DISCUSSION PAPER SERIES

No. 7844

THE EFFECT OF EDUCATION ON EQUITY HOLDINGS

Dmytro Hryshko, María José Luengo-Prado
and Bent E Sørensen

INTERNATIONAL MACROECONOMICS

Centre for Economic Policy Research

www.cepr.org

Available online at: **www.cepr.org/pubs/dps/DP7844.asp**

ISSN 0265-8003

THE EFFECT OF EDUCATION ON EQUITY HOLDINGS

Dmytro Hryshko, University of Alberta
María José Luengo-Prado, Northeastern University
Bent E. Sørensen, University of Houston and CEPR

Discussion Paper No. 7844
June 2010

Centre for Economic Policy Research
53–56 Gt Sutton St, London EC1V 0DG, UK
Tel: (44 20) 7183 8801, Fax: (44 20) 7183 8820
Email: cepr@cepr.org, Website: www.cepr.org

CEPR Discussion Paper No. 7844

June 2010

ABSTRACT

The Effect of Education on Equity Holdings

We study the effect of education on equity ownership in the form of stocks or mutual funds (outside of retirement accounts). We find a causal effect of education on stockholding using the number of colleges in the county where the respondent grew up as an instrument and data from the Panel Study of Income Dynamics. The effect is particularly strong for whites from non-privileged backgrounds. We explore the channels through which education affects equity holdings using the Wisconsin Longitudinal Survey and find that, controlling for family fixed effects, gaining a white-collar job appears to be the main channel.

JEL Classification: G11
Keywords: college openings and portfolio demand

Dmytro Hryshko
University of Alberta
8-14 HM Tory Building
Edmonton
AB, T6G 2H4
CANADA

Email: dhryshko@ualberta.ca

For further Discussion Papers by this author see:
www.cepr.org/pubs/new-dps/dplist.asp?authorid=172142

Maria Luengo-Prado
Departament of Economics
Northeastern University
301 Lake Hall Boston
MA 02115-5000
USA

Email: M.Prado@neu.edu

For further Discussion Papers by this author see:
www.cepr.org/pubs/new-dps/dplist.asp?authorid=172109

Bent E Sorensen
Department of Economics
University of Houston
204 McElhinney Hall
Houston, TX 77005
USA

Email: bent.sorensen@mail.uh.edu

Submitted 11 May 2010

We demonstrate that higher education is associated with higher propensity to own stocks using data from the Panel Study of Income Dynamics (PSID). A high correlation between education and equity ownership need not reflect a causal relation because unobserved variables such as household attitudes, preferences, and abilities are likely to affect both the propensity to own equity and the choice of education. We uncover the causal effect of education on household participation in the stock market using instrumental variable techniques. As an instrument for education, we use the number of 4-year colleges when the respondent was 17 in the county where he or she grew up. The identifying assumption is that college openings are orthogonal to unobserved household traits that may determine future propensities to hold stocks. We find a strong and significant effect of education on the incidence of household stockholding with a stronger effect for white individuals who report growing up in non-privileged households. "Stockholding," when we analyze the PSID data, refers to holdings of stocks and mutual funds outside of retirement accounts.

Having established a causal relation using the PSID, we use the Wisconsin Longitudinal Survey (WLS) to examine potential mechanisms. We confirm, using OLS, the strong correlation between a college degree and direct holding of equity (which includes bonds in the analysis of the WLS data). We then, one-by-one, include variables that are likely to be impacted by going to college, such as holding a white-collar job. If the inclusion of a set of variables renders the college dummy insignificant, the channels through which college affects the propensity to hold equity must be within this group of variables. The WLS is well suited for this task because of the rich information collected—for example, siblings are interviewed, which allows us to control for family background using family-fixed effects. We find that holding a white-collar job appears to be the most important channel through which college affects the propensity to own stock. Our two-pronged approach is similar to the strategy followed by Cole and Shastry (2009). They examine financial market participation using a large U.S. Census dataset, where they identify a household as participating in financial markets if it reports having dividend, interest, or rental income. They use state-variation in compulsory schooling as an instrument but

explore, with no instruments, the channels through which education may affect financial market participation using detailed survey data from the National Longitudinal Survey of Youth (NLSY). Our paper complements Cole and Shastry (2009): we are able to use instrumental variables to examine stock holdings while their instrumental variables regressions are limited to broader asset holdings which include savings accounts and bonds in addition to equity. Our paper seems to be the first paper that examines the relation between stockholding and education in the U.S. using instrumental variables.

Many households in the United States choose not to participate in the stock market. Understanding this non-participation is important for several reasons. First, there is substantial heterogeneity in household wealth holdings after controlling for household demographic characteristics and income (e.g., Campbell 2006). Low participation rates in the stock market may help explain part of this heterogeneity as households who have stocks in their portfolios may end up with higher wealth (compared to otherwise similar households) because stocks offer, on average, higher returns than other assets. Low participation rates can also result in low wealth holdings of the average household at the time of retirement. Furthermore, low participation in the stock market can contribute towards an explanation of the equity premium puzzle—the inability of the consumption-based asset pricing model to reconcile the low correlation between aggregate consumption growth and stock excess returns with plausible levels of risk aversion. Indeed, as suggested by several studies (e.g., Attanasio, Banks, and Tanner 2002, Vissing-Jørgensen 2002a) what may matter for the determination of stock excess returns and stock prices in the economy is the consumption risk of those who do participate in the stock market.

College openings will potentially lower the cost of college education for a family by reducing the distance between a college of choice and the family's residence. This may allow students to live at home and save on the cost of shelter or it may make it easier for students to hold a part-time job near the college. For students who need to finance college mainly by borrowing, or students who face relatively high costs of borrowing during college years, this lower cost of college may have a strong impact on the tendency to obtain a

college degree.[1] College openings may potentially affect the propensity to go to college through other channels, for example, by aggressive recruiting of students. We show that the effect of college openings is stronger for children from less advantaged homes which is consistent with the cost of college education being an important determinant of college completion.

Why does education affect the probability of participating in the market for risky assets? Haliassos and Bertaut (1995) highlight inertial factors while Alan (2006) shows that small fixed entry costs in the stock market—such as gathering and processing relevant information—can account for non-participation.[2] Education may lower these fixed entry costs making households with more educated members more likely to hold stock. van Rooij, Lusardi, and Alessie (2007) explore the effect of households' financial literacy—the knowledge and understanding of basic financial concepts related to stocks and bonds—on households' stock ownership. Using exposure to economics courses while in high school as an instrument for the respondent's financial literacy, they find a significant and large effect of household's financial knowledge on the probability of owning stock. In a related paper, Ameriks, Caplin, and Leahy (2003) show that households with higher propensities to plan—in particular, those with better mathematical skills and those who are keen on detailed vacation planning—accumulate more wealth. Similarly, Christelis, Jappelli, and Padula (2006) find that the propensity to invest in stocks is strongly associated with cognitive abilities using data from the Survey of Health, Ageing and Retirement in Europe, as do Cole and Shastry (2009) using NLSY data. More schooling therefore can raise the likelihood of households' participation in the stock market by enhancing cognition or the mathematical ability of students.[3] Thus, IQ and cognitive skills constitute measurable inputs which may affect the ability to devise and implement financial plans. The WLS has collected measures of these variables, along with the major field of study for respondents, which allow us to further explore these two channels (financial literacy and cognition)

[1] For a formal model, see Cameron and Taber (2004).

[2] Similarly, Perraudin and Sørensen (2000) argue that observed zero holdings of stocks and bonds by many U.S. households can be attributed to the fixed and proportional costs of holding non-zero amounts of these assets.

[3] Hansen, Heckman, and Mullen (2006) show that individuals with more schooling achieve higher scores on standardized tests of individual mathematical reasoning.

along with others.

Hong, Kubik, and Stein (2004) find that "more sociable" households who know their neighbors and frequently attend church have a higher probability of participating in the stock market. Sociability can also be affected by schooling: while in college, individuals enroll in many elective courses and this fosters interaction among students who hone their communication skills and expand their social networks. The results of Hong, Kubik, and Stein (2004) are consistent with our findings—working in a white-collar job exposes an individual to a relatively higher number of direct stockholders which may impact the individual's own choices over stockholding through learning, mimicking, or even the enjoyment of talking about stocks.

Interestingly, even after controlling for financial literacy (van Rooij, Lusardi, and Alessie 2007) and the intensity of social interaction (Hong, Kubik, and Stein 2004), the above-mentioned studies still find that education is positively correlated with stock market participation. This implies that there are many other potential channels besides social interaction, financial literacy, and mathematical abilities through which education may affect individual decisions to own stocks.

The rest of the paper is organized as follows. In Section 1, we describe the PSID data used and present Probit and IV-Probit results using the PSID, while Section 2 describes the WLS and presents Probit and OLS regressions with and without family fixed effects. Section 3 concludes.

