Financial Education and the Debt Behavior of the Young

Meta Brown[§], John Grigsby*, Wilbert van der Klaauw[‡], Jaya Wen[†], and Basit Zafar[ǂ]

First version: April 2013, this version: May 2014

Abstract

Young Americans are heavily reliant on debt, and have clear financial literacy shortcomings, yet evidence on the relationship between financial education and youths' subsequent debt behavior remains mixed. In this paper, we study the effects of exposure to financial training on debt outcomes in early adulthood among a two percent sample of all Americans aged 19 to 29. Variation in exposure to financial training comes from statewide changes in financial literacy, economics, and mathematics high school graduation requirements mandated over the 1990s and 2000s. The FRBNY Consumer Credit Panel provides debt outcomes based on quarterly Equifax credit reports from 1999 to 2013. Our analysis, based on a flexible event study approach, reveals significant effects of quantitative training on debt-related outcomes of youth. We find that math and financial literacy education exposure decreases the incidence of adverse outcomes – such as bankruptcies – and reduces the likelihood of youth carrying debt. These effects fade out with age. On the other hand, economic education leads to an increase in debt market participation, with the effects accumulating over the course of early adulthood. It increases debt balances in later youth, increasing the likelihood of adverse outcomes (such as collections and delinquencies), and leads to a decline in youths' average risk scores. We find that, during a difficult era for young first time homebuyers, exposure to all three types of financial education delay entry into homeownership. Our results suggest that financial education programs, increasingly promoted by policy-makers, do have significant impacts on the financial decision-making of youth, but their impacts may depend on the content of the programs.

We would like to thank Brian Bucks, Chris Carroll, Tullio Jappelli, Henry Korytkowski, David Laibson, Silvia Magri, Olivia Mitchell, Dekuwmini Mornah, Shannon Mudd, Anna Paulson, Max Schmeiser, Joseph Tracy, Didem Tuzeman, Carly Urban, Jonathan Willis, and seminar and conference participants at the American Economic Association meetings, the Association for Education Finance and Policy meetings, the Eastern Economic Association meetings, ECB 2013 Conference on Household Finance & Consumption, the Federal Reserve Banks of Kansas City, New York, and Philadelphia, and University of Michigan 2013 Aspen Conference on Economic Decision-making for comments. The views and opinions offered in this paper do not necessarily reflect the position of the Federal Reserve Bank of New York or the Federal Reserve System.

[§] Meta.Brown@ny.frb.org. Research & Statistics. Federal Reserve Bank of New York.
*John.Grigsby@ny.frb.org. Research & Statistics. Federal Reserve Bank of New York.
[‡] Wilbert.vanderklaauw@ny.frb.org. Research & Statistics. Federal Reserve Bank of New York.
[†] jaya.wen@gmail.com. Department of Economics, Yale University.
[ǂ] Basit.Zafar@ny.frb.org. Research & Statistics. Federal Reserve Bank of New York.

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 $22,911.[1] Despite this extensive interaction with lending markets, a majority of high school and college students fail basic financial literacy tests (Hastings, Madrian, and Skimmyhorn, 2013; Markow and Bagnaschi, 2005; Shim et al., 2010).[2] The low financial literacy rates among US youth and an effective delinquency rate of over 30% on student loans for young borrowers in repayment (Brown et al., 2013a), along with the well-established correlation between financial literacy and financial well-being,[3] has prompted policy-makers and the media to push for more financial education.[4] However, evidence of the causal effect of financial training on *debt outcomes for the young* is based largely on field and natural experiments of modest scale, and is, at best, mixed.[5]

Our analysis addresses the question of the effectiveness of financial education by analyzing large-scale changes in financial training exposure in a two percent sample of young Americans, and tracking their debt outcomes over the decade immediately following the high school training. Given weak prior evidence, we attempt to identify meaningful effects of financial training where we think they are most likely to exist. We look for effects of very recent changes in financial training, which involve large increases in required classroom hours and apply to millions of US students, and we look for these effects

---

[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). Financial mistakes are most common among the youngest and oldest consumers (Agarwal et al.) 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), are more likely to withdraw housing equity (Duca and Kumar, forthcoming), 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] Fernandes, Lynch, and Netemeyer (forthcoming) conduct a meta-analysis of 168 prior studies of the relationship between financial education and financial behavior, and find that financial education (or literacy) explains 0.1% of the variation in financial behavior observed in the studies.

in the years immediately following the training, in debt decisions that are relevant to most of the treated population. Failure to find effects of financial training in this context could, following Fernandes et al., both unite and reinforce the findings of several smaller and disparate field studies. On the other hand, evidence of meaningful effects of financial training in this context could derive from some or all of a number of adjustments to the methodology. The technology of financial training may have improved over recent decades. Effects may appear only following very intensive interventions, at earlier ages only, or only in a much larger population. Finally, it may be necessary to track outcomes at very young ages, shortly after training occurs, and in debt choices that are relevant to the majority of the treated population.

For this purpose, we use variation in financial education – more specifically, 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 and ongoing quarterly panel on consumer debts drawn from credit reports from Equifax, one of three major national credit reporting agencies. It consists of the credit reports of five percent of the US population with credit reports.[6]

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 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 (except one) 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 fully flexible time trends for each state, and fully flexible time trends for each cohort, in addition to a rich set of local time-varying controls, uses these staggered policy changes to identify the causal impact of financial education on debt-related outcomes of youth.[7] Conditional on this extensive set of controls, our identifying assumption hinges on states' implementation of these reforms being uncorrelated with those omitted determinants of financial outcomes that vary at the state-cohort level. [8]

---

[6] 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 Avery, Calem, and Canner (2003), Bricker et al. (2012), and Brown, Haughwout, Lee, and van der Klaauw (2013b).

[7] 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). And, as mentioned above, we do not assume linear state-specific time trends either, but allow the trends to be fully flexible.

[8] For example, we assume states' reform implementation is uncorrelated with omitted factors determining the relative debt outcomes in 2012 of West Virginians born in 1993 and West Virginians born in 1994, conditional on (1) debt outcomes for West Virginian youth in 2012, (2) a West Virginia flexible time trend in the evolution of debt outcomes, (3) a rich set of time-varying local cohort controls, including such elements as local income, local

The empirical analysis reveals that exposure to financial and quantitative education has sizable impacts on the debt-related outcomes of 19 to 29 year olds. Additional mathematics training leads to improved creditworthiness (as measured by the Equifax risk score, which is similar to the FICO score), and decreases adverse outcomes such as bankruptcy and delinquencies. It also leads to (economically and statistically) significant impacts in the propensity to have debt: an additional year of math, for example, decreases the probability of having mortgage debt by 0.7 percentage points (on a base of 9%), and of having auto/credit card debt by 0.9 percentage points (on a baseline prevalence of 78%). Math education, however, has no impact on the extensive margin, that is, the likelihood of having a credit report. Impacts of math education seem to fade out over time in early adulthood.

Financial literacy exposure increases the prevalence of credit reports in this age group, which suggests improved understanding of the value of credit history.[9] As in the case of mathematics education, along the intensive margin, financial literacy reduces exposure to housing and consumption debt, reduces the prevalence of bankruptcy, but increases the chance of third party collections. Financial literacy training also leads to a lower likelihood of having any outstanding debt (a decrease of 1.4 percentage points on a base of 76.2%), and of ever having housing or auto/credit card debt. As in the case of math education, the impacts of financial literacy training also seem to fade out with age.

Economic education seems to have impacts similar to math and financial literacy in certain dimensions, such as the decline in the likelihood of having auto/credit card and mortgage debt, and a decline in bankruptcies. 2002-2012 was a difficult era for first-time home buyers. We find that all three types of financial education significantly delay homeownership for our young sample. These effects fade for 28 year olds, but are significant at earlier ages. Indeed, math and financial literacy education are associated with a decrease in cumulative homeownership rates of 8 and 14 percent of the average among 22 year-olds.

In marked contrast to the estimated impacts of mathematics and financial literacy education, we see that economic education leads to an increase in the likelihood of having outstanding debt, and a decline of 1.3 points, on average, in youths' creditworthiness (the sample standard deviation in risk scores is 94 points). We find little impact of economics education on the propensity of youth having a credit report. The effects of economics education also strengthen with age, suggestive of economics training demystifying borrowing and credit markets for the young, and arguably dispelling debt aversion amongst those with credit reports. We see that the average balances – for housing and auto/credit card debt –

educational spending, and other high school course requirements for graduation faced by the 1993 and 1994 West Virginian birth cohorts, and (4) the difference between the 2012 debt outcomes of U.S. youth born in 1993 and U.S. youth born in 1994.

[9] For example, having older credit accounts typically increases credit scores. (Federal Reserve Bank of Philadelphia, 2012).

increase with age. This higher debt leads to increases in repayment problems, with a greater incidence of delinquencies and collections with age, and a substantial decline in average risk scores. For example, economics education leads to declines of 2, 7, and 10 points, on average, in youth's credit scores at ages 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 largely stable over time. For some outcomes, the effects tend to augment several years after the reforms are implemented, suggestive of a lag between the passage of legislation and (effective) implementation of new curricula. We also report a series of sensitivity analysis to test the robustness of our findings. Our results are robust to correcting the standard errors for multiple hypotheses testing, and a falsification test implementing placebo reforms.

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 (that we discuss below) regarding the legitimacy of funding financial education programs in the U.S.[10] 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, a change in bankruptcies due to exposure to quantitative training. While default may be unwelcome, the failure to exploit the bankruptcy option in certain states of the world may itself be a source of inefficiency in a consumer's intertemporal decision-making.[11] 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 some relevant prior studies, and our 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. Literature and Data

a. Prior literature

[10] See, for example, Cole et al. (2013), and the debate as discussed in Hastings et al. (2013).
[11] See, for example, Fay, Hurst, and White (2002).

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.[12] The effect of financial training on retirement saving is of obvious importance. But saving is considerably less relevant in early adulthood. To the extent that financial literacy interventions occur during high school, debt behavior may be an outcome of more immediate relevance. For example, while 94 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.[13] 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.[14] 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. Fernandes, Lynch, and Netemeyer (forthcoming), in a comprehensive meta-analysis, contrast the strong relationships between measured financial literacy and financial behavior demonstrated by more correlational studies with the weak effects of financial interventions on later financial behavior shown in experimental studies. 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. Given the timing of compulsory schooling reforms, these outcomes are necessarily studied in a middle-aged sample.

We are aware of two studies that investigate the causal effect of financial education on debt-related outcomes. Cole, Paulson, and Shastry (2013) establish an identification approach quite similar to the one we adopt, and investigate the impact of state financial education mandates between 1957 and 1982 (as in Bernheim et al., 2001) and mathematics reforms between 1984-1994 on investment and debt-

[12] For example, Bayer, Bernheim, and Scholz (2009), Choi, Laibson, and Madrian (2011), Lusardi (2004), and Bernheim and Garrett (2003).

[13] 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.

[14] An alternate approach uses randomized access to financial education. Drexler et al. (2012), discussed below, experimentally varied access to financial education for small-scale entrepreneurs, and found no effect of financial principles-based training on financial management practices a year later, but significant effects of rule of thumb-based training. Other randomized trials that reveal little effect of financial training include Gartner and Todd (2005), Servon and Kaestner (2008), and Choi et al. (2011). Hastings et al. (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.

related outcomes of middle-aged individuals.[15] While they find a sizable impact of mathematics education on outcomes, they find little effect of financial education on either asset accumulation or successful repayment of debt by middle age.

In a second study of special relevance to this paper, Skimmyhorn (2013) investigates the impact of a financial management course for new soldiers in the US Army. As in this study, the subjects of the intervention are young, and the outcomes of interest involve debt. Skimmyhorn 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.

Our conclusions regarding the impact of financial education differ in some meaningful ways from the results of these two studies, and from the weak evidence on financial education effects produced by the broader literature. What may potentially reconcile the latter with our evidence of successful financial education is the age difference in our samples, and our focus on debt-related outcomes (instead of asset accumulation). Relative to Cole et al. (2013), we look for effects of financial education immediately after high school. In addition, we study the effects of more recent financial education reforms. Our results may, in part, reflect improvements in the technology of financial training over the past two decades. Relative to Skimmyhorn, our approximately representative sample of 19-29 year old US consumers may behave differently from a sample of new soldiers. Further, the effects of an eight-hour training program may differ from those of a year-long high school course.[16]

These possibilities are reinforced by the similarity between our results and those of a new study that emerged as we revised this paper. In detailed and careful analysis of the realized path of implementation of new financial education requirements for high school graduation in Georgia, Idaho, and Texas, Brown, Collins, Schmeiser, and Urban (2014) demonstrate modestly improved debt outcomes for 18-22 year old (former) students subject to the reform. They present their results as evidence in favor of K-12 financial education.

b. Data

We use panel data derived from several complementary sources. Our financial behavior outcome variables originate from the CCP. 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.

[15] 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.
[16] This is in no way intended to suggest that either soldiers' debt practices or shorter interventions for relevant populations are uninteresting.

