Aydemir, Abdurrahman B.; Ersan, Yasar

Working Paper

Does Education Improve Financial Outcomes? Evidence from Stock Market and Retirement Accounts in Türkiye

IZA Discussion Papers, No. 17927

Provided in Cooperation with:
IZA – Institute of Labor Economics

Suggested Citation: Aydemir, Abdurrahman B.; Ersan, Yasar (2025) : Does Education Improve Financial Outcomes? Evidence from Stock Market and Retirement Accounts in Türkiye, IZA Discussion Papers, No. 17927, Institute of Labor Economics (IZA), Bonn

This Version is available at:
https://hdl.handle.net/10419/320521

I Z A Institute
of Labor Economics
Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 17927

Does Education Improve Financial Outcomes? Evidence from Stock Market and Retirement Accounts in Türkiye

Abdurrahman B. Aydemir
Yasar Ersan

MAY 2025

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 17927

Does Education Improve Financial Outcomes? Evidence from Stock Market and Retirement Accounts in Türkiye

Abdurrahman B. Aydemir
Sabanci University and IZA

Yasar Ersan
Ankara University

MAY 2025

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Does Education Improve Financial Outcomes? Evidence from Stock Market and Retirement Accounts in Türkiye*

We examine the causal effect of education on financial outcomes related to stock markets and retirement savings, leveraging a major compulsory school reform and a unique data set covering the universe of investors in Türkiye. The estimates show no effects on participation rates, portfolio composition, or return performance. Moreover, education does not appear to influence behavioral biases or heuristics in retirement plans. The reform leads to a 3% increase in pension savings for females, with no significant effect on males. Higher earnings and increased employment with employer-sponsored pension plans appear as potential mechanisms driving the wealth effect.

JEL Classification: I21, I26, G11, G41, G50, G53, J32
Keywords: education, retirement, wealth, investment decisions

Corresponding author:
Yasar Ersan
Ankara University
Dögol Caddesi 06100 Tandoğan
Ankara
Türkiye
E-mail: yasarersan@ankara.edu.tr

* We appreciate extremely helpful feedback and comments from Hoyt Bleakley, John Bound, Uday Rajan, and Dean Yang as well as Emir Murathanoglu, Mahmut Aydogmus, and participants of Michigan's Economic Development Seminar (EDS), METU Economics Departmental Seminar and Spring 2023 WEFIDEV Seminar Series. We thank Guzhan Gulay for helping us gain access to the administrative data sets, and Ozgur Uysal, Nagihan Altinisik, Adem Gun, and Ugur Arslan for assembling the data. This research was supported in part through computational resources and services provided by Advanced Research Computing at the University of Michigan, Ann Arbor.

1 Introduction

With the expansion of financial markets and the growing complexity of financial products, making informed financial choices has become increasingly important. In this context, education may shape financial behavior through various channels, including its potential effects on cognitive skills, risk aversion, and financial market participation. Alternatively, inherent traits may influence both educational attainment and financial decisions, creating a correlation between the two. While previous research documents a strong association between education and financial behavior[1], evidence on the causal effect of education on financial behavior remains limited (Cole *et al.* (2014), Black *et al.* (2018), and Gray *et al.* (2021)). Existing studies focus on stock market participation, risk-taking behavior, investment income, equity ownership, bankruptcy, foreclosure, savings, and debt behavior. This paper extends the scope of analysis by estimating the effects on portfolio performance, common financial mistakes, behavioral biases, and heuristics, in addition to several outcomes examined in previous research. Additionally, it broadens the context beyond the stock market to include retirement savings, providing a more comprehensive perspective on the relationship between education and financial decision-making.

Leveraging a major compulsory school reform in Türkiye that extended mandatory education by three years, we examine financial outcomes using comprehensive administrative data covering the entire population of investors in the stock market and retirement accounts. This setting is significant, as stock market assets and retirement portfolios represent a substantial portion of household wealth, thus playing a crucial role in individual financial well-being.[2] In particular, the prominence of private pension plans has grown significantly over time, with total assets reaching nearly $42.5 trillion in the OECD area as of 2018 (OECD (2019)).[3] This increase occurs amid rising concerns about retirement

[1] Gomes *et al.* (2021)

[2] In the EU area the share of equity and investment funds was 32.8% of total household assets in 2022 while insurance, pensions, and standardized guarantees had a share of 27.8% (Eurostat, 2023). The total value of assets in equity and investment funds was about 11 trillion euros while the total value of assets in insurance, pensions, and standardized guarantees was 9.3 trillion in the EU area in 2022 (`https://ec.europa.eu/eurostat/statistics-explained/index.php?title=Households_-_statistics_on_financial_assets_and_liabilities#Assets_and_liabilities`). Survey of Consumer Finance in 2019 in the US, on the other hand, documents that the share of households with direct stock holdings was 15% while those with either direct or indirect stock holdings was 53%. Indirect stock holdings increased over time, partly due to the increasing role of 401(k) pension plans.

[3] Since the mid-1990s, many countries have introduced individual private pensions, leading to a shift from defined benefit (DB) pension plans to defined contribution (DC) pension plans. Defined benefit (DB) pension plans offer specified payment amounts in retirement, whereas defined contribution (DC)

savings and elderly poverty, driven by longer life expectancies, declining birth rates, and inadequate individual savings.[4] Given individuals' exposure to the inherent risks of these financial markets, understanding the role of general education is of significant policy relevance.

Education may be causally related to different margins of financial decisions. In particular, education may raise earnings, enhancing both the capacity to save and the likelihood of investing in financial products, which could influence the decision to participate in financial markets.[5] Education may also shape risk preferences, affecting both risk aversion and the allocation of wealth toward risky assets.[6] Moreover, education could enable individuals to make better financial decisions by enhancing cognitive skills that reduce the costs of gathering and processing financial market information.[7] We examine a comprehensive set of outcomes, offering insights into potential impacts of education through these margins. These include the decision to participate in the stock market and retirement plans, and a detailed analysis of the variation in investors' portfolios—including risky share and wealth. Additionally, we study portfolio performance, common financial mistakes, behavioral biases, and heuristics in the stock market for the first time in the literature. These latter outcomes are particularly interesting as they directly test the potential role of education in improving financial decisions through its impact on cognitive and critical thinking skills.

We first establish the effects of education reform on educational outcomes. Our findings indicate that the reform increased years of schooling by half a year for individuals born after January 1987, with larger effects observed among women. Moreover, although

pension plans allow individuals to contribute and invest on their own. For more details, see Arcanjo (2019)

[4]Increasing life expectancy and falling birth rates that accelerate population aging put immense pressure to sustain adequate and financially sustainable levels of pensions. OECD (2019) documents that the number of people over 65 for every 10 workers in the OECD area was 2 in 1980, 3 in 2020, and projected to be 6 in 2060. In both developed and developing countries financial hardships in old age are common due to under saving for retirement (Chetty (2015)). Almost half of all working-age households in the US had zero retirement savings in their retirement accounts in 2016 (Morrissey (2016)). The proportion of the elderly population with incomes below 50% of relative poverty thresholds is the largest among OECD countries in the US, Mexico, and Türkiye (OECD (2021)). Older people are the poorest in Türkiye as the old age average social pension salary was only 21% of the poverty line in 2017 (ILO (2021)).

[5]See for example, Aydemir and Kirdar (2017) in the Turkish context; Ichino and Winter-Ebmer (1999) in the German context; Devereux and Hart (2010) in the UK context.

[6]Jung (2015), Tawiah (2022), and Black *et al.* (2018) study the effect of education on risk aversion and risk-taking behavior. Sutter *et al.* (2020) studies the effect of financial literacy education on risk aversion.

[7]For example, Black *et al.* (2011) show that education increases intelligence test scores while Hanushek and Woessmann (2008) discuss the effects on cognitive abilities.

the main goal of the reform was to promote junior high school completion, it also had significant spillover effects that promoted high school and college completion.[8]

We next estimate the effects of education reform on financial market participation. OLS estimates of the relationship between education and financial behavior reveal a strong positive correlation between schooling and participation in the stock market and defined-contribution (DC) pension plans. However, we find no causal evidence that the reform-induced increase in schooling improves participation in either DC pension plans or the stock market.[9]

We first examine stock market behavior, focusing on portfolio composition, portfolio returns, and stock market wealth. The results show no effect of education reform on the composition of stock market portfolios, in terms of the likelihood of holding bonds, funds, risky assets, and stocks that are safer and more liquid. In line with these results, we find no significant effect on the share of wealth allocated across different stock market assets or on total stock market wealth. Finally, the estimates reveal no evidence that exposure to education reform results in higher portfolio returns. Overall, these findings suggest that general education plays at best a limited role in shaping differences in the composition of investors' stock market portfolios.

Analysis of the DC pension plans reveals heterogeneity in the effects of education reform on pension wealth by gender. The reform leads to a 3% increase in wealth in retirement accounts for females, with no significant effect for males. This suggests that schooling may play an influential role in enhancing pension wealth within DC plans.[10] Given the positive wealth effect, we next examine various plausible proxies for financial literacy and sophistication, and for behavioral biases and heuristics prevalent in DC pension plans. Our analysis indicates no economically significant impact of education reform on equity exposure in portfolios, including the likelihood of holding equity funds, or the share of wealth invested in equity funds, stocks, or risky assets. Results also indicate that general education does not reduce the tendency of financial mistakes, behavioral biases, and heuristics. Finally, portfolio returns in the DC pension plans do not significantly vary by education.[11] Overall, human capital does not appear to be a significant input in

[8]Other studies of the reform also report significant spillover effects (e.g. Kirdar *et al.* (2016); Aydemir and Kirdar (2017)).

[9]Our findings on OLS estimates being larger than causal estimates are consistent with the previous studies such as Calvet and Sodini (2014), and Black *et al.* (2018)

[10]Scaling those reduced-form estimates by the increase in years of schooling induced by the education reform reveals that an extra year of schooling improves pension wealth or savings by 5-6% for females while having no effect for males.

[11]Our results are also in line with Fagereng *et al.* (2020), which shows that general education does not

constructing financially desirable portfolios.

What could then be driving the positive wealth effects in DC pension plans? To explore the potential causal mechanisms underlying this wealth effect, we analyze the first enrollment type in DC plans—whether individuals join for the first time through voluntary, automatic, or employer-sponsored enrollment. We find that first-time participation in DC pension plans through employer-sponsored pension plans increases among females affected by the education reform. Next, we estimate the effect of education reform on financial skills and cognitive skills. We find no effect on financial skills using proxies of trust in the stock market, financial knowledge, peer effects, and risk taking, but find suggestive evidence for improvement in cognitive skills, through the proxies of literacy and numeracy capabilities, among females. We also estimate the causal effect of education reform on wages, which shows a significant increase for women and small and imprecisely estimated effects for males.[12] Our analysis also reveals that females become more likely to work in large firms and less likely to work in low-paying occupations. Thus, the underlying mechanism driving more wealth in DC pension plans appears to be education-induced improvements in labor market skills, which lead to higher earnings and better contracts with additional retirement benefits among women, rather than by enhancements in financial decision making.

For the causal identification, we exploit the substantial exogenous variation in schooling across month-year birth cohorts brought about by the 1997 Education Reform in Türkiye that extended compulsory schooling from 5 to 8 years. Taking advantage of the discontinuity at the month-year of birth of January 1987 arising from the education reform, we employ a regression discontinuity design (RD). Those born after January 1987 serve as the treatment group, whereas those born before January 1987 form the control group in our quasi-experimental research design.[13]

We use Household Labor Force Surveys assembled by the Turkish Statistical Institute (TURKSTAT) to estimate the reform-induced changes in schooling outcomes. For financial outcomes, we utilize two novel administrative datasets owned by the Borsa Istanbul Group, the legal authority maintaining records of individual transaction accounts and individual retirement accounts in defined contribution (DC) pension plans in Türkiye.

seem to be a factor leading to higher capital returns.

[12]These results are consistent with earlier findings from Türkiye that show low labor market returns to schooling for males but higher returns for females using the same reform (Aydemir and Kirdar (2017)).

[13]Previous literature has examined the effects of the 1997 reform on various outcomes including earnings (e.g. Aydemir and Kirdar (2017)), domestic violence (e.g. Erten and Keskin (2018)), internal migration (Aydemir *et al.* (2022)), and savings (Aydemir (2021)).

Stock market data refers to December of 2021 and 2022 while retirement account data refers to December of 2019 and 2020.[14] These data include month-end snapshots of the universe of stock market investors and individual retirement accounts in Türkiye respectively. These datasets eliminate concerns of mismeasurement or reporting bias due to their universal coverage and provide exceptionally rich information. In particular, stock market data provides information on portfolio composition, including the specific stock market assets, and the share of wealth invested in each asset, and allows calculation of the return of portfolios over time. Data on retirement accounts offers information on portfolios including fund choices, account balances, trading, and contribution amounts. A shortcoming of these data is that they do not include schooling levels but instead involve the month and year of birth of individuals which determines exposure to the 1997 Education Reform. We can thus identify the reduced-form impact of the policy –via individuals' policy exposure using RD– on financial market participation, portfolio variation, and performance. We also use Household Budget Surveys of TURKSTAT for ordinary least squares (OLS) estimation of the effect of education on financial outcomes, which serves as a benchmark for the RD analysis.

Our work adds to the literature on financial and non-financial returns of schooling (Oreopoulos and Salvanes (2011)).[15] Previous studies show that education is positively related to a higher level of financial literacy and sophistication, higher likelihood of investing in risky assets, and higher share of wealth allocated to risky assets.[16] Yet, few articles estimate the causal effect of general education on financial behavior. Using exogenous variation in education due to compulsory school reforms, these studies estimate effects on investment income, equities ownership, the probability of foreclosure, bankruptcy, and loan delinquency (Cole *et al.* (2014) in the US context), stock market participation and risk-taking behavior (Black *et al.* (2018) in Sweden), savings and debt behavior (Gray *et al.* (2021) in Britain). Our study is closely related to Black *et al.* (2018) and Cole *et al.* (2014) yet differs from these studies in important ways. We extend the literature by

[14]The analysis in this paper was first conducted for retirement savings and later extended to stock markets. The access to data was granted by the legal authority for two different years due to internal regulations leading to the one-year gap between the two data sets.

[15]An extensive set of studies documents the effects of schooling on labor income, health, and fertility, domestic violence, crime, cognitive skills, capital returns, and financial behavior. See, among others, Duflo (2001), Oreopoulos (2006), Acemoglu and Angrist (2000), Angrist and Keueger (1991), and Aydemir and Kirdar (2017) for labor income; Lleras-Muney (2005) for health and fertility; Erten and Keskin (2018), Akyol and Kırdar (2022) for domestic violence; Lochner and Moretti (2004) for crime; Carlsson *et al.* (2015) for cognitive skills; Fagereng *et al.* (2020) for capital returns; Cole *et al.* (2014), Black *et al.* (2018), and Gray *et al.* (2021) for financial behavior.

[16]Lusardi and Mitchell (2014), and Calvet *et al.* (2007, 2009a,b)

studying the causal impacts of schooling on financial behavior and savings for retirement in DC pension plans for the first time in literature[17] and analyze a richer set of outcomes offered by detailed administrative data. Unlike the findings of existing studies, overall, we find no economically significant effect of education on stock market participation and the outcomes related to stock market portfolios. Our results show that education promotes savings in DC pension plans, however, it does not impact the decision to join DC pension plans and the variation in investors' pension portfolios. Existing studies have provided evidence on the causal effect of education on financial behavior within developed country contexts. Our study contributes to this literature by offering evidence from a middle-income country context, Türkiye.

A growing body of literature hypothesizes that human capital is a close substitute for safe assets such as bonds rather than stocks. Hence, those with more education are more likely to invest in equities and participate in the stock market.[18] Following this prediction, prior research reports a strong positive correlation between education and equity participation, both directly through stock holdings and indirectly through DC pension plans (Gomes *et al.* (2021), and Egan *et al.* (2021)). Black *et al.* (2018) and Cole *et al.* (2014) find causal evidence that education promotes participation in equity in the stock market in Norway and the US, respectively. Our study complements this literature by providing the first causal estimates of education on equity exposure and risky share in DC pension plans. Pension plans are the only financial portfolios for the vast majority of the population, thus compared to existing studies with the non-retirement account financial portfolios, we can test the prediction regarding the impact of education on equity in a broader cross section.[19] Despite the statistical precision, point estimates are small in magnitude, implying general education does not significantly impact equity exposure and risky share in either DC pension plans or the stock market.

A burgeoning literature emphasizes that investors are prone to behavioral biases and

[17]Prior studies have emphasized that demographics (Engström and Westerberg (2003), and Duflo *et al.* (2006)), behavioral problems (Benartzi and Thaler (2007)), peer effects (Duflo and Saez (2002, 2003)), the complexity of the pension plans (Iyengar and Kamenica (2010)), and financial education (Lusardi and Mitchell (2014)) are important drivers of participation and savings in DC pension plans. Our study extends this literature by studying the role of general education in participation and wealth in DC pension plans.

[18]Cocco *et al.* (2005), Bodie *et al.* (1992), Fagereng *et al.* (2017), Guiso and Sodini (2013), Vissing-Jørgensen (2002), and Viceira (2001).

[19]Survey of Consumer Finance in 2019 in the US documents that the share of households with direct stock holdings fell from 17% to 15% whereas households with either direct or indirect stock holdings increased from 52% to 53%. The increase in indirect stock holdings is partly due to the increasing role of 401(k) pension plans.

heuristics that might result in welfare losses (Benartzi and Thaler (2007)).[20] The underlying reason for default effects —a prevalent behavioral problem in DC pension plans— is mainly the cognitive costs of evaluating different saving alternatives (Blumenstock *et al.* (2018)) and the limited computational capacity (Madrian and Shea (2001)). An extensive literature documents that education is positively associated with financial literacy and sophistication which avoids financial mistakes (Calvet *et al.* (2007, 2009a,b)), and Lusardi and Mitchell (2014)). Arguably, individuals gain more cognitive and numeracy skills with more education, so they are more financially literate and sophisticated (Carlsson *et al.* (2015)). Our study adds to the literature by documenting the first causal evidence on the impacts of schooling on common financial mistakes, behavioral biases, and heuristics. We find overall no significant evidence that education causally lowers financial mistakes and behavioral problems, and no evidence that education increases financial literacy. Our results further demonstrate that education does not have a causal explanatory power in portfolio returns.

In sum, in a unique setting from Türkiye involving a comprehensive range of financial outcomes from the stock market and pension plans, combined with a credible identification strategy, our findings suggest that human capital has a limited influence on financial market participation and financial behavior through risk preferences or cognitive skill enhancements, implying that intrinsic abilities may be an important driver of the observed correlation between education and financial outcomes.

The rest of the article is organized as follows. In the next section, we discuss the 1997 Education Reform in Türkiye. Section 3 briefly describes the Turkish Stock Market and the Pension System. Section 4 introduces the data and renders the details of the research design with a particular emphasis on how we identify the causal impacts of education. Sections 5 and 6 present the findings and robustness checks, respectively. Section 7 concludes the article with a broad discussion of the findings.

[20]For example, most of the investors do not join in DC pension plans despite their advantages such as tax deduction and subsidy, however they opt-in after automatic enrollment nudges (Madrian and Shea (2001) and Thaler and Benartzi (2004)). Even after participation in pension plans, stickiness to default options (Cronqvist and Thaler (2004), Choi *et al.* (2005), Blumenstock *et al.* (2018), Beshears *et al.* (2009), Chetty *et al.* (2014), and Brune *et al.* (2017)), naive diversification strategies such as the conditional 1/N heuristic (Benartzi and Thaler (2001), Agnew (2006) and Huberman and Jiang (2006)) and inertia in trading i.e. lack of portfolio rebalancing or reshuffling (Agnew *et al.* (2003) and Sialm *et al.* (2015)) are commonly observed behavioral biases and heuristics in portfolios.

2 The 1997 Education Reform

The Turkish Education system consisted of a compulsory five years of primary school, a voluntary three years of junior high school, and a voluntary three years of high school until 1997. Following the completion of 5-years of primary school, students were free to choose between general stream, technical, and religious schools for three voluntary years of junior high school. [21] During the 1990s in Türkiye, pro-Islamist parties gained sizeable popular support, which led a pro-Islamist party to form a government in 1996. On February 28, 1997, the pro-Islamist party and many of its members were forced to resign from the government and banned from politics (Atılgan *et al.* (2015)).

The new cabinet made various decisions to block the rise of the pro-Islamist social movement in Türkiye. The most radical decision appeared in education policy. On August 18, 1997, the Turkish parliament passed a law extending compulsory schooling from 5 to 8 years. By the new law, primary and junior high school was united under a new institution, primary education. Unlike some major schooling and health reforms implemented in response to economic developments, the 1997 Education Reform was primarily a political decision rather than an economic initiative. Also, the reform did not overlap with any other structural or policy changes that could have influenced the outcomes examined in this study.

The Turkish Education Reform became effective for the 1997-98 schooling year. It compelled to attend mandatory 8 years of compulsory schooling for the students who completed grade 5 or a lower grade at the end of the 1996-97 schooling year, and those who did not hold a primary school degree at the beginning of the 1997-98 schooling year. The education reform was carried out nationwide and was strictly implemented by the legal authorities. The school starting age in Türkiye was 6. Therefore, those born before January 1987 were not affected by the education reform, so they could either drop out or continue further schooling. However, those born after January 1987 and who did not have a primary school degree were mandated to continue at least three more years of schooling. The imperfect compliance with the school starting age or the repetition of the grade might have led to some cases not fully fitting this rule. So, students born after January 1987 were more likely to comply with the new compulsory schooling than the older cohorts.

Before 1997, the dropout rate was quite high after the completion of primary school.

[21] In addition to the standard curriculum taught in the general stream, students in religious schools were taught courses on religious subjects. The share of technical school students among junior high school students was 1.3students during the 1996-97 school year

The enrollment ratios reported by national education statistics reveal that one year before the 1997 Education Reform, almost 40% of students just dropped out of school after obtaining a primary school degree. In contrast, after the 1997 Education Reform, the number of students enrolled in grades 1-8 increased by almost 16% (Aydemir and Kirdar (2017)). The Ministry of Education responded to the jump in enrollment with various measures to prevent the non-schooling of some students because of any shortage or decline in the quality of schooling. Those measures included new school constructions, hiring of new teachers, transportation of students in rural areas, and boarding school constructions (Kirdar *et al.* (2016)).

Whether the outstanding increase in student population might have deteriorated the school quality might be a concern. The TIMSS 1999 and 2007, measuring the cognitive abilities of students across various countries in an internationally standardized way, conversely suggest that the mathematics score of Türkiye rose by 3 points while the average score among all participating countries dropped by 37 points from 1999 to 2007.[22] Similar patterns also appear in science scores. Therefore, it is quite hard to argue that the 1997 Education Reform deteriorated the quality of schooling.[23]

3 Turkish Stock Market and Pensions

Turkish financial markets have a long history, as stock market trading dates back to 1873. The Turkish Stock Market, Borsa Istanbul, ranked 20^{th} among global markets regarding the total trading volume and $1st$ in stock turnover velocity in 2021. The total market capitalization was around 150 billion dollars, although there was a dramatic depreciation of the Turkish Lira against the dollar in the following years. There are four main markets in Borsa Istanbul: the equity market, the debt securities market, the derivatives market, and the precious metals and diamonds market. Investing in financial securities is straightforward through individual transaction accounts.

