The Effects of State Mandated Financial Education on College Financing Behaviors

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Abstract

Students entering college have limited financial experience while making complex borrowing decisions. This paper examines a policy lever that may improve these decisions: high school personal finance graduation requirements. We use a difference-in-difference strategy exploiting differential timing of state mandates to determine their effects on the financial aid decisions of incoming freshmen at four-year institutions. Our results suggest financial education graduation requirements shifts students from high-cost to low-cost financing. The requirements increase aid applications and acceptance of federal loans, while decreasing the likelihood of holding credit card balances. Students from less affluent family backgrounds further reduce their likelihoods of working while enrolled and borrowers from more affluent family backgrounds reduce private loan amounts. The mandates do not change college attendance or choice of institution type.

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1 Introduction

Student loan balances currently total over \$1.41 trillion in the United States, surpassing credit cards as the second largest source of debt after mortgages (Federal Reserve Bank of New York 2018). While the popular press is full of dire statements about the high and rising levels of student loan borrowing, these high levels of student loan debt alone do not imply that student borrowers are behaving sub-optimally. The fact that the average young adult has limited experience in the financial market when navigating decisions about how to finance his or her college education, however, suggests reason for concern. Results from the 2015 Programme for International Student Assessment financial literacy exam additionally show that only 29 percent of 15 year olds in the U.S. can compare loans with different interest rates and terms (National Center for Education Statistics 2017), an important skill for selecting aid packages.[1] Further, student loan borrowers often express regret: less than a third (29%) of student borrowers report that they would make the same loan choices if given the opportunity to repeat the process (Lusardi 2016).

Over the past several decades, states have increasingly required high school students to meet graduation standards in personal finance to improve the financial literacy among young adults. As of 2017, 25 states require coverage of personal finance topics prior to graduation, including material on interest rates, saving, credit, debt, and income. Some state standards explicitly include financing postsecondary education as a required component. This paper is the first to determine the causal effect of these financial education graduation standards on the ways in which four-year college students make their initial decisions when financing their postsecondary educations.

[1] (Lusardi, Mitchell and Curto 2010) further show that young adults have low levels of financial literacy: only 27 percent of 23-28 year olds understand basic financial concepts such as inflation, interest, and risk diversification.

The analysis uses a difference-in-difference approach to compare incoming freshmen at four-year institutions from states with personal finance graduation requirements before and after implementing the requirement to comparable students whose states lack such a mandate. We use the National Postsecondary Student Aid Study (NPSAS) 1999 through 2011 waves to draw on a rich set of outcomes, including whether students applied for and accepted financial aid, their federal and private student loan amounts, their grants and scholarships, whether or not students carry a credit card balance, and whether or not students work while enrolled.

Our study adds novel results to the existing literature on the impact of financial education on knowledge, credit, and debt. In previous work, Urban et al. (2018) find that personal finance graduation requirements increase credit scores and decrease severe delinquencies for 18-22 year olds. The paper most closely related to this study is Brown et al. (2016*b*), who find that financial literacy exposure reduces non-student debt, increases credit scores, and improves repayment for those under 30.[2] Brown et al. (2016*b*) further find that financial literacy reforms only modestly increase total student loan debt (combined federal and private balances).

We differ from Brown et al. (2016*b*) in several ways, adding key new insights. First, we focus on the broad portfolio of initial financing decisions, distinguishing between federal and private forms of borrowing as well as work and grant aid, as opposed to aggregate balances. This generates novel understanding of the mechanisms through which financial education may impact financial behaviors (e.g., applying for aid, substituting lower cost sources of credit for higher cost forms, or seeking non-loan forms of finance). Second, our data contain individual-level demographic characteristics that are not observable in credit report data. This allows us to explore

[2]The findings are mixed regarding savings and debt by middle-age (Bernheim, Garrett and Maki 2001; Cole, Paulson and Shastry 2013), where these studies focus on earlier mandates that offer different content than those enacted post 2000. See Urban and Schmeiser (2015) for more on the early mandates.

heterogenous responses for students with different family backgrounds. In doing so, we identify important distinct patterns based on expected family contribution. Third, we focus on initial behaviors of students in the years immediately after high school graduation. This will give us a clean perspective for studying the immediate and direct impacts of the high school graduation standards, as some colleges and universities provide additional financial education that affects total student loan balances at older ages. Consequently, these results may be a partial effect of financial education, as there may be cumulative effects of the high school requirements over the course of a student's college career including effects on persistence, graduation, and post-education financial behaviors.

We further examine whether financial education influences other college entry decisions, including college attendance and choice of institution type. If financial education affects college-going decisions, the sample of those immediately entering four-year colleges upon high school graduation may be endogenously selected. In the NPSAS data, which is based on students attending a post-secondary institution, we demonstrate that graduation requirements do not affect the type of institution attended conditional on enrollment. Exposed students are no more or less likely to attend a two-year as opposed to a four-year school, a public instead of a private school, or a school with relatively lower tuition. We further supplement the main analysis with data from the Current Population Survey (CPS) and the Integrated Postsecondary Education Data System (IPEDS) to show that graduation requirements do not change college enrollment rates or the type of institutions selected. This suggests that personal finance information in high school does not change students' decisions about attending college or their choice of institution type.

Although financial education does not affect college attendance or institution type, it does affect how students finance their education. Our results point toward a shift from higher-cost borrowing to lower-cost borrowing for four-year college students exposed to financial education. The findings suggest that personal finance graduation requirements significantly increase the average incoming freshman student's use of federal aid for financing a four-year degree: these students are 3.3 percentage points more likely to apply for aid and 5.3 percentage points more likely to take out a direct federal Stafford loan. The increase in applications leads exposed students to also be 3.3 percentage points more likely to have grants. At the same time, these students reduce higher cost forms of borrowing; exposed students are 2 percentage points less likely to carry a credit card balance. Conditional on borrowing, students reduced their private loan balances by roughly $1,500.

These average effects mask heterogeneity in student responses to the education. Students from families with lower than median EFC have a larger increase in use of subsidized Stafford loans and are less likely to hold a credit card balance. On the other hand, students from families with above median EFC decrease private loan borrowing by larger amounts (roughly $2,400) but are no more or less likely to hold a credit card balance. Another difference across these groups is the effect of the education on working while enrolled. Exposed students from lower EFC families are less likely to work, while students from higher EFC families do not exhibit this response. Since working while enrolled in school can potentially detract from studies and decrease the probability of graduation (Stinebrickner and Stinebrickner 2003; Darolia 2014), this shift may be important for those from less affluent families.

