonds in the DC account and tax-efficient equities in the taxable account, raising the taxable risky share when m increases.

8. **DRRA vs. mental accounting (sign depends on the relevant wealth base).** Under DRRA (A14), risky exposure rises with W^{ref}. The effect of DC enrollment depends on the degree of wealth integration:

- If $\omega = 1$ (full integration), then higher DC wealth increases $W^{\text{ref}} = W^T + m$, predicting a positive risky-share response despite the crowd-out of taxable wealth.
- If $\omega = 0$ (mental accounting), then $W^{\text{ref}} = W^T$ only, and DC enrollment crowds out taxable wealth without expanding the relevant wealth base. This attenuates or reverses the risky-share response, potentially yielding negative effects.
- Intermediate values $\omega \in (0, 1)$ generate intermediate predictions, with the sign depending on the relative magnitudes of the crowd-out and wealth effects.

9. **Liquidity / precautionary channel (negative participation and risky-share effects).** Set $\lambda < 1$ so that DC wealth is illiquid. DC enrollment then reduces liquid wealth $W^{\text{liq}} = W^T + \lambda m$ relative to a counterfactual where resources were saved in taxable accounts (which would yield $W^{\text{liq}} = T$). With reduced liquidity buffers, households may increase precautionary demand for safe, liquid assets in taxable accounts, dampening stock market entry and tilting taxable portfolios toward safer assets. This channel predicts negative effects on both participation and risky share, working against the learning and IPC channels.

10. **Negative taxable risky-share effects for high pre-treatment risky share (rebalancing/corners).** If the DC default is equity-heavy ($\varphi_R \to \infty$ and $\bar{\alpha}^R$ high), then DC enrollment mechanically raises $X^R = \bar{\alpha}^R m$. Holding desired total risky dollars X^* fixed (or slowly adjusting under gradual learning), (A15) implies α^T can fall with m. This effect is strongest for households initially near the upper bound $\alpha^T \approx 1$, who have no room to increase taxable risk-taking and must reduce α^T to accommodate the new DC equity exposure. This generates heterogeneous treatment effects: positive for households with low initial risky shares (who have room to adjust) and negative for households already at the corner.

Summary of predictions. Table A1 summarizes the directional predictions of each channel for the two main outcomes: stock market participation (entry) and risky share among existing stockholders.

The net effect of DC enrollment is theoretically ambiguous and depends on which channels dominate empirically. Positive effects on participation point toward learning and IPC channels; positive effects on risky share point toward learning, tax optimization, or DRRA with wealth integration. Negative or null effects would be consistent with mental accounting, liquidity/precautionary mo-

Table A1: Summary of Theoretical Predictions by Channel

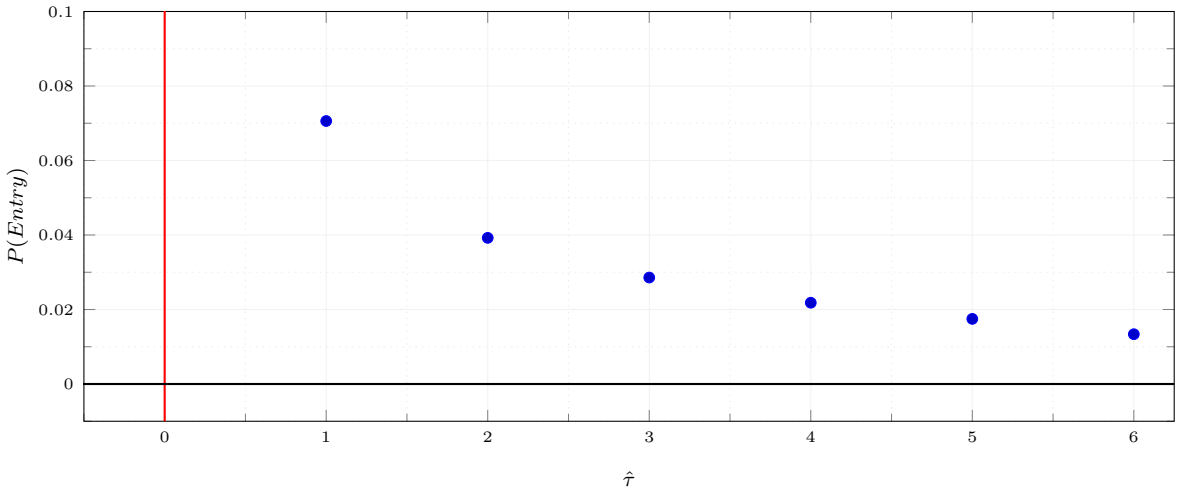
Channel	Participation	Risky Share
MPC (monetary participation costs)	0 or $-$	0
IPC (informational costs) / Learning-about-costs	$+$	0 or $+$
Learning-about-returns	$+$	$+$
Preference (stable)	0	0
Income hedging	0	0
Tax optimization / Asset location	0[40]	$+$
DRRA with integration ($\omega = 1$)	0[41]	$+$
Mental accounting ($\omega = 0$)	0 or $-$	0 or $-$
Liquidity / Precautionary	$-$	$-$
Rebalancing (high initial α^T)	0	$-$

This table summarizes the predicted sign of DC enrollment effects on non-retirement stock market participation and risky share under each theoretical channel. "+" indicates a positive effect, "−" a negative effect, and "0" no effect. Multiple signs (e.g., "0 or +") indicate that the prediction depends on parameter values or is theoretically ambiguous within the channel.

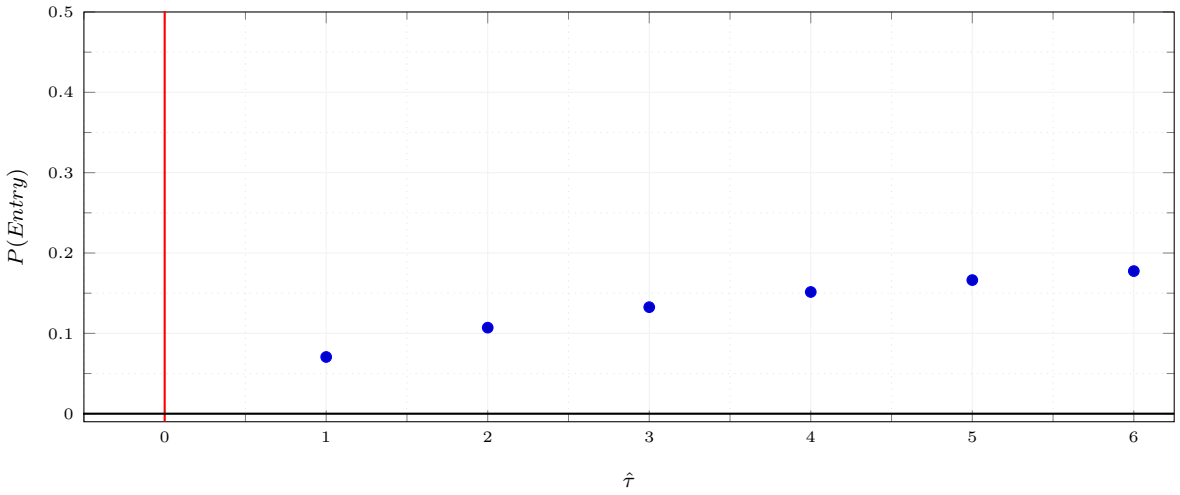
tives, or rebalancing. The empirical analysis in the main text uses these predictions to interpret the estimated treatment effects and to design mechanism tests that can discriminate among competing explanations.

