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**State Mandated Financial Education and the Credit Behavior of  
Young Adults**

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# State Mandated Financial Education and the Credit Behavior of Young Adults<sup>\*,†</sup>

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## Abstract

In the U.S., a number of states have mandated personal finance classes in public school curricula to address perceived deficiencies in financial decision-making competency. Despite the growth of financial and economic education provided in public schools, little is known about the effect of these programs on the credit behaviors of young adults. Using a panel of credit report data, we examine young adults in three states where personal financial education mandates were implemented in 2007: Georgia, Idaho, and Texas. We compare the credit scores and delinquency rates of young adults in each of these states pre- and post-implementation of the education to those of students in a synthetic control state and then bordering states without financial education. We find that young people who are in school after the implementation of a financial education requirement have higher relative credit scores and lower relative delinquency rates than those in control states.

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# 1 Introduction

A growing body of literature shows a correlation between an individual's level of financial knowledge and his financial behaviors. Lower levels of measured financial literacy are associated with lower rates of planning for retirement, lower rates of asset accumulation, lower participation in the stock market, higher rates of using alternative financial services and higher levels of debt (Lusardi and Mitchell 2014, 2007; Lusardi, Mitchell and Curto 2010; Lusardi and Tufano 2009; Meier and Sprenger 2010; van Rooij, Lusardi and Alessie 2012). One group of particular concern is young adults, as they have been shown to have particularly low levels of financial literacy (Lusardi, Mitchell and Curto 2010) and to be prone to engage in high-cost credit, such as using payday loans, paying interest on credit card balances, and accruing late fees (FINRA Investor Education Foundation 2013).

The complexity of personal financial decisions facing American consumers has prompted an increased emphasis amongst policymakers on promoting financial education at all stages of life. The 2008 financial crisis further galvanized public sentiment to improve financial literacy, as demonstrated by the launching of Presidential advisory boards, governor's councils, and state and federal commissions on financial literacy. The rationale for this emphasis on financial literacy is that better informed consumers might engage in more prudent financial behaviors and avoid behaviors that could trigger broader economic problems. However, the existing body of research on the effectiveness of financial literacy education has yielded limited evidence that it changes financial behaviors or outcomes (Fernandes, Lynch and Netemeyer 2014; Willis 2011).

Even in the absence of empirical support, policymakers at the state level have expanded and strengthened personal finance and economic education requirements for K-12 students.<sup>1</sup> Economic education has been taught in K-12 public schools in the U.S. since the 1950s. In the last decade, more personal financial management topics have been added to economic education curricula, and personal financial management is being taught to

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<sup>1</sup>Personal finance and economic education are similar, but have different types of applications. We use the terms separately unless referring to the general field of education in this area.

students at more grade levels. Given the scarcity of educational time and resources, the expansion of financial education in the K-12 curriculum could come at the expense of other valuable education topics. Schools have a limited amount of instructional time available, and thus the opportunity costs of adding financial educational content may be high.

Prior studies of state financial education requirements are instructive, but also highlight the challenges of estimating causal effects of curricular changes (Bernheim, Garrett and Maki 2001; Brown et al. 2013; Cole, Paulson and Shastry 2013; Tennyson and Nguyen 2001). Researchers often estimate the effect of state mandates by comparing states with ‘financial education’ to those with ‘no financial education’ despite significant heterogeneity in the course content, course requirements, and implementation across states. Some states offer intensive financial education programs that require multiple courses and performance testing. Other states may simply *recommend* that schools offer some form of instruction on personal finance, but have no graduation or testing requirements on these topics. By combining more rigorous mandates and weaker recommendations, the estimation of the ‘average’ state mandate effect on student outcomes could be biased towards finding no effect. Moreover, mandates for financial education might be enacted at the same time as broader school reforms, or shifts in economic conditions, exacerbating the aggregation problem across time and geography.

Another difficulty is that the timing and quality of implementation of the classroom financial education is often unclear. Political and bureaucratic challenges may result in a time lapse between the enactment of a personal financial education mandate and the first year a graduating class is subject to the requirement. This is in addition to the years it may take schools across the state to have a well-developed curriculum in place and teachers prepared to teach the material. Studies often use the passage of a mandate as the start date for exposure to financial education, rather than the date of implementation. This again could result in understated estimates of the effectiveness of the education.

This paper focuses on analyzing the effects of financial education mandates in three specific states on the credit behavior of young adults. Georgia, Idaho, and Texas each

implemented mandated personal finance course requirements in 2007. These states have well-documented and specific requirements, and there were no other curriculum changes in these states at this time. We also identify other states with no such personal finance mandates or comparable curricular shifts in the same time period. This provides an opportunity to estimate more precise effect of the financial education mandate that is specific to each state’s particular education program, rather than an aggregate of across states with heterogeneous financial education approaches.

We use two methods to estimate the effects of these financial education mandates on credit outcomes. First, we use a synthetic control methods as in Abadie, Diamond and Hainmueller (2010) and Abadie and Gardeazabal (2003) to create a weighted set of comparison states using trends in state-level demographic characteristics measured prior to the imposition of financial mandates. This approach allows us to compare each ‘treated’ state (Georgia, Idaho, and Texas) to other states that never implemented any form of personal finance education (and also had no other relevant changes in mathematics or economics requirements). Second we use a border-state approach. This allows us to compare geographically homogeneous areas. In both methods we use a difference-in-difference framework to compare the change in financial outcomes for students likely exposed to financial education in high school before and after implementation to the change in outcomes for students in comparison states without mandated financial education. We measure credit behavior using individual-level credit bureau data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP) dataset.

Our results suggest that in each of these three states, students exposed to financial education mandates had higher credit scores and lower delinquency rates through age 22 when compared to those in the control states relative to these differences before the mandate. The magnitude of these effects increases with each additional year after the mandate was initially implemented, suggesting these is important—and often unmeasured—heterogeneity in implementation. We find that a student exposed to the personal finance education three years following implementation had on average a 29 point

higher credit score in Georgia, 7 point higher credit score in Idaho, and a 13 point higher credit score in Texas (or in effect sizes relative to standard deviations, these suggest a  $0.33\sigma$ ,  $0.08\sigma$ , and  $0.15\sigma$  effect, respectively). We further find reductions in the incidence of 90 plus day delinquency of 3.6 percentage points in Georgia, 1 percentage point in Idaho, and 3.3 percentage points in Texas for those exposed to the education three years post-implementation. If we alter the timing of ‘implementation’ to the year the mandate was passed rather than when it was actually implemented in the schools, the magnitude of the results falls closer to zero, and in some cases changes sign, which is consistent with the findings from Cole, Paulson and Shastry (2013). This suggests that specifying the timing of the policy *implementation* is crucial to accurately estimating the effects of financial education on financial behaviors. All of these results are similar when using states that are geographically contiguous as the controls.

## **2 Background: The Effects of State Mandated K-12 Education**

In 2006, the National Association of State Boards of Education recommended states include financial education in their curricula. According to a report by the Council for Economic Education, 19 states required a personal finance course in curricula standards in 2013. Given the scarcity of instructional time and demands of providing support for science, math and related subjects, this focus on prescriptive mandates deserves careful consideration by policymakers and educators. Yet, there are currently no national standards for K-12 financial education (McCormick 2009).

Evaluations of existing financial education policies are hampered by dramatic variance in scale, scope, and timing of program implementation. Even across states supportive of financial education, the types of financial education programs implemented range from comprehensive year-long lesson plans taught in multiple grade-levels to sporadic lectures or

events during the students' senior year of high school. Existing studies that have examined state mandated personal finance education programs have reached varying conclusions about their effectiveness depending on the particular program or programs studied.

Among the most prominent of these studies, Bernheim, Garrett and Maki (2001) use data from a unique cross-sectional household survey conducted by Merrill Lynch that gathered information on household balance sheets, the state in which respondents attended high school, and their self-reported exposure to financial education, as well as to standard economic and demographic characteristics. The authors combine these data with information drawn from a number of different sources on state financial education mandates implemented in the 1950s through the 1980s. They find that people exposed to state mandated financial education had higher reported rates of savings and higher net worth. Moreover, consistent with there being a delay between the passage of a state mandate and when it is fully implemented in the classroom, the authors find that the effect on savings and net worth increases as the number of years post-mandate that the student was exposed increases.

Another study of the effect of state personal finance mandates by Tennyson and Nguyen (2001) uses data from a 1997 survey of high school seniors across the U.S. conducted by the Jumps\$tart Coalition that included a test of personal financial literacy. The authors examined how contemporaneous state personal financial education mandates affect student performance on the financial literacy test. They find that in models where a simple indicator is used for the presence of a mandate—averaging the types of mandates—there is no effect of personal financial education on student test scores. However, when the type of mandate is disaggregated into specific types—having a standard only, having a course requirement, and requiring testing—they find that only mandates with a course requirement were positively and significantly related to financial literacy test scores.

