Learning to Take Risks?
The Effect of Education on Risk-Taking in Financial Markets*

Sandra E. Black
Department of Economics
University of Texas, Austin
NHH, IZA and NBER
sblack@austin.utexas.edu

Paul J. Devereux
School of Economics and Geary Institute
University College Dublin
CEPR and IZA
devereux@ucd.ie

Petter Lundborg
Department of Economics
Lund University
IZA and CED Lund
petter.lundborg@nek.lu.se

Kaveh Majlesi
Department of Economics
Lund University
Knut Wicksell Centre for Financial Studies
kaveh.majlesi@nek.lu.se

January 2018

Abstract

We investigate whether acquiring more primary education has long-term effects on risk-taking behavior in financial markets. Using exogenous variation in education from a compulsory schooling change combined with wealth data for the Swedish population, we estimate the effect of education on stock market participation and on the share of financial wealth invested in stocks, conditional on participation. For men, an extra year of education increases market participation by two percentage points and the share of financial wealth allocated to stocks by 10%. We find suggestive evidence that greater financial wealth is a potential channel through which education increases participation, consistent with the existence of fixed costs. Lower risk aversion is a potential channel through which education increases the stock share. The reform has less effect on female schooling attainment and there is no evidence that this additional education affects women's asset allocation. There is no evidence of spillovers to children.

* The data used in this paper come from the Swedish Interdisciplinary Panel (SIP) administered at the Centre for Economic Demography, Lund University, Sweden. This work was partially supported by the Research Council of Norway through its Centres of Excellence Scheme, FAIR project No 262675.

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. However, is this relationship causal and do policies that increase educational levels actually change people's investment behavior and, if so, 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; for example, Christelis et al. (2010) and Grinblatt et al. (2011) show that portfolio choice is related to cognitive abilities. This suggests that observed correlations with education 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 use a differences-in-differences approach to identify the causal effect of the increase in education induced by the legislative change on the financial 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.

We find a causal effect of education on men's investment decisions. Among men, more education increases the likelihood of stock market participation and the likelihood that he holds risky financial assets in general. One additional year of schooling increases stock market participation by 2 percentage points from a baseline of 42 percent and risky market participation by 1 percentage point, from a baseline of 63 percent. An additional year of schooling also increases the proportion of his financial assets invested in stocks by 10 percent, conditional on participation.

We find no evidence that these effects persist across generations and spillover to children. While our estimates are somewhat imprecise, we find no evidence of an effect of education on the financial decisions of women; this is likely because pre-reform education levels were higher for women, and the reform has less effect on their schooling attainment. These findings are robust to a variety of specification tests. Historically, stock market investment has had a high return compared to safer financial assets such as bonds and money market funds. Our results for men suggest that the large literature estimating the return to education in terms of earnings may underestimate the financial benefits from increasing educational attainment.

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. Another possible factor is that more education leads to greater financial wealth that enables investments in risky financial assets. Finally, more educated individuals may have lower costs of gathering and processing information about the risks and returns in the market.[1] When we study mediating variables, we find that a partial explanation for our findings for men is that greater education leads to greater financial wealth and this in turn leads to a greater likelihood of holding stocks. However, none of our large set of potential mediating variables can explain the effect of education on the share of financial assets invested in stocks and risky assets, conditional on participation.

[1] See Bertaut (1998); Guiso et al. (2003); Hong et al. (2004); Bogan (2008); and Christiansen et al. (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).

Overall, we conclude that reduced fixed costs through increased financial wealth is a likely channel through which education affects stock market participation of men. 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. The fact that our mediating variables have no impact on the effect of education on the risky share provides further evidence that education may affect risk aversion.

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 et al., 2007, 2009; Barnea et al., 2010 among others).[2] However, despite the presence of a robust positive 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 that studies 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 (Barnea et al., 2010; Calvet and Sodini, 2014). However, twin differences in education may be correlated with twin differences in other characteristics (Bound and Solon, 1999).

Cooper and Zhu (2016) adopt another approach and estimate a structural life-cycle model of the relationship between education and risky market participation. They conclude that education affects stock market participation mainly through increasing income, and that higher educational attainment is associated with a lower stock market entry cost and a larger

[2] In a related literature, financial literacy has also been shown to be correlated with stock market participation and portfolio diversification. Van Rooij et al. (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.

discount factor. However, they do not have exogenous variation in education induced by a policy change.

Most closely related to our own work is that by Cole et al. (2014) who use variation in state compulsory schooling laws between 1914 and 1978 in the U.S. to examine the effect of education on a variety of wealth and credit measures.[3] They find that an additional year of education increases the probability of owning equities by 4 percentage points (significant at the 10% level). Cole et al. (2014) do not include a full set of region-by-year controls and, in addition, they cluster standard errors by state-year rather than by state and so do not allow for serial correlation at the state-level; recent research has shown that these choices may lead to unreliable estimates.[4] We show that our estimates are robust to the inclusion of region-by-year controls and we cluster by municipality to allow for serial correlation.

The paper unfolds as follows. Section 2 describes the relevant institutional background and the data. Section 3 outlines our empirical strategy. Section 4 presents our main results. Section 5 explores potential mechanisms through which education affects investment decisions. Section 6 investigates whether there are intergenerational spillovers and Section 7 then concludes.

---

[3] In contemporaneous work, Park and Son (2015) examine the effects of a college expansion in Korea on a variety of outcomes including whether the household holds risky assets. They find positive effects of education. Their paper differs from ours in many respects including that we study a policy intervention that affects the lower half of the education distribution (we study primary education while they study college) and we have information on actual holdings so can study the share of financial wealth invested in risky assets.

[4] 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. Bertrand et al. (2004) show that clustering standard errors by state-year can lead to greatly underestimated standard errors and Black et al. (2008) demonstrate this for U.S. compulsory schooling law analysis.

2. Institutional Background

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 to nine years from seven (in a few larger cities, mandatory schooling was eight years before the reform). 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. 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. 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.

In addition to an increase in the years of compulsory schooling, the reform changed some 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 and could self-track. 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.[5] 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.

[5] 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

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.[6] There is a substantial literature that shows that, despite these adjustment costs, there are beneficial impacts of the compulsory schooling law change on a variety of outcomes including income, health, and crime (Meghir and Palme, 2005; Holmlund et al., 2011; Lundborg et al., 2014; Meghir et al., 2012; Hjalmarsson et al., 2015). For a thorough overview of the schooling reform, see Holmlund (2008) and Lundborg et al. (2014).