Turkish pension system consists of three pillars: i) mandatory pay-as-you-go public pension system, ii) occupational mostly defined benefit (DB) pension plans, and

[22]The scale is a random variable with a mean of 500 and a standard deviation of 100. For more details, see:
https://timss.bc.edu/timss2007/pdf/timss2007_internationalmathematicsreport.pdf

[23]Following the 1997 reform, the national education curriculum introduced in 1968 remained in effect (Dulger (2004)). To accommodate the resulting increase in enrollment, the Ministry of National Education implemented substantial measures, including hiring new teachers and expanding classroom capacity by approximately 30% between 1997 and 2002 (Dulger (2004); World Bank (20005)). These investments were likely instrumental in sustaining the quality of education during the post-reform period.

iii) the voluntary private pension system with fully funded defined contribution (DC) schemes. Private pension accounts were introduced in 2003 with three different pension plans, namely voluntary, automatic, and employer-sponsored plans. We also note that in Türkiye there is no pension plan with mandatory contribution as well as defined benefit plans in private pension accounts. For automatic enrollment pension plans, while the government initiates enrollment in the DC plans for those younger than 45 working formally, opting out is a choice for investors. The benefit of signing up for pension plans or staying enrolled in DC pension plans lies in the government's extra contribution which amounts to 0.25 Turkish Lira for each Turkish Lira contributed to pension accounts by pension investors, implying a matching rate of 25%.[24] Investors are entitled to the full pension benefits once they are 56 years old with a minimum of 10 years of coverage period. At the end of 2019, 18 licensed pension firms and 26 portfolio companies existed to manage 404 different pension funds.[25]

Investors decide whether to participate or not in pension plans, how to invest, how much to invest, and how to allocate their contributions including the choice of funds unless they participate in an employer-sponsored pension plan.[26] In the voluntary pension plans, no limit exists on the amount for investment. The minimum contribution rate is 3% of the monthly wage in the automatic enrollment pension plans, but raising it is possible if investors would like to contribute more. In addition, in automatic enrollment plans, individuals choose whether to opt-out as opposed to opting in.

4 Data and Research Design

4.1 Data

We benefit from a variety of data sources. First, we use the nationally representative 2018 Türkiye Household Labor Force Survey (HLFS) assembled by the Turkish Statistical Institute (TURKSTAT). We generate several schooling outcomes. The primary measure of interest is the schooling in years. Note that we use the years of schooling and schooling in years interchangeably.[27] Additionally, we construct three indicator variables equal to

[24]The matching rate became 30% by January 22, 2022.

[25]For detailed information, see Peksevim and Akgiray (2019).

[26]In employer-sponsored pension plans, the employer manages the value in the pension accounts. Thus, in our estimations, we exclude those accounts that are only 1.5% of all pension accounts.

[27]The HLFS data has information on educational attainment but not the actual years of schooling. Therefore, we assigned 8 years of schooling for the junior high school degree, 11 years for the high school degree, 15 years for the college degree, and 17 years for the master's degree.

one if an individual has at least completed the relevant schooling category from the set of junior high school, high school, and college degrees. The relevant schooling outcomes are described in Panel A in Table B.1 in the bandwidth of those born 60 months before and after January 1987 since the analysis sample usually falls into the corresponding bandwidth.

The primary data to quantify the causal impacts of education on financial outcomes is an administrative data set provided by Borsa Istanbul Group, covering the universe of individual retirement accounts with defined contribution (DC) pension plan portfolios and non-retirement accounts with the stock market portfolios in Türkiye on December 31, 2021, and 2022, for the retirement accounts and on December 31, 2019, and 2020, for the stock market accounts, respectively. One caveat is that the stock market and pension data are from different years. This is because Borsa Istanbul Group first provided the data for individual retirement accounts and the data for stock market accounts later leading to distinct periods. However, the administrative data set is extraordinarily high quality and detailed as it has information on portfolio details, account balances, and demographic information on birth date, gender, and province of birth registration.

Borsa Istanbul Group is the legal entity keeping the records of individual retirement and stock market accounts by law in Türkiye. Thus, it is not prone to reporting or measurement bias as it has the entire population of individual retirement and stock market accounts. Moreover, almost 55% of the participants in the DC pension plans and 45% of the stock market participants are 25-44 years old, mostly overlapping our optimal bandwidth, which also rules out the concern that the population in our study is too young to invest in either the stock market or the DC pension plans. Yet, even for younger cohorts, the presence of a government match—25 Turkish Lira for every 100 Lira contributed—represents a clear financial incentive. Given that this subsidy is available at any time, one would expect it to influence saving behavior. Understanding the long-run implications of education on retirement wealth is critical since more educated individuals might find it optimal to concentrate on additional voluntary retirement contributions during their working life.

Participation in the stock market, participation in pension plans, and ownership of different assets are financial outcomes. However, the administrative records lack information about those who do not have a transaction or retirement account. Since the treatment, the 1997 Education Reform, is at the birth cohort levels in months, we study these outcomes at the birth cohort level by merging information on the number of account holders with information on the number of individuals in each month-year birth cohort

produced by TURKSTAT. That is to say, we compute the ratio of investors having a positive balance in their stock market and pension accounts in each month-year birth cohort. We then estimate the causal impacts of the 1997 Education Reform through regressions at the birth cohort level. Similarly, we calculate the ratio of participants in DC pension plans and holding equity funds in their pension portfolio. We present the summary statistics of these variables in Panel B in Table B.1.

For the variation in investors' investment behavior in the stock market and pension portfolios, we use individual-level outcomes in our estimations. In Panel C of Table B.1, we report the summary statistics of the share of wealth invested in distinct assets in stock market portfolios. To do this, we compute the share of wealth directly invested in stocks, risky assets, bonds, and funds. The administrative data set has the information on which stocks investors hold in their stock market portfolios, so we calculate the share of wealth invested in the stocks included in the BIST-30 benchmark index tracking the performance of the 30 most liquid in trading and largest companies in market capitalization. The concerning outcome is crucial to measure how much an individual refrains from risky and illiquid stocks. Moreover, it helps us to study how education determines investment in the most liquid stocks. We also document the share of wealth invested in equity funds in pension portfolios, which might be treated as a proxy for risk-taking and equity participation in pension plans.

Panel D in Table B.1 presents the logarithm of the stock market and pension wealth and the annual rate of return of the stock market and pension portfolios. To compute the rate of returns, we consider the performance of portfolios annually, which is realized returns in the retirement accounts similar to Fagereng *et al.* (2020). Due to data confidentiality, we compute the annual returns for the stock market portfolios by assuming that an investor keeps the portfolio at the beginning of the year and over the year.

Panel E in Table B.1 displays the summary statistics of the outcomes for behavioral biases and heuristics. We consider only the pension portfolios to examine the education effects on behavioral biases and heuristics for two reasons. First, the stock market data lacks the proxies for behavioral problems. Secondly, the existing literature emphasizes that behavioral biases and heuristics leading to welfare losses are more commonly observed in retirement accounts (Benartzi and Thaler (2001, 2007)). We first explore various indicator variables. Our initial focus is on whether an investor contributes to DC pension plans. The contribution is profitable, as the government also contributes with a matching rate of 25%. Later, we focus on whether an investor is sticky to the default option, i.e., the default pension fund or ownership of only the default option, one of the most commonly

observed behavioral biases in pension plans (Blumenstock *et al.* (2018).

We last analyze the share of wealth invested in default funds in percentage terms in pension portfolios and, following Huberman and Jiang (2006), we define an indicator variable equal to one if an investor tends to follow the conditional $\frac{1}{N}$ heuristic that is allocating money evenly to all pension funds while investing. Moreover, a commonly observed fact is that the inertia in portfolio choices and trading in pension plans, for instance, 401(k) plans in the US are prevalent (Agnew *et al.* (2003)). Motivated by this fact, to examine whether inertia in portfolio reshuffling or rebalancing varies with education, we construct an indicator variable equal to one if an investor buys a pension fund other than existing funds in her portfolio over the year.

The administrative data have no information about the educational attainment of investors. As the 1997 Education Reform induced a sharp increase in schooling for those born after January 1987, we mainly estimate the reduced-form impacts of the education reform. To show the association between years of schooling and participation in the stock market or pension plans, we employ the Türkiye Household Budget Surveys in 2018 and 2019.

4.2 Research Design

4.2.1 Identification

The 1997 Education Reform and the school starting age of 6 mandated those born after January 1987 to complete junior high school or 8 years of schooling. Using the cutoff of January 1987 in the birth cohorts in months, we adopt a regression discontinuity design (RD) with a running variable in the month-year of birth to establish the causal link between schooling and financial outcomes. While those born before January 1987 form the control group, those born after January 1987 form the treatment group in our research design. The identifying assumption is that other than exposure to the 1997 Education Reform there are no systematical differences between two cohorts born one month apart. Given that this assumption is satisfied, the RD design delivers a treatment assignment as good as random. In Section 4.2.2, we will perform a set of validity checks to support the relevant assumption.

In line with previous research (Oreopoulos (2006), Erten and Keskin (2018), and Aydemir *et al.* (2022)), we exploit the discontinuity in birth cohorts in months to gauge

the causal effects of education and estimate the following equation in a sharp RD design:

$$\begin{aligned} y_i &= \alpha + \beta T_i + f(x_i) + \epsilon_i \\ &\forall x_i \in (c-h, c+h) \end{aligned} \tag{1}$$

where y_i is the specific outcome variable for the month-year of birth cohort or individual i. T_i stands for the treatment status, and β is the main parameter of interest, x_i is the running variable in months, which is re-centered around zero by subtracting the month-year of birth from January 1987 that is the cutoff value determining the treatment status, and h is the bandwidth around the cutoff point of c. The slope on each side of the cutoff value varies in the RD design. $f(x_i)$ is the control function with a continuous nth-order polynomial function of the running variable on each side of the cutoff point c. In all estimations, we use the local linear approach proposed by Cattaneo *et al.* (2019) and also provide the estimates relying on a quadratic control function in the Appendix. Since local linear RD estimates are often sensitive to the choice of bandwidth, we choose it in a data-driven, and automatic way to avoid specification search and ad-hoc decisions. Thus, we implement the optimal bandwidth algorithm proposed by Calonico *et al.* (2014), which considers the conventional mean squared error optimality based on the fundamental bias-variance trade-off. For each outcome variable, we estimate the specific bandwidths separately using the optimal bandwidth algorithm proposed by Calonico *et al.* (2014). We also report in the appendix the local linear RD estimates in fixed bandwidth and the estimates with kink RD design to check whether the 1997 Education Reform changed the slope around the cutoff point of January 1987. The results are similar and not sensitive to different RD designs.

Furthermore, following Lee and Card (2008), we adjust the standard errors by clustering them at the month-year of birth to avoid any specification error concerns, as the treatment is assigned at the month-year birth level, and the running variable is discrete. All regressions include month of birth fixed effects. Whenever the unit of analysis is the individual investors, we also control the region of birth registration. The full sample regressions also control for gender.

We mainly report the reduced-form estimates for two reasons. The first is data limitation since the stock market and pension administrative data lack investor's education information. Following Angrist and Krueger (1992), it is, nonetheless, quite straightforward to calculate two-sample instrumental variable (TSIV) estimates. We report them whenever the reduced-form estimates are significant. For all outcomes, TSIV estimates

are available upon request. The second and relatively more important reason lies in the instrumental variable framework exploiting the 1997 Education Reform might not satisfy the exclusion restriction, since the financial decisions are mainly determined at the household level. Put another way, suppose that the 1997 Education Reform impacted the schooling of other household members, for instance, spouse education. Then, the impacts of education reform on financial decisions would operate not only through the education of investors but also through spousal education. Consequently, we mostly report the reduced-form estimates of the 1997 Education Reform.

4.2.2 Validity Checks

Possible manipulation of the running variable by individuals is a threat to validity. Yet, it is unlikely that individuals could manipulate their birth date as the 1997 Education Reform was executed when they were at the age of 11. To reinforce the related assertion, we provide three standard validity checks suggested by Cattaneo *et al.* (2019).

The first validity check tests whether the density of the running variable is smooth around the cutoff point. If units could manipulate their birth date, then a jump in the density of the running variable at the cutoff would be expected. The share of the Turkish population in each month-year birth cohort in Figure 1 implies that the running variable is smooth around the cutoff, and no sign of sorting appears. We also note that the numbers in each month-year birth cohort cover the full population in Türkiye in 2019 provided by TURKSTAT, suggesting that there is no concern of bias that could be caused by misreporting or measurement errors. In addition, we provide local RD estimates on whether the 1997 Education Reform impacted the fraction of the population in the birth cohorts in months in the appendix. Results show no evidence of birth date manipulation, as all point estimates are small and indistinguishable from zero.

The second validity check relies on the idea that predetermined covariates are continuous around the cutoff if the treatment is as good as random. However, data for predetermined covariates only exists for women. The Domestic Violence Against Women Surveys have information on predetermined childhood regions and mother tongue.[28] In Figure 2, we plot the binned means of the corresponding predetermined variables against the running variable in months in the optimal bandwidth. Graphical evidence indicates no significant differences in the predetermined covariates by the 1997 Education Reform. We also report local RD estimates consistent with the graphical evidence in the Appendix

[28]For more information, see Erten and Keskin (2018)

Table B.4. Results indicate no overall significant evidence that those predetermined covariates are discontinuous at the cutoff as all point estimates are small and most are close to zero.

5 Results

5.1 Schooling Outcomes

We first plot the binned means of years of schooling in panel A and the completion of the junior high school in panel B against the running variable in months in Figure 3. In all graphs, we restrict the sample to those whose birth dates in months overlap with the optimal bandwidth. Panel A in Figure 3 reveals years of schooling jump discontinuously at the cutoff for all samples. Panel B in Figure 3 illustrates a marked increase in the percentage of those with a junior high school degree. These graphs all together reinforce that the 1997 Education Reform increased the likelihood of having at least a junior high school degree by 10-15% and an increase in years of schooling by 0.5-1 year. We next document the local linear RD estimates to quantify the causal impacts more parsimoniously.

Column 1 in Table 1 presents that the reform-induced increase in years of schooling is 0.45, 0.38, and 0.44 for the full, male, and female samples, respectively.[29] As the 1997 Education Reform made junior high school completion compulsory, column 2 reveals a large and precise increase in the fraction of those who completed junior high school education. The 1997 Education Reform encouraged those born after January 1987 to complete high school in all samples. Females born after January 1987 are 10% more likely to complete high school relative to the mean of the control group. That pattern is even more amplified for a college degree. Relative to the control group mean, the 1997 Education Reform promoted having at least a college degree by more than 10% in the full sample and around 14% in the female sample. Thus, the 1997 Education Reform with its spillover effects went beyond its primary purpose, inducing those born after January 1987 to complete education beyond junior high school.[30] The estimates are also in line with the previous studies examining the impact of the 1997 Education Reform on schooling outcomes.[31]

[29]Our findings by gender are consistent with the previous studies by Aydemir and Kirdar (2017), Aydemir *et al.* (2022), and Baltagi *et al.* (2019) showing significant positive effects for men and women.

[30]Figure A.1 in the Appendix illustrates the jump in the high school and college education

[31]Aydemir *et al.* (2022), Erten and Keskin (2018), and Gulesci *et al.* (2019)

The 1997 Education Reform right-shifted the entire distribution of years of schooling for those born after January 1987 as shown in Figure 4. The findings on high school and college education are noteworthy in at least one respect. One might claim that education reform only promoted the propensity to hold at least a junior high school degree, which might not be enough for schooling to drive the financial outcomes that supposedly require a higher level of education. Yet, it seems unlikely that this is a valid concern in our setting as the education reform shifted the whole distribution of schooling.

We also perform a further validity check through artificial or placebo cutoffs. Absent the treatment, there would not be abrupt changes or jumps around the placebo cutoffs. We perform estimations with two placebo cutoffs, January 1980 and January 1994. No economically and statistically significant treatment effects in Figure 5 on schooling outcomes appear in the placebo cut-offs, while there is a substantial increase at the true cutoff of January 1987. We further report the local RD estimates for years of schooling and the propensity to have at least a junior high school degree in the Appendix Table B.5. Estimates with placebo cutoffs are small and indistinguishable from zero, verifying that the 1997 Education Reform changed the schooling landscape in Türkiye.

We also note that in the HLFS data, for around 5% of the observations, the month of birth is missing. That might be a threat to the validity of estimates. To address this, we estimate the impacts of the 1997 Education Reform on attrition in the month of birth by two different empirical exercises. First, we use the year of birth as the running variable. However, since our running variable is in years, the number of data points is small and this might violate the requirements of the continuity-based RD approach. Thus, following Cattaneo *et al.* (2019), we employ a local randomization RD design to examine the causal impacts of the 1997 Education Reform on schooling outcomes in the closest window, i.e., one year, around the cutoff on each side.[32] In the Appendix Table B.6, point estimates with local randomization RD design are very similar to the local RD estimates with a continuity-based approach.

Using the local randomization RD design, we also assess whether the 1997 Education Reform causes the attrition discussed above. For different window lengths ranging between one to 6-year length, point estimates reveal that the attrition in the month of birth is orthogonal to the education reform as displayed in the appendix. In sum, we conclude that the 1997 Education Reform significantly increased schooling in Türkiye, equipping

[32]Local randomization RD design is based on the following procedure. It considers the closest window around the cutoff and then implements the Fisherian randomization by testing the null hypothesis of no treatment effect. For more details, see Cattaneo *et al.* (2019).

us with a statistically powerful natural experiment to shed light on how general education affects financial outcomes.

5.2 Financial Outcomes

5.2.1 Financial Participation and Asset Ownership

We begin our analysis by providing graphical evidence to show the statistical association between years of schooling and financial participation. To do this, we treat stock market participation as the primary indicator of financial participation. The administrative data has no information on the educational attainments of investors. Yet, the 2018-19 Household Budget Surveys have information on educational attainment and stock market participation. We plot the propensity to participate in the stock market and years of schooling in Figure 6. The concerning graph exhibits a robust positive correlation in our study sample. We restricted our sample to 39 months around January 1987 as the optimal bandwidth falls into the corresponding interval.

We subsequently report the OLS estimates in Table 2, demonstrating that an additional year of schooling promotes stock market participation by 2.7% in the sample spanning those born 39 months before or after January 1987. Nonetheless, as Figure 6 illustrates the stock market participation rate is nonlinear in years of schooling. Moreover, we observe that the education reform shifted right the whole distribution of schooling in Türkiye. The shift in educational attainment induced by education reform makes the comparison of the OLS coefficient resulting from regressing the propensity to participate in the stock market in years of schooling with the causal estimates challenging. Thus, to have a comparable benchmark OLS estimate relative to the causal estimate, we estimate an OLS coefficient adapting a different functional form mimicking the shift in the distribution of schooling induced by the education reform.

To do this, we first regress the stock market participation dummy on indicator variables for each educational degree where the reference category is those who completed primary school at most. OLS estimates in column 2 in Table 2 reveal that the higher the educational attainment, the greater the participation in the stock market. Next, we multiply each OLS coefficient with the local RD point estimate for the corresponding increase for each degree induced by the education reform. Then, we sum up these products, which allows us to have a benchmark OLS estimate to compare with the causal estimate. Assuming that the covariances are zero across these products, we also calculate the standard errors of the point estimate. Column 2 in Table 2 presents that the benchmark OLS

coefficient is 1.2%, implying that the shift in the distribution of educational attainment induced by the education reform is positively correlated with the propensity to participate in the stock market. However, those OLS estimates are prone to bias. Thus, we continue our analysis with causal estimates. Panel A in Figure 7 indicates that the causal impacts are not as large as the OLS estimates for the stock market participation. The upward jump in pension plans in Panel B does not even reveal a significant change as revealed by smaller jumps and larger confidence intervals.

We present the local linear RD estimates in Table 3. The coefficients are close to half a percent. The corresponding TSIV estimates imply that an extra year of schooling increases formal participation, proxied by having a transaction account, by around 1%. The point estimate in column 2 shows that the 1997 Education Reform increases the share of stock market participants by 0.175 percentage points (pp) for the full sample, 0.233 for the male sample, and 0.123 for the female sample. All of these point estimates are statistically significant. However, a simple comparison of the local linear RD coefficients with the OLS coefficients reveals that the OLS estimates are upward biased. Even if we consider the TSIV estimates by dividing the relevant point estimates by the reform-induced increase in years of schooling, the contribution of an additional year of schooling to the stock market participation is less than half a percentage point.

We next document the effects of the education reform on risky asset ownership, which denotes the ownership of any stock market assets except money market funds which might be treated as risk-free. In column 4, point estimates are precisely estimated for the full and male samples, but imprecise for the female sample. Put another way, the results demonstrate that those with more education are more likely to hold risky assets in their stock market portfolio but the magnitude is negligible. Consistently, we focus on the propensity to have liquid stock, bond, and fund in columns 5, 6, and 7, respectively. The impacts are small and indistinguishable from zero. Column 8 indicates that the 1997 Education Reform does not improve the take-up of DC pension plans as the point estimates are both small and indistinguishable from zero. The last column reports the point estimates for equity fund ownership through pension plans, an indirect way of equity participation. Despite the precision, the estimates are small in magnitude. In general, we fail to find significant evidence that financial participation and asset accumulation vary by education.

After examining the financial outcomes at the extensive margin, we analyze whether schooling affects savings in retirement accounts and stock market accounts. Since savings are conditional on participation, we employ the administrative data covering the universe

of the stock market and pension investors, so the unit of analysis is individual investors. Column 1 in Table 4 displays the local linear RD estimates of the 1997 Education Reform on the size of the stock market portfolio, i.e., the stock market wealth. The point estimates are neither large nor precise. Column 2 uncovers that those born after January 1987 accumulate more pension wealth, but a significant heterogeneity arises. The point estimate is 0.015 for the full population, implying that the 1997 Education Reform led to higher wealth in DC pension plans by 1.5%. Despite the null effects for the male population, the gradient of the education reform for the female population is 0.027, an increase of 2.7%.

5.2.2 Portfolio Choices and Return Performance

We proceed with the outcomes of the share of wealth invested in distinct assets in the stock market and pension portfolios separately. We remind that the unit of analysis is individuals, and we employ individual-level administrative data. We begin presenting the local linear RD estimates of the education reform on the share of wealth allocated to stocks in the stock market portfolios in column 1 in Table 5. The coefficient of education reform is insignificant and close to zero for the whole population as well as for males and females.

Next, we report the estimates for the share of wealth invested in risky assets, i.e. the risky share. Results indicate that education has no significant effect on the risky share in all groups. In the remaining columns, we document the causal impacts on the share of wealth invested in bonds, funds, and liquid stocks in stock market portfolios. Yet, for all outcomes, results show no significant effects of the education reform. The last column presents the point estimates of the education reform on the share of wealth invested in equity funds in pension portfolios, a proxy for the risky share in pension portfolios. The estimates are small in magnitude, revealing no significant effect. Altogether, we fail to find any causal evidence that education is a driver of the variation in investors' portfolios regarding the share of wealth invested in different assets.

In Table 6, we present the causal impacts of the 1997 Education Reform on portfolio returns. The first column reports the education effects on the stock market portfolio returns, while the second column documents the local RD estimates for the pension portfolio returns. No economically significant evidence appears that schooling increases the stock market and pension portfolio returns, so it turns out that education does not seem to contribute to portfolio performance in financial markets.