1 PSID results

1.1 The PSID data

1.1.1 Stock ownership

We compile household wealth, income, and demographic data from the PSID. Information on household wealth is obtained from the PSID wealth supplements available at five-year intervals starting in 1984, and every other year from 1999 to 2007. Households may hold stock directly, by purchasing shares in publicly traded companies, or indirectly, in their

pension funds and retirement accounts. In 1984 and 1989, heads of household were asked the following question about family stockholding:

> "Do you (or anyone in your family living there) have any shares of stock in publicly held corporations, mutual funds, or investment trusts, including stocks in IRAs?"

In 1999, 2001 2003, 2005, and 2007 the PSID has a similar question that asked respondents to disregard stock holdings in employer-based pensions or IRAs and, in those years, there is a separate question on whether the household had any money in private annuities or IRAs. In 1994, the question about stock ownership excluded stocks held in IRAs and employment-based pensions but the PSID did not include a separate question about money held in IRAs and private annuities.[4] Thus, for the 1994 through 2007 wealth supplements we are able to construct measures of households' stockholding within and outside of retirement accounts, while we cannot separate out stocks in retirement accounts when using 1984 and 1989 data. Our measure of stock ownership is a dummy variable equal to one if a household holds stocks outside of retirement accounts, zero otherwise. For brevity, we will use the shorthand "direct stockholdings" for holdings of stocks and mutual funds outside of retirement accounts.

Table 1 provides some summary statistics for the PSID data used in our empirical analysis. The share of households that directly own stock is stable over time at about 25 percent (although the fraction owning stock directly or indirectly has increased steadily over time).

1.1.2 The instrument and environmental variables

Our sample selection rules are the following. First, we drop individuals who are not heads of household in any year when household wealth was recorded. We then construct consistent measures of the head's race, age, and background variables. Race, as well as other demographic information, is recorded at the time an individual enters the survey

[4]Since our measure of stock ownership uses the answers in all of the supplements, we believe this data discrepancy will be of no importance for our analysis.

as head for the first time, asked again any other time after an interruption in headship, and asked of every head, new or existing, in 1985, 1990, 1997, and 2005. As a result, race can be recorded multiple times for any head in the sample, and different records may not agree with each other. We consider the first record of the head's race and ignore records reported by any family member other than the head. We drop heads who give inconsistent answers about race.

Individual schooling and holdings of risky assets may be affected by the quality of the environment and individual experiences in childhood and adolescence. We are able to construct some measures of the overall quality of the county in which the respondent grew up.[5] We focus on county median income but also explored the effect of the percentage of urban population and the median house value in the county (results not reported for brevity).[6] In addition, the PSID allows us to measure individual-specific background variables. In particular, heads of household were asked about their fathers' and mothers' schooling, whether they lived with both parents, and whether their family was poor, of average well-being, or rich when they were growing up.

The PSID recorded father's education for the first time in 1968 and mother's education in 1974 for heads of household. Individuals report this information the first time they appear as heads of household or after an interruption in the headship. The information was updated for every head, new or existing, in 1997. As a result, heads may have several records on mother's and father's education which may not necessarily agree with each other. To construct a consistent measure of father's and mother's education we proceed as follows. First, we ignore records on education of the head's parents other than the first record and records reported by the heads themselves (e.g., after an interruption in the headship or in 1997).[7] The father's and mother's schooling variables in the PSID

[5]We obtain county-level information from Haines (2004) who compiled county-level data for 1790–2000 from historical decennial census and county data books (for the more recent years).

[6]These measures are highly correlated and we decided to control for median income in the county when an individual was 17 in all our regressions. Our results are robust to inclusion of all three county measures.

[7]We do the same for any other demographic measure that can be recorded multiple times. The PSID contains a variable that indicated who was the respondent to the survey questions. Background variables that pertain to the head may be recorded differently from one survey to another (for example, after an interrupted headship) if someone other than the head answers the survey questions. Thus, in our selection

are categorical and may be reported with some noise. For example, a head may report that his/her father finished 9–11 grades (some high school) in one year and 12 years (a high school graduate) in some other year. To avoid loss of information due to inconsistent records on parents' education, we construct the following categorical measure of the father's and mother's education. A parent's schooling is an indicator variable equal to one if the head consistently reports that the parent finished at least high school, and zero otherwise. Because father's and mother's schooling are highly correlated and our sample sizes are not large, we cannot statistically distinguish their separate effects on individual schooling or holdings of risky assets and we, therefore, set parental education to the maximum of the mother's and father's education dummies. We call this variable parents' education. It is equal to zero if neither parent completed high school and equal to one if at least one of the parents finished at least high school. The typical head of household in our sample grew up in a family with at least one parent who finished high school or more education (see Table 1).

Further, we construct an indicator variable for whether the head of household grew up in a poor family, a family of average wealth, or a rich family. We also construct a dummy, "Rich Parents," which is equal to one if the head reports his or her family was rich and equal to zero otherwise.[8] The dummy is set to missing if the records on parental wealth at childhood are inconsistent. We consider the records to be inconsistent if the head claims growing up in a rich family in one survey year and reports growing up in a poor family or family of average well-being some other year. In a similar manner, we construct consistent measures of whether the head lived with both parents while growing up and whether he grew up on a farm or in a city. About 28 percent of heads in our sample recall growing up in a rich family; 16 percent grew up on a farm. The majority of our sample heads, about 73 percent, grew up in a family of two parents.

rules, we "trust" the first record on background variables and any other records provided by the head himself.

[8]In 1999 and 2001, the records on parental wealth recall and whether heads lived with both parents while growing up were misrecorded. We therefore ignore the records in those years. Since the PSID follows split-offs of original families over time, in principle, it is possible to collect actual parental wealth for some individuals. However, samples constructed this way are too small for reliable regression analysis and we use recall variables instead.

Age in the PSID does not necessarily change in adjacent surveys since information can be collected at different months of a year. Also, some individuals have inconsistent age series which, among other things, may reflect typing errors by interviewers. We utilize information on the year of birth to construct a cleaner measure of age for those heads who have this information in the individual file. Otherwise, we use an individual's age at the time he/she first appears as a head in the survey to impute his/her age in other years.

We utilize data on head's years of schooling from the individual file of the PSID. Education records, first collected in 1968, are not updated annually. Rather, education is first recorded at the time an individual enters the PSID as head of household and it is updated if an individual reenters the survey as head after an interrupted headship. It was also updated for any head, new or existing, in 1975 and 1985. Our samples consist of heads who completed their education by the time they turned 23 and have consistent education records.

In the 1970–1993 waves, the PSID collected information on the county and state where the respondent grew up. Because we do not have data on the county of residence when the respondent was 17 (the age when college availability has potentially the largest effect on individuals' schooling decisions as they approach high school graduation), we utilize this variable instead. We drop heads with inconsistent records on the state and/or county of growing up.[9]

The instrument for own schooling is the number of colleges per 1,000 college-age persons in the county where the head grew up (college-age defined as being 18–22 years of age). Currie and Moretti (2003) construct a dataset that contains the availability of colleges in U.S. counties for 1960–1996. Our final samples contain only heads who turned 17 during this period.

The average head turned 17 in 1972 and grew up in a county with 0.09 colleges per 1,000 college-age persons (see Table 1). There is substantial variation in the availability of colleges, with "college-scarce" counties having zero colleges, and "college-abundant" counties having nearly 5 colleges per 1,000 persons aged 18–22. Household heads, on

[9]Multiple records on the state and/or county where the head grew up are possible if an individual reenters the PSID as the head after an interrupted headship.

average, are high school graduates, are predominantly male (74 percent), and 64 percent have been (not necessarily continuously) married during the sample period.

1.2 Estimation using PSID data

1.2.1 First Stage: The effect of college openings on education

Currie and Moretti (2003) study the effect of maternal education on health outcomes of children at birth. We follow Currie and Moretti (2003), instrumenting years of schooling with the number of colleges per 1,000 college-age persons in the head's county when he or she was 17, where "head's county" is short-hand for the county in which the head grew up. Currie and Moretti (2003) provide a detailed discussion of the validity of the instrument. For our purposes, the instrument is valid if it is effective in predicting education and it is unrelated to unobservable variables that affect household risky asset holdings such as, for example, heads' or their parents' attitudes towards risk or household earning capacities. We present some evidence on the effectiveness of the instrument in Tables 2 and 3. The most likely source of correlation between parents' attitudes and college availability would be if certain parents systematically moved to counties with more colleges. Currie and Moretti (2003) explore this issue in detail and find little evidence of such a pattern. They further guard against such correlations by including county dummies. Our sample is too small for doing this but we include state dummies and Currie and Moretti (2003) also verify, for a smaller sample from the National Longitudinal Survey of Youth (NLSY), that the first stage estimates are very close whether county or only state dummies are included. Another potential problem is that colleges are not randomly assigned to counties. Universities may be opened in wealthier areas where parents can afford to pay tuition, and one might worry that college openings correlate with features of counties such as high average wealth and education that might lead to children becoming more financially savvy.[10] However, our county-level income variable is designed to control for

[10]Inspecting the data, it appears that most college openings take place in suburbs rather than in old cities with established universities—areas that often are predominantly populated by white middle-class families. Examples of colleges opened during our sample period are Montserrat College in Essex, MA; Saddleback College and Concordia University in Orange County, CA, and York College in Queens, NY.

this potential problem.

