LEARNING TO TAKE RISKS? THE EFFECT OF EDUCATION ON RISK-TAKING
IN FINANCIAL MARKETS

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ABSTRACT

We investigate whether acquiring more education when young has long-term effects on risk-taking behavior in financial markets and whether the effects spill over to spouses and children. There is substantial evidence that more educated people are more likely to invest in the stock market. However, little is known about whether this is a causal effect of education or whether it arises from the correlation of education with unobserved characteristics. Using exogenous variation in education arising from a Swedish compulsory schooling reform in the 1950s and 1960s, and the wealth holdings of the population of Sweden in 2000, we estimate the effect of education on stock market participation and risky asset holdings. We find that an extra year of education increases stock market participation by about 2% for men but there is no evidence of any positive effect for women. More education also leads men to hold a greater proportion of their financial assets in stocks and other risky financial assets. We find no evidence of spillover effects from male schooling to the financial decisions of spouses or children.

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There is a strong correlation between educational attainment and participation in financial markets—according to the 2010 Survey of Consumer Finances in the United States, 37.1% of households headed by college graduates participated in the stock market, while this fraction was only 5.6% among households headed by high school dropouts. Even after controlling for wealth and/or income, this correlation remains. But is this a problem? That depends on what is driving this observed difference. To the extent this difference reflects differences in preferences or other characteristics that are also correlated with education, we may see no reason for concern. However, if this is the result of a lack of information or financial acumen, it may suggest a role for public policy. Financial decision-making often involves an understanding of complicated issues that may limit the ability of less-educated adults to make smart investment decisions.[1] But could more education ameliorate this problem? More generally, what is the role of education in financial decision-making? Does more education actually change people's investment behavior? And, if yes, how?

A key difficulty with identifying the causal effect of education on investment behavior is that education is likely correlated with many unobserved individual characteristics that may be related to investment behavior, such as risk tolerance, IQ, and family background; this suggests that observed correlations may reflect these unobserved differences and not a causal relationship. To address this issue, we take advantage of an education reform in Sweden in the 1950s and 1960s that increased compulsory schooling from 7 to 9 years in different municipalities at different times. The change in schooling induced by this reform is thus uncorrelated with other individual characteristics related to wealth and asset allocation. Using administrative data that includes information on the wealth portfolios of the population of Sweden in 2000, we can

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[1] Christelis et al. (2010) and Grinblatt et al. (2011) show that portfolio choice is related to cognitive abilities.

identify the causal effect of the increase in education induced by the legislative change on the wealth portfolios of those born in the period around the law change. By further linking the data to the *children* of these men and women, we are also able to study whether any positive effects of schooling spill over to the next generation.

There are several channels through which education might affect the decision to participate in the equity market and the share of assets invested in equities. One possibility is that more education reduces risk aversion, which is considered a major factor in willingness to bear financial risk.[2] Another possible factor is through income; obtaining more education typically leads to higher earnings and greater wealth that then enables greater investments in risky financial assets. Finally, the cost of gathering and processing information about the risks and returns in the market might be lower for more educated individuals.[3] To the extent that education may increase financial acumen, this suggests that the large literature estimating the return to education in terms of earnings may actually underestimate the financial benefits from increasing educational attainment.

In addition to broadening our knowledge about the financial returns to education, this work contributes to our knowledge of the underlying determinants of equity market participation. Evidence suggests that there is substantial welfare loss from the decision not to hold stocks in one's portfolio; historically, stock market investment has had a high return compared to safer financial assets such as bonds and money market funds. As a result, it is important to understand

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[2] Standard models imply that, in a frictionless market, differences in risk preferences and wealth (if risk taking is a function of wealth) are the main sources of cross-sectional variation in the share in equities, (e.g., Samuelson, 1969; Merton, 1969).

[3] See Bertaut (1998); Guiso et al. (2003); Hong et al. (2004); Bogan (2008); and Christiansen, Joensen, and Rangvid (2008). In markets with frictions, transaction and information costs may explain why many individuals do not invest in stocks or other financial assets (Haliassos and Bertaut, 1995; Vissing-Jorgensen, 2002).

why individuals may under-invest.[4]

We find a causal effect of education on men's investment decisions. Among men, more education increases the likelihood of stock market participation, the likelihood that he holds risky financial assets in general, and the proportion of his financial assets that he invests in stocks and other risky assets. The evidence also suggests that greater financial wealth is a possible mechanism underlying this relationship. However, we find no evidence that these effects persist across generations or that these effects spillover to spouses. There is no evidence of an effect of education on the financial decisions of women. These findings are robust to a variety of specification tests. To the extent that there may be substantial welfare loss when individuals choose not to hold stocks, these results suggest there may be a role for policy to influence financial risk-taking.[5]

The paper unfolds as follows. Section 2 discusses the related literature, while Section 3 describes the relevant institutional background. Section 4 describes the compulsory schooling reform and our data. Section 5 outlines our empirical strategy and Section 6 presents our results. Section 7 then concludes.

2. Related Literature

Although it is not the primary focus of the research, several previous studies have documented a strong relationship between the level of education and equity market participation in both Sweden and the United States (Campbell, 2006; Calvet, Campbell, and Sodini 2007,

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[4] In addition, understanding the determinants of stock market investment also provides insight into the determinants of the distribution of wealth (Guvenen 2006), ownership of individual retirement accounts (Bernheim and Garrett 2003), and wealth effects on consumption (Dynan and Maki 2001).

[5] Cocco et al. (2005) show that in calibrated life-cycle models the welfare loss from no stockholding is between 1.5 and 2 percent of consumption.

2009; Barnea et al. 2010 among others).[6] This positive relationship remains even in specifications with multiple controls.[7] However, despite the presence of a robust correlation between education and investment in risky financial assets, there is only limited work identifying the causal effect of education on equity holding.

There is also a small literature--primarily focused on distinguishing genetic versus environmental determinants of investment behavior--that uses differences in investment behavior among twins. These studies sometimes include education as a control variable in specifications with twin fixed effects, where the education estimates are based on differences in education within twin pairs. Using Swedish data, Barnea et al. (2010) show that the association between an advanced degree (college or graduate) and stock market participation drops (and in the case of a college degree becomes statistically insignificant) when they control for twin fixed effects, suggesting that the effect of education on participation in the stock market is significantly lower after controlling for genetic factors. Also using a panel of Swedish twins, Calvet and Sodini (2014) find that education is not significantly correlated with risky market participation and the risky share of financial assets once they control for yearly twin fixed effects. Unfortunately, while informative, twin studies are quite limited in their ability to determine the causal impact of any particular characteristic and are better suited for isolating the genetic versus environmental components.

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[6] In a related literature, financial literacy has also been shown to be correlated with stock market participation and portfolio diversification. Van Rooij, Lusardi, and Alessie (2011) find that those with low financial literacy are much less likely to invest in stocks and Guiso and Jappelli (2008) show that measures of financial literacy are strongly correlated with the degree of portfolio diversification. Hastings et al. (2013) provide a survey of the literature on the relationship between financial literacy and financial outcomes.

[7] For example, using the U.S. Survey of Consumer Finances and controlling for wealth, income, age, race, and risk tolerance, Campbell (2006) reports a large and significant role for education in predicting whether a household owns public equity. The same study warns that education variables in demographic regressions could be endogenous and overstate the effects of exogenous increases in education on investment behavior.