5.2.3 Behavioral Biases and Heuristics

We explore pension portfolios because prior research suggests behavioral biases and heuristic behavior are most commonly observed in pension plans (Benartzi and Thaler (2007)). We estimate the impacts of the 1997 Education Reform on some prevalent behavioral biases and heuristics in DC pension plans documented by Benartzi and Thaler (2007) such as non-participation, inadequate contribution, stickiness to default fund or not exercising the right to choose, naive diversification strategies, i.e., the conditional $\frac{1}{N}$ heuristic that evenly allocates money across funds while investing, and lack of portfolio reshuffling or rebalancing.

We have already shown that the impacts of schooling on participation in pension plans are minimal. In line with this, column 1 in Table 7 shows that the share of those contributing to pension plans does not improve by education, since the point estimates are small despite the statistical precision. Consistently, columns 2 and 3 document that the 1997 Education Reform does not depress the propensity to own the default pension fund, and to hold only the default fund, implying that education does not seem to be a remedy against stickiness to the default option. Column 4 documents that the 1997 Education Reform lowers the share of default funds in pension portfolios, but the point estimate in magnitude is small. Columns 5 and 6 also show that schooling is not a factor in avoiding the conditional $\frac{1}{N}$ naive diversification strategy and the lack of portfolio reshuffling. Overall, results indicate that schooling is not a significant factor in mitigating the behavioral biases and heuristics common in retirement accounts.

5.3 Mechanisms Driving the Wealth Effect in DC Plans

Our results indicate that general education does not promote financial participation in the stock market or pension plans. Moreover, it does not causally influence portfolio allocation, as more educated individuals do not invest a larger share in risky assets, stocks, or equity funds in pension portfolios. Education also does not significantly reduce behavioral biases or heuristics in pension plans or lead to higher portfolio returns. Despite these findings, we find that general education increases accumulated wealth in DC pension plans, with notable gender heterogeneity, primarily benefiting females. This leads to the question of what mechanism drives the relationship between education and higher pension wealth or increased retirement savings.

We examine several potential mechanisms. First, we analyze the first enrollment type in DC plans—whether individuals join for the first time through voluntary, automatic,

or employer-sponsored enrollment. While we find a positive and significant effect of education reform on voluntary enrollment, as indicated in column 1 in Table 8, the effect size is very small relative to the control mean. Yet, this might be a sign that individuals with more education— affected by the 1997 Education Reform— perhaps tend to be more prepared for the retirement.

Education is also likely to reduce informality and promote formal employment, as documented by Bleakley and Gupta (2020). In Türkiye, the automatic enrollment policy mandates formal workers to join pension plans. Thus, increased formal employment could drive higher pension wealth through automatic enrollment. However, the point estimates in column 2 in Table 8 are too small and imprecise, suggesting that formal employment is not a significant channel behind our findings.

Column 3 of Table 8, on the other hand, shows that first-time participation in DC pension plans through employer-sponsored plans is substantially higher for females affected by the education reform, relative to the control mean. This indicates that higher education increases the likelihood of employment in firms offering better contracts.

Next, we analyze several auxiliary datasets to examine how education reform influences financial skills, cognitive abilities, and earnings—other potential channels through which education may enhance retirement savings. To assess financial skills, we use the 2018 Survey on the Financial Perceptions and Attitudes of Turkish Households (SF-PATH). Since this dataset lacks birth month information, we use birth year as the running variable. This results in a limited number of data points, potentially violating the continuity-based RD design requirements. Following Cattaneo *et al.* (2019), we instead apply a local randomization regression discontinuity approach with alternative bandwidths of three and four years around the cutoff.

The outcomes examined include standardized trust index, standardized financial knowledge index, the probability of peers affecting the financial decisions, the likelihood of getting financial advice from financial institutions, the indicator for willingness to take risk, and the summary index accounting for the former variables, similar to Gomes *et al.* (2021). Figure 8 presents the results for the full sample, as well as for males and females separately.[33] We find no robust evidence that the reform improves financial skills, aligning with our findings of no significant effects on behavioral biases, heuristics, or portfolio returns. In particular, the null effects on willingness to take risks suggest that, although

[33]The corresponding coefficient estimates for Figure 8 are provided in Online Appendix Table B.9. Additionally, Panel A of Table 8 offers suggestive evidence on the reform's effect on high school completion.

education increases financial wealth for females through pension plans, it does not lead to changes in their risk-taking behavior. Thus, in our setting, greater wealth is unlikely to be a driving force for taking more risks.

To assess the impact of the education reform on cognitive skills, we use data from the 2014 Programme for the International Assessment of Adult Competencies (PIAAC). Like the SFPATH dataset, PIAAC lacks birth month information. Therefore, we apply a local randomization RD approach using birth years and present estimates for alternative bandwidths. The outcomes examined are standardized literacy and numeracy scores. Figure 9, reports results for the full sample, as well as for males and females separately, providing suggestive evidence of cognitive skill improvements among females.[34]

Finally, we use the 2018 HLFS data and a standard RD design based on birth months to estimate the education reform's impact on earnings. Column 1 of Table 9 shows a significant wage increase for women, while the estimates for men are small and imprecise. This aligns with prior findings from Türkiye, which indicate low labor market returns to schooling for males but higher returns for females under the same reform (Aydemir and Kirdar (2017)).[35] We also examine the reform's effect on the likelihood of working in a high-paying occupation, a low-paying occupation, and employment in a large firm, with results reported in columns 2, 3, and 4 of Table 9. The findings suggest that the reform increases the probability of women working in large firms while reducing their likelihood of being in low-paying occupations.

In sum, our findings suggest that increased wealth in DC pension plans is primarily driven by labor market effects, where the 1997 Education Reform leads to higher earnings and greater employment in firms offering employer-sponsored pension plans. In contrast, we find little evidence that education-induced improvements in financial decision-making play a significant role.

6 Robustness Checks

Participation bias, extensively discussed in the literature, might be a threat to validity (Lee (2009)). In other words, portfolio outcomes are only observed for those who participate in the stock market and pension plans. Figuring out whether the 1997 Edu-

[34]In the Online Appendix Panel B of Table B.8 confirms a statistically strong first stage. Online Appendix Table B.10 reports the corresponding coefficient estimates for Figure 9

[35]The large wage effect for women suggests that fixed participation costs are unlikely to be the main factor behind the null effects of education on financial participation.

cation Reform changes portfolio outcomes through different channels rather than solely by urging participation in pension plans is crucial. However, the 1997 Education Reform does not significantly affect stock market participation, as the point estimates are small. Moreover, it is indistinguishable from zero for participation in pension plans. Lee (2009) suggests that if treatment has no effects on participation, then participation rates are similar between control and treatment groups, so estimates comparing control and treatment groups are valid. Thus, the participation bias does not seem to be an issue in the estimates.

We also adapt the strategy proposed by Duflo (2001) to address the issue of participation bias, testing the stability of estimates by adding the quadratic polynomials of participation rates in each month-year birth cohort to the regressions as controls. Moreover, the administrative data allows us to compute the true participation rates in each month-year birth cohort, which enables us to assess whether exact participation rates alter estimates. Correspondingly, we add the participation rates in a quadratic polynomial form to regressions and check whether the estimates are robust to their inclusion. For brevity, we present the concerning estimations in the Appendix Table B.11. The results show that the point estimates are not sensitive to the inclusion of participation rates in a quadratic polynomial form. Overall, the participation bias does not seem to drive our results.

7 Conclusion

Our study examines the causal effects of education on financial outcomes, including stock market and DC pension plan participation, wealth, and investment decisions in the stock market and DC pension portfolios. To identify these effects, we leverage the exogenous variation generated by the 1997 Education Reform in Türkiye, which extended compulsory schooling from 5 to 8 years for individuals born after January 1987. The reform increased schooling by nearly half a year on average, with significant heterogeneity by gender, showing larger effects for females.

Using administrative data sets covering the universe of individuals with transaction accounts and DC retirement accounts in Türkiye, we find no significant evidence that general education drives financial participation through the stock market or DC pension plans, despite a strong positive correlation between education and financial participation. This suggests that the observed association is not causal. We further examine the impact of education on the ownership of stock market instruments—such as stocks, funds, bonds,

risky assets, and liquid stocks—as well as the likelihood of having a transaction account. However, the estimates show no significant effects of general education on these outcomes.

We also investigate the causal effects of general education on portfolio composition, analyzing the share of wealth allocated to risky assets, stocks, bonds, funds, and liquid stocks in stock market portfolios, as well as equity fund investments in pension portfolios. The estimates for these outcomes are small and imprecise. Additionally, our findings indicate that general education does not enhance portfolio performance. For the first time in the literature, we assess whether general education mitigates behavioral biases and heuristics in pension plans, yet our results suggest no causal impact. Overall, general education does not appear to be a key determinant of investment behavior.

We find strong evidence that schooling contributes to higher pension wealth or savings in DC plans, with notable gender heterogeneity. The 1997 Education Reform leads to approximately 3% greater pension wealth for females, while two-sample instrumental variable estimates suggest that an additional year of schooling increases pension wealth by 5–6% for women but has no effect for men. To understand the underlying mechanisms, we examine the reform's impact on first enrollment type in DC plans, financial skills, cognitive skills, and wages. Our findings indicate that education enhances pension wealth primarily through labor market channels, including higher earnings and increased employment in firms offering employer-sponsored pension plans. In contrast, we find little evidence that improvements in financial decision-making significantly contribute to this effect.

Prior research consistently finds a strong positive correlation between education and financial behavior. However, this relationship may not be causal, as it could be driven by confounding factors such as genetic traits and family background. While general education plays a crucial role in developing labor market skills, its impact on financial market skills may be more limited. This suggests that integrating financial education into the general curriculum could be a valuable policy approach.

References

Acemoglu, D. and Angrist, J. (2000). How large are human-capital externalities? evidence from compulsory schooling laws. *NBER Macroeconomics Annual*, **15**, 9–59.

Agnew, J., Balduzzi, P. and Sundén, A. (2003). Portfolio choice and trading in a large 401(k) plan. *American Economic Review*, **93** (1), 193–215.

Agnew, J. R. (2006). Do behavioral biases vary across individuals? evidence from individual level 401(k) data. *Journal of Financial and Quantitative Analysis*, **41** (4), 939–962.

Akyol, P. and Kirdar, M. G. (2022). Compulsory schooling reform and intimate partner violence in turkey. *European Economic Review*, **150**, 104313.

Angrist, J. D. and Keueger, A. B. (1991). Does Compulsory School Attendance Affect Schooling and Earnings? *The Quarterly Journal of Economics*, **106** (4), 979–1014.

— and Krueger, A. B. (1992). The effect of age at school entry on educational attainment: An application of instrumental variables with moments from two samples. *Journal of the American Statistical Association*, **87** (418), 328–336.

Arcanjo, M. (2019). Retirement pension reforms in six european social insurance schemes between 2000 and 2017: More financial sustainability and more gender inequality? *Social Policy and Society*, **18** (4), 501–515.

Atilgan, G., Aytekin, E. A., Ozan, E. D., Saraçoğlu, C., Şener, M., Uslu, A. and Yeşilbağ, M. (2015). *Osmanlı'dan günümüze Türkiye'de siyasal hayat*. Yordam Kitap.

Aydemir, A. and Kirdar, M. G. (2017). Low wage returns to schooling in a developing country: Evidence from a major policy reform in turkey. *Oxford Bulletin of Economics and Statistics*, **79** (6), 1046–1086.

Aydemir, A. B. (2021). *The impact of education on savings and financial behavior*. Tech. rep., Technical Report.

—, Kirdar, M. G. and Torun, H. (2022). The effect of education on internal migration of young men and women: incidence, timing, and type of migration. *Labour Economics*, **74**, 102098.

Baltagi, B. H., Flores-Lagunes, A. and Karatas, H. M. (2019). The effect of education on health: Evidence from the 1997 compulsory schooling reform in turkey. *Regional Science and Urban Economics*, **77**, 205–221.

Benartzi, S. and Thaler, R. (2007). Heuristics and biases in retirement savings behavior. *Journal of Economic Perspectives*, **21** (3), 81–104.

— and Thaler, R. H. (2001). Naive diversification strategies in defined contribution saving plans. *American Economic Review*, **91** (1), 79–98.

Beshears, J., Choi, J. J., Laibson, D. and Madrian, B. C. (2009). The importance of default options for retirement saving outcomes: Evidence from the united states. In *Social security policy in a changing environment*, University of Chicago Press, pp. 167–195.

Black, S. E., Devereux, P. J., Lundborg, P. and Majlesi, K. (2018). Learning to Take Risks? The Effect of Education on Risk-Taking in Financial Markets. *Review of Finance*, **22** (3), 951–975.

—, — and Salvanes, K. G. (2011). Too Young to Leave the Nest? The Effects of School Starting Age. *The Review of Economics and Statistics*, **93** (2), 455–467.

Bleakley, H. and Gupta, B. (2020). Mind the gap: Schooling, informality and fiscal externalities in nepal. *Informality and Fiscal Externalities in Nepal*.

Blumenstock, J., Callen, M. and Ghani, T. (2018). Why do defaults affect behavior? experimental evidence from afghanistan. *American Economic Review*, **108** (10), 2868–2901.

Bodie, Z., Merton, R. C. and Samuelson, W. F. (1992). Labor supply flexibility and portfolio choice in a life cycle model. *Journal of Economic Dynamics and Control*, **16** (3), 427–449.

Brune, L., Giné, X., Goldberg, J. and Yang, D. (2017). Savings defaults and payment delays for cash transfers: Field experimental evidence from malawi. *Journal of Development Economics*, **129**, 1–13.

Calonico, S., Cattaneo, M. D. and Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, **82** (6), 2295–2326.

Calvet, L., Campbell, J. and Sodini, P. (2007). Down or out: Assessing the welfare costs of household investment mistakes. *Journal of Political Economy*, **115** (5), 707–747.

Calvet, L. E., Campbell, J. Y. and Sodini, P. (2009a). Fight or Flight? Portfolio Rebalancing by Individual Investors. *The Quarterly Journal of Economics*, **124** (1), 301–348.

—, — and — (2009b). Measuring the financial sophistication of households. *American Economic Review*, **99** (2), 393–98.

— and Sodini, P. (2014). Twin picks: Disentangling the determinants of risk-taking in household portfolios. *The Journal of Finance*, **69** (2), 867–906.

Carlsson, M., Dahl, G. B., Öckert, B. and Rooth, D.-O. (2015). The Effect of Schooling on Cognitive Skills. *The Review of Economics and Statistics*, **97** (3), 533–547.

Cattaneo, M. D., Idrobo, N. and Titiunik, R. (2019). *A practical introduction to regression discontinuity designs: Foundations*. Cambridge University Press.

Chetty, R. (2015). Behavioral economics and public policy: A pragmatic perspective. *American Economic Review*, **105** (5), 1–33.

—, Friedman, J. N., Leth-Petersen, S., Nielsen, T. H. and Olsen, T. (2014). Active vs. Passive Decisions and Crowd-Out in Retirement Savings Accounts: Evidence from Denmark. *The Quarterly Journal of Economics*, **129** (3), 1141–1219.

Choi, J. J., Laibson, D. I. and Madrian, B. C. (2005). Are empowerment and education enough? underdiversification in 401 (k) plans. *Brookings Papers on Economic Activity*, **2005** (2), 151–213.

Cocco, J. F., Gomes, F. J. and Maenhout, P. J. (2005). Consumption and Portfolio Choice over the Life Cycle. *The Review of Financial Studies*, **18** (2), 491–533.

Cole, S., Paulson, A. and Shastry, G. K. (2014). Smart Money? The Effect of Education on Financial Outcomes. *The Review of Financial Studies*, **27** (7), 2022–2051.

Cronqvist, H. and Thaler, R. H. (2004). Design choices in privatized social-security systems: Learning from the swedish experience. *American Economic Review*, **94** (2), 424–428.

Devereux, P. J. and Hart, R. A. (2010). Forced to be Rich? Returns to Compulsory Schooling in Britain. *The Economic Journal*, **120** (549), 1345–1364.

Dohmen, T., Falk, A., Huffman, D., Sunde, U., Schupp, J. and Wagner, G. G. (2011). Individual risk attitudes: Measurement, determinants, and behavioral consequences. *Journal of the European Economic Association*, **9** (3), 522–550.

Duflo, E. (2001). Schooling and labor market consequences of school construction in indonesia: Evidence from an unusual policy experiment. *American Economic Review*, **91** (4), 795–813.

—, Gale, W., Liebman, J., Orszag, P. and Saez, E. (2006). Saving Incentives for Low- and Middle-Income Families: Evidence from a Field Experiment with H amp;R Block. *The Quarterly Journal of Economics*, **121** (4), 1311–1346.

— and Saez, E. (2002). Participation and investment decisions in a retirement plan: the influence of colleagues' choices. *Journal of Public Economics*, **85** (1), 121–148.

— and — (2003). The Role of Information and Social Interactions in Retirement Plan Decisions: Evidence from a Randomized Experiment*. *The Quarterly Journal of Economics*, **118** (3), 815–842.

Dulger, I. (2004). Case study on turkey: Rapid coverage for compulsory education program. in conference on scaling up poverty reduction, shanghai, china.

Egan, M. L., MacKay, A. and Yang, H. (2021). *What Drives Variation in Investor Portfolios? Evidence from Retirement Plans*. Working Paper 29604, National Bureau of Economic Research.

Engström, S. and Westerberg, A. (2003). Which individuals make active investment decisions in the new swedish pension system? *Journal of Pension Economics and Finance*, **2** (3), 225–245.

Erten, B. and Keskin, P. (2018). For better or for worse?: Education and the prevalence of domestic violence in turkey. *American Economic Journal: Applied Economics*, **10** (1), 64–105.

Fagereng, A., Gottlieb, C. and Guiso, L. (2017). Asset market participation and portfolio choice over the life-cycle. *The Journal of Finance*, **72** (2), 705–750.

—, Guiso, L., Holm, M. B. and Pistaferri, L. (2020). K-returns to education. *Available at SSRN 3526034*.

Gomes, F., Haliassos, M. and Ramadorai, T. (2021). Household finance. *Journal of Economic Literature*, **59** (3), 919–1000.

Gray, D., Montagnoli, A. and Moro, M. (2021). Does education improve financial behaviors? quasi-experimental evidence from britain. *Journal of Economic Behavior Organization*, **183**, 481–507.

Guiso, L. and Sodini, P. (2013). Household finance: An emerging field. In *Handbook of the Economics of Finance*, vol. 2, Elsevier, pp. 1397–1532.

Gulesci, S., Meyersson, E. and Trommlerová, S. K. (2019). The Effect of Compulsory Schooling Expansion on Mothers' Attitudes Toward Domestic Violence in Turkey. *The World Bank Economic Review*, **34** (2), 464–484.

Hanushek, E. A. and Woessmann, L. (2008). The role of cognitive skills in economic development. *Journal of Economic Literature*, **46** (3), 607–68.

Huberman, G. and Jiang, W. (2006). Offering versus choice in 401(k) plans: Equity exposure and number of funds. *The Journal of Finance*, **61** (2), 763–801.

Ichino, A. and Winter-Ebmer, R. (1999). Lower and upper bounds of returns to schooling: An exercise in iv estimation with different instruments. *European Economic Review*, **43** (4), 889–901.

ILO (2021). *World Social Protection Report 2020–22: Social Protection at the Crossroads – in Pursuit of a Better Future*. Tech. rep., ILO.

Iyengar, S. S. and Kamenica, E. (2010). Choice proliferation, simplicity seeking, and asset allocation. *Journal of Public Economics*, **94** (7), 530–539.

Jung, S. (2015). Does education affect risk aversion? evidence from the british education reform. *Applied Economics*, **47** (28), 2924–2938.

Kirdar, M. G., Dayioğlu, M. and Koç, (2016). Does longer compulsory education equalize schooling by gender and rural/urban residence? *World Bank Economic Review*, **30** (3), 549–579.

Kling, J. R., Liebman, J. B. and Katz, L. F. (2007). Experimental analysis of neighborhood effects. *Econometrica*, **75** (1), 83–119.

Lee, D. S. (2009). Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects. *The Review of Economic Studies*, **76** (3), 1071–1102.

— and Card, D. (2008). Regression discontinuity inference with specification error. *Journal of Econometrics*, **142** (2), 655–674, the regression discontinuity design: Theory and applications.

Lleras-Muney, A. (2005). The Relationship Between Education and Adult Mortality in the United States. *The Review of Economic Studies*, **72** (1), 189–221.

Lochner, L. and Moretti, E. (2004). The effect of education on crime: Evidence from prison inmates, arrests, and self-reports. *American Economic Review*, **94** (1), 155–189.

Lusardi, A. and Mitchell, O. S. (2014). The economic importance of financial literacy: Theory and evidence. *Journal of Economic Literature*, **52** (1), 5–44.

Madrian, B. C. and Shea, D. F. (2001). The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior. *The Quarterly Journal of Economics*, **116** (4), 1149–1187.

Morrissey, M. (2016). The state of american retirement. *Economic Policy Institute, Washington, DC*.

OECD (2019). *Pensions at a Glance 2019: OECD and G20 Indicators*. Tech. rep., OECD.

— (2021). *Pensions at a Glance 2021: OECD and G20 Indicators*. Tech. rep., OECD.

Oreopoulos, P. (2006). Estimating average and local average treatment effects of education when compulsory schooling laws really matter. *American Economic Review*, **96** (1), 152–175.

— and Salvanes, K. G. (2011). Priceless: The nonpecuniary benefits of schooling. *Journal of Economic Perspectives*, **25** (1), 159–84.

Peksevim, S. and Akgiray, V. (2019). *Reforming the Pension System in Turkey: Comparison of Mandatory and Auto-Enrolment Pension Systems in Selected OECD Countries*. Tech. rep., OECD.

Sialm, C., Starks, L. T. and Zhang, H. (2015). Defined contribution pension plans: Sticky or discerning money? *The Journal of Finance*, **70** (2), 805–838.

Sutter, M., Weyland, M., Untertrifaller, A. and Froitzheim, M. (2020). Financial literacy, risk and time preferences-results from a randomized educational intervention.

Tawiah, B. B. (2022). Does education have an impact on patience and risk willingness? *Applied Economics*, **54** (58), 6687–6702.

Thaler, R. and Benartzi, S. (2004). Save more tomorrow™: Using behavioral economics to increase employee saving. *Journal of Political Economy*, **112** (S1), S164–S187.

Viceira, L. M. (2001). Optimal portfolio choice for long-horizon investors with nontradable labor income. *The Journal of Finance*, **56** (2), 433–470.

Vissing-Jørgensen, A. (2002). Limited asset market participation and the elasticity of intertemporal substitution. *Journal of Political Economy*, **110** (4), 825–853.

World Bank (20005). *Turkey—Education Sector Study: Sustainable Pathways to an Effective, Equitable and Efficient Education System for Preschool through Secondary School Education*. Report, World Bank, Washington, DC.