Appendix C Additional Descriptive Statistics

Figure A1: Unconditional Mean of Entry Probability: (a) Annual Change; (b) Cumulative effect



(a) Annual Change in Entry Probability



(b) Cumulative Change in Entry Probability

Figure A1 depicts the unconditional mean of stock market entry probability in the sample. The probabilities are calculated by identifying a sample of non-participants in a specific year and tracking their stock market participation over time (up to six years). An entry is recorded if an individual participated in the stock market at least once by year τ. Given the time-series dimension of the sample, I observe the evolution of entry probability over the full six-year period only for the 2000, and 2001 year groups. For subsequent year groups, the observation period is shorter. To compute the unconditional mean entry probability, I first calculate the series of probabilities for each year group and then average these probabilities across all year groups for each τ.

Appendix D Additional Empirical Results

In this section, I provide supplementary analyses to assess the robustness of my main findings.

Appendix D.1 Effects on the Level of Participation

This appendix complements the main analysis of stock market entry by estimating the effect of occupational DC enrolment on the level of participation, $Partic_{it}$, measured at the end of year t. Whereas the entry and exit regressions isolate one-year transitions conditional on participation status in $t-1$, the level specification summarizes how these flows cumulate into the stock of participants in the full population.

I estimate the same event-study design as in the baseline analysis, but use $Partic_{it}$ as the outcome and include all individuals:

$$Partic_{it} = \alpha + \sum_{j \in \mathcal{J}} \beta_j \, I[_{\hat{\tau}^{DC}=j}] + \theta X_{it} + I[a_{it}] + I[\text{occupation} \times y_t] + I[y_t] + \varepsilon_{it}, \tag{A16}$$

where $\hat{\tau}^{DC}$ measures event time relative to the year before DC eligibility/enrollment, $\hat{\tau}^{DC} = 0$ is omitted as the reference period, and $\mathcal{J}$ contains the same lead/lag indicators as in the main event study. All specifications include age dummies, year fixed effects, and occupation-by-year fixed effects. Standard errors are clustered at the treatment-time-by-occupation level; confidence intervals and significance levels are based on the wild cluster bootstrap.

Table A2 reports the event-time coefficients. The pre-treatment coefficients are small and statistically indistinguishable from zero, providing additional evidence against differential pre-trends in participation levels. Post-treatment coefficients are positive and statistically significant at intermediate horizons. Figure A2 illustrates that the level response is hump-shaped: the estimated participation gain rises after enrollment, peaks at $\hat{\tau}^{DC} = 3$ (5.44 pp), and then attenuates to 3.77 pp by $\hat{\tau}^{DC} = 6$, with wider confidence intervals at longer horizons.

Notably, this hump shape does not imply higher exits among treated individuals. The level outcome is a net stock measure: it reflects both entry and exit in the population, and therefore need not track the cumulative sum of entry-flow coefficients. In particular, the results of an exit event study in Table 5 show no evidence that DC eligibility increases the probability of exit conditional on participating. Instead, the attenuation of the level effect is consistent with gradual catch-up in

the control group and with treated individuals entering earlier and therefore constituting a larger participation stock at short and medium horizons.

Table A2: Stock Market Participation (Level): Event-Study Estimates

	Participation level ($Partic_{it}$)
$\hat{\tau}^{DC} = -2$	-0.00117 [-0.0113, 0.0147]
$\hat{\tau}^{DC} = -1$	0.0000988 [-0.0253, 0.0340]
$\hat{\tau}^{DC} = 1$	0.0372*** [0.0210, 0.0569]
$\hat{\tau}^{DC} = 2$	0.0511*** [0.0216, 0.0946]
$\hat{\tau}^{DC} = 3$	0.0544*** [0.0272, 0.0853]
$\hat{\tau}^{DC} = 4$	0.0490*** [0.00786, 0.0728]
$\hat{\tau}^{DC} = 5$	0.0431** [0.00140, 0.0701]
$\hat{\tau}^{DC} = 6$	0.0377 [-0.0749, 0.119]
Year and Age FE	Yes
Region FE	Yes
Occupation×Year FE	Yes
N	4,403,234
Adj. R^2	0.103

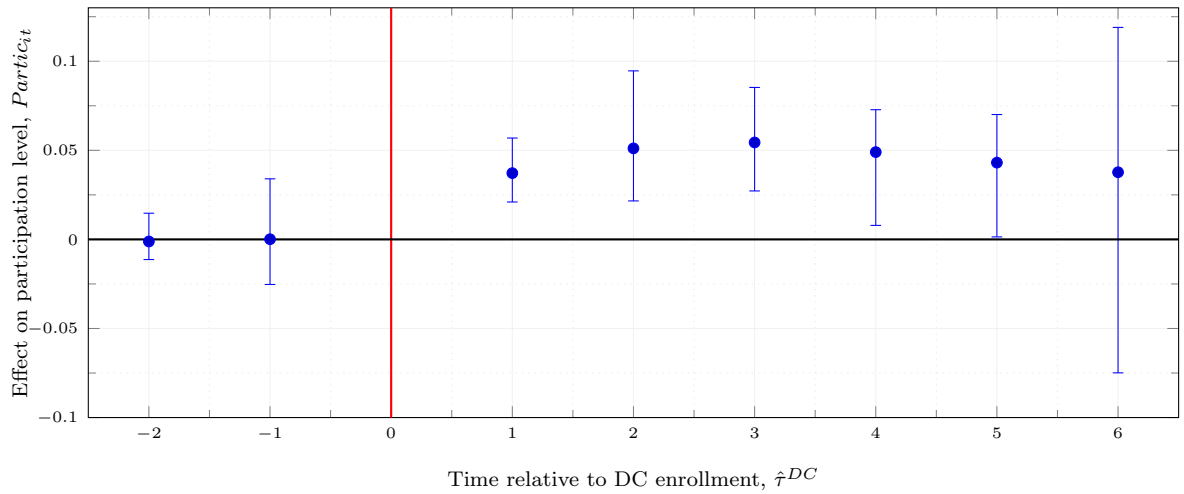
Standard errors are clustered at the treatment-time-by-occupation level. Brackets report 95% confidence intervals based on the wild cluster bootstrap; significance levels are based on the corresponding wild-bootstrap p-values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2 reports event-study estimates for the level of stock market participation, $Partic_{it}$, using the full sample (participants and nonparticipants). The regressors are event-time indicators relative to the year before occupational DC enrollment, $I[\hat{\tau}^{DC} = j]$, with $\hat{\tau}^{DC} = 0$ omitted as the reference period. The specification includes age dummies, year fixed effects, and occupation-by-year fixed effects; inference is based on wild cluster bootstrap confidence intervals and p-values with clustering at the treatment-time-by-occupation level.

Appendix D.2 Robustness to Pre-trend Violations

To assess sensitivity to potential violations of parallel trends, I follow Rambachan and Roth (2023). The method relaxes the sharp restriction that bias from a difference in trends is exactly zero ($\delta_{post} = 0$) and instead imposes economically motivated bounds on how post-treatment trend violations can evolve relative to the estimated pre-trends. This yields uniformly valid confidence sets that incorporate both sampling uncertainty in the pre-period coefficients and additional identification

Figure A2: Stock Market Participation (Level): Event-Study Coefficients



Notes: The figure plots event-study coefficients from Eq. (A16) with $\hat{\tau}^{DC} = 0$ omitted as the reference period. Error bars show 95% wild cluster bootstrap confidence intervals with standard errors clustered at the treatment-time-by-occupation level.

uncertainty from extrapolating pre-trends into the post period.

