High School Curriculum and Financial Outcomes

The Impact of Mandated Personal Finance and Mathematics Courses

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ABSTRACT

Financial literacy and cognitive capabilities are convincingly linked to the quality of financial decision-making. Yet, there is little evidence that education intended to improve financial decision-making is successful. Using plausibly exogenous variation in exposure to state-mandated personal finance and mathematics high school courses, affecting millions of students, this paper answers the question "Can high school graduation requirements impact financial outcomes?" The answer is yes, although not via traditional personal finance courses, which we find have no effect on financial outcomes. Instead, we find additional mathematics training leads to greater financial market participation, investment income, and better credit management, including fewer foreclosures.

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I. Introduction

The recent financial crisis has focused a spotlight on household financial decision-making with many policymakers arguing that poor decision-making exacerbated the crisis as borrowers took out mortgages they could not repay. Indeed, post-crisis regulatory reform has sought to improve financial decision-making. The Dodd-Frank Act established an "Office of Financial Education" within the Consumer Financial Protection Bureau to develop and implement a strategy to improve the financial literacy of consumers (Dodd-Frank Act, Title X, Section 1013). This federal effort comes in addition to state initiatives requiring high schools to include personal finance in their standard curriculums. High school provides an opportunity to offer programs that can achieve near-universal coverage. As of 2009, 44 U.S. states included "personal finance" in their standard high school curriculum (Council for Economic Education 2010).

Advocates of financial education programs point to a well-documented association between financial literacy and the quality of financial decision-making (for example, Hogarth and O'Donnell 1999; Hilgert and Hogarth 2003; Campbell 2006; Lusardi and Mitchell 2007; Mandell 2007; Stango and Zinman 2009; Lusardi and Tufano 2009; van Rooij, Lusardi, and Alessie 2011).[1] However, evidence that financial education has a causal effect on financial outcomes is at best mixed. Financially illiterate households are likely to be poorer and less educated than financially literate households making it difficult to isolate the impact of financial literacy from other factors associated with poor financial outcomes. Mandell (2007) finds that students who earn high scores on financial literacy tests tend to come from well-off, well-educated households. As a result, researchers find it difficult to determine the causal impact of financial education.

In this paper, we overcome identification concerns by exploiting plausibly exogenous variation in exposure to personal finance and math courses induced by changes in state-level high school curriculum requirements. We study whether exposure to these courses has a causal impact on savings, investment, and credit management outcomes. We use three large data sets that together provide a wealth of information about financial outcomes: the 2000 U.S. Census, the Federal Reserve Bank of New York Consumer Credit Panel/Equifax Data (FRBNY CCP), and the Survey of Income and Program Participation (SIPP).

In contrast to a previous, influential study by Bernheim, Garrett, and Maki (2001), we find that state mandates requiring high school students to take personal finance courses had no effect on investment or credit management outcomes such as probability of reporting any investment income, the level of investment income, credit score, credit card delinquency, and the probability of bankruptcy or foreclosure. Nor do these mandates have a detectable effect on total financial assets or real estate equity. Second, exploiting state-mandated changes in high school mathematics curricula first studied by Goodman (2012), we demonstrate that requiring students to take an additional high

1. Another strand of literature links cognitive ability to financial decision-making: For example, Agarwal and Mazumder (2013) find that individuals with low math abilities are more likely to make costly financial mistakes, such as misreporting house values on loan applications. Similarly, Grinblatt, Keloharju, and Linnainmaa (2011, 2012) find that individuals with higher IQs are more likely to participate in the stock market and make better stock-picking decisions.

school math course increases the propensity to accumulate assets and the amount of real estate equity while reducing credit card delinquency and the probability of experiencing foreclosure.

The first substantive contribution of this paper is to provide compelling evidence that the mandated high school personal finance courses in the United States have not affected the financial outcomes of treated populations in a measurable way. We adopt a flexible empirical approach, which compares individuals in a given state who graduated just before a personal finance mandate went into effect to those in the same state who graduated just after the mandate. This framework allows us to show that Bernheim, Garret, and Maki's 2001 finding that mandating personal finance courses in high school can increase savings is not robust to the simple inclusion of state fixed effects.

In addition to an emphasis on savings, personal finance courses also promote the importance of credit management: budgeting, paying bills on time, and not taking on too much costly debt. We study these outcomes using the FRBNY CCP, a large, nationally representative data set maintained by a leading credit bureau. We find no effect of high school personal finance mandates on credit scores, late payments, or the probability of experiencing bankruptcy or foreclosure. These findings contrast with Brown et al. (2013), who study the impact of recent changes in personal finance, math, and economics high school curricula on credit management using the FRBNY CCP and find that financial literacy and math courses improve creditworthiness, but that economics education increases debt balances.[2] Brown et al. study policy changes that occurred much more recently (between 1999 and 2012) than the ones we study (1957–82 for financial education and 1984–94 for math). As a result, the population in their sample is quite young (aged 22–28). It is possible that material taught in a personal finance course in high school is more relevant to credit decisions made early in the life cycle or that the effects dissipate with age.[3]

Our findings do not necessarily imply that financial literacy does not matter or that financial education is never effective. Other interventions such as employer-provided education have been shown to improve savings behavior (Duflo and Saez 2003). Skimmyhorn (2013) studies a course provided to new Army recruits and finds improved retirement savings behavior but more limited impacts on credit management. Even outside of high school, however, the literature is mixed on the impact of financial education (Caskey 2006; Hastings, Madrian, and Skimmyhorn 2013). Gartner and Todd (2005) find no effect of a credit education course offered to first-year college students. Choi, Laibson, and Madrian (2011) find that teaching employees about the value of the employer match does not affect future savings plan contributions. Note that, even if they are effective, financial education programs provided through employers or in colleges are likely to miss a large fraction of U.S. households, particularly those that may have the most to lose from poor financial decision-making.

2. While our personal finance results conflict, our math results are consistent with Brown et al. (2013). Another closely related study is Bruhn et al. (2013), which evaluates a high school-based financial education program in Brazil using a large-scale randomized control trial. The authors find significant improvements in financial knowledge and self-reported financial behaviors for the students and their parents.

3. An alternative mechanism could be that, as in Agarwal et al (2008), individuals learn from costly financial mistakes and that high school math or financial coursework facilitates that learning.

In randomized controlled trials outside the United States, Cole, Sampson, and Zia (2011) find that a financial program targeted at unbanked individuals had at best a weak effect and only on those with very low initial financial literacy. Bruhn, Ibarra, and McKenzie (2013) evaluate a large program in Mexico and find low takeup and no impact on financial outcomes. Carpena et al. (2011) find that a financial education program in India improved product awareness and attitudes toward making decisions, but did not improve decisions that required numerical skills. Surveying the literature, Xu and Zia (2012) indicate that while there are strong correlations between financial behavior and financial literacy across a range of data sets and contexts, "there is little experimental evidence" that financial education can affect savings and retirement decisions and that the nonexperimental evidence is "mixed." Moreover, they highlight a near complete lack of knowledge as to whether course content, design, and delivery methods matter.

The second focus of our paper relates to the impact of math coursework on financial decision-making. A growing body of evidence finds that financial mistakes are more likely among those with worse math skills (for example, Agarwal et al. 2009 and Agarwal and Mazumder 2013). While many households invest in a narrow set of financial products, even credit card contracts and mortgages involve complicated tradeoffs. Stango and Zinman (2009) find that many individuals greatly underestimate the speed at which compound interest accumulates and that those that make the biggest mistakes borrow the most. There is also a tight link between math skills and financial literacy. Two of the three standard questions used to measure financial literacy, pioneered by Lusardi and Mitchell (2011), are mathematical: 1. What is the future value of \$100 saved over five years at a 2 percent interest rate? And 2. How does the real value of savings change in an environment with 1 percent interest and 2 percent inflation?[4] The evidence we present suggests that math education may be an important tool for improving financial decision-making. We provide clear, causal evidence that additional math training can improve financial outcomes. Those required to take additional math courses in high school report \$1,500–3,000 higher home equity (from a base of \$15,500) and are 0.4 percentage points less likely to experience a foreclosure (from a base of 9 percent). A caveat to this finding is that the math reforms were sometimes accompanied by changes in graduation requirements for other subjects. We control for the number of other courses required, but we do not have enough statistical power to separately estimate the effect of each subject.

There are many possible channels through which math courses may affect financial outcomes. One possibility is that additional math courses increase labor income, enabling people to save more, earn more investment income, and borrow less. Math education may directly affect human capital, and it may channel students into higher paid majors and occupations (see Rose and Betts 2004, for example). While it is certainly possible that some of the effect of math courses on financial outcomes works through these channels, improved financial decisions that lead to increased savings rates or improved investment choices are also likely to be important. When we control flexibly for earned income, educational attainment, or occupation, the results do not change: Math courses have an effect even conditional on earned income, education, and occupation. More generally, our findings suggest that estimates of the return to education on

4. The third question asks whether it is riskier to buy a single company's stock or a mutual fund.

wages understate the true private return to schooling since they do not take into account future investment income. In addition, some of the outcome variables we study (foreclosure, for example) have important social costs, indicating that measures of the social return to education that ignore financial outcomes are also likely to be underestimated. These results complement the finding reported in Cole, Paulson, and Shastry (2014), which uses compulsory school laws to document that additional years of schooling increase financial market participation.

This paper proceeds as follows: The next section describes the three sources of data we use. Section III describes the empirical strategy used to analyze both natural experiments. Sections IV and V describe how financial outcomes are affected by personal finance and mathematics courses, respectively. Section VI provides a discussion of the results and Section VII concludes.

II. Context and Data

Mandated high school curriculum reforms present a uniquely attractive opportunity to study the causal relationship between different educational treatments and financial outcomes. A key challenge, however, is assembling data with sufficiently large samples to provide statistical power and sufficient coverage of financial outcomes. We focus on two key outcomes: asset accumulation, which relates directly to the concern that individuals do not save enough for emergencies or retirement (Lusardi and Tufano 2009); and credit management, which relates to the concern that many individuals take on too much debt (Leigh et al. 2012). The specific credit outcomes that we study are credit score, credit card delinquency, consumer bankruptcy, and mortgage foreclosure. We use three data sets to measure different aspects of financial behavior: the 5 percent sample from the 2000 U.S. Census, pooled panels of the SIPP, and the FRBNY CCP.

A. Asset Accumulation

We use two complementary data sets to measure different aspects of asset accumulation. We take advantage of the large sample size of the 2000 U.S. Census and augment these data with various waves of the SIPP, which allows us to explore a richer set of outcome variables. In 2000, one out of six households was sent the census long form, which includes detailed questions about each individual in the household, including their education, race, occupation, and income.[5] We use a 5 percent sample from the Public Use Census Data, which is a random, representative sample of the U.S. population.

The primary advantage to using census data is the sample size: The baseline specification using these data is based on 2.7 million observations. The large sample size allows for precise estimates and enables us to use flexible specifications that would not be possible with smaller data sets, such as including state and year of birth fixed effects. Whereas the census does not collect detailed information on wealth or financial decisions, information on all components of household income, including investment

5. The census questions are available on pp. 6–8 of this document: http://www.census.gov/dmd/www/pdf/d-61b.pdf.

income, is available. Thus, as one measure of financial asset accumulation, we use the census variable "income from interest, dividends, net rental income, royalty income, or income from estates and trusts." Individuals are instructed to report even small amounts credited to an account (Ruggles et al. 2004). We refer to this variable as investment income or asset income.[6]

Other, more specialized, data sets, such as the Survey of Consumer Finances (SCF), that are collected with particular attention toward correctly measuring complex financial information, suggest that the census measure of asset income provides a good proxy for financial wealth (see Appendix A for additional details).[7] The main limitation of using investment income rather than assets accumulated is that one cannot back out precise investment levels from investment income. While investment income is likely increasing in the quality of investment decisions, the former is likely not a perfect proxy for the latter. In addition, focusing on the sample of individuals who have investment income may lead to selection bias because individuals who own financial assets may have unobservable characteristics that distinguish them from those who do not. For these reasons, our analysis focuses on a dummy variable equal to one if the individual reports any investment income (positive or negative). The binary outcome measure can be thought of as a measure of financial market participation. We find similar results if we redefine the investment income dummy to be equal to one only if the absolute value of investment income an individual reports is more than \$500: This cutoff represents having a substantially greater level of financial market participation.

We also report results for the level of investment income[8] and the individual's position in the distribution of investment income, measured by the percentile rank in the nationwide distribution of investment income divided by total income.[9] Panel A of Table 1 provides summary statistics on demographics and financial outcomes in the census data.