8 Tables and Figures

Tables

Table 1: Education Reform vs Schooling Outcomes

	(1) Schooling (in years)	(2) Junior High School	(3) High School	(4) College
		Full		
Education Reform	0.451***	0.055***	0.043***	0.031**
	(0.105)	(0.010)	(0.012)	(0.014)
Control Mean	9.25	0.68	0.50	0.28
Bandwidth	57.21	30.82	37.35	34.20
Observations	59347	31744	39279	35837
		Male		
Education Reform	0.381***	0.042***	0.038**	0.025
	(0.148)	(0.015)	(0.019)	(0.017)
Control Mean	9.96	0.76	0.57	0.30
Bandwidth	51.41	35.32	42.65	37.25
Observations	25664	17678	21301	18902
		Female		
Education Reform	0.448**	0.081***	0.049**	0.035*
	(0.210)	(0.021)	(0.023)	(0.019)
Control Mean	8.56	0.57	0.44	0.26
Bandwidth	49.82	40.32	39.94	38.87
Observations	26722	21889	21311	20820

Notes: Local linear RD estimates in all columns. All columns use data from the 2018 Household Labor Force Survey by TURKSTAT. The unit of analysis is individuals. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. In column 1, the outcome is years of schooling. In column 2, the outcome is a dummy variable equal to one if an individual has at least a junior high school degree. In column 3, the outcome is a dummy variable equal to one if an individual has at least a high school degree. In column 4, the outcome is a dummy variable equal to one if an individual has at least a college degree. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects and the regressions in the full sample additionally include controls for gender. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2: Education vs Stock Market Participation

	(1)	(2)
	Stock Market Participant	
Schooling (in years)	0.027***	
	(0.001)	
Junior High School		0.032***
		(0.011)
High School		0.103***
		(0.013)
College		0.175***
		(0.016)
Weighted $\hat{\beta}$		0.012***
		(0.003)
Control Mean	0.19	0.19
Observations	6905	6905

Notes: OLS estimates in all columns. All columns use data from Household Budget Surveys of 2018-19 by TURKSTAT. The sample includes those born 39 months before or after January 1987 since the optimal bandwidth in the RD design is 39 months for the outcome of stock market participation. The unit of analysis is individuals. In all columns, the outcome is a dummy variable equal to one if the individual participates in the stock market. In the first column, the explanatory variable is years of schooling. In the second column, the explanatory variables are indicator variables for junior high school, high school, and college degrees, respectively. The reference category is the primary school degree at most. Weighted $\hat{\beta}$ is the weighted average of the point estimates of the indicator variables for the indicator variables of degrees in the second column regarding the shift induced by the Education Reform in degrees. All regressions include controls for gender with a dummy variable of being female and year of survey fixed effects for each survey year. The control mean displays the mean of the corresponding outcome in the control group. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 3: Education Reform vs Stock Market Participation and Asset Ownership

	(1) Transaction Account	(2) Stock Market	(3) Direct Stock	(4) Risky Asset	(5) Liquid Stock	(6) Bond	(7) Fund	(8) Pension Fund	(9) Equity Fund
					Full				
Education Reform	0.445***	0.175***	0.131***	0.113***	0.060***	0.004	0.040	0.118	0.077***
	(0.109)	(0.054)	(0.036)	(0.041)	(0.022)	(0.003)	(0.027)	(0.087)	(0.023)
Control Mean	55.47	8.94	5.27	6.15	2.29	0.10	2.94	24.75	2.81
Bandwidth	30.40	39.10	52.24	42.10	65.90	56.13	39.14	35.05	39.64
Observations	61	79	105	85	131	113	79	71	79
					Male				
Education Reform	0.388***	0.233***	0.192***	0.176***	0.059	0.013***	0.050	0.077	0.089***
	(0.089)	(0.064)	(0.054)	(0.057)	(0.039)	(0.005)	(0.037)	(0.119)	(0.034)
Control Mean	72.12	13.07	8.02	9.22	3.46	0.14	4.18	29.82	3.33
Bandwidth	27.60	41.62	54.85	47.95	46.59	71.28	40.38	26.05	45.12
Observations	55	83	109	95	93	143	81	53	91
					Female				
Education Reform	0.472***	0.123**	0.064*	0.055	0.040**	-0.013***	0.027	0.205**	0.071***
	(0.146)	(0.049)	(0.037)	(0.040)	(0.018)	(0.003)	(0.024)	(0.105)	(0.024)
Control Mean	38.19	4.80	2.43	3.02	0.99	0.06	1.69	19.46	2.36
Bandwidth	30.73	43.66	41.94	42.58	48.85	42.53	42.34	37.59	48.16
Observations	61	87	83	85	97	85	85	75	97

Notes: Local linear RD estimates in all columns. All columns use data from population numbers assembled by TURKSTAT and the relevant investor numbers assembled through administrative records by Borsa Istanbul Group in 2019 and 2021. The unit of analysis is the month-year birth cohorts. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the percentage of those having a transaction account in column 1, and participating in the stock market through any financial instruments traded in the stock market in column 2. The outcome is the percentage of holding directly stocks in column 3, risky assets in column 4, liquid stocks in column 5, bonds in column 6, and funds in their stock market portfolio in column 7. In column 8, the outcome is the percentage of those participating in DC pension plans, and in column 9 holding equity funds in their DC pension portfolio. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4: Education Reform vs Stock Market and Pension Wealth

	(1) Stock Market (in logs)	(2) DC Pension (in logs)
	Full	
Education Reform	-0.017	0.015*
	(0.018)	(0.008)
Bandwidth	45.89	37.76
Observations	808937	1900641
	Male	
Education Reform	-0.014	0.007
	(0.020)	(0.010)
Bandwidth	45.87	36.08
Observations	595797	1146511
	Female	
Education Reform	-0.027	0.027***
	(0.027)	(0.010)
Bandwidth	46.56	34.99
Observations	217709	666926

Notes: Local linear RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the log of the value of stock market portfolios in column 1 and the log of the value of pension portfolios in Turkish Lira in the last column. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth and for the birth registration certificate region fixed effects and the regressions in the full sample additionally have controls for gender. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Education Reform vs Portfolio Choices

	(1) Stock	(2) Risky Asset	(3) Bond	(4) Fund	(5) Liquid Stock	(6) Equity Fund
			Full			
Education Reform	0.001	-0.000	-0.000	-0.001	0.002	0.000**
	(0.002)	(0.002)	(0.000)	(0.002)	(0.001)	(0.000)
Control Mean	0.55	0.65	0.01	0.32	0.13	0.02
Bandwidth	38.42	48.95	42.07	37.34	44.36	49.74
Observations	685035	864439	755220	668661	791630	2506174
			Male			
Education Reform	0.000	0.001	0.001***	-0.000	0.002	0.000***
	(0.002)	(0.002)	(0.000)	(0.003)	(0.001)	(0.000)
Control Mean	0.57	0.67	0.01	0.31	0.14	0.02
Bandwidth	39.14	53.06	53.92	39.18	44.74	46.87
Observations	517965	694984	694984	517965	582992	1451167
			Female			
Education Reform	0.002	-0.002	-0.003***	-0.005	0.000	0.000
	(0.004)	(0.004)	(0.001)	(0.004)	(0.002)	(0.000)
Control Mean	0.48	0.60	0.01	0.37	0.11	0.02
Bandwidth	42.54	51.50	35.84	40.90	45.17	42.97
Observations	198736	240273	164691	188984	213140	823369

Notes: Local linear RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the share of wealth invested in stocks in column 1, risky assets in column 2, bonds in column 3, funds in column 4, and liquid stocks listed in the BIST-30 that tracks the stock performance of the 30 largest companies in Türkiye in column 5 in stock market portfolios. The outcome is the share of wealth invested in equity funds in pension portfolios in the last column. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects and birth registration certificate region fixed effects, and the regressions in the full sample additionally include controls for gender. Control mean displays the mean of the corresponding outcome for those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6: Education Reform vs Portfolio Returns

	(1) Stock Market	(2) DC Pension
	Full	
Education Reform	0.525	0.011*
	(0.428)	(0.006)
Control Mean	105.40	20.27
Bandwidth	44.72	40.84
Observations	791630	2049373
	Male	
Education Reform	0.779	0.017***
	(0.525)	(0.007)
Control Mean	107.25	20.23
Bandwidth	47.73	36.14
Observations	618526	1146511
	Female	
Education Reform	-0.138	-0.000
	(0.989)	(0.010)
Control Mean	99.71	20.33
Bandwidth	44.82	39.76
Observations	208638	764488

Notes: Local linear RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the annual rate of return of investor's stock market portfolios in column 1, and pension portfolios in percentages in the last column. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects and birth registration certificate region fixed effects, and the regressions in the full sample additionally include controls for gender. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 7: Education Reform vs Behavioral Biases and Heuristics

	(1) Contribution	(2) Default	(3) Only Default	(4) Default Share	(5) 1/N Heuristic	(6) Portfolio Reshuffling
			Full			
Education Reform	0.120**	-0.043	-0.067*	-0.356**	0.001	-0.004*
	(0.056)	(0.044)	(0.035)	(0.152)	(0.001)	(0.002)
Control Mean	13.88	13.01	10.98	46.73	0.37	0.44
Bandwidth	34.05	28.57	27.90	50.57	28.60	30.59
Observations	69	57	55	2551985	1436505	1535546
			Male			
Education Reform	0.043	-0.043	-0.109	-0.351**	-0.001	-0.002
	(0.071)	(0.044)	(0.082)	(0.167)	(0.002)	(0.002)
Control Mean	15.64	13.01	14.55	50.91	0.34	0.48
Bandwidth	25.41	28.57	26.24	50.66	30.41	34.88
Observations	51	57	53	1574751	946276	1071550
			Female			
Education Reform	0.208***	-0.043	0.018	-0.274	0.006***	-0.007**
	(0.075)	(0.044)	(0.057)	(0.221)	(0.002)	(0.003)
Control Mean	11.97	13.01	7.37	40.45	0.41	0.38
Bandwidth	39.49	28.57	33.93	41.04	26.29	30.37
Observations	79	57	67	803354	511930	589270

Notes: Local linear RD estimates in all columns. Columns 1-3 use data from population numbers assembled by TURKSTAT and the relevant investor numbers assembled through administrative records by Borsa Istanbul Group in 2019 and 2021. Columns 4-6 use administrative data covering the universe of all DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is the month-year birth cohorts in columns 1-3 and individuals in the remaining columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the percentage of those actively contributing to pension plans in column 1, holding the default fund in column 2, and only the default fund in column 3 in pension portfolios. In column 4, the outcome is the share of wealth invested in the default fund in percentages in pension portfolios. In column 5, the outcome is a dummy variable equal to one if a pension investor follows the conditional $\frac{1}{N}$ heuristic while allocating money to pension funds. In column 6, the outcome is a dummy variable equal to one if a pension investor changed her funds in a year. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth for columns 1-3 and columns 4-6 also include controls for the birth registration certificate region fixed effects, and the regressions in the full sample additionally include controls for gender. Control mean displays the mean of the corresponding outcome for those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 8: Education Reform vs First Enrollment Type

	(1) Voluntary	(2) Automatic	(3) Employer
		Full	
Education Reform	0.155**	-0.059	0.042**
	(0.071)	(0.074)	(0.021)
Control Mean	23.71	27.58	1.64
Bandwidth	42.60	29.00	27.82
Observations	85	59	55
		Male	
Education Reform	0.056	-0.174	0.022
	(0.069)	(0.131)	(0.037)
Control Mean	27.30	37.91	2.29
Bandwidth	29.53	27.25	28.56
Observations	59	55	57
		Female	
Education Reform	0.322***	0.048	0.066***
	(0.084)	(0.105)	(0.017)
Control Mean	19.76	17.04	0.97
Bandwidth	46.47	27.53	28.13
Observations	93	55	57

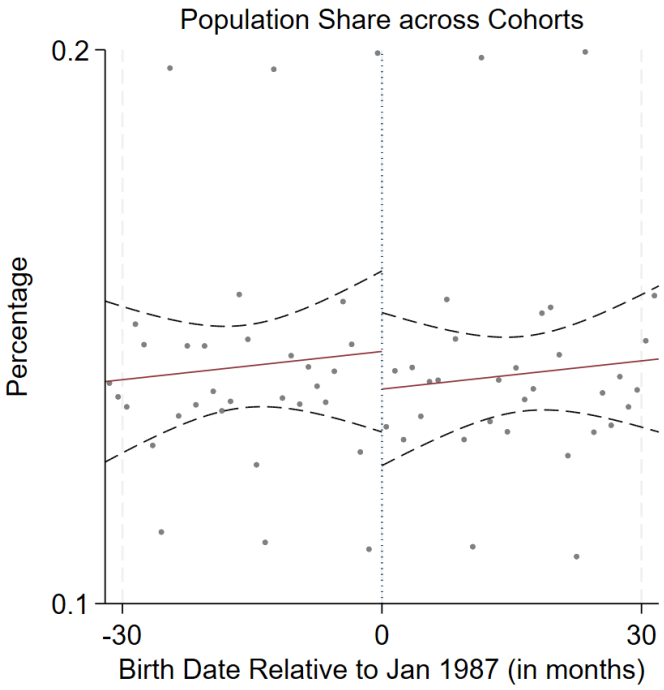
Notes: Local linear RD estimates in all columns. All columns use data from population numbers assembled by TURKSTAT and the relevant investor numbers assembled through administrative records by Borsa Istanbul Group in 2019 and 2021. The unit of analysis is the month-year birth cohorts in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the percentage of those enrolled first through DC voluntary retirement accounts in column 1, DC automatic enrollment retirement accounts in column 2, and DC employer-sponsorship retirement accounts in column 3. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 9: Education Reform vs Labor Market Outcomes

	(1) Wage (in logs)	(2) High Occupation	(3) Low Occupation	(4) Large Firm
		Full		
Education Reform	0.042***	0.019	0.002	0.026*
	(0.016)	(0.018)	(0.010)	(0.014)
Control Mean	2.50	0.38	0.18	0.51
Bandwidth	45.62	41.36	47.75	52.17
Observations	21355	26708	30323	33434
		Male		
Education Reform	0.030	0.019	0.010	0.019
	(0.022)	(0.018)	(0.010)	(0.015)
Control Mean	2.49	0.34	0.15	0.52
Bandwidth	41.97	54.91	51.80	47.15
Observations	13557	23299	22193	20462
		Female		
Education Reform	0.088***	0.014	-0.036**	0.041*
	(0.027)	(0.033)	(0.017)	(0.025)
Control Mean	2.54	0.47	0.26	0.49
Bandwidth	58.70	38.85	65.84	58.77
Observations	8526	7963	13555	12115

Notes: Local linear RD estimates in all columns. All columns use data from the 2018 Household Labor Force Survey by TURKSTAT. The unit of analysis is individuals. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. In column 1, the outcome is the logarithm of labor income. In column 2, the outcome is a dummy variable equal to one if an individual works in high paying occupation, whereas low paying occupation in column 3. In column 4, the outcome is a dummy variable equal to one if an individual works in a large firm whose number of workers is greater than 50. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects and the regressions in the full sample additionally include controls for gender. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

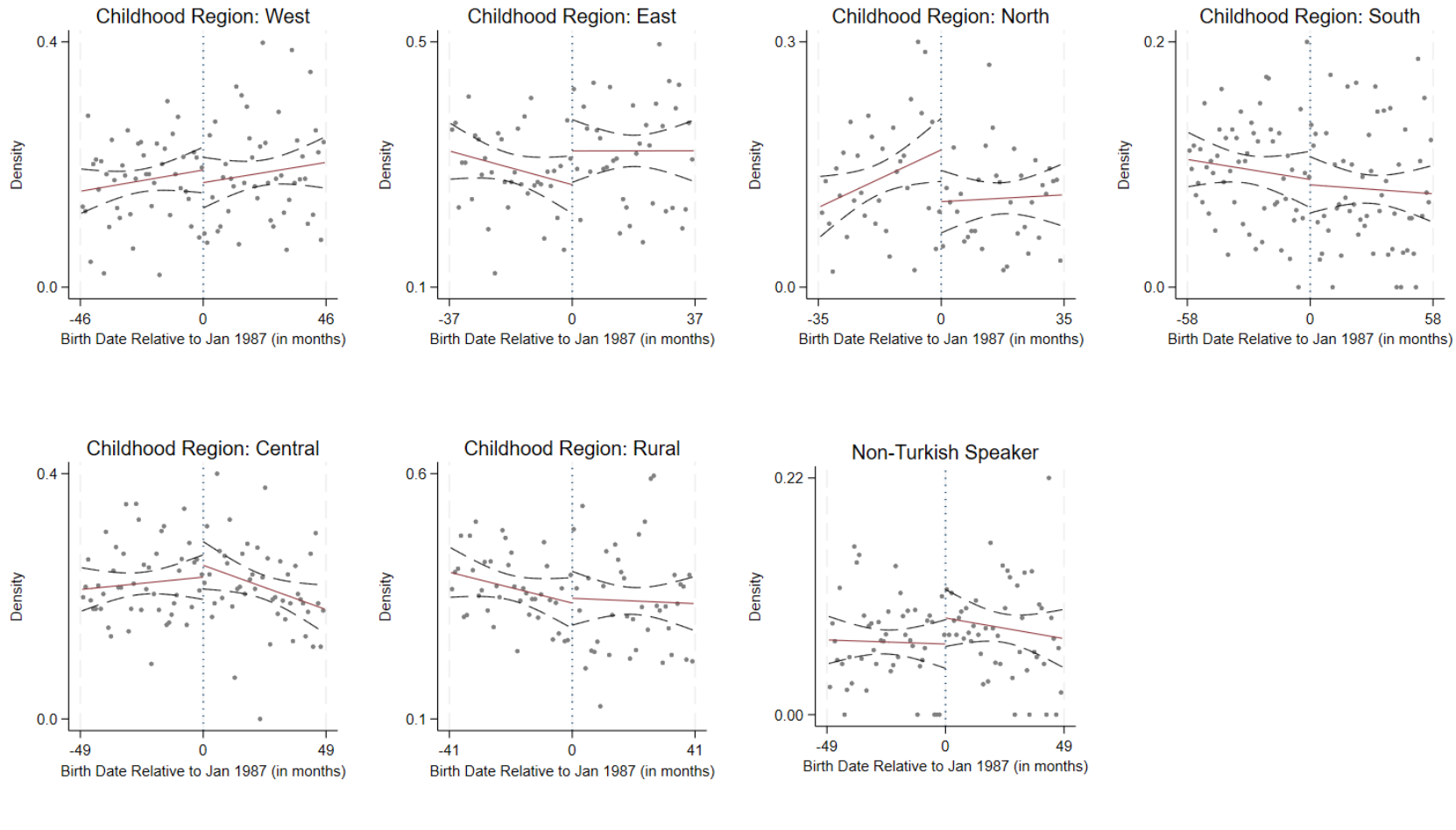
Figures



Source: TURKSTAT (2019)

Figure 1: Density of the Running Variable (in months)

Notes: The figure uses data covering the Turkish Population Share in each month-year of the birth cohort in 2019 assembled by TURKSTAT, which plots the population share in percentage terms in monthly bins against month-year of birth in the optimal bandwidth calculated through the algorithm by Calonico *et al.* (2014). The vertical line in the graph indicates the cutoff point, which is January 1987. The black dashed line indicates 95% confidence intervals around the mean of the bins.



Source: TURKSTAT 2008-14 DVS

Figure 2: Balanced Covariates

Notes: All graphs use data from the 2008 and 2014 National Survey on Domestic Violence against Women in Türkiye by TURKSTAT. The figures plot predetermined covariates in monthly bins against the month-year of birth in the optimal bandwidth calculated through the algorithm by Calonico *et al.* (2014). The vertical lines in the graphs indicate the cutoff point, which is January 1987. Black dashed lines indicate 95% confidence intervals around the mean of bins.

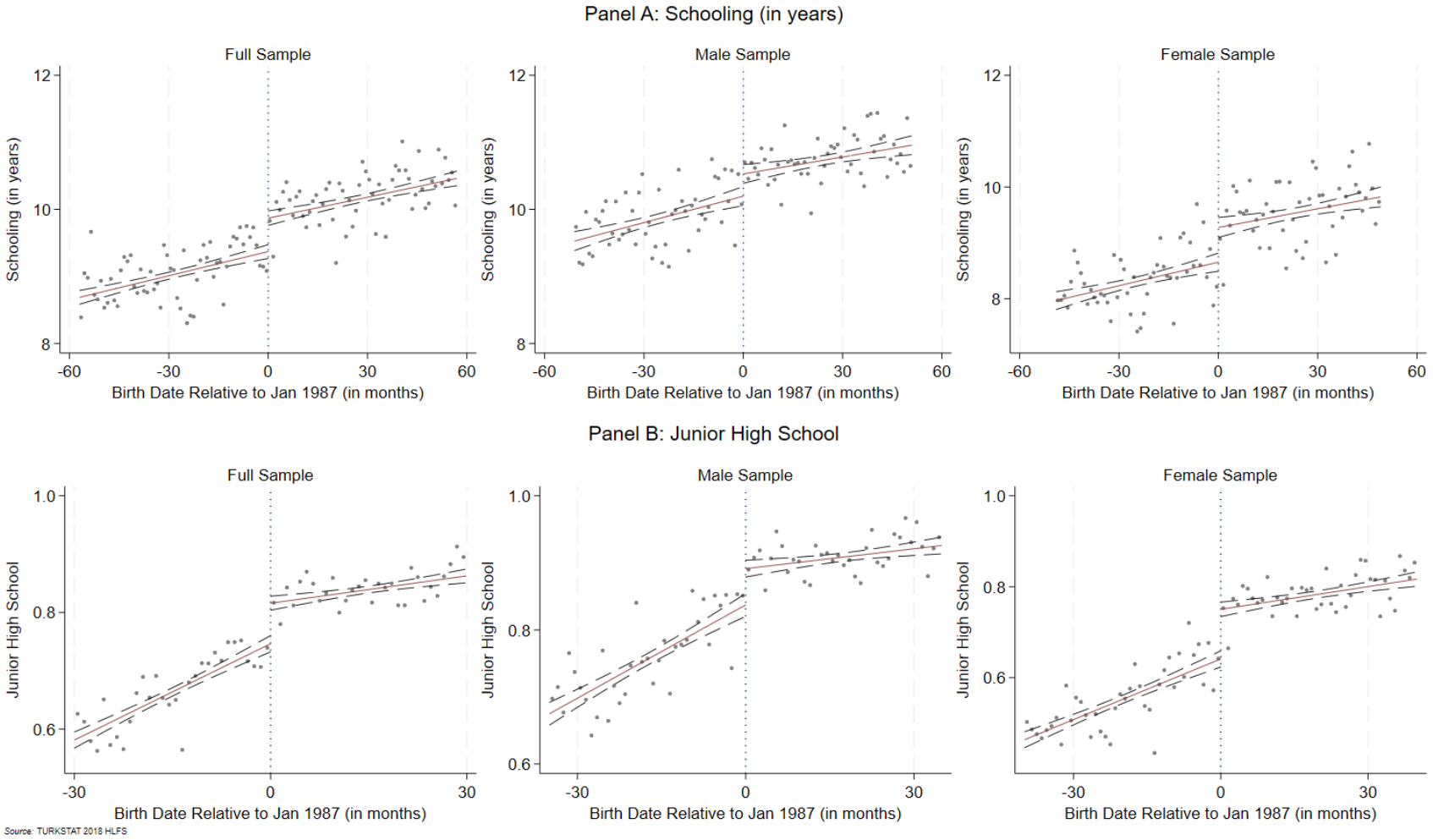


Figure 3: Education Reform vs Schooling, and Junior High School Degree

Notes: All graphs use data from the 2018 Household Labor Force Survey assembled by TURKSTAT. The figures in Panel A plot schooling in years and the figures in Panel B plot the propensity to hold at least a junior high school degree in monthly bins against the month-year birth cohorts in the optimal bandwidth calculated through the algorithm by Calonico *et al.* (2014). The vertical lines in the graphs indicate the cutoff point, which is January 1987. Black dashed lines indicate 95% confidence intervals around the mean of bins. Full, male, and female sample figures are reported, respectively.