Census (Census).

b.1. Educational reforms in economics, financial literacy, and mathematics

To proxy for individual exposure to economics, financial literacy, and mathematics education, we track state-level policy changes from 1998 through 2012. Our focus on this time period is motivated by data availability, as well as our interest in recent debt outcomes for young borrowers. The earliest surveys of the NCEE – the only comprehensive and centralized source of recent economics and financial literacy high school requirement data – date back to 1998/1999. Table 1 reports a national summary of these reforms. [17]

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; more specifically, the analysis uses the timing of the legislation of the mandate. Likewise, for financial literacy education, the policy reform of interest is whether or not (and when) a state legislated that all high school students complete at least one financial literacy course before graduation. This definition yields meaningful variation over the course of our 1998 to 2012 time period. Between 1999 and 2012, the number of states requiring a financial literacy course for graduation grew from 1 to 17; the number requiring an economics course for graduation doubled from 10 to 20.[18]

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 states excepting North Dakota had some sort of mathematics requirement for high school graduation. The object of interest is the required years of math education for graduation. Variation in this variable across states (and within states over time) is generated by whether or not (and when) a state enacted a policy reform requiring a one-year increase in math education for graduation.

[17] Note that, while Bernheim et al. (2001) report several states with consumer education reforms well before 1999, the difference between their reform history and ours is explained by the narrow consideration of only *required* high school financial education courses in this paper. The narrow focus on required courses is motivated in part by the results of their November 1995 survey, which indicate that elective courses had no significant effect on the rate at which middle aged survey respondents recalled having received high school financial education.

[18] 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.

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.

The theoretical motivation for using these proxies is twofold. First, such 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; Goodman 2012).[19] 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.

Second, early research (Mayer 1989, Bernheim et al. 2001) 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 may not be driven by potentially endogenous trends in public opinion. Earlier research has not uncovered significant socioeconomic or educational differences between states that implement consumer education policies and those that do not (Ford, 1977).[20] However, Cole et al. (2013) argue that states that introduced financial education mandates between 1957 and 1982 were trending differently from states that did not introduce such mandates. In light of this mixed evidence, we estimate model specifications that allow for 50 + 11 separate, flexibly parameterized state-time and cohort-time trends, described by a total of 913 parameters, as well as for the possibility of differences in trends between states that enact policies and

---

[19] Bernheim et al. (2001) used an independently conducted survey to study the effects of a wave of consumer education reforms in the 1960s and 70s on the realized financial education of high school students. The reforms they consider include those requiring financial education for high school students, through both separate financial education courses and material added to existing high school courses, and also those adding high school electives. They find that the likelihood that a survey respondent between the ages of 30 and 49 recalls exposure to financial education increases by (a significant) two percentage points for each year following a state consumer education mandate. In broad averages, they find that 43 percent of those who were exposed to a consumer education mandate, as opposed to 28 percent of those who were not, recalled some high school financial education.

Goodman (2012) finds that the math graduation requirement reforms of 1984-1994, typically increases of one required course in each reform state, induced black males to complete 0.40 more math courses, black females to complete 0.28 more math courses, white males to complete 0.19 more math courses, and white females to complete (an insignificant) 0.10 more math courses.

It is worth noting that the reforms we study may have effects on the number of financial literacy, economics, and mathematics courses completed that may differ from those found by Bernheim et al. and by Goodman. Where Bernheim et al. consider a range of consumer education mandates, we rely only on *required* stand-alone financial literacy and economics courses. Both the stringency of these requirements and the findings in Bernheim et al. suggest that this should lead to larger realized changes in financial education exposure for students. Mathematics requirement reforms will have no effect on the large number of students who would have taken at least the newly required number of math courses in any case. Hence Goodman's mathematics requirement reform effects are difficult to extrapolate to subjects like financial literacy, which may not have been taught in the relevant high schools before the reform. This is a second reason that the effects of financial literacy and economics stand-alone course requirements may have a larger effect on completed coursework than the effects estimated by Bernheim et al. and Goodman.

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

those that do not. Hence any common prior debt patterns among states that implement the reform are absorbed in these flexible state-time paths, and differences in debt outcomes by birth cohorts are absorbed by the flexible cohort-time trends. Identification of the effects of the reforms comes from differences in debt outcomes in a given year for state residents who would have graduated from high school before and after the reform.

b.2. 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. 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 immigrants enter the sample.[21]

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.[22] Because we focus on the impact of recent education reforms on the credit behavior of the young, we restrict our dataset to individuals born in or after 1981. These cohorts will graduate high school in or after 1999, coinciding with the start of our economics and financial literacy education reform data.[23] 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

---

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

[22] See Lee and van der Klaauw (2010) and Brown et al.(2013b) for details.

[23] As mentioned earlier, we are constrained to this period because of lack of reliable data on economics and financial literacy mandates at the high school level during the 1990s.

have credit reports.[24]

To accommodate the annual nature of our other variables, we use only fourth quarter Equifax data from the years 1999 through 2013. 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 5.59 million total observations,[25] and data from 1,016,669 distinct individuals.[26] On average, the panel contains 429,956 observations per year, though as a result of our age constraint the data are heavily concentrated in later years.

We use a number of consumer debt metrics as our outcome variables. First, we look at the Equifax risk score of the individual. This risk score is similar to the FICO score, in that both model 24 month default risk as a function of credit report measures. It varies between 280 and 840 and represents an assessment of the individual's credit-worthiness. We also study each individual's number of delinquent accounts and proportion of debt balance that is delinquent, where delinquency is defined as any debt payment that is reported as 30 or more days past due, and an indicator for having had a balance in collections in the past 7 years. 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. Exploiting the panel nature of the dataset, we also study whether the individual *ever* had any housing debt (indicative of home ownership), and *ever* had a student loan.[27]

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

[24] Jacob and Schneider (2006) find that 10 percent of U.S. adults had no credit reports in 2006, and Brown et al. (2013b) 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.

[25] The initial 2% sample consists of 6,317,757 observations. We are forced to drop 728,335 observations: we drop individuals in some of the outlying territories (such as Puerto Rico and Guam), and those with missing zip codes, since we do not have region-level controls data for 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).

[26] 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.

[27] In a sample of consumers in their twenties, any history of home-secured debt is a reasonably complete proxy for past or present homeownership. Few homeowners this young own their homes outright. The National Association of Realtors reports a median age at first purchase for US homeowners that is roughly stable at 30 over this period.

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.[28] Among those who appear in the panel at age 18, Table A1 shows the percentage of individuals living in the same state as the state in which they graduated from high school: 93.7% of the 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 modest.[29]

b.3. 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 are 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 and English by including a continuous variable representing the number of years required by each state for graduation from high school (at the time when the individual

[28] 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.

[29] Furthermore, if movement across states is random (both in terms of individuals who choose to migrate and the choice of destination), misclassification of the individual's state of high school should attenuate the estimates in the baseline specification towards zero, and bias us against finding an effect of the reforms.

was in high school).[30] 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 of these 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.

b.4. 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 2013.

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 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 2 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. Fifty-three percent of our sample was exposed to an economic education reform (with 10 percent out of the 53 percent also being exposed to financial literacy education), 16 percent to a financial literacy education reform, and 32 percent to a mathematics reform. Further, 15 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 23 and 28 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 the courses may have on students' credit-related outcomes.

[30] 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.

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 themes appear consistently in state financial literacy curricula: decision-making, career planning, personal budgeting, borrowing, and investing.[31]

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 and fewer delinquencies. The impact on debt balances is not entirely clear- given that prior research finds little

---

[31] 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.

impact of financial education on earnings, financial literacy education may help youth balance their budget sheets better and hence may lead to 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. 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. Similarly, the net effect on the individual's risk score is unclear.

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 poorer cognitive skills are linked with worse financial decision-making – for example, Agarwal and Mazumder (2013) find that poorer 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 poorer 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, 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.[32]

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 and a lower likelihood of delinquencies. The impact on debt usage and balances is, however, ambiguous since more math training also leads to higher incomes.

b. Empirical Analysis

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

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

where $D^n_{i(sc)}$ 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 course in subject n before graduation, and is zero otherwise. We take 18 as the high school graduation age. So $D^n_{i(sc)}$ 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 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).[33] γ_{st} is a vector of state-year fixed effects, and δ_{ct} is a vector of birth cohort-year fixed effects; the staggered implementation of the reforms across states and over time (as well as our large

[32] 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.

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

sample size) 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 coefficients of interest are: β_{post}^{econ}, β_{post}^{finlit}, and β_{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.

To interpret the results as causal, any study that exploits state-level reforms has to deal with the concern that reform implementation and timing may be correlated with relevant state- and cohort-specific factors. Our I1 specification, which we also refer to as our baseline specification, attempts to account for these concerns through its flexibility. It does not assume common trends across states, which has been shown to be problematic in studies of state compulsory schooling laws (see Stephens and Yang, 2013). [34] Furthermore, the vector γ_{st} accounts flexibly for state-specific and aggregate time trends in the outcomes (for example, an increase in credit card usage in a given state), and controls for differences across states that may be related to the enactment of the reform in a state.[35] Differing 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 policy changes. Our identifying assumption, then, is that, conditional on this extensive set of controls, implementation of financial education reforms is uncorrelated with other (state- or cohort-specific) omitted determinants of financial outcomes.

The β_{post}^{n} estimate in the baseline model is simply the average treatment effect across all years after the enactment of the reform. A limitation of this approach is that states may take a few years to implement a new reform effectively, and therefore its effects may not be homogenous across years. Or states may put the mandates into effect with some delay following the legislation, in which case the effects may also vary over time. We cannot test for time-varying effects of the reform in the baseline model. In addition, while the baseline specification includes different time trends by state, it does not allow us to investigate whether states that enact a policy have an average pre-trend that is systematically

[34] 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. It should be pointed out that our large sample size here is instrumental in allowing us to use such a flexible specification.
[35] Note that our approach is quite flexible compared to the common approach of including a set of state- or region-specific linear time trends, in studies that exploit state-level variation in different applications. The assumption of linear trends is questionable in this context where debt outcomes may not evolve in a linear way over a long time horizon.

different from that of states that never enact a policy change. To allow for these possibilities, we estimate the following event-study specification:

$$Y_{izt} = \gamma_{st} + \delta_{ct} + \beta^X X_{zt} + \textstyle\sum_n \left(\sum_{j=-4}^{4} \beta_j^n D_{j,i(sc)}^n\right) + \beta_{post}^{math} M_{i(sc)} + \varepsilon_{i(sc)zt}. \quad \text{(ES1)}$$

$D_{j,i(sc)}^n$ 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, where $n \in \{economics, financial\ literacy\}$. For example, $D_{-2,i(sc)}^{econ}$ 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 enactment. 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 enactment. We omit the same-year indicator in the estimation of this model. 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 ($\sum_{j=-4}^{-1} \beta_j^n$) should be zero. Evidence of a treatment effect requires that ($\sum_{j=1}^{4} \beta_j^n$) are jointly different from ($\sum_{j=-4}^{-1} \beta_j^n$). To interpret these numerous coefficients, we compute a Wald test on the difference between the average of the pre-trends and the average of the post-trends. In addition, several figures depict β_j^n series for outcomes of interest. Henceforth, we refer to this event-study specification as the ES1 model.

This specification is our most flexible one. It allows pre-reform trends 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, states 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 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. Baseline Model

a.1. Impact on the Pooled Sample

Estimates of equation (I1) are presented in Table 3. Looking across the first row, we see that exposure to additional mandatory math years has a significant effect on many of our outcomes of interest. It leads to a small but statistically precise increase of 0.5 points, on average, in individuals' risk scores; given a sample standard deviation of 94 points, this is equivalent to an increase of a 0.005 of the standard deviation in the individuals' risk score. An additional year of math requirement leads to a decrease in both the number of delinquent accounts and the percent of balance in delinquent accounts; the estimates imply moderate effects - for example, an additional year of math education decreases the proportion of debt held in delinquent accounts by 0.1 percentage points (given a sample mean of 5.6%). We see that additional math education decreases the likelihood of the individual experiencing bankruptcy in the past 24 months, but has no significant effect on the likelihood of having accounts in collections.

We next turn to the effect of an additional year of math on the likelihood of having outstanding debt. On net, column (6) shows that an additional year of math schooling does not significantly change the probability of having outstanding debt of any kind. However, interesting patterns emerge when we look at specific debt categories.

Column (7) and (8) show the impact of additional exposure to math on housing debt. A history of housing debt drawn within the panel for our twenty-something consumers is a fairly reliable indicator of any past or present homeownership. As noted above, the greatest mass of observations in our sample occur over the 2004-2012 period. This was a turbulent era for young homeowners. Hence early entry into housing markets may be an undesirable outcome for this cohort. The column (7) estimate indicates that an additional year of math decreases the likelihood of the individual having held any housing debt within the panel by 0.69 percentage points (on a base of 9.0 percent). Math exposure, however, seems to have little impact on home-secured debt balances. An additional year of math has a similar effect on the prevalence of auto/credit card debt, reducing the likelihood of ever having this debt by 0.9 percentage points (on a base of 78%) but having no meaningful impact on balances.

The decline in the likelihood of having these other debts as a result of additional math is counteracted by a 0.66 percentage point increase in the probability of having student loans (on a base of 32.2 percent). In separate analysis (available from the authors upon request), we find no evidence of state-level math education mandates affecting state-wide high school graduation rates, so we can rule out that channel as a possible explanation for the increase in student loan take-up.[36]

Moving to the impacts of mandatory financial literary education, we find that they are qualitatively similar to those of math education. However, unlike math education, financial literacy education has no significant impact on the individuals' risk scores, and leads to a greater likelihood of

[36] This finding – of math having little impact on high school graduation rates – has previously been demonstrated for the 1980s math reforms by Goodman (2009).

accounts being in collection. The main difference between the math and financial literacy results is that, in several instances, financial literacy coefficients are larger in magnitude and more precise. For example, financial literacy exposure significantly reduces the likelihood of possessing any outstanding debts, and increases average student loan balance. Indeed, financial literacy education leads to a $299 average *increase* in student debt. Exposure to financial literacy courses is associated with an imprecisely-estimated average increase of $776 of housing debt and decline of $33 for auto and credit card debt. Like math, a year of financial literacy education decreases the early homeownership rate of the sample over this turbulent period by roughly 1 percentage point, and reduces the probability of using credit cards or auto loans for consumption by 1.2 percentage points.