We augment the outcome variables available in census data with outcomes from the SIPP. We pool the 1996, 2001, 2004, and 2008 SIPP panels. Each panel is a nationally representative sample of the civilian, noninstitutionalized population, with a total size of around 80,000 people. The sample size for analysis is smaller, approximately 20,000–53,000, because we focus on individuals born relatively close to changes in curricular

6. Investment income is reported for each individual, although households may pool investments. Our results are robust to aggregation by household.

7. Appendices can be found online at http://uwpress.wisc.edu/journals/journals/jhr-supplementary.html.

8. 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 losses. The income variable for individuals with investment income above a year-specific limit (\$50,000 in 2000) is replaced with the median income of all individuals in that state earning above that limit and all losses in excess of \$10,000 are replaced with \$10,000. The percentage of top-coded and bottom-coded observations is very low at 0.51 percent and 0.065 percent respectively. When necessary, we drop these observations. Of course, the dependent variable "any investment income" avoids this problem entirely. We also drop all observations where these values were imputed. We note that neither set of reforms affect the probability that an individual's investment income is imputed and that our results are robust to including these imputed values.

9. We include this measure to best match Bernheim, Garret, and Maki's outcome variable, described later on, and because it minimizes the impact of outliers. Because we are using investment income as a proxy for assets accumulated, we take the absolute value of investment income when calculating the percentile rank. For example, this specification treats someone with a loss of \$5,000 as having more assets than someone with zero investment income. The results are similar when we do not take the absolute value. Our investment income results are also nearly identical whether we include negative numbers for losses or take the absolute value of losses.

Table 1
Summary Statistics

	Personal Finance Sample		Math Sample	
	Mean	Standard Deviation	Mean	Standard Deviation
	1	2	3	4
Panel A: U.S. Census data				
Demographic				
Age	44.09	5.63	30.38	3.72
Male	0.49	0.50	0.48	0.50
Black	0.11	0.31	0.12	0.32
Married	0.71	0.45	0.60	0.49
Years of education	13.85	2.21	14.29	1.68
Affected by personal finance mandates	0.11	0.31		
Affected by increased math mandates			0.33	0.47
Earned income	34,838	43,958	29,607	22,236
Income from investments				
Any	0.23	0.42	0.15	0.36
Amount	728	3,358	314	2,134
Amount if nonzero	3,326	6,477	2,198	5,199
Percentile	27.96	42.06	14.86	33.94
Panel B: SIPP data				
Demographic				
Age	47.42	6.75	33.60	5.12
Male	0.47	0.50	0.47	0.50
Black	0.13	0.34	0.13	0.34
Married	0.63	0.48	0.59	0.49
Years of education	13.94	2.47	14.42	1.96
Affected by personal finance mandates	0.11	0.31		
Affected by increased math mandates			0.34	0.48
Earned income	25,469	24,994	25,184	22,573
Amount in				
Financial assets	16,179	38,042	9,420	25,101
Property equity	34,796	53,628	15,452	33,630

(*continued*)

Table 1 (*continued*)

	Personal Finance Sample		Math Sample	
	Mean	Standard Deviation	Mean	Standard Deviation
	1	2	3	4
Panel C: FRBNY CCP				
Demographic				
Age	44.08	5.65	29.82	3.39
Affected by personal finance mandates	0.12	0.33		
Affected by increased math mandates			0.38	0.49
Credit outcomes				
Credit score	692.55	94.56	651.57	95.46
Percent balance current	0.95	0.12	0.93	0.13
Percent quarters delinquent	0.10	0.17	0.12	0.18
Declared bankrupt	0.18	0.39	0.20	0.40
Foreclosed upon	0.08	0.27	0.09	0.29

Notes: This table reports summary statistics for the different samples used in this paper. Panel A uses the 5 percent sample of the 2000 census. Panel B uses the 1996, 2001, 2004, and 2008 panels of the SIPP. Panel C uses a 5 percent sample of American borrowers who have data in every quarter of the FRBNY CCP from 1999 to 2011. All amount variables are restricted to those observations that are not top- or bottom-coded. In all three panels, we include individuals born between 1946 and 1965 (the personal finance sample) in Columns 1 and 2 and individuals born between 1964 and 1976 (the math sample) in Columns 3 and 4.

mandates. Each household is surveyed every four months (waves) for three to four years. The survey is built around a core set of demographic and income questions, but each wave also includes topical modules.[10] The SIPP includes detailed questions on assets and liabilities, such as the ownership and market value of different types of assets, including stocks, bonds, mutual funds, IRAs, and 401(k)s.[11] The primary dependent variables derived from the SIPP data are total financial assets (amounts in savings and checking accounts, bonds and other securities, stocks, mutual funds, government savings bonds, 401(k)s, IRAs, Keogh accounts, and mortgages and other money owed to the respondent as well as equity in other financial assets) and total equity in real estate (own home, rental property, and other real estate).[12] Summary statistics for the SIPP data

10. For birth state, we use the second wave from each panel, and for the income, asset, and liabilities variables, we use the twelfth wave of the 1996 panel, the third waves of the 2001 and 2004 panels, and the fourth wave of the 2008 panel to ensure reasonably similar age ranges across data sets.
11. In addition, the SIPP distinguishes between accounts held solely in the respondent's name and those held jointly with a spouse. The value of jointly held assets is divided evenly and half the value is attributed to each spouse.
12. We drop values that are imputed or top-coded.

are given in Panel B of Table 1. The census and the SIPP are complementary, with the SIPP providing a broader range of outcome measures, but the census providing a much larger sample that generates precise estimates, which are particularly useful when documenting "zero" or no-effect results.

B. Credit Management

The third source of data is the FRBNY CCP, a quarterly panel of credit bureau data that begins in the first quarter of 1999 and continues to the third quarter of 2011. The information provided is similar to the data in an individual's credit report (see Lee and van der Klaauw 2010 for a detailed description). We use the primary sample, a randomly selected 5 percent sample of U.S. residents aged 18 or older who have a credit report. The sample is a nationally representative cross-section within each quarter, conditional on having a credit report. There are 3.7 million observations per quarter.

We use five outcome variables to measure credit management: credit score, the proportion of an individual's credit card debt that is current, the proportion of quarters in which an individual has any delinquent credit card balance, a bankruptcy indicator, and a foreclosure indicator. The credit score, similar to a FICO score, uses past credit management behavior to predict the likelihood that an individual will be 90 or more days delinquent over the next 24 months. Credit scores range from 280 to 850, with higher scores implying a lower probability of being delinquent. The credit score and the proportion of credit card debt that is current are averaged across all quarters. The bankruptcy and foreclosure variables indicate whether an individual has ever undergone bankruptcy or foreclosure, respectively, between 1992 and 2011.[13] Summary statistics for this data set are given in Panel C of Table 1.

III. Empirical Strategy

Identifying a causal effect of education is challenging. Studies that compare students who took certain courses to those who did not are likely to suffer from selection bias: Unless there is plausibly random variation in who enrolls in a course, the "treatment" and "comparison" groups are likely to vary along observable and unobservable characteristics (Meier and Sprenger 2013).[14] These issues may explain why studies find conflicting effects of financial literacy programs. Comparing students who participated in any high school financial literacy program to those who did not, Mandell (2007) finds no difference in financial literacy, while FDIC (2007) finds that a Money Smart financial education course has measurable effects on savings.

To ensure that we identify causal effects, we rely on two natural experiments previously identified in Bernheim, Garrett, and Maki (2001) (BGM hereafter)[15] and

13. We track these outcomes to 1992 because the credit bureau maintains bankruptcy Chapters 7 and 13 records for ten and seven years from the date of filing, respectively.
14. Glazerman, Levy, and Myers (2003) make this point convincingly when they compare nonexperimental and experimental studies and find that nonexperimental methods often provide incorrectly significant effects.
15. Another compelling study is Christiansen, Joensen, and Rangvid (2008), who use panel data and instrumental variables to demonstrate that economics education increases the likelihood of holding stock.

Goodman (2012). BGM use the imposition of state-mandated high school personal finance courses and study their impact on household savings, while Goodman uses changes in state laws regarding the number of math courses required for high school graduation and studies their impact on labor earnings.

One of the most methodologically compelling studies of the impact of financial education, BGM use a difference-in-difference approach that relies upon the assumption that changes in state-mandated high school requirements are unrelated to household savings, and therefore behavior changes following the mandate can be interpreted causally. BGM document that, between 1957 and 1982, 14 states imposed the requirement that high school students take a consumer education course with personal finance topics.[16] Working with Merrill Lynch, BGM conducted a telephone survey of 2,000 households, eliciting information on exposure to financial literacy training and savings behavior. They confirm that the mandates were implemented: Individuals who graduated following their imposition were more likely to report that they received financial education. They also find that those individuals save more: Those graduating five years after the mandate reported savings rates 1.5 percentage points higher than those who were not exposed to the mandate. One potential weakness of the BGM approach is that they do not include state or year fixed effects. If residents of different states differ in any way that is correlated with whether the states imposed a mandate, the estimates may be biased. Our findings suggest this is an issue. We reexamine this natural experiment, exploiting the larger sample size of the census and the FRBNY CCP as well as the wealth of financial outcome variables available in the SIPP. Our preferred specification is a flexible event study specification, but we also estimate specifications that are similar to those in BGM.

Studying math requirements, Goodman (2012) describes state policies on student coursework and reforms prompted by a 1983 National Commission on Excellence in Education report, "A Nation at Risk." The report recommended that state graduation requirements be strengthened and provided specific guidelines, recommending that high school students take four years of English, three years of math, science, and social studies, and one semester of computer science in order to graduate. Prior to the report, no state required three years of math and many states responded by increasing the number of math courses required for graduation, though not always to the recommended levels.[17]

16. Following BGM, we focus on the 14 states that included personal finance topics in the consumer education mandate: Delaware, Florida, Georgia, Hawaii, Idaho, Illinois, Nevada, New Mexico, North Carolina, Oklahoma, Oregon, South Carolina, Texas, and Wisconsin. In Figure A1, we provide a map that shows the timing of these mandates. We find qualitatively similar results using the consumer education mandates themselves. We have been unable to obtain data from state boards of education that would allow us to include mandates after 1982.

17. While many states increased the number of courses required to graduate in English, science, and social studies as well, we follow Goodman in focusing on the math requirements. We do so because math skills are closely related to financial literacy and financial decision-making but also because the math reforms were the most common reform: Thirty-eight of the 40 states that passed a reform specified the number of math courses required, while only 18 specified English requirements, 23 specified social studies requirements, and 32 specified science requirements. Only two states passed reforms without specifying math requirements, Montana and New York, and both specified only social studies requirements. In our main results, we follow Goodman and include as a control the total number of nonmath courses required (this allows us to compare our results to his first stage estimates). In robustness checks, we confirm that the results are unchanged when we control for the number of English, science, social studies, and other courses required individually, and that the results generally persist when we exclude states with concurrent changes in these other subjects taken one by one. We discuss other possible responses to the report, such as changes in instruction time, in Section VIB.

The reforms occurred between 1984 and 1994, and most of the first affected cohorts graduated from high school in 1987, 1988, or 1989.[18] Using a nationally representative sample of high school transcripts, Goodman (2012) shows that state math requirements increased the number of completed math courses by about 0.1–0.4 math courses, with larger point estimates for black individuals. Using a two-sample instrumental variable strategy and the same census data that we use, Goodman shows that an additional year of math significantly increases labor market earnings for black men (with weaker evidence for black women). He does not find significant evidence that additional high school math courses affect earnings for white men or women. We use a similar approach to study the impact of increased math courses on financial outcomes. Both natural experiments will identify causal effects if the appropriate assumptions are met.[19,20]

A. Empirical Model

The large size of the U.S. Census and the FRBNY CCP allows us to estimate flexible treatment specifications and include a large set of controls. We begin with a straightforward difference-in-difference specification but quickly follow that with our preferred event study specification. As will be seen, the event study results highlight the need to use a flexible specification that focuses on cohorts graduating close to the years the curricular changes were implemented or risk omitted variables bias from differential trends. While the straightforward difference-in-difference specification potentially suffers from this identification challenge, it is easy to interpret and facilitates the presentation of the event study analysis. We first estimate the following equation:

$$(1)\quad y_{isb} = \alpha_s + \gamma_b + \beta E_{isb} + \beta X_{isb} + \varepsilon_{isb}$$

where y_{isb} is a financial outcome and E_{isb} is a dummy variable for whether individual i, born in year b, was 17 or younger the year the mandate was implemented in his or her state of birth, s.[21] We include fixed effects for state of birth, α_s, and year of birth, γ_b. The

18. The temporal and geographic distribution of math graduation requirements is given in Figure A1.

19. While Goodman estimates the impact of taking an additional math course (in the two-sample IV strategy), we follow BGM and present the reduced form effects of both the math and personal finance curricular mandates. BGM do so because of heterogeneous course quality, making IV difficult to interpret, and the possibility of systematic measurement error in whether individuals remember taking such a course. We do this out of necessity: The data that we use do not provide information about personal finance or math courses taken in high school. While still a causal effect, the interpretation differs. We estimate the effect of being exposed to a mandate requiring a specific course, while an IV strategy (if feasible) would estimate the causal effect of taking the course.

