# Financial Education and the Debt Behavior of the Young

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## Abstract

More than three quarters of US households bear consumer debt, yet we have little understanding of the relationship between financial education and the debt behavior of US consumers.  In this paper, we study the effects of exposure to financial training on debt outcomes in early adulthood. Identification comes from variation in financial literacy, economics, and mathematics course offerings and graduation requirements mandated over the 1990s and 2000s by state-level high school curricula. The FRBNY Consumer Credit Panel provides debt outcomes based on quarterly Equifax credit reports from 1999 to 2012. Our analysis, based on a flexible event study approach, reveals significant effects of financial education on debt-related outcomes of youth. On the extensive margin, financial literacy education has a sizable impact on the propensity of youth having a credit report. Conditional on having a credit report, on the intensive margin, math and financial literacy education exposure reduces the incidence of adverse outcomes – such as accounts in collections and delinquent accounts – and reduces both the likelihood of youth carrying debt and their average debt balances. The net effect of both math and financial literacy education is an increase in the youths' average creditworthiness, as measured by the Equifax risk score. On the other hand, economic education increases the likelihood of individuals carrying balances, leads to significant increases in debt balances – in particular, debt used to support consumption – and, at the same time, increases the likelihood of adverse credit outcomes, leading to a decline in youths' average risk scores. The effects of these financial education policies accumulate over the course of early adulthood. Our results suggest that financial education programs, increasingly promoted by policy-makers, are likely to have significant impacts on the financial decision-making of youth, but the effects depend on the content of these programs.

Young adults in the US are heavily reliant on debt, and their level of financial literacy is low. Seventy-nine percent of 25-year olds in the FRBNY Consumer Credit Panel (CCP) in 2012 held consumer debt. The average debt balance among all 2012 CCP 25 year olds was $23,041.[1] Despite this extensive interaction with lending markets, a majority of high school and college students fail basic financial literacy tests (Markow and Bagnaschi, 2005; Shim et al., 2010).[2] The low financial literacy rates among US youth, along with the well-established correlation between financial literacy and financial well-being,[3]— has prompted policy-makers and the media to push for more financial education.[4] But, surprisingly, there is little evidence regarding the relationship between financial education and debt use, and particularly whether financial illiteracy is the cause of poor debt-related outcomes in early adulthood. Our analysis attempts to fill this gap in the literature.

For this purpose, we use variation in financial education – more specifically, finance, economics, and mathematics – graduation requirements mandated by state-level high school curricula over the 1990s and 2000s, in combination with detailed consumer liability data from the CCP. The CCP is a new panel on consumer debts based on credit reports from Equifax, one of three major national credit reporting agencies. The CCP comprises the credit reports of five percent of the population of U.S. individuals with credit reports, drawn on a quarterly basis from 1999 to 2012, ongoing.[5]

Our identification strategy exploits variation in the timing of enactment of financial education reforms in high school curricula across as well as within states. In 1999, ten states

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[1] Figures based on authors' calculations. All financial variables in the paper are reported in 2012 US dollars. Looking to another highly reliable source on debt, 78 percent of 2010 SCF households aged 35 and under reported consumer debt, and the median debt balance among these households was $39,600 (see Bricker, et al., 2012).

[2] The lack of financial literacy extends to the general US population: in the 2004 Health and Retirement Study, only about half of over-50 US individuals displayed basic comprehension of both interest and inflation (Lusardi and Mitchell, 2011). Similarly, only 21% percent of Americans were aware of the inverse relationship between interest rates and bond prices (Lusardi, 2011).

[3] 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). These mistakes are costly: households with low levels of financial literacy borrow at higher interest rates (Lusardi and Tufano, 2008; Stango and Zinman, 2009), are less likely to plan for retirement (Lusardi and Mitchell, 2007; Banks and Oldfield, 2007; Banks, O'Dea, and Oldfield, 2010), are less likely to have savings (Banks and Oldfield, 2007; Smith, McArdle, and Willis, 2010), are more likely to default on mortgage payments (Gerardi, Goette, and Meier, 2013), and are less likely to participate in financial markets ((Christelis, Jappelli, and Padula, 2010; van Rooij, Lusardi, and Alessie, 2007; Calvet, Campbell, and Sodini, 2007; 2009; Kimball and Shumway, 2007; Smith et al., 2010). However, most of these studies are correlational, and so are unable to shed light on whether financial illiteracy is the cause of poor financial decisions.

[4] 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).

[5] This dataset, and its representativeness at younger ages, is discussed below. Debt prevalence for 25 year olds is comparable to that of most other pre-retirement age groups in the CCP and SCF. For further evidence, and similar youth debt rates, see Brown, Haughwout, Lee, and van der Klaauw (2011) and Avery, Calem, and Canner (2003), and Bricker et al. (2012).

required high school enrollment in economics courses, a number which doubled to 20 by 2012. Similarly, only one out of 50 states required a financial literacy course for graduation in 1999; by 2012, this number had increased to 17. And, though every state had some math graduation requirement in place at the start of our time period, 19 states revised their standards upward by at least a full year between 1999 and 2012. Our baseline empirical strategy, which employs flexible state-time and cohort-time fixed effects, uses these staggered policy changes to identify the causal impact of financial education on debt-related outcomes of youth.[6]

The empirical analysis reveals that exposure to financial and quantitative education has different but sizable impacts on the debt-related outcomes of youth. While mathematics education has no impact on the extensive margin (that is, the likelihood of having a credit report), it has a meaningful impact on the intensive margin. Additional mathematics training leads to improved creditworthiness (as measured by the Equifax risk score, which is similar to the FICO score), decreases adverse outcomes (such as delinquencies and collections), and improves debt savvy among the youth, as indicated by a higher likelihood of exercising the bankruptcy option. It also leads to (economically and statistically) significant declines in average debt balances: an additional year of math, for example, decreases auto loan and credit card balances – debt that is generally used to support consumption – by $890 (or 11% of the standard deviation).

Financial literacy exposure appears to increase debt savvy of youth—in that it increases the prevalence of credit reports in this age group, suggestive of youth understanding the importance of building a credit report. As in the case of Mathematics education, along the intensive margin, financial literacy education leads to improvements in the average consumer's creditworthiness, lowers average debt balances (particularly, auto loan and credit card debt), and decreases adverse debt-related outcomes.

The impact of economic education is in marked contrast to the estimated impacts of mathematics and financial literacy education. While economic education has little impact on the prevalence of credit reports, it seems to dispel debt aversion amongst those with credit reports: exposure to economic education leads to significantly higher average debt balances – particularly auto loan and credit card debt, which are generally used to support consumption – and an increase in repayment problems. The significant decline in the average consumer's creditworthiness that results from economics education suggests that, while economics training demystifies borrowing and credit markets for the young, it does not improve their decision-making ability sufficiently to overcome these adverse outcomes.

We also explore the effects of these financial education reforms over the course of early adulthood (ages 22 to 28), and find that estimated impacts persist at all ages. While our data identify the effects less precisely at later ages, their magnitudes generally get stronger and accumulate over the individuals' ages. For example, financial literacy exposure leads to an average decline of $538, $1,087, and $1,437 in auto loan and credit card debt balances at ages

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[6] In particular, we do not assume common time trends across states, an assumption which has been shown to be problematic in the context of studies that use changes in compulsory schooling laws (Stephens and Yang, 2013).

22, 25, and 28, respectively. We also incorporate heterogeneous treatment effects (by high school graduation cohorts) in our analysis, and find that the effects of financial literacy and economic education are larger for individuals who graduate two or more years after the reforms are implemented, suggestive of a lag between the passage of legislation and effective implementation of new curricula.

Our conclusions are robust to allowing for a different average pre-trend for treatment states (states that implement a reform) and control states; this should not be surprising since our baseline specification already includes state-specific time trends, something that is possible given the size of our data set. Our results are also robust to correcting the standard errors for multiple hypotheses testing, and a falsification test implementing placebo reforms.

Our paper is related to the literature on financial education and financial decision-making (see footnote 3). This literature primarily emphasizes saving rates and investment income as targets of quantitative education.[7] The effect of financial training on retirement saving is of clear 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 percent of Survey of Consumer Finances (SCF) households with heads under 35 years of age in 2010 report holding financial assets, the conditional median value of these assets is just $5500.[8] The evidence suggests that debt, rather than asset accumulation, is the primary financial concern of early adulthood. Secondly, 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 Rooj et al. (2007), Jappelli and Padula (2011), and Cole, Paulson, and Shastry (2012). For causal inference, these studies rely either on ability and literacy measures that predate the relevant financial decisions, or, as we do, on state-level compulsory schooling or state-mandated courses.[9] For example, Bernheim et al. (2001) find that state financial education mandates in the 1970s and 80s increased both exposure to financial information and subsequent asset accumulation during adulthood. Cole et al. (2012), exploiting variation in compulsory schooling laws, find that education increases financial market participation, and decreases the likelihood of adverse debt-related outcomes. However, given the timing of compulsory schooling reforms, these outcomes are necessarily studied in a middle-aged sample.

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[7] See also Bayer, Bernheim, and Scholz (2009), Choi, Laibson, and Madrian (2011), Lusardi (2004), and Bernheim and Garrett (2003).

[8] Note that these financial assets include bank accounts. These figures can be compared to the 78 percent debt prevalence and $39,600 conditional median debt for the same SCF 2010 households with heads under 35, as mentioned in footnote 1.

[9] An alternate approach uses randomized access to financial education. Drexler et al. (2012) 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. Other randomized trials that reveal little effect of financial training include Gartner and Todd (2005), Servon and Kaestner (2008), and Choi et al. (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.

We are aware of two studies that investigate the causal effect of financial education on debt-related outcomes. Cole, Paulson, and Shastry (2013) use a similar identification approach to ours, and investigate the impact of state financial education mandates between 1957 and 1982 (the same ones as in Bernheim et al., 2001) and mathematics reforms between 1984-1994 on debt-related outcomes of middle-aged individuals.[10] 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. Similarly, in his investigation of the impact of a financial management education course for new soldiers in the US Army, Skimmyhorn (2013) finds moderately-sized effects on a few credit-related outcomes (such as credit card and consumer finance loan balances), but little impact on credit scores, adverse legal actions, and having active credit.[11]

Our conclusions regarding the impact of financial education stand in stark contrast to results from these two studies. What may potentially reconcile the weak effects of financial education uncovered by the prior literature 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). While most previous research considers the financial decisions of older consumers, we narrow the focus to decisions made shortly after high school financial training, and to debt—a comparatively pertinent outcome for US consumers, particularly the young. Alternatively, or 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.

Finally, our findings of sizable impacts, coupled with our result that impacts of high school financial education accumulate over the individuals' ages, may quell concerns raised by the prior literature regarding the legitimacy of funding financial education programs in the U.S.[12] 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- this involves no normative or efficiency claims regarding the impacts themselves. 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, an increase in bankruptcies due to exposure to quantitative training. The failure to exploit the bankruptcy option in certain states of the world

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[10] Note that debt outcomes are measured from 1999 to 2011 in the CCP, and therefore their mathematics reform effects are estimated largely for consumers in their thirties, and financial education effects are estimated for somewhat older consumers.

[11] The Skimmyhorn study is of special relevance to us, as it considers the effects of exogenous variation in financial education for a relatively young population. However, its sample may represent an atypical subset of US youth. Furthermore, the eight hour financial management course that is the focus of that study may not be comparable to the content of state-mandated high school economic and financial literacy courses.

[12] See, for example, Cole et al. (2013), and the debate as discussed in Hastings et al. (2013).

may itself be a source of inefficiency in a consumer's intertemporal decision-making.[13] Our goal is to identify the response of various debt behaviors to financial and quantitative training, whether desirable or undesirable.

This paper proceeds as follows. We describe the main sources of data in the next section. Section II outlines the empirical strategy, while the empirical analysis is reported in Section III. We conclude with a discussion of our results and the welfare implications of these reforms in Section IV.

## I. Data

We use panel data derived from several complementary sources. Our financial behavior outcome variables originate from the Federal Reserve Bank of New York Consumer Credit Panel dataset. The educational reform data come from two sources: the National Council for Economic Education's (NCEE) biennial Survey of the States and the Council of Chief State School Officers biennial report on key state education policies. Finally, we obtain zip code- and state-level controls from the Internal Revenue Service (IRS), the Bureau of Labor Statistics (BLS), and the U.S. Census (Census). All financial variables are reported in units of 2012 dollars.

*a. 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 NCEE – the only comprehensive and centralized source of recent economics and financial literacy education data – date back to 1998/1999. Table 1 reports a national summary of these reforms.

For economics and financial literacy, our policy data come from the National Council for Economic Education's (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. Likewise, for financial literacy education, the policy reform of interest is whether or not a state legislated that all high school students complete at least one financial literacy course before graduation. This definition yields meaningful variation over the course of our 1998 to 2012 time period. In 1999, only one out of fifty states (Illinois) required a financial literacy course for high school graduation; by 2012, the number of states with such a requirement had increased to seventeen. Similarly, there were ten states that required high school enrollment in economics courses in 1999, a number which doubled to twenty by 2012.[14]

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[13] See, for example, Fay, Hurst, and White (2002).

