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Smart Money? The Effect of Education on Financial Outcomes

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Household financial decisions are important for household welfare, economic growth, and financial stability. Yet our understanding of the determinants of financial decision making is limited. Exploiting exogenous variation in state compulsory schooling laws in both standard and two-sample instrumental variable strategies, we show that education increases financial market participation, measured by investment income and equities ownership, while dramatically reducing the probability that an individual declares bankruptcy, experiences a foreclosure, or is delinquent on a loan. Further results and a simple calibration suggest that the result is driven by changes in savings or investment behavior, rather than simply increased labor earnings. (*JEL* D14, I20, G11)

Individuals face an increasingly complex set of financial decisions. On the asset side of the balance sheet, the shift to defined contribution pension plans and the growing importance of private retirement accounts require individuals to choose the amount they save, as well as the mix of assets in which they invest. On the liability side, a dramatic increase in the range and complexity of credit products available to households has been accompanied by increased default, bankruptcy, and foreclosures. In May 2013, only 46% of nonretired Americans reported that they expected to have enough money to support a "comfortable retirement."[1] These facts, along with the recent financial crisis,

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[1] Gallup Poll, May 2013, www.gallup.com/poll/162842/americans-optimistic-comfortable-retirement.aspx, accessed June 2013.

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have sparked a vigorous debate about whether individuals are well equipped to make informed financial decisions. For example, the director of the Consumer Financial Protection Bureau has testified that "education is the cornerstone" for the capability of managing financial affairs,[2] and several mortgage lenders have admitted to steering borrowers with low levels of education toward unattractive (but profitable) mortgages.[3] Yet to date, we have only a limited understanding of which factors affect financial market participation and the responsible use of credit.

Using data and estimation techniques new to the literature, this paper provides precise, causal estimates of the effect of education on financial market participation, income from investments in financial instruments, and credit management. Previous work has established a strong correlation between education and financial outcomes, but to date, there has been no measure of a causal relationship. Education and financial market outcomes may be correlated with unobservable characteristics (such as ability or family background), causing potentially spurious correlation. It is also important for policy makers to have a precise causal estimate, so they can better understand how the changing educational environment may affect financial outcomes.

To estimate a causal effect, we exploit exogenous variation in education caused by changes in compulsory schooling laws. In our preferred specification, using a sample of U.S. census data for almost 15 million individuals, we find that an additional year of education increases the probability that an individual has any nonzero investment income by 7–8 percentage points, holding other factors, including labor market income, constant. Using a second dataset, we find that an additional year of education increases the probability of owning equities by 4 percentage points. The size of this effect is economically important, both on its own and in the context of previously identified correlates of financial participation, such as trust (Guiso, Sapienza, and Zingales 2008), peer effects (Hong, Kubik, and Stein 2004), prior stock market experience (Malmendier and Nagel 2011), or institutional quality (Osili and Paulson 2008).

To study the effect of education on financial outcomes beyond simple participation in financial markets, we implement a two-sample instrumental variables strategy, combining census data with a new dataset, the Federal Reserve Bank of New York Consumer Credit Panel/Equifax dataset. We find that exogenous increases in education lead to substantial reductions in the probability of bankruptcy and foreclosure, slightly higher credit scores, and fewer delinquent credit card payments. The effect of education on foreclosure was particularly pronounced during the recent financial crisis.

Establishing a causal link between education and financial outcomes is a key contribution of this paper, and it is important to be clear about what our

[2] www.consumerfinance.gov/speeches/prepared-remarks-of-richard-cordray-at-the-federal-reserve-bank-of-chicago-visa-inc-financial-literacy-and-education-summit/, accessed August 2013.

[3] Kristof, Nicholas, *New York Times*, November 30, 2010.

identification strategy estimates. We measure a "local average treatment effect (LATE)," that is, the effect of additional education on financial outcomes for the set of individuals whose ultimate educational attainment was altered by changes in compulsory schooling laws. This group includes many individuals whose financial situation is of concern to policy makers, namely, the lower-income segment of the population.[4]

The final portion of our paper explores the potential mechanisms by which education affects financial outcomes. This is made difficult by the fact that we cannot observe commonly studied financial behaviors, such as the alpha of individuals' portfolios. One obvious channel is that better-educated individuals earn higher wages, enabling them to accumulate more assets and earn additional investment income as a result. However, a simple back-of-the-envelope calculation demonstrates that the estimated effect of education on the level of investment income is too large to come solely from this wage return to education, without a concurrent change in savings rates or investment decisions. This calibration, along with the finding that educated people are more likely to participate in the stock market, accumulate any return-yielding assets, and stay current with their credit card debt, suggests that education may improve financial management and decision making. We discuss support for this interpretation in Section 4.

This paper contributes to a growing body of literature on household finance. Much attention has focused on three features of household behavior that may be inconsistent with standard models. The first is the low level of participation in equity markets relative to the returns offered by stocks: in 2004, only 48.6% of households held stocks, either directly or indirectly (Bucks, Kennickell, and Moore 2006). Haliassos and Bertaut (1995) consider and reject risk aversion, belief heterogeneity, and other potential explanations for the limited participation puzzle, instead favoring departures from expected-utility maximization. Our paper shows that low levels of education may help explain limited participation in equity markets.

A second "puzzle" to which our work relates is the apparently low savings rate of the U.S. population, particularly among lower-income individuals. Lusardi, Schneider, and Tufano (2011) report that only one-quarter of the U.S. population has the capacity to come up with $2000 within 30 days to meet an unexpected expense. Our results demonstrate that education dramatically affects savings outcomes among more vulnerable population segments, specifically those on the margin of completing high school.

Finally, researchers have been paying more attention to the possibility that behavioral biases may cause consumers to choose the wrong credit products or borrow too much. For example, Campbell, Giglio, and Pathak (2011) suggest that consumers make financial mistakes that result in significant costs not only

[4] Gallup Poll, May 2013, www.gallup.com/poll/162239/middle-aged-americans-worried-finances.aspx, accessed June 2013.

to themselves but to the stability of the financial system and that this behavior is correlated with low levels of education. Gross and Souleles (2002) note that individuals borrow from credit cards, even when they hold large bank account balances.

Although survey evidence has proven useful in demonstrating factors that are correlated with such behaviors,[5] there is much less understanding of what the causal drivers are. This paper contributes to the literature by showing that variation in educational attainment across the U.S. population can help to explain some of these puzzles.

More generally, the depth and breadth of financial market participation are thought to be important in determining the equity premium, the volatility of markets, and household expenditure (Mankiw and Zeldes 1991; Heaton and Lucas 1999; Brav, Constantinides, and Gezcy 2002; Vissing-Jørgensen 2002). Financial behavior may also affect the political economy of financial regulation, as those holding financial assets may have different attitudes toward corporate and investment income tax policy, as well as risk sharing and redistribution.

1. Data

This paper uses three complementary datasets: the U.S. Census, the Survey of Income and Program Participation (SIPP), and the Federal Reserve Bank of New York Consumer Credit Panel/Equifax dataset (FRBNY-CCP). Summary statistics are presented in Table 1.

1.1 The census

We first use a 5% sample from the 1980, 1990, and 2000 Public Use Census Data, representing a random draw of the U.S. population. The key advantage of this dataset is its size: with over 14 million observations, we can use nonparametric controls, obtain precise estimates, and, most importantly, use instrumental variable strategies that would not be possible with most other smaller datasets.

The main limitation to using the census is that it does not collect any information on financial wealth. Because of this, the census is not typically used to study financial behavior (an exception is Carroll, Rhee, and Rhee 1999). However, the census does collect detailed income data, including income derived from investments. Thus, the main financial indicator we use from the census is "income from interest, dividends, net rental income, royalty income, or income from estates and trusts," received during the previous year, which we term "investment income." Note that investment income can be negative

5 Previous work has demonstrated that financial behavior is, not surprisingly, correlated with income, education (Bertaut and Starr-McCluer 2001, among others), measured financial literacy (Lusardi and Mitchell 2007), social connections (Hong, Kubik, and Stein 2004), trust (Guiso, Sapienza, and Zingales 2008), experience with the stock market (Malmendier and Nagel 2011), and cognitive ability (Grinblatt, Keloharju, and Linnainmaa 2011).