The third row of Table 3 shows that exposure to mandatory economic education leads to impacts that are somewhat different from those of math and financial literacy education. They include an average decline of 1.3 points in the individual's risk score, and a reduction in debt balances across the board – mandating economics education decreases average home-secured balance by $1376, student loan balances by $286 and auto/credit card loans by $148 (only the latter two are statistically significant at conventional levels). Like mathematics and financial education requirements, exposure to required economics education decreases the likelihood of early homeownership. In fact, the magnitude of the effect of the economics requirement is greater: required economics decreases early homeownership by two full percentage points, on a base of 9 percent. Regarding delinquency behavior, we see that economic education significantly reduces the probability of having a bankruptcy within the last 2 years by 0.1 percentage points, but has little effect on the number of delinquent accounts or percent of balance delinquent. It increases the likelihood of individuals carrying any outstanding debts by 1.2 percentage points, on average. This effect seems to be entirely driven by an increase in the likelihood of positive student loan balances by 1.2 percentage points, along with increases in "other" consumer debts, since the likelihood of housing or consumption (credit card/auto) debt in fact decreases.

A notable pattern in Table 3 is that all three kinds of financial education lead to a decline in the likelihood of housing debt, particularly during a time when entry into housing markets may have been undesirable. This finding is consistent with Duca and Kumar (forthcoming) who find that low financial literacy individuals were more likely to withdraw equity during the boom. We also see that exposure to these mandates pulls students away from credit card and auto loans, debt that are generally used to subsidize consumption.

a.2. Impact by Age

To explore how the effects of these financial education reforms evolve over the course of early adulthood, Table 4 presents estimates of the I1 specification, estimated for 22, 25, and 28 year olds, separately.[37] The patterns we find are not unique to this set of ages; in the appendix, we present plots of reform effects for all ages from 19-29 years old. This age-specific specification, as mentioned above, includes state and time fixed effects.

First, we see that the impact that an additional year of math requirement has on the majority of our outcomes fades with age. This must be considered in the context of smaller samples sizes for 28 year olds (an artifact of our sample of individuals born after 1980). We see that while 22 year olds are 0.3 percentage points less likely to have home-secured debt and 0.9 percentage points less likely to have had positive auto or credit card balances as a result of an additional year of math, 28 year olds are 0.3 percentage points *more* likely to have either kind of debt (though this increase is imprecisely estimated). Similarly, the effect of an additional year of math requirement on bankruptcies and collections is statistically significant at the 10% level for 22 year olds, but smaller and insignificant for 28 year olds. Interestingly, an additional year of math requirement increases average risk scores by 0.58 and 0.09 points for 22 and 25 year-olds, respectively, but actually decreases the average risk score of 28 year-olds (but none of these estimates are precise).

Turning to financial literacy education, again we see that the age-specific estimates largely fade with time. For example, while the financial literacy requirement reduces the probability of having positive home-secured and auto/credit card balances by 0.5 and 1.9 percentage points respectively for 22 year olds, this effect falls rapidly over time. We see similar fade out effects for bankruptcies, collections, and the number of delinquent accounts. The exceptions to this fade out pattern are in risk scores and debt balances. The decline in risk scores gets larger in magnitude, while debt balances generally grow larger with age, though most of the estimates are imprecise.

Age-specific estimates regarding economics education generally strengthen over time, and corroborate findings of the pooled sample. Table 4 shows that, as we move from 22 to 28 year olds, the effects of the economics requirement on individuals' risk scores, numbers of delinquent accounts and proportion of debt that is delinquent grow in magnitude. For example, the 10.3 point average decline in age 28 risk scores that results from requiring economics education is nearly five times as large as the decline at age 22. This reflects an increase with age in the number of delinquent accounts, percent of delinquent balance, and collections probability. The effect on home-secured debt and auto/credit card debt balances also grows larger over time.

b. Event Study Specification

[37] An additional value of this approach is that each individual appears only one time, obviating the need for individual effects.

We next move to the discussion of estimates of the Event Study (ES1) model. For our twelve debt-related outcomes, the various panels of Figures 1 and 2 visually show estimates of the $\beta_j^n|_{j=-4}^{4}$, coefficients for financial literacy and economics education, respectively; we account for math years in this specification the same way as in the baseline (I1) model, and those estimates (not reported here) are qualitatively identical to the baseline estimates.[38] Each panel, besides reporting the baseline I1 model estimate, also reports the "average difference", that is, the difference between the average post- and average pre- treatment coefficients: $\frac{1}{4}\ (\sum_{y=1}^{4}\beta_j^n) - \frac{1}{4}\sum_{j=-4}^{-1}\beta_j^n)$. As mentioned earlier, the excluded coefficient is for year zero, the year of the reform. An average difference statistically different from zero is evidence of a non-zero impact of the reform. It is worth noting that the baseline estimates implicitly place additional weight on earlier cohorts, because we have more observations of people graduating 1 year after the reform than we do of people graduating 3 or 4 years after a reform. Thus the baseline model would find a stronger effect if the reform has an initial but fading impact, and a weaker effect if the reform's influence grows.

The first thing of note in the various panels of the two figures is that estimates of the pre-treatment coefficients $(\sum_{j=-4}^{-1}\beta_j^n)$ are not jointly zero in many instances, which indicates that the treatment states (states that implement the reform) and control states had systematically different pre-treatment trends. Turning to financial literacy education (Figure 1), even allowing for separate pre-trends, it is visually clear that the post-treatment estimates, $(\sum_{j=1}^{4}\beta_j^n)$, are different from the pre-treatment estimates for many outcomes. In fact, the average differences between the post- and pre- treatment coefficients for the various outcomes are qualitatively similar to those in the baseline model. Furthermore, the "average difference" is statistically significant for all outcomes that were precisely estimated in the baseline model. Regarding the heterogeneity in treatment effects over time, a mixed picture emerges. The various panels of Figure 1 show that the post-treatment coefficients are generally stable over time, indicating that the reforms have persistent effects. In fact, for some outcomes – such as accounts in collection or bankruptcies – estimates are larger for later cohorts.

Moving to the effects of economic education in Figure 2, the "average difference" is qualitatively similar to the baseline estimate for nearly all the outcomes. However, we now lose precision on several of the statistically significant outcomes in the baseline, such as risk score, bankruptcies in the past 24 months, student loan and auto/credit card balances. In instances where there are significant effects (such

[38] We also estimate a model that allows for an event study approach for math education. Instead of using the variation in the number of math years, we code a math reform as a dummy that equals 1 if the individual's high school state implements an increase in required years of high school math. The interpretation of the estimates is now different since the baseline model shows the impact of an additional year of math, while event study approach shows the impact of exposure to additional math. Estimates for this specification, available from the authors upon request, are qualitatively similar to those for the baseline model.

as the likelihood of any outstanding debt, and student loan balances), we see that the effects are larger for cohorts that graduate in later years. For example, in the case of student loan balances, the estimates are a decline of \$186, \$420, \$323, and \$803 for cohorts that graduate one, two, three, and four or more years after the reform, respectively.

Overall, our ES1 estimates are 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 qualitative 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 specification is that it shows how the average pre-trends differ for the two sets of states. Additionally, incorporation of the heterogeneous treatment effects (by cohorts) indicates that the effects of economic education and of financial literacy are stable over time, and in some instances grow larger for later graduating cohorts. This pattern suggests either that states become better at teaching financial education over time, or a lag between the passage of legislation and implementation of new curricula in some of the treated states.

c. Robustness Checks

In this section, we provide additional evidence on the robustness of our findings. Our empirical analysis so far has focused on impacts of financial education on debt outcomes, conditional on having a credit report (that is, the intensive margin). However, as we discuss in Section II, financial education may also have an impact on the likelihood of youth having a credit report (that is, the extensive margin). We investigate this in this section. In addition, below we report how the results hold up once we correct the standard errors for multiple hypotheses testing, and show results from a falsification test. Finally, we consider the potential bias in our baseline estimates due to the 2009 CARD Act.

c.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, using a panel of states, we estimate:

$$R_{st} = \alpha_{A(s)t} + \gamma_s + \beta^X X_{st} + \sum_n (\beta^n_{post} I^n_{st}) + \varepsilon_{st}, \qquad \text{(E1)}$$

where the dependent variable, R_{st}, is the proportion of 20-29 year olds in state s in year t who have a credit report. The policy interventions are indexed by n, where $n \in \{mathematics, economics, financial\ literacy\}$. I^n_{st} 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. γ_s is a set of state fixed effects, $\alpha_{A(s)t}$ is a set of census region-year fixed effects, and ε_{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 region-year fixed effects control for aggregate region-specific time trends in the prevalence of credit reports among 20-29 year olds. Region-level time-varying controls allow us to account for changes in the macroeconomic conditions of the regions that may correlate with the enactment of the policy changes. The coefficients of interest are the β_{post}^{n}'s. To address heteroscedasticity, we cluster standard errors at the state level.[39]

Estimates of β_{post}^{n} in equation (E1) for $n \in \{mathematics, economics, financial\ literacy\}$ are presented in column 1 of Table 5. Estimates for math and economics are small in magnitude, and not statistically different from zero. On the other hand, exposure to a financial literacy education requirement leads to an increase in credit report prevalence amongst the treated youth. The coefficient, which is precisely estimated, implies an increase of 1.6 percentage points in the proportion of 20-29 year olds with credit reports. Based on our calculations, in 2013, 92.5% of 20-29 year olds in the US had credit reports. Therefore, the impact of a financial literacy education requirement is non-trivial.

To interpret the results as causal, one may worry that states that implemented reforms differ from those that did not, and that the implementation and timing of reforms may be correlated with observable and unobservable state and cohort factors. To address these concerns, E1 allows for census region-specific time trends, state fixed effects, and a rich set of time-varying state-level controls. E1, however, assumes that credit prevalence in states that implement a reform (treatment group) and those that do not (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 an additional 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:

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

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 β_{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

[39] Note that our (null) results are similar when we estimate with national, and not regional, year effects.

trend in credit prevalence amongst youth after the enactment of the policy, and would be evidence of a causal effect of the policy.

The second column of Table 5 reports estimates of $(\beta^n_{post} - \beta^n_{pre})$. We see that the estimates are very similar to the E1 estimates. This suggests that the common trends assumption may be accurate in this context. Overall, these findings indicate that the math and economic education requirements have no impact on the extensive margin, while financial literacy education requirements lead to a small (and precisely estimated) increase in the prevalence of credit reports. Since the impact of requiring financial education on the extensive margin is quite small, it is unlikely that the impacts that we find on the intensive margin (that is, the credit report outcomes) are a result of the compositional changes in credit report holders.

c.2. Multiple Testing Corrections

Our empirical analysis employs twelve 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 false hypothesis is accepted by chance alone can be quite large in such cases.[40, 41]

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 6 reports the p-value of each significant coefficient in our baseline I1 model for the pooled sample (Table 3). 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.[42] 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 ($N=12$ 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, 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

[40] Tests of the relationship between financial education and, for example, bankruptcy and number of accounts in collection are clearly not independent. The case of twelve independent tests provides an upper bound on the odds of accepting a false hypothesis.
[41] For example, if 10 hypotheses are being tested at the same time, one expects one true null hypothesis to be falsely 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 $1 - 0.9^{10} = 0.65$.
[42] See, for example, Romano, Shaikh, and Wolf (2010).

then multiplied by the number of ex-ante null hypotheses (again, $N=12$ 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 11, the third by 10, 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=12) divided by its rank (N-1, that is, 11), and so on.

Looking across the last column in Table 6 we see that, when using the Benjamini-Hochberg correction, nearly all of our estimates from the baseline specification that were found to be statistically different from zero, continue to be so at conventional levels; the only exception being risk score for math years, with an adjusted p-values of at 0.11. When using the more stringent corrections in columns (2) and (3), we do lose significance of several of the outcomes, but many remain significant at conventional levels. Hence our conclusions are robust to various multiple hypothesis testing corrections.

risk scores.

c.3. Falsification Test

As a further 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. We estimate model I1 on this placebo reform sample; estimates are presented in Table 7. 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 7 estimates, with the exception of any debt for financial literacy, having mortgage debt for math years, and student loan and credit card/auto balances for economics, the only estimates that are statistically significant have a sign that is opposite to that of the actual baseline estimates in Table 3.[43] Thus, this falsification gives us a greater degree of confidence that our baseline results capture the true effect of educational reforms rather than other (state- or time-specific) confounding effects.

c.4. The CARD Act

We conducted one more check which is not reported (but is available from the authors upon request). One potential concern is that our estimates may be biased by the Credit Card Accountability and Responsibility and Disclosure (CARD) Act of 2009, which affected provision of credit to individuals younger than 21 (see Agarwal, Chomsisengphet , Mahoney, and Stroebel (2013) and Debbaut, Ghent, and Kudlyak (2013) for discussion and evaluation of the Act). Since our identification exploits time by cohort by state variation, and the Act affects only the youngest cohorts after 2009 in a manner that varies by state, this could be an issue. While the Act was implemented in phases, the provisions of the Act that affected credit access to youth under the age of 21 took effect in early 2010. Therefore, as a sensitivity check, we re-estimate our baseline model using data through 2009 only. Estimates are qualitatively similar to those using the full sample period (Table 3), suggesting that CARD Act requirements are not biasing our estimates.