[14] 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.

Our mathematics education data come from a biennial survey conducted by the Council of Chief State School Officers (CCSSO). The report, Key State Education Policies on PK-12 Education, contains state-level data on school attendance policies, graduation requirements, content standards, and other critical metrics. By 1998, all fifty states 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. Nineteen states introduced at least one one-year increase in math education during our sample period. Furthermore, as shown in Table 1, eight of these states enacted second one-year increases in years of math education.

The theoretical motivation for using these proxies is twofold. First, these policy reforms are causally correlated with our treatment variables of interest: exposure to subject-level education in economics and financial literacy, and years of mathematics education (Bernheim et al., 2001; Cole, Paulson and Shastry, 2012, 2013). The metric of a required course represents a true increase in exposure to education in the given subject better than, for example, a state-wide requirement that high schools offer a course in the given subject or a requirement that students in certain grades receive subject-related testing (NCEE Survey of the States). Second, early research (Mayer 1989) indicates that consumer education reforms are primarily precipitated by the action of specific lobbyists and legislators rather than large-scale pressure from public opinion, suggesting these reforms influence subject-level exposure in a way that is not driven by potentially endogenous trends in public opinion. Moreover, there are no significant socioeconomic or educational differences between states that implement consumer education policies and those that do not (Ford, 1977).[15] In our empirical analysis, however, we estimate flexible model specifications that allow for state-specific trends, as well as for the possibility of differences in trends between states that enact policies and those that do not.

*b. Consumer credit behavior*

The FRBNY Consumer Credit Panel (CCP) is a new longitudinal dataset on consumer liabilities and repayment. It is built from quarterly consumer credit report data provided by Equifax, one of three major national credit reporting agencies. Data are collected quarterly since 1999Q1, and the panel is ongoing. Sample members have Social Security numbers ending in one of five arbitrarily selected pairs of digits (for example, 10, 30, 50, 70, or 90), which are assigned randomly within the set of Social Security number holders. Therefore the sample comprises 5 percent of U.S. individuals with credit reports (and Social Security numbers).The CCP sample design automatically refreshes the panel by including all new reports with Social Security numbers ending in the above-mentioned digit pairs. Therefore the panel remains representative for any given quarter, and includes both representative attrition, as the deceased and emigrants leave the sample, as well as representative entry of new consumers, as young borrowers and

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[15] Note that many states passed consumer education reforms predating Ford (1977), as described by Bernheim et al. (2001).

immigrants enter the sample.[16]

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

While the sample is representative only of those individuals with credit reports, the coverage of credit reports is fairly complete in the U.S. Aggregates extrapolated from the data match those based on the American Community Survey, Flow of Funds Accounts of the United States and SCF well.[17] 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 1984. These cohorts will enter high school in or after 1998, 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 Klaauw (2010) extrapolate similar populations of U.S. residents aged 18 and over using the CCP and the American Community Survey (ACS), suggesting that the vast majority of US individuals at younger ages have credit reports.[18]

To accommodate the annual nature of our other variables, we use only fourth quarter Equifax data from the years 1999 through 2012. 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 2012 with 3.09 million total observations,[19] and data from 613,178 distinct individuals.[20] On average, the panel contains 308,602 observations per year, though as a result of our age constraint the data are heavily concentrated in the years after 2004.

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 it predicts the same 24 month default risk. It varies between 280 and 840 and represents an assessment of the individual's credit-worthiness. We also study each individual's number of delinquent accounts and total delinquent debt balance, where delinquency is defined as any past due debt payment, and an indicator for having had a balance in collections in the past 12 months.

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[16] See Lee and van der Klaauw (2010) for details on the sample design.

[17] See Lee and van der Klaauw (2010) and Brown, Haughwout, Lee, and van der Klaauw (2011) for details.

[18] Jacob and Schneider (2006) find that 10 percent of U.S. adults had no credit reports in 2006, and Brown, Haughwout, Lee, and van der Klaauw (2011) estimate that 8.33 percent of the (representative) Survey of Consumer Finances (SCF) households in 2007 include no member with a credit report. They also find a proportion of household heads under age 35 of 21.7 percent in the 2007 SCF, 20.64 in the 2007Q3 CCP, and 20.70 from Census 2007 projections, suggesting good representation of younger households in the CCP. Their comparison does suggest a modest under-representation of retirement age households in the CCP.

[19] The initial 2% sample consists of 3,311,743 observations. We are forced to drop 225,720 observations. More specifically, 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 such cases. Furthermore, data on the number of Math, Science, or English years required for graduation are missing for some zip codes, since those are determined by local school boards (and we do not have those data).

[20] For example, for an individual born in 1984 (and who appears in the credit Bureau data for each year), we would have 14 observations, one for each year over the period 1999-2012.

The size of our sample allows us to estimate reliable models of rare events, and we take as an additional outcome of interest whether the individual has experienced a bankruptcy over the past 24 months. In addition to these repayment measures, we look at debt balances, distinguishing between housing debt (mortgage or home equity debt), non-housing debt (credit cards and auto loans), and student loans. Finally, we consider whether the individual has any outstanding debt, as a measure of exposure to credit markets.

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, we use the state of residence of the individual when they first appear in the panel as a proxy for the state in which the individual attended high school.[21] Among those who appear in the panel at age 18, Table 2 shows the percentage of individuals living in the same state as the state in which they graduated from high school: 90.5% of the 22 year olds were residing in the same state in which they were living at age 18; this proportion remains high even among the oldest individuals in our sample. The low cross-state movement among the young suggests that the attenuation of the impact of state-level education policy reforms should be minimal.

*c. State-level educational 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, subject course requirements, and state educational spending. Our state educational spending data are drawn from the U.S. Census Bureau's historical archives on state and local government finances. Since variation in education spending across states may be confounded by differences in school-going population across states, we instead use per capita state education spending. For this purpose, we use the Census's Intercensal estimates of the state-level school-going (ages 5-24) population. For the last 2 years of the panel where the population count series is missing, we use linear extrapolation.

The data on compulsory schooling and other course requirements is from the CCSSO Key State Education Policies on PK-12 Education. 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 Key State Education Policies on PK-12 Education report. We control for requirements in place when the individual was in high school in the subjects of Natural Science, English, and Social Studies by including a continuous variable representing the number of years required by each state for

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[21] Cole et al. (2013) 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.

graduation from high school (at the time when the individual was in high school).[22] Over our time period, English and science requirements vary between one and four years, while social studies and math requirements vary between zero and four years. All three variables display an increase with time.

We also use state-level data on the population of young individuals in each year. These intercensal estimates of the resident population for each state are drawn from the U.S. Census Bureau, which reports counts of 20-24 and 25-29 year olds for each year.

*d. Zip code-level economic controls*

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 2011. We apply a within-zip code quadratic regression to extrapolate to 2012.

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 of returns. We use a linear regression to interpolate income values for each year with missing data (data are missing for 1999, 2003, and 2009 onwards), yielding an annual, zip code-level panel. Table 3 displays summary statistics for our outcome and control variables. It provides some helpful information regarding the empirical variation that identifies our central parameters of interest. Forty-nine percent of our sample was exposed to an economic education reform, 17 percent to a financial education reform, and 33 percent to a mathematics reform. Further, 13 percent of the sample did not experience an economics reform but resided in a state that would eventually enact an economics reform, identifying pre-reform trends. The analogous rates for financial education and mathematics reforms are 20 and 21 percent, respectively.

## II. Empirical Strategy

*a. Motivation*

We first briefly summarize 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 they may have on students' credit-related outcomes.

*a.1. Financial Literacy Education*

Though each state with mandatory high school financial literacy education maintains slightly different curriculum standards, there are overwhelming similarities in content across state lines, partly due to a centralized national effort to implement these educational reforms (U.S. Department of the Treasury, 2013; Jumpstart Coalition, 2013). In particular, five central

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[22] The required number of years captures the full variation in the required number of courses as well, for no state requires multiple courses in the same year (NCEE Survey of the States). Since there is no additional variation from incorporating the number of courses, we use the number of required years.

themes appear consistently in state financial literacy curricula: decision-making, career planning, personal budgeting, borrowing, and investing.[23]

The first two ask students to consider the relationship between finances and personal financial goals, and to analyze how career choices impact income, and, as a result, financial constraints. The third theme, personal budgeting, involves methods of accounting for personal income and expenditures. In this unit, students employ systems for recording income and spending, learn about different payment methods like cash or bank cards, and analyze consumer decisions in the context of maintaining a balanced budget (Indiana Department of Education, 2009; Maryland State Board of Education, 2010; Utah State Office of Education, 2013; Oklahoma State Department of Education, 2013). Furthermore, students are instructed on the definition of bankruptcy and ways to improve their credit scores after adverse financial events (Maryland State Board of Education, 2010; Oklahoma State Department of Education, 2013).

The fourth topic area – borrowing – requires students to "evaluate how to use debt beneficially, …evaluate the advantages and disadvantages of credit products and services, …analyze sources of credit,…use numeracy skills to calculate the cost of borrowing, … and analyze credit scores and reports" (Maryland State Board of Education, 2010; Oklahoma State Department of Education, 2013). Finally, the last major topic area within state financial literacy introduces students to saving and investment strategies, relevant quantitative concepts like compound interest and inflation, and frameworks for assessing risk (Indiana Department of Education, 2009).

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, fewer delinquencies, and lower debt, particularly debt that is used to support consumption, such as credit card and auto debt.

*a.2. Economic Education*

High school economics curricula in nearly all U.S. states require that students understand basic concepts like scarcity, allocation, maximization subject to a constraint, opportunity cost, marginal benefit, marginal cost, incentives, trade, comparative advantage, markets, the business cycle, prices, money, interest rates, income, exchange rates, investment, national accounts, unemployment, and monetary policy (The State Education Department of New York, 2002; The New Hampshire Department of Education, 2006; The California State Board of Education, 1998; Texas Education Agency, 2010). Frequently, these concepts are introduced with historical or cultural context: the discussion of national accounts often incorporates a history of the U.S.

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[23] See: Personal Financial Responsibility Instruction: Guidelines for Implementation. Indiana Department of Education. http://www.doe.in.gov/sites/default/files/career-education/stbrdguidelinespersfinrespapproved.pdf ;
The Maryland State Curriculum for Personal Financial Literacy Education. Maryland State Board of Education: http://mdk12.org/instruction/curriculum/financial_literacy/financialLiteracy_STANDARDS.pdf ;
Personal Financial Literacy. Oklahoma State Department of Education: http://ok.gov/sde/personal-financial-literacy;
Instructional Materials Evaluation Criteria – General Financial Literacy. Utah State Office of Education: http://www.schools.utah.gov/CURR/imc/Rubrics-CTE/General-Financial-Literacy.aspx.

federal budget and a lesson on monetary policy will typically include a brief history of the Federal Reserve System (The State Department of New York, 2002; Texas Education Agency, 2010). Likewise, lessons on trade, exchange rates, and comparative advantage are often complemented by a discussion of international trade and globalization (The New Hampshire Department of Education, 2006; The State Education Department of New York, 2002). Finally, and perhaps most relevant in our context, lessons on markets cover topics of supply, demand, prices, and interest rates.

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 comfortable with debt 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.

*a.3. Math Education*

Greater exposure to math education in high school has been shown to lead to improvements in knowledge and cognitive skills, through enhancements in skills such as clarity in expressions, logical reasoning and inference, as well as imagination and ingenuity (Alexander and Pallas, 1984). Since lower cognitive skills are linked with worse financial decision-making – for example, Agarwal and Mazumder (2013) find that lower math skills result in costly financial mistakes, such as mis-reporting of housing values on loan applications, while Stango and Zinman (2009) find that individuals with lower cognitive skills borrow more, and do so at higher interest rates – this would suggest that additional math should lead to better credit-related outcomes. In fact, using changes in number of math courses required for high school students during 1984 and 1994, Cole et al. (2012) find that additional high school math education increases the propensity of middle-aged individuals to accumulate assets, while reducing the probability of being delinquent on credit card debt and the probability of declaring bankruptcy or experiencing foreclosure.

There is also a large literature on the impact of math education on labor market earnings and educational attainment, which finds either positive or no effects.[24]

Based on all this evidence, the effect of math exposure on individuals' likelihood of having a credit report is unclear. However, conditional on having a credit report, we expect greater math exposure to lead to more favorable debt-related outcomes, such as improved credit scores, a lower likelihood of delinquencies, and a lower likelihood of debt use. The impact on debt balances is, however, ambiguous since greater math also leads to higher incomes.

b. Empirical Analysis

Our empirical analysis proceeds in two stages. We first investigate whether financial education

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[24] Altonji (1995) finds negligible effects of math coursework on wages (or educational outcomes), while Goodman (2009) finds positive effects of additional math education for low-skilled students only. On the other hand, Rose and Betts (2004) and Joensen and Nielsen (2009) find large positive effects of exposure to additional math education.

had an impact on the extensive margin, that is, the likelihood of youth entering credit markets. We next investigate whether there is an impact on the intensive margin, specifically whether, conditional on having a credit report (and, hence, participating in credit markets), financial education impacts debt-related outcomes of youth.