20. We cannot estimate the combined impact of both mandates because the cohorts in our two samples overlap for only two years.

21. We define this variable based on when the mandate was passed in the individual's state of birth for the census and SIPP data (as does Goodman) and current state of residence for the FRBNY CCP data. BGM use the state in which the student attended high school. Neither the census nor the FRBNY CCP data provide this information, but even if they did, it is not obvious we would want to use it. It is possible that households may have moved in response to new educational policies, making mandate status in the state of high school attendance potentially endogenous. Nevertheless, our results are robust to using only individuals who are residing in the same state in which they were born. We present these results in Table A1, but it is not our preferred specification as nonmovers may not be a representative sample. The differences are, however, not large: 75 percent of high school aged children live in their state of birth according to the 1980 Census, approximately when our sample cohorts were in high school.

vector X_{isb} includes race, gender, census division linear trends, and other controls listed below.[22] Standard errors are clustered by state of birth to allow for within-state serial correlation (Bertrand, Duflo, and Mullainathan 2004).[23] Following BGM and Goodman (2012), we restrict the personal finance sample to those born between 1946 and 1965 (aged 35–54 in 2000) and the math sample to those born between 1964 and 1976 (aged 24–36 in 2000).[24,25]

The state and year of birth fixed effects help isolate the effect of the curriculum changes from unobserved time-invariant state and nationwide cohort characteristics that may be correlated with the reforms. To deal with the possibility of differential trends, we (1) allow separate linear time trends for each census division, and (2) estimate a more flexible event study specification that allows us to examine pre-existing trends as well as estimate separate treatment coefficients for each graduating class without assuming that the effect of the mandates was immediate, constant, or linear. The primary remaining challenge to identification is the possibility that other changes were introduced at the state level concurrent to the reforms we study. Following Goodman (2012), we control for a number of variables that capture other education policies affecting each graduating cohort in our math study.[26] Thus, the identifying assumption is that, conditional on state and year of birth, census division-specific trends, and these other control variables, cohorts that graduated before the reforms were no different from cohorts that graduated after the reforms.[27] This

22. Our results are robust to the inclusion of dummies for years of schooling, marital status, state of residence, occupation, industry, and a cubic polynomial for earned income. These are not in our preferred specification because they may be endogenous to the reforms. We also restrict our sample to two races (white and black). Note that these variables are not available in the FRBNY CCP data.

23. Standard errors do not change substantially when we cluster by state of birth X year of birth. In general they fall, but the conclusion that the personal finance mandates had no effect on investment income remains.

24. We limit the math sample to those graduating from high school between 1982 and 1994, which makes our results comparable with Goodman. We find similar results when we include cohorts graduating after 1994, but these cohorts are only 18–23 years old in 2000.

25. It is possible that the temporal distance between cohorts affected by math and personal finance interventions contributes to the differential effects we observe. We believe this is unlikely. Many of our outcome variables, such as investment income and net worth, accumulate over time; we would expect any effects to show up more for older cohorts (those affected by personal finance mandates) than for younger cohorts (affected by math requirements). We note that the personal finance results are quite similar using the 1990 versus the 2000 census and that both sets of results in the credit bureau data are quite similar if we compare 1999q2–2003q1 to 2003q2–2007q1. (We do not include periods after 2007q1 because these individuals were affected by the Great Recession.) This provides some suggestive, but not conclusive, evidence that lifecycle differences do not drive the differences.

26. We include these variables in the analysis of the increased math requirements but not personal finance courses because of data availability. We discuss them further in Section V.

27. One potential problem with our estimation strategy is the possibility that the student body in public and private schools changes in response to the imposition of educational mandates: If perceived improvements in public education, for example, led children to switch from higher-quality private schools to lower-quality public schools, we would underestimate the positive effects of mandates. We acknowledge that our math results are net of any such compositional changes. However, we believe any potential bias is quite small: The fraction of children attending private school is low. Our estimates focus on cohorts graduating within five years of the curriculum changes. Presumably, even those parents whose decisions might have been affected by curricular requirements would have already enrolled their children in either a public or private school and may not be likely to switch. We thank an anonymous referee for pointing this out.

assumption is clearly more defensible for cohorts closer to the reform date, which is why we prefer the event study analysis.[28]

For the event study analysis, we estimate the impact of state-mandated changes in math and personal finance course requirements through a series of event-year dummies. This provides an estimate of the average level of each outcome variable for individuals who graduated a given number of years before or after the implementation of the mandate, without imposing equality on the cohorts prior to, or following, the implementation, as is done in the simpler pre/post analysis. This strategy is perhaps easiest to convey graphically: Figure 1 plots the results (described in detail below) for the state mandates requiring a personal finance course. The line plots the level of the investment outcomes for cohorts that graduated from high school prior to the implementation of the mandates (left of the vertical line) and cohorts that graduated after the mandates (right of the vertical line) after controlling for state of birth, year of birth, division-specific trends, and other demographic variables. This specification allows the impact of the mandates to change over time, possibly as school systems learned how to comply (for example, trained teachers to teach personal finance or hired additional math teachers).

We implement this strategy by defining two sets of dummy variables to capture these different event-years. The first set of dummy variables, $D_{isb}^{1}, \ldots, D_{isb}^{T-1}, D_{isb}^{Tplus}$, denotes that the individual graduated from high school a given number of years after a mandate was implemented in his or her state of birth. For example, the D_{isb}^{1} takes on the value 1 if individual i born in state s and year b graduated from high school one year after the mandate was implemented in her state of birth and D_{isb}^{Tplus} equals 1 for individuals graduating T or more years after the mandate. We use a T of 15 for personal finance and six for math since we have fewer cohorts graduating after the math mandates in our data. The second set of dummies, $D_{isb}^{-(T+1)plus}, D_{isb}^{-T}, D_{isb}^{-(T-1)}, \ldots, D_{isb}^{-1}$, allows us to test the identification assumptions by examining the trend in financial outcomes for cohorts graduating prior to the mandates. These capture whether the individual graduated from high school a given number of years before the mandate was passed. For example, D_{isb}^{-1} takes on the value 1 for individuals who graduated one year before the mandate was passed in their state of birth and $D_{isb}^{-(T+1)plus}$ equals 1 for individuals who graduated $T + 1$ or more years before the mandate passed. The omitted category is individuals born in states that never implemented a mandate or who graduated from high school the year the mandate was passed: All $2T + 1$ dummies are zero. The state fixed effects ensure that the coefficients on these dummy variables are conditional on state of birth.

We thus estimate the following equation:

$$(2) \quad y_{isb} = \alpha_s + \gamma_b + \gamma_{-(T+1)} D_{isb}^{-(T+1)plus} + \sum_{k=-T}^{-1} \gamma_k D_{isb}^{k} + \sum_{k=1}^{T-1} \gamma_k D_{isb}^{k} + \gamma_T D_{isb}^{Tplus} + \beta X_{isb} + \varepsilon_{isb}$$

where y_{isb}, X_{isb}, α_s, and γ_b are as defined above.

28. It is possible to focus on these cohorts using the simple difference-in-difference strategy but not without making some assumptions about how many graduating cohorts are similar "enough." Instead, the event study allows us to view all the cohorts graphically.

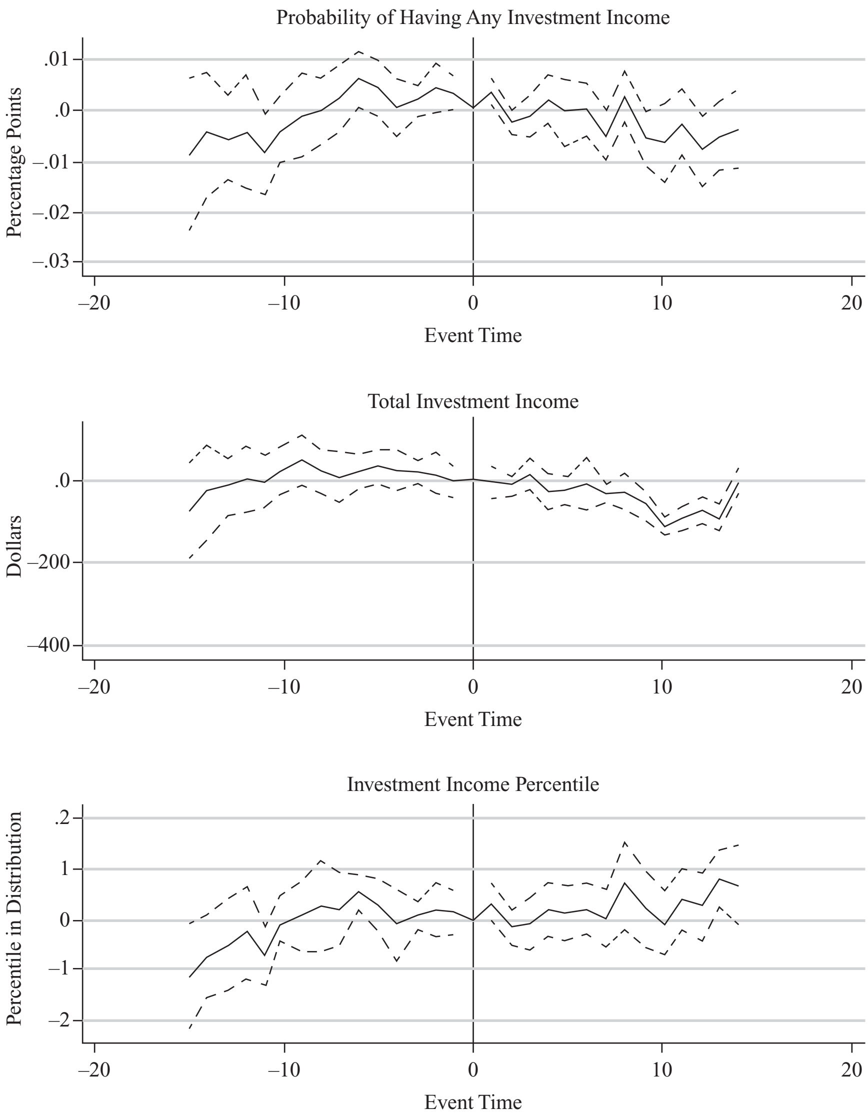

Figure 1

Personal Finance Mandates and Investment Income

Notes: This figure plots the evolution of three data series prior to and following the imposition of state-mandated personal finance courses. The dotted lines show confidence intervals at the 95 percent level. The vertical line indicates the year in which the personal finance course was mandated.

Using event-year dummies has two important advantages. First, it allows the data to determine how the mandate affects the outcome: The effect can be constant, increasing, decreasing or even nonmonotonic.[29] Second, it provides a clear and compelling comparison to the simple difference-in-difference strategies in Specification 1 and the specification used by BGM. The simple difference-in-difference strategy relies on the assumption that trends in financial outcome variables would have been the same between states that did and did not impose the mandates. While it is impossible to test this assumption exactly, our flexible specification allows us to examine trends prior to the mandates to see if they differ for the states that eventually passed the mandates.

The following finding would provide strong and convincing evidence that financial literacy education is effective: The coefficients D^k_{isb}, for $k<0$, would be statistically indistinguishable from zero and display no obvious trend and the coefficients on $D^1_{isb}, \ldots, D^{T-1}_{isb}$ and D^{Tplus}_{isb} would be positive and statistically significant. In other words, prior to the imposition of the mandates, financial outcomes would not have been trending up or down differentially in states that imposed the mandate and the mandates would lead to improved outcomes for cohorts graduating after they were implemented. Figure 1 provides a preview of the results for the personal finance mandates—namely, that there is no effect of personal finance education on investment income.

IV. Impacts of the Personal Finance Mandates

In this section, we present estimates of the reduced form impact of state personal finance mandates on financial outcomes using the census, SIPP, and FRBNY CCP data. Using both a difference-in-difference specification that accounts for unobserved state and birth year heterogeneity and the flexible specification described above, we find no impact of financial education mandates on a range of financial outcomes, in stark contrast to BGM. To understand whether the different data sources could account for this, we estimate BGM's specification using our data and replicate their findings. We show that the divergent results stem from the fact that states that imposed personal finance mandates were systematically different from those that did not. We discuss suggestive evidence that states imposed mandates during periods of particularly high economic growth. This implies that the exclusion restriction required for BGM's specification to be valid may not hold. In other words, the imposition of the mandates appears to be related to other potential drivers of household savings behavior. Our strategy, which includes controls for unobserved state and birth-year heterogeneity, accounts for this because it does not simply compare those who were exposed to the mandates to those who were not exposed, but instead focuses on those who graduated within the same state within a few years of the mandates taking effect.[30]

29. For example, the effect could be nonmonotonic if a state takes time to fully implement reforms but eventually loses interest in enforcing the reform.

