Financial Education and the Debt Behavior of the Young

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Young Americans are heavily reliant on debt and have clear financial literacy shortcomings. In this paper, we study the effects of exposure to financial training on debt outcomes in early adulthood among a large and representative sample of young Americans. Variation in exposure to financial training comes from statewide changes in high school graduation requirements. Using a flexible event study approach, we find that both mathematics and financial education, by and large, decrease reliance on nonstudent debt and improve repayment behavior. Economics training, on the other hand, increases both the likelihood of holding outstanding debt and the prevalence of repayment difficulties. (*JEL* D14, I21)

Received July 9, 2014; accepted December 5, 2015 by Editor Stefan Nagel.

We would like to thank Zach Bleemer and Michael Stewart for invaluable research assistance, Brian Bucks, Chris Carroll, Rajeev Darolia, Tullio Jappelli, Henry Korytkowski, David Laibson, Maria Luengo Prada, Silvia Magri, Olivia Mitchell, Dekuwmini Mornah, Shannon Mudd, Anna Paulson, Max Schmeiser, Kartini Shastry, Joseph Tracy, Didem Tuzeman, Carly Urban, Jonathan Willis, and seminar and conference participants at the American Economic Association meetings, the Association for Education Finance and Policy meetings, the Eastern Economic Association meetings, ECB 2013 Conference on Household Finance & Consumption, 2014 NBER Summer Institute Children's Workshop, 2015 European Conference on Household Finance, the Federal Reserve Banks of Kansas City, New York, and Philadelphia, and University of Michigan 2013 Aspen Conference on Economic Decision-making for comments. The views and opinions offered in this paper do not necessarily reflect the position of the Federal Reserve Bank of New York or the Federal Reserve System. Supplementary data can be found on *The Review of Financial Studies* web site. Send correspondence to Basit Zafar, Research and Statistics, Federal Reserve Bank of New York, 33 Liberty St., New York, NY 10045; telephone: 212-720-5648. E-mail: basit.zafar@ny.frb.org.

doi:10.1093/rfs/hhw006 Advance Access publication February 24, 2016

Young adults in the United States are heavily reliant on debt, and their level of financial literacy is low. Of twenty-five year olds in the FRBNY Consumer Credit Panel (CCP) in 2012, 79% held consumer debt. The average debt balance among all 2012 CCP twenty-five year olds was $22,911; similar evidence of youth debt can be found in the 2010 SCF (Bricker et al. 2012). Despite this extensive interaction with lending markets, a majority of high school and college students fail basic financial literacy tests (Hastings, Madrian, and Skimmyhorn 2013; Markow and Bagnaschi 2005; Shim et al. 2010). The low financial literacy rates among U.S. youth and an effective delinquency rate of over 30% on student loans for young borrowers in repayment (Brown et al. 2013a), along with the well-established correlation between financial literacy and financial well-being, which we discuss later in the paper, has prompted policy makers and the media to push for more financial education.[1] However, evidence of the causal effect of financial training on debt outcomes for the *young* is largely based on field and natural experiments of modest scale and is, at best, mixed (see Fernandes, Lynch, and Netermeyer 2014).

Our analysis addresses the question of the effectiveness of financial education by analyzing large-scale changes in financial training exposure in a 2% sample of young Americans and tracking their debt outcomes over the decade immediately following the high school training. Given weak prior evidence, we attempt to identify meaningful effects of financial training in places in which we think they are most likely to exist. We look for effects of very recent changes in financial training, which involve large increases in required classroom hours and apply to millions of U.S. students, and we look for these effects in the years immediately following the training, in debt decisions that are relevant to most of the treated population. Failure to find effects of financial training in this context could, following Fernandes, Lynch, and Netermeyer (2014), both unite and reinforce the findings of several smaller and disparate field studies. On the other hand, evidence of meaningful effects of financial training in this context could derive from some or all of a number of adjustments to the methodology. The technology of financial training may have improved over recent decades. Effects may appear only following very intensive interventions, at earlier ages only, or only in a much larger population. Finally, it may be necessary to track outcomes at very young ages, shortly after training occurs, and in debt choices that are relevant to the majority of the treated population.

For this purpose, we use variation in financial education—more specifically, financial literacy, economics, and mathematics—graduation requirements mandated by state-level high school curricula over the late 1990s and 2000s, in combination with detailed consumer liability data from the CCP. The CCP

[1] See, for example, Ferguson (2012) and Surowiecki (2010). Jack Lew, the Treasury Secretary, recently said "In today's economy, it is also essential for Americans to develop basic financial knowledge and learn how to navigate a complex financial system. We need to make sure young people can make smart decisions about what financial products to use. That young people can plan and save for the long term while managing expenses and debt in the short-term." (Treasury Department 2013).

is an ongoing quarterly panel on consumer debts comprising a 5% sample of U.S. credit reports from Equifax, one of three major national credit reporting agencies.

Our identification strategy exploits variation in the timing of enactment of financial education reforms in high school curricula across and within states. In 1999, ten states required high school enrollment in economics courses, a number that doubled to twenty by 2012. Similarly, only one out of fifty states required a financial literacy course for graduation in 1999; by 2012, this number had increased to seventeen. And, though every state (except one) had some math graduation requirement in place at the start of our time period, nineteen states revised their standards upward by at least a full year between 1999 and 2012. Our baseline empirical strategy, which employs fully flexible time trends for each state, and fully flexible time trends for each cohort, in addition to a separate linear cohort trend for each state and a rich set of local time-varying controls, uses these staggered policy changes to identify the causal impact of financial education on debt-related outcomes of youth. In particular, we do not assume common time trends across states, an assumption that has been shown to be problematic in the context of studies that use changes in compulsory schooling laws (Stephens and Yang 2014). That is, our empirical specification directly controls for the possibility that states that implement financial training mandates may have preexisting trends that differ from those that do not, and that trends in the outcomes across different birth cohorts may differ. Conditional on this extensive set of controls, our identifying assumption hinges on states' implementation of these reforms being uncorrelated with those omitted determinants of financial outcomes that vary nonlinearly from cohort to cohort within a state and are not shared either by all young residents of a given state in the current year or by all members of a given U.S. cohort in the current year.

The empirical analysis reveals that exposure to financial and quantitative education has significant, if moderate, impacts on the debt-related outcomes of 19 to 29 year olds. Additional mathematics training leads to improved creditworthiness (as measured by the Equifax risk score, which is similar to the FICO score) and decreases adverse outcomes, such as accounts in collection. It also leads to significant positive impacts on the propensity to hold student debt and on student debt balances. Math education, however, has no impact on the extensive margin, that is, the likelihood of having a credit report. The impacts of math education seem to fade over time in early adulthood. The exception is student borrowing, which accumulates as the borrower ages.

Financial literacy exposure increases the prevalence of credit reports in this age group. Since having older credit accounts typically increases credit scores (Federal Reserve Bank of Philadelphia 2012), this suggests improved understanding of the value of credit history. Along the intensive margin, financial literacy training leads to a modest, but highly significant, decline in the likelihood of having any outstanding debt for this large population (a decrease of 0.6 percentage points on a base of 76.4%). It also brings a small decline in delinquency. As in the case of math education, the impacts of financial literacy training also seem to fade with age.

In marked contrast to the estimated impacts of mathematics and financial literacy education, we see that economic education leads to a modest increase in the likelihood of holding outstanding debt among our large estimation sample, driven by similar upticks in the rates of holding both nonhousing and housing debt, and that economic education leads to small, but significant, increases in repayment difficulties. We find little impact of economics education on the propensity of youth having a credit report. The effects of economics education also strengthen with age. For example, estimated repayment difficulties emerge gradually. By the time sample members have reached age twenty-seven, those experiencing an economic education reform are two percentage points more likely to have an account in collections, have a 0.8% greater share of debt balance in delinquency, and, on average, have credit scores that are 9.2 points lower than those not exposed to economics education mandates.

We also incorporate heterogeneous treatment effects (by high school graduation cohorts) in our analysis. For several of the outcomes described above, the effects of economics or financial literacy training reforms tend to augment several years after the reforms are implemented, suggesting a lag between the passage of legislation and (effective) implementation of new curricula. We also report a series of sensitivity analysis to test the robustness of our findings. Our results are robust to correcting the standard errors for multiple hypotheses testing, accounting for confounds, such as the CARD Act, which may have impacted younger cohorts differentially, and a falsification test implementing placebo reforms. Exploiting the fact that some cohorts in certain states are exposed to both economics and financial literacy reforms, we investigate the possibility that the impacts economics and financial literacy may interact. However, since we do not find evidence of this, the impacts are likely additive.

Finally, our findings of nontrivial impacts, coupled with our result that the impacts of high school economics education accumulate over the individuals' ages, may quell concerns raised by the prior literature (that we discuss below) regarding the legitimacy of funding financial education programs in the United States (see, for example, Cole, Paulson, and Shastry 2015; the debate as discussed in Hastings, Madrian, and Skimmyhorn 2013). Given the unprecedented rise in household leverage over the 2000s (Mian and Sufi 2011), news regarding the effectiveness of financial education in improving debt behavior is particularly relevant. It is worth noting, however, that the objective of this study is to identify the causal effects of quantitative and financial education training on debt outcomes—no normative or efficiency claims are made regarding the impacts. Assessing the welfare implications of these impacts is challenging since, as we discuss later, economic and quantitative education is positively related with income and wealth. Our paper offers no framework for evaluating the desirability of, for example, a change in bankruptcies due to exposure to quantitative training. While default may be unwelcome, the failure to exploit the bankruptcy option in certain states of the world may itself be a source of inefficiency in a consumer's intertemporal decision making (Fay, Hurst, and White 2002). Our goal is to identify the response of various debt behaviors to financial and quantitative training, whether desirable or undesirable.