Cooper and Zhu (2014) adopt yet another approach and use U.S. data to estimate a structural life-cycle model of the relationship between education and risky market participation. They conclude that education affects household finance mainly through increasing income and that higher educational attainment is associated with a lower stock market entry cost and a larger discount factor. However, this approach relies on functional form for identification as opposed to exogenous variation in education induced by a policy change.

Most closely related to our own work is that by Cole, Paulson, and Shastry (2014) who use variation in state compulsory schooling laws between 1914 and 1978 in the US to examine the effect of education on a variety of wealth and credit measures. Using data from the Census as well as the FRBNY Consumer Credit Panel/Equifax dataset, they provide compelling evidence that education affects investment income and reduces the probability that an individual declares bankruptcy. Using the Survey of Income and Program Participation (SIPP), they find that an additional year of education increases the probability of owning equities by 4 percentage points (statistically significant at the 10% level). Unfortunately, for outcomes on stock ownership, they are limited by relatively small sample sizes; in addition, recent research has suggested there are a number of limitations to using compulsory schooling laws in the U.S. for identification that are not relevant to the Swedish case. Our paper advances our understanding of the effect of education on stock ownership by using an arguably cleaner source of variation combined with population data from Sweden; in these data, we can observe total wealth, stock ownership, as well as information about the risk distribution of the assets.[8]

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[8] Stephens and Yang (2014) demonstrate that IV estimates using U.S. compulsory schooling laws often change sign and significance with the addition of region by year controls and so are not robust across reasonable specifications. In addition, Cole et al. cluster standard errors by state-year rather than by state and so do not allow for serial correlation at the state-level. Bertrand et al. (2004) show that this can lead to greatly underestimated standard errors and Black, Devereux, and Salvanes (2008) demonstrate this for U.S. compulsory schooling law analysis.

3. Institutional Background

It is important to understand the institutional context for our study, particularly the role of pension programs as a component of savings. While the social welfare and pension systems are quite generous in Sweden, a large proportion of financial wealth is held outside of pension funds. Non-retirement wealth accounts for almost 84 percent of aggregate household financial wealth (Calvet et al 2007), and it is this form of wealth that is the focus of our study.

Relative to countries such as the U.S., Sweden's pension system would be considered quite generous. Sweden has a mix of public and private pension schemes, and individuals are allocated to different pension systems depending on the public or private sector affiliation and year of birth of the individual. The longer one works, the higher pension one receives. The retirement age is flexible and individuals can claim retirement benefits beginning at age 61.

In 2000, when we measure asset allocation, the public pension system almost entirely consisted of a national pension plan financed on a pay-as-you-go basis.[9] In addition, most people receive an occupational pension from their employer. According to the Swedish Pensions Agency, about 90% of employees receive some pension benefits from their employer as a condition of employment. On average, around 4.5% of the employee's salary is put into employer provided schemes (Thörnqvist and Vardardottir, 2014). Swedish residents also have tax incentives to invest in private pension savings that are only accessible after retirement. However, as mentioned earlier, individuals still hold a substantial fraction of their wealth in non-retirement

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[9] An individual account system known as the Premium Pension System (PPS) was introduced in 1999 but, because these funds were so new, investment in the PPS funds was very low when we are measuring asset allocation. Therefore, the existence of these funds is unlikely to have had any important effect on asset allocation.

wealth. In addition, stock market participation rates are higher in Sweden than in many other countries such as the United States (Guiso, Haliassos, and Jappelli, 2001).[10]

Because we examine both male and female investment behavior separately, it is important to understand whether there are incentives to transfer wealth holdings from one spouse to another. There do not appear to be any such incentives. In the event of a divorce, in the absence of a prenuptial agreement, all assets are split equally among spouses. Until 2006, there was a 1.5% tax on wealth above 1.5 million SEK for single tax filers and 3 million for a married couple filing jointly. The value of jointly owned assets was split between the two tax filers. Thus, there were no incentives for husbands and wives to strategically allocate assets between themselves in order to reduce their wealth tax bill.

Finally, people who face greater labor income risk may be less likely to choose risky financial portfolios. Consequently, the unemployment insurance system could potentially affect individuals' risk-taking behavior in financial markets; a more generous system could create an incentive to take more risk with one's portfolio. In 2000, while the formal replacement rate was at 80 per cent of wages, the effective replacement rate taking earnings-ceilings into account was around 65 percent. (Carling et al, 2001).[11] Because of this, it is difficult to imagine that high risky market participation in Sweden compared to many other countries can be explained by the "generous" unemployment insurance system.

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[10] There is also a guaranteed pension for those who have had little or no income from work, and the size of this guaranteed pension is based on how long the person has lived in Sweden. In 2000, the maximum guaranteed pension, which applies to those who have lived in Sweden for at least 40 years, is 2394 SEK per month ($254) before taxes for those who are married, and 2928 SEK per month ($311) for a single person. A tax rate of 30 percent is then applied.

[11] There is an earnings ceiling above which no additional benefits are paid. In 1996, it was estimated that 75 percent of employees had monthly earnings exceeding the ceiling (Bharadwaj et al. 2014).

4. Empirical Strategy

Because education is correlated with unobservable characteristics that are also likely related to portfolio allocation, we use variation in educational attainment induced by a change in the compulsory schooling law to identify the causal effect of education on risky market participation.

The Compulsory Schooling Reform

The Swedish comprehensive primary schooling reform was implemented across municipalities at various times during the 1950s and 60s. A parliamentary committee first initiated the reform in 1948, and its most notable feature was an increase in the mandatory years of schooling from seven to nine years.[12] To facilitate an evaluation of the reform, it was implemented gradually across municipalities in a manner meant to be representative of the country's population and geography.[13] In 1949, 14 municipalities introduced the reform, and additional municipalities were added year by year. (Marklund 1981). In 1962, the parliament mandated that all municipalities implement the reform by 1969.[14]

In addition to an increase in the years of compulsory schooling, the reform changed a number of features of the public school system. In the pre-reform school system, students were tracked at grade 6, based on their performance; after the reform, this early tracking was abolished and students were instead integrated until 9th grade. In practice, however, the change was less dramatic, since students in the new system were able to choose between different types of courses

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[12] In a few larger cities, mandatory schooling was eight years before the reform.

[13] Both before and after the reform, Swedish children normally started school during the calendar year in which they turned seven, meaning that compulsory schooling after the reform usually lasted until the age of 16.

[14] In general, the reform was implemented in all school districts within a municipality. The exceptions were the three largest cities, Stockholm, Göteborg, and Malmö, where the reform was implemented in different school districts in different years. We use information on the parish the person grew up in to correctly allocate the reform variable for these cities.

and could self-track.[15]   The reform also changed the national curriculum. English became a compulsory subject in reform schools and was taught beginning in the fifth grade. However, in 1955, even non-reform schools were required to make this change to the curriculum. Beyond this, the reform did not lead to any other changes in the total number of hours taught or to the distribution of hours designated to different subjects.[16]

There is a substantial literature that uses changes in the compulsory school laws in Sweden to study a variety of outcomes, including income, health, and crime. Meghir and Palme (2005) showed that the reform increased educational attainment and led to higher labor incomes. Holmlund et al. (2011) used the reform as an instrument for parental schooling, and found evidence of a causal effect of parent's educational attainment on child's educational attainment, and Lundborg et al. (2014) used a similar strategy to establish a positive effect of maternal education on the health and skills of sons at age 18. Finally, Meghir, Palme, and Schnabel (2012) use the Swedish reform to show that extra education reduced crime rates both for the individuals affected and for their children.[17] Thus, there is much evidence that the compulsory schooling reform had substantial and meaningful effects on the cohorts affected by it.