### b.1. Impact on the Extensive Margin

To investigate whether financial education impacts the propensity of youth to enter credit markets, we exploit the staggered policy changes in economic, financial, and mathematics education across states. Specifically, we estimate:

$$R_{st} = \alpha_t + \gamma_s + \beta^X X_{st} + \sum_n (\beta_{post}^n I_{st}^n) + \varepsilon_{st}, \qquad (E1)$$

where the dependent variable, $R_{st}$, is the proportion of 20-28 year olds in state $s$ in year $t$ who have a credit report. The policy interventions are indexed by $n$, where $n \in \{$mathematics, econonmics, financial literacy$\}$. $I_{st}^n$ is an indicator that equals 1 if state $s$ implements a policy change in subject $n$ prior to year $t$, and equals zero otherwise. For the few states that enact changes in math years twice (see Table 1), we use the year of the first policy change. $\gamma_s$ is a set of state fixed effects, $\alpha_t$ is a set of year fixed effects, and $\varepsilon_{st}$ is an idiosyncratic error. $X_{st}$ is a vector of time-varying state-level controls: unemployment rate; gross state product; per capita state educational spending; subject requirements for graduation; and years of compulsory schooling. The state fixed effects control for time-invariant differences across states, while the year fixed effects control for aggregate time trends in the prevalence of credit reports among 20-28 year olds. State-level time-varying controls allow us to account for changes in the macroeconomic conditions of the states that may correlate with the enactment of the policy changes

The coefficients of interest are the $\beta_{post}^n$'s. To address heteroscedasticity, we cluster standard errors at the state level.

E1 assumes that credit prevalence in states exposed to a reform (treatment group) and those who are not exposed to the reform (control) would trend similarly in the absence of the reforms. While this counterfactual is not inherently testable, the panel data allow us to test whether states that implement policy changes were trending similarly in the years prior to the adoption of the reform to those that did not implement a policy change. Therefore, as a robustness check, we estimate the following specification which allows for the possibility of a different average pre-reform trend in states that enacted a policy change, relative to those that did not:

$$R_{st} = \alpha_t + \gamma_s + \beta^X X_{st} + \sum_n (\beta_{pre}^n P_{st}^n + \beta_{post}^n I_{st}^n) + \varepsilon_{st}. \qquad (E2)$$

This specification has an additional term compared to E1: $P_{st}^n$, which equals 1 if state $s$ implements a policy change in subject $n$ in or after year $t$, and is zero otherwise. This variable allows us to test whether treated and control states had similar average pre-trends. A suggestive

test of the common trend assumption is that the pre-treatment coefficient $\beta_{pre}^n$ is zero. When presenting the results, we instead show estimates of $(\beta_{post}^n - \beta_{pre}^n)$; an estimate statistically different from zero would show a break of the trend in credit prevalence amongst youth after the enactment of the policy, and would be evidence of a causal effect of the policy.

*b.2. Impact on the Intensive Margin*

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, $Y_{i(sc)zt}$, is the CCP debt-related outcome of individual *i*, of birth cohort *c* in high school-attendance state *s*, and is residing in zip code *z* in year *t*. Our baseline specification is as follows:

$$Y_{i(sc)zt} = \gamma_{st} + \delta_{ct} + \beta^X X_{zt} + \sum_n (\beta_{post}^n D_{i(sc)}^n) + \beta_{post}^{math} M_{i(sc)} + \varepsilon_{i(sc)zt}, \quad (11)$$

where $D_{i(sc)}^n$ is an indicator for whether *i* was exposed to education in field *n*, where $n \in \{economics, financial\ literacy\}$, in state *s*. It equals 1 if *i*'s cohort *c* graduates from high school *after* her state enacts the legislation requiring students to complete at least one economics course before graduation, and is zero otherwise. We take 18 as the high school graduation age. So $D_{i(sc)}^n$ equals 1 if *i*'s cohort *c* turns 18 in a year *after* her state enacts the legislation, and equals zero if *i*'s cohort *c* turns 18 *in or before* the year that 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*).[25] $\gamma_{st}$ is a vector of state-year fixed effects, and $\delta_{ct}$ is a vector of birth cohort-year fixed effects; the staggered implementation of the reforms across states and over time allows us to identify both state-time and cohort-time fixed effects. $\varepsilon_{i(sc)zt}$ is an idiosyncratic error. $X_{zt}$ 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; gross state product; per capita state educational spending; state-level subject requirements for graduation; and state-level compulsory years of schooling.

The state-year fixed effects account for state-specific and aggregate time trends in the outcomes (for example, an increase in credit card usage), and control for differences across states that may be related to the enactment of the reform in a state. Differential trends in the outcomes across different birth cohorts are accounted for by the cohort-year fixed effects. 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 the policy changes.

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[25] Since our specification includes state fixed effects, the variation in mandatory years of math education comes from state legislative changes.

The coefficients of interest are: $\beta_{post}^{econ}$, $\beta_{post}^{finlit}$, and $\beta_{post}^{math}$. Since the error terms may be correlated among those with the same high school-attendance state and year, we cluster the standard errors at the state-year level. Our I1 specification, which we will also call our baseline specification, is very flexible and does not assume common trends across states,[26] which has been shown to be problematic in the context of studies that use exogenous changes in state compulsory schooling laws (see Stephens and Yang, 2013). Our next two specifications build on this baseline specification, and use a slightly more flexible, event-study approach.

*b.2.1. Event Study Model I (ES1):*

$$Y_{i(sc)zt} = \gamma_{st} + \delta_{ct} + \beta^X X_{zt} + \sum_n (\beta_{pre}^n P_{i(sc)}^n + \beta_{post}^n D_{i(sc)}^n) + \beta_{post}^{math} M_{i(sc)}$$
$$+ \varepsilon_{i(sc)zt}, \tag{ES1}$$

This specification builds on the baseline model by adding a pre-treatment indicator for economics and financial literacy reforms, $P_{i(sc)}^n$. This variable equals 1 if individual *i*'s cohort *c* graduates from high school (that is, turns 18) *in or before* the year that her state (of high school attendance) *s* enacts the policy reform, and is zero otherwise. While our baseline specification already includes differential time trends by state, inclusion of this variable allows us to investigate whether states that enact a policy have an average pre-trend that is different from those of states that never enact a policy change (that is, whether $\beta_{pre}^n$ is zero). When presenting the results, we instead show estimates of $(\beta_{post}^n - \beta_{pre}^n)$; an estimate statistically different from zero would be evidence of a causal impact of the policy change.

The advantage of this specification is the ability to account for any average pre-existing temporal trends in financial behavior (between states that enact a reform in our sample period and those that do not); the difference between the pre- and post-treatment coefficients can be interpreted as the true net effect of an educational policy reform. To compute this difference and its statistical significance, we apply a Wald test to each set of pre- and post- treatment coefficients. Henceforth, we refer to this event-study specification as the ES1 model.

*b.2.2. Event study model II (ES2):*

The $\beta_{post}^n$ estimate in the baseline and ES I models is simply the average treatment effect across all years after the reform. A limitation of this approach is that a new reform may take a few years before affecting debt outcomes, and the effects may not be homogenous across years. We cannot test for time-varying effects of the reform in the specifications above. Furthermore,

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[26] That is, we do not assume that states that institute changes in their financial education curriculum experience trends similar to those that do not institute such policies.

$P^n_{i(sc)}$ allows for the possibility of a different average pre-trend (pooled across all years before the reform) for the treatment and control states. However, trends may diverge only in years right before a state introduces a reform. To allow for these possibilities, we estimate the following specification:

$$Y_{izt} = \gamma_{st} + \delta_{ct} + \beta^X X_{zt} + \sum_n \left( \sum_{j=-4}^{4} \beta_j^n D^n_{j,i(sc)} \right) + \beta_{post}^{math} M_{i(sc)} + \varepsilon_{i(sc)zt}. \quad \text{(ES2)}$$

$D^n_{j,i(sc)}$ is an indicator that equals 1 if $i$ of cohort $c$ graduates from high school in state $s$ (that is, turns 18) $j$ years after the state implements a policy change in subject $n$. For example, $D^{econ}_{-2,i(sc)}$ is a dummy that equals 1 if student $i$ graduates from high school 2 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 implementation. The bins represent the following graduation timings: four or more 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 implementation. If states that enact the reforms have an average pre-trend similar to the control states (those that do not introduce a reform), the pre-treatment coefficients ($\sum_{j=-4}^{0} \beta_j^n$) should be zero. Evidence of a treatment effect requires that ($\sum_{j=1}^{4} \beta_j^n$) are jointly different from ($\sum_{j=-4}^{0} \beta_j^n$). We provide a visual presentation of the 9 estimates (for several outcomes of interest) in the empirical results specification. Conversely, to interpret these numerous coefficients, we also compute a Wald test on the difference between the average of the pre-trends and the average of the post-trends, and report the estimates.

This specification (ES2) is our most flexible one. It allows pre-reform trends (for policy changes in financial literacy and economic education) to differ across states as well as the impact of reforms to change over time. This flexibility allows us to discern plausible situations in which, for example, schools become better at teaching financial education over time and the impact of the reforms grows larger for later cohorts.

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 intensive margin models (I1 and ES1) on outcomes for the individual at ages 22, 25, and 28. This allows us to investigate the effects of these reforms at particular points 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 ($\gamma_s + \delta_t$), and continue to cluster the standard errors at the state-year level.

## III. Results

### a. Impact on the Extensive Margin

We first discuss the impact of financial education on the likelihood of youth entering credit

markets, that is, on the prevalence of credit reports amongst the 20-28 year olds. Estimates of equations (E1) and (E2) are presented in columns 1 and 2 of Table 4, respectively. The first column reports estimates of $\beta_{post}^n$ for $n \in \{$mathematics, econonmics, financial literacy$\}$, while the second column reports estimates of $(\beta_{post}^n - \beta_{pre}^n)$. The estimates for math and economics are small in magnitude, and not statistically different from zero. On the other hand, exposure to financial literacy education leads to an increase in credit report prevalence amongst the treated youth. The coefficient, which is precisely estimated, implies an increase of 1.2 percentage points in the proportion of 20-28 year olds with a credit report. Based on our calculations, in 2012, 84.7% of 20-29 year olds in the US had a credit report. Therefore, the impact of financial literacy education is non-trivial. It is also notable that the E2 estimates, based on the model which allows for different average pre-trends for treated and control states, are very similar to the E1 estimates. This suggests that the common trends assumption may be accurate in this context.

### b. *Impact on the Intensive Margin*

We next present estimates of the impact of financial education exposure on debt-related outcomes.

### *b1. Impact on the Pooled Sample*

Estimates of equation (I1) are presented in Table 5. Looking across the first row, we see that exposure to additional mandatory math years has a significant effect on all our outcomes of interest. It leads to a small but statistically precise increase of 1.8 points, on average, in individuals' risk scores (given a sample standard deviation of 91 points). An additional year of math leads to a decrease in both the number of delinquent accounts and the balance in delinquent accounts; the estimates imply sizable effects- for example, an additional year of math education decreases delinquent balances by $285 (given a sample mean balance of $975). We see that additional math education increases the likelihood of the individual experiencing bankruptcy in the past 24 months, and decreases the likelihood of having accounts in collections. Column (6) shows that, conditional on having a credit report and hence being a participant in credit markets, an additional year of math decreases the likelihood of the individual having any outstanding debt by 5 percentage points (on a base of 76 percent). The last three columns show that math exposure leads to a decline in balances across all debt categories. Again, the impacts are sizable: an additional year of math education decreases auto and credit card debt – debt that is generally used to support consumption, as opposed to student loans and housing debt which generally help individuals accumulate human capital or assets – by $890, that is, by 11 percent of the standard deviation of auto and credit card balances.

Exposure to mandatory economic education leads to impacts that are considerably different from the impacts of math education. It leads to an average decline of 4 points in the individual's risk score, an increase in the balance and number of delinquent accounts, and an increase in in the likelihood of having an account in collections. Exposure to economic education

decreases the likelihood of individuals experiencing bankruptcy. However, economic education increases the likelihood of the individual carrying any outstanding debts by 0.02 points. Regarding the impact on debt balances, we see that economic education leads to a significant and substantial increase of $554 in credit card and auto debt.

Moving to the impact of mandatory financial literary education, we see that the impacts are quite similar to those of math education. The main difference being that, in most instances, coefficients are smaller in magnitude, and less precise. For example, financial literacy exposure has little effect (both statistically and economically) on the likelihood of individual experiencing bankruptcies, on the likelihood of possessing any outstanding debts, and on the number of delinquent accounts. Like math education, financial literacy education leads to an improvement of about 2 points, on average, in the individual's risk score, and to a lower likelihood of accounts being in collection. Columns (7)-(9) also show that, with the exception of student loan balances, financial literacy education leads to lower average debt balances; the effect sizes are non-trivial. For example, financial literacy exposure decreases credit card and auto loan balances by $580, or about 7 percent of the sample credit card and auto loan standard deviation.