Our analysis captures the impact of a required year of financial education, and not of an actual year of financial education. That is, our estimates measure the intent-to-treat (ITT) effects of these financial mandates. The ITT estimates provide the average effect of the mandates on youth, including those for whom the mandates had no impact on actual course-taking. Therefore, our analysis is likely to give a conservative estimate of the effect of an additional year of financial education (what is generally referred to as the treatment-on-the-treated (TOT) estimate), since some youth in the treated states are likely to have already been taking financial education courses and some youth in control states are likely to have taken these courses even in the absence of a requirement. Below, we show evidence suggesting that these mandates do seem to have sizable impacts on students' measured financial literacy.

1. Literature and Data

1.1 Prior literature

A large collection of evidence suggests a high cost of limited financial knowledge. Individuals with lower cognitive ability and lower financial knowledge are more likely to make financial mistakes (Kimball and Shumway 2007; Agarwal et al. 2009; Agarwal and Mazumder 2013; Benjamin, Brown, and Shapiro 2013). Financial mistakes are most common among the youngest and oldest consumers (Agarwal et al. 2009), as well as those with low levels of education (Campbell, Giglio, and Pathak 2011). And these are costly: households with low levels of financial literacy are less likely to plan for retirement (Lusardi and Mitchell 2014; Banks and Oldfield 2007; Banks, O'Dea, and Oldfield 2010), are less likely to have savings (Banks and Oldfield 2007; Smith et al. 2010), are likely to borrow at higher interest rates (Lusardi and Tufano 2015; Stango and Zinman 2009), are more likely to default on mortgage payments (Gerardi, Goette, and Meier 2013), are more likely to withdraw housing equity (Duca and Kumar 2014), and are less likely to participate in financial markets (Christelis, Jappelli, and Padula 2010; van Rooij et al. 2007; Calvet, Campbell, and Sodini 2007; 2009; Kimball and Shumway 2007; Smith et al. 2010).

Our paper is related to the above literature on financial education and financial decision making. This literature primarily emphasizes saving rates and investment income as targets of quantitative education (see, for example, Bayer, Bernheim, and Scholz 2009; Choi, Laibson, and Madrian 2011; Lusardi

2004; Bernheim and Garrett 2003). The effect of financial training on retirement saving is of obvious importance. But saving is considerably less relevant in early adulthood. To the extent that financial literacy interventions occur during high school, debt behavior may be an outcome of more immediate relevance. For example, while 94% of Survey of Consumer Finances (SCF) households with heads under thirty-five years of age in 2010 report holding financial assets, the conditional median value of these assets is just $5,500. The evidence suggests that debt, rather than asset accumulation, is the primary financial concern of early adulthood. Next, this literature is largely correlational and hence unable to inform us about the causal impacts of financial education. Exceptions include Bernheim, Garrett, and Maki (2001), van Rooij et al. (2007), Jappelli and Padula (2013), and Cole, Paulson, and Shastry (2014). For causal inference, these studies rely on either ability and literacy measures that predate the relevant financial decisions or, as we do, state-level compulsory schooling or state-mandated courses.[2] For example, Bernheim, Garrett, and Maki (2001) find that state financial education mandates in the 1970s and 1980s increased both exposure to financial information and subsequent asset accumulation during adulthood. Cole, Paulson, and Shastry (2014), exploiting variation in compulsory schooling laws, find that education increases financial market participation and decreases the likelihood of adverse debt-related outcomes. Given the timing of compulsory schooling reforms, these outcomes are necessarily studied in a middle-aged sample.

We are aware of two studies that investigate the causal effect of financial education on debt-related outcomes. Cole, Paulson, and Shastry (2015) establish an identification approach quite similar to the one we adopt and investigate the impact of state financial education mandates between 1957 and 1982 (as in Bernheim, Garrett, and Maki 2001) and mathematics reforms from 1984–1994 on investment and debt-related outcomes of middle-aged individuals (primarily consumers in their thirties, forties, and fifties) in the CCP from 1999 onward. While they find a sizable impact of mathematics education on outcomes, they find little effect of financial education on either asset accumulation or successful repayment of debt by middle age.

In a second study of special relevance to this paper, Skimmyhorn (2013) investigates the impact of a financial management course for new soldiers in the U.S. Army. As in this study, the subjects of the intervention are young, and the outcomes of interest involve debt. Skimmyhorn (2013) finds moderately sized effects on a few credit-related outcomes (such as credit card and consumer

[2] An alternate approach uses randomized access to financial education. Drexler, Fischer, and Schoar (2014), discussed below, experimentally varied access to financial education for small-scale entrepreneurs and found no effect of financial principles-based training on financial management practices a year later, but significant effects of rule of thumb-based training. Other randomized trials that reveal little effect of financial training include Gartner and Todd (2005), Servon and Kaestner (2008), and Choi, Laibson, and Madrian (2011). Hastings, Madrian, and Skimmyhorn (2013) includes a rich, up-to-date discussion of the state of the literature on financial training effects and concludes that there is little robust positive evidence.

finance loan balances), but little impact on credit scores, adverse legal actions, and having active credit.

Our conclusions regarding the impact of financial education differ in some meaningful ways from the results of these two studies and from the weak evidence on financial education effects produced by the broader literature. What may potentially reconcile the latter with our evidence of successful financial education is the age difference in our samples and our focus on debt-related outcomes (instead of asset accumulation). Relative to Cole, Paulson, and Shastry (2015), we look for effects of financial education immediately after high school. In addition, we study the effects of more recent financial education reforms. Our results may, in part, reflect improvements in the technology of financial training over the past two decades. Relative to Skimmyhorn (2013), our approximately representative sample of young U.S. consumers may behave differently from a sample of new soldiers. Further, the effects of an eight-hour training program may differ from those of a year-long high school course.

1.2 Data

1.2.1 Educational reforms in economics, financial literacy, and mathematics. To proxy for individual exposure to economics, financial literacy, and mathematics education, we track state-level policy changes from 1998 through 2012. Our focus on this time period is motivated by data availability, as well as our interest in recent debt outcomes for young borrowers. The earliest surveys of the National Council for Economic Education (NCEE)—the only comprehensive and centralized source of recent economics and financial literacy high school requirement data—are from 1998–1999. Table 1 reports a national summary of these reforms. We only consider those reforms that require high school financial education courses (opposed to reforms that offered elective courses in these areas). This is because a metric of a required course is a better representation of the true increase in exposure to education in the given subject than, for example, a state-wide requirement that high schools offer a course in the given subject (see Bernheim, Garrett, and Maki 2001 for evidence of the lack of impact of elective offerings on recalled financial education).

For economics and financial literacy, our policy data come from the NCEE biennial Survey of the States, which reports each state's status in several aspects of economic or financial literacy education, like curriculum inclusion and mandatory testing. For economics education, the policy reform of interest is whether or not a state legislated that all high school students complete at least one economics course before graduation; more specifically, the analysis uses the timing of the legislation of the mandate. Likewise, for financial literacy education, the policy reform of interest is whether or not (and when) a state legislated that all high school students complete at least one financial literacy

Table 1
Education policy reforms, by state

State	Year of : Fin lit mandate[a]	Economics mandate[a]	Mathematics reform[b]
Alabama	2000	< 1998	
Alaska			
Arizona		2009	2008
Arkansas		2009	2004
California		< 1998	
Colorado			
Connecticut			< 1998
Delaware			
District of Columbia			
Florida		< 1998	
Georgia	2005	< 1998	2004 (then again in 2008)
Hawaii			
Idaho	2000	< 1998	2006
Illinois	< 1998		< 1998 (then again in 2006)
Indiana		2007	2006
Iowa			
Kansas			2006
Kentucky	2002	2002	< 1998
Louisiana	2007	< 1998	
Maine			
Maryland	2009		
Massachusetts			
Michigan		2007	
Minnesota			
Mississippi			< 1998
Missouri	2007	2007	
Montana			
Nebraska			
Nevada			2000
New Hampshire		< 1998	2006
New Jersey	2009	2009	< 1998
New Mexico		2000	< 1998 (then again in 2008)
New York	2000		2006
North Carolina	2011	< 1998	< 1998 (then again in 2006)
North Dakota			
Ohio			< 1998 (then again in 2002)
Oklahoma	2009		2002
Oregon			
Pennsylvania			
Rhode Island			2006 (then again in 2008)
South Carolina			< 1998 (then again in 2000)
South Dakota	2007	2002	2006
Tennessee	2009	< 1998	
Texas		< 1998	
Utah	2005		
Vermont			
Virginia	2009	2009	< 1998
Washington			2008
West Virginia	2011		< 1998 (then again in 2006)
Wisconsin			
Wyoming			

[a] from the National Council on Economic Education.
[b] from the Council of Chief State School Officers; reform is defined as a one-year increase in required math for high school graduation; states with two reforms have subsequent years reported in parentheses.

course before graduation. This definition yields meaningful variation over the course of our 1998 to 2012 time period, as described already.[3]

Our mathematics education data come from a biennial survey, Key State Education Policies on PK-12 Education, conducted by the Council of Chief State School Officers (CCSSO). By 1998, all states excepting North Dakota had some sort of mathematics requirement for high school graduation. The object of interest is the required years of math education for graduation. Variation in this variable across states (and within states over time) is generated by whether or not (and when) a state enacted a policy reform requiring a one-year increase in math education for graduation. As shown in Table 1, eleven states enacted a single one-year increase, and eight states enacted repeated one-year increases.