Data

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[15] The tracking before grade nine was abolished in 1962 but in Math and foreign languages, students were still able to choose between harder or easier classes. In a detailed description of the schooling reform, Marklund (1987, p. 180) notes that "the reform school between 1955 and 1960 conformed to a streaming system that in terms of routes was not too much different from the old parallel school with one common school route and one junior secondary school route."

[16] Because it expanded compulsory schooling, the reform led to an increase in demand for teachers. This induced some schools in the early years of the reform to hire teachers who were not formally qualified. Over time, as several teacher colleges were opened, the shortage began to ease in the mid-60s (Marklund 1981). Municipalities were compensated by the government for the additional financial burden of hiring teachers and expanding school facilities. For a thorough overview of the schooling reform, see Holmlund (2008) and Lundborg et al. (2014).

[17] Hjalmarsson et al. (2014) used the same reform to study the effect of education on adult crime.

We begin with a comprehensive dataset based on merged administrative registers that contains information on all Swedish citizens born between 1930 and 1980; this dataset includes information on educational attainment, municipality of residence, basic demographic information, and detailed wealth data.

In order to assign reform exposure status to individuals, we need to know in which municipalities individuals grew up. To do that, we link data from the 1960 and 1965 censuses that include information on municipality of residence. For cohorts born between 1943 and 1949, we use information from the 1960 census and for those born between 1950 and 1955 we use information from the 1965 census. We drop individuals who were born prior to 1943.[18]

In order to determine which individuals were exposed to the reform, we make use of a reform algorithm constructed by Helena Holmlund. Using birth year and municipality and parish of residence when growing up, the algorithm assigns a binary reform exposure variable to each individual in these cohorts. The algorithm is able to assign reform exposure to 90 percent of individuals born 1943-1955 who have non-missing information on municipality and parish of residence.[19]

In order to assign years of schooling to our sample, we use data from the education register in 1990. The register contains information on highest educational degree completed,

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[18] The reason for restricting our sample to those born 1943 and onwards is that the 1960 census provides a less accurate measure of municipality of residence for those born prior to 1943. A person born in 1940, for instance, may have moved out of his parent's place in order to work or study by the time of the census. Holmlund (2008) documents that the fraction of individuals living with their mothers in 1960 drops substantially for the pre-1943 cohorts.

[19] Only 0.19 percent of the population living in Sweden at the time of the 1960 and 1965 censuses lack information on municipality of residence. The reform indicator is subject to measurement error. First, the reform exposure algorithm assumes that the students were in the right grade for their age. Svensson (2008) showed that 88 percent of all children born in 1949 were in the right grade in 1961, reflecting both that some students repeated a class and that some students started school earlier. Second, it is not always possible to assign a sharp starting date of the reform. These measurement problems only concern the cohorts born right around the assumed starting date of the reform and do not affect the consistency of the instrumental variables estimator we use.

which we use to impute years of schooling.[20] Table 1 shows the distribution of schooling in our

sample 2 years before and 2 years after the reform.[21]

For data on asset allocation, we predominantly rely on the Swedish Wealth Data

(Förmögenhetsregistret) from the year 2000. These data were collected by the government's

statistical agency, Statistics Sweden, for tax purposes between 1999 and 2007, when the wealth

tax was abolished.[22] The data includes all financial assets held outside retirement accounts at the

end of a tax year, December 31st, reported by a variety of different sources, including the

Swedish Tax Agency, welfare agencies, and the private sector. Financial institutions provided

information to the tax agency on their customers' security investments and dividends, interest

paid or received, and deposits.[23] Since the information is based on statements from financial

institutions, it is likely to have very little measurement error and, since the entire population is

observed, selection bias is not a problem.

In this paper, we have data on the aggregate value of bank accounts, mutual funds, stocks,

options, bonds, and capital endowment insurance as well as total financial assets and total assets

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[20] We follow Holmlund et al. (2011) and impute years of schooling in the following way: 7 for (old) primary school, 9 for (new) compulsory schooling, 9.5 for (old) post-primary school (realskola), 11 for short high school, 12 for long high school, 14 for short university, 15.5 for long university, and 19 for a PhD university education. Since the education register does not distinguish between junior-secondary school (realskola) of different lengths (9 or 10 years), it is coded as 9.5 years. For similar reasons, long university is coded as 15.5 years of schooling.

[21] In the table, there is no category for 8 years of schooling as the education register does not distinguish between different lengths of pre-reform primary school. Some school districts in the big cities had already implemented 8 years of mandatory schooling at the time of the reform.

[22] The wealth tax used to be paid on all the assets of the household, including real estate and financial securities, with the exception of private businesses and shares in small public businesses (Calvet et al. 2007) and was levied at a rate of 1.5 percent on net household wealth exceeding SEK 900,000 in year 2000. The Swedish krona traded at $0.106 at the end of 2000, so this threshold corresponds to $95,400.

[23] Importantly, nontaxable securities and securities owned by investors below the wealth tax threshold were included in the reports (Calvet et al. 2007).

for the population of Sweden.[24] For the analysis, we use data on the value of stocks, the value of risky assets (stocks plus mutual funds with a risky component), and total financial wealth held by individuals.

For data on income, we use the Income Register which includes income beginning in 1968. Our measure of earnings includes earnings from employed labor as well as self-employment.

Our final sample for analysis includes more than 1.3 million individuals born between 1943 and 1955 for whom we have complete information on schooling, municipality and parish of residence when growing up, and wealth in 2000. While 1.8 million Swedish citizens were born between 1943 and 1955, twenty percent of those did not participate in the 1960 census; however, this group consists almost entirely of immigrants who had not arrived in Sweden by 1960 and, consequently, were not exposed to the educational reform. Out of the remaining individuals, 95 percent survived until 2000. Only 2 percent lack information in the education register of 1990.

We analyze equity market participation using four outcome variables constructed from the 2000 Wealth Register.[25] The first is an indicator variable for whether the individual owns stocks directly – we refer to this as stock market participation or direct equity participation. The second variable is an indicator variable for participation through either direct stock holding or mutual funds with a stock component – we refer to this as risky market participation.[26] Our final two

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[24] We have incomplete information on small bank accounts as they did not need to be reported by banks to the Swedish Tax Agency unless there was more than 100 SEK (about $10) in interest during the year.

[25] The choice of year 2000 is somewhat arbitrary. As a result, we later show that our results are quite similar when we use the years 1999, 2001, and 2002. An advantage of using data from the earlier years of the Wealth Register is that all individuals in the sample are below the age of retirement. Fagereng, Gottlieb and Guiso (2013) show a rebalancing of the portfolio away from stocks as investors approach retirement, and stock market exit after retirement.

