

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

Alexandra Brown[†], J. Michael Collins[‡], Maximilian Schmeiser[§], Carly Urban[¶]

Abstract

Policymakers have increasingly emphasized financial education as a solution to perceived failures in household financial decision-making. In the U.S., a number of states have mandated personal finance classes in public school curricula. Despite a long history of financial and economic education in public schools, little is known about the outcomes of these programs on the credit management behaviors of young adults as they begin to establish financial independence from their parents. If young people are naive about the ramifications of taking on credit and paying bills on time, financial education in public schools may raise the salience of paying attention to applying for and managing credit as well as paying bills on time. Using a panel of credit report data, this analysis examines three states (Georgia, Idaho, and Texas) where a new personal financial education mandates was implemented. This policy shift is used to estimate credit scores and delinquencies in young adulthood by cohorts of students estimated to be exposed to the school system before and after the policy. Young people who are in school after the implementation of state mandates show evidence of modestly greater credit scores and lower delinquency rates. These effects are robust to a variety of matching and differencing estimators and, to the extent improved credit behaviors are a policy objective, these results may support the implementation of similar financial and economics education in the K-12 curricula of other states.

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[†]Project Manager, Consumer and Community Development Research Section, Federal Reserve Board, Washington, DC

[‡]Associate Professor, Department of Consumer Science, University of Wisconsin-Madison

[§]Senior Economist, Consumer and Community Development Research Section, Federal Reserve Board

[¶]Corresponding Author: Assistant Profess of Economics, Montana State University, 208 Linfield Hall P.O. Box 172920 Bozeman, MT 59717-2920. Email: carly.urban@montana.edu.

1 Introduction

A growing body of literature shows a correlation between an individual's level of financial knowledge and financial behaviors. Lower levels of measured financial literacy have been associated with lower rates of planning for retirement, lower rates of asset accumulation, using higher cost financial services, lower participation in the stock market, and higher levels of debt (Lusardi and Mitchell 2014, 2007; Lusardi *et al.* 2010; Lusardi and Tufano 2009; Meier and Sprenger 2010; van Rooij *et al.* 2012).

Particularly in light of the 2008 financial crisis, policymakers have intensified their interest in promoting greater financial literacy in the U.S., launching initiatives including Presidential commissions at the Federal level, and governor's councils at the state level. The rationale for this emphasis on financial literacy is that better informed consumers might engage in more prudent credit behaviors and avoid household financial behaviors that could trigger broader economic problems for financial markets. Yet, the existing body of research on the effectiveness of financial literacy education has been far from conclusive (Fernandes *et al.* 2013; 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.¹ Economic education has been taught in K-12 public schools in the U.S. since the 1950s, and in the last decade this content has expanded to include more personal financial management topics, in addition to expanding to students at more grade levels. Given the scarcity of educational time and resources, it is important to determine whether mandating an increased focus on financial education in the K-12 curriculum yields tangible improvements in the students financial outcomes. Moreover, determining which particular financial education programs yield the greatest benefits would allow states design an effective, yet efficient curriculum. 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 *et al.* 2001; Brown *et al.* 2013a; Cole *et al.* 2013; Tennyson and Nguyen 2001). For example, state mandates

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

that result in identifying ‘financial education’ versus ‘no financial education’ conditions can be highly heterogeneous. States with intensive financial education programs could require multiple courses and performance testing. These mandates are often combined with states that suggest schools offer (but not require) any form of instruction on personal finance. By combining weaker and stronger mandates, the estimation of the effect of the ‘average’ state mandate on student outcomes could be biased towards finding little or no effect. Moreover, mandates for financial education might be enacted at the same time as broader school reforms, or shifts in the economic cycle, exacerbating the aggregation problem across time and geography.

A further difficulty in attempting to estimate the effect of state financial education mandates on financial outcomes is that the exact timing and quality of implementation of education is often unobserved. Given the time that elapses between the enactment of a personal financial education mandate and when schools across the state have a well-developed curriculum in place and instructors are trained to teach the material, we would expect some lag from when a mandate passes to when students are actually exposed to its full effects. Because implementation is typically unobserved, studies often use the passage of a mandate as the start date for exposure to financial education; this could result in understated estimates of the effectiveness of the education.

This paper focuses on analyzing the effect of well-documented financial education mandates in three specific states. Georgia, Idaho, and Texas each implemented significant personal finance course requirements in 2007. We document the specific requirements in each of these states, including (1) the testing requirements, (2) the teacher training involved, and (3) the number of credit hours devoted to the new course. We verify that there were no other curriculum changes at the time of implementation, and also identify other states with no such personal finance mandates or comparable curricular shifts. This more localized and more precise approach yields a local average treatment effect for each state, allowing us to relax the assumption that all financial education is equal.

To perform this analysis, we use a synthetic control method as in Abadie *et al.* (2010) and Abadie and Gardeazabal (2003) to create a weighted set of comparison states for each of our treated states. We do this by using trends in state level variables such as unemployment rates, population density, income, delinquency rates, house price indexes, and credit scores,

all of which are measured prior to the imposition of financial mandates in the focal states. The synthetic control procedure gives us a weighting for each potential control state that sums to one. For example, the weights the procedure recommends for Idaho are: North Dakota (0.435), Washington (0.319), and Nebraska (0.246). We also estimate effects using geographic homogeneity (bordering contiguous states), as a robustness check, finding similar results. These approaches allow 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).