Table 1
Summary statistics

	Mean	SD	N
Panel A: Household survey data			
Demographics (census)			
Age	45.60	(14.74)	14,913,356
Years of schooling	12.91	(2.69)	14,913,356
Compulsory attendence <= 8	0.153	(0.360)	14,913,356
Compulsory attendence = 9	0.404	(0.491)	14,913,356
Compulsory attendence = 10	0.101	(0.301)	14,913,356
Compulsory attendence = 11	0.342	(0.474)	14,913,356
Income from investments (census)			
Indicator for nonzero income	0.289	(0.453)	14,913,356
Indicator: Income > 500 or negative	0.189	(0.391)	14,913,356
Indicator: Income > predicted savings account interest (SCF)* or negative	0.278	(0.448)	14,913,356
Indicator: Income > predicted savings account interest (SIPP)* or negative	0.296	(0.456)	4,053,909
Amount	1,810.62	(9,250.44)	14,913,356
Income from retirement savings (census)			
Indicator: Income > 0	0.224	(0.417)	4,150,828
Amount	3,315.67	(10,635.99)	4,150,828
Transactions account (SIPP)			
Indicator for having	0.766	(0.423)	168,946
Bonds or government securities (SIPP)			
Indicator for having	0.148	(0.355)	262,245
Stocks or mutual funds (SIPP)			
Indicator for having	0.219	(0.413)	270,316
Panel B: Credit bureau data			
Bankruptcy indicator	0.144	(0.351)	5,750,005
Foreclosure indicator	0.058	(0.234)	5,750,005
Credit score	714.67	(90.57)	5,732,690
% Balance current	0.956	(0.113)	5,329,619
% Quarters delinquent	0.075	(0.152)	5,750,005

This table reports summary statistics for data used in this paper. Panel A reports summary statistics from the 5% sample of the census (1980, 1990, and 2000) as well as various SIPP waves (1984–2008). Indicators for having bonds or government securities and stocks or mutual funds are from all 1984–2008 SIPP waves, whereas the indicator for having a transaction account is from the 1990–2008 SIPP waves. Panel B reports summary statistics for data from the FRBNY Consumer Credit Panel/Equifax. The sample comprises a 5% panel of American borrowers, restricted to borrowers who have data in every quarter of the panel from 1999 to 2011. Bankruptcy and Foreclosure are indicators for having undergone bankruptcy or foreclosure at least once, respectively, between 1992 and 2011. Credit Score is averaged for each individual across all quarters of data, and it can range from 280 to 850. The % of Balance Current represents the nondelinquent balance on credit cards divided by the total credit card balance, averaged over the entire panel. The % of Quarters Delinquent represents the proportion of quarters an individual has any delinquent balance on his/her credit card bills.

or positive and that households are instructed to "report even small amounts credited to an account" (Ruggles et al. 2004). A second type of income we use is "retirement, survivor, or disability pensions," received during the previous year, which we term "retirement income." This is distinct from Social Security and Supplemental Security Income, both of which are reported separately.

We note a number of limitations to using the amount of investment income received without specific information on investment allocations. First, investment income is only partially informative about the amount and type of investments held by the respondent. This would make it difficult to rely on census data for structural estimates of investment levels (such as calibrating models of the cost of participating in financial markets). In our analysis of the census data, however, we primarily focus on the decision to accumulate *any* return-yielding assets, for which we define a dummy variable equal to one if the household reports any nonzero investment income (positive or negative). Throughout the paper, we will refer to this outcome as "any investment income."[6] Second, one may be concerned that small amounts of investment income simply represent interest from savings accounts. As a robustness check, we rerun our analysis defining participants as either (1) those who report investment losses or investment income greater than $500 or (2) those who report investment losses or investment income above a cutoff predicted using the savings account interest earnings from the Survey of Consumer Finances (SCF) or SIPP. Third, it is possible that an individual may hold assets that do not yield a return within the year, such as growth stocks or zero-coupon bonds. In our view it is unlikely that such an individual would not also have a savings account that earned interest income.

Finally, unlike the SCF, the census is not specifically aimed at measuring complex financial information. Therefore, in Online Appendix Tables A1 and A2, we compare the census data with data from the SCF. We find that the census data yield very similar estimates of means, medians, and percentiles for our measures of participation, investment, and retirement income. We also explore the relationship between reported investment income and more traditional measures of financial market participation. In particular, we find a large jump in the use of transactions accounts as individuals move from zero to any positive amount of investment income. For example, 78% of households reporting no investment income possess a checking account, whereas 92% of those reporting investment income between $1 and $100 have checking accounts. There is a similar, strongly positive and nearly linear relationship between reported investment income and participation in equity markets. Further details of this comparison are described in the Online Data Appendix.

1.2 The Survey of Income and Program Participation (SIPP)

We complement the binary measure of any investment income from the census with data from a second source: direct data on equity ownership from the SIPP. The SIPP, conducted by the Census Bureau, is a series of national panel surveys that began in 1984. We use all panels from 1984 to 2008 to generate a sample size large enough to exploit the compulsory schooling instrumental-variable strategy. Each panel is a nationally representative sample of 14,000–37,000 households; households are surveyed every four months for four years. The survey is built around a core set of demographic and income questions that include ownership of different types of assets, such as transaction accounts,

[6] In Online Appendix Table A6, we also examine whether individuals report negative investment income, but in the paper, the outcome we study is equal to one if an individual reports positive or negative investment income.

stocks, bonds, and mutual funds.[7] The SIPP has a broader range of financial variables, and we employ it as a complement to the census. Our primary analysis focuses on the census dataset, which provides a sample size that is fifty times larger than that of the SIPP and, therefore, yields more precise estimates and greater confidence in the validity of the instrumental variable strategy.

1.3 The FRBNY Consumer Credit Panel/Equifax dataset (FRBNY-CCP)

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

We focus on five key outcome variables from this dataset: a bankruptcy indicator, a foreclosure indicator, a credit score, the proportion of an individual's credit card debt that is not delinquent, and the proportion of quarters in which an individual has any delinquent credit card balance. The bankruptcy and foreclosure variables indicate whether an individual has undergone bankruptcy or foreclosure at least once, respectively, between 1992 and 2011. These indicators are able to track bankruptcies and foreclosures back through 1992 because credit bureaus maintain records on these proceedings for seven years. The credit score, similar to a FICO score, predicts the likelihood of being 90 or more days delinquent over the next 24 months. Credit scores range from 280 to 850, and higher scores imply a lower probability of being seriously delinquent in the future. Both the credit score and the proportion of an individual's credit card debt that is not delinquent are averaged across all quarters. We do this because even though there is time-series variation in the outcome variables, the exogenous variation in education is cross-sectional and does not vary at the individual level over time. Calculating averages is one way to address potential serial correlation in the same individual's credit scores and delinquency from month to month (Angrist and Pischke 2008).[8]

[7] Each survey wave also includes topical modules that gather additional information on assets and liabilities—for example, the monetary value of stocks and bonds—but these questions are not available in all years. Thus, the sample size falls substantially when we use these variables, rendering the instruments too weak for interpretation. For this reason, we focus on a binary measure of financial market participation, whether or not respondents own any equity, rather than the extent of their participation in financial markets.

[8] The size of the dataset precludes using all of the data and clustering.

2. The Effect of Education on Asset Accumulation and Financial Market Participation

2.1 Empirical strategy

While researchers have documented a positive correlation between educational attainment and financial behavior (for example, Campbell [2006] notes educated households in Sweden have more diversified portfolios), the literature has not produced credible estimates of the causal effect of education on financial outcomes.[9] Education and behavior are both likely to be correlated with factors like ability, making it hard to isolate the causal impact of education (Griliches 1977).

To overcome this problem, we adopt an instrumental variables (IV) strategy, first developed by Acemoglu and Angrist (2000). We use changes in state compulsory education laws as an instrument for educational attainment. This provides exogenous variation in education: revisions to state laws affect individual educational attainment but are not correlated with individual ability, parental characteristics, or other potentially confounding factors.

In particular, we follow the strategy laid out by Lochner and Moretti (2004, hereafter LM), who use changes in state schooling requirements to measure the effect of education on incarceration rates. States revised compulsory schooling laws numerous times from 1914 to 1978, and not always in the direction of requiring additional schooling. We use data from the 1980, 1990, and 2000 censuses and focus on individuals between 18 and 75 years old, who were born in or before 1964.[10] The principal 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 educational attainment in the years prior to changes in schooling laws and that compulsory schooling laws do not affect college attendance, supporting the identifying assumption that, conditional on the controls (such as state and year of birth), the compulsory schooling laws in effect when a student turned fourteen are uncorrelated with omitted determinants of education or financial outcomes. We provide evidence below that these laws do influence at least some students to acquire more schooling, a necessary condition for the IV strategy to be valid.