The estimates in Table 5 are based on the pooled data, where a given individual may appear multiple times (depending on when they enter the panel). Since the individual random effect may affect the variance of the dependent variable, we also estimate a version of equation (I1), which includes an individual random effect. Estimates of this specification are presented in Table A1. The estimates are very similar in magnitude to the corresponding estimates in Table 5.

*b.2. Disaggregated Sample*

To explore how the effects of these financial education reforms evolve over the course of early adulthood, Table 6 next presents estimates of the I1 specification, estimated for 22, 25, and 28 year olds, separately. This age-specific specification, as mentioned above, includes state and time fixed effects.

First, looking at the impact of math education, we see that the positive effect of math education on risk score persists and actually grows with age; the impact is in fact quite large at later ages—an additional year of mandatory math increases the individual's risk score at age 28, on average, by 7 points (an effect size that is about 7% of the standard deviation in the sample risk score). Similarly, the estimated effects of mathematics training on adverse credit events – delinquent accounts, collections incidence, and delinquent balances – grow increasingly negative with age. The impact of math education on the likelihood of the individual having any outstanding debt is negative and fairly stable for the three ages. The negative impact of math education on the different kinds of debt balances also accumulates over the individuals' ages (in terms of magnitudes but also effect sizes). For example, the estimated decline in home-secured debt is $455, or about 6% of the standard deviation of housing debt, at age 22, and grows to $5,633, or about 8% of a standard deviation, by age 28. Lastly, we see that the estimated positive impact of additional math education on bankruptcies in the pooled sample is driven by the 22-year-olds.

Age-specific estimates regarding economics education are also generally consistent with those of the pooled sample. Table 6 shows that, as we move from 22 to 28 year olds, the effects of economics education on individuals' risk scores and numbers of delinquent accounts grow in magnitude. For example, the 10.4 point average decline in age 28 risk scores that results from economics education is nearly twice as large as the decline at ages 22 and 25. While economics education increases the likelihood of individuals holding debt at all ages, the effect is no longer precise at conventional levels of significance by age 28. Similarly, those who are exposed to economics education have (statistically and economically) significantly higher auto loan and credit card debts at ages 22 and 25, but the impact is no longer precise by age 28.

Turning to financial literacy education, again we see that the age-specific estimates largely corroborate our findings for the pooled sample. Exposure to financial literacy education leads to significant average declines in non-student loan balances at each of the ages. Moreover, the impact sizes get larger over time. For example, financial literacy exposure leads to a $538 decline (or a 3 percent standard deviation decline) in auto loan and credit card debt at age 22, but the impact size increases to a $1,087 decline at age 25, and a $1,437 decline (or a 13 percent standard deviation decline) at age 28. While we see that financial literacy leads to a 3.8 point risk score improvement at age 22, the estimated effect is no longer precise at ages 25 and 28. Financial education decreases the likelihood of having accounts in collection by a growing amount as sample members age, first by 1, then 2, and then 4 percentage points, though the latter estimate is not quite significant at the ten percent level.[27] Financial literacy education leads to a significant positive increase of 0.3 percentage points (on a sample mean of 1.1 percent) in bankruptcies of 28-year-olds, an effect that is not present either at other ages or in the pooled sample.

Overall, these results are generally consistent with the estimates of the pooled sample. The effects of math education on debt-related outcomes persist at all ages, with the positive effects accumulating over the individuals' ages. Age-specific estimates of financial literacy exposure show that, while in some instances the impact sizes strengthen with age (such as average non-student loan balances and collections incidence), the positive effect on other outcomes (such as average risk scores) is precise only at younger ages. Economic education seems to have similar age persistence as financial literacy, with the impact on some outcomes (such as risk scores and delinquency) accumulating over the individuals' ages, while the impact on other outcomes (such as the likelihood of outstanding debt and debt balances) remains reasonably consistent (but loses precision) with age.

*b.3. Event Study Specification*

We next discuss estimates of our event study specifications.

Estimates of the ES1 specification (which allows for a different average pre-trend among states that implement policy changes) for the pooled sample are shown in Table 7. The table

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[27] Given our need to observe sample members' locations in early adulthood, and the limited duration of our panel, our 28 year old sample is necessarily smaller than our 25 and 22 year old samples, and as a result has less power.

reports estimates of the $(\beta_{post}^n - \beta_{pre}^n)$ comparison for financial literacy and economics, and estimates of the $\beta_{post}^n$ term for math years. An estimate that is statistically different from zero is evidence of a causal impact of the reform.

The results for math education in the ES1 model, unsurprisingly, are identical to those for the I1 model (presented in Table 5). Additional mandatory math education increases average risk scores; lowers the likelihood of adverse credit events (delinquencies); and decreases credit market participation, with reductions in both the likelihood of holding any outstanding debts as well as average balance holdings.

Our ES1 results regarding exposure to economics education reforms are qualitatively similar to the baseline model estimates. One exception is the set of coefficients on student loan and housing-related debt balances, which are negative in the ES1 specification but positive in the baseline model. However, none of the estimates are statistically significant at the 95 percent level of confidence. The impact of economic education on adverse outcomes – delinquent accounts and balances – is qualitatively similar but no longer statistically significant. Overall, allowing for pre-reform trends to differ between states that do and do not enact an economics education reform change strengthens our conclusion that economics decreases risk scores, increases the likelihood of individuals holding debt, and also increases average auto and credit card debt balances.

Estimates for financial education in the ES1 model indicate that now the only statistically significant effects of the reform are the lower likelihood of having any debt, and lower non-student debt balances. Estimates on average balance reductions, in columns (7)-(9), are similar to the corresponding estimates in Table 5.

We also estimate the ES1 model for each of the three ages, 22, 25, and 28. Estimates are presented in Table A2, and are qualitatively similar to the I1 age-specific model estimates (reported in Table 6). As in the pooled ES1 model estimates (Table 7), the statistically significant impact of financial education is restricted to the likelihood of any debt and average debt balance outcomes.

We next move to the discussion of estimates of the ES2 model. For our nine debt-related outcomes, the various panels of Figures 1 and 2 visually show estimates of the $(\sum_{j=-4}^{4} \beta_j^n)$ coefficients for economics and financial literacy education, respectively; we account for math years in this specification the same way as in the baseline (I1) model, and those estimates (not reported here) are identical to the baseline estimates. As can be seen in the various panels of the two figures, estimates of the pre-treatment coefficients $(\sum_{j=-4}^{0} \beta_j^n)$ are not jointly zero in many instances, which indicates that the treatment states (states that implement the reform) and control states had different pre-treatment trends.

In the case of economics education (Figure 1), even allowing for separate pre-trends, it is however visually clear that the post-treatment estimates, $(\sum_{j=1}^{4} \beta_j^n)$, are different from the pre-treatment estimates for all those outcomes that were statistically significant in the baseline model (except for the number of delinquent accounts); this is indicative of a non-zero treatment effect. Each panel also reports the average difference between the post- and pre- treatment coefficients,

that is, $\frac{1}{4}$ $(\sum_{y=1}^{4} \beta_j^n) - \frac{1}{5}\sum_{j=-4}^{0} \beta_j^n)$. The average treatment effect in this specification for outcomes that were statistically significant in the baseline – risk score, delinquent balances, bankruptcy, collections, likelihood of any debt, and auto loans and credit card debt – are qualitatively similar to those in the I1 model. As was the case in the ES1 model, the coefficients on home-secured debt and student loan balances now reverse signs, but are not precisely estimate. The various panels of Figure 1 show that the post-treatment coefficients get larger in magnitude over time—the greatest effects on outcomes among those exposed to economics education are primarily driven by individuals who graduate two or more years after the reform is implemented. . This pattern suggests a lag between the passage of legislation and effective implementation of new curricula. For example, in the case of the impact of economic education on risk scores, the estimates are -3 (statistically significant at 5%), -2.4 (not precise at conventional levels), -7 (significant at 1%), and -6 (significant at 1%) for cohorts that graduate one, two, three, and four or more years after the reform, respectively.

Moving to the effects of financial literacy education in Figure 2, the average difference between the post- and pre- treatment coefficients for the various outcomes are qualitatively similar to those in the baseline model. However, as was the case in the E1 specification, we lose precision on some of the statistically significant outcomes in the baseline (risk score and accounts in collections), and continue to have sizable declines in non-student loan balances. Regarding the heterogeneity in treatment effects over time, a mixed picture emerges. The size of the impact on certain outcomes – such as risk scores, total delinquent balances, and student loan balances – is larger for later cohorts, while the magnitude of the impact on non-student loan balances, while still sizable, is smaller for cohorts that graduate two or more years after the reform is implemented.

Overall, our ES2 estimates are consistent with the ES1results, which were qualitatively similar to the baseline model estimates. While it is the case that average pre-treatment trends are different for treatment and control states, accounting for these trends, has little impact on our baseline estimates. This should not be surprising since our baseline model already includes state-year fixed effects. The added value of the event study specifications is that they show how the average pre-trends differ for the two sets of states. Additionally, incorporation of the heterogeneous treatment effects (by cohorts) in ES2 indicates that the effects of economic education (and of financial literacy for some outcomes) become larger over time. This would be consistent with states becoming better at teaching financial education over time, and the impact of the reforms growing larger for later cohorts as a result.

## c. Robustness Checks

In this section, we provide additional evidence on the robustness of our findings.

### c1. Multiple Testing Corrections
Our empirical analysis employs nine dependent variables, and hence testing for the impact of a reform on outcomes involves the simultaneous testing of several hypotheses. In the

analysis so far, we have not taken the multiplicity of tests into account. This can be problematic because the probability that some of the true null hypotheses are rejected by chance alone can be quite large in such cases.[28] For example, if 10 hypotheses are being tested at the same time, one expects one true hypothesis to be rejected at the 10% level. Further, if all tests are mutually independent, then the probability that at least one true null hypothesis will be rejected at the 10% level is $1 - 0.9^{10} = 0.65$.

Being mindful of the potential for false positives, we next employ multiple testing corrections to our p-values and adjust them downward, in an effort to minimize false findings. The first column of Table 8 reports the p-value of each significant coefficient in our baseline I1 model for the pooled sample (Table 5). The next three columns show three corrected p-values, each representing a different method of enforcing a family-wise false discovery rate.

The corrections that we apply are fairly standard in the literature of multiple hypotheses testing (see Romano, Shaikh, and Wolf, 2010). The first method, the Bonferroni correction, is the most conservative, and is computed simply by multiplying the standard p-value by the number of ex ante null hypotheses (*N=9* in our case). The Bonferroni correction makes the very conservative assumption that the null hypotheses are uncorrelated. However, since many aspects of consumer credit behavior are intimately linked or driven by the same underlying forces, we believe the Bonferroni correction is more strict than necessary. The next correction, the Bonferroni-Holm (Bonferroni step-down), is slightly less conservative. It is implemented by ranking the baseline coefficients from most to least significant. The first p-value is then multiplied by the number of ex-ante null hypotheses (again, *N=9* in our case), just as it would be in the Bonferroni correction. Subsequently, the $n^{th}$ ranked coefficient's p-value is multiplied by the remaining number of null hypotheses, *N-(n-1)*. Hence, the second-most significant p-value in our regressions is multiplied by (9 - (2 - 1)) = 8, the third-most significant p-value is multiplied by 7, and so on. The third multiple testing correction is the Benjamini-Hochberg False Discovery Rate, and it is the least stringent of our three p-value corrections. We believe it offers the best balance between capturing significant effects and avoiding false positives. It is implemented by ranking all the coefficients by p-value from smallest p-value to largest. The largest p-value remains unchanged. The second-largest p-value is multiplied by the number of ex ante null hypotheses (N=9) divided by its rank (N-1, that is, 8). The third-largest p-value is multiplied by the number of ex ante null hypotheses (10) divided by its rank (8), and so on.

Looking across the last column in Table 8 we see that, when using the Benjamini-Hochberg correction, almost all of our estimates from the baseline specification that were found to be statistically different from zero, continue to be so. In fact, even when using the more stringent corrections in columns (2) and (3), with the exception of 3 outcomes out of 23 baseline significant outcomes (likelihood of collections for economics and financial literacy education; delinquent balance for financial literacy education; student loan balance for financial literacy education), all estimates continue to be statistically different from zero at conventional levels of significance.

We conduct the same standard error corrections for our age-specific baseline model estimates (Table 6). Of the 45 outcomes that are statistically significant in the baseline model, 42 continue to be statistically significant at the 15% level or higher, when we apply the Benjamini-

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[28] Tests of the relationship between financial education and, for example, bankruptcy and number of accounts in collection are clearly not independent. The case of ten independent tests provides an upper bound on the odds of falsely rejecting a true hypothesis.

Hochberg correction (results available from the authors upon request). Hence our conclusions are robust to various multiple hypothesis testing corrections.