We use individual-level credit bureau data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP) dataset on people who were likely exposed (or not exposed) to the financial education, based on state of residence and likely year of high school graduation.² We use these quarterly data from 2000 through 2013 to measure credit data outcomes in young adulthood, including credit score, overall credit delinquency rates, and then delinquency rates specifically among credit cards—a form of debt especially relevant for young adults and of intense regulatory interest in recent years. We use a difference-in-difference framework comparing the people who were likely to have been in schools in states mandating education before or after the mandate was in place, as well as people in comparison states who were likely to never be exposed to financial education in both the pre- and post-mandate periods. We compare cohorts of individuals who were exposed to financial education (treatment) to individuals in prior cohorts in the same state as well as to similar cohorts in comparison states (controls) along the same time horizon. We also include controls for the unemployment rate at the year of graduation, as well as time invariant state fixed effects.

We focus only on individuals who are under age 22 by 2013, so that the individuals who were treated with financial education are of comparable ages to those who were not. Individuals in the sample may be as young as 18 and as ‘old’ as 22, and we are careful to drop individuals who are still in high school (18 years of age) in the later years of our sample (2011-2013). We estimate our results separately for each graduating class for each year up to three years after the education mandate was first implemented. This analysis of multiple post reform years allows us to account

²We 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 young as age 17; this will serve to add noise to these estimates and downward bias our results.

for the time it takes teachers to settle into teaching the new course and become comfortable with the material, as well as any variance in age at graduation (and therefore exposure to the mandate).

Our results suggest that in these three states, students subject to the augmented financial education requirements have greater credit scores and lower delinquency rates on credit when compared to those in the comparison group. The magnitude of each of these effects increases as each year after the year the mandate was initially implemented.

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 in the students senior year of high school. Thus, 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 *et al.* (2001) used 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, in addition to standard economic and demographic characteristics. These data were combined with information drawn from a number of different sources on state financial education mandates implemented in the 1950s through the 1980s. The authors find that those people who were 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) used data from a 1997 survey of high school seniors across the U.S. conducted by the Jump\$Start Coalition that included a test of personal financial literacy. The authors examined how contemporaneous state personal financial education mandates affected 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 the course requirement mandate was positively and significantly related to financial literacy test scores.

More recent work by Cole *et al.* (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.* (2013b) pursue a similar strategy to Cole *et al.* (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.* (2013b) 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 *et al.* 2011) and used has been used as an evaluation outcome for financial interventions Birkenmaier *et al.* (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, either 30 or 60 days behind (1 or 2 missed payments). These behaviors suggest inattention or a lack of cash flow management. We would predict use of credit cards would be most salient for young adults after leaving high school. These are relevant for a broad cross-section of high school graduates and their use of these products might plausibly be influenced by their exposure to courses teaching about how credit works and how to manage bill payments.

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 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 impacted 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. Missed mobile phone payments, utility bills, insurance payments and even rent (in some cases) 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.

We propose that the mandate of education on financial management led to an exogenous shift among young people residing in states with mandates to invest in acquiring 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 early in life 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 credit and how using loans impacts future borrowing costs, will result in more prudent behavior, on the margin. These classes will not reduce economic shocks or distress that will result in ruined credit, but will influence young people on the margin of taking on one more credit card or other loan, or focus their attention on the salience of due dates for a range of bill payments reported to credit bureaus.

3 Data

Following Brown *et al.* (2013a) and Cole *et al.* (2013), 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 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, so we limit our sample to individuals under the age of 22 (we drop individuals the quarter they turn 22). 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 observe a student for at most 4 years, starting with the graduating class of 2000 and follow each subsequent cohort in the pre-period, graduating classes from 2001-2006. Next, Figure 3 reports the three graduation year cohorts we follow after the course mandate was implemented in red: the classes of 2007, 2008, and 2009. We stop our estimation with the class of 2009, so we can follow them for the same four year period after graduation. This way, we will not be estimating an effect of financial education based on a systematic difference in ages in the pre- and post- period sample composition, with younger borrowers in the latter treated periods.

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 Educations Survey of the States Council for Economic Education (2014). These data were supplemented with information collected directly from each of the states we analyze, by reviewing legislation, reading graduation requirements, and the learning 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. We first examine credit scores, and would predict 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 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

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

only a brief credit history. 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 60+ days delinquent on any credit account, and 30 days delinquent on a credit card account.³

3.1 Selected States with Mandates

We select three states that changed mandates after 2000, and previously had not mandated financial education in high schools, each with well-documented interventions: Georgia, Idaho, and Texas.

The three states share some common features. First, they all have some form of standardized curriculum across states. Second, each state integrated the personal finance requirement into an economics requirement for high school students. Table 1 summarizes these state’s policies below. We discuss each state’s course in more depth below.

3.1.1 Georgia

The Board of Education Georgia 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 is called “Let’s Make it Personal,” and incorporates the fundamentals of microeconomics, macroeconomics, international economics, and personal finance into a year-long course.

³It should be noted that the CARD Act (Credit Accountability Resp Act of 2009) also went into effect in this period and could account for some effects among educated students. Among other provisions the CARD Act drastically reduced access to credit cards for people under age 21. This law was national in scope, however and would apply equally to the treatment and control states.