30. The simple difference-in-difference strategy we present first could also suffer from this problem despite the division-specific trends, which is why we prefer the event study analysis.

A. Asset Accumulation

Table 2 presents results from Equations 1 and 2 using census data (Columns 1–3) and SIPP data (Columns 4–5). Column 1 presents the estimates for a linear probability model, with any investment income, a dummy variable equal to 1 if the household reports any asset income, as the dependent variable. The dependent variable in Column 2 is the level of total investment income, and in Column 3 it is the individual's location in the nationwide distribution of the ratio of investment income to total income. The outcome variables in Columns 4 and 5 are the value of all financial assets and equity in real estate, respectively. Panel A presents the estimates of Equation 1, the difference-in-difference regression, displaying only the coefficient on the dummy indicating an individual was exposed to the reform. None of the coefficients are significant at the 5 percent level and, in fact, most of them are negative. The estimates using the census data (Columns 1–3) are also very precisely estimated due to the extremely large sample size. Not only do we see that the mandate had no statistically significant effect on whether an individual had any investment income, we can rule out effects bigger than a 0.108 percentage point increase, on a base of 23 percent. Similarly, we can rule out a positive effect of more than \$3 on investment income with 95 percent confidence.[31]

Panel B presents the estimates of Equation 2. The specifications include all event-years from 15 years prior to the imposition of the mandates to 15 years after the mandates, but to conserve space only the coefficients on the five event-years on either side of the imposition of the mandate are included in the table. Recall that the coefficients represent the estimated difference in the outcome between the particular cohort and the cohort that graduated in the year the mandate was implemented, conditional on state of birth. Note that these changes are not time or age effects since the birth-year dummies absorb any common changes.

The event study results confirm that personal finance mandates did not have any measurable impact on asset accumulation. Consider the first dependent variable, any investment income: There is no sustained increase for cohorts graduating after the mandate. Individuals who graduated exactly one year after the mandates were imposed in their state of birth are significantly more likely to report any investment income, but this "effect" goes away immediately, suggesting it is spurious. We formally test this hypothesis by comparing the average propensity to accumulate assets in the five cohorts before to the five cohorts after the mandate was imposed. For any investment income, the average value of γ_k is 0.0029 for $k \in \{-5, -4, -3, -2, -1\}$, and 0.00042 for $k \in \{1, 2, 3, 4, 5\}$. The F-test (p-values reported in the final rows of Table 2) of the hypothesis that these averages are equal to each other cannot be rejected: Any investment income is the same for cohorts graduating within five years of the mandates regardless of whether they graduated before or after it was imposed. The standard errors tell us the

31. As described above, we focus on reduced-form, intent-to-treat estimates of these mandates because we do not have information on individuals' high school coursework. If we are willing to use BGM's first stage (which was estimated without state fixed effects) to scale up the effects we find (which were estimated with state fixed effects), we can construct bounds. Based on estimates from BGM's Table 3, people graduating from high school five years after the mandates were implemented (the mean among our exposed sample) would be seven percentage points more likely to take a personal finance course relative to those not exposed. Thus, we can rule out a 1.6 (=0.109/0.07) percentage point effect on those influenced to take such a course when the dependent variable is reporting any investment income and a \$40 effect on investment income.

Table 2
Estimate of the Effect of Personal Finance Mandates on Asset Accumulation

	Dependent Variable:				
	Any Investment Income	Investment Income	Investment Income Percentile	Value of Financial Assets	Equity in Real Estate
	Data Source:				
	U.S. Census	U.S. Census	U.S. Census	SIPP	SIPP
	1	2	3	4	5
Panel A					
Exposed	–0.0027	–30.07*	–0.09	–302	52
	(0.0019)	(16.74)	(0.15)	(989)	(1,103)
N	2,742,012	2,726,073	2,742,012	36,313	51,459
Panel B					
Five years prior	0.0044	32.22	0.26	1,073	573
	(0.0027)	(20.82)	(0.27)	(2,271)	(1,460)
Four years prior	0.0005	24.22	–0.11	948	–2,623
	(0.0030)	(25.49)	(0.36)	(2,741)	(1,684)
Three years prior	0.0020	20.28	0.08	–678	–4,105
	(0.0016)	(16.67)	(0.15)	(2,251)	(2,795)
Two years prior	0.0045*	15.63	0.19	–627	–2,407
	(0.0025)	(23.33)	(0.27)	(2,598)	(1,873)
One year prior	0.0033*	–6.84	0.14	2,589	468
	(0.0017)	(18.30)	(0.22)	(2,994)	(1,588)
First affected One year post	0.0039***	–3.61	0.33*	–2,436	–4,380**
	(0.0014)	(20.09)	(0.19)	(1,683)	(1,975)
Two years post	–0.0023*	–12.73	–0.18	2,177	–1,020
	(0.0012)	(13.85)	(0.19)	(3,495)	(1,302)
Three years post	–0.0011	13.20	–0.07	1,694	–2,140
	(0.0021)	(19.44)	(0.26)	(2,370)	(1,848)
Four years post	0.0023	–30.93	0.21	2,938	–1,846
	(0.0025)	(23.16)	(0.29)	(2,821)	(1,633)
Five years post	–0.0008	–26.30	0.10	341	–1,392
	(0.0031)	(17.30)	(0.28)	(2,805)	(1,663)
N	2,742,012	2,726,073	2,742,012	36,313	51,459

(*continued*)

Table 2 (*continued*)

	Dependent Variable:				
	Any Investment Income	Investment Income	Investment Income Percentile	Value of Financial Assets	Equity in Real Estate
	Data Source:				
	U.S. Census	U.S. Census	U.S. Census	SIPP	SIPP
	1	2	3	4	5
F-tests of prior versus post					
P-value (one year)	0.77	0.79	0.44	0.04	0.01
P-value (two)	0.09	0.45	0.64	0.25	0.06
P-value (three)	0.04	0.45	0.39	0.96	0.58
P-value (four)	0.21	0.21	0.98	0.59	0.86
P-value (five)	0.16	0.11	0.81	0.76	0.67
P-value (nine)	0.17	0.04	0.96	0.78	0.88
P-value (14)	0.53	0.01	0.21	0.55	0.7

Notes: This table describes the evolution of financial outcomes for individuals graduating prior to and following the imposition of mandated personal finance education in high schools. Columns 1–3 use data from the 2000 census: 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; in Column 3, it is the individual's percentile ranking in the nationwide investment income distribution. Columns 4 and 5 use data from the SIPP: In Column 4, the dependent variable is the value of financial assets the individual holds, and in Column 5, it is the value of equity the individual holds in real estate. In Panel A, the independent variable of interest is a dummy variable for whether the individual graduated from high school after the imposition of the mandate. In Panel B, the independent variables of interest are event-time variables indicating whether an individual graduated from high school *X* years prior to the imposition of the mandate or *X* years following the imposition of the mandate. We estimate these event-time variables for 15 years prior to and following the imposition of a mandate, a single dummy for "15 or more years" following the mandate, and a single dummy for "16 or more years" prior to the mandate; the omitted group is individuals graduating the year the mandate was passed. For brevity, only years −5 to 5 are reported. Additional controls in all regressions include sex, race, state of birth dummies, year of birth dummies, and census division-specific linear time trends. All samples include individuals born between 1946 and 1965. Top- and bottom-coded values (see text for details) are dropped in Columns 2, 4, and 5. The final lines of the table test whether the average value of the coefficients on the years immediately prior to the imposition of the mandate are equal to those indicating the years immediately following the mandate. Standard errors, corrected for arbitrary correlation within state of birth, are in parentheses. Numbers with *** indicate significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level.

precision of the estimate of a zero effect. Comparing the coefficients five years pre and post, we can rule out an average effect on any investment income as small as 0.1 percentage point, at the 5 percent level, from a base of 23 percent. We find similar results with tests using 1, 2, 3, 4, 9, and 14 years around the mandate. While there are some *F*-tests that suggest a significant difference, all of them have the "wrong" sign: Any investment income is lower for cohorts graduating after the mandates compared to those graduating before.

Table 3
Comparison of Bernheim, Garret, Maki (2001) with Census and SIPP Data

	Data Source:						
	BGM Results	Census		SIPP	Census		SIPP
	Dependent Variable:						
	Net Worth Percentile	Any Investment Income	Investment Income	Equity in Real Estate	Any Investment Income	Investment Income	Equity in Real Estate
	1	2	3	4	5	6	7
Treat (state ever imposed mandate)	−1.59	−0.033**	−100**	−7,694***			
	(2.65)	(0.015)	(39)	(2,639)			
Exposed to mandate	9.48**	0.032***	103***	6,348***	0.0027	28	2,278
	(4.05)	(0.005)	(31)	(1,228)	(0.0059)	(36)	(1,560)
Married	−2.05	−0.007***	−91***	10,690***	−0.0086***	−91***	10,874***
	(2.11)	(0.002)	(12)	(584)	(0.0019)	(12)	(602)
College educated	3.19	0.145***	446***	20,347***	0.1419***	431***	19,575***
	(2.03)	(0.003)	(15)	(854)	(0.0029)	(15)	(790)
Age	0.94***	0.007***	40***	1,580***			
	(0.18)	(0.0003)	(2)	(88)			
Total Earnings/10^5	5.31***	0.211***	1,450***	33,454***	0.2084***	1,439***	31,070***
	(1.67)	(0.005)	(24)	(1,786)	(0.0052)	(24)	(1,573)

Additional fixed effects	None	None	None	None	State of birth Year of birth	State of birth Year of birth	State of birth Year of birth
R-squared		0.11	0.05	0.13	0.11	0.05	0.15
N	910	2,735,306	2,719,483	50,877	2,735,306	2,719,483	50,877
Mean of dependent variable		0.23	726.47	34,132	0.23	726.47	34,132.1

Notes: This table replicates the Bernheim, Garret, and Maki (2001)(BGM) specification using data from the census and the SIPP, without state fixed effects (Columns 2–4) and with state and year of birth fixed effects (Columns 5–7). Column 1 reports the original regression from BGM, which uses data from a telephone survey conducted by the authors in 1995; Columns 2, 3, 5, and 6 use the 2000 census; and Columns 4 and 7 use the SIPP. The dependent variables of interest are household position in the distribution of net worth (Column 1), whether the household reported any investment income (Columns 2 and 5), the value of investment income (Columns 3 and 6), and the value of all real estate equity (Columns 4 and 7). The independent variable of interest is whether the individual was exposed to the mandate (having graduated from high school after the mandate was imposed). All samples include individuals born between 1946 and 1965. Standard errors in Column 1 are taken from BGM. Standard errors are in parentheses; in Columns 2–7, they are clustered to account for correlation at the state level. Numbers with *** indicate significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level.

The top panel of Figure 1 demonstrates that the propensity to accumulate assets was trending up for individuals who graduated long before the mandates went into effect and that the mandates did not affect this trend. If anything, the graph suggests that the mandates reversed the trend, since the difference in the likelihood of having any investment income declines after the mandate. We show in an appendix of Cole, Paulson, and Shastry (2013) that the trend mirrors a trend in state gross domestic product, suggesting that the latter contributed to observed changes in financial outcomes. It appears that there were different long-term trends in asset accumulation across states that were correlated with states' decisions to implement personal finance mandates. This casts doubt on BGM's identifying assumption: If no mandates had been imposed, then the difference in outcomes between premandate and postmandate cohorts in treated states would have been the same as the difference in outcomes between the same cohorts in untreated states. The identifying assumption for our simple difference-in-difference strategy is more credible because we include census division trends, but it could still be biased by state-specific trends. Thus, we focus on the event study results. The *F*-test we described above is much less sensitive to this concern, because we concentrate on individuals who graduated within five years of the mandates. The identifying assumption is that, conditional on being born in the same state and graduating within five years of the imposition of a personal finance mandate, whether an individual is affected (graduated later) or not affected (graduated before) is uncorrelated with any omitted variables.[32]

Column 2 of Table 2 performs an identical analysis using the level of investment income as the dependent variable, and the middle panel of Figure 1 plots the results. As in the tendency to report having earned any investment income, there is a general upward trend in the level of investment income approximately ten years prior to the mandate and a gradual decline after the mandate but no clear trend break at the imposition of the mandate. An *F*-test of the five pre γ_k against the five post γ_k fails to reject equality, and we can rule out an effect size as small as \$7 on a base of \$728. Column 3 and the bottom panel of Figure 1 perform the same analysis using the percentile rank of where the household falls in the distribution of investment income to total income. The observed patterns are quite similar to those for any investment income.