More recent work by Cole, Paulson and Shastry (2013) uses data from a variety of sources, including the Survey of Income and Program Participation, the 2000 U.S. Census, and credit bureau records, to examine the average effect of state personal finance and

math education requirements for people born between 1946 and 1965, and between 1964 and 1976, respectively. Using a difference-in-difference identification strategy that aggregates the various types of education programs implemented across states, the authors find that only math education is associated with improved financial outcomes, such as a reduced probability of experiencing foreclosure, defaulting on a credit card, and declaring bankruptcy.

Brown et al. (2013) pursue a similar strategy to Cole, Paulson and Shastry (2013), by analyzing the effect of state personal finance, economic, and math education mandates on later life credit outcomes using a difference-in-difference identification strategy. However, they examine a more recent set of state mandates that were implemented between 1998 and 2012, and thus, a younger cohort. The authors find that both math education and personal finance education have a significant effect on credit outcomes in early adulthood, increasing credit scores, lowering credit delinquency, and lowering the overall amount of debt held by an individual. The magnitude of the effect of these mandates on credit outcomes increases with additional time post-passage. In contrast to the math and personal finance mandates, Brown et al. (2013) find that high school economic education appears to be associated with higher debt and greater delinquency later in life.

An important consideration in evaluating financial education programs is determining what measures ought to be used to judge the program's effectiveness, and whether changes in those measures resulting from the education actually relate to subsequent changes in financial behavior and outcomes (Miller et al. 2014). Prior studies largely focus on knowledge gains, showing that students can and do learn specific information from the courses. However, learning content on economics or finance is merely a proxy for the broader policy goal of promoting 'responsible' financial management. Outcomes such as rates and amount of savings may have some merit, but given the concepts of lifecycle income, savings and borrowing, it might be rational and reasonable for young adults to borrow more and save less until their incomes' plateau and they begin to accumulate assets. In this study we do not have data on savings, but do have administrative data on credit use (as

opposed to self-reported data). Credit score is often considered a good measure of credit behavior (Arya, Eckel and Wichman 2011) and has been used as an evaluation outcome for financial interventions (Birkenmaier, Curley and Kelly 2012). However, credit scores are summary measures and may not respond in a timely way to small changes in credit use. Another way to estimate responsible credit use is to examine delinquent payments on any credit account, either 30 or 90 or more days behind (one missed payment or more than two missed payments). A single missed payment may simply suggest inattention or a lack of cash flow management, while more than two missed payments suggests a more fundamental financial problem. Moreover, as auto loans are the most common type of non-revolving credit for young adults, delinquency on auto loan payments may be especially relevant. Because much of the content of financial education includes budgeting and credit issues, payment behaviors might plausibly be influenced by exposure to these courses.

Establishing credit is one of the first financial management activities observed among young adults. Young people can establish a credit history by applying for a credit card on their own, being an authorized user on another account (such as a parent), having an account co-signed by someone with established credit or taking out a secured credit card. The Credit Card Accountability Responsibility and Disclosure Act of 2009, or CARD Act, was implemented in February 2010. This law requires credit card applicants younger than age 21 to have a source of income or an adult co-signer. About half of our sample is affected by this law, which results in a decline in credit availability and a shift in the sample towards higher credit quality and fewer delinquencies. Young people also establish credit records based on their bill payment habits. Utility bills and other payments may result in reports to credit bureaus and would lower credit scores. Other young people may have personal loans, retail store lines of credit, or automobile loans in their credit records, although the most common are credit cards (Fry 2013; Sotiropoulos and d’Astous 2012).

Following the human capital investment model for acquisition of financial knowledge described in Lusardi and Mitchell (2014), we argue that the mandate of education on

financial management led to an exogenous shift among young people residing in states with mandates towards accumulating additional financial knowledge. The fact that education was mandated and taught in schools made the costs of acquiring the information much lower, and the costs of not acquiring the information higher. Some students may directly gain the information, others may gain it from social networks, or may even view the state's mandate as a signal that financial literacy information is valuable. This would result in young people exposed to these state mandates acquiring financial knowledge earlier in life than they otherwise would; before they have opportunities to run into credit management problems. We propose that skills such as budgeting and planning, as well as knowing the cost of different types of credit, the determinants of credit score, and the consequences of negative credit behaviors, will result in more prudent behavior, on the margin. While financial education will not reduce exposure to economic shocks or broader financial distress, it may influence young people on the margin of taking on one more credit card or other loan, enhance their financial management and budgeting, and focus their attention on the salience of due dates for a range of bill payments reported to credit bureaus. We would expect these behaviors to manifest in higher credit scores and lower rates of delinquency.

### **3 Data**

The primary source of data for this study is the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP). The CCP is a 5 percent random sample of credit report data for U.S. persons with Social Security numbers drawn from the files of the credit reporting agency Equifax. The 5 percent random sample is then supplemented with the credit report data for all persons who reside at the same address as the primary individual, yielding a total sample of approximately 40 million credit files each quarter. The panel begins in the first quarter of 1999 and data are collected each quarter on an ongoing basis. The panel is regularly updated to include new credit files and remove the files of deceased

persons or those with inactive credit files, so as to maintain its representativeness of U.S. persons with credit reports and Social Security numbers. For a detailed description of the CCP sample design, as well comparisons of CCP estimates of outstanding credit to other aggregate national estimates, see Lee and van der Klaauw (2010).

In Georgia, Idaho, and Texas the mandates began with the class of 2007. We limit our sample to individuals under the age of 22 (we drop individuals the quarter they turn 22), so that we have exactly four years of data for all cohorts through the class of 2009. In terms of treated students, we observe approximately 60,000 people in Georgia, 12,000 in Idaho and 167,000 in Texas. Sample sizes vary based on the controls used and missing data. Figure 3 visually describes the sample composition in this dataset. We begin our analysis with the graduating class of 2000 and follow them, and each subsequent cohort, for up to four years post-graduation. Our pre-treatment cohorts include the graduating classes from 2000 through 2006. The three graduation year cohorts we follow after the personal finance course mandates were implemented are show in red in Figure 3: the classes of 2007, 2008, and 2009. We stop our estimation with the class of 2009, as we only have data available through the fourth quarter of 2013. By following each cohort for four years post-graduation, we will not bias our estimates of the effect of financial education due to a systematic difference in ages and length of exposure in the pre- and post-period sample.

Our data on the specific state personal finance education mandates comes from a variety of sources. The first is the website of the Jump\$tart Coalition for Personal Financial Literacy Jumpstart Coalition for Personal Financial Literacy (2013). The second is the 2013 National Report Card on State Efforts to Improve Financial Literacy in High Schools published by The Center for Financial Literacy at Champlain College Champlain College Center for Financial Literacy (2013). The third is various years of the Council for Economic Education’s Survey of the States Council for Economic Education (2014). These data were supplemented with information collected directly from each of the states we analyze, including reviewing legislation, graduation requirements, and the standardized

curricula for each of the courses.

We analyze the credit behavior of young adults starting at age 18 (or at the time of their first credit report if the file is too thin at age 18) until they reach age 22.<sup>2</sup> We first examine the Equifax Risk Scores (credit scores), and expect that the average credit score for the young people exposed to the mandated financial education would increase due to their having acquired additional knowledge about credit management and positive financial behaviors. However, the effect on one's credit score is likely to be small in magnitude, as they are mostly just being established during the age range we examine, and it is difficult to establish a substantially higher credit score than one's peers with only a brief credit history.<sup>3</sup> Next, we consider the possibility that exposure to financial education could help young individuals reduce negative credit outcomes. Specifically, we consider ever being 30 or 90 plus days delinquent on any credit account, and 30 or 90 plus days delinquent on an auto loan. As credit cards are the primary form of credit held by people ages 18 to 22, the results for the outcome credit card delinquency are nearly identical to those for any delinquency. Moreover, the CARD Act (Credit Accountability Resp Act of 2009) went into effect during our sample period and, while national in scope, may have affected credit card related outcomes.<sup>4</sup> We therefore omit the results for credit cards from our analysis and discussion.

### 3.1 Treatment States

We select three states that changed financial education mandates after 2000, and that previously had not mandated financial education in high schools: Georgia, Idaho, and Texas. Each of these states had well-documented interventions that are considered rela-

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<sup>2</sup>We observe the age of the individual in the credit record and the zip code of their address when they first enter the credit bureau database, in most cases starting at age 18. Some students may graduate as old as age 19 and as young as age 17; this will only serve to add noise to these estimates and bias our results downwards.

<sup>3</sup>The Equifax Risk Score included in the CCP is similar to the FICO score, but is based on a different algorithm; however, it predicts the same likelihood of severe delinquency over the next 24 months as a FICO score. The CCP credit score ranges from 280 to 850, with a higher score indicative of the person being a better credit risk.