*c2. Falsification Tests*

As a final investigation of whether our results reflect the true impact of educational reforms, we perform a falsification analysis by defining artificial implementation years and running our baseline model on those modified data.

Our falsification methodology is as follows. First, we use only data from the subset of states which apply a policy implementation in economics, financial literacy, or mathematics after 2006. Then, we code a counterfactual policy implementation year for economics, financial literacy, and mathematics education by moving each state's implementation year five years earlier than its actual implementation year. For example, Michigan first required high school economics education in 2007. In our falsification test, we code Michigan's economics implementation year as 2002. In order to create a truly counterfactual dataset, we also drop all observations from years after 2006. This refinement of the data ensures that the only members of the falsification post-treatment group are individuals who, in reality, were not exposed to an educational reform. This measure prevents dilution of our falsification coefficients by accurately coded individuals. We estimate model I1 on this placebo reform sample; estimates are presented in Table 10. If the pattern of consumer credit behavior elucidated in our results is truly the result of the education reforms, repeating our baseline analysis on the panel with fictitious timing should yield coefficients that are either zero, or significantly different from our baseline estimates. Looking at the Table 10 estimates for economics and financial literacy education, we see that the only estimates that are statistically significant have a sign that is opposite to that of the actual baseline estimates in Table 5. For mathematics years, estimates for the coefficients on number of delinquent outcomes, collections, likelihood of holding any debt, auto loans and credit card balances, and student loan balances are statistically significant and of the same sign as the corresponding baseline estimate (Table 5). However, with the exception of the coefficient on collections and the number of delinquent accounts in this placebo test, these estimates are statistically different and smaller than the baseline estimates (at conventional levels of significance). Thus, this falsification gives us greater degree of confidence that our baseline results capture the true effect of educational reforms rather than other (state or time specific) confounding effects.


**IV. Discussion and conclusions**

More than three quarters of U.S. households bear consumer debt (Brown et al., 2011). But we have little understanding of the factors that influence the debt behavior of US consumers. In this paper, we investigate whether one particular factor – financial education – causally impacts the financial behavior of the young. To our knowledge, ours is the first paper to analyze the relationship between financial education and debt outcomes in early adulthood, and to investigate whether the relationship is causal.

For this purpose, we use variation in finance, economics, and mathematics course offerings and graduation requirements mandated by state-level high school curricula: the

requirements across as well as within the states vary substantially over the 1990s and 2000s. This state-level curriculum variation, in conjunction with the Equifax-sourced FRBNY Consumer Credit Panel, allows us to study the effects of exposure to financial, economic, and general quantitative training on early-life debt, delinquency, and default.

Our results illustrate different roles for different types of quantitative education in shaping young consumers' debt experiences. Mathematics education, on the whole, appears to raise perceived creditworthiness, decrease debt use, and decrease delinquency in its various forms.[29] Results from Goodman (2009) and Cole et al. (2013) 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.[30] Though our analysis includes no model with which to infer welfare responses, higher income and asset levels, in combination with lower debt, suggest higher net consumption both now and in the future. This, in turn, suggests an increase in welfare with math training. All of this is consistent with the positive effects of mathematics-related cognitive skills (or the negative effects of their absence) demonstrated by Alexander and Pallas, Agarwal and Mazumder, and Stango and Zinman, which we reviewed in section II.a.3.

Of course, it is not clear that the content of math training is optimal. In particular, the observed decrease in debt that funds assets, for example mortgage and student debt, when combined with higher income levels, may suggest an increase in debt aversion resulting from math training. It is possible that increased debt aversion could damage efficiency, as young consumers miss fairly-priced opportunities to smooth consumption or make debt-funded investments.

The significant, (arguably) positive effects of mathematics training on debt outcomes that we find align fairly well with the conclusions of Cole et al. (2013). They study the effects of earlier (1984-94) mathematics reforms on a sample of largely 30-something consumers, and find that increased mathematics requirements are associated with significantly less delinquency, bankruptcy, and foreclosure during the years visible in the CCP. The primary discrepancy between the two studies is that, while we find significant risk score increases a few years after high school, Cole et al. (2013) find no significant effects of math requirements on risk score by the time borrowers are in their thirties. These differences may either be informative regarding the timing of the effects of mathematics training, or may be driven by differences in mathematics training and course content between the 1980s and 2000s.

Our findings for the debt effects of financial education are reasonably similar to our findings for mathematics education, in that they can be described broadly as improvements in repayment behavior and decreases in reliance on debt. We find that financial education requirements increase risk scores, decrease collections, and decrease auto, credit card, and housing debt balances. They at least appear to increase debt savvy, in that they increase the prevalence of

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[29] An exception is bankruptcy, which is significantly more common for those exposed to more mathematics education among our youngest borrowers.

[30] Note that Goodman (2009) found significant income growth for black men, but weaker income effects for black women.

credit reports and, among 28 year olds, they increase the rate at which consumers exploit the bankruptcy option, all without increasing consumers' reliance on debt. Greater creditworthiness, less delinquency, 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 II.a.1, presuming they were effective.

The estimated effects of economics requirements on debt outcomes differ substantially from those of mathematics and financial education. These findings, to some degree, substantiate our speculation in section II.a.2 regarding the ability of economics training to demystify borrowing and dispel some amount of debt aversion. Further, our results indicate that economic training's effect of warming young consumers to credit markets increases their repayment problems significantly. This effect is not overcome by any improved economic understanding imparted by the courses.

Other research indicates that economic education is associated with higher income and assets, and even a decreased level of risk aversion.[31] Hence the net welfare effect of economic training may be unclear. While the estimated debt effects of economic education in this paper appear to be welfare-decreasing, they may in fact be symptomatic of changes that bring overall welfare enhancements. More risk-loving 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.

This leads somewhat naturally to a discussion of the distributional impacts of our three categories of quantitative training. Marginal economics, mathematics, and financial literacy requirements may have different impacts on students at different ends of the distribution of quantitative ability. For example, the added math year may bind for lower quantitative ability students and yet have no effect on higher quantitative ability students. Financial education may be useful for students with rudimentary ex-ante financial literacy, and yet may displace higher-return courses for students with extensive ex-ante financial literacy. Economic concepts may be disrupting for students in some socioeconomic circumstances, and yet valuable for students in others.

Though the CCP offers limited demographic information at the individual level on which to base distributional studies of the impact of quantitative high school requirements, we can look at the impact of the three types of training on the 10th and 90th percentiles of the risk score distribution. We estimated quantile regressions at the 10th and 90th percentiles of risk scores in a specification otherwise identical to the version of specification (I1); the results are available from the authors. We find that the effects of economic education are negative at both high and low risk scores, but most negative at the 90th percentile. Therefore economic education both worsens perceived creditworthiness and narrows its dispersion. Conversely, financial literacy training increases risk scores across the board, but increases them most at the high end of the distribution,

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[31] See Blinder and Kruger (2004), Van der Klaauw et al. (2010), and Altonji, Blom, and Meghir (2012) on the positive association between economics education and income.

expanding the dispersion of risk scores. Counter to our expectations, mathematics requirements increase risk scores significantly at the top of the distribution, and yet decrease them at the bottom of the distribution, substantially increasing the dispersion of risk scores among young borrowers. On the whole, mathematics, economics, and financial education requirements have meaningfully different impacts at different points in the risk score distribution, some of which are of surprising direction and magnitude, and warrant further research.

Shortcomings of the analysis in this paper include our inability, given available data, to break down training effects by demographic category, following related literature on the heterogeneous effects by demographics of changes in schooling laws.[32] 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 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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[32] See, for example, Cole et al. (2012, 2013), Goodman (2009), and Stephens and Yang (2013).

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# Figure 1: ES2 estimates (Economics Reforms)
(Source: FRBNY Consumer Credit Panel/Equifax)



**Risk score - Economics**

Avg difference: -3.297***

**Number of Delinquent Accounts - Economics**

Avg difference:   0.0013
Baseline: 0.0159***

**Total Delinquent Balance - Economics**

Avg difference:  84.89*
Baseline:  163.9***

**Bankruptcy within the past 24 months - Economics**

Avg difference:  -0.0056***
Baseline:  -0.0025***

**Collections flag - Economics**

Avg difference:  0.0158***
Baseline:  0.0119**

**Any Debt - Economics**

Avg difference:   0.0150*
Baseline:     0.0215***
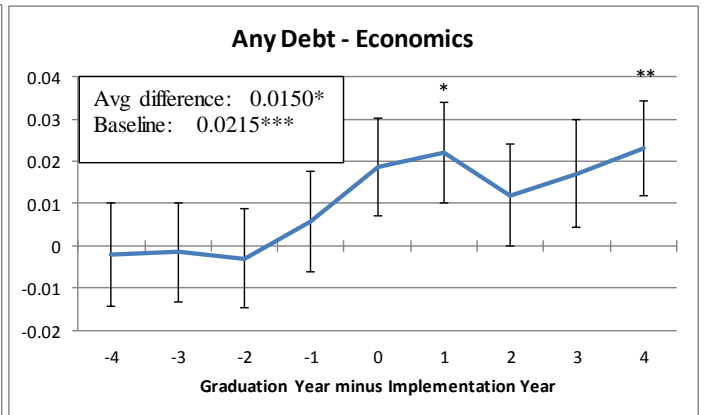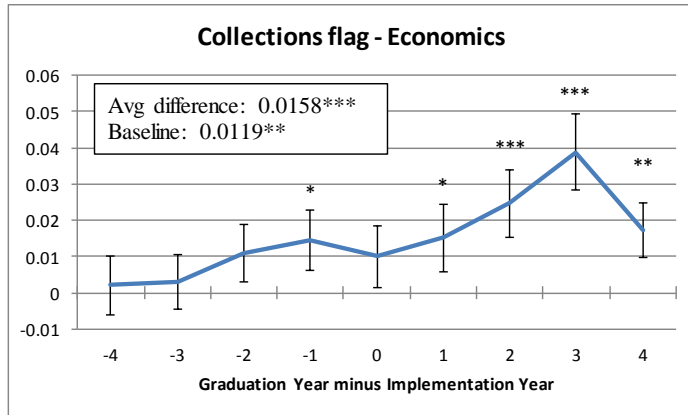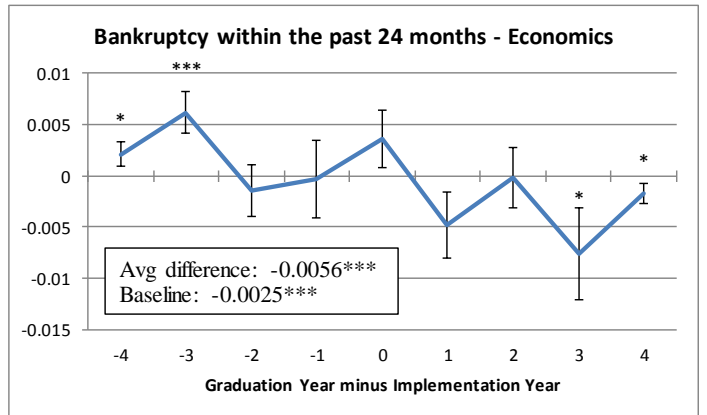
**Figure 1: ES2 estimates (Economics Reforms) - continued**
(Source: FRBNY Consumer Credit Panel/Equifax)