In Table 2, we regress individual years of schooling on the number of 4-year colleges per 1,000 persons aged 18–22 in the heads' county at age 17. In addition, we control for parental education, an indicator for growing up in a rich family, an indicator for growing up with both parents, and median income in the county when the head was 17. More recent cohorts attain, on average, more years of schooling and have access to more colleges in their county. We, therefore, included a full set of year-of-birth dummies in order to control for the correlation between the availability of colleges and individual years of schooling due to aggregate trends in schooling. Geographical areas may have different endowments and industrial structure (e.g., agricultural versus manufacturing states) and therefore may permanently differ in their demand for an educated workforce. Areas with relatively higher demand for skilled workers might attract more educated individuals (parents) and build more colleges in order to support a sustainable supply of skilled workers. If parental education is higher in some states and is correlated with unobserved traits that affect offspring's education, we might find the number of colleges correlating with education even if there were no direct causal effect on education from college availability. To hedge against such effects, we include dummies for the state where the head grew up.

In column (1), we present results for the entire sample. We find that adding one more 4-year college per 1,000 college-age persons, holding everything else constant, increases individual education by about 0.5 years.[11] The effect is significant at the 5 percent level. Children who grew up in better environments—in more educated, richer, and stable families, and high-income counties—attain higher levels of schooling.

In column (2), we add 2-year colleges but this variable is not statistically significant while the estimated impact of 4-year colleges is unchanged. In columns (3) and (4), we drop respondents who grew up in rich families who may not be constrained by the availability of local colleges. As expected, we find a stronger impact of 4-year college openings for this sample, but still no significant effect of 2-year college openings.

[11]The effect is somewhat smaller than the one found in Currie and Moretti (2003) but their result is based on a sample of females, while our sample consists predominantly of males as seen in Table 1. Perhaps, the difference is due to the fact that education of males is less affected by changes in the availability of local colleges.

Columns (5) and (6) examine white and non-white respondents, respectively. The effect of 4-year colleges on education is larger for non-white respondents, likely because the cost of college is more important for this group. For non-white respondents, the effect of 2-year colleges is statistically significant with a large (1.8 years) point estimate. Most parental controls, such as "Lived with Both Parents" and "Grew up on a Farm" are insignificant for non-white respondents.

We want to ascertain that college openings affect college graduation by increasing the number of individuals who continue past high school. (College graduation rates may increase due to more students going to college after high school or due to to fewer students dropping out of college. We believe that an increase in college graduation due to more students going to college is consistent with college openings having a causal effect on graduation while the alternative might be due to underlying trends.) In Table 3, we first consider the effect of 4-year colleges on whether the head is a high school drop-out or finished high school and no more—column (1). One more 4-year college per 1,000 college-age persons in the county reduces the probability that the head finished 12 or less years of schooling by 11 percentage points. Similarly to Currie and Moretti (2003), we find that 4-year colleges do not affect some college—column (2), while the effect of 4-year colleges on the likelihood of individual college graduation is strong and significant at the 1 percent level. One more 4-year college per 1,000 college-age persons increases the probability of college graduation by about 12 percent. Thus, it appears that the increase in the college graduation probability is due to a reduction in the number of persons who finish high school but do not take any college courses. The magnitude of the effect of 4-year colleges on the likelihood of graduating from college is larger for the sample of households whose heads grew up in less well-off families. For this sub-sample, the net effect is a decline in the number of individuals who stop education after graduating from high school, an unchanged number of individuals with some college, and an increase in the number of college graduates.

1.2.2 Equity holdings. Probit and IV-probit Regressions

Years of schooling. Table 4 presents probit regressions of direct stock ownership on years of education and the estimates are presented in terms of the marginal impact on the probability of owning equity outside of retirement accounts.[12] Our probit estimations "regress" household risky equity ownership on heads' education, parents' education, and exogenous background variables and demographic controls. Consistent with previous studies, we find that more educated households have larger propensities to own equity. Holding other variables constant, one more year of schooling increases the probability of owning equity by 6 percent. Male-headed households and older households are more likely to own stock, as are households whose heads have more educated parents and those who grew up in high-wealth households or in high-income counties.

Own education is correlated with many unobserved household characteristics, such as preferences towards risk and abilities, and it may capture the effects of these omitted variables on the probability of direct equity holding. To eliminate such effects, we use as instruments the number of 4-year colleges when the head was 17, in the county where he or she grew up. Column (2) in Table 4 reports our results. The point estimate for the effect of education is clearly larger than in the non-instrumented regression, while the other variables, except for sex, become insignificant. One more year of schooling raises the probability of owning risky equity, holding other factors constant, by about 16 percent, an effect almost three times larger than that found using non-instrumented probit-estimation.

In column (3), we include some further but potentially endogenous controls (income, wealth, marital status, and family size).[13] Clearly, income and wealth are endogenous to education and wealthier households have larger propensities to own a stock—the wealth coefficient is nearly significant at the 10 percent level—consistent with previous studies (e.g., Campbell 2006, Vissing-Jørgensen 2002b). We find that family size is inversely

[12] In the literature many papers use simple OLS-regressions on cross-sectional data. Estimating linear regressions with OLS provided results similar to the probit results and we chose not to display them.

[13] Our measure of wealth is household net worth inclusive of net business wealth; income is the average combined labor and transfer income of the head and wife for 1981–1997, 1999, 2001, 2003, 2005, and 2007.

related to equity ownership, maybe due to households with more children being more risk averse. Introducing these additional controls lowers somewhat the point estimate on the education variable, from 0.164 to 0.149 which is consistent with wealth-accumulation being one channel through which education affects stockholding—the coefficient is no longer significant in this column but the overall picture from Table 4 is nonetheless that education has a significant, economically large, impact on stock holdings. Apparently, college education affects holdings of risky equity through many channels—wealth, income, ability to process information about the economy and financial markets, ability to plan, preferences towards time, etc.

In columns (4)–(7) of Table 4, we limit the sample to heads with non-rich parents and white heads with non-rich parents, (4)–(5) and (6)–(7) respectively. The results are similar to those of the previous two columns but significantly stronger. For example, the inclusion of income and wealth no longer makes the estimated impact of education smaller for white heads with non-rich parents—in fact, it is significant at the 1 percent level whether these variables are included or not.

It is somewhat puzzling that the IV-estimates are much larger than the OLS-estimates. However, this pattern has been found in related studies using similar instruments to estimate the effect of schooling on labor market outcomes. Card (2001) is an important paper that provides the following summary: "One interpretation of this finding is that marginal returns to education among the low education sub-groups, typically affected by supply-side innovations tend to be relatively high, reflecting their high marginal costs of schooling, rather than low ability that limits their return to education." We subscribe to this interpretation and conjecture that our instrument mainly affects the disadvantaged. Individuals face a trade-off between the cost of college and the benefits of having a college degree. If a college gets built nearby, the cost of attending college for disadvantaged individuals goes down as the student can live at home instead of having to move and pay dorm fees. The larger effect on own schooling when instrumented can be explained by a higher propensity of stockholding by that group. If those born into families with less wealthy parents are more likely to be credit constrained then our parental variables

may be capturing credit constraints broadly defined. The stronger effect for individuals with non-rich parents is consistent with this interpretation even if the split itself does not allow us to uniquely separate individuals that are constrained from those who are not.[14] The large IV-coefficient to income will mechanically depress the coefficients to variables such as parental education which are correlated with the instrument. Likely, parental education is an important determinant of college and stockholding, but the goal in this section is not to map out the role of parental education but rather to establish the causal effect of education on stockholding.

College degree. The effect of education may be mainly due to college graduation. In Table 4, we explore this issue estimating probit regressions of the same form as in the previous table, but instead of years of schooling, we use an indicator variable that equals one if the head graduated from college. The results for the total sample are presented in columns (1)-(3). If the head of household finishes college or attains some education beyond college, the probability of a household holding risky equity increases by about 26 percent according to the non-instrumented results in column (1) and by about 56 percent according to the instrumented results in column (2). These results are similar to those of the previous table in the sense that college graduation has an effect similar to a little more than three years of schooling.[15]

As in the regressions with a continuous measure of education, the effect of college graduation on the incidence of risky asset holdings is to a large degree driven by the sub-sample of households with heads from non-rich family backgrounds. For this group, the head's college graduation raises the probability of household holdings of risky equity by about 65 percent. The estimated effect is significant at the 1 percent level.