I implement HonestDiD using the relative-magnitude restriction Δ^{RM} with two pre-period coefficients ($\hat{\tau}^{DC} \in \{-2, -1\}$) and six post-period coefficients ($\hat{\tau}^{DC} \in \{1, \ldots, 6\}$). For stock market entry, the impact effect at $\hat{\tau}^{DC} = 1$ has a conventional 95% confidence interval of [0.004, 0.025] and remains strictly positive for a wide range of deviations from parallel trends, with the robust lower bound reaching zero only at approximately $M \simeq 1.3$. For the average post-treatment effect (equal weights over $\hat{\tau}^{DC} = 1, \ldots, 6$), the conventional 95% confidence interval is [0.004, 0.012]; the robust interval remains nonnegative under moderate deviations (roughly up to $M \simeq 0.3$) but allows negative values for larger M.

I apply the same procedure to the active change in risky share. The first-year effect remains robust to relatively large deviations, with the robust interval excluding zero up to about $M \simeq 1.3$. Robustness declines at longer horizons: the robust interval remains positive up to approximately $M \simeq 0.4$ at $\hat{\tau}^{DC} = 2$ and up to about $M \simeq 0.2$ at $\hat{\tau}^{DC} = 3$. For $\hat{\tau}^{DC} \geq 4$, robust intervals include zero for small deviations (around $M \simeq 0.1$), so inference at longer horizons is sensitive and best interpreted as suggestive about dynamics rather than conclusive about long-run magnitudes.

Overall, HonestDiD indicates that the short-run increases in entry and active risky-share adjustment are robust to sizable departures from parallel trends, whereas inference on longer-horizon averages is (as expected) more sensitive.

Appendix D.3 Callaway–Sant'Anna

As another robustness check to staggered adoption, I re-estimate the dynamic effects using the Callaway and Sant'Anna (2021) framework (CS). I implement the panel doubly robust inverse-probability-weighted estimator (DRIPW), using not-yet-treated individuals as the comparison group. Standard errors are clustered at the treatment-time-by-occupation level, matching the baseline inference. The covariate set mirrors the baseline specifications in Tables 2 and 6, but is measured pre-treatment (at the last observed period before first eligibility) as required by the estimator.

Table A3 reports the resulting event-study estimates for stock market entry and the active change in risky share in Columns (1) and (2), respectively.

Two patterns stand out. First, the pre-treatment dynamics provide no evidence of differential trends in stock market entry: $\widehat{\text{Pre}}_{\text{avg}}$ for entry is statistically indistinguishable from zero. For the active change in risky share, $\widehat{\text{Pre}}_{\text{avg}}$ is negative and statistically significant at the 5% level (-0.0080, s.e. 0.0040), indicating that treated individuals were, if anything, on a slightly declining risky-share trajectory relative to controls before enrollment. Critically, because the pre-trend and the post-treatment effect run in *opposite* directions, this pre-trend biases the estimated $\widehat{\text{Post}}_{\text{avg}}$ downward, making the intensive-margin finding conservative rather than spurious. This interpretation is consistent with the sensitivity analysis in Appendix D.2, which shows that the first-year risky-share effect is robust to pre-trend violations substantially larger than what is observed here. Second, $\widehat{\text{Post}}_{\text{avg}}$ is positive and statistically significant at the 1% level for the active change in risky share (0.0153, s.e. 0.0029), corroborating the baseline TWFE and ETWFE findings on the intensive margin.

The dynamic profile for active rebalancing is also consistent with gradual adjustment: the estimate in the first post-enrollment year ($\hat{\tau}^{DC} = 1$) is smaller, but effects in later years are of similar magnitude. Overall, the Callaway–Sant'Anna estimates corroborate the main conclusion that DC enrollment affects portfolio adjustment on the intensive margin, while also providing a direct test of parallel pre-trends in this staggered setting.

Table A3: Dynamic effects for entry and active risky-share changes (Callaway–Sant'Anna-style group-time ATT; event-time coefficients).

	$P(Entry_{it})$	Δw_{it}^{active}
$\hat{\tau}^{DC} = -6$	-0.0037	-0.0588
	(0.0032)	(0.0386)
$\hat{\tau}^{DC} = -5$	0.0280*	0.0039
	(0.0148)	(0.0169)
$\hat{\tau}^{DC} = -4$	-0.0135	0.0065
	(0.0091)	(0.0085)
$\hat{\tau}^{DC} = -3$	0.0030*	0.0015
	(0.0018)	(0.0074)
$\hat{\tau}^{DC} = -2$	0.0037	0.0097*
	(0.0037)	(0.0053)
$\hat{\tau}^{DC} = -1$	0.0004	-0.0104**
	(0.0015)	(0.0048)
$\hat{\tau}^{DC} = 0$	0.0015*	0.0152***
	(0.0009)	(0.0048)
$\hat{\tau}^{DC} = 1$	-0.0006	0.0073**
	(0.0017)	(0.0034)
$\hat{\tau}^{DC} = 2$	-0.0008	0.0106**
	(0.0029)	(0.0050)
$\hat{\tau}^{DC} = 3$	0.0022	0.0098**
	(0.0039)	(0.0046)
$\hat{\tau}^{DC} = 4$	0.0058	0.0176*
	(0.0051)	(0.0090)
$\hat{\tau}^{DC} = 5$	0.0095*	0.0058
	(0.0054)	(0.0077)
$\hat{\tau}^{DC} = 6$	0.0094	0.0412***
	(0.0167)	(0.0141)
	Group-time weighted averages	
Pre_{avg}	0.0030	-0.0080**
	(0.0027)	(0.0040)
$Post_{avg}$	0.0039	0.0153***
	(0.0040)	(0.0029)

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3 presents the estimation results for the dynamic effects from regression for stock market entry probability and the active change in risky share using Callaway and Sant'Anna (2021). The model estimates all the dynamic effects (from 6 years before getting a DC to 6 years after) using Doubly Robust IPW (DRIPW) estimation method, and individuals who did not get enrolled to the occupational DC yet as a control group. The estimator further assumes unbalanced panel data, and SEs clustered at treatment-time-by-occupation level. The specifications use the conditional parallel trends assumption, and include a set of controls similar to models reported in the baseline result Tables 2 and 6.

Appendix E Calculation Details for Relative Sharpe Ratio Loss

Estimating expected returns and covariances directly from individual-level realized returns is noisy in short panels. Instead, I follow Calvet et al. (2007) and infer asset moments from a factor pricing model. Specifically, I use the Fama–French three-factor model and estimate factor moments as the sample mean $\bar{f}$ and covariance matrix Σ_F of the factor vector f_t (where market excess return is measured as the US dollar return of the world index in excess of the US T-bill, while SMB and HML are the US size and value factors obtained from Ken French's website.).

For each asset j from the universe of assets owned by investors in my panel, I estimate factor loadings $b_j = (\beta_j, \gamma_j, \delta_j)'$ by regressing the asset's excess return on the factors (without an intercept), and I estimate the asset's idiosyncratic variance as the time-series mean of squared residuals. This procedure yields the implied mean excess return $\mu_j = b_j' \bar{f}$ and the implied return covariance matrix

$$\Sigma = B \Sigma_F B' + D, \tag{A17}$$

where B stacks the b_j and D is diagonal with entries $\sigma^2_{u,j}$.

