The Effects of State Policies Requiring High School Personal Finance Education on Credit Scores*

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Abstract

Young adults frequently have poor credit histories, which can impede economic opportunities. Several states in the United States have implemented policies mandating personal finance courses prior to graduation from high school. These policies could result in young adults engaging in positive credit behaviors earlier in life, which in turn will result in higher credit scores. This quasi-experimental study estimates the effects of personal finance education requirements in three states on credit scores, relative to students in nearby states without financial education requirements. Overall, students graduating from high school in states with newly implemented personal finance education policies have 15 to 19 points higher credit scores. These results are consistent with state financial education requirements providing financial benefits for young adults.

Keywords: financial education; credit behavior; credit default

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Introduction

Young adults often show patterns of negative credit histories, which can limit access to credit, increase the costs of credit and become a barrier to economically valuable consumption and investment (Jiang & Dunn 2013; Lusardi, Mitchell, & Curto 2010; Sherraden & Grinstein-Weiss 2015). The relative naivety of young adults could lead them to neglect the importance of credit management, engaging instead in financial behaviors that may even be counter to their own long-run preferences (Laibson 1997; Stango & Zinman 2014).

In the United States, a growing number of states are adopting policies to require personal finance and economic education content. While economics courses in public K-12 education date to the 1950s, personal finance is a newer addition to state curricula. An important policy question is if these new policies actually benefit students. Since schools have a limited amount of instructional time available, policymakers must carefully assess the value of adding new content.

Prior studies do not provide clear evidence on the effectiveness of state financial education mandates in the United States (Bernheim, Garrett, & Maki 2001; Brown, Grigsby, van der Klaauw, Wen, & Zafar 2016; Cole, Paulson, & Shastry 2013; Tennyson & Nguyen 2001). One challenge to producing robust estimates of the effects of financial education policies is that there is significant heterogeneity in state requirements, content and implementation, both across and within states (Herman et al. 2015). There is also a lag between a policy being enacted and its eventual implementation. Moreover, new policies for financial education might be enacted at the same time as other education reforms or during economic downturns, which might bias estimates. The optimal approach would be a randomized experiment within or across states. Indeed, the results of randomized experiments by Batty, Collins, and Odders-White (2015) and Bruhn, de Souza Leao, Legovini, Marchetti, and Zia (2013) are encouraging in that students can learn specific financial content and even participate in basic tasks of financial planning. However, the former study was conducted among in elementary schools who have yet to establish economic independence, and the latter took place in Brazil. The effects of a state financial education policies on the behaviors young adults after graduating from school in the U.S. is still understudied.

This paper focuses on financial education policies implemented in 2007 in states where no other curricular reforms were simultaneously enacted: Georgia, Idaho, and Texas. In each

state, there was a bordering state with no comparable education policy change. This provides a framework for a difference-in-differences model to estimate the change in credit scores for students exposed to financial education policies before and after implementation to students in nearby states without financial education policies. The results indicate that 22-year-olds in states with new financial education policies had a 15-point higher credit score in Georgia, a 16-point higher credit score in Idaho, and a 19-point higher credit score in Texas. The estimated effect sizes are larger in the third year of implementation compared to the first year of implementation, highlighting the importance of taking into account implementation lags when estimating the effect of changes in education polices.

This paper begins with a brief background on personal finance education policies, as well as the specific policies implemented in 2007 in Georgia, Idaho, and Texas. After describing the data and the identification strategy used to estimate the direction and magnitude of the effects of state financial education policies on credit scores, the this paper summarizes the empirical findings and concludes with a discussion of implications for research and policy.

Background: Financial Education Policy in Public Schools

Policymakers describe a range of rationales for adding personal finance into school-based programs, but a primary motivation is based on the idea that students with more specific content knowledge will engage in more responsible financial behaviors. Of course, people acquire financial knowledge in a variety of ways; school-based programs may simply accelerate a natural developmental learning process. However, learning financial management skills earlier may be beneficial. Young adults often exhibit credit management problems as they become economically independent. Focusing students on the salience of credit management at a young age may help prevent early problems, and ultimately result in better credit scores for young adults. Some students may directly benefit from the information provided in classes; other students may learn from advice and the personal finance behavior modeled by other young adults in their age cohort. Young people may also view the state's course requirement as a signal that acquiring financial knowledge is valuable, leading to independent learning about the required topics.

While much of policy research on public school curriculum requirements is related to knowledge gains from STEM education (Johnson 2012), standards (Carr, Bennett, & Strobel 2012),

or No Child Left Behind (Dee & Jacob 2011), personal finance curriculum standards are one of a number of policies designed to shift the behavior of students later in life. Other examples of education policies intended to influence behavior are related to public health issues (Jones 2008), such as education on how risky behaviors can cause HIV infections (Anderson 2013), or relationship education (Gardner & Boellaard 2007). Studies have also examined the effects of anti-smoking education and sex education on the behavior of young adults (Thomas, McLellan, & Perera 2013; Walcott, Chenneville, & Tarquini 2011). These studies generally find modest, or mixed results, raising questions about whether or not these approaches are worth the costs.

In 2006, the National Association of State Boards of Education recommended states implement a policy of requiring financial education in K-12 curricula. By the 2014-15 academic year, about half of states implemented some form of personal finance content into statewide curricula, although with varying intensity and rigor (Council for Economic Education 2014).

We examined curricula publicly available from state departments of education for K-12 programs nationally (see `http://bit.ly/1IxewJk`). A total of 17 states had personal finance education requirements in place at some point during the 2000s, as shown in Table 1. Of these, five pre-dated 2000. A total of 12 states implemented personal finance education requirements beginning on or after the year 2000.

We set several criteria to select which of the 12 newly implemented state policies to use to estimate the effects of financial education on credit scores. First, we focus on states with curriculum changes occurring after 2005. We select this year to provide enough pre-policy periods to verify the parallel trends assumption of the difference-in-differences specification after the year 2000 (the first year we have credit data available). Second, we select states with no other changes in math and economics education requirements simultaneous to the financial education requirements, or where a bordering state had a major change in personal finance, mathematics, or economics education curricula. These selection criteria result in three states: Georgia, Idaho, and Texas. Two of these three have a large number of students, and all implemented substantive high school-based courses with graduation requirements and statewide support for teacher training and materials. These three states all implemented these requirements in 2007— and represents the single year with the most significant implementation of state personal finance education requirements. Each of these states had well-documented curricula and met or exceed

the standards of the Council for Economic Education (Council for Economic Education 2014). All three states integrated personal finance education requirements into a course for high school students. Other aspects of these state policies were more unique.

Georgia

The Georgia Board of Education first approved a statewide mandate incorporating financial education into the K-12 curriculum in 2004. The first students affected by this policy graduated in the spring of 2007. Prior to 2007, a one-half credit economics course was required to be taught in Georgia high schools, but the course was not required to address personal finance topics.

The year-long, required class was developed by the Georgia Council for Economic Education and titled "Let's Make it Personal''. Topics included financial planning, savings, insurance, and credit. The course included economic simulations to encourage students to engage in applied learning (Hinojosa et al. 2007, 2009; Walstad & Buckles 2008). Prior studies of the effects of simulations have shown positive results, although prior studies are likely to be biased by the positive selection of more affluent schools and students into simulations offered as extra curricular programs or elective courses (Harter & Harter 2010; Mandell & Klein 2007; Mandell & Schmid Klein 2009; McCormick 2009).

Notably, Georgia included a systematic implementation of a standardized set of course content across schools, as well as student performance testing on personal finance topics. Topics included: (1) budgeting, (2) savings, (3) the role of financial institutions, (4) monetary and fiscal policy, (5) the costs and benefits of using credit, (6) the role of insurance, and (7) employment and earnings.

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'' (Section 53A-1-402). Beginning with the graduating class of 2007, all students in Idaho were required to take one semester of economics as part of a three-credit social studies graduation requirement.

The curriculum for this course was developed by Idaho State University and included five

segments: (1) the basics of economics; (2) credit and debt, including how to apply for loans and how to manage credit and credit reports; (3) saving and investing; (4) money management, including forming goals, managing income, paystubs and taxes; and (5) weighing the costs and benefits of making a purchase. The course also included other topics, such as making a resume and applying for jobs, as well as understanding consumer rights and responsibilities and avoiding fraud.

Texas

A 2004 amendment of the Texas Education Code (Section 1A-28-28.0021) required economics classes in grades 9 through 12 to include personal finance in the economics curriculum. Schools were required to implement standardized materials beginning with the graduating class of 2007. The course content included: (1) understanding interest and managing 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 how to be a low-risk borrower, (11) understanding insurance, and (12) charitable giving.