The personal finance topics mainly focus on financial planning. 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. The course, as designed by the Georgia Council for Economic Education, involves simulations regarding financial portfolios, personal savings/investment, insurance, and credit. As of 2007, 32,000 students in the state participated in a simulation called the stock market game.⁴ 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.⁵

The Georgia Council of Economic Education expressed the following goal of this mandated course: “Students leaving school prepared for their economic roles as workers, consumers, and citizens.” The learning objectives of the course include, the student will: (1) apply rational decision making to personal spending and saving choices; (2) explain that banks and other financial institutions are businesses which channel funds from savers to investors; (3) explain how changes in monetary and fiscal policy can impact 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 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 6 segments

⁴One 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). 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, however (Harter and Harter 2010; Mandell and Klein 2007; Mandell and Schmid Klein 2009; McCormick 2009).

⁵Even if a large portion of schools were teaching personal finance within the economics course prior to the introduction of the mandate, this would bias our estimates against finding an effect.

of which 20% is devoted to traditional economics topics, and then 15% 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% of the course is on saving and investing decisions, followed by a unit on money management skills (another 20% 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.⁶ 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.

⁶Some school districts could additionally appeal to the Commissioner of Education to delay the start of financial education into graduation requirements.

3.1.4 Control States

We selected 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 also 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. 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),⁷ 12 states implemented mandates beginning with the class of 2009 or beyond,⁸ three control states had changes in math or economics mandates over the period,⁹ and the remaining seven states passed mandates between 2003 and 2006 but could not be included for other idiosyncratic reasons.¹⁰

4 Empirical Strategy

Our empirical strategy relies on comparing the changes in credit scores and delinquency rates before and after the implementation of the policies across states with and without personal finance mandates. To estimate the effect of financial education mandates on later credit behaviors, we use synthetic control methods for comparative case studies that has been used in previous work by Abadie and Gardeazabal (2003), Abadie *et al.* (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. 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. This includes the following

⁷NY began its mandate in 1985.

⁸These states are AZ, CO, IN, MO, NJ, NV, OH, OK, SD, TN, UT, VA.

⁹RI experienced a change in mathematics curriculum, MI experienced a change in Economics curriculum, and MD experienced several mandates in personal finance that were placed and lifted during the sample period.

¹⁰LA's mandate took place in conjunction with Hurricane Katrina; NH's mandated only affected 7th-8th graders, lagging its effect period; 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 un-treated 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.

state-level variables: GDP, median household income, poverty rate, Housing Price Index (HPI), unemployment rate, percent graduated from high school, percent 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. We also drop the District of Columbia from the sample due to its small sample size. 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.¹¹ This data comes 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, and provide 2-4 as tests of robustness.

First, we find that Georgia is best mimicked by Alabama, Connecticut, Florida, Hawaii, Kentucky, and Minnesota, with Florida and Kentucky comprising the highest proportion and the remainder comprising less than 6 percent each. When we remove Hawaii, which likely is the ex-ante outlier, 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, Alabama replaces Florida as the second highest contributor. The same thing happens in Specification 4, where Florida does not have test score data available.

Second, North Dakota, Nebraska, and Washington are most comparable to Idaho. 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 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.

¹¹The control states removed when we add math scores are AK, FL, IA, PA, WA, WI.

Third, the prominent states comparable to Texas are California, Kentucky, and Mississippi, where each comprises about a third of the sample. 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 dark 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 synthetic control samples and the sample from the treatment states are statistically different along all but a few covariates at the ten percent level, we still believe this method yields the best potential control sample.

Specifically, we employ a difference-in-difference specification where we exploit variation: 1) across individuals within the same state before and after the implementation of the mandate 2) across treatment and control states within the same time period.

Using the synthetic control samples discussed above, we estimate Equation (1) for each treated state and its comparison group separately. 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 to the national economy that change the probability of default in a given period.

$$\begin{aligned}
Y_{ist} &= \alpha_0 + \alpha_1 T_s + \beta_1(T_s \times \text{Post}_1) + \beta_2(T_s \times \text{Post}_2) + \beta_3(T_s \times \text{Post}_3) \\
&+ \gamma_1 u_{ist} + \delta_s + \kappa_t + \eta_q \epsilon_{ist}
\end{aligned}$$

The outcomes of interest, labeled Y_{ist} in Equation (1) are based upon the individual’s credit scores from age 18 until just turning 22. Over this period, we examine credit score, followed by a range of negative credit behaviors, including ever being 30 or 60 or more days delinquent on any account, as well as late on credit card payments. To account for unobserved time trends and local area factors, all estimates include county-level and month-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. We then interact the treated state dummy with the three subsequent years. Specifically, Post_3 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 required curriculum, will be used to estimate the β_3 coefficient. As shown in Figure 3, we follow each graduation year cohort in the pre- and post-periods for at most four years (and fewer if individuals take longer to establish a credit file). In the post-period, as shown in red, we follow individuals who graduated high school at the latest in 2009. This way, we are not simply comparing younger borrowers to older borrowers in our estimation, where younger borrowers simply have less time to become delinquent.

We measure the unemployment rate in each state and year as u_{ist} to control for labor market conditions across cohorts. η_q incorporates quarter fixed effects. Finally, δ_s and κ_t are county and graduation year fixed effects, respectively.¹² This way we account for changes within state before and after the mandate, as well as comparing students across treatment states within the same graduation cohort. When we estimate Equation 1, we use the weights from Table 2 Column (1) to weight the least squares regression, where we weight the treated state as one.