IV. Discussion and Conclusions

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

For this purpose, we use variation in finance, economics, and mathematics 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

[43] Note that the coefficient on home-secured balance for financial literacy education, which is not significant in the baseline, but significant at the 5% level in the falsification test.

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. Increased mathematics requirements, on the whole, appear to raise perceived creditworthiness, decrease reliance on certain categories of debt, and decrease both bankruptcy and delinquency.[44] 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.[45] Though our analysis includes no model with which to infer welfare responses, higher income and asset levels, in combination with lower or unchanged 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 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.

Our findings for the debt effects of financial education requirements 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 collections, decrease bankruptcies, and decrease the prevalence of auto, credit card, and housing debt balances. They at least appear to increase debt savvy, in that they increase the prevalence of credit reports 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. It is worth noting that, relative to the estimated effects of economics requirements, the effects of mathematics and financial literacy education requirements appear to dissipate with age.

---

[44] Note again, however, that the welfare implications of a bankruptcy decrease are ambiguous, particularly in light of recent insights on the passive, informal process that produces a majority of unsecured debt defaults from Drozd and Serrano-Padial (2013).

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

In marked contrast to the estimated impacts of mathematics and financial literacy education, we see that economic education leads to an increase in the likelihood of having outstanding debt, and a decline of 1.3 points, on average, in youths' creditworthiness (the sample standard deviation in risk scores is 94 points). 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. Unlike mathematics and financial literacy education, the estimated effects of economics requirements are strongest at older ages. Balance effects, repayment difficulties, and rsk score effects all seem to accumulate with age. Interestingly, all three types of quantitative training significantly delay homeownership in our sample of twentysomething Americans over a difficult period for young homeowners..

The Drexler et al. (2012) study offers one noteworthy parallel to our estimated effects by course type. Just as we find more successful debt outcomes in response to financial literacy courses (whose stated content is practical), and less successful debt outcomes in response to economics courses (with generally more abstract content), Drexler et al. see substantially better outcomes in response to rule-of-thumb financial training (compared to principles-based accounting training). It may be the case that teaching simple rules for real-world choices is most effective in curing debt problems.

Other research indicates that economic education is associated with higher income and assets.[46] Hence the net welfare effect of economic training may be unclear. While the estimated debt effects of economic education in this paper appear to have ambiguous welfare effects, they may in fact be symptomatic of changes that bring overall welfare enhancements. More economics students may experience both increased delinquency and increased asset returns, though the latter are not documented in these data. To the extent that higher debts are associated with steeper income profiles, they may also be an indication of improved welfare.

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.[47] In addition, for a given course category, the treatments implemented by states were certainly heterogeneous both at and below the state level. Our estimates merely reflect an average effect of these varied interventions.[48] Brown, Collins, Schmeiser, and Urban

[46] 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. In addition, Carter and Irons (1991) and Frank, Gilovich, and Regan (1993) show that students in fields such as Economics are less trusting, less cooperative, and more selfish.
[47] See, for example, Cole et al. (2012, 2013), Goodman (2009), and Stephens and Yang (2013).
[48] One dimension of this heterogeneity is the quality of instruction. Lusardi and Mitchell (forthcoming) and Way and Holden (2009) include helpful discussion of the quality of instruction in high school personal finance courses, and its role in the debate.

(2014) emphasize heterogeneous details of implementation, and, accounting carefully for the realized implementation paths in Georgia, Idaho, and Texas, uncover financial literacy education effects that are quite similar to what we observe at a national level. In addition, it is unclear (and difficult to measure) what uses of student time are being crowded out by each requirement, and how different these may be from state to state - in that sense, our treatment effects should be interpreted as the net effect of the financial education and the classes that are being crowded out. Further, the results presented here give little evidence of the mechanisms by which math, economics, and financial literacy requirements exert their effects on young borrowers. Given 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.

References

Agarwal, Sumit, John Driscoll, Xavier Gabaix, and David Liabson, 2009. The Age of Reason: Financial Decisions Over the Life Cycle with Implications for Regulation. *Brookings Paper on Economic Activity*, 51-117

Agarwal, Sumit, and Bhashkar Mazumder. 2013. Cognitive Abilities and Household Financial Decision Making. *American Economic Journal: Applied Economics*, forthcoming.

Agarwal, Sumit, Souphala Chomsisengphet, Neale Mahoney, and Johannes Stroebel. 2013. Regulating Consumer Financial Products: Evidence from Credit Cards. Working Paper, New York University.

Alexander, Karl. and Aaron Pallas. 1984. Curriculum Reform and School Performance: An Evaluation of the "New Basics", *American Journal of Education*, 92(4): 391-420.

Almenberg, Johann, and Anna Dreber, 2011. Gender, Financial Literacy and Retirement Preparation in the Netherlands. *Journal of Pension Economics and Finance*. 10(4): 527-545.

Altonji, Joseph, The Effect of high school Curriculum on Education and Labor Market Outcomes, *Journal of Human Resources*, 30 (3): 409-438.

Altonji, Joseph G., Erica Blom, and Costas Meghir. 2012. "Heterogeneity in Human Capitla Investmetns: High School Curriculum, College Major, and Careers," NBER working paper 17985.

Avery, Robert, Paul Calem, and Glenn Canner, 2003. An Overview of Consumer Data and Credit Reporting. The Federal Reserve Board of Governors.

Bayer, Patrick, B. Douglas Bernheim, and John Karl Scholz. 2009. The Effects of Financial Education in the Workplace: Evidence from a Survey of Employers. *Economic Inquiry*, 47(4): 605-624.

Banks, James and Zoe Oldfield. 2007. Understanding pensions: cognitive function, numerical ability and retirement saving. *Fiscal Studies*, 28(2): 143–170.

Banks, James, Cormac O'Dea, and Zoe Oldfield. 2010. Cognitive Function, Numeracy and Retirement Saving Trajectories. *Economic Journal*, 120(548): F381-F410.

Benjamin, Daniel J., Sebastian A. Brown, and Jesse M. Shapiro, 2013. Who is 'Behavioral'? Cognitive Ability and Anomalous Preferences. *Journal of the European Economics Association*, forthcoming.

Bernheim, Douglass, Daniel Garrett, and Dean Maki, 2001. Education and Saving: The long-term effects of high school financial curriculum mandates. *The Journal of Public Economics*, 80:435-465.

Bernheim, Douglass, and Daniel Garrett. 2003. The effects of financial education in the workplace: evidence from a survey of households. *Journal of Public Economics.* 87: 1487-1519.

Blinder, Alan S. and Alan B. Krueger. 2004. "What Does the Public Know About Economic Policy, and How Does it Know It?"

Bricker, Jesse, Arthur B. Kennickell, Kevin B. Moore, and John Sabelhaus, "Changes in U.S. Family Finances from 2007 to 2010: Evidence from the Survey of Consumer Finances," *Federal Reserve Bulletin,* June 2012.

Brown, Alexandra, J. Michael Collins, Maximillian Schmeiser, and Carly Urban. 2014. "State Mandated Financial Education and the Credit Behavior of the Young." Manuscript, Federal Reserve Board of Governors.

Brown, Meta, Andrew Haughwout, Donghoon Lee, Joelle Scally, and Wilbert van der Klaauw. 2013a. Just Released: Press Briefing on Household Debt and Credit. *Federal Reserve Bank of New York Liberty Street Economics Blog*, February 2013.

Brown, Meta, Andrew Haughwout, Donghoon Lee, and Wilbert van der Klaauw. 2013b. Do We Know What We Owe? A Comparison of Borrower- and Lender-Reported Consumer Debt. *Federal Reserve Bank of New York Staff Report* no. 523.

Bureau of Labor Statistics, 1999, 2000, 2001, 2002, 2003, 2004, 2005, 2006, 2007, 2008, 2009, 2010, and 2011. Local Area Unemployment Statistics. http://www.bls.gov/lau/#tables. Accessed 1 Feb 2013.

Calvet, Laurent, John Campbell, and Paolo Sodini. 2007. Down or Out: Assessing the Welfare Costs of Household Investment Mistakes. *Journal of Political Economy*, 115: 707-747.

Calvet, Laurent, John Campbell, and Paolo Sodini. 2009. Measuring the Financial Sophistication of Households. *American Economic Review, Papers and Proceedings*, 99: 393-398.

Carter, John and Michael Irons. 1991. Are Economists Different, and If So, Why? *Journal of Economic Perspectives*, 5(2): 171-177.

Choi, James, David Laibson, and Brigitte Madrian. 2011. $100 Bills on the Sidewalk: Suboptimal Investment in 401(k) Plans. *Review of Economics and Statistics*, 93(3) 748-763.

Cole, Shawn, Anna Paulson, and Gauri Shastry, 2012. Smart Money: The Effect of Education on Financial Behavior. Manuscript, Harvard Business School.

Cole, Shawn, Anna Paulson, and Gauri Shastry, 2013. High School and Financial Outcomes: The Impact of Mandated Personal Finance and Mathermatics Courses. Manuscript, Harvard Business School.

Council of Chief State School Officers, 1998, 2000, 2002, 2004, 2006, and 2008. Key State Education Policies on PK-12 Education.

Debbaut, Peter, Andra Ghent, and Marianna Kudlyak. 2013. Are Young Borrowers Bad Borrowers? Evidence from the Credit CARD Act of 2009. Working Paper.

Drexler, Alejandro, Greg Fischer and Antoinette Schoar. 2011. "Keeping it Simple: Financial Literacy and Rules of Thumb." Working Paper.

Drozd, Lukasz, and Ricardo Serrano-Padial. 2013. Modeling the Credit Card Revolution: The Role of Debt Collection and Informal Bankruptcy.

Duca, John and Anil Kumar. Forthcoming. "Financial literacy and mortgage equity withdrawals," *Journal of Urban Economics*.

Fay, Schott, Erik Hurst, and Michelle J. White. 2002. The Household Bankruptcy Decision. *American Economic Review*, 92(3): 706-718.

Federal Reserve Bank of Philadelphia. 2012. "Understanding & Improving Your Credit Score." https://www.philadelphiafed.org/consumer-resources/publications/your-credit-score.pdf (Last visited December 19, 2013.)

Ferguson, Roger. "Op-Ed: Improving Financial Literacy is Essential to Our Nation's Economic Health," *Time Magazine*, April 9, 2012.

Fernandes, Daniel, John G. Lynch, and Richard G. Netermeyer. Forthcoming. "Financial Literacy, Financial Education and Downstream Financial Behaviors." *Management Science.*

Ford, Gary, 1977. State characteristics affecting the passage of consumer education legislation. *Journal of Consumer Affairs*. 11(1):177-182.

Frank, Robert, Thomas Gilovich, and Dennis Regan. 1993. Does Studying Economics Inhibit Co- operation? *Journal of Economic Perspectives*, 7(2): 159-171.

Gartner, Kimberly and Richard M. Todd. 2005. Effectiveness of online early intervention financial education programs for credit-card holders. *Federal Reserve Bank of Chicago Proceedings*.

Gerardi, Kristopher, Lorenz Goette and Stephan Meier, 2013. Numerical Ability Predicts Mortgage Default. *Proceedings of the National Academy of Science*, forthcoming.

Goodman, Joshua. 2012. The Labor of Division: Returns to Compulsory Mathematics Coursework. Working Paper, Harvard Kennedy School.

Hastings Justine, Brigitte Madrian and William Skimmyhorn. 2013. Financial Literacy, Financial Education and Economic Outcomes. *Annual Review of Economics*, 5: 347-373.

Internal Revenue Service, 2002, 2004, 2005, 2006, 2007, 2008, and 2012. SOI Tax Stats, Individual Income Tax Statistics, ZIP Code Data. http://www.irs.gov/uac/SOI-Tax-Stats-Individual-Income-Tax-Statistics-ZIP-Code-Data-(SOI). Accessed 7 Jan 2013.

Jacob, Katy, and Rachel Schneider. 2006. Market Interest in Alternative Data Sources and Credit Scoring. *Center for Financial Services Innovation*.

Jappelli, Tullio, and Mario Padula. 2011. Investment in financial literacy and saving decisions. CFS Working Paper Series 2011/07.

Joensen Juanna., and Helena Nielsen. 2009. Is there a Causal Effect of High School Math on Labor Market Outcomes? *Journal of Human Resources*, 44(1): 171-198.

Jump Start Coalition for Personal Financial Literacy. Jump Start Coalition Mission Statement. 11 Jul 2013. http://www.jumpstart.org/mission.html

Kimball, Miles, and Tyler Shumway. 2007. Investor Sophistication and the Home Bias, Diversification, and Employer Stock Puzzles. Working Paper.

Lee, Donghoon and Wilbert van der Klaaw, 2010. An Introduction to the FRBNY Consumer Credit Panel, *Federal Reserve Bank of New York Staff Reports*, no. 479.

Lusardi A. 2004. Saving and the effectiveness of financial education. In *Pension Design and Structure: New Lessons from Behavioral Finance*, ed. O Michell, S Utkus. pp.157- 184. New York: Oxford Univ.

Lusardi, Annamaria, and Peter Tufano. 2009. Debt Literacy, Financial Experiences, and Over-indebtedness. *NBER Working Paper Series*, 14808.

Lusardi, Annamaria. 2011. Americans' Financial Capability. *NBER Working Paper Series,* 17103.