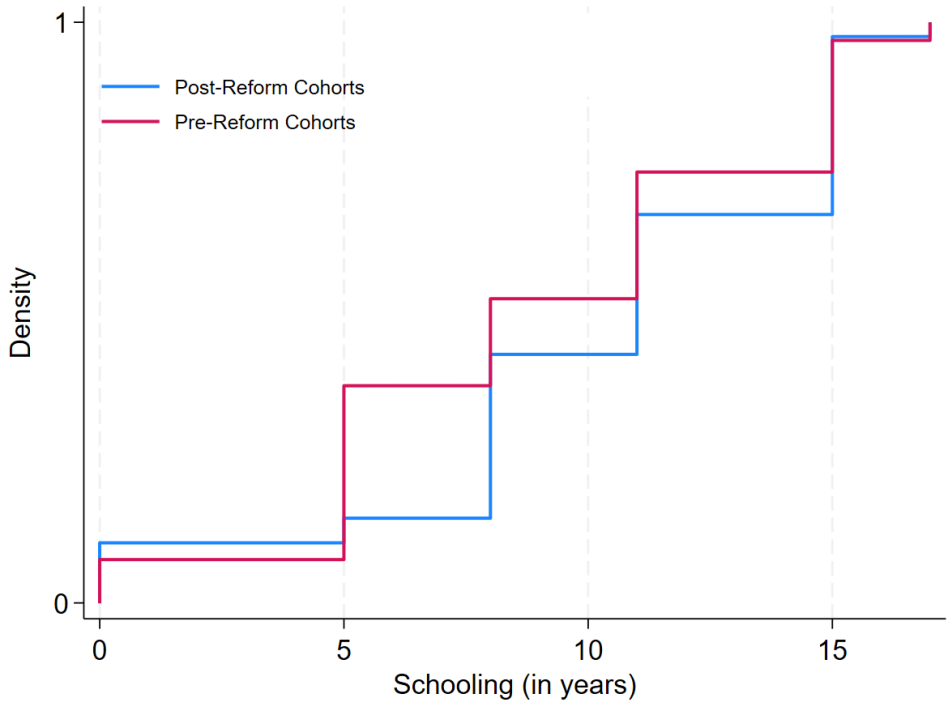


Figure 4: CDF of Schooling in Years across Treatment and Control Cohorts

Notes: All graphs use data from the 2018 Household Labor Force Survey assembled by TURKSTAT. The sample producing this graph includes those born 57 months before or after January 1987 since the optimal bandwidth is 57 months. The figures plot the cumulative density of schooling in years by the cohorts of those born before and after January 1987, which is the determinant of treatment status arising from the 1997 Education Reform.

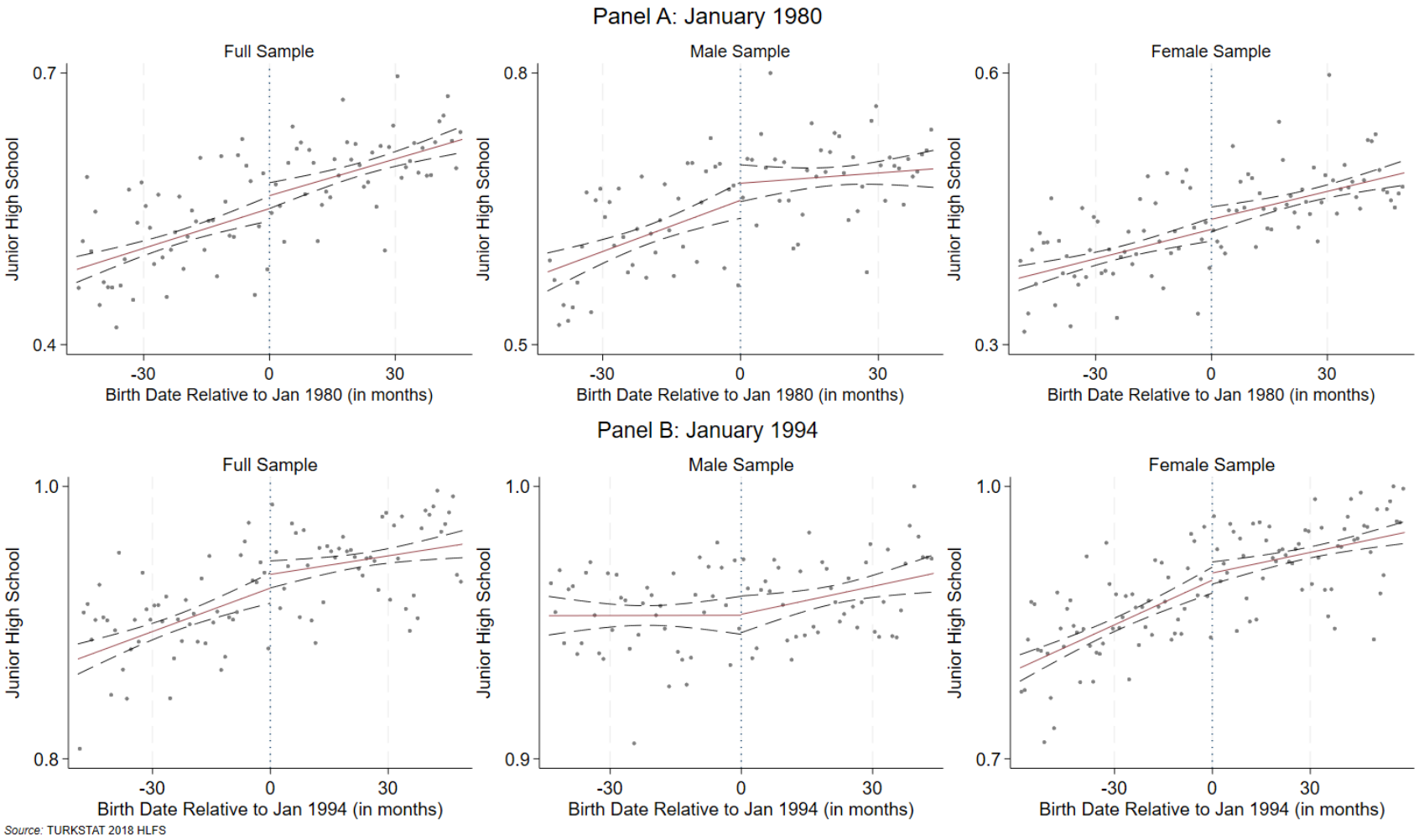


Figure 5: Placebo Cutoffs vs Junior High School Degree

Notes: All graphs use data from the 2018 Household Labor Force Survey by TURKSTAT. The figures plot the propensity of having at least a junior high school degree in monthly bins against the month-year of birth in the optimal bandwidth calculated through the algorithm by Calonico *et al.* (2014). The vertical lines in the graphs indicate the cutoff point, which is January 1987. Black dashed lines indicate 95% confidence intervals around the mean of bins. Full, male, and female sample figures are reported, respectively.

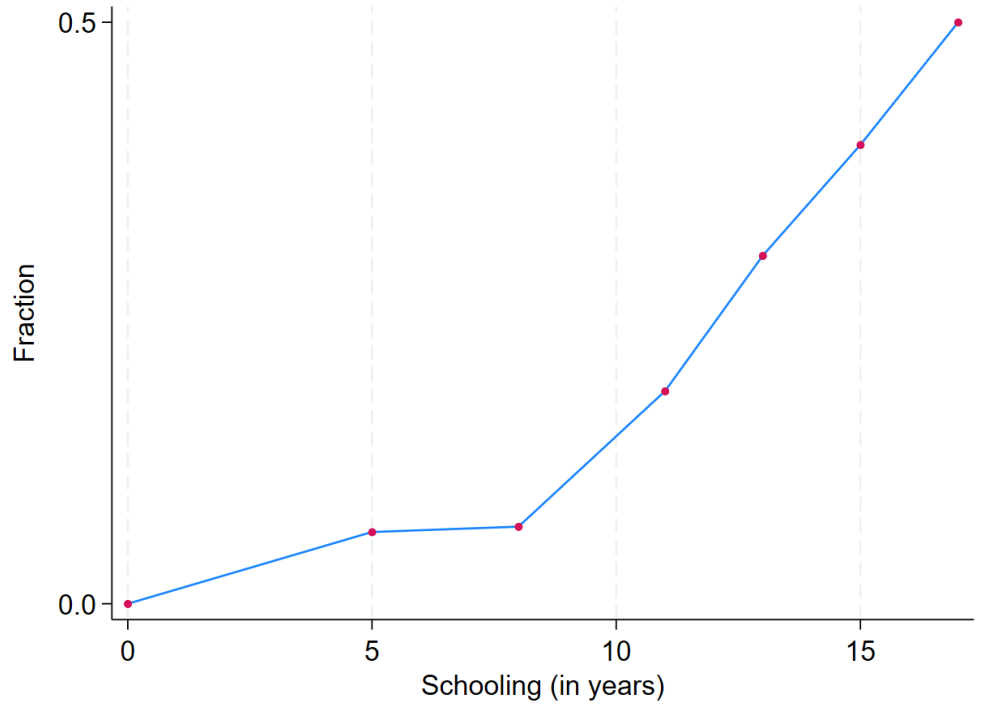


Figure 6: Schooling vs Stock Market Participation

Notes: The graph uses data from Household Budget Surveys of 2018-19 by TURKSTAT. The sample includes those born 39 months before or after January 1987 since the optimal bandwidth in the RD design is 39 months for the outcome of participation in DC pension plans. The figures plot the fraction of those participating in the stock market against the binned years of schooling.

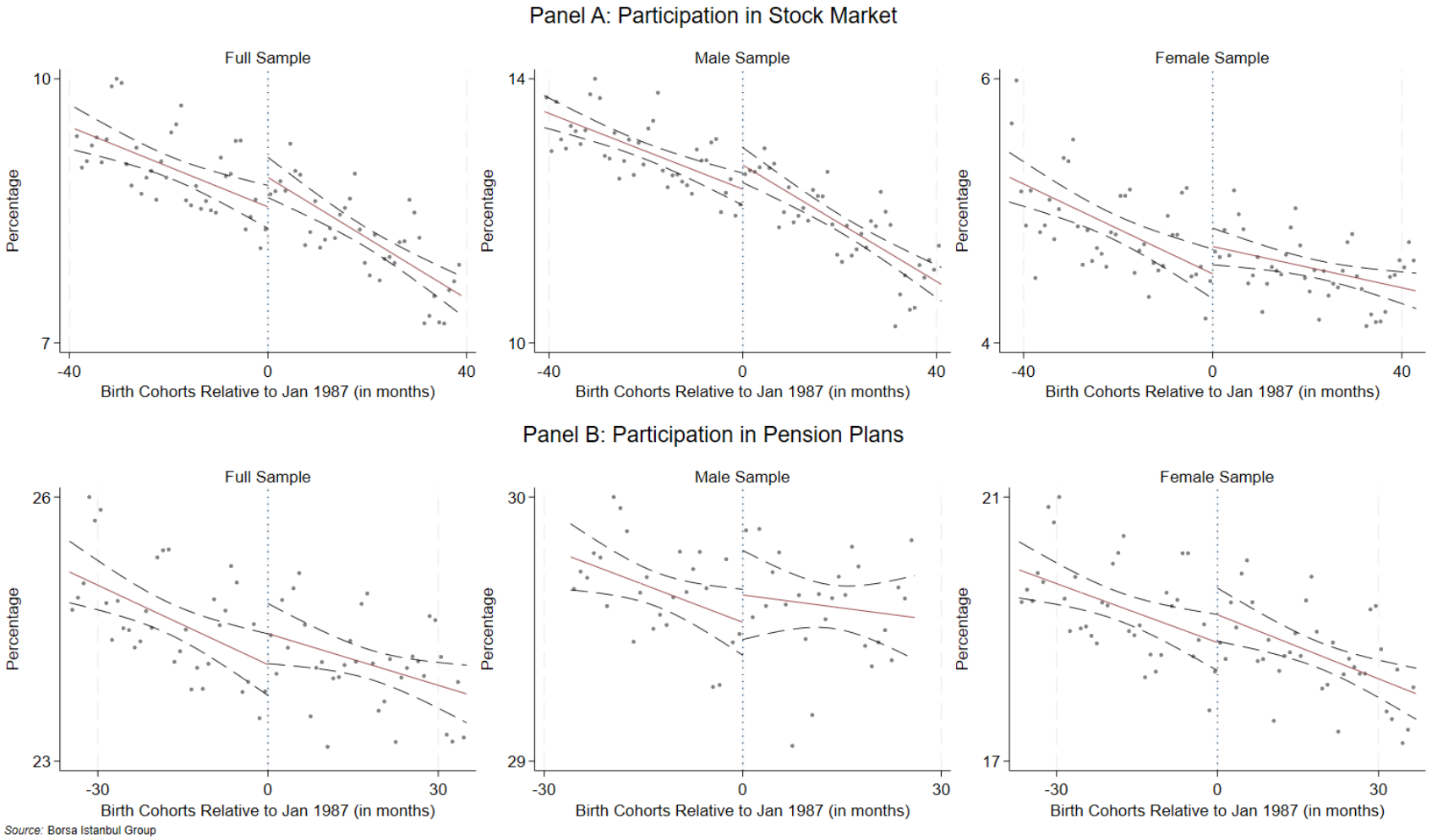
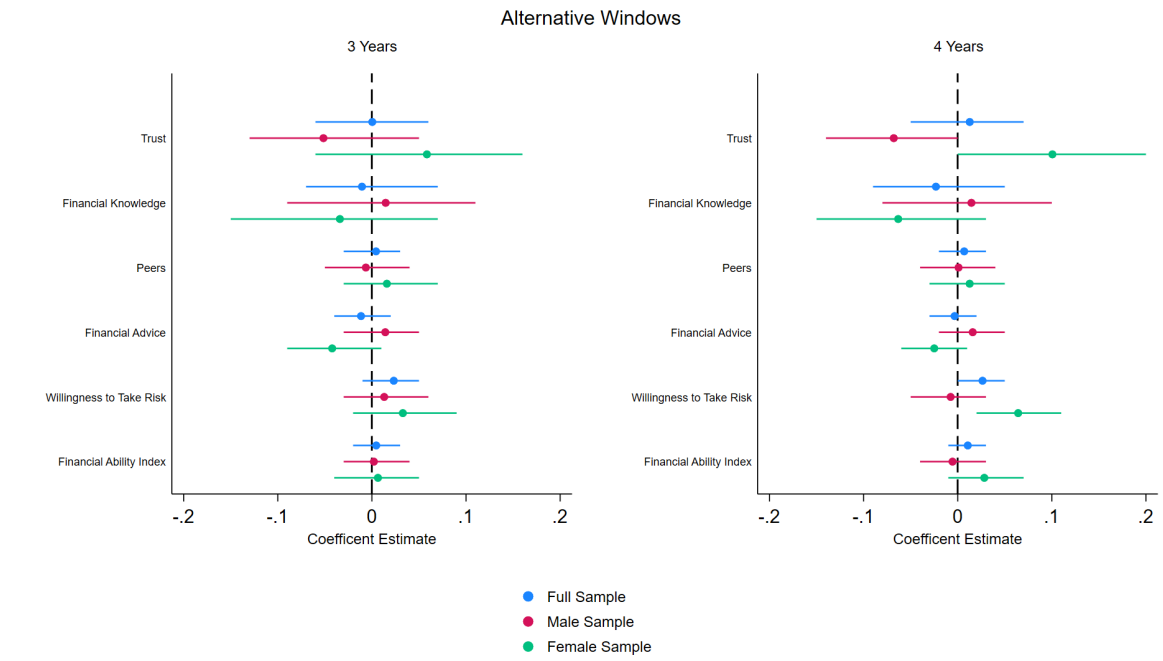


Figure 7: Education Reform vs Participation in Stock Market and DC Pension Plans

Notes: All graphs use administrative data assembled by Borsa Istanbul Group and TURKSTAT in 2019. The figures in Panel A plot the percentage of those who first participated in DC pension plans through voluntary, in Panel B automatic and employer-sponsored enrollment in Panel C in monthly bins against the month-year birth cohorts in the optimal bandwidth calculated through the algorithm by Calonico *et al.* (2014). The vertical lines in the graphs indicate the cutoff point, which is January 1987. Black dashed lines indicate 95% confidence intervals around the mean of bins. Full, male, and female sample figures are reported, respectively.



Source: FPS

Figure 8: Education Reform vs Financial Skills

Notes: The graphs present the local linear RD estimates of the education reform on outcomes related to various financial skills. The unit of analysis is individuals. All graphs use data from the Survey on the Financial Perceptions and Attitudes of Turkish Households in 2018. The figures present the point estimates for the window of 3, and 4. The outcomes are the standardized trust index, standardized financial knowledge index, the probability of peers affecting the financial decisions, the likelihood of getting financial advice from financial institutions, the indicator for willingness to take risk, and the summary index accounting for the former variables. The dots plot the point estimates whereas the blue, red, and green lines present the 95% confidence intervals. The dashed line indicates the value of zero.

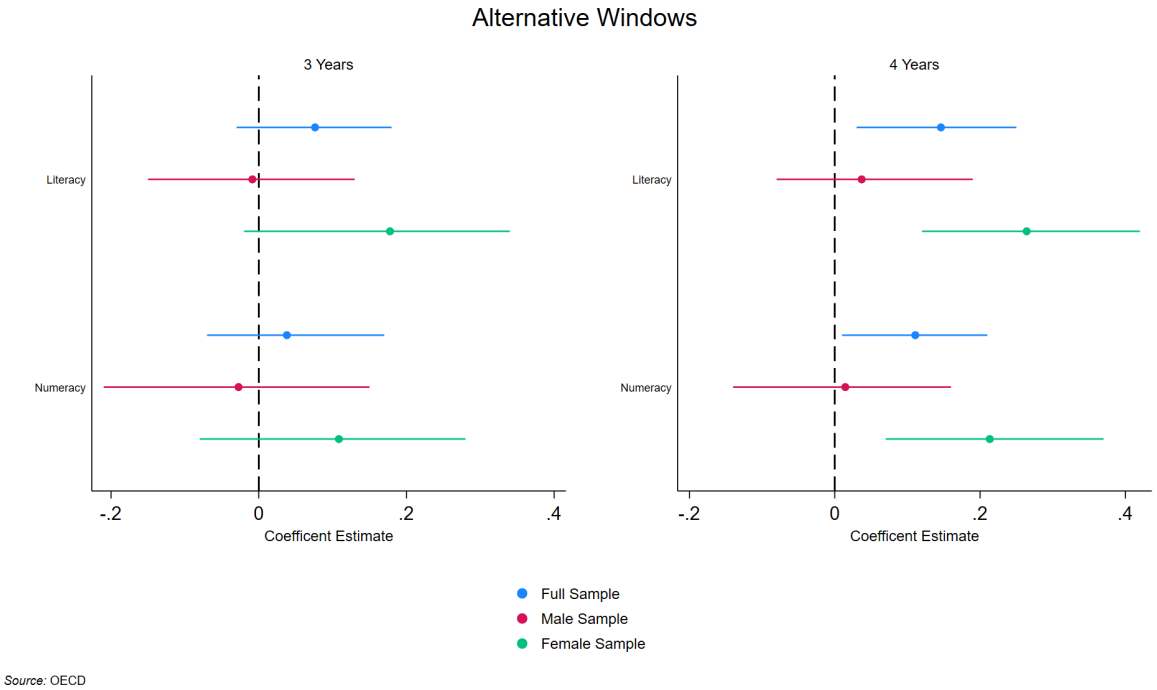


Figure 9: Education Reform vs Cognitive Skills

Notes: The graphs present the local linear RD estimates of the education reform on outcomes related to the cognitive skills. The unit of analysis is individuals. All graphs use data from PIAAC in 2015. The figures present the point estimates for the window of 3, and 4. The outcomes are the standardized literacy and numeracy scores. The dots plot the point estimates whereas the blue, red and green lines present the 95% confidence intervals. The dashed line indicates the value of zero.

ONLINE APPENDIX

A Figures

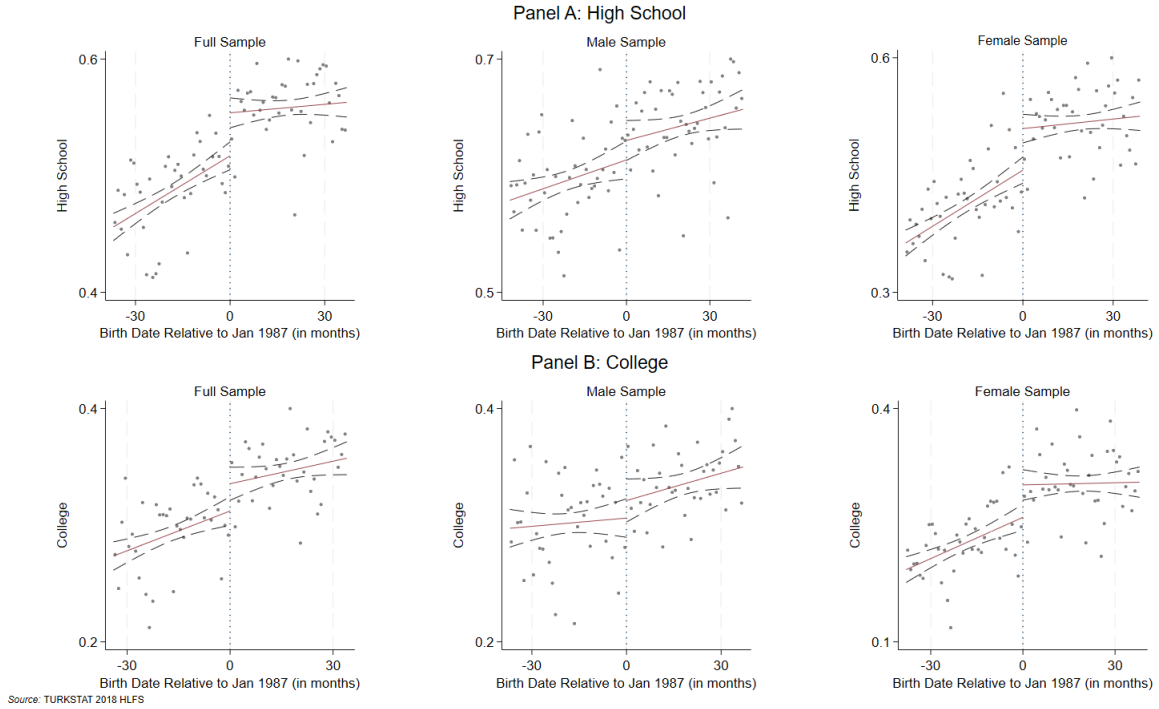


Figure A.1: Education Reform vs High School, and College Degree

Notes: All graphs use data from the 2018 Household Labor Force Survey by TURKSTAT. The figures plot the propensity of having at least a high school degree in monthly bins against the month-year of birth in the optimal bandwidth calculated through the algorithm by Calonico *et al.* (2014). The vertical lines in the graphs indicate the cutoff point, which is January 1987. Black dashed lines indicate 95% confidence intervals around the mean of bins. Full, male, and female sample figures are reported, respectively.