Next, we provide some motivation for using these proxies of financial education. Such policy reforms have been shown to be causally correlated with our treatment variables of interest: exposure to subject-level education in economics and financial literacy, and years of mathematics education (Bernheim, Garrett, and Maki 2001; Cole, Paulson, and Shastry 2015; Goodman 2012). As mentioned above, our analysis, which exploits the variation in financial education mandates across states and over time, yields ITT estimates and addresses the policy question of the causal impact of financial education mandates. TOT estimates (which would inform us of the causal impacts of exposure to additional financial education) would require knowledge of the proportion of youth impacted by these mandates. To our knowledge, there is limited and insufficient data that would allow us to obtain credible TOT estimates from our ITT estimates.[4]

Even though we cannot directly investigate the extent to which these mandates impact actual course-taking, we can analyze the impact of financial literacy requirements on youth's financial literacy using the 2004, 2006, and 2008 National Jump$tart Coalition Survey of High School Students.[5] We conduct a simple difference-in-differences exercise, using states that implement financial literacy reforms from 2005–2007 and for whom we have aggregate statistics in the relevant Jump$tart surveys (i.e., at least one survey observation before and after the mandate year) as treated states, and states for which we have the Jump$tart data in the relevant years and do not implement the mandate

[3] We code any missing years as equal to the last available observation for the state. For example, though the NCEE did not publish a survey for 2006, we extrapolate 2005 data forward instead of leaving all variables as missing values in 2006. This method allows us to capitalize on more variation in the outcome and control variables. As mentioned above, the NCEE surveys are biennial and were conducted in 1998, 2000, 2002, 2005, 2007, 2009, and 2011.

[4] Neither the Education Longitudinal Study of 2002 (which has transcript data on a sample of high school sophomores in 2002) nor the NLSY97 (which consists of youth who turn 18 between 1998 and 2002) provides sufficient variation over time and across states; in addition, the transcript data do not have detailed information on economics and financial literacy courses.

[5] The Jump$tart Coalition has been conducting biannual surveys since 1998 to measure the financial literacy of a nationally representative sample of (public school) high school seniors. We were able to obtain state-level statistics for 2004, 2006, and 2008. However, state-level aggregates are only available for a subset of states in each of those years.

as control states. Pooling across these years, we find that financial literacy mandates (in Louisiana, Missouri, and Utah), on average, led to an increase of 3.9 points on students' financial literacy score on the exam. This effect is precisely estimated (p-value = 0.000), and is sizable—it corresponds to a one-standard-deviation increase in students' scores (the mean score is 50.5, with a standard deviation of 3.8 points). Data limitations prevent us from providing any further conclusive evidence on the impact of these mandates on students' quantitative skills, but this rudimentary analysis suggests that such mandates do have sizable impacts on skills. This is consistent with Lusardi et al. (2014), who find that online financial educational programs do increase self-efficacy and financial literacy.

Another reason for using these reforms as proxies for financial education is early research (Mayer 1989; Bernheim, Garrett, and Maki 2001), which indicates that consumer education reforms are primarily precipitated by the action of specific lobbyists and legislators rather than by large-scale pressure from public opinion, suggesting these reforms influence subject-level exposure in a way that may not be driven by potentially endogenous trends in public opinion. While earlier research has not uncovered significant socioeconomic or educational differences between states that implement consumer education policies and those that do not (Ford 1977), Cole, Paulson, and Shastry (2015) argue that states that introduced financial education mandates between 1957 and 1982 were trending differently from states that did not introduce such mandates. In light of this mixed evidence, our empirical specification allows for flexibly parameterized state-time and cohort-time trends.

Table 2 provides some helpful information regarding the empirical variation that identifies our central parameters of interest. Notably, 54% of our sample was exposed to an economic education reform (with 11% out of the 54% also being exposed to financial literacy education); 17% was exposed to a financial literacy education reform; and 34% was exposed to a mathematics reform. Further, 14% of the sample did not experience an economic reform but resided in a state that would eventually enact an economic reform, identifying prereform trends. The analogous rate for financial education reforms is 22%.

1.2.2 Consumer credit behavior. The FRBNY Consumer Credit Panel (CCP) is a longitudinal dataset on consumer liabilities and repayment. It is built from consumer credit report data provided by Equifax. Data are collected quarterly beginning in 1999Q1, and the panel is ongoing. The sample comprises a randomly selected 5% of U.S. individuals with credit reports (and Social Security numbers). The CCP sample design automatically refreshes the panel by including all new credit report holders who meet the (time-fixed) criteria for inclusion and hence remains representative for any given quarter (Lee and van der Klaaw 2010). In sum, the CCP permits unique insight into the question at hand as a result of the size, representativeness, frequency, and recentness of the dataset. Its sampling scheme allows extrapolation to

Table 2
Summary statistics for the estimation sample

Variable	N	Mean	SD	Min	Median	Max	Zeros
Outcome variables							
Risk score	5,989,930	629.25	93.63	280	643	845	0.00%
Number of delinquent accounts	6,493,042	0.178	0.718	0	0	33	89.90%
Percent of balance in delinquent accounts	6,493,042	5.50%	20.69%	0%	0%	100%	89.93%
Bankruptcy in next 24 months	5,735,206	0.56%	7.46%	0%	0%	100%	99.44%
Collections flag	6,450,004	40.00%	48.99%	0%	0%	100%	60.00%
Any debt	6,493,042	76.38%	42.48%	0%	100%	100%	23.62%
Ever had nonhousing debt	6,493,042	85.35%	35.36%	0%	100%	100%	14.65%
Log nonhousing debt balance	6,493,042	5.929	4.906	−2.30	8.08	16.09	0.00%
Nonhousing debt balance	6,493,042	$11,251	$20,603	$0	$3,230	$9,743,665	24.22%
Ever had auto/credit card debt	6,493,042	78.45%	41.12%	0%	100%	100%	21.55%
Log auto/credit card balance	6,493,042	4.527	5.072	−2.30	6.68	16.09	0.00%
Auto/credit card balance	6,493,042	$5,883	$12,155	$0	$792	$9,743,665	33.55%
Ever had home-secured debt	6,493,042	8.96%	28.57%	0%	0%	100%	91.04%
Log home-secured debt balance	6,493,042	−1.257	3.685	−2.30	−2.30	16.09	0.00%
Home-secured debt balance	6,493,042	$11,448	$51,177	$0	$0	$9,698,306	92.52%
Ever had student loan debt	6,493,042	32.92%	46.99%	0%	0%	100%	67.08%
Log student loan balance	6,493,042	0.943	5.226	−2.30	−2.30	13.35	0.00%
Student loan balance	6,493,042	$5,368	$16,267	$0	$0	$627,965	71.87%
Education reform-related variables							
Went to HS before state enacted Econ reform	6,493,042	0.140	0.347	0	0	1	85.99%
Exposed to Econ reform only	6,493,042	0.425	0.494	0	0	1	57.46%
Went to HS before state enacted Fin Lit reform	6,493,042	0.215	0.411	0	0	1	78.53%
Exposed to Financial Literacy reform only	6,493,042	0.059	0.236	0	0	1	94.09%
Exposed to both Fin Lit and Econ reforms	6,493,042	0.111	0.314	0	0	1	88.94%
Exposed to Math reform	6,493,042	0.342	0.474	0	0	1	65.77%
State # of years of math required to graduate	6,493,042	2.672	0.639	0	3	4	0.20%
Control variables							
County-level income per capita (in millions)	6,493,042	0.060	0.017	0.00	0.06	0.30	0.00%
County-level unemployment rate	6,493,042	6.845	2.642	0.93	6.43	29.63	0.00%
# of years of state compulsory schooling	6,493,042	10.268	0.794	8	10	11	0.00%
State grad requirement: # of years English	6,493,042	3.724	0.507	1	4	4	0.00%
State grad requirement: # of years Science	6,493,042	2.521	0.687	1	3	4	0.00%
Birth year	6,493,042	1985.7	3.386	1981	1985	1995	0.00%

[a] 2% panel of Equifax CCP, Q4 of years 1999–2014,individuals born after 1980. Source of outcome vars: FRBNY Consumer Credit Panel/Equifax.

national aggregates and spares us most concerns regarding attrition and representativeness over the course of a long panel.

While the sample represents only those individuals with credit reports, the coverage of credit reports is fairly complete in the United States. Aggregates extrapolated from the data match well those based on the American Community Survey, Flow of Funds Accounts of the United States, and SCF (Lee and van der Klaaw 2010; Brown et al. 2013b). Because we focus on the impact of recent education reforms on the credit behavior of the young, we restrict our dataset to individuals born in or after 1981 and those who are over eighteen years old (implying that our youngest cohort is born in 1995). These cohorts will graduate high school in or after 1999, coinciding with the start of our economics and financial literacy education reform data. One might be concerned about the representativeness of younger individuals in the CCP. However, Lee and van der Klaaw (2010) and Brown et al. (2013b) extrapolate similar populations of U.S. residents or households, grouped by age, using the CCP and the American Community Survey (ACS), SCF, and Census, suggesting that the vast majority of U.S. individuals at younger ages have credit reports. Bleemer et al. (2014) provide further evidence on the strength of CCP coverage at young ages.