One potential threat to this identification strategy arises if state policies are themselves endogenous or correlated with omitted factors. This might be a particular concern if, for example, the financial crisis influenced states' decisions to require personal finance and the recessionary conditions simultaneously changed student aid decisions. Previous work by Brown et al. (2016*b*) and Urban et al. (2018)

demonstrates that personal finance requirements are not endogenous to other policy changes or economic conditions. We build upon these findings and explicitly test whether graduation requirements occurred in conjunction with factors related to the state's economy or if states passed other educational policies that might be conflated with the mandate. We further provide evidence that suggests that the difference-in-difference assumption of parallel trends is likely to be satisfied by showing that states that pass laws do not have different preexisting trends in financial aid outcomes than those that do not.

To some degree, the estimates may understate the effects of financial education mandates on postsecondary financing decisions, as the control states include schools that voluntarily offer financial education courses. To explore this possibility, the final section examines the impact of personal finance course offerings in a state without a mandate: Montana. We document which high schools offered a course covering personal finance materials and the year in which the course began. This is paired with administrative student loan data from the four-year institutions in the Montana University System (MUS). These results suggest that the effects of offering a course, without mandating specific graduation standards, has negligible effects on financial aid decisions.

2 Background

There are several channels through which learning personal finance content may improve choices of student aid packages. To the extent that borrowers learn to shop for interest rates, this policy could shift students from private loan and credit card borrowing toward lower cost public loans. If the content emphasizes investing up front in searching for scholarships and grants or makes the potential future burden of student loan debt more salient, it could reduce debt in favor of grant and scholarship support. Explicit coverage of financial aid applications may result in fewer errors or more timely applications, generating more financial aid offers. However, if students are already optimally choosing their postsecondary education financing strategies, the added information in personal finance coursework would not affect behavior.

A sizable body of literature suggests that many students still under-invest in their schooling (Avery and Turner 2012; Cowan 2014; Lochner and Monge-Naranjo 2015, 2011). Some studies show that students are aware of self-control problems, which could be one reason they decline aid (Cadena and Keys 2013; Johnson 2013). This paper does not tackle students' ability to optimally choose a level of investment in higher education. Instead, we turn the discussion to the specific components of financing behavior: federal loans, private loans, credit card balances, grants and scholarships, and working while enrolled.

In the last decade, states increasingly imposed requirements for personal finance coursework in high school that aims to reduce financial distress among young adults. The standard material typically covers interest rates, saving, investing, and borrowing, and each state customizes its standards to fit the population and relevant concerns in the state. Specific graduation standards cover a range of topics including mortgages, auto loans, the stock market, checking and savings accounts, insurance, income volatility, shopping for loans, credit scores, credit cards, timely payments, and financing postsecondary education.

States often include student loan and financing postsecondary education content explicitly in the state standards. For example, Utah's standards include the FAFSA process directly, where students are taught the mechanics of the process and the benefits of completing the FAFSA. Tennessee's state standards include the following content: "Demonstrate an understanding of Free Application for Federal Student Aid (FAFSA) requirements to apply for postsecondary education financial aid by completing an application. Identify strategies for reducing the overall cost of postsecondary education, including the impact of scholarships, grants, work study, and other assistance." The Texas standards are perhaps the most obvious example of a clear channel through which financial education can affect student loan decisions. The Texas State Board of Education requires that all students "understand the various methods available to pay for college and other postsecondary education and training." The standards include requiring that students understand how to complete the FAFSA; research and evaluate scholarship opportunities; compare student grant options; analyze student loan options; evaluate work-study options; investigate nontraditional methods of paying for postsecondary education. We note that in the years our data cover, only Tennessee required students fill out the FAFSA as part of the state-mandated financial education.[3]

Table 1 lists the states with personal finance graduation requirements, as well as the year in which the first graduating class was required to complete the material. This classification is relatively conservative, as some states will require a full standalone course in personal finance, while others require that the material be taught within another subject, such as social studies, math, or economics. In all cases, states document personal finance requirements in graduation standards.[4] Figure 1 provides a map of the states that implemented these graduation requirements. It shows that there is no clear geographic pattern in either the implementation or form of these policies. Further, nearly all states have proposed legislation at some point initiating personal finance learning standards in K-12 education, making trends in the states without requirements a good counterfactual for those whose policies were passed.[5]

[3] If in need of assistance, students would still need to actively seek out one-on-one attention, especially to achieve the level of the attention in Bettinger et al. (2012).

[4] More on the collection of these data can be found in Urban and Schmeiser (2015).

[5] Since 2011, eight states have passed financial education graduation requirements, and four

There is heterogeneity in state laws that requires us to make some judgment calls in classifying policies. For example, in Georgia students are required to take a one semester course that merges economics and personal finance and has a detailed list of standards covering mortgages, credit scores, interest rates, and risk. Georgia trains teachers, funds the requirement in schools where teachers are properly certified, and gives sample evaluations for teachers to use. This is one of the strongest state mandates. At the other end of the spectrum, Wyoming requires personal finance topics be covered in the Social Studies curricula, but it does not have specific content requirements. We classify Wyoming as having a requirement. Further, three states (Nebraska, New Mexico, and South Dakota) require that schools *offer* a course in personal finance but do not require that all students take the course. As this is the only policy in Nebraska and New Mexico, we classify these states as not having a requirement. In South Dakota, however, students are required to take *either* Economics or Personal Finance; we thus classify South Dakota as a state with a personal finance graduation requirement, though we acknowledge that all students will not take it.

In Table A.4, we show that our results are robust to dropping all of the states with policies that have some gray area in assignment. Additionally, there are four states (Arizona, Connecticut, Virginia, and West Virginia) that mandate personal finance in some form but leave it to the county or school district to determine how these mandates are carried out. In our analysis, we count all four as having personal finance. In the event that these programs are not enforced, this would bias us against finding an effect. Again in Table A.4, we show that our results are robust to dropping states with local control.

required standards be implemented into any course. Additional states have bills currently being prepared for House votes.

3 Data

The bulk of the analysis draws on data from the NPSAS to determine the causal effect of financial education on the student aid decisions of those attending four-year institutions straight from high school. We focus on this set of students for several reasons. First, it capture students at the pivotal point when they make their initial decisions on how to finance their postsecondary education. Second, this approach prevents our estimates from being contaminated with any effects from additional financial education and financial counseling offered by colleges and universities. As we do not observe college-level financial education efforts, we are unable to determine if college-specific policies are more frequently (or less frequently) offered in states with personal finance high school graduation requirements. Third, this age window reduces the mismatch between a student's (reported) state of residence and the (unreported) state where they attended high school.