3. Data

To assign reform exposure status to individuals, we need to know the municipality in which 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.[7]

To determine which individuals were exposed to the reform, we make use of a reform algorithm constructed by Holmlund (2008). Using birth year and municipality and parish of

the old parallel school with one common school route and one junior secondary school route."

[6] Between 1953 and 1956, a means tested scholarship compensated families in reform municipalities for foregone earnings from keeping their children longer in school. The scholarship was given to families with children in the 9th grade. In 1957, the system was replaced with a universal system of financial support for all children aged 16-18 who were in school. Since our earliest cohort was born in 1943, all the birth cohorts in our article were covered by the universal system, which made no distinction between pupils in reform and non-reform municipalities.

[7] 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 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.

residence when growing up, the algorithm assigns a binary reform exposure variable to each individual in these cohorts. Parish information is used for the large cities (Stockholm, Gothenburg, and Malmö), where the reform was sometimes implemented in different parishes at different times. The algorithm assigns reform exposure to 90 percent of individuals born 1943-1955 who have non-missing information on municipality and parish of residence.[8]

We use schooling data from the education register in 1990. The register contains information on highest educational degree completed, which we use to impute years of schooling.[9] Table I shows the distribution of schooling in our sample 2 years before and 2 years after the reform for both men and women. There are some interesting gender differences. Pre-reform, men were more likely to have less than 9 years of schooling (19% for men versus 13% for women), so the reform had a larger effect on their schooling attainment. Also, the proportion of men whose education was 11 years or more increases by 4.8 percentage points while that for women increases by 3.6 percentage points. Even when we control for cohort and municipality dummies, this effect is larger for men (1.5 versus 0.7 percentage points). Overall, the reform had much larger effects on educational attainment of men than of women.

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, Campbell, and Sodini 2007), and it is this form of wealth that is the focus of

[8] 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.

[9] 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.

our study. For data on asset allocation, we predominantly rely on the Swedish Wealth Data (Förmögenhetsregistret). These data were collected by the government's statistical agency, Statistics Sweden, for tax purposes between 1999 and 2006, when the wealth tax was abolished. The data include 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 financial institutions. Importantly, nontaxable securities and securities owned by investors below the wealth tax threshold were included in the reports (Calvet et al., 2007). 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 our analysis, we focus on wealth in the year 2000

Between 1999 and 2005, banks were not required to report small bank accounts to the Swedish Tax Agency unless the account accrued more than 100 SEK (about $11) in interest during the year. From 2006 onwards, banks were required to report all bank accounts above 10,000 SEK. In our data, 47% of people do not have a reported bank account.[10] Since almost everybody actually has a bank account, even if not reported (in surveys, the fraction of Swedes aged 15 and above that have a bank account has consistently been 99 percent (Riksbanken, 2014)), we follow Calvet and Sodini (2014) and impute bank account balances for persons without a bank account. We use the subsample of individuals in year 2000 (about 700,000) for which we observe the bank account balance even though the earned interest is less than 100 kronor. We regress the balance onto the following observable characteristics: age and age squared, household size, real estate wealth, level and squared level of disposable income, and financial wealth other than bank accounts. We use the coefficients from this regression to impute the account balances of individuals who report no bank account.

We analyze equity market participation using four outcome variables. The first

[10] This is consistent with Calvet, Campbell and Sodini (2007) who find that 2 million out of a total of 4.8 million households do not have a reported bank account in 2002.

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

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.

Table II 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 0.30 and 0.25 and the average risky share was 0.52 and 0.54 respectively.

Research has found that women tend to be more risk-averse in general and in their financial decision making (Zinkhan and Karande, 1991; Barsky et al., 1997; Jianakoplos and Bernasek, 1998; Barber and Odean, 2001; Eckel and Grossman, 2008; Croson and Gneezy, 2009 among others). Also, the change in the compulsory schooling reform we study may have had different effects on boys and girls. Therefore, in all our analyses we split the sample by gender to allow for the fact that the responsiveness of risk-taking behavior may differ between men and women.[11]

4. Empirical Specification

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

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

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:

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

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

[11] There were no incentives for husbands and wives to strategically allocate assets between themselves. In the event of a divorce, in the absence of a prenuptial agreement, all assets are split equally among spouses. Also, for the wealth tax that existed until 2006, the value of jointly owned assets was split equally between the two tax filers.

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 characteristics, 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 characteristics, municipality-specific trends and region by cohort dummies), exposure to the reform is as good as random.[12]

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 AI. The available family background characteristics include education of each parent, income of each parent in 1968 (the first year the income register is available), year-of-birth of each parent, whether each parent is Swedish, and whether the mother is married in 1968. Each column shows the coefficients from a municipality by birth cohort level regression where we regress a binary indicator of reform exposure on the average family background characteristics of that cohort in that municipality. Table AI(a) shows unweighted estimates and Table AI(b) shows estimates where we weight each municipality-cohort cell by the number of individuals in that cell. Once we include municipality fixed effects (in column (2)), the estimates of all variables in column (2) are small and statistically insignificant (except for that on mother married in the weighted regression which is significant at the 10% level). This continues to be the case in the

[12] In the three biggest cities (Stockholm, Gothenburg, and Malmö) the reform was implemented in different parishes in different years. So, there is some variation in reform status even conditioning on municipality and cohort indicators. We show later that our results are robust to dropping these cities.

specifications with region-by-birth-cohort fixed effects and municipality-specific trends (columns 3 and 4), suggesting that our identifying assumption may be reasonable. In the Online Appendix, we provide further evidence on this by showing our main estimates are robust to adding family background variables as additional controls.

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.

5. Results

First Stage

We first examine the relationship between exposure to the reform and years of education. In Table III, we present the regression results of the first-stage effects of reform exposure on education using three 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. When we next add region by year of birth fixed effects to the base specification (Column 2), this leads to a slight increase in the coefficients to 0.31 for men and 0.18 for women. Finally, column (3) 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 IV shows the effect of schooling on stock market participation and risky asset holding. Panel A presents the estimates for stock market participation of men and panel B for women. The first column shows the OLS relationship between schooling and stock market participation. Consistent with the existing literature, 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 percentage point increase in the probability of stock market participation. Columns (2) to (4) then present the IV estimates for a variety of specifications. All specifications include cohort and municipality dummies and columns (3) and (4) also include controls for region-by-year-of-birth fixed effects and municipality-specific trends, respectively. Among males, in the baseline specification (Column 2), an extra year of education increases stock market participation by 2 percentage points from a base of 42%. Stephens and Yang (2014) show that, in the U.S., estimates using compulsory schooling laws for identification are generally not robust to adding interactions of census region and year-of-birth to the regression. In column (3) 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. Among women, although the OLS estimates are similar to those for 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 IV, we study a broader measure of risky financial behavior -- risky market participation. The IV estimate is now weaker for men, with a magnitude of about 1 percentage point, and is marginal in terms of statistical significance. This suggests that the effect of education operates through the decision to purchase individual stocks instead of through mutual funds. The estimates for women continue to be negative and statistically insignificant.

