If You Are So Smart, Why Aren't You Rich? The Effects of Education, Financial Literacy and Cognitive Ability on Financial Market Participation

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Abstract

Household financial market participation affects asset prices and household welfare. Yet, our understanding of this decision is limited. Using an instrumental variables strategy and dataset new to this literature, we provide the first precise, causal estimates of the effects of education on financial market participation. We find a large effect, even controlling for income. Examining mechanisms, we demonstrate that cognitive ability increases participation; however, and in contrast to previous research, financial literacy education does not affect decisions. We conclude by discussing how education may affect decision-making through: personality, borrowing behavior, discount rates, risk-aversion, and the influence of employers and neighbors.

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1 Introduction

Individuals face an increasingly complex menu of financial product choices. 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. Moreover, we have only a limited understanding of what factors cause participation.

Using a very large dataset new to the literature, this paper studies the determinants of financial market participation. Exploiting exogenous variation in education caused by changes in schooling laws, we provide a precise, causal estimate of the effect of education on participation. One year of schooling increases the probability of financial market participation by 7-8%, holding other factors, including income, constant. The size of this effect is larger than previously identified correlates of behavior, such as trust (Guiso, Sapienza, and Zingales, 2008), peer effects (Hong, Kubik, and Stein, 2004), or life experience from the stock market (Malmendier and Nagel, 2007).

We then turn to explore why this is so. A leading hypothesis is that students receive financial education in school, which changes behavior. Indeed, states increasingly require high schools to teach financial literacy. Twenty-eight states have mandatory financial literacy content standards for high school students; many other high schools offer optional courses. Exploiting a natural experiment studied in an influential paper by Bernheim, Garrett, and Maki (2001), we test whether financial literacy education affects participation. Using a sample several orders of magnitude larger than theirs, with a more flexible specification, we find in fact that high school financial literacy programs did not affect participations.

A second possibility is that education affects cognitive ability, which in turn increases participation. By exploiting within-sibling group variation in cognitive ability, we show that indeed higher levels of cognitive ability leads to greater financial market participation. Importantly, these estimates are not confounded by unobserved background and family characteristics.

Finally, we explore other ways in which education might affect financial behavior, including effects on personality, borrowing behavior, discount rates, risk-aversion, and the influence of

employers and neighbors.

This paper adds to a growing literature on the correlates and determinants of financial participation. Participation is important for many reasons. For the household, participation facilitates asset accumulation and consumption smoothing, with potentially significant effects on welfare. For the financial system as a whole, the depth and breadth of participation are important determinants of the equity premium and the volatility of markets, and household expenditure (Mankiw and Zeldes, 1991; Heaton and Lucas, 1999; Vissing-Jorgensen, 2002; and Brav, Constantinides, and Gezcy, 2002). Participation 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 than those without such assets, as well as different attitudes towards risk-sharing and redistribution.

The 2004 Survey of Consumer Finances indicates that the share of households holding stock, either directly or indirectly, was only 48.6% in 2004, down three percentage points from 2001 (Bucks, Kennickell, and Moore, 2006). Some view this limited participation 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." Guiso, Sapienza, and Zingales (2008) find that individuals lack of trust may limit participation in financial markets. Others argue that limited participation may be rationally explained, by small fixed costs of participation. Vissing-Jorgensen (2002), using data from a household survey, estimates that an annual participation cost of $275 (in 2003 dollars) would be enough to explain the non-participation of 75% of households. This paper sheds some light on this debate by examining whether exogenous shifts in education, and cognitive ability, and financial literacy training affect participation decisions.

This paper proceeds as follows. The next section introduces our main source of data, the U.S. census, which has not yet been used to study financial market participation. Sections 3, 4, and 5 examine how financial market participation is affected by education, financial literacy education, and cognitive ability, respectively. Section 6 discusses additional mechanisms through which education could affect participation, and section 7 concludes.

2 Patterns in Financial Market Participation

2.1 Data

We introduce new data for use in analyzing financial market participation decisions. The U.S. census, a decennial survey conducted by the U.S. government, asks questions of households that Congress has deemed necessary to administer U.S. government programs. One out of six households is sent the "long form," which includes detailed questions about an individual, including information on education, race, occupation, and income. We use a 5% sample from the Public Use Census Data, which is a random representative sample drawn from the United States population.

There are several important advantages to using census data. First and foremost, by combining data from 1980, 1990, and 2000, our baseline specification contains more than 14 million observations. This allows precise estimates, and most importantly, enables us to use instrumental variable strategies that would simply not be possible with other data sets. The large sample size also allows non-parametric controls for most variables of interest, and the inclusion of state, cohort, and age fixed-effects.

The census does not collect any information on financial wealth, but does collect detailed questions on household income. 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 will 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.[1]

[1] Two data points deserve mention. First, one 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).

Second, to 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 limits for investment income are $75,000, $40,000 and $50,000 in 1980, 1990 and 2000 respectively. The limits for retirement income are $30,000 and $52,000 in 1990 and 2000 respectively. (The 1980 Census did not separate retirement income from other sources of income.) 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

A limitation of using the amount of investment income, rather than amount invested, is that the level of investment need not be monotonically related to the level of income from investments. An individual with $10,000 in bonds may well report more investment income than a household with $30,000 in equity. This would make it difficult to use the data for structural estimates of investment levels (such as estimates of participation costs). In this work, 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 positive investment income. Approximately 22% of respondents do so, which is close to 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. The data appendix compares the data from the SCF and the Census in greater detail.

We report results for both the level of investment income (in 2000 dollars), and place of that individual in the overall distribution of investment income. The latter term we measure by the household's percentile rank in the distribution of investment income divided by total income.

The limitations of the data on household wealth are counterbalanced by the size of the dataset: a sample of 14 million observations allows for non-parametric analysis along multiple dimensions, as well as the use of innovative identification strategies to measure causal effects.

2.2 Patterns of Participation

Correlates of participation in financial markets are well understood. Campbell (2006) provides a careful, recent review of this literature. Previous work has demonstrated that participation is, not surprisingly, increasing in income, as well as education (Bertaut and Starr-McCluer, 2001, among others), measured financial literacy (Lusardi and Mitchell, 2007, and Rooij, Lusardi, and Alessie, 2007), social connections (Hong, Kubik, and Stein, 2004), and trust (Guiso, Sapienza, and Zingales, 2008), and experience with the stock market (Malmendier and Nagel, 2007). Tortorice (2008), however, finds that income and education only slightly reduce the likelihood that individuals make expectational errors regarding macroeconomic variables, and that these errors adversely affect buying attitudes and financial decisions.

'any investment income' avoids the top-coding problem entirely. Nevertheless, as an alternative approach, we run Tobit regressions, and find very similar results (available upon request).

In this section, we explore the link between financial market participation, income, age, and education. Taking advantage of the large size of the census dataset, we employ non-parametric analysis. There are at least two significant advantages of non-parametric analysis. First, instead of imposing a linear (or polynomial) functional form, it allows the data to decide the shape of the relationship between variables. This yields the correct non-linear relationship. Second, and more importantly, if a parametric model is not correctly specified, it biases the estimates of all the parameters in the model. Allowing an arbitrary relationship between income and participation, for example, ensures that the education variable is not simply picking up non-linear loading on income.

The left three graphs of Figure 1 depict simple means, indicating how participation varies with age, educational attainment, and earned income. The solid line indicates the share reporting investment income, while the dotted line indicates the average amount (right axis). Throughout the paper, households with no reported investment income are kept in the data, including when we calculate average investment income. Because education and income are strongly correlated, these graphs confound those two factors. As an alternative method, we take advantage of the large sample size of the census, and instead estimate non-parametric relationships, in the following manner. We regress measures of financial market participation, y, on categorical variables for age, α_i, level of education, γ_i, and amount of income, β_i. We use a separate dummy for each \$1,000 income range, e.g., a dummy β_0 indicates income between \$0 and \$999, while β_1 indicates income between \$1,000 and \$2,000, etc.:

$$y_i = \alpha_a + \gamma_e + \beta_w + \varepsilon_i \tag{1}$$

These relationships, all derived from estimating equation 1, are presented in the right three graphs of Figure 1. Again, the solid line gives the share reporting income, while the dotted line gives the amount. These graphs give the incremental effect of one factor (e.g., age), controlling for the other two factors (income and education).

Participation in financial markets increases strongly with age. Approximately 17 percent of individuals report positive investment income at the age of 35; this number increases by about 11 points by age 55. Controlling for wealth and education do not affect this relationship: the

top-right panel gives the regression coefficients α_j, describing the incremental increase for each year of age, for a given level of wealth and education. Average investment income also increases with age, consistent with the life cycle hypothesis.

The share reporting investment income, and amount, increases steadily with education, though this relationship is tempered when age and income are controlled for. Averages for various levels of education are graphed in the second row[2]. Moving from a high-school education to a college degree, for example, is associated with a 10 percentage point increase in participation.

Finally, the bottom two graphs indicate how investment income varies with total individual income. The share of individuals who participate in financial markets increases at a decreasing rate with total income, reaching a peak of approximately 60% for households with earned income levels of $150,000. The bottom-right plot, which controls for age and education, looks quite similar for financial market participation, but describes a much steeper relationship between earned income and earned investment income than if one does not control for age and education.

Of course, even careful partial correlations do not imply causal relationships, as unobserved factors, such as ability, may affect education, income, and financial market participation. One important factor, which we cannot measure, is the intergenerational transmission of saving and investment behavior. Mandell (2007) finds that high school students cite their parents as their primary source of information on financial matters, and finds that students who score high on financial literacy tests come from well-off, well-educated households. Charles and Hurst (2003) find that investment behavior transmitted from parent to child explains a substantial fraction of the correlation of wealth across generations.

In the remainder of the paper, we develop precise causal estimates of the relative importance of factors that affect financial market participation.

[2]They are 5^{th} through 8^{th} grade, 9^{th} grade, 10^{th} grade, 11^{th} grade, 12^{th} grade but no diploma, high-school diploma or GED, some college (HS+), associate degree-occupational program (C1), associate degree-academic program (C2), bachelor's (B.A.), master's (M.A.), professional degree (M.A.+), and doctorate (Ph.D.).