We also conduct the main body of the analysis for students at four-year public and private institutions, for several reasons. First, tuition and aid packages tend to be larger and more consistent across institutions at this level. Second, two-year and for-profit students are much less likely than four-year students to enroll immediately after high school, and a focus on traditionally-aged incoming freshmen at these institutions is therefore not a representative sample.[6] We note that this sample selection is not atypical in studies of financial aid behaviors.[7] We do estimate the effect of personal finance education on student loans for the full sample of two-year and for-profit students with the same age restrictions. These results are reported in Table A.1. Not surprisingly, the results are muted relative to our baseline specifications.

[6]Two-year and for-profit students (median age of 24) tend to be older than four-year students (median age of 21).

[7]See for example, Cadena and Keys (2013) who examine loan take up rates among a similar population.

As the main results rely upon this specific sample, we use data from the CPS and IPEDS to examine enrollment and type of institution chosen for all freshmen. We show that financial education requirements do not affect students' decisions to enroll in college or to attend a specific type of institution (two-year, four-year, private, for-profit, or with higher tuition).

Finally, to augment our findings from the NPSAS, we use administrative data from the Montana University System (MUS) to examine how elective financial education courses affect financial aid decisions. These data supplement the main analysis. We describe each dataset below.

3.1 NPSAS data

The NPSAS is a nationally representative study of students enrolled in institutions of higher education. It contains detailed data on financial aid extracted from institutional records, including whether a student filled out the FASFA, federal loan amounts, and EFC. These administrative records are paired with student and parent interview responses about demographics, high school degree, family background, private loans, credit card balances, and work.[8] While the federal loan data are administrative, the private loan data are based on student survey responses. Our results use data from the 1999, 2003, 2007, and 2011 waves of this survey. We choose this period of focus because nearly all states implementing financial education requirements did so after 2000. Those implementing before 2000 had content that was more oriented towards consumer economics, with substantially less focus on postsecondary education financing and credit card debt explicitly.[9] Furthermore, a

[8] We do not use the parent surveys in this study.

[9] Only three states implemented personal finance graduation requirements between 1989 and 1999; 19 adopted between 1999 and 2011. Cole, Paulson and Shastry (2013) also argue the early adopters (those prior to 1985) were trending differently than non-early adopters. A series of robustness checks also examines heterogeneity in state policies.

series of financial aid questions were added in 1999, meaning surveys beginning with this year have more consistent outcomes and more information about non-federal aid and credit card debt.

Important for our study is that the NPSAS reports a student's legal state of residence, drawn from the student's reported permanent address.[10] This address is likely to be the student's home address, as opposed to a dormitory or temporary apartment the individual rents for college. Thus, we are not required to assume that students go to school in the same state in which they attended high school.[11] However, there are some cases in which the legal state of residence is not the state in which the student attended high school, potentially creating measurement error. For example, some students relocate to a new independent permanent address for higher education, and in some cases parents or students may establish residency in another state in order to obtain tuition benefits associated with in-state status at a public institution. Consequently, we restrict the sample to U.S.-born students between the ages of 17 and 19 that are in their first year of higher education who graduated in the same calendar year or one year prior to enrollment.[12]

We drop any students who did not complete a traditional high school degree as they would not be exposed to the personal finance curriculum; this eliminates students with GEDs (3% of the sample), students who were homeschooled (< 1% of the sample), and students who did not have a high school certificate (1% of the sample).[13] This results in a sample of 44,729 students, with 2,696 in 1999, 13,652 in 2003, 11,259 in 2007, and 17,122 in 2011.[14]

[10]The NPSAS report that the question is coded in the following way "First based on the federal financial aid application; if not available, student records were used. If both were not available, the student interview was used."

[11]84% of students in our NPSAS sample attend a school in the same state as their legal state of residence.

[12]11.6% of the sample are foreign born.

[13]If we instead preserve these individuals in our sample, our results remain robust.

[14]The 1999 wave is smaller than in later years because of the smaller target number of students

Table 2 shows the characteristics of the sample by state personal finance requirement. Across the states, over 90 percent of incoming four-year freshmen apply for some type of aid, although this does not indicate whether applications were timely or completed correctly.[15] Over half (55%) of these students have a Stafford loan, which is substantially higher than the 11 percent of students that have private loans. Conditional on having a loan, average private loan amounts are larger than Stafford loans, \$7,065 in private loans compared to nearly \$2,871 in total Stafford unsubsidized and subsidized loans combined. Nearly three-fourths of students receive some type of grant or scholarship (largely Pell grants), and slightly less than half (45%) of students work while a college freshman in some capacity. About 10 percent hold a balance on a credit card in their freshmen year. The NPSAS sample is roughly 55 percent female, 70 percent white, and just over 18 years of age, with 97 percent of students dependents. Expected Family Contributions (EFCs) are roughly \$14,700 on average, meaning parents potentially are able to contribute roughly that amount annually.[16] About 20 percent of students have parents without any college education.

3.2 CPS data

Using data from the CPS, we test the extent to which financial education course requirement change college enrollment decisions. These data span from 1995-2013, where we trim the sample to match the previous results. First, we include 18 year-olds after the August survey month and 19 and 20 year olds. Second, we remove

for the sample.

[15]While the Department of Education provides data on FAFSA filings by state and year from 2006-present, these data are unfortunately not cut by age, making them unusable with our high school graduation year-based identification strategy. FAFSA filings by high school have only been collected from the 2016 academic year onward.

[16]The EFC is based on the financial information provided on the FAFSA, and it is calculated according to a standard formula that does not vary based on tuition and fees. It is highly skewed, with a median of \$5,000 for our sample of four-year incoming freshmen.

foreign-born students, as these are the least likely to have completed high school education in their current state of residence. Third, we remove individuals who are still in high school or did not respond to the school or college attendance question.[17] We assume that students remain in the same state in which they attended high school until they are age 20.[18] Table C.1 reports summary statistics for this group, where we see no clear differences in individual-level characteristics across states with and without personal finance requirements. We confirm these findings with four-year enrollment data from the Integrated Postsecondary Education Data System (IPEDS).

3.3 MUS data

We employ the MUS data to understand how voluntary offering of financial education affects student financial aid decisions. These data are drawn from the two largest four-year campuses in the state of Montana: the University of Montana and Montana State University to make the results comparable to our main results with the NPSAS data.[19] The MUS data are novel for the detailed individual-level college funding information provided. In addition to reporting students' high schools, demographic information, the campus attended, and the degree pursued, these data identify the source of funds (such as federal, institutional, state, or other), the type and amount of award (need-based, merit-based, athletic payments, work study, loans, etc.), and the amounts of federal and state loans. However, these data do

[17]If we instead include those who are still in high school, we still find no effect of personal finance education in high school on college attendance.