In addition to influencing participation decisions, education could also affect the allocation decision between risky and less risky assets. In Table V, we look at the intensive margin and study the effects of education on the log (share) of stock holdings (Panels A and B) and risky assets (Panels C and D) in financial wealth, conditional on participation. One more year of education increases the share of stocks in financial wealth for men by 10 percent. Relative to the average share of 30 percent, this is a 3 percentage point increase. However, as at the extensive margin, there is no evidence of any effect for women. When we turn to the share of financial wealth in risky assets, the effects for men are slightly smaller than for stock holding -- one year more of education increases the risky share by 6-8 percent. As before, there is no evidence of an effect for women. [13]

Our instrumental variables estimates measure a local average treatment effect, as they represent the effect of education for those who would have obtained 7 years of education but who, after the reform, obtain 9 (that is, the effect for compliers). We next examine whether the compliers resemble the population as a whole. To do so, we define a binary treatment (whether education is greater than or equal to 9 years) and use the approach proposed by Abadie (2003), who extended the methods of Imbens and Rubin (1997) for cases with control variables. We are then able to characterize the proportion of compliers in the sample as well

[13] One interesting question is whether higher education leads to greater diversification of financial investments. Since direct holding of individual stocks involves less diversification than mutual funds, we have directly evaluated the effect on diversification by restricting the sample to persons who hold risky assets and using the mutual fund share as the dependent variable. We found no evidence of a causal effect of education on the mutual fund share (the IV coefficient for men is small and statistically insignificant -- see Table AII). Given that the mutual fund share could be thought of as a proxy for diversification (Calvet et al., 2009), this suggests that more education does not necessarily lead to more diversified portfolios or improve financial sophistication. Cronqvist and Siegel (2014) find a similar result, using a sample of Swedish twins.

as the average values of the dependent variables and of selected covariates for the set of compliers in addition to for the full sample.[14] Table AIII shows that the compliers have about 10% lower average stock market and risky market participation than the full sample but average parental education and parental income levels are similar. Overall, for both men and women, the complier group is sufficiently like the full population that we expect our estimates to generalize beyond this group.

What aspects of the reform matter?

The discussion so far has assumed that the effects of the reform only work through increased educational attainment. However, as discussed earlier, the reform decreased the degree of tracking somewhat in grades 6 to 9 and so affected the peer composition faced by individual students in these grades. We now study whether our estimates are likely to have been influenced by this aspect of the reform.

One possibility is that the positive effect among males could be coming from positive peer effects on lower Socio-Economic Status (SES) boys due to greater exposure to high SES peers post-reform. If so, we would expect a greater effect of the reform in more heterogeneous SES municipalities. We explore this issue by defining a heterogeneous parish as one where the standard deviation (SD) of parental schooling is above the mean standard deviation.[15] We then create an interaction term between reform exposure and a variable indicating a parish being above the mean SD. Including the reform dummy, the interaction effect, and the being-above-mean-dummy (along with birth cohort and municipality fixed effects), results in an insignificant interaction term for stock holding and risky holding for both males and females (results are in the Online Appendix). This suggests that the effects of the reform are similar in

[14] We refer readers to Abadie (2003) for further details.

[15] In Sweden, a parish is a unit of geography that is smaller than the municipality; a municipality can contain multiple parishes.

both low and high heterogeneity parishes, and it is therefore unlikely that the changing peer effects induced by tracking have a large effect on our estimates.

Comparison to Results from Previous Literature

Our results are generally consistent with those from the Swedish twin studies of Barnea et al. (2010) and Calvet and Sodini (2014), as both these studies find that adding twin fixed effects results in positive (albeit sometimes statistically insignificant) effects of education on stock holding and, like ours, their "causal" estimates are smaller than the cross-sectional OLS estimates. However, these estimates are not directly comparable to ours because they use indicator variables for education levels and we use years of education.

Using variation in U.S. state compulsory schooling laws, Cole et al. (2014) find that an additional year of education increases the probability of owning equities by 4 percentage points (statistically significant at the 10% level). This estimate is about twice as large as our estimate of the effect of education on male stock holding in Sweden but it is quite imprecisely estimated and so is not statistically different from our findings. They do not consider the effect of education on the share of financial assets in stocks or other risky assets, conditional on participation. Overall, our results are consistent with the small number of prior estimates in the literature.

6. Mediating Variables and Gender Differences

We have found that, among men, exogenous increases in education lead to greater stock market participation and increases in the share of financial wealth allocated to stocks and risky assets, conditional on participation. We now explore the mechanisms underlying this relationship and why the estimates differ between men and women. We begin by analyzing the male results and looking systematically for causal pathways by examining the sensitivity of the male education coefficients to the addition of various control variables.[16] We acknowledge that many of these variables, such as earnings, are themselves potentially influenced by education. As such, these could be described as "bad controls" in the terminology of Angrist and Pischke (2009). Note, though, that this is a standard mediation analysis, as our goal is to see how the coefficient on education changes when these variables are added as controls. If adding a particular control changes the estimated coefficient on education, it suggests that the effects of education on risk taking may be occurring as a result of the effects of education on the variable included.

These estimates are in Table VI. Column (1) of Table VI has our baseline estimates without additional controls. In subsequent columns, we add controls for (2-digit) industry and (3-digit) occupational indicators, earnings uncertainty, number of children, whether married, the savings rate, the number of hospital nights, and net worth (using indicators for quartile of the net worth distribution). These have minimal effects on any of the coefficients, suggesting none of these constitute important mechanisms.[17]

One channel, emphasized by Cooper and Zhu (2016), is that education increases earnings and, hence, leads to greater risky market participation.[18] This could be because a stable return to human capital can partially substitute for bond holding, or because the fixed costs of investment decrease with financial wealth and hence with earnings.[19] Andersen and

---

[16] We do not study mediating variables for women as, for them, there are no significant effects of education to explain.

