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

What determines whether an individual participates in financial markets? In particular, are those with more education, greater exposure to financial topics or higher cognitive ability more likely to invest in financial instruments? This is a difficult question to answer, as each of these three factors is closely correlated with a host of other individual characteristics, such as parental income and ability, which may independently affect investment decisions. We use instrumental variables and panel regression techniques to overcome this identification challenge. To study the effect of general education, we make use of changes in compulsory education laws, which induce exogenous variation in schooling. To study financial literacy education in schools, we use cohort analysis and state laws mandating such education. Finally, we study cognitive ability by focusing on sibling pairs that grew up in the same household, therefore controlling for unobserved family characteristics. We find that greater cognitive ability and educational attainment lead to significant increases in financial market participation. However, and in contrast to previous findings, we find no evidence that high school financial literacy education affects savings or investment decisions.

1 Introduction

Individuals are increasingly making complex financial decisions. The shift from defined benefit to defined contribution pension plans, as well as the growth in importance of private retirement accounts, require individuals to choose the amount they save, as well as the mix of assets in which to invest. Yet, numerous surveys show that a large fraction of households have only

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a rudimentary understanding of basic financial concepts. Moreover, participation in financial markets is far from universal in the United States, and individuals with low levels of education and financial literacy are the least likely to participate in financial markets. These correlations have motivated policy makers to devote substantial resources to financial literacy education, including outreach programs for adults. Fourteen states require high school students to take a course on financial literacy, while many other high schools offer optional courses.

What is not clear, however, is whether these correlations warrant causal interpretations. For example, individuals with low levels of education and financial literacy are also likely to have low levels of income and wealth. For these individuals, the financial rewards of participating in the stock market may not justify the fixed costs of participation. In this paper, we measure the causal effects of education, financial literacy, and cognitive ability on financial market participation. we find compelling evidence that education increases participation: one additional year of schooling increases the probability of financial market participation by 3-4%, for whites and blacks. However, and in contrast to previous work, we find no evidence that high-school financial literacy education affects financial market participation. We also find that participation, and net worth, increase with cognitive ability.

This paper advances our understanding of financial market participation in two ways. First, we introduce two datasets which, while not new, have not previously been used to answer these questions. One is the U.S. census, which contains a much larger sample than has ever been used to study these questions. The other is the National Longitudinal Survey of Youth, which includes detailed information on siblings' financial market participation decisions.

Our second major contribution is to overcome the standard identification problem, and provide precise, causal estimates of determinants of financial market participation. We start by examining the effect of general education on participation, making use of changes in state compulsory education laws, which induced exogenous variation in schooling in the U.S. population. We then examine a specific type of education, financial literacy education in schools, using cohort analysis in a natural experiment identified by Bernheim, Garrett, and Maki (2003). Finally, we study the relationship between raw cognitive ability and financial market participation, exploiting within-sibling group variation in cognitive ability, therefore controlling for unobserved background and family characteristics.

This paper adds to a growing literature on the correlates and determinants of financial participation. The level of 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 (Mankiw and Zeldes, 1991). Participation may also affect society: individual participation in financial markets may affect attitudes towards taxation of financial income.

Yet, participation in financial markets is far from universal: the 2004 Survey of Consumer Finances indicates 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, 2004). Some view 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 (2007) 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 (2003), 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, and analyzes patterns in financial participation. Sections 3, 4, and 5 examine how financial market participation is affected by education, financial literacy education, and cognitive ability, respectively. Section 6 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 consists of approximately 14 million observations.

While the census does not collect any information on financial wealth, it does ask detailed questions on household income. The main measure of financial market participation will be "income from interest, dividends, net rental income, royalty income, or income from estates and trusts," which we will term "investment income." While this is not a perfect measure of financial market participation, it would include income from most financial instruments. Households are instructed to "report even small amounts credited to an account." (Ruggles et al., 2004). A second type of income of interest is "retirement, survivor, or disability pensions," which we term "retirement income." This is distinct from Social Security and Supplemental Security Income, both of which are reported on separate lines.

A significant limitation of using measurements of investment income, rather than levels of investment, is that the level of investment need not be monotonically related to the level of income. An individual with $10,000 in bonds may well report more investment income than a household with $30,000 in equity. This limitation 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, 2004), 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.

The level of income from financial assets conveys information, and we therefore report results for both the level of investment income (in 2000 dollars), and the relative importance of investment income to the household. 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 tease out causal effects.

2.2 Patterns of Participation

Correlates of participation in financial markets are well understood. Campbell (2006b) provides a careful, recent review of this literature. Previous work has demonstrated that participation is increasing in income and 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, 2005), and trust (Guiso, Sapienza, and Zingales, 2007).

In this section, we explore the link between financial market participation, income, education, and race, using non-parametric analysis of data from the census. 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, rather than the one specified by the econometrician. 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.

Simple means are plotted in Figure 1. In each graph, the solid line gives the unconditional average value for the population as a whole, while the dotted line (typically above the solid line) gives values for whites, and the dashed line gives values for blacks. The left column's panels give the percentage of individuals reporting investment income, while the right panels give the average value of investment income. Throughout the paper, households with no reported investment income are included in the data, including calculations of average investment income.

Participation in financial markets increases strongly with age. Approximately 17 percent of individuals report positive investment income at the age of 35. This number grows steadily, reaching a peak of 28 percent for individuals aged 55. Average investment income also increases nearly linearly with age.

The share of households reporting investment income increases steadily with education. The second pair of panels give figures for 13 levels of educational attainment[1]. Investment income is increasing in education, but the relationship is substantially steeper for whites than blacks. The

[1] 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 (C1), associate degree-occupational program (C2), associate degree-academic program (C3), bachelor's (B.A.), master's (M.A.), professional degree (M.A.+), and doctorate (Ph.D.).

only exception is Ph.Ds, who earn less investment income than those with professional degrees (though they are more likely to save).

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 above $150,000. Average investment income increases nearly linearly with earned income: this result would obtain if savings rates (and returns to investment) did not vary with income.

The non-linear relationships in Figure 1 suggest that linear regression may not correctly tease out the relative importance of age, education, and income. Taking advantage of the large sample available in the census, we instead estimate non-parametric partial correlations, 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 coefficients (smoothed using a local linear regression) are graphed in Figure 2, with a dotted line for white households, and a solid line for black households. The partial correlations yield similar results. Holding constant any two of three factors (age, education, and income), participation and investment income increase as the third factor grows. Nearly all observed relationships are significantly flatter, as would be expected, when controlling for the other two factors.

The slope between investment and age is still linear; white households aged 55 are 11 percent more likely to report savings than those aged 35. The difference for black households is only four percent. The striking differences in investment returns to education between blacks and whites persist.

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. Mendell (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 (2005) 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.

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 a policy experiment identified by Acemoglu and Angrist (2000) to measure the impact of education on compulsory education.

We use changes in state compulsory education laws between 1914 and 1978. In particular, 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,

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

where s_i is years of education for individual i, and X_i is a set of controls, including age, gender, state of birth, state of residence, and cohort of birth fixed effects. 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 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. In addition, we drop observations that were top-coded by the survey; these individuals reported greater than \$50,000 (\$40,000) for investment income and \$52,000 (\$30,000) for retirement income in 2000 (1990).

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 Lochner and Moretti 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

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

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, 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.

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 censuses; data is available from 1980-2005. We describe results from pooled data from 1990 and 2000, but they are robust to including other years. 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. Since we have compulsory schooling laws up until 1978, individuals in our sample are aged 26 - 75. 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.