Overall, the effect of education, moved by changes in the number of local colleges, on individuals from poor or average families is quite substantial. Our results suggest that the construction of colleges in college-poor counties may be an effective policy not

[14]We do not find significant effects using the highly disadvantaged Survey of Economic Opportunity (SEO) sub-sample of the PSID. Probably individuals in this group are unlikely to ever own stock, regardless of whether a college opened nearby or not.

[15]The IV-coefficient to the college dummy is around 0.60, while the coefficient to years of schooling in the previous table is around 0.18 and the average head in the sample finished about 13 years of schooling.

only for increasing the number of skilled workers but also for affecting saving patterns of individuals from less advantaged backgrounds. Since risky equity, on average, generates higher wealth, increasing the education levels of the disadvantaged may better prepare them for retirement and help them buffer adverse shocks to their incomes.

2 Channels (Estimations using WLS data)

Having established a causal effect of college education on direct stockholding, we next provide a broad picture of the possible channels through which the effect manifests itself. We use data from the Wisconsin Longitudinal Survey (WLS), a long-term survey of a random sample of 10,317 men and women who graduated from Wisconsin's high schools in 1957. Although the WLS is limited in its geographical and cohort scope (rendering the college-opening instrument ineffective for this sample), it is very rich in other dimensions which allows us to give a deeper look at channels than is possible with PSID data. We explore different ways in which education may affect financial behavior including the effect of education on cognitive ability, occupational choice, wealth accumulation, marital status, and financial literacy associated with major choice.

Our strategy is to add these variables one-by-one to the (probability) regression of stockholding on college and examine which variables reduce the estimated impact of education. For example, if cognitive ability measured after college affects financial behavior after controlling for IQ at a young age, this indicates that cognitive ability is a potential channel for education to influence financial behavior. This approach is slightly unorthodox, in the sense that such a pattern in standard multiple regression analysis may simply imply that cognitive ability after college is a left-out variable. What validates the approach is the result of the previous section that exogenous changes in education affect equity holding and this exogenous effect cannot be rendered insignificant by a component of cognitive ability which is not caused by education.

Consider first the case when college completion, $Coll_i$, and cognitive ability after college, Cog_i, share the influence of a common unobserved component, u_i. College completion is further affected by college openings $CollOp_i$ (as in our first-stage regression), an un-

observed component v_i, and cognitive ability is affected by an unobserved component ϵ_i. Assume that v_i and ϵ_i are uncorrelated. These relations can be described by the following two equations: $Coll_i = \alpha_0 + \alpha_1 u_i + \alpha_2 CollOp_i + v_i$, and $Cog_i = \beta_0 + \beta_1 u_i + \epsilon_i$. In this case, by the Frisch-Waugh theorem, the coefficient to college completion in an OLS regression of the incidence of stock ownership on college completion and cognition after college will largely depend on the covariation of stock ownership with $CollOp_i$ and v_i.[16] From the second stage IV-regressions we know that college openings affect the probability of holding equity.[17] College openings are exogenous and not a function of u_i and, therefore, the effect of college (via college openings) on stock holdings cannot be rendered insignificant (except by chance) because of cognition capturing the effect of u_i. However, the effect of college openings on stockholding can be rendered insignificant by the inclusion of cognition after college in the probability estimate for stockholding if cognition itself is caused by college (and therefore by college openings); i.e., $Cog_i = \tilde{\beta}_0 + \tilde{\beta}_1 u_i + \tilde{\beta}_2 Coll_i + \tilde{\epsilon}_i$. Our interpretation of the results is that college openings affect college attendance which affects cognitive abilities measured after college which again affects the probability of holding equity. This pattern is consistent with the IV- and OLS-estimates considered together.

Further, the WLS-data is well suited to control for unobserved variables that share influences on cognition, college attendance and stockholding, in particular through pre-college measures of IQ and the inclusion of sibling fixed effects. The inclusion of such controls would make the "u"-term less important and make the causal effect from college to cognition more likely to be the dominant source of the correlation between these variables. The inclusion of other variables, besides cognition measured after college, in the regressions below are subject to similar considerations.

[16] To be precise, the coefficient to $Coll_i$ can be found by regressing the incidence of stockholding on the residual from regressing $Coll_i$ on Cog_i. For simplicity, normalize β_0 to zero; the residual, for the case outlined, equals $const + \alpha_1(1-\kappa)u_i + \alpha_2 CollOp_i + v_i - \frac{\alpha_1}{\beta_1}\kappa\epsilon_i$, where $\kappa \equiv 1/(1+\frac{\sigma_\epsilon^2}{\beta_1^2\sigma_u^2})$. If the variation in u_i is high and dominates the variation in cognition (so that κ approaches one), the inclusion of cognition will remove most of the variation in u_i from the college completion variable. Alternatively, if the variation in u_i is low, there is no left-out variable problem—in the OLS setting, the inclusion of cognition will not change much the estimated coefficient to the college completion variable.

[17] In the linear probability model, our IV coefficient, ignoring the covariates other than college, equals $\frac{cov(P_i, CollOp_i)}{cov(Coll_i, CollOp_i)}$, where P_i denotes the incidence of stockholding.

2.1 The WLS data

The WLS coverage is extensive with information on social background, schooling, labor market experience, family characteristics, social participation, psychological characteristics, etc. The survey has followed respondents throughout the life cycle collecting data from the original respondents in 1957, 1964, 1975, 1992, and 2004. Most important for our study is the availability of information on asset holdings, as well as several cognition measures for each respondent (IQ-tests from high school and several cognition measures in the various survey years). The WLS has also collected information for a selected sibling of each original respondent (if not an only child) in 1977, 1994, and 2005, which allows us to control for unobserved family characteristics.

Our measure of stock market participation is constructed from the 2004 question (2005 for siblings) "Do you or your spouse have stocks, bonds or shares in mutual funds?" We consider this to be a measure of "direct" stock-bond market participation different from participation through retirement accounts because respondents are also asked "Do you or your spouse have any retirement plans that accumulate an account balance—examples include IRA's, 401(k) plans, and profit sharing plans." (Respondents also provide information on the value of these assets). We focus on stock-bond holdings in 2004 and we use "current" (2004) controls except in the case of occupation where we use prime-age data from 1992.[18]

Table 6 presents summary statistics for the key variables in the sample we use in our regressions, which includes all main respondents answering the stock-bond market participation question in 2004. 55 percent of these report direct participation on financial markets beyond retirement accounts. This figure is much larger than the fraction of PSID participants who report owning stock, but this is not surprising because respondents in the WLS are on average more educated—they have at least finished high school. (Also, the questions regarding stock market participation are not exactly the same in both surveys as the PSID question does not include "bonds" in the wording.) The average number of

[18]In the 1992 survey, the questions regarding asset holdings are different and it is not possible to construct a clear measure of direct participation in financial markets. Respondents are asked if they have money on either savings or investment accounts instead.

years of schooling in our WLS sample is 13.5 with a standard deviation of 2 years, and 26 percent of the respondents have completed 4 or more years of college. To investigate the effect of major of study on stock market participation, we create a dummy variable that identifies respondents with an economics/business college degree, which is held by 6 percent of the sample.[19] The average age for respondents in 2004 is 64 with a small standard deviation of 0.7 years, and 47 percent of respondents are female. In 2004, 78 percent are married and have (ever had) three kids on average.[20] There is a great deal of variation in net worth within the sample, with average wealth at $667,026 and median wealth at $334,000.

To study the effect of occupation on stock-bond market participation, we construct a dummy variable that divides respondents into two groups according to their profession in the 1992 interview, when respondents are on average 52 years old, which we consider prime age. We say a respondent is "white collar" if he/she is a professional, a technical or kindred worker, a manager, an official, a proprietor, or a sales worker outside retail trade. All other respondents are considered "non-white collar." Similarly, we construct an aspiration white collar dummy, which indicates if the respondent wanted to be in one of these professions when first interviewed in 1957. 46 percent of respondents are "white collar" in 1992, while 42 percent wanted to be "white collar" when they were finishing high school.

The WLS has several intelligence measures for respondents over the years. We consider a high-school IQ measure which accounts for differences in intellectual ability before entering college, as well as a 2004 measure of cognition, arguably affected by college attendance. The high-school IQ score is mapped from a raw Henmon-Nelson test score (a 30 minute test consisting of 90 items presented in order of increasing difficulty), which all Wisconsin secondary school students took from 1933 to the late 1950s or early 1960s according to the WLS documentation. The test includes "vocabulary, sentence completion, disarranged sentences, classification, logical selection, series completion, directions,

[19]The majors included in our econ/business major dummy are accounting and finance, business, and economics and includes B.A., B.Sc., and graduate degrees in these fields.