3 The Effect of Education on Financial Market Participation

3.1 Empirical Strategy

The patterns described in section 2 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 likely influence the decisions. Unbiased estimates of the effect of education on investment behavior can be identified by exploiting variation in education that is not correlated with any of these unobserved characteristics. In this section, we exploit an instrumental variables strategy identified by Acemoglu and Angrist (2000)- changes in state compulsory education laws - to provide 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 use changes in state compulsory education laws between 1914 and 1978. We follow the strategy used in Lochner and Moretti (2004, hereafter LM), who use changes in schooling requirements to measure the effect of education on incarceration rates. The principle advantage of following LM closely is that they 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 years of schooling 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,

$$y_i = \alpha + \beta s_i + \gamma X_i + \varepsilon_i \tag{2}$$

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. When the sample includes blacks, we also 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 Brown vs. Board of Education. Cohort of birth is defined, following LM, as 10-year birth intervals. Standard errors are corrected for intracluster correlation within state of birth-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 \$50,000 (\$40,000; \$75,000) for investment income and \$52,000 (\$30,000) for retirement income.

We account for endogeneity in educational attainment by using exogenous variation in schooling that comes from changes in state compulsory education laws. These compulsory schooling laws usually set one or more of the following: the earliest age a child is required to be enrolled in school, the latest age she is required to be in school and the minimum number of years she is required to be enrolled. Following Acemoglu and Angrist and LM, we define the years of mandated schooling as the difference between the latest age she 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 maximum. We then 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

$$s_i = \alpha + \delta_9 * Comp9 + \delta_{10} * Comp10 + \delta_{11} * COMP11 + X_i + \varepsilon_i \tag{3}$$

These laws were changed numerous times from 1914 to 1978, even within a state and not always in the same direction. It is important to note that while state-mandated compulsory schooling may be correlated at the state or individual level with preferences for savings, risk preferences or discount rates or ability, the validity of these instruments rests solely on the assumption that the timing of these law changes is orthogonal to these unobserved characteristics

conditional on state of birth, cohort of birth, state of residence and census year. An alternate explanation would have to account for how these unobserved characteristics changed discretely for a cohort born exactly 14 years before the law change relative to those born a year earlier in the same state.

Our sample differs from LM in two ways. First, LM limit their attention to census data from 1960, 1970 and 1980, as their study requires information on whether the respondent resides in a correctional institution. Investment income is available in a later set of census years; data are available from 1980-2000. We describe results from pooled data from 1980, 1990 and 2000. The second difference from LM is in the sample selection: we include individuals as old as 75, rather than limiting analysis to individuals aged 20-60. The addition of older cohorts also allows us to study reported retirement income where we focus on individuals between the ages of 50 and 75. Individuals in our sample are aged 18 - 75.

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 or more. We translate these categories into years of schooling by assigning each range of grades the highest years of schooling. 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.[3] 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, we draw some comfort from Oreopoulos (2006), who studies a compulsory schooling reform that affected a very large fraction of the population in the UK. While Oreopoulos focuses on earnings rather than financial market participation, he finds that the relationship between schooling and earnings estimated in the United States from a small fraction of the population is quite similar to the relationship in the United Kingdom, which was estimated using a very large fraction of the population.

[3]Imbens and Angrist (1994) provides a discussion of Local Average Treatment Effects.

3.2 Results

OLS estimates of equation (2) are presented in Table 1. 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. In panel C the left-hand side variable is the individual's location in the nationwide distribution of the ratio of investment income to total income.[4] The sample size varies between 4 million and 14 million observations, depending on the sample used (we restrict attention to those over 50 for retirement income). In addition to years of schooling, we include (but do not report) race and gender dummies, age (3-year age groups), birth cohort (10 year cohorts), state of birth, state of residence, census year and a cubic polynomial in earned income. Regressions also include state of residence fixed effects interacted with a dummy variable for being born in the South and turning age 14 in 1958 or later to account for the impact of Brown vs. Board of Education for blacks. This specification mimics Lochner and Moretti.

The OLS results have the expected sign and are precisely estimated. An additional year of education is associated with a 3.55 percentage point higher probability of financial market participation, and a 2.4 percentage point increase for retirement income. An additional year of schooling is associated with approximately $273 more in investment income, and $548 more in retirement income, which represent an approximately 3.4 and 2.3 percentage point increase in the distribution of investment and 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. We therefore implement the IV strategy described above.

For an instrument to be valid it must affect education: we first present evidence that compulsory schooling laws did increase human capital accumulation. The results are presented in Table 2, where we include only observations which contain information on investment income. The omitted group is states with no compulsory attendance laws or laws that require 8 or fewer years of schooling. Clearly, the state laws do influence some individuals - when states mandate a greater number of years of schooling, some individuals are forced to attain more education than they otherwise would have acquired. A 9^{th} year or 10^{th} year of mandated schooling increases

[4] That is, all individuals are sorted by total investment income / total income, and are assigned a percentile ranking.

years of completed education by 0.2 years, while requiring 11 years of education increases education by 0.26 years. In fact, forcing students to remain in school for even one more year (9 years of required schooling) increases the probability of graduating high school by 3.8%. The average years of schooling and the share of high school graduates are monotonic in the required number of years. These estimates are similar to those in Table 4 of LM's work.[5]

Table 3 presents 2SLS estimates of equation (2). Panel A reveals that an additional year of schooling increases the probability of having any investment income by 7.6%. For retirement investments, an additional year of schooling increases the probability of non-zero income by about 5.9%. These estimates are somewhat larger than the OLS estimates in table 1, 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 quite large, substantially larger than the OLS estimates; an additional year of schooling increases investment income and retirement income by $1767 and $979 respectively.[6] Education also improves an individual's position in the distribution of investment income (as a percentage of total income) as shown in panel C. These results are robust to using high school completion, rather than years of schooling, as the measure of educational attainment. 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 how many income controls we include, we find persistent, large, differences in participation by education.

To get a sense of these point estimates, we conduct a 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,[7] saved a constant 10% of his income at the end of each year and earned a

[5] 'Weak instrument' bias is not a problem in this context. We report the F-statistics of the excluded instruments in Table 3. The F-statistics range from 44.5 to 49.9, well above the critical values proposed by Stock and Yogo (2005).

[6] Using IV Tobit for investment income yields very similar results; results are available on request.

[7] Using the average income at each age gives very similar estimates.

5% return on his assets. Assuming one additional year of schooling boosts wage income by of 7% (an estimate from Acemoglu and Angrist 2000), if the individual's savings rate did not vary with schooling, an additional year would increase his savings by ($20,000)*(0.07)*(0.1) = $140 per year. At the age of 49, his accumulated savings would be greater by about $9,000, and his income from investments approximately $450 higher.[8]

In contrast, if we assume that the year of education also increased our hypothetical 49-year-old's savings rate by 1 percentage point, his annual savings would increase by $354, yielding an approximately $22,500 greater asset base by age 49, and a corresponding increase in income of $1,200.[9]

The point estimates on investment income, $1,769 per year, are much closer to this latter figure, suggesting education affected the savings rate. Finally, it is also possible that education affects the choice of asset allocation: better educated individuals choose portfolios that yield higher returns, perhaps with lower fees and less tax impact.

4 Financial Literacy and Financial Market Participation

Having identified causal effects of education on financial market participation, it is important to understand the mechanisms through which education may matter. One possibility is that education increases participation through actual content: financial education may increase financial literacy. A growing literature has found strong links between financial literacy and savings and investment behavior. Lusardi and Mitchell (2007), for example, show that households with higher levels of financial literacy are more likely to plan for retirement, and that planners arrive at retirement with substantially more assets than non-planners. Other work links higher levels of financial literacy to more responsible financial behavior, such as writing fewer bounced checks, and paying lower interest rates on mortgages (see Mandell 2007, among others, for an overview).

For these reasons, improving financial literacy has become an important goal of policy makers and businesses alike. Governments fund dozens of financial literacy training programs, aimed at the general population (e.g., high school financial education courses), as well as specific target

[8] $140 \cdot \frac{(1-\exp(0.05\cdot(49-20)))}{(1-\exp(0.05))} \approx \$9000$. A 5% return would yield approximately $450.

[9] These estimates do not depend on the assumed savings rate of 10%, but on a function of the two savings rates and the return to education: If an individual saved 5% of his income each year, and one year of schooling increased this rate to 6.3%, the estimates would be identical.

groups (e.g., low-income individuals, first-time home buyers, etc.). Businesses provide financial guidance to employees, with an emphasis, but not exclusive focus, on how and how much to save for retirement.

There is some evidence suggesting that financial literacy education can affect both levels of financial literacy and financial behavior.[10] Bernheim and Garrett (2003) examine whether employees who attend employer-sponsored retirement seminars are more likely to save for retirement, and find they do, after controlling for a wide variety of characteristics. However, as Caskey (2006) points out, it is difficult to interpret this evidence as causal, as "stable firms tend to offer financial education and people who are most future-oriented in their thinking are attracted to stable firms" (p. 24). Indeed, any study that compares individuals who received training to those who did not receive training is likely to suffer from selection problems: unless the training is randomly assigned, the 'treatment' and 'comparison' groups will almost surely vary along observable or unobservable characteristics.[11] This may explain why other studies find conflicting effects of literacy training programs. Comparing students who participated in any high school program to those who did not, Mandell (2007) finds no effect of high school financial literacy programs. In contrast, FDIC (2007) find that a "Money Smart" financial education course has measurable effects on household savings.

One of the most methodologically compelling studies that links financial education to savings behavior is Bernheim, Garrett, and Maki (2001, BGM hereafter). BGM use the imposition of state-mandated financial education to study the effects of financial literacy training on household savings. The advantage of this study, which uses a difference-in-difference approach, is that if the state laws are unrelated to trends in household savings behavior, then the estimated effects can be given a causal interpretation.

BGM begin by noting that between 1957 and 1982, 14 states imposed the requirement that high school students take a financial education course prior to graduation. Working with Merrill Lynch, they conducted a telephone survey of 3,500 households, eliciting information on exposure to financial literacy training, and savings behavior. They find that the mandates were

[10] For a careful and thorough review of this literature, see Caskey (2006).

[11] Glazerman, Levy, and Myers (2003) make this point forcefully when they compare a dozen non-experimental studies to experimental studies, and find that non-experimental methods often provide significantly incorrect estimates of treatment effects.

effective, and that individuals who graduated following their imposition were more likely to have been exposed to financial education. They also find that those individuals save more, with those graduating five years after the imposition of the mandate reporting a savings rate 1.5 percentage points higher than those not exposed.

In this section, we first use census data to replicate the findings of BGM. Using their specification, we find positive and significant effects of financial education. We then extend BGM's research in two directions. First, the large sample size allows the inclusion of state fixed effects, as well as non-parametric controls for age and education levels. Second, we are able to carefully test whether the identification assumption necessary for their approach to be valid holds.