Comparison States

For each treatment state (Georgia, Idaho and Texas), we selected a bordering state that did not have a change in personal finance education requirements, or a change in mathematics requirements, based on the Education Commission of the States Reports and the Center for the Study of Mathematics Curriculum. These border states were selected in part because they were more likely to be similar to their neighboring states, providing a counterfactual for the difference-in-differences model. Figure 1 shows a map of these states.

Georgia has five bordering states: Florida, South Carolina, Alabama, Tennessee and North Carolina. Alabama is a demographically similar state bordering Georgia that did not have a personal finance requirement change during the study period. Alabama does have a pre-existing

one-half credit economics requirement dating back to 1989. However, there were no changes to this requirement during the sample period. Other bordering states had changes in curricula during this period. Florida was rejected as a comparison for Georgia for a variety of factors, including its heterogeneity, high degree of local control over curriculum and broader education reforms implemented during the study period.

Idaho has six bordering states: Washington, Oregon, Nevada, Utah, Montana, and Wyoming. Montana shares similar demographic characteristics to Idaho and had no economics or personal finance requirement during the study period. Other states were rejected due to recent changes in mathematics, economics or personal finance requirements.

Texas has four bordering states: Arkansas, Louisiana, New Mexico and Oklahoma. While Oklahoma imposed rigorous standards for economics and personal finance in 2014, there were no requirements prior to that date. New Mexico and Arkansas were rejected as comparison states for Texas due to the high degree of local control over curricula and a lack of clear statewide mandates. Louisiana was not an ideal comparison state because the state's education policies and practices were strongly impacted by natural disasters in the region during the 2000s.

We recognize that the selection of comparison states in this manner could result in a biased counterfactual. To address this concern, we only use a difference-in-differences model, where the border states establish a control condition with parallel trends before 2007, prior to the implementation of the financial education policy in Georgia, Idaho and Texas. We also offer a series of robustness tests and variations in estimating standard errors to address concerns about a relatively small set of areas with and without policy changes. We also offer a secondary analysis that uses a synthetic controls method (Abadie, Diamond, & Hainmueller 2010; Abadie & Gardeazabal 2003) for comparison states (included in the Appendix).

Data

The source of credit 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 is regularly updated to include new credit files and remove the files of deceased persons or those with inactive credit files. Lee and Van der Klaauw (2010) provide a detailed description of the CCP sample design, as well as comparisons of CCP estimates of outstanding credit to other aggregate national estimates. The data are generally considered robust and represent the credit profiles of people with Social Security identifiers.

We begin our analysis with people who are the same age as the graduating class of 2000 and follow them, and each subsequent cohort of high school graduates, for four years (from age 18 through 21). We observe the age of the individual in the credit record and the ZIP code when they first enter the credit bureau database, in most cases starting at age 18. The personal finance mandates being studied were first implemented with the graduating class of 2007. The three graduation-year cohorts we followed after the personal finance course mandates were implemented are shown in Figure 2: the classes of 2007, 2008, and 2009. Three graduating classes is the limit to estimating effects because the control states begin to implement course requirement changes beyond this sample period. There were no systematic differences in the length of observation across cohorts. Individuals are dropped from the sample in the quarter that they turn 22 years old. We compared the trajectory of each pre-policy cohort to post-policy cohorts, relative to people in other states in each cohort not subject to any education mandates. We did not compare younger borrowers to older borrowers, as younger borrowers simply have less time to establish a credit history or become delinquent.

Establishing credit is one of the first financial management activities observed among young adults. Credit scores are largely determined by the length of a person's credit record, his utilization rates of revolving accounts, and his history of on-time payments (Avery, Brevoort, & Canner 2009). Young people can establish a credit history by applying for and receiving credit, being an authorized user on another person's account (such as a parent), or having an account co-signed by someone with established credit.

A credit score is a summary measure of credit behavior (Arya, Eckel, & Wichman 2011) and has been used in prior studies as an outcome for evaluations of financial education programs (Birkenmaier, Curley, & Kelly 2012). However, credit scores may not be updated in a timely way to reflect small changes in credit use, especially among young people with few accounts or

much credit use. Given the importance of the length of the credit record for determining a credit score, it is difficult for young people to have very high credit scores. Missed payments are the most common reason for low credit scores, and are common among younger borrowers. 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. Given that young people are just establishing credit at ages 18 to 21, we are measuring relatively small relative differences in scores among students exposed to financial education policies.

We estimated the effects of financial education requirements on Equifax Risk Scores credit scores, similar to the FICO score. These scores range from 280 to 850, with a higher score indicative of the borrower being a better credit risk. In order to better understand if students exposed to financial education are paying off credit or taking on more credit, we also examined delinquency rates and total debt borrowed. Delinquency is defined as being 30 days behind on a payment, a relatively mild delinquency, or 90 days behind, which is a more serious delinquency. Total debt is aggregated across all active credit accounts, including credit cards, student loans, mortgages, auto loans, and all other consumer credit.

Historically, young people enter into the credit market using revolving credit cards (Fry 2013; Sotiropoulos & d'Astous 2012). However, the Credit Card Accountability Responsibility and Disclosure Act of 2009, or CARD Act (Public Law 111–24) went into effect in February 2010 (see Debbaut, Kudlyak, and Ghent (2014) for an assessment of the effects of the CARD Act). The law required credit card applicants younger than age 21 to have a source of income or an adult co-signer before obtaining a card. However, this law was national in scope, meaning both treatment and control states were impacted. Moreover, the class of 2007 would not have been prevented from obtaining a credit card, and the class of 2008 would have only been affected by one year or less. The class of 2009, the last class we examined in this analysis, would have had more barriers to credit, but not when they initially graduated from high school (most would have been 19 or 20 when the CARD Act was implemented).

Empirical Methods

We compared the changes in credit scores before and after the implementation of the financial education mandates across states with and without personal finance education. We use a

difference-in-differences 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 state within the same time period. Table 2 shows descriptive statistics; the border states appear to closely approximate the treatment states, with only a few statistically significant differences that are small in magnitude.

The difference-in-differences approach requires three assumptions. First, individuals begin their credit records in the same state where they attended high school, which is consistent with Brown et al. (2016), who documented that 93 percent of individuals stay in the same state from ages 18 through 22. Second, students graduating from Georgia, Idaho and Texas were exposed to the financial education graduation requirement if they turned 18 in 2007 or later. However, if some of those people who we classify as being in the financial education treatment did not in fact receive the financial education, it would only serve to bias our estimates toward zero. The third assumption is that individuals in the states with financial education course requirements would have had similar relative trends in financial outcomes to those in the control state in the absence of the policy. We are only able to provide evidence of this by showing patterns in the data that are consistent with this assumption.

The panel structure of the data allows for contemporaneous estimates in any given period, as shown in the following specification:

$$\begin{aligned} Y_{ist} = \alpha_0 + \beta_1(T_s \times \mathrm{P1}_{it}) + \beta_2(T_s \times \mathrm{P2}_{it}) + \beta_3(T_s \times \mathrm{P3}_{it}) \\ + \gamma_1 u_{it} + \kappa X_{it} + \eta_t + \epsilon_{ist} \end{aligned} \tag{1}$$

The outcome of interest, labeled Y_{ist}, is the individual's credit score in each quarter, t. We also estimate three additional dependent variables that each contribute to the credit score, namely 30 and 90 day delinquencies on any credit account and total debt levels.

T_s is a dummy variable that equals one if the individual lived in the treated state in the sample (Georgia, Idaho, or Texas) versus a control state. $\mathrm{P1}_{it}$ equals one if the individual turned 18 in the first year the financial education requirement was enacted, or 2007. $\mathrm{P2}_{it}$ equals one if the individual graduated high school in 2008. Finally, $\mathrm{P3}_{it}$ equals one if the individual graduated high school in the third year the financial education requirement was enacted, or 2009. This

coefficient estimates the effects of the state mandates for each subsequent graduating class from 2007 through 2009.

u_{it} is the unemployment rate in the young adult's current county of residence; this is intended to control for contemporaneous labor market conditions that may also affect credit supply and repayment. X_{it} includes the number of credit accounts an individual has in a given period and the number of quarters he has been observed in the credit panel data. These variables are included to account for differences in exposure to credit risks. Finally, η_t incorporates quarter-by-year fixed effects to account for quarter- and year-specific factors not observed in other variables.

Standard errors are estimated clustered at the individual level, although different levels of clustering provide similar results.