[18]Brown et al. (2016*b*) show that roughly 93% of individuals stay in the same state from 18 to 22. In the NPSAS sample, 84% of students began college in the same state in their states of legal residence.

[19]This excludes four public four-year institutions: Montana State University-Billings , Montana State University-Northern, Montana Tech, and University of Montana-Western. Total enrollment across these four institutions is roughly 8,000. Financial aid information from these smaller institutions is incomplete.

not include information on private loans. While Montana is a relatively low income state, average student debt levels, tuition as a fraction of state personal income, graduation rates, and Pell grant levels are similar for Montana and the nation as a whole.

Our data span the years 2002 through 2014, or 36 semesters of data. We limit our analysis to in-state undergraduate students so we are able to identify the high school attended. We contact each high school in the state directly to determine whether or not they offered a stand-alone personal finance course and in what years. We use administrative transcript data from the Office of Public Instruction to confirm that students generally take these courses in their junior or senior year,[20] and we match students based on their age to whether the course would have been offered during their high school years or not. We only include first semester freshmen's aid packages to parallel our previous results. Table B.1 provides descriptive statistics of students exposed and not exposed to personal finance course offerings, where we see no statistical differences across students in schools with and without personal finance offerings at the 10 percent level.[21] Figure 2 further shows that there are no visible patterns in schools offering and not offering financial education courses based on their geography or distance from main highways in the state.

[20]Unfortunately, the transcript data are only available from 2013-present, and the higher education financing data are only available through the 2013-2014 academic year, providing no overlap of high school students and their subsequent college enrollment.

[21]While in a t-test it appears that students from high schools offering personal finance are more likely to attend Montana State University than the University of Montana, there is no difference in school of attendance in a regression specification with school and year fixed effects.

4 Empirical Strategy

This paper uses a difference-in-difference strategy to determine the causal effect of financial education graduation requirements on postsecondary financing decisions.[22] We compare students who graduated in states before and after a financial education graduation requirement was implemented to the same difference over time for students from states without graduation requirements. Standard errors are clustered at the state level, as the policies under consideration are state specific. In all specifications, we include state fixed effects to account for differences in financial aid and higher education policies that are consistent within a state over time and year fixed effects to account for national trends in higher education financing.

$$Y_{i,s,t} = \alpha_0 + \alpha_1 \mathrm{PF}_{s,t} + \boldsymbol{\beta X_i} + \delta_s + \gamma_t + \epsilon_{i,s,t} \tag{1}$$

We estimate Equation 1 for a suite of dependent variables $Y_{i,s,t}$ that capture how individual i with permanent residency in state s entering college in year t financed his or her postsecondary education. The majority of these outcomes are dummy variables, including whether a student applied for financial aid, had a Subsidized (or Unsubsidized) Stafford, had grants and/or scholarships, had a private loan, held a credit card balance, and worked while enrolled. While we use linear probability models throughout, our results are robust to probit specifications as shown in Table A.4.

Our independent variable of interest, $\mathrm{PF}_{i,s,t}$, equals one if individual i in state s graduated from high school in a year t after the state mandated a personal finance

[22] While Urban et al. (2018) use a synthetic control approach in the CCP data, the smaller sample size of the NPSAS data does not allow for a state-representative population in pre- and post- periods, and thus we rely upon a panel diff-in-diff among states that have relatively similar policies: those that require personal finance coursework be completed by all students prior to high school graduation. The NPSAS is also not designed to be representative at the state level, further limiting this usefulness of a synthetic control approach.

graduation requirement. Thus, this variable captures a binding personal finance requirement for the specific student.

Equation 1 includes a rich set of individual-level characteristics ($\boldsymbol{X_i}$), including an indicator for male students, dummies for white, black, and Hispanic demographic groups, age dummies, and dummy variables for parental education groups. We also include a dummy variable for whether or not a student is a dependent for the purposes of financial aid, although this is true for 97 percent of the sample. Our specifications include EFC in quartiles. The EFC is based on a measures related to income, assets, state of residence, and the enrollment in higher education of other family members. As such, it captures family income and wealth and any correlated factors, such as preferences, depth of financial knowledge, or level of access to credit markets. It also determines eligibility for need-based aid at both the federal and state level. The terms δ_s and γ_t are state and year fixed effects.

4.1 Endogenous College Enrollment Decisions

Our main NPSAS sample focuses on students who enter four-year institutions after completing high school. If financial education requirements make students more averse (or more inclined) to borrow, there might be a concern that these requirements change the type of institutions students attend or even influence whether or not students enroll in higher education. For example, if the way this information is delivered causes students become more concerned about college costs, they might be more likely to attend a two-year school than a four-year institution, more likely to attend a public than a private school, more likely to choose a school with lower tuition, or less likely to attend college at all. In contrast, if students may find that borrowing is less intimidating than they previously thought, they may be more inclined to attend a private school than a public school. Some of these changes may result in improved outcomes for students. However, if students misinterpret the information they are given (e.g., become unduly load averse) or if the instruction leads students to adopt rules of thumb that are not appropriate (e.g., "in-state schools are always a better choice"), there is a possibility that the instruction might lead to worse matches between students and schools. In that case, these policies could have unintended consequences that reduce lifetime income.

The NPSAS data include only enrolled students, so we turn to CPS data to examine whether personal finance education requirements change individuals' decisions to attend college. We include the sample of individuals aged 18-20 over the period 1995-2013.[23] College attendance includes any postsecondary education: public, private, or for-profit colleges or universities with two- or four-year programs. We separately investigate full-time and part-time college attendance, as well as the combination of the two. Table C.1 shows the average dependent variables by whether or not the state ever required personal finance prior to graduation, using the CPS sample weights.[24] There are no significant differences across the two sets of states, and the average college attendance rate is roughly 54 percent, with 48 percent attending postsecondary education full time and only 6 percent going to school part-time. There are no notable differences across the two samples in terms of demographic characteristics of individuals within those states.

$$Y_{i,s,t} = \alpha_0 + \alpha_1 \mathrm{PF}_{i,s,t} + \boldsymbol{\beta X_i} + \delta_s + \gamma_t + \zeta_m + +\epsilon_{i,s,t} \tag{2}$$

Next, we estimate the effect of personal finance education on college attendance using Equation 2. Our dependent variable, $Y_{i,s,t}$, equals one if individual i in state

[23]If we restrict the sample to 18 year olds, we obtain the same result. We rely on the CPS to exploit the longer period relative to the American Community Survey to provide support for the parallel trends assumption.

