Smart Money: The Effect of Education on Financial Behavior

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Abstract

Household financial decisions are important for both households and the greater economy. Yet, our understanding of the process of financial decision-making is limited. Applying standard and two-sample instrumental variables strategies to census and credit bureau data, we provide the first precise, causal estimates of the effects of education on financial behavior. Education has large effects on financial market participation and smaller, but statistically and economically significant effects on financial management. We find that education improves credit scores, and dramatically reduces the probability of declaring bankruptcy or suffering foreclosure during the financial crisis. Examining mechanisms, we show that cognitive ability increases financial participation, and discuss how education may affect decision-making through: attitudes, borrowing behavior, discount rates, risk-aversion, and the influence of coworkers and neighbors.

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1 Introduction

Individuals face an increasingly complex menu of financial products. On the asset side of the balance sheet, the shift from defined benefit to defined contribution pension plans, and the growing importance of private retirement accounts, require individuals to choose the amount they save, as well as the mix of assets in which they invest. Yet, participation in financial markets is far from universal in the United States, and we have only a limited understanding of what factors influence participation. On the liabilities side, a dramatic increase in the range and complexity of credit products to households has been accompanied by increased default, bankruptcy, and foreclosures. These trends have sparked a vigorous debate about whether individuals are well-equipped to make informed financial decisions.

Using data and estimation techniques new to the literature, this paper provides precise, causal estimates of the effect of education on financial market participation and financial management. We exploit exogenous variation in education caused by changes in compulsory schooling laws. We find an additional year of education increases the probability of financial market participation by 7-8 percentage points, holding constant other factors, including income. The size of this effect is economically important both on its own and in the context of previously identified correlates of financial participation, such as trust (Guiso, Sapienza, and Zingales, 2008), peer effects (Hong, Kubik, and Stein, 2004), prior life experience with the stock market (Malmendier and Nagel, 2011), or institutional quality (Osili and Paulson, 2008).

To study the effect of education on other aspects of financial behavior, we employ a two-sample instrumental variables strategy using the same compulsory schooling laws, together with a new data set on consumer credit behavior. We find that exogenous increases in education lead to higher credit scores, more responsible financial behavior (fewer delinquent credit-card payments), and importantly, substantial reductions in the probability of bankruptcy and foreclosure. This effect is particularly pronounced during the recent financial crisis.

We then explore why education influences financial behavior. Agarwal et al (2007) and
Agarwal and Mazumder (2010) demonstrate the importance of cognitive ability for sound financial decision making. By exploiting within-sibling group variation in cognitive ability, we show that indeed higher levels of cognitive ability lead to greater financial market participation. Importantly, these estimates are not confounded by unobserved background and family characteristics.

Finally, we describe ways in which education might affect financial behavior. We find education effects a measure of confidence; borrowing decisions (such as whether to take a second mortgage); the probability one has a pension, through occupational choices; and the type of peers one has, through residential choices. Education does not affect the probability of moving to another city, which may correlate with willingness to take risks.

Financial management is important for many reasons. For the household, it facilitates asset accumulation and consumption smoothing, with potentially significant effects on welfare. For the financial system as a whole, the depth and breadth of financial market participation are important determinants of the equity premium, the volatility of markets, and household expenditure (Mankiw and Zeldes, 1991; Heaton and Lucas, 1999; Vissing-Jorgensen, 2002; and Brav, Constantinides, and Gezcy, 2002). Financial behavior may also affect the political economy of financial regulation, as those holding financial assets may have different attitudes towards corporate and investment income tax policy, as well as different attitudes towards risk-sharing and redistribution.

Several aspects of financial behavior, such as limited equity market participation, low savings rates, and, more recently, a high incidence of bankruptcy and foreclosures have drawn attention from economists as potentially inconsistent with standard models of optimizing behavior. While survey evidence has proven useful in demonstrating what factors are correlated with such behaviors\(^1\), there is much less understanding of what the causal drivers are. This paper contributes

\(^1\) Previous work has demonstrated that financial behavior is, not surprisingly, correlated with income, as well as education (Bertaut and Starr-McCluer, 2001, among others), measured financial literacy (Lusardi and Mitchell, 2007), social connections (Hong, Kubik, and Stein, 2004), trust (Guiso, Sapienza, and Zingales, 2008), experience with the stock market (Malmendier and Nagel, 2011), and cognitive ability (Grinblatt, Keloharju, and Linnainmaa, 2011).
to the literature by demonstrating an important causal determinant of financial behaviors that have been poorly understood.

In 2004 only 48.6% of households held stocks, either directly or indirectly (Bucks, Kennickell, and Moore, 2006). Some view this limited participation in the stock market as a puzzle: Haliassos and Bertaut (1995) consider and reject risk aversion, belief heterogeneity, and other potential explanations, instead favoring “departures from expected-utility maximization.” Our paper shows that low levels of education may help explain the limited participation puzzle.

At the lower end of the income spectrum, economists have focused on individuals’ low savings rates and propensity to declare bankruptcy, and take on mortgages they cannot repay. Gross and Souleles (2002) note individuals borrow from credit cards when holding large bank account balances. Stango and Zinman (2009) show households systematically underestimate the returns to saving. Lusardi et al. (2011) find that a quarter of Americans would be unable to come up with $2,000 if needed within 30 days. Our second set of results sheds light on this financial management puzzle. We show exogenous increases in education improve individuals’ credit scores. More educated individuals pay off a greater share of their outstanding credit balance, are less likely to be delinquent on their credit card bills, and are less likely to declare bankruptcy or experience a foreclosure.

2 Data

This paper uses three complementary data sets: the U.S. Census, the Federal Reserve Bank of New York Consumer Credit Panel/Equifax dataset, and the National Longitudinal Survey of Youth (NLSY). Summary statistics for the first two are presented in Table I, while summary statistics for the NLSY are presented in Online Appendix Table A1.

We use a 5 percent sample from the 1980, 1990, and 2000 Public Use Census Data, representing a random draw of the US population. The key advantage of this data set is its size: with

The 2010 census did not include a “long form,” and hence does not have information on investment income.
over 14 million observations, we can use non-parametric controls, obtain precise estimates, and most importantly, use an instrumental variable strategies that would simply not be possible in most other, smaller, data sets.

The Census does not collect any information on financial wealth, and is very rarely used to study financial decision-making (an exception is Carroll, Rhee and Rhee, 1999). However, it does collect detailed income data. Thus, the main measure of financial market participation we will use is “income from interest, dividends, net rental income, royalty income, or income from estates and trusts,” received during the previous year, which we term “investment income.” Households are instructed to “report even small amounts credited to an account.” (Ruggles et al., 2004). A second type of income we use is “retirement, survivor, or disability pensions,” received during the previous year, which we term “retirement income.” This is distinct from Social Security and Supplemental Security Income, both of which are reported on separate lines.³

A limitation of using the amount of investment income, rather than the amount invested, is that it is only partially informative about the amount and type of investments held by the respondent. This would make it difficult to use the data for structural estimates of investment levels (such as calibrating models of participation costs). In this work, however, we focus primarily on the decision to participate in financial markets, for which we define a dummy variable equal to one if the household reports any non-zero investment income. Approximately 22% of respondents do so, which is close to the 21.3% of families that report holding equity in the 2001 Survey of Consumer Finances (Bucks, Kennickell, and Moore, 2006), but lower than the 33% of households reporting any investment income in the 2001 SCF.