[20]These include biological, adopted, step, or foster children as well as other children respondents considered to be part of their family.

analogies, anagrams, proverb interpretation, and arithmetic problems. Spatial, as well as verbal and numerical materials, are employed. The different types of items are not segregated, but are arranged in a scrambled sequence." The average score for the respondents is 102 with a standard deviation of 14.6 points. Scores range from 61 to 145. The 2004 (similarities) cognition measure is constructed from answers to nine of fourteen items from the Weschler Adult Intelligence Scale (WAIS). The WLS eliminates the five simplest items from the WAIS because "the general ability of the sample is high enough to cause little variation in response to simple items." For those not familiar with WAIS, typical questions would range from "in what way are an orange and a banana alike? to "in what way are praise and punishment alike?" The average for this cognition measure is 6.6 in our sample, with a standard deviation of 2.4 points, scores ranging from 0 to 12.

In our regressions, we control for socioeconomic background with family fixed effects (when including the sample of siblings) or by including the following variables (for the sample of main respondents): father's occupation (white collar or not), parental income in 1957, father's education, and a dummy variable that identifies respondents as being well-off when first interviewed in 1957.[21] 33 percent of respondents identify their parents as white-collar workers, 21 percent consider themselves as being well-off when interviewed in 1957, fathers have on average 10.4 years of education, and parental income is $43,708, on average, in 1957 (2004 dollars).

Table 7 presents summary statistics for the relevant variables for the sample of siblings. Siblings' characteristics are quite similar, except they, by construction of the sample, have more variation in age (average age is 64 with a standard deviation of 7 years).

In several of our regression specifications, we use the logarithm of the variables described (income, wealth, number of children, intelligence measures, etc.) To make sure that we include observations where the variable in question takes the value of 0, we rather take the logarithm of the original variable plus 1. In the case of net worth, we use the transformation log(1+abs(wealth))× sign(wealth), to include respondents reporting

[21]The exact wording of the question is "How does your family income or wealth compare with families in your community?" Our dummy takes the value 1 if the respondent says "somewhat above average" or "considerably above average" and 0 otherwise (considerably below average, somewhat below average or average).

negative wealth as well.[22]

2.2 Regression Results

We first show the results of probit regressions. In the first column of Table 8, we find a coefficient of 0.149 to college education (i.e., a college education increases the probability of direct stockholding by 15 percent). The coefficient to college is smaller than the coefficient to college found for the PSID but this is likely due to the WLS sample being on average better educated. Female and age are negatively significant and high-school IQ is strongly positively significant.[23] We then add further controls with the aim of pinning down family influence (conditions in the family when the respondent was in high school) and covariates such as wealth and occupation which are functions of education and cognitive skills which we also consider a function of education in a setting where IQ at high school age is controlled for. Column (2) includes childhood controls: whether father was a white-collar worker, whether the family was well-off, family income in 1957, and father's education. These variables are all significant with expected signs and because family background is correlated with the decision of going to college the estimated impact of college declines to 0.118. We next consider cognition in column (3): cognitive ability is a significant predictor of stock holding and including cognitive ability lowers the coefficient to college slightly to 0.116 indicating that cognitive ability is a partial channel from college to stockholding. Next, we include occupation. The results in column (4) reveal that white-collar workers are significantly more likely to hold stock and, from column (5), this finding is robust to inclusion of a white-collar job aspirations of the respondent when in high school. After controlling for occupation the effect of college goes down to 0.096. Further schooling allows access to jobs that provide retirement accounts such as 401(k) plans and reduces the cost of indirect participation in the stock market, and may lower the cost of direct participation. Our finding is consistent with Hong, Kubik, and Stein (2004) who stress

[22]We also apply this transformation when using PSID data.

[23]Leaving out IQ does not increase the estimated effect of college by much, so this variable does not explain the difference to the PSID results. We do not include age squared in the regressions reported in Tables 8 and 9 because there is little age variation amongst the WLS main respondents.

the importance of social interaction as individuals may also learn from or mimic work peers.

Wealth is a highly significant determinant of stockholding and its inclusion lowers the impact of college to 0.092. Married respondents are more likely to hold stock and the impact of college drops to 0.077 when marriage is included. The propensity to marry and stay married is likely higher for college educated individuals, although both could be caused by unobserved personality traits. The impact of children is estimated to be significantly negative which could be due to potential parental financial obligations making more liquid savings optimal. In the final column, we examine if the type of major matters, specifically if an economics degree predicts higher stock ownership as found by Christiansen, Joensen, and Rangvid (2007). We find a positive effect but it is not significant at reasonable levels.

Table 9 reports the same regressions using a linear regression framework. The linear regression results are very similar to the probit results but we display them because we want to include family fixed effects which is more naturally done in the linear regression framework. The results with family fixed effects are summarized in Table 10. Many parental influences may be unmeasurable or badly approximated by the controls included in the previous table. The WLS also interviews siblings of the main respondents (although not for all covariates) and we can therefore control for family influence by including family fixed effects. We find that college is strongly significant still but the coefficient is smaller at 0.077. The sex of respondent becomes insignificant while the effect of IQ is smaller and less significant compared to the previous table. Including cognitive ability lowers the effect of college to 0.072 consistent with cognitive ability being a channel for college education. Having a white-collar job is highly significant and lowers the coefficient to college to only 0.042 and removes statistical significance. This implies that occupation or, at the least, skills that are highly correlated with occupational choice, is an important channel for the effect of education on stock holdings. Wealth is still highly significant and lowers the coefficient to college from 0.042 to 0.037 while the inclusion of marriage drives the education coefficient very close to 0. Finally, economics/business major is significant

at the 10 percent level, but as before the impact of the choice of major seems quite independent of the impact of other variables.

Overall, the coefficient to education is to a large extent explained by unobserved family background characteristics. Controlling for this, the results are consistent with college having its main effect through occupational choice, with smaller effects from wealth accumulation and cognitive ability. Married individuals are significantly more likely to hold equity and inclusion of the marriage dummy lowers the impact of education implying that marriage may be yet another channel trough which college affects asset holdings but we hesitate to stress this as a channel as it may be more likely to capture unobserved character traits that correlate with college.

3 Conclusion

More educated individuals are more likely to hold stocks. This is partly due to the more educated having higher wealth but the effect of schooling goes beyond wealth. The level of schooling is partly a function of unobserved ability, attitudes, and taste variables but we isolate the causal effect of college-level schooling by instrumenting it with the number of colleges in the county where and when the household head grew up. We find, using the PSID, a strong positive effect of college education on the propensity to own equity for households who report growing up in families with low or average wealth. Using the WLS and non-instrumented regressions, we find that obtaining a college degree clearly affects the probability to own equity and/or bonds and the main channels through which this works are occupational choice with wealth accumulation and cognitive ability being less important.

References

Alan, Sule, 2006, Entry Costs and Stock Market Participation Over the Life Cycle, *Review of Economic Dynamics* 9, 588–611.

Ameriks, John, Andrew Caplin, and John Leahy, 2003, Wealth Accumulation and the Propensity to Plan, *Quarterly Journal of Economics* 118, 1007–1047.

Attanasio, Orazio P., James Banks, and Sarah Tanner, 2002, Asset Holding and Consumption Volatility, *Journal of Political Economy* 110, 771–792.

Cameron, Stephen V., and Christopher Taber, 2004, Estimation of Educational Borrowing Constraints Using Returns to Schooling, *Journal of Political Economy* 112, 132–182.

Campbell, John, 2006, Household Finance, *Journal of Finance* 61, 1553–1604.

Card, David, 2001, Estimating the Return to Schooling: Progress on Some Persistent Econometric Problems, *Econometrica* 69, 1127–1160.

Christelis, Dimitris, Tullio Jappelli, and Mario Padula, 2006, Cognitive Abilities and Portfolio Choice, Discussion Paper, 5735 C.E.P.R. Discussion Papers.

Christiansen, Charlotte, Juanna Schroter Joensen, and Jesper Rangvid, 2007, Are Economists More Likely to Hold Stocks?, *Review of Finance* 12, 465–496.

Cole, Shawn, and Gauri Kartini Shastry, 2009, Smart Money: The Effect of Education, Cognitive Ability, and Financial Literacy on Financial Market Participation, *Working Paper, Harvard Business School* 09-071.

Currie, Janet, and Enrico Moretti, 2003, Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings, *Quarterly Journal of Economics* 118, 1553–1604.