To accommodate the annual nature of our other variables, we use only fourth quarter Equifax data from the years 2000 through 2014. Additionally, as the time-series aspect of our study drastically increases the number of observations, we employ a random 2%, rather than the full random 5%, sample of the eligible U.S. population. Our final dataset is therefore an annual (unbalanced) panel from 1999 to 2014, with 7.11 million total observations,[6] and data from 1,234,381 distinct individuals. On average, the panel contains 444,395 observations per year. As a result of our age constraint, the data are heavily concentrated in later years.

We use a number of consumer debt metrics as our outcome variables. First, we look at the Equifax risk score of the individual. This risk score is similar to the FICO score, in that both model twenty-four-month severe delinquency risk as a function of credit report measures. It varies between 280 and 840 and represents an assessment of the individual's credit worthiness. We also study each individual's proportion of debt balance that is delinquent, where delinquency is defined as any debt payment that is reported as thirty or more days past due, and an indicator for having had a balance in collections in the past seven years. The size of our sample allows us to estimate reliable models of rare events, and we take as an additional outcome of interest if the individual experiences a bankruptcy over the next twenty-four months. In addition to these repayment measures, we look at debt balances, distinguishing between housing debt (mortgage or home equity debt), nonhousing debt (credit cards and auto loans), and student loans. All the debt variables are in 2012 dollars.[7] Finally, we consider whether the individual has any outstanding debt, as a measure of exposure to credit markets. Exploiting the panel nature of the dataset, we also study whether the individual ever had any housing debt (which, in a sample of consumers in their twenties, is a reasonably complete proxy for past or present home ownership) and/or a student loan.

In our empirical analysis of the impact of financial education on an individual's debt outcomes, we exploit the timing of the change in the education policy of the state in which the individual resided during high school. In the CCP, we only observe residence during the panel. For the purposes of our analysis, as a proxy for the state in which the individual attended high school,

[6] The initial 2% sample consists of 7,337,012 observations. We drop individuals in some of the outlying territories (such as Puerto Rico and Guam) and those with missing ZIP codes, since we do not have region-level controls data for these cases. Furthermore, data on the number of math, science, or English years required for graduation are missing for some ZIP codes, since those mandates are determined by local school boards, and we do not have those data. All told, we are forced to drop 843,970 observations from our analysis.

[7] While holding a mortgage is a somewhat rare behavior in our young estimation sample (with a sample prevalence of 9%), the mortgage balance conditional on borrowing shows a long upper tail. Estimates reported in Section 3 include the log of mortgage balance among the set of outcomes, as an attempt to address the potential for the few large balances to drive the results. Estimates based on alternative specifications are available from the authors.

we use the individual's state of residence at the time when they first appear in the panel.[8] Among those who appear in the panel at age eighteen, Online Appendix Table A1 shows the percentage of individuals living in the same state as the state in which they graduated from high school: 93.7% of the twenty-two year olds were residing in the same state in which they were living at age eighteen; this proportion remains high even among the oldest individuals in our sample. If movement across states is random (both in terms of individuals who choose to migrate and the choice of destination), misclassification of the individual's state of high school should attenuate the estimates in the baseline specification toward zero and bias us against finding an effect of the reforms. The low cross-state movement among the young suggests that mobility-related attenuation of the estimated impact of state-level education policy reforms should be modest.

1.2.3 Other controls. We include a number of state-level educational controls in our specification to account for any variation in consumer credit behavior that may arise from differences in compulsory schooling laws and subject course requirements. Data on compulsory schooling and other course requirements are from the above-mentioned CCSSO report. We compute total required years of schooling by subtracting the age at which children are required to enroll in school from the minimum dropout age. During our time period, states required between 8 and 11 years of school; in the empirical specification, we code this information as a categorical variable.

The subject graduation requirement controls also come from the CCSSO report. We control for requirements in place when the individual was in high school in the subjects of natural science and English by including a continuous variable representing the number of years required by each state for graduation from high school (at the time when the individual was in high school). Over our time period, English and science requirements vary between one and four years, and social studies and math requirements vary between zero and four years. All of these variables display an increase with time.

To address differences in financial behavior due to variation in economic factors, we include ZIP-code-level controls for unemployment and income. Granular unemployment rates, reported as a percent of the local population at the county level, come from the Bureau of Labor Statistics' Local Area Unemployment Statistics, which we obtain for every year from 1999 to 2014.

Income data are available at the ZIP code level from the Internal Revenue Service's Individual Income Tax Statistics. To calculate per capita income, we divide each ZIP code region's adjusted gross income by the region's number

[8] Cole, Paulson, and Shastry (2015) use the same proxy when evaluating the impact of high school personal finance courses mandated by states between 1957 and 1982. It is particularly valid for our application, in that we first observe most of our sample members during their late teens or early 20s.

of returns. We interpolate income values for the three years with missing data (2010, 2013, and 2014), yielding an annual ZIP-code-level panel.

Table 2 displays summary statistics for our outcome and control variables.

2. Empirical Strategy

2.1.1 Motivation. Our Online Appendix briefly summarizes the main themes that appear in the curricula of high school financial literacy and economics courses, since those may be informative about the kinds of impacts the courses may have on students' credit-related outcomes. Here, based on this analysis, we describe what effects one might expect the three types of curriculum reform to have on consumers' borrowing and repayment behavior.

Lesson topics in state financial literacy courses include "Why Credit Matters," "Making a Budget," and "Staying out of Debt." Based on this, we may expect exposure to financial literacy to increase the likelihood of individuals entering credit markets in order to build a credit history. That is, it may increase the proportion of youth who have a credit report. And, conditional on having a credit report, we expect financial literacy education to lead to more favorable outcomes, such as a higher credit score and fewer delinquencies. The impact on debt balances is not entirely clear—given that prior research finds little impact of financial education on earnings, financial literacy education may help youth balance their budgets better and hence may lead to lower debt, particularly debt that is used to support consumption, such as credit card and auto debt.

State high school economics curricula include lessons on "markets," which typically cover topics of supply, demand, prices, and interest rates. This content seems most relevant to our objectives. The potential impact of economic education on an individual's probability of having a credit report is unclear. However, conditional on having a credit report, exposure to basic economic concepts may make students more familiar with financial products and increase their participation in credit markets. For example, we may observe a higher likelihood of having debt and larger debt balances. Predictions regarding delinquency are decidedly ambiguous, as greater debt implies greater risk of delinquency, and yet understanding economic concepts might help young borrowers avoid delinquency. Similarly, the net effect on the individual's risk score is unclear.

Based on evidence in the literature that math education leads to improvements in cognitive skills (Alexander and Pallas 1984) and greater asset accumulation by middle age (Cole, Paulson, and Shastry 2015), we expect greater math exposure to lead to more favorable debt-related outcomes, such as improved credit scores and a lower likelihood of delinquencies. However, the expected impact on debt usage and balances is ambiguous, given that more math training also leads to higher labor market earnings (Goodman 2012; Rose and Betts 2004; Joensen and Nielsen 2009). Relatedly, the expected effect of math exposure on individuals' likelihood of having a credit report is unclear.

2.1.2 Empirical analysis. To estimate the policy effects of financial education on debt-related outcomes, we would like to compare the debt-related outcomes of an individual who is exposed to financial education when in high school to those of an individual who graduates prior to the enactment of financial education policies. We identify the policy effects from the staggered changes (over time and across states) in economic, financial, and mathematics education policy. The dependent variable, $\boldsymbol{Y_{i(sc)zt}}$, is the CCP debt-related outcome of individual i of birth cohort c in high school attendance state s residing in ZIP code z in year t. Our baseline specification is as follows:

$$Y_{i(sc)zt}=\gamma_{st}+\delta_{ct}+\beta^X X_{zt}+\sum n(\beta^n_{post} D^n_{i(sc)})+\beta^{math}_{post} M_{i(sc)}+\alpha_s c_{i(sc)}+\varepsilon_{i(sc)zt}, \quad \text{(I1)}$$

where $D^n_{i(sc)}$ is an indicator for whether i was exposed to education in field n, where $n \epsilon \{economics, financial\ literacy\}$, in state s. $D^n_{i(sc)}$ equals one if i's cohort c graduates from high school after her state enacts the legislation requiring students to complete at least one course in subject n before graduation, and is zero otherwise. We take eighteen as the high school graduation age. So $D^n_{i(sc)}$ equals one if i's cohort c turns eighteen in a year *after* her state enacts the legislation, and equals zero if i's cohort turns eighteen during or before the year in which the state enacts the legislation (or if the state never enacts a policy change). $M_{i(sc)}$ is the mandatory years of math during the high school years of individual i (of cohort c in high school attendance state s).[9] γ_{st} is a vector of state-year fixed effects, and δ_{ct} is a vector of birth cohort-year fixed effects; the staggered implementation of the reforms across states and over time (as well as our large sample size) allows us to identify both state-time and cohort-time fixed effects. α_s allows for a linear state-specific cohort trend. $\varepsilon_{i(sc)zt}$ is an idiosyncratic serror. X_{zt} is a vector of time-varying ZIP code and state controls: a third-order polynomial of average ZIP code per capita gross income, county-level unemployment rate, state-level subject requirements for graduation, and state-level compulsory years of schooling.