Let w_{ijt} denote the value weight of asset j in individual's i's risky portfolio at time t. The household's portfolio factor exposure is

$$b_{i,t} = \sum_{j \in \mathcal{J}_{i,t}} w_{ijt}\, b_j, \tag{A18}$$

so the implied mean and variance of the individual's risky excess return are

$$\mu_{i,t} = b_{i,t}' \bar{f}, \tag{A19}$$

$$\sigma^2_{i,t} = b_{h,t}' \Sigma_F b_{i,t} + \sum_{j \in \mathcal{J}_{i,t}} w^2_{ijt}\, \sigma^2_{u,j}, \tag{A20}$$

where the second term aggregates idiosyncratic risk under the maintained diagonal structure of D. I then compute $S_{i,t} = \mu_{i,t}/\sigma_{i,t}$ and $RSRL_{i,t} = 1 - S_{i,t}/S_B$.

I set S_B to the Sharpe ratio of the benchmark portfolio implied by the same factor model and factor moments.

Appendix F Evidence from the US

In this Appendix I repeat my analysis in the U.S. context. The U.S. setting does not provide a clean identification since I cannot exploit exogenous plan assignment as in the Swedish setting. Furthermore, the sample size is much smaller since I have to rely on survey data as opposed to the full population tax registry. On the other hand, the data covers a more recent sample period.

Appendix F.1 Institutional setting

Retirement plans in the United States are structured through a combination of government and employer-sponsored programs. These plans can broadly be categorized into two main types: DB plans and DC plans, but hybrid plans also exist.

DB plans used to be the main vehicle for retirement savings in the U.S., both in the public and private sector.[42] Over time, DB plans have become less common in the private sector due to their high costs and complexity.[43] Nonetheless, they remain prevalent in the public sector, where they continue to provide secure retirement income for many government employees.[44] However, in the past two decades, there has been a noticeable shift towards DC plans as the predominant retirement savings vehicle in the U.S. among private-sector employers, as shown in Figure A3. In DC plans employees contribute a share of their pre-tax income to an individual account with employers often making additional contributions through matching programs, up to a certain limit.[45] Unlike DB plans, the investment risk in DC plans is borne by the employee. The retirement benefit is not predetermined but instead depends on the amount contributed and the performance of the investments chosen by the employee.

[42] Under these plans, employers promise to pay employees a specific monthly benefit upon retirement, determined by a formula that typically takes into account factors such as salary history, years of service, and the employee's age at retirement. The employer is responsible for funding these plans and bears all the investment risk. These benefits are usually paid out as a lifetime annuity, providing retirees with a steady income stream.

[43] In March 2023, only 15 percent of private industry workers had access to a defined benefit plan (Bureau of Labor Statistics (2024)).

[44] In March 2016, 75 percent of state and local government workers participated in defined benefit pension plans. Of these workers, 57 percent participated in frozen defined benefit plans, or plans that are not open to new employees (Bureau of Labor Statistics (2017)).

[45] There are several types of DC plans. The most common is the 401(k) plan, which is offered by private-sector employers. Similarly, 403(b) plans are available to employees of public schools, non-profit organizations, and certain ministers, operating in much the same way as 401(k) plans. 457 plans cater to state and local government employees, as well as some non-profit workers.

Figure A3: Share of DB and DC over time, conditional on participating in a pension plan

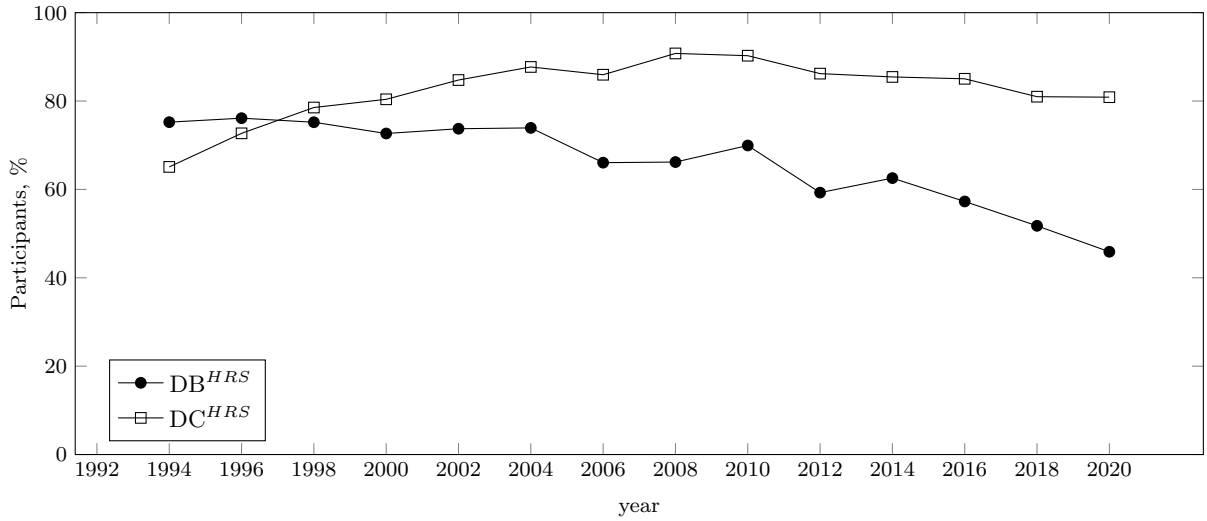


Figure A3 illustrates the evolution of the proportion of DB and DC pension plans over time among participants in pension plans, based on data from the Health and Retirement Study (HRS). The solid black lines represent the shares of DB and DC plans in the HRS sample, with circles and squares indicating DB and DC plans, respectively. The x-axis displays the years from 1988 to 2020 (with data available for every even year corresponding to the HRS survey wave), and the y-axis shows the percentage of participants in the plan. The figure shows a clear trend over time: the share of participants with DB plans has generally decreased, while the share with DC plans has increased.

Appendix F.2 Data

To analyze the link between pension plan design and stock market participation decisions outside retirement plan in the US, I use 15 waves of Health and Retirement Study (HRS) data covering the period from 1992 through 2020. The sample in HRS is nationally representative of the American population 50 years old and above, and surveys approximately 12,000 to 20,000 people biannually. The survey is a panel study, and on average it tracks a person for 9 consecutive waves.

Appendix F.2.1 Sample Selection

I start with 42,406 individuals (29,478 households) from the RAND HRS 2020 file. Firstly, I filter out everyone above age 65, to make sure that the sample includes only those who did not reach the full retirement age to avoid issues relating to the decumulation of assets due retirement following the previous literature (Poterba, Venti, and Wise (1996), Engen et al. (1994), Engen, Gale, and Scholz (1996) and Benjamin (2003)). I also set a minimum age of 50 to make sure there are enough observations at each age. This leaves 29,805 individuals (21,143 households).

Around 55% of the working sample participates in an employer-sponsored pension plan as shown in Figure A4. Next, I keep only those who had a pension account provided by the employer at least once over the surveyed period - 14,467 respondents (from 12,252 households) were eligible for such an account. As the line "filtered" in Figure A4 shows, in any particular wave around 90% of working people in the truncated sample have a work-related pension plan.