The censuses do not code a continuous measure of years of schooling, but rather identify 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 matter for our estimates since individuals who fall within the ranges of grades 1-8 and 1-3 years of college will not be much affected by the compulsory schooling laws.

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.[2] The sample size varies between 475,000 and 10 million observations, depending on the sample (race and age) and dependent variable. The OLS results are as expected and highly significant. Education increases the likelihood that an individual has any level of assets, as indicated by a non-zero income from

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

investments and retirement income. An additional year of schooling increases the probability of having any investment income by 4.4% for whites and 1.7% for blacks. For retirement income, this estimate is 1.2% for whites and 1.7% for blacks. In addition, schooling increases the dollar amount of investment (by \$50-\$300) and retirement income (by about \$360) for both blacks and whites and increases an individual's location in the distribution of income for both asset classes. However, 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.

We first present evidence that compulsory schooling laws increase human capital accumulation, the first stage of our two state least squares (2SLS) estimation strategy. 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. An 9th year of mandated schooling increases years of completed education by 0.1 years for whites and almost 0.3 years for blacks. Relative to 8 or fewer years, requiring 10 years of education increases years of schooling by 0.13 years for whites and 0.3 for blacks, while requiring 11 years of education causes whites to get 0.23 years more schooling and blacks to get 0.31 more years. In fact, forcing these students to remain in school for even one more year (9 years of required schooling) increases the probability of graduating high school by 2% for whites and 3.6% for blacks. The average years of schooling and the share of high school graduates are monotonic in the required number of years for whites, although the average years of schooling deviates slightly in the case of blacks. These estimates are similar to those in Table 4 of LM's work.[3]

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 3.9% for whites and 3% for blacks. For retirement investments, an additional year of schooling increases the probability of non-zero income by about 6.5% for whites and 7.9% for blacks. These estimates are somewhat

[3] '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 12.7 to 26.5, well above the critical values proposed by Stock and Yogo (2003).

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 income for both whites and blacks. The magnitudes are quite large, substantially larger than the OLS estimates; an additional year of schooling increases investment income and retirement income for whites by $1605 and $1697 respectively and for blacks by $178 and $1280 respectively. 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.[4] The striking fact is that no matter how many income controls we include, we find persistent differences in participation by education.

The magnitude of the dollar effects warrants discussion. They are larger than siblings fixed-effects estimates reported below. This may be because education is endogenously determined within sibling pairs, but is also likely due to the differences in ages of the sample. To get a sense of whether these point estimates are realistic, 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,[5] saved a constant 10% of his income at the end of each year and earned a 5% return on his assets. We assume a return to one year of schooling of 7%, estimated by Acemoglu and Angrist (2000) using this same identification strategy. Even if our 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.[6]

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

[4] Results are not shown, but available on request.

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

[6] $140 \cdot \frac{(1-\exp(0.05\cdot(49-20)))}{(1-\exp(0.05))} \approx \$9000$. A 5% return would yield approximately $450.

$22,500 greater asset base by age 49, and a corresponding increase in income of $1,200.[7] The high level of the estimated coefficients suggest that education likely affected the savings rate, as well as the level of income. Finally, it is also possible that education affects the choice of asset allocation, and better educated individuals choose portfolios that yield higher returns.

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 (forthcoming), 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 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 much, and how, to save for retirement.

There is some evidence suggesting that financial literacy education can affect both levels of financial literacy and financial behavior.[8] 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

[7] 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.

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

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.[9] 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 of exposure to financial literacy training, and savings behavior. They find that the mandates were 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.

[9] 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.

4.1 Bernheim, Garret, and Maki Replication

The main results from Bernheim et. al. are reproduced in columns (1) and (2) of Table 4. BGM estimate the following equation:

$$y_i = \alpha_0 + \beta_0 * Treat_s + \beta_1 * (MandYears) + \beta_2 * Married + \beta_3 * College + \beta_4 * Age + \beta_5 * Earnings + \varepsilon_i \quad (3)$$

where Treat_s is a dummy for whether state s was ever treated, 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, using a median regression to limit the influence of outliers. Column (2) reports the percentile rank of the household's savings rate, compared to peers, again 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 in both specifications, suggesting that exposure to financial literacy education leads to an increased savings rate. Graduating five years following the mandate would lead to an increase in savings rate of 1.5 percentage points. 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 (3) - (6) we replicate BGM's results, estimating equation (3) 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. 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.[10] 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 birthyear-state level.

[10] 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 strikingly similar results.

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 with the census. Instead, we focus on reported income from savings and investments, which should be informative of the level of assets held by the household: income earned from investments, dividends, and rental payments.

Columns (3) and (4) Table in 5 present estimation of equation (3) using "any investment income", a dummy equal to 1 if the household reports any income from investment or savings, as the dependent variable. Column (3) estimates a linear regression model, while column (4) 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 coefficient, .33, suggests that each year the mandate had been in effect raised the share of households reporting savings income by .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 .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 .37 percentage points, evaluated at the mean dependent variables.

Column (5) estimates equation 3 using the dollar value of investment income as the dependent variable. This regression suggests that an additional year of mandate exposure increases savings income by approximately $18. 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 household 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 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, rather than simply including the regressor 'number of years since mandate,' we "dummy out" the pre and post period.

To do this, we define a set of 11 dummy variables, $D_{isb}^{-5}, D_{isb}^{-4}$,..$D_{isb}^{0}, D_{isb}^{1}, ..., D_{isb}^{4}, D_{isb}^{5plus}$. The variable D_{isb}^{0} is set to one if an individual i in state s, born in year b, in the first cohort in her or his state to be exposed to the mandate, and zero otherwise. Similarly, D_{isb}^{k}, for k=1,...,4, indicate that the individual graduated from high school k years after the mandate originally went into effect. D_{isb}^{-1} is set to 1 if an individual is in the oldest cohort to graduate from high school in a state, s, that was affected by the mandate, and zero otherwise (e.g., $\mathrm{D}_{isb}^{-1} = 1$ for the cohort graduating in New Mexico in 1978, one year prior to the 1979 mandate. $\mathrm{D}_{isb}^{-1} = 0$ for all cohorts in Massachusetts, as the state did not pass any mandates.) Finally, 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)"[11]. We thus estimate the following equation:

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

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

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 birthyear-state 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: prior to the imposition of the mandates, savings behavior was not trending up or down in states in which the mandate was imposed. (Because there are state fixed-effects, D_{isb}^k do not compare cohorts in treated states to cohorts in comparison states, but rather measure differences in savings behavior for cohorts within the state.) If, as BGM find, the imposition of financial literacy education led to increased savings, we should expect the coefficients on D_{isb}^0,...D_{isb}^4 and D_{isb}^{5p} to start out small (perhaps indistinguishable from zero), but increase over time, and be positive and statistically significant for higher values of k.

4.2.2 Results

Results are presented in Tables 5 and 6. Table 5 presents results from model 4. 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[12] on the left-hand side, and column (3) uses the individuals location in the nationwide distribution of investment income to total income as the dependent variable.

The information from the tables is perhaps best conveyed graphically, in Figure 3. Each

[12] 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.