The coefficients of interest are β^{econ}_{post}, β^{finlit}_{post}, and β^{math}_{post}. Since the error terms may be correlated among those with the same high school-attendance state and year, as well as over time, we use Driscoll-Kraay (D-K) standard errors (Driscoll and Kraay 1998). The D-K estimator has a cluster interpretation: it is equivalent to state-year clustering, along with use of the Newey-West method to account for serial correlation, which allows for correlations that span different states and years (Foote 2007). Our application relies on state-by-cohort-by-year variation. On the other hand, the textbook case in Bertrand, Duflo, and Mullainathan (2004) involves a panel with state-by-year variation. In fact, the Newey-West correction is one of the remedies suggested

[9] Note that since our specification includes state fixed effects, the variation in mandatory years of math education identifying β^{math}_{post} comes from state legislative changes.

by Bertrand, Duflo, and Mullainathan (2004). The other remedy that they suggest is clustering at the state level. Doing so renders several of our results insignificant, indicative of our identifying variation being too small relative to the residual variation. We prefer the D-K estimator since it has been shown to outperform competing corrections in large-N, moderate-T simulations (as in our case) in the presence of autocorrelation and cross-sectional dependence (Hoechle 2007) and because cluster-robust estimators after pooled OLS do not work very well, even when the number of clusters is as large as 40 or 50 (Wooldridge 2003). Regarding the number of Newey-West lags for the D-K standard error estimator, we rely on a heuristic taken from the first step of the Newey and West (1994) "plug-in" estimator. The number of lags as a function of the length of the panel is $m(T) = \text{floor}[4(T/100)^{2/9}]$. This formula yields $m(16) = \text{floor}[2.66]$. However, given evidence of a tendency of this data-independent heuristic approach to select too short a lag length (Hoechle 2007), we have chosen to estimate using three lags rather than two.

To interpret the results as causal, any study that exploits state-level reforms has to deal with the concern that reform implementation and timing may be correlated with relevant state- and cohort-specific factors. Our I1 specification, which we also refer to as our baseline specification, attempts to account for these concerns through its flexibility. It does not assume common trends across states, as this has been shown to be problematic in studies of state compulsory schooling laws (see Stephens and Yang 2014). Furthermore, the vector γ_{st} accounts flexibly for state-specific and aggregate time trends in the outcomes (e.g., an increase in credit card usage in a given state) and controls for differences across states that may be related to the enactment of the reform in a state. Our approach is quite flexible compared to the common practice of including a set of state- or region-specific linear time trends, in studies that exploit state-level variation in different applications. Differing trends in the outcomes across different birth cohorts are accounted for by the nation-wide cohort-year fixed effects. The state-cohort linear trend allows for the possibility that cohorts within a state may be trending in a specific way that is not accounted for by state-time trends shared among eleven contiguous youth cohorts (we also experimented with higher order polynomials, but they do not seem to qualitatively impact the results). Time-varying controls at the ZIP code (state) level control for changes in the resources and macroeconomic conditions of the ZIP codes (states) that may correlate with the enactment of policy changes. Our identifying assumption, then, is that, conditional on this extensive set of controls, implementation of financial education reforms is uncorrelated with other (state- and cohort-specific) omitted determinants of financial outcomes and, conditional on this extensive set of controls, treatment and control groups have parallel growth.

The β^n_{post} estimate in the baseline model is simply the average treatment effect across all years after the enactment of the reform. States may take a few years to implement a new reform effectively, or they may put the mandates into effect with some delay following the legislation—the effects may vary over time in both cases. To allow for these possibilities, we estimate the following event-study specification:

$$Y_{izt} = \gamma_{st} + \delta_{ct} + \beta^X X_{zt} + \sum_n \left(\sum_{j=-4}^{4} \beta_j^n D_{j,i(sc)}^n \right) + \beta_{post}^{math} M_{i(sc)}$$

$$+ \alpha_s c_{i(sc)} + \varepsilon_{i(sc)zt}. \quad \text{(ES1)}$$

$D_{j,i(sc)}^n$ is an indicator that equals one if i of cohort c graduates from high school in state s (i.e., turns 18) j years after the state implements a policy change in subject n, where $n \epsilon \{economics,\ financial\ literacy\}$. For example, $D_{-2,i(sc)}^{econ}$ is a dummy that equals one if student i graduates from high school two years before the state implements the policy change in economics, and zero otherwise. The specification subdivides the pre- and post- graduation cohorts into nine bins, based on the difference between each individual's graduation year and their home state's year of policy enactment. The bins represent the following graduation timings: four years prior, three years prior, two years prior, one year prior, the same year, one year after, two years after, three years after, or four or more years after policy enactment. The omitted group consists of cohorts that graduate more than four years prior to the reform. Since identification is within state, the beta parameters are estimated off of states that have enough of a pre-trend, that is, have observations for four cohorts prior to the year of the implementation of the reform. This choice was prompted so that we have enough of a pre-trend for the untreated cohorts in treated states; setting the omitted group to cohorts graduating more than 3 years prior makes little difference. Note that states that never implement a reform or those that do not have cohorts graduating from high school more than four years prior to the reform (e.g., Kentucky, which introduces an economics mandate in 2002 and has the oldest cohort graduating from high school in 1999) still contribute to the identification of the nation-wide cohort-time trends, as well as the state-year, and state-cohort linear trends.

The ES1 specification is conceptually equivalent to estimating a separate event study model for each state that implemented a reform and then averaging over these conditional state-specific estimates. It allows us to visualize whether outcomes for untreated cohorts in treated states, on average, trended similarly to those in control states (i.e., whether $\sum_{j=-4}^{-1} \beta_j^n$ are jointly equal to zero), as would be implied by a parallel trends assumption. A lack of trend differences immediately before treatment occurs would also suggest that the enactment of the mandate is not correlated with unobserved factors. Given the many political and legislative hurdles to enacting state-wide mandates, it is unlikely that states can control adoption of reforms with such yearly precision. Another advantage of the ES1 specification is that it allows us to discern plausible situations in which, for example, states become better at teaching financial education over time and the impact of the reforms grows larger for later cohorts, or where states implement the mandates with a delay following enactment. Evidence of a treatment effect requires that $(\sum_{j=1}^{4} \beta_j^n)$ are jointly different from $(\sum_{j=-4}^{-1} \beta_j^n)$. To interpret these numerous coefficients, we compute a Wald test on the difference between the average of the pre-trends and the average of the post-trends. In addition, several figures depict β_j^n series for outcomes of interest. Henceforth, we refer to this event-study specification as the ES1 model.

In addition to estimating the models using outcomes from the pooled sample (where a given individual may appear at different ages), we also estimate the models (I1 and ES1) on outcomes for the individual at ages 22, 25, and 27. This allows us to investigate the effects of these reforms at multiple points early on in the life cycle. When estimating these models, we replace the $(\gamma_{st} + \delta_{ct})$ terms with a state fixed effect and a time fixed effect $(\gamma_s + \delta_t)$, eliminate the state-specific linear cohort trend, and continue to use D-K standard errors.

3. Results

3.1 Baseline model

3.1.1 Impact on the pooled sample. Estimates of Equation (I1) are presented in Table 3. Looking across the first row, we see that exposure to an additional mandatory math year has a significant effect on several of our outcomes of interest. It leads to a small, but statistically precise, increase of 1.1 points, on average, in individuals' risk scores; given a sample standard deviation of 94 points, this is equivalent to an increase of a 0.01 of the standard deviation in the individuals' risk score. An additional year of math requirement also leads to a modest decline in the likelihood of having accounts in collections.

Next, we turn to the effect of an additional year of required math on the likelihood of having outstanding debt. On net, Column (5) shows that an additional year of math schooling increases the probability of having outstanding debt of any kind by a modest 0.2 percentage points (pp) on a base of 76.4%, though the estimate is not statistically significant. Looking at specific debt categories, we see that additional required math exerts its most decisive effect on student loans. An additional year of required math leads to a statistically significant average increase of 0.5 percentage points in the probability of ever having student loans (on a base of 32.9% and an increase of \$212 in mean student loan balances. In separate analysis (available from the authors upon request), we find no evidence of state-level math education mandates affecting state-wide high school graduation rates (a finding similar to that of Goodman 2012 for the 1980s math reforms), so we can rule out that channel as a possible explanation for the increase in student loan take-up. Notably, an additional year of math has little impact on the prevalence of nonhousing (auto and credit card) debt or homeownership (proxied for by the presence of housing debt on the respondent's credit report, which for our twenty-something consumers is a fairly reliable indicator of any past or present homeownership). Instead, the effect of the additional math requirement lies