Our analysis requires three identifying assumptions. First, we assume that individuals begin

¹²We are careful to ensure that each county contains enough individuals with which to estimate these fixed effects. All contain over 30 individuals, and approximately 90% of counties contain more than 100 individuals.

their credit file in the same state they attended high school, which is consistent with Brown *et al.* (2013a) where they document that over 90% of individuals stay in the same state from age 18 through age 22.¹³ 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. Figure 4 shows the unconditional trends in the various outcomes (credit score, any account delinquency, credit card delinquency) for the treatment and synthetic control states pre- and post-implementation of the financial education mandate (plotting 2000-2012). Across states and outcomes the pre-implementation trends appear largely parallel, with divergence accruing around the time of implementation.

5 Results

Table 3 show summary statistics for the baseline periods. These summary statistics clearly show people with still emerging credit profiles. Credit scores are in the low 600s, showing overall low credit quality. But on average people only have 2 to 3 open accounts, and over 10% have missed payments on any account or a credit card in their record.

We begin with Figure 4 showing the difference-in-difference synthetic control results for each state (in each column) around the policy implementation in 2007. The first row shows mean credit scores, the second 30 day delinquencies on any account, the third shows 60 days behind (which implies serious delinquency, not just errors of attention), and the last row of graphs shows 30 day delinquencies on credit cards, a form of credit of interest among young adults. Notice overall the trends is toward increase credit scores and declining delinquencies over time, especially since 2005. We assume this is related to credit tightening precluding the recession in 2008, as well as regulatory pressures coming into the major financial reforms implemented 2009-2010. A similar finding is reported in Fry (2013), reporting that young adults acquired less debt after the recession. Sotiropoulos and d’Astous (2012) further document that young adults

¹³Again, we choose 22, as this is the oldest a student could be during our sample period if she graduated in 2009, our final treatment year.

consume and take on credit in similar ways to their peers, potentially further decreasing the debt of this population. Visually it is not obvious that the slope shifts between the focal state and comparisons, although in several cases the treated states appear to catch up or overtake comparison states, especially for 30 day delinquencies overall and among credit cards specifically. This is consistent with education boosting attention to payments, but not changing the probability for negative financial shocks (income drops, for example, which lead to 60 or more day late payments).

Initial regression results are shown in of Table 4 for each state across the four dependent variables of credit score, 30 day late pays, 60 day late pays and 30 days late specific to credit cards. Estimates for Georgia are shown in Panel A of Table 4. In the first column of results, we see that those young adults in school immediately following the implementation of the financial education mandate experience a modest, though significant, 4.6 point increase in their subsequent credit score relative to those in the synthetic control state one year after their year of graduation, relative to these same differences in the pre-policy period (before 2007). Identification of the *Post2* and *Post3* variables comes from comparing individuals graduating in Georgia to those in the control states successive years after the mandate. As expected, the magnitude of the effect on credit score increases with the number of years post implementation, with the biggest jump occurring between those after the first year. The curriculum in Georgia was intensive, including teachers adapting to interactive learning modules; it is possible the first year required ongoing learning and tailoring of the content and approach. As shown in Table 3, the average credit score for young adults in this sample is around 600 and a standard deviation of 90, so that a movement of even 7 points in the best case scenario may appear small.

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. Here, we find no effect (statistically or in terms of effect size) for those exposed in the first year post-implementation, but increasing effects for subsequent years. Those students exposed to the education two years post-implementation see a decrease in their probability of being 30 days delinquent of 1.5 percentage points or 10 percent relative to a base rate of being 30 day delinquent of 15.1 percent. The effect more than doubles for those exposed for three years, with a decrease in 30 day delinquency of 3.5 percentage points or 23 percent relative to the

base. These latter estimates are significant at the 1% level. The third column of Table 4 in Panel A shows estimates for 60 plus days delinquent on any account. The coefficient estimates here largely mirror those found for 30 day delinquency, albeit at even larger magnitudes each year after the policy was implemented. The last column in Table 4 Panel A shows credit card delinquencies. As to be expected, these look much like the second column. Quite likely credit cards make up most of any accounts young people have; hence credit card 30 delinquencies should closely mirror all delinquencies.

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 estimates effects on credit scores and delinquencies look quite similar. These estimates are slightly larger in magnitude, but with larger error terms (in part since Idaho has much smaller sample size—less than 12,000 students compared to over 50,000 in Georgia). Panel C of Table 4 shows results for Texas. Again, these results are comparable to those found in Georgia.

Table 5 shows the same specifications as Table 4 but using contiguous border states. The results are generally consistent, with some notable exceptions. Credit scores in Georgia now show small but *negative* estimates in Panel A. The estimates for delinquencies in Georgia are more consistent with the synthetic controls however. In Panel B, the estimates for Idaho are consistent for credit scores but *positive* for 30 day delinquencies in some periods. These results are all very small in magnitude however. The border comparison results for Texas most closely resemble the synthetic controls model. The border models are hampered by smaller sample sizes and measures with high variance. We assert that the synthetically matched controls are more appropriate than the geographically contiguous controls, but do offer some assurance that the general findings of the prior estimates are robust. Further robustness tests (not shown) include quarter dummies and student fixed effects.