[17] In the Online appendix, we describe how we construct these variables and show that there is no evidence that education affects them.

[18] We show in the Online Appendix that, using 2SLS, the earnings return to education is about 3% for men but there is no evidence of a positive return for women. The low 2SLS earnings returns to education are consistent with estimates from previous Swedish studies (Meghir and Palme, 2005) and with many other studies using European data (Devereux and Hart, 2010; Pischke and von Wachter, 2008).

[19] We show in the Online Appendix that education increases financial wealth of men, particularly at the bottom of the financial wealth distribution. There is no evidence of an effect of education on the financial wealth of women.

Nielsen (2011) and Briggs et al. (2015) provide evidence that sizeable increases in wealth have positive but fairly small effects on stock market participation. Less is known about the causal effect of wealth on risk attitudes and these papers do not study the share of financial wealth allocated to stocks conditional on stock market participation. However, it is plausible that greater financial wealth might lead to a higher risky share conditional on participation.[20]

For stock market participation, adding log(earnings) -- the log of average earnings between 1980 and 2000, including only those years with positive earnings -- in column (9) causes the male coefficient on education to fall from 0.018 in column (1) to 0.014. Adding indicators for financial wealth quartile in column (10) reduces it to a statistically insignificant 0.011.[21] Including all mediating variables (column 11), gives a coefficient on education of 0.010 for men in the stock participation regression, which is very similar to when the only controls are for financial wealth, suggesting that, once one controls for financial wealth, the other mediating variables add little. This suggests that an important channel through which education affects stock market participation of men is through higher financial wealth that is likely due in part to greater labor market earnings.

Strikingly, none of the mediating variables affect the education coefficient when the dependent variable is the stock share or the risky share. This is despite the fact that our mediating variables include industry, occupation, earnings, earnings volatility, financial wealth, health, marital status, number of children, and the savings rate. This finding suggests that education changes the stock share through its effect on unmeasured variables such as risk aversion. These findings are consistent with many standard portfolio choice models, where fixed costs affect the likelihood of investing in stocks but risk aversion determines the

[20] Calvet and Sodini (2014) report a strong positive relation between financial wealth and the risky share in a sample of Swedish twins.

[21] 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 through investment returns.

proportion of financial wealth invested in stocks, conditional on participation.

Overall, we conclude that reduced fixed costs through increased financial wealth is a likely channel through which education affects the stock market participation of men. 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. The fact that our mediating variables have no impact on the effect of education on the risky share provides further evidence that education affects risk aversion. However, given we have no data on risk preferences, this can only be speculative.

Explaining the Results for Women

There are several potential reasons why we find no evidence of an effect of the reform on financial decision making of women. First, as we showed earlier, the reform had a much weaker effect on education levels of women and, as a result, our estimates for women are much less precise. Second, the findings that exposure to the reform had little effect on investment behavior of women is consistent with research examining the effect of exposure to the reform on other outcomes. For example, Hjalmarsson, Holmlund, and Lindquist (2015) find negative effects on crime for males but not for females, and in our own analysis we find no evidence of a positive effect of the reform on earnings or financial wealth of women (or, indeed, on any of the mediating variables we study). Given these results, it is unsurprising that we do not find evidence that the reform increased the financial risk taking of women.

7. Intergenerational Spillovers

So far, we have considered the role of individual education on own investment behavior. Because of the richness of our data, we can also examine whether the effects of education are transmitted to the portfolio choices of the children of the affected cohorts.

While we are unaware of prior work on this question, Hryshko et al. (2011) find a negative effect of parental education on the measured risk aversion of their children using variation induced by compulsory schooling laws and data from the Panel Study of Income Dynamics.

We study children born in 1980 or earlier and, in order to study outcomes of the children at the oldest possible age, we focus on our last year of wealth data (2006) when the children are aged at least 26. [22] More details about the sample and some robustness checks are in the online appendix. Table VII summarizes the main results. Panel A shows the effects for fathers and Panel B for mothers. Panels A and B of Table VII maximize sample sizes by treating each parent separately; in Panel C, we instead include education of both parents in the specification and instrument each with the reform. While this allows the child choice to be influenced by the education of both parents, it is also restrictive, as we require both parents to have been born in the reform interval (1943-1955). Irrespective of the specification, our instrumental variables estimates show no evidence of intergenerational effects. This contrasts with the OLS estimates that suggest significant and positive correlations between parental schooling and children's holdings. The difference between the OLS and IV estimates likely arises because the OLS estimates suffer from omitted variable bias due to correlations between parental education and genetic and environmental factors that influence children.

8. 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, we estimate the causal relationship between education and portfolio choices in the financial market. We find a positive effect of education on stock market participation for men but not for women. The evidence also suggests that

[22] Using Norwegian data, Fagereng, Gottlieb, and Guiso (2017) document life cycle patterns in stock market participation and portfolio allocation.

greater financial wealth is a possible mechanism underlying this relationship, consistent with a theory of fixed costs of participation.

We also find that, for men who invest in risky assets, there is a positive and statistically significant effect of education on the share of financial wealth held in stocks and in risky assets more broadly. This result is not explained by any of our large number of potential mediators including industry, occupation, earnings, earnings volatility, financial wealth, health, marital status, number of children, and the savings rate. Therefore, a plausible explanation is that greater education leads to less risk aversion. In many standard portfolio choice models, fixed costs affect the likelihood of investing in stocks but risk aversion determines the proportion of financial wealth invested in stocks, conditional on participation. 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. However, given we have no data on risk preferences, this can only be speculative.

Interestingly, we find no evidence that higher education of men leads to their children being more likely to hold a higher proportion of financial wealth in risky assets. Overall, our findings suggest that education policy has impacts on asset allocation decisions but that these do not seem to spillover to individuals who are not directly affected. Compulsory schooling laws have been shown to have many beneficial impacts on later outcomes of persons affected. Our findings suggest that, at least for men, they may also lead to greater investment in the stock market and, perhaps, higher returns on investments.

References

Abadie, A., (2003) Semiparametric instrumental variable estimation of treatment response models, *Journal of Econometrics* 113, 231–263.

Andersen, S. and Nielsen, K.M., (2011) Participation constraints in the stock market: Evidence from unexpected inheritance due to sudden death, *Review of Financial Studies 24*(5), 1667–1697.

Angrist, J.D. and J.S. Pischke (2008) *Mostly Harmless Econometrics: An Empiricist's Companion*, Princeton: Princeton University Press.