Table 3
I1 (baseline) model estimates, for pooled sample

	Risk score	Percent of balance delinquen	Bankruptcy in next 24 months	Collections flag	Any outstanding Debt	Ever had nonhousing debt	Nonhome balance	Ever had mortgage debt	Log home-secured balance	Ever had student loans	Student loan balance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Math years	1.104***	−0.0335	0.0178	−0.642***	0.184	−0.0212	151.9*	−0.0176	−0.00877***	0.521***	211.5**
	(0.348)	(0.0445)	(0.0223)	(0.104)	(0.137)	(0.0655)	(85.20)	(0.0231)	(0.00228)	(0.0970)	(78.19)
Economics reform	−0.614	0.233**	0.0694**	−0.319	0.568***	0.566***	−435.3*	0.404**	0.0534*	−0.159	−643.7***
	(0.593)	(0.0992)	(0.0278)	(0.496)	(0.188)	(0.134)	(206.6)	(0.154)	(0.0261)	(0.241)	(170.4)
Fin Lit reform	0.205	−0.129**	−0.0447	0.240	−0.632***	−0.293	6.130	−0.203**	−0.0214	0.214	222.0
	(0.123)	(0.0495)	(0.0282)	(0.266)	(0.167)	(0.200)	(128.0)	(0.0844)	(0.0146)	(0.124)	(148.7)
N	5989930	6493042	5735206	6450004	6493042	6493042	6493042	6493042	6493042	6493042	6493042
Mean of dep var	629.2	5.503	0.560	40.00	76.38	85.35	11250.8	8.964	−1.257	32.92	5367.6
Std dev of dep var	93.63	20.69	7.463	48.99	42.48	35.36	20603.1	28.57	3.685	46.99	16267.3

[a] All regressions include state-year and birth cohort-year fixed effects.
[b] Variables in Columns 2, 3, 4, 5, 6, 8, and 10 are expressed on a 0-100 scale (for binary outcomes, no is coded as 0 and yes is coded as 100).
[c] Driscoll-Kraay standard errors are reported in parentheses and rely on three Newey-West lags. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.
Source: FRBNY Consumer Credit Panel/Equifax.

almost exclusively in better measured creditworthiness and increased student borrowing.

Moving to the impacts of mandatory economics education, we see they are quite different from those of math education. They include an average decline of 0.6 points in the individual's risk score (though the estimate is not significant), an increase in the proportion of balances that are delinquent, and a small, but precisely estimated, increase of 0.07 percentage points in the probability of bankruptcy over the next two years (on a base of 0.56%). Like math, economics education leads to an increase in the prevalence of outstanding debt. However, the magnitude of the estimated effect for economics is three times as large, at 0.6 percentage points on average, and it is highly significant. Further, in this case the debt prevalence increase seems to arise from decisive increases in both nonhousing and housing debt and no increase in participation in student borrowing. Economics requirements, then, are followed by meaningful increases in the prevalence of all debt categories that we consider, save student debt (and including auto and card debt, in estimates available from the authors), and, perhaps subsequently, by small but significant increases in difficulties with repayment.

The third row of Table 3 shows that mandatory financial education leads to a decline in the proportion of balance that is delinquent. Unlike the other two mandates, financial literacy education actually leads to a decline of a 0.6 percentage points in the likelihood of having any outstanding debt. The impacts by specific debt categories are very similar to those of math education—financial education results in a decline in the prevalence of all debt types, except student loans (though only the estimate on mortgage debt is significant).

Overall, these results indicate that math and financial education lead to an improvement in financial outcomes, in particular a decline in the prevalence of arguably adverse repayment outcomes, as well as a shift out of reliance on other standard debts and into reliance on student loans (though the student loan change is smaller and not quite significant for the case of financial education). Economic education, on the other hand, seems to lead to the converse.

3.1.2 Impact by age. To explore how the effects of these financial education reforms evolve over the course of early adulthood, Table 4 presents estimates of the I1 specification, estimated separately for 22, 25, and 27 year olds. The patterns we find are not unique to this set of ages; results are qualitatively similar for all ages from 19–29 years old, though the sample size is smaller at later ages (plots available from the authors upon request). This age-specific specification, as mentioned above, includes state and time fixed effects.

The impact of an additional year of math requirement across outcomes varies by age. In some cases, such as the likelihood of having any outstanding debt and having student loans, the impacts strengthen with age. In other cases, the estimated effects decline or even reverse signs as borrowers age: for example, while math shows a significant decline in the likelihood of ever having auto

Table 4
Model I1 estimates, by age

	Risk score	Percent of balance delinquen	Bankruptcy in next 24 months	Collections flag	Any outstanding Debt	Ever had nonhousing debt	Nonhome balance	Ever had mortgage debt	Log home-secured balance	Ever had student loans	Student loan balance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Mathematics											
22-year-old	0.666*	−0.00974	−0.0680	−0.605***	−0.200	−0.364**	39.89	−0.369***	−0.0437***	0.383	123.7**
	(0.320)	(0.0506)	(0.0475)	(0.166)	(0.173)	(0.140)	(45.68)	(0.0846)	(0.00879)	(0.290)	(40.65)
25-year-old	0.165	−0.0333	−0.110**	−0.949***	0.243**	0.132	384.9**	−0.350***	−0.0322**	0.658*	397.4**
	(0.266)	(0.0710)	(0.0417)	(0.179)	(0.0896)	(0.0727)	(135.3)	(0.0751)	(0.0102)	(0.295)	(133.4)
27-year-old	−1.365*	0.253*	−0.0549	0.0815	0.754*	0.357**	699.0***	−0.309	−0.0378	1.571***	661.9**
	(0.565)	(0.109)	(0.0704)	(0.349)	(0.326)	(0.122)	(147.0)	(0.237)	(0.0213)	(0.415)	(190.6)
Financial Literacy											
22-year-old	−0.926	−0.365**	−0.247***	2.039**	−1.434***	−1.461***	−11.48	−0.480***	−0.0505**	0.575	161.4
	(1.126)	(0.122)	(0.0398)	(0.829)	(0.195)	(0.112)	(177.8)	(0.124)	(0.0186)	(0.489)	(109.8)
25-year-old	−1.321***	0.186***	−0.205	0.377	−0.0406	−0.432*	454.6***	−1.018***	−0.0912**	0.740	365.3**
	(0.320)	(0.0505)	(0.223)	(0.495)	(0.101)	(0.201)	(65.56)	(0.269)	(0.0275)	(0.797)	(112.1)
27-year-old	0.839	0.127	0.00858	−0.375	0.0596	−0.350*	627.2***	−0.0625	0.0472	0.0560	435.1**
	(0.451)	(0.107)	(0.0550)	(0.398)	(0.626)	(0.178)	(82.76)	(0.321)	(0.0309)	(0.420)	(136.8)
Economics											
22-year-old	−2.006**	−0.0383	−0.0861	0.133	0.361	0.510**	197.1	−0.0860	−0.0260	0.286	140.2
	(0.789)	(0.0883)	(0.0756)	(0.311)	(0.293)	(0.210)	(190.6)	(0.101)	(0.0169)	(0.818)	(169.7)
25-year-old	−3.094	0.661**	−0.00632	0.584	0.335	0.385	27.32	0.101	0.0250	−0.132	−208.2
	(2.388)	(0.281)	(0.149)	(1.137)	(0.329)	(0.337)	(179.2)	(0.337)	(0.0513)	(0.741)	(152.3)
27-year-old	−9.206***	0.799**	−0.430*	2.170***	−0.520	0.110	245.9	−0.561**	−0.0653**	1.259	−7.890
	(1.250)	(0.223)	(0.209)	(0.373)	(0.968)	(0.407)	(356.1)	(0.202)	(0.0249)	(0.954)	(311.4)

(continued)

Table 4
Continued

	Risk score	Percent of balance delinquen	Bankruptcy in next 24 months	Collections flag	Any outstanding Debt	Ever had nonhousing debt	Nonhome balance	Ever had mortgage debt	Log home-secured balance	Ever had student loans	Student loan balance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Number of observations											
22-year-old	676191	735418	667744	730307	735418	735418	735418	735418	735418	735418	735418
25-year-old	594217	642849	565420	640338	642849	642849	642849	642849	642849	642849	642849
27-year-old	471650	508675	431391	506797	508675	508675	508675	508675	508675	508675	508675
Dependent variable mean											
22-year-old	623.9	5.190	0.493	38.15	75.83	83.34	9494.2	3.369	−1.896	30.62	4523.5
25-year-old	629.0	5.835	0.689	48.16	77.48	88.37	13718.8	11.22	−0.964	34.95	6577.3
27-year-old	636.9	6.105	0.853	48.91	77.84	90.89	15361.6	18.29	−0.179	37.09	7399.4
Dependent variable standard deviation											
22-year-old	91.81	20.06	7.007	48.57	42.81	37.26	19031.9	18.04	2.331	46.09	11982.6
25-year-old	96.73	21.10	8.272	49.97	41.77	32.06	22746.5	31.56	4.122	47.68	18495.4
27-year-old	99.21	21.65	9.194	49.99	41.53	28.78	26477.7	38.66	5.045	48.31	22013.3

[a] All regressions include state fixed effects and time fixed effects.
[b] Variables in Columns 2, 3, 4, 5, 6, 8, and 10 are expressed on a 0-100 scale (for binary outcomes, no is coded as 0 and yes is coded as 100).
[c] Driscoll-Kraay standard errors are reported in parentheses and rely on three Newey-West lags. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.
Source: FRBNY Consumer Credit Panel/Equifax.

and credit card (nonhousing) debt at age 22, the impact fades by age 25, and even reverses sign by 27; similarly, the marginally significant positive effect of math on risk scores estimated at age 22 is small and insignificant by age 25, and actually reverses sign and becomes a marginally significant negative risk score effect by age 27.

Turning to financial literacy education, we see that the estimates largely fade with age. For example, a financial literacy requirement reduces the probability of having outstanding debt by 1.4 percentage points for 22 year olds, but the estimate is essentially zero for 27 year olds. We see similar fade out effects for bankruptcies, collections, the proportion of balance that is delinquent, housing debt prevalence and log balance, and auto and credit card prevalence. The clear exceptions to this pattern are nonhousing and student debt balances, where we observe growth. For example, the increment to student debt that follows financial education reform is from an insignificant increase of $161 at 22 to a significant $435 by age 27.