In Table 6, we repeat the synthetic control estimates based on the education level of the population in the Zip code. We do this in part to account for the differential value of obtaining financial education depending on the economic and education status of the individual. The sample in Idaho is too small to effectively sub-divide across the periods of the change in financial education mandates, so only Georgia and Texas are included. While there is a statistical difference between high and low education Zip codes, with slightly higher returns to education

for the low-education areas, the differences are small in magnitude. Zip code level education levels are a crude proxy for student education level (and by definition our sample at a minimum has some high school education and at most has 3-4 years of post-secondary education). We would predict that students who were mandated to receive education might show stronger effects among lower-education levels, but fail to observe such trends in these data.

6 Conclusion

This study uses credit bureau data to and three quasi-experiments stemming from changes in required curricula. Using an event study setup of differences-in-differences, we show how mandates affected credit behavior among adults just beginning steps toward financial independence. Among people who turned 18 after these mandates, relative to age cohorts in the same state before the mandate, and relative to comparable students in other states, it appears financial education is related to greater credit scores and decreased delinquencies. These effects generally take some time to become established, perhaps due to ongoing implementation and adaptation.

Mandated courses taught in schools reduce marginal costs of acquiring the financial management knowledge, and make the marginal costs of avoiding acquiring the information higher. Students may not only learn the information from classes, but also pick it up from peers or see the state's mandate as a signal that financial literacy information in general is valuable and worth their attention. State mandates appear to at least modestly reduce credit management problems early in life.

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7 Tables and Figures

Table 2: Synthetic Controls Selection

Panel A: GA				
State	Specification 1	Specification 2	Specification 3	Specification 4
AK		0.03		
AL	0.035	0.084	0.071	0.262
CA		0.021		0.042
CT	0.061		0.013	0.026
DC			0.037	0.027
DE		0.001	0.111	
FL	0.145		0.151	
HI	0.042	0.021		
IN				0.103
KY	0.699	0.696	0.657	0.541
MD		0.037		
MI			0.071	
MN	0.016			

Panel B: ID				
State	Specification 1	Specification 2	Specification 3	Specification 4
ND	0.435	0.441	0.31	0.64
NE	0.246	0.247	0.12	
OR			0.57	
WA	0.319	0.312		
WY				0.36

Panel C: TX				
State	Specification 1	Specification 2	Specification 3	Specification 4
AL			0.083	
CA	0.318	0.277	0.02	0.32
KY	0.382	0.342	0.15	0.387
MS	0.3	0.324	0.259	0.294
NM		0.057	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. 2: Treatment and Control Samples

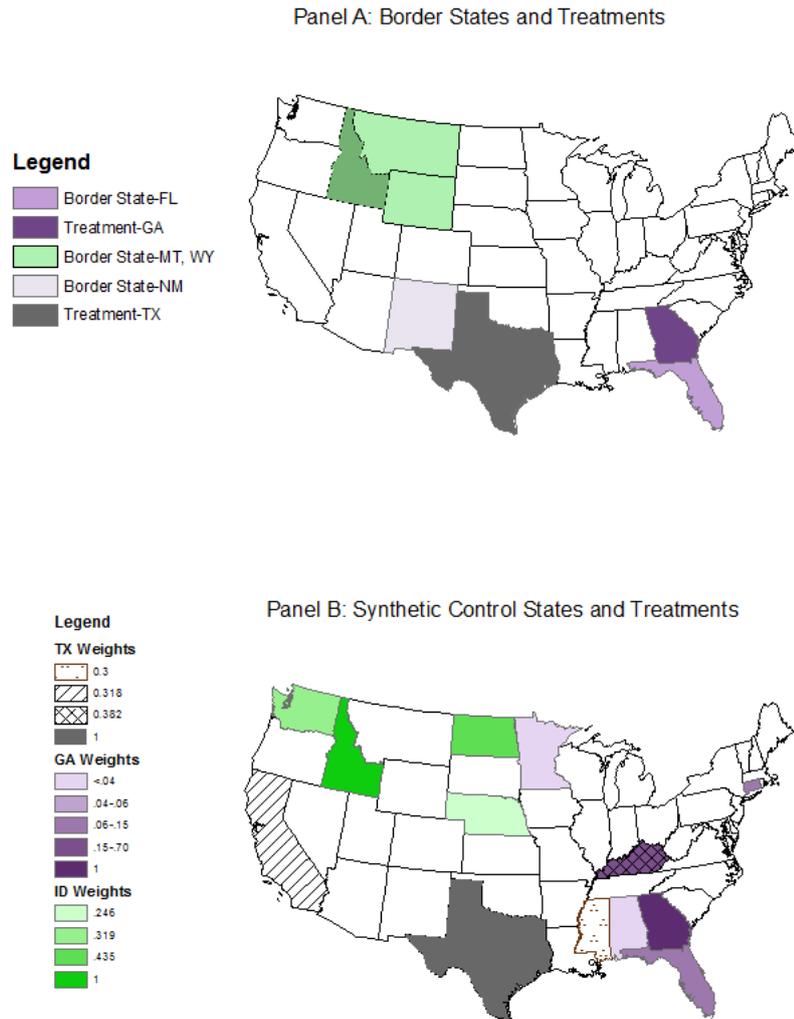
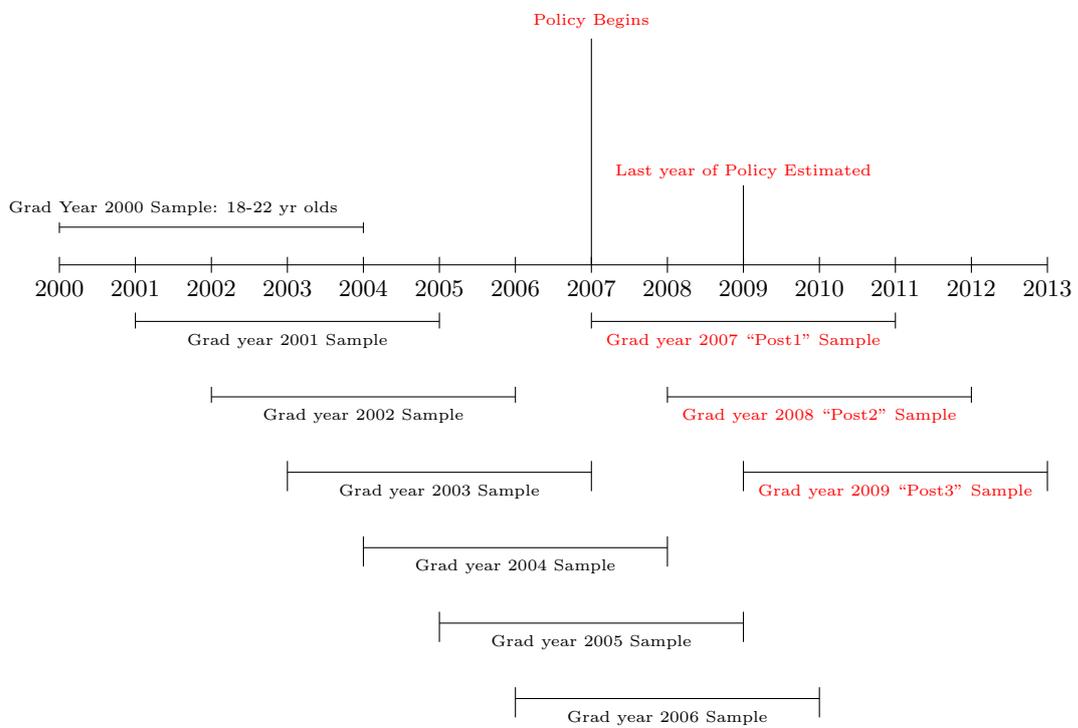