Haines, Michael R., 2004, Historical, Demographic, Economic, and Social Data: The United States, 1790-2000 [computer file], Inter-university Consortium for Political and Social Research, ICPSR02896-v2.

Haliassos, Michael, and Carol C. Bertaut, 1995, Why Do So Few Hold Stocks?, *Economic Journal* 105, 1110–1129.

Hansen, Karsten T., James J. Heckman, and Kathleen J. Mullen, 2006, The Effect of Schooling and Ability on Achievement Test Scores, *Journal of Econometrics* 121, 39–98.

Hong, Harrison, Jeffrey D. Kubik, and Jeremy C. Stein, 2004, Social Interaction and Stock-Market Participation, *Journal of Finance* 59, 137–163.

Perraudin, William R.M., and Bent E. Sørensen, 2000, The Demand for Risky Assets: Sample Selection and Household Portfolios, *Journal of Econometrics* 97, 117–144.

van Rooij, Maarten, Annamaria Lusardi, and Rob Alessie, 2007, Financial Literacy and Stock Market Participation, NBER Working Paper # 13565.

Vissing-Jørgensen, Annette, 2002a, Limited Asset Market Participation and the Elasticity of Intertemporal Substitution, *Journal of Political Economy* 110, 825–853.

——— , 2002b, Towards an Explanation of Household Portfolio Choice Heterogeneity: Nonfinancial Income and Participation Cost Structures, NBER Working Paper # 8884.

Table 1: Summary statistics. PSID Data

Variable	Mean	Std. Dev.	Min.	Max.	N
Direct or indirect stockholding					
Ever owned stock	0.414	0.493	0	1	4840
Owned stock 1984	0.172	0.378	0	1	2602
Owned stock 1989	0.211	0.408	0	1	3096
Owned stock 1994	0.268	0.443	0	1	3634
Owned stock 1999	0.396	0.489	0	1	2339
Owned stock 2001	0.428	0.495	0	1	2297
Owned stock 2003	0.420	0.494	0	1	2225
Owned stock 2005	0.420	0.494	0	1	2110
Owned stock 2007	0.429	0.495	0	1	2030
Direct stockholding only					
Owned stock 1994	0.268	0.443	0	1	3634
Owned stock 1999	0.247	0.431	0	1	2339
Owned stock 2001	0.271	0.445	0	1	2297
Owned stock 2003	0.246	0.431	0	1	2225
Owned stock 2005	0.245	0.43	0	1	2110
Owned stock 2007	0.245	0.43	0	1	2030
Education (yrs.)	12.814	2.054	4	16	4840
Parents' edu: dum	0.699	0.459	0	1	4740
Lived with both parents	0.73	0.444	0	1	4615
Rich Parents	0.283	0.451	0	1	4489
Year of birth	1957.01	7.378	1943	1976	4840
County Median Inc./10000	2.506	0.681	0.473	5.336	4791
Log avg. income	9.878	0.854	5.006	12.888	4834
Log avg. net worth	7.793	5.65	–12.989	16.046	4840
Fam. size	2.891	1.288	1	10.333	4840
No. kids	1.036	1.058	0	7.333	3813
College 4/1000	0.088	0.155	0	4.747	4806
College 2/1000	0.055	0.101	0	2.849	4806
Grew up on a farm	0.155	0.362	0	1	4746
White	0.596	0.491	0	1	4840
Married	0.639	0.48	0	1	4839
Male	0.743	0.437	0	1	4840
<=12 yrs. of schooling	0.617	0.486	0	1	4840
13–15 yrs. of schooling	0.188	0.39	0	1	4840
College grad.	0.195	0.396	0	1	4840

Notes: "Direct stockholding" refers to ownership of stocks and mutual funds outside of retirement accounts while "Direct and indirect stockholding" adds stocks and mutual funds in retirement accounts.

Table 2: Regressions of Own Education on College Availability

	All		Poor or Avg. (All Races)		Poor or Avg. White	Poor or Avg. Non White
	(1)	(2)	(3)	(4)	(5)	(6)
College 4/1000	0.498**	0.505**	0.657***	0.665***	0.683**	0.744**
	(2.48)	(2.51)	(2.71)	(2.73)	(2.19)	(1.99)
College 2/1000		0.305		0.436	0.177	1.854**
		(0.95)		(1.02)	(0.36)	(2.06)
White	0.442***	0.438***	0.305***	0.297***		
	(5.43)	(5.36)	(3.21)	(3.12)		
Male	–0.027	–0.028	0.005	0.003	–0.060	0.042
	(–0.37)	(–0.38)	(0.05)	(0.04)	(–0.48)	(0.34)
Parents' edu: dum	1.429***	1.430***	1.429***	1.429***	1.720***	0.999***
	(17.47)	(17.46)	(16.62)	(16.60)	(13.22)	(9.01)
Lived with both parents	0.283***	0.284***	0.254***	0.257***	0.425***	0.079
	(3.91)	(3.93)	(3.00)	(3.05)	(3.41)	(0.67)
Rich Parents	0.194***	0.194***				
	(3.12)	(3.12)				
Grew up on a farm	–0.325***	–0.327***	–0.339***	–0.338***	–0.345**	–0.280
	(–3.24)	(–3.25)	(–2.85)	(–2.86)	(–2.52)	(–1.26)
County Median Inc.	0.293***	0.298***	0.325***	0.333***	0.447***	0.104
	(3.70)	(3.74)	(3.66)	(3.71)	(4.22)	(0.63)
Constant	9.579***	9.542***	12.439***	12.444***	9.061***	10.178***
	(22.27)	(21.97)	(16.20)	(15.90)	(15.74)	(13.91)
Year of birth dummies	Y	Y	Y	Y	Y	Y
State grew up dummies	Y	Y	Y	Y	Y	Y
Adj. R sq.	0.220	0.220	0.198	0.198	0.213	0.110
N	4094	4094	2933	2933	1776	1157

Notes: The left-hand side variable is the respondent's reported years of completed schooling."College 2/1000" is the number of 2-year colleges per 1,000 persons aged 18–22 in the county where the respondent grew up when s/he was 17. "College 4/1000" is defined analogously for the number of 4-year colleges. Standard errors in parentheses clustered by the county where the respondent grew up. "Parents' edu: dum" is a dummy variable equal to 0 if parents did not finish high school (HS); 1—if one of them finished HS or more.*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 3: Regressions of Education Dummies on College Availability

	All			Poor or Average		
	$\leq$ 12 yrs	13–15 yrs.	$\geq$ 16 yrs	$\leq$ 12 yrs	13–15 yrs.	$\geq$ 16 yrs
	(1)	(2)	(3)	(4)	(5)	(6)
College 4/1000	–0.111***	–0.012	0.123***	–0.155***	0.001	0.154***
	(–2.65)	(–0.39)	(2.68)	(–2.82)	(0.02)	(3.23)
White	–0.089***	–0.026*	0.116***	–0.049**	–0.041**	0.090***
	(–4.55)	(–1.65)	(7.41)	(–2.15)	(–2.28)	(4.88)
Male	0.003	0.003	–0.006	–0.001	–0.010	0.011
	(0.17)	(0.21)	(–0.45)	(–0.07)	(–0.55)	(0.75)
Parents' edu: dum	–0.279***	0.110***	0.170***	–0.281***	0.109***	0.172***
	(–16.60)	(8.89)	(11.85)	(–14.72)	(7.00)	(11.40)
Lived with both parents	–0.056***	0.005	0.051***	–0.038*	–0.009	0.047***
	(–2.97)	(0.35)	(3.74)	(–1.80)	(–0.54)	(2.93)
Rich Parents	–0.035**	–0.003	0.039***			
	(–2.13)	(–0.24)	(2.81)			
Grew up on a farm	0.070***	–0.038**	–0.032*	0.060**	–0.041**	–0.020
	(3.16)	(–2.22)	(–1.85)	(2.37)	(–2.10)	(–1.01)
County Median Inc.	–0.066***	–0.006	0.071***	–0.071***	–0.003	0.075***
	(–3.66)	(–0.40)	(4.42)	(–3.32)	(–0.19)	(4.25)
Year of birth dummies	Y	Y	Y	Y	Y	Y
State grew up dummies	Y	Y	Y	Y	Y	Y
Adj. R sq.	0.165	0.036	0.150	0.148	0.045	0.132
N	4094	4094	4094	2933	2933	2933

Notes: The left-hand side variable is a dummy equal to one if the respondent's years of schooling fall into any of the indicated categories. "College 4/1000" is the number of 4-year colleges per 1,000 persons aged 18–22 in the county where the respondent grew up when s/he was 17. Standard errors in parentheses clustered by the county where the respondent grew up. "Parents' edu: dum" is a dummy variable equal to 0 if parents did not finish high school; 1—if one of them finished HS or more.*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 4: Panel Probit Regressions of Risky Assets' Ownership on Education: Direct Stockholding Only