9 Most of the literature suggests a positive correlation between education and financial outcomes. At the same time, Tortorice (2012) finds that education only slightly reduces the likelihood that individuals make expectational errors regarding macroeconomic variables and that these errors affect buying attitudes and financial decisions.

10 LM use the 1960, 1970, and 1980 censuses, which contain information on correctional facility residence and focus on a narrower age group, ages 20–60. The census does not code a continuous measure of years of schooling, but rather identifies categories of educational attainment: preschool, grades 1–4, grades 5–8, grade 9, grade 10, grade 11, grade 12, 1–3 years of college, and college degree or more. We translate these categories into years of schooling by assigning each range of grades the highest number of years of schooling for that category. This should not affect our estimates, because individuals who fall within the ranges of grades 1–4, 5–8, and 1–3 years of college will not be influenced by the compulsory schooling laws that affect grades 9–12.

The structural equation of interest is the following:

$$y_i = \alpha + \beta s_i + \gamma X_i + \varepsilon_i \tag{1}$$

where y_i is a financial outcome for individual i; s_i is years of education for individual i; and X_i is a set of controls that include age, gender, race, state of birth, state of residence, census year, cohort of birth fixed effects and a cubic polynomial in earned income. The financial outcome variable can be an indicator for having any investment or retirement income, the level of investment or retirement income, or an indicator for whether the individual owns specific types of assets (such as equity). When the outcome variable is the amount of investment or retirement income, we drop top- or bottom-coded observations.[11] We control for age through a series of indicator variables for each three-year age group from 20 to 75, and year effects are indicator variables for each census year. We exclude people born in Alaska and Hawaii[12] but include those born in the District of Columbia; thus, we have 49 state-of-birth controls but 51 state-of-residence controls. Again following LM, we include state-of-birth controls interacted with an indicator variable equal to one for individuals born in the South who turned fourteen in or after 1958 to allow for the impact of the *Brown vs. Board of Education* decision. A cohort of birth is defined as a ten-year birth interval. Standard errors are corrected for intracluster correlation within state of birth * year of birth.

Following Acemoglu and Angrist (2000) and LM, we create indicator variables for whether the years of required schooling are eight or fewer, nine, ten, and 11 or more.[13] These variables are based on the law in place in an individual's state of birth when the person turns 14 years of age. As LM note, migration between birth and age 14 will add noise to this estimation, but the IV strategy is still valid.[14] The first stage for the IV strategy can then be

[11] To preclude the possibility of revealing personal information, the census "top codes" values for individuals earning large amounts of investment income and "bottom codes" values for individuals with large investment losses. Specifically, they replace the income variable for individuals with investment income above a year-specific limit with the median income of all individuals in that state earning above that limit and replace all losses in excess of a year-specific limit with the limit itself. Retirement income is top coded similarly, but not bottom coded. The percentage of top-coded and bottom-coded observations is very low: 0.48% are top coded and 0.04% are bottom coded for investment income, and 0.23% are top coded for retirement income. Of course, using an indicator variable for any investment income as the dependent variable avoids this issue entirely. Although Angrist and Pischke (2008, 105–106) express concerns about IV Tobit, we nevertheless run Tobit regressions to account for top coding and find very similar results (available in the Online Appendix). Observations on investment income were bottom- or top coded if they were outside the range of –\$9,990 to \$75,000 in 1980, –\$9,999 to \$40,000 in 1990, and –\$10,000 to \$50,000 in 2000. Observations on retirement income were top coded if they were greater than \$30,000 in 1990 and greater than \$52,000 in 2000. The 1980 census did not separate retirement income from other (noninvestment) sources of income. We also drop all observations in which these values were imputed.

[12] This follows Acemoglu and Angrist (2000) and Lochner and Moretti (2004). Alaska and Hawaii did not become states until 1959, well after the first cohorts included in the analysis were born.

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

[14] In fact, even if we had state of high school attendance, we might prefer to use state of birth to avoid any endogeneity resulting from households who moved states as a response to education-related laws.

written as

$$s_i = \alpha + \delta_9 Comp9 + \delta_{10} Comp10 + \delta_{11} Comp11 + \gamma X_i + \varepsilon_i \quad (2)$$

where s_i is years of schooling; *Comp9, Comp10*, and *Comp11* are indicator variables that specify the required number of years of schooling that individual i completed; and X_i is the same set of controls defined above. (The omitted category is laws that required eight or fewer years of schooling.)

As discussed previously, the estimates produced here are local average treatment effects (LATE), which measure the effect of education on financial market participation for those whose educational attainment was affected by changes in compulsory education laws.[15] We note that those who are in fact affected by the laws are likely to have low levels of financial market participation and, thus, constitute a relevant study population. Using a compulsory schooling reform that affected a large fraction of the United Kingdom's population, Oreopoulos (2006) finds a LATE estimate of the effect of education on earnings that is very similar to the LATE estimated in the United States from a small fraction of the population.

2.2 Empirical results

We begin, as is customary, with the naive OLS relationship between education and participation (Equation 1). These results match most closely what has been done in the previous literature and serve as a useful point of reference but are likely subject to omitted variable bias. Panel A of Table 2 presents the OLS estimates using the census data, whereas Panel B presents estimates using SIPP data. In Panel A, the dependent variable is an indicator for any investment income (Column 1) or any retirement income (Column 3) and the amount of investment or retirement income (Columns 2 and 4, respectively). In Panel B, the dependent variable is an indicator variable for whether the respondent has any transactions account (Column 1), bonds or government securities (Column 2), or stocks or mutual funds (Column 3). The OLS estimates produce the expected positive correlation between education and financial market participation, and the census and SIPP estimates are comparable.

Before discussing the causal estimates from the IV estimation, we demonstrate the validity of the first stage of our analysis and show that the compulsory schooling laws did, in fact, influence educational attainment.[16] In Table 3, we present the first-stage regression of years of schooling (Columns 1 and 3) or high school graduation (Columns 2 and 4) on the three instrumental variables (*Comp9*, *Comp10*, and *Comp11*) and the controls discussed earlier. Clearly, when states mandate a greater number of years of schooling, some

15 Imbens and Angrist (1994) provide a discussion of local average treatment effects.

16 Lochner and Moretti (2004) report a range of tests examining the exclusion restriction and demonstrate that the education mandates are not systematically correlated with other policies that might affect outcomes.

Table 2
OLS estimates of the effect of years of schooling on income from various sources

Panel A: Census outcomes

	Indicator: Any income from investments	Amount of income from investments	Indicator: Any income from retirement savings	Amount of income from retirement savings
	(1)	(2)	(3)	(4)
Years of schooling	0.035***	271.55***	0.024***	548.42***
	(0.0001)	(5.02)	(0.0002)	(4.81)
No. of observations	14,913,356	14,838,407	4,150,828	4,117,987
R^2	0.184	0.092	0.177	0.147

Panel B: SIPP outcomes

	Indicator: Any transactions account (1)	Indicator: Any bond or government securities (2)	Indicator: Any stocks or mutual funds (3)
Years of schooling	0.026***	0.0162***	0.029***
	(0.0005)	(0.0003)	(0.0003)
No. of observations	168,946	262,245	270,316
R^2	0.133	0.064	0.128

This table reports results from regressions of income and assets on years of schooling, gender, race, age (3-year age groups), birth cohort (10-year cohorts), state of birth, state of residence, survey year, and a cubic polynomial in earned income. Only the coefficient on education is reported. Regressions also include state of birth 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*. We include 18- to 75-year-olds (50- to 75-year-olds when considering retirement income). In Panel A, the sample comprises individuals reported in the 5% samples of the 1980, 1990, and 2000 censuses. The dependent variable of interest is whether the household receives income from investments or retirement savings and the amount. In Panel B, the sample comprises individuals from the 1990–2008 SIPP waves (Column 1) and the 1984–2008 SIPP waves (Columns 2 and 3). In Panel A, Columns 2 and 4, top-coded individuals (see text) are dropped. Standard errors, corrected for arbitrary correlation within state of birth-year of birth, are in parentheses. (Numbers with *** indicate significance at the 1% level.)

individuals obtain more education than they would have otherwise. Using census data, requiring nine or ten years of schooling is estimated to increase average years of completed education by approximately 0.2 years, whereas requiring 11 years of education is estimated to increase education by 0.27 years (Column 1). Requiring students to remain in school for nine years of schooling increases their probability of graduating high school by 3.9 percentage points (Column 2). Columns 3 and 4 use the SIPP data to estimate the first stage and produce reassuringly similar estimates.[17]

Table 4 presents IV estimates of Equation (1) for the impact of education on asset accumulation and financial market participation. Panel A provides results using data from the census. Column 1 omits the cubic polynomial in earned income, because income could be affected by education, and therefore captures the total causal effect of education on whether an individual reports any investment income. An additional year of schooling increases the probability

17 Weak instrument bias is not a problem in this context. We report the F-statistics of the excluded instruments in Tables 3 and 4. The F-statistics for the census range from 37.7 to 52.4, well above the critical values proposed by Stock and Yogo (2005). The F-statistics for the SIPP are lower (due to the smaller sample size) but are still within the range of appropriate critical values.