In online appendix tables A2 and A3, we compare our census data to data from the SCF. We find the census data yield very similar estimates of means, medians and percentiles for our measures of participation and investment and retirement income. Regressions of investment

³One may be concerned that small amounts of investment income simply represent interest from savings accounts. As a robustness check, we rerun our analysis considering only those who receive income greater than $500 (or, alternatively, $1000) in investment income as "participating." The results are very similar (available on request).
income and financial market participation on individual characteristics in the two data sets yield strikingly similar coefficients. Finally, we explore the relationship between reported investment income and more traditional measures of financial market participation. In particular, we find a large jump in the use of transactions accounts as individuals move from 0 to any positive amount of investment income. For example, 78.4% of households reporting no investment income possess a checking account, while 91.9% of those reporting income between 1 and 100 dollars have checking accounts. There is a similar, strongly positive and nearly linear relationship between reported investment income and participation in equity markets.

The FRBNY Consumer Credit Panel/Equifax dataset is a quarterly longitudinal panel of individual credit bureau data, similar to information that would be contained in an individual’s credit report. It is described in detail in Lee and van der Klaauw (2010). The panel begins in the first quarter of 1999 and continues to the third quarter of 2011. The primary sample is made up of a random 5% sample of all U.S. residents aged 18 and up who have a credit report. The sample selection procedures ensure that, in any given quarter, there is a nationally representative cross-section, conditional on having a credit report. We restrict attention to individuals who are aged 36 to 75 in the third quarter of 2000, matching our census sample. Ultimately, we have approximately 5 million observations per quarter.

We focus on five key outcome variables from this data set: credit score, the proportion of an individual’s credit card debt that is not delinquent, the proportion of quarters in which an individual has any delinquent credit card balance, a bankruptcy indicator, and a foreclosure indicator. The credit score, similar to a FICO score, predicts the likelihood of being 90 or more days delinquent over the next 24 months. Credit scores range from 280 to 850, and higher scores imply a lower probability of being seriously delinquent in the future. Both credit score and the proportion of an individual’s credit card debt that is not delinquent are averaged across all quarters. The bankruptcy and foreclosure variables indicate whether an individual has undergone bankruptcy or foreclosure at least once, respectively, between 1992 and 2011. These
indicators are able to track bankruptcies and foreclosures back through 1992 because credit bureaus maintain records on bankruptcy and foreclosure proceedings for 7 years.

The NLSY79 is a survey of 12,686 Americans aged 14 to 22 in 1979, with annual follow-ups until 1994, and biennial follow-ups afterwards. Each survey contained detailed questions on the individual’s savings decisions and accumulated assets. Importantly, in 1980 survey respondents completed the Armed Services Vocational Aptitude Battery (ASVAB), a set of 10 exams that measure cognitive ability and knowledge. More details on these data are included in an online appendix.

3 The Effect of Education on Financial Market Participation

3.1 Empirical Strategy

Results from previous literature and simple regressions on our data strongly suggest that households with higher levels of education are more likely to participate in financial markets. Campbell (2006), for example, notes that educated households in Sweden diversify their portfolios more efficiently. However, the simple relationship between financial decisions and education levels omits many other important factors, such as ability or family background, that are also likely to influence financial decisions. In this section we exploit an instrumental variables strategy developed by Acemoglu and Angrist (2000) - changes in state compulsory education laws - which provides exogenous variation in education. Revisions in state laws affected individuals’ education attainment, but are not correlated with individual ability, parental characteristics, or other potentially confounding factors.

In particular, we follow the strategy laid out by Lochner and Moretti (2004, hereafter LM), who use changes in schooling requirements between 1914 and 1978 to measure the effect of education on incarceration rates. The principal advantage of following LM closely is that they

4At the same time, Tortorice (2012) finds that education only slightly reduces the likelihood that individuals make expectational errors regarding macroeconomic variables, and that these errors affect buying attitudes and financial decisions.
have conducted a battery of specification checks, demonstrating the validity of using compulsory schooling laws as a natural experiment. For example, LM show that there is no clear trend in educational attainment in the years prior to changes in schooling laws and that compulsory schooling laws do not affect college attendance.

The structural equation of interest is the following,

\[ y_i = \alpha + \beta s_i + \gamma X_i + \varepsilon_i \]  

where \( s_i \) is years of education for individual \( i \), and \( X_i \) is a set of controls, including age, gender, race, state of birth, state of residence, census year, cohort of birth fixed effects and a cubic polynomial in earned income. Age effects are defined as dummies for each 3-year age group from 20 to 75, while year effects are dummies for each census year. Following LM, we exclude people born in Alaska and Hawaii but include those born in the District of Columbia; thus we have 49 state of birth dummies, but 51 state of residence dummies. Again following LM, we include state of birth dummies interacted with a dummy variable for cohorts born in the South who turn 14 in or after 1958 to allow for the impact of the Brown vs. Board of Education decision. Cohort of birth is defined as 10-year birth intervals. Standard errors are corrected for intracluster correlation within state of birth \times year of birth. The outcome variable is either an indicator for having any investment or retirement income or the actual level of investment or retirement income. When studying the amount of income, we drop observations that were top-coded by the survey; in 1980 (1990; 2000) these individuals reported amounts greater than $75,000 ($40,000; $50,000) for investment income and in 1990 (2000) these individuals reported amounts greater than $52,000 ($30,000) for retirement income.\(^6\)

5The 1980 Census did not separate retirement income from other sources of income.

6To preclude the possibility of revealing personal information, the Census “top-codes” values for very rich individuals. Specifically, they replace the income variable for individuals with investment income or retirement income above a year-specific limit with the median income of all individuals in that state earning above that limit. The percentage of topcoded observations is very low: 0.47% for investment income and 0.22% for retirement income. Of course, using as a dependent variable “any investment income” avoids the top-coding problem entirely. Nevertheless, as an alternative approach, we run Tobit regressions to account for top-coding, and find very similar results (available upon request).
Following Acemoglu and Angrist (2000) and LM, we create dummy variables for whether the years of required schooling are 8 or less, 9, 10, and 11 or more. These dummies are based on the law in place in an individual’s state of birth when an individual turns 14 years of age. As LM note, migration between birth and age 14 will add noise to this estimation, but the IV strategy is still valid. The first stage for the IV strategy can then be written as

\[ s_i = \alpha + \delta_9 Comp9 + \delta_{10} Comp10 + \delta_{11} Comp11 + \gamma X_i + \varepsilon_i, \]  

(2)

where \( s_i \) is years of schooling, \( Comp9 - Comp11 \) are dummy variables that indicate the required number of years of schooling that individual \( i \) was exposed to, and \( X_i \) is the same set of controls defined above.