Results: Credit Scores

Table 3 presents difference-in-differences estimates of the effect of state personal financial education on credit outcomes for young adults in each of our three treatment states. Panels A, B, and C show the effect of the financial education mandates in Georgia, Idaho, and Texas, respectively. We begin in Column (1) of Table 3, where we show that the education delayed the number of quarters observed in the credit data in all states. This delay is smaller for the first graduating cohort (roughly one to one and and a half months) and becomes larger for the third graduating cohort, reaching almost three months in Georgia and Idaho and four months in Texas. This suggests that perhaps those exposed to the education became more risk averse, delaying their initial debt accumulation. However, the magnitude of this effect is small.

In Column (2) of Table 3, we show the effect of each state's personal finance course requirement on credit scores. In all cases, the implementation of state financial education mandates is associated with relatively higher credit scores for young adults, with the effect becoming greater in magnitude for each subsequent cohort. The increasing magnitude for each subsequent cohort could be driven by delays in implementation across all schools within a state, thus reducing the number of students actually treated, or by teachers becoming more familiar with the course material over time and therefore better able to educate the students. The relative difference may also be related to stronger peer and social network influences on credit behavior. Regardless, the general trend is a stronger relative score each year after 2007, controlling for other factors

and using a differences model.

In Georgia, the first cohort to receive the financial education is estimated to have a credit score 2.2 points greater than the comparison group, increasing to 9.9 points for the second cohort and 15.1 points for the third cohort. The magnitudes for the increases in credit score for Idaho are similar to those found in Georgia, at 2.8 points, 12.6 points, and 16.4 points for the first, second, and third cohorts, respectively. Texas' financial education mandate appears to have been most effective from the outset, with the first cohort having a credit score that is 6.6 points greater, increasing to 13.7 points for the second cohort and 19.3 points for the third cohort. To provide some context for the size of these estimates, the standard deviation for credit score was 88 in Georgia and Texas and 83 in Idaho, meaning that our maximum estimate implies a 0.22 standard deviation (sigma unit) increase in average credit score. This is a modest effect size relative to the general education literature (Hill, Bloom, Black, & Lipsey 2008).

The different effect sizes by state highlight two important issues regarding the heterogeneity of the financial education mandates. First, average credit scores are relatively low in Texas, at just under 610. In Idaho, the average credit score is closer to 635, almost 25 points higher. Thus, the differences in the effect size could be generated by a lower baseline credit score in Texas than Idaho. Second, the differences in the intensity of the education program may also generate different effect sizes across states. For example, Texas and Georgia have similar average credit scores. However, the effect size in Texas is double the effect size in Georgia in the first year. By year three, these two estimates are closer in magnitude, suggesting that implementation may factor into the magnitude of the effect across states. These two points highlight the need for understanding the specific education provided. Studies looking at average effects across all states miss these heterogeneous effects.

Delinquency

Columns (3) and (4) of Table 3 show estimates of the effect of the financial education on being delinquent 30 days or 90-plus days on any credit account. The pattern of estimates is similar to that found for credit scores, with the magnitude of the effect generally increasing for each subsequent cohort. However, we observe greater heterogeneity in the effect of the financial education mandates on delinquency across the three states. In Georgia, the first cohort

experiences only a modest, and marginally significant, reduction in 30-day delinquency and no significant decline in 90-plus-day delinquency. However, the reduction in delinquency becomes more pronounced and statistically significant for the second cohort. By the third cohort, 30-day delinquency is estimated to decline by 0.6 percentage points and 90-plus-day delinquency by 1.4 percentage points. Mean 30-day delinquency of 5 percent and mean 90-plus-day delinquency of 19 percent imply 9 percent and 7 percent respective declines in delinquency for the third cohort that received the financial education. The greater effect of financial education on 30-day delinquency suggests that Georgia's education may be increasing the attention that young adults devote to the payment of their bills, while having more modest effects on the types of financial problems that result in severe delinquency.

Idaho's financial education mandate appears to have no effect on delinquency for the first cohort, but then sharply reduces delinquency for the second and third cohorts. As in the case of Georgia, Idaho's education has similar effects on both 30-day and 90-plus-day delinquency, reducing 30-day delinquency by 11 percent and 90-plus-day delinquency by 9 percent for the third cohort.

Texas' financial education mandate appears to have been the most effective when measured by declines in delinquency relative to the other state estimates. From the first cohort, Texas young adults experienced large and statistically significant declines in both 30-day and 90-plus-day delinquency that continued to increase for each subsequent cohort. By the third cohort, the financial education reduced 30-day delinquency by 0.6 percentage points and 90-plus-day delinquency by 2.1 percentage points, or 16 percent and 12 percent, respectively.

Debt Levels

A priori, the effect of financial education on the average amount of debt accumulated by young adults is ambiguous. A financially well-informed young person may rationally choose to acquire more debt to finance greater current consumption and invest in his human capital, or the education may encourage saving and reduce borrowing, especially for students not choosing to invest heavily in their human capital. Another possibility is that a financially-educated person is more risk averse, leading him to seek out and accumulate less debt. Table 4 shows estimates for the effect of each state's financial education mandate on total debt, relative to comparison states.

Given the right-skewed distribution of debt for young adults, we first present estimates for the log of total debt in Column (1), followed by estimates from a quantile regression at the 25th, 50th, and 75th percentile of the total debt distribution. Panel A shows an estimated increased amount of debt accumulated in Georgia, by 14 percent for the second cohort and 30 percent for the third cohort. However, as shown by the quantile regressions, this overall average estimate misses important variation across the distribution of debt balances. At the 25th percentile ($700), the third cohort is estimated to accumulate an additional $629 in debt. At the 50th percentile ($4,331), the third cohort is estimated to accumulate an additional $809 in debt. At the 75th percentile ($11,766), debt actually declines by $453. Thus, those young adults at the low end of the debt distribution borrow substantially more in percentage terms after graduating under financial education mandates, while those at the high end of the debt distribution reduce their borrowing. This pattern is broadly consistent with financial education recalibrating both low-debt and high-debt level borrowers towards relatively more moderate debt levels.

The effect of Idaho's financial education on the amount of debt accumulated by young adults is substantially different than that observed for Georgia. Only the estimate for the third cohort shows statistically significant effects on debt levels, with an estimated 7 percent increase in borrowing. These average estimates still mask variation that becomes evident once performing quantile estimates by level of debt. All but one of the estimates from the quantile regressions show a negative effect of financial education on total debt, with a particularly pronounced reduction at the first quantile for all cohorts. The average effect appears to be a result of large percentage increases in debt for people in the first quantile of the debt distribution. These effects suggest Idaho's financial education mandate may have discouraged some young adults from borrowing at all. The reduction in delinquency we find in Columns (3) and (4) in Panel B may be in part due to a shift in the composition of young people entering the credit market and how much those who enter the market end up borrowing.

The results for Texas, presented in Panel C, are similar to those for Idaho. In Column (1), only the coefficient estimate for the third cohort is statistically significant, indicating a 12 percent increase in average total debt. However, all coefficients from the quantile regressions are large in relative magnitude and statistically significant. For every cohort and at all three points in the distribution of total debt, we observe declines in debt for Texas young adults who received

financial education. Focusing on the third cohort, we see declines in total debt of $42 at the first quantile ($584), $642 at the second quantile ($3,889), and $1,742 at the third quantile ($11,990). As with the estimates for Idaho, the positive average effect in Texas appears to be associated with large percentage increases in debt for people at the low end of the debt distribution.

Robustness Checks

The unbiased identification of the effect of the financial education mandates on subsequent financial outcomes depends on the parallel trends assumption–that is that the treatment and control states had consistent relative differences before treatment. Figure 3 provides a visual comparison and is reassuring. We also repeat this for account balances in Figure 4. However, we also pursue several extensions in order to demonstrate the robustness of the prior findings.

First, we restrict our three estimation samples to individuals who we first observe in counties on the *contiguous borders* of *both* treatment and control states. By focusing our comparison on individuals who are geographically more proximate, we hope to minimize any potential unobserved heterogeneity that could bias the prior estimates. Results from these regressions are reported in Table 5 and are highly similar to those found in Table 3 using the entirety of both treatment and control states.

Second, we estimate the results shown in Table 6, where all the states shown in prior tables are combined together in one specification with six states total. The average treatment effect on credit scores for Georgia, Idaho, and Texas combined, relative to the three control state combined, is estimated using the same difference-in-differences approach as in the main results. In this model, we are also able to cluster standard errors at the state level (six clusters)—the unit of analysis of the policy being evaluated. This estimate provides evidence that the correlations within states over time are not biasing the standard errors to such an extent that we are falsely claiming statistically significant effects.