4.1 Bernheim, Garret, and Maki Replication

The main results from BGM are reproduced in column (1) of Table 4. BGM estimate the following equation, with individual savings rates as the dependent variable y_i :

$$y_{is} = \alpha_0 + \beta_0 * Treat_s + \beta_1 * (MandYears_{is}) + \beta_2 * Married_i + \beta_3 * College_i + \beta_4 * Age_i + \beta_5 * Earnings_i + \varepsilon_i \quad (4)$$

Treat_s is a dummy for whether state s ever required students to take financial literacy education, MandYears_{is} indicates the number of years financial literacy mandates had been in place when the individual graduated from high school, Married_i and College_i are indicator variables for marital status and college education, and earnings is total earnings / 100,000. Column (1) gives the results for savings rate percentile, compared to peers, which BGM use to reduce the influence of outliers. Consistent with the patterns reported above and elsewhere, BGM find savings increases in education and earnings. They suggest that the strong relationship between age and income explains why the savings rate is not correlated with age.

The main regressor of interest, β_1, is positive and significant, suggesting that exposure to financial literacy education leads to an increased savings rate. Graduating five years following the mandate would induce an individual to move approximately 4.5 percentage points up in the distribution of savings rate, equivalent to a 1.5 percentage points shift in savings rate. BGM also note that the fact that β_0 is statistically indistinguishable from zero supports the identification strategy: treated states were not different from non-treated states prior to the imposition of the

mandate.

In columns (2) - (5) we replicate BGM's results, estimating equation (4) using data from the census. There are two important differences between the census data and the BGM sample. First, the BGM sample was collected in 1995, five years prior to the 2000 census. Our sample is aged 35-54 in 2000. When using the census data, we focus on households born in the same years as the BGM sample, so the birth-cohorts are five years older.[12] Second, the census sample size is substantially larger, at 3.6 million, compared to BGM's 1,900 respondents. We cluster standard errors at the state of birth-year of birth level.

The primary dependent variable used by BGM was the savings rate, defined as unspent take-home pay plus voluntary deferrals, divided by income. This information is not available in the census. Instead, we focus on reported income from savings and investments, dividends and rental payments, which should be informative of the level of assets held by the household.

Columns (2) and (3) in Table 4 present the estimation of equation (4) using "any investment income", a dummy equal to 1 if the household reports any income from investment or savings, as the dependent variable. Column (2) estimates a linear regression model, while column (3) estimates probit. Similar to BGM, we find a positive relationship between savings behavior and age, income, college education, and total income.

The main coefficient of interest, on years since mandate, is positive and statistically significant, at the one percent level. The point estimate, in column (2) 0.33, suggests that each year the mandate had been in effect the share of households reporting savings income increased by 0.33 percentage points. The mean level of participation is 22.13 percentage points, while the standard deviation is 41 percentage points. The effect is therefore modest: the effect, five years following the imposition of mandates, would be 1.5 percentage points, or approximately 0.05 standard deviation. However, the effect is highly statistically significant (t-stat 5.18). Column (4) reports the coefficients from the probit regression. The size of the marginal effect is nearly identical, at 0.37 percentage points, evaluated at the mean dependent variables.

Column (4) estimates equation 4 using the dollar value of investment income as the dependent variable. This regression suggests that an additional year of mandate exposure increases savings

[12] We do not think it likely that any of the differences from our findings and BGM are attributable to the timing of the data collection. Using census data from 1990 (or 1980) gives very similar results.

income by approximately $19. The average amount of investment income is $1199, while the median amount is $0. Assuming a return on investments of 5%, an increase of $18 would suggest an increase in total savings of about $360 for each year of exposure to the mandate. The average individual that had been exposed to the mandate in the sample had been exposed for five years, suggesting a roughly $1,800 increase in total savings.

Finally, we use the households placement in the entire distribution of investment income to total income. This is close to BGM's percentile rating, though it is based on investment income, rather than savings rate. Again, we find a positive and statistically significant effect of exposure to financial education.

The results are, at first glance, encouragingly consistent with BGM. One notable difference is that the coefficient on Treat, β_0, is negative, and statistically and economically significant, in all regressions. A crucial assumption for the BGM approach to be valid is that cohorts in the states in which the mandates were imposed were not trending differently than those in which the mandates were not imposed. While a negative β_0 does not necessarily indicate the BGM identification strategy is not valid, it does raise a cautionary flag. In the next section, we expand on the BGM methodology, taking advantage of a substantially increased sample size, to examine how savings behavior of individual cohorts varies with the timing of the mandates.

4.2 A More Flexible Approach

4.2.1 Empirical Strategy

In this section, we improve upon the BGM identification strategy in several ways. First, we add state fixed-effects, which will control for any unobserved, time-invariant heterogeneity in savings behavior across states. Second, rather than include a linear trend for age, we include a fixed effect for each birth-year cohort α_a, controlling for both age and cohort effects. Finally, and most importantly, the extremely large sample size allows for a much more careful measurement of the impact of literacy education. Rather than a standard difference-in-difference, which compares the average in the 'post' to the average in the 'pre,' we include event-year dummies, which estimate the average level of participation individually for each event-year before and after the implementation of the mandates. This strategy is perhaps best conveyed graphically, in Figure 2. The line plots average participation for cohorts that were not exposed (left of the vertical

line) and exposed (right of the vertical line) to the mandates.

We do this by defining a set of 11 dummy variables, $D_{isb}^{-5}, D_{isb}^{-4}, ..D_{isb}^{0}, D_{isb}^{1}, ..., D_{isb}^{4}, D_{isb}^{5plus}$, which divide our sample into eleven groups. The omitted group is individuals who were born in states in which mandates did not go into effect, or who would have graduated from high school more than five years before the mandates kicked in; all 11 dummies are zero. D_{isb}^{0} indicates that an individual graduated in the year the mandates took effect; D_{isb}^{1} in the first year after the mandates took effect. $\mathrm{D}_{isb}^{2} - D_{isb}^{4}$ are defined analogously, while D_{isb}^{5p} is set to 1 if an individual graduates five or more years after the first cohort in that state was affected by the mandate. We use five as a cut-off for simplicity and because BGM suggest the mandate would have achieved maximal effect "in short order (within a couple of years)"[13]. An important test of a difference-in-difference strategy is that there are no pre-existing trends. D_{isb}^{-1} is set to one for the last cohort to graduate $before$ mandates took effect, with D_{isb}^{-k} defined analogously for cohorts born 2-5 years before the mandate took effect. We thus estimate the following equation:

$$y_{isb} = \alpha_s + \gamma_b + \sum_{k=-5}^{4} \gamma_k D_{isb}^{k} + \gamma_{5p} D_{isb}^{5p} + \beta X_i + \varepsilon_{isb} \tag{5}$$

The vector X_i includes controls for race, college education, whether the household is married, and household income. To account for within-cohort correlation, standard errors are clustered at the state of birth-year of birth level.

Using dummies, rather than a single variable, has two important advantages: first, it provides a clear and compelling test of the identification strategy: were cohorts in states in which the mandate was eventually to be imposed similar to those in which no mandate was imposed. Second, it allows the data to decide how the efficacy of financial literacy affects savings behavior: the effect can be constant, increasing, or decreasing. By using $MandYears$, BGM constrain the effect to be linear in years since the mandate was imposed.

A finding consistent with the results from BGM would be the following: the coefficients D_{isb}^{k}, for k<0, would be statistically indistinguishable from zero and the coefficients on D_{isb}^{0},...D_{isb}^{4} and D_{isb}^{5p} would start out small (perhaps indistinguishable from zero), but increase over time, and be positive and statistically significant for higher values of k. In other words, prior to the

[13] p. 12. Very similar patterns obtain if a ten year window is used.

imposition of the mandates, savings behavior was not trending up or down in states in which the mandate was imposed and the imposition of financial literacy education led to increased savings over time.

4.2.2 Results

Table 5 presents results from equation (5). Column (1) presents the estimates for the linear probability model, with "any investment income" as the dependent variable. Column (2) uses the level of total investment income[14] on the left-hand side, and column (3) uses the individual's location in the nationwide distribution of investment income to total income as the dependent variable.

Figure 2 plots each γ_k coefficient, along with a 95% confidence interval. These coefficients represent the difference in financial participation between the particular cohort, and the cohorts that graduated more than five years prior to the imposition of mandates. (These changes are not time or age effects, since the birth year dummies absorb any common change in savings behavior). The red vertical line indicates the first cohort that was affected by the mandate,with cohorts not affected (born earlier) to the left, and cohorts affected (born later) to the right.

The results, presented in Table 5, disconfirm the findings of BGM: the financial literacy mandates did not increase savings. Consider the results for the dependent variable "any investment income." Individuals born in states both before and after the mandates were imposed are substantially more likely to report investment income, relative to those born in states in which mandates were not imposed. Most importantly, there is no increase in investment income for cohorts that were born after the mandate. We can formally test this hypothesis by comparing the average participation in treated states just before to just after the mandates.[15]. For financial participation, the average value of γ_k is 1.12 for k$\in \{-4, -3, -2,$ and -1$\}$, and 1.15 for k$\in \{1, 2, 3, 4\}$. An F-test (reported in the final two rows of Table 5) indicates that financial participation did not change following the mandates. The two rows test the hypothesis that the sum of the four 'pre' year coefficients are equal to the sum of four 'post' coefficients, and fail to reject equality.

[14] It is not obvious that the effect would be a level effect, rather than a proportional effect. However, because many observations are zero or negative, we do not use log income as a dependent variable.

[15] Formally, we test $\frac{1}{4}\left(\gamma_{-4}+\gamma_{-3}+\gamma_{-2}+\gamma_{-1}\right)=\frac{1}{4}\left(\gamma_1+\gamma_2+\gamma_3+\gamma_4\right)$

Column (2) of Table 5 performs an identical analysis, using the level of investment income as the dependent variable. Again, troublingly for the BGM identification strategy, investment income is above average for cohorts graduating prior to the imposition of the mandate. There is an apparent general upward trend, but no clear trend break at the time of the imposition of the mandate. A test of the four pre γ_k against the four post γ_k indicates that the latter are significantly higher. However, given that there is positive trend before the mandates are implemented, and that the effect appears to disappear after four years (γ_{5p} is statistically indistinguishable from zero), the results do not suggest the mandates had an effect.

Finally, column (3) performs the same analysis, using the percentile rank of where the household falls in the percentile distribution of investment income to total income. The observed patterns are quite similar to those for any investment income.