Table 3
Estimates of the effect of compulsory schooling laws on education

	Years of schooling (1)	High school (2)	Years of schooling (3)	High school (4)
Compulsory attendence = 9	0.214***	0.039***	0.0260	0.0120**
	(0.018)	(0.003)	(0.034)	(0.005)
Compulsory attendence = 10	0.199***	0.041***	0.1660***	0.0265***
	(0.024)	(0.004)	(0.046)	(0.007)
Compulsory attendence = 11	0.266***	0.055***	0.1747***	0.0428***
	(0.028)	(0.005)	(0.040)	(0.006)
No. of observations	14,913,356	14,913,356	276,079	276,079
R^2	0.234	0.178	0.182	0.137
Data source	census	census	SIPP	SIPP
F-stat of excluded instruments	47.2	52.4	11.5	20.8

This table reports the first-stage relationship between compulsory school laws and educational attainment. In Columns 1 and 2, the sample comprises individuals reported in the 5% samples of the 1980, 1990, and 2000 censuses. In Columns 3 and 4, the sample comprises individuals from the 1984–2008 SIPP waves. We include 18- to 75-year-olds. The dependent variables of interest are the number of years of schooling attained (Columns 1 and 3) and an indicator for whether the individual graduated high school (Columns 2 and 4). The independent variables of interest indicate whether the state in which the individual was born prohibited dropout until a child had completed 9th, 10th, or 11th grade and higher (requiring 8 or fewer years of schooling is the omitted category). Other controls include fixed effects for gender, race, 3-year age groups, 10-year birth cohorts, state of birth, state of residence, survey year, and a cubic polynomial in earned income. Regressions also include state of birth 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*. Standard errors, corrected for arbitrary correlation within state of birth-year of birth, are in parentheses. (Numbers with ** or *** indicate significance at the 5% or 1% level, respectively.)

that an individual reports any investment income by 6.9 percentage points. Column 2 of Panel A includes a cubic control for earned income (which includes wages and income from one's own business or farm).[18] Although income itself may be affected by education, it is useful as a specification check to examine whether the impact of education on financial outcomes is entirely due to changes in earnings. In fact, we find that the point estimate on schooling is nearly identical when we control for earned income in a flexible manner. This suggests that increased income is not the only mechanism driving the result: education increases the probability of accumulating any return-yielding assets, conditional on noninvestment income. The striking fact is that no matter how flexibly we control for earned income (such as with an earned income spline, see Online Appendix Table A3), we find a persistent and large impact of education on having any investment income.

In Columns 3–5, we consider the possibility that our measure of investment income might simply reflect interest-bearing savings accounts, rather than a shift toward investment in higher return financial products. We redefine the

18 Duflo et al. (2006, 3,949) point out that including controls, such as income in our case, that may be affected by the experiment can lead to biased estimates. The census dataset does not include any measures of wealth, but even if it did, we do not believe it would be an appropriate control. It also suffers from this econometric issue, but the problem is even worse for wealth than for income because wealth is, in fact, the outcome we care about. Accumulated wealth is the aggregation of years of past financial decisions regarding saving, investing, and borrowing; if we controlled for it, we would essentially be searching for an effect of education on this particular year's financial outcomes, conditioning on a summary measure of all past financial outcomes.

Table 4
IV estimates of the effect of years of schooling on income from various sources

Panel A: Census outcomes

	Indicator: Any income from investments		Indicator: Income from investments > \$500	Indicator: Income from investment > "cutoff" (SCF)	Indicator: Income from investment > "cutoff" (SIPP)	Amount of income from investments	Indicator: Any income from retirement savings	Amount of income from retirement savings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years of schooling	0.069***	0.075***	0.090***	0.161***	0.096***	1761.95***	0.059***	965.59***
	(0.005)	(0.005)	(0.007)	(0.019)	(0.016)	(128.59)	(0.010)	(129.66)
No. of observations	14,913,356	14,913,356	14,913,356	14,913,356	4,053,909	14,838,407	4,150,828	4,117,987
F-stat of excluded instruments	37.7	47.2	47.2	47.2	8.08	47.1	45.0	44.5
Cubic control for income	no	yes	yes	yes	yes	yes	yes	yes

Panel B: SIPP Outcomes

	Indicator: Any transactions account		Indicator: Any bond or government securities		Indicator: Any stocks or mutual funds	
	(1)	(2)	(3)	(4)	(5)	(6)
Years of schooling	−0.002	0.002	0.0650***	0.0689***	0.040*	0.041*
	(0.031)	(0.027)	0.02168	(0.020)	(0.024)	(0.021)
No. of observations	171,361	168,946	265,173	262,245	273,329	270,316
F-stat of excluded instruments	8.416	11.485	8.416	11.485	8.416	11.485
Cubic control for income	no	yes	no	yes	no	yes

This table reports results from 2SLS regressions of income and assets on years of schooling, gender, race, age (3-year age groups), birth cohort (10 year cohorts), state of birth, state of residence, survey year, and (where noted) a cubic polynomial in earned income. Only the education coefficient is reported. The first stage is a regression of years of schooling on compulsory attendance dummies for grades 9, 10, and 11 or more (requiring 8 or fewer years is the omitted category). Regressions also include state of birth 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*. We include 18- to 75-year-olds (50- to 75-year-olds when considering retirement income). In Panel A, the sample comprises individuals reported in the 5% samples of the 1980, 1990, and 2000 censuses. The dependent variable of interest is whether the household receives income from investments or retirement savings and the amount. In Column 4, "Cutoff (SCF)" is predicted from an OLS regression of other interest income on age, race, sex, earned income, and year indicators, where the sample includes households from the 1983, 1992, and 2001 SCF with other interest income less than \$50,000. In Column 5, "Cutoff (SIPP)" is predicted from an OLS regression of savings account interest on age, earned income, race, sex, and state of residence indicators, where the sample includes individuals from the 2001 SIPP. In Panel B, the sample comprises individuals from the 1990–2008 SIPP waves (Columns 1 and 2) and the 1984–2008 SIPP waves (Columns 3–6). In Panel A, Columns 6 and 8, top-coded individuals (see text) are dropped. Standard errors, corrected for arbitrary correlation within state of birth-year of birth, are in parentheses. (Numbers with * or *** indicate significance at the 10% or 1% level, respectively.)

outcome variable in two ways. First, we define a dummy equal to one if an individual has income from investments greater than $500 or any losses, presuming that an individual whose only financial asset is a savings account would have less than $500 in interest income and no losses. Columns 4 and 5 take this approach one step further by using the detailed financial data in the SCF or the SIPP to predict an individual's savings account interest based on the individual's age, earned income, race, sex and either survey year indicators or state of residence indicators, depending on data availability.[19] The outcome variable in these regressions is an indicator variable that is equal to one if an individual's investment income as reported in the census surpasses the threshold estimated from the second dataset or is negative.

In Column 6, we study the amount of income from investments and find a large and significant effect of education. The magnitude is substantial: an additional year of schooling increases investment income by $1760.[20] Finally, Columns 7 and 8 estimate the impact of education on retirement income. An additional year of schooling increases the probability of having any retirement income by 5.9 percentage points and the amount of retirement income by $966. The estimates are somewhat larger than the naive OLS estimates presented in Table 2, suggesting that the OLS estimates produce a downward bias in the impact of schooling on financial outcomes. We find similar effects when we use high school completion as the measure of schooling (see Online Appendix Table A5).

Panel B of Table 4 presents IV estimates of the effect of years of schooling on financial market participation using SIPP data. The first two columns show that education does not have a statistically significant impact on whether or not an individual has a transactions account, regardless of whether we control for a cubic polynomial in earned income. Columns 3–6 demonstrate that the positive relationship between years of schooling and ownership of bonds, government securities, stocks, or mutual funds persists even after addressing the omitted variable bias (with the instrumental variable strategy) and conditioning flexibly for noninvestment earnings. Note that the F-statistics of the excluded instruments are just strong enough (8.4–11.5) to satisfy the "nonweak" instrument criteria established by Stock and Yogo (2005). Unfortunately, data coverage for the value of assets held in these accounts is very often missing (the SIPP did not ask for this information every year), so we are not able to report estimates for the level of asset holdings using the SIPP data.