**Auto Loans & Credit Card Balance - Economics**

Avg difference: 301.6**
Baseline: 554.1***

X-axis: Graduation Year minus Implementation Year



**Home-secured Balance - Economics**

Avg difference: -409.5
Baseline: 233.7

X-axis: Graduation Year minus Implementation Year



**Student Loan Balance - Economics**

Avg difference: -235.1
Baseline: 42.70

X-axis: Graduation Year minus Implementation Year
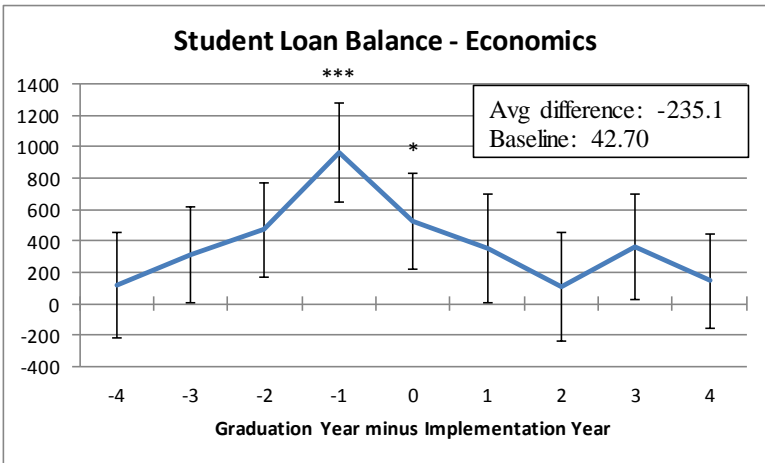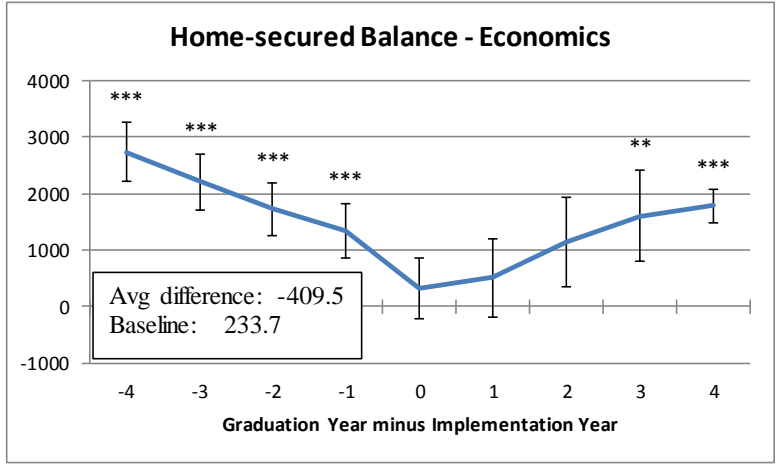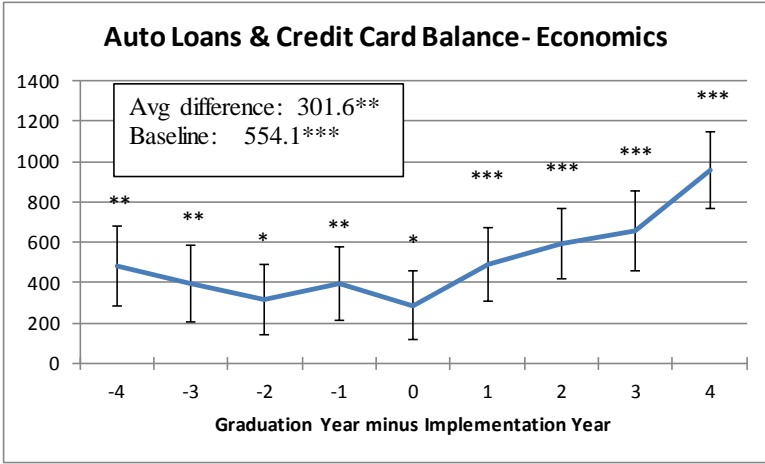
**Figure 2: ES2 estimates (Financial Literacy Reforms)**
(Source: FRBNY Consumer Credit Panel/Equifax)



**Risk score - Financial Literacy**

Avg difference: 1.254
Baseline: 1.983***

Graduation Year minus Implementation Year

**Number of Delinquent Accounts - Financial Literacy**

Avg difference: -0.0022
Baseline: -0.0039

Graduation Year minus Implementation Year

**Total Delinquent Balance - Financial Literacy**

Avg difference: -104.8**
Baseline: -90.72**

Graduation Year minus Implementation Year

**Bankruptcy within the past 24 months - Financial Literacy**

Avg difference: -0.0002
Baseline: -0.0006

Graduation Year minus Implementation Year

**Collections flag - Financial Literacy**

Average difference: -0.0027
Baseline: -0.0105**

Graduation Year minus Implementation Year

**Any Debt - Financial Literacy**

Avg difference: -0.0172*
Baseline: -0.0120

Graduation Year minus Implementation Year

31

## Figure 2: ES2 estimates (Financial Literacy Reforms) - continued
(Source: FRBNY Consumer Credit Panel/Equifax)



Auto Loans & Credit Card Balance- Financial Literacy

Avg difference: -550.2***
Baseline: -579.2***

Home-secured Balance - Financial Literacy

Avg difference: -2595.9***
Baseline: -2407.5***

Student Loan Balance - Financial Literacy

Avg difference: 227.9
Baseline: 451.7*
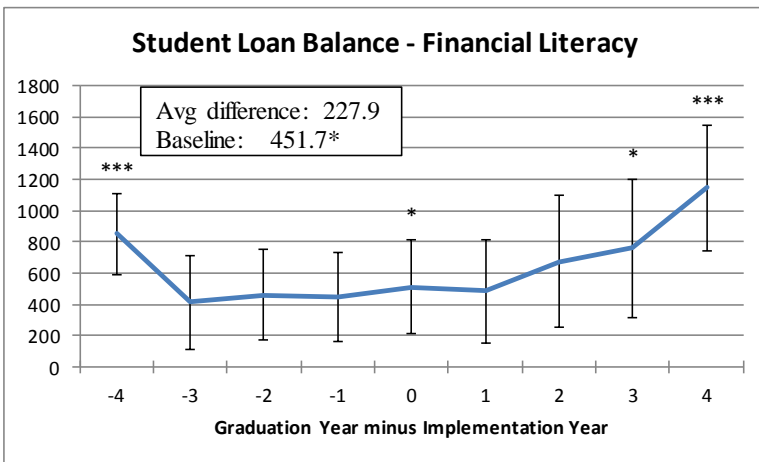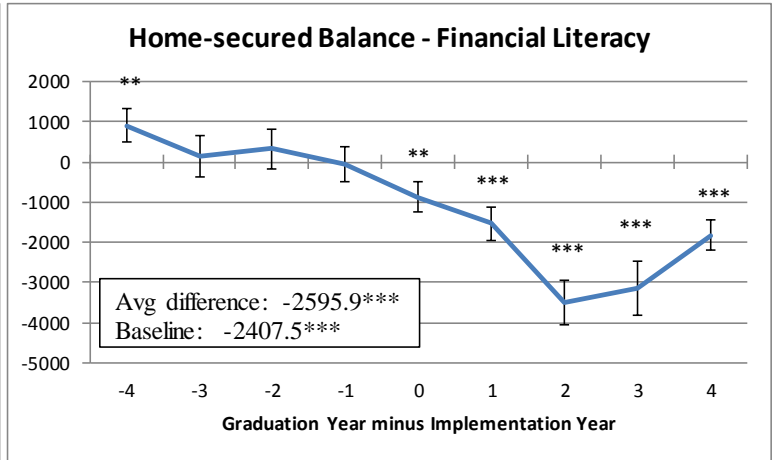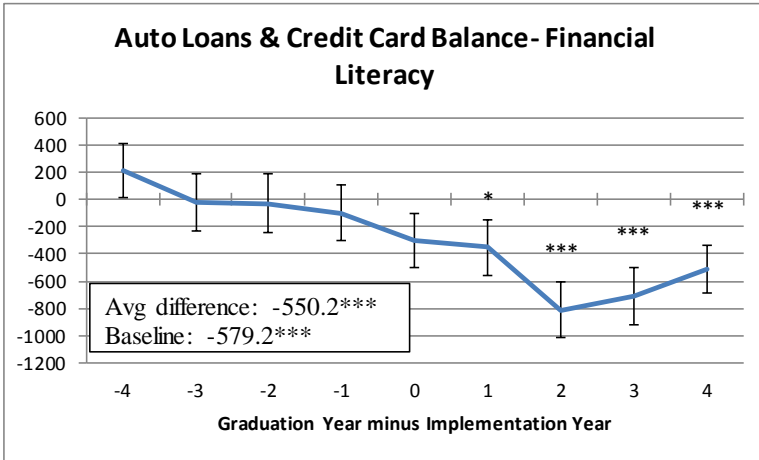
**Table 1: Education policy reforms by state**

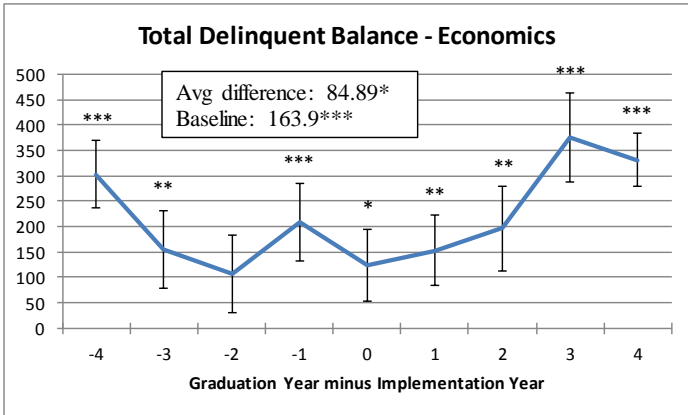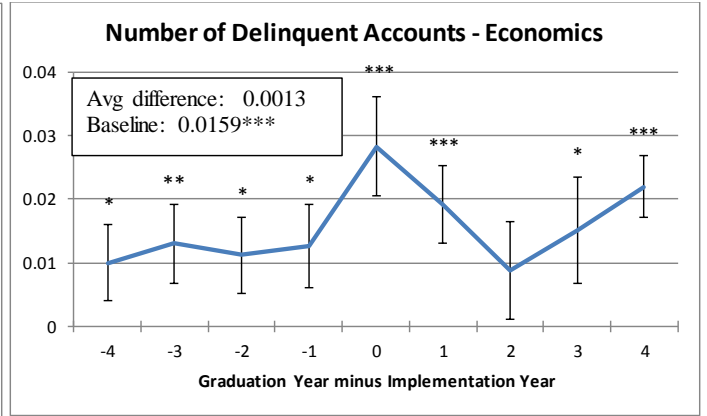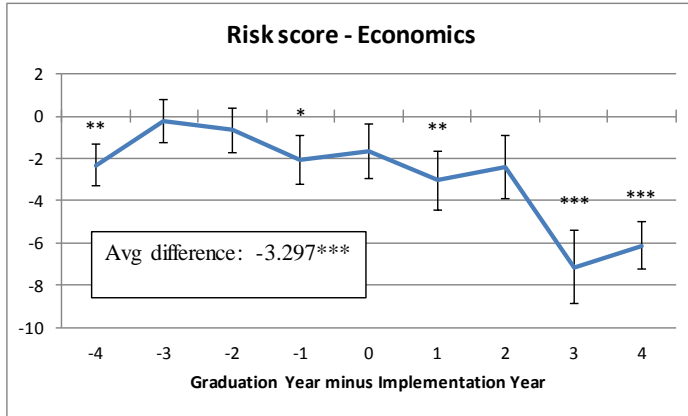| State | Year | Financial literacy mandate* | Year | Economics mandate* | Year | Mathematics reform** |
|---|---|---|---|---|---|---|
| Alabama | 2000 | Yes | <1998 | Yes | | |
| Alaska | | | | | | |
| Arizona | | | 2009 | Yes | 2008 | Yes |
| Arkansas | | | 2009 | Yes | 2004 | Yes |
| California | | | <1998 | Yes | | |
| Colorado | | | | | | |
| Connecticut | | | | | <1998 | Yes |
| Delaware | | | | | | |
| District of Columbia | | | | | | |
| Florida | | | <1998 | Yes | | |
| Georgia | 2005 | Yes | <1998 | Yes | 2004 | Twice (2008) |
| Hawaii | | | | | | |
| Idaho | 2000 | Yes | <1998 | Yes | 2006 | Yes |
| Illinois | <1998 | Yes | | | <1998 | Twice (2006) |
| Indiana | | | 2007 | Yes | 2006 | Yes |
| Iowa | | | | | | |
| Kansas | | | | | 2006 | Yes |
| Kentucky | 2002 | Yes | 2002 | Yes | <1998 | Yes |
| Louisiana | 2007 | Yes | <1998 | Yes | | |
| Maine | | | | | | |
| Maryland | 2009 | Yes | | | | |
| Massachusetts | | | | | | |
| Michigan | | | 2007 | Yes | | |
| Minnesota | | | | | | |
| Mississippi | | | | | <1998 | Yes |
| Missouri | 2007 | Yes | 2007 | Yes | | |
| Montana | | | | | | |
| Nebraska | | | | | | |
| Nevada | | | | | 2000 | Yes |
| New Hampshire | | | <1998 | Yes | 2006 | Yes |
| New Jersey | 2009 | Yes | 2009 | Yes | <1998 | Yes |
| New Mexico | | | 2000 | Yes | <1998 | Twice (2008) |
| New York | 2000 | Yes | | | 2006 | Yes |
| North Carolina | 2011 | Yes | <1998 | Yes | <1998 | Twice (2006) |
| North Dakota | | | | | | |
| Ohio | | | | | <1998 | Twice (2002) |

| | | | | | | |
|---|---|---|---|---|---|---|
| Oklahoma | 2009 | Yes | | | 2002 | Yes |
| Oregon | | | | | | |
| Pennsylvania | | | | | | |
| Rhode Island | | | | | 2006 | Twice (2008) |
| South Carolina | | | | | <1998 | Twice (2000) |
| South Dakota | 2007 | Yes | 2002 | Yes | 2006 | Yes |
| Tennessee | 2009 | Yes | <1998 | Yes | | |
| Texas | | | <1998 | Yes | | |
| Utah | 2005 | Yes | | | | |
| Vermont | | | | | | |
| Virginia | 2009 | Yes | 2009 | Yes | <1998 | Yes |
| Washington | | | | | 2008 | Yes |
| West Virginia | 2011 | Yes | | | <1998 | Twice (2006) |
| Wisconsin | | | | | | |
| Wyoming | | | | | | |

* from the National Council on Economic Education

** 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

## Table 2: Mobility of young credit report holders

| Age | % of individuals living in the same state as at age 18 |
|---|---|
| 22 | 90.52% |
| 23 | 87.52% |
| 24 | 85.04% |
| 25 | 82.87% |
| 26 | 81.32% |
| 27 | 80.39% |
| 28 | 78.31% |

Source: FRBNY Consumer Credit Panel/ Equifax.