Lusardi, Annamaria, and Olivia S. Mitchell. 2011. Financial Literacy and Planning: Implications for Retirement Wellbeing. In Financial Literacy: Implications for Retirement Security and the Financial Marketplace. Eds. O. S. Mitchell and A. Lusardi. Oxford, Oxford University Press: 17-39.

Lusardi, Annamaria, and Olivia Mitchell, forthcoming. The Economic Importance of Financial Literacy: Theory and Evidence. *Journal of Economic Literature.*

Lusardi, Annamaria, and Carlo de Bassa Scheresberg, 2012. Financial Literacy and High-Cost Borrowing in the United States. Working Paper, 2012 APPAM Fall Research Conference.

Markow, Dana, and Kelly Bagnaschi. 2005. What American Teens & Adults Know About Economics. *National Council on Economic Education Report*.

Mayer, Robert, 1989. The Consumer Movement: Guardians of the Marketplace. Twayne Publishers, Boston: MA.

Mian, Atif and Amir Sufi. 2011. "House Prices, Home Equity-Based Borrowing, and the US Household Leverage Crisis." *American Economic Review*, 101 (august 2011): 2132-2156.

National Council on Economics Education, 1998, 2000, 2002, 2005, 2007, 2009, and 2011. Survey of the States: Economic and Personal Finance Education in Our Nation's Schools.

Romano, Joseph, Azeem Shaikh, and Michael Wolf (2010). Hypothesis Testing in Econometrics. *Annual Review of Economics*, Vol. 2, p. 75-104.

Rose, Heather. and Julian Betts (2004), The Effect of high school Courses on Earnings, *The Review of Economics and Statistics*, 86(2): 497-513.

Servon, Lisa, and Robert Kaestner. 2008. Consumer Financial Literacy and the Impact of Online Banking on the Financial Behavior of Lower-Income Bank Customers. *Journal of Consumer Affairs*, 42: 271–305.

Shim, Soyeon, Bonnie Barber, Noel Card, Jing Xiao and Joyce Serido. 2010. Financial Socialization of First-year College Students: The Role of Parents, Work, and Education. Journal of Youth & Adolescence 39(12): 1457-1470.

Skimmyhorn, William. 2013. Assessing Financial Education: Evidence from a Personal Financial Management Course. Working Paper.

Smith, James, John, McArdle, and Robert Willis. 2010. Financial Decision Making and Cognition in a Family Context. The Economic Journal, 120(548): F363-F380.

Stango, Victor, and Jonathan Zinman. 2009. Exponential Growth Bias and Household Finance. *Journal of Finance*, 64(6): 2807-2849.

Stephens, Melvin, and Dou-Yan Yang. 2013. "Compulsory Education and the Benefits of Schooling." NBER Working Paper No. 19369.

Surowiecki, James. "Greater Fools," *The New Yorker*, July 5, 2010.

Treasury Department. 2013. Remarks of Secretar Lew before the Financial Literacy Education Commission (FLEC). May 14, 2013. http://www.treasury.gov/resource-center/financial-education/Documents/Lew%20Remarks%20May%2014%202013.pdf (Last visited September 12, 2013.)

United States Census Bureau, 2002, 2004, 2005, 2006, 2007, 2008, 2009, and 2010. State & Local Government Finance. http://www.census.gov/govs/estimate/historical_data.html. Accessed 7 Jan 2013.
United States Department of the Treasury. Financial Literacy and Education Commission. 11 Jun 2013. http://www.treasury.gov/resource-center/financial-education/Pages/commission-index.aspx

van Rooij, Maarten, Annamaria Lusardi, and Rob Alessie. 2007. Financial Literacy and Stock Market Participation, Michigan Retirement Research Center Research Paper No. 2007-162.

Way, Wendy and Karen Holden. 2009. "Teachers' Background and Capacity ot Teach Personal Finance: Results of a National Study." *Journal of Financial Counseling and Planning* 20(2):64-78.

Figure 1: ES1 estimates (Financial Literacy Reforms)

(Source: FRBNY Consumer Credit Panel/Equifax)

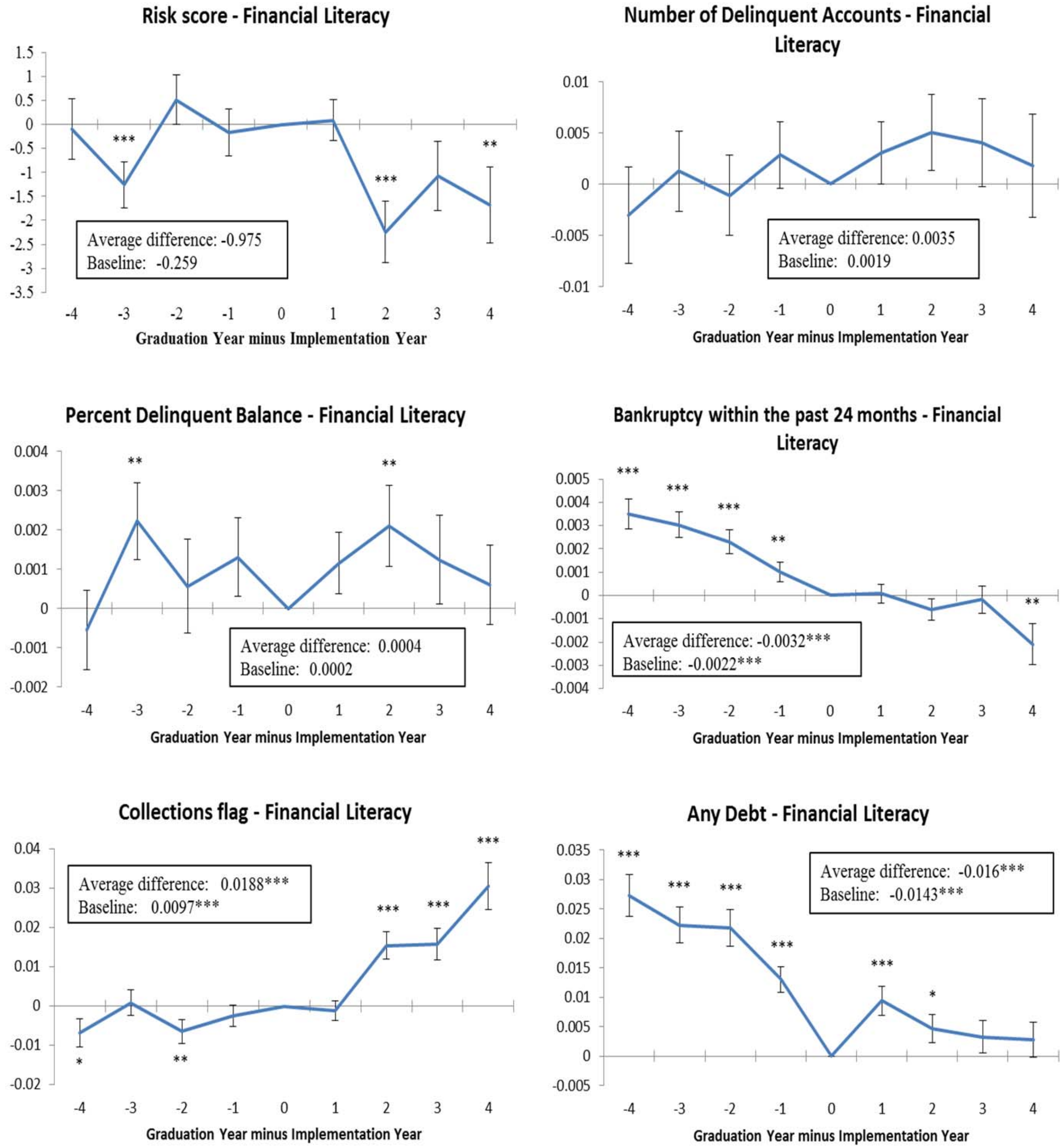

Figure 1: ES1 estimates (Financial Literacy Reforms) - continued

(Source: FRBNY Consumer Credit Panel/Equifax)

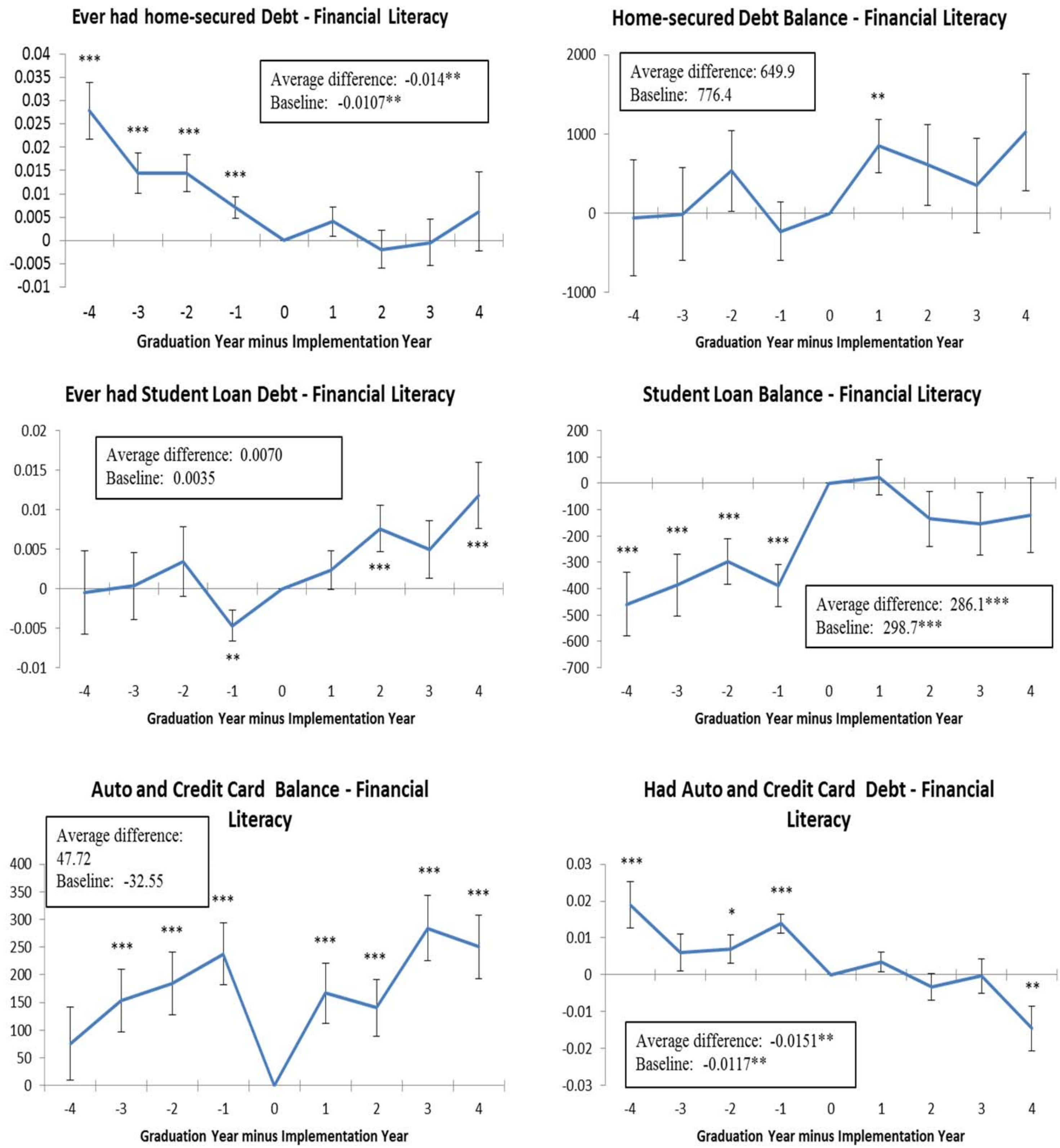

Figure 2: ES1 estimates (Economics Reforms)
(Source: FRBNY Consumer Credit Panel/Equifax)