<sup>4</sup>Among other provisions, the CARD Act limited access to credit cards for people under age 21.

tively rigorous by the Council for Economic Education Council for Economic Education (2014). The three states' mandates share some common features. First, they all have some form of standardized personal finance curriculum. Second, each state integrated the personal finance requirement into an economics requirement for high school students. Table 1 summarizes key parameters of each states' financial education policy, and we discuss each state's program in depth below.

### 3.1.1 Georgia

The Georgia Board of Education first approved a mandate for incorporating financial education in the K-12 curriculum in 2004. These Georgia Performance Standards began in the fall of 2006 and the first class affected by this mandate graduated in the spring of 2007. The required class was developed by the Georgia Council for Economic Education and is called "Let's Make it Personal." It incorporates the fundamentals of microeconomics, macroeconomics, international economics, and personal finance into a year-long course. The personal finance topics mainly focus on financial planning, including savings, insurance, and credit. Students also participate in a simulations like the stock market game.<sup>5</sup> Prior to the "Let's Make it Personal," course, a  $\frac{1}{2}$  credit course in Economics was required to be taught, but was not required to cover personal finance topics.<sup>6</sup>

According to the Georgia Council of Economic Education the goal of "Let's Make it Personal" is that: "Students leaving school are prepared for their economic roles as workers, consumers, and citizens." The learning objectives of the course include, the

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<sup>5</sup>One common approach used in high school economics and finance classes is to conduct a competition to invest in a mock investment portfolio. These activities encourage students to engage in applied learning, and in the process appear to gain financial knowledge (Hinojosa et al. 2009, 2007; Walstad and Buckles 2008). However, studies showing the effects of this approach may be biased by the selection of schools to offer the course and students to enroll in the course (Harter and Harter 2010; Mandell and Klein 2007; Mandell and Schmid Klein 2009; McCormick 2009). Notably, the state mandated a systematic implementation of a standardized set of content across schools, as well as student performance testing on personal finance content.

<sup>6</sup>Even if a large portion of schools were teaching personal finance within the economics course prior to the introduction of the mandate, this would simply bias our estimates against finding an effect.

student will: (1) apply rational decision making to personal spending and saving choices; (2) explain that banks and other financial institutions are businesses that channel funds from savers to investors; (3) explain how changes in monetary and fiscal policy can affect an individual's spending and savings choices; (4) evaluate the costs and benefits of using credit; (5) describe how insurance and other risk-management strategies protect against financial loss; (6) describe how the earnings of workers are determined in the marketplace.

### **3.1.2 Idaho**

In 2003, the Idaho State Board of Education mandated that schools should “include instruction stressing general financial literacy from basic budgeting to financial investments, including bankruptcy, etc” (Section 53A-1-402). Beginning with the graduating class of 2007, all students in the state were required to take one semester of economics to graduate as part of a 3-credit social studies requirement. The curriculum for this course was developed by family and consumer economics faculty at Idaho State University. The intent of the course was for students to “learn their roles as producers, consumers, and citizens.” The course is comprised of five segments of which 20 percent is devoted to traditional economics topics, and then 15 percent focuses solely on credit and debt, where students learn how and when to apply for loans and the value of their credit scores and credit reports. The next 20 percent of the course is on saving and investing decisions, followed by a unit on money management skills (another 20 percent of the course), including how to interpret paystubs, taxes, and make cost-benefit decisions when making a purchase. The remainder of the course is related to family finances, designing a resume and applying for jobs, as well as consumer roles, rights, and responsibilities, being an informed consumer, understanding fraud, identity theft, and how to set financial goals, and using tools such as *Consumer Reports* magazines to make informed decisions.

### 3.1.3 Texas

A 2004 amendment of the Texas Education Code (Section 1A -28-28.0021) required economics classes in grades 9 to 12 to include personal financial literacy within the economics curriculum, beginning with the 2006-2007 school year.<sup>7</sup> Specifically, each school district and open-enrollment charter school is to incorporate personal finance material into economics courses required for graduation. Each school must use standardized materials approved by the State Board of Education.

Any school district may include additional material, but each school must teach the following topics at a minimum: 1) understanding interest and avoiding and eliminating credit card debt; 2) understanding the rights and responsibilities of renting or buying a home; 3) managing money to make the transition from renting a home to home ownership; 4) starting a small business; 5) being a prudent investor in the stock market and using other investment options; 6) beginning a savings program and planning for retirement; 7) bankruptcy; 8) the types of bank accounts available to consumers and the benefits of maintaining a bank account; 9) balancing a checkbook; 10) the types of loans available to consumers and becoming a low-risk borrower; 11) understanding insurance; and 12) charitable giving.

### 3.1.4 Control States

We select 25 potential control states, each of which did not have a personal finance education course requirement during the study period according to Jump\$tart.org and the Council of Economic Education. These states, also displayed in the map in Figure 1, include: AK, AL, AR, CA, CT, DC, DE, FL, HI, IA, KY, MA, ME, MN, MS, MT, ND, NE, NM, OR, PA, VT, WA, WI, WY. Importantly, these states did not change their mathematics requirements over the time period of interest, based on the Education Commission of the States Reports and the Center for the Study of Mathematics Curriculum.

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<sup>7</sup>Some school districts could additionally appeal to the Commissioner of Education to delay the start of financial education in graduation requirements.

Of the remaining 23 states not included in the study, one state had a mandate prior to 1997 (the beginning of our sample from the CCP),<sup>8</sup> 12 states implemented mandates beginning with the class of 2009 or beyond,<sup>9</sup> three control states had changes in math or economics mandates over the period,<sup>10</sup> and the remaining seven states passed mandates between 2003 and 2006 but could not be included for other idiosyncratic reasons.<sup>11</sup>

## 4 Empirical Strategy

Our empirical strategy relies on comparing the changes in credit scores and delinquency rates before and after the implementation of the financial education mandates across states with and without personal financial education. To estimate the effect of financial education mandates on later credit behaviors, we use the synthetic control method for comparative case studies that has been used in previous work by Abadie and Gardeazabal (2003), Abadie, Diamond and Hainmueller (2010), and Hinrichs (2010) to calculate a local average treatment effect (LATE). For each treatment state, we consider the states with no financial education mandates after 2000 as potential controls.

We use state characteristics in 2000 to construct the synthetic control sample, using four sets of control variables. We first look at both financial and education-based variables in Specification 1.<sup>12</sup> These include the following state-level variables: GDP, median household income, poverty rate, Housing Price Index (HPI), unemployment rate, percent with less than a high school degree, percent graduated from high school, percent

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<sup>8</sup>NY began its mandate in 1985.

<sup>9</sup>These states are AZ, CO, IN, MO, NJ, NV, OH, OK, SD, TN, UT, VA.

<sup>10</sup>RI experienced a change in mathematics curriculum, MI experienced a change in Economics curriculum, and MD experienced several mandates in personal finance that were added and lifted during the sample period.

<sup>11</sup>LA's mandate took place in conjunction with Hurricane Katrina; NH's mandated only affected 7th-8th graders, lagging its effect on young adults; IL passed a mandate but still allows county-by-county variation in implementation; SC passed a mandate but never required a class; NC passed their mandate in 2005, though there is no untreated border state for comparison; WV implemented a financial literacy component to a civics course, combining civics, economics and geography but little is known about the breakdown of these courses across the state; KS passed a mandate requiring standards implementation, though most of these are implemented in grades 4 and 8.

<sup>12</sup>Specification 1 excludes DC since GDP data is not available or directly comparable to the other states.

graduated from college, percent with some college, Census region and division, percent of private schools, race and ethnic composition, expenditures per pupil, and total schooling expenditures. Specification 2 retains all of the variables from Specification 1, but drops GDP. Specification 3 only includes demographic and schooling variables: poverty rate, unemployment rate, education levels, Census region and division, percent of private schools, race and ethnic composition, expenditures per pupil, and total schooling expenditures. Specification 4 adds fourth and eighth grade math scores to Specification 3, which reduces the subsample of control states.<sup>13</sup> These data come from the National Assessment of Educational Progress (NAEP) provided by the National Center for Educational Statistics (NCES). Table 2 displays the states without mandates chosen for each of the treatment states with each specification, and the percentages of each comprised to make the synthetic control sample for each state. We choose Specification 1 as our preferred specification given that it allows us to include the broadest set of possible control states; however, our results are robust to the use of Specifications 2 through 4 instead.

Based on Specification 1, Georgia is best mimicked by a combination of Alabama, Alaska, California, Delaware, Hawaii, Kentucky, and Maryland, with Kentucky comprising the highest proportion and each remaining state comprising less than 11 percent each. When we remove Hawaii and Alaska, which likely are ex-ante outliers, our results remain comparable. Similarly, Specifications 2-4 provide comparable results, where Kentucky is the leading contributor throughout. In Specification 2, where we no longer include GDP, Florida becomes the second highest contributor at 15 percent. In Specification 4 Florida does not have test score data available, and Alabama becomes a bigger contributor (26 percent).