**Table 3: Summary statistics for the estimation sample**

| Variable | N | Mean | SD | Min | Median | Max | Zeros |
|---|---|---|---|---|---|---|---|
| **Outcome Variables** | | | | | | | |
| Risk Score | 3,080,106 | 628.59 | 91.62 | 280 | 646 | 840 | 0.00% |
| Number of Delinquent Accounts | 3,311,743 | 0.166 | 0.666 | 0 | 0 | 25 | 90.22% |
| Balance in Delinquent Accounts | 3,311,743 | $975.87 | $11,184.99 | $0.00 | $0.00 | $2,948,607 | 90.23% |
| Bankruptcy within past 24 months | 3,296,940 | 0.027 | 0.468 | 0 | 0 | 9 | 99.39% |
| Collections Flag | 3,311,743 | 0.371 | 0.483 | 0 | 0 | 1 | 62.87% |
| Any Debt | 3,311,743 | 0.761 | 0.426 | 0 | 1 | 1 | 23.87% |
| Non-home, non-SL Balance | 3,311,743 | $4,060.68 | $8,322.40 | $0.00 | $276.83 | $1,099,403 | 43.54% |
| Home-secured Balance | 3,311,743 | $6,971.57 | $38,197.38 | $0.00 | $0.00 | $6,003,286 | 95.00% |
| Student Loan Balance | 3,311,743 | $4,994.88 | $14,478.32 | $0.00 | $0.00 | $543,183 | 70.65% |
| | | | | | | | |
| **Education Reform-related Variables** | | | | | | | |
| Went to HS before state enacted Econ reform | 3,311,743 | 0.127 | 0.333 | 0 | 0 | 1 | 87.26% |
| Exposed to Econ Reform | 3,311,743 | 0.492 | 0.500 | 0 | 0 | 1 | 50.75% |
| Went to HS before state enacted Fin Lit reform | 3,311,743 | 0.202 | 0.401 | 0 | 0 | 1 | 79.81% |
| Exposed to Financial Literacy Reform | 3,311,743 | 0.166 | 0.372 | 0 | 0 | 1 | 83.39% |
| Went to HS before state enacted Math reform | 3,311,743 | 0.210 | 0.407 | 0 | 0 | 1 | 78.98% |
| Exposed to Math Reform | 3,311,743 | 0.339 | 0.473 | 0 | 0 | 1 | 66.15% |
| State # of years of math required to graduate | 3,140,999 | 2.827 | 0.653 | 0 | 3 | 4 | 0.30% |
| | | | | | | | |
| **Control Variables** | | | | | | | |
| Zip-code Income Per Capita ($thousands) | 3,271,550 | 33.43 | 27.81 | 0 | 27.4 | 3182.77 | 0.00% |
| State Educational Spending per Capita | 3,252,822 | 3055.8 | 881.3 | 1536.89 | 2938.96 | 11300.88 | 0.00% |
| County-level Unemployment Rate | 3,206,093 | 7.586 | 2.832 | 0.7 | 7.35 | 29.9 | 0.00% |
| # of years of state compulsory schooling | 3,311,743 | 8.011 | 0.167 | 8 | 8 | 11 | 0.00% |
| State grad requirement: Social Studies # of yrs | 3,311,743 | 2.768 | 0.697 | 0.5 | 3 | 4 | 0.00% |
| State gradaution requirement: English # of yrs | 3,311,743 | 3.778 | 0.440 | 1 | 4 | 4 | 0.00% |
| State graduation requirement: Science # of yrs | 3,311,743 | 2.712 | 0.692 | 1 | 3 | 4 | 0.00% |
| Gross State Product ($millions) | 3,302,778 | 615,682 | 554,670 | 23,443 | 401,488 | 1,987,881 | 0.00% |
| Birth Year | 3,311,631 | 1986.6 | 2.162 | 1984 | 1986 | 1993 | 0.00% |

*2% panel of Equifax CCP, Q4 of years 1999-2012, individuals born after 1983

Source of outcome variables: FRBNY Consumer Credit Panel/Equifax

**Table 4: Impact of Financial Education on the Extensive Margin**

|                            | E1 Model | E2 Model |
|----------------------------|:--------:|:--------:|
|                            |   (1)    |   (2)    |
| Math Reform                | -0.0008  | -0.0007  |
|                            | (0.0073) | (0.0273) |
| Economics Reform           | -0.0039  | -0.0014  |
|                            | (0.0090) | (0.0139) |
| Financial Literacy Reform  | 0.0124** | 0.0132** |
|                            | (0.0049) | (0.0108) |
| Number of Observations     |   402    |   402    |

Dep. Var is the proportion of 20-28 year olds with a credit report (in a state-year).

All regressions have state and year fixed effects.

Standard errors clustered at the state level reported in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively.

Coefficients in Column 2 reflect difference between post- and pre- dummies.
Source: FRBNY Consumer Credit Panel/Equifax

## Table 5: I1 (baseline) Model Estimates, for Pooled Sample

| | Risk Score | Number of Delinquent Accounts | Balance in Delinquent Accounts | Bankruptcy within past 24 months | Collections Flag | Any Debt | Auto Loans and Credit Card Balance | Home-secured Balance | Student Loan Balance |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Math years | 1.848*** | - 0.0403*** | - 284.5*** | 0.0101*** | - 0.00629*** | - 0.0505*** | - 889.7*** | - 1846.7*** | - 2027.0*** |
| | (0.460) | (0.00418) | (41.27) | (0.00284) | (0.00226) | (0.00388) | (70.33) | (250.3) | (163.3) |
| Economics Reform | - 4.013*** | 0.0159*** | 163.9*** | - 0.00248*** | 0.0119** | 0.0215*** | 554.1*** | 233.7 | 42.70 |
| | (0.801) | (0.00388) | (37.76) | (0.000927) | (0.00530) | (0.00798) | (137.1) | (396.1) | (208.4) |
| Fin Lit Reform | 1.983*** | - 0.00387 | - 90.72** | - 0.000619 | - 0.0105** | - 0.0120 | - 579.2*** | - 2407.5*** | 451.7* |
| | (0.668) | (0.00316) | (38.99) | (0.00134) | (0.00422) | (0.00771) | (129.6) | (404.5) | (249.4) |
| N | 2,790,504 | 3,005,020 | 3,005,020 | 2,991,645 | 3,005,020 | 3,005,020 | 3,005,020 | 3,005,020 | 3,005,020 |
| Mean of Dep. Var. | 627.7 | 0.168 | 975.4 | 0.0274 | 0.378 | 0.760 | 4059.3 | 6914.0 | 5008.0 |
| Std. Dev of Dep Var. | 91.93 | 0.671 | 11242.0 | 0.467 | 0.485 | 0.427 | 8329.9 | 38087.7 | 14506.3 |

All regressions include state-year and birth cohort-year fixed effects.

Standard errors clustered at state-year level reported in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively.

Source: FRBNY Consumer Credit Panel/Equifax

## Table 6: Model I1 Estimates, by Age

| | Risk Score | Number of Delinq. Accounts | Balance in Delinquent Accounts | Bankruptcy within past 24 months | Collection Flag | Any Debt | Auto Loans and Credit Card Balance | Home-secured Balance | Student Loan Balance |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **22-year-olds** | | | | | | | | | |
| Math years | 0.970 | -0.0297*** | -74.27 | 0.0195*** | -0.00474 | -0.0478*** | -577.0*** | -454.7** | -1596.2*** |
| | (0.789) | (0.00523) | (56.22) | (0.00393) | (0.00391) | (0.00644) | (100.1) | (220.1) | (217.3) |
| Econ Refrom | -6.059*** | 0.0269*** | 72.47 | -0.00170 | 0.00574 | 0.0167* | 498.2*** | 58.79 | 53.26 |
| | (1.168) | (0.00786) | (73.62) | (0.00188) | (0.00526) | (0.00908) | (164.5) | (304.5) | (216.9) |
| Fin Lit Reform | 3.813*** | -0.0174** | -18.11 | -0.000665 | -0.0108** | -0.00899 | -538.0*** | -529.8*** | 253.9 |
| | (1.084) | (0.00781) | (77.52) | (0.00310) | (0.00499) | (0.00951) | (153.2) | (196.3) | (264.7) |
| N | 395503 | 426665 | 426665 | 424916 | 426665 | 426665 | 426665 | 426665 | 426665 |
| Mean of Dep Var | 624.1 | 0.168 | 805.9 | 0.0166 | 0.390 | 0.759 | 3889.9 | 3725.7 | 4922.0 |
| St dev of Dep Var | 93.28 | 0.635 | 10603.0 | 0.364 | 0.488 | 0.427 | 7996.5 | 30749.2 | 12964.9 |
| **25-year-olds** | | | | | | | | | |
| Math years | 2.735*** | -0.0551*** | -354.7*** | 0.00215 | -0.0183*** | -0.0664*** | -1286.0*** | -2572.7*** | -3022.0*** |
| | (0.883) | (0.00807) | (99.99) | (0.00167) | (0.00547) | (0.00732) | (134.3) | (445.8) | (284.8) |
| Econ Refrom | -5.132*** | 0.0144* | 168.1 | -0.00160* | 0.0141 | 0.0215* | 824.3*** | 833.4 | 229.8 |
| | (1.616) | (0.00816) | (137.8) | (0.000951) | (0.0103) | (0.0127) | (241.8) | (634.1) | (386.4) |
| Fin Lit Reform | 2.306 | -0.00312 | -74.58 | 0.00207 | -0.0200** | -0.0126 | -1087.2*** | -3589.9*** | 261.9 |
| | (1.690) | (0.00786) | (131.6) | (0.00142) | (0.00994) | (0.0143) | (177.8) | (689.0) | (570.8) |
| N | 260864 | 279419 | 279419 | 278923 | 279419 | 279419 | 279419 | 279419 | 279419 |
| Mean of Dep Var | 630.8 | 0.210 | 1642.4 | 0.00952 | 0.487 | 0.771 | 5375.5 | 12961.2 | 6801.6 |
| St dev of Dep Var | 95.58 | 0.816 | 14845.9 | 0.177 | 0.500 | 0.420 | 9533.2 | 49846.2 | 18942.3 |
| **28-year-olds** | | | | | | | | | |
| Math years | 7.104*** | -0.0642*** | -767.9*** | -0.00107 | -0.0301*** | -0.0565*** | -1495.4*** | -5632.6*** | -3202.5*** |
| | (1.608) | (0.0141) | (248.2) | (0.00196) | (0.00928) | (0.0103) | (277.6) | (1109.2) | (402.5) |
| Econ Refrom | -10.37*** | 0.0471*** | 181.6 | -0.00261 | 0.0368 | 0.0130 | 611.0 | -23.84 | 416.0 |
| | (3.799) | (0.0176) | (307.5) | (0.00183) | (0.0227) | (0.0208) | (442.5) | (1425.0) | (893.6) |
| Fin Lit Reform | 4.177 | -0.00559 | -34.62 | 0.00357** | -0.0391 | -0.0102 | -1436.6*** | -9134.0*** | -188.2 |
| | (3.842) | (0.0133) | (328.8) | (0.00176) | (0.0247) | (0.0238) | (250.1) | (1202.5) | (1312.6) |
| N | 67922 | 72983 | 72983 | 72861 | 72983 | 72983 | 72983 | 72983 | 72983 |
| Mean of Dep Var | 642.0 | 0.219 | 2163.9 | 0.0109 | 0.493 | 0.781 | 6654.4 | 25218.4 | 7520.8 |
| St dev of Dep Var | 99.88 | 0.910 | 16510.8 | 0.157 | 0.500 | 0.414 | 11208.8 | 69507.3 | 22059.4 |

All regressions include state and year fixed effects.

Standard errors clustered at state-year level reported in parentheses. ***, **, * denote sig at the 1, 5, and 10% levels, respectively.