Fig. 3: Policy Timeline and Sample Composition



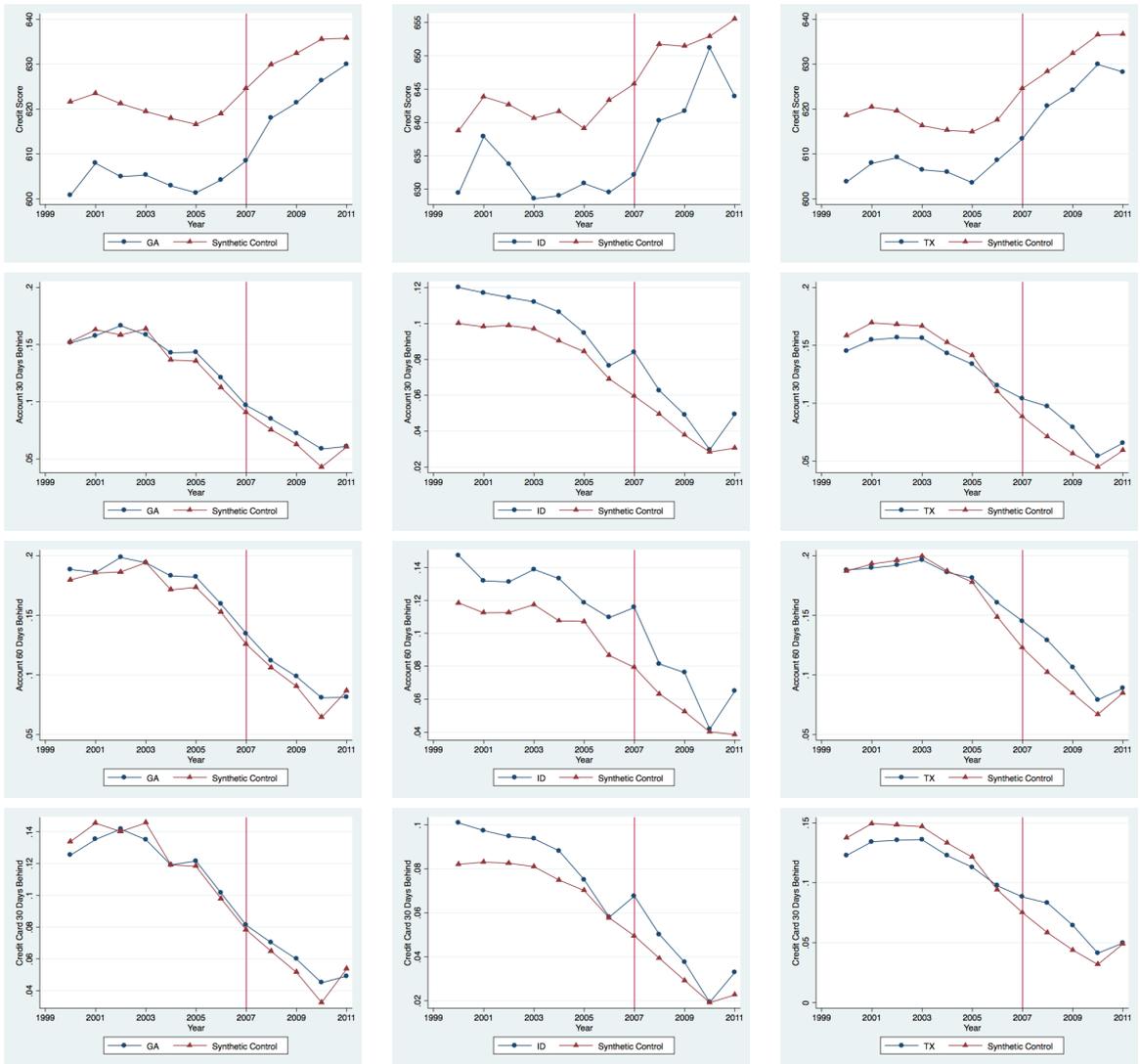


Fig. 4: Difference-In-Differences Plots: States Implementing Education Mandates in 2007 Compared to Controls, By Credit Report Outcome

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	616.1237 (87.4580)	600.7826 (90.6270)	602.5242 (89.8826)	632.5986 (81.5713)	629.3725 (85.9400)	635.8623 (80.6736)	624.2257 (85.1430)	603.7953 (89.7608)	610.4501 (85.6767)
Number of Accounts	2.5311 (2.5791)	2.2675 (2.2885)	2.7126 (2.7040)	2.6527 (2.5290)	2.3672 (2.2280)	2.4402 (2.3977)	2.5762 (2.5114)	2.5477 (2.6058)	2.3754 (2.3389)
Account 30	0.1549 (0.3618)	0.1514 (0.3585)	0.1543 (0.3612)	0.1255 (0.3313)	0.1201 (0.3251)	0.1250 (0.3307)	0.1289 (0.3351)	0.1452 (0.3523)	0.1690 (0.3748)
Account 60 Plus	0.1849 (0.3882)	0.1884 (0.3910)	0.1892 (0.3917)	0.1456 (0.3527)	0.1472 (0.3543)	0.1462 (0.3533)	0.1574 (0.3642)	0.1880 (0.3907)	0.2013 (0.4010)
Credit Card 30	0.1357 (0.3424)	0.1252 (0.3309)	0.1340 (0.3406)	0.1072 (0.3094)	0.1009 (0.3013)	0.1057 (0.3074)	0.1089 (0.3115)	0.1229 (0.3283)	0.1441 (0.3512)
Unemployment Rate	3.9997 (1.1051)	3.5880 (0.8154)	3.7783 (0.7378)	4.3095 (1.4009)	4.5703 (1.4887)	4.4051 (1.0505)	5.0003 (1.8210)	4.4986 (1.5390)	5.0495 (1.5703)
% HS	31.2109 (5.4836)	29.1261 (6.6952)	28.7905 (4.3687)	25.8031 (5.0711)	28.1812 (4.9503)	30.2555 (5.0163)	23.3707 (5.8689)	25.3612 (5.0057)	26.5846 (4.0925)
% Some College	26.8944 (4.3975)	26.3529 (4.1759)	29.0162 (3.1066)	34.1580 (3.7313)	35.1299 (3.7716)	33.0702 (3.3323)	29.0025 (4.2287)	27.9002 (3.6449)	29.5378 (3.0412)
% College	22.4402 (8.5569)	25.6262 (11.5864)	25.1372 (6.5006)	28.3575 (9.1205)	23.2739 (8.6589)	26.3909 (8.9301)	26.1100 (9.0620)	24.3224 (8.5484)	24.0129 (8.5484)