The discrepancy between our conclusions and those of BGM is substantial, and we consider several approaches to reconcile them. By analyzing the SIPP data, we can study outcome variables closer to those studied by BGM. Columns 4 and 5 in Table 2 present these results and qualitatively confirm the conclusions from the census data. Looking at the *p*-values at the bottom of the table, the total values of financial assets and equity in real estate are not significantly different in the five years after the mandate relative to the

32. While we believe this identification strategy is compelling, we concede that it is not a randomized controlled experiment and that there are some long-term trends in the data that could undermine the identification strategy. We note that two of 25 coefficients in the preperiod are statistically significant in Table 2. Four of 25 are significant in the postperiod, but taken on face value they would be difficult to reconcile. For example, cohorts graduating one year after the mandate are more likely to have investment income but less home equity. Further tests, such as comparing cohorts graduating one to three years prior to the mandate to those graduating four to six years prior to the mandate, fail to identify trends (*p*-value 0.85 for "any investment income" and 0.65 using "real estate equity"). We also test this assumption by estimating the same regression with years of schooling, race, gender, and earned income on the lefthand side and find little evidence of significant differences between cohorts graduating on either side of the mandates (results available upon request).

five years before. Because of the substantially smaller sample size of the SIPP data, we are unable to be as precise about this "zero" effect. We are only able to reject an effect of $2,100 on all financial assets on a base of $16,200, and an effect of $1,940 on equity in real estate on a base of $35,000.

It is also possible that personal finance courses are more effective among certain populations. For example, Cole, Sampson, and Zia (2011) find that an education program on bank accounts has a larger effect on households with low levels of initial financial literacy. In results not reported here, we split the sample by educational attainment, race, and gender. All estimates yield the same pattern: Financial outcomes trend up prior to the imposition of mandates and there is no evidence of a trend break at imposition or soon after.[33]

Comparison with previous work

One likely suspect for the difference in results is the fact that BGM use a different data set. The SIPP helps us rule out the possibility that investment income (our outcome variable from the census) and net worth (BGM's outcome variable) have different temporal patterns. To further investigate whether the census outcome variables explain the discrepancy, we estimate the specification used by BGM with census data. BGM estimate the following equation:

$$(3) \quad y_{is} = \alpha_0 + \beta_0 Treat_s + \beta_1 Exposed_{is} + \beta_2 Married_i + \beta_3 College_i + \beta_4 Age_i + \beta_5 Earnings_i + \varepsilon_{is}$$

where the dependent variable, y_{is}, is the population percentile of the ratio of a household's wealth to earnings and the independent variable of interest, $Exposed_{is}$, indicates that individual i graduated from high school in state s after the mandate was imposed. BGM use population ranks to mitigate the effect of outliers.[34] Instead of state fixed effects, BGM include $Treat_s$, a dummy for whether state s ever required a personal finance course. In addition, they control for marital status, an indicator for college education, age, and total earnings.

The main result from BGM (from their paper) is reproduced in Column 1 of Table 3.[35] The coefficient of interest, β_1, is positive and significant, a result suggesting that personal finance courses lead to increased net worth. Graduating after the mandate induces an individual to move 9.5 percentage points up in the distribution. BGM also note that

33. Our analysis includes high school dropouts (as does BGM); our finding of no effect is unchanged if we exclude dropouts from our sample or focus on people with different levels of education (see Table A3). There does appear to be a small, positive, and significant effect for black men 11–13 years after the mandates were imposed using the position in the distribution of investment income variable, but there is little evidence of a trend break at imposition and the result is not robust. In addition, there is a significant coefficient on the event-dummy for four years after the mandates were passed for white men using the financial assets variable in the SIPP but not for other years around this year. Multiple hypothesis logic suggests that these occasional significant coefficients are to be expected.

34. We show results for a similar population rank of investment income and confirm that we find similar results with population ranks for the SIPP net worth variables (results available upon request), but note that the "any investment income" measure is not affected by outliers.

35. Table 6, Column 2 in BGM, on page 458.

β_0 is statistically indistinguishable from zero, supporting the identification strategy: Treated states were not different from nontreated states prior to the mandates.[36]

In Columns 2 and 3, we replicate the BGM results using census data. There are several additional differences between the two data sources besides the difference in outcomes emphasized above. First, the BGM sample was collected in 1995, five years prior to the census. We focus on households born in the same years as the BGM sample, so the cohorts are five years older: Our sample is aged 35–54 in 2000.[37] Second, the census sample is substantially larger, at 2.7 million, compared to BGM's 1,900 respondents (910 with data on net worth).[38]

Column 2 in Table 3 presents the estimates of Equation 3 using any investment income as the dependent variable. We use a linear regression model, but a probit model (not shown) yields similar results. The main coefficient of interest, on "Exposed to mandate," is positive and statistically significant at the 1 percent level. Individuals graduating after the mandate was passed are 3.2 percentage points more likely to report asset income. The mean level of participation is 23 percent, while the standard deviation is 42 percent. The effect is therefore modest (approximately 0.08 standard deviations) but highly statistically significant.

Column 3 estimates Equation 3 using the dollar value of investment income as the dependent variable. This regression suggests that mandate exposure increases savings income by approximately \$103. The average amount of investment income is \$726, while the median amount is \$0. Assuming a return on investments of 5 percent, an increase of \$103 would suggest an increase in total assets of about \$2,060 due to exposure to the mandate. We also use the household's placement in the distribution of investment income to total income, but do not report the results in the interest of space. This is close to BGM's percentile ranking, though it is based on investment income rather than savings rate or net worth. Again, we find a positive and statistically significant effect of personal finance courses.

Column 4 estimates Equation 3 with the SIPP data using real estate equity as the dependent variable. The primary coefficient of interest, "Exposed to mandate," is positive and statistically significant at the 1 percent level suggesting that exposure to personal finance courses increases real estate equity by \$6,348. The results are similar for the dollar value of all financial assets, which are not shown in the interest of space.

These results are consistent with BGM's finding in contrast to the findings using the difference-in-difference specification and the more flexible specification reported in

36. BGM also use the population rank of an individual's savings rate (defined as unspent take-home pay plus voluntary deferrals divided by income) as a dependent variable, but we focus on net worth in the interest of space and because our outcome variables are more similar to net worth. When using savings rates, BGM use a different treatment variable: the number of years the mandates had been in place when individual *i* graduated from high school. They find a significant effect of exposure to the mandates on savings percentile. Graduating five years after the mandate induces an individual to move four percentage points up in the distribution or a 1.5 percentage point shift in savings rate. Estimating BGM's specifications with our data using "years since mandate" also produces results similar to BGM (results available upon request).

37. We do not think it likely that any of the differences between our findings and BGM are attributable to the timing of the data collection. Using census data from 1990 gives very similar results (results available upon request).

38. We cluster by state of birth. The robust standard errors used in BGM likely overstate the precision of their estimates (Bertrand, Duflo, and Mullainathan 2004).

Table 2. The fact that we obtain similar results with BGM's specification using census data suggests it is unlikely that data differences are responsible for the difference in findings. Columns 1–4 of Table 3 suggest an alternate explanation for why our results differ, however. The coefficient on *Treat*, β_0, is often statistically significant using census data, implying that among cohorts not affected by the mandates (older cohorts), states that imposed mandates had statistically different savings outcomes from states that did not impose mandates. The *Treat* variable could theoretically account for all of these differences but only if the differences were constant across states and over time. While a statistically significant β_0 does not necessarily invalidate BGM's identification strategy, it does raise a cautionary flag.[39]

Unlike BGM, our specification accounts for differences not only between states that imposed mandates and those that did not grouped together but also for differences between states within a group, because we include state fixed effects. Columns 5–7 of Table 3 estimate Specification 3, adding state and year of birth fixed effects. The main coefficient of interest, on "Exposed to mandate," is much smaller and never statistically significant, indicating that part of BGM's results are driven by state differences that are not adequately controlled for by the variable *Treat*.[40]

In the appendix of Cole, Paulson, and Shastry (2013), we consider this possibility directly, examining whether the passage of mandates is correlated with state GDP growth. We find that it is and that a strategy that compares the broad group of "affected" individuals with "unaffected" individuals may generate spurious results. Focusing on the cohorts graduating just before and just after the mandates is a more plausible identification strategy.[41]

Another difference between our results and those of BGM is that they find that impacts of personal finance mandates are concentrated among individuals who report that their parents were not frugal. The census data do not allow us to divide the sample in this way. However, since we do not find any positive effect of the mandates in the entire sample, we can conclude that any effect on individuals with nonfrugal parents must be very small. According to BGM, 67 percent of individuals report having nonfrugal parents. If the effect on children of frugal parents were zero, the largest possible difference between cohorts graduating five years before and after the imposition of a mandate that we would not reject among those with nonfrugal parents would be approximately 0.15 percentage points ("any investment income") off a base of 23 percent.

39. The negative β_0 says that, on average, individuals in states that imposed the mandates had lower likelihoods of having investment income than individuals in states that did not impose the mandates. The results in Table 2 indicated that those individuals graduating around the year of the mandate had higher propensities to earn investment income than those graduating 15 years or more prior to the mandate, conditional on state of birth. This suggests that, on average, individuals in the mandate states had lower propensities to earn investment income but that relative to this (lower) state average those graduating around the time of the mandate were doing better.

40. The inclusion of these fixed effects means we cannot estimate the variables *Treat* and *age*. Regressions without year fixed effects (not shown) yield similar results.

41. This can also be seen when our flexible Specification 2 is estimated without state fixed effects. Results (not reported) suggest a trend break before the mandates were implemented, which could yield spurious results in a simple difference-in-difference estimation.

B. Credit Management

State-mandated financial education covered a range of topics. For example, the curriculum guide in South Carolina in 1972 (a treatment state) includes consumer credit, financing a home, insurance, savings, investment, taxes, and financial record-keeping (State of South Carolina 1972). We analyze the FRBNY CCP data to see whether the personal finance mandates had an impact on credit management outcomes. Table 4 presents estimates of Equations 1 and 2 using FRBNY CCP data. The dependent variable in Column 1 is an individual's credit score, in Column 2 it is the fraction of an individual's credit card balance that is not delinquent, both averaged across all quarters. In Column 3, the dependent variable is the proportion of quarters an individual has any delinquent credit card balance, and in Columns 4 and 5 it is an indicator for having declared bankruptcy or been foreclosed upon between 1992 and 2011, respectively. As in Table 2, Panel A presents the difference-in-difference results while Panel B presents the event study; the event study specifications include fixed effects for all cohorts graduating 15 years on either side of the mandate, but we report only five years in the interest of space.

The results clearly indicate that exposure to financial education mandates did not have a measurable impact on these indicators of credit management. As in Table 2, the *F*-tests presented at the bottom of the table test the hypothesis that the average value of the coefficients for the years prior to the mandates is equal to the average value of the coefficients for the years after the mandates. For none of the outcomes or timeframes are we able to reject equality at the 5 percent level. (We can reject equality at 6–7 percent levels for bankruptcy for the first two years, but the significant effect disappears almost immediately.) In fact, when we average the five coefficients on either side of the mandate, we find that credit outcomes deteriorate for postmandate cohorts for the first three outcome variables (albeit insignificantly). As with the census data, the size of the FRBNY CCP data allows us to be precise about this "zero" result. We can rule out a positive effect on credit scores as small as 1.7 points and on percent balance current of 0.06 percentage points on a base of 95 percent. Similarly, we can rule out a negative effect as small as 0.07 percentage points on quarters delinquent on a base of 10 percent, on bankruptcy of 1.2 percentage points on a base of 18 percent, and on foreclosure of 0.35 percentage points on a base of 8 percent.[42,43]

Taken together, these results suggest that an emphasis on expanding access to or increasing the intensity of personal finance education in high school may be misguided. Requiring a high school personal finance course appears to have had no causal impact on asset accumulation or credit management.

V. Impacts of Increased Math Courses

In this section, we use the census, the SIPP, and the FRBNY CCP data to examine the impact of increasing high school math requirements on financial outcomes. As described above, we follow Goodman (2012) and exploit state responses to

42. We did not include these outcome variables in Figure 1 in the interest of space.
43. The specification used by BGM with these data does not yield any significant results.