Figure A4: Participation in employer-provided pension plans, conditional on working

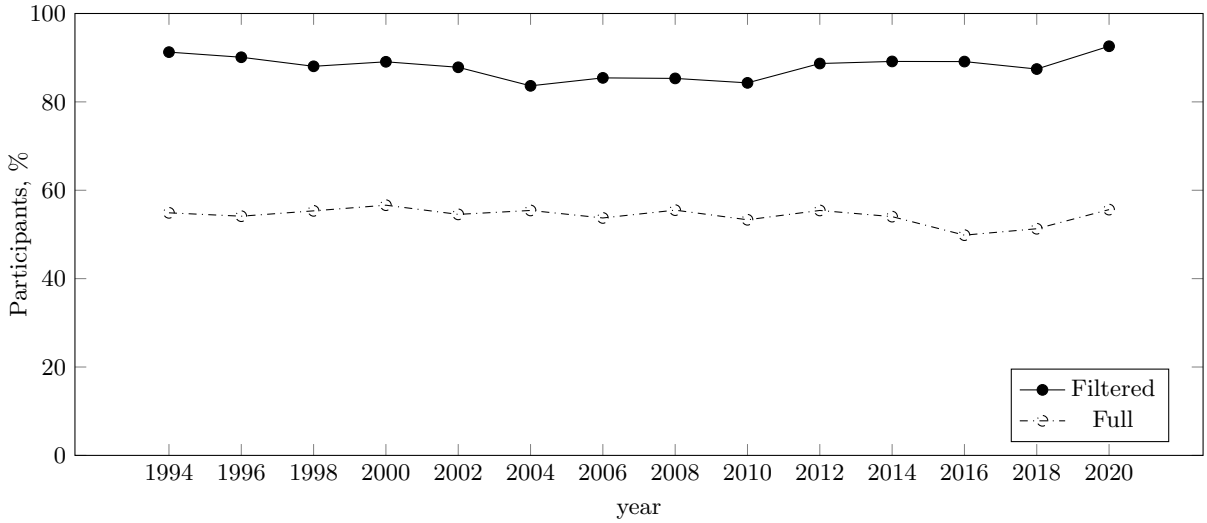


Figure A4 illustrates the percentage of workers participating in employer-provided pension plans over the period from 1994 to 2020, conditional on being employed. Among all employed individuals in the HRS dataset, participation rates start at 54.89% in 1994 and fluctuating slightly over the years, reaching 55.62% in 2020, as shown by the dashed line with empty circle markers. Filtered sample includes those who had a pension account provided by the employer at least once over the surveyed period. The participation rates in this group are consistently high, starting at 91.26% in 1994, experiencing a slight decline to 83.63% in 2004, and then rising to 92.59% by 2020, as reflected by the solid line with solid circle markers.

Table A4 summarizes the number of DB to DC switchers and new entries to DC in the sample.

As discussed in Section Appendix F.4, the dependent variable in the analysis is a stock market entry decision, thus, I require at least two consecutive observations for each individual in the sample. This additional filter leaves me with 13,623 individuals (from 11,478 households).

I additionally drop the respondents who were retired, self-employed or changed job. I also require that a household has minimal financial savings of 50$ and a household income of 5,000$ (2010 prices). This final filter leaves 10,525 individuals (from 8,710 households) corresponding to 28,387 observations.

Table A4: Number of DB to DC switchers and new entries to DC

year	(1) Switch	(2) Entry	(3) Total
1994	218	124	342
1996	145	93	238
1998	97	58	155
2000	123	76	199
2002	60	38	98
2004	47	29	76
2006	97	78	175
2008	65	42	107
2010	27	11	38
2012	127	69	196
2014	36	61	97
2016	8	22	30
2018	75	108	183
2020	13	50	63
Total	1,138	859	1,997

Table A4 provides detailed data on the number of individuals who switched from DB pension plans to DC pension plans, as well as new entries into DC plans, among the subsample of Health and Retirement Study (HRS) respondents during survey waves between 1994 and 2020. The column (1) (Switch) represents participants who transitioned from a DB plan to a DC plan within the given year. The column (2) (Entry) includes individuals who newly entered into a DC plan without prior participation in a DB plan within the dataset. The column (3) sums the number of switchers and new entrants for each year.

Appendix F.2.2 Pension plan data

The Health and Retirement Study collects a large range of pension-related data (section G on Employment before 2000, section J on Employment starting in 2002 and section J2 on Pension starting in 2016). The survey provides information on plan history for up to 4 plans, plan type and policy, contributions (self- and employer-made), accumulated benefits, and how the funds in the plan account are invested.

The question on the plan type provides three options, namely, defined benefit (formula), defined contribution (account) or mixed. I assign DB plan type presence if the person reported that respective type for at least one of the reported plans. Identifying DC plans is less straightforward, as some patterns in the responses suggest potential inconsistency in the data. To make sure the DC plans are correctly identified, I rely on additional information about plans available, namely whether the individual reported positive contributions made by employer and employee, and if DC

account value is positive. I also impute DC or DB account presence if the information is missing in the current survey wave, but such an account is reported in both the previous and the next wave (conditional on the person not changing employer during this period).

Figure A3 shows the evolution of DB and DC plan shares in the selected sample of HRS pension plan participants.[46] DB plans in the sample peaked at 76% in the 1996 and decreased almost two-folds to 45% by 2020. DC plans, on the contrary, have been actively growing from 58% to 90% between 1994 and 2010, and slightly decreased to 81% in 2020.

Next, I identify switchers and first-time entrants to the DC plans. Switchers are those who had DB pension plan before and now have a DC plan (this could be due to a change in the plan, or an additional plan).

Figure A5 illustrates the share of participants who either switched to a defined contribution (DC) plan or entered a DC plan for the first time, over the period from 1992 to 2020, conditional on participating in any pension plan. As this Figure shows, the share of participants who switched to a DC plan starts at around 8% in 1992 and generally declines over time, reaching its lowest point of about 2% in 2016, with some peak fluctuations in 2006, 2012, and 2018. Similarly, the share of first-time entrants into DC plans follows a downward trend, consistently remaining lower than the switchers' share throughout the period.

Appendix F.2.3 Financial Variables and Other Controls

In the analysis I include different measures of wealth, such as liquid wealth, financial wealth and total wealth. Financial wealth includes both risky and riskless assets. Risky assets include shares of stock in publicly held corporations, mutual funds, or investment trusts and corporate, municipal, government, or foreign bonds, or any bond funds (outside IRA and Keogh accounts). Riskless assets sum the value of checking, savings, and money market accounts, and value of CDs, government savings bonds, and treasury bills. Stock market participation is defined as equal to 1 for an individual if risky assets has a positive value and 0 otherwise.

Liquid wealth is financial wealth less uncollaterized debt (i.e. credit card balances, medical debts, life insurance policy loans, loans from relatives). Total wealth is liquid wealth plus the net value of house equity (house value minus current mortgage value).

Savings in IRA and Keogh accounts are tracked separately in the HRS, so there is a separate

[46] Some people have multiple plans and both types, so the sum of shares may exceed 100%

Figure A5: Share of DC switchers and first entrants over time, conditional on participating in a pension plan.