γ_k coefficient is plotted, 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 do not confirm the findings of BGM. Consider the results for the dependent variable "any investment income." Individuals born after the mandates went into effect are substantially more likely to report investment income, relative to those born well before the mandates were imposed: the point estimates hover around one, meaning the share of individuals with financial income is two percent higher in the treated states. However, the γ_k for values k<0 are just as high, suggesting that this increase began prior to the implementation of the mandates. Even cohorts born five years prior to the imposition of the mandates are more likely to report investment income than the comparison group.

This suggests that the mandates were imposed in states during a period in which financial market participation was high. We evaluate this more formally by testing the hypothesis that $\frac{1}{4}\left(\gamma_{-4}+\gamma_{-3}+\gamma_{-2}+\gamma_{-1}\right)=\frac{1}{4}\left(\gamma_1+\gamma_2+\gamma_3+\gamma_4\right)$. That is, we test whether the average participation or income for the cohorts graduating four years prior to the mandates is different than those graduating in the four years following the mandates. 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. reveals while the mean value for γ_k for k$\in\{1,2,3,4\}$ is 1.15. 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.

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 nearly identical to those for total 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 4 using only data from individuals who report a maximum educational attainment of 11^{th} or 12^{th} grade, or some college. Results are reported in Table 5. The estimated coefficients 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 above historic levels prior to the imposition of mandates, and no 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.[13] We estimate equation an equation very similar to 4:

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

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

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

the imposition of mandates, growth was on average .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, 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). Th evidence therefore suggests that both cross-sectional and panel estimates should be treated with caution.

5 Cognitive Ability and Savings

5.1 Empirical Strategy

Education is likely to be related to cognitive ability, which may also affect financial market participation. Financial decisions are often complicated. The household mortgage decision is tremendously important for the average household, yet it was only six years ago that Agarwal, Driscoll, and Laibson (2001) report providing "the first analytically tractable model of optimal mortgage refinancing." 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 (2002) 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.

Only one study, to our knowledge, links actual measures of cognitive ability to investment decisions. Christelis, Jappelli, and Padula (2007) 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[14]. 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 may be biased upwards[15].

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

[14]For example, the correlation between parental IQ and children reared apart is approximately .24, providing strong evidence that genes influence IQ. Similarly, the correlation between two unrelated individuals (at least one adopted) raised in the same household is approximately .25.

[15]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.

ability is thus attributable to the random allocation of genes to each particular child[16].

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. Nevertheless, comparing siblings is still a useful exercise since it provides, at the very least, an upper bound to estimates of the effect of cognitive ability.

Benjamin and Shapiro (2006) employ this to study how cognitive ability 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 two directions. First, we look at a range of financial assets. Second, we consider both the extensive and intensive margins. 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 calculated an estimate of the respondent's percentile score in the Armed Forces Qualifying Test (AFQT). Further details are provided in the data appendix. Using this score as a measure of cognitive ability, we estimate the effect of cognitive ability and education on financial decision making with the following equation

$$y_{it} = \beta ability_i + \delta education_{it} + \gamma X_{it} + SG_i + \varepsilon_{it} \tag{6}$$

where $ability_i$ is the measure of cognitive ability, $education_{it}$ is the highest grade individual i has completed by year t and X_{it} includes age, gender and survey year effects, and SG_i are sibling-group fixed effects. Standard errors are corrected for intracluster correlation within individual. Following Benjamin and Shapiro (hereafter, BS), we proxy for permanent income by controlling for the log of family income[17] in every available survey year from 1979 to 2002 and including

[16] 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.

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

dummy variables for missing data. Our specification differs from BS only in that we control for education. The sample is large enough to run these regressions by race. We also drop all observations which are topcoded; the cut-off varies by year and outcome variable, but typically does not exclude many individuals. Finally, 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.[18]

5.2 Results

Results are presented in Tables 7 and 8 for whites and blacks, respectively. In both tables and 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 (as a percentage of total income, multiplied by 10,000). The first two columns in panel A replicate the results found in BS for whites. 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 for whites - an increase of one standard deviation in AFQT score (28% for whites, 18% for blacks) increases the propensity to have accumulated assets by about 5% for whites and 2% for blacks. Note that this result is after controlling for education. Education itself does not significantly increase asset accumulation for whites, but does for blacks by 1% per year of schooling. Respondents were then asked to estimate how much money would be left over - we find that cognitive ability has no 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 columns in Tables 7 and 8 examine stock market participation. The NLSY

[18] 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.

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 AFQT score increases the participation margin by 5% for whites and 3% for blacks. Education also has a strongly significant effect on stock market participation of about 1.35% per year of additional education, for both whites and blacks. Column (2) in panels B and C demonstrates that AFQT score is also significantly associated with how much money an individual has in stocks, bonds or mutual funds (increasing the amount by about $35 per standard deviation) and the individual's rank in the distribution of such assets.

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)?[19]" Cognitive ability increases an individual's propensity to save: one standard deviation in the AFQT increases the propensity to save by 3% for whites and almost 5% for blacks. Education increases non-zero savings for whites by 1.4% per year and for blacks by 2.4% per year. Cognitive ability increases the amount of savings for whites (by $50 per standard deviation) and for blacks (by $30). Education does not increase amount of savings for whites significantly but does for blacks (by $262 per year of schooling). Both cognitive ability and education increase an individual's position in the distribution of savings as a percent of income.

We find similar results when we focus on savings in IRAs and Keogh accounts (column (4)) and 401Ks and pre-tax annuities (column (5)). The estimates also tend to be larger for blacks than for whites. Cognitive ability increases the probability an individual has money in an IRA or Keogh account by 1.3% for whites and 2.3% for blacks per standard deviation while one

[19] 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.

year of schooling increases this probability by 1%. Cognitive ability increases participation in tax-deferred accounts such as 401Ks by 1.4% for whites and 6% for blacks. The effects are substantially smaller for certificates, loans and mortgage assets (column (6)), particularly for blacks.

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 for whites. In contrast, the results for blacks suggest some compensatory effect. While the probability of receiving an inheritance does not vary by cognitive ability, the amount respondents expect to receive is lower for those with lower scores. In contrast, those with higher education levels expect greater inheritances.

Finally, in column (8) we look at an outcome variable, classified as "other income" from 1979 to 2002. 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."[20] Cognitive ability and education have a significant effect on income from these sources: one standard deviation in AFQT score increases the probability of having any such income by about 5% and one year of schooling by 1.5% for both whites and blacks. Similarly, cognitive ability and education increase the individual's percentile ranking in such income and the amount earned.

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

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

6 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. Guiso, Sapienza, and Zingales (2007) find that levels of trust are correlated with stock market participation. Malmendier and Nagel (2007) find that individuals who have experienced higher stock market returns throughout their life are more likely to participate in financial markets.

We explore three important determinants of participation in financial markets, with a focus on discovering causal mechanisms. First, we find that education has important effects on investment income. Individuals with one more year of schooling are 3% more likely to report positive investment income. Similarly, those graduating from high school are significantly more likely to report income from retirement savings. Second, 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).

Finally, 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, IRAs, tax-deferred accounts, and CDs. The size of the effect is large: movement from the 25^{th} to 75^{th} percentile in cognitive ability is associated with a 10 percentage point increase in probability of owning stocks, bonds, or mutual funds for whites and 3.4 percentage point increase for blacks. Individuals with higher levels of cognitive ability also tend to hold more money in financial instruments.

Persistently lower participation rates among blacks than whites, even when one controls for differences in education, income, and financial literacy, have led some to explore whether

culture, or other mediating factors depress participation. However, in this paper, we show that participation among blacks responds to education and cognitive ability in similar ways. While the relationship between education and participation is steeper for whites than for blacks, schooling has a larger effect on retirement income for blacks than whites.