N	252712	59405	118569	63350	11649	11479	296160	160884	12970

Table 4: Synthetic Control Sample Results

Panel A: Georgia				
	(1)	(2)	(3)	(4)
	Credit Score	Account 30 Days Delinquent	Account 60 Plus Days Delinquent	Credit Card 30 Days Delinquent
Post1	4.642*** (0.388)	-0.000557 (0.00131)	-0.00899*** (0.00147)	0.000411 (0.00121)
Post2	6.767*** (0.455)	-0.0151*** (0.00151)	-0.0234*** (0.00171)	-0.0120*** (0.00139)
Post3	7.002*** (0.562)	-0.0348*** (0.00182)	-0.0425*** (0.00208)	-0.0361*** (0.00164)
N	3136698	3592376	3592376	3592376
Panel B: Idaho				
	(1)	(2)	(3)	(4)
	Credit Score	Account 30 Days Delinquent	Account 60 Plus Days Delinquent	Credit Card 30 Days Delinquent
Post1	2.265*** (0.821)	0.000384 (0.00253)	-0.00599** (0.00284)	0.00237 (0.00229)
Post2	3.859*** (0.915)	-0.0185*** (0.00268)	-0.0143*** (0.00319)	-0.0171*** (0.00238)
Post3	12.37*** (1.102)	-0.0406*** (0.00294)	-0.0481*** (0.00338)	-0.0404*** (0.00247)
N	764978	843758	843758	843758
Panel C: Texas				
	(1)	(2)	(3)	(4)
	Credit Score	Account 30 Days Delinquent	Account 60 Plus Days Delinquent	Credit Card 30 Days Delinquent
Post1	2.430*** (0.253)	0.00651*** (0.000895)	-0.00140 (0.00101)	0.00550*** (0.000836)
Post2	2.688*** (0.268)	-0.0114*** (0.000930)	-0.0203*** (0.00106)	-0.0142*** (0.000855)
Post3	3.803*** (0.348)	-0.0412*** (0.00109)	-0.0457*** (0.00128)	-0.0450*** (0.000977)
N	4619515	5190941	5190941	5190941

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models include county-level and month by year fixed effects, unemployment rate state and year of graduation, year of graduation fixed effects. Samples are weighted by the synthetic control weights in Table 2.