Age-specific estimates regarding economics education generally strengthen over time and corroborate the findings of the pooled sample. Table 4 shows that the effect of the economics requirement on individuals' risk scores, proportion of debt that is delinquent, and the likelihood of accounts in collection grow in magnitude with age. For example, the 9.2 point average decline in age 27 mean risk scores that results from requiring economics education is more than four times as large as the decline at age 22.

Overall, a mixed picture emerges regarding the impact of financial education mandates in early adulthood. This could be a result of genuinely heterogeneous impacts of these mandates over the lifecycle. Conversely, this could be driven by differences in the content of financial education across states—note that when analyzing the results by age, different treated states may be contributing to identification of the parameters of interests at different ages. Nevertheless, a broad pattern of early (protective) effects of required financial literacy training, which then fade with age, and of accumulating repayment difficulties between ages 22 to 27 in response to economics requirement reforms, is apparent.

3.1.3 Event study specification. Next, we move to the discussion of estimates of the event study (ES1) model. For our eleven debt-related outcomes, the various panels of Figures 1 and 2 depict estimates of the $\beta_j^n|_{j=-4}^4$, coefficients for financial literacy and economics education, respectively; we account for math years in this specification the same way as in the baseline (I1) model, and those estimates (which are not reported here) are qualitatively identical to the baseline estimates.[10] Each panel, besides reporting the baseline I1

[10] We also estimate a model that allows for an event study approach for math education. Instead of using the variation in the number of math years, we code a math reform as a dummy that equals one if the individual's high school state implements an increase in required years of high school math. The interpretation of the estimates is now different since the baseline model shows the impact of an additional year of math requirement (using the

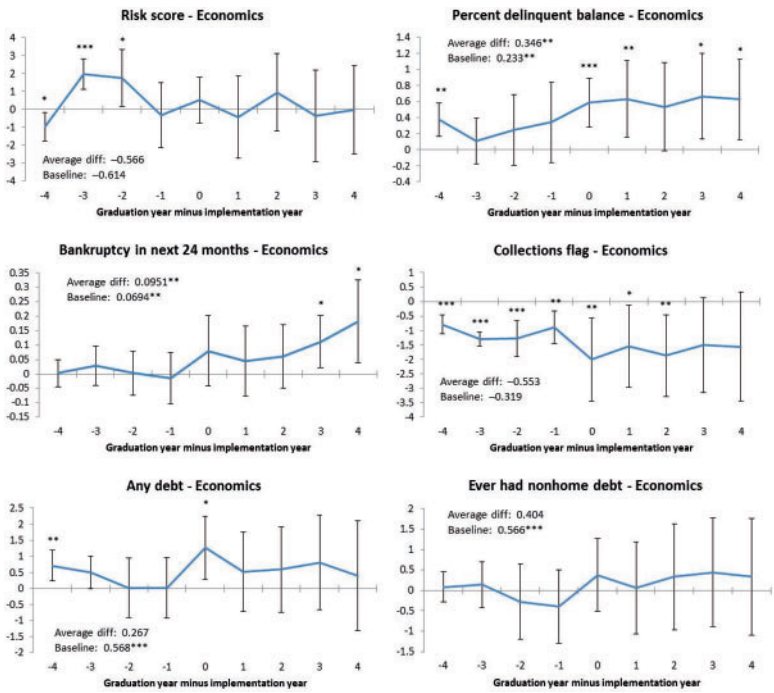

Figure 1
ES1 estimates (economics reforms)
Source: FRBNY Consumer Credit Panel/Equifax.

model estimate, also reports the "average difference," that is, the difference between the average post- and average pretreatment coefficients: $\frac{1}{4}\left(\sum_{y=1}^{4}\beta_j^n\right) - \frac{1}{4}\sum_{j=-4}^{-1}\beta_j^n$). As mentioned earlier, the excluded group is of cohorts that graduate more than four years prior to the reform (and hence includes all cohorts in the untreated states). An average difference that is statistically different from zero is evidence of a nonzero impact of the reform. It is worth noting that the baseline estimates implicitly place additional weight on earlier cohorts, because we have more observations of people graduating 1 year after the reform than we do of people graduating 3 or 4 years after a reform. Thus, the baseline model would find a stronger effect if the reform has an initial, but fading, impact and a weaker effect if the reform's influence grows. We denote significance for the estimated average difference, and for each of the eight β estimates, with asterisks in the figure.

continuous measure of years of math education), whereas the event study approach shows the impact of exposure to additional math requirement. Estimates for this specification, available from the authors upon request, are qualitatively similar to those for the baseline model.

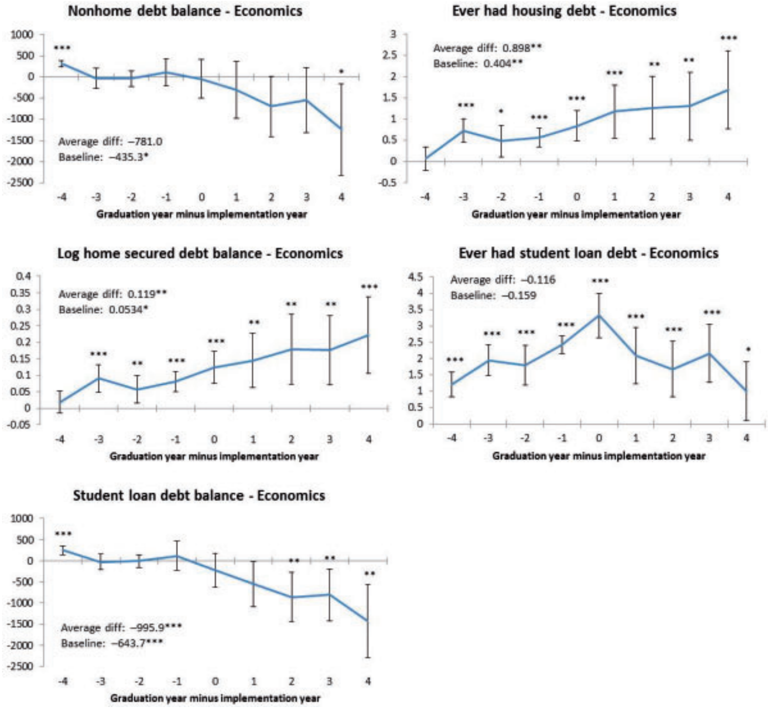

Figure 1
Continued.

The first thing of note in the various panels of the two figures is that estimates of the pretreatment coefficients ($\sum_{j=-4}^{-1}\beta_j^n$) are essentially flat (and jointly zero in most instances). This is reassuring since this lends credence to our parallel growth assumption for treatment and control states. We also see little evidence of a trend difference immediately before treatment occurs (i.e., for $j=-1$ or $j=-2$), suggesting that the enactment of the mandate is not correlated with unobserved factors.

Turning to economic education in Figure 1, even allowing for separate pre-trends, it is visually clear that the post-treatment estimates, ($\sum_{j=1}^{4}\beta_j^n$), are different from the pretreatment estimates for many outcomes. The "average difference" is qualitatively similar to the baseline estimate for nearly all the outcomes. All variables that were significant in the baseline specification, with the exception of having any outstanding debt (and, relatedly, any nonhome debt), continue to be significant. We also see that, in instances in which there are significant impacts, the effects are larger for cohorts that graduate in later years. For example, in the case of the likelihood of having a bankruptcy in the next twenty-four months, the estimates are an increase of 0.05, 0.06, 0.11, and 0.18 percentage points for cohorts that graduate one, two, three, and four or more years after the reform, respectively.

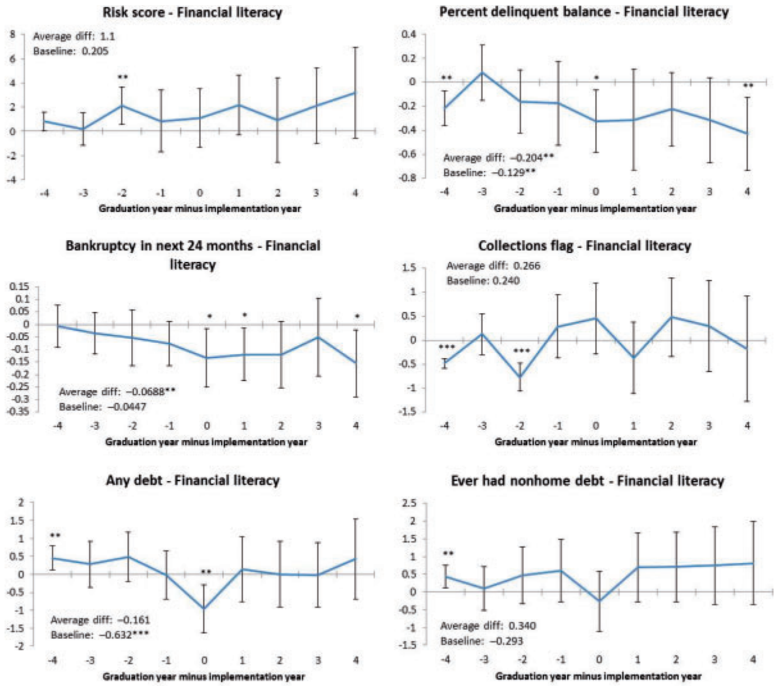

Figure 2
ES1 estimates (financial literacy reforms)
Source: FRBNY Consumer Credit Panel/Equifax.