Looking at general ownership levels, we find that one more year of schooling increases the likelihood that an individual owns any bonds or government securities by about 6.5 percentage points and any stocks or mutual funds by 4

[19] We thank an anonymous referee for this suggestion.

[20] Using IV Tobit for investment income yields very similar results; results are in Online Appendix Table A4.

percentage points (p-value 0.06). These magnitudes are close to those in Panel A, Columns 1–3, supporting our interpretation of any investment income as a measure of financial market participation. This interpretation receives further support from the finding that increased education does not seem to increase transaction account ownership but does increase ownership of higher yielding investments. The "any investment income" measure from the census appears to be a useful proxy for broader financial market participation.

The point estimates of the causal impact of education suggest that it is a very important determinant of financial market participation. A convenient metric to compare the relative importance across different studies is the effect size, which is the effect of a one-standard-deviation change in the independent variable on participation. The effect size of education on any investment income is about 19 percentage points, and the effect size of education on having bonds or government securities and stocks or mutual funds is about 11 percentage points. The magnitudes of these effects are larger than the magnitudes of trust (4 percentage points; Guiso, Sapienza, and Zingales 2008), peer effects (1.15 percentage points; Hong, Kubik, and Stein 2004), and experience with stock market returns (4.2 percentage points; Malmendier and Nagel 2011).

Three studies of retirement savings plan participation serve as additional benchmarks for evaluating the quantitative importance of education for financial outcomes. Duflo and Saez (2003) present evidence from a randomized evaluation that minor incentives ($20 for university staff attending a benefits fair) can increase retirement plan participation rates by 1.25 percentage points. Duflo et al. (2006) offered low-income tax filers randomly assigned levels of IRA contribution matches. They find that an offer of a 50% match increased IRA participation by 14 percentage points, which is comparable to two years of education in our analysis. However, no determinants of retirement plan participation have been found to be more effective than simply changing the default enrollment status for 401(k) plans. Beshears et al. (2008) find changing the default to enroll, increases participation by as much as 35 percentage points.

Taken together, having used a credible identification strategy with two different datasets, we find that these results present a consistent picture: more education causes households to be more likely to invest in high-return assets, such as equities, and to report higher levels of financial income.

3. Education and Credit Management

3.1 Empirical strategy

Our analysis of the effects of education on credit management is complicated by the fact that the credit bureau data do not have information on the key right-hand side variable, education, rendering standard OLS and IV estimation impossible. We take two approaches to deal with this problem. First, we estimate the reduced-form relationship between compulsory schooling laws and credit management as represented by the following equation:

$$y_i = \alpha + \beta_9 Comp9 + \beta_{10} Comp10 + \beta_{11} Comp11 + \gamma X_i + \varepsilon_i, \quad (3)$$

where y_i is a credit management outcome, and *Comp9, Comp10,* and *Comp11* are dummy variables for the number of years an individual was required to attend school. The vector X_i includes control variables that are similar to the ones used in the analysis of the SIPP and census datasets. Because the credit bureau data do not contain information on race, gender, or income, these variables are omitted. The credit bureau does, however, include the ZIP code at which an individual lives, and we use ZIP code-level fixed effects to control for income and other sources of heterogeneity in some specifications.

The coefficients β_9, β_{10}, and β_{11} represent the effect of additional years of compulsory schooling on credit outcomes, which is the policy-relevant effect of the compulsory schooling laws. Because we have already shown that there is a strong positive relationship between these compulsory schooling variables and education (see Table 3), we can infer a lot about the relationship between education and credit outcomes from the estimated coefficients in Equation (3). For example, if *Comp9 – Comp11* are positively related to an individual's credit score, we can infer that education is positively related to an individual's credit score. We also estimate a variation on Equation (3), in which *Comp9 – Comp11* are represented as a single variable equal to the number of years an individual was required to attend school.

Although the reduced-form strategy is easy to interpret and is of interest for policy because it captures the impact of compulsory schooling law changes on the population, it does not provide a sense of the magnitude of the structural parameter of interest and is not comparable to the LATE estimates discussed earlier. To produce comparable estimates of the causal effect of education on credit outcomes, we take a two-sample instrumental variables approach, following Angrist (1990).[21] This strategy requires only that the instrumental variables and other right-hand side variables are available in both datasets, a requirement that is satisfied by the census and credit bureau dataset because they both contain information necessary to create the instrumental variables: state of birth and year of birth.[22]

Specifically, we use the census data to produce the first-stage regression of education on compulsory schooling (Equation (2), similar to the results presented in Column (1) of Table 3, except that the sample is restricted to data from the 2000 census, so that it is aligned with the credit bureau data). Because all the variables used to predict years of schooling are available in

[21] Two-sample IV is relatively rare in the finance literature but is used in Bitler, Moskowitz, and Vissing-Jørgensen (2005). We thank the editor for this suggestion.

[22] We use state of residence in the first quarter of the credit bureau panel to proxy for state of birth, because the FRBNY CCP/Equifax data do not include state of birth. Migration between birth and this date will add noise and make it more difficult to find an effect of education on credit management outcomes.

both the census and credit bureau data, we then use the point estimates from this regression to create a "predicted" level of education for each individual in the credit bureau data. Finally, we regress the credit outcomes of interest on this predicted level of education. The only complication is in how to correct standard errors for the fact that the right-hand side variable is predicted. We estimate standard errors in two ways. First, we provide robust standard errors, as described by Murphy and Topel (1985). Second, we use a block bootstrap technique to generate a distribution for the point estimate and use the standard deviation of this distribution for hypothesis testing.[23]

3.2 Empirical results

We begin by discussing the reduced-form estimates of the effect of compulsory education on credit management (Equation (3)). These estimates are presented in Table 5. The outcomes we examine are the probability of filing for bankruptcy or experiencing a foreclosure, credit scores, the fraction of a borrower's credit balance that is nondelinquent (averaged over the period that is covered by the data, 1999–2011), and the fraction of quarters that a borrower has any delinquent credit. Columns 1 through 3 of Panel A present evidence that compulsory schooling laws reduce the probability that an individual declares bankruptcy. Cohorts who are required to attend school through the 11th grade have a 0.98 percentage point lower probability of declaring bankruptcy than do cohorts who are not required to attend school beyond the 8th grade. The compulsory attendance dummies are jointly significant at the 1% level. Using years of schooling required (Column 2) yields an estimate that each additional year of required schooling reduces the probability of bankruptcy by 0.2 percentage points, significant at the 1% level. Column 3 adds ZIP code fixed effects, which control for geographic heterogeneity at a very fine level (there are approximately 43,000 ZIP codes in the United States). Given the limitations of the credit bureau data, the inclusion of ZIP code fixed effects is as close as we can come to controlling for income. The point estimate remains similar in magnitude and still significant.

Columns 4–6 study the effect of compulsory schooling on the probability that a household experiences a foreclosure. Relative to those who were able to drop out before 9th grade, cohorts in states that required attendance through the 11th grade were 1.2 percentage points less likely to experience a foreclosure. Finally, Table 5, Panel B, Columns 1–9 examine the reduced-form relationship between compulsory education laws and credit management, studying the credit score, the fraction of borrower balance that is nondelinquent (averaged over the period for which we have credit bureau data, 1999–2011), and the fraction of quarters a borrower has any delinquent credit. We find statistically significant effects on all three outcomes, but they are small in magnitude. Each year

[23] For a more detailed discussion of the two-sample instrumental variables technique, please see Section 4.4 of Angrist and Pischke (2008).