For Idaho, Specification 1 suggests that a weighting of North Dakota, Nebraska, and Washington yields the most comparable control state. This remains consistent if we remove GDP in Specification 2. However, when we look only at education and demographic variables in Specification 3, Oregon replaces Washington. Finally, Specification 4, which

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<sup>13</sup>The control states removed when we add math scores are AK, FL, IA, PA, WA, WI.

drops Washington due to lack of data, replaces Nebraska, Oregon, and Washington with Wyoming. Each of these chosen states are located within the same region and appear to be comparable ex-ante.

Finally, Specification 1 finds that a one-third weighting for each of California, Kentucky, and Mississippi yields the most comparable control state for Texas. While California may be most comparable to Texas in its sheer size, Kentucky and Mississippi likely possess more similar demographic characteristics in terms of racial and ethnic composition, as well as economic characteristics (e.g. poverty rate, unemployment). Specification 3 also picks neighboring state New Mexico as a large contributor (almost half) to the control sample.

We begin by examining the states that will form the counterfactual for our estimates. As shown in Figure 1, the three darkly shaded states are the treated areas, while the lightly shaded areas are potential controls. Figure 2 shows the border states used in the analysis in Panel A and matched synthetic controls in Panel B. While geography is one method to provide more homogeneous comparisons, matching states on observables can provide more robust comparisons. As shown in Table 2, the states in this matched sample are weighted based on a specification of state-level demographic and education data.

Table 3 shows descriptive statistics for the treatment and synthetic control samples. It further provides control samples based on border states models, another approach commonly used in difference-in-difference studies, under the assumption that geographically proximate states are more homogeneous. The synthetic control sample closely matches the treated state, as does the border state sample. While the summary statistics for the border state samples appear to more closely approximate the treatment states than those for the synthetic control samples, the differences are generally not statistically significant.

Our empirical approach employs a difference-in-difference specification where we exploit variation: 1) across individuals within the same state before and after the implementation of the mandate and 2) across individuals in the treatment and control states within the same time period. Using the synthetic control samples discussed above, we

estimate Equation (1) separately for each pair of treatment and control states. We choose to retain the panel structure of the CCP data in order to control for the contemporaneous probability of default in any given period with quarter by year fixed effects. This way, we control for any shifts in the national economy that change the probability of default in a given period.

$$\begin{aligned}
Y_{ist} = & \alpha_0 + \beta_1(T_s \times P1_{it}) + \beta_2(T_s \times P2_{it}) + \beta_3(T_s \times P3_{it}) \\
& + \gamma_1 u_{it} + \delta_s + \kappa X_{it} + \eta_t + \epsilon_{ist}
\end{aligned} \tag{1}$$

The outcomes of interest, labeled  $Y_{ist}$  in Equation (1) are the individual's credit score, delinquency on any credit account, and delinquency on an auto loan in a given quarter  $t$  from age 18 until just turning age 22. To account for unobserved time trends and local area factors, all estimates include state-level and quarter-by-year fixed effects.

In Equation (1),  $T_s$  is a dummy variable that equals one if the individual lived in the treated state in the sample (i.e. Georgia, Idaho, or Texas). We interact the treated state dummy with an indicator for the year in which the policy was enacted (2007). We then interact the treated state dummy with each of the two subsequent years (2008 and 2009). Thus,  $P3_{it}$  equals one if the individual graduated high school (turned 18) in the third year the financial education requirement was enacted. For example, a student in Idaho that graduated high school in 2009, three years after the course requirement was added to the curriculum, will be used to estimate the  $\beta_3$  coefficient. As we include state fixed effects and quarter-by-year fixed effects, the post-implementation period indicator variable  $P$  is omitted due to collinearity. We follow each individual in the pre- and post-periods for at most four years (and fewer if individuals take longer to establish a credit file). This way, we are not simply comparing younger borrowers to older borrowers in our estimation, where younger borrowers simply have less time to establish a credit history or become delinquent.

We measure the unemployment rate in each county in the year of graduation as  $u_{it}$

to control for labor market conditions across cohorts.  $\delta_s$  are state fixed effects that account for any time invariant state specific characteristics that may affect credit scores or delinquency rates, and  $X_{it}$  is the number of credit accounts an individual has in a given period. Finally,  $\eta_t$  incorporates quarter-by-year fixed effects to account for quarter and year specific factors that may affect credit outcomes, such as economic conditions. When we estimate Equation (1), we use the weights from Table 2 Column (1) to weight the least squares regression, with the treated state weighted by one. All standard errors are clustered at the individual level.

Our analysis requires three identifying assumptions. First, we assume that individuals begin their credit file in the same state they attended high school, which is consistent with Brown et al. (2013) where they document that over 90 percent of individuals stay in the same state from age 18 through age 22. Second, we assume that everyone in our sample was actually exposed to the financial education while in high school; however, if some of those students that we classify as treated did not in-fact receive the financial education, this would only serve to bias our estimates towards zero. Third, we assume that individuals in the states with financial education course requirements would have had similar trends in financial outcomes to those in the control states in the absence of the policy.

## 5 Results

Table 3 shows summary statistics over the sample period for each state, its synthetic control state, and its border control state(s). These summary statistics clearly show a sample of people just beginning the process of developing their credit profiles. Across states, the credit scores are in the low 600s, indicative of generally low credit quality in our sample. On average, the people in our sample have about two open credit accounts, over 10 percent of our observations are 30 days behind on at least one account and over 12 percent of our observations are 90 plus days behind on at least one account. Average

delinquency on auto loans is far lower than for any account, with approximately 3 percent of observations 30 days delinquent on an auto loan and approximately 1 percent 90 plus days delinquent.

Figure 4 shows the unconditional trends in credit score and any account delinquency by cohort for each set of treatment and synthetic control states pre- and post-implementation of the financial education mandates in 2007 (plotting 2000-2012). Across states and outcomes the pre-implementation trends appear largely parallel, suggesting that the synthetic control state is effectively mimicking the treatment state. While the treatment and control trends appear reasonably similar following implementation in 2007, some small changes in the relative trend of the treatment state are visible. The first row of figures shows mean credit scores, followed by 30 then 90 plus day delinquencies on any account. While there is an overall trend toward higher credit scores over time, credit scores in treatment states appear to increase more relative to control states beginning in 2008. However, any changes in the relative trends in delinquency rates between treatment and controls states are less apparent visually.

Initial regression results are shown for each state in Table 4 across the five credit outcomes of interest: credit score; ever being 30 or 90 days delinquent on any account; and ever being 30 or 90 days delinquent on an auto loan. Estimates for Georgia are shown in Panel A of Table 4. In the first column, we see that those young adults exposed to the financial education mandate for only one year directly following implementation—*Post1*—show only marginally higher credit scores than those young adults in the synthetic control state. However, the magnitude of the effect of financial education on credit score increases substantially with successive years of exposure following the implementation of the mandate. The estimate on the *Post2* coefficient indicates that those young adults exposed to Georgia’s mandate for two years have credit scores 13.4 points higher than those young adults in the synthetic control state. The effect is even greater for those young adults exposed to Georgia’s mandate three years following implementation, with the coefficient on *Post3* indicating that their credit scores are on average 28.7 points higher

than those of young adults in the synthetic control state. Both the two and three years post implementation estimates are significant at the 1% level. Given that the curriculum in Georgia was rather intensive and involved the need for teachers to adapt to a complex curriculum, the increasing magnitude likely reflects the teachers' ongoing learning and tailoring of the content and approach so as to be more effective for their students.

In the second column of results in Table 4 Panel A for Georgia, we examine the effect of exposure to financial education on the probability of having any account 30 days delinquent. In the first year post implementation, we see a modest, though statistically significant, 0.2 percentage point decline in the incidence of 30 day delinquency among young adults exposed to Georgia's financial education. Again, the magnitude of the effect increases substantially with additional years since the implementation of the mandate, with those exposed two years after implementation having a 0.5 percentage point lower rate of 30 day delinquency, and those exposed three years after implementation having a 1.5 percentage point lower rate of delinquency. To put this 1.5 percentage point decline in context, from Table 5 the average rate of 30 day delinquency for the Georgia estimation sample is 15.6 percent, implying that those who were exposed to the financial education three years post-implementation were approximately 10 percent less likely to be 30 days delinquent over the sample period. This lower rate of delinquency among young adults who received financial education is consistent with their also having a higher credit score, as making payments on-time has a substantial effect on the credit score of someone with a brief credit history.