Barber, B. M., and Odean, T. (2001) Boys will be boys: Gender, overconfidence, and common stock investment, *Quarterly journal of Economics* 261–292.

Barnea, A, Cronqvist, H, and S. Siegel (2010) Nature or nurture: what determines investor behavior? *Journal of Financial Economics* 98, 583–604.

Barsky, R. B., Juster, F. T., Kimball, M. S., and Shapiro, M. D. (1997) Preference parameters and behavioral heterogeneity: an experimental approach in the health and retirement study, *The Quarterly Journal of Economics* 112, 537–579.

Bernheim, B. D., and Garrett, D. M. (2003) The effects of financial education in the workplace: evidence from a survey of households, *Journal of Public Economics 87*(7), 1487–1519.

Bertaut, C. C. (1998) Stockholding behavior of US households: evidence from the 1983–1989 survey of consumer finances, *Review of Economics and Statistics 80*(2), 263-275.

Bertrand, M., Duflo, E., and Mullainathan, S. (2004) How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics 119*(1), 249–275.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2008) Staying in the classroom and out of the maternity ward? the effect of compulsory schooling laws on teenage births, *The Economic Journal 118*(530), 1025-1054.

Bogan, V. (2008) Stock market participation and the internet, *Journal of Financial and Quantitative Analysis* 43(1), 191–211.

Bound, J., and Solon, G. (1999) Double trouble: on the value of twins-based estimation of the return to schooling, *Economics of Education Review* 18(2), 169–182.

Briggs, J., Cesarini, D., Lindqvist, E. and Ostling, R., 2015. Wealth and stock market participation: estimating the causal effect from Swedish lotteries, Working Paper.

Calvet, L. E., Campbell, J. Y., 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. (2009) Fight or flight? portfolio rebalancing by individual investors, *The Quarterly Journal of Economics 124*.

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

Campbell, J. Y. (2006) Household finance, *The Journal of Finance* 61(4), 1553–1604.

Carling, K., Holmlund, B., and Vejsiu, A. (2001) Do benefit cuts boost job finding? Swedish evidence from the 1990s, *The Economic Journal 111*(474), 766–790.

Christelis, D., Jappelli, T., and Padula, M. (2010) Cognitive abilities and portfolio choice, *European Economic Review 54*(1), 18–38.

Christiansen, C., Joensen, J. S., and Rangvid, J. (2008) Are economists more likely to hold stocks? *Review of Finance 12*(3), 465-496.

Cole, S., Paulson, A., and Shastry, G. K. (2014) Smart money? the effect of education on financial outcomes, *Review of Financial Studies* 27(7), 2022–2051.

Cooper, R., and Zhu, G. (2016) Household finance over the life-cycle: what does education contribute? *Review Of Economic Dynamics* 20, 63–89.

Cronqvist, H., and Siegel, S. (2014) The genetics of investment biases, *Journal of Financial Economics 113*(2), 215–234.

Croson, R., and Gneezy, U. (2009) Gender differences in preferences, *Journal of Economic literature* 448-474.

Devereux, Paul J. and Robert A. Hart. (2010) Forced to be rich? returns to compulsory schooling in Britain, *Economic Journal* 120:1345–1364.

Dynan, K. E., and Maki, D. M. (2001) Does stock market wealth matter for consumption? Board of Governors of the Federal Reserve System (No. 2001-23).

Eckel, C. C., and Grossman, P. J. (2008) Differences in the economic decisions of men and women: Experimental evidence, *Handbook of experimental economics results* 1, 509–519.

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

Grinblatt, M., Keloharju, M., and Linnainmaa, J. (2011) IQ and stock market participation, *The Journal of Finance* 66(6), 2121–2164.

Guiso, L., Haliassos, M., and Jappelli, T. (2001) Household portfolios: An international comparison, *Household Portfolios*.

Guiso, L., Haliassos, M., and Jappelli, T. (2003) Household stockholding in Europe: where do we stand and where do we go? *Economic Policy* 18(36), 123–170.

Guiso, L., and Jappelli, T. (2008) Financial literacy and portfolio diversification. *EIEF WP 12/08.*

Guvenen, F. (2006) Reconciling conflicting evidence on the elasticity of intertemporal substitution: A macroeconomic perspective, *Journal of Monetary Economics* 53(7), 1451-1472.

Hjalmarsson, R., Holmlund, H, Lindqvist, R. (2015) The effect of education on criminal convictions and incarceration: causal evidence from micro-data. *Economic Journal* 125(587), 1290–1326.

Haliassos, M., and Bertaut, C. C. (1995) Why do so few hold stocks? *The Economic Journal* 1110–1129.

Hastings, J. S., Madrian, B. C., and Skimmyhorn, W. L. (2013) Financial literacy, financial education and economic outcomes. *Annual Review of Economics* 5, 347.

Holmlund, H. (2008) A researcher's guide to the Swedish compulsory school reform*, CEE DP 87*. Centre for the Economics of Education. London School of Economics and Political Science, Houghton Street, London, WC2A 2AE, UK.

Holmlund, H., Lindahl, M., and Plug, E. (2011) The causal effect of parents' schooling on children's schooling: a comparison of estimation methods, *Journal of Economic Literature* 49(3), 615-651.

Hong, H., Kubik, J. D., and Stein, J. C. (2004) Social interaction and stock market participation, *The Journal of Finance 59(1), 137-163.*

Hryshko, D., Luengo-Prado, M. J., and Sorensen, B. E. (2011) Childhood determinants of risk aversion: The long shadow of compulsory education. *Quantitative Economics* 2(3), 37–72.

Imbens, G. W., and Rubin, D. B. (1997) Estimating outcome distributions for compliers in instrumental variables models, *Review of Economic Studies* 64 (4), 555-574.

Jianakoplos, N. A., and Bernasek, A. (1998) Are women more risk averse? *Economic inquiry*, 36(4), 620.

Koijen, R. S. J., Van Nieuwerburgh, S., and Vestman, R. (2014) Judging the quality of survey data by comparison with "truth" as measured by administrative records: Evidence from Sweden, In *Improving the Measurement of Consumer Expenditures*. University of Chicago Press.

Lundborg, P., Nilsson, A., and Rooth, D. F. (2014) Parental education and offspring outcomes: evidence from the Swedish compulsory school reform, *American Economic Journal: Applied Economics* 6(1), 253-78.