[24]If we do not weight these samples, the averages and the differences across groups remain consistent.

s at time t attends college and zero otherwise. Our independent variable of interest, $\text{PF}_{i,s,t}$, equals one if individual i living in state s with a personal finance requirement in place prior to the time that individual graduated from high school. We include state fixed effects (δ_s), year fixed effects (γ_t), and CPS survey month fixed effects (ζ_m), as well as individual-level characteristics ($\boldsymbol{X_i}$) that include male, white, black, hispanic, married, a metropolitan-resident dummy, and age dummies.

Table 3 reports the results from Equation 2. Our baseline specification shows that personal finance graduation requirements do not change college attendance rates, where these effects are precisely estimated zeros.[25] Table C.2 provides evidence that suggests that the parallel trends assumption required for the difference-in-difference estimation strategy is likely to be satisfied, as the years before the requirement in states with personal finance requirements show no difference in the outcome variables. There are no clear trends from the excluded group, those who graduated more than 13 years before a graduation requirement came into effect, and each year before the requirement. The coefficients on PF -1 through PF -13 are not statistically different from one another. This gives us confidence that there are no differences across states with and without personal finance requirements in college enrollment in the pre- or post- policy change years.

Since the CPS data include the current state of residence and not the state one attended high school, we supplement this analysis with data from IPEDS (2001-2015) to use the state of permanent residence and determine enrollment effects. We sum first-time college attendees by state of residence over time to determine the number of enrolled students from a state and divide this by the number of 18 year olds in the state in that year to produce four-year college enrollment rates. While we

[25]When we perform additional robustness tests to drop early adopters or those with locally-controlled policies, we again find no effects of personal finance on postsecondary education attendance. In all specifications, the results are nearly zero in magnitude. Thus, we think we have tightly estimated a null effect of financial education on college attendance.

would like to do this for two-year institutions, this field is often left blank for many two-year institutions or is reported inconsistently. This gives us little confidence in the two-year measure. Thus, we focus on four-year enrollment. Changes in four-year enrollment could be due to either shifts toward two-year enrollment or lack of attendance. Table C.3 confirms that we see no effect of financial education on four-year enrollment when using the resident address. We show that our results are comparable when we instead use the state of the postsecondary institution instead of the state of residence of the student (Column (2)). Finally, we show that there is support for parallel trends in the pre-period (Columns (3)-(4)).

Finally, using the NPSAS data, we can address the likelihood of observing an enrolled student at different types of institutions. Table 4 reports results for the effect of personal finance graduation requirements, controling for other demographic characteristics, on institutional choices. These include whether or not a student enrolled at a private institution (conditional on enrollment at a four-year institution), the tuition and fees paid at the four-year institution, the likelihood that the student stayed in-state for postsecondary education, and whether or not the student enrolled in a four-year, as opposed to a two-year, college. Across each of these outcomes, personal finance graduation requirements do not appear to play a role in the type of institution a student attends. The α_1 coefficients from Equation 1 are small and imprecisely estimated, with none of the estimates approaching statistical significance at even the 10 percent level.

5 Results

Table 5 reports the causal effects of personal finance graduation requirements on financing behaviors for incoming freshmen at four-year institutions (α_1 from Equation 1). The first three columns focus on the discrete decision to apply for and to accept federal aid. The dependent variable for Column 1 is a binary variable equal to one if the student applied for aid,[26] and the dependent variables for Columns 2 and 3 are binary variables equal to one if the student accepted a direct federal Stafford loan and a direct federal subsidized Stafford loan, respectively. Column 4 indicates whether or not a student had grants and/or scholarships in the aid package. Column 5 includes results using a binary variable equal to one if the student took out a private loan to finance their education. The dependent variables for Column 6 and 7 are binary variables equal to one for students who self-report carrying a positive credit card balance and whether or not the student worked while enrolled in school, respectively. While all results reported are linear probability models, we show that results are robust to a probit specification in Table A.4.

The results in Table 5 indicate that personal finance requirements change student behavior on important margins.[27] Students subject to these requirements were 3.3 percentage points more likely to apply for aid, a sizebale response given that 91 percent of students in non-exposed states already apply for aid. Take-up of subsidized Stafford loans, which do not accrue interest during college, increase by 5.7 percentage points due to the education. Column (3) reports that students exposed to the graduation requirements were also 3.3 percentage points more likely to have aid packages with grants or scholarships, implying that most of the new applicants generated by the policy were aid eligible. This grants measure does not include scholarships that are given directly to students, such as Rotary Club Scholarships.[28]

While the evidence so far suggests that affected students are electing additional federal support through subsidized loans and additional reliance on grants

[26]This equals one if the student completed the FAFSA, or reported that they applied for aid in the NPSAS interview.

[27]Estimates of control variables for Table 5 are in Table A.2.

[28]Average grant receipt is roughly $7,200, although this is heterogenous across school due to variation in tuition.

and scholarships, we see no evidence that fewer students are taking out private student loans–the point estimate of the effect is a tight zero. However, students exposed to financial education are 2.1 percentage points less likely to hold a credit card balance. Credit cards are a common way college freshmen can smooth consumption to purchase books and food, but many students may not use them optimally if they lack knowledge about their use or if they are used instead of lower cost sources of funds.[29] A survey across college campuses shows that only 9.4 percent of students with credit cards pay their balance in full each month, leaving the remainder with interest and late fees (Ludlum et al. 2012). Further, the authors find 75 percent of students are unaware of late fee charges on their credit cards. The students reducing their likelihood of holding a balance may be either substituting from credit card balances to subsidized Stafford loans, or they may be increasing their use of grants. To the extent that students were over-using credit cards prior to the intervention, financial education may be a policy lever to improve information around credit cards for college students.

For whom are these personal finance requirements most likely to affect behavior? We focus on heterogeneity by family resources, where Table 6 reports the α_1 coefficient from Equation 1 for those from families above and below median EFC levels (roughly $5,000). The coefficient estimates are bolded where the coefficient for the sub-group is statistically different from the average effect.

The results indicate that the bulk of our effects are driven by students from families with fewer resources. The effect sizes on the increased probability of having a subsidized Stafford loan and the decreased probability of a holding credit card balance are three times as large for below median EFC students as higher EFC students. Lower EFC students exposed to financial education appear to be making

[29]Brown et al. (2016*a*) report that 2015 average credit card balances for 20 year olds were $176.

a financially savvy decision to substitute federal aid for more costly credit card debt.