Source: FRBNY Consumer Credit Panel/Equifax

**Table 7: Event Study 1 (ES1 model) estimates (pooled sample)**

|  | Risk Score | Number of Delinquent Accounts | Balance in Delinquent Accounts | Bankruptcy within past 24 months | Collections Flag | Any Debt | Auto Loans & Credit Card Balance | Home-secured Balance | Student Loan Balance |
|---|---|---|---|---|---|---|---|---|---|
|  | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Math years | 1.837*** | -0.0403*** | -285.1*** | 0.0101*** | 0.00616*** | -0.0506*** | -891.6*** | -1858.5*** | -2031.1*** |
|  | (0.465) | (0.00419) | (41.30) | (0.00284) | (0.00228) | (0.00386) | (70.43) | (250.0) | (163.6) |
| Economics Reform | -3.474*** | 0.0083 | 49.43 | -0.0046*** | 0.0122*** | 0.0223** | 358.5** | -985.6* | -128.3 |
|  | (1.154) | (0.0062) | (64.46) | (0.0014) | (0.0086) | (0.0128) | (214.6) | (516.9) | (333.8) |
| Fin Lit Reform | 0.583 | -0.0023 | -73.32 | -0.0007 | 0.0037 | -0.0239*** | -644.3*** | -2815.9*** | 130.6 |
|  | (0.986) | (0.0056) | (61.48) | (0.0017) | (0.0071) | (0.0124) | (207.4) | (499.2) | (350.1) |
| N | 2790504 | 3005020 | 3005020 | 2991645 | 3005020 | 3005020 | 3005020 | 3005020 | 3005020 |
| Mean of Dep. Var. | 627.7 | 0.168 | 975.4 | 0.0274 | 0.378 | 0.76 | 4059.3 | 6914.0 | 5008.0 |
| Std. Dev of Dep Var. | 91.93 | 0.671 | 11242 | 0.467 | 0.485 | 0.427 | 8329.9 | 38087.7 | 14506.3 |

Difference between post- and pre- reform coefficients reported for Economics and Financial Literacy reforms, and post coefficeints reported for Math years. All regressions include state-year and birth cohort-year fixed effects.

Standard errors clustered at state-year level reported in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively.

Source: FRBNY Consumer Credit Panel/Equifax

**Table 8: Adjusted p-values for I1 Model (pooled sample) Estimates**

| Variable | P-values based on: | | | |
|---|---|---|---|---|
| | Standard | Bonferroni | Bonferroni-Holm | Benjamini-Hochberg |
| | (1) | (2) | (3) | (4) |
| **Mathematics Years** | | | | |
| Risk Score | 0.000 | 0.00 | 0.00 | 0.00 |
| Number of Delinquent Account | 0.000 | 0.00 | 0.00 | 0.00 |
| Total Delinquent Balance | 0.000 | 0.00 | 0.00 | 0.00 |
| Bankruptcy w/in past 24 months | 0.000 | 0.00 | 0.00 | 0.00 |
| Any Debt | 0.000 | 0.00 | 0.00 | 0.00 |
| Credit Card + Auto Debt Balance | 0.000 | 0.00 | 0.00 | 0.00 |
| Home-Secured Debt Balance | 0.000 | 0.00 | 0.00 | 0.00 |
| Student Loan Balance | 0.000 | 0.00 | 0.00 | 0.00 |
| Collections | 0.006 | 0.06 | 0.01 | 0.01 |
| | | | | |
| **Economics Reform** | | | | |
| Risk Score | 0.000 | 0.00 | 0.00 | 0.00 |
| Number of Delinquent Account | 0.000 | 0.00 | 0.00 | 0.00 |
| Total Delinquent Balance | 0.000 | 0.00 | 0.00 | 0.00 |
| Credit Card + Auto Debt Balance | 0.000 | 0.00 | 0.00 | 0.00 |
| Any Debt | 0.007 | 0.07 | 0.04 | 0.01 |
| Bankruptcy w/in past 24 months | 0.008 | 0.08 | 0.03 | 0.01 |
| Collections | 0.026 | 0.26 | 0.08 | 0.03 |
| **Financial Literacy Reform** | | | | |
| Credit Card + Auto Debt Balance | 0.000 | 0.00 | 0.00 | 0.00 |
| Home-Secured Debt Balance | 0.000 | 0.00 | 0.00 | 0.00 |
| Risk Score | 0.003 | 0.03 | 0.02 | 0.01 |
| Collections | 0.013 | 0.13 | 0.08 | 0.03 |
| Total Delinquent Balance | 0.020 | 0.20 | 0.10 | 0.04 |
| Student Loan Balance | 0.071 | 0.71 | 0.28 | 0.11 |

Table reports corrected p-values for estimates of the I1 model that are statistically significant at the 10% or higher in the baseline (Table 5).

# Table 10: Falsification Test (based on model I1)

| | Risk Score | Number of Delinquent Accounts | Balance in Delinquent Accounts | Bankruptcy within past 24 months | Collections Flag | Any Debt | Auto Loans & Credit Card Balance | Home-secured Balance | Student Loan Balance |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Math years | -0.561 | -0.0289** | -65.43 | -0.0009* | -0.0250** | -0.0238*** | -400.4* | -184.2 | -1208.8*** |
| | (0.889) | (0.0132) | (98.97) | (0.0005) | (0.0111) | (0.0084) | (239.9) | (276.4) | (176.6) |
| Economics Reform | 2.201*** | -0.0037 | -148.8** | -0.0005 | -0.0133* | 0.0054 | -197.4** | -318.6 | 268.9** |
| | (0.724) | (0.0050) | (72.86) | (0.0004) | (0.00679) | (0.0046) | (85.08) | (578.8) | (110.3) |
| Fin Literacy Reform | -1.215 | 0.0039 | 53.08 | 0.0009 | 0.0362** | -0.0002 | 281.5 | 224.8 | -144.7 |
| | (1.300) | (0.0098) | (101.9) | (0.0007) | (0.0158) | (0.0093) | (189.2) | (704.4) | (207.5) |
| N | 231124 | 237312 | 237312 | 237312 | 237312 | 237312 | 237312 | 237312 | 237312 |
| Mean of Dep. Var. | 623.3 | 0.137 | 398.5 | 0.00145 | 0.198 | 0.795 | 3101.3 | 2133.3 | 2319.7 |
| Std Dev. Of Dep. Var. | 84.06 | 0.521 | 6193.0 | 0.0380 | 0.399 | 0.404 | 7490.0 | 25282.9 | 7150.0 |

All regressions include state-year and birth cohort-year fixed effects.

Standard errors clustered at state-year level reported in parentheses. We do not cluster risk score standard errors due to small clusters in some cases. ***, **, * denote significance at the 1, 5, and 10% levels, respectively.

Source: FRBNY Consumer Credit Panel/Equifax

**Table A1: Impact of Financial Education on the Intensive Margin in Pooled Sample, with Individual Random Effects**

| | Risk Score | Number of Delinquent Accounts | Balance in Delinquent Accounts | Bankruptcy within past 24 months | Collections Flag | Any Debt | Auto and Credit Card Balance | Home-secured Balance | Student Loan Balance |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Math years | 1.964*** | -0.0403*** | -284.5*** | 0.0165*** | -0.0140*** | -0.0511*** | -935.3*** | -2090.3*** | -2007.5*** |
| | (0.367) | (0.00145) | (24.40) | (0.00356) | (0.00188) | (0.00175) | (26.74) | (113.7) | (52.35) |
| Economics Reform | -2.928*** | 0.0159*** | 163.9*** | -0.000816 | 0.00501*** | 0.0129*** | 489.4*** | 272.3** | -49.41 |
| | (0.272) | (0.00213) | (35.69) | (0.00159) | (0.00142) | (0.00145) | (28.12) | (126.9) | (42.29) |
| Fin Lit Reform | 1.521*** | -0.00387* | -90.72*** | -0.00151 | -0.0108*** | -0.00735*** | -607.9*** | -2236.0*** | 359.3*** |
| | (0.279) | (0.00206) | (34.61) | (0.00165) | (0.00145) | (0.00148) | (28.17) | (126.4) | (43.17) |
| N | 2790504 | 3005020 | 3005020 | 2991645 | 3005020 | 3005020 | 3005020 | 3005020 | 3005020 |
| Mean of Dep. Var. | 627.7 | 0.168 | 975.4 | 0.0274 | 0.378 | 0.760 | 4059.3 | 6914.0 | 5008.0 |
| Std. Dev of Dep Var. | 91.93 | 0.671 | 11242.0 | 0.467 | 0.485 | 0.427 | 8329.9 | 38087.7 | 14506.3 |

Note: All regressions include cohort-year and state-year fixed effects, as well as individual random effects.

Standard errors reported in parentheses. ***, **, * denote significance at the 1, 5, and 10% levels, respectively.

Source: FRBNY Consumer Credit Panel/Equifax

# Table A2: Event Study 1 (ES1 model) estimates, for Ages 22, 25, and 28

| | Risk Score | Number of Delinq. Accounts | Balance in Delinquent Accounts | Bankruptcy within past 24 | Collection Flag | Any Debt | Auto & Credit Card | Home-secured Balance | Student Loan Balance |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **22-year-olds** | | | | | | | | | |
| Math Years | 0.959 | 0.0296*** | -74.12 | 0.0195*** | -0.0046 | 0.0479*** | -578.5*** | -455.5** | 1599.8*** |
| | (0.789) | (0.0052) | (56.14) | (0.0039) | (0.0039) | (0.0064) | (100.5) | (221.1) | (217.2) |
| Econ Reform | -6.800*** | 0.0344*** | 157.5 | -0.0039 | 0.0103 | 0.0145 | 205.4 | -337.6 | -161.6 |
| | (1.818) | (0.0129) | (118.8) | (0.0032) | (0.0088) | (0.0146) | (268.6) | (526.4) | (361.1) |
| Fin Lit Reform | 3.085** | -0.0133 | -13.42 | -0.0008 | 0.0011 | -0.0179* | -630.7*** | -562.5** | -5.868 |
| | (1.608) | (0.0124) | (123.9) | (0.0039) | (0.0076) | (0.0160) | (244.7) | (380.2) | (376.5) |
| Observations | 395503 | 426665 | 426665 | 424916 | 426665 | 426665 | 426665 | 426665 | 426665 |
| Mean of Dep Var | 624.1 | 0.168 | 805.9 | 0.0166 | 0.390 | 0.759 | 3889.9 | 3725.7 | 4922.0 |
| Std Dev of Dep V | 93.28 | 0.635 | 10603.0 | 0.364 | 0.488 | 0.427 | 7996.5 | 30749.2 | 12964.9 |
| **25-year-olds** | | | | | | | | | |
| Math Years | 2.765*** | 0.0552*** | -357.0*** | 0.0021 | 0.0185*** | 0.0663*** | 1286.0*** | 2581.4*** | 3017.9*** |
| | (0.884) | (0.0081) | (99.78) | (0.0017) | (0.0055) | (0.0073) | (134.5) | (445.9) | (284.7) |
| Econ Reform | -3.574* | 0.0083 | -121.0 | -0.0029** | 0.0095 | 0.0209 | 624.1** | -896.0 | 201.7 |
| | (2.372) | (0.0130) | (222.2) | (0.0015) | (0.0156) | (0.0188) | (338.5) | (1025.4) | (610.7) |
| Fin Lit Reform | -0.0987 | -0.0042 | -128.4 | 0.0028 | 0.0005 | -0.0299* | 1355.6*** | 4697.9*** | -352.6 |
| | (2.059) | (0.0114) | (193.1) | (0.0017) | (0.0134) | (0.0188) | (274.0) | (841.4) | (652.9) |
| Observations | 260864 | 279419 | 279419 | 278923 | 279419 | 279419 | 279419 | 279419 | 279419 |
| Mean of Dep Var | 630.8 | 0.210 | 1642.4 | 0.00952 | 0.487 | 0.771 | 5375.5 | 12961.2 | 6801.6 |
| Std Dev of Dep V | 95.58 | 0.816 | 14845.9 | 0.177 | 0.500 | 0.420 | 9533.2 | 49846.2 | 18942.3 |
| **28-year-olds** | | | | | | | | | |
| Math Years | 7.132*** | 0.0642*** | -766.6*** | -0.0011 | 0.0303*** | 0.0564*** | 1498.2*** | 5640.1*** | 3194.3*** |
| | (1.596) | (0.0141) | (248.3) | (0.0019) | (0.0093) | (0.0103) | (279.7) | (1108.5) | (403.5) |
| Econ Reform | -12.12*** | 0.0403 | 283.6 | -0.0060** | 0.0513* | 0.00733 | 145.2 | -3865.3* | 259.8 |
| | (5.371) | (0.0307) | (431.6) | (0.0032) | (0.0337) | (0.0315) | (627.0) | (2161.8) | (1438.6) |
| Fin Lit Reform | -0.623 | -0.0116 | -97.31 | 0.0038 | -0.0076 | -0.0303 | 1514.3*** | 11781.0** | -1363.4 |
| | (4.793) | (0.0232) | (421.0) | (0.0027) | (0.0308) | (0.0313) | (528.5) | (1800.4) | (1539.8) |
| Observations | 70763 | 76038 | 76038 | 75908 | 76038 | 76038 | 76038 | 76038 | 76038 |
| Mean of Dep Var | 642.4 | 0.218 | 2141.0 | 0.0109 | 0.490 | 0.781 | 6648.8 | 25417.7 | 7502.3 |
| Std Dev of Dep V | 99.89 | 0.906 | 16383.3 | 0.155 | 0.500 | 0.414 | 11178.2 | 69673.9 | 21936.1 |

Difference between post- and pre- reform coeff reported for Econ and Fin Lit reforms, and post coefficeints for Math years. Standard errors clustered at state-year level reported in parentheses. ***, **, * denote sig at 1, 5, and 10% levels, respectively. All regressions include state and birth year fixed effects.