Online Appendix for "Learning to Take Risks? The Effect of Education on Risk-Taking in Financial Markets"

Sandra E. Black, Paul J. Devereux, Petter Lundborg, Kaveh Majlesi

January 2018

This Internet Appendix provides robustness checks and further information about our data. It is arranged in the following order:

1. Additional Robustness Checks
2. Effects of Peer Composition
3. Effects of Education on Mediating Variables
4. Further Information about the Intergenerational Analysis

1. Additional Robustness Checks

Here, we describe several further robustness checks that we conducted for our main analysis.

Adding Extra Control Variables

We show in the paper that there is little evidence that the timing of reform implementation is related to the family background characteristics of children. As an additional check, we have re-estimated the specifications in Tables IV and V with the addition of extra control variables. All these regressions control for mother's and father's years of schooling, (log)income of mother and father, birth year of each parent, citizenship status of parents, marital status of the mother, and indicators for missing values on parental variables. These estimates are reported in Tables OAI and OAII. The 2SLS estimates are almost identical to those in tables IV and V, consistent with reform implementation being close to random, conditional on municipality and cohort indicators.

Varying the Cohorts and Municipalities Included

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 OAIII 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.

Unlike the rest of the country, in the three largest cities, Stockholm, Gothenburg, and Malmö, 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 (so there is variation at the parish level even conditioning on municipality). In case this leads to error and because of uncertainty regarding the exact number of pre-reform years of schooling in these three cities, 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 in these cities due to their generally higher levels of pre-reform education -- 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.

Effect of Year of Wealth Data

Our decision to use wealth data from 2000 was arbitrary, and Tables OAIV and OAV show 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. The column (5) estimates are almost identical to the estimates using 2000 data; this is unsurprising, as investment behavior tends to change slowly over time.

The only surprising finding in Table OAIV is that, for men, the effects found for direct stock investment are smaller for 1999 than for the other years, but the effect on mutual fund investment is bigger. 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 in 2000. However, the PPS introduction increased familiarity with mutual funds and might explain why the effect of schooling on risky market participation decreases after 1999. This suggests that, when it comes to investment in mutual funds, information provision can reduce the role of education. Another possible explanation is that the stock market was booming in 1999 and fell during 2000. Less educated persons may be more likely to leave the riskier stock market and invest in less risky mutual funds when the investment environment is less positive. We find some evidence that, for men, more education increased the probability of entering the market in 2000 and lowered the probability of exiting the market between 1999 and 2000.

2. Effects of Peer Composition

As discussed in the paper, 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.[1] 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 Table OAVI). This suggests that the effects of the reform are similar in both low and high heterogeneity parishes and therefore it is unlikely that the changing peer effects induced by tracking have a large effect on our estimates.

3. Effects of Education on Mediating Variables

Financial Wealth

Because the distribution of financial wealth is quite skewed--most people have low financial wealth but a relatively small number of people have very high levels-- we split the financial wealth distribution into quartiles and examine how education affects the likelihood

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

of being in a particular quartile. The estimates are in Table OAVII. We find that an extra year of education increases the probability of having financial wealth above the first quartile by 0.014 (significant at the 10% level) for men. However, there is no evidence that more education increases financial wealth higher up in the distribution for men. This is unsurprising given that the reform only increased education levels of people who would have had very low education levels. There is no evidence of a positive effect of education on financial wealth for women. This finding is consistent with our finding that education affects asset allocation of men but not women.

Earnings

In column (1) of Table OAVIII, we estimate the earnings return 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.[2] Using 2SLS, the earnings return to education is about 3% for men but there is no evidence of a positive return for women. Thus, it could be that because education affects earnings for men (but not women), asset allocation is more affected by education for men than women.

Savings Rates

More education may affect individual time preferences and induce people to save more and invest more in risky assets -- we would expect higher savings rates to increase stock market participation but it is unclear that they would impact the share of financial wealth allocated to stocks conditional on participation. Using information on income and changes in wealth in the 1999-2006 period, we are able to calculate the average savings rate over this

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

period for almost every household in our sample.