Third, we further re-calculate the standard errors from the main estimates for credit scores to address the small number of clusters in our data. In the difference-in-differences literature, there are concerns that a small number of clusters will bias the standard errors (Bertrand, Duflo, & Mullainathan 2004; Cameron, Gelbach, & Miller 2008). The estimates in prior analyses are all based on panel data of individuals over time, with standard errors clustered by individual. In

Table 7 we show confidence intervals for estimates of the effects of financial education mandates on credit scores using alternative clustering techniques. Column (1) of Table 7 shows the 95 percent confidence intervals for the prior estimates in Table 3. Column (2) shows estimates with state level clustering. Column (3) repeats state level clustering of standard errors using bootstrapping. Column (4) shows two-level clustering by state and year as discussed by Cameron et al. (2008) and Cameron, Gelbach, and Miller (2011) and Cameron and Miller (2015). In Column (2), we can no longer rule out that the true effect of state mandates on credit scores is zero in Georgia. However, in Column (3) where we use bootstrapping methods to cluster the standard errors, with 400 repetitions as suggested by Cameron and Miller (2015), the 95 percent confidence interval does not contain zero for any state. The results remain consistent, albeit less precise, for both Idaho and Texas across all alternative clustering models. Using two-way clusters across state and time in Column (4) also produces 95 percent confidence intervals of credit score levels that are consistent with the effect sizes in the main estimates in Table 3.

Fourth, as a further robustness check we supplement our border counties analysis with an analysis using a synthetic control approach similar to Abadie and Gardeazabal (2003) and Abadie et al. (2010) to select and weight control states. We explain this methodology in the Appendix, and also show that the results are similar to those found using the border state estimates in Table 3.

Implementation Timing

Prior studies on financial education policies have generally used the date that the state mandate was approved to estimate the effects of a new policy. In reality, the date when students actually receive any required financial education might be several years later. We repeat our border state analysis from Table 3, but instead of using an indicator for the year the mandate was implemented, we use the date of mandate was enacted by the state. As shown in Table 8, and in stark contrast to the results presented in Table 3, the vast majority of the coefficient estimates are no longer statistically significant when using the date a state approved a mandate instead of the year of implementation. For example, only one coefficient remains statistically significant for Idaho. For Texas, the use of the mandate passage rather than implementation date yields only a handful of statistically significant coefficients, several in the opposite direction.

The results for Georgia yield only small increases in credit scores and reductions in 90-plus-day delinquency. This result is also shown in Table 9. Overall, these tables show that using the date the mandate was approved, rather than the date of implementation, biases the estimates towards zero.

Conclusions

The political economy of state education policy is complex. The trend toward offering personal finance graduation standards is based on the belief that additional financial education has the potential to endow students with financial knowledge that will in turn result in future positive financial behaviors. Although previous research studying the role of education on behavior has been mixed, this study suggests that, at least in the case of three specific state implementations of personal finance standards, these policies may have positive effects on the credit behaviors of young adults.

As a stylized illustration, consider that about 265,000 students graduated from high school in Texas in 2009. Given an average credit default (90 days+) rate of 18 percent, about 47,700 young people would experience a default by age 22. The prior estimates show that students who graduated after Texas's high school personal finance education mandate had a 2 percentage point lower default rate. Applying this 16 percent rate (instead of the 18 percent rate) means that 5,300 fewer young people would have experienced a serious credit default. Given the higher costs of credit associated with a history of serious default, these students are likely better off, at least in terms of lower interest rates and fees, as well as credit access. The state's policy allows affected students to consume more of their income in future periods and spend less on borrowing costs. These policies may potentially be a benefit for lenders as well, by allowing them to avoid collections and loan write-off costs for loans that would otherwise be delinquent in the absence of financial education. To the extent that defaults have negative externalities for families and communities, other costs may be avoided through the implementation of high school financial education.

Of course, there are opportunity costs for students and schools associated with a personal finance graduation requirement, and potentially a more robust math, art, or other requirement might also result in positive benefits for young people. We cannot assess the relative merit of a

personal finance mandate versus other mandates. We also must remain circumspect regarding how long young people will sustain these positive credit repayment habits. Education could simply end up delaying problematic behavior. It is possible that making mistakes in managing credit helps people to manage credit better later in life than a formal high school course. Future studies can use shifts in education standards to track the pre- and post- mandate cohorts later into adulthood to measure how well behaviors are maintained (or atrophy).

In several ways, these results complement the findings of Cole et al. (2013) and Brown et al. (2016). We look at the effects of financial education in only three states that had rigorously implemented mandates, providing an upper bound for the effect of financial education mandates in the high school curriculum. In contrast, both Cole et al. (2013) and Brown et al. (2016) look at all mandates passed, estimating an average overall effect. These estimates will be diluted by weaker policies, delayed implementation and differential impacts across the distribution.

Overall, however, the findings of this analysis are promising. To the extent that credit behaviors are a policy goal, K-12 curricula may prove to be an effective mechanism to shift the credit trajectory of young adults.

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Tables and Figures

Figure 1. Treatment and Control Samples

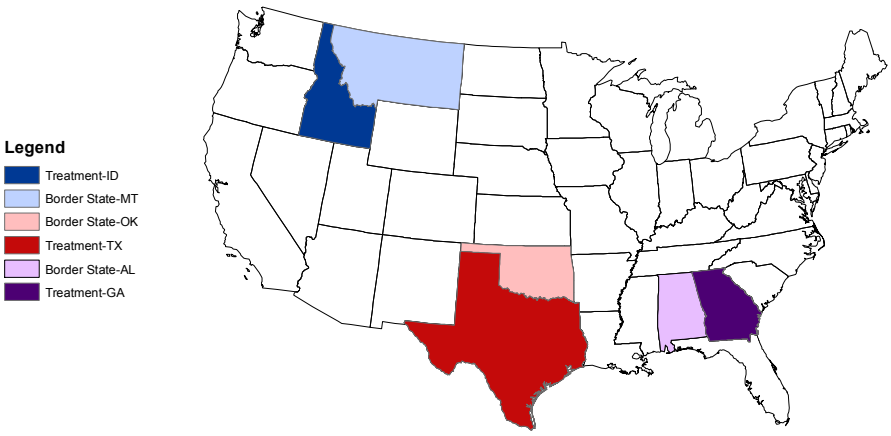
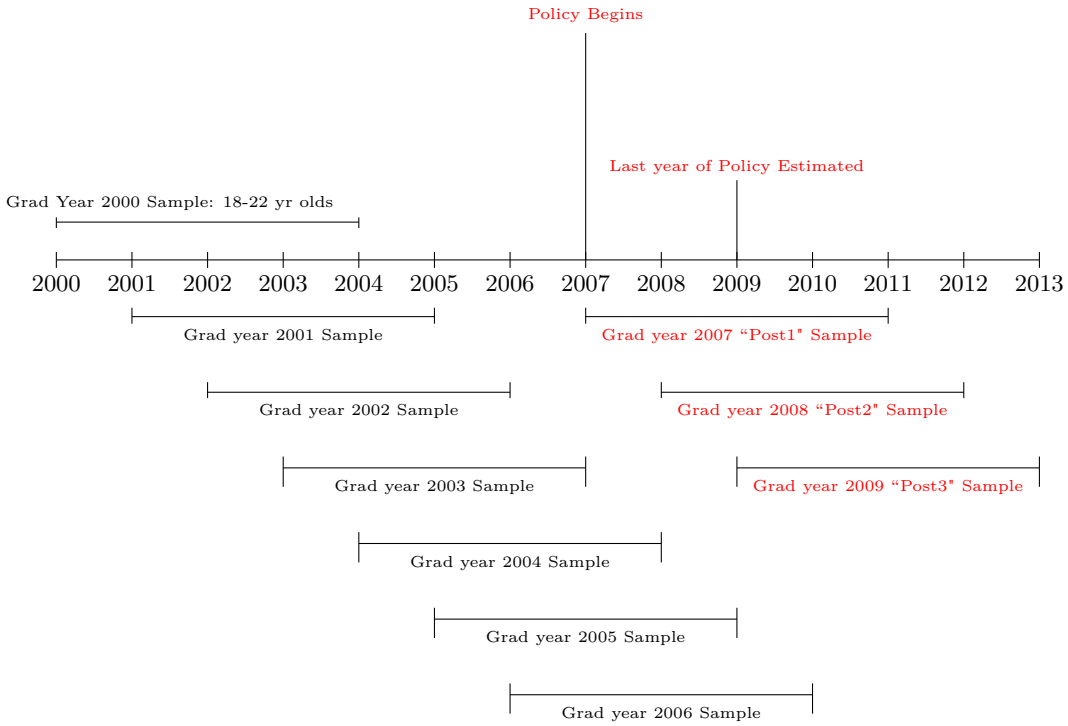