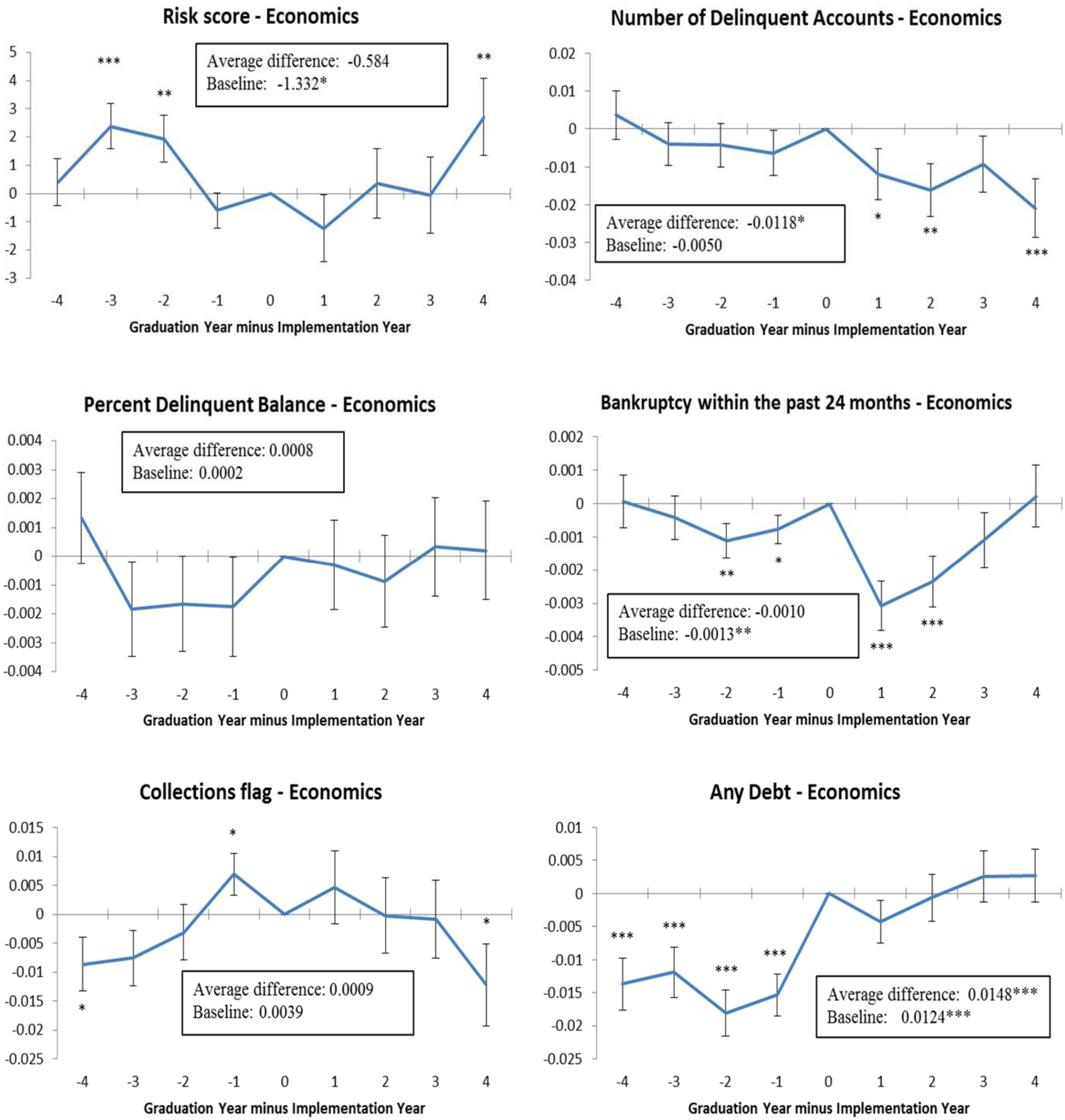

Figure 2: ES1 estimates (Economics Reforms) - continued

(Source: FRBNY Consumer Credit Panel/Equifax)

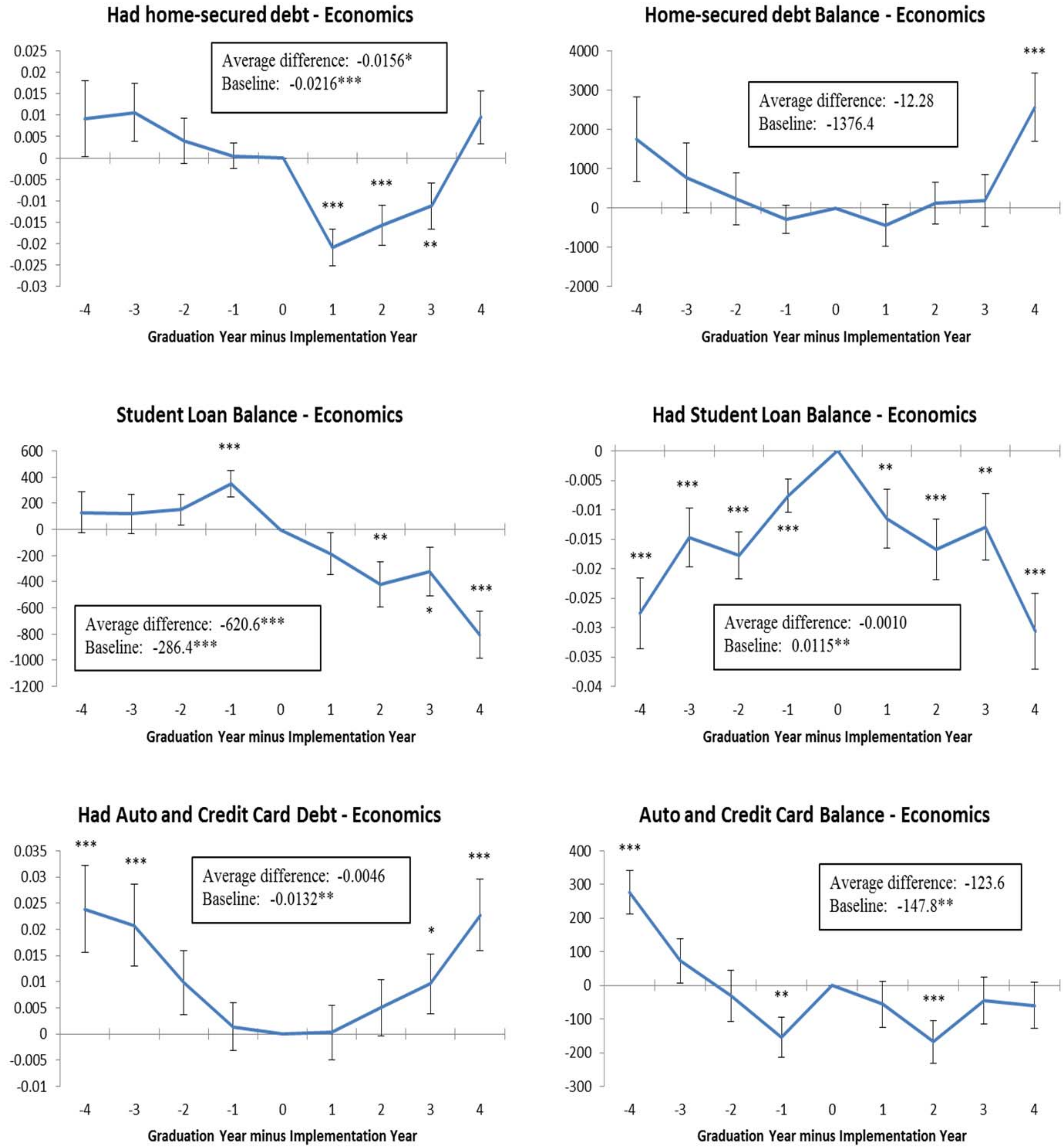

Table 1: Education policy reforms by state

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

continued….

Table 1 continued….

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

* 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: Summary statistics for the estimation sample

Variable	N	Mean	SD	Min	Median	Max	Zeros
Outcome Variables							
Risk Score	5,164,684	628.08	93.67	280	641	840	0.00%
Number of Delinquent Accounts	5,589,422	0.181	0.716	0	0	33	89.67%
Percent of Balance in Delinquent Accounts	5,589,422	5.65%	20.94%	0%	0%	100%	89.71%
Bankruptcy within past 24 months	5,554,390	0.006	0.074	0	0	1	99.45%
Collections flag	5,554,237	0.402	0.490	0	0	1	59.85%
Any Debt	5,589,422	0.762	0.426	0	1	1	23.75%
Ever Had Home-Secured Debt	5,589,422	0.090	0.286	0	0	1	91.01%
Home-Secured Debt Balance	5,589,422	$11,400.74	$51,029.89	$0	$0	$ 6,251,332	92.52%
Had Auto/Credit Card Debt	5,589,422	0.784	0.412	0	1	1	21.60%
Auto/Credit Card Balance	5,589,422	$ 5,812.96	$12,149.86	$0	$803	$ 9,615,548	33.32%
Had Student Loan Debt	5,589,422	0.322	0.467	0	0	1	67.81%
Student Loan Balance	5,589,422	$ 5,104.12	$15,646.60	$0	$0	$ 622,272	72.54%
Education Reform-Related Variables							
Went to HS before state enacted Econ reform	5,589,422	0.146	0.353	0	0	1	85.39%
Exposed to Econ Reform Only	5,589,422	0.424	0.494	0	0	1	57.61%
Went to HS before state enacted Fin Lit reform	5,589,422	0.226	0.418	0	0	1	77.44%
Exposed to Financial Literacy Reform Only	5,589,422	0.056	0.231	0	0	1	94.35%
Exposed to Both Fin Lit and Econ Reforms	5,589,422	0.104	0.306	0	0	1	89.56%
Went to HS before state enacted Math reform	5,589,422	0.278	0.448	0	0	1	72.15%
Exposed to Math Reform	5,589,422	0.324	0.468	0	0	1	67.59%
State # of years of math required to graduate	5,589,422	2.648	0.633	0	3	4	0.19%
Control Variables							
Zip-code Income Per Capita ($Thousands)	5,589,422	32.9	27.2	0.0	26.8	1,252.9	0.03%
State Educational Spending per capita	5,589,422	2,935.9	900.7	0.0	2,756.9	11,030.3	0.24%
County-level Unemployment Rate	5,589,422	7.487	2.868	0.7	7.13333	37.0	0.00%
# of years of state compulsory schooling	5,589,422	10.258	0.800	8	10	11	0.00%
State grad requirement: # of years of Social Stud	5,589,422	2.589	0.691	0.5	3	4	0.00%
State graduation requirement: # of years of Engli	5,589,422	3.720	0.513	1	4	4	0.00%
State graduation requirement: # of years of Scier	5,589,422	2.507	0.691	1	3	4	0.00%
Gross State Product ($Thousands)	5,589,422	643,792	563,472	22,471	421,259	1,980,601	0.00%
Birth Year	5,589,422	1985.2	3.124	1981	1985	1994	0.00%

*2% panel of Equifax CCP, Q4 of years 1999-2012, individuals born after 1980. Source of outcome variables: FRBNY Consumer Credit Panel/Equifax

Table 3: I1 (Baseline) Model Estimates, for Pooled Sample

	Risk Score	Number of Delinquent Accounts	Percent of Balance Delinquent	Bankruptcy within past 24 months	Collections Flag	Any Debt	Ever Had Mortgage Debt	Home-secured Balance	Ever Had Student Loans	Student Loan Balance	Had Auto + Credit Card Debt	Auto + Credit Card Balance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Math years	0.526**	-0.0015	-0.0010**	-0.0006*	0.0007	-0.0002	-0.0069**	217.8	0.0066***	56.81	-0.0089***	23.42
	(0.262)	(0.0019)	(0.0004)	(0.0003)	(0.0020)	(0.0011)	(0.0031)	(360.3)	(0.0019)	(46.41)	(0.0025)	(34.34)
Fin Lit Reform	-0.259	0.0019	0.0002	-0.0022***	0.0097***	-0.0143***	-0.0107**	776.4	0.0035	298.7***	-0.0117**	-32.56
	(0.492)	(0.0030)	(0.0007)	(0.0005)	(0.0029)	(0.0026)	(0.0048)	(589.6)	(0.0038)	(75.24)	(0.0047)	(44.84)
Economics Reform	-1.332*	-0.0050	0.0002	-0.0013**	0.0039	0.0124***	-0.0216***	-1376.4	0.0115**	-286.4***	-0.0132**	-147.8**
	(0.728)	(0.0051)	(0.0012)	(0.0006)	(0.0039)	(0.0034)	(0.0067)	(865.9)	(0.0048)	(101.7)	(0.0065)	(63.23)
N	5164684	5589422	5589422	5554390	5554237	5589422	5589422	5589422	5589422	5589422	5589422	5589423
Mean of Dep Var	628.1	0.181	0.0565	0.0055	0.402	0.762	0.0899	11400.7	0.322	5104.1	0.784	5813.1
Std Dev of Dep Var	93.67	0.716	0.209	0.0742	0.490	0.426	0.286	51029.9	0.467	15646.6	0.412	12149.10

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 4: Model I1 Estimates, by Age