Marklund, S. (1981) Från reform till reform: Skolsverige 1950–1975, Del 2, Försöksverksamheten. Stockholm: Skolöverstyrelsen och UtbildningsFörlaget.

Marklund, S. (1987) Från reform till reform: Skolsverige 1950–1975, Del 5, Försöksverksamheten. Stockholm: Skolöverstyrelsen och UtbildningsFörlaget.

Meghir, C., and Palme, M. (2005) Educational reform, ability, and family background, *American Economic Review* 414-424.

Meghir, C., Palme, M., and Schnabel, M. (2012) The effect of education policy on crime: an intergenerational perspective, *National Bureau of Economic Research* (No. w18145).

Park, W., and Son H. (2015) The impact of college education on labor market outcomes and household financial decisions, Working Paper.

Pischke, J.-S. and von Wachter, T. (2008) Zero returns to compulsory schooling in Germany: evidence and interpretation, *Review of Economics and Statistics* 90(3), 592-598.

Riksbanken (2014) Har kontanter någon framtid som lagligt betalningsmedel? Ekonomiska kommentar. Nr 9 2014.

SCB (2011) Olika generationers barnafödande. Demografiska rapporter 2011:3.

Stephens Jr, M., and Yang, D. Y. (2014) Compulsory education and the benefits of schooling, *American Economic Review 104*(6), 1777–1792.

Svensson, A. (2008) Har dagens tonåringar sämre studieförutsättningar? En studie av förskjutningar i intelligenstestresultat från 1960-talet och framåt. Pedagogisk Forskning i Sverige 13 (4): 258–77.

Thörnqvist, T., and Vardardottir, A. (2014) Bargaining over risk: the impact of decision power on household portfolios. *Manuscript.*

Van Rooij, M., Lusardi, A., and Alessie, R. (2011) Financial literacy and stock market participation, *Journal of Financial Economics 101*(2), 449-472.

Vissing-Jorgensen, A. (2002) Towards an explanation of household portfolio choice heterogeneity: Nonfinancial income and participation cost structures, *National Bureau of Economic Research* (No. w8884).

Zinkhan, G. M., and Karande, K. W. (1991) Cultural and gender differences in risk-taking behavior among American and Spanish decision makers, *The Journal of Social Psychology 131*(5), 741–742.

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

This table reports descriptive statistics on years of schooling. In the table, "Before" indicates the education distribution of cohorts in the two years prior to the reform, while "After" indicates the distribution two years post reform. Column 3 reports the difference between columns (2) and (1) and the significance level of t-tests on the difference between (2) and (1), where *** p<0.01, ** p<0.05, and * p<0.1. Column 4 reports the number of observations in each subgroup. Note that because the reform occurred in different municipalities at different times, the actual year of the reform varies by municipality.

Years of schooling	Before	After	Difference (2)-(1)	Observations
	(1)	(2)	(3)	(4)
		Men		
7	18.9%	2.8%	-16.0***	19,762
9	8.8%	22.7%	13.8***	25,709
9.5	3.0%	0.4%	-2.5***	3,122
11	27.6%	31.3%	3.6***	49,935
12	15.6%	14.7%	0.9***	25,868
14	5.4%	6.5%	1.1***	10,015
15.5	19.8%	20.6%	0.8***	34,418
19	1.0%	1.0%	0.0	1,678
N	93,279	77,228		
		Women		
7	12.7%	1.8%	-10.9***	12,826
9	8.0%	18.4%	10.4***	20,839
9.5	3.7%	0.6%	-3.1***	3,836
11	37.0%	38.0%	1.0***	61,585
12	8.7%	9.4%	0.8***	14,863
14	1.9%	2.2%	0.3***	3,352
15.5	27.8%	29.3%	1.5***	46,832
19	0.2%	0.2%	0.0	385
N	90,289	74,228		

Table II. Summary Statistics.

This table reports descriptive statistics on selected outcome variables and independent variables. * 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.

	Males			Females		
	Mean	Standard Deviation	Obs	Mean	Standard Deviation	Obs
Portfolio characteristics:						
Financial wealth*	323,918	3,370,000	662,096	203,043	710,105	642,119
Direct equity participation**	0.42	0.49	662,096	0.30	0.46	642,119
Direct equity share***	0.30	0.28	275,389	0.25	0.26	194,618
Risky market participation**	0.63	0.48	662,096	0.63	0.48	642,119
Risky share***	0.52	0.31	417,957	0.54	0.30	403,705
Financial characteristics:						
Real Estate wealth*	656,229	2,184,561	662,096	459,735	1,315,905	642,119
Total wealth*	1034649	34,400,000	662,096	678,698	7,460,986	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 (years of schooling)	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

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

This table reports OLS estimates of effect of reform exposure on years of schooling. All specifications include birth cohort and municipality fixed effects. In addition, column (2) includes region-by-cohort fixed effects, and column (3) includes municipality-specific linear trends. Standard errors clustered by municipality are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)
		Panel A: Males	
Reform exposure	0.266	0.305	0.323
	(0.043)***	(0.023)***	(0.024)***
Observations	662,096	662,096	662,096
Adjusted R^2	0.07	0.07	0.07
F-statistic	38.79	177.67	186.33
		Panel B: Females	
Reform exposure	0.164	0.183	0.213
	(0.031)***	(0.018)***	(0.020)***
Observations	642,119	642,119	642,119
Adjusted R^2	0.05	0.05	0.05
F-statistic	27.18	106.76	114.63
Region by cohort FE	NO	YES	NO
Mun. linear trends	NO	NO	YES

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

This table reports OLS and instrumental variables estimates of the effect of years of schooling on participation in stock markets (Panels A-B) and risky markets (Panels C-D). Column 1 reports OLS estimates of the relationship whereas columns (2)-(4) show instrumental variables estimates of the effect of schooling from specifications including birth cohort and municipality fixed effects. In addition, column (3) includes region-by-cohort fixed effects, and column (4) includes municipality-specific linear trends. Standard errors clustered by municipality are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)
	Panel A: Stock market participation, males			
Years of Schooling	0.034	0.018	0.022	0.016
	(0.001)***	(0.008)**	(0.009)**	(0.008)**
Observations	662,096	662,096	662,096	662,096
Adjusted R^2	0.05			
	Panel B: Stock market participation, females			
Years of Schooling	0.029	-0.018	-0.025	-0.012
	(0.000)***	(0.017)	(0.014)*	(0.011)
Observations	642,119	642,119	642,119	642,119
Adjusted R^2	0.04			
	Panel C: Risky market participation, males			
Years of Schooling	0.033	0.012	0.020	0.012
	(0.001)***	(0.008)	(0.008)**	(0.007)*
Observations	662,096	662,096	662,096	662,096
Adjusted R^2	0.06			
	Panel D: Risky market participation, females			
Years of Schooling	0.036	-0.021	-0.014	-0.016
	(0.000)***	(0.018)	(0.013)	(0.011)
Observations	642,119	642,119	642,119	642,119
Adjusted R^2	0.06			
Region by cohort FE	NO	NO	YES	NO
Municipality linear trends	NO	NO	NO	YES