Table 1
State Personal Finance Graduation Requirements in the 2000s

State	Year	Course	Assessment
Georgia	2007	Economics	Yes
Idaho	2007	Economics	No
Texas	2007	Economics	Yes
Other States			
Arizona	2005	Economics	No
Arkansas	2005	Economics	No
Colorado	2009	Economics/Math	No
Illinois	1970s	Social Studies	Yes
Indiana	2008		No
Michigan	1998	Career course	No
New Hampshire	1993	Economics	No
New York	1996	Economics	No
North Carolina	2005	Economics	No
Rhode Island	1980		No
South Carolina	2009	Social Studies	No
Utah	2008		Yes
Washington	2000		No
Wyoming	2002	Social Studies	No

Figure 2. Policy Timeline and Sample Composition



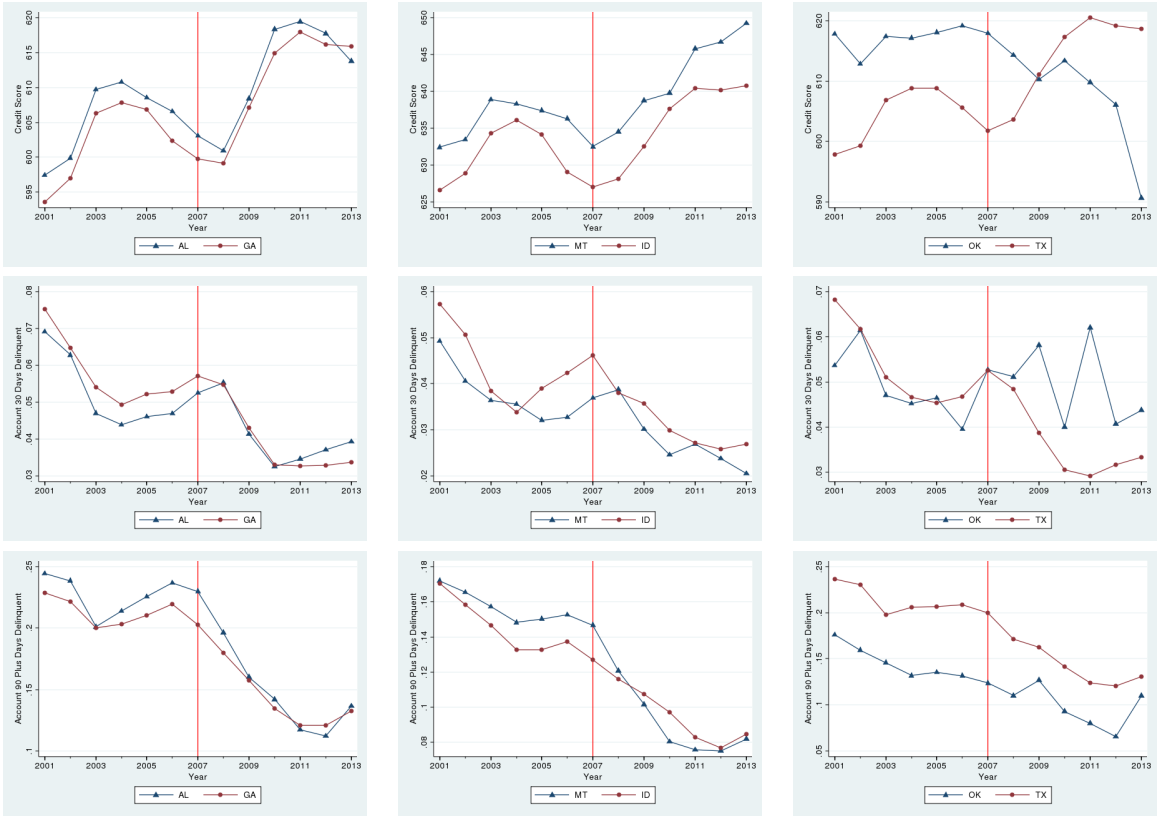


Figure 3. Difference-in-Differences Plots: States Implementing Education Mandates in 2007 Compared to Controls, by Credit Report Outcome

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

Table 2
Summary Statistics, Treatment versus Border States

	GA	Border (AL)	GA Sample	ID	Border (MT)	ID Sample	TX	Border (OK)	TX Sample
Credit Score	606.48	609.49	607.41	632.34	638.42	634.62	609.28	616.85	609.41
	(89.47)	(85.9)	(88.39)	(85.57)	(77.73)	(82.77)	(88.55)	(85.94)	(88.51)
N	571,513	256,141	827,654	108,932	65,414	174,346	1,474,186	27,180	1,501,366
30 Days Behind	0.05	0.05	0.05	0.04	0.03	0.04	0.04	0.05	0.04
	(0.21)	(0.21)	(0.21)	(0.19)	(0.18)	(0.19)	(0.20)	(0.21)	(0.20)
N	468,046	217,188	685,234	94,859	58,011	152,870	1,278,873	23,926	1,302,799
90+ Days Behind	0.18	0.19	0.19	0.12	0.13	0.13	0.18	0.13	0.18
	(0.39)	(0.39)	(0.39)	(0.33)	(0.34)	(0.33)	(0.38)	(0.34)	(0.38)
N	528,752	250,127	778,879	102,367	63,846	166,213	1,436,822	25,739	1,462,561
Total Balance	8185.17	8423.87	8260.03	6981.77	7868.91	7317.17	8150.87	9137.26	8169.81
	(11606)	(10918)	(11395)	(9621)	(10103)	(9816)	(11038)	(11731)	(11053)
N	473,200	216,197	689,397	95,746	58,203	153,949	1,225,920	24,003	1,249,923
County Unemployment	6.49	5.85	6.29	5.19	4.61	4.97	6.11	4.73	6.09
	(2.57)	(2.81)	(2.66)	(2.42)	(2.01)	(2.29)	(1.78)	(1.19)	(1.78)
N	615,672	283,896	899,568	111,241	68,709	179,950	1,604,445	29,012	1,633,457
Number of Accounts	2.08	2.23	2.13	2.29	2.49	2.36	2.37	2.60	2.37
	(2.25)	(2.39)	(2.3)	(2.17)	(2.41)	(2.27)	(2.49)	(2.49)	(2.49)
N	594,549	268,067	862,616	110,892	66,667	177,559	1,548,152	27,842	1,575,994
Number of Quarters	7.45	7.23	7.38	7.51	7.51	7.51	7.51	7.40	7.50
	(4.71)	(4.58)	(4.67)	(4.72)	(4.71)	(4.72)	(4.70)	(4.70)	(4.70)
N	655,255	301,006	956,261	118,400	72,502	190,902	1,706,101	29,655	1,735,756

Table 3
Border State Sample Results

Panel A: Georgia

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	-0.280***	2.199**	-0.00290*	-0.000942
	(0.0736)	(1.062)	(0.00154)	(0.00241)
β_2	-0.557***	9.899***	-0.00262**	-0.00709***
	(0.0858)	(1.212)	(0.00122)	(0.00181)
β_3	-0.919***	15.11***	-0.00605***	-0.0136***
	(0.0967)	(1.286)	(0.00125)	(0.00320)
N	808,729	771,632	644,279	644,279
Mean	7.38	607.41	0.047	0.185

Panel B: Idaho

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	-0.269***	2.848***	-0.00100	0.00459
	(0.0703)	(0.904)	(0.00277)	(0.00277)
β_2	-0.548***	12.58***	-0.00674**	-0.0111***
	(0.0802)	(1.186)	(0.00269)	(0.00329)
β_3	-0.907***	16.38***	-0.00434*	-0.0120***
	(0.0871)	(1.506)	(0.00253)	(0.00312)
N	166,979	163,568	144,268	144,268
Mean	7.51	634.62	0.036	0.125

Panel C: Texas

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	-0.518***	6.599***	-0.00324***	-0.00898***
	(0.0641)	(1.519)	(0.00110)	(0.00228)
β_2	-0.842***	13.71***	-0.00552***	-0.0181***
	(0.0644)	(1.775)	(0.00144)	(0.00221)
β_3	-1.241***	19.28***	-0.00644***	-0.0213***
	(0.0615)	(1.711)	(0.00163)	(0.00328)
N	1,479,654	1,401,100	1,224,241	1,224,241
Mean	7.50	609.41	0.044	0.177

Notes: Robust standard errors clustered at the individual level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. β_1 corresponds to the first graduating class affected by the requirement; β_2 corresponds to the following graduating class; β_3 is two classes after the first graduating class. Models include state-level and quarter-by-year fixed effects, current county-level unemployment rate, number of quarters the individual has appeared in the credit data, and their total number of accounts.