While there is no effect when using the entire population, perhaps the effect is heterogenous. Households with lower levels of education may benefit most from basic financial literacy training provided in high schools. To test the hypothesis, we re-estimate equation 5 using only data from individuals who report a maximum educational attainment of 11^{th} or 12^{th} grade, or some college. Results (not reported) are very similar in this subsample: there is no effect of the mandates on savings behavior.

Similar findings hold when data from the 1980 or 1990 census are used, or when the sample is restricted to blacks only or whites only. All estimates display the same pattern, financial market participation is above historic levels prior to the imposition of mandates, and does not increase following the mandate.

As a final check of the identification strategy, we use state-level GDP growth data to examine whether the imposition of mandates was correlated with states' economic situation. The data, from the Bureau of Economic Analysis, for 1963-1990 are used, giving 1,296 observations.[16] We estimate an equation very similar to 5:

$$y_{sy} = \sum_{k=-5}^{4} \gamma_k D_{sy}^k + \gamma_{5p} D_{sy}^{5p} + \varepsilon_{sy}, \tag{6}$$

where y_{sy} is GDP growth in state s in year y, and D_{sy}^k is a dummy for whether the state s

[16] As before, DC, Hawaii, and Alaska are excluded. This exclusion makes no difference.

imposed a mandate that first affected the graduating high school class in year k. Results are presented in Table 6. The first column suggests why participation was increasing both before and after the mandates became effective: the mandates were passed after periods of abnormally high economic growth. The average growth rate in the five years leading up to the mandates was 0.26 log points higher than previous years. Similarly, in the four following years, growth was 0.125 log points higher than the base period, while in the period more than five years after the imposition of mandates, growth was on average 0.2 log points *lower* than the base period. Mandates were passed during periods of strong growth in states. The patterns in GDP growth are similar to those observed for financial participation, and may well explain why financial participation increased prior to the passage of the mandates.

Columns (2)-(5) of Table 6 add, progressively, state fixed effects, and linear, quadratic and fixed-effect controls for time. The last three rows of the table jointly test various combinations of the D_{sy}^{k} coefficients. The most flexible specification includes year and state fixed-effects. Neither the 'pre' dummies taken together, nor the 'post' dummies, are jointly statistically significant. However, the joint hypothesis that $D_{sy}^{k} = 0$ for all k can be rejected at the 1% level. (p-value $<.0001$). The evidence therefore suggests that both cross-sectional and panel estimates should be treated with caution.

5 Cognitive Ability and Savings

Recent evidence suggests that the primary value of education is to increase cognitive ability (Hanushek and Woessman, 2008). Financial decisions are often complicated. The household mortgage decision is tremendously important for the average household. 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, compound interest, etc.

Some evidence in favor of the hypothesis that cognitive ability matters for financial decision making has already been collected. 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 (2007) show that households who 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. Korniotis and Kumar (2007a) examine portfolio choice of individual investors, and find that stock-selection ability declines dramatically after the age of 64, which is approximately when cognitive ability declines. Korniotis and Kumar (2007b) compare the stock-selection performance of individuals likely to have high cognitive abilities to those likely to have low cognitive abilities, and find that those likely to have higher cognitive abilities earn higher risk-adjusted returns. Agarwal et al. (2007) find that individuals' financial sophistication varies over the life-cycle, peaking at 53, and note that this pattern is similar to the relationship between cognitive ability and age. Zagorsky (2007) finds that IQ test scores are positively correlated with income and related to measures of financial distress such as trouble paying bills or going bankrupt, but not correlated with wealth. Finally, Dhar and Zhu (2006) find individuals in professional occupations are less likely to exhibit the disposition effect in their trading.

Only one other study, 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 investment in the stock market. These results are correlations, and the degree to which causal interpretation may be assigned depends on the determinants of cognitive ability.

A limitation of that 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.[17] The importance of background suggests that the coefficient from a regression of investment behavior on measured IQ which does not correctly control for parental circumstances

[17]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.

may be biased upwards.[18]

5.1 Empirical Strategy

One compelling way to overcome the potential confound of environment is to study sibling pairs, 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.[19]

There are limitations to this approach as well. Only children 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 all 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 is mitigated in the case of cognitive ability because educational attainment is a choice variable, while cognitive ability is not. While unobserved characteristics such as motivation and discount rates may affect how many years of schooling different siblings get, these are unlikely to affect measures of their 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, we consider both the extensive and intensive margins, and finally we unpack cognitive ability into two components, 'knowledge' and 'ability.' The former is meant to capture factual aspects of cognitive ability that are 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 fast can

[18] 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.

[19] 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.

the respondent look up a number in a table).

Following Benjamin and Shapiro (hereafter, BS), we use the National Longitudinal Survey of Youth from 1979. 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. In 1980, survey respondents took the Armed Services Vocational Aptitude Battery (ASVAB), a set of 10 exams that measure ability and knowledge, and calculated 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 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

$$y_{it} = \beta_1 knowledge_i + \beta_2 ability_i + \delta education_{it} + \gamma X_{it} + SG_i + \varepsilon_{it} \tag{7}$$

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 and 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[20] in every available survey year from 1979 to 2002 and including dummy variables for missing data. [21]

[20] We actually take log (family income + \$1) so as to not drop individuals with zero income.

[21] 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 and we also drop households that only have 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.

5.2 Results

Results are presented in Table 7. For each type of asset (column), panel A provides estimates when the outcome variable is whether the individual has any money in this type of asset (multiplied by 100), panel B examines how cognitive ability impacts how much money the individual has in this type of asset and panel C uses the individual's position in the distribution of asset accumulation. The first two columns in panel A replicate the results found in BS. Column (1) uses as an outcome variable a dummy for whether 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?" 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% while an increase in one standard deviation in ability (41 points out of 214 or 19%) increases the propensity by about 3.6%. Note that this result is after controlling 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) or on the individual's position in the overall distribution (column (1) in panel C).

The second column in Table 7 examines stock market participation. The NLSY question is "Not counting any individual retirement accounts (IRA or Keogh) 401K or pre-tax annuities... Do you [or your spouse] have any common stock, preferred stock, stock options, corporate or government bonds, or mutual funds?" There is a positive and significant effect: a one standard deviation increase in knowledge or ability increases the participation margin by 3.4% for knowledge and 1.8% for ability. Education also has a strongly significant effect on stock market participation of about 1.5% per year of additional education. Column (2) in panels B and C demonstrates that ability is not significantly associated with how much money an individual has in stocks, bonds or mutual funds or the individual's rank in the distribution of such assets, but knowledge and education are.

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 following 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)?[22]" Innate ability increases an individual's propensity to save: one standard deviation increases the propensity to save by 5%. Education increases the share with positive savings by 1.65% per year of education. Ability and education also increase the amount of savings and an individual's position in the distribution of savings as a percent of income.

We find similar results when we focus on savings in 401Ks and pre-tax annuities (column (5)). Ability and knowledge are not individually significant, but 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%. One year of schooling increases both participation in IRAs and Keogh accounts by 1% and participation in tax-deferred accounts by 1.3% per. The effects are substantially smaller for certificates of deposit, loans and mortgage assets (column (6)).

Column (7) presents the results for the question of whether the respondent expects to receive inheritance (estate or investment trust), shedding light on the interpretation of our results. If parents treated children with different cognitive abilities differently, the mechanism through which cognitive ability matters may not be individual decisions, but rather increased (or decreased) parental transfers. The coefficients in this column are not statistically distinguishable from zero, suggesting that parents do not treat children with different cognitive ability differently.

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,[23] which

[22] 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 & 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 almost identical to those reported in Table 7.

[23] 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

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%, one standard deviation in ability increases the probability by about 4% and one year of schooling by 1.5%. Similarly, ability, knowledge and education increase the individual's percentile ranking in such income, although only education affects the amount earned.

These results suggest that both ability and knowledge acquired in school and education increase participation in financial markets. [24] 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-statistics at the bottom of Panel A indicate that knowledge and ability are always 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 by 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 market participation.

6 Other Mechanisms

How else might education affect financial market participation? We begin this last section by exploring whether education affects borrowing behavior, whether it matters because of peer effects at home or at 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 obtain a salaried job at a large corporation, which facilitates financial market participation. We test for this in the following manner. We iden-

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.

[24] Columns (1)-(3) of Appendix Table A4 demonstrate that the relationship between schooling and cognitive ability holds in sibling pairs.

tify the share of individuals aged 65-70 in each occupation in each state receiving a pension, using data from the 1970 census. We use the 1970 census because it includes the individuals' occupation from 5 years prior to the survey.[25] 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 state laws as an instrument, as in equation (2). The result is presented in Table 8, 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 would increase the probability of receiving a pension by 1%. All of the estimates in Table 8 mimic the education specification, with controls for age, cohort, birth state, state of 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 (2). 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 8. 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.[26]

A commonly advanced view is that education tempers impatience. Indeed, in a field study, Harrison (2002) finds 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 out first and 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

[25] 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.

[26] 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.

(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. One important risk an individual can take is to move, in search of better opportunities. We find no evidence that individuals are more likely to move away from their city (column 5) or state (not reported) in the past five years. We 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 market participation through beliefs and attitudes. Graham et al. (2005) find that educated investers 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.

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 more larger among individuals with lower levels of cognitive ability.

To examine the relationship between self control and financial market participation, 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 9. Comparing two siblings within the same family, we find that those who report difficulty with self 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 8 percentage points less likely to report having money left at the end of the month.

7 Conclusion

Household 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. While participation has been increasing substantially over the previous fifty years, this increase seems to have stalled: direct ownership of stock declined slightly from 2001 to 2004, as did the fraction of families with retirement accounts.

This paper contributes to a growing body of literature exploring the importance of non-neoclassical factors to household investment decisions. We explore three important determinants of participation in financial markets, with a focus on discovering causal mechanisms. We begin by studying the effect of education and find that education significantly increases investment income. Individuals with one more year of schooling are 7.6% more likely to report positive investment income. Similarly, those graduating from high school are significantly more likely to report income from retirement savings than those not graduating. We next explore the direct mechanisms through which education may affect investment decisions.

The first mechanism we study is that education may affect savings behavior through actual content: students acquire financial literacy in school. We show that a set of financial literacy education programs, mandated by state governments, did not have an effect on individual savings decisions. Those who graduated just prior to the imposition of mandates (and therefore were not exposed to financial literacy education) have identical participation rates as those who graduated following the mandates (and were therefore exposed to the program).