B Tables

B.1 Descriptive Statistics

Table B.1: Descriptive Statistics for Those Born 60 Months Before and After January 1987

	(1) Control	(2) Treatment	(3) Difference (2)-(1)
Panel A: Schooling			
Junior High School Degree	0.64	0.86	0.23
	(0.48)	(0.34)	(0.00)
High School Degree	0.49	0.59	0.1
	(0.50)	(0.49)	(0.00)
College Degree	0.27	0.35	0.08
	(0.45)	(0.48)	(0.00)
Schooling (in years)	9.2	10.31	1.15
	(4.72)	(4.64)	(0.04)
Panel B: Participation and Asset Ownership			
Transaction Account (%)	55.81	52.07	-3.73
	(1.36)	(2.91)	(0.41)
Stock Market Participation (%)	9.14	8.08	-1.05
	(0.51)	(0.44)	(0.10)
Direct Stock (%)	5.25	5.04	-0.22
	(0.26)	(0.28)	(0.05)
Risky Asset (%)	6.18	5.82	-0.37
	(0.30)	(0.32)	(0.06)
Liquid Stock (%)	2.27	1.96	-0.32
	(0.15)	(0.17)	(0.03)
Bond (%)	0.10	0.07	-0.03
	(0.01)	(0.01)	(0.00)
Fund (%)	3.06	2.40	-0.67
	(0.22)	(0.22)	(0.04)
Pension Fund (%)	25.05	24.28	-0.77
	(0.63)	(0.44)	(0.10)
Equity Fund in Pensions (%)	2.21	2.91	-0.7
	(0.30)	(0.20)	(0.05)
Panel C: Portfolio Choices			
Stock Share	0.54	0.59	0.06
	(0.49)	(0.48)	(0.00)

Continued on next page

Table B.1 – continued from previous page

Risky Share	0.65	0.7	0.05
	(0.47)	(0.45)	(0.00)
Bond Share	0.01	0.01	0.00
	(0.09)	(0.08)	(0.00)
Fund Share	0.33	0.30	-0.03
	(0.46)	(0.45)	(0.00)
Liquid Stock Share	0.13	0.13	0.00
	(0.29)	(0.30)	(0.00)
Equity Fund Share in Pensions	0.02	0.01	0.01
	(0.08)	(0.07)	(0.00)
Panel D: Wealth and Performance			
Stock Market Wealth (in logs)	6.69	6.65	-0.31
	(3.74)	(3.64)	(0.01)
Pension Wealth (in logs)	7.18	6.76	-0.42
	(2.35)	(2.26)	(0.00)
Stock Market Portfolio Return (%)	20.46	20.25	-0.21
	(2.29)	(2.39)	(0.00)
Pension Portfolio Return (%)	104.13	112.83	8.7
	(120.45)	(126.04)	(0.24)
Panel E: Behavioral Biases and Heuristics in DC Pension Plans			
Contributor (%)	14.18	13.07	-1.11
	(0.58)	(0.47)	(0.10)
Default Fund (%)	14.5	12.94	1.57
	(0.93)	(0.21)	(0.12)
Only Default Fund (%)	12.54	10.84	1.57
	(0.93)	(0.21)	(0.12)
Default Fund Share (%)	46.4	54.86	8.46
	(48.76)	(48.64)	(0.06)
1/N Heruistic	0.65	0.67	0.01
	(0.47)	(0.48)	(0.00)
Portfolio Reshuffling	0.44	0.49	0.06
	(0.50)	(0.50)	(0.00)

Notes: The table displays the mean, standard deviations in parenthesis, and the difference between the treatment and control groups. The treatment group covers those born after January 1987 while the control group is those born before January 1987. Panel A uses data from the 2018 Household Labor Force Survey by TURKSTAT and presents descriptive statistics for individuals. Panel B and C use data at the cohort level. The remaining panels use the stock market and pension administrative data with the month-end snapshots of either retirement or stock market accounts at the investor level on December 31, 2019, and 2021 respectively, provided by Borsa Istanbul Group. The variable definitions are provided in the Data Appendix.

B.2 OLS Estimates for Education vs Participation in Pension Plans

Table B.2: Education vs Participation in Pension Plans

	(1)	(2)
	Pension Participant	
Years of Schooling	0.027***	
	(0.001)	
Junior High School		0.043***
		(0.012)
High School		0.107***
		(0.015)
College		0.162***
		(0.018)
Weighted β		0.012***
		0.03
Control Mean	0.20	0.20
Observations	6113	6113

Notes: OLS estimates in all columns. All columns use data from Household Budget Surveys of 2018-19 by TURKSTAT. The sample includes those born 39 months before or after January 1987 since the optimal bandwidth in the RD design is 39 months for the outcome of pension plan. The unit of analysis is individuals. In all columns, the outcome is a dummy variable equal to one if the individual participates in the stock market. In the first column, the explanatory variable is years of schooling. In the second column, the explanatory variables are indicator variables for junior high school, high school, and college degrees, respectively. The reference category is the primary school degree at most. Weighted $\hat{\beta}$ is the weighted average of the point estimates of the indicator variables for the indicator variables of degrees in the second column regarding the shift induced by the Education Reform in degrees. All regressions include controls for gender with a dummy variable of being female and year of survey fixed effects for each survey year. The control mean displays the mean of the corresponding outcome in the control group. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

B.3 Regression Evidence for Validity Checks

Table B.3: Education Reform vs Population Shares of Month-Year Birth Cohorts

	(1) Linear RD h bandwidth	(2) Quadratic RD h bandwidth	(3) Linear RD h/2 bandwidth	(4) Linear RD 2h bandwidth
			Full	
Education Reform	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Control Mean	0.13	0.13	0.12	0.13
Bandwidth	33.00	55.58	18.86	65.99
Observations	65	111	37	131
			Male	
Education Reform	-0.001	-0.001	-0.001*	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Control Mean	0.13	0.13	0.13	0.13
Bandwidth	33.12	55.53	19.61	66.24
Observations	67	111	39	133
			Female	
Education Reform	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Control Mean	0.12	0.12	0.12	0.13
Bandwidth	32.92	55.82	18.09	65.83
Observations	65	111	37	131

Notes: Local RD estimates in all columns. All columns use data covering the Turkish Population Share in each month of birth cohorts in 2019 assembled by TURKSTAT. The unit of analysis is the month-year birth cohorts. The main explanatory variable namely Education Reform is a dummy variable equal to one for the cohorts born after January 1, 1987. The outcome is the population share in each month of birth cohorts. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Column 1 reports RD estimates with the optimal bandwidth and a linear polynomial function on each side of the cutoff value. Column 2 reports RD estimates with a quadratic polynomial function on each side of the cutoff value. Columns 3 and 4 report RD estimates with a linear polynomial function on each side of the cutoff value in half and twice the optimal bandwidth estimated in column 1. All regressions include controls for the month of birth fixed effects with dummy variables for each month. Control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.4: Balanced Covariates

	Mother Tongue	Childhood Region					
	(1) Non-Turkish	(2) Rural	(3) West	(4) East	(5) North	(6) South	(7) Central
Education Reform	0.024	0.014	-0.037	0.075**	-0.040	0.009	0.000
	(0.016)	(0.030)	(0.034)	(0.036)	(0.027)	(0.024)	(0.028)
Control Mean	0.07	0.38	0.20	0.31	0.14	0.11	0.23
Bandwidth	48.74	41.42	46.40	37.36	34.55	57.54	48.61
Observations	3534	3030	3370	2733	2508	4197	3510

Notes: Local linear RD estimates in all columns. All columns use data from 2008 and National Surveys on Domestic Violence against Women in Türkiye by TURKSTAT. The unit of analysis is females. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. In column 1, the outcome is a dummy variable equal to one if the specific female individual has a mother tongue other than Turkish. In column 2, the outcome is a dummy variable equal to one if the specific female individual grew up in rural Türkiye. In column 3, the outcome is a dummy variable equal to one if the specific female individual grew up in Western Türkiye. In column 4, the outcome is a dummy variable equal to one if the specific female individual grew up in Eastern Türkiye. In column 5, the outcome is a dummy variable equal to one if the specific female individual grew up in Northern Türkiye. In column 6, the outcome is a dummy variable equal to one if the specific female individual grew up in Southern Türkiye. In column 7, the outcome is a dummy variable equal to one if the specific female individual grew up in Central Türkiye. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects with dummy variables for each month and survey year fixed effects with dummy variables for each survey year. All regressions include controls for the month of birth and survey year dummies. Control mean and standard deviation (SD) display the mean and standard deviation of the corresponding outcome of those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.5: Education Reform, Placebo Cutoffs vs Schooling Outcomes

	Schooling (in years)			Junior High School Degree		
	(1) January 1987	(2) January 1980	(3) January 1994	(4) January 1987	(5) January 1980	(6) January 1994
			Full			
Treatment	0.451***	-0.047	-0.005	0.055***	0.003	-0.005
	(0.105)	(0.112)	(0.100)	(0.010)	(0.012)	(0.007)
Control Mean	9.25	8.27	10.48	0.68	0.52	0.90
Bandwidth	57.21	49.38	36.52	30.82	46.55	49.70
Observations	59347	57174	34591	31744	53595	46336
			Male			
Treatment	0.381***	0.040	0.005	0.042***	-0.002	-0.001
	(0.148)	(0.121)	(0.129)	(0.015)	(0.016)	(0.007)
Control Mean	9.96	9.20	10.63	0.76	0.62	0.94
Bandwidth	51.41	40.19	33.01	35.32	42.99	44.97
Observations	25664	22928	15247	17678	23792	19878
			Female			
Treatment	0.448**	0.029	0.032	0.081***	0.011	-0.010
	(0.210)	(0.144)	(0.153)	(0.021)	(0.015)	(0.011)
Control Mean	8.56	7.32	10.30	0.57	0.42	0.85
Bandwidth	49.82	62.74	40.88	40.32	50.09	59.31
Observations	26722	36464	19778	21889	30009	28839

Notes: Local linear RD estimates in all columns. All columns use data from the 2018 Household Labor Force Survey by TURKSTAT. The unit of analysis is individuals. The main explanatory variable namely Treatment is a dummy variable equal to one for those born after January 1, 1987, in columns 1 and 4, or for those born after January 1, 1980, in columns 2 and 5, or for those born after January 1, 1994, in columns 3 and 6. In columns 1-3, the outcome is schooling in years. In columns 4-6, the outcome is a dummy variable equal to one if the specific individual has at least a junior high school degree. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects with dummy variables for each month and the regressions in the full sample additionally include controls for gender with a dummy variable of being female. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

B.4 Local Randomization RD Estimates with Schooling Outcomes

Table B.6: Local Randomization RD Estimates of Schooling Outcomes

Outcome	Obs Left	Obs Right	Estimate	Finite Sample p-value	Large Sample p-value	Control Mean
			Full			
Schooling (in years)	6594	6660	0.412	0.000	0.000	9.367
Junior High School Degree	6594	6660	0.097	0.000	0.000	0.710
High School Degree	6594	6660	0.041	0.000	0.000	0.508
College Degree	6594	6660	0.025	0.000	0.001	0.277
Summary Index	6594	6660	0.079	0.000	0.000	0.473
			Male			
Schooling (in years)	3202	3178	0.341	0.000	0.001	10.116
Junior High School Degree	3202	3178	0.079	0.000	0.000	0.807
High School Degree	3202	3178	0.029	0.022	0.021	0.568
College Degree	3202	3178	0.014	0.206	0.207	0.294
Summary Index	3202	3178	0.064	0.000	0.000	0.604
			Female			
Schooling (in years)	3392	3482	0.500	0.000	0.000	8.660
Junior High School Degree	3392	3482	0.117	0.000	0.000	0.618
High School Degree	3392	3482	0.054	0.000	0.000	0.451
College Degree	3392	3482	0.035	0.000	0.001	0.261
Summary Index	3392	3482	0.097	0.000	0.000	0.350

Notes: Local Randomization RD estimates. All columns use data from the 2018 Household Labor Force Survey by TURKSTAT. The unit of analysis is individuals. The column of Outcome reports the corresponding schooling outcome. Column of Bandwidth reports the closest window length of one around the cutoff value of 1987. Columns of Obs Left and Obs Right report the number of observations in the right and left window of the cutoff value, 1987. Column of Estimate reports the local randomization RD estimates, that is the impact of Education Reform. Column of Finite Sample p-value displays the corresponding p-value for the relevant local randomization RD estimate in finite samples whereas the Column of Large Sample p-value documents the corresponding p-value for the relevant local randomization RD estimate asymptotically. Column of Control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Full, male, and female sample estimates are reported, respectively.

B.5 Attrition

Table B.7: Local Randomization RD Estimates of Attrition

Bandwidth	Obs Left	Obs Right	Estimate	Finite Sample p-value	Large Sample p-value	Control Mean
			Full			
1	6594	6660	0.002	0.586	0.522	0.043
2	13374	13071	-0.000	0.920	0.931	0.043
3	20374	18944	0.000	0.930	0.976	0.044
4	27274	25103	0.001	0.638	0.606	0.045
5	34337	30904	0.002	0.246	0.250	0.045
6	42279	36871	0.004	0.004	0.005	0.045
			Male			
1	3202	3178	-0.000	1.000	0.956	0.045
2	6491	6276	-0.001	0.834	0.782	0.045
3	9835	9143	0.000	1.000	0.979	0.046
4	13185	12091	0.004	0.166	0.180	0.046
5	16616	14873	0.006	0.016	0.022	0.045
6	20501	17692	0.007	0.002	0.001	0.045
			Female			
1	3392	3482	0.005	0.394	0.335	0.040
2	6883	6795	0.001	0.928	0.874	0.042
3	10539	9801	0.000	1.000	0.987	0.043
4	14089	13012	-0.002	0.516	0.537	0.045
5	17721	16031	-0.001	0.522	0.510	0.046
6	21778	19179	0.001	0.504	0.481	0.046

Notes: Local Randomization RD estimates. All columns use data from the 2018 Household Labor Force Survey by TURKSTAT. The unit of analysis is individuals. The outcome is a dummy variable equal to one if an individual lacks the month of birth information. The column of Bandwidth reports the window length around the cutoff value of 1987. Columns of Obs Left and Obs Right report the number of observations in the right and left window of the cutoff value, 1987. Column of Estimate reports the local randomization RD estimates, that is the impact of Education Reform. The column of Finite Sample p-value displays the corresponding p-value for the relevant local randomization RD estimate in finite samples whereas the column of Large Sample p-value documents the corresponding p-value for the relevant local randomization RD estimate asymptotically. Column of Control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Full, male, and female sample estimates are reported, respectively.

B.6 Local Randomization RD Estimates for the Junior High School Completion in PIAAC Data in 2015 and the Survey on the Financial Perceptions and Attitudes of Turkish Households in 2018

Table B.8: Local Randomization RD Estimates of Junior High School Completion

Bandwidth	Obs Left	Obs Right	Estimate	Finite Sample p-value	Large Sample p-value	Control Mean
Panel A: Estimates in the Survey on the Financial Perceptions and Attitudes of Turkish Households in 2018						
				Full		
3	1225	1519	0.053	0.000	0.000	0.853
4	1597	1943	0.059	0.000	0.000	0.852
				Male		
3	660	865	0.058	0.000	0.000	0.868
4	845	1083	0.070	0.000	0.000	0.857
				Female		
3	565	654	0.044	0.030	0.030	0.835
4	752	860	0.044	0.022	0.010	0.846
Panel B: Estimates in the PIAAC Data in 2015.						
				Full		
3	434	403	0.126	0.000	0.000	0.707
4	587	515	0.172	0.000	0.000	0.675
				Male		
3	228	202	0.118	0.002	0.000	0.803
4	302	262	0.147	0.000	0.000	0.785
				Female		
3	206	201	0.144	0.006	0.002	0.602
4	285	253	0.201	0.000	0.000	0.558

Notes: Local Randomization RD estimates. All columns use data from the Survey on the Financial Perceptions and Attitudes of Turkish Households in 2018 in Panel A, and the PIAAC data in 2015 in Panel B. The unit of analysis is individuals. The outcome is a dummy variable equal to one if the individual at least completed a junior high school degree. The Bandwidth reports the 3-year window length around the cutoff value of 1987. Columns of Obs Left and Obs Right report the number of observations in the right and left window of the cutoff value, 1987. Column of Estimate reports the local randomization RD estimates, that is the impact of Education Reform. The column of Finite Sample p-value displays the corresponding p-value for the relevant local randomization RD estimate in finite samples whereas the column of Large Sample p-value documents the corresponding p-value for the relevant local randomization RD estimate asymptotically. Column of Control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Full, male, and female sample estimates are reported, respectively.

B.7 Local Randomization RD Estimates for the Financial Skills

Table B.9: Local Randomization RD Estimates of Financial Skills

Outcome	Obs Left	Obs Right	Estimate	Finite Sample p-value	Large Sample p-value	Control Mean
Panel A: 3 Year Bandwidth						
				Full		
Trust	1225	1520	0.000	0.940	0.992	-0.078
Financial Knowledge	1225	1520	-0.011	0.776	0.795	-0.076
Peers	1225	1520	0.004	0.852	0.807	0.679
Financial Advice	1225	1520	-0.012	0.464	0.513	0.307
Willingness to Take Risk	1225	1520	0.023	0.220	0.225	0.450
Financial Ability Index	1225	1520	0.005	0.754	0.782	0.067
				Male		
Trust	660	866	-0.052	0.296	0.309	-0.010
Financial Knowledge	660	866	0.015	0.810	0.790	-0.139
Peers	660	866	-0.006	0.838	0.787	0.697
Financial Advice	660	866	0.014	0.508	0.545	0.285
Willingness to Take Risk	660	866	0.013	0.630	0.613	0.474
Financial Ability Index	660	866	0.002	0.852	0.921	0.076
				Female		
Trust	565	654	0.058	0.314	0.334	-0.158
Financial Knowledge	565	654	-0.034	0.580	0.575	-0.002
Peers	565	654	0.016	0.612	0.557	0.658
Financial Advice	565	654	-0.042	0.136	0.113	0.333
Willingness to Take Risk	565	654	0.033	0.242	0.248	0.421
Financial Ability Index	565	654	0.006	0.780	0.807	0.056
Panel B: 4 Year Bandwidth						
				Full		
Trust	1597	1944	0.013	0.664	0.711	-0.102
Financial Knowledge	1597	1944	-0.023	0.536	0.520	-0.069
Peers	1597	1944	0.007	0.680	0.661	0.682
Financial Advice	1597	1944	-0.003	0.850	0.830	0.298
Willingness to Take Risk	1597	1944	0.026	0.104	0.117	0.446
Financial Ability Index	1597	1944	0.011	0.450	0.465	0.060
				Male		
Trust	845	1084	-0.068	0.136	0.133	-0.000
Financial Knowledge	845	1084	0.015	0.756	0.765	-0.124
Peers	845	1084	0.001	1.000	0.966	0.695
Financial Advice	845	1084	0.016	0.458	0.443	0.279
Willingness to Take Risk	845	1084	-0.007	0.744	0.744	0.482
Financial Ability Index	845	1084	-0.006	0.714	0.772	0.081
				Female		
Trust	752	860	0.101	0.054	0.055	-0.215
Financial Knowledge	752	860	-0.063	0.242	0.237	-0.008
Peers	752	860	0.013	0.614	0.588	0.668
Financial Advice	752	860	-0.025	0.308	0.279	0.319
Willingness to Take Risk	752	860	0.064	0.014	0.009	0.406
Financial Ability Index	752	860	0.028	0.194	0.214	0.036

Notes: Local Randomization RD estimates. All columns use data from the Survey on the Financial Perceptions and Attitudes of Turkish Households in 2018. The unit of analysis is individuals. The outcomes are the standardized trust index, standardized financial knowledge index, the probability of peers affecting the financial decisions, the likelihood of getting financial advice from financial institutions, the indicator for willingness to take risk, and the summary index accounting for the former variables. The Bandwidth reports the 3-year window length around the cutoff value of 1987. Columns of Obs Left and Obs Right report the number of observations in the right and left window of the cutoff value, 1987. Column of Estimate reports the local randomization RD estimates, that is the impact of Education Reform. The column of Finite Sample p-value displays the corresponding p-value for the relevant local randomization RD estimate in finite samples whereas the column of Large Sample p-value documents the corresponding p-value for the relevant local randomization RD estimate asymptotically. Column of Control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Full, male, and female sample estimates are reported, respectively.

B.8 Local Randomization RD Estimates for the Cognitive Skills

Table B.10: Local Randomization RD Estimates of Cognitive Skills

Outcome	Obs Left	Obs Right	Estimate	Finite Sample p-value	Large Sample p-value	Control Mean
Panel A: 3 Year Bandwidth						
				Full		
Numeracy	587	515	0.146	0.014	0.012	0.178
Literacy	587	515	0.111	0.064	0.061	0.184
				Male		
Numeracy	302	262	0.037	0.616	0.628	0.387
Literacy	302	262	0.014	0.870	0.858	0.295
				Female		
Numeracy	285	253	0.264	0.002	0.002	-0.045
Literacy	285	253	0.213	0.010	0.013	0.066
Panel B: 4 Year Bandwidth						
				Full		
Numeracy	587	515	0.146	0.014	0.012	0.178
Literacy	587	515	0.111	0.064	0.061	0.184
				Male		
Numeracy	302	262	0.037	0.616	0.628	0.387
Literacy	302	262	0.014	0.870	0.858	0.295
				Female		
Numeracy	285	253	0.264	0.002	0.002	-0.045
Literacy	285	253	0.213	0.010	0.013	0.066

Notes: Local Randomization RD estimates. All columns use data from the Programme for International Assessment of Adult Competencies (PIAAC) in 2014. The unit of analysis is individuals. The outcomes are the standardized trust index, standardized literacy and numeracy scores. The Bandwidth reports the 3-year window length around the cutoff value of 1987. Columns of Obs Left and Obs Right report the number of observations in the right and left window of the cutoff value, 1987. Column of Estimate reports the local randomization RD estimates, that is the impact of Education Reform. The column of Finite Sample p-value displays the corresponding p-value for the relevant local randomization RD estimate in finite samples whereas the column of Large Sample p-value documents the corresponding p-value for the relevant local randomization RD estimate asymptotically. Column of Control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Full, male, and female sample estimates are reported, respectively.