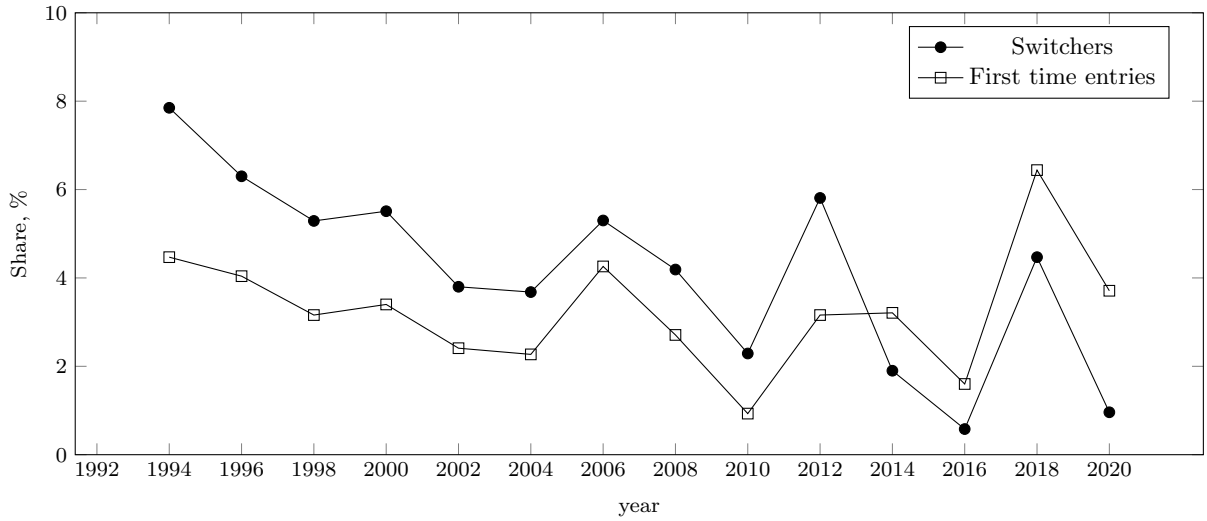


Figure A5 illustrates the share of participants who either switched to a defined contribution (DC) plan or entered a DC plan for the first time over the period from 1992 to 2020. The share is conditional on participating in any pension plan in both the current and previous wave for switchers, or participating in the current wave and having no employer-based retirement plan in the previous wave for first-time DC entrants.

IRA wealth variable, and an IRA dummy to indicate if a person has any such accounts, and risky share in IRA, if any.

Total family income is total income of the head of household and their spouse or partner for the last calendar year. It includes respondent and spouse earnings, pensions and annuities, SSI and Social Security Disability, Social Security retirement, unemployment and workers compensation, other government transfers, household capital income, and other income.

I deflate all nominal variables to 2010 using the consumer CPI series from the St. Louis Federal Reserve. I then take logarithms and first differences to obtain growth rates. The wealth and income variables are winsorized at the 1st and 99th percentiles.

Following the literature, I include "life-cycle controls" and "preference shifters" such as age, age squared, gender, race, education, change in the marriage status, change in the children dummy, (change in) home ownership status, region of residence.

Table A5: Summary Statistics by DC Group

Variable	(1) N^{DB}	(2) N^{DC}	(3) $Mean^{DB}$	(4) $Mean^{DC}$	(5) $diff^{DB-DC}$	(6) Std Err	(7) t-value	(8) p-value
Demographics								
Age	4621	26418	57.893	57.855	0.037	0.056	0.65	0.501
Gender	4621	26418	0.546	0.533	0.013	0.008	1.65	0.098
Race	4616	26399	1.318	1.254	0.064	0.009	6.75	0.000
Married	4621	26418	0.762	0.783	-0.022	0.007	-3.25	0.001
Children	4621	26418	0.920	0.919	0.002	0.005	0.35	0.725
Education	4621	26416	3.662	3.767	-0.104	0.021	-5.00	0.000
Union	4423	25591	0.398	0.231	0.168	0.008	21.45	0.000
Wealth Measures								
FW	4621	26418	87.267	120.422	-33.154	5.654	-5.85	0.000
$\Delta \log FW$	4184	24434	0.135	0.111	0.025	0.022	1.10	0.261
LW	4621	26418	80.778	113.720	-32.943	5.668	-5.80	0.000
$\Delta \log LW$	3047	17915	0.139	0.114	0.025	0.026	0.95	0.341
TW	4621	26418	246.720	337.380	-90.660	10.335	-8.75	0.000
$\Delta \log TW$	3962	23095	0.095	0.104	-0.009	0.014	-0.60	0.550
FAMINC	4621	26418	103.877	122.845	-18.968	6.224	-3.05	0.003
$\Delta \log FAMINC$	4507	25829	0.032	0.025	0.007	0.009	0.70	0.479
Owner	4621	26418	0.865	0.891	-0.026	0.005	-4.90	0.000
IRA	4621	26418	0.527	0.686	-0.159	0.008	-20.20	0.000
Participation and Entry/Exit								
Participation	4621	26418	0.314	0.386	-0.072	0.007	-9.70	0.000
Entry (% non-partic)	3176	16089	0.128	0.166	-0.037	0.007	-5.70	0.000
Entry (% all)	4621	26418	0.088	0.101	-0.013	0.005	-2.85	0.005
Exit (% partic)	1358	9806	0.248	0.248	0.000	0.013	0.00	0.991
Exit (% all)	4621	26418	0.073	0.092	-0.019	0.004	-4.55	0.000
Conditional Response								
cond. RS	1450	10199	0.615	0.636	-0.021	0.009	-2.40	0.016
cond. ΔRS	1021	7374	0.014	0.013	0.001	0.009	0.05	0.953
cond. Δ^aRS	1021	7374	-0.009	-0.007	-0.002	0.009	-0.20	0.846

Table A5 presents summary statistics comparing individuals in Defined Benefit (DB) and Defined Contribution (DC) pension plans. Columns report sample sizes (N^{DB}, N^{DC}), group means ($Mean^{DB}$, $Mean^{DC}$), differences in means ($diff^{DB-DC}$), standard errors, and p-values for the difference. Statistical significance is indicated by p-values less than 0.05.

Appendix F.3 Descriptive statistics

Table A5 presents summary statistics comparing key demographic and financial characteristics between individuals in the Defined Benefit (DB) and Defined Contribution (DC) groups. Each variable is accompanied by sample sizes (N), mean values for both groups, the difference in means (diff^{DB-DC}), standard errors, t-values, and p-values.

In the demographic category, the mean age for both groups is similar, with a negligible and statistically insignificant difference. The gender distribution shows a slight but marginally significant difference, with the DB group having a marginally higher proportion of males. There is a notable and significant difference in the race variable, with the DB group having a higher average racial index. The proportion of married individuals is slightly lower in the DB group compared to the DC group, and this difference is statistically significant. The average education level is significantly lower in the DB group, and union membership is considerably higher, with a significant difference indicating that more DB participants are unionized.

For the wealth measures, both mean financial wealth (FW) and liquid wealth (LW) are significantly lower in the DB group, with substantial differences of approximately 33 and 32 thousand dollars, respectively. However, the growth rates of financial and liquid wealth, measured as changes in their logarithmic values, show no significant differences between the groups. Total wealth (TW) is also lower in the DB group, with a significant gap. Family income (FAMINC) and ownership of assets also show significant differences, with the DB group having lower values. Furthermore, the proportion of individuals holding an IRA is notably lower in the DB group.

Participation in the stock market is higher among the DC group, with significant differences in both overall participation and entry rates. Specifically, the entry rate among non-participants and the overall entry rate are higher for the DC group. However, the exit rate among participants does not differ between groups, while the overall exit rate is significantly higher in the DB group.

Finally, conditional response statistics indicate that the conditional risky share (cond. RS) is slightly lower for the DB group, and this difference is statistically significant. However, changes in the risky share, both in absolute and active terms, do not exhibit significant differences between the groups. Overall these comparisons confirm that there are some significant differences between the two groups, as expected.