Table 5: Border State Sample Results

Panel A: Georgia (Florida)				
	(1)	(2)	(3)	(4)
	Credit Score	Account 30 Days Delinquent	Account 60 Plus Days Delinquent	Credit Card 30 Days Delinquent
Post1	0.937** (0.384)	-0.000896 (0.00129)	-0.00637*** (0.00145)	-0.0000535 (0.00119)
Post2	-1.866*** (0.448)	-0.00620*** (0.00146)	-0.00782*** (0.00167)	-0.00387*** (0.00135)
Post3	-2.134*** (0.557)	-0.0279*** (0.00178)	-0.0284*** (0.00203)	-0.0301*** (0.00160)
N	1765081	2064524	2064524	2064524
Panel B: Idaho (Montana, Wyoming)				
	(1)	(2)	(3)	(4)
	Credit Score	Account 30 Days Delinquent	Account 60 Plus Days Delinquent	Credit Card 30 Days Delinquent
Post1	5.220*** (0.876)	0.00506* (0.00272)	-0.00246 (0.00305)	0.00838*** (0.00246)
Post2	3.538*** (1.054)	0.00291 (0.00317)	0.00554 (0.00370)	0.00488* (0.00283)
Post3	9.671*** (1.311)	-0.00977*** (0.00368)	-0.0183*** (0.00420)	-0.00926*** (0.00317)
N	234232	256116	256116	256116
Panel C: Texas, (New Mexico)				
	(1)	(2)	(3)	(4)
	Credit Score	Account 30 Days Delinquent	Account 60 Plus Days Delinquent	Credit Card 30 Days Delinquent
Post1	5.087*** (0.295)	-0.00254** (0.00104)	-0.0112*** (0.00117)	-0.00334*** (0.000968)
Post2	4.489*** (0.411)	-0.0319*** (0.00145)	-0.0410*** (0.00163)	-0.0356*** (0.00134)
Post3	5.357*** (0.522)	-0.0687*** (0.00177)	-0.0729*** (0.00200)	-0.0738*** (0.00162)
N	1710105	1983611	1983611	1983611

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models include county and quarter-by-year fixed effects, unemployment rate state and year of graduation, year of graduation fixed effects. Samples are weighted by the synthetic control weights in Table 2.

Table 6: Synthetic Control Sample Results By Zip Code-level Education

		Low Education				High Education			
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Georgia		Credit Score	Account 30	Account 60 Plus	Credit Card 30	Credit Score	Account 30	Account 60 Plus	Credit Card 30
		Days Delinquent	Days Delinquent	Days Delinquent	Days Delinquent	Days Delinquent	Days Delinquent	Days Delinquent	Days Delinquent
Post1	4.276*** (0.557)	0.000205 (0.00201)	-0.00601*** (0.00225)	0.00235 (0.00185)	4.299*** (0.528)	-0.00228 (0.00167)	-0.0123*** (0.00189)	-0.00247 (0.00154)	
Post2	3.935*** (0.667)	-0.0134*** (0.00232)	-0.0198*** (0.00263)	-0.00965*** (0.00213)	4.470*** (0.607)	-0.00700*** (0.00191)	-0.0135*** (0.00218)	-0.00529*** (0.00178)	
Post3	3.492*** (0.867)	-0.0382*** (0.00292)	-0.0433*** (0.00334)	-0.0393*** (0.00261)	5.882*** (0.718)	-0.0230*** (0.00221)	-0.0293*** (0.00253)	-0.0254*** (0.00201)	
N	1351821	1610500	1610500	1610500	1788240	1986627	1986627	1986627	
Panel B: Idaho ... Omitted due to small sample size ...									
Panel C: Texas		Low Education				High Education			
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Credit Score	Account 30	Account 60 Plus	Credit Card 30	Credit Score	Account 30	Account 60 Plus	Credit Card 30		
	Days Delinquent	Days Delinquent	Days Delinquent	Days Delinquent	Days Delinquent	Days Delinquent	Days Delinquent		
Post1	4.068*** (0.358)	0.00551*** (0.00134)	-0.00387** (0.00151)	0.00519*** (0.00125)	2.441*** (0.350)	0.00488*** (0.00118)	-0.00238* (0.00133)	0.00358*** (0.00111)	
Post2	2.866*** (0.384)	-0.0136*** (0.00139)	-0.0223*** (0.00159)	-0.0171*** (0.00126)	2.190*** (0.365)	-0.00793*** (0.00122)	-0.0168*** (0.00137)	-0.00998*** (0.00114)	
Post3	4.021*** (0.510)	-0.0411*** (0.00172)	-0.0454*** (0.00200)	-0.0460*** (0.00153)	3.062*** (0.462)	-0.0388*** (0.00133)	-0.0431*** (0.00156)	-0.0418*** (0.00120)	
N	2454523	2810420	2810420	2810420	2169994	2386947	2386947	2386947	

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Low education is defined as below median (less than 81 percent) of high school graduates in a given zip code. High education defines zip codes with greater than median percent of high school graduates. Models include county-level and month by year fixed effects, unemployment rate state and year of graduation, year of graduation fixed effects. Samples are weighted by the synthetic control weights in Table 2.