When examining the effect of Georgia's financial education on the more severe outcome being 90 plus days delinquent on any account, we continue to observe that the magnitude of the reduction in delinquency increases with additional years post-implementation. As shown in the third column of Table 4, the financial education appears to have a particularly pronounced effect on the rate of 90 plus day delinquency, with reductions of 0.5 percentage points, 2.0 percentage points, and 3.6 percentage points in years one, two, and three post-implementation, respectively. All results are significant at the 1% level. With a base rate

of 17.8 percent 90 plus days delinquent in the estimation sample, the three year estimate of 3.6 percentage points implies a 20.2 percent lower probability of severe delinquency.

In addition to delinquency on any credit account, we focus on delinquency on auto loans, which are relatively common among young adults. In Column 4 of Table 4 we find that Georgia young adults exposed to financial education for one year have a 0.2 percentage point lower rate of 30 day auto delinquency than those in the synthetic control state, though this is not statistically different from zero. After two years of exposure to the education, the effect on 30 day auto loan delinquency nearly doubles in magnitude to a 0.5 percentage point decline and becomes statistically significant at the 5% level. Three years post-implementation, the decline in 30 day auto loan delinquency further increases in magnitude to 1.7 percentage points. With an average rate of 30 day delinquency on auto loans in the sample of 3.4 percent these reductions are quite large, with three years of exposure reducing delinquency by 50 percent. In the final column, the effect of Georgia's financial education on being 90 plus days delinquent on an auto loan appears to be more muted, likely due to the rarity of severe auto loan delinquency. While the education is estimated to reduce delinquency two and three years post-implementation, the magnitude is little different from zero.

Panel B in Table 4 shows the same estimates for Idaho. While Idaho is quite different from Georgia demographically, and the education mandate had different features, the estimated effects on credit scores and delinquencies are similar. Young adults in Idaho exposed to the financial education only one year post-implementation appear to perform worse than those in the control, with the estimates suggesting a decline in credit score of 7.2 points. However, exposure two years after implementation is estimated to increase credit score by 2 points, and after three years the effect further increases to 6.6 points. This delay in finding the expected effects suggests that Idaho's new financial education program may have taken more time to gain traction than that in Georgia. With regards to the estimates for any account being 30 days delinquent, we find little effect regardless of years post-implementation the young adults are exposed. However, the effect on having

any account 90 plus days delinquent is more pronounced, with those exposed two years post-implementation estimated to have a 1 percentage point lower rate of any 90 plus day delinquency, and those with three years of exposure having a 0.7 percentage point lower rate. Focusing only on auto loans in Columns 4 and 5, we see statistically significant reductions in 30 day delinquency two and three years post-implementation of 0.7 and 1.2 percentage points, respectively. However, for 90 plus days of delinquency on auto loans the reduction is only significant three years post-implementation. The generally smaller magnitude of the point estimates for Idaho compared to Georgia is consistent with the Idaho's financial education being somewhat less rigorous, requiring only a half-year course and no mandatory testing.

Finally, panel C of Table 4 shows the results for Texas. Here, we observe a very small 0.9 point negative effect of one year post-implementation exposure to the financial education on credit score, which switches to a 4.9 point increase in credit score for exposure two years after implementation, and a 13.0 point increase for exposure three years after implementation. There appears to be no effect of the financial education on the rate of any account being 30 days delinquent in Texas; however, we find large and significant reductions in having any account 90 plus days delinquent in every year post-implementation. Those young adults exposed to the Texas education one year post-implementation are 0.8 percentage points less likely to have any account 90 plus days delinquent, increasing in magnitude to 2.2 percentage points less likely after two years, and 3.3 percentage points less likely after three years. Based on the 15.9 percent average rate of having any account 90 plus days delinquent for the Texas sample shown in Table 5, the 3.3 percentage points reduction from three years post-implementation exposure translates into 20.7 percent fewer severe delinquencies among Texas young adults.

As a robustness test of the synthetic controls identification strategy, Table 6 shows the same specifications as Table 4, but using contiguous border states rather than a synthetic control state. The results here are generally consistent with the findings reported in Table 4, although the magnitude of the effects increases substantially in some instances.

In Idaho, the credit score increase for young adults three years post-implementation is 16 points, while in Texas it reaches nearly 32 points. Similarly, the reduction in having any account 90 plus days delinquent with three years post-implementation exposure is now 1.9 percentage points in Idaho and 5.8 percentage points in Texas. While the magnitude of some of the estimates from the border states model appear somewhat large, the across the board consistency with our previous estimates lends credibility to the results from the synthetic controls specifications.

For consistency with the existing literature, we next use the year the mandated was passed as the first treatment year and compare them to those found when using year of implementation. We estimate the same equation as in Table 4, and create a similar sample comparison, where we drop all individuals who graduated from high school (turned 18) three years after the mandate was passed.<sup>14</sup> As shown in Table 7, using the mandate year yields somewhat similar results for Georgia and Texas, although the magnitudes are generally lower and less precisely estimated. Moreover, the results are less intuitive than those presented in Table 4 in that we don't observe linear improvement in outcomes with additional time post-mandate. For example, in Panel A, Georgia's financial education is estimated to yield a 6.7 point increase in credit score for exposure one year post-mandate, increase to 10 points after two years, and yield a 9.2 point increase after three years. In Texas, the pattern of credit score increase is now 5.2 points after one year, 1.1 points after two years, and 3.7 points after three years. These coefficient magnitudes are substantially smaller than the effect we find in Table 4. For Idaho, when using the mandate year the effect on credit score is now consistently negative, while almost none of the delinquency coefficients are statistically significant. Looking only at these alternative results using mandate year as the treatment could easily lead one to conclude that financial education has little positive effect on financial behavior. It's also worth emphasizing again that the states examined here have particularly rigorous mandates, so any aggregation of these

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<sup>14</sup>Idaho passed its mandate in 2003, while Georgia and Texas passed mandates in 2004. For Idaho, we end the sample with individuals who turned 18 after 2005; for Texas and Georgia, we end the sample with individuals who turned 18 after 2006. The sample remains comparable to Figure 3.

states' education with less rigorous mandates would only further bias the results towards null effects.

Table 8 then presents the results from the border state model when mandate year is used as the treatment rather than implementation year. In this specification, the results provide even less support for the effectiveness of financial education. The effect on credit score in each of the three states is trivial and often statistically insignificant. In Georgia and Idaho, the effects on the various delinquency outcomes are similarly trivial, while in Texas the coefficients are small in magnitude and often suggest that financial education increases delinquency.

Overall, our findings are consistent with students exposed to the personal finance education (based on three years of implementation) had between a 7 point higher credit score (in Idaho) and a 29 point higher credit score (in Georgia). These translate into relative sizes (relative to standard deviations) of between  $0.08\sigma$  units and  $0.33\sigma$  units. This appears to be driven by reductions in the incidence delinquencies among students exposed to the education, which supports the role of education in boosting financial capability and cash flow management.

## 6 Conclusion

The results of this study demonstrate that state financial education mandates in the three states examined yield positive effects on subsequent credit behaviors for the young adults exposed to these programs after implementation. Three years after implementation, we observe larger increases in credit scores and reductions in delinquency among the treated young adults. This is in stark contrast to the results from models where the year in which a mandate is initially enacted is used as the treatment, which show little effect of financial education on subsequent outcomes.

Our results contribute to literature in several ways. First, these results highlight the importance of financial literacy education researchers to focus on documenting and test-

ing the actual implementation of programs in the classroom, rather than using the year of enactment. Second, we illustrate the utility of credit report data as a measure of financial behavior as an example of how ‘downstream’ financial behaviors can be measured to evaluate educational interventions given variation over geography and time. Third, we contribute to the education evaluation literature with an example of synthetic control methods, including a side-by-side comparison to geographic border approaches. Overall, we demonstrate that these three standardized and relatively rigorous education programs do appear to alter credit behavior in early adulthood, and that the effects are increasing, consistent with schools refining and improving implementation. The estimated effect sizes are relatively large, ranging from one-tenth to one-third of a standard deviation. This is larger than found in prior studies of financial education (see Fernandes, Lynch and Netemeyer (2014) for a discussion) and relatively large in magnitude based on experiments in education more generally (see Hill et al. (2008) for a discussion).

These results add to the debate about state curriculum standards in education in by providing a more precise estimate of the effects of the education mandate for personal financial content, but also raise important questions. Given that there is a finite amount of time in the school curriculum, an expansion of financial education courses invariably comes at the cost of less time devoted to other subjects. Thus, we cannot evaluate the overall effects of expanded financial education without knowing the specific trade-offs involved. We leave it to subsequent research to determine whether these positive effects persist further into adulthood, and to weigh the costs of providing financial education in school relative to its benefits. If the goal of policymakers is to influence debt repayment behavior, and the opportunity costs of providing this form of education are relatively low, then mandating financial education may prove to be a reasonable strategy.