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

This table reports OLS and instrumental variables estimates of the effect of years of schooling on (log)share of stocks (Panels A-B) and risky holdings (Panels C-D) out of total financial wealth. Column 1 reports OLS estimates of the relationship whereas columns (2)-(4) show instrumental variables estimates of the effect of schooling from specifications including birth cohort and municipality fixed effects. In addition, column (3) includes region-by-cohort fixed effects, and column (4) includes municipality-specific linear trends. The estimates are conditional on holding any stock (panels A-B) or any risky asset (panels C-D). Standard errors clustered by municipality are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)
	Panel A: (log)share of stocks, males			
Years of Schooling	0.048	0.107	0.098	0.086
	(0.002)***	(0.048)**	(0.046)**	(0.046)*
Observations	275,389	275,389	275,389	275,389
Adjusted R^2	0.03			
	Panel B: (log)share of stocks, females			
Years of Schooling	0.029	-0.012	-0.003	-0.051
	(0.002)***	(0.099)	(0.073)	(0.081)
Observations	194,618	194,618	194,618	194,618
Adjusted R^2	0.03			
	Panel C: (log)share of risky assets, males			
Years of Schooling	0.041	0.077	0.059	0.061
	(0.001)***	(0.030)***	(0.027)**	(0.026)**
Observations	417,957	417,957	417,957	417,957
Adjusted R^2	0.02			
	Panel D: (log)share of risky assets, females			
Years of Schooling	0.033	-0.036	-0.067	-0.018
	(0.001)***	(0.049)	(0.043)	(0.034)
Observations	403,705	403,705	403,705	403,705
Adjusted R^2	0.02			
Region by cohort FE	NO	NO	YES	NO
Municipality linear trends	NO	NO	NO	YES

Table VI. Effect of education on stock market participation, (log)stock share, and (log)risky share, controlling for possible mediators. Males.

This table reports instrumental variables estimates of the effect of schooling on financial outcomes, controlling for potential mediators. The mediators are measured as follows: Sector: 2-digit sector codes. Occupation (occ): 3-digit occupation codes. # children: number of children. Earnings uncertainty: Standard deviation of mean earnings 1980-2000. Married: dummy variable indicating being married or not. Savings rate: ((Disposable income – consumption)/Disposable income) averaged 2000-2006. Hospital nights: total number of nights spent in hospital between 1990 and 1999. Net total wealth: dummy variables indicating position (quartile) of the net wealth distribution. Labor Income: (log) average earnings between 1980 and 2000. Financial wealth: dummy variables indicating position (quartile) in the financial wealth distribution. All specifications control for birth cohort fixed effects and municipality fixed effects. Standard errors clustered by municipality are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
					Panel A: Stock market participation						
Schooling	0.018	0.018	0.019	0.017	0.016	0.018	0.018	0.015	0.014	0.011	0.010
	(0.008)**	(0.008)**	(0.008)***	(0.008)**	(0.008)*	(0.008)**	(0.008)**	(0.007)**	(0.008)*	(0.007)	(0.07)
Obs.	662,096	662,096	662,096	662,096	662,096	662,096	662,096	662,096	662,096	662,096	662,096
					Panel B: (log)share of stocks						
Schooling	0.107	0.106	0.107	0.107	0.106	0.107	0.107	0.112	0.110	0.108	0.107
	(0.048)**	(0.047)**	(0.048)**	(0.048)**	(0.048)**	(0.048)**	(0.048)**	(0.048)	(0.052)**	(0.048)**	(0.052)**
Obs.	275,389	275,389	275,389	275,389	275,389	275,389	275,389	275,389	275,389	275,389	275,389
					Panel C: (log)share of risky assets						
Schooling	0.077	0.077	0.076	0.076	0.076	0.076	0.077	0.075	0.074	0.078	0.076
	(0.030)***	(0.030)***	(0.030)**	(0.030)***	(0.030)**	(0.030)**	(0.030)***	(0.028)**	(0.032)**	(0.030)***	(0.031)**
Obs.	417,957	417,957	417,957	417,957	417,957	417,957	417,957	417,957	417,957	417,957	417,957
Sector/occ.	NO	YES	NO	NO	NO	NO	NO	NO	NO	NO	YES
# children	NO	NO	YES	NO	NO	NO	NO	NO	NO	NO	YES
Earn uncert	NO	NO	NO	YES	NO	NO	NO	NO	NO	NO	YES
Married	NO	NO	NO	NO	YES	NO	NO	NO	NO	NO	YES
Saving rate	NO	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES
Hosp days	NO	NO	NO	NO	NO	NO	YES	NO	NO	NO	YES
Net wealth	NO	NO	NO	NO	NO	NO	NO	YES	NO	NO	YES
Labor inc.	NO	NO	NO	NO	NO	NO	NO	NO	YES	NO	YES
Fin. wealth	NO	NO	NO	NO	NO	NO	NO	NO	NO	YES	YES

Table VII. Results for children's outcomes.

This table shows estimates of 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 for the sample of married men and women. Panel A shows the effect of father's schooling, Panel B of mother's schooling, and Panel C of both parents schooling (where both parents are born between 1943-1955). Robust standard errors in parentheses. Standard errors clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

	(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	0.021	-0.048	0.030	-0.038
	(0.000)***	(0.009)	(0.000)***	(0.010)	(0.002)***	(0.092)	(0.00)***	(0.049)
Observations	713,886	713,886	713,886	713,886	147,729	147,729	351,909	351,909
Adjusted R^2	0.03		0.03		0.005		0.01	
				Panel B: Mothers				
Schooling	0.018	-0.013	0.024	-0.022	0.017	-0.022	0.027	0.0001
	(0.000)***	(0.014)	(0.000)***	(0.015)	(0.002)***	(0.116)	(0.001)***	(0.061)
Observations	920,148	920,148	920,148	920,148	199,629	199,629	458,204	458,204
Adjusted R^2	0.03		0.03		0.005		0.006	
				Panel C: Mothers and Fathers				
Schooling (mother)	0.013	-0.003	0.018	-0.023	-0.000	-0.167	0.004	0.008
	(0.000)***	(0.011)	(0.000)***	(0.014)	(0.002)	(0.147)	(0.001)***	(0.044)
Schooling (father)	0.012	0.009	0.013	0.008	0.012	-0.043	0.009	0.005
	(0.000)***	(0.011)	(0.000)***	(0.015)	(0.002)	(0.190)	(0.001)***	(0.046)
Observations	412,465	412,465	412,465	412,465	95,459	95,459	222,395	222,395
Adjusted R^2	0.03		0.04		0.01		0.003	
Birth FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipal. FE	YES	YES	YES	YES	YES	YES	YES	YES

Appendix Tables

Table AI(a). Predicting reform exposure by parental characteristics (Unweighted Regressions).