Table 5
Reduced-form estimates of the effect of eduction on credit outcomes, FRBNY Consumer Credit Panel/Equifax

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)
	Bankruptcy			Foreclosure		
Compulsory attendance = 9	−0.0024*			−0.0036***		
	(0.0014)			(0.0012)		
Compulsory attendance = 10	−0.0132***			−0.0061***		
	(0.0027)			(0.0015)		
Compulsory attendance = 11	−0.0098***			−0.0122***		
	(0.0017)			(0.0021)		
Years of compulsory schooling		−0.0021***	−0.0019***		−0.0022***	−0.0020***
		(0.0003)	(0.0003)		(0.0004)	(0.0003)
No. of observations	5,750,005	5,750,005	5,750,005	5,750,005	5,750,005	5,750,005
R^2	0.032	0.032	0.057	0.020	0.020	0.038
p-value for F-stat of compulsory attendance	0.000			0.000		
Additional fixed effects	none	none	ZIP code	none	none	ZIP code

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Credit score			% Balance current			% Quarters delinquent		
Compulsory attendance = 9	−1.096**			0.0003			0.0007		
	(0.510)			(0.0004)			(0.0006)		
Compulsory attendance = 10	1.461**			0.0006			0.0002		
	(0.619)			(0.0004)			(0.0007)		
Compulsory attendance = 11	1.669***			0.0012***			−0.0021***		
	(0.480)			(0.0003)			(0.0005)		
Years of compulsory schooling		0.253**	0.056***		0.0002**	0.0001***		−0.0003**	−0.0001
		(0.106)	(0.106)		(0.0001)	(0.0001)		(0.0001)	(0.0001)
No. of observations	5,732,690	5,732,690	5,732,690	5,329,619	5,329,619	5,329,619	5,750,005	5,750,005	5,750,005
R^2	0.141	0.141	0.231	0.020	0.020	0.051	0.045	0.045	0.083
p-value for F-stat of compulsory attendance	0.000			0.003			0.000		
Additional fixed effects	none	none	ZIP code	none	none	ZIP code	none	none	ZIP code

This table reports cross-sectional regressions of credit outcomes on education, measured by changes in compulsory attendance laws. The sample comprises a 5% panel of American borrowers, restricted to borrowers who have data in every quarter of the panel from 1999 to 2011. We include 35- to 75-year-olds. Bankruptcy and Foreclosure are indicators for having undergone bankruptcy or foreclosure at least once, respectively, between 1992 and 2011. Credit Score is averaged for each individual across all quarters of data, and it can range from 280 to 850. The % of Balance Current represents the nondelinquent balance on credit cards divided by the total credit card balance, averaged over the entire panel. The % of Quarters Delinquent represents the proportion of quarters an individual has any delinquent balance on his/her credit card bills. The independent variables of interest indicate whether the state in which the individual was born prohibited dropout until a child had completed 9th, 10th, or 11th grade and higher (requiring 8 or fewer years of schooling is the omitted category). Control variables included (coefficients not reported) in these regressions are dummies for 3-year age cohorts, 10-year birth cohorts, and state-of-residence. Regressions also include state of birth 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. State of birth is proxied by an individual's state of residence in the first quarter of 1999. Standard errors, corrected for arbitrary correlation within state of birth-year of birth, are in parentheses. (Numbers with *, **, or *** indicate significance at the 10%, 5%, or 1% level, respectively.)

of required schooling increases credit scores by 0.253 points, increases the percentage of borrower balance that is current by 0.02 percentage points, and reduces the percentage of quarters delinquent by 0.03 percentage points. Note that it is not surprising that these effects are small: these are the effects of an additional year of required schooling, not an additional year of actual schooling. For many individuals, an additional year of required schooling will have no effect on actual schooling. The reduced-form results provide the average effect on the entire exposed cohort, including those for whom the change in compulsory schooling laws did not change their eventual years of education.

We are also interested in the structural effect of an additional year of schooling on individuals whose educational attainment was affected by the law. We use an instrumental variable strategy to explore this. As described above, we use a two-sample IV approach, because education levels are not available in the credit bureau data. The results are presented in Table 6. Panel A presents estimates using the entire time period from the first quarter of 1999 to the last quarter of 2011, whereas Panel B divides the data into pre- and postfinancial crisis periods. Within each panel, the top results are estimates of Equation (1) using the predicted level of education as the key independent variable and using Murphy and Topel (1985) standard errors. The bottom two rows of each panel repeat the same estimates using the standard deviation of the block bootstrapped point estimates as the standard error.

The results suggest that education has important causal effects on credit outcomes. The point estimate on the coefficient for years of schooling in Column 1, –0.033, is significant at the 1% level using Murphy and Topel standard errors, suggesting that an additional year of schooling would reduce the probability of declaring bankruptcy by 3.3 percentage points. This result is not significant when we use block-bootstrapped standard errors. In Column 2, we see that an additional year of schooling is estimated to reduce the probability of experiencing foreclosure by 5.7 percentage points, and this result is statistically significant at the 1% level using either Murphy and Topel standard errors or the bootstrap. These effects are strikingly large, especially relative to the mean. Over the 1992 to 2011 period, 14.4% of individuals declare bankruptcy, and 5.8% experience at least one foreclosure. However, it is important to note two things. First, because these outcomes are particularly bad outcomes, they may be especially relevant for the group of individuals whose education was affected by changes in compulsory schooling laws. It is possible that the LATE is larger than the effect of education on the average individual for credit management outcomes. This is in contrast to estimates of the impact of education on income, where LATE estimates are similar to the population parameters. Second, standard confidence intervals include smaller effects as well: as small as 1.1 percentage points for bankruptcy and 2.2 percentage points for foreclosure.

Table 6
Two-sample IV estimates of the effect of schooling on credit outcomes, FRBNY Consumer Credit Panel/Equifax

Dependent variable:	Bankruptcy	Foreclosure	Credit score	% Balance current	% Quarters delinquent
	(1)	(2)	(3)	(4)	(5)
Panel A: Entire time period					
Murphy Topel standard errors					
Years of schooling	−0.033***	−0.057***	7.705***	0.0052**	−0.0133***
	(0.011)	(0.018)	(2.781)	(0.0023)	(0.0035)
No. of observations	5,198,529	5,198,529	5,182,364	4,852,175	5,198,529
Bootstrap estimates					
Years of schooling	−0.033	−0.057***	7.705	0.0052*	−0.0133**
	(0.023)	(0.010)	(4.762)	(0.0031)	(0.0053)

Panel B: Pre- and postcrisis

	1999Q2–2007Q3		2007Q3–2011Q4	
	Bankruptcy (1)	Foreclosure (2)	Bankruptcy (3)	Foreclosure (4)
Murphy Topel standard errors				
Years of schooling	−0.022	−0.016***	−0.016*	−0.045**
	(0.014)	(0.005)	(0.008)	(0.022)
No. of observations	5,198,529	5,198,529	4,507,270	4,997,041
Bootstrap estimates				
Years of schooling	−0.022	−0.016*	−0.016***	−0.045***
	(0.024)	(0.009)	(0.005)	(0.008)

This table reports cross-sectional second-stage two-sample IV regressions of credit outcomes on education. The sample comprises a 5% panel of American borrowers, restricted to borrowers who have data in every quarter of the panel from 1999 to 2011. We include 35- to 75-year-olds. Bankruptcy and Foreclosure are indicators for having undergone bankruptcy or foreclosure at least once, respectively, between 1992 and 2011. Credit Score is averaged for each individual across all quarters of data, and it can range from 280 to 850. The % of Balance Current represents the nondelinquent balance on credit cards divided by the total credit card balance, averaged over the entire panel. The % of Quarters Delinquent represents the proportion of quarters an individual has any delinquent balance on his/her credit card bills. The independent variable, years of schooling, is instrumented using compulsory schooling laws. Control variables included (coefficients not reported) in these regressions are dummies for 3-year age cohorts, 10-year birth cohorts, and state-of-residence. Regressions also include state of birth 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. State of birth is proxied by an individual's state of residence in the first quarter of 1999. Panel A reports second-stage results for variables using all available quarters in the panel dataset. Panel B reports results for bankruptcy and foreclosure indicators separately for the precrisis period (1999Q2–2007Q3) and the postcrisis period (2007Q3–2011Q4). The top half of each panel reports robust standard errors, following Murphy and Topel (1985). The bottom half of each panel reports the standard deviation of the point estimates from 100 bootstraps using a block bootstrap method. (Numbers with *, **, or *** indicate significance at the 10%, 5%, or 1% level, respectively.)

Estimates of the causal impact of education on other aspects of credit management are somewhat smaller. A one-standard-deviation increase in education (2.7 years) would raise an individual's credit score by 20 points, increase the fraction of credit card balances kept current by 1.4 percentage points relative to an unconditional average of 95.6%, and reduce the percentage of quarters delinquent by 3.5 percentage points from a mean of 7.5 percent. A 20-point movement in the credit score is less than one standard deviation in credit score. However, there are certainly ranges in which such perturbations can be very important. For example, Chomsisengphet and Pennington-Cross (2006) document how a 20-point difference in credit score can affect both the cost and availability of certain home mortgage products.