[26] This includes holdings of mutual funds that only include stocks but also includes mutual funds that have a mixture of stocks and other financial instruments such as bonds. By definition, persons with zero financial wealth are

measures are the share of financial assets held in stocks, and the share of financial assets held in equities, both conditional on participation. This latter variable is defined as the proportion of financial assets that are either in stocks or in mutual funds that have a stock component.

Table 2 provides summary statistics for our data. On December 31 2000, 36 percent of individuals in our sample directly held stocks outside retirement accounts, while 63 percent held equities either through direct stock holding or mutual funds with a stock component. Although there is no difference in men and women's rate of participation in risky financial markets, men held more of their risky assets as stocks while women preferred mutual funds with an equity component. Conditional on participation, the average equity share for men and women was 0.37 and 0.31 and the average risky share was 0.54 and 0.67 respectively.

5. Empirical specification

Our empirical specification is based on the two following equations. Our first stage equation is:

$$S_{icm} = \pi_0 + \pi_1 R_{cm} + \theta_m + \delta_c + \varepsilon_{icm}. \qquad (1)$$

where S_{icm} denotes the number of years of schooling of individual i, belonging to cohort c, and growing up in municipality m. Reform exposure, R_{cm}, is a dummy variable taking the value of one if the individual was exposed to the reform. θ_m and δ_c denote municipality and birth cohort fixed effects.

The main equation of interest is:

$$FIN_{icm} = \gamma_0 + \gamma_1 S_{icm} + \theta_m + \delta_c + \varepsilon_{icm}. \qquad (2)$$

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considered to have no holdings of stocks or risky assets.

where FIN_{icm} denotes our outcomes of interest, such as stock market participation or the share of risky assets. Here, S_{icm} is instrumented with reform exposure, according to Equation (1). The parameter of interest is γ_1 which captures the causal effect of schooling on financial outcomes. Standard errors are clustered at the municipality level to allow for heteroskedasticity and arbitrary serial correlation across cohorts within municipalities. In addition to the basic specification in (1), we also estimate specifications with controls for parental education, with municipality-specific trends, and with region by cohort dummies. As we will see later, the estimates are quite robust to the exact choice of specification.

As described earlier, the reform was not randomly implemented across municipalities. Our empirical approach is therefore based on the assumption that, conditional on birth cohort fixed effects and municipality fixed effects (and, in some specifications, parental education, municipality-specific trends and region by cohort dummies), exposure to the reform is as good as random.

While we cannot test this assumption directly, we can provide evidence that suggests it is valid. We first examine whether the timing of reform implementation is related to observable characteristics; these results are presented in Table A1. The first column shows the relationship between parental schooling and our binary indicator of reform exposure without controlling for municipality dummies. Here, both mothers and fathers' schooling is positively and significantly associated with reform exposure, suggesting that, in a given year, the reform was more likely to be implemented in municipalities where the parents were better educated on average. However, when we include municipality fixed effects in column (2), this wipes out these significant correlations; the estimates of parental schooling in column (2) are tiny and statistically insignificant. This continues to be the case in the specifications with region by birth cohort fixed

effects and municipality-specific trends (columns 3 and 4), suggesting that our identifying assumption may be reasonable.

Finally, a concern may be that some parents may have responded to the reform by moving to municipalities that were early implementers in order to ensure their child would benefit from the reform. Such endogenous mobility has previously been investigated by Meghir and Palme (2005) and by Holmlund (2008); both studies find little reason for concern. Only between 3 and 4 percent moved from a municipality that had not yet implemented the reform to a one that had, and an equal share moved in the opposite direction. In addition, mobility was not found to be systematically related to observable characteristics that are associated with education, such as parent's education.

6. Results

First Stage

We first examine the relationship between exposure to the reform and years of education. In Table 3, we present the regression results of the first-stage effects of reform exposure on education using four different specifications. Column (1) includes controls for birth cohort fixed effects and municipality fixed effects; we find the reform increases education by 0.27 years for men and 0.16 years for women. To control for possible omitted variables, we next include additional controls for parental schooling to our first-stage regressions. Column (2) in Table 3 adds controls for mother's schooling; this has no effect on the estimates of the effect of the reform. When we next add region by year of birth fixed effects to the base specification (Column 3), this leads to a slight increase in the coefficients to 0.3 for men and 0.18 for women. Finally, column (4) adds municipality-specific linear trends to the base specification; once again, the addition of these controls leads to a small increase in the first stage estimates to 0.32 for men

and 0.21 for women. For both men and women, the F-statistics suggest that the instrument is sufficiently strong in all specifications.

Effects of Education on Stock and Risky Asset Holding

Table 4 shows the effect of schooling on individual stock market and risky asset participation. Panel A presents the estimates for stock market participation of men and panel B presents the corresponding results for women. The first column shows the OLS relationship between schooling and stock market participation. Not surprisingly, this estimate is positive and highly significant for both men and women, with magnitudes of 0.034 and 0.029 respectively, implying that an extra year of schooling is associated with about a 3% increase in the probability of stock market participation. Columns (2) to (5) then present the IV estimates for a variety of specifications. All specifications include cohort and municipality dummies and columns (3) – (5) also include controls for mother's schooling, region by year of birth fixed effects, and municipality-specific trends, respectively. Among males, in the baseline specification (Column 2), the estimate suggests that an extra year of education increases stock market participation about 2% from a base of 42%.   Importantly, our results are quite robust to specification choice with estimates for men of about 2% in all columns.[27] Among women, although the OLS estimates are quite similar to those of men, the IV estimates provide no evidence for a positive effect of education on stock market participation. Indeed the point estimates are negative, albeit statistically insignificant.

In panels C and D of Table 4, we study a broader measure of risky financial behavior -- risky market participation. This variable is one for people who either own stocks directly or who

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[27] Stephens and Yang (2014) show that, in the US, estimates of compulsory schooling are generally not robust to adding interactions of census region and year of birth to the regression. In column (4) we show that our estimates are robust to adding region by year of birth fixed effects. Sweden has 21 counties that we use to construct these fixed effects.

own stocks indirectly through mutual funds. The IV evidence of an effect of education is now weaker for men, with point estimates of about 1% that are marginal in terms of statistical significance. This suggests that the effect of education may in fact be working through the decision to purchase individual stocks instead of holding mutual funds with a stock component. The estimates for women continue to be negative and statistically insignificant.

In addition to influencing the participation decisions, education could also affect the allocation decision between risky and less risky assets. In Table 5, we look beyond the extensive margin and study the effects of education on the log (share) of stock holdings and risky assets in financial wealth, conditional on participation. Panels A and B show the results for the share of financial wealth directly invested in stocks and Panels C and D have the analogous estimates for log (share) of financial wealth in risky assets. For men, there is a positive and statistically significant effect of education on the log (share) of wealth held in stocks. The estimates imply that, conditional on participation, one more year of education results in around a 10 percent increase in the share of stocks in financial wealth for men. Relative to the average share of 37 percent, this is equivalent of around 3.7 percentage point increase in the share of stocks for men.[28] However, as at the extensive margin, there is no evidence of any effect for women. Also, as was the case at the extensive margin, when we turn to the share of financial wealth in risky assets (stocks and stock-containing mutual funds), the effects for men are positive but smaller (and less statistically significant) than for stock holding. As before, there is no evidence of an effect for women.