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 substantial effects of education, but the changes in education levels we observe are quite large. In contrast, most schools offer only a short-course covering basic topics of financial literacy. It may be that some programs are effective, or that programs are effective only if students have sufficient math skills. Clearly further research is needed: the best and most compelling evidence would come from randomized evaluations.

While we find that financial market participation is not affected by financial education, a body of evidence that suggests that household savings decisions are sensitive to even small perturbations. Beshears et al. (2007) provide evidence that defaults affect participation, savings, and allocation decisions. Finally, a pair of compelling randomized evaluations find that savings behavior can be affected by policy interventions. Duflo and Saez (2005) present evidence that minor incentives ($20 for university staff attending a benefits fair) can increase TDA participation rates by 1.25 percentage points. Duflo et al (2007) study the effect of major incentives on low-income households (a 20 or 50 percent contribution match for retirement accounts) and find large effects on take-up rates. We therefore remain optimistic that financial literacy education may in fact have substantial effect on savings behavior.

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8 Data Appendix

8.1 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.[21]

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 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.

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, 2006, 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.

8.2 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

[21] 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.

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

8.3 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 respondent's 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 were used to estimate each respondent's percentile score in the Armed Forces Qualifying Test (AFQT). There are two methods of calculating AFQT scores - in the text, results were presented using only the AFQT80, but they are robust to AFQT89.

Table 1: OLS Estimates of the Effect of Schooling on Income from Various Sources

Dependent Variable:	Income from Investments		Income from Retirement Savings	
Sample:	Whites	Blacks	Whites	Blacks
	(1)	(2)	(3)	(4)
Panel A: Any Income from Asset				
Years of schooling	3.60 ***	1.16 ***	2.00 ***	2.22 ***
	(0.01)	(0.01)	(0.02)	(0.03)
Num of Observations	10342137	1174130	4745546	475297
R-Squared	0.142	0.071	0.163	0.121
Panel B: Amount of Income from Asset				
Years of schooling	273.23 ***	38.79 ***	510.77 ***	426.82 ***
	(5.04)	(0.98)	(4.54)	(6.29)
Num of Observations	10342137	1174130	4745546	475297
R-Squared	0.088	0.019	0.141	0.113
Panel C: Percentile of Amount/Income in Distribution				
Years of schooling	342.53 ***	108.19 ***	202.87 ***	232.24 ***
	(1.38)	(1.34)	(1.86)	(2.92)
Num of Observations	9497305	1057689	4366472	431653
R-Squared	0.128	0.063	0.171	0.139

Note: Standard errors are clustered by state of birth X year of birth. Data from the 1990 and 2000 census are used. This sample contains only 36-75 year olds (50-75 year olds when considering retirement income) and includes both men and women. Other controls include fixed effects for gender, 3-year age groups, 10-year birth cohorts, state of birth, state of residence and a cubic polynomial in earned income. Regressions for blacks 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. Top-coded individuals (those with income from investments greater than $50,000 or retirement income greater than $52,000) are dropped.

Table 2: Estimates of the Effect of Compulsory Schooling Laws on Education, 2000

Dependent Variable:	Years of schooling		High school	
Sample:	Whites	Blacks	Whites	Blacks
	(1)	(2)	(3)	(4)
Comp. Att. = 9	0.116 ***	0.289 ***	0.022 ***	0.036 ***
	(0.024)	(0.037)	(0.004)	(0.005)
Comp. Att. = 10	0.139 ***	0.293 ***	0.024 ***	0.039 ***
	(0.028)	(0.056)	(0.004)	(0.007)
Comp. Att. = 11	0.252 ***	0.330 ***	0.051 ***	0.047 ***
	(0.036)	(0.054)	(0.006)	(0.008)
Num of Observations	10342137	1174130	10342137	1174130
R-Squared	0.188	0.259	0.117	0.208

Note: Standard errors are clustered by state of birth X year of birth. Data from the 1990 and 2000 census are used. This sample contains only 36-75 year olds (50-75 year olds when considering retirement income) and includes both men and women. Other controls include fixed effects for 3-year age groups, 10-year birth cohorts, state of birth, state of residence and a cublic polynomial in earned income. Regressions for blacks 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. Top-coded individuals (those with income from investments greater than $50,000 or retirement income greater than $52,000) are dropped.

Table 3: 2SLS Estimates of the Effect of Schooling on Investment Income, 2000

Dependent Variable:	Income from Investments		Income from Retirement Savings	
Sample:	Whites	Blacks	Whites	Blacks
	(1)	(2)	(1)	(2)
Panel A: Any Income from Asset				
Years of schooling	4.94 ***	3.20 ***	3.91 ***	6.92 ***
	(0.56)	(0.42)	(1.30)	(0.88)
Num of Observations	10342137	1174130	4745546	475297
F-stat of excluded instruments	16.5	20.7	36.8	29.8
Panel B: Amount of Income from Asset				
Years of schooling	1526.66 ***	182.64 ***	1113.57 ***	1142.74 ***
	(131.92)	(32.61)	(183.37)	(136.19)
Num of Observations	10342137	1174130	4745546	475297
F-stat of excluded instruments	16.5	20.7	36.8	29.8
Panel C: Percentile of Amount/Income in Distribution				
Years of schooling	498.05 ***	302.00 ***	416.89 ***	689.17 ***
	(51.93)	(39.32)	(114.05)	(84.62)
Num of Observations	9497305	1057689	4366472	431653
F-stat of excluded instruments	17.9	21.4	36.2	30.5

Note: Standard errors are clustered by state of birth X year of birth. Data from the 1990 and 2000 census are used. This sample contains only 36-75 year olds (50-75 year olds when considering retirement income) and includes both men and women. Other controls include fixed effects for gender, 3-year age groups, 10-year birth cohorts, state of birth, state of residence and a cubic polynomial in earned income. Regressions for blacks 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. Top-coded individuals (those with income from investments greater than $50,000 or retirement income greater than $52,000) are dropped.

Table 4: BGM Results and Replication Using Census Data

	BGM Original Result	Census Replication			
Dependent Variable	Savings Percentile	Any Savings Income		Inv. Income	Inv. Income Percentile
Estimation Technique	OLS	OLS	Probit	OLS	OLS
	(2)	(3)	(4)	(5)	(6)
State ever imposed mandate	-1.25	-1.90 ***	-0.07 ***	-122.93 ***	-178.76 ***
	(1.59)	(0.38)	(0.02)	(22.50)	(27.99)
Years since mandate	0.80 **	0.33 ***	0.01 ***	18.95 ***	34.47 ***
	(0.35)	(0.06)	(0.00)	(3.77)	(4.65)
Married	3.18 **	-0.83 ***	-0.02 ***	-88.77 ***	476.25 ***
	(1.40)	(0.06)	(0.00)	(11.19)	(7.22)
College Educated	9.28 ***	12.38 ***	0.50 ***	930.95 ***	1070.64 ***
	(1.36)	(0.09)	(0.00)	(21.27)	(8.12)
Age	0.17	0.62 ***	0.02 ***	66.70 ***	59.05 ***
	(0.12)	(0.02)	(0.00)	(2.53)	(2.09)
Total Earnings/10^5	5.65 ***	37.00 ***	1.17 ***	2139.50 ***	-460.70 ***
	(1.04)	(0.16)	(0.01)	(33.84)	(31.24)
R^2		0.11		0.01	0.02
N	1,869	3,601,777	3,601,777	3,579,595	3,601,777

Note: Columns (1) and (2) reproduce columns (1) and (2) from Table 5 of Bernheim, Garret, and Maki (2001), which used data from a 1995 telephone survey of individuals. The dependent variable is savings rate in column (1), and the households location in the national distribution of savings rates in column (2). Columns (3)-(6) replicate this approach, using data from the 2000 U.S. census. The dependent variable in column (3) and (4) is a dummy variable indicating whether the individual reported receiving any investment income. The dependent variable in column (5) is the level of investment income received, and in (6) is the individual's percentile ranking in the nationwide investment income distribution.