Students from families with lower EFCs are also 3.5 percentage points less likely to be working while in school, suggesting that the additional federal aid may be used to replace work for these students.[30] This is an important finding given that Stinebrickner and Stinebrickner (2003) and Darolia (2014) show that for full-time students at four-year universities, working is detrimental to academic performance.

Since we see that financial education affects the application margin, one may posit that increasing federal loan amounts may be a mechanical response to the increased application rates if students simply accept the maximum federal financial aid offers. More applications might mean more acceptances of aid without students' making proactive decisions. While we recognize that applying is endogenous to the education, we show suggestive evidence that conditional on applying for aid, students exposed to financial education make different decisions. Table 7 shows that appliers exposed to the education were no more or less likely to have a grant than others without financial education. We suspect that it is obvious to applying students to accept any grant aid offered (such as Pell grants), as grants are "free money."

In contrast to the grant results, Table 7 shows that exposed students who apply for aid are *more* likely to accept Stafford loans, particularly subsidized Stafford loans, than those who also applied but were not exposed to the education. Required financial education encourages more students to accept additional federal loans, especially those that are interest free. Compared to grants, it may be less obvious to determine whether accepting federal loans is a smart financial decision. The results provide suggestive evidence that students are making more informed decisions, as

[30]There is not a statistical difference in work study earnings on average or for the low EFC population due to the financial education, though the confidence intervals on these estimates are quite large ranging from -$104 to $72.

opposed to simple mechanical changes stemming from increased applications.[31] The effects conditional on applying are similar across EFC groups, though they are larger in magnitude for students with lower EFCs.

In addition to showing that our results are not solely driven by the application margin, we look at the population of borrowers to see if there is an intensive margin effect on amount borrowed in the final three columns of Table 7. Here, we see that conditional on borrowing, the amounts of subsidized and unsubsidized Stafford loans do not change due to financial education. This is somewhat unsurprising, since students may be likely to just take the maximum amount offered. However, conditional on taking out a private loan, students exposed to financial education reduce their loan amounts by $1,300 on average and by $2,350 for students with higher EFCs. While only 11 percent of borrowers use private loans, and we see no extensive margin effects on taking out a private loan, students who do finance college in part through private loans reduce the amount borrowed by about a standard deviation.

While there is some increase in use of Stafford loans for students with higher EFCs, the reduction in private loan amounts for this population may suggest that the courses encourage these students to identify alternative methods of payment not included in our analysis. Brown, Stein and Zafar (2015) show that there is little evidence for home equity as a substitute for student loans, but there are other channels through which more affluent households can adjust, such as scholarships from parents' places of employment, informal networks for lending from extended family members, additional parental employment opportunities, or selling off of

[31]We cannot completely rule out that the increase in applications due to the policy results in a mechanical increase in Stafford loan takeup. However, for this to be true, no students could jointly be deciding about applying and taking out loans and students on the margin of applying would need to have a higher propensity to accept loans regardless of the education. The contrast with grants and the evidence that the increase largely comes from subsidized and not unsubsidized Staffords makes this hypothesis unlikely.

assets.[32] The absence of a decrease in private borrowing among low EFC families could be because lower income families may not have had as much initial access to the private loan market (indeed, average loan balances for this group are half of that for higher EFC families), or it may be due less access to these alternative methods that could substitute for private loans.

These results shed light on the mechanisms behind the graduation requirements: financial education increases subsidized borrowing at the federally advantageous rates. The increase in subsidized borrowing also suggests that these students may have mistakenly assumed they were credit constrained when they were not or may have chosen to decline subsidized loan offers. At the same time, graduation requirements reduce more costly forms of borrowing, including credit cards (for students from less affluent backgrounds) and private loans (for students from more affluent backgrounds).

5.1 Robustness

In Table A.4, we consider four threats to internal validity for the baseline results and, where appropriate, test the robustness of the results to alternative measures or specifications.

First, the private loan amounts are from student survey responses, begging the question: how well do students know their private loan amounts?[33] This is particularly a concern if students exposed to financial education give more accurate reports of their finances, including identifying the existence of a private loan vs.

[32]Since private loans are nearly always co-signed by parents, parental involvement is required for these decisions.

[33]This exact wording on the survey is "How much did you borrow in private or alternative loans for the XX school year? Do not include any money borrowed in federal loans or any money borrowed from family or friends in your answer. (If you are unsure of the amount of your private loans, please provide your best guess.)"

a public loan and knowing whether or not they have a credit card balance.[34] In particular, Karlan and Zinman (2008) show that individuals are more likely to lie about having higher interest loan and Brown et al. (2015) show that both credit card debt and student loan debt is underestimated in the Survey of Consumer Finance when compared to the CCP.[35]

To shed some light on this possibility, we follow the analysis in Akers and Chingos (2014). They also use the NPSAS administrative data, which provide accurate reports of federal loans, matched to the student survey reports of federal loans. They show that students in four-year public and private schools are more likely to under-estimate than over-estimate their student debt. This is consistent with Brown et al. (2015) who find that the Survey of Consumer Finance understates student loan amounts by roughly 25% when compared to the CCP.

While we would ideally estimate the effect of financial education on the accuracy of reporting, the questions to infer a mis-report are unfortunately only asked in the 2011-2012 wave of the NPSAS. In that cross-section, students in the sample from states with financial education graduation requirements are no more or less likely to under-estimate their federal debt than students in states without such requirements. The average discrepancy is $1,414 (about 34%), with a standard deviation of $8,387. Students from states without public finance graduation requirements have a discrepancy of $1,459, while students from states with the requirement have an average discrepancy of $1,325, which is statistically indistinguishable at the 10 percent level. The variances of the two samples are also nearly identical. Furthermore, as there is a greater likelihood of under-reporting on average, we expect that if financial education affects students' knowledge of their actual debt, this would bias us

[34] We have no reason to believe students would misreport whether or not they work while enrolled based on exposure to financial education.

[35] In a related literature, Bucks and Pence (2008) show that homeowners are more likely to know their mortgage payments than their actual terms of the loan.

against finding an effect. Thus, we think our results will understate the true effect of financial education on private loan amounts.

Second, a common assumption in literature estimating the effect of financial education on financial outcomes is that individuals stay in the same state in which they attended high school. An advantage to the NPSAS data is that we have both permanent resident state and state of college attended. Having both variables allows us to say how this assumption can change estimates. In our NPSAS sample, 85 percent of incoming freshmen students attend a four-year college in their state of permanent residence. Table A.5 reports the baseline results using the state of the college instead of the state of the student's permanent residence. At first glance, the results seem to point to the lack of an effect of financial education. However, the standard errors are substantially larger in this specification. Even though the signs of the effects often flip, the 95 percent confidence intervals of each estimate can never rule out the main effect found in Table 5. We take this as evidence that using state attended high school can be important when reducing measurement error. While we show this for the four-year college-going population, who is most likely to move, we cannot say that this is similar for the population as a whole, especially when large samples are employed as in Brown et al. (2016*b*) using the CCP.