Table 4
Estimates of the Effect of Personal Finance Mandates on Credit Management

	Dependent Variable: Credit Score 1	Percent Balance Current 2	Percent Quarters Delinquent 3	Bankruptcy Flag 4	Foreclosure Flag 5
Panel A					
Exposed	0.2504	–0.0003	0.0005	–0.0060	–0.0011
	(0.8817)	(0.0006)	(0.0010)	(0.0038)	(0.0019)
N	3,678,868	3,451,100	3,692,865	3,692,865	3,692,865
Panel B					
Five years prior	0.2549	0.0026**	–0.0004	0.0085**	0.0027
	(1.0881)	(0.0010)	(0.0014)	(0.0039)	(0.0017)
Four years prior	0.4777	0.0022**	–0.0008	0.0033	0.0021
	(0.9817)	(0.0009)	(0.0014)	(0.0028)	(0.0015)
Three years prior	1.4613	0.0017	–0.0024	0.0013	0.0004
	(1.0908)	(0.0010)	(0.0018)	(0.0026)	(0.0017)
Two years prior	–0.2262	–0.0002	0.0016	0.0048***	0.0014
	(0.5398)	(0.0008)	(0.0015)	(0.0018)	(0.0017)
One year prior	0.1607	0.0010**	0.0001	0.0039**	0.0025***
	(0.6509)	(0.0005)	(0.0006)	(0.0018)	(0.0007)
First affected					
One year post	0.3465	0.0017***	–0.0001	0.0002	0.0023*
	(0.4289)	(0.0004)	(0.0006)	(0.0013)	(0.0013)
Two years post	0.3107	0.0004	0.0008	0.0007	0.0007
	(0.5426)	(0.0012)	(0.0015)	(0.0019)	(0.0017)
Three years post	–0.0941	–0.0000	0.0010	–0.0019	0.0027
	(0.8147)	(0.0006)	(0.0008)	(0.0018)	(0.0022)
Four years post	0.9335	0.0014	0.0002	0.0013	–0.0007
	(0.9495)	(0.0009)	(0.0014)	(0.0046)	(0.0016)
Five years post	0.4917	0.0016*	0.0013	–0.0012	–0.0008
	(0.8327)	(0.0009)	(0.0011)	(0.0045)	(0.0025)
N	3,678,868	3,451,100	3,692,865	3,692,865	3,692,865
F-tests of prior versus post					
P-value (one year)	0.753	0.130	0.826	0.071	0.915
P-value (two)	0.483	0.442	0.633	0.063	0.794
P-value (three)	0.709	0.803	0.379	0.134	0.771

(*continued*)

Table 4 (*continued*)

	Dependent Variable:				
	Credit Score	Percent Balance Current	Percent Quarters Delinquent	Bankruptcy Flag	Foreclosure Flag
	1	2	3	4	5
P-value (four)	0.904	0.576	0.302	0.253	0.837
P-value (five)	0.974	0.420	0.241	0.217	0.466
P-value (nine)	0.502	0.924	0.638	0.214	0.375
P-value (14)	0.215	0.624	0.927	0.116	0.218

Notes: This table describes the evolution of credit outcomes for individuals graduating prior to and following the imposition of mandated personal finance education in high schools. The sample comprises individuals from a 5 percent panel of American borrowers who were born between 1946 and 1965 and have data in every quarter of the panel from 1999–2011. The dependent variable in Column 1 is the credit score averaged for each individual across all quarters of data; in Column 2, it is the nondelinquent balance on credit cards divided by the total credit card balance; in Column 3, it is the proportion of quarters an individual has any delinquent balance on his/her credit card bills; and in Columns 4 and 5, it is an indicator for having undergone bankruptcy or foreclosure, respectively, at least once between 1992 and 2011. In Panel A, the independent variable of interest is a dummy variable for whether the individual graduated from high school after the imposition of the mandate. In Panel B, the independent variables of interest are event-time variables indicating whether an individual graduated from high school *X* years prior to the imposition of the mandate or *X* years following the imposition of the mandate. We estimate these event-time variables for 15 years prior to and following the imposition of a mandate, a single dummy for "15 or more years" following the mandate, and a single dummy for "16 or more years" prior to the mandate; the omitted group is individuals graduating the year the mandate was passed. For brevity, only years −5 to 5 are reported. The final lines of the table test whether the average value of the coefficients on the years immediately prior to the imposition of the mandate are equal to those indicating the years immediately following the mandate. Additional controls in all regressions include state of birth dummies, year of birth dummies, and census division-specific linear time trends. Standard errors, corrected for arbitrary correlation within state of birth, are in parentheses. Numbers with *** indicate significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level.

the 1983 National Commission on Excellence in Education report. We study the reduced form impact of the new math requirements on asset accumulation and credit management.[44]

As above, we must be careful that the assumptions necessary to interpret the results as causal are valid. Specifically, we need to be concerned that (1) the states that imposed reforms may differ from states that did not, (2) the number of math courses required post reform may be correlated with state-level omitted variables, (3) the timing of the reforms may be correlated with unobservable state and cohort factors, and (4) other policy changes might have occurred at the same time. We take these possibilities into account (as does Goodman 2012) in a number of ways: (1) by including state-of-birth and year-of-birth fixed effects, (2) by focusing on the timing of the reforms rather than the number

44. We are able to replicate Goodman's results for earnings with our reduced form regressions using the census data (results not shown).

of courses required post reform, (3) by allowing for separate linear time trends for each census division, and (4) by controlling for the total number of other courses required in each state for each graduating cohort (English, science, social studies, etc.), a dummy variable for an exit exam requirement, state per-student expenditures on education, the student-teacher ratio, the state poverty rate, and the state unemployment rate, all measured in the year the individual turned 17.[45] Recall that the identifying assumption is that conditional on state and year of birth, census division-specific trends and these other control variables, cohorts that graduated before the reforms were no different from cohorts that graduated after the reforms, and we focus on cohorts close to the reform date. We present the straightforward difference-in-difference estimates but also an event study specification with separate treatment coefficients for cohorts graduating within six years of the reform.[46]

A. Asset Accumulation

Table 5 presents the effect of the math reforms on asset accumulation, estimating Equations 1 and 2 using data from the census (Columns 1–3) and the SIPP (Columns 4–5). Column 1 presents the estimates using any investment income as the dependent variable, Column 2 uses total investment income, and Column 3 uses the individual's location in the nationwide distribution of the ratio of investment income to total income. Columns 4 and 5 use the amount of all financial assets and all real estate equity, respectively. Panel A presents estimates of Equation 1, the difference-in-difference regression, displaying only the coefficient on the dummy variable indicating the individual was required to take additional math courses in order to graduate from high school. While the coefficients on the "exposed" variable in Columns 2 and 4 are not statistically significant from zero, the coefficients in Columns 1, 3, and 5 are significant at the 5 percent level. In addition, the magnitudes of these effects are not trivial: Having to take more math courses moves an individual 38 percentage points up in the distribution of investment income to total income and increases equity in property by $1,519.

Panel B presents estimates of Equation 2. The specifications include event-years from six years prior to the law change to six years after the mandate. Recall that the coefficients represent the difference in the outcome between the particular cohort and the cohort that graduated in the year the new requirements were passed, conditional on state of birth. Recall, also, that these changes are not time or age effects, due to the inclusion of birth-year dummies.

Consider the coefficients in the first column. The coefficients on the five cohorts graduating immediately prior to the law change average approximately –0.17 percentage points and four of the five coefficients are indistinguishable from the omitted group (those graduating the same year as the reform). The coefficients on the five cohorts graduating immediately after the reform average 0.37 percentage points (half a percentage point higher) and two of the five coefficients are significantly different from

45. The results are robust to not including these controls or to including the variables measured in the year the individual turned 14 (corresponding to commencement of high school).

46. We use six years rather than 15 as we did when studying the personal finance mandates because the math sample is younger at the time of the survey and there are few individuals graduating from high school more than six years after the first affected cohort.

Table 5
Estimates of the Effect of Increased Math Requirements on Asset Accumulation

	Dependent Variable: Any Investment Income	Investment Income	Investment Income Percentile	Value of Financial Assets	Equity in Real Estate
	Data Source: U.S. Census	U.S. Census	U.S. Census	SIPP	SIPP
	1	2	3	4	5
Panel A					
Exposed	0.0035***	10.56	37.57***	–797	1,519**
	(0.0009)	(10.29)	(9.81)	(758)	(643)
N	1,454,334	1,451,309	1,347,143	20,527	28,191
Panel B					
Five years prior	–0.0026	–3.75	–34.64*	2,216	–3,021**
	(0.0018)	(13.52)	(18.68)	(1,495)	(1,471)
Four years prior	0.0002	–2.83	–6.76	2,318	–1,586
	(0.0020)	(15.59)	(20.18)	(1,479)	(1,365)
Three years prior	–0.0014	–8.71	–18.96	1,344	–1,512
	(0.0016)	(12.58)	(16.29)	(1,352)	(1,194)
Two years prior	–0.0025*	–9.01	–29.92**	1,298	–443
	(0.0013)	(9.84)	(13.57)	(1,010)	(1,054)
One year prior	–0.0021	–6.42	–23.23	1,484*	272
	(0.0016)	(9.71)	(16.81)	(742)	(586)
First affected					
One year post	0.0027*	10.02	22.95	1,623**	1,320
	(0.0015)	(8.63)	(15.58)	(763)	(1,206)
Two years post	0.0022	16.27*	19.93	1,344*	2,076*
	(0.0018)	(9.48)	(18.35)	(722)	(1,164)
Three years post	0.0062***	1.57	53.96***	1,469	1,764*
	(0.0021)	(11.86)	(20.50)	(1,013)	(997)
Four years post	0.0035	8.64	26.45	2,481**	1,578
	(0.0025)	(15.94)	(23.86)	(1,042)	(987)
Five years post	0.0041	10.05	30.63	2,757***	3,346***
	(0.0027)	(19.23)	(25.57)	(1,026)	(1,105)
N	1,454,334	1,451,309	1,347,143	20,527	28,191

(*continued*)

Table 5 (*continued*)

	Dependent Variable:				
	Any Investment Income	Investment Income	Investment Income Percentile	Value of Financial Assets	Equity in Real Estate
	Data Source:				
	U.S. Census	U.S. Census	U.S. Census	SIPP	SIPP
	1	2	3	4	5
F-tests of prior versus post					
P-value (one year)	0.003	0.219	0.004	0.890	0.430
P-value (two)	0.001	0.060	0.001	0.920	0.090
P-value (three)	0.000	0.147	0.000	0.910	0.040
P-value (four)	0.000	0.277	0.000	0.910	0.010
P-value (five)	0.001	0.340	0.001	0.850	0.000

Notes: This table describes the evolution of financial outcomes for individuals graduating prior to and following the imposition of increased math requirements in high school. Columns 1–3 use data from the 2000 census: 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; in Column 3, it is the individual's percentile ranking in the nationwide investment income distribution. Columns 4–5 use data from the SIPP (see text for details on which panels and waves): In Column 4, the dependent variable is the value of financial assets the individual holds, and in Column 5, it is the value of equity the individual holds in real estate. In Panel A, the independent variable of interest is a dummy variable for whether the individual graduated from high school after the imposition of the mandate. In Panel B, the independent variables of interest are event-time variables indicating whether an individual graduated from high school *X* years prior to the imposition of the mandate or *X* years following the imposition of the mandate. We estimate these event-time variables for six years prior to and following the imposition of a mandate, a single dummy for "six or more years" following the mandate, and a single dummy for "seven or more years" prior to the mandate; the omitted group is individuals graduating the year the mandate was passed. For brevity, only years −5 to 5 are reported. Additional controls in all regressions include sex, race, state of birth dummies, year of birth dummies, and census division-specific linear time trends. The regressions also include per-pupil expenditures on education, pupil-teacher ratios, the number of nonmath course requirements, a dummy variable for an exit exam requirement, the unemployment rate, and the poverty rate in the state and year the individual turned 18. All samples include individuals born between 1964 and 1976. Top- and bottom-coded values (see text for details) are dropped in Columns 2, 4, and 5. The final lines of the table test whether the average value of the coefficients on the years immediately prior to the imposition of the mandate are equal to those indicating the years immediately following the mandate. Standard errors, corrected for arbitrary correlation within state of birth, are in parentheses. Numbers with *** indicate significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level.

the omitted group. The *F*-statistic and *p*-value presented at the bottom of the table test the equality of these two averages and reject equality at the 1 percent level. We perform similar tests using cohorts one, two, three, and four years around the mandate and find similar results. While this effect (half a percentage point) seems small, note that only 15 percent of individuals in this sample report any investment income compared to the 23 percent in the sample available for the study of the personal finance mandates. This is likely due to the age of the individuals in the sample, which averages 30 years, compared to 44 years for the personal finance sample. Note also that this is an intent-to-treat effect. Goodman (2012) finds that the reforms increase the number of math courses taken by 0.1–0.4 courses (depending on demographic group). Thus, the treatment on the treated effect of an additional math course is substantially higher: 1.25 to five percentage points on the probability of reporting any investment income.[47]

In the second column, one sees that the level of investment income is also greater for those graduating after the law change compared to those graduating before the reform, but the average difference is not statistically significant. In Column 3, we find that being subject to increased math requirements moves an individual up in the distribution of investment income. Figure 2 plots the coefficients on all the event-year dummies from six years before the reforms to five years after for the outcomes in Columns 1–3. The graphs do not suggest any differential trends prior to the reforms, but the top and bottom panels reflect the significant difference between the earlier and later cohorts. The flexible specification we estimate allows the data to inform us whether the effect of the reforms was immediate and constant or gradual. We cannot reject that all six posttreatment coefficients are equal to each other (p-value 0.45 for Column 1), suggesting an immediate and constant effect. Note that it is not unreasonable to expect that school districts can make adjustments to comply with these reforms in the short run: High schools already offer math courses and the courses have well-established curricula and widely available textbooks.