To calculate the savings rate, we first must calculate household consumption. To calculate consumption at the household level, we apply a methodology detailed in work by Koijen, Van Nieuwerburgh, and Vestman (2014). They propose a measure of consumption that is essentially the residual from the household's budget constraint, where consumption is equal to the amount of money taken in (including income and realized returns on assets) less the amount spent or saved.[3] This calculation requires the detailed information on asset portfolios that we have in our data as well as information on the prices of each individual asset that we have collected ourselves from various sources. Once we calculate consumption, we calculate the savings rate of the household as 1-(Average Consumption/Average Disposable Income) where the averaging is over the 2000 to 2006 period.

Column (2) of Table OAVIII shows how education affects the savings rate. The estimates for both men and women are very small and statistically insignificant, indicating that the increase in education due to the reform did not lead to changes in savings behavior.

Earnings Uncertainty

People with greater earnings uncertainty are likely to choose less risky portfolios. We

[3] More specifically, $c_{it} = y_{it} + d_{it} - \left(1 + r_{it}^{d}\right)d_{it-1} - a_{it} + a_{it-1}(1 + r_{it}^{a})$ where y_{it} represents total disposable income for household i in period t, d_{it} is the total debt at the end of year t, r_{it}^{d} is the household-specific interest rate on debt between t-1 and t, a_{it} represents the total value of the asset portfolio at the end of year t, and r_{it}^{a} represents the household-specific return on the asset portfolio between t-1 and t. To compute the household-specific return on the asset portfolio, using each security's ISIN, we collect data on the prices and returns for each stock, coupons for each bond, and net asset values per share for each mutual fund in the database from a number of sources, including Datastream, Bloomberg, SIX Financial Information, Swedish House of Finance, and the Swedish Investment Fund Association (FondBolagens Förening). More income that is not invested or used to reduce debt leads to higher measured consumption, as do declines net increases in debt. See Koijen, Van Nieuwerburgh, and Vestman (2014) for more details. These authors use a subsample of the Swedish wealth data to calculate this consumption measure and then match it to two other measures of consumption (including a more standard survey of individuals); when they compare their proposed measure to the more traditional survey measure, they find that, while the mean and median of the consumption distributions are similar, survey data overstate consumption of the bottom quintile of the distribution while understating consumption at the top.

have calculated the standard deviation of earnings (only including years with positive earnings) over the years 1980 to 2000 and use this as a measure of earnings uncertainty faced by the person. We find no evidence (column 3 of Table OAVIII) that education affects earnings uncertainty so this is unlikely to be a channel through which education operates.

Hospital Nights

More education may lead to better health and health status may have a direct effect on financial decision making -- like earnings uncertainty, health uncertainty may lead people to choose less risky portfolios. Our measure of health is the number of nights spent in hospital by the person between 1990 and 1999. We find no evidence that this is affected by education (Table OAVIII, Column 4).

Marital Status

Married men in our sample are 30 percent more likely to hold risky assets than unmarried men and having more education may increase the likelihood that they get married. In column (5), we find that this is in fact the case as education increases the probability a man is married. However, there is no evidence of a similar effect for women. Finally, in column (6), we examine the effect of education on the number of children the person has. We find no evidence of any effect of education for either men or women.

4. Further Information about the Intergenerational Analysis

To link children to their parents, we use the Swedish multigenerational register. With our data, we can 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.[4]

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. Table OAIX 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.

There are two potential concerns with the intergenerational analysis. 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 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. The estimates are -0.07 for males and -0.17 for females. In addition, we have run the analyses in Table VII controlling for the age of the child in 2006. The conclusions were robust to this change.