Table 4
Border State Sample Results: Total Balances

Panel A: Georgia

	OLS	Quantile Regressions		
	ln(Total Balance)	25^{th} Percentile	50^{th} Percentile	75^{th} Percentile
β_1	0.0265	-16.50**	91.93*	-83.05
	(0.0261)	(8.387)	(47.12)	(60.85)
β_2	0.142***	4.071	573.2***	-119.0*
	(0.0272)	(15.35)	(58.41)	(68.35)
β_3	0.298***	629.5***	808.9***	-453.3***
	(0.0252)	(63.13)	(62.18)	(66.46)
N	594,853	645,470	645,470	645,470
	Mean	25^{th}	50^{th}	75^{th}
	$8,260	$700	$4,331	$11,766

Panel B: Idaho

	OLS	Quantile Regressions		
	ln(Total Balance)	25^{th} Percentile	50^{th} Percentile	75^{th} Percentile
β_1	-0.0290	-73.77***	-133.7	327.0**
	(0.0351)	(19.67)	(83.06)	(149.5)
β_2	-0.0187	-110.1***	-507.1***	-236.6
	(0.0323)	(18.45)	(94.99)	(161.4)
β_3	0.0699**	-98.87***	-87.90	-485.7***
	(0.0316)	(30.02)	(124.9)	(172.1)
N	129,044	144,788	144,788	144,788
	Mean	25^{th}	50^{th}	75^{th}
	$7,317	$529	$3,785	$10,313

Panel C: Texas

	OLS	Quantile Regressions		
	ln(Total Balance)	25^{th} Percentile	50^{th} Percentile	75^{th} Percentile
β_1	-0.0348	-43.91***	-365.5***	-1069.7***
	(0.0326)	(4.645)	(16.91)	(54.65)
β_2	0.0466	-65.98***	-513.3***	-1230.3***
	(0.0342)	(7.669)	(28.47)	(61.08)
β_3	0.118***	-41.96***	-642.0***	-1742.1***
	(0.0374)	(11.02)	(43.34)	(71.29)
N	1,072,908	1,170,589	1,170,589	1,170,589
	Mean	25^{th}	50^{th}	75^{th}
	$8,170	$584	$3,889	$11,990

Notes: Robust standard errors clustered at the individual level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. $\beta_1$cool corresponds to the first graduating class affected by the requirement; β_2 corresponds to the following graduating class; β_3 is two classes after the first graduating class. Models include state-level and quarter-by-year fixed effects, current county-level unemployment rate, number of quarters the individual has appeared in the credit data, and their total number of accounts.

Table 5
Border County Sample Results

Panel A: Georgia

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	-0.439***	4.927***	-0.000777	-0.00229
	(0.0581)	(1.183)	(0.00171)	(0.00293)
β_2	-0.747***	14.37***	0.000278	-0.0131***
	(0.0647)	(1.326)	(0.00156)	(0.00253)
β_3	-1.144***	21.69***	-0.00328*	-0.0201***
	(0.0710)	(1.529)	(0.00181)	(0.00327)
N	392,399	374,695	310,280	310,280
Mean	7.46	609.64	0.045	0.181

Panel B: Idaho

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	-0.222***	4.962***	-0.00766*	0.00130
	(0.0691)	(1.247)	(0.00423)	(0.00493)
β_2	-0.473***	5.765***	-0.00558	-0.00451
	(0.0785)	(1.295)	(0.00451)	(0.00461)
β_3	-0.814***	16.68***	-0.0147**	-0.0104***
	(0.0880)	(1.332)	(0.00590)	(0.00329)
N	60,053	58,860	52,182	52,182
Mean	7.54	637.95	0.035	0.120

Panel C: Texas

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	-0.513***	6.917***	-0.00294**	-0.00613**
	(0.0651)	(1.417)	(0.00138)	(0.00285)
β_2	-0.833***	15.22***	-0.00441**	-0.0148***
	(0.0652)	(1.534)	(0.00173)	(0.00240)
β_3	-1.247***	22.38***	-0.00608***	-0.0183***
	(0.0620)	(1.409)	(0.00208)	(0.00440)
N	380,629	358,195	308,426	308,426
Mean	7.56	611.61	0.040	0.174

Notes: Robust standard errors clustered at the individual level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. β_1 corresponds to the first graduating class affected by the requirement; β_2 corresponds to the following graduating class; β_3 is two classes after the first graduating class. Models include state-level and quarter-by-year fixed effects, current county-level unemployment rate, number of quarters the individual has appeared in the credit data, and their total number of accounts.

Table 6
Combined States Results

	(1) Credit Score	(2) Account 30 Days Behind	(3) Account 90+ Days Behind
β_1	3.437***	-0.00205*	-0.00389*
	(1.200)	(0.00102)	(0.00196)
β_2	10.46***	-0.00345***	-0.0121***
	(1.643)	(0.00109)	(0.00206)
β_3	15.39***	-0.00438***	-0.0155***
	(1.735)	(0.00126)	(0.00327)
N	2,013,114	2,013,114	2,013,114

Notes: Robust standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. β_1 corresponds to the first graduating class affected by the requirement; β_2 corresponds to the following graduating class; β_3 is two classes after the first graduating class. Models include state-level and quarter-by-year fixed effects, current county-level unemployment rate, number of quarters the individual has appeared in the credit data, and their total number of accounts.

Table 7
Standard Error Robustness

	Level of Clustering			
	(1) Individual	(2) State	(3) State (Bootstrap)	(4) Two-way State-year
Georgia				
β_1	[0.117, 4.281]	[-26.14, 30.51]	[1.323, 3.046]	[-2.541, 6.909]
β_2	[7.523, 12.28]	[-29.67, 49.20]	[8.720, 10.80]	[3.207, 16.31]
β_3	[12.59, 17.63]	[-36.77, 66.87]	[13.81, 16.28]	[6.483, 23.61]
Idaho				
β_1	[1.076, 4.620]	[-0.565, 5.820]	[0.743, 4.512]	[1.576, 3.679]
β_2	[10.26, 14.91]	[10.37, 14.26]	[10.11, 14.52]	[11.08, 13.55]
β_3	[13.43, 19.33]	[12.93, 19.55]	[13.75, 18.74]	[14.68, 17.81]
Texas				
β_1	[3.622, 9.576]	[0.972, 12.24]	[5.969, 7.245]	[5.321, 7.892]
β_2	[10.23, 17.19]	[6.236, 21.20]	[12.96, 14.48]	[12.37, 15.07]
β_3	[15.93, 22.63]	[10.38, 28.04]	[18.32, 20.10]	[17.64, 20.78]

Notes: 95% Confidence Intervals Presented. β_1 corresponds to the first graduating class affected by the requirement; β_2 corresponds to the following graduating class; β_3 is two classes after the first graduating class. Models include state-level and quarter-by-year fixed effects, current county-level unemployment rate, number of quarters the individual has appeared in the credit data, and their total number of accounts.

Table 8
Mandate Year Results, Border State

Panel A: Georgia

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	0.0200	7.060***	-0.00441***	-0.00689***
	(0.123)	(1.331)	(0.00131)	(0.00234)
β_2	0.0200	5.684***	-0.00284	-0.00801***
	(0.136)	(1.203)	(0.00205)	(0.00283)
β_3	0.00168	5.926***	-0.00189	-0.00884***
	(0.120)	(1.146)	(0.00200)	(0.00325)
N	808,729	607,913	517,125	517,125

Panel B: Idaho

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	0.0334	-2.800**	-0.00147	0.00216
	(0.120)	(1.318)	(0.00169)	(0.00420)
β_2	0.0128	-1.404	-0.00249	-0.00101
	(0.131)	(1.352)	(0.00270)	(0.00380)
β_3	0.0163	0.0768	0.00362**	-0.00420*
	(0.115)	(0.705)	(0.00156)	(0.00242)
N	166,979	143,657	126,856	126,856

Panel C: Texas

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	0.0131	0.932	-0.00195**	-0.000335
	(0.136)	(1.429)	(0.000897)	(0.00192)
β_2	-0.000924	-3.819**	0.00128	0.00576***
	(0.155)	(1.452)	(0.00107)	(0.00204)
β_3	-0.0236	-2.447*	0.000660	0.00366*
	(0.136)	(1.257)	(0.000803)	(0.00214)
N	1,479,654	1,398,714	1,222,054	1,222,054

Notes: Robust standard errors clustered at the individual level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. β_1 corresponds to the year the mandate was passed; β_2 corresponds to the year following the mandate; β_3 is two classes after the mandate year. Models include state-level and quarter-by-year fixed effects, current county-level unemployment rate, number of quarters the individual has appeared in the credit data, and their total number of accounts.