Finally, looking at Columns 4 and 5 of Table 5, we do not find a statistically significant effect on accumulated financial assets, but we find a large and significant effect (of \$3,275 averaging over the two five-year windows) on real estate equity. The treatment on the treated effect of an additional math course on real estate equity ranges from \$8,000–\$33,000.[48] Again, we cannot reject that the six posttreatment coefficients are equal to each other (p-value = 0.25).[49]

B. Credit Management

Table 6 presents the effect of required math courses on credit outcomes. Column 1 studies the impact on an individual's credit score averaged across all quarters, Column 2 the impact on the fraction of an individual's credit card balance that is not delinquent, Column 3 the impact on the proportion of quarters with delinquent credit card balance, and Columns 4 and 5 the impact on the likelihood of bankruptcy or foreclosure. As above, Panel A presents the difference-in-difference effect, while Panel B presents the event-year dummies indicating cohorts graduating within six years of the year the

47. It is worth noting that these mandates primarily affected completion of beginner and intermediate math courses; Rose and Betts (2004) find that advanced mathematics courses have larger effects on income than the more basic courses. This may suggest that (1) our effects may be less driven by changes in income, or (2) that the effects we observe might be even larger if more advanced coursework were required.

48. Note that Goodman reports a strong first stage among blacks and a weaker first stage among whites using his limited sample size. Altonji, Blom, and Meghir (2012) suggest that, therefore, analyses using this instrument should be done on data sets with large sample sizes. This is the case for the census though not for our SIPP data. Hence, the SIPP results should be treated with some caution.

49. Our main results follow Goodman in excluding high school dropouts, but we confirm in Table A4 that the inclusion of dropouts does not affect the results.

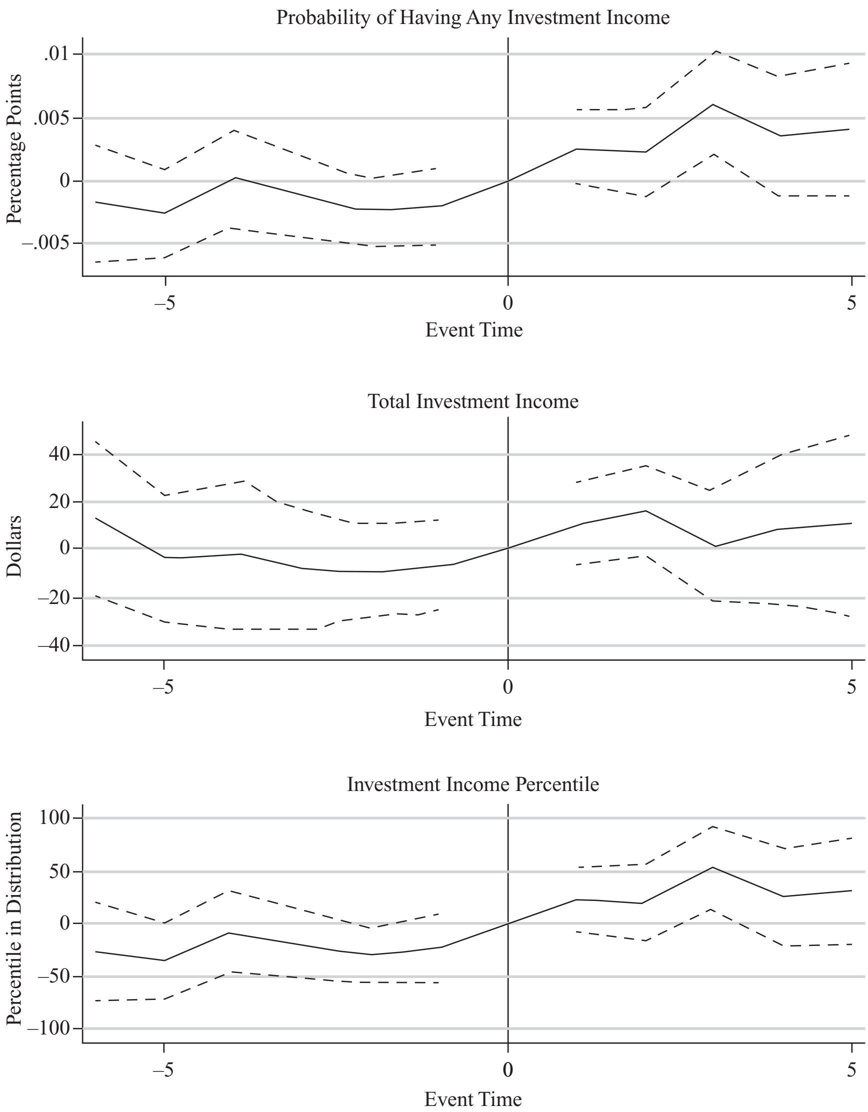

Figure 2

Increased Math Requirements and Investment Income

Notes: This figure plots the evolution of three data series prior to and following the imposition of state-mandated increases in math course requirements. The dotted lines show confidence intervals at the 95 percent level. The vertical line indicates the year in which the increased requirements were mandated.

Table 6
Estimates of the Effect of Increased Math Requirements on Credit Management

	Dependent Variable:				
	Credit Score 1	Percent Balance Current 2	Percent Quarters Delinquent 3	Bankruptcy Flag 4	Foreclosure Flag 5
Panel A					
Exposed	0.8640	0.0006	–0.0005	–0.0002	–0.0020
	(0.5401)	(0.0004)	(0.0007)	(0.0018)	(0.0013)
N	2,138,397	1,958,335	2,147,306	2,147,306	2,147,306
Panel B					
Five years prior	–0.9686	–0.0002	–0.0003	–0.0031	0.0013
	(0.9081)	(0.0006)	(0.0010)	(0.0021)	(0.0018)
Four years prior	–0.0601	–0.0001	–0.0011	–0.0058***	0.0012
	(0.6891)	(0.0006)	(0.0009)	(0.0019)	(0.0017)
Three years prior	–0.3965	–0.0001	–0.0003	–0.0045***	0.0009
	(0.6744)	(0.0005)	(0.0010)	(0.0017)	(0.0016)
Two years prior	–0.5250	–0.0004	0.0000	–0.0018	0.0015
	(0.6067)	(0.0005)	(0.0009)	(0.0016)	(0.0016)
One year prior	–0.9061**	–0.0007	0.0005	0.0013	0.0010
	(0.4472)	(0.0006)	(0.0009)	(0.0016)	(0.0015)
First affected					
One year post	0.2729	0.0006	–0.0006	–0.0007	–0.0007
	(0.3540)	(0.0006)	(0.0007)	(0.0013)	(0.0012)
Two years post	0.9376*	0.0008	–0.0016*	–0.0055***	–0.0019
	(0.5492)	(0.0007)	(0.0009)	(0.0020)	(0.0015)
Three years post	0.5453	0.0000	–0.0021**	–0.0053**	–0.0028*
	(0.7185)	(0.0008)	(0.0010)	(0.0021)	(0.0017)
Four years post	1.2711	0.0011	–0.0035***	–0.0084***	–0.0037**
	(0.8534)	(0.0009)	(0.0012)	(0.0023)	(0.0016)
Five years post	0.7872	0.0011	–0.0031**	–0.0091***	–0.0045**
	(1.0410)	(0.0009)	(0.0013)	(0.0034)	(0.0022)
N	2,138,397	1,958,335	2,147,306	2,147,306	2,147,306
F-tests of prior versus post					
P-value (one year)	0.05	0.10	0.19	0.33	0.19
P-value (two)	0.04	0.03	0.10	0.18	0.07

(*continued*)

Table 6 (*continued*)

	Dependent Variable: Credit Score 1	Percent Balance Current 2	Percent Quarters Delinquent 3	Bankruptcy Flag 4	Foreclosure Flag 5
P-value (three)	0.10	0.15	0.09	0.34	0.04
P-value (four)	0.11	0.15	0.06	0.35	0.02
P-value (five)	0.12	0.12	0.03	0.22	0.01

Notes: This table describes the evolution of credit outcomes for individuals graduating prior to and following the imposition of increased math requirements in high schools. The sample comprises individuals from a 5 percent panel of American borrowers who were born between 1965 and 1976 and have data in every quarter of the panel from 1999 to 2011. The dependent variable in Column 1 is the credit score averaged for each individual across all quarters of data; in Column 2, it is the nondelinquent balance on credit cards divided by the total credit card balance; in Column 3, it is the proportion of quarters an individual has any delinquent balance on his/her credit card bills; and in Columns 4 and 5, it is an indicator for having undergone bankruptcy or foreclosure, respectively, at least once between 1992 and 2011. In Panel A, the independent variable of interest is a dummy variable for whether the individual graduated from high school after the imposition of the mandate. In Panel B, the independent variables of interest are event-time variables indicating whether an individual graduated from high school *X* years prior to the imposition of the mandate or *X* years following the imposition of the mandate. We estimate these event-time variables for six years prior to and following the imposition of a mandate, a single dummy for "six or more years" following the mandate, and a single dummy for "seven or more years" prior to the mandate; the omitted group is individuals graduating the year the mandate was passed. For brevity, only years –5 to 5 are reported. Additional controls in all regressions include state of birth dummies, year of birth dummies, and census division-specific linear time trends. The regressions also include per-pupil expenditures on education, pupil-teacher ratios, the number of nonmath course requirements, a dummy variable for an exit exam requirement, the unemployment rate, and the poverty rate in the state and year the individual turned 18. The final lines of the table test whether the average value of the coefficients on the years immediately prior to the imposition of the mandate are equal to those indicating the years immediately following the mandate. Standard errors, corrected for arbitrary correlation within state of birth, are in parentheses. Numbers with *** indicate significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level.

mandate was enforced (we only report five years in the table). None of the columns in Panel A reveal a statistically significant outcome. While not all the dummies for cohorts graduating after the mandates are significant at conventional levels in Panel B, the *F*-statistic presented at the bottom of the table tests the equality between the average value of the pre- and the post-years and rejects equality at conventional levels for the fraction of delinquent quarters and the probability of foreclosure. While the *p*-values for the other outcomes are not within conventional significance levels, they are close (0.12 for the first two columns and 0.22 for Column 4), and the trends are consistent with cohorts graduating after the mandates having better financial outcomes. It is worth noting that these results corroborate those from the SIPP: Math courses increase real estate equity (Table 5, Column 5) and reduce the probability of foreclosure by an average of 0.4 percentage points in the five years after the law change relative to the five years before, on a base of 9 percent. Using Goodman's (2012) first stage estimates (0.1–0.4 math courses), this suggests a one to four percentage point treatment on the treated effect of one more math course, which is substantial, although not sufficient to eliminate bad financial outcomes. In addition, math courses reduce the fraction of quarters an individual is delinquent on credit card bills by 0.2 percentage points from a base of 12 percent.

VI. Discussion

A. Financial Outcomes Versus Financial Decision-Making

In this paper, we study the impact of two high school curriculum reforms on financial outcomes. We show that the personal finance mandates passed by U.S. states between 1957 and 1982 had no causal effect on asset accumulation or credit management. However, increases in math requirements implemented between 1984 and 1994 appear to have positively impacted both asset accumulation and credit management. While we do not observe financial decision-making directly, the financial outcomes we study reflect important behavior. For example, "any investment income" captures the decision to save and accumulate assets, and the fraction of quarters an individual is current on credit card debt captures the decision to avoid finance charges by paying credit card bills on time (and to manage one's finances so as to be able to do so). Both of these are examples of good financial decisions.

However, the exact mechanism through which the requirement that individuals take more math in high school affects these decisions is difficult to establish and an important topic for further work. It could be that individuals learn how to make better financial decisions because of the material or skills taught in math classes (such as calculating compound interest) or even through changes in preferences (such as instilling patience). Alternatively, additional math courses could affect career choices or the type of firm an individual works for. Rose and Betts (2004) find that these channels account for a little less than one-fifth of the earnings increase associated with additional math coursework. Career choices and the type of firm could, in turn, impact financial outcomes if certain occupations or firms are more likely to offer financial education or a 401(k) plan. It is difficult to separate these possibilities with the strategy used in this paper. However, including industry or occupation fixed effects does not affect our results (available upon request), which suggests that this channel is not responsible for the entire effect we document.