Compulsory schooling laws were changed numerous times from 1914 to 1978, even within a state and not always in the same direction. We use data from the 1980, 1990, and 2000 censuses, and focus on individuals who are between 18 and 75 years old, and born on or before 1964. The census does not code a continuous measure of years of schooling, but rather identifies categories of educational attainment: preschool, grades 1-4, grades 5-8, grade 9, grade 10, grade 11, grade 12, 1-3 years of college, and college degree or more. We translate these categories into years of schooling by assigning each range of grades the highest number of years of schooling for that category. This should not affect our estimates since individuals who fall within the ranges of grades 1-8 and 1-3 years of college will not be influenced by the compulsory schooling laws.

Finally, it is worth noting that the estimates produced here are Local Average Treatment Effects, which measure the effect of education on participation for those who were affected by the compulsory education laws. We note that those who are in fact affected by the laws are likely to have low levels of participation, and thus constitute a relevant study population. Moreover,

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7 Specifically, we define the years of mandated schooling as the difference between the latest age an individual is required to stay in school and earliest age she is required to enroll when states do not set the minimum required years of schooling. When these two measures disagree, we take the larger value.

8 LM use the 1960, 1970, and 1980 censuses, which contain information on correctional facility residence, and focus on a narrower age group, ages 20-60.

9 Imbens and Angrist (1994) provides a discussion of Local Average Treatment Effects.
we draw some comfort from Oreopoulos (2006), who studies a compulsory schooling reform that affected a very large fraction of the population in the United Kingdom. Studying the effect of education on earnings, Oreopoulos finds the LATE effect estimated in the United States from a small fraction of the population is quite similar to the estimated effect in the UK from a very large fraction of the population.

3.2 Education and Financial Market Participation

OLS estimates of equation (1) are presented in Table II. Panel A presents the results for the linear probability model, using “any income” as the dependent variable, and panel B studies the level of total income, and includes the controls described in the previous section. The OLS results have the expected signs: an additional year of education is associated with a 3.54 percentage point higher probability of financial market participation, $271 more in investment income, and $548 more in retirement income. We caution that these estimates are likely plagued by omitted variables bias - educational attainment is correlated with unobserved individual characteristics that may also affect savings.

In Table III, we present the first stage, demonstrating that compulsory schooling laws did increase human capital accumulation. Clearly, the state laws do influence some individuals - when states mandate a greater number of years of schooling, some individuals are required to obtain more education than they otherwise would have acquired. A 9th year or 10th year of mandated schooling increases average years of completed education by 0.2 years, while requiring 11 years of education increases education by 0.26 years. Requiring students to remain in school for even one more year (9 years of required schooling) increases the probability of graduating high school by 3.9%.

Table IV presents 2SLS estimates of equation (1). Panel A reveals that an additional year of schooling increases the probability of having any investment income by 7.5 percentage points.\footnote{Weak instruments’ bias is not a problem in this context. We report the F-statistics of the excluded instruments in Table IV. The F-statistics range from 44.5 to 49.9, well above the critical values proposed by Stock and Yogo (2005).}
For retirement investments, an additional year of schooling increases the probability of non-zero income by about 5.9 percentage points. These estimates are somewhat larger than the OLS estimates in Table II, suggesting a downward bias in the OLS.

In Panel B, we study the amount of income from these assets and find a large and significant effect on both types of investment income. The magnitudes are substantially larger than the OLS estimates: an additional year of schooling increases investment income and retirement income by $1760 and $966 respectively.\textsuperscript{11} We find similar effects if we use high school completion as the measure of schooling. Including a cubic in earned income (which includes wages and income from one’s own business or farm) as a control does not affect the results appreciably. The striking fact is that no matter what type of income control we include, we find a persistent and large impact of education on participation.

Our analysis of the Survey of Consumer Finances suggests a magnitude in terms of equity market participation. In the SCF data, an increase in investment income of $1,760 is associated with a 5 percentage point increase in the probability of owning stocks.\textsuperscript{12}

Another way to get at the economic importance of these estimates is to conduct the following back-of-the-envelope calibration exercise. This calibration also helps us to understand the source of the increase: does education raise investment earnings simply because households earn more money, while keeping the fraction of income saved constant, or does it affect the savings rate as well?

The average individual in our sample is 49 years old. To simplify the algebra, we assume he earned a constant $20,000 (the average income for high school graduates in our sample) since he was 20 years old,\textsuperscript{13} saved a constant 10% of his income at the end of each year and earned a 5% return on his assets. Assuming one additional year of schooling boosts wage income by 7% (an estimate from Acemoglu and Angrist 2000), if the individual’s savings rate did not vary with

\textsuperscript{11}Using IV Tobit for investment income yields very similar results; results are available on request.
\textsuperscript{12}The coefficient of a regression of equity ownership on investment income (regression not reported) is \textsuperscript{.0000287 (t-stat 3.1)}, and $1,760^{*.0000287=:.055}$.
\textsuperscript{13}Using the average income at each age gives very similar estimates.
schooling, an additional year would increase his savings by $140 per year, though he would earn income for one fewer year. At the age of 49, his accumulated savings would be $493 higher, and his income from these assets approximately $25 greater.\footnote{2140 * \(\left(\frac{1.05^{28}-1}{.05}\right) - 2000 * \left(\frac{1.05^{28}-1}{.05}\right)\) = $492.91}

In contrast, if we assume that the year of education also increased our hypothetical individual’s income by 7 percent and his savings rate by 2 percentage points, an additional year of schooling would increase his annual savings contribution by $568, yielding an approximately $27,167 greater asset base by age 49, and a corresponding increase in investment income of $1,358.\footnote{2568 * \(\left(\frac{(1+.05)^{29}-1}{.05}\right) - 2000 * \left(\frac{(1+.05)^{30}-1}{.05}\right)\) = $27,167.}

The point estimates on investment income, $1,759 per year, are much closer to this latter figure, suggesting education increased the savings rate. Finally, it is also possible that education affects the choice of asset allocation: better educated individuals may choose portfolios that yield higher returns, perhaps with lower fees and less tax impact.