This table reports estimates of the relationship between parental characteristics and reform exposure. Data is aggregated at the municipality-birth cohort level. The dependent variable is 1 if the reform is in place in the municipality for that cohort and 0 otherwise. 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. Parental income measured as log of income in 1968. Standard errors clustered by municipality are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)
Father schooling	0.0698	-0.0043	-0.0104	0.0009
	(0.0165)***	(0.0129)	(0.0123)	(0.0122)
Mother schooling	-0.0371	-0.0077	-0.0125	-0.0043
	(0.0221)*	(0.0155)	(0.0143)	(0.0148)
Father (log)income	0.0116	0.0114	0.0522	0.0018
	(0.0704)	(0.0512)	(0.0471)	(0.0469)
Mother (log)income	-0.0659	-0.0413	-0.0447	-0.0601
	(0.0823)	(0.0542)	(0.0482)	(0.0499)
Father birth year	0.0056	-0.0007	0.0003	-0.0006
	(0.0058)	(0.0042)	(0.0039)	(0.0042)
Mother birth year	0.0115	-0.0043	-0.0050	-0.0048
	(0.0065)*	(0.0048)	(0.0044)	(0.0045)
Mother married	0.5640	-0.0900	-0.0990	-0.0873
	(0.1132)***	(0.0784)	(0.0742)	(0.0698)
Mother Swedish	-0.6033	0.1473	0.0746	-0.0415
	(0.2723)**	(0.2525)	(0.2625)	(0.1999)
Father Swedish	-0.3191	-0.1207	-0.1532	-0.0835
	(0.2497)	(0.2518)	(0.2284)	(0.2178)
Observations	13,086	13,086	13,086	13,086
Adjusted R^2	0.360	0.657	0.722	0.753
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

Table A1(b). Predicting reform exposure by parental characteristics (Weighted Regression)

This table reports estimates of the relationship between parental characteristics and reform exposure. Data is aggregated at the municipality-birth cohort level and the estimates are weighed by the number of individuals at the municipality-birth cohort level. The dependent variable is 1 if the reform is in place in the municipality for that cohort and 0 otherwise. 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. Parental income measured in log of income in 1968. Standard errors clustered by municipality are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)
Father schooling	0.0783	0.0098	-0.0016	0.0177
	(0.0321)**	(0.0238)	(0.0211)	(0.0303)
Mother schooling	-0.0023	-0.0314	-0.0204	-0.0304
	(0.0421)	(0.0315)	(0.0215)	(0.0231)
Father (log)income	-0.0822	-0.0916	-0.0333	0.0625
	(0.1134)	(0.1019)	(0.0818)	(0.0647)
Mother (log)income	0.4109	0.0594	-0.0709	0.0383
	(0.1497)***	(0.1327)	(0.0852)	(0.1016)
Father birth year	0.0107	0.0027	-0.0041	-0.0014
	(0.0106)	(0.0066)	(0.0052)	(0.0059)
Mother birth year	0.0175	-0.0034	-0.0031	0.0015
	(0.0117)	(0.0066)	(0.0059)	(0.0066)
Mother married	0.4142	-0.6760	-0.2045	-0.4430
	(0.2932)	(0.3585)*	(0.1208)*	(0.2119)**
Mother Swedish	-1.0744	0.3440	0.0299	0.2207
	(0.4895)**	(0.5271)	(0.4009)	(0.5324)
Father Swedish	0.1468	0.5280	0.5858	0.2649
	(0.4835)	(0.4755)	(0.4105)	(0.4564)
Observations	13,086	13,086	13,086	13,086
Adjusted R^2	0.436	0.680	0.761	0.759
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

Table AII: The effect of education on the share of risky assets that are in mutual funds.

This table reports OLS and instrumental variables estimates of the effect of schooling on share of risky assets that are in mutual funds. All specifications control for birth cohort fixed effects and municipality fixed effects. Standard errors are clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

	(OLS)	(IV)	(OLS)	(IV)
	Share of Mutual funds	Share of Mutual funds	Share of Mutual funds	Share of Mutual funds
	Males		**Females**	
Schooling	-0.006	0.014	0.007	0.017
	(0.002)***	(0.041)	(0.001)***	(0.057)
N	326,394	326,394	355,930	355,930
Adjusted R^2	0.02		0.01	
Birth FE	YES	YES	YES	YES
Municipal. FE	YES	YES	YES	YES

Table AIII. Characterizing compliers. Males and Females.

This table reports overall means, complier means, and the ratio between the two means for selected variables using the methods of Abadie (2003). Also reported is the estimated proportion of compliers. Parental income is the average of the parents' income in 1968 (the first year of data in the Income Register). Monetary values are reported in Swedish Krona on December 31, 2000. At the time, the exchange rate was 1 USD = 9.42 SEK. The fraction of compliers is obtained from the coefficient of reform exposure in a first-stage regression where the dependent variable is an indicator variable for having 9 or more years of schooling. The specification includes birth cohort and municipality fixed effects.

	(1)	(2)	(3)
Means:	**Overall**	**Compliers**	**Ratio (2)/(1)**
		Panel A: Males	
Stock market participation	0.416	0.365	0.88
Risky market participation	0.631	0.584	0.93
Parental schooling	8.296	8.298	1.00
Parental income	151,883	146,482	0.96
Fraction compliers		0.124	
		Panel B: Females	
Stock market participation	0.303	0.246	0.81
Risky market participation	0.629	0.559	0.89
Parental schooling	8.278	8.220	0.99
Parental income	150,616	149,182	0.99
Fraction compliers		0.082	