Our primary findings in the baseline specification for exposure to financial literacy education are a modest decrease in delinquency, a clear decrease in debt prevalence, and a clear decrease in homeownership. The event study in Figure 2 shows some evidence of a significant decline in the housing debt outcomes, as well as a significant and steady decline in delinquency, each of which seems fairly consistent with the baseline estimates. However, the negative estimated effect of financial education reforms on overall debt prevalence in the baseline model is no longer significant. Other estimated effects of financial education requirements in the baseline model are small or very near zero, and insignificant. Corroborating the estimated zero effects, the event study series is flat in Figure 2 for nearly every outcome with no estimated financial education effect in the baseline estimates. The lone exception to this rule is the prevalence of student debt, where the event study depicts a small, but significant, decline in student debt holding, despite the small, insignificant positive point estimate we found for this outcome using our baseline specification.

Overall, our ES1 estimates are qualitatively similar to the baseline model estimates. Incorporation of heterogeneous treatment effects (by cohorts) indicates that the effects of economic education and of financial literacy are most often stable over time, and in some instances grow larger for later graduating

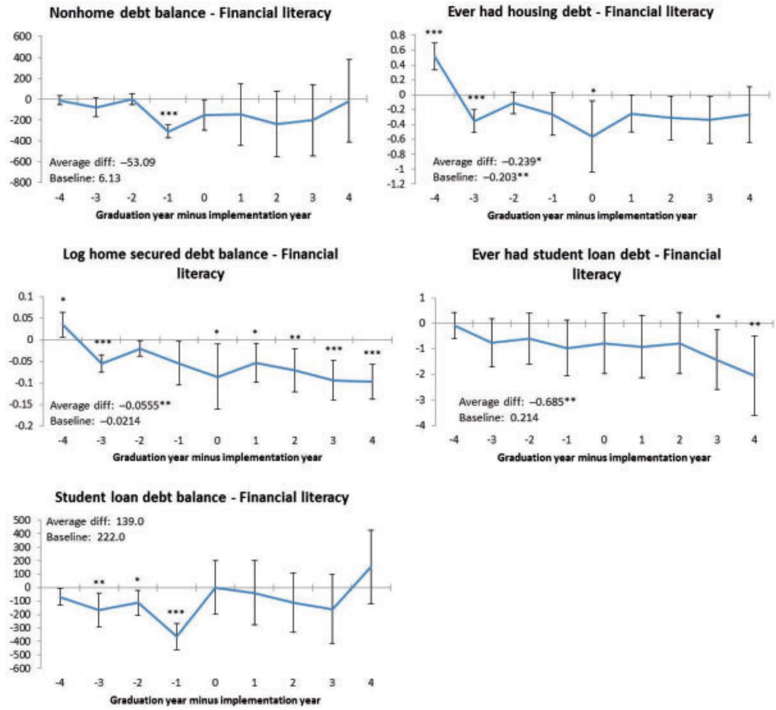

Figure 2
Continued.

cohorts. This pattern suggests either that states become better at teaching financial education over time or a lag between the passage of legislation and implementation of new curricula in some of the treated states.

3.1.4 Robustness checks. We conduct additional analyses to gauge the robustness of our findings. First, as described in Section 2, financial education may have an impact on the likelihood of youth having a credit report (i.e., the extensive margin). If that is the case, a concern is that the impact of financial education on debt outcomes conditional on having a report (i.e., the intensive margin results) may possibly be driven by compositional changes in the pool of borrowers. The Online Appendix shows a positive, but small, impact of financial education on the extensive margin, indicative of this not biasing our intensive margin results.

The Online Appendix also shows that the results hold once we correct the standard errors for multiple hypotheses testing, and shows results from a falsification test that bode well for our conclusions. In addition, we show that our baseline estimates are robust to accounting for the 2009 CARD Act, which would have impacted the youngest cohorts in our estimation sample, but not the older cohorts.

Finally, we also estimate a specification in which we do not force the impacts of economic and financial literacy education to be additive (as is the case in the models above), but instead estimate a joint effect. The results indicate that economics and financial literacy education do not interact and that our additive assumption is quite reasonable.

4. Discussion and Conclusions

The vast majority of young U.S. consumers bear consumer debt, and a rich landscape of education policy is aimed at improving the financial behavior of young Americans. Yet existing evidence regarding the effectiveness of financial training at improving the debt behavior of U.S. youth is, at best, mixed. In this paper, we investigated the impact of statewide mathematics, economics, and financial education reforms, affecting large populations of high school students, on students' debt outcomes in the decade immediately following high school. To our knowledge, ours is the first paper to analyze the relationship between financial education and debt outcomes in early adulthood for a representative sample of U.S. consumers and to investigate whether the relationship is causal.

Our results illustrate different roles for different types of quantitative education in shaping young consumers' debt experiences. Increased mathematics requirements, on the whole, appear to raise perceived creditworthiness, leave unchanged or decrease reliance on all debt categories, except for student loans, and decrease the likelihood of accounts in collections. Results from Goodman (2012) and Cole, Paulson, and Shastry (2015) on income and asset effects extend the picture of the effect of mathematics training on outcomes in adulthood: students exposed to more math training realize higher average incomes and savings. Though our analysis includes no model with which to infer welfare responses, higher income and asset levels, in combination with approximately unchanged debt and fewer repayment difficulties, suggest higher net consumption, both now and in the future. This is consistent with the positive effects of mathematics-related cognitive skills (or the negative effects of their absence) found in the prior literature.

Our findings for the debt effects of financial education requirements are qualitatively similar to our findings for mathematics education, in that they can be described broadly as improvements in repayment behavior and decreases in reliance on nonstudent debt. They at least appear to increase debt savvy, in that they increase the prevalence of credit reports without increasing consumers' reliance on debt. Lower delinquency rates, less debt (particularly auto and credit card debt, which typically fund consumption), and greater debt savvy are all outcomes we speculated might be generated by the states' financial education curricula in Section 2.1, presuming they were effective. It is worth noting that the effects of mathematics and financial literacy education requirements generally appear to dissipate with age (student debt being the main exception to this). This might partially explain why existing evidence on the efficacy of financial education has been mixed, since previous studies have largely focused on outcomes in middle age.

In marked contrast to the estimated impacts of mathematics and financial literacy education, we see that economic education leads to an increase in the likelihood of having outstanding debt and significant increases in both delinquency and bankruptcy. These findings, to some degree, substantiate our speculation in Section 2.1 regarding the potential effects of economics course content that may familiarize students with interest rates and financial products. Unlike mathematics and financial literacy education, the estimated effects of economics requirements are strongest at older ages. Both repayment difficulties and risk score effects seem to accumulate with age. Existing research indicates that an economic education is associated with higher income and assets (see Blinder and Krueger 2004; Allgood et al. 2011; Altonji, Blom, and Meghir 2012). Hence, the net welfare effect of economic training may be unclear. Further, increased reliance on debt is not unambiguously welfare decreasing (Karlan and Zinman 2010). While the estimated debt effects of economic education in this paper appear to have ambiguous welfare effects, they may in fact be symptomatic of changes that bring overall welfare enhancements. More economics students may experience both increased delinquency and increased asset returns, though the latter are not documented in these data. To the extent that higher debts are associated with steeper income profiles, they may also be an indication of improved welfare.

One noteworthy parallel to our estimated effects by course type are the findings of Drexler, Fischer, and Schoar (2014). Just as we find (modestly) more successful debt outcomes in response to financial literacy courses (whose stated content is practical), and less successful debt outcomes in response to economics courses (with generally more abstract content), Drexler, Fischer, and Schoar (2014) see (substantially) better outcomes in response to rule-of-thumb financial training when compared to principles-based accounting training. It may be the case that teaching simple rules for real-world choices is most effective in curing debt problems.

Limitations of the analysis in this paper, given the available data, include our inability to break down the training effects by demographic category, following the related literature on the heterogeneous effects by demographics of changes in schooling laws. In addition, for a given course category, the treatments implemented by states were certainly heterogeneous, both at and below the state level. Our estimates merely reflect an average effect of these varied interventions.[11] Brown et al. (2014) emphasize the heterogeneous details of implementation, and, accounting carefully for the realized implementation paths in Georgia, Idaho, and Texas, uncover financial literacy education effects

[11] One dimension of this heterogeneity is the quality of instruction. Lusardi and Mitchell (2014) include a helpful discussion of the quality of instruction in high school personal finance courses.

that are quite similar to what we observe at a national level. In addition, it is unclear (and difficult to measure) what uses of student time are being crowded out by each requirement and how different these may be from state to state. In that sense, our intent-to-treat effects should be interpreted as the net effect of the financial education and the classes that are being crowded out. Further, the results presented here give little evidence of the mechanisms by which math, economics, and financial literacy requirements exert their effects on young borrowers. Given the substantial and varied estimated effects of these three categories of quantitative training on early debt outcomes, research that refines our understanding of the relationship between training content and youth outcomes would be valuable to the design of policy. Finally, this study exploits schooling reforms as proxies for growth in quantitative skills, but includes no direct measures of quantitative skills or financial literacy. Progress in the measurement of financial literacy within consumer finance data is of great potential use to the field.

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