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[28] As is common in the literature on the returns to education, the IV estimates here are larger than the OLS estimates despite priors that OLS is likely biased upwards due to omitted variable bias. This can be explained by a variety of factors, the most likely being that the IV represents the local average treatment effect—the effect of education for those who would have obtained 7 years of education but who, after the reform, obtain 9. The effects of education on this sample may be larger than the average effects for the population.

Robustness Checks

We also conducted a number of robustness checks; these are presented in Appendix Tables 2 and 3. For parsimony, we only report estimates for our baseline specification with municipality and cohort fixed effects. One possible concern is that we are including too many years before and after the reform and should look more closely around the law change. Panel A of Table A2 shows our main results when we include only cohorts born within 5 years of the first reform cohort in that particular municipality to verify that we are making appropriate comparisons; the results are quite robust to this restriction. Because of uncertainty regarding the exact number of pre-reform years of schooling in the three cities of Stockholm, Göteborg, and Malmö, Panel B, presents estimates when we re-run our main specification dropping these cities. The estimates are similar but the standard errors fall somewhat. This probably arises because the first stage is weaker due to the generally higher levels of pre-reform education in these cities.[29]

Finally, Appendix Table 3 presents results when we consider different years of data. Our decision to use wealth data from 2000 was arbitrary, and Appendix Table 3 shows our results are robust to this choice by reporting estimates using data from 1999, 2000, 2001, and 2002 in columns (1) to (4) respectively. The last column then shows estimates using averages of the dependent variable for each individual across the 4 years. These estimates are almost identical to the estimates using 2000 data; this is unsurprising as investment behavior tends to change slowly over time.

Intra-Household Spillovers

So far, we have considered the role of individual education on his/her own investment behavior. However, when one considers families, it may be that one spouse influences the

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[29] Excluding these three cities increases the first stage coefficient in the baseline specification from 0.27 to 0.34 for males and from 0.16 to 0.21 for females.

other's investment behavior.[30]   To test this empirically, we can estimate the effects of one spouse's education on the portfolio allocation of his/her spouse.[31]

In Table 6, we look for spillovers within households directly by studying how one spouse's education affects the asset holding of the other. We do this with and without a control for the education of the other spouse, where we instrument both own education and spousal education with reform exposure. We find no evidence of any spillover effects.[32]

Intergenerational Spillovers

Finally, because of the richness of our data we are able to examine whether the effects of education on financial risk-taking are transmitted to the children. To do so, we estimate similar specifications, but now our outcome measures are the risk allocation of the *children* of the affected cohorts.

In order to link children to their parents, we make use of the Swedish multigenerational register. With our data, we are able to link children born 1980 or earlier to our main sample born 1943-1955. For our female sample, we observe 920,148 children distributed across 492,224 mothers. For fathers, we observe 713,886 children among 404,982 fathers.[33]

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[30] As we noted earlier, there is no tax incentive for spouses to reallocate assets from one to the other.

[31] For married people, we can also examine the effect of their education level on the investment decisions of the household, not just their individual investments. When we do this, we continue to find evidence of an effect of education on individual investment behavior within two-adult households (even in this reduced sample) but find no evidence of an effect on total household asset allocation.

[32] The reason for the smaller number of observations in these regressions is that the sample is restricted to cases where also the spouse has non-missing information on education, birth year, and reform exposure. In these analyses, we put no restriction on the birth year of the spouse.

[33] We thus lack information on children for 23 percent of the women, which reflects that some women remain childless throughout their life and that some children were born after 1980. From external sources we know that about 12 percent of women born during our study period remained childless (SCB 2011). We lack information on children for 39 percent of our males, where the higher rate reflects the fact that a higher proportion of males in our

In order to study outcomes of the children at the oldest possible age, we focus on data from the wealth register as of 2006. This is the last year of data in the register; at this point, the youngest child is at least 26, which is well past the age at which most young Swedish adults have completed their education. Table A4 shows summary statistics for the sample of children, revealing that the average age of the children in 2006 was 33. Despite their relatively young age, almost half of the children hold some risky asset and about a fifth hold stocks directly.

Table 7 summarizes the results, using the previous IV specification that includes municipality and birth cohort fixed effects. Panel A shows the effects for men. In the first column, the estimate of the effect of father's schooling on children's stock market participation is 0.002 and insignificant. We get similarly tiny and insignificant estimates for children's risky market participation and for their log share of stocks or risky assets. The corresponding results for mothers, shown in Panel B of Table 7, are small and insignificant in all cases, suggesting that mothers' schooling does not causally influence children's financial holdings. This is in contrast to the OLS estimates that suggest significant and positive correlations between parental schooling and children's holdings.[34]

There are two potential concerns with the analysis above. First, parental schooling may be related to the probability of observing a child in our data, which could bias our results. This would be the case if schooling increases the age at first birth or the probability of remaining childless. We can test this directly; to do so, we estimate the effect of schooling on the probability of not observing any child our data. Doing so, we get positive but statistically insignificant effects of schooling for both males and females, suggesting no systematic

study cohorts remain childless and that, on average, males have their first child later than females (SCB 2011).

[34] This finding appears inconsistent with recent U.S. evidence that used variation induced by compulsory schooling laws and data from the Panel Study of Income Dynamics that found a negative effect of parental education on the measured risk aversion of their children (Hryshko et al. 2011).

differences in the probability of being in our sample by parental education.

A second concern is that, among the parents where we observe at least one child, schooling might be negatively related to the age of the child in 2006. If younger children hold fewer assets, this might bias us against finding an effect of schooling. To test this, we estimate the effect of schooling on age of the child in 2006. When we do this, we again find small and insignificant effects for both males and females.[35]

Discussion

We have found that exogenous increases in education lead to greater stock market participation among men. This is an important finding, as it suggests that risk-taking behavior in financial markets is partly determined by educational policies. However, the question remains: what are the mechanisms underlying the effect of male education on investment behavior?

While we cannot definitively answer this question, we next explore some of the potential mechanisms through which education could operate. One channel, emphasized by Cooper and Zhu (2014), is that education increases earnings and, hence, leads to greater risky market participation. This could be because higher stable return to human capital can partially substitute for bond holding, or perhaps because the fixed costs of investment decrease with earnings.

For comparison to the existing literature, we first estimate the returns to education using the compulsory schooling instrument and our sample. In this case, the dependent variable is the log of average earnings between 1980 and 2000, including only those years with positive earnings. These results are presented in column (1) of Table A5. Using 2SLS, the return to education is about 3% for men but there is no evidence of a positive return for women.[36] It could

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[35] The estimates are -0.07 for males and -0.17 for females. In addition, we have run the analyses in Table 7 controlling for the age of the child in 2006. The conclusions were robust to this change.

[36] The OLS estimates suggest a return of about 7% for both men and women. The low 2SLS earnings returns to

be that education affects earnings for men, and these increased earnings affect their risk-taking behavior. As a crude way of testing this channel, columns (2) to (5) of Table A5 include controls for log(earnings). As a result, there are falls in the size of the coefficients of education on risk taking but the changes are not large, suggesting that the earnings channel may not be very important.

Another possible mechanism is that the changes in portfolio allocation are due to differences in financial wealth.[37] While education could increase financial wealth through its effect on earnings, it could also affect it directly by changing saving or consumption behavior. If people become less risk averse as their wealth increases, we would expect to see increased wealth due to increases in education leading to changes in financial risk-taking.[38] As with labor earnings, we next include log(financial wealth) as a control in our regressions; we can then attribute the changes in our coefficients of interest as the fraction "explained" by differences in financial wealth.[39] We also report 2SLS estimates of the effect of education on log(financial wealth).