Table 5: Estimates of the Causal Effect of Financial Literacy Training

	Any Investment Income (1)	Investment Income (2)	Investment Income Percentile (3)
5 Years Prior	1.15 ***	152.17 *	100.16 ***
	(0.28)	(77.81)	(26.88)
4 Years Prior	1.01 ***	90.92 **	78.42 ***
	(0.33)	(44.79)	(29.14)
3 Years Prior	1.12 ***	68.72	80.05 ***
	(0.14)	(55.00)	(13.91)
2 Years Prior	1.16 ***	111.13 **	67.67 **
	(0.23)	(49.35)	(26.59)
1 Year Prior	1.18 ***	53.04	84.18 ***
	(0.19)	(38.55)	(20.03)
First Affected	1.13 ***	116.26 ***	63.29 ***
	(0.17)	(33.94)	(19.61)
1 Year Post	1.19 ***	151.74 ***	62.80 ***
	(0.20)	(34.70)	(18.83)
2 Years Post	0.95 ***	115.47 ***	41.31 *
	(0.22)	(44.35)	(21.40)
3 Years Post	1.34 ***	161.27 ***	88.76 ***
	(0.34)	(43.89)	(28.07)
4 Years Post	1.11 ***	246.64 ***	59.74 **
	(0.26)	(46.04)	(26.96)
5 Years or More	0.29	68.10	0.43
	(0.30)	(50.09)	(27.70)
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

Note: This table describes the evolution of financial market participation prior to, and following, the imposition of mandated financial education in high schools. 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, birthyear dummies, educational attainment dummies, and a cubic control for income.
The last 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.

Table 6: Estimates of the Causal Effect of Financial Literacy Training Among Individuals with Educational Attainment of 11th, 12th grade, or Some College

	Any Investment Income	Investment Income	Investment Income Percentile
	(1)	(2)	(3)
5 Years Prior	1.15 ***	100.25	100.59 ***
	(0.28)	(73.80)	(34.02)
4 Years Prior	1.08 ***	29.92	85.57 ***
	(0.29)	(45.50)	(27.09)
3 Years Prior	1.26 ***	17.46	111.65 ***
	(0.20)	(46.06)	(17.77)
2 Years Prior	1.05 ***	61.39 *	57.95 ***
	(0.20)	(34.25)	(22.41)
1 Year Prior	1.15 ***	99.95 ***	91.00 ***
	(0.23)	(34.09)	(21.21)
First Affected	1.16 ***	71.30 **	69.19 ***
	(0.25)	(35.99)	(23.30)
1 Year Post	1.38 ***	111.81 ***	93.32 ***
	(0.19)	(32.13)	(20.87)
2 Years Post	0.87 ***	99.14 **	49.17 *
	(0.28)	(44.76)	(28.61)
3 Years Post	1.46 ***	97.96 ***	112.73 ***
	(0.27)	(27.08)	(22.14)
4 Years Post	1.16 ***	159.90 ***	80.83 ***
	(0.30)	(37.34)	(27.60)
5 Years or More	0.56 *	77.77 *	28.95
	(0.30)	(43.43)	(27.60)
N	2,376,290	2,376,290	2,376,290
Test of Prior vs. Post			
F-Statistic	0.26	7.40	0.03
p-value	0.61	0.01	0.86

Note: This table describes the evolution of financial market participation prior to, and following, the imposition of mandated financial education in high schools. 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, birthyear dummies, educational attainment dummies, and a cubic control for income.
The last 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.

Table 7: State GDP Growth and Passage of Financial Literacy Education Requirements

	Log GDP Growth	Log GDP Growth	Log GDP Growth	Log GDP Growth	Log GDP Growth
	(1)	(2)	(2)	(2)	(2)
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

Note: 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 $100*(\log(gdp_t/gdp_{t-1}))$. 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.

Table 8: OLS Estimates of the Effect of Cognitive Ability of Savings, NLSY, Whites

	Replication		New Outcome Variables					
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)
Cognitive Ability	0.187 ***	0.187 ***	0.110 ***	0.075 *	0.075 *	0.040 *	-0.020	0.315 ***
	(0.031)	(0.030)	(0.023)	(0.044)	(0.044)	(0.021)	(0.013)	(0.026)
Years of Education	-0.185	1.414 ***	1.381 ***	1.123 **	1.420 ***	0.300	-0.216	1.441 ***
	(0.309)	(0.336)	(0.211)	(0.479)	(0.488)	(0.235)	(0.145)	(0.240)
Num of Observations	15817	21381	27581	8530	8512	8538	21388	47852
R-Squared	0.3	0.3	0.4	0.4	0.4	0.2	0.2	0.3
Panel B: Amount								
Cognitive Ability	-48.09	36.08 **	47.49 ***	19.62	87.71 **	5.30	-31.93	0.35
	(100.53)	(15.06)	(17.44)	(23.93)	(39.06)	(12.03)	(25.15)	(1.57)
Years of Education	2003.15	447.46 **	240.94	828.42 **	865.05 *	30.94	-428.03	116.15 ***
	(1203.49)	(208.49)	(205.33)	(333.09)	(477.70)	(143.47)	(274.74)	(22.15)
Num of Observations	10732	20466	28215	8115	7899	8437	20902	46208
R-Squared	0.5	0.2	0.3	0.3	0.4	0.2	0.1	0.1
Panel B: Percentile of Amount/Income in Distribution								
Cognitive Ability	0.495	14.298 ***	8.157 ***	6.786 *	6.154 *	2.072	-1.436	20.068 ***
	(2.474)	(2.338)	(1.662)	(3.473)	(3.302)	(1.616)	(1.003)	(1.940)
Years of Education	-32.995	115.612 ***	102.755 ***	49.059	68.736 *	34.374 *	-8.714	124.292 ***
	(25.531)	(26.275)	(16.054)	(37.778)	(36.896)	(18.671)	(11.130)	(18.771)
Num of Observations	9681	17540	24473	6924	6765	7120	17792	39984
R-Squared	0.4	0.4	0.4	0.4	0.4	0.3	0.2	0.4