In Panel B of Table 6, we analyze whether the impact of education on bankruptcy and foreclosure differs before and during the recent financial crisis. In Column 1, the dependent variable is whether the individual declared bankruptcy between the second quarter of 1999 and the third quarter of 2007, conditional on not having declared bankruptcy in the seven years prior to 1999. In Column 3, the dependent variable is equal to one if the individual declared bankruptcy between the third quarter of 2007 and the fourth quarter of 2011, conditional on not having declared bankruptcy before 2007. The point estimates for the effect of education on bankruptcy in both periods are similar, although the effect is only significant in the crisis period.

The estimated effect of education on foreclosures, by contrast, is strikingly different across the two periods. Whereas during the precrisis period an additional year of schooling reduced the probability of foreclosure by 1.6 percentage points, the effect nearly triples to 4.5 percentage points during the period that includes the financial crisis and its aftermath. These results are significant because bankruptcy and foreclosure are costly, both to individuals (resulting in lower credit scores and reduced access to credit) and to society (through the deadweight costs of debt collection [Cohen-Cole, Duygan-Bump, and Montoriol-Garriga 2009] and by reducing the property value of neighboring houses [Campbell, Giglio, and Pathak 2011]).[24]

4. How Does Education Affect Financial Outcomes?

The evidence presented so far shows that education has a causal impact on a broad range of financial outcomes. In this section, we examine whether this effect operates exclusively through higher labor income or whether education affects financial behavior directly.

4.1 Does labor income explain all the effect?

Although it is likely that some of the impact of education on financial outcomes is due to the fact that people with more education earn higher wages, our analysis suggests that this is not the only mechanism at work. First, as seen in Table 4 and in Online Appendix Table A3, education continues to have a strong impact on whether an individual has any financial income, retirement income, or owns stocks, bonds, or other financial assets when earned income is controlled for, either as a cubic polynomial or a ten-part spline.[25] This supports the claim that education increases investment income, retirement income, and ownership of stocks and bonds, *conditional* on an individual's wages.

[24] Campbell, Giglio, and Pathak (2011) estimate that a foreclosure reduces the value of the foreclosed house by $44,000, but depresses the value of neighboring houses by $148,000–$477,000.

[25] We include Zip code fixed effects when studying credit outcomes that capture a lot of the variation in income, because income itself is not available in the FRBNY CCP/Equifax dataset.

Second, a back-of-the-envelope calibration exercise suggests that the estimated increase in investment income is likely too large to be explained by higher wage earnings alone. Specifically, the following calibration helps us to think about the following question: does education raise investment earnings simply because households earn more money and continue to save the same fraction of income, or does education influence the savings rate as well? We caution that this calibration exercise is merely suggestive rather than definitive.[26]

Consider a 45-year-old individual. We assume (by way of simplifying the algebra) that he has earned a constant $20,000 (the average income for high school graduates in our sample) since the age of 20,[27] saves a constant 10% of his income at the end of each year, and earns a 5% return on his assets. We also assume that one additional year of schooling boosts his wage income by 10% (Acemoglu and Angrist [2000] estimate a wage increase of 7% per year of schooling). If the individual's savings rate did not vary with schooling, an additional year would increase his contribution to savings by $200 (income * return to education * savings rate = $20,000*10%*10%) per year, although the additional year of schooling would mean that he earned wages for one fewer year. At the end of his 45th year, this individual's accumulated savings would be $2800 higher,[28] and his investment income would be approximately $140 greater. This is substantially lower than even the lower bound of our point estimate's confidence interval, $1,500. In other words, the increase in investment earnings associated with the earnings impact of an additional year of schooling appears to be too small to explain our findings.

By contrast, if we assume that the year of education increased our hypothetical individual's income by 10% as well as his savings rate by 2.6 percentage points, an additional year of schooling would increase his annual savings contribution by $772 ($20000*1.1*0.126-$20000*0.1), yielding by age 45 an approximately $30,000 greater asset base[29] and a corresponding increase in investment income of $1504.

Alternatively, we can ask what the returns to education for labor income would have to be to yield the $1500 increase in investment income we observe,

26 For example, this exercise cannot rule out more elaborate mechanisms that operate through wage income, but does provide some indication of how large their impact would have to be. Alternative mechanisms that do not operate through education-induced changes in financial behavior would include, for example, matching with more attentive financial planners, who induce greater savings. Alternatively, increased wage income may lead to marrying a spouse with higher income and in turn greater financial market participation and higher investment earnings. We analyze individual, rather than household, outcomes, so we think it is unlikely that the effects documented are explained by spousal income. We thank an anonymous referee for pointing out these possibilities.

27 Using the average income at each age gives similar estimates. In the following estimates, we use the annuity formula (*Amount Saved* $\frac{(1+i)^n-1}{i}$), where n is the number of years an individual saves, and i is the rate of return he earns on savings.

28 $20{,}000*1.1*0.1*\frac{(1+0.05)^{25}-1}{0.05}-20{,}000*0.1*\frac{(1+0.05)^{26}-1}{0.05}=2773$

29 $20{,}000*1.1*0.126*\frac{(1+0.05)^{25}-1}{0.05}-20{,}000*0.1*\frac{(1+0.05)^{26}-1}{0.05}=30{,}073$

if education did not affect the savings rate or investment returns: the answer is 38.6% per year of additional schooling, an amount much higher than the 10% estimated in the literature.[30] As a final alternative, we could accept the 10% return to education, but assume that baseline savings were higher. This would require a baseline savings rate of 108.2% of income[31] (holding baseline income constant) or a baseline annual income of $216,500 (holding the baseline savings rate at 10%).[32] Even jointly adjusting the parameters to obtain the observed increase in investment income produces baseline income, returns to schooling, and savings rates that are much higher than found in the literature: a $32,000 annual income (without an extra year of schooling), together with a return to schooling of 18% and a savings rate of 18%, for example, will produce the observed increase in investment income.[33] In each case, at least one parameter (baseline income, savings rate, wage return to schooling) is calibrated much higher than its estimated value in the literature, suggesting that wages alone cannot explain the estimated increase in investment income.

A 2.6 percentage point increase in the savings rate is economically significant. In our view, the most plausible conclusion from these exercises is that the estimated minimum effect of an additional year of schooling on investment income ($1,500) is likely the result of both higher labor market earnings and faster financial asset accumulation—individuals accumulate assets faster, both because they save more and because they save in assets with higher returns (e.g., equities).

We can use additional outcome variables from the census to further explore the mechanisms by which education affects financial outcomes. As before, these estimates of Equation (1) use the compulsory schooling laws as instruments, and they are available in Online Appendix Table A6. The first outcome we examine is an indicator variable that is equal to one if an individual reports negative investment income, conditional on reporting any positive or negative investment income. Individuals with more education are significantly less likely (p-value of 6%) to report negative investment income (see Column (1) of Online Appendix Table A6). Because the S&P 500 annual returns in 1979, 1989, and 1999 (the years for which investment income is reported in the 1980, 1990, and 2000 censuses, respectively) were generally quite high (12.31%, 27.25%, and 19.53%, respectively), negative investment income in these years may suggest investment mistakes or, at a minimum, deviation from the standard market portfolio. Of course, other circumstances can produce negative investment income: individuals may sell investments at a loss for liquidity and ex ante

30 $20{,}000*1.386*0.1*\frac{(1+0.05)^{25}-1}{0.05}-20{,}000*0.1*\frac{(1+0.05)^{26}-1}{0.05}=30{,}073$

31 $20{,}000*1.1*1.082*\frac{(1+0.05)^{25}-1}{0.05}-20{,}000*1.082*\frac{(1+0.05)^{26}-1}{0.05}=30{,}001$

32 $216{,}500*1.1*0.1*\frac{(1+0.05)^{25}-1}{0.05}-216{,}500*0.1*\frac{(1+0.05)^{26}-1}{0.05}=30{,}015$

33 $32{,}000*1.18*0.18*\frac{(1+0.05)^{25}-1}{0.05}-32{,}000*0.18*\frac{(1+0.05)^{26}-1}{0.05}=29{,}978$

good investments can go sour. Nevertheless, this evidence is consistent with education leading to better financial decision making.