Table 9
Mandate Year Results, Border County

Panel A: Georgia

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	0.0268	8.114***	-0.00325*	-0.00777**
	(0.135)	(1.215)	(0.00183)	(0.00288)
β_2	0.0306	5.369***	0.00132	-0.00985**
	(0.150)	(1.716)	(0.00241)	(0.00373)
β_3	0.00404	5.924***	0.00109	-0.0115**
	(0.131)	(1.699)	(0.00287)	(0.00449)
N	392,551	291,960	247,172	247,172

Panel B: Idaho

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	0.0476	-4.222***	-0.00148	0.000778
	(0.116)	(1.289)	(0.00524)	(0.00509)
β_2	0.0112	-1.350	0.00286	0.00382
	(0.123)	(1.474)	(0.00491)	(0.00496)
β_3	0.0145	4.622***	0.00558	-0.00213
	(0.105)	(1.047)	(0.00440)	(0.00332)
N	60,053	49,052	43,568	43,568

Panel C: Texas

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	0.00598	-0.206	-0.00206	-0.00129
	(0.138)	(1.339)	(0.00131)	(0.00238)
β_2	-0.00542	-3.905***	0.000149	0.00372*
	(0.155)	(1.294)	(0.00144)	(0.00197)
β_3	-0.0255	-2.133*	0.000660	0.00185
	(0.134)	(1.198)	(0.00117)	(0.00248)
N	380,603	357,509	307,787	307,787

Notes: Robust standard errors clustered at the individual level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. β_1 corresponds to the year the mandate was passed; β_2 corresponds to the year following the mandate; β_3 is two classes after the mandate year. Models include state-level and quarter-by-year fixed effects, current county-level unemployment rate, number of quarters the individual has appeared in the credit data, and their total number of accounts.

Appendix

Figure 4 looks at the trends in balances for all accounts, automobile debt amounts, and credit card balances across treatment and border states over time.

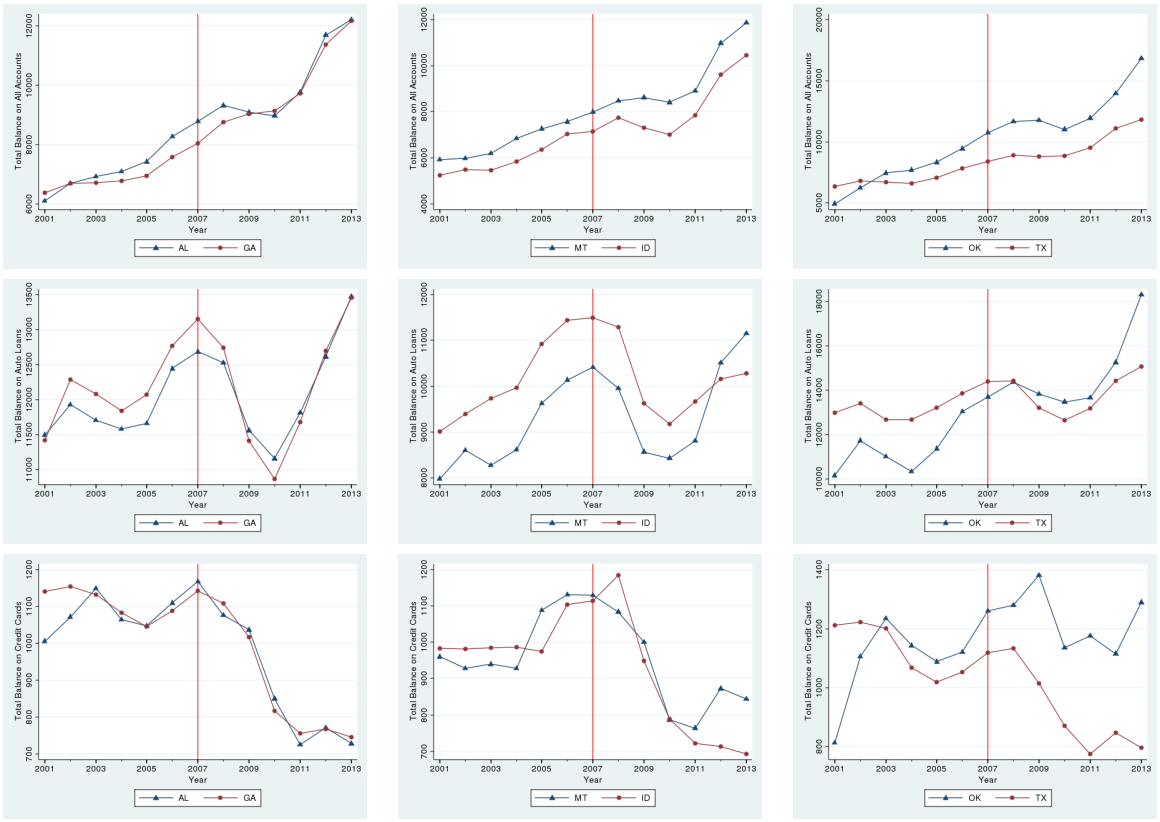


Figure 4. Difference-in-Differences Plots: States Implementing Education Mandates in 2007 Compared to Controls, by Credit Report Outcome

Notes: The figures above shows trends in account balances for the most common types of credit for each of the three treatment states and their respective border control state. Trends for Georgia are shown in the first column of figures, followed by Idaho and Texas. Row 1 shows the trend in average total balances on all accounts. Row 2 shows the trend in average total auto balances. Row 3 shows the trend in average credit card balances.

Synthetic Controls

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 et al. (2010), and Hinrichs (2010). 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 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 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 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. Table 10 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 display Specification 1 (using Specification 2 provides similar results).

As shown in Figure 6 and Table 10, the states in this matched sample are weighted based on state-level demographic and education data. Table 11 shows descriptive statistics for the treatment and synthetic control samples. We use the weights from Table 10 Column (1) to estimate an OlS regression, with the treated state weighted by one. Further, we show the difference-in-differences plots in Figure 5 using the weights to construct the control sample. In each case, the treatment state's outcome closely tracks the control state's outcome before the education takes places. Table 12 shows the results for the synthetic controls sample, using the weights from Table 10. In all cases, these results are comparable to those found in Table 3.

Table 10
Synthetic Controls Selection

Panel A: GA		
State	Specification 1	Specification 2
AK	0.03	
AL	0.084	0.071
CA	0.021	
CT		0.013
DC		0.037
DE	0.111	
FL		0.151
HI	0.021	
KY	0.696	0.657
MD	0.037	
MI		0.071
Panel B: ID		
State	Specification 1	Specification 2
ND	0.441	0.31
NE	0.247	0.12
OR		0.57
WA	0.312	
Panel C: TX		
State	Specification 1	Specification 2
AL		0.083
CA	0.318	0.02
KY	0.382	0.15
MS	0.3	0.259
NM		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, and Total Expenditures. Specification 2: Poverty Rate, Unemployment, Education Levels, Region, Division, Percent of Private Schools, Expenditure per Pupil, Race and Ethnicity, and Total Expenditures