	Risk Score	Number of Delinquent Accounts	Percent of Balance Delinquent	Bankruptcy within past 24 months	Collections Flag	Any Debt	Ever Had Mortgage Debt	Home-secured Balance	Ever Had Student Loans	Student Loan Balance	Had Auto + Credit Card Debt	Auto + Credit Card Balance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Mathematics												
22 year-olds	0.579	-0.0006	0.0002	-0.0007*	-0.0061*	-0.0023	-0.0029**	-202.0	0.0020	115.6	-0.0090***	-63.58
	(0.529)	(0.0028)	(0.0010)	(0.0004)	(0.0032)	(0.0022)	(0.0011)	(158.5)	(0.0041)	(123.4)	(0.0031)	(61.57)
25 year-olds	0.094	0.0040	-0.0002	-0.0011*	-0.0080**	0.0012	-0.0028	218.1	0.0078*	457.6***	-0.0010	7.706
	(0.904)	(0.0061)	(0.0014)	(0.00062)	(0.0040)	(0.0025)	(0.0031)	(719.8)	(0.0040)	(169.0)	(0.0026)	(81.50)
28 year-olds	-1.257	0.0111	0.0025	-0.00004	-0.0059	0.0058*	0.0026	1027.6	0.0077*	528.7**	0.0027	140.7
	(1.116)	(0.0084)	(0.0019)	(0.00101)	(0.0052)	(0.0035)	(0.0039)	(1113.9)	(0.0045)	(268.3)	(0.0034)	(133.2)
Financial Literacy												
22 year-olds	-0.538	-0.0067	-0.0044**	-0.0008	0.0168***	-0.0159***	-0.0049**	-24.68	0.0017	17.63	-0.0185***	-215.5
	(1.118)	(0.0053)	(0.0019)	(0.0006)	(0.0061)	(0.0035)	(0.0024)	(241.5)	(0.0067)	(214.8)	(0.0056)	(134.7)
25 year-olds	-0.192	0.0089	0.0003	-0.0010	-0.0033	0.0005	-0.0082	689.8	-0.0003	281.7	-0.0052	61.98
	(1.288)	(0.0083)	(0.0021)	(0.0013)	(0.0066)	(0.0063)	(0.0052)	(1051.5)	(0.0128)	(332.3)	(0.0037)	(176.2)
28 year-olds	-1.900	0.0143	0.0026	-0.0004	0.0068	0.0040	0.0014	231.7	0.0015	638.1*	-0.0033	74.52
	(1.839)	(0.0124)	(0.0034)	(0.0016)	(0.0082)	(0.0059)	(0.0059)	(1116.1)	(0.0070)	(355.9)	(0.0036)	(204.3)
Economics												
22 year-olds	-2.180*	0.0087	-0.0009	-0.0001	0.00028	0.0028	-0.0001	-268.4	0.0079	97.79	-0.0042	39.31
	(1.306)	(0.0064)	(0.0021)	(0.0006)	(0.0061)	(0.0049)	(0.0028)	(418.2)	(0.0085)	(314.9)	(0.0057)	(118.6)
25 year-olds	-6.805***	0.0246	0.0105***	0.0030*	0.0251***	-0.0026	0.0028	2806.7***	0.0072	-331.5	0.0056	355.3*
	(1.896)	(0.0175)	(0.0037)	(0.0018)	(0.0076)	(0.0083)	(0.0062)	(905.4)	(0.0141)	(345.3)	(0.0053)	(206.4)
28 year-olds	-10.34***	0.0370*	0.0173***	-0.0023	0.0285***	0.0015	-0.0051	2342.7*	0.0102	-87.85	-0.0003	206.3
	(2.005)	(0.0218)	(0.0036)	(0.0025)	(0.0092)	(0.0117)	(0.0044)	(1396.4)	(0.0092)	(396.3)	(0.0060)	(265.7)
Number of obs												
22 year-olds	600110	651121	651121	647337	646892	651121	651121	651121	651121	651121	651121	651122
25 year-olds	509427	550278	550278	548839	548271	550278	550278	550278	550278	550278	550278	550279
28 year-olds	328103	353658	353658	353002	352428	353658	353658	353658	353658	353658	353658	353659
Dep. Var. Mean												
22 year-olds	623.0	0.163	0.0531	0.0033	0.386	0.760	0.0354	3868.1	0.304	4407.0	0.759	4998.9
25 year-olds	628.3	0.211	0.0598	0.0071	0.486	0.773	0.117	15112.9	0.343	6354.1	0.851	7174.1
28 year-olds	640.0	0.228	0.0640	0.0115	0.495	0.775	0.224	29280.2	0.373	7238.6	0.895	8104.1
Dep. Var. Std Dev												
22 year-olds	92.08	0.620	0.203	0.0578	0.487	0.427	0.185	29850.9	0.460	11871.9	0.428	15492.6
25 year-olds	96.79	0.811	0.213	0.0839	0.500	0.419	0.322	58461.2	0.475	18150.8	0.356	12809.6
28 year-olds	99.75	0.891	0.222	0.107	0.500	0.418	0.417	79020.0	0.484	22103.5	0.306	14299.6

All regressions include high school-state 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 5: Impact of Financial Education on the Extensive Margin

	Baseline	Event Study
	(1)	(2)
Financial Literacy Reform	0.0164**	0.0185**
	(0.0070)	(0.0108)
Economics Reform	-0.0003	0.0037
	(0.0090)	(0.0139)
Math years Reform	0.0017	0.0020
	(0.0071)	(0.0233)
N	594	594
ymean	0.595	0.595
ysd	0.304	0.304

Dependent variable is the proportion of 20-29 year olds in a state-year. All regressions include region * year fixed effects, and standard errors clustered at the state level. Standard errors reported in parentheses. ***, **, * denote significance at the 1, 5, and 10% level, respectively. Coefficients in Column 2 reflect difference between pre- and post-reform dummies. Source: FRBNY Consumer Credit Panel/Equifax

Table 6: Adjusted p-values for I1 Model (pooled sample) Estimates

Rank	Variable	Standard	Bonferroni	Bonferroni-Holm	Benjamini-Hochberg
Mathematics Years					
1	Student Loan Balance	0.001	0.01	0.01	0.01
2	Had auto/credit card debt	0.001	0.01	0.01	0.01
3	Percent of balance delinquent	0.021	0.25	0.19	0.08
4	Had home-secured debt	0.026	0.31	0.21	0.08
5	Risk score	0.045	0.54	0.32	0.11
6	Bankruptcy within last 24 months	0.051	0.61	0.31	0.10
Financial Literacy Reform					
1	Bankruptcy within last 24 months	0.000	0.00	0.00	0.00
2	Any Debt	0.000	0.00	0.00	0.00
3	Student Loan Balance	0.000	0.00	0.00	0.00
4	Collections Flag	0.001	0.01	0.01	0.00
5	Percent of Balance delinquent	0.020	0.24	0.14	0.05
6	Home-secured balance	0.027	0.32	0.16	0.05
Economics Reform					
1	Any Debt	0.000	0.00	0.00	0.00
2	Had home-secured debt	0.001	0.01	0.01	0.01
4	Student loan balance	0.005	0.06	0.04	0.02
5	Had student loan debt	0.017	0.20	0.12	0.04
6	Auto/Credit card balance	0.020	0.24	0.12	0.04
7	Bankruptcy within last 24 months	0.028	0.34	0.14	0.05
8	Risk score	0.068	0.82	0.27	0.10

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

Table 7: Falsification Test (based on model I1)

	Risk Score	Number of Delinquent Accounts	Percent of Balance Delinquent	Bankruptcy within past 24 months	Collections Flag	Any Debt	Ever Had Mortgage Debt	Home-secured Balance	Ever Had Student Loans	Student Loan Balance	Had Auto + Credit Card Debt	Auto + Credit Card Balance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Math Years	-1.936	-0.0001	-0.0011	0.0016*	0.0022	0.0015	-0.0082**	-391.3	0.0061	-112.3*	0.0022	-102.3
	(1.197)	(0.0041)	(0.0015)	(0.0008)	(0.0048)	(0.0040)	(0.0036)	(585.1)	(0.0056)	(66.52)	(0.0068)	(141.5)
Fin Lit Reform	1.338	-0.0099	-0.0054***	-0.0005	-0.0071	-0.0124**	0.0029	1587.0**	-0.0035	104.7	-0.0064	-283.4
	(1.058)	(0.0078)	(0.0020)	(0.0010)	(0.0085)	(0.0051)	(0.0056)	(760.9)	(0.0081)	(133.6)	(0.0080)	(198.8)
Economics Reform	-0.842	-0.0036	-0.0021	-0.0012	0.0075	0.0059	0.00003	-951.1	0.0118	-325.7***	-0.0058	-236.2*
	(1.096)	(0.0044)	(0.0017)	(0.0011)	(0.0071)	(0.0047)	(0.00616)	(812.9)	(0.0096)	(106.9)	(0.0069)	(138.7)
N	345936	382726	382726	379396	380439	382726	382726	382726	382726	382726	382726	382726
Mean of Dep Var	618.8	0.159	0.0541	0.0066	0.353	0.746	0.0570	5999.4	0.222	2444.3	0.748	5679.2
Std Dev od Dep Var	89.35	0.590	0.204	0.0808	0.478	0.435	0.232	33759.0	0.416	8234.4	0.434	18985.8

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 A1: Mobility of young credit report holders

Age	% of individuals living in the same state as at age 18
19	98.87%
20	97.33%
21	95.75%
22	93.72%
23	91.27%
24	89.00%
25	87.18%
26	85.55%
27	84.18%
28	83.01%
29	82.06%