In addition, increased coursework in math may increase wages, resulting in more savings and better credit outcomes. This mechanism is unlikely to explain the entire effect we find, however. First, Goodman (2012) finds that math courses increase labor income for black men, while we find effects across the entire population.[50] When we disaggregate by race and gender, we find math courses improve financial outcomes for all four groups: white men, white women, black men, and black women. Table 7 repeats the analysis presented in Table 5 by demographic group. In the interest of economy, we present only the simple difference-in-difference estimates and the *F*-statistic comparing

50. Recall that Goodman finds that the law changes led to increased math courses for four groups: white men (0.16 math courses), white women (0.1 courses), black men (0.4 courses), and black women (0.27 courses). He finds an effect of math courses on labor market earnings for black men but weaker evidence for black women and no evidence of an effect for white men or women.

Table 7
The Effect of Increased Math Requirements on Asset Accumulation by Race and Gender

	Dependent Variable:				
	Any Investment Income	Investment Income	Investment Income Percentile	Value of Financial Assets	Equity in Real Estate
	Data Source:				
	U.S. Census	U.S. Census	U.S. Census	SIPP	SIPP
	1	2	3	4	5
White men	0.0041**	6.27	37.91**	–912	1,568
	(0.0020)	(17.26)	(18.02)	(1,485)	(1,224)
N	620,581	618,734	606,577	8,120	11,740
P-value (five years)	0.06	0.81	0.06	0.49	0.11
White women	0.0032**	19.98*	41.66***	–1,095	1,747*
	(0.0015)	(11.83)	(15.47)	(866)	(956)
N	659,465	658,413	580,902	9,388	12,827
P-value (five years)	0.002	0.07	0.001	0.61	0.004
Black men	–0.0002	4.48	–1.58	1,480	2,401
	(0.0032)	(23.18)	(31.87)	(3,085)	(3,079)
N	74,714	74,641	67,678	1,147	1,400
P-value (five years)	0.79	0.28	0.85	0.94	0.01
Black women	0.0047	–12.31	42.90	217	–215
	(0.0032)	(17.06)	(32.11)	(592)	(1,650)
N	99,574	99,521	91,986	1,872	2,224
P-value (five years)	0.02	0.15	0.048	0.10	0.57

Notes: This table describes the evolution of financial outcomes for individuals graduating prior to and following the imposition of increased math requirements in high schools, separately by race and gender. The coefficient reported is from the dummy variable for whether the individual graduated from high school after the reforms. The P-value reported is from the event-study specification, comparing the five cohorts graduating before and after the reforms. Please see the notes for Table 5 for more details. Standard errors, corrected for arbitrary correlation within state of birth, are in parentheses. Numbers with *** indicate significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level.

the five cohorts graduating before the reform to the five cohorts graduating right after the reform from the event study specification (additional results available upon request).[51] The effect of the math reforms using the simple specification and the five-year *F*-statistics is statistically significant (sometimes marginally) for a few of the outcomes for

51. Splitting the sample by educational attainment (with the caveat that educational attainment itself may be affected by the reform) suggests that the census results are driven by individuals with high school degrees but not college degrees, while the SIPP result on real estate equity is driven by those with a college degree. The FRBNY CCP data do not include information on race or gender, precluding any such analysis.

white men, white women, and black women. While most of these tests do not find an effect for black men, we do see a significant effect (from the F-test) for real estate equity. Thus, while part of the effects we estimate could be working through additional labor market income, we find effects on financial outcomes for demographic groups that took more math courses but did not experience a statistically significant increase in labor market earnings.[52] Second, in a robustness check, we control for a cubic polynomial in wage earnings and find that our results persist. (We use zip code fixed effects to proxy for wages in the credit bureau data; results available upon request.)

Requiring further mathematics courses may also induce students to stay in school longer. Joensen and Nielsen (2009) and Altonji, Blom, and Meghir (2012) suggest that a nontrivial fraction of the effect of additional math courses on wage income may work through this channel by considering how much the coefficients on additional math training change when educational attainment controls are included. However, when we include fixed effects for years of schooling, our results do not change substantially (results available upon request).

B. Policy Implementation

While we find diverging results for personal finance courses mandated between 1957 and 1982 and changes in mathematics requirements mandated between 1984 and 1994, the generalizability and policy implications of our findings depend on how the policies were implemented. While both BGM and Goodman (2012) estimate a first stage indicating that cohorts graduating after the mandates did take more personal finance and math courses, respectively, than those graduating before, it would be helpful to know more about the quality of these courses, including course content and teacher quality.[53] For example, one possible reason the math reforms had an effect while the personal finance mandates did not is that math teachers may be of higher quality than the teachers chosen to teach the personal finance courses. Another possibility is that the additional mandated math courses were well-established courses with widely available syllabi and textbooks (Algebra II, for example), while personal finance teachers may not have had such resources for course development. Our data and sources of exogenous variation do not allow us to examine these different interpretations directly, so we consider qualitative evidence about how the two reforms were implemented.

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52. This breakdown also allows us to compute treatment on the treated estimates, using Goodman's first stage results by gender and race. These results are very similar to the treatment on the treated effects discussed earlier. For example, we find that for white men, cohorts graduating in the five years after reforms were implemented are 0.5 percentage points more likely to report nonzero investment income (p-value = 0.06) than cohorts graduating in the five years before the reforms. The treatment on the treated effect is, therefore, three percentage points (= 0.5/0.16). For white women, the treatment on the treated effect is seven percentage points, and for black women it is 2.7 percentage points. These are substantial increases on a basis of 20 percent, 13 percent, and 4 percent, respectively. For black men, the heterogeneity analysis does not suggest an increase in reporting investment income, but we estimate a $30,000 treatment on the treated effect on real estate equity. Note that these calculations rely on coefficients not reported in Table 7, in the interest of space.

53. If the personal finance mandates were not implemented well, this would be similar to having a weak first stage and could explain our finding of no effect (although such a view would be difficult to reconcile with BGM's results). This does not change the policy implication of our results—such mandates by themselves, as they were enforced or implemented then, are unlikely to work—but would not rule out the possibility that personal finance training in high school, implemented well, could have effects.

1. Implementation of personal finance mandates

The content and quality of personal finance education appears to have varied widely across the 14 states that enacted personal finance mandates. While four states (GA, ID, SC, and OR) required the creation of dedicated courses that included personal finance, other states allowed personal finance material to be integrated into existing courses. All but two states (HI and NV) included provisions for workshops or other teacher training to prepare instructors. In addition to this training, three states (ID, IL, and OR) also had specific educational requirements for teachers to be certified to teach the material. Data on funding for these specific programs are not generally available. With regard to course content, common topics included banking, budgeting, credit, savings, taxes, and investment (Alexander 1979).

One state that had an especially well-documented program was Idaho. The state mandate required students to take a one-semester high school course in consumer education, including personal finance, in order to graduate. The course was described as "a practical and theoretical course with a focus on critical analysis of consumer issues." It was designed to teach students basic economic theories as well as practical skills including money management, banking, credit, investments, insurance, and how to make comparative buying decisions on major purchases such as a home or car. Teachers were required to be certified in social studies, business, or home economics and had to complete six credits of related coursework. The State Education Agency also conducted in-service training programs to prepare teachers (Alexander 1979).

States with more loosely defined programs include Nevada and Hawaii. Neither state required schools to create a specific class. Instead, the material was to be infused into the K–12 curriculum as deemed appropriate. Teachers also did not receive any kind of training or certification in these states (Alexander 1979).

2. Mathematics: Response to "A Nation at Risk" report

In contrast to the personal finance mandates, the particular impetus for the math reforms we study is clearer. The 1983 "A Nation at Risk" report laid out very specific guidelines for the number and types of courses students should take during high school: four years of English, three years of math, science, and social studies each, and one semester of computer science. Although not all states implemented the levels recommended by the commission, only ten states did not respond at all. Of the 40 states that passed a reform, all but two specified the number of math courses, while only 18 specified English requirements, 23 specified social studies requirements, and 32 specified science requirements. Thus, a challenge to our interpretation of the results is that some states included additional mandated courses in English, social studies, and science. While we control for the total number of other courses required, we do not have enough statistical power to separately estimate the effect of each subject. In a robustness check, we exclude states with concurrent changes in these other subjects, taken one by one; this reduces statistical power, but the estimated impact of math courses generally persists (results available upon request). The commission's report also made recommendations in categories other than graduation requirements. However, a 2008 report found that little to no progress was made in these areas (Strong American Schools 2008). The same report gave the nation an "A" for raising high school graduation requirements. We conclude that it is unlikely that our results are driven by other changes in education policy resulting from “A Nation at Risk” or occurring over the same time period.

Math teacher training and accreditation also varies across states, but is more standardized than for personal finance. The 1983 report did not make concrete recommendations for changes in teacher accreditation (other than to note that teachers should “demonstrate competence in an academic discipline”) and, according to Woellner (1982) and Burks (1985), teacher certification requirements for high school teachers did not change much from 1982–83 to 1985–86. Thus, it is unlikely that our math results are due to more qualified math teachers as opposed to more math courses. However, established requirements for teachers could be one reason the math policy impacted financial outcomes while the personal finance policy did not. State teaching credentials are typically based on postsecondary coursework, student teaching, and general knowledge of subject specific tests. By 1987–88, approximately 91 percent of high school students had math teachers with either a major, minor, or state certification in mathematics (U.S. Department of Education 2004). As of 1995, 19 states required high school teachers to have a major in their subject area to be certified. Another nine states required either a major or minor in their field. Ten states that did not require a major or minor had minimum college coursework requirements for teachers ranging from 18–45 credits in their field (Council of Chief State School Officers 2000).

VII. Conclusion

This paper contributes to a growing body of literature exploring the importance of non-neoclassical factors such as financial literacy on financial outcomes. Previous literature has found a strong correlation between financial literacy and sound financial decisions and a causal effect of high school personal finance courses on financial behavior. We began by reexamining the impact of state mandates that required high school students to study personal finance. An influential paper by Bernheim, Garrett, and Maki (2001) found that these mandates were effective at improving savings behavior. Using a much larger sample from the U.S. Census and a more flexible specification, we show that these programs did not, in fact, increase savings. Those who graduated just prior to the imposition of mandates (and were not exposed to financial literacy education) have identical asset accumulation outcomes compared to those who graduated following the mandates (and were therefore exposed to the program). Our findings suggest that states that imposed the personal finance mandates were inherently different from those that did not. We provide suggestive evidence that states imposed these mandates during periods of fast economic growth, which might have an independent effect on savings behavior of concurrent high school graduates. Finally, we confirm that these personal finance mandates do not have a measurable effect on credit management outcomes using FRBNY CCP data.

We next turned to another set of interventions that led to increased math coursework in high school. Studying the same reforms that Goodman (2012) used in his study of labor market outcomes, we showed that individuals who were exposed to greater math requirements in high school are more likely to accumulate assets, have more real estate equity, are less likely to be delinquent on their loans, and are less likely to undergo foreclosure. An important caveat to this finding is that math reforms were sometimes accompanied by changes in requirements for other subjects. While our analysis controls for the total number of other courses required, we do not have enough statistical power to separately estimate the effect of each subject.

Because high school education interventions can impact asset accumulation, credit management, and investment income, studies that focus solely on the impact of education on wage earnings may underestimate both the private and social returns to investment in human capital. In addition, given that we find no effect of high school personal finance courses, one might reasonably ask whether the substantial financial resources devoted to financial literacy education are well spent. Our focus is on high school curriculum reforms. High school interventions are attractive from a policy perspective because they reach a broad "captive audience," and evidence suggests that those who most need it may be the least likely to seek out financial education (Meier and Sprenger 2013). Moreover, many young people have low levels of financial literacy (Lusardi, Mitchell, and Curto 2010). Nevertheless, our findings suggest that policies to expand high school financial literacy education—19 additional states implemented content standards for such courses between 1997 and 2007 (National Council on Economics Education 2008)—may be misguided. Instead, our findings suggest that increasing math requirements would be a more effective way to improve financial outcomes. Increased high school math instruction has a small, but meaningful, effect on financial outcomes, even on individuals as young as 24–36.

While these results are clearly relevant to policymakers, they are also important for our understanding of financial markets. An increasing body of evidence (for example, Stango and Zinman 2009; Grinblatt, Keloharju, and Linnainmaa 2011) suggests that many individuals make suboptimal financial decisions because they do not understand the costs and benefits of the choices available to them. The fact that financial outcomes can be altered by exposure to more math instruction suggests that these frictions should be taken seriously and that it is important to understand how they may influence a broad range of financial behavior, from mortgage contract choice to investment and insurance product selection.

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