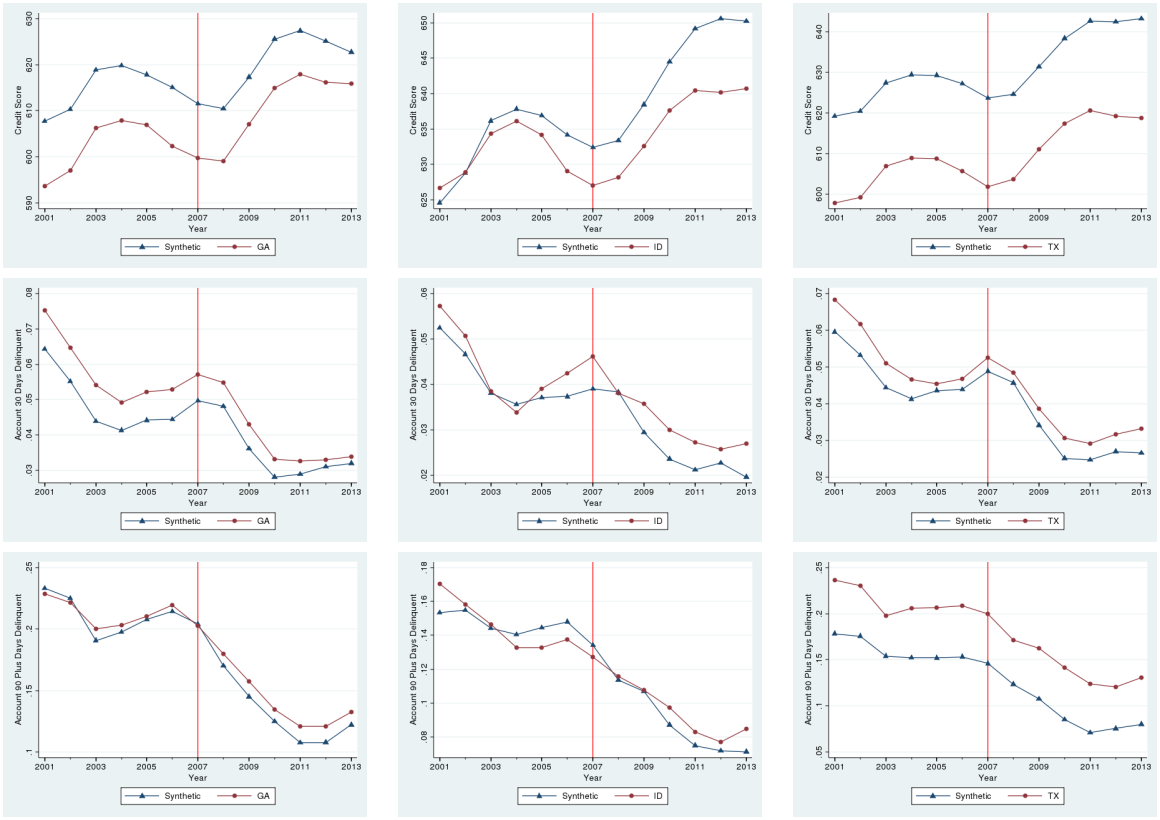


Figure 5. Difference-in-Differences Plots: States Implementing Education Mandates in 2007 Compared to Synthetic Controls, by Credit Report Outcome

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 with weighting from Table 10. Trends for Georgia are shown in the first column of figures, followed by Idaho and Texas. Row 1 shows the trend in 30-day delinquency on any account. Row 2 shows the trend in 90-plus-day delinquency on any account. Row 3 shows the trend in average credit score.

Figure 6. Treatment and Synthetic Control States

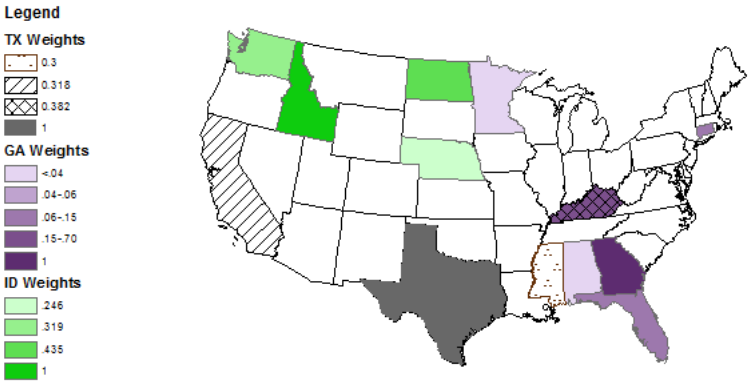


Table 11
Summary Statistics, Treatment versus Synthetic Control States

	GA	Synthetic	GA Sample	ID	Synthetic	ID Sample	TX	Synthetic	TX Sample
Credit Score	606.48	618.14	611.68	632.32	638.12	635.97	609.33	630.30	617.14
	(89.45)	(85.58)	(87.94)	(85.47)	(80.27)	(82.29)	(88.52)	(85.46)	(87.98)
N	571,007	3,334,363	3,905,370	109,273	595,364	704,637	1,473,924	2,713,297	4,187,221
Account 30 Days Behind	0.048	0.041	0.045	0.037	0.034	0.035	0.044	0.039	0.042
	(0.214)	(0.198)	(0.207)	(0.190)	(0.180)	(0.184)	(0.204)	(0.194)	(0.201)
N	467,455	2,945,653	3,413,108	95,161	524,736	619,897	1,278,612	2,405,089	3,683,701
Account 90+ Days Behind	0.182	0.175	0.179	0.122	0.121	0.121	0.178	0.126	0.159
	(0.386)	(0.380)	(0.383)	(0.327)	(0.326)	(0.326)	(0.383)	(0.332)	(0.366)
N	528,248	3,174,438	3,702,686	102,693	571,121	673,814	1,436,551	2,587,532	4,024,083
Total Balance	8164	7980	8080	6973	7716	7443	8144	6932	7679
	(11,577)	(10,661)	(11,170)	(9607)	(10,605)	(10,256)	(11,034)	(10,722)	(10,931)
N	472,654	2,917,160	3,389,814	96,074	531,189	627,263	1,225,811	2,368,434	3,594,245
Unemployment Rate	6.49	6.49	6.49	5.19	5.82	5.59	6.11	7.65	6.67
	(2.57)	(2.76)	(2.66)	(2.42)	(2.41)	(2.43)	(1.78)	(3.20)	(2.51)
N	615,369	3,461,311	4,076,680	111,636	619,976	731,612	1,604,387	2,806,734	4,411,121
Number of Accounts	2.08	2.31	2.18	2.29	2.46	2.39	2.37	2.37	2.37
	(2.25)	(2.40)	(2.32)	(2.17)	(2.39)	(2.31)	(2.49)	(2.35)	(2.44)
N	594,069	3,443,890	4,037,959	111,254	610,260	721,514	1,547,917	2,795,509	4,343,426
Number of Quarters	7.48	7.38	7.44	7.55	7.47	7.50	7.52	7.52	7.52
	(4.71)	(4.63)	(4.68)	(4.73)	(4.70)	(4.71)	(4.70)	(4.70)	(4.70)
N	654,862	3,672,675	4,327,537	118,786	656,645	775,431	1,705,856	2,977,952	4,683,808

Table 12
Synthetic Controls Sample Results

Panel A: Georgia

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	-0.161**	3.065***	-0.00312**	-0.00317
	(0.0729)	(1.001)	(0.00145)	(0.00251)
β_2	-0.408***	11.31***	-0.00330***	-0.00967***
	(0.0822)	(1.186)	(0.00111)	(0.00175)
β_3	-0.740***	16.89***	-0.00690***	-0.0164***
	(0.0912)	(1.300)	(0.00117)	(0.00293)
N	3,794,346	3,656,193	3,212,739	3,212,739
Mean	7.44	611.68	0.045	0.179

Panel B: Idaho

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	-0.134*	0.115	0.00232	0.00725***
	(0.0798)	(1.310)	(0.00254)	(0.00238)
β_2	-0.386***	9.316***	-0.00323	-0.00800**
	(0.0891)	(1.630)	(0.00240)	(0.00353)
β_3	-0.714***	12.22***	0.000772	-0.00840***
	(0.0959)	(1.771)	(0.00219)	(0.00293)
N	679,205	661,731	584,384	584,384
Mean	7.50	635.97	0.035	0.121

Panel C: Texas

	(1) Number Quarters	(2) Credit Score	(3) Account 30 Days Behind	(4) Account 90+ Days Behind
β_1	-0.227***	-2.343	0.00140	0.000868
	(0.0726)	(1.894)	(0.000857)	(0.00208)
β_2	-0.494***	3.550	0.000503	-0.00678**
	(0.0815)	(2.659)	(0.00128)	(0.00288)
β_3	-0.828***	7.770***	0.000872	-0.00868**
	(0.0898)	(2.838)	(0.00169)	(0.00407)
N	4,080,598	3,916,681	3,464,888	3,464,888
Mean	7.52	617.14	0.042	0.159

Notes: Robust standard errors clustered at the individual level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. β_1 corresponds to the first graduating class affected by the requirement; β_2 corresponds to the following graduating class; β_3 is two classes after the first graduating class. Models include state-level and quarter-by-year fixed effects, current county-level unemployment rate, number of quarters the individual has appeared in the credit data, and their total number of accounts. These regressions use the weights from Table 10.