In order to consider financial wealth as a possible mechanism, one much first deal with the issue that many people (24%) are recorded as having zero financial wealth. This arises because, as mentioned earlier, small bank accounts are underrepresented in our data. Since almost

education are consistent with estimates from previous Swedish studies (Meghir and Palme 2005). Small earnings returns to education are common findings in European data and have recently been found for Britain (Devereux and Hart, 2010) and Germany (Pischke and von Wachter, 2008).

[37] In a related literature, Behrman et al. (2012) show that greater financial literacy leads to higher wealth accumulation in Chile.

[38] However, even with constant relative risk aversion (CRRA), more education could lead to greater risk tolerance and greater financial wealth through higher returns. In this scenario, greater financial wealth is a result of greater risk-taking rather than a mechanism that leads to greater risk taking.

[39] It is important to note that identifying the role of financial wealth is murkier than identifying the role of income—in the case of financial wealth, changing risk-taking behavior will itself affect financial wealth; income does not have this same endogeneity problem.

everybody has a bank account, in reality the people who we measure as having zero financial wealth probably in fact have some small amount of financial wealth.[40] In addition, the financial wealth distribution is extremely skewed, with a small number of individuals having very large holdings. For these reasons, we use the log of financial wealth in the analysis and we impute financial wealth as being 1 for cases with zero reported wealth.[41] To verify that our conclusions are not being driven by outliers, we also conduct robustness checks where we trim the top of the financial wealth distribution so as to reduce the impact of individuals with extremely high financial wealth.

Table 8 presents the results using our baseline specification with cohort and municipality fixed effects along with controls for financial wealth. As we can see, the addition of a control for financial wealth has a substantial effect on the IV estimates. For example the effect of education on the stock market participation of men falls from 0.018 (Table 4) to 0.012 with the addition of the control for log financial wealth. The last column of Table 8 directly estimates the effect of education on log financial wealth. The IV estimates are statistically insignificant for both men and women. However the point estimate for men (11%) is quite substantial. As noted earlier, one concern is that these estimates are strongly influenced by outliers with very large financial wealth. In Table A6, we show that omitting observations with the top 5% or 10% of financial wealth leads to a larger and more precisely estimated statistically significant 18% effect of a year of education on financial wealth. Overall, the evidence suggests that education increases financial

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[40] In surveys, the fraction of Swedes aged 15 and above that have a bank account has consistently been 99 percent (Riksbanken, 2014).

[41] Using Inverse Hyperbolic Sine function instead, that is defined for zero financial wealth, generates identical results.

wealth substantially and that this is probably an important mechanism through which education increases stock market participation.[42]

Another possible mechanism is that effects of education on portfolio allocation are due to the effects of education on attitudes towards risk-taking. While information deficits and fixed costs are plausible reasons for why education may increase the likelihood of investing in stocks, risk aversion may be a more likely explanation for why education increases the proportion of financial wealth invested in stocks, conditional on participation.[43] Our findings for men that education increases risk-taking at both the extensive and intensive margins are consistent with an effect of education on risk-attitudes over and above any effects on information and fixed costs.[44]

7. Conclusion

By using the increased educational attainment induced by the change in the compulsory schooling legislation in Sweden in combination with a rich dataset containing wealth information for the entire population of the country, we are able to estimate the causal relationship between education and risk-taking in the financial market. We find evidence of a positive effect of education on stock holding for men but not for women. Interestingly, we find no evidence of spillovers from husbands to wives; husband's education does not affect the stock market participation behavior of his wife and vice-versa. We also find no evidence that higher education of men leads to their children being more likely to hold a higher proportion of financial wealth in

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[42] Interestingly, adding log(earnings) as an control in addition to log(financial wealth) has no effect on the effect of education on risk-taking, suggesting that the effects of income probably work through their effects on financial wealth.

[43] In a standard asset pricing model (assuming CRRA and Independently and Identically Distributed returns), the risk preference parameter for an individual is proportional to the share that the individual invests in equities.

[44] However, Jung (2015) finds using UK survey data and compulsory schooling laws that self-reported risk aversion is increased by extra schooling.

risky assets. These results are robust to a variety of specifications and a number of robustness checks. The evidence also suggests that greater financial wealth is a possible mechanism underlying this relationship. Given existing research suggesting substantial welfare costs to choosing not to hold stocks (Cocco et al. 2005), these findings suggest a role for government policy; this is yet one more benefit of increasing educational attainment.

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Table 1. Distribution of years of schooling two years before and after the reform.

Years of schooling	Before	After
7	15.8%	2.3%
9	8.4%	20.6%
9.5	3.3%	0.5%
11	32.2%	34.6%
12	12.2%	12.1%
14	3.7%	4.4%
15.5	23.7%	24.9%
19	0.6%	0.6%
N	183,568	151,456

Notes: Before indicates education distribution of cohorts in the two years prior to the reform, while After indicates the distribution of those two years post reform. Note that because the reform occurred in different municipalities at different times, the actual year of the reform varies by municipality.

Table 2. Summary Statistics

	Males			Females		
	Mean	Standard Deviation	Obs	Mean	Standard Deviation	Obs
Portfolio characteristics:						
Financial wealth*	266,225	1,210,262	662,057	186,596	753,577	642,110
Direct equity participation**	0.42	0.49	662,096	0.30	0.46	642,119
Direct equity share***	0.37	0.34	275,389	0.31	0.32	194,618
Risky market participation**	0.63	0.48	662,096	0.63	0.48	642,119
Risky share***	0.54	0.39	417,957	0.67	0.33	403,705
Financial characteristics:						
Real Estate wealth*	656,229	2,184,561	662,096	459,735	1,315,905	642,119
Total wealth*	1030242	34,400,000	662,096	674,205	7,461,090	642,119
Total liabilities*	306,904	1,430,661	662,096	183,937	996,811	642,119
Demographic characteristics:						
Age in year 2000	51.17	3.72	662,096	51.20	3.72	642,119
Education	11.34	2.88	662,096	11.66	2.79	642,119
Married	0.60	0.49	662,096	0.62	0.49	642,119
Born in Sweden	0.97	0.16	662,096	0.97	0.17	642,119

Notes: * All monetary values are reported in Swedish Krona on December 31, 2000. At the time, the exchange rate was 1 USD = 9.42 SEK.

** A dummy, taking a value of 1 if the individual participates.

*** Conditional on participation

Table 3. First-stage regressions. Males and Females.

	(1)	(2)	(3)	(4)
	Panel A: Males			
Reform exposure	0.266	0.266	0.305	0.323
	(0.043)***	(0.039)***	(0.023)***	(0.024)***
N	662,096	662,096	662,096	662,096
F-stats	38.79	46.42	177.67	186.33
	Panel B: Females			
Reform exposure	0.164	0.163	0.183	0.213
	(0.031)***	(0.029)***	(0.018)***	(0.020)***
N	642,119	642,119	642,119	642,119
F-stats	27.18	30.40	106.76	114.63
Mother's schooling	NO	YES	NO	NO
Region by cohort FE	NO	NO	YES	NO
Mun. trends	NO	NO	NO	YES

Notes: Columns (1)-(4) show the effect of reform exposure on years of schooling from specifications including birth cohort and municipality fixed effects. In addition, columns (2)-(4) include: (2) controls for mothers' schooling and an indicator of missing information on mothers' schooling, (3) region-by-county fixed effects, and (4) municipality-specific linear trends. Standard errors are clustered at the municipality level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4. Effect of education on participation in stock markets and risky markets. Males and females.