Appendix F.4 Empirical Setup

In the US setting, I focus on the change in the type of individual's retirement plan from DB to DC, conditional on the fact they have remained in the same job. To further account for personal saving preferences, I include as controls, variables such as changes in financial wealth and income, home ownership, and demographic factors.

The main empirical test aims to evaluate how the transition from a DB plan (or no pension plan) to a DC plan affects individuals' likelihood of entering the stock market and risky share for current stock market participants. So, the analysis focuses on a sample of individuals who, at time $t-2$, either had a DB plan or no pension and did not have a DC plan. By time t, these individuals may have switched to a DC plan. By analogy, I use the similar dynamic DiD setting, however, the variation comes from the staggered changes in the employer-specific pension rules.

The key variable of interest is the presence of a DC plan for period $\hat{\tau}^{DC} = j$ at time t, with control variables including individual characteristics, financial factors, and time effects. The objective is to determine whether the shift to a DC plan increases the probability of entering the stock market, compared to those who remained in a DB plan.

$$P(Entry_{it}) = \alpha + \sum_{j=-4}^{-2} \beta_j I[_{\hat{\tau}^{DC}=j}] + \beta_1 I[_{\hat{\tau}^{DC}=1}] + \sum_{j=4}^{12} \beta_j I[_{\hat{\tau}^{DC}=j}] + \\ + \theta X_{it} + I[a_{it}] + I[y_t] + \varepsilon_{it} \quad \text{(A21)}$$

$Entry_{it}$ represents whether individual i enters the stock market at time t, this means that individual did not participate in the stock market before ($Partic_{t-1,t} = 0$). $\hat{\tau}^{DC}$ denotes years before/after getting DC (age_{it} - $age_t^{rule} + 1$). Indicator $I[\hat{\tau}^{DC} = j]$ equals 1 if $\hat{\tau}^{DC} = j$ is true. Coefficient β_j captures the effect of being treated (having a DC plan) for j periods between time $t-1$ and t on the likelihood of entering the stock market at time t. X_{it} is vector of time-varying characteristics that are not perfectly correlated across individuals. $I[a_{it}]$ is set of age dummies. $I[y_t]$ represents time fixed effects to account for any temporal factors that could influence stock market entries over the period. ε_{it} is a (regression) residual.

As in the Swedish setting, a similar analysis is conducted for the change in (active) risky share for stock market participants:

$$\Delta\omega_{it}^{(a)} = \alpha + \sum_{j=-4}^{-2} \beta_j I[\hat{\tau}^{DC}=j] + \beta_1 I[\hat{\tau}^{DC}=1] + \sum_{j=4}^{12} \beta_j I[\hat{\tau}^{DC}=j] + \\ + \theta X_{it} + I[a_{it}] + I[y_t] + \varepsilon_{it} \quad \text{(A22)}$$

Appendix F.5 Results

Table A6 presents regression results analyzing the effect of DC pension plan enrollment on stock market entry over various time horizons using HRS data. The estimates are displayed in two sets: the Baseline model (Column 1) and the Baseline model with an additional control for IRA ownership (Columns 2). The dependent variable measures the likelihood of stock market entry, and the results are reported as marginal effects. The standard errors are presented in parentheses below the coefficient estimates, and clustered at individual level.[47]

For specific time horizons, the effect of DC plan enrollment on stock market entry is noteworthy. At $\hat{\tau}^{DC} = 4$, the marginal effect is positive and statistically significant at the 1% and 10% levels across the Baseline and IRA-controlled models, respectively. This suggests a marked increase in the probability of stock market entry four years after DC plan enrollment. At $\hat{\tau}^{DC} = 10$ and $\hat{\tau}^{DC} = 12$, the effect is even more pronounced, with statistically significant increases at the 1% level. Specifically, the effect at $\hat{\tau}^{DC} = 10$ reaches a substantial magnitude, indicating a robust influence of DC plan enrollment.

The wealth growth variable, $\Delta \log FW_t$, consistently shows a significant positive relationship with stock market entry, emphasizing the importance of financial wealth in predicting stock market behavior. Additionally, the inclusion of an IRA control variable demonstrates a significant and positive impact, indicating that IRA ownership is a strong predictor of stock market participation.

The results indicate that DC pension plan enrollment has a temporal impact on stock market entry, with significant effects manifesting several years after enrollment.

Analogously, Table A7 presents regression results analyzing the effect of DC pension plan enrollment on risky share over various time horizons using HRS data. There is some evidence of the positive effect on the (active) change in risky share for stock market participants. Although there

[47] Alternative clustering at age-year level does not change the significance and reported in Table A8.

is no statistical significance in any horizon[48], the effect at $\hat{\tau}^{DC} = 6$ and $\hat{\tau}^{DC} = 10$ is economically meaningful (around 3.65 and 5 percentage point increase relative to unconditional risky share in the first year, respectively, and summing to 10.9 percentage point over 12 years).

To sum up, although the identification is much weaker in case of the US setting, and the therefore the results are prone to endogeneity and reverse causality concerns, and the sample size is small, I find some evidence of positive effect of a DC account enrollment on stock market entry and risky share.

[48]The result is unchanged under alternative clustering at age-year level reported in Table A9.

Table A6: Regression results for DC Pension Plan effect on Stock Market Entry

	Baseline	IRA Control
	(1)	(2)
$\hat{\tau}^{DC} = -4$	0.004	-0.008
	(0.020)	(0.021)
$\hat{\tau}^{DC} = -2$	0.003	-0.003
	(0.021)	(0.021)
$\hat{\tau}^{DC} = 2$	0.002	-0.002
	(0.012)	(0.012)
$\hat{\tau}^{DC} = 4$	0.032***	0.021*
	(0.012)	(0.012)
$\hat{\tau}^{DC} = 6$	0.006	-0.003
	(0.015)	(0.015)
$\hat{\tau}^{DC} = 8$	0.028*	0.018
	(0.017)	(0.017)
$\hat{\tau}^{DC} = 10$	0.082***	0.067***
	(0.020)	(0.020)
$\hat{\tau}^{DC} = 12$	0.032***	0.023***
	(0.007)	(0.008)
$\Delta \log FW_t$	0.079***	0.079***
	(0.002)	(0.002)
IRA		0.077***
		(0.006)
Life Cycle Controls	Yes	Yes
Preference Controls	Yes	Yes
Year and Regions FE	Yes	Yes
N	17,370	16,862

Individual-clustered standard errors in parentheses
Marginal effects evaluated at sample averages
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6 presents the regression results for the Equation A21. Sample includes only individuals who did not participate in the stock market in the previous period. The key independent variables are time indicators relative to DC plan enrollment, denoted by $\hat{\tau}^{DC} = j$, where j represents years before ($j < 0$) or after ($j > 0$) DC enrollment. Column (1) shows the baseline specification, while Column (2) includes an additional control for IRA ownership. The reported effects are marginal effects are evaluated at the sample averages. Standard errors clustered at the individual level are reported in parentheses.