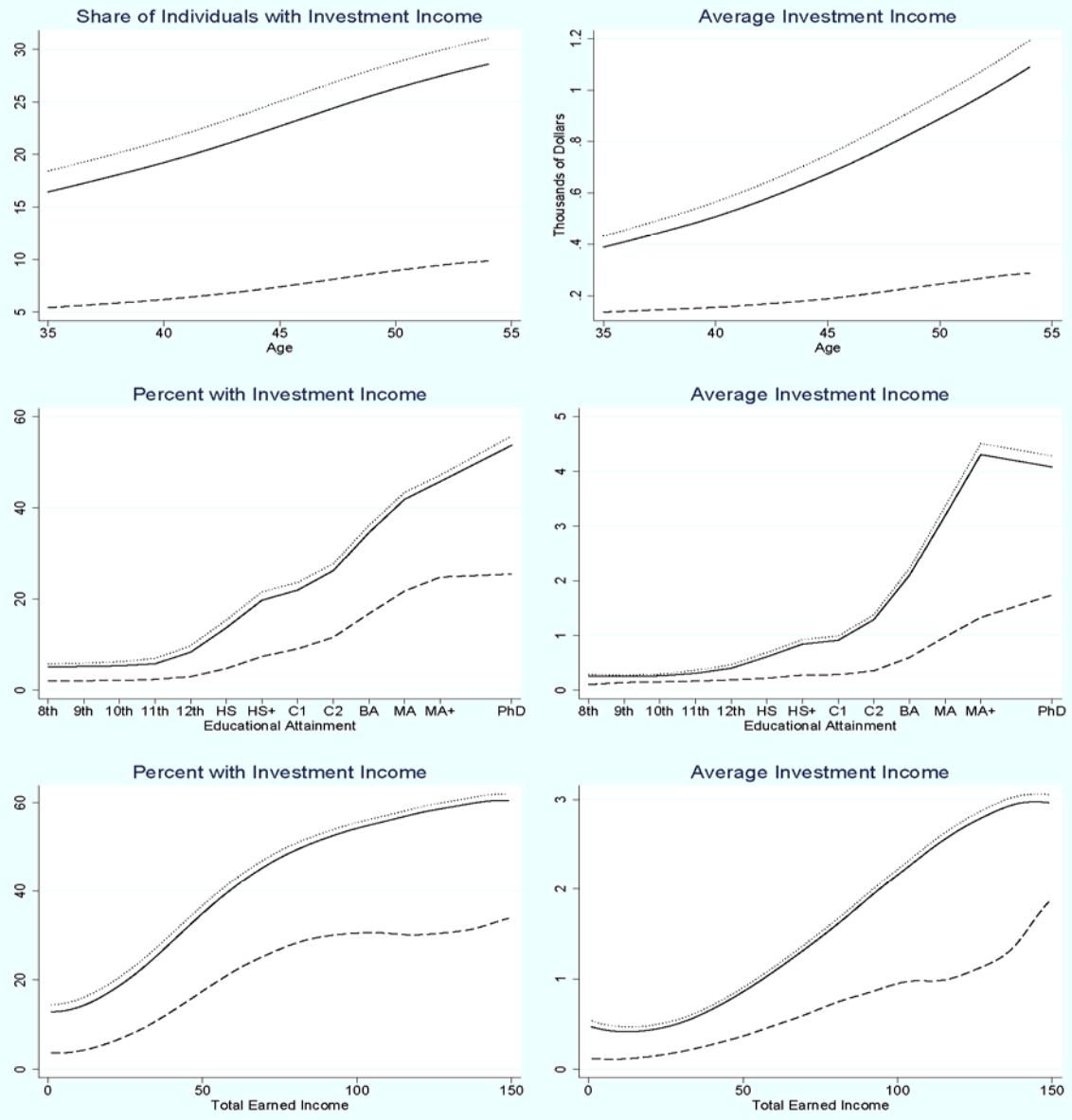
Note: Standard errors are clustered by individual. Other controls include log family income in every year with dummies proxying for missing data and fixed effects for age, gender, survey year and household.

Table 9: OLS Estimates of the Effect of Cognitive Ability of Savings, NLSY, Blacks

	Replication	New Outcome Variables						
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)
Cognitive Ability	0.115 *	0.154 ***	0.273 ***	0.126 ***	0.345 ***	0.034	-0.021	0.262 ***
	(0.066)	(0.044)	(0.049)	(0.046)	(0.065)	(0.024)	(0.012)	(0.033)
Years of Education	1.076 *	1.348 ***	2.437 ***	1.117 ***	1.316 **	0.433 **	0.049	1.479 ***
	(0.550)	(0.311)	(0.417)	(0.373)	(0.549)	(0.206)	(0.119)	(0.254)
Num of Observations	10133	13219	16347	5668	5662	5680	13245	28373
R-Squared	0.3	0.2	0.4	0.3	0.4	0.2	0.1	0.2
Panel B: Amount								
Cognitive Ability	112.13	40.37 ***	32.07 **	16.98	144.94 ***	2.82	-72.81 **	2.86 **
	(124.13)	(15.18)	(15.99)	(12.04)	(28.86)	(7.15)	(31.20)	(1.13)
Years of Education	210.33	-73.58	261.89 ***	70.36	210.07	21.86	624.71 *	22.22 *
	(996.80)	(103.53)	(97.89)	(88.72)	(221.29)	(71.85)	(352.47)	(11.46)
Num of Observations	4343	12927	16142	5508	5255	5645	13153	27925
R-Squared	0.4	0.1	0.2	0.2	0.3	0.2	0.1	0.1
Panel B: Percentile of Amount/Income in Distribution								
Cognitive Ability	6.822	12.178 ***	16.327 ***	6.707 *	23.199 ***	0.575	-1.895	16.034 ***
	(4.839)	(3.152)	(3.375)	(3.432)	(4.855)	(1.910)	(1.135)	(2.467)
Years of Education	-44.558	68.800 ***	119.735 ***	89.990 ***	35.628	38.468 **	-1.151	87.885 ***
	(41.761)	(22.608)	(27.324)	(30.363)	(42.318)	(16.977)	(11.135)	(19.645)
Num of Observations	3488	9172	11826	3878	3698	3961	9308	20407
R-Squared	0.4	0.3	0.4	0.4	0.4	0.3	0.2	0.2