Third, to be sure that the results are not driven by our particular measure of income or the possibility that EFC might be affected by the policy, we remove all controls and estimate the model examining only dependent students in Table A.4.[36] To further validate that EFC and all of our other controls are not affected by the policy, we show that the policy does not influence demographic characteristics or EFC in Table A.3. We also replicate our main results while controlling for state-level unemployment rates.

[36] If we instead just remove EFC or replace it with family income or tuition, the results are unchanged.

Fourth, we show that our classification of having versus not having personal finance education is robust to alternate considerations. Most states passed personal finance mandates after 2000, but four states passed an early version of personal finance graduation requirement in 1998 or earlier.[37] As noted, these early state mandates began with a consumer economics focus that is substantively different from post-2000 mandates that focus more on timely financial management topics, like credit scores, mortgages, retirement saving, and student loans. In addition, these states that passed requirements before 2000 have altered their curricula over time in discrete ways that are challenging to identify. Because these early laws may vary in significant ways from both their later forms, we confirm that our results are robust to dropping these states (Table A.4). In addition, our results are robust to excluding states that mandated personal finance be taught but allowed school district or county flexibility in the way the mandate was implemented, leading to variation in the timing and stringency of the requirement across the state.[38]

5.2 Policy Endogeneity

A final threat to the validity of our estimates is the potential that the policies are themselves endogenous or correlated with omitted factors. Concern may arise that policies are passed within a state when either (1) the states' economic condition warrants these graduation requirements and these economic conditions also affect financing decisions or (2) when the state changes other education policies and these policies affect student outcomes. Specifically, we examine whether graduation requirements occurred in conjunction with factors related to the state's economy, or if states passed other educational policies that might be conflated with the mandate.

[37]IL passed in 1970, MI in 1998, NH in 1993, and NY in 1996.

[38]We also remove Louisiana in this specification, as Hurricane Katrina happened in the year that the first graduating class was expected to fulfill the personal finance education requirement and three states that implemented beginning with intensive pilots (Kansas, New Jersey, and Oregon).

First, do states that pass mandates have fundamentally different economic contexts at the time of passage? We formally test the correlation between state-level economic conditions and personal finance requirements using data from the University of Kentucky's Poverty Center (2016) and our personal finance requirement database. We estimate Equation 3.

$$PF_{s,t} = \alpha + \boldsymbol{\beta X_{s,t}} + \delta_s + \gamma_t + \epsilon_{s,t} \tag{3}$$

Included in $\boldsymbol{X_{s,t}}$ are whether or not the governor is a Democrat, population (in millions), gross state product (in billions), the unemployment rate, Medicaid beneficiaries, SSI recipients, the poverty rate, and average monthly SNAP participants. δ_s and γ_t are state and year fixed effects, respectively.

Table A.6 shows the results from estimating Equation 3. None of the state-level characteristics are predictive of any personal finance graduation requirement. In addition, the magnitudes for each coefficient are close to zero. For example, increasing a state's population by 1 percent in a given year increases the probability of having a rigorous graduation requirement by 0.77 percentage points. Table A.4 also shows that including the state unemployment rate in estimates of Equation 1 yields results that are nearly identical to the baseline results in Table 5.

Second, do states pass financial education graduation requirements at the same time as other graduation requirements that might also affect student borrowing decisions? We examine four such large-scale policy changes that have taken place over this period: changes in the total number of Carnegie units required for graduation, changes in the number of math courses students are required to take in high school for graduation, changes in the highest level of math classes required for graduation, and the requirement that all students take a college placement exam (SAT or

ACT).[39,40]

Information on the courses required for graduation (overall, and math specific) for the graduating classes of 2007 and 2011 comes from the Education Commission of the States[41] We supplement this with the Council of Chief State School Officers reports "Key State Education Policies on PK-12 Education," which is available for 2004, 2006, and 2008.[42] States that have no statewide policies but rely on local school boards to determine graduation requirements are omitted from the analysis. We identify states with current policies using ACT and College Board reported data, supplemented with the Education Commission of the States (ECS) State Policy Database.[43]

We explore the sensitivity of the baseline results (reported in Table 5) to the inclusion of these policies. Table A.7 indicates that when controlling for total credits required, total math credits required, the highest level of math required, and college entrance exam requirements, the coefficient on personal finance education (α_1) remains remarkably stable.

Third, concern may arise that states are taking on other education policies affecting higher education at the same time as personal finance education. We investigate two such policies: implementing automatic in-state scholarships and the level of state appropriations for higher education.[44] Table A.7 shows that controlling

[39]See Hyman (2016); Bulman (2015) for analyses of these policies.

[40]We could not find any other major state-level high school or higher education funding policy changes post 2000.

[41]See `http://ecs.force.com/mbdata/mbprofall?Rep=HS01`. Retrieved December 20, 2016.

[42]Where these sources differ, we refer to state statutes. Some states have two sets of graduation recommendations, one for a college prep track and one for a career track. We use the lowest level of requirements as this is the binding requirement. One Carnegie credit is equivalent to a year of school; for states that use other accounting methods we normalize to a year-long course. We code the highest level of math class as zero for states with no requirement, 1 for states that require Algebra I, 2 for those requiring Geometry (or a course beyond Algebra I), and 3 for those requiring Algebra II (or a course with a similar prerequisite).

[43]See `http://www.edweek.org/ew/articles/2014/10/29/10satact.h34.html` for the 2014 map of participating states. State Policy Database retrieved December 22, 2016.

[44]One example of a state scholarship is the Georgia HOPE scholarship, where students meeting a

for state scholarship programs does not change the effect of financial education requirements on financing behaviors. Similarly, accounting for changes in higher education spending by state over time also does not change the overall effect of financial education on financing behaviors. The sample for Table A.7 is restricted to public institutions as they receive the public funds. Overall, we find no evidence that the estimates are influenced by other state economic conditions, high school graduation requirements, or higher education policies.