	(1)	(2)	(3)	(4)	(5)
	Panel A: Stock market participation, males				
Schooling	0.034	0.018	0.019	0.022	0.016
	(0.001)***	(0.008)**	(0.009)**	(0.009)**	(0.008)**
N	662,096	662,096	662,096	662,096	662,096
	Panel B: Stock market participation, females				
Schooling	0.029	-0.018	-0.019	-0.025	-0.012
	(0.000)***	(0.017)	(0.016)	(0.014)*	(0.011)
N	642,119	642,119	642,119	642,119	642,119
	Panel C: Risky market participation, males				
Schooling	0.033	0.012	0.012	0.020	0.012
	(0.001)***	(0.008)	(0.008)	(0.008)**	(0.007)*
N	662,096	662,096	662,096	662,096	662,096
	Panel D: Risky market participation, females				
Schooling	0.036	-0.021	-0.021	-0.014	-0.016
	(0.000)***	(0.018)	(0.017)	(0.013)	(0.011)
N	642,119	642,119	642,119	642,119	642,119
Mother's schooling	NO	NO	YES	NO	NO
Region by cohort FE	NO	NO	NO	YES	NO
Mun. trends	NO	NO	NO	NO	YES

Notes: (1) OLS regression on the relationship between schooling and stock market participation (panels A-B) or risky market participation (panels C-D). Columns (2)-(5) show instrumental variables estimates of the effect of schooling from specifications including birth cohort and municipality fixed effects. In addition, columns (3)-(5) include: (3) controls for mothers' schooling, (4) region-by-county fixed effects, and (5) municipality-specific linear trends. Standard errors are clustered at the municipality level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5. Effect of education on (log)share of stocks and risky holdings out of total financial wealth. Males and females.

	(1)	(2)	(3)	(4)	(5)
	Panel A: (log)share of stocks, males				
Schooling	0.025	0.103	0.104	0.092	0.085
	(0.002)***	(0.049)**	(0.050)**	(0.048)*	(0.045)*
N	275,389	275,389	275,389	275,389	275,389
	Panel B: (log)share of stocks, females				
Schooling	0.010	0.003	-0.012	0.012	-0.038
	(0.002)***	(0.094)	(0.106)	(0.075)	(0.077)
N	194,618	194,618	194,618	194,618	194,618
	Panel C: (log)share of risky assets, males				
Schooling	0.016	0.048	0.048	0.023	0.035
	(0.001)***	(0.027)*	(0.027)*	(0.025)	(0.024)
N	417,957	417,957	417,957	417,957	417,957
	Panel D: (log)share of risky assets, females				
Schooling	0.012	0.023	0.023	0.003	0.013
	(0.001)***	(0.037)	(0.039)	(0.038)	(0.029)
N	403,705	403,705	403,705	403,705	403,705
Mother's schooling	NO	NO	YES	NO	NO
Region by cohort FE	NO	NO	NO	YES	NO
Mun. trends	NO	NO	NO	NO	YES

Notes: (1) OLS regression on the relationship between schooling and share of stocks (panels A-B) and share of risky assets (panels C-D) out of financial wealth. Columns (2)-(5) show instrumental variables estimates of the effect of schooling from specifications including birth cohort and municipality fixed effects. In addition, columns (3)-(5) include: (3) controls for mothers' schooling, (4) region-by-county fixed effects, and (5) municipality-specific linear trends. The estimates are conditional on holding any stock (panels A-B) or any risky asset (panels C-D). Standard errors are clustered at the municipality level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 6. Results for married males and females. Outcome is spousal stock market participation, risky market participation, (log) share of stocks and risky assets.

	(1)		(2)		(3)		(4)	
	Stock market participation		Risky market participation		(log)share of stocks		(log)share of risky assets	
				Panel A: Males				
Schooling	-0.016	-0.018	0.003	0.002	0.012	0.003	0.036	0.033
	(0.014)	(0.014)	(0.012)	(0.012)	(0.082)	(0.080)	(0.031)	(0.030)
N	350,865	350,865	350,865	350,865	108,832	108,832	228,306	228,306
				Panel B: Females				
Schooling	-0.007	-0.013	0.006	0.004	-0.103	-0.079	-0.027	-0.006
	(0.016)	(0.013)	(0.016)	(0.012)	(0.084)	(0.064)	(0.043)	(0.036)
N	362,879	362,879	362,879	362,879	174,115	174,115	258,930	258,930
Birth FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipal. FE	YES	YES	YES	YES	YES	YES	YES	YES
Spousal schooling	NO	YES	NO	YES	NO	YES	NO	YES

Notes: The table shows the effect of schooling on spousal stock market participation, risky market participation, and the (log)share of stocks and risky holdings out of total financial wealth for the sample of married men and women. Estimates are shown with and without controls for spousal education, where both own and spousal education are instrumented with reform exposure. Robust standard errors in parentheses. Standard errors clustered at the municipality level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7. Results for children's outcomes

	(1)		(2)		(3)		(4)	
	Stock market participation		Risky market participation		(log)share of stocks		(log)share of risky assets	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
				Panel A: Fathers				
Schooling	0.018	0.002	0.023	-0.005	0.011	-0.039	0.010	-0.013
	(0.000)***	(0.009)	(0.000)***	(0.010)	(0.001)***	(0.084)	(0.001)***	(0.039)
N	713,886	713,886	713,886	713,886	147,730	147,730	351,909	351,909
				Panel B: Mothers				
Schooling	0.018	-0.013	0.024	-0.022	0.006	-0.000	0.007	0.025
	(0.000)***	(0.014)	(0.000)***	(0.015)	(0.002)***	(0.105)	(0.001)***	(0.051)
N	920,148	920,148	920,148	920,148	199,629	199,629	458,204	458,204
Birth FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipal. FE	YES	YES	YES	YES	YES	YES	YES	YES

Notes: The table shows the effect of parental schooling on the children's stock market participation, risky market participation, and the (log)share of stocks and risky holdings out of total financial wealth. All models include birth cohort fixed effects and municipality fixed effects. Robust standard errors in parentheses. Standard errors clustered at the municipality level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8. Effect of education on financial decisions (controlling for financial wealth). Males and females.

	Stock market participation	Risky market participation	(log)share of stocks	(log)share of risky assets	(log) Financial wealth
	Panel A: Males				
Schooling	0.012	0.004	0.087	0.049	0.115
	(0.007)	(0.006)	(0.045)*	(0.026)*	(0.096)
N	662,096	662,096	275,389	417,957	662,096
	Panel B: Females				
Schooling	-0.012	-0.010	-0.046	0.006	-0.154
	(0.012)	(0.009)	(0.103)	(0.037)	(0.181)
N	642,119	642,119	194,618	403,705	642,119
Birth FE	YES	YES	YES	YES	YES
Municipal. FE	YES	YES	YES	YES	YES
Financial wealth	YES	YES	YES	YES	

Notes: All columns control for birth cohort fixed effects, municipality fixed effects. Columns (1)-(4) show instrumental variables estimates of the effect of schooling, controlling for log (financial wealth). Column (5) shows the instrumental variable estimates of the effect of schooling on log (financial wealth). Standard errors are clustered at the municipality level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A1. Exogeneity of the reform. Predicting reform exposure by parental schooling.