	Probit	IV-Probit	IV-Probit	IV-Probit		IV-Probit	
	Total sample	Total sample	Total sample	Poor or Avg.		Poor or Avg.	White
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Education (yrs.)	0.063***	0.164*	0.149	0.178**	0.179	0.187***	0.188***
	(17.48)	(1.84)	(0.98)	(2.25)	(1.57)	(3.52)	(2.68)
Age	0.024***	0.013	0.011	0.007	0.004	0.017	0.017**
	(5.28)	(0.84)	(1.05)	(0.40)	(0.39)	(1.30)	(1.97)
Age sq./100	–0.019***	–0.013	–0.013**	–0.008	–0.008	–0.020**	–0.023**
	(–3.46)	(–1.33)	(–2.17)	(–0.72)	(–1.08)	(–2.14)	(–2.38)
White	0.170***	0.088	0.087	0.067	0.062		
	(11.29)	(0.81)	(0.78)	(0.67)	(0.65)		
Male	0.110***	0.096***	0.068	0.100**	0.062	0.086*	0.031
	(7.47)	(3.07)	(1.13)	(2.36)	(1.34)	(1.65)	(0.63)
Parents' edu: dum	0.032*	–0.120	–0.109	–0.130	–0.136	–0.162	–0.169
	(1.84)	(–0.76)	(–0.51)	(–0.86)	(–0.79)	(–1.10)	(–1.08)
Lived with both parents	0.005	–0.024	–0.029	–0.024	–0.030	–0.015	–0.024
	(0.31)	(–0.72)	(–0.92)	(–0.73)	(–1.11)	(–0.39)	(–0.67)
Rich parents	0.033**	0.009	0.005				
	(2.29)	(0.28)	(0.14)				
Grew up on a farm	0.003	0.044	0.031	0.035	0.030	0.050	0.042
	(0.18)	(0.99)	(0.47)	(0.74)	(0.48)	(1.15)	(0.82)
County median inc.	0.038***	–0.012	–0.009	–0.051	–0.050	–0.040	–0.043
	(2.67)	(–0.21)	(–0.13)	(–0.95)	(–0.77)	(–0.72)	(–0.71)
If married			0.015		0.055**		0.060**
			(0.82)		(2.28)		(2.04)
No. kids			–0.011*		–0.011		–0.028***
			(–1.81)		(–1.62)		(–2.95)
Log inc.			–0.001		–0.030		–0.023
			(–0.01)		(–0.31)		(–0.36)
Log wealth			0.015		0.011		0.015
			(1.47)		(0.95)		(1.53)
Year dummies	Y	Y	Y	Y	Y	Y	Y
State grew up dummies	Y	Y	Y	Y	Y	Y	Y
N	12887	12843	12736	8895	8826	5833	5809

Notes: The left-hand side variable is a dummy equal to 1 if a household owns stock, 0—otherwise. Marginal effects are reported. The instrument for years of schooling is "College 4/1000." Standard errors in parentheses clustered by the individual. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 5: Panel Probit Regressions of Risky Assets' Ownership on College Dummy: Direct Stockholding Only

	Probit	IV-Probit	IV-Probit	IV-Probit		IV-Probit	
	Total sample	Total sample	Total sample	Poor or Avg.		Poor or Avg.	White
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
College dummy	0.259***	0.560**	0.416	0.694***	0.648**	0.621***	0.593***
	(14.43)	(2.03)	(1.00)	(3.57)	(2.06)	(4.22)	(2.96)
Age	0.026***	0.023***	0.016***	0.018**	0.012**	0.022**	0.018**
	(5.60)	(3.66)	(3.46)	(2.15)	(2.28)	(2.18)	(2.28)
Age sq./100	–0.020***	–0.022***	–0.016*	–0.020***	–0.015*	–0.021**	–0.020**
	(–3.71)	(–3.93)	(–1.96)	(–2.99)	(–1.85)	(–2.38)	(–2.01)
White	0.171***	0.125**	0.112**	0.082	0.069		
	(11.00)	(2.04)	(2.22)	(1.15)	(1.11)		
Male	0.113***	0.108***	0.034	0.106***	0.027	0.100**	0.010
	(7.45)	(6.18)	(1.19)	(3.85)	(0.89)	(2.52)	(0.22)
Parents' edu: dum	0.068***	0.020	0.019	–0.002	–0.008	0.008	–0.000
	(4.17)	(0.32)	(0.33)	(–0.03)	(–0.10)	(0.09)	(–0.00)
Lived with both parents	0.010	–0.002	–0.013	–0.005	–0.017	0.011	–0.004
	(0.59)	(–0.11)	(–0.71)	(–0.20)	(–0.76)	(0.32)	(–0.13)
Rich parents	0.036**	0.023	0.018				
	(2.47)	(1.01)	(0.89)				
Grew up on a farm	–0.009	0.005	–0.009	–0.014	–0.021	0.004	–0.008
	(–0.50)	(0.19)	(–0.36)	(–0.52)	(–0.85)	(0.12)	(–0.22)
County median inc.	0.042***	0.016	0.020	–0.029	–0.024	–0.008	–0.008
	(2.88)	(0.46)	(0.58)	(–0.70)	(–0.51)	(–0.17)	(–0.17)
Married			0.015		0.041*		0.052*
			(0.80)		(1.85)		(1.72)
No. kids			–0.015**		–0.016**		–0.029***
			(–2.17)		(–2.25)		(–2.99)
Log inc.			0.048		0.025		0.020
			(1.32)		(0.51)		(0.48)
Log wealth			0.019***		0.016***		0.020***
			(6.37)		(3.56)		(3.32)
Year dummies	Y	Y	Y	Y	Y	Y	Y
State grew up dummies	Y	Y	Y	Y	Y	Y	Y
N	12887	12843	12736	8895	8826	5833	5809

Notes: The left-hand side variable is a dummy equal to 1 if a household owns stock, 0—otherwise. Marginal effects are reported. The instrument for years of schooling is "College 4/1000." Standard errors in parentheses clustered by the individual. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 6: Summary statistics. Main Respondents. WLS Data

Variable	Mean	Std. Dev.	Min.	Max.	N
Direct Stockholding	0.55	0.5	0	1	6887
College dummy	0.26	0.44	0	1	6887
Education (years)	13.53	2.22	12	20	6886
Econ/Business Major	0.06	0.24	0	1	6887
Age	64.34	0.70	63	67	6887
Sex	0.47	0.5	0	1	6887
Married in 2004	0.78	0.42	0	1	6123
Number of kids	3.05	1.72	0	10	6887
Log of number of kids	1.29	0.49	0	2.4	6887
Wealth in 2004	667,026	1,254,039	–15,000	12,000,000	6885
Log of wealth in 2004	12.06	3.38	–9.62	16.3	6885
White collar in 1992	0.46	0.5	0	1	6887
Wanted to be white collar	0.42	0.49	0	1	6887
IQ measure	102.01	14.64	61	145	6887
Log of IQ	4.61	0.15	4.11	4.98	6887
Cognition-Similarities 2004	6.61	2.37	0	12	6510
Log of cognition in 2004	1.97	0.37	0	2.56	6510
Father white collar	0.33	0.47	0	1	6887
Family well-off	0.21	0.41	0	1	6700
Parental income in 1957 (2004 dollars)	43,708	43,037	0	670,897	6540
Log parental inc. 1957	10.45	0.69	0	13.42	6540
Father Education (years)	10.41	3.19	7	18	6393

Notes: "Direct stockholding" refers to ownership of stocks, bonds or mutual funds outside retirement accounts.

Table 7: Summary statistics. Siblings. WLS Data

Variable	Mean	Std. Dev.	Min.	Max.	N
Direct Stockholding	0.53	0.5	0	1	3972
College dummy	0.35	0.48	0	1	3972
Education (years)	13.95	2.59	0	21	3845
Econ/Business Major	0.04	0.19	0	1	3972
Age	63.8	7.13	34	87	3972
Sex	0.48	0.5	0	1	3972
Married in 2004	0.76	0.43	0	1	3374
Number of kids	2.9	1.81	0	10	3972
Log of number of kids	1.24	0.52	0	2.4	3972
Wealth in 2004	582,065	922,562	–14,508	6,789,954	3968
Log of wealth in 2004	11.73	3.86	–9.58	15.73	3968
White collar in 1992	0.45	0.5	0	1	3972
Wanted to be white collar	0.63	0.48	0	1	905
IQ measure	104.64	15.48	61	145	3292
Log of IQ	4.64	0.15	4.11	4.98	3292
Cognition-Similarities 2004	6.69	2.39	0	12	3693
Log of cognition in 2004	1.98	0.38	0	2.56	3693

Notes: "Direct stockholding" refers to ownership of stocks, bonds or mutual funds outside retirement accounts.