\section{Education and Financial Management}

Analysis of the effects of education on personal financial management is complicated by the fact that data on credit and education are derived from two distinct data sets; as the census data contain no personal identifiers (and the FRBNY Consumer Credit/Equifax panel has only anonymous identifiers), it is not possible to match individuals across the data sets. We therefore follow Angrist (1990) and adopt a two-sample instrumental variables approach.\footnote{Two-sample IV is relatively rare in finance, but is used in Bitler, Moskowitz, and Vissing-Jorgensen (2005).} We use the census data to estimate the relationship between compulsory schooling laws and education, and the FRBNY Consumer Credit Panel/Equifax to calculate the reduced form relationship between compulsory schooling laws and financial management.\footnote{For a detailed discussion of the two-sample instrumental variables technique, please see section 4.4 of Angrist and Pischke (2008).}

The reduced form is estimated using one dataset, the FRBNY Consumer Credit Panel/Equifax,
which has data on individuals’ state of residence and year of birth.\textsuperscript{18} Table V provides reduced-form estimates of the effect of compulsory education on credit score and credit management, and the probability of filing for bankruptcy or experiencing a foreclosure. The specification parallels that of equation 1 except that the dependent variable is now a measure of credit management and the set of control variables does not include race, gender, census year or the cubic polynomial in earned income, since this information is not available in the credit bureau data. Column (1) through (3) present strong evidence that compulsory schooling laws improve an individual’s credit score. The reduced form effect indicates that cohorts who are required to attend school through the 11\textsuperscript{th} grade have on average credit scores that are 1.7 points higher compared to cohorts not required to attend school beyond the 8\textsuperscript{th} grade. The compulsory attendance dummies are jointly significant at the one-percent level in every specification. Using years of schooling required rather than dummy variables yields an estimate that each year of required schooling increases credit scores by 0.253 points, significant at the five percent level. Column (3) adds zipcode level fixed effects, which control for geographic heterogeneity at a very fine level (there are approximately 43,000 zip codes in the U.S.) The point estimate is smaller, but years of compulsory attendance is significant at the 1 percent level.

Columns (4)-(9) examine the reduced form relationship between compulsory education laws and credit behavior, studying both the fraction of borrower balance that is non-delinquent (averaged over the period for which we have credit bureau data, 1999-2011), and the fraction of quarters a borrower has any delinquent credit. We find statistically significant effects on both. Finally, columns (10)-(15) study the effect of compulsory schooling on the probability a household declares bankruptcy or experiences a foreclosure. Relative to those who were able to drop out before 9\textsuperscript{th} grade, cohorts in states that required attendance through the 11\textsuperscript{th} grade were one percentage point less likely to have declared bankruptcy, and one percentage point less likely to experience a foreclosure.

\textsuperscript{18}The FRBNY CCP/Equifax data do not include state of birth information, so we construct a state-of-birth proxy from an individual’s state-of-residence in the first quarter of their inclusion in the panel. This creates attenuation bias, making it more difficult to find an effect of education on financial outcomes.
The reduced form results provide the average effect on the exposed cohort. To understand the structural effect of education on financial management, we turn to instrumental variables estimation, which provides the effect of education on the individuals who are affected by compulsory schooling. As stated above, because the credit score and bankruptcy data come from a different source than the education data, we cannot use a conventional instrumental variables strategy. Instead, we use a two-sample IV approach. The first-stage regression specification, equation 2, is the same as used in Table III, from the census data set, except that we only use data from the 2000 Census to estimate it in order to match the credit bureau data. The split sample IV estimates are constructed by combining moments from the first-stage with moments from the credit bureau dataset. We estimate standard errors in two ways. First, we provide robust standard errors, as described by Murphy and Topel (1985). Second, we use a block bootstrap technique to account for possible correlation within birthyear-state groups.

Results are presented in Table VI. The point estimates using either estimation technique are quite similar, and suggest that education has important causal effects on financial management. The point estimate on the coefficient for years of schooling, 7.7, is significant at the one percent level using Murphy and Topel standard errors, suggesting that a one standard deviation increase in education (2.7 years) would raise an individual’s credit score by 20 points. This result is significant at the ten percent level using a block bootstrap estimation technique.

A 20 point movement in the credit score is less than one standard deviation in credit score, but there are certainly ranges where such perturbations can be very important. For example, Chomsisengphet and Pennington-Cross (2006) document how a 20 point difference in credit score can impact both the cost and availability of certain home mortgage products. The effect sizes for credit management are similar: a one-standard deviation increase in education increases the fraction of credit card balances kept current by 1.4 percentage points, relative to an unconditional average of 95.6%, and reduces the number of quarters delinquent by 3.5 percentage points, from a mean of 7.5 percentage points.

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19 We thank the editor for this suggestion.
The point estimates of the effect of education on bankruptcy and foreclosure are striking. Over the sample period, 14.4% of individuals declare bankruptcy, and 5.8% experience at least one foreclosure. An additional year of education reduces the probability of filing for bankruptcy by 3.3 percentage points, and foreclosure by 5.7 percentage points. These effects are significant at the 1 percent level (using Murphy and Topel standard errors), but the confidence intervals do admit smaller effect sizes, as small as 1.15 percentage points for bankruptcy and 2.18 percent for foreclosure.

In Panel B of Table VI, we analyze whether education affects bankruptcy and foreclosure similarly throughout our sample, or whether there is a differential effect during the recent financial crisis. We find that during non-crisis periods (1999Q2-2007Q3), education does not reduce bankruptcies or foreclosures. However, during the period since the financial crisis (2007Q3-2011Q4), we observe substantial effects: an additional year of education reduces the probability of bankruptcy by 2.3 percentage points, and the probability of foreclosure by 4.6 percentage points.

Because these outcomes are “worst-case” scenarios, they may be particularly relevant for the group of individuals affected by our instrument. In this case, and in contrast to estimates that examine the effect of education on income, the LATE estimates may not characterize the population parameter.

The economic implications of these results are important. Bankruptcy is costly to individuals, as it results in lower credit scores, and reduced access to credit, and to society, through the deadweight costs of debt collection (Cohen-Cole et al., 2009). Perhaps of even greater importance are the costs of foreclosure. Campbell, Giglio, and Pathak (2011) estimate that a foreclosure reduces the value of the foreclosed house by $44,000, but depresses the value of neighboring houses by a total of $148,000-$477,000.
5 Cognitive Ability and Savings

Having established the impact of education on financial behavior, we turn to examine some possible mechanisms. Recent evidence suggests that the primary value of education is to increase cognitive ability (Hanushek and Woessman, 2008). Financial decisions are often complicated and cognitive ability may play an important role in helping households navigate these complications. For example, household mortgage decisions are tremendously important, yet individuals regularly make costly mistakes when deciding whether to refinance their mortgage (Schwartz, 2007). Even decisions such as which credit card to use, which bank to use, or in which mutual fund to invest, can involve complex trade-offs that require a nuanced understanding of probability and compound interest.

Some evidence in favor of the hypothesis that cognitive ability matters for financial decision making has already been documented. Chevalier and Ellison (1999) find that mutual fund managers who graduated from institutions with high average SAT scores outperform those who graduated from less selective institutions. Stango and Zinman (2009) show that households that exhibit the cognitive bias of systematically miscalculating interest rates from information on nominal repayment levels hold loans with higher interest rates, controlling for individual characteristics. In a study closely related to this section, Grinblatt et al. (2011a) find that Finnish individuals with higher IQs are more likely to participate in equity markets.

Only two other studies, to our knowledge, links actual measures of cognitive ability to investment decisions. Christelis, Jappelli, and Padula (2006) use a survey of households in Europe, which directly measured household cognitive ability using math, verbal, and recall tests. They find that cognitive abilities are strongly correlated with stock market participations. These results are correlations, and the degree to which causal interpretation may be assigned depends on the determinants of cognitive ability. Grinblatt et al (2011b) find that high-IQ traders select better stocks and exhibit fewer behavioral biases than low-IQ traders.