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## 7 Tables and Figures

*Table 1: Selected State Mandates*

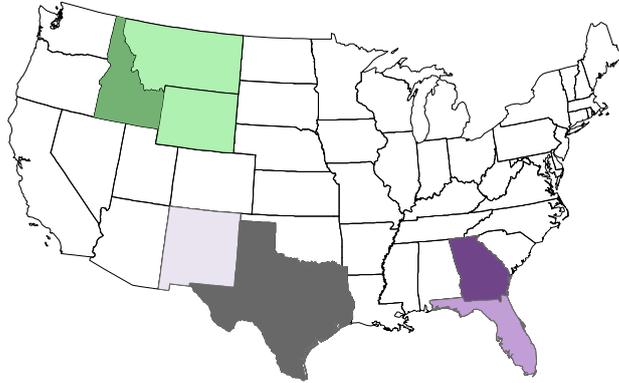
State	Yr Implemented	Length	Grade	Testing
Georgia	2007	1yr	HS-econ	Yes
Idaho	2007	0.5 yr	HS-econ	No
Texas	2007	1yr	HS-econ	Yes



Fig. 2: Treatment and Control Samples

**Legend**

- Border State-FL
- Treatment-GA
- Border State-MT, WY
- Border State-NM
- Treatment-TX



**Legend**

**TX Weights**

- 0.3
- 0.318
- 0.382
- 1

**ID Weights**

- .247
- 312
- .441
- 1

**GA Weights**

- <0.05
- 0.084
- 0.111
- 0.696
- 1

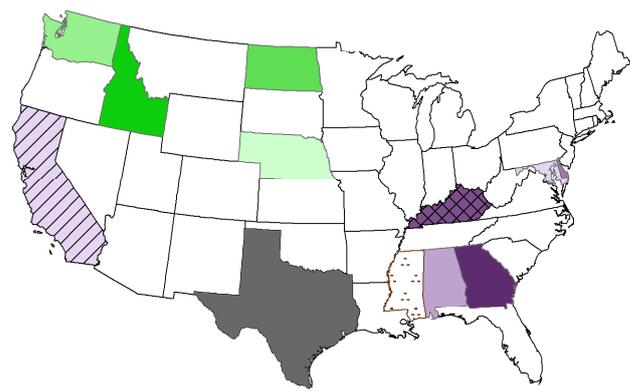


Table 2: Synthetic Controls Selection

<b>Panel A: GA</b>				
State	Specification 1	Specification 2	Specification 3	Specification 4
AK	0.03			
AL	0.084	0.056	0.071	0.262
CA	0.021			0.042
CT		0.059	0.013	0.026
DC			0.037	0.027
DE	0.111	0.014		
FL		0.154	0.151	
HI	0.021	0.027		
IN				0.103
KY	0.696	0.681	0.657	0.541
MD	0.037			
MI			0.071	
MN		0.008		

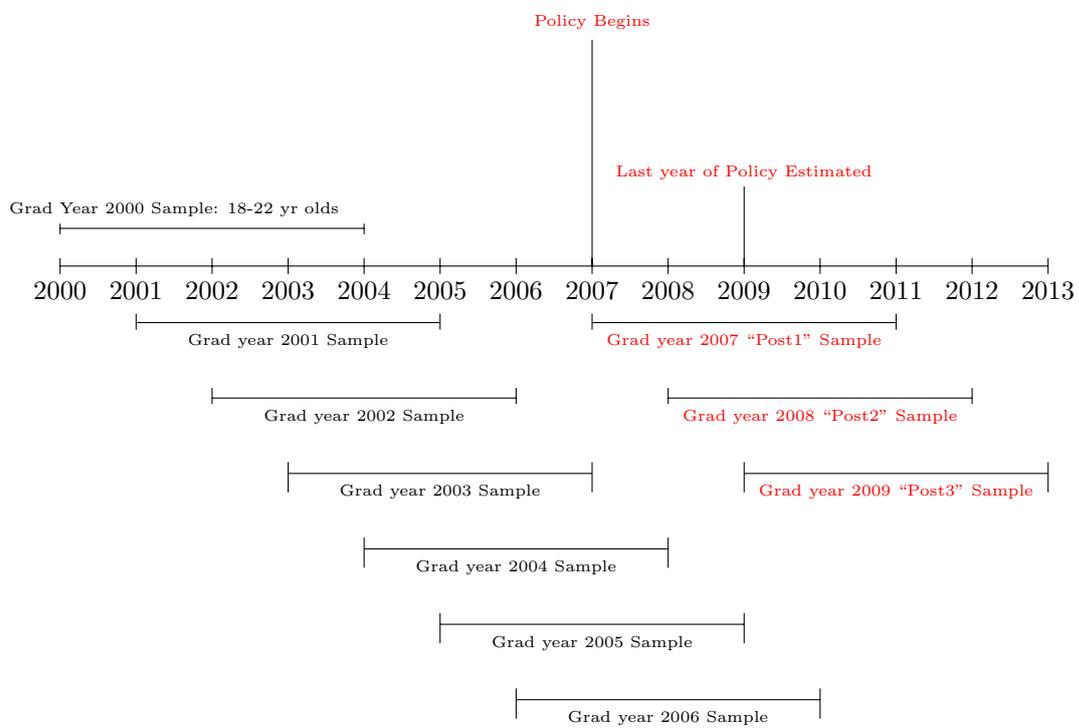
<b>Panel B: ID</b>				
State	Specification 1	Specification 2	Specification 3	Specification 4
ND	0.441	0.436	0.31	0.64
NE	0.247	0.246	0.12	
OR			0.57	
WA	0.312	0.317		
WY				0.36

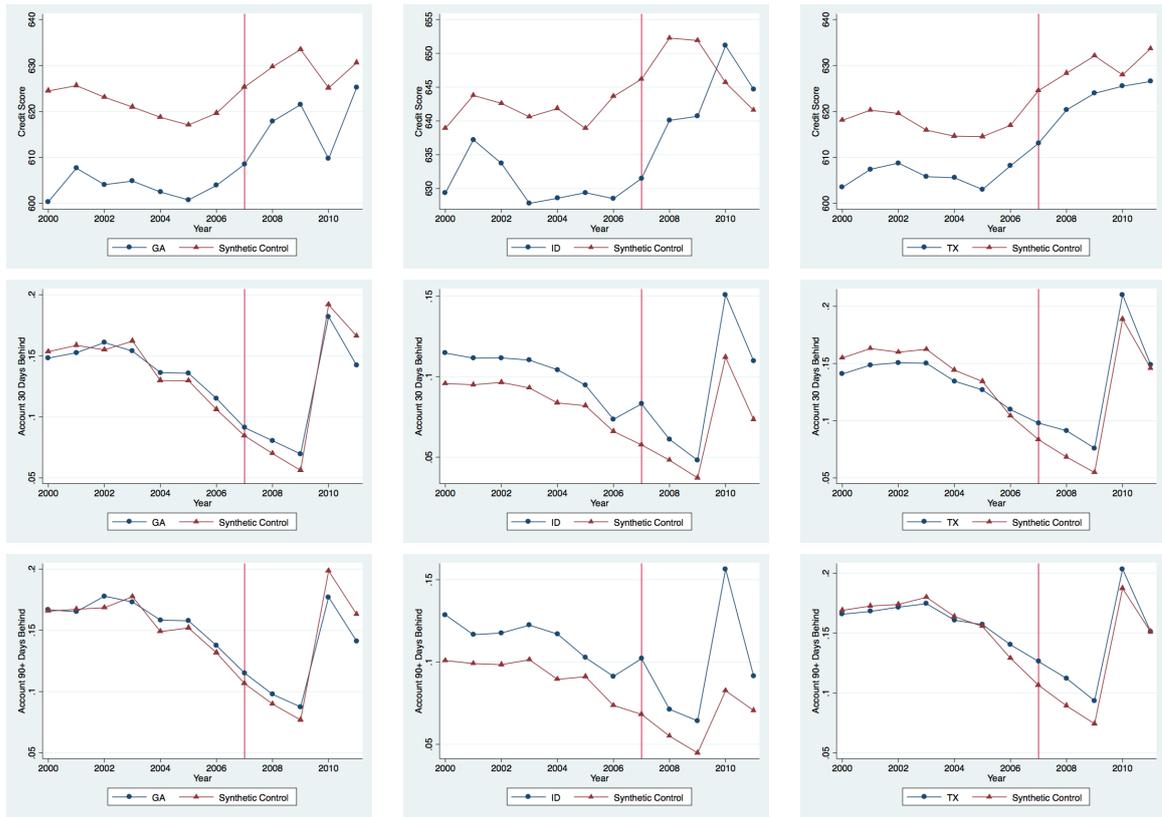
  

<b>Panel C: TX</b>				
State	Specification 1	Specification 2	Specification 3	Specification 4
AL			0.083	
CA	0.318	0.274	0.02	0.32
KY	0.382	0.34	0.15	0.387
MS	0.3	0.326	0.259	0.294
NM		0.06	0.487	

Notes: Each synthetic control sample constructed using 2000 state-level characteristics. Specification 1: GDP, Median Household Income, Poverty Rate, HPI, Unemployment, Education Levels, Region, Division, Percent of Private Schools, Expenditure per Pupil, Race and Ethnicity, Total Expenditures. Specification 2: Specification 1, less GDP (excludes DC) Specification 3: Poverty Rate, Unemployment, Education Levels, Region, Division, Percent of Private Schools, Expenditure per Pupil, Race and Ethnicity, Total Expenditures Specification 4: Specification 3 with math scores at grades 4 and 8 (which is a subsample of states).