Next, we find that cognitive ability is important. Controlling for family background, those with higher test scores are more likely to hold a wide variety of financial instruments, including stocks, bonds, and 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 participation. 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 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 benchmark of these effect. 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 compares to two years of education in our measure. No proposed 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.

We conclude with a brief policy discussion. First, we point 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; this suggests adjusting earlier cost-benefit analyses of educational programs. Second, do our results suggest that financial education is not worth pursuing? Between 1998 and 2007, an additional 19 states have implemented content standards for financial education in high schools.[27] Given that we find an effect of education, but not financial literacy education, one might reasonably ask whether the substantial financial resources devoted to financial literacy education are well spent? We do not feel that the data warrant this conclusion. We find education effective, but an additional year of schooling is potentially life-changing event. In contrast, financial literacy courses tend to be short, covering basic topics. However, we do feel it is imperative to conduct rigorous evaluations of these programs to measure their efficacy. The best and most compelling evidence would come from randomized evaluations.

[27] "Economic, Personal Finance & Entreprenuership Education in Our Nation's Schools in 2007," National Council on Economics Education, http://www.ncee.net/about/survey2007/NCEESurvey2007.pdf, accessed 10/1/2008. p. 3.

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9 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 standard data sources. 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 survey of consumer finances, 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 in Appendix 2). Retirement income is measured in the SCF as the sum of current account-type pension benefits and non-account-type benefits.[28]

Appendix Table A1 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 35-75, 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

[28] The 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.

percent fewer report receiving any invesment 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 75^{th}, 90^{th}, and 99^{th} percentiles

The apparent underreporting of retirement income in the U.S. Census is more severe: the average reported in the census is approximately 30% lower than the average in the SCF, and approximately twenty 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. The patterns described in section 2 correspond closely with those observed using other datasets.

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.)

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. The use of location in the distribution of investment and retirement income should also serve to mitigate concerns about non-linear effects.

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

b. Self-employment income from own nonfarm businesses or farm businesses, including proprietorships and partnerships - Report NET income after business expenses.

OYes

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

$[ ][ ][ ],[ ][ ][ ].00

O No

e. Supplemental Security Income (SSI)

O Yes

Annual amount - Dollars

$[ ][ ][ ],[ ][ ][ ].00

O No

f. Any public assistance or welfare payments from the state or local welfare office

O Yes

Annual amount - Dollars

$[ ][ ][ ],[ ][ ][ ].00

O No

g. Retirement, survivor, or disability pensions - Do NOT include Social Security.

O Yes

Annual amount - Dollars

$[ ][ ][ ],[ ][ ][ ].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

$[ ][ ][ ],[ ][ ][ ].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

$[ ][ ][ ],[ ][ ][ ].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 A2.

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 adminstered 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.

Table I

OLS Estimates of the Effect of Schooling on Income from Various Sources

This table reports results from a regression of investment income on education, gender, race, age (3-year age groups), birth cohort (10 year cohorts), state of birth, state of residence, census year and a cubic polynomial in earned income. Only the education coefficient is reported. The sample comprises individuals reported in the 5% samples of the 1980, 1990, and 2000 census. We include 18-75 year olds (50-75 year olds when considering retirement income). The dependent variable of interest is whether the household receives income from investments or retirement savings (Panel A), the amount (Panel B), and where the household falls in the entire distribution of investment or retirement income (Panel C). Regressions also include state of residence fixed effects interacted with a dummy variable for being born in the South and turning age 14 in 1958 or later to account for the impact of Brown v. Board of Education for blacks. Top-coded individuals (see text) are dropped in panels B and C. Standard errors, corrected for arbitrary correlation within state of birth-year of birth, are in parentheses. (Numbers with *** indicate significance at the 1-percent level.)

	Income from Investments (1)	Income from Retirement Savings (2)
Panel A: Any Investment Income		
Years of schooling	3.55 ***	2.40 ***
	(0.01)	(0.02)
Num of Observations	14,727,879	4,218,820
R-Squared	0.183	0.177
Panel B: Amount of Investment Income		
Years of schooling	273.43 ***	548.52 ***
	(5.00)	(4.84)
Num of Observations	14,655,392	4,185,100
R-Squared	0.090	0.147
Panel C: Percentile of Investment Income in Distribution		
Years of schooling	3.37 ***	2.28 ***
	(0.01)	(0.02)
Num of Observations	13,255,980	3,854,646
R-Squared	0.168	0.179

Table II

Estimates of the Effect of Compulsory Schooling Laws on Education

This table reports the first-stage relationship between compulsory school laws and educational attainment. The sample comprises individuals reported in the 5% samples of the 1980, 1990, and 2000 census. We include 18-75 year olds. The dependent variables of interest are the number of years of schooling attained in column (1) and an indicator for whether the individual graduated high school in column (2). The independent variables of interest indicate whether the state in which the individual was born prohibited drop-out until a child had completed 9th, 10th, or 11th grade. Other controls include fixed effects for gender, race, 3-year age groups, 10-year birth cohorts, state of birth, state of residence, census year and a cublic polynomial in earned income. Regressions also include state of residence fixed effect interacted with a dummy variable for being born in the South and turning age 14 in 1958 or later, to account for the impact of Brown v. Board of Education for blacks. Standard errors, corrected for arbitrary correlation within state of birth-year of birth, are in parentheses. (Numbers with *** indicate significance at the 1-percent level.)

	Years of schooling (1)	High school (2)
Compulsory Attendence = 9	0.208 ***	0.038 ***
	(0.018)	(0.003)
Compulsory Attendence = 10	0.194 ***	0.040 ***
	(0.023)	(0.004)
Compulsory Attendence = 11	0.259 ***	0.055 ***
	(0.027)	(0.005)
Num of Observations	17,033,260	17,033,260
R-Squared	0.231	0.178

Table III

2SLS Estimates of the Effect of Schooling on Income from Various Sources

This table reports the second-stage relationship between financial market participation and educational attainment. The sample comprises individuals reported in the 5% samples of the 1980, 1990, and 2000 census. We include 18-75 year olds (50-75 year olds when considering retirement income). The dependent variable of interest is whether the household receives income from investments or retirement savings (Panel A), the amount (Panel B), and where the household falls in the entire distribution of investment or retirement income. Years of schooling is instrumented with compulsory schooling laws. In addition, we include as controls, but do not report, fixed effects for gender, race, 3-year age groups, 10-year birth cohorts, state of birth, state of residence, census year and a cubic polynomial in earned income. Regressions also include state of residence fixed effects interacted with a dummy variable for being born in the South and turning age 14 in 1958 or later to account for the impact of Brown v. Board of Education for blacks. Top-coded individuals (see text) are dropped in panels B and C. Standard errors, corrected for arbitrary correlation within state of birth-year of birth, are in parentheses. (Numbers with *** indicate significance at the 1-percent level.)

	Income from Investments (1)	Income from Retirement Savings (2)
Panel A: Any Investment Income		
Years of schooling	7.59 ***	5.93 ***
	(0.51)	(1.03)
Num of Observations	14,727,879	4,218,820
F-stat of excluded instruments	47.0	45.6
Panel B: Amount of Investment Income		
Years of schooling	1767.49 ***	979.61 ***
	(128.66)	(130.10)
Num of Observations	14,655,392	4,185,100
F-stat of excluded instruments	46.9	45.1
Panel C: Percentile of Amount/Income in Distribution		
Years of schooling	7.64 ***	5.43 ***
	(0.49)	(0.92)
Num of Observations	13,255,980	3,854,646
F-stat of excluded instruments	49.9	44.5

Table IV

BGM Results and Replication Using Census Data

The dependent variables of interest are household position in the distribution of savings rate (column (1)) and of investment income (column (5)), whether the household reported any investment income (columns (2) and (3)), and the level of investment income (column (4)). The independent variable of interest is whether the state imposed a mandate requiring high school students to receive financial education, and how many years after the mandate a student graduated from high school. Column (1) reports the original regression from Bernheim, Garret, and Maki (2001), which uses data from a telephone survey conducted by the authors in 1995, while columns (2)-(5) use individuals from the 5% sample of the 2000 census. Both samples include individuals born between 1946 and 1965. Top-coded individuals (those with income from investments greater than $50,000 or retirement income greater than $52,000) are dropped in columns(4) and (5). Standard errors in columns (2)-(5) are corrected for arbitrary correlation within state of birth-year of birth, are in parentheses. (Numbers with *** indicate significance at the 1-percent level, while ** indicates significant at the 5-percent level.)

	BGM Original Results	Census Estimates			
	Savings Percentile	Any Investment Income		Inv. Income	Inv. Income Percentile
Estimation Technique	OLS	OLS	Probit	OLS	OLS
	(1)	(2)	(3)	(4)	(5)
State ever imposed mandate	-1.25	-1.90 ***	-0.07 ***	-122.93 ***	-1.79 ***
	(1.59)	(0.38)	(0.02)	(22.50)	(0.28)
Years since mandate	0.80 **	0.33 ***	0.01 ***	18.95 ***	0.34 ***
	(0.35)	(0.06)	(0.00)	(3.77)	(0.05)
Married	3.18 **	-0.83 ***	-0.02 ***	-88.77 ***	4.76 ***
	(1.40)	(0.06)	(0.00)	(11.19)	(0.07)
College Educated	9.28 ***	12.38 ***	0.50 ***	930.95 ***	10.71 ***
	(1.36)	(0.09)	(0.00)	(21.27)	(0.08)
Age	0.17	0.62 ***	0.02 ***	66.70 ***	0.59 ***
	(0.12)	(0.02)	(0.00)	(2.53)	(0.02)
Total Earnings/10^5	5.65 ***	37.00 ***	1.17 ***	2139.50 ***	-4.61 ***
	(1.04)	(0.16)	(0.01)	(33.84)	(0.31)
R^2		0.11		0.01	0.02
N	1,869	3,601,777	3,601,777	3,579,595	3,601,777
Mean of Dependent Variable		22.13	22.13	1199.00	2832.58

Table V

Estimates of the Effect of Financial Literacy Education

This table describes the evolution of financial market participation prior to, and following, the imposition of mandated financial education in high schools. The sample comprises individuals from the 5% sample of the 2000 Census, aged 35 to 54. Top-coded individuals (those with income from investments greater than $50,000 or retirement income greater than $52,000) are dropped in columns (2) and (3). The dependent variable in column (1) is a dummy for whether the household reported any investment income; in column (2), it is the amount of investment income received; and in column (3) it is the individual's percentile ranking in the nationwide investment income distribution. Additional controls include sex and marital status, year of birth dummies, educational attainment dummies, and a cubic control for income. The final two lines of the table test whether the average value of the dummy variables indicating the years four, three, two, and one year prior to the imposition of mandates are equal to those indicating the years one, two, three and four years following the mandate. Standard errors are corrected for arbitrary correlation within state of birth-year of birth, are in parentheses.