A limitation of this approach is that cognitive ability itself is correlated with other factors
that also affect financial decision making. Bias could occur if, for example, measured cognitive ability is correlated with wealth or the transfer of human capital from parent to child. This is likely the case. Plomin and Petrill (1997), in a survey of the literature, find that both genetic variation and shared environment play a significant role in explaining variation in measured cognitive ability. The importance of family background suggests that the coefficient from a regression of investment behavior on measured IQ which does not correctly control for parental circumstances may be biased upwards.

5.1 Empirical Strategy

One compelling strategy to remove the potential confound of family environment is to study siblings, who grew up with similar backgrounds. Labor economists have used this technique extensively to identify the effect of education on earnings (see, e.g., Ashenfelter and Rouse 1998). Including a sibling group fixed-effect provides a substantial advantage, as it controls for a wide range of observed and unobserved characteristics. Most of the remaining variation in cognitive ability is thus attributable to the random allocation of genes to each particular child.

There are limitations to this approach as well. Children without siblings are of course excluded. The errors-in-variables bias is potentially exacerbated when differencing between siblings (Griliches 1979). Finally, as demonstrated in Bound and Solon (1999), if the endogenous variation is not eliminated when comparing between siblings, the resulting bias may constitute an even larger proportion of the remaining variation than in traditional cross-sectional studies. This concern may be less severe in the case of cognitive ability when measured at an early age, because individuals do not choose cognitive ability in the way they choose how many years of

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20 For example, the correlation between parental IQ and children reared apart is approximately 0.24, providing evidence that genes influence IQ. Similarly, the correlation between two unrelated individuals (at least one adopted) raised in the same household is approximately 0.25.

21 Mayer (2002) surveys evidence on the relationship between parental income and childhood outcomes, and describes a strong consensus that higher parental income and education is associated with higher measured cognitive ability among children.

22 Plomin and Petrill (1997) note that the correlation in IQ of monozygotic (identical) twins raised together is much higher than dizygotic (fraternal) twins raised together.
schooling to obtain. While unobserved characteristics such as motivation and discount rates may affect educational attainment, they are unlikely to affect measures of childhood cognitive ability.

Benjamin and Shapiro (2007) employ this method to study how cognitive ability is correlated with various behaviors, including financial market participation, using data from the National Longitudinal Survey of Youth (NLSY). They regress a dummy for stock market participation on a set of controls, a sibling group fixed-effect, and a measure of cognitive ability. We expand this analysis in several directions. We look at a range of financial assets, considering both the extensive and intensive margins, and finally unpack cognitive ability into two components, knowledge and ability. The former is meant to capture factual aspects of cognitive ability that are taught, such as general science (what is an eclipse?). The latter captures functional abilities, which may or may not be taught: mathematical skills, or coding speed (how quickly the respondent can look up a number in a table).

Following Benjamin and Shapiro (hereafter, BS), we use the National Longitudinal Survey of Youth from 1979, as described above. In 1980, respondents took the Armed Services Vocational Aptitude Battery (ASVAB), a set of 10 exams that measure ability and knowledge, which yields an estimate of the respondent’s percentile score in the Armed Forces Qualifying Test (AFQT). The AFQT comprises mostly questions that measure reasoning abilities, such as math skills, paragraph comprehension and numerical operations. To calculate a measure of knowledge that may have been acquired in school, we include ASVAB test scores such as general science, auto and shop information and electronics information. These scores are then normalized by subtracting the mean and dividing by the standard deviation. Further details are provided in the online data appendix.

Using these test scores, we estimate the effect of cognitive ability, knowledge and education on financial decision making (y_{it}) with the following equation

\[ y_{it} = \beta_1 \text{knowledge}_i + \beta_2 \text{ability}_i + \delta \text{education}_{it} + \gamma X_{it} + SG_i + \epsilon_{it} \]  

(3)
where \( ability_i \) is a measure of innate ability, \( knowledge_i \) is a measure of acquired knowledge, \( education_{it} \) is the highest grade individual \( i \) has completed by year \( t \), \( X_{it} \) includes age, race, gender and survey year effects, and \( SG_i \) are sibling-group fixed effects. Standard errors are corrected for intracluster correlation within an individual over time. We proxy for permanent income by controlling for the log of family income\(^{23}\) in every available survey year from 1979 to 2002, and including dummy variables for missing data.\(^{24}\)

### 5.2 Results

Results are presented in Table VII. In the first column, the outcome variable is equal to one if the respondent answers "something left over" to the following NLSY question: “Suppose you [and your spouse] were to sell all of your major possessions (including your home), turn all of your investments and other assets into cash, and pay all of your debts. Would you have something left over, break even, or be in debt?” The other answers are coded as zero. We find a significantly positive effect of both knowledge and ability - an increase of one standard deviation in knowledge (22 points out of 120 or 18%) increases the propensity to have accumulated assets by about 2.6 percentage points, while an increase in one standard deviation in ability (41 points out of 214 or 19%) increases the propensity by about 3.6 percentage points. Note that this result includes controls for education. The point estimate on education alone is not statistically significant. Respondents were then asked to estimate how much money would be left over - we find that neither ability nor knowledge has an effect on this amount (Column (1) in panel B).

The second column in Table VII examines stock market participation. The NLSY question is:

\(^{23}\)We use log (family income + $1) to include individuals with zero income.

\(^{24}\)We also drop all observations which are top-coded; the cut-off varies by year and outcome variable, but typically does not exclude many individuals. We do not include individuals who are cousins, step-siblings, adopted siblings, or only related by marriage or households with only one respondent.

To ensure that our results are not driven by large cognitive differences between siblings due to mental handicaps, we cut the data in two ways. Our results are robust to dropping all households where any individual is determined to be mentally handicapped at any time between 1988 and 1992 when the question was asked. In addition, our results are robust to dropping siblings with a cognitive ability difference greater than 1 standard deviation of the sample by race.
Do you [or your spouse] have any common stock, preferred stock, stock options, corporate or government bonds, or mutual funds?” Knowledge and education have positive and significant effects: a one standard deviation increase in knowledge or ability increases the participation margin by 3.4 and 1.8 percentage points, respectively. An additional year of education increases stock market participation by 1.5 percentage points. Knowledge and ability are not significantly associated with how much money an individual has in stocks, but education is.

We extend the analysis in BS by studying a number of other outcomes regarding whether and how much individuals save in different financial instruments. In Column (3) we study how respondents answer the question: “Do you [and your spouse] have any money in savings or checking accounts, savings & loan companies, money market funds, credit unions, U.S. savings bonds, individual retirement accounts (IRA or Keogh), or certificates of deposit, common stock, stock options, bonds, mutual funds, rights to an estate or investment trust, or personal loans to others or mortgages you hold (money owed to you by other people)?”\(^{25}\) Innate ability increases an individual’s propensity to save: one standard deviation increases the propensity to save by 5 percentage points. An additional year of education increases the share with positive savings by 1.65 percentage points.