While the analysis of the credit bureau data suggests that additional education prevents poor credit decisions, the census data also provide some information about credit usage. In particular, individuals are asked whether they have first and second mortgages. We find that education has no effect on whether a household takes out a first mortgage (Online Appendix Table A6, Column 2) but that an additional year of schooling significantly reduces the likelihood a household takes out a second mortgage (Online Appendix Table A6, Column 3). Taking on a second mortgage suggests a preference for greater consumption, relative to ability to pay. This finding is consistent with better-educated individuals choosing lower levels of leverage to acquire an asset, housing, with volatile prices. This result is also consistent with our finding that better-educated individuals experienced lower foreclosure levels.[34]

4.2 Why does education matter: Specific knowledge or improved cognitive ability?

What is it about additional schooling that improves financial outcomes? Does the improvement come from course content (such as from a personal finance course) or other skills or abilities they may acquire? One possibility that has received some attention is the fact that high school students in many states are required to attend financial education courses. Bernheim, Garrett, and Maki (2001) study mandatory high school financial education requirements, finding that increased exposure to financial curricula raises subsequent asset accumulation. However, Cole, Paulson, and Shastry (2013) revisit this question using U.S. census, SIPP, and credit bureau data and provide evidence that high school financial education, as mandated by states, did not in fact have any effect on financial outcomes. Instead, Cole, Paulson, and Shastry (2013) find that exposure to high school math courses affects the same financial outcomes studied in this paper, such as investment income, bankruptcy, foreclosure, delinquency, and additional outcomes, such as real estate equity.

Recent evidence from the labor literature suggests that a principal benefit of education is to increase cognitive ability (Hanushek and Woessmann 2008). To attribute our findings to education's impact on cognitive ability would require both a causal effect of education on cognitive ability and, in turn, a causal impact of cognitive ability on financial decisions. We cite previous literature to establish the first link.[35] For the second link, we first note that a growing body

[34] Other IV estimates using the census data indicate that individuals whose educational attainment was increased by changes in compulsory schooling laws are more likely to have jobs that provide pensions and that they are more likely to live in neighborhoods in which a higher share of older individuals have retirement income other than Social Security. See Online Appendix Table A6, Columns 4 and 5. This finding is consistent with individuals choosing to live in places in which their neighbors' behavior may reinforce good financial decision making. Hong, Kubik, and Stein (2004) find that peer effects are important determinants of financial market participation.

[35] Cascio and Lewis (2006) use variation in schooling generated by school entrance cutoff dates to show that teenagers with an additional year of high school score higher on the Armed Forces Qualifying Test (AFQT).

of literature has documented a strong correlation between cognitive ability and financial decision making.[36] A limitation of this literature, however, is that cognitive ability itself may be 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.[37] The importance of family background implies that the coefficient from a regression of investment behavior on measured IQ that does not correctly control for parental circumstances may be biased upward.[38]

In Online Appendix Table A8, we provide compelling evidence that cognitive ability increases financial market participation by studying siblings, who grew up with similar backgrounds. Labor economists have used this technique extensively to identify the effect of education on earnings (see, e.g., Ashenfelter and Rouse 1998). Including a sibling-group fixed effect controls for a wide range of observed and unobserved characteristics, including family background, and most of the remaining variation in cognitive ability is thus attributable to the random allocation of genes to each child.[39,40]

We use the National Longitudinal Survey of Youth (NLSY), which includes various measures of cognitive ability, to study the effect of cognitive ability

Black, Devereux, and Salvanes (2011) find a small effect of additional schooling in Norway on IQ scores measured at age 18, using variation in school starting age and test date. Carlsson, Dahl, and Rooth (2012) use similar variation from Sweden and find that schooling affects certain types of intelligence tests (synonym and technical comprehension), but not others (spatial and logic tests), using random variation in the assigned test date for 18-year-old males.

36 Christelis, Jappelli, and Padula (2010) use a survey of households in Europe that directly measured household cognitive ability using math, verbal, and recall tests. They find that cognitive abilities are strongly correlated with stock market participation. Grinblatt, Keloharju, and Linnainmaa (2011) find that Finnish individuals with higher IQs are more likely to participate in equity markets. Grinblatt, Keloharju, and Linnainmaa (2012) find that high-IQ traders select better stocks and exhibit fewer behavioral biases than do low-IQ traders. These papers indicate that the quality of financial decision making is correlated with cognitive ability. The degree to which causal interpretation may be assigned depends on the determinants of cognitive ability.

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

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

39 Plomin and Petrill (1997) note that the correlation in IQ of monozygotic (identical) twins raised together is much higher than that for dizygotic (fraternal) twins raised together.

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

on the ownership of a range of financial products.[41] We find significant positive effects of proxies for cognitive ability on investment income, savings, ownership of stocks, bonds, or mutual funds, participation in tax-deferred accounts, ownership of certificates of deposit, and borrowing behavior. This analysis suggests that education improves financial decision making. Education improves cognitive ability, and cognitive ability appears to improve financial outcomes (controlling for family background and other potentially confounding effects), likely by helping individuals reason through complex financial decisions.

Linking education definitively to "smarter" financial decision making (e.g., alpha) is extremely challenging because education may affect many intermediate factors, such as labor market opportunities and the quality of financial advice, as well as more nebulous factors, such as temperament and discount rates (see, e.g., Bauer and Chytilová 2010). One approach might be to try to isolate other factors by conducting a laboratory-style elicitation of the knowledge and preferences of 18 year olds; this would come with the cost of examining only artificial decisions. We therefore view our analysis as providing suggestive evidence that education causes smarter financial decision making, rather than definitive proof. We find that better-educated individuals systematically exhibit behaviors that are associated with increased savings and better financial management: greater financial market participation, increased equity ownership, higher credit scores, fewer instances of negative investment earnings, less leverage when purchasing a house, less delinquency, and fewer instances of foreclosure. These findings persist when we control for earned income and the magnitudes are likely too large to be attributable solely to the impact of education on wages.

5. Conclusion

This paper contributes to a growing body of literature that explores the importance of nonneoclassical factors in household investment decisions. We provide precise estimates of the causal effect of education on financial management outcomes and explore potential mechanisms. We first use instrumental variable techniques to show that education significantly increases investment income. Individuals with one more year of schooling are 7.5 percentage points more likely to report nonzero (positive or negative) investment income.[42] Similarly, those with more years of schooling are

[41] In a working paper, Benjamin, Brown, and Shapiro (2006) compare siblings in the NLSY to examine the relationship between cognitive ability and outcomes related to behavioral biases, one of which is low financial market participation. We estimate the impact on a wider range of assets and use broader measures of cognitive ability. More details are provided in the Online Appendix.

[42] As described in detail in Section 1.1, the census collects limited information on financial wealth, resulting in some limitations to this measure of financial market participation. We addressed these concerns by comparing the distribution of investment income to other financial outcomes in the SCF, by confirming that our results are

significantly more likely to report income from retirement savings. We find large causal effects on the intensive margin as well—individuals with more education report more of both types of income. We also show, using the SIPP data, that individuals with more schooling are more likely to own any bonds or government securities and stocks or mutual funds.

Second, we use two-sample IV techniques to show that cohorts induced to receive higher levels of education have higher credit scores, on average, and are significantly less likely to be delinquent, declare bankruptcy, or experience a foreclosure. Some of these effects are less dramatic than is the effect of education on financial market participation: an additional year of schooling raises an individual's credit score by 8 points (roughly 9% of a standard deviation). Other results are more dramatic: one year of schooling reduces the probability of bankruptcy by 3.3 percentage points from a base of 14.4%.

Having established the causal impact of education on a variety of financial outcomes, we provide support for our conclusion that education improves financial decision making. We demonstrate that education has important effects on financial outcomes, even when we control for income in flexible ways. In addition, we provide evidence that the point estimate of education's impact on investment income is difficult to explain with higher wages alone. We also show that education lowers the likelihood of having negative financial income or taking on a second mortgage, which suggests that education causes better financial decision making. Finally, we discuss evidence that, although specific knowledge gained in school (through personal finance courses) is not related to financial outcomes, the skills acquired in math courses or as measured by tests of cognitive ability do have a causal effect on similar financial outcomes. Importantly, the cognitive ability results control for family background by comparing siblings raised together.

The conclusion that education affects financial outcomes has implications for education policy. Specifically, considering only the increases in labor earnings when evaluating education would mean underestimating both the private and social returns to human capital investment. For example, education reduces bankruptcy and foreclosure, both of which are likely to have significant social costs. Moreover, a growing body of evidence suggests that individuals do often make financial mistakes (Agarwal et al. 2007), and both microevidence (Agarwal and Mazumder 2013) and recent experience suggests that some of these mistakes can be quite costly. Increasing educational attainment in the United States could dramatically improve households' financial management, reduce bankruptcy and default rates, and potentially support overall financial stability (Mian and Sufi 2011).

robust to alternate definitions of financial market participation, and including data on direct equity ownership in the SIPP.

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