(Numbers with *** indicate significance at the 1-percent level, ** indicates significant at the 5-percent level and * indicates significance at the 10-percent level.)

	Any Investment Income (1)		Investment Income (2)		Investment Income Percentile (3)	
5 Years Prior	1.15	***	152.17	*	1.00	***
	(0.28)		(77.81)		(0.27)	
4 Years Prior	1.01	***	90.92	**	0.78	***
	(0.33)		(44.79)		(0.29)	
3 Years Prior	1.12	***	68.72		0.80	***
	(0.14)		(55.00)		(0.14)	
2 Years Prior	1.16	***	111.13	**	0.68	**
	(0.23)		(49.35)		(0.27)	
1 Year Prior	1.18	***	53.04		0.84	***
	(0.19)		(38.55)		(0.20)	
First Affected	1.13	***	116.26	***	0.63	***
	(0.17)		(33.94)		(0.20)	
1 Year Post	1.19	***	151.74	***	0.63	***
	(0.20)		(34.70)		(0.19)	
2 Years Post	0.95	***	115.47	***	0.41	*
	(0.22)		(44.35)		(0.21)	
3 Years Post	1.34	***	161.27	***	0.89	***
	(0.34)		(43.89)		(0.28)	
4 Years Post	1.11	***	246.64	***	0.60	**
	(0.26)		(46.04)		(0.27)	
5 Years or More	0.29		68.10		0.00	
	(0.30)		(50.09)		(0.28)	
N	3,579,595		3,579,595		3,579,595	
Test of Prior vs. Post						
F-Statistic	0.04		9.51		0.91	
p-value	0.85		0.00		0.34	

Table VI

State GDP Growth and Passage of Financial Literacy Education Requirements

This table describes the evolution of state GDP growth prior to, and following, the imposition of mandated financial education in high schools. The dependent variable in each column is 100*(log(gdpt/gdpt-1)). The independent variables of interest are event-window dummies indicating years before and years following the imposition of state-mandated high school financial education. Standard errors are clustered at the state level. Columns (2)-(5) include state fixed effects. Column (3) includes a linear time trend, Column (4) a cubic polynomial, and Column (5) year fixed effects.

	Log GDP Growth (1)	Log GDP Growth (2)	Log GDP Growth (3)	Log GDP Growth (4)	Log GDP Growth (5)
5 Years Prior	0.480 ***	0.394 ***	0.433 ***	0.290 ***	0.207 *
	(0.105)	(0.102)	(0.108)	(0.096)	(0.109)
4 Years Prior	0.240 **	0.168 *	0.196 *	0.083	0.092
	(0.112)	(0.098)	(0.119)	(0.096)	(0.099)
3 Years Prior	0.208 **	0.136	0.175 *	0.041	0.007
	(0.100)	(0.095)	(0.105)	(0.091)	(0.063)
2 Years Prior	0.186 *	0.114	0.165 *	0.015	-0.062
	(0.096)	(0.096)	(0.098)	(0.092)	(0.072)
1 Year Prior	0.206 **	0.134	0.197 **	0.037	-0.073
	(0.090)	(0.091)	(0.097)	(0.085)	(0.066)
Law Passed	0.196	0.124	0.198	0.033	-0.070
	(0.128)	(0.135)	(0.131)	(0.126)	(0.080)
1 Year Post	0.242 **	0.170	0.256 **	0.091	0.067
	(0.105)	(0.115)	(0.111)	(0.116)	(0.144)
2 Years Post	0.274 **	0.202	0.299 **	0.140	0.072
	(0.122)	(0.123)	(0.131)	(0.127)	(0.101)
3 Years Post	0.107	0.035	0.144	-0.005	-0.028
	(0.125)	(0.128)	(0.124)	(0.112)	(0.088)
4 Years Post	-0.124	-0.196	-0.075	-0.207 *	-0.105
	(0.118)	(0.123)	(0.113)	(0.108)	(0.078)
5 Years or More	-0.205 ***	-0.265 ***	-0.101	-0.059	-0.062
	(0.059)	(0.061)	(0.063)	(0.057)	(0.057)
R2	0.05	0.14	0.08	0.27	0.49
N	1,296	1,296	1,296	1,296	1,296
P-value from F-Test of:					
5 Years Prior - 1 Year Prior	0.00	0.00	0.00	0.02	0.20
1 Year Post - 5+ Years Post	0.03	0.00	0.14	0.32	0.51
All Treatment Dummies	0.00	0.00	0.00	0.00	0.00
State Fixed Effects	No	Yes	Yes	Yes	Yes
Year Controls	None	None	Linear	Cubic	Fixed Effects

Table VII
Estimates of the Effect of Knowledge and Ability on Savings, NLSY

Data are from the National Longitudinal Survey of Youth. Panel A reports whether the individual has any money left at the end of the month (column 1), or in any of the listed assets (columns 2-8). Panel B gives the amount in dollars, and Panel C the individual's location in the distribution. Cognitive ability (knowledge and ability) is measured by tests given around age 17. Additional controls included are sibling-pair fixed-effects, family income in every year with dummies proxying for missing data and fixed effects for age, gender, survey year, birth order and birth year. Standard errors are clustered at the individual level.

Dependent Variable: Any $ in Asset	Money Left	Stocks, Bonds & Mutual Funds	Savings	IRAs & Keogh	Tax-Deferred Accounts	CDs, Loans, Mortgage Assets	Rights to Estate, Investment Trust	Income from Other Sources (Interest, Rent, Dividends, etc.)
Years	1990 - 2004	1988 - 2000	1985 - 2000	1994 - 2000	1994 - 2000	1994 - 2000	1988 - 2000	1988 - 2000
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Any Investment Income								
Knowledge	2.559 **	3.390 ***	1.073	1.167	0.538	-0.002	-0.610	5.317 ***
	(1.287)	(1.134)	(0.935)	(1.515)	(1.709)	(0.844)	(0.424)	(0.907)
Ability	3.632 ***	1.832 *	5.183 ***	1.926	5.333 ***	1.180 *	0.115	4.092 ***
	(1.226)	(0.942)	(0.938)	(1.360)	(1.512)	(0.706)	(0.419)	(0.844)
Years of Education	0.173	1.508 ***	1.653 ***	1.055 **	1.262 **	0.290	-0.111	1.477 ***
	(0.372)	(0.329)	(0.256)	(0.468)	(0.512)	(0.232)	(0.140)	(0.225)
Num of Observations	25993	34663	44006	14220	14195	14239	34696	76372
R-Squared	0.38	0.33	0.42	0.42	0.39	0.24	0.22	0.39
F-stat of Knowledge & Ability	0.00	0.00	0.00	0.07	0.00	0.10	0.28	0.00
Panel B: Amount								
Knowledge	2394.04	674.29	127.19	736.83	1936.09	-590.09	-326.16	30.58
	(4551.32)	(607.41)	(660.78)	(835.33)	(1303.73)	(439.07)	(1002.24)	(56.95)
Ability	-430.40	342.06	1065.39 *	-457.13	1258.74	581.39	-464.75	-18.81
	(4047.50)	(473.52)	(564.26)	(751.17)	(1218.08)	(449.94)	(778.82)	(45.02)
Years of Education	1568.23	314.63 *	300.68 *	598.55 **	630.97	36.03	-83.11	90.20 ***
	(1349.05)	(183.30)	(181.25)	(299.95)	(448.32)	(128.83)	(269.48)	(17.97)
Num of Observations	15089	33455	44432	13645	13174	14103	34118	74277
R-Squared	0.48	0.19	0.26	0.34	0.38	0.16	0.12	0.11
F-stat of Knowledge & Ability	0.82	0.06	0.02	0.68	0.01	0.36	0.58	0.86
Panel C: Percentile of Amount/Income in Distribution								
Knowledge	1.165	3.197 ***	0.256	1.236	1.023	0.018	-0.400	3.746 ***
	(1.092)	(0.912)	(0.657)	(1.297)	(1.365)	(0.739)	(0.345)	(0.700)
Ability	0.353	0.989	3.521 ***	0.803	2.595 **	0.608	0.012	2.412 ***
	(1.079)	(0.751)	(0.651)	(1.139)	(1.241)	(0.590)	(0.337)	(0.647)
Years of Education	-0.301	1.078 ***	1.048 ***	0.625	0.604	0.294	-0.058	1.151 ***
	(0.319)	(0.269)	(0.183)	(0.397)	(0.418)	(0.202)	(0.113)	(0.180)
Num of Observations	13181	26743	36344	10813	10472	11091	27132	60481
R-Squared	0.45	0.36	0.47	0.46	0.45	0.25	0.21	0.40
F-stat of Knowledge & Ability	0.29	0.00	0.00	0.20	0.01	0.41	0.37	0.00

Table VIII

2SLS Estimates of the Effect of Schooling on Possible Mechanisms from Various Sources

The sample comprises individuals reported in the 5% samples of the 1980, 1990, and 2000 census, except column (2) which only uses 1990 and 2000 data and column (5) which uses data only from 1980 due to changes in the variables reported. This sample contains only 18-75 year olds and includes both men and women. The independent variable, years of schooling, is instrumented using compulsory schooling laws. Other controls include fixed effects for gender, race, 3-year age groups, 10-year birth cohorts, state of birth, state of residence, census year and a cubic polynomial in earned income. Regressions also include state of residence fixed effects interacted with a dummy variable for being born in the South and turning age 14 in 1958 or later to account for the impact of Brown v. Board of Education for blacks. In column 1, the dependent variable is the share of individuals aged 65-70 in 1970 in the same occupation and state who report receiving retirement income. In column 2, the dependent variable is the share of neighbors in the current year aged 65 and above who report receiving retirement income other than Social Security.