We find similar results when we focus on savings in 401Ks and pre-tax accounts. Ability and knowledge are jointly significant at the ten percent level. Education has a significant effect on savings in IRAs and Keogh accounts (Column (4)). Ability increases participation in tax-deferred accounts such as 401Ks by 5 percentage points. One year of schooling increases both participation in IRAs and Keogh accounts by 1.1 percentage points and participation in tax-deferred accounts by 1.3 percentage points. The effects are substantially smaller for certificates of deposit, loans and mortgage assets (Column (6)).

Our results might be confounded by strategic parents, who increase or decrease parental

\(^{25}\) In following years, respondents were asked a variant of this question - each few years, the list of types of savings changes slightly. For example, in 1988 and 1989, respondents were no longer asked about savings and loan companies while stocks, bonds and mutual funds were asked in a separate question. While our survey year fixed effects should take these changes into account, we also test the robustness of this specification by recoding a new variable with a consistent list of assets. The estimates are nearly identical.
transfers to children as a function of their cognitive ability. Column (7), which examines respondents’ anticipated transfers, shows that this does not happen.

Finally, in Column (8) we look at an outcome variable, classified as “other income” from 1979 to 2002, which includes income from investment and other sources of income,\textsuperscript{26} which corresponds closely to our measure of investment income from the Census. Ability, knowledge and education all have a positive and significant effect on income from these sources: one standard deviation in knowledge increases the probability of having any such income by 5.3 percentage points, one standard deviation in ability increases the probability by 4.1 percentage points, and one year of schooling by 1.5 percentage points.

These results suggest that education, ability and knowledge acquired in school increase participation in financial markets.\textsuperscript{27} Acquired knowledge matters only for one investment class (stocks, bonds, and mutual funds), while cognitive ability is associated with all assets and methods of investing measured in the data. The F-test reported at the bottom of Panel A indicate that knowledge and ability are jointly significant at either the five or ten percent level.

Our finding that cognitive ability is more important than acquired knowledge is consistent with a growing recognition of the key role of cognitive ability in determining economic outcomes (Hanushek and Woessman, 2008). Our analysis suggests one channel through which schooling may matter: it affects cognitive ability, which in turn affects savings and investment decisions. The magnitudes of the effects we identify are large, and may well account for a substantial fraction of unexplained variation in financial behavior.

\textsuperscript{26}The question asks “(Aside from the things you have already told me about,) During [year], did you [or your (husband/wife) receive any money, even if only a small amount, from any other sources such as the ones on this card? For example: things like interest on savings, payments from social security, net rental income, or any other regular or periodic sources of income.”

The list of assets changes slightly from year to year, but always includes interest on savings, net rental income, any regular or periodic sources of income. In 1987, the question also lists worker’s compensation, veteran’s benefits, estates or trusts and up until 1987, also includes payments from social security. From 1987 to 2002, the interviewer also listed interest on bonds, dividends, pensions or annuities, royalties.

Due to the wording of the question (asking for “any other source” of income), we treat this question as constant. The results are robust to focusing only on questions which ask about precisely the same set of assets.

\textsuperscript{27}Columns (1)-(3) of Appendix Table A4 demonstrate that the relationship between schooling and cognitive ability holds in sibling pairs.
6 Other Mechanisms

How else might education affect financial management? One possibility that has received some attention is the fact that high school students in many states are required to attend financial education courses. Bernheim, Garrett, and Maki (2001) study mandatory high school financial education requirements, finding large effects. However, Cole and Shastry (2011) revisit this question using the U.S. Census, and provide evidence that high school financial education did not in fact have any effect on individuals’ financial behavior.

We begin our search for mechanisms by exploring whether the impact of education works through changing the set of people an individual interacts with, either at home or work. Finally, we examine whether education may affect preferences and beliefs, such as attitudes towards risk and feelings of control.

Education changes the set of job opportunities available to individuals. For example, a high-school degree may lead an employee to a salaried job at a large corporation, which facilitates financial market participation. We test for this in the following manner. Using data from the 1970 census, we identify the share of individuals aged 65-70 in each occupation in each state receiving a pension. We use the 1970 census because it includes the individuals’ occupation from 5 years prior to the survey.\(^{28}\) We use this fraction as a measure of the pension probability for each individual in our dataset from 1980, 1990, and 2000, and regress this probability on education, using the state laws as instruments, as in equation (1). The result is presented in Table VIII, Column (1). We find a positive relationship between education and the probability of finding a job in which a pension is offered, statistically significant at the 1 percent level. One year of schooling increases the probability of receiving a pension by 1%. All of the estimates in Table VIII mimic the education specification, with controls for age, cohort, birth state, state of

\(^{28}\)The 1970 census does not include the retirement income variable we have been using this far. Instead, it groups pension income into "income from other sources," such as unemployment compensation, child support and alimony. We therefore define an individual over 65 as having a pension if they received more than $1000 (in 1970 dollars) in other income during the previous year. The results are robust to using $2,500, $5,000 or $10,000 instead.
residence, gender, race, and income.

Hong et al. (2004) find that peer effects are important determinants of financial market participation. To test this channel, we use a similar approach: we calculate the percent of individuals aged 65 and older in every "neighborhood" in the U.S. who received retirement income, and use this as the dependent variable in equation (1). Neighborhoods are defined as county groups, single counties or census-defined "places" with a population of approximately 100,000. Results are presented in Column (2) of Table VIII. We find a remarkably similar effect to that in Column (1): one year of schooling increases the share of retired neighbors with retirement income other than Social Security income by 1 percentage point.29

A commonly advanced view is that education tempers impatience. Indeed, in a field study, Harrison et al. (2002) find that discount rates are strongly negatively correlated with levels of education. Of course, this correlation is hard to interpret: does education reduce discount rates, or do more impatient individuals select to enter the labor market earlier? While we cannot measure discount rates, we do observe whether households take first or second mortgages. We find that education does not have an effect on whether a household takes out a first mortgage (Column (3)), but does significantly reduce the likelihood a household takes out a second mortgage (Column (4)).

As a final direct mechanism, we explore whether education affects individuals' willingness to take risks. Halek and Eisenhauer (2001) find a strong negative correlation between risk aversion and education. We do not have a good measure of attitudes towards risk from the census. However, one important risk an individual can take is to move in search of better opportunities. Heitmueller (2005), for example, argues that risk aversion is an important determination of within-EU migration. We find no evidence that more educated individuals are more likely to move away from their city (Column (5)) or state (not reported) in the past five years.30 We

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29 The F-statistic of the excluded instruments in this column is much lower than that in previous results because we lose data from 1980 when more people were affected by the laws. The 1980 census does not include the public use microdata area identifiers. This suggests this result may suffer from weak instruments bias.

30 Information on whether an individual moved to a different city is available only in the 1980 census.
find evidence that more educated individuals are less likely to have moved into a different house within the city in the previous five years.