Source: FRBNY Consumer Credit Panel

Figure A1: Plots of Financial Literacy Reform Effects by Age

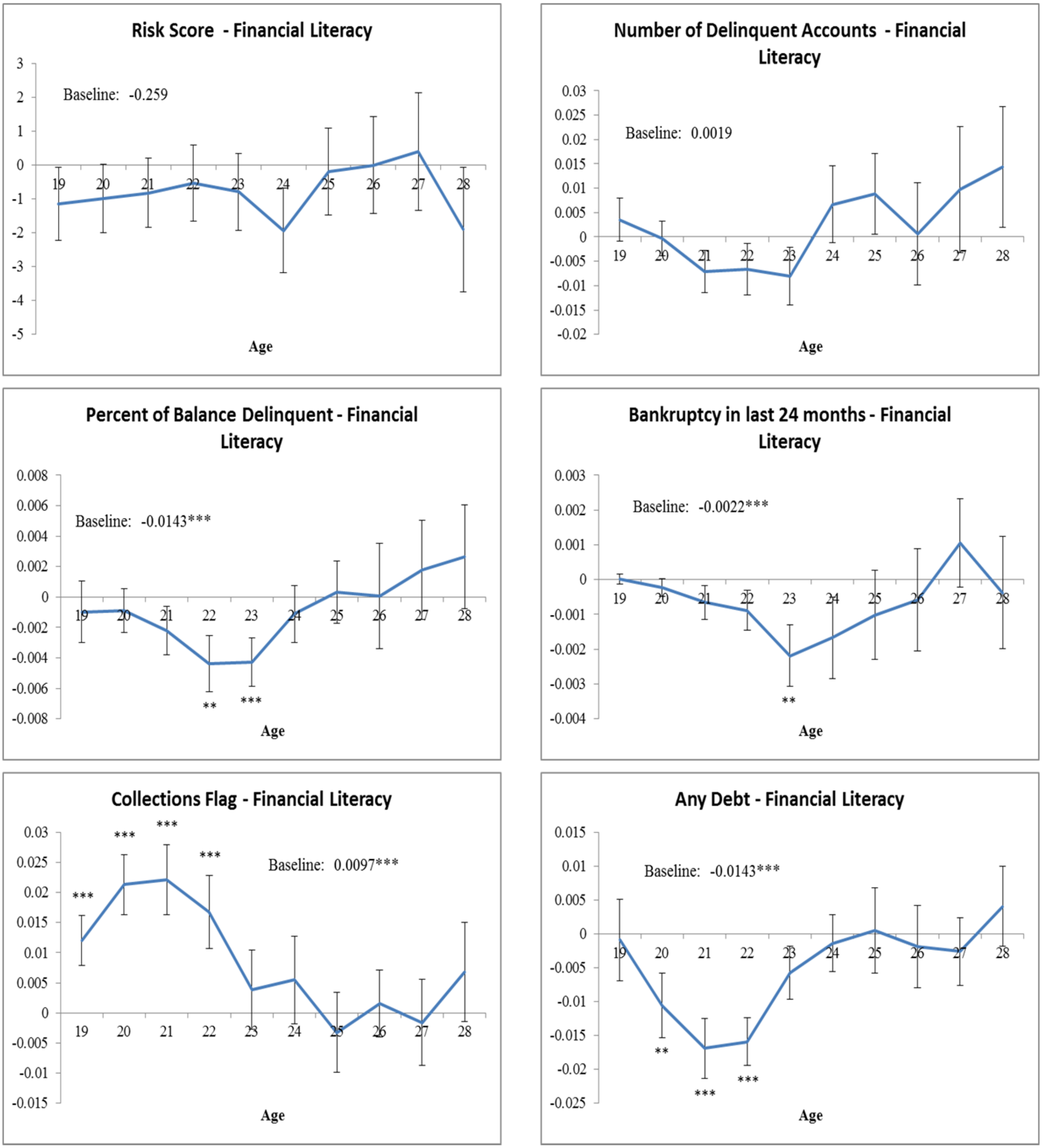

Figure A1: Plots of Financial Literacy Reform Effects by Age – Continued

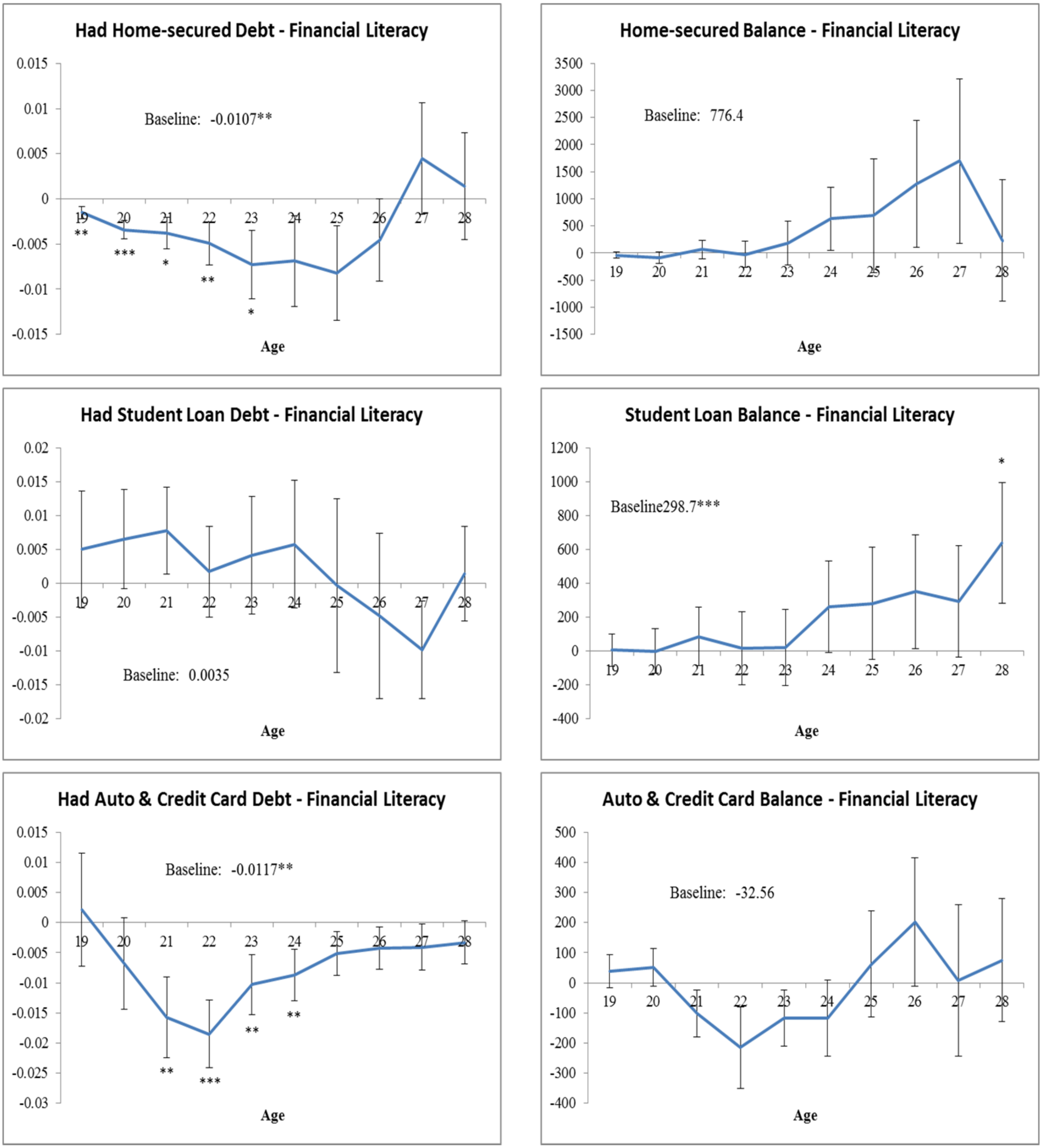

Figure A2: Plots of Economics Reform Effects by Age

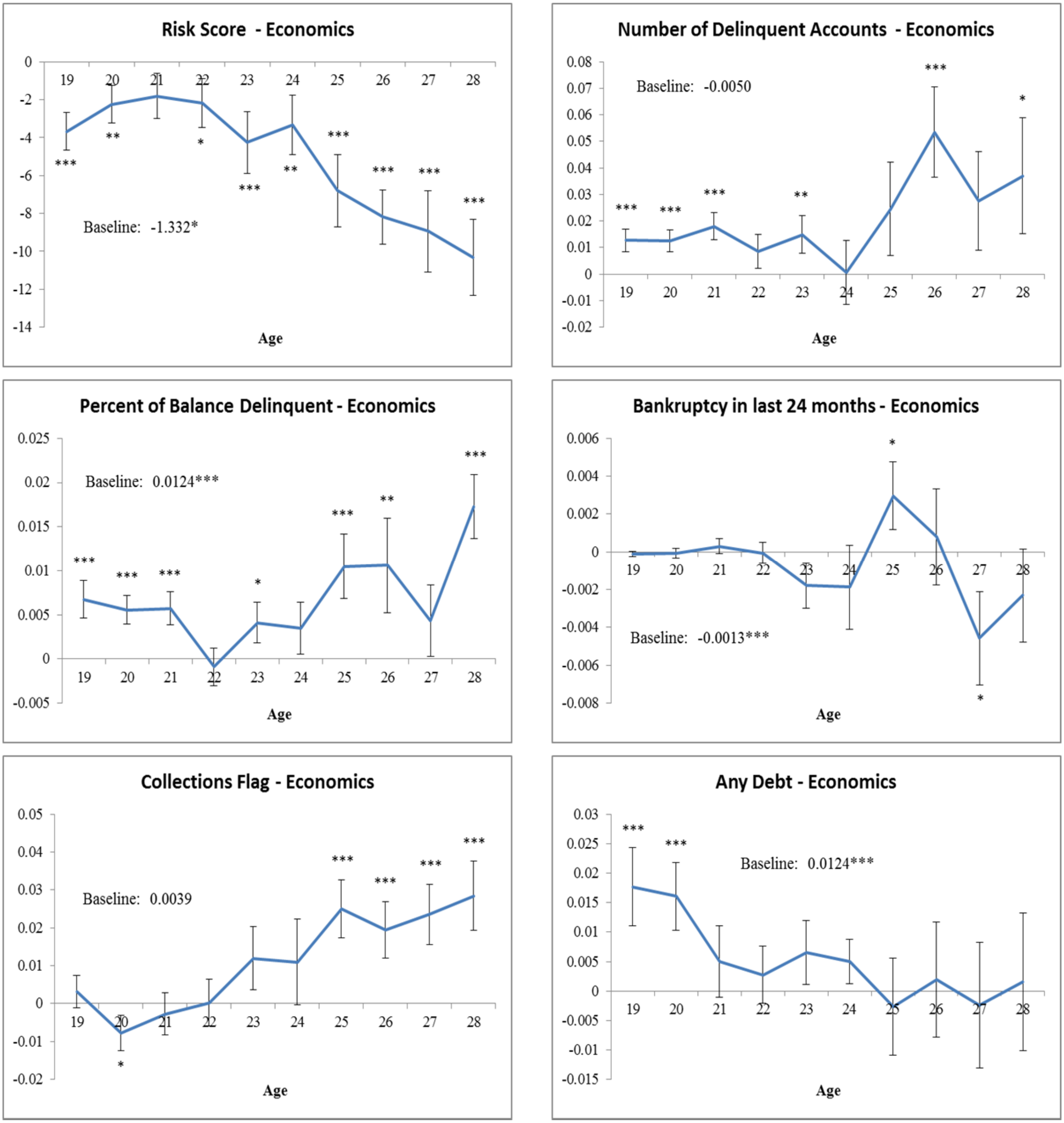

Figure A2: Plots of Economics Reform Effects by Age - Continued

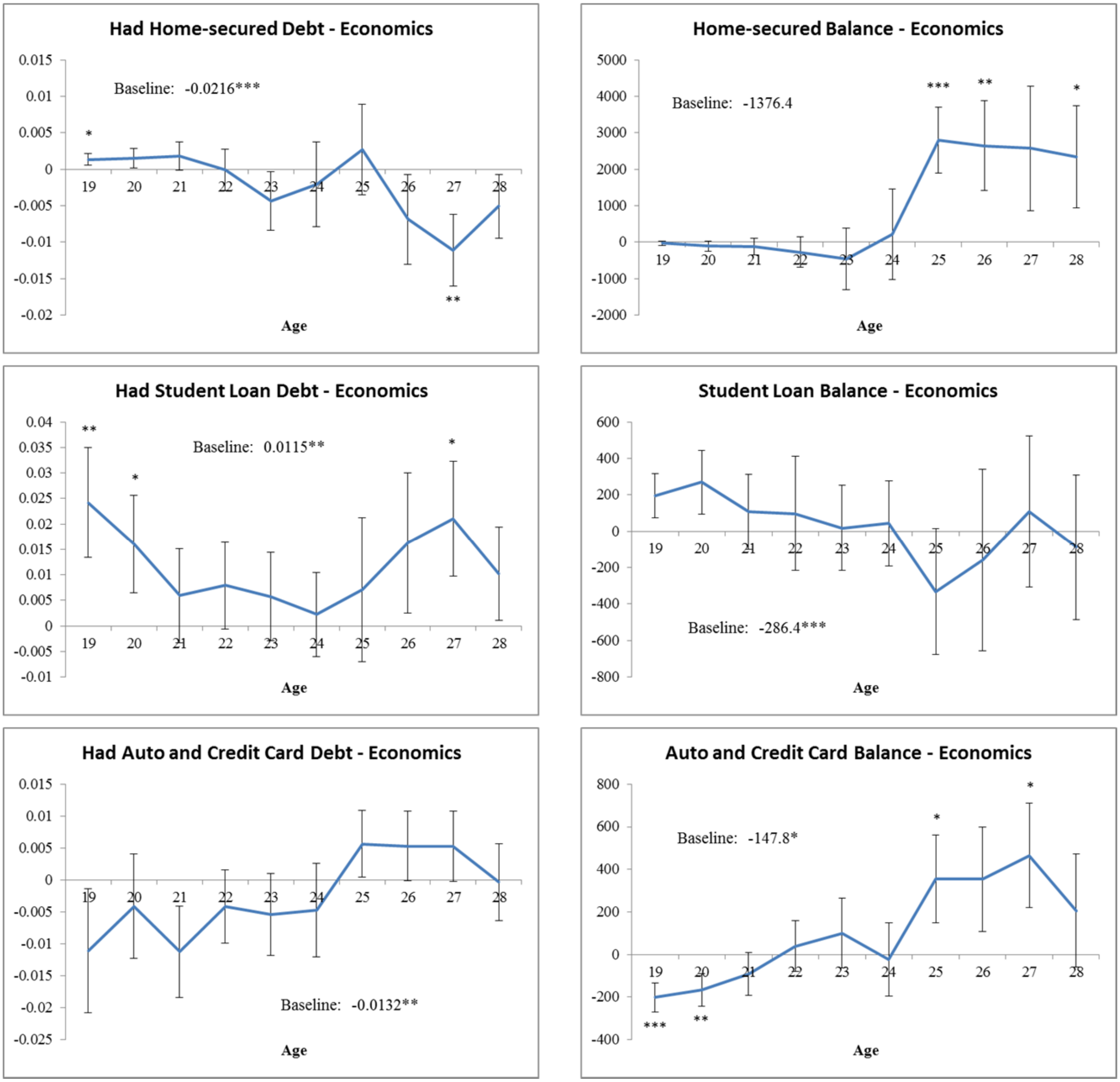

Figure A3: Mathematics Event Study Plots

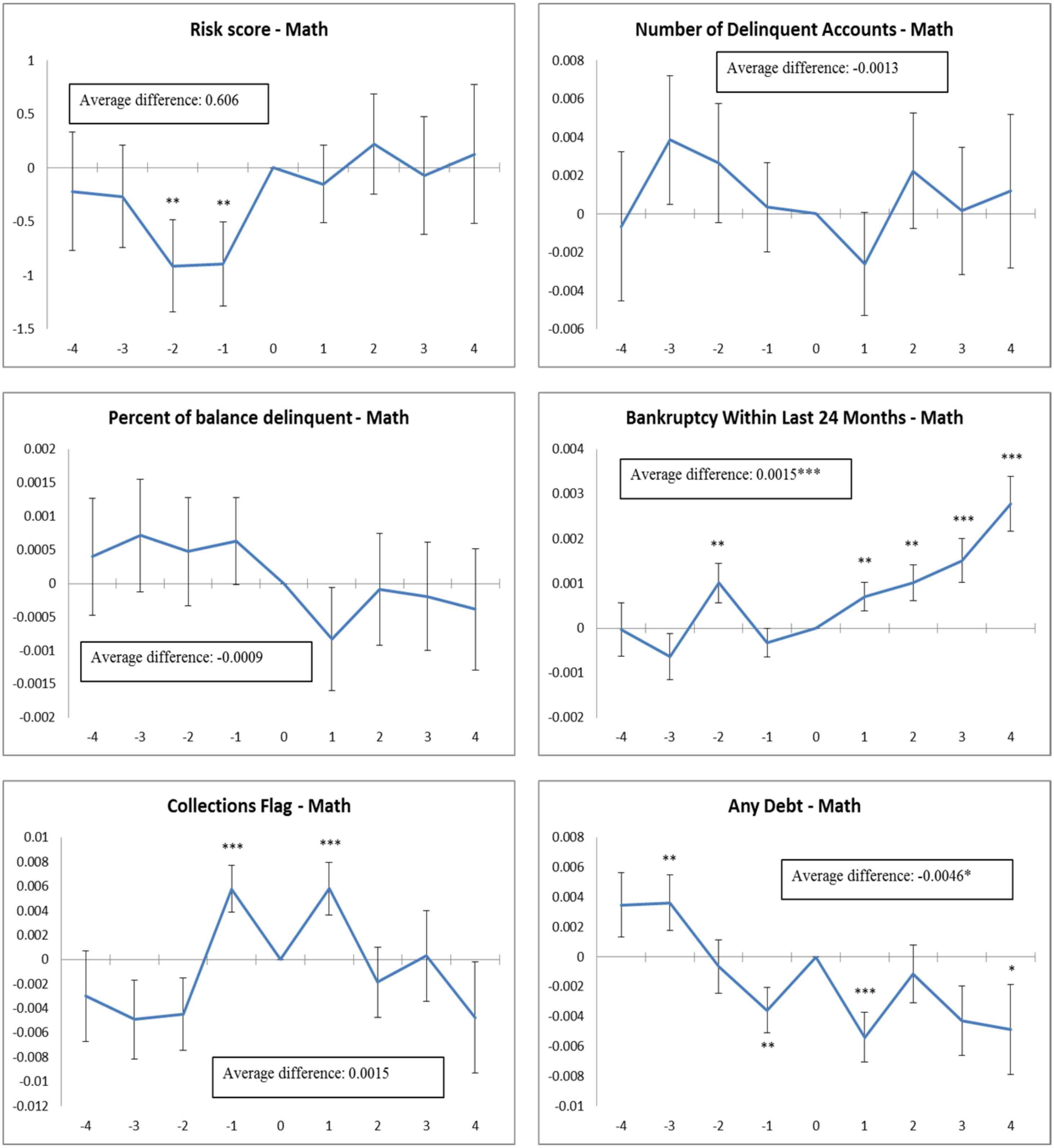

Figure A3: Mathematics Event Study Plots – Continued