B.9 Robustness Checks on Participation Bias

Table B.11: Education Reform vs Stock Market and Pension Wealth

	(1) Stock Market (in logs)	(2) DC Pension (in logs)
	Full	
Education Reform	-0.028	0.013
	(0.017)	(0.008)
Bandwidth	50.48	37.76
Observations	893811	1900641
	Male	
Education Reform	-0.021	0.005
	(0.019)	(0.010)
Bandwidth	48.09	36.08
Observations	636653	1146511
	Female	
Education Reform	-0.049*	0.028***
	(0.025)	(0.010)
Bandwidth	49.79	34.99
Observations	231400	666926

Notes: Local linear RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the log of the value of stock market portfolios in column 1 and the log of the value of pension portfolios in Turkish Lira in the last column. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for quadratic polynomials of pension participation rate, the month of birth fixed effects, and birth registration certificate region fixed effects with dummy variables for each month and each relevant region, and the regressions in the full sample additionally include controls for gender with a dummy variable of being female. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.12: Education Reform vs Portfolio Choices

	(1) Stock	(2) Risky Asset	(3) Bond	(4) Fund	(5) Liquid Stock	(6) Equity Fund
			Full			
Education Reform	-0.001	-0.002	-0.000	-0.002	0.002	0.000***
	(0.002)	(0.002)	(0.000)	(0.002)	(0.001)	(0.000)
Control Mean	0.55	0.65	0.01	0.32	0.13	0.02
Bandwidth	36.81	48.78	42.36	34.57	48.14	49.74
Observations	654324	864439	755220	613254	864439	2506174
			Male			
Education Reform	-0.002	-0.001	0.001***	0.000	0.002	0.000***
	(0.002)	(0.002)	(0.000)	(0.003)	(0.001)	(0.000)
Control Mean	0.57	0.67	0.01	0.31	0.14	0.02
Bandwidth	38.34	47.53	57.96	39.18	48.36	46.87
Observations	505503	618526	748167	517965	636653	1451167
			Female			
Education Reform	0.003	-0.003	-0.003***	-0.006	-0.000	0.000
	(0.004)	(0.004)	(0.001)	(0.004)	(0.002)	(0.000)
Control Mean	0.48	0.60	0.01	0.37	0.11	0.02
Bandwidth	42.77	50.37	37.08	41.00	48.29	42.97
Observations	198736	235673	175150	193815	227786	823369

Notes: Local linear RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the share of wealth invested in stocks in column 1, risky assets in column 2, bonds in column 3, funds in column 4, and liquid stocks listed in the BIST-30 that tracks the stock performance of the 30 largest companies in Türkiye in column 5 in stock market portfolios. The outcome is the share of wealth invested in equity funds in pension portfolios in the last column. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for quadratic polynomials of pension participation rate, the month of birth fixed effects, and birth registration certificate region fixed effects with dummy variables for each month and each relevant region, and the regressions in the full sample additionally include controls for gender with a dummy variable of being female. Control mean displays the mean of the corresponding outcome for those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.13: Education Reform vs Portfolio Returns

	(1) Stock Market	(2) DC Pension
	Full	
Education Reform	0.508	0.012**
	(0.427)	(0.006)
Control Mean	105.31	20.27
Bandwidth	45.59	40.84
Observations	808937	2049373
	Male	
Education Reform	0.532	0.020***
	(0.533)	(0.006)
Control Mean	107.33	20.23
Bandwidth	46.31	36.14
Observations	608200	1146511
	Female	
Education Reform	-0.338	-0.001
	(0.907)	(0.010)
Control Mean	98.08	20.33
Bandwidth	58.09	39.76
Observations	273839	764488

Notes: Local linear RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the annual rate of return of investor's stock market portfolios in column 1, and pension portfolios in percentages in the last column. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for quadratic polynomials of pension participation rate, the month of birth fixed effects, and birth registration certificate region fixed effects with dummy variables for each month and each relevant region, and the regressions in the full sample additionally include controls for gender with a dummy variable of being female. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.14: Education Reform vs Behavioral Biases and Heuristics

	(1) Default Share	(2) 1/N Heuristic	(3) Portfolio Reshuffling
		Full	
Education Reform	-0.386***	0.001	-0.004*
	(0.148)	(0.001)	(0.002)
Control Mean	46.73	0.37	0.44
Bandwidth	50.57	28.60	30.59
Observations	2551985	1436505	1535546
		Male	
Education Reform	-0.360**	-0.002	-0.003
	(0.167)	(0.002)	(0.002)
Control Mean	50.91	0.34	0.48
Bandwidth	50.66	30.41	34.88
Observations	1574751	946276	1071550
		Female	
Education Reform	-0.264	0.006***	-0.007**
	(0.220)	(0.002)	(0.003)
Control Mean	40.45	0.41	0.38
Bandwidth	41.04	26.29	30.37
Observations	803354	511930	589270

Notes: Local linear RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. In column 1, the outcome is the share of wealth invested in the default fund in percentages in pension portfolios. In column 2, the outcome is a dummy variable equal to one if a pension investor follows the conditional $\frac{1}{N}$ heuristic while allocating money to pension funds. In column 3, the outcome is a dummy variable equal to one if a pension investor changed her funds in a year. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for quadratic polynomials of pension participation rate, the month of birth fixed effects, and birth registration certificate region fixed effects with dummy variables for each month and each relevant region, and the regressions in the full sample additionally include controls for gender with a dummy variable of being female. Control mean displays the mean of the corresponding outcome for those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

B.10 Local Quadratic RD Estimates

Table B.15: Education Reform vs Schooling Outcomes

	(1) Schooling (in years)	(2) Junior High School	(3) High School	(4) College
		Full		
Education Reform	0.403***	0.049***	0.036**	0.032*
	(0.128)	(0.013)	(0.016)	(0.017)
Control Mean	9.02	0.64	0.50	0.27
Bandwidth	86.98	67.14	55.16	58.74
Observations	91154	69926	57247	60380
		Male		
Education Reform	0.356*	0.036*	0.034	0.031
	(0.198)	(0.019)	(0.027)	(0.020)
Control Mean	9.75	0.72	0.55	0.29
Bandwidth	76.24	70.97	59.08	58.10
Observations	38624	35368	29523	29048
		Female		
Education Reform	0.445*	0.096***	0.041	0.033
	(0.255)	(0.023)	(0.028)	(0.023)
Control Mean	8.37	0.53	0.43	0.26
Bandwidth	83.75	95.05	64.15	60.34
Observations	45357	52247	34567	32608

Notes: Local quadratic RD estimates in all columns. All columns use data from the 2018 Household Labor Force Survey by TURKSTAT. The unit of analysis is individuals. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. In column 1, the outcome is years of schooling. In column 2, the outcome is a dummy variable equal to one if an individual has at least a junior high school degree. In column 3, the outcome is a dummy variable equal to one if an individual has at least a high school degree. In column 4, the outcome is a dummy variable equal to one if an individual has at least a college degree. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a quadratic polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects and the regressions in the full sample additionally include controls for gender. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.16: Education Reform vs Stock Market Participation and Asset Ownership

	(1) Transaction Account	(2) Stock Market	(3) Direct Stock	(4) Risky Asset	(5) Liquid Stock	(6) Bond	(7) Fund	(8) Pension Fund	(9) Equity Fund
					Full				
Education Reform	0.410***	0.194***	0.070	0.107**	0.055*	0.003	0.056*	0.078	0.054*
	(0.154)	(0.062)	(0.049)	(0.050)	(0.031)	(0.004)	(0.032)	(0.116)	(0.028)
Control Mean	55.54	9.17	5.28	6.19	2.29	0.10	3.10	24.90	2.90
Bandwidth	31.44	66.05	53.95	63.28	70.68	69.78	68.77	43.05	57.09
Observations	63	133	107	127	141	139	137	87	115
					Male				
Education Reform	0.510***	0.246***	0.107	0.151**	0.056	0.019***	0.078*	0.095	0.119***
	(0.140)	(0.078)	(0.075)	(0.071)	(0.050)	(0.006)	(0.044)	(0.154)	(0.038)
Control Mean	72.27	13.28	8.02	9.23	3.52	0.14	4.41	30.02	3.52
Bandwidth	30.27	63.04	55.84	60.55	65.34	91.55	76.97	47.45	89.23
Observations	61	127	111	121	131	183	153	95	179
					Female				
Education Reform	0.312*	0.158***	0.080*	0.087*	0.048**	-0.016***	0.038	0.091	0.056*
	(0.180)	(0.057)	(0.042)	(0.046)	(0.020)	(0.004)	(0.031)	(0.134)	(0.032)
Control Mean	38.21	4.93	2.43	3.05	1.03	0.06	1.80	19.72	2.40
Bandwidth	35.17	74.74	74.29	80.07	83.83	65.49	69.27	47.83	60.66
Observations	71	149	149	161	167	131	139	95	121

Notes: Local quadratic RD estimates in all columns. All columns use data from population numbers assembled by TURKSTAT and the relevant investor numbers assembled through administrative records by Borsa Istanbul Group in 2019 and 2021. The unit of analysis is the month-year birth cohorts. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the percentage of those having a transaction account in column 1, and participating in the stock market through any financial instruments traded in the stock market in column 2. The outcome is the percentage of holding directly stocks in column 3, risky assets in column 4, liquid stocks in column 5, bonds in column 6, and funds in their stock market portfolio in column 7. In column 8, the outcome is the percentage of those participating in DC pension plans, and in column 9 holding equity funds in their DC pension portfolio. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a quadratic polynomial function on each side of the cutoff value. All regressions include controls for the month of birth. Control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.17: Education Reform vs Stock Market and Pension Wealth

	(1) Stock Market (in logs)	(2) DC Pension (in logs)
	Full	
Education Reform	-0.032	0.014
	(0.026)	(0.010)
Bandwidth	52.42	54.42
Observations	927699	2751779
	Male	
Education Reform	-0.024	0.005
	(0.030)	(0.011)
Bandwidth	52.40	55.67
Observations	682633	1731225
	Female	
Education Reform	-0.031	0.029**
	(0.032)	(0.014)
Bandwidth	62.39	42.92
Observations	292028	823369

Notes: Local quadratic RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the log of the value of stock market portfolios in column 1 and the log of the value of pension portfolios in Turkish Lira in the last column. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a quadratic polynomial function on each side of the cutoff value. All regressions include controls for the month of birth and for the birth registration certificate region fixed effects and the regressions in the full sample additionally have controls for gender. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.18: Education Reform vs Portfolio Choices

	(1) Stock	(2) Risky Asset	(3) Bond	(4) Fund	(5) Liquid Stock	(6) Equity Fund
			Full			
Education Reform	-0.000	-0.003	-0.000	-0.002	0.001	0.000**
	(0.003)	(0.003)	(0.000)	(0.003)	(0.001)	(0.000)
Control Mean	0.54	0.65	0.01	0.33	0.13	0.02
Bandwidth	50.46	55.33	68.04	49.23	57.60	76.87
Observations	893811	982059	1210679	878229	1017341	3929283
			Male			
Education Reform	-0.001	-0.002	0.001**	-0.000	0.001	0.000*
	(0.003)	(0.003)	(0.000)	(0.003)	(0.002)	(0.000)
Control Mean	0.57	0.67	0.01	0.31	0.14	0.02
Bandwidth	51.12	56.21	69.31	51.73	50.41	57.16
Observations	670196	735823	901753	670196	658138	1798166
			Female			
Education Reform	0.001	-0.003	-0.003***	-0.005	-0.001	0.000
	(0.006)	(0.005)	(0.001)	(0.006)	(0.002)	(0.000)
Control Mean	0.47	0.59	0.01	0.37	0.11	0.02
Bandwidth	55.56	67.44	70.62	58.63	65.78	84.51
Observations	259952	316453	331113	273839	306270	1668592

Notes: Local quadratic RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the share of wealth invested in stocks in column 1, risky assets in column 2, bonds in column 3, funds in column 4, and liquid stocks listed in the BIST-30 that tracks the stock performance of the 30 largest companies in Türkiye in column 5 in stock market portfolios. The outcome is the share of wealth invested in equity funds in pension portfolios in the last column. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a quadratic polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects and birth registration certificate region fixed effects, and the regressions in the full sample additionally include controls for gender. Control mean displays the mean of the corresponding outcome for those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.19: Education Reform vs Portfolio Returns

	(1) Stock Market	(2) DC Pension
	Full	
Education Reform	0.283	0.008
	(0.626)	(0.008)
Control Mean	104.55	20.26
Bandwidth	56.97	35.63
Observations	1000546	1780686
	Male	
Education Reform	0.655	0.020**
	(0.727)	(0.009)
Control Mean	106.96	20.22
Bandwidth	53.45	35.23
Observations	694984	1097773
	Female	
Education Reform	-0.597	-0.009
	(1.298)	(0.012)
Control Mean	97.52	20.33
Bandwidth	63.18	38.19
Observations	296339	745603

Notes: Local quadratic RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the annual rate of return of investor's stock market portfolios in column 1, and pension portfolios in percentages in the last column. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a quadratic polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects and birth registration certificate region fixed effects, and the regressions in the full sample additionally include controls for gender. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.20: Education Reform vs Behavioral Biases and Heuristics

	(1) Contribution	(2) Default	(3) Only Default	(4) Default Share	(5) 1/N Heuristic	(6) Portfolio Reshuffling
			Full			
Education Reform	0.131*	-0.028	-0.063	-0.265*	0.000	-0.005*
	(0.072)	(0.059)	(0.048)	(0.155)	(0.002)	(0.003)
Control Mean	14.17	12.99	10.91	46.63	0.37	0.44
Bandwidth	57.97	45.42	43.81	52.22	40.69	36.74
Observations	115	91	87	2650108	2049373	1858355
			Male			
Education Reform	0.066	-0.041	-0.117	-0.307	-0.002	-0.002
	(0.082)	(0.116)	(0.115)	(0.200)	(0.002)	(0.003)
Control Mean	15.95	16.82	14.39	50.66	0.34	0.48
Bandwidth	48.77	42.95	40.58	55.96	42.89	37.66
Observations	97	85	81	1731225	1324678	1173036
			Female			
Education Reform	0.281***	-0.014	-0.001	-0.195	0.005**	-0.008**
	(0.098)	(0.097)	(0.082)	(0.279)	(0.002)	(0.004)
Control Mean	12.30	9.05	7.38	40.35	0.41	0.38
Bandwidth	67.76	43.86	46.03	43.67	37.81	39.50
Observations	135	87	93	843404	727605	764488

Notes: Local quadratic RD estimates in all columns. Columns 1-3 use data from population numbers assembled by TURKSTAT and the relevant investor numbers assembled through administrative records by Borsa Istanbul Group in 2019 and 2021. Columns 4-6 use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is the month-year birth cohorts in columns 1-3 and individuals in the remaining columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the percentage of those actively contributing to pension plans in column 1, holding the default fund in column 2, and only the default fund in column 3 in pension portfolios. In column 4, the outcome is the share of wealth invested in the default fund in percentages in pension portfolios. In column 5, the outcome is a dummy variable equal to one if a pension investor follows the conditional $\frac{1}{N}$ heuristic while allocating money to pension funds. In column 6, the outcome is a dummy variable equal to one if a pension investor changed her funds in a year. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a quadratic polynomial function on each side of the cutoff value. All regressions include controls for the month of birth for columns 1-3 and columns 4-6 also include controls for the birth registration certificate region fixed effects, and the regressions in the full sample additionally include controls for gender. Control mean displays the mean of the corresponding outcome for those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.21: Education Reform vs First Enrollment Type

	(1) Voluntary	(2) Automatic	(3) Employer
		Full	
Education Reform	0.094	-0.068	0.030
	(0.091)	(0.083)	(0.025)
Control Mean	23.86	27.10	1.62
Bandwidth	48.69	50.74	57.96
Observations	97	101	115
		Male	
Education Reform	0.206**	-0.272	0.009
	(0.098)	(0.171)	(0.042)
Control Mean	27.42	37.29	2.29
Bandwidth	32.53	45.86	57.42
Observations	65	91	115
		Female	
Education Reform	0.096	0.129	0.047**
	(0.107)	(0.119)	(0.021)
Control Mean	19.83	16.69	0.92
Bandwidth	49.78	55.46	63.83
Observations	99	111	127

Notes: Local quadratic RD estimates in all columns. All columns use data from population numbers assembled by TURKSTAT and the relevant investor numbers assembled through administrative records by Borsa Istanbul Group in 2019 and 2021. The unit of analysis is the month-year birth cohorts in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the percentage of those enrolled first through DC voluntary retirement accounts in column 1, DC automatic enrollment retirement accounts in column 2, and DC employer-sponsorship retirement accounts in column 3. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with a quadratic polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects. Control mean displays the mean and standard deviation of the corresponding outcome of those born before January 1, 1987. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

B.11 Local Kink RD Estimates

Table B.22: Education Reform vs Schooling Outcomes

	(1) Schooling (in years)	(2) Junior High School	(3) High School	(4) College
		Full		
Education Reform	-0.008	-0.005***	-0.003	-0.001
	(0.011)	(0.001)	(0.002)	(0.002)
Control Mean	9.09	0.64	0.50	0.28
Bandwidth	73.95	65.70	50.30	49.05
Observations	77043	67860	52433	51460
		Male		
Education Reform	-0.003	-0.005***	-0.004	0.001
	(0.015)	(0.001)	(0.003)	(0.002)
Control Mean	9.80	0.72	0.56	0.29
Bandwidth	71.40	69.32	53.96	60.43
Observations	35806	34812	26578	30358
		Female		
Education Reform	-0.020	-0.002	-0.002	-0.002
	(0.022)	(0.001)	(0.003)	(0.003)
Control Mean	8.48	0.52	0.44	0.26
Bandwidth	68.46	96.20	59.20	50.43
Observations	36786	53191	31788	27211

Notes: Local kink RD estimates in all columns. All columns use data from the 2018 Household Labor Force Survey by TURKSTAT. The unit of analysis is individuals. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. In column 1, the outcome is years of schooling. In column 2, the outcome is a dummy variable equal to one if an individual has at least a junior high school degree. In column 3, the outcome is a dummy variable equal to one if an individual has at least a high school degree. In column 4, the outcome is a dummy variable equal to one if an individual has at least a college degree. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with the first derivative of a linear function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects and the regressions in the full sample additionally include controls for gender. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.23: Education Reform vs Stock Market Participation and Asset Ownership

	(1) Transaction Account	(2) Stock Market	(3) Direct Stock	(4) Risky Asset	(5) Liquid Stock	(6) Bond	(7) Fund	(8) Pension Fund	(9) Equity Fund
					Full				
Education Reform	-0.025	0.001	-0.011*	-0.007	0.001	0.000	-0.000	-0.016	-0.001
	(0.032)	(0.005)	(0.006)	(0.005)	(0.002)	(0.000)	(0.003)	(0.015)	(0.002)
Control Mean	55.54	9.16	5.25	6.19	2.29	0.10	3.09	24.80	2.91
Bandwidth	31.03	63.29	42.36	51.96	66.95	62.23	65.16	40.13	60.80
Observations	63	127	85	103	133	125	131	81	121
					Male				
Education Reform	0.021	-0.010	-0.017*	-0.009	-0.004	0.000	0.001	-0.013	-0.000
	(0.029)	(0.007)	(0.010)	(0.008)	(0.004)	(0.000)	(0.003)	(0.019)	(0.002)
Control Mean	72.21	13.26	7.98	9.23	3.51	0.14	4.39	29.97	3.50
Bandwidth	29.35	57.72	44.13	49.68	59.00	81.27	73.78	42.31	83.26
Observations	59	115	89	99	117	163	147	85	167
					Female				
Education Reform	-0.031	0.009**	0.003	0.007**	0.002	0.000	-0.004	-0.024	-0.000
	(0.034)	(0.004)	(0.003)	(0.003)	(0.002)	(0.000)	(0.003)	(0.015)	(0.003)
Control Mean	38.21	4.94	2.45	3.04	1.02	0.07	1.77	19.70	2.39
Bandwidth	35.90	70.59	69.20	74.05	72.52	66.82	58.79	46.47	55.45
Observations	71	141	139	149	145	133	117	93	111

Notes: Local kink RD estimates in all columns. All columns use data from population numbers assembled by TURKSTAT and the relevant investor numbers assembled through administrative records by Borsa Istanbul Group in 2019 and 2021. The unit of analysis is the month-year birth cohorts. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the percentage of those having a transaction account in column 1, and participating in the stock market through any financial instruments traded in the stock market in column 2. The outcome is the percentage of holding directly stocks in column 3, risky assets in column 4, liquid stocks in column 5, bonds in column 6, and funds in their stock market portfolio in column 7. In column 8, the outcome is the percentage of those participating in DC pension plans, and in column 9 holding equity funds in their DC pension portfolio. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with the first derivative of a linear function on each side of the cutoff value. All regressions include controls for the month of birth. Control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.24: Education Reform vs Stock Market and Pension Wealth

	(1) Stock Market (in logs)	(2) DC Pension (in logs)
	Full	
Education Reform	-0.002	0.002**
	(0.002)	(0.001)
Bandwidth	54.76	50.82
Observations	963363	2551985
	Male	
Education Reform	-0.002	0.001
	(0.003)	(0.001)
Bandwidth	50.53	53.10
Observations	658138	1664757
	Female	
Education Reform	0.001	0.004***
	(0.003)	(0.002)
Bandwidth	58.10	45.90
Observations	273839	883063

Notes: Local kink RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the log of the value of stock market portfolios in column 1 and the log of the value of pension portfolios in Turkish Lira in the last column. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with the first derivative of a linear function on each side of the cutoff value. All regressions include controls for the month of birth and for the birth registration certificate region fixed effects and the regressions in the full sample additionally have controls for gender. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.25: Education Reform vs Portfolio Choices

	(1) Stock	(2) Risky Asset	(3) Bond	(4) Fund	(5) Liquid Stock	(6) Equity Fund
				Full		
Education Reform	-0.000	0.000	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Control Mean	0.55	0.65	0.01	0.33	0.13	0.02
Bandwidth	45.69	54.16	65.64	47.76	50.47	63.84
Observations	808937	963363	1155637	839920	893811	3220574
				Male		
Education Reform	-0.001**	-0.000	0.000	0.000	-0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)
Control Mean	0.57	0.67	0.01	0.31	0.14	0.02
Bandwidth	42.91	52.16	64.50	46.58	42.60	57.46
Observations	556484	682633	836061	608200	556484	1798166
				Female		
Education Reform	0.000	0.000	0.000	-0.000	0.000	-0.000
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.003)
Control Mean	0.47	0.60	0.01	0.37	0.11	0.02
Bandwidth	47.89	57.00	76.38	53.34	60.14	66.28
Observations	221394	264723	359211	249795	284176	1292849

Notes: Local kink RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the share of wealth invested in stocks in column 1, risky assets in column 2, bonds in column 3, funds in column 4, and liquid stocks listed in the BIST-30 that tracks the stock performance of the 30 largest companies in Türkiye in column 5 in stock market portfolios. The outcome is the share of wealth invested in equity funds in pension portfolios in the last column. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with the first derivative of a linear function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects and birth registration certificate region fixed effects, and the regressions in the full sample additionally include controls for gender. Control mean displays the mean of the corresponding outcome for those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.26: Education Reform vs Portfolio Returns

	(1) Stock Market	(2) DC Pension
	Full	
Education Reform	0.001	0.001
	(0.064)	(0.001)
Control Mean	104.73	20.26
Bandwidth	54.11	35.02
Observations	963363	1780686
	Male	
Education Reform	-0.020	-0.000
	(0.070)	(0.002)
Control Mean	106.96	20.22
Bandwidth	53.08	35.16
Observations	694984	1097773
	Female	
Education Reform	0.078	0.002
	(0.127)	(0.002)
Control Mean	98.24	20.33
Bandwidth	57.87	36.93
Observations	269174	711844

Notes: Local kink RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the annual rate of return of investor's stock market portfolios in column 1, and pension portfolios in percentages in the last column. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with the first derivative of a linear function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects and birth registration certificate region fixed effects, and the regressions in the full sample additionally include controls for gender. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.27: Education Reform vs Behavioral Biases and Heuristics