	(1)	(2)	(3)	(4)
	Panel A: Males			
Father's years of schooling	0.013 (0.002)***	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
Mother's years of schooling	0.005 (0.001)***	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
N	662,096	662,096	662,096	662,096
	Panel B: Females			
Father's years of schooling	0.013 (0.002)***	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Mother's years of schooling	0.005 (0.001)***	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
N	642,119	642,119	642,119	642,119
Birth FE	YES	YES	YES	YES
Municipal. FE	NO	YES	YES	YES
Region by cohort FE	NO	NO	YES	NO
Mun. trends	NO	NO	NO	YES

Notes: The table shows estimates of the relationship between parental schooling and reform exposure. Column (1) shows results including only birth cohort fixed effects. Column (2) shows estimates from a specification including birth cohort and municipality fixed effects. In addition, columns (3)-(4) include: (3) region by cohort fixed effects, and (4) municipality-specific linear trends. All estimations include indicator variables for missing schooling for fathers and mothers. Missing schooling is replaced by municipality by cohort means of schooling. Standard errors are clustered at the municipality level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A2. Robustness checks.

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Stock market participation	Risky market participation	(log) share of stocks	(log) share of risky assets	Stock market participation	Risky market participation	(log) share of stocks	(log) share of risky assets
Panel A: Shrinking window to cohorts -5 to +5 years around reform								
Schooling	0.019	0.023	0.086	0.031	-0.014	-0.016	0.077	0.054
	(0.009)**	(0.008)***	(0.054)	(0.028)	(0.015)	(0.016)	(0.093)	(0.041)
Birth FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipal. FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	438,180	438,180	178,688	272,761	424,597	424,597	125,765	263,462
Panel B: Dropping the three largest citites								
Schooling	0.021	0.016	0.077	0.027	-0.002	-0.002	0.039	0.022
	(0.007)***	(0.007)**	(0.040)*	(0.020)	(0.010)	(0.010)	(0.063)	(0.030)
Birth FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipal. FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	579,156	579,156	242,135	369,479	560,947	560,947	168,694	354,925

Notes: The table shows various robustness checks on the effect of schooling on the children's stock market participation, risky market participation, (log) shares of stocks and risky assets. Panel A only includes cohorts born 5 years prior to or 5 years after the cohorts that were first exposed to the reform. Panel B drops the three biggest cities. All models include birth cohort fixed effects and municipality fixed effects. Robust standard errors in parentheses. Standard errors clustered at the municipality level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A3. Effect of education on participation in stock markets and risky markets. Males and females.

	(1)	(2)	(3)	(4)	(5)
Year	1999	2000	2001	2002	Average 1999-2002
Panel A: Stock market participation, males					
Schooling	0.008	0.018	0.018	0.022	0.017
	(0.009)	(0.008)**	(0.008)**	(0.008)***	(0.008)**
N	664,606	662,096	659,572	656,710	655,912
Panel A: Stock market participation, females					
Schooling	-0.024	-0.018	-0.014	-0.010	-0.017
	(0.019)	(0.017)	(0.015)	(0.015)	(0.016)
N	643,732	642,119	640,356	638,445	637,928
Panel C: Risky market participation, males					
Schooling	0.016	0.012	0.011	0.012	0.012
	(0.009)*	(0.008)	(0.008)	(0.008)	(0.008)
N	664,606	662,096	659,572	656,710	655,912
Panel D: Risky market participation, females					
Schooling	-0.029	-0.021	-0.017	-0.016	-0.022
	(0.020)	(0.018)	(0.016)	(0.017)	(0.017)
N	643,732	642,119	640,356	638,445	637,928
Birth FE	YES	YES	YES	YES	YES
Municipal. FE	YES	YES	YES	YES	YES

Notes: Columns (1)-(5) show instrumental variables estimates of the effect of schooling from specifications including birth cohort and municipality fixed effects. Columns (1)-(4) show the results for data from 1999 to 2002, respectively. In Column (5), the outcome variable is the average of participation across the four years represented in Columns (1)-(4) and the sample is limited to those who are represented in the sample in all four years. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A4. Summary Statistics for children.

	Children of fathers			Children of mothers		
	Mean	Standard Deviation	Obs	Mean	Standard Deviation	Obs
Portfolio characteristics:						
Direct equity participation**	0.21	0.41	713886	0.22	0.41	920148
Direct equity share***	0.08	0.21	550067	0.09	0.22	712696
Risky market participation**	0.49	0.50	713886	0.50	0.50	920148
Risky share***	0.33	0.37	550067	0.33	0.37	712696
Demographic characteristics:						
Age in year 2006	32.05	4.21	713886	33.34	4.68	920148
Female	0.49	0.50	713886	0.49	0.50	920148

Notes: * A dummy, taking a value of 1 if the individual participates. ** Conditional on participation. *** A dummy, taking a value of 1 if the child is a female.

Table A5. Effect of education on financial decisions (controlling for earnings). Males and females.

	(1)	(2)	(3)	(4)	(5)
	Earnings	Stock market participation	Risky market participation	(log)share of stocks	(log)share of risky assets
Schooling	0.030	0.015	0.008	0.114	0.049
	(0.012)***	(0.008)*	(0.008)	(0.053)**	(0.029)*
N	658,479	658,479	658,479	274,029	416,117
Schooling	-0.039	-0.015	-0.018	0.017	0.024
	(0.030)	(0.015)	(0.016)	(0.094)	(0.036)
N	638,412	638,412	638,412	193,488	401,680
Birth FE	YES	YES	YES	YES	YES
Municipal. FE	YES	YES	YES	YES	YES
Earnings		YES	YES	YES	YES

Notes: All columns control for birth cohort fixed effects, municipality fixed effects. Column (5) shows the instrumental variable estimates of the effect of schooling on earnings. Columns (2)-(5) show instrumental variables estimates of the effect of schooling, controlling for log(earnings). Standard errors are clustered at the municipality level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A6. Effect of education on log (financial wealth) for different wealth cutoffs. Males and females.

Wealth cutoff	Full sample	99th percentile	95th percentile	90th percentile	75th percentile
			Panel A: Males		
Schooling	0.115	0.131	0.171	0.181	0.158
	(0.096)	(0.092)	(0.084)**	(0.080)**	(0.083)*
N	662,096	655,475	628,991	595,886	496,572
			Panel A: Females		
Schooling	-0.154	-0.133	-0.088	0.027	0.030
	(0.181)	(0.159)	(0.141)	(0.121)	(0.118)
N	642,119	635,697	610,013	577,906	481,589
Birth FE	YES	YES	YES	YES	YES
Municipal. FE	YES	YES	YES	YES	YES

Notes: All columns control for birth cohort fixed effects, municipality fixed effects. Columns (1)-(5) show instrumental variables estimates of the effect of schooling on financial wealth for different wealth cutoffs. Standard errors are clustered at the municipality level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.