It is also possible that education affects financial behavior through beliefs and attitudes. Graham et al. (2005) find that educated investors report higher levels of confidence and invest more abroad. Puri and Robinson (2007) show that optimistic individuals invest a greater share of their portfolio in equities, as compared to other financial instruments. We do not have a view on how education affects optimism; it may well foster discipline and views on achieving specific goals, by changing individuals’ beliefs and self-control. While few datasets consider personality and investment decisions in detail, the NLSY does ask respondents to indicate their agreement with the statement “I have little control over the things that happen to me,” with 1 indicating strong disagreement and 4 indicating strong agreement. Individuals who feel more in control (or have greater self-control) may well be more likely to participate in financial markets.

Online Appendix Table A4, using the same within-family identification strategy, provides evidence from the NLSY that feelings of lack of control are greater among less educated individuals, and weakly greater among individuals with lower levels of cognitive ability.

To examine the relationship between control and financial decisions, we focus on investment decisions made after 1993, the year the personality measure was taken, using the same identification strategy as for cognitive ability. Results are presented in Table IX. Comparing two siblings within the same family, we find that those who report feelings of lack of control are less likely to have money left at the end of the month, less likely to report investment income, and less likely to report having a positive savings balance (Panel A). The magnitudes are quite substantial: moving from strong disagreement to strong agreement with the statement is associated with an individual being 4.3 percentage points less likely to have investment income, and 7.5 percentage points less likely to report having money left at the end of the month.
7 Conclusion

Household financial management is an important determinant of financial welfare (Campbell, 2006). Participation in financial markets is limited. While over 90% of households have transactions accounts, the fraction of families that own bonds (17.6%), stock (20.7%), and other assets is relatively small. Low levels and low returns on savings may well be an important contributing cause to consumer bankruptcy.

This paper contributes to a growing body of literature exploring the importance of non-neo-classical factors to household investment decisions. We explore how education affects financial management, with a focus on discovering causal mechanisms. We first show education significantly increases investment income. Individuals with one more year of schooling are 7.5 percentage points more likely to report positive investment income. Similarly, those with more years of schooling are significantly more likely to report income from retirement savings.

Second, we study how education affects consumers’ borrowing and credit behavior. We find that cohorts induced to receive higher levels of education have higher credit scores on average and are significantly less likely to be delinquent, declare bankruptcy or experience a foreclosure. Some of these effects are less dramatic than the effect of education on financial market participation: an additional year of schooling raises an individual’s credit score by 8 points (roughly 9% of a standard deviation). Others are even more dramatic: one year of schooling reduces the probability of bankruptcy by 3.3 percentage points, from a base of 14.4%.

Examining mechanisms, we find that cognitive ability itself is an important determinant of financial behavior. Controlling for family background, those with higher test scores are more likely to hold a wide variety of financial instruments, including stocks, bonds, mutual funds, savings accounts, tax-deferred accounts, and CDs. When cognitive ability is decomposed into innate abilities and acquired abilities or knowledge, the innate abilities matter for a greater number of financial instruments, but both types of ability affect key measures of financial market participation such as having any accumulated assets and owning any stocks, bonds or mutual
funds.

The point estimates on education suggest that it is a very important determinant of behavior. A convenient metric to compare the relative importance across different studies is the “effect size”, which is the effect of a one standard deviation change in the dependent variable on participation. The “effect size” of education is 19.8 percentage points, which compares to an effect size of trust (Guiso, Sapienza, and Zingales) of 4 percentage points, peer effects (Hong and Stein) of 1.15 percentage points, and experience with stock market returns (Malmendier and Nagel) of 4.2 percentage points.

Three studies serve as potential benchmarks for these effects. Duflo and Saez (2003) present evidence from a randomized evaluation that minor incentives ($20 for university staff attending a benefits fair) can increase TDA participation rates by 1.25 percentage points. Duflo et al. (2006) offered low-income tax filers randomly assigned amounts of matching to contribute to IRAs. They find that an offer of a 50 percent match increased participation by 14 percentage points, which is comparable to two years of education in our analysis. However, no determinants of participation have been found to be more effective than simply changing the default enrollment status for 401(k) plans. Beshears et al. (2006) find changing the default to “enroll” increases participation by as much as 35 percentage points.

Concern about financial decision-making is not often cited as an important determinant of educational policy. Yet, it is worth pointing out that because education affects financial market participation, studies that focus on wage earnings may in fact underestimate the returns to investment in human capital. Such estimates miss the reduction in the probability of bankruptcy and foreclosure. This suggests adjusting earlier cost-benefit analyses of educational programs. Moreover, a growing body of evidence suggests that individuals do often make financial mistakes (Agarwal et al., 2007). Both micro evidence (Agarwal and Mazumder, 2010) and recent experience suggest that some of these mistakes can be quite costly. Increasing educational attainment in the US could dramatically improve financial management, with important effects
on bankruptcy and default, and may even facilitate a more stable financial system (Mian and Sufi, 2011).

8 Bibliography


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9 Not for Publication: On-Line Data Appendix

9.1 Comparison of Census and Survey of Consumer Finances Data

Census data have not been used much to track investment income, and one may naturally have concerns about the reliability of the data, as well as comparability with more standard data sources, such as the Survey of Consumer Finances (SCF). In this appendix, we compare the means and distributions of the variables of interest, and describe the relationship between investment income and financial wealth. In the census data, we use the variable “INCINVST” as a measure of investment income, and the variable “INCRETIR” for retirement income (see Ruggles et. al, 2004). For the SCF, we use the sum of non-taxable investment income (x5706), other interest income (x5708), dividends (x5710), and income from net rent, trusts, or royalties (x5714). In both the census and the SCF, reported numbers appear to be pre-tax income, though the census figures are less precise. Neither the SCF nor census measure includes capital gains. (The income portion of the questionnaire for the census is reproduced below). Retirement income is measured in the SCF as the sum of current account-type pension benefits and non-account-type benefits.31

Table A2 presents the means, standard deviations, ranges, and percentiles for the investment income and retirement income variables. Analysis is limited to a sample of households aged 36-75, who earn investment income below $50,000. (This is the same sample used to evaluate the effect of education on investment income.) Relative to the SCF, census respondents appear to underreport both investment and retirement income. The mean investment income is 17 percent lower, at $1,264, compared to the SCF average figure of $1,515. A nearly identical percent fewer report receiving any investment income: the figure is 33% in the SCF, and 27% in the Census. We speculate that the reason for this is that the survey of consumer finances is much more detailed than the census, and that the SCF is done in person. Nonetheless, the distributions appear to be comparable, with a median of zero in both datasets, and similar 75th, 90th, and 99th percentiles.

The apparent underreporting of retirement income in the U.S. Census is more severe: the average reported in the census is approximately 30 percent lower than the average in the SCF, and approximately 20 percent fewer individuals report any retirement income in the U.S. Census: 22 percent, against 27 percent in the SCF. Nonetheless, again the two distributions appear to track one another reasonably closely.