Table A7: Regression results for DC Pension Plan effect on Risky Share

	$\Delta\omega$		$\Delta\omega^a$	
	(1)	(2)	(3)	(4)
$\hat{\tau}^{DC} = -4$	0.009	0.008	0.008	0.007
	(0.023)	(0.023)	(0.023)	(0.023)
$\hat{\tau}^{DC} = -2$	0.040	0.039	0.038	0.037
	(0.026)	(0.026)	(0.026)	(0.026)
$\hat{\tau}^{DC} = 2$	0.006	0.005	0.005	0.004
	(0.016)	(0.016)	(0.015)	(0.015)
$\hat{\tau}^{DC} = 4$	-0.006	-0.007	-0.006	-0.007
	(0.015)	(0.015)	(0.015)	(0.015)
$\hat{\tau}^{DC} = 6$	0.025	0.024	0.023	0.022
	(0.017)	(0.017)	(0.017)	(0.017)
$\hat{\tau}^{DC} = 8$	0.009	0.008	0.009	0.008
	(0.019)	(0.019)	(0.019)	(0.019)
$\hat{\tau}^{DC} = 10$	0.032	0.031	0.031	0.031
	(0.027)	(0.027)	(0.027)	(0.027)
$\hat{\tau}^{DC} = 12$	0.001	0.000	0.001	0.000
	(0.007)	(0.007)	(0.007)	(0.007)
$\Delta \log FW_t$	0.079***	0.079***	0.079***	0.079***
	(0.002)	(0.002)	(0.002)	(0.002)
IRA		0.009		0.009
		(0.009)		(0.009)
Life Cycle Controls	Yes	Yes	Yes	Yes
Preference Controls	Yes	Yes	Yes	Yes
Year and Regions FE	Yes	Yes	Yes	Yes
N	8,379	8,379	8,379	8,379

Individual-clustered standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7 presents the regression results for the Equation A22. Sample includes only individuals who participate in the stock market in the previous and current period. The key independent variables are time indicators relative to DC plan enrollment, denoted by $\hat{\tau}^{DC} = j$, where j represents years before ($j < 0$) or after ($j > 0$) DC enrollment. Column (1) shows the baseline specification, while Column (2) includes an additional control for IRA ownership for the change in risky share. Columns (3) and (4) present the results for the active change in risky share, accordingly. Standard errors clustered at the individual level are reported in parentheses.

Table A8: Regression results for DC Pension Plan effect on Stock Market Entry with Alternative SEs.

	Baseline	IRA Control
	(1)	(2)
$\hat{\tau}^{DC} = -4$	0.004	-0.008
	(0.023)	(0.024)
$\hat{\tau}^{DC} = -2$	0.003	-0.003
	(0.020)	(0.020)
$\hat{\tau}^{DC} = 2$	0.002	-0.002
	(0.013)	(0.013)
$\hat{\tau}^{DC} = 4$	0.032***	0.021*
	(0.012)	(0.012)
$\hat{\tau}^{DC} = 6$	0.006	-0.003
	(0.014)	(0.014)
$\hat{\tau}^{DC} = 8$	0.028*	0.018
	(0.017)	(0.017)
	(0.016)	(0.016)
$\hat{\tau}^{DC} = 10$	0.082***	0.067***
	(0.019)	(0.019)
$\hat{\tau}^{DC} = 12$	0.032***	0.023***
	(0.007)	(0.008)
$\Delta \log FW_t$	0.079***	0.079***
	(0.002)	(0.002)
IRA		0.077***
		(0.006)
Life Cycle Controls	Yes	Yes
Preference Controls	Yes	Yes
Year and Regions FE	Yes	Yes
N	17,370	16,862

Standard errors clustered at age-year level in parentheses
Marginal effects evaluated at sample averages
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8 presents the regression results for the Equation A21: $P(Entry_{it}) = \alpha + \sum_{j=-4}^{-2} \beta_j I[_{\hat{\tau}^{DC}=j}] + \beta_1 I[_{\hat{\tau}^{DC}=1}] + \sum_{j=4}^{12} \beta_j I[_{\hat{\tau}^{DC}=j}] + \theta X_{it} + I[a_{it}] + I[y_t] + \varepsilon_{it}$ assessing the effect of DC pension plan enrollment on the probability of stock market entry in the HRS data for the US. Sample includes only individuals who did not participate in the stock market in the previous period, so the dependent variable $P(Entry_{it})$ is a binary indicator of whether an individual enters the stock market. The key independent variables are time indicators relative to DC plan enrollment, denoted by $\hat{\tau}^{DC} = j$, where j represents years before ($j < 0$) or after ($j > 0$) DC enrollment. Column (1) shows the baseline specification, while Column (2) includes an additional control for IRA ownership. Models incorporate life cycle controls, preference controls, and fixed effects for year and region. The coefficients for $\hat{\tau}^{DC}$ variables indicate the change in the probability of stock market entry at specific time intervals relative to DC plan enrollment. The reported effects are marginal effects are evaluated at the sample averages. Standard errors clustered at the age-year level are reported in parentheses.

Table A9: Regression results for DC Pension Plan effect on Risky Share with Alternative SEs.

	$\Delta\omega$		$\Delta\omega^a$	
	(1)	(2)	(3)	(4)
$\hat{\tau}^{DC} = -4$	0.009	0.008	0.008	0.007
	(0.039)	(0.040)	(0.039)	(0.040)
$\hat{\tau}^{DC} = -2$	0.040	0.039	0.038	0.037
	(0.028)	(0.028)	(0.027)	(0.028)
$\hat{\tau}^{DC} = 2$	0.006	0.005	0.005	0.004
	(0.021)	(0.021)	(0.021)	(0.021)
$\hat{\tau}^{DC} = 4$	-0.006	-0.007	-0.006	-0.007
	(0.016)	(0.016)	(0.016)	(0.016)
$\hat{\tau}^{DC} = 6$	0.025	0.024	0.023	0.022
	(0.024)	(0.024)	(0.023)	(0.023)
$\hat{\tau}^{DC} = 8$	0.009	0.008	0.009	0.008
	(0.020)	(0.020)	(0.021)	(0.021)
$\hat{\tau}^{DC} = 10$	0.032	0.031	0.031	0.031
	(0.031)	(0.032)	(0.030)	(0.030)
$\hat{\tau}^{DC} = 12$	0.001	0.000	0.001	0.000
	(0.013)	(0.013)	(0.012)	(0.012)
$\Delta \log FW_t$	0.079***	0.079***	0.079***	0.079***
	(0.006)	(0.006)	(0.006)	(0.006)
IRA		0.009		0.009
		(0.009)		(0.009)
Life Cycle Controls	Yes	Yes	Yes	Yes
Preference Controls	Yes	Yes	Yes	Yes
Year and Regions FE	Yes	Yes	Yes	Yes
N	8,379	8,379	8,379	8,379

Standard errors clustered at age-year level in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9 presents the regression results for the Equation A22: $\Delta\omega_{it}^{(a)} = \alpha + \sum_{j=-4}^{-2} \beta_j I[_{\hat{\tau}^{DC}=j}] + \beta_1 I[_{\hat{\tau}^{DC}=1}] + \sum_{j=4}^{12} \beta_j I[_{\hat{\tau}^{DC}=j}] + \theta X_{it} + I[a_{it}] + I[y_t] + \varepsilon_{it}$ assessing the effect of DC pension plan enrollment on the change in (active) risky share for stock market participants in the HRS data for the US. Sample includes only individuals who participate in the stock market in the previous and current period. The key independent variables are time indicators relative to DC plan enrollment, denoted by $\hat{\tau}^{DC} = j$, where j represents years before ($j < 0$) or after ($j > 0$) DC enrollment. Column (1) shows the baseline specification, while Column (2) includes an additional control for IRA ownership for the change in risky share. Columns (3) and (4) present the results for the active change in risky share, accordingly. Models incorporate life cycle controls, preference controls, and fixed effects for year and region. The coefficients for $\hat{\tau}^{DC}$ variables indicate the (active) change risky share at specific time intervals relative to DC plan enrollment. Standard errors clustered at the age-year level are reported in parentheses.