Table 8: Channels of the Impact of College on Direct Stockholding. Main Respondents. Probits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
College dummy	0.149***	0.118***	0.116***	0.099***	0.096***	0.092***	0.077***	0.071***
	(10.19)	(7.22)	(6.82)	(5.54)	(5.05)	(4.80)	(3.81)	(3.41)
Sex	–0.022*	–0.026**	–0.025*	–0.031**	–0.030**	–0.050***	–0.068***	–0.071***
	(–1.81)	(–1.97)	(–1.80)	(–2.20)	(–2.18)	(–3.58)	(–4.54)	(–4.69)
Age	–0.036***	–0.039***	–0.041***	–0.040***	–0.040***	–0.036***	–0.033***	–0.032***
	(–4.03)	(–3.97)	(–4.04)	(–3.95)	(–3.95)	(–3.59)	(–3.01)	(–3.00)
Log of IQ	0.251***	0.188***	0.157***	0.137**	0.133**	0.091	0.056	0.053
	(5.51)	(3.74)	(2.84)	(2.46)	(2.36)	(1.60)	(0.92)	(0.87)
Father white collar		0.041**	0.041**	0.039**	0.038**	0.034**	0.038**	0.038**
		(2.55)	(2.49)	(2.39)	(2.33)	(2.04)	(2.18)	(2.16)
Family well-off		0.037**	0.041**	0.040**	0.040**	0.037**	0.043**	0.042**
		(2.20)	(2.37)	(2.30)	(2.29)	(2.13)	(2.31)	(2.29)
Log parental inc. 1957		0.035***	0.036***	0.034***	0.034***	0.031***	0.035***	0.035***
		(3.33)	(3.28)	(3.16)	(3.12)	(2.80)	(2.94)	(2.92)
Father Education (years)		0.005**	0.005**	0.005**	0.005*	0.005**	0.005*	0.005*
		(2.21)	(2.06)	(1.98)	(1.94)	(2.08)	(1.91)	(1.93)
Log of cognition in 2004			0.045**	0.040*	0.039*	0.032	0.027	0.027
			(2.13)	(1.87)	(1.82)	(1.51)	(1.17)	(1.18)
White collar in 1992				0.047***	0.045***	0.029*	0.029*	0.028*
				(3.04)	(2.93)	(1.88)	(1.77)	(1.70)
Wanted to be white collar					0.009	0.008	0.013	0.014
					(0.53)	(0.50)	(0.73)	(0.77)
Log of wealth in 2004						0.030***	0.030***	0.030***
						(9.84)	(8.77)	(8.76)
Married in 2004							0.130***	0.130***
							(7.04)	(7.03)
Log of number of kids							–0.029*	–0.029*
							(–1.88)	(–1.91)
Econ/Business Major								0.040
								(1.27)
N	6887	5951	5632	5632	5632	5630	5056	5056

Notes: The left-hand side variable is a dummy equal to 1 if a household owns stock/bonds/mutual funds outside retirement accounts, 0—otherwise. Marginal effects are reported. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Robust standard errors.

Table 9: Channels of the Impact of College on Direct Stockholding. Main Respondents. Linear Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
College dummy	0.146***	0.114***	0.111***	0.094***	0.091***	0.085***	0.071***	0.065***
	(10.08)	(7.11)	(6.70)	(5.41)	(4.90)	(4.69)	(3.71)	(3.32)
Sex	–0.022*	–0.025*	–0.023*	–0.030**	–0.029**	–0.046***	–0.062***	–0.065***
	(–1.82)	(–1.94)	(–1.77)	(–2.21)	(–2.18)	(–3.45)	(–4.42)	(–4.57)
Age	–0.035***	–0.037***	–0.039***	–0.038***	–0.038***	–0.035***	–0.031***	–0.030***
	(–4.05)	(–3.99)	(–4.06)	(–3.95)	(–3.96)	(–3.62)	(–3.03)	(–3.01)
Log of IQ	0.247***	0.185***	0.154***	0.134**	0.130**	0.089	0.054	0.051
	(5.55)	(3.78)	(2.87)	(2.48)	(2.38)	(1.64)	(0.95)	(0.89)
Father white collar		0.040**	0.040**	0.038**	0.037**	0.032**	0.036**	0.036**
		(2.53)	(2.47)	(2.37)	(2.30)	(2.02)	(2.18)	(2.16)
Family well-off		0.034**	0.038**	0.037**	0.037**	0.034**	0.039**	0.038**
		(2.11)	(2.29)	(2.22)	(2.21)	(2.13)	(2.27)	(2.25)
Log parental inc. 1957		0.033***	0.033***	0.032***	0.031***	0.028***	0.032***	0.032***
		(3.33)	(3.27)	(3.15)	(3.11)	(2.80)	(2.93)	(2.91)
Father Education (years)		0.005**	0.005**	0.005**	0.005*	0.005**	0.005*	0.005*
		(2.18)	(2.04)	(1.96)	(1.92)	(2.03)	(1.82)	(1.83)
Log of cognition in 2004			0.045**	0.040*	0.039*	0.029	0.026	0.026
			(2.18)	(1.91)	(1.86)	(1.45)	(1.20)	(1.21)
White collar in 1992				0.047***	0.046***	0.030**	0.030*	0.029*
				(3.12)	(3.00)	(2.03)	(1.94)	(1.87)
Wanted to be white collar					0.009	0.009	0.014	0.015
					(0.58)	(0.57)	(0.82)	(0.86)
Log of wealth in 2004						0.026***	0.026***	0.026***
						(12.81)	(11.50)	(11.48)
Married in 2004							0.121***	0.121***
							(7.02)	(7.01)
Log of number of kids							–0.027*	–0.028*
							(–1.91)	(–1.94)
Econ/Business Major								0.037
								(1.37)
Constant	1.643***	1.681**	1.850***	1.889***	1.915***	1.624**	1.449**	1.455**
	(2.61)	(2.45)	(2.62)	(2.68)	(2.71)	(2.32)	(1.97)	(1.97)
Adj. R sq.	0.033	0.042	0.045	0.047	0.047	0.074	0.079	0.079
F	66.1	37.8	34.7	32.2	29.3	44.3	38.9	36.5
N	6887	5951	5632	5632	5632	5630	5056	5056

Notes: The left-hand side variable is a dummy equal to 1 if a household owns stock/bonds/mutual funds outside retirement accounts, 0—otherwise. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Robust standard errors.

Table 10: Channels of the Impact of College on Direct Stockholding with Family Fixed Effects. Linear Regressions.

	(1)	(2)	(3)	(4)	(5)	(6)
College dummy	0.077**	0.072*	0.042	0.037	0.013	0.001
	(2.30)	(1.94)	(1.08)	(0.96)	(0.29)	(0.03)
Sex	–0.016	–0.002	–0.014	–0.024	–0.023	–0.032
	(–0.61)	(–0.08)	(–0.47)	(–0.84)	(–0.67)	(–0.91)
Age	0.070**	0.061*	0.056	0.045	0.029	0.022
	(2.15)	(1.70)	(1.58)	(1.27)	(0.70)	(0.53)
Age sq./100	–0.053**	–0.045	–0.042	–0.032	–0.020	–0.015
	(–2.10)	(–1.64)	(–1.52)	(–1.20)	(–0.63)	(–0.46)
Log of IQ	0.245**	0.178	0.139	0.100	0.045	0.034
	(2.16)	(1.38)	(1.08)	(0.78)	(0.30)	(0.23)
Log of cognition in 2004		0.087**	0.077*	0.077*	0.103*	0.103*
		(1.96)	(1.73)	(1.76)	(1.95)	(1.95)
White collar in 1992			0.093***	0.084**	0.077**	0.076**
			(2.79)	(2.53)	(2.00)	(1.98)
Log of wealth in 2004				0.018***	0.016***	0.016***
				(4.23)	(3.28)	(3.28)
Married in 2004					0.109***	0.108***
					(2.60)	(2.59)
Log of number of kids					–0.047	–0.046
					(–1.32)	(–1.29)
Econ/Business Major						0.138*
						(1.90)
Constant	–2.930**	–2.513*	–2.170	–1.844	–1.147	–0.877
	(–2.36)	(–1.84)	(–1.60)	(–1.37)	(–0.71)	(–0.54)
Adj. R sq.	0.099	0.104	0.110	0.122	0.119	0.121
F	3.3	3.0	3.7	5.8	3.9	3.8
N	10179	9575	9575	9572	8474	8474

Notes: The left-hand side variable is a dummy equal to 1 if a household owns stock/bonds/mutual funds outside retirement accounts, 0—otherwise. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Robust standard errors.