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

Table OAI. Effect of education on participation in stock markets and risky markets with additional controls included. 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) when additional control variables are included. 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, columns (3)-(4) include: (3) region-by-county fixed effects, and (4) municipality-specific linear trends. All regressions control for mother's and father's years of schooling, (log)income of mother and father, birth year, citizenship, marital status, and indicators for missing values on parental variables. 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	0.030	0.020	0.024	0.015
	(0.001)***	(0.008)**	(0.008)***	(0.008)*
Observations	662,096	662,096	662,096	662,096
Adjusted R^2	0.06			
	Panel B: Stock market participation, females			
Years of Schooling	0.024	-0.018	-0.025	-0.024
	(0.000)***	(0.015)	(0.013)*	(0.013)*
Observations	642,119	642,119	642,119	642,119
Adjusted R^2	0.05			
	Panel C: Risky market participation, males			
Years of Schooling	0.030	0.014	0.021	0.011
	(0.001)***	(0.007)*	(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	0.032	-0.021	-0.015	-0.018
	(0.000)***	(0.016)	(0.013)	(0.012)
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 OAII. Effect of education on (log)share of stocks and risky holdings out of total financial wealth with additional controls included. 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 when additional control variables are included. 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, columns (3)-(4) include: (3) region-by-county fixed effects, and (4) municipality-specific linear trends. All regressions control for mother's and father's years of schooling, (log)income of mother and father, birth year, citizenship, marital status, and indicators for missing values on parental variables. 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	0.039	0.110	0.100	0.088
	(0.002)***	(0.049)**	(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	0.015	-0.026	-0.023	-0.057
	(0.001)***	(0.109)	(0.083)	(0.082)
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	0.037	0.077	0.059	0.060
	(0.001)***	(0.029)***	(0.026)**	(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	0.029	-0.039	-0.075	-0.020
	(0.001)***	(0.049)	(0.046)	(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 OAIII. Robustness checks.

This 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.

	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	0.089	0.056	-0.014	-0.016	0.036	0.009
	(0.009)**	(0.008)***	(0.053)*	(0.031)*	(0.015)	(0.016)	(0.087)	(0.045)
N	438,180	438,180	178,688	272,761	424,597	424,597	125,765	263,462
	Panel B: Dropping the three largest cities							
Schooling	0.021	0.016	0.081	0.053	-0.002	-0.002	0.034	-0.013
	(0.007)***	(0.007)**	(0.038)**	(0.022)**	(0.010)	(0.010)	(0.061)	(0.033)
N	579,156	579,156	242,135	369,479	560,947	560,947	168,694	354,925
Birth FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipal. FE	YES	YES	YES	YES	YES	YES	YES	YES

Table OAIV. Effect of education on participation in stock markets and risky markets. Results by year.
This table reports instrumental variables estimates of the effect of schooling on stock market and risky market participation year by year. All specifications include 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. Standard errors clustered by municipality are reported in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)	(5)
Year	1999	2000	2001	2002	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 B: 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

Table OAV. Effect of education on (log)share of stocks and risky holdings out of total financial wealth. Results by year.
This table reports instrumental variables estimates of the effect of schooling on (log) shares of stocks and risky assets year by year. All specifications include 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.

	(1)	(2)	(3)	(4)	(5)
Year	1999	2000	2001	2002	1999-2002
			Panel A: (log)share of stocks, males		
Schooling	0.119	0.107	0.082	0.096	0.122
	(0.050)**	(0.048)**	(0.045)*	(0.045)**	(0.046)***
N	223,744	275,389	270,879	270,160	206,312
			Panel B: (log)share of stocks, females		
Schooling	0.112	-0.012	-0.057	-0.032	0.080
	(0.105)	(0.099)	(0.125)	(0.125)	(0.098)
N	149,971	194,618	192,449	192,059	138,692
			Panel C: (log)share of risky assets, males		
Schooling	0.062	0.077	0.071	0.072	0.074
	(0.032)*	(0.030)***	(0.030)**	(0.031)**	(0.028)***
N	369,494	417,957	414,129	412,884	349,606
			Panel D: (log)share of risky assets, females		
Schooling	0.033	-0.036	-0.028	-0.000	0.014
	(0.046)	(0.049)	(0.052)	(0.051)	(0.042)
N	351,543	403,705	401,409	400,481	334,392
Birth FE	YES	YES	YES	YES	YES
Municipal. FE	YES	YES	YES	YES	YES

Table OAVI. Peer Effects. Heterogeneous effects of reform exposure by the standard deviation of parental schooling.