Fig. 3: Policy Timeline and Sample Composition





*Fig. 4: Difference-In-Differences Plots: States Implementing Education Mandates in 2007 Compared to Controls, By Credit Report Outcome*

Notes: The figures above show trends in average credit score and delinquency rates for each of the three treatment states and their respective synthetic control state. Trends for Georgia are shown in the first column of figures, followed by Idaho and then Texas. Row 1 shows the trend in average credit score. Row 2 the trend in 30 day delinquency on any account. Row 3 the trend in 90 plus day delinquency on any account.

Table 3: Summary Statistics, Treatment versus Synthetic Control and Border State Comparisons

	Control	GA	Border (FL)	Control	ID	Border (WY, MT)	Control	TX	Border (NM)
Credit Score	618.1239 (85.6048)	606.5294 (89.4437)	611.1519 (88.1336)	638.1212 (80.2629)	632.3341 (85.5644)	637.6713 (78.4124)	630.2958 (85.4562)	609.3161 (88.5174)	614.2561 (87.2455)
Number of Accounts	2.3075 (2.3996)	2.0766 (2.2480)	2.4485 (2.5576)	2.4552 (2.3890)	2.2857 (2.1718)	2.4190 (2.3192)	2.3680 (2.3532)	2.3674 (2.4861)	2.2232 (2.1841)
Account 30 Days Delinquent	0.1535 (0.3604)	0.1576 (0.3644)	0.1581 (0.3648)	0.1115 (0.3147)	0.1079 (0.3102)	0.1148 (0.3188)	0.1076 (0.3099)	0.1488 (0.3559)	0.1375 (0.3444)
Account 90 + Days Delinquent	0.1751 (0.3801)	0.1818 (0.3857)	0.1809 (0.3849)	0.1205 (0.3256)	0.1217 (0.3269)	0.1237 (0.3292)	0.1260 (0.3318)	0.1781 (0.3826)	0.1585 (0.3652)
Auto 30 Days Delinquent	0.0320 (0.1759)	0.0362 (0.1867)	0.0310 (0.1732)	0.0207 (0.1425)	0.0229 (0.1495)	0.0220 (0.1467)	0.0311 (0.1735)	0.0323 (0.1768)	0.0297 (0.1698)
Auto 90 + Days Delinquent	0.0115 (0.1066)	0.0127 (0.1120)	0.0103 (0.1008)	0.0066 (0.0810)	0.0087 (0.0927)	0.0098 (0.0983)	0.0078 (0.0879)	0.0084 (0.0911)	0.0110 (0.1043)
County Unemployment Rate	5.4147 (2.0015)	5.1083 (1.7033)	4.9050 (1.7631)	5.2876 (1.9481)	4.5014 (1.7014)	4.0817 (1.3199)	6.3177 (2.2683)	5.5631 (1.5715)	5.1239 (1.5824)
State Level % HS	30.3124 (5.9031)	28.8088 (6.6356)	28.2024 (4.5362)	26.2757 (4.9153)	27.7915 (4.6254)	30.5185 (4.6046)	22.5005 (5.7071)	24.4926 (5.0727)	26.4578 (3.9937)
State Level % Some College	25.6285 (4.4345)	25.9442 (4.5836)	28.8917 (3.0727)	34.1367 (3.7079)	34.9968 (3.7863)	32.7795 (3.0704)	29.3504 (4.5767)	27.7133 (3.8612)	28.8203 (3.1082)
State Level % College	19.9410 (8.7657)	24.1176 (11.1407)	22.9093 (6.1405)	26.3180 (8.5031)	22.3523 (8.1839)	24.5864 (8.0167)	24.2742 (8.4989)	23.5814 (8.6020)	23.0754 (8.3715)
Number of Individuals	329160	55081	112735	62678	11310	10999	270322	153807	12625

Table 4: Synthetic Control Sample Results

<b>Panel A: Georgia</b>					
	(1)	(2)	(3)	(4)	(5)
	Credit Score	Account 30 Days Delinquent	Account 90 + Days Delinquent	Auto 30 Days Delinquent	Auto 90 + Days Delinquent
P1	0.666 (0.414)	-0.00232** (0.00103)	-0.00550*** (0.00135)	-0.00229 (0.00235)	0.000768 (0.00149)
P2	13.40*** (0.419)	-0.00509*** (0.00108)	-0.0205*** (0.00140)	-0.00515** (0.00237)	-0.00249* (0.00145)
P3	28.71*** (0.508)	-0.0149*** (0.00127)	-0.0364*** (0.00169)	-0.0174*** (0.00264)	-0.00257 (0.00184)
N	3894181	3412901	3412901	687659	687659
<b>Panel B: Idaho</b>					
	(1)	(2)	(3)	(4)	(5)
	Credit Score	Account 30 Days Delinquent	Account 90 + Days Delinquent	Auto 30 Days Delinquent	Auto 90 + Days Delinquent
P1	-7.332*** (0.917)	0.00388* (0.00204)	0.00846*** (0.00262)	0.00257 (0.00350)	-0.000784 (0.00192)
P2	2.183** (0.861)	-0.00206 (0.00191)	-0.0114*** (0.00232)	-0.00670** (0.00309)	0.00297 (0.00240)
P3	6.509*** (0.977)	-0.00180 (0.00224)	-0.00686** (0.00288)	-0.0115*** (0.00355)	-0.00929*** (0.00178)
N	703386	620045	620045	138733	138733
<b>Panel C: Texas</b>					
	(1)	(2)	(3)	(4)	(5)
	Credit Score	Account 30 Days Delinquent	Account 90 + Days Delinquent	Auto 30 Days Delinquent	Auto 90 + Days Delinquent
P1	-0.938*** (0.261)	0.00137** (0.000601)	-0.00810*** (0.000825)	-0.00608*** (0.00117)	0.000483 (0.000654)
P2	4.943*** (0.262)	0.000253 (0.000599)	-0.0218*** (0.000811)	-0.00621*** (0.00123)	-0.00124* (0.000637)
P3	13.01*** (0.284)	-0.000685 (0.000666)	-0.0327*** (0.000887)	-0.00568*** (0.00142)	-0.00198*** (0.000712)
N	4174049	3683648	3683648	752678	752678

Notes: Robust standard errors clustered at the individual level in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . P1 corresponds to the first graduating class affected by the requirement; P2 corresponds to the following graduating class; P3 is two classes after the first graduating class. Models include state-level and quarter by year fixed effects, unemployment rate in the state and year of graduation, and number of accounts. Samples are weighted by the synthetic control weights in Table 2.

Table 5: Summary Statistics by Area

	<b>Georgia</b>		<b>Idaho</b>		<b>Texas</b>	
	Border	Synthetic	Border	Synthetic	Border	Synthetic
Credit Score	609.5427 (88.6166)	611.7006 (87.9413)	634.9071 (82.2373)	635.9380 (82.2913)	609.6397 (88.4706)	617.1378 (87.9755)
Number of Accounts	2.3214 (2.4635)	2.1796 (2.3197)	2.3501 (2.2451)	2.3921 (2.3127)	2.3584 (2.4664)	2.3676 (2.4380)
Account 30 Days Delinquent	0.1579 (0.3647)	0.1557 (0.3626)	0.1113 (0.3145)	0.1102 (0.3131)	0.148 (0.3551)	0.1337 (0.3403)
Account 90+ Days Delinquent	0.1812 (0.3852)	0.1788 (0.3831)	0.1227 (0.3280)	0.1210 (0.3261)	0.1767 (0.3814)	0.1589 (0.3656)
Auto 30 Days Delinquent	0.0324 (0.1772)	0.0342 (0.1818)	0.0224 (0.1481)	0.0216 (0.1455)	0.0321 (0.1764)	0.0319 (0.1757)
Auto 90+ Days Delinquent	0.0110 (0.1041)	0.0121 (0.1095)	0.0092 (0.0956)	0.0074 (0.0857)	0.0086 (0.0924)	0.0082 (0.0900)
Unemployment Rate	4.9749 (1.7456)	5.2456 (1.8492)	4.2976 (1.5425)	4.9990 (1.8998)	5.531 (1.5764)	5.8352 (1.8883)
% HS	28.4097 (5.3584)	29.4824 (6.3620)	29.1153 (4.8123)	26.8333 (4.8669)	24.6347 (5.0287)	23.7742 (5.3957)
% Some College	27.8797 (3.9209)	25.8028 (4.5202)	33.9204 (3.6306)	34.4526 (3.7586)	27.7939 (3.8231)	28.3037 (4.2076)
% College	23.3261 (8.2289)	22.2466 (10.3561)	23.4368 (8.1797)	24.8596 (8.6040)	23.5457 (8.5863)	23.8313 (8.5714)
Number of Individuals	167855	384241	22316	73988	196643	424129