The results suggest that the dollar figures estimated from the census may not be precisely correct. Nevertheless, the two data sources are not strikingly different, and the effect on estimated coefficients is likely relatively small.

An alternative check of the comparability of the two datasets is to regress the dependent variables used in our main paper on individual characteristics, such as age, income, race, and education level. The coefficients obtained from the SCF and Census are quite similar. Indeed, equality cannot be rejected for 35 out of 36 demographic variables. (Results not reported.)

Appendix Table A3 provides a detailed breakdown of financial market participation, using the 2001 SCF. Each row reports the fraction of households that use a variety of financial services, for a given range of investment income. The ranges were chosen so that there would be at least

31The former are, x6464, x6469, x6474, x6479, x6484, and x6489, and the latter are x5326, x5326, x5334, x5418, x5426, x5434. All values are converted to annual figures, in 2000 dollars.
30 observations in each range. For example, 61% of households report earning no investment income. Among this population, 88% have a transaction account (checkings, savings, or money-market fund). As reported investment income increases, financial market participation generally increases.

A second potential concern with the use of census data is that information is available on investment income, not financial wealth. In particular, if the relationship between financial wealth and investment income is highly non-linear, results using one measure may not translate well to the other. Figure A1 plots the relationship between investment income and financial wealth, from a Fan local linear regression, using data from the 2001 Survey of Consumer Finances. While visual inspection reveals a slight increase in slope around the point of $25,000 (consistent with evidence from Calvet, Campbell, and Sodini, 2007, that investors with higher income achieve higher risk-adjusted returns), to a first approximation, the relationship is linear.

9.2 Census Income Questions

We reproduce here the questions on income from the 2000 Census “long form.”

31. INCOME IN 1999 - Mark [X] the “Yes” box for each income source received during 1999 and enter the total amount received during 1999 to a maximum of $999,999. Mark [X] the “No” box if the income source was not received. If net income was a loss, enter the amount and mark [X] the “Loss” box next to the dollar amount.

For income received jointly, report, if possible, the appropriate share for each person; otherwise, report the whole amount for only one person and mark the “No” box for the other person. If exact amount is not known, please give best estimate.

a. Wages, salary, commissions, bonuses, or tips from all jobs - Report amount before deductions for taxes, bonds, dues, or other items.
   O Yes
   Annual amount - Dollars
   $[[]][[]][][][][][][][][][][].00
   O No
   O Loss

b. Self-employment income from own nonfarm businesses or farm businesses, including proprietorships and partnerships - Report NET income after business expenses.
   O Yes
   Annual amount - Dollars
   $[[]][[]][][][][][][][][][][][][][].00
   O No
   O Loss

c. Interest, dividends, net rental income, royalty income, or income from estates and trusts - Report even small amounts credited to an account.
   O Yes
   Annual amount - Dollars
   $[[]][][][][][][][][][][][][][][].00
   O No

d. Social Security or Railroad Retirement
   O Yes
Annual amount - Dollars
$[ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ]0.00
O No
e. Supplemental Security Income (SSI)
O Yes
Annual amount - Dollars
$[ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ]0.00
O No
f. Any public assistance or welfare payments from the state or local welfare office
O Yes
Annual amount - Dollars
$[ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ]0.00
O No
g. Retirement, survivor, or disability pensions - Do NOT include Social Security.
O Yes
Annual amount - Dollars
$[ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ]0.00
O No
h. Any other sources of income received regularly such as Veterans’ (VA) payments, unemployment compensation, child support, or alimony - Do NOT include lump-sum payments such as money from an inheritance or sale of a home.
O Yes
Annual amount - Dollars
$[ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ]0.00
O No

32. What was this person’s total income in 1999? Add entries in questions 31 a-31 h; subtract any losses. If net income was a loss, enter the amount and mark [X] the “Loss” box next to the dollar amount.
O None OR Annual amount - Dollars
$[ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ][ ]0.00
O Loss

9.3 National Longitudinal Survey of Youth 1979

The NLSY79 cohort is a nationally representative sample of young people aged 14-22 when the survey began in 1979. Respondents were interviewed annually until 1994 and then biennially since 1996. While each survey contains different questions and often special sets of questions on topics such as military participation, time-use or alcohol and substance abuse, each survey contains a core set of questions on respondents’ labor force experience, labor market attachment, investments in education and training. Summary statistics on the variables used in this paper are available in Appendix Table A1.

From these questions, staff at the Center for Human Resource Research create consistently coded variables on a number of demographic characteristics. Two such variables are used in the estimates above. Information on educational attainment and enrollment has been used to create a variable for highest grade completed as of May 1 of the survey year. Separate questions on
income from various sources have been used to create a consistent estimate of "total net family income". This variable summarizes all income received in the household, and does not account for taxes or other adjustments. From 1979 to 1986 total net family income was calculated from a Household Interview administered to parents for respondents who lived with their parents. While 19 sources of income are asked separately (such as wages, military income, farm income, business income, inheritance and gifts), income from investments is included in the "Other Income" category: "Aside from the things you have already told me about, during 19XX, did you (or your spouse/partner) receive any money from any other sources such as the ones on this card? For example, things like interest on savings, payments from social security, net rental income, or any other regular or periodic sources of income?" Questions on different types of assets, such as IRAs and Keogh accounts or 401Ks and pre-tax annuities, differ slightly across years, resulting in question-specific sample periods.

In 1980, respondents in the NLSY79 sample were administered the Armed Services Vocational Aptitude Battery (ASVAB) in a joint effort of the U.S. Departments of Defense and Military Services to update the ASVAB norms. In total, 11,914 NLSY79 respondents (94% of the sample) participated in the test. The ASVAB measures different aspects of ability, knowledge and skill in 10 tests, each in one of the following areas: general science, arithmetic reasoning, word knowledge, paragraph comprehension, numerical operations, coding speed, auto and shop information, mathematics knowledge, mechanical comprehension and electronics information. Scores on these tests are used to estimate each respondent’s percentile score in the Armed Forces Qualifying Test (AFQT), as well as our measures of knowledge and ability. The AFQT score is a function of the individual’s score on tests in arithmetic reasoning, word knowledge, paragraph comprehension and numerical operations. Our measure of innate ability uses these tests plus a test in coding speed, while our measure of acquired knowledge includes tests in general science, auto and shop information, mathematics knowledge, mechanical comprehension and electronics information. Our results are robust to slightly different decompositions.
10 Tables Ordering

sumstats

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knowledge

control

A1-Census Summary Statistics

A2-NLSY Summary Statistics

A3-SCF/NLSY Comparison

A4-Effect of Education on Cognitive Ability

3-First stage for education

first

Table III: Table Caption
4-IV Education on Investment Income
Table IV: Table Caption

5-Reduced Form Credit Bureau
rf_cb
Table V: Table Caption

6-Two Sample IV Credit / Bankruptcy
tsiv
Table VI: Table Caption

7-Knowledge / Ability on Savings
know
Table VII: Table Caption

8-Mechanisms
mech
Table VIII: Table Caption

9-Control on Savings
control
Table IX: Table Caption