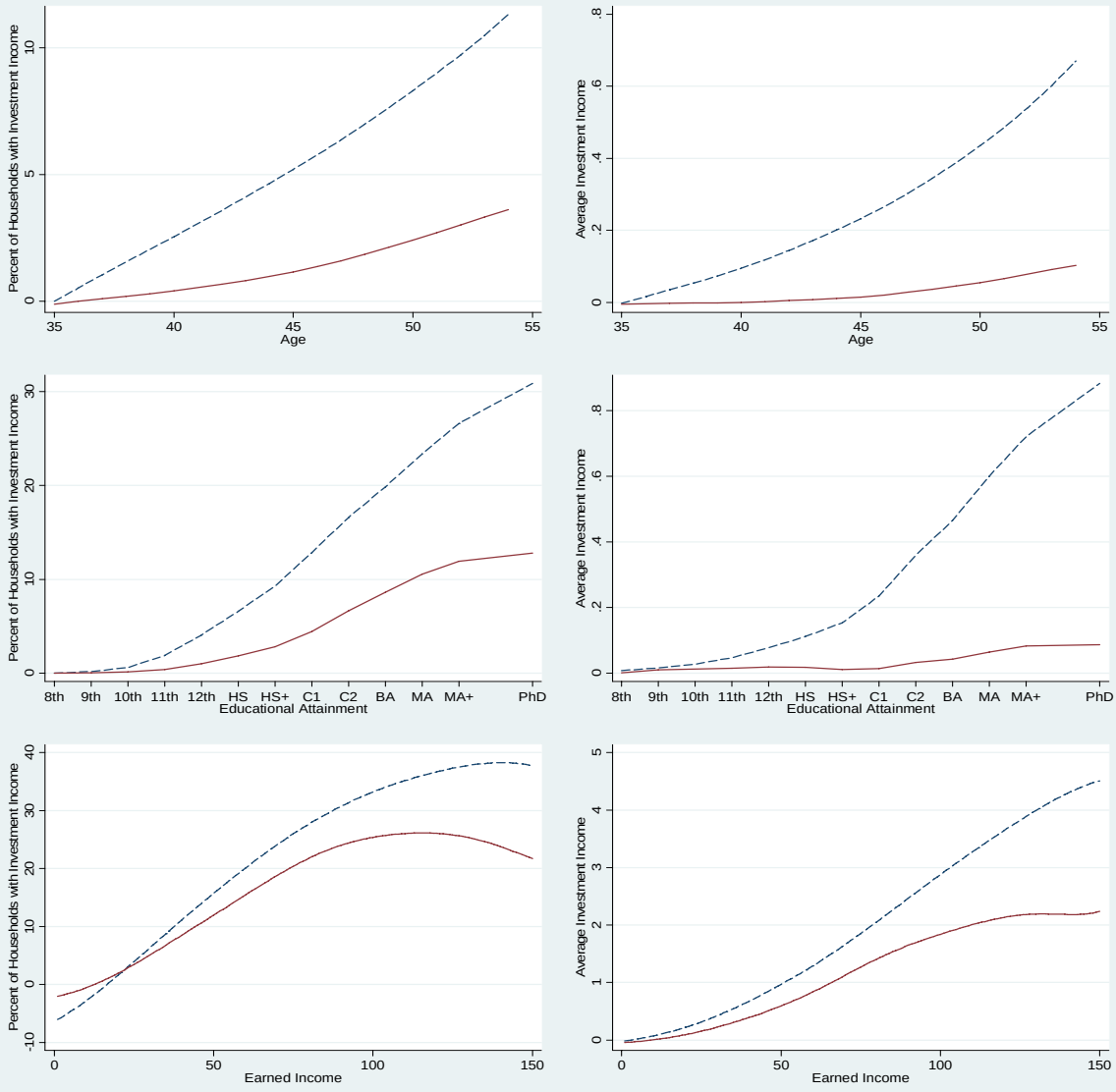
Note: Standard errors are clustered by individual. Other controls include log family income in every year with dummies proxying for missing data and fixed effects for age, gender, survey year and household.

Figure 1: Non−Parametric Relationships



The lines give the unconditional average (smoothed using Fan's local linear regression) for total population (solid line), whites (dotted line), and blacks (dashed line).

Figure 2: Non-Parametric Partial Regression



The line gives the (smoothed) estimated coefficients from a regression of investment income on age dummies, education dummies, and income dummies. The top-left panel gives therefore the non-parametric relationship between investment income and age, controlling for earned income and educational attainment. The dashed line indicates the relationship for whites, while the solid line indicates the relationship for blacks. Data are from the 2000 US census. The sample size is approximately 3,200,000.

Figure 3. Financial Literacy Education and Saving

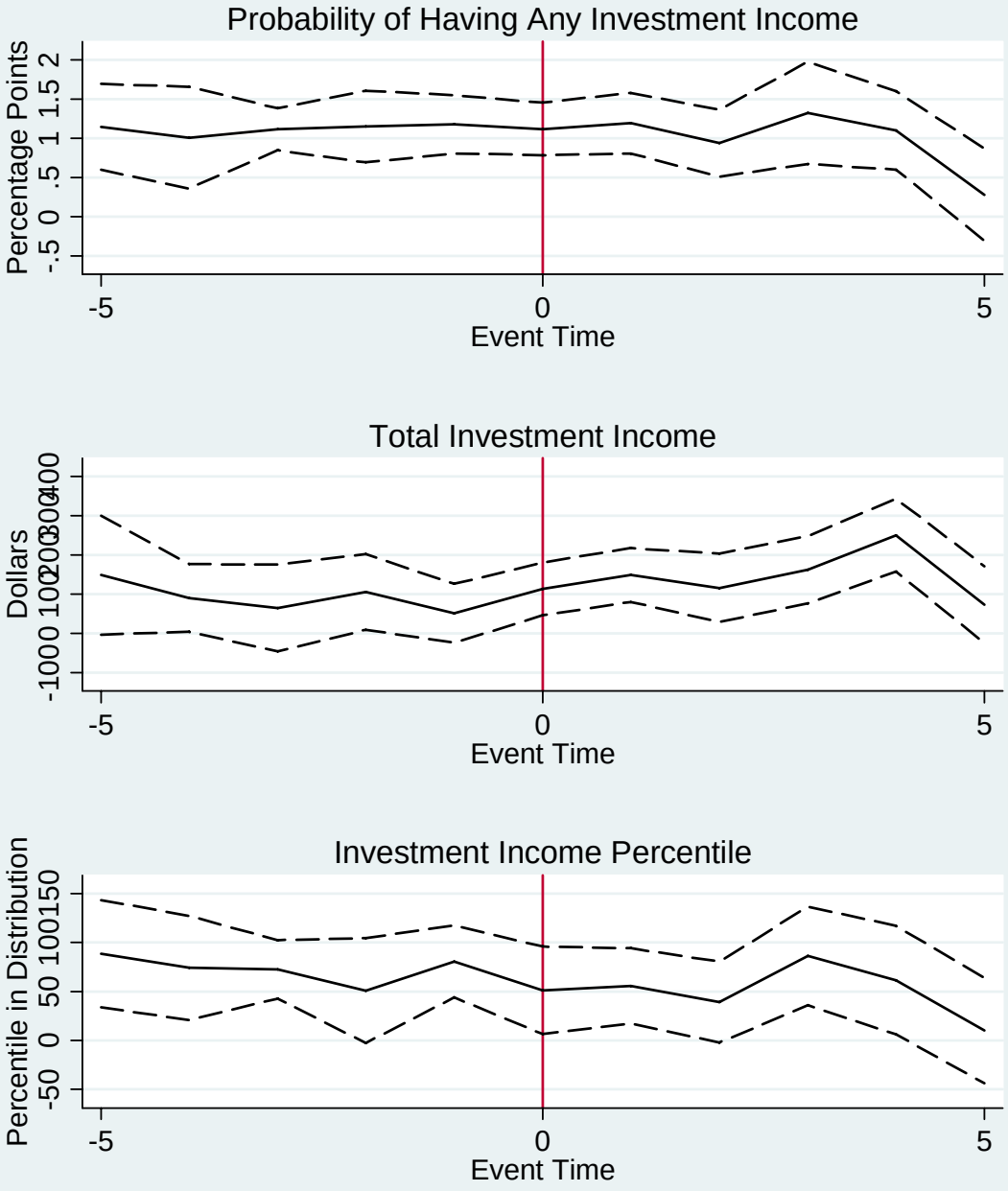


Figure 3 plots the evolution of three data series prior to, and following, the imposition of state-mandated financial literacy programs. The vertical red line indicates the year in which the financial literacy program was mandated.