Table 6: Border State Sample Results

<b>Panel A: Georgia (Florida)</b>					
	(1)	(2)	(3)	(4)	(5)
	Credit Score	Account 30 Days Delinquent	Account 90 + Days Delinquent	Auto 30 Days Delinquent	Auto 90 + Days Delinquent
P1	-0.529 (0.407)	-0.00197* (0.00100)	-0.00501*** (0.00133)	-0.00284 (0.00228)	0.00229 (0.00147)
P2	6.293*** (0.410)	-0.00152 (0.00105)	-0.0136*** (0.00137)	-0.00451** (0.00229)	-0.00112 (0.00138)
P3	10.89*** (0.486)	-0.00653*** (0.00118)	-0.0181*** (0.00160)	-0.0128*** (0.00243)	-0.000574 (0.00171)
N	1632241	1407663	1407663	329800	329800
<b>Panel B: Idaho (Montana, Wyoming)</b>					
	(1)	(2)	(3)	(4)	(5)
	Credit Score	Account 30 Days Delinquent	Account 90 + Days Delinquent	Auto 30 Days Delinquent	Auto 90 + Days Delinquent
P1	-4.276*** (0.969)	0.00156 (0.00216)	0.00524* (0.00277)	0.00176 (0.00363)	0.000989 (0.00205)
P2	8.143*** (0.938)	-0.00651*** (0.00213)	-0.0187*** (0.00255)	-0.00805** (0.00336)	0.00288 (0.00253)
P3	16.19*** (1.131)	-0.00897*** (0.00265)	-0.0199*** (0.00332)	-0.0145*** (0.00412)	-0.0125*** (0.00245)
N	210165	185194	185194	49477	49477
<b>Panel C: Texas, (New Mexico)</b>					
	(1)	(2)	(3)	(4)	(5)
	Credit Score	Account 30 Days Delinquent	Account 90 + Days Delinquent	Auto 30 Days Delinquent	Auto 90 + Days Delinquent
P1	5.182*** (0.299)	0.000860 (0.000693)	-0.0150*** (0.000945)	-0.00763*** (0.00135)	-0.00129* (0.000752)
P2	16.30*** (0.324)	-0.00178** (0.000745)	-0.0353*** (0.00102)	-0.0105*** (0.00153)	-0.00408*** (0.000822)
P3	31.71*** (0.388)	-0.00566*** (0.000924)	-0.0576*** (0.00124)	-0.0130*** (0.00193)	-0.00628*** (0.000979)
N	1585593	1669260	1669260	1669260	1669260

Notes: Robust standard errors clustered at the individual level in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . P1 corresponds to the first graduating class affected by the requirement; P2 corresponds to the following graduating class; P3 is two classes after the first graduating class. Models include state-level and quarter by year fixed effects, unemployment rate in the state and year of graduation, and number of accounts.

Table 7: Synthetic Control Mandate Year Results

<b>Panel A: Georgia</b>					
	(1)	(2)	(3)	(4)	(5)
	Credit Score	Account 30 Days Delinquent	Account 90 + Days Delinquent	Auto 30 Days Delinquent	Auto 90 + Days Delinquent
M1	6.743*** (0.437)	-0.00331*** (0.00115)	-0.0106*** (0.00140)	-0.00702*** (0.00210)	-0.00137 (0.00135)
M2	10.23*** (0.460)	-0.00345*** (0.00120)	-0.0186*** (0.00147)	-0.00685*** (0.00235)	-0.00705*** (0.00140)
M3	9.251*** (0.476)	-0.00149 (0.00123)	-0.0222*** (0.00153)	-0.00966*** (0.00243)	-0.00770*** (0.00142)
N	2869079	2547209	2547209	552460	552460
<b>Panel B: Idaho</b>					
	(1)	(2)	(3)	(4)	(5)
	Credit Score	Account 30 Days Delinquent	Account 90 + Days Delinquent	Auto 30 Days Delinquent	Auto 90 + Days Delinquent
M1	-3.290*** (0.920)	-0.000663 (0.00223)	-0.00203 (0.00255)	-0.00501 (0.00323)	0.00175 (0.00228)
M2	-4.411*** (0.994)	-0.00123 (0.00231)	-0.00320 (0.00266)	-0.00545 (0.00340)	0.000956 (0.00220)
M3	-6.742*** (0.976)	0.00743*** (0.00238)	-0.00530** (0.00259)	0.000782 (0.00364)	-0.00170 (0.00194)
N	448347	401074	401074	93344	93344
<b>Panel C: Texas</b>					
	(1)	(2)	(3)	(4)	(5)
	Credit Score	Account 30 Days Delinquent	Account 90 + Days Delinquent	Auto 30 Days Delinquent	Auto 90 + Days Delinquent
M1	5.179*** (0.272)	-0.00369*** (0.000670)	-0.0118*** (0.000856)	-0.00997*** (0.00120)	-0.000571 (0.000662)
M2	1.074*** (0.283)	0.000611 (0.000694)	-0.0101*** (0.000896)	-0.00732*** (0.00128)	-0.00278*** (0.000662)
M3	3.733*** (0.294)	0.000818 (0.000709)	-0.0182*** (0.000914)	-0.0117*** (0.00136)	-0.00367*** (0.000696)
N	3074161	3205133	3205133	3205133	3205133

Notes: Robust standard errors clustered at the individual level in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . M1 corresponds to the mandate year, not the first graduating class that was affected; M2 is one year after the mandate, and M3 is two years after the mandate. Models include state-level and quarter by year fixed effects, unemployment rate in the state and year of graduation, and number of accounts. Samples are weighted by the synthetic control weights in Table 2.

Table 8: Border State Sample Mandate Year Results

<b>Panel A: Georgia (Florida)</b>					
	(1)	(2)	(3)	(4)	(5)
	Credit Score	Account 30 Days Delinquent	Account 90 + Days Delinquent	Auto 30 Days Delinquent	Auto 90 + Days Delinquent
M1	-0.157 (0.418)	-0.00120 (0.000897)	0.000683 (0.00110)	-0.000231 (0.000292)	0.000123 (0.000201)
M2	-0.601 (0.424)	-0.000724 (0.000881)	-0.00211* (0.00109)	-0.00000915 (0.000299)	-0.000718*** (0.000170)
M3	-1.665*** (0.414)	0.000288 (0.000831)	-0.00317*** (0.00105)	-0.00151*** (0.000244)	-0.000864*** (0.000154)
N	1362887	1433459	1433459	1433459	1433459
<b>Panel B: Idaho (Montana, Wyoming)</b>					
	(1)	(2)	(3)	(4)	(5)
	Credit Score	Account 30 Days Delinquent	Account 90 + Days Delinquent	Auto 30 Days Delinquent	Auto 90 + Days Delinquent
M1	-0.224 (0.418)	-0.00199* (0.00109)	-0.000319 (0.00134)	-0.00464** (0.00201)	-0.00149 (0.00128)
M2	-0.503 (0.424)	-0.00165 (0.00110)	-0.00411*** (0.00136)	-0.00411* (0.00221)	-0.00534*** (0.00126)
M3	-1.634*** (0.414)	-0.000191 (0.00106)	-0.00604*** (0.00134)	-0.00677*** (0.00218)	-0.00609*** (0.00125)
N	1362832	1181488	1181488	282967	282967
<b>Panel C: Texas, (New Mexico)</b>					
	(1)	(2)	(3)	(4)	(5)
	Credit Score	Account 30 Days Delinquent	Account 90 + Days Delinquent	Auto 30 Days Delinquent	Auto 90 + Days Delinquent
M1	1.747*** (0.277)	-0.00254*** (0.000677)	-0.00113 (0.000868)	-0.00401*** (0.00125)	0.000792 (0.000687)
M2	-4.070*** (0.283)	0.00173** (0.000682)	0.00625*** (0.000893)	0.00134 (0.00130)	-0.00111 (0.000690)
M3	-3.144*** (0.276)	0.00161** (0.000645)	0.00433*** (0.000871)	-0.000281 (0.00128)	-0.00189*** (0.000678)
N	1555038	1356423	1356423	301574	301574

Notes: Robust standard errors clustered at the individual level in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . M1 corresponds to the mandate year, not the first graduating class that was affected; M2 is one year after the mandate, and M3 is two years after the mandate. Models include state-level and quarter by year fixed effects, unemployment rate in the state and year of graduation, and number of accounts.