Dependent Variable:	Share of Employees in Occupation with Pension	Share of Neighbors with Pension	Has a First Mortgage	Has a Second Mortgage	Moved in Past Five Years	
					City	House
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Any Investment Income						
Years of schooling	0.985 ***	0.921 ***	1.226	-2.687 ***	0.564	-1.818 ***
	(0.318)	(0.299)	(1.341)	(0.730)	(0.816)	(0.645)
Num of Observations	13,013,023	9,400,737	10,097,332	6,042,040	2,971,369	13,944,243
F-stat of excluded instruments	48.4	5.1	51.8	45.2	61.4	38.3

Table IX
Estimates of the Effect of Not Feeling in Control on Savings, NLSY

Data are from the National Longitudinal Survey of Youth. Panel A reports whether the individual has any money left at the end of the month (column 1), or in any of the listed assets (columns 2-8). Panel B gives the amount in dollars, and Panel C the individual's location in the distribution. The independent variable of interest is a measure of whether the respondent agrees with the statement "I have little control over the things that happen to me," with 1 indicating strong disagreement and 4 indicating strong agreement. Additional controls included are sibling-pair fixed-effects, family income in every year with dummies proxying for missing data and fixed effects for age, gender, survey year, birth order and birth year. Standard errors are clustered at the individual level.

Dependent Variable:	Money Left	Stocks, Bonds & Mutual Funds	Savings	IRAs & Keogh	Tax-Deferred Accounts	CDs, Loans, Mortgage Assets	Rights to Estate, Investment Trust	Income from Other Sources (Interest, Rent, Dividends, etc.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Any Money								
Little Control	-2.82 ***	0.01	-2.09 **	-1.20	-1.54	-0.26	0.10	-1.44 *
	(0.95)	(0.79)	(0.89)	(0.92)	(1.08)	(0.48)	(0.34)	(0.77)
Num of Observations	21,229	21,261	17,593	13,831	13,807	13,851	21,292	24,503
R-Squared	0.41	0.38	0.49	0.42	0.40	0.24	0.26	0.45
Panel B: Amount								
Little Control	-10323.00 ***	27.73	186.16	-221.19	-1276.97	3.88	-85.34	-14.40
	(3090.18)	(527.41)	(612.54)	(510.84)	(835.30)	(198.17)	(743.49)	(67.58)
Num of Observations	12,575	20,399	18,720	13,277	12,823	13,721	20,924	23,558
R-Squared	0.50	0.24	0.37	0.35	0.38	0.16	0.17	0.21
Panel C: Percentile of Amount/Income in Distribution								
Little Control	-1.35	-0.56	-1.71 ***	-1.28 *	-1.30	0.06	0.17	-1.69 ***
	(0.87)	(0.65)	(0.62)	(0.77)	(0.89)	(0.39)	(0.27)	(0.64)
Num of Observations	10,987	16,283	15,374	10,564	10,236	10,834	16,556	19,356
R-Squared	0.46	0.41	0.53	0.47	0.46	0.26	0.25	0.46

Appendix Table A1
Summary Statistics, Census

This table reports summary statistics from the 5% sample of the census, from 1980, 1990, and 2000, for individuals aged. We include individuals aged 36-75.

	Whole Sample	
	Mean	St. Dev.
	(1)	(2)
Demographic		
Years of schooling	13.19	2.62
High school graduation	0.85	0.36
Compulsory Attendence = 9	0.40	0.49
Compulsory Attendence = 11	0.11	0.31
Compulsory Attendence = 10	0.36	0.48
Age	48.89	13.05
Male	0.48	0.50
Black	0.10	0.30
Income from Investments		
Any	28.23	45.01
Amount	1302.94	4659.14
Percentile	25.92	39.12
Income from Retirement Savings		
Any	10.85	31.11
Amount	1278.63	4928.85
Percentile	11.10	30.30

Appendix Table A2

Summary Statistics, NLSY

Summary statistics from the National Longitudinal Survey of Youth. The survey was conducted annually from 1979-1984, and biennially since 1996. Column (1) indicates the years from which data are used.

Variable	Year(s)	Mean	St. Dev.	Min	Max
	(1)	(2)	(3)	(4)	(5)
Knowledge	1981	58.71	22.47	0	119
Ability	1981	117.82	40.74	0	213
Cognitive Ability	1981	36.81	28.19	1	99
Years of Education	1985 - 2004	12.94	2.33	0	20
Age (in year 1979)	1979	17.16	2.06	14	22
Male	1979	0.52	0.50	0	1
Total Net Family Income	1979 - 2004	51197.32	85407.59	0	1637987
Pearlin "Little Control Over Life"	1992	1.80	0.67	1	4
Any …					
Money Left	1990 - 2004	65.60	47.50	0	100
Savings	1985 - 2000	63.66	48.10	0	100
Stocks, Bonds & Mutual Funds	1988 - 2000	16.16	36.81	0	100
IRAs & Keogh	1994 - 2000	19.48	39.60	0	100
Tax-Deferred Accounts	1994 - 2000	32.51	46.84	0	100
CDs, Loans, Mortgage Assets	1994 - 2000	4.96	21.72	0	100
Rights to Estate, Investment Trust	1988 - 2000	3.22	17.65	0	100
Income from Other Sources	1979 - 2002	29.90	45.78	0	100
Amount …					
Money Left	1990 - 2004	65147	103348	1	989100
Savings	1985 - 2000	6699	24144	0	835000
Stocks, Bonds & Mutual Funds	1988 - 2000	2556	19379	0	989100
IRAs & Keogh	1994 - 2000	4671	23928	0	549500
Tax-Deferred Accounts	1994 - 2000	9513	33984	0	549500
CDs, Loans, Mortgage Assets	1994 - 2000	1014	11397	0	549500
Rights to Estate, Investment Trust	1988 - 2000	2236	40288	0	3114800
Income from Other Sources	1979 - 2002	500	3373	0	168780
Percentile …					
Money Left	1990 - 2004	42	25	0	89
Savings	1985 - 2000	37	29	0	87
Stocks, Bonds & Mutual Funds	1988 - 2000	11	27	0	82
IRAs & Keogh	1994 - 2000	13	28	0	81
Tax-Deferred Accounts	1994 - 2000	21	32	0	81
CDs, Loans, Mortgage Assets	1994 - 2000	3	16	0	80
Rights to Estate, Investment Trust	1988 - 2000	2	12	0	82
Income from Other Sources	1979 - 2002	21	32	0	86

Appendix Table A3

Comparison of Data from 2001 SCF and 2000 Census

Note: This table compares the means, standard deviations, and percentiles for the key variables, using data from both the Census and the Survey of Consumer Finances. The Census data are from the 2000 census, while the SCF data are from the 2001 survey of consumer finances. The sample for investment income vairables in both surveys is adults aged 36-75 who report investment income below $50,000. For retirement income, the sample is individuals aged 50 to 75 who report retirement income less than or equal to $52,000. N indicates the number of unique individuals used to estimate numbers; for the SCF, appropriate weights were used.

	Investment Income		**Any Investment Income**		**Retirement Income**		**Any Retirement Income**	
	SCF	Census	SCF	Census	SCF	Census	SCF	Census
Mean	1515	1264	0.33	0.27	4114	2866	0.27	0.22
Standard Deviation	5089	4543	0.47	0.45	9545	7477	0.44	0.41
Min	0	0	0	0	0	0	0	0
Max	49800	49900	1	1	50400	51000	1	1
Percentiles								
1%	0	0	0	0	0	0	0	0
10%	0	0	0	0	0	0	0	0
25%	0	0	0	0	0	0	0	0
Median	0	0	0	0	0	0	0	0
75%	200	50	1	1	1320	0	1	0
90%	3500	2900	1	1	15600	11165	1	1
99%	29000	25000	1	1	43200	36539	1	1
N	2,720	5,427,616	2,735	5,450,827	1,739	2,883,474	1,739	2,883,474

Appendix Table A4

Estimates of the Effect of Education on Cognitive Ability, Knowledge, Ability and Being in Control, NLSY

This table uses variation within families to estimate the relationship between educational attainment and cognitive ability, and the relationship between feeling in control and cognitive ability and education. Data are from the National Longitudinal Survey of Youth. Cognitive ability is measured using the AFQT test but is also decomposed into knowledge and innate ability. The dependent variable in column (4) is a measure of whether the respondent agrees with the statement "I have little control over the things that happen to me," with 1 indicating strong disagreement and 4 indicating strong agreement. Other controls include sibling-pair fixed-effects, log family income in every year with dummies proxying for missing data and fixed effects for age, gender, race, survey year, birth order and birth year. Standard errors are clustered at the individual level.

Dependent Variable:	Cognitive Ability (AFQT)	Cognitive Ability (Knowledge)	Cognitive Ability (Ability)	LittleControl
Year	1981	1981	1981	1993
	(1)	(2)	(3)	(4)
Panel A:				
Years of Education	5.193 ***	4.312 ***	6.428 ***	-0.033 ***
	(0.303)	(0.264)	(0.435)	(0.010)
Num of Observations	4594	4604	4604	3807
R-Squared	0.9	0.9	0.8	0.5
Panel B:				
Cognitive Ability				-0.0017 *
				(0.0010)
Num of Observations				3682
R-Squared				0.5