Appendix Table A1: Summary Statistics, Census

Variable	Whole Sample		Whites		Blacks	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)
Years of schooling	13.19	2.62	13.29	2.55	12.32	3.04
High school graduation	0.85	0.36	0.86	0.35	0.73	0.44
Comp. Att. = 9	0.40	0.49	0.39	0.49	0.44	0.50
Comp. Att. = 10	0.11	0.31	0.11	0.31	0.10	0.31
Comp. Att. = 11	0.36	0.48	0.37	0.48	0.30	0.46
Age	48.89	13.05	49.05	13.07	47.53	12.73
Male	0.48	0.50	0.48	0.50	0.44	0.50
Black	0.10	0.30				
Income from Investments						
Any	28.23	45.01	30.50	46.04	8.16	27.37
Amount	1302.94	4659.14	1424.17	4861.49	235.06	1862.55
Percentile	2592.18	3912.44	2797.65	3995.98	747.16	2364.69
Income from Retirement Savings						
Any	10.85	31.11	11.00	31.28	9.61	29.47
Amount	1278.628	4928.848	1310.17	5005.48	999.33	4179.30
Percentile	1109.67	3029.51	1120.98	3041.38	1007.63	2918.11

Appendix Table A2: Summary Statistics, NLSY

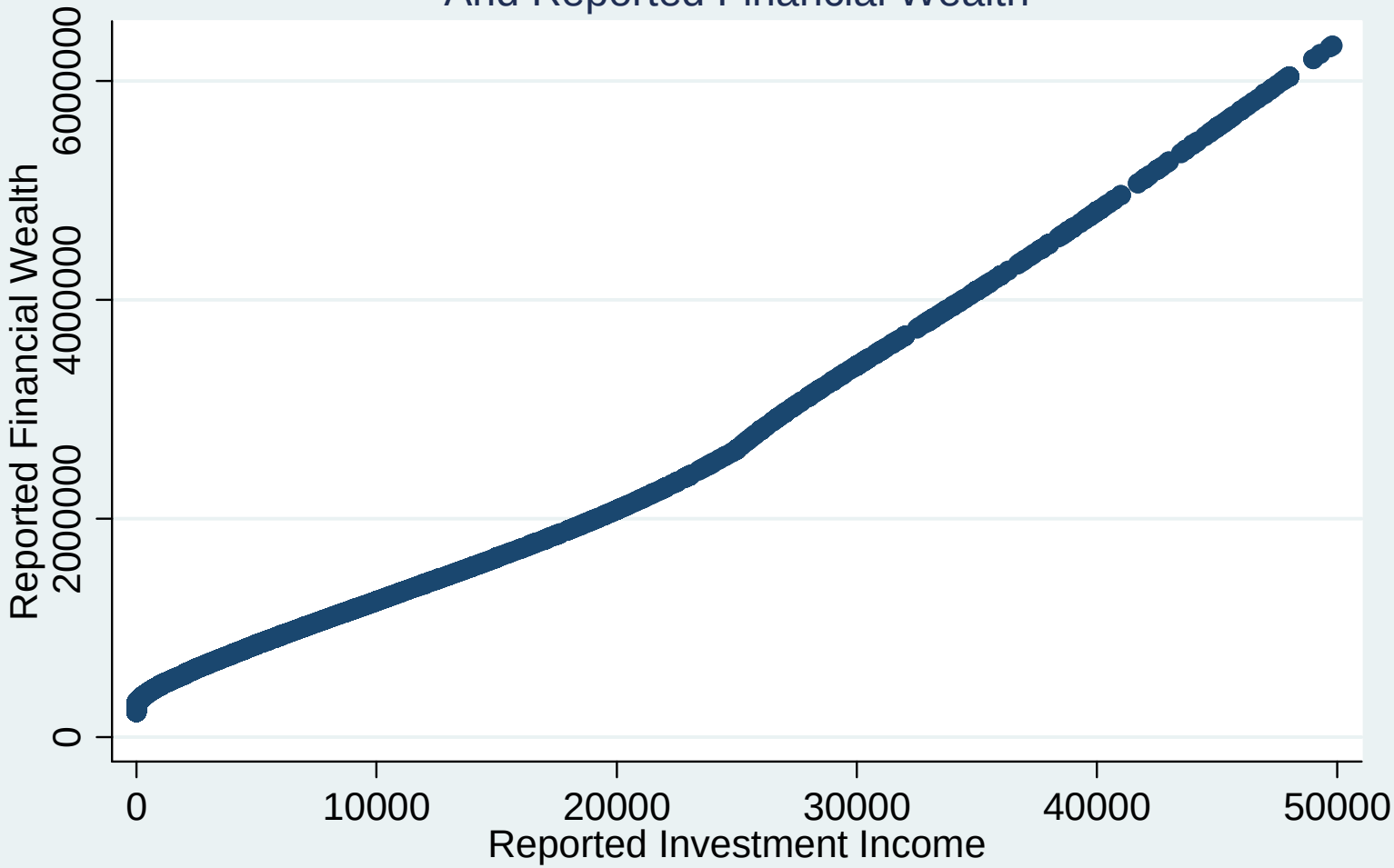
Variable	Year(s)	Whites		Blacks	
		Mean	St. Dev.	Mean	St. Dev.
	(1)	(2)	(3)	(4)	(5)
Cognitive Ability	1981	48.70	28.42	19.96	18.24
Years of Education	1985 - 2004	13.35	2.47	12.62	2.01
Age (in year 1979)	1979	17.14	2.08	17.23	2.04
Male	1979	0.51	0.50	0.52	0.50
Total Net Family Income	1979 - 2004	60964.86	99865.25	35461.28	51636.37
Pearlin "Little Control Over Life"	1992	1.74	0.62	1.85	0.70
Any …					
Money Left	1990 - 2004	77.96	41.45	48.68	49.99
Savings	1985 - 2000	75.77	42.85	46.68	49.89
Stocks, Bonds & Mutual Funds	1988 - 2000	23.79	42.58	7.76	26.76
IRAs & Keogh	1994 - 2000	29.31	45.52	8.24	27.50
Tax-Deferred Accounts	1994 - 2000	41.03	49.19	22.57	41.81
CDs, Loans, Mortgage Assets	1994 - 2000	7.19	25.83	2.50	15.61
Rights to Estate, Investment Trust	1988 - 2000	4.60	20.95	1.63	12.66
Income from Other Sources	1979 - 2002	43.09	49.52	12.74	33.34
Amount …					
Money Left	1990 - 2004	84145	115436	28523	59001
Savings	1985 - 2000	9807	30028	2465	11992
Stocks, Bonds & Mutual Funds	1988 - 2000	4281	25528	656	9119
IRAs & Keogh	1994 - 2000	7692	30866	1002	8392
Tax-Deferred Accounts	1994 - 2000	15077	43346	3186	16432
CDs, Loans, Mortgage Assets	1994 - 2000	1472	13165	332	5396
Rights to Estate, Investment Trust	1988 - 2000	3410	52389	879	22888
Income from Other Sources	1979 - 2002	743	4176	172	1576
Percentile …					
Money Left	1990 - 2004	4720	2413	3140	2328
Savings	1985 - 2000	4347	2751	2553	2761
Stocks, Bonds & Mutual Funds	1988 - 2000	1654	3102	522	1892
IRAs & Keogh	1994 - 2000	1985	3250	508	1843
Tax-Deferred Accounts	1994 - 2000	2670	3388	1352	2681
CDs, Loans, Mortgage Assets	1994 - 2000	485	1877	167	1123
Rights to Estate, Investment Trust	1988 - 2000	261	1410	96	863
Income from Other Sources	1979 - 2002	2996	3465	910	2350

Appendix Table A3: Comparison of Data from 2001 SCF and 2000 Census

	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

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.

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



This graph presents a local linear regression of the relationship between reported investment income and reported Financial Wealth, for households whose reported investment income was less than $50,000.
Data are from the 2001 Survey of Consumer Finances.