This table reports the reduced form effect of reform exposure on stock market and risky market participation. In addition, it shows the effect of the interaction term between the reform indicator and an indicator of going to school in a municipality where the standard deviation of parental schooling is above the mean in the sample, as calculated at the birth cohort level (Reform*High SD parental schooling). All specifications control for birth cohort fixed effects, 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)
	Stock market participation	Risky market participation	Stock market participation	Risky market participation
	Males		Females	
Reform	0.003	0.002	-0.002	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)
High SD parental schooling	0.013	0.010	0.0132	0.010
	(0.003)***	(0.003)***	(0.004)***	(0.003)***
Reform*High SD parental schooling	0.003	0.002	-0.002	0.001
	(0.004)	(0.004)	(0.004)	(0.005)
N	662,096	662,096	642,119	642,119
Birth FE	YES	YES	YES	YES
Municipal. FE	YES	YES	YES	YES

Table OAVII. Effect of education on financial wealth.

This table shows instrumental variables estimates of the effect of schooling on financial wealth cutoffs. All specifications control for birth cohort fixed effects and municipality fixed effects. Columns 1-3 show the effect of schooling on the likelihood of having wealth above the (1) 1st quartile, (2) above median, and (3) above 3rd quartile of the financial wealth distribution. Standard errors clustered by municipality are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)
	>Q1	>Q1-Q2	>Q3
		Men	
Schooling	0.014	0.011	0.006
	(0.008)*	(0.008)	(0.011)
Observations	662,096	662,096	662,096
		Women	
Schooling	-0.021	-0.002	-0.008
	(0.016)	(0.015)	(0.015)
Observations	642,119	642,119	642,119
Birth FE	YES	YES	YES
Municipality FE	YES	YES	YES

Table OAVIII. Effect of education on potential mediating variables.

This table reports instrumental variables estimates of the effect of schooling on various outcomes. The outcomes are measured as follows: Labor Income: (log) average earnings between 1980 and 2000. Savings rate: ((Disposable income – Consumption)/Disposable income) averaged 2000-2006. Earnings uncertainty: Standard deviation of mean earnings 1980-2000. Hospital nights: total number of nights spent in hospital between 1990 and 1999. Married: dummy variable indicating being married or not. # Children: number of children. 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.

	Labor income	Savings rate	Earnings uncertainty	Hospital nights	Married	# Children
			Panel A: Males			
Schooling	0.028	0.003	1194	0.561	0.016	0.034
	(0.011)***	(0.017)	(1030)	(0.615)	(0.008)**	(0.022)
Observations	658,383	652,302	658,479	662,096	662,096	662,096
Mean	11.171	0.088	80230	5.541	0.603	1.113
SD	(0.702)	(0.869)	(65831)	(30.055)	(0.489)	(1.074)
			Panel B: Females			
Schooling	-0.030	0.008	-1521	1.802	0.003	-0.120
	(0.025)	(0.020)	(1122)	(1.316)	(0.013)	(0.072)*
Observations	637,945	636,015	638,412	642,144	642,144	642,144
Mean	10.851	0.081	66783	6.431	0.629	1.482
SD	(0.764)	(0.598)	(29989)	(31.624)	(0.483)	(1.075)
Birth FE	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES

Table OAIX. Summary Statistics for children.

This table reports descriptive statistics for children. 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.

	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*	0.27	0.27	147729	0.28	0.27	199629
Risky market participation*	0.49	0.50	713886	0.50	0.50	920148
Risky share**	0.41	351909	0.30	0.41	0.30	458204
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