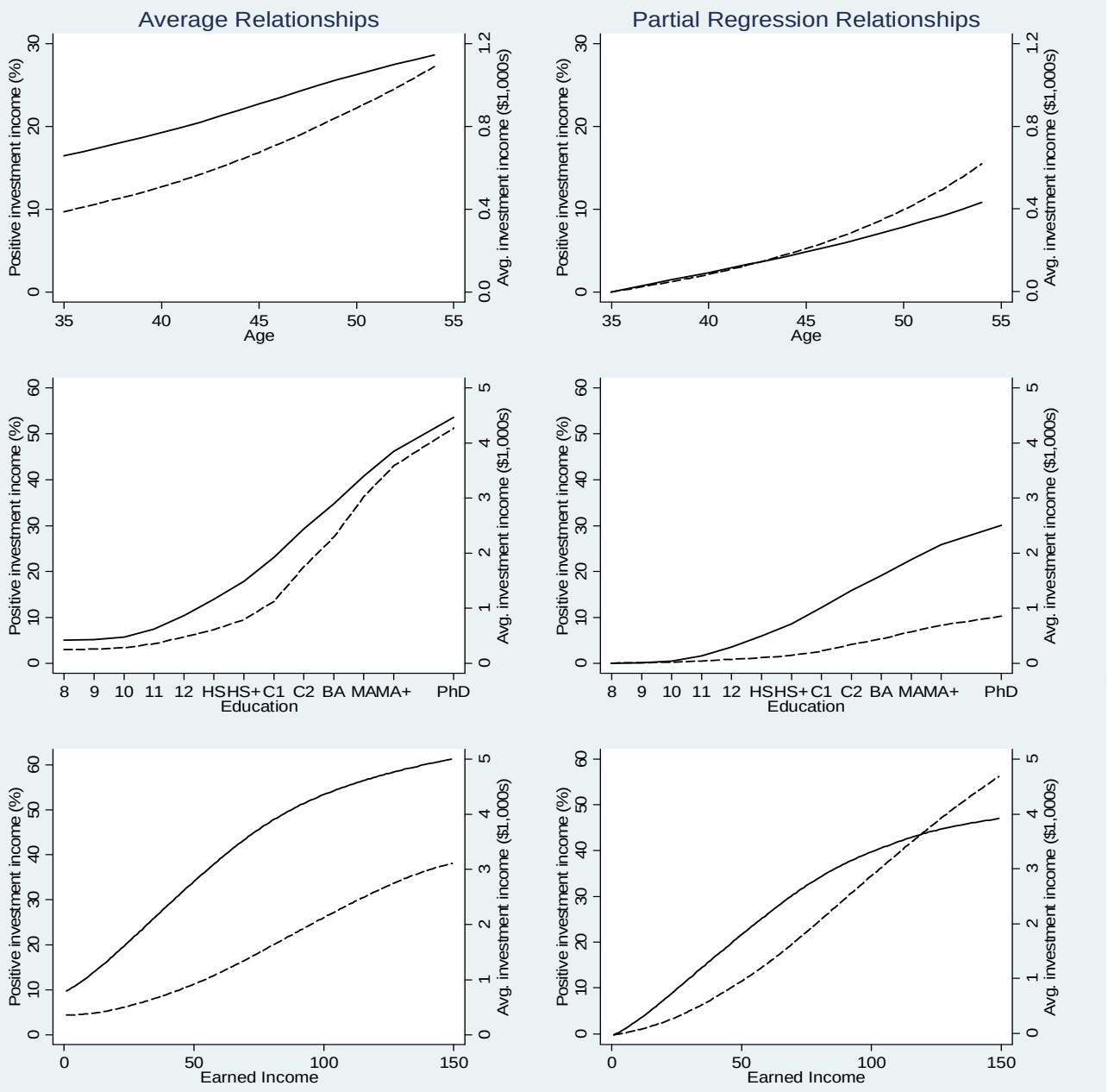

Figure 1. Non-Parametric Relationships and Partial Regressions.

The figures in the left column give the unconditional average (smoothed using Fan's local linear regression) for average investment income (dashed line corresponding to the right-hand vertical axis) and the share of the population with investment income (solid line corresponding to the left-hand vertical axis). In the right column the lines give the (smoothed) estimated coefficients from a regression of investment income on age, education and income dummies. The top-right panel gives therefore the non-parametric relationship between investment income and age, controlling for earned income and educational attainment. Data are from the 2000 US census, restricted to households with less than $50,000 in investment income. The sample size is approximately 3,200,000.

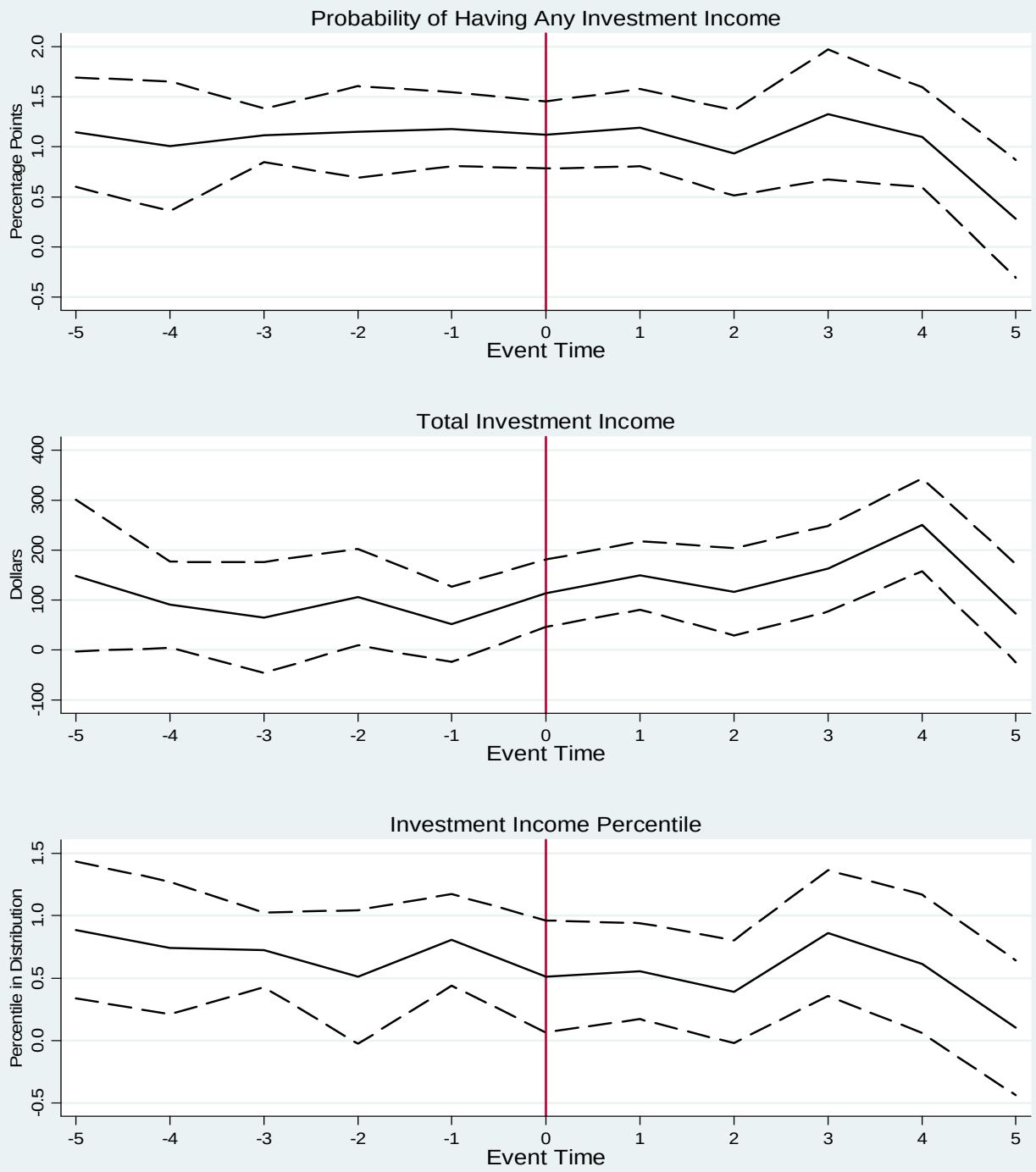

Figure 2. Financial Literacy Education and Saving.

This figure plots the evolution of three data series prior to, and following, the imposition of state-mandated financial literacy programs. The dotted lines show confidence intervals at the 5% level. The vertical line at 0 indicates the year in which the financial literacy program was mandated.

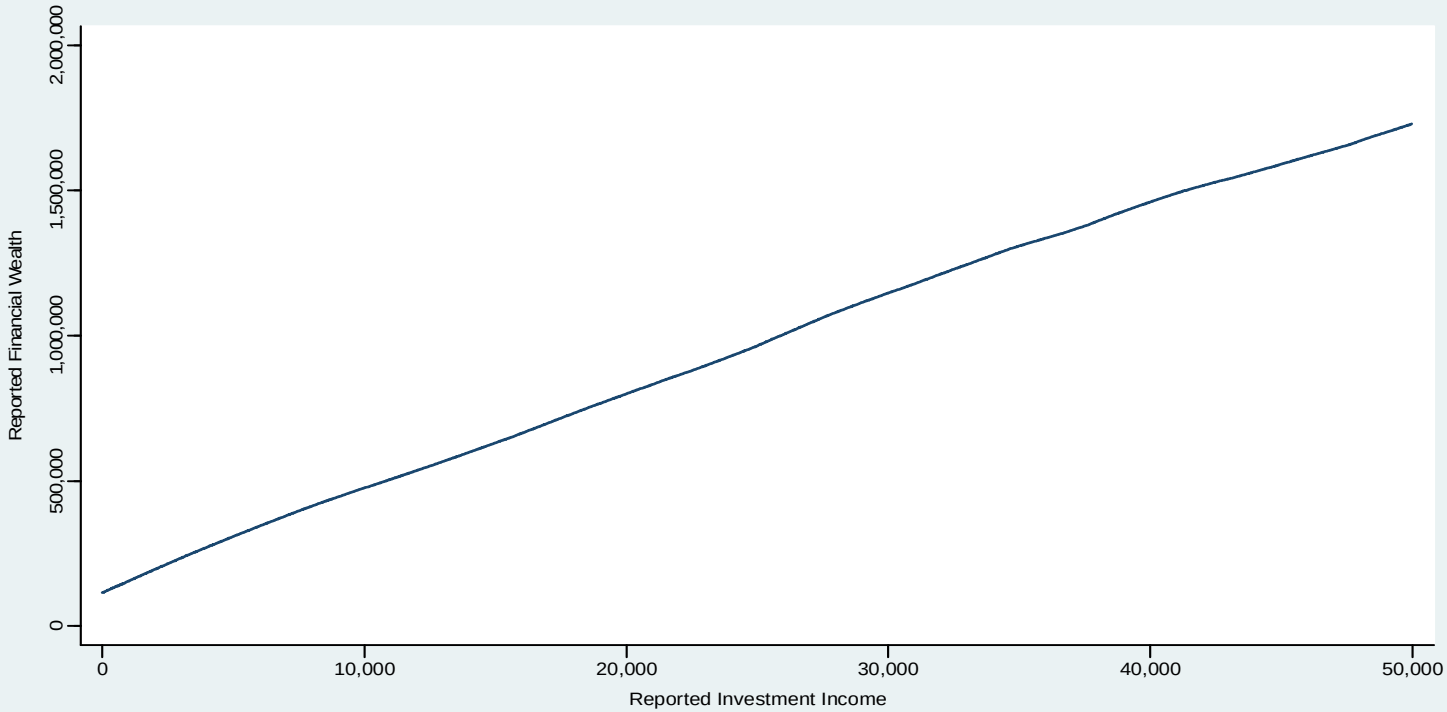

Figure A1. Relationship between Reported Investment Income and Reported Financial Wealth.

This figure presents a local linear regression of the relationship between reported investment income and reported financial wealth, for households with reported investment income between $0 and $50,000. Data are from the 2001 Survey of Consumer Finances.