	(1) Contribution	(2) Default	(3) Only Default	(4) Default Share	(5) 1/N Heuristic	(6) Portfolio Reshuffling
			Full			
Education Reform	0.009	-0.014*	-0.008	-0.009	0.000	-0.001*
	(0.006)	(0.008)	(0.006)	(0.019)	(0.000)	(0.000)
Control Mean	14.20	12.98	10.91	46.94	0.37	0.44
Bandwidth	63.27	41.55	45.94	44.12	33.28	30.08
Observations	127	83	91	2253462	1690646	1535546
			Male			
Education Reform	-0.004	-0.015	-0.003	0.012	0.000	-0.000
	(0.007)	(0.014)	(0.015)	(0.022)	(0.000)	(0.000)
Control Mean	16.05	16.83	14.42	50.95	0.34	0.48
Bandwidth	57.11	41.13	37.79	49.88	33.74	35.88
Observations	115	83	75	1546455	1042058	1097773
			Female			
Education Reform	0.011	-0.013	-0.016*	-0.020	0.000	-0.001**
	(0.009)	(0.011)	(0.009)	(0.030)	(0.000)	(0.001)
Control Mean	12.26	9.06	7.37	40.30	0.41	0.38
Bandwidth	63.74	44.30	44.40	46.99	37.01	34.43
Observations	127	89	89	901950	727605	666926

Notes: Local kink RD estimates in all columns. Columns 1-3 use data from population numbers assembled by TURKSTAT and the relevant investor numbers assembled through administrative records by Borsa Istanbul Group in 2019 and 2021. Columns 4-6 use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is the month-year birth cohorts in columns 1-3 and individuals in the remaining columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the percentage of those actively contributing to pension plans in column 1, holding the default fund in column 2, and only the default fund in column 3 in pension portfolios. In column 4, the outcome is the share of wealth invested in the default fund in percentages in pension portfolios. In column 5, the outcome is a dummy variable equal to one if a pension investor follows the conditional $\frac{1}{N}$ heuristic while allocating money to pension funds. In column 6, the outcome is a dummy variable equal to one if a pension investor changed her funds in a year. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with the first derivative of a linear function on each side of the cutoff value. All regressions include controls for the month of birth for columns 1-3 and columns 4-6 also include controls for the birth registration certificate region fixed effects, and the regressions in the full sample additionally include controls for gender. Control mean displays the mean of the corresponding outcome for those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.28: Education Reform vs First Enrollment Type

	(1) Voluntary	(2) Automatic	(3) Employer
		Full	
Education Reform	-0.021	0.010	0.005**
	(0.016)	(0.011)	(0.002)
Control Mean	23.49	27.23	1.63
Bandwidth	35.16	44.72	52.79
Observations	71	89	105
		Male	
Education Reform	-0.017	0.007	0.002
	(0.017)	(0.021)	(0.004)
Control Mean	27.49	37.38	2.29
Bandwidth	37.99	42.43	55.93
Observations	75	85	111
		Female	
Education Reform	-0.040***	0.020	0.011***
	(0.012)	(0.013)	(0.002)
Control Mean	19.93	16.77	0.94
Bandwidth	52.65	49.58	55.84
Observations	105	99	111

Notes: Local kink RD estimates in all columns. All columns use data from population numbers assembled by TURKSTAT and the relevant investor numbers assembled through administrative records by Borsa Istanbul Group in 2019 and 2021. The unit of analysis is the month-year birth cohorts in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the percentage of those enrolled first through DC voluntary retirement accounts in column 1, DC automatic enrollment retirement accounts in column 2, and DC employer-sponsorship retirement accounts in column 3. RD estimates have the optimal bandwidth with a triangular type kernel function calculated through the algorithm by Calonico *et al.* (2014) in all columns. Each column reports RD estimates with the first derivative of a linear function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects. Control mean displays the mean and standard deviation of the corresponding outcome of those born before January 1, 1987. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

B.12 Local Linear RD Estimates with Fixed Bandwidth

Table B.29: Education Reform vs Schooling Outcomes

	(1) Schooling (in years)	(2) Junior High School	(3) High School	(4) College
		Full		
Education Reform	0.460***	0.087***	0.051***	0.026**
	(0.105)	(0.010)	(0.011)	(0.011)
Control Mean	9.20	0.64	0.49	0.27
Bandwidth	60.00	60.00	60.00	60.00
Observations	61311	61311	61311	61311
		Male		
Education Reform	0.406***	0.064***	0.042**	0.016
	(0.141)	(0.014)	(0.017)	(0.014)
Control Mean	9.87	0.73	0.55	0.29
Bandwidth	60.00	60.00	60.00	60.00
Observations	29523	29523	29523	29523
		Female		
Education Reform	0.514***	0.110***	0.060***	0.035**
	(0.195)	(0.018)	(0.020)	(0.015)
Control Mean	8.52	0.56	0.44	0.26
Bandwidth	60.00	60.00	60.00	60.00
Observations	31788	31788	31788	31788

Notes: Local linear RD estimates in all columns. All columns use data from the 2018 Household Labor Force Survey by TURKSTAT. The unit of analysis is individuals. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. In column 1, the outcome is years of schooling. In column 2, the outcome is a dummy variable equal to one if an individual has at least a junior high school degree. In column 3, the outcome is a dummy variable equal to one if an individual has at least a high school degree. In column 4, the outcome is a dummy variable equal to one if an individual has at least a college degree. RD estimates have a fixed bandwidth of 60 with a triangular-type kernel function in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects and the regressions in the full sample additionally include controls for gender. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.30: Education Reform vs Stock Market Participation and Asset Ownership

	(1) Transaction Account	(2) Stock Market	(3) Direct Stock	(4) Risky Asset	(5) Liquid Stock	(6) Bond	(7) Fund	(8) Pension Fund	(9) Equity Fund
					Full				
Education Reform	1.101***	0.204***	0.142***	0.142***	0.059***	0.004	0.041*	0.093	0.089***
	(0.110)	(0.045)	(0.034)	(0.037)	(0.022)	(0.003)	(0.023)	(0.072)	(0.021)
Control Mean	55.81	9.14	5.25	6.18	2.27	0.10	3.06	25.05	2.91
Bandwidth	60.00	60.00	60.00	60.00	60.00	60.00	60.00	60.00	60.00
Observations	119	119	119	119	119	119	119	119	119
					Male				
Education Reform	1.018***	0.250***	0.196***	0.199***	0.075**	0.015***	0.042	-0.045	0.099***
	(0.107)	(0.058)	(0.053)	(0.054)	(0.035)	(0.005)	(0.034)	(0.089)	(0.031)
Control Mean	72.77	13.26	7.98	9.23	3.50	0.13	4.32	30.11	3.41
Bandwidth	60.00	60.00	60.00	60.00	60.00	60.00	60.00	60.00	60.00
Observations	119	119	119	119	119	119	119	119	119
					Female				
Education Reform	1.192***	0.157***	0.088***	0.085**	0.042**	-0.009***	0.040*	0.241**	0.080***
	(0.143)	(0.043)	(0.032)	(0.035)	(0.017)	(0.003)	(0.021)	(0.095)	(0.024)
Control Mean	38.34	4.89	2.45	3.05	1.01	0.06	1.77	19.85	2.40
Bandwidth	60.00	60.00	60.00	60.00	60.00	60.00	60.00	60.00	60.00
Observations	119	119	119	119	119	119	119	119	119

Notes: Local linear RD estimates in all columns. All columns use data from population numbers assembled by TURKSTAT and the relevant investor numbers assembled through administrative records by Borsa Istanbul Group in 2019 and 2021. The unit of analysis is the month-year birth cohorts. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the percentage of those having a transaction account in column 1, and participating in the stock market through any financial instruments traded in the stock market in column 2. The outcome is the percentage of holding directly stocks in column 3, risky assets in column 4, liquid stocks in column 5, bonds in column 6, and funds in their stock market portfolio in column 7. In column 8, the outcome is the percentage of those participating in DC pension plans, and in column 9 holding equity funds in their DC pension portfolio. RD estimates have a fixed bandwidth of 60 with a triangular-type kernel function in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.31: Education Reform vs Portfolio Choices

	(1) Stock	(2) Risky Asset	(3) Bond	(4) Fund	(5) Liquid Stock	(6) Equity Fund
				Full		
Education Reform	0.002	0.001	0.000	-0.003	0.002*	0.000***
	(0.002)	(0.002)	(0.000)	(0.002)	(0.001)	(0.000)
Control Mean	0.54	0.65	0.01	0.33	0.13	0.02
Bandwidth	60.00	60.00	60.00	60.00	60.00	60.00
Observations	1049097	1049097	1049097	1049097	1049097	3006699
				Male		
Education Reform	0.003	0.001	0.001***	-0.002	0.003*	0.000***
	(0.002)	(0.002)	(0.000)	(0.002)	(0.001)	(0.020)
Control Mean	0.54	0.65	0.01	0.33	0.13	0.02
Bandwidth	60.00	60.00	60.00	60.00	60.00	60.00
Observations	771442	771442	771442	771442	771442	1856373
				Female		
Education Reform	0.002	-0.002	-0.002***	-0.005	0.000	0.000
	(0.004)	(0.004)	(0.001)	(0.004)	(0.002)	(0.030)
Control Mean	0.54	0.65	0.01	0.33	0.13	0.02
Bandwidth	60.00	60.00	60.00	60.00	60.00	60.00
Observations	277655	277655	277655	277655	277655	1150326

Notes: Local linear RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the share of wealth invested in stocks in column 1, risky assets in column 2, bonds in column 3, funds in column 4, and liquid stocks listed in the BIST-30 that tracks the stock performance of the 30 largest companies in Türkiye in column 5 in stock market portfolios. The outcome is the share of wealth invested in equity funds in pension portfolios in the last column. RD estimates have a fixed bandwidth of 60 with a triangular-type kernel function in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects and birth registration certificate region fixed effects, and the regressions in the full sample additionally include controls for gender. Control mean displays the mean of the corresponding outcome for those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.32: Education Reform vs Stock Market and Pension Wealth

	(1) Stock Market (in logs)	(2) DC Pension (in logs)
	Full	
Education Reform	-0.016	0.014*
	(0.016)	(0.008)
Bandwidth	60.00	60.00
Observations	1049097	3006699
	Male	
Education Reform	-0.010	0.005
	(0.018)	(0.009)
Bandwidth	60.00	60.00
Observations	771442	1856373
	Female	
Education Reform	-0.032	0.028***
	(0.026)	(0.010)
Bandwidth	60.00	60.00
Observations	277655	1150326

Notes: Local linear RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the log of the value of stock market portfolios in column 1 and the log of the value of pension portfolios in Turkish Lira in the last column. RD estimates have a fixed bandwidth of 60 with a triangular-type kernel function in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth and for the birth registration certificate region fixed effects and the regressions in the full sample additionally have controls for gender. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.33: Education Reform vs Portfolio Returns

	(1) Stock Market	(2) DC Pension
	Full	
Education Reform	0.620	0.017***
	(0.394)	(0.006)
Control Mean	104.13	20.29
Bandwidth	60.00	60.00
Observations	1049097	3006699
	Male	
Education Reform	0.885*	0.022***
	(0.484)	(0.006)
Control Mean	104.13	20.26
Bandwidth	60.00	60.00
Observations	771442	1856373
	Female	
Education Reform	-0.150	0.009
	(0.903)	(0.010)
Control Mean	104.13	20.34
Bandwidth	60.00	60.00
Observations	277655	1150326

Notes: Local linear RD estimates in all columns. All columns use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is individuals in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the annual rate of return of investor's stock market portfolios in column 1, and pension portfolios in percentages in the last column. RD estimates have a fixed bandwidth of 60 with a triangular-type kernel function in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth fixed effects and birth registration certificate region fixed effects, and the regressions in the full sample additionally include controls for gender. The control mean displays the mean of the corresponding outcome of those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.34: Education Reform vs Behavioral Biases and Heuristics

	(1) Contribution	(2) Default	(3) Only Default	(4) Default Share	(5) 1/N Heuristic	(6) Portfolio Reshuffling
			Full			
Education Reform	0.111**	-0.096***	-0.102***	-0.454***	0.001	-0.004**
	(0.051)	(0.036)	(0.029)	(0.168)	(0.001)	(0.002)
Control Mean	14.18	12.94	10.84	46.40	0.37	0.44
Bandwidth	60.00	60.00	60.00	60.00	60.00	60.00
Observations	119	119	119	3006699	3006699	3006699
			Male			
Education Reform	-0.002	-0.192***	-0.175***	-0.369**	-0.001	-0.004*
	(0.053)	(0.063)	(0.057)	(0.169)	(0.001)	(0.002)
Control Mean	16.06	16.70	14.23	50.57	0.34	0.47
Bandwidth	60.00	60.00	60.00	60.00	60.00	60.00
Observations	119	119	119	1856373	1856373	1856373
			Female			
Education Reform	0.228***	0.007	-0.023	-0.581**	0.004**	-0.005*
	(0.071)	(0.054)	(0.046)	(0.246)	(0.002)	(0.003)
Control Mean	12.24	9.06	7.35	39.87	0.41	0.38
Bandwidth	60.00	60.00	60.00	60.00	60.00	60.00
Observations	119	119	119	1150326	1150326	1150326

Notes: Local linear RD estimates in all columns. Columns 1-3 use data from population numbers assembled by TURKSTAT and the relevant investor numbers assembled through administrative records by Borsa Istanbul Group in 2019 and 2021. Columns 4-6 use administrative data covering the universe of all stock market and DC pension portfolios in 2019-2021 assembled by Borsa Istanbul Group. The unit of analysis is the month-year birth cohorts in columns 1-3 and individuals in the remaining columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the percentage of those actively contributing to pension plans in column 1, holding the default fund in column 2, and only the default fund in column 3 in pension portfolios. In column 4, the outcome is the share of wealth invested in the default fund in percentages in pension portfolios. In column 5, the outcome is a dummy variable equal to one if a pension investor follows the conditional $\frac{1}{N}$ heuristic while allocating money to pension funds. In column 6, the outcome is a dummy variable equal to one if a pension investor changed her funds in a year. RD estimates have a fixed bandwidth of 60 with a triangular-type kernel function in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cutoff value. All regressions include controls for the month of birth for columns 1-3 and columns 4-6 also include controls for the birth registration certificate region fixed effects, and the regressions in the full sample additionally include controls for gender. Control mean displays the mean of the corresponding outcome for those born before January 1, 1987. Standard errors are clustered at the month-year birth cohort level. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.35: Education Reform vs First Enrollment Type

	(1) Voluntary	(2) Automatic	(3) Employer
		Full	
Education Reform	0.253***	0.032	0.105***
	(0.070)	(0.056)	(0.017)
Control Mean	24.07	26.84	1.61
Bandwidth	60.00	60.00	60.00
Observations	119	119	119
		Male	
Education Reform	0.080	0.023	0.087***
	(0.075)	(0.092)	(0.028)
Control Mean	27.99	36.81	2.28
Bandwidth	60.00	60.00	60.00
Observations	119	119	119
		Female	
Education Reform	0.433***	0.048	0.124***
	(0.082)	(0.082)	(0.015)
Control Mean	20.03	16.58	0.93
Bandwidth	60.00	60.00	60.00
Observations	119	119	119

Notes: Local linear RD estimates in all columns. All columns use data from population numbers assembled by TURKSTAT and the relevant investor numbers assembled through administrative records by Borsa Istanbul Group in 2019 and 2021. The unit of analysis is the month-year birth cohorts in all columns. The main explanatory variable namely Education Reform is a dummy variable equal to one for those born after January 1, 1987. The outcome is the percentage of those enrolled first through DC voluntary retirement accounts in column 1, DC automatic enrollment retirement accounts in column 2, and DC employer-sponsorship retirement accounts in column 3. RD estimates have a fixed bandwidth of 60 with a triangular-type kernel function in all columns. Each column reports RD estimates with a linear polynomial function on each side of the cut-off value. All regressions include controls for the month of birth fixed effects. Control mean displays the mean and standard deviation of the corresponding outcome of those born before January 1, 1987. Robust standard errors are in parentheses. Full, male, and female sample estimates are reported, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

C Data Appendix

Variables for Schooling Outcomes

Schooling (in years) is generated by assigning 0, 5, 8, 11, 15, and 17 for those who have no degree, at least a primary school degree, at least a junior high school degree, at least high school degree, at least college degree, and at least master degree, respectively.

Junior High School is a dummy variable equal to 1 if a specific individual at least holds a junior high school degree with 8 years of schooling.

High School is a dummy variable equal to 1 if a specific individual at least holds a high school degree with 11 years of schooling.

College is a dummy variable equal to 1 if a specific individual at least holds a college degree with 13-15 years of schooling.

Variables for the Participation and Asset Ownership

Transaction Account is the percentage of those having transaction accounts generated by dividing the number of individuals with a transaction account by the number of individuals in the general population in the specific month-year birth cohort on December 31, 2021.

Stock Market is the percentage of those participating in the stock market i.e. those having a positive amount of wealth in the transaction account generated by dividing the number of individuals with positive balances by the number of individuals in the general population in the specific month-year birth cohort on December 31, 2021.

Direct Stock is the percentage of those holding stocks in their stock market portfolio generated by dividing the number of individuals owning stocks by the number of individuals in the general population in the specific month-year birth cohort on December 31, 2021.

Risky Asset is the percentage of those holding risky assets in their stock market portfolio generated by dividing the number of individuals owning risky assets by the

number of individuals in the general population in the specific month-year birth cohort on December 31, 2021. Risky assets exclude money market funds.

Liquid Stockis the percentage of those holding blue-chip stocks in their stock market portfolio generated by dividing the number of individuals owning blue-chip stocks by the number of individuals in the general population in the specific month-year birth cohort on December 31, 2021. The blue-chip stocks are the stocks that are included in the BIST-30 Index, which tracks the 30 largest market capitalization companies on December 31, 2021.

Bond is the percentage of those holding bonds in their stock market portfolio generated by dividing the number of individuals owning bonds by the number of individuals in the general population in the specific month-year birth cohort on December 31, 2021.

Fund is the percentage of those holding funds in their stock market portfolio generated by dividing the number of individuals owning funds by the number of individuals in the general population in the specific month-year birth cohort on December 31, 2021.

Pension Fund is the percentage of those having a positive amount in their retirement accounts in any pension plans generated by dividing the number of individuals with positive balances by the number of individuals in the general population in the specific month-year birth cohort on December 31, 2019.

Equity Fund is the percentage of those holding equity funds in their portfolios generated by dividing the number of individuals with equity funds in their portfolios by the number of individuals in the general population in the specific month-year birth cohort on December 31, 2019.

Variables for Stock Market and Pension Wealth

Stock Market (in logs) is the logarithm of the portfolio size on December 31, 2021.

DC Pension (in logs) is the log of the total wealth an investor has accumulated or simply the portfolio size in Turkish Liras on December 31, 2019.

Variables for Portfolio Choices

Stock is the share of wealth invested in stocks directly in stock market portfolios on December 31, 2021.

Risky Assets is the share of wealth invested in risky assets in stock market portfolios on December 31, 2021.

Bond is the share of wealth invested in bonds in stock market portfolios on December 31, 2021.

Liquid Stock is the share of wealth invested in blue-chip stocks in stock market portfolios on December 31, 2021.

Equity Fund is the percentage of wealth invested in equity funds in DC pension portfolios on December 31, 2019.

Variable for Portfolio Returns

Stock Market is the annual rate of return of stock market portfolios on December 31, 2021, in percentages from December 31, 2021, to December 31, 2022.

DC Pension is the annual realized rate of return of DC pension portfolios in percentages from December 31, 2019, to December 31, 2020.

Variables for Behavioral Biases and Heuristics

Contributor Rate is the percentage of those contributing to any pension plans generated by dividing the number of individuals contributing to any pension plans by the number of individuals in the general population in the specific month-year birth cohort on December 31, 2019.

Default is the percentage of those holding default funds in their portfolios generated by dividing the number of individuals with default funds in their portfolios by the number

of individuals in the general population in the specific month-year birth cohort. We treat a fund as the default fund if it is the starting fund or automatic enrollment standard fund on December 31, 2019.

Only Default is the percentage of those holding only default funds in their portfolios generated by dividing the number of individuals with only default funds in their portfolios by the number of individuals in the general population in the specific month-year birth cohort on December 31, 2019.

1/N Heuristic is a dummy variable equal to one if the condition we explain below is satisfied following Huberman and Jiang (2006):
Let s_{ij} be the share of investor i's contribution in fund j, and n_i is the total number of funds in i's portfolio, thus $\sum_{j=1}^{n_i} s_{ij} = 1$. Then, the Herfindahl index, defined for each investor i's portfolio as the sum of the squared fractions of contributions in each fund as follows:
$H_i = \sum_{j=1}^{n_i} s_j^2$
The value H_i is bounded between $\frac{1}{n_i}$ and 1 and it is equal to $\frac{1}{n_i}$ if investor i equally divides the contribution amount among n_i funds.
I treat an investor with the Herfindahl index close to $\frac{1}{n}$ as an investor prone to $\frac{1}{n}$ rule or naive diversification. Accordingly, I classify an investor as an investor with the $\frac{1}{n}$ rule if her Herfindahl index is bounded from above by the index that would lead to a portfolio in which the total deviation from an $\frac{1}{n}$ allocation is 20%.

Portfolio Reshuffling is a dummy variable equal to one if a pension investor buys a fund that has not been held in her portfolio over the year 2020.

Variables for First Enrollment Type

Voluntary is the percentage of those first participating in DC pension plans through voluntary pension plans generated by dividing the number of individuals first participating in DC pension plans through voluntary pension plans by the number of individuals in the general population in the specific month-year birth cohort on December 31, 2019.

Automatic is the percentage of those first participating in DC pension plans through

automatic pension plans generated by dividing the number of individuals first participating in DC pension plans through automatic pension plans by the number of individuals in the general population in the specific month-year birth cohort on December 31, 2019.

Employer Sponsored Pension Participation Rate is the percentage of those first participating in DC pension plans through employer-sponsored pension plans generated by dividing the number of individuals first participating in DC pension plans through employer-sponsored pension plans by the number of individuals in the general population in the specific month-year birth cohort on December 31, 2019.

Variables for Labor Market Outcomes

Wage (in logs) is the logarithm of labor market hourly earnings.

High Occupation is an indicator variable if a specific individual works in managerial, professional, technical, clerical, and service jobs.

Low Occupation is an indicator variable if a specific individual works in agricultural and elementary jobs.

Large Firm is an indicator variable if a specific individual works in a firm of a size greater than 20.

Variables for Financial Skills

Trust is a standardized trust index.

Financial Knowledge is a standardized financial knowledge and expertise in the stock market.

Peers is an indicator variable if a specific individual's investment decisions are affected by family, friends, and work environment.

Financial Advice is an indicator variable if a specific individual gets financial advice from financial intermediaries.

Willingness to Take Risk is an indicator variable if a specific individual's risk score is greater than 5 out of 10, similar to the risk variable in Dohmen *et al.* (2011).

Financial Ability Index is a summary index using the above five variables following the strategy proposed by Kling *et al.* (2007).

Variables for Cognitive Skills

Literacy is the standardized literacy score.

Numeracy is the standardized numeracy score.

Control Variables

Birth Month Indicator Variables are 12 indicator variables for each month.

Birth Registration Certificate Region Indicator Variables are 26 indicator variables for each birth registration certificate region.

Gender is an indicator variable if a certain investor is female.