5.3 Parallel Trends

Difference-in-difference strategies assume that the treatment and control groups would have had parallel trends in the absence of the policy. This assumption is required for the non-treatment group to represent a proper counterfactual. While this assumption can never be definitively validated, it is common to examine the period prior to the implementation of the policy to see if there were preexisting differences in trends that led up to the treatment period. If such preexisting trends are found, this makes the assumption of parallel trends more suspect.[45] Since the NPSAS data are not collected annually, but rather every three to four years, and the survey measures from earlier waves change somewhat over time, we provide an event study-style test of parallel trends based on waves.

minimum GPA and ACT or SAT requirement can earn scholarships if they attend public or private HOPE-eligible colleges in Georgia. Tennessee has a similar program: students that graduate from a Tennessee eligible high school after 2004 with a minimum ACT of 21 and 3.0 GPA can earn up to $1,750 in scholarships as freshmen if attending a public in state four-year school.

[45]Work by Stephens and Yang (2014) shows that region-by-year fixed effects benefit research designs when policies are correlated by region and time. They show the importance of regional changes in compulsory schooling estimates, where other education investments in specific regions were correlated with both laws and outcomes. In our setting, there are roughly 2-5 states implementing within each of the four Census Regions and across different time periods. When we control for variables like state higher education spending over time, our estimates of financial education do not change, and we do not have reason to think financial education investment or financial learning change by region over our sample period. When we include region-by-year fixed effects, we lose power and the standard errors increase. In no case does the inclusion of region-by-year fixed effects rule out the effects from our baseline specification.

Table 8 shows that there are no definitive trends in the pre-treatment period. There does appear to be something of a pattern of the point estimates for some of the coefficients, with rising differences betweeen the treatment and control states particularly for "have Stafford." However, only one of the 21 coefficients tested in the pre-period is statistically different from zero at the five percent level. No other coefficients are statistically different from zero at the 10 percent level. Even more importantly, our α coefficient remains statistically indistinguishable from our effects in Table 5. Optically, the coefficient on having a credit card balance is no longer statistically different from zero, but it is not statistically different from the average effect in Table 5. Instead, the standard error has increased, likely due to the increased number of parameters estimated.

We further use our supplemental data from the MUS and the CPS to confirm that there are also parallel trends in financial aid and enrollment, respectively. Using the MUS data, we show that in schools with personal finance course offerings, there is not a statistical difference in subsidized Stafford amounts, unsubsidized Stafford amounts, the probability of having grants or scholarships, and non-loan aid amounts in the years before the offering. These results are in Table B.2. Using the CPS data, we show that states requiring personal finance further have no clear pre-trends in college enrollment, full-time college enrollment, and part-time college-enrollment in Table C.2.[46] In both cases, the evidence supports the parallel trends assumption.

6 Voluntary Offerings of Financial Education

Even in states where personal finance graduation requirements do not exist, high schools have the autonomy to offer a course. We seek to estimate the effect of personal finance courses when enrollment is optional. We examine this question

[46]We show this with IPEDS data as well in Table C.3.

in a state without a mandate, relying on local variation in personal finance course *offerings* to determine the intent-to-treat effect of personal finance courses on aid packages. This detailed analysis informs the previous state-based analysis in two ways. First, it indicates how a less stringent requirement for schools to offer an elective course in personal finance may influence average financial aid packages. This is particularly relevant if take-up rates are low or self-selecting students have different effects than the average student. Second, it helps us measure the degree to which the effect found in the NPSAS analysis is likely to be a lower bound of the true effect of financial education. If students complete effective courses in states without mandates or in states with mandates prior to their passage, the initial analysis will understate the effect of financial education on financial aid packages.

We include in our analysis high school fixed effects, year fixed effects, and individual characteristics, such as a white and missing race dummy, age dummies, a male indicator, ACT scores,[47] and campus dummies. We are careful to cluster our standard errors at the high school level as this is where policies vary.

An advantage of administrative data in a localized setting is to understand the characteristics of schools that had financial education prior to state mandates. This distinction is in Table B.1, where we compare all of our dependent and independent variables by whether or not a school ever offered a personal finance class. Note that this does not take into consideration the timing of adding the course. Table B.1 shows that there are no clear differences in financial aid packages across the two groups. Student-level characteristics are not notably different across the two groups. Figure 2 documents that there are no clear geographic patterns in implementation, such as clustering in one area of the state, or proximity to major cities or highways. Thus, it is reasonable to assume that adding personal finance as an elective

[47]For students that send SAT scores instead of ACT scores, we convert these scores to ACT using the College Board's transformation.

is idiosyncratic across schools. Table B.2 suggests that the difference-in-difference assumption of parallel trends in our outcome variables based on the course offering is likely to be met. Those who graduated 1 through 7 years before the course was first offered in the school have no differences in outcomes when compared to those graduating 8 or more years before the course was offered, and the coefficients on PF Offering -1 through PF Offering -7 are not statistically different from each other, confirming there are no clear trends.

Table 9 reports the results, where across Columns (1)-(3), offering financial education has no statistically significant effect on having subsidized Stafford loans, having unsubsidized Stafford loans, or having a grant.[48] This could be due to low take-up rates, poor implementation of courses, or minimal effects on the type of students who self-select these courses. While the estimates are close to zero in magnitude, the confidence intervals for the estimates in Columns (1)-(3) are wide. The presence of elective courses when mandates are absent do not appear to have similar effects as state requirements for all students.

7 Conclusions

Student loan reform has been a pressing policy topic for the last few years. Our results show that high school financial education graduation requirements can significantly impact key student financial behaviors. These mandates increase the likelihood that students apply for aid and increase reliance on both grants and subsidized federal loans. At the same time, these requirements decrease private loan amounts for borrowers and decrease the likelihood of carrying a credit card balance.

These results are complementary to those in Brown et al. (2016*b*) but more di-

[48] In these data we cannot determine if students work while in school. However, we see no evidence that students change their rate of work study participation.

reclty show the education improves financial decisionmaking. Brown et al. (2016*b*) find that personal finance coursework is associated with a modest and statistically insignificant average increase in total student debt (roughly $161 for 22 year-olds).[49] Our findings suggest that latter effect has two sources. Financial education *increases* federal loan amounts, but for the subset of private loan borrowers, financial education *reduces* balances.

We further flesh out this finding by showing that the average effect obscures differential responses by demographic group. Our data indicate that the increases in public loans and decreases in carrying a credit card balance are largely from students with lower EFCs, while the decreases in private loans stem from more advantaged students (with higher EFCs) who typically have greater access to multiple forms of credit. For students from lower EFC families, we find that financial education reduces working while enrolled. In the long-run, this could mean that students are more likely to persist and graduate. Understanding these differential effects can help policymakers to better adjust policy that encourages information and skills over one-size-fits all postsecondary education financing regimes.

The costs of financial education requirements primarily stem from the opportunity cost of displacing other courses or content and training and supporting teachers to take on a new course. The opportunity costs are likely to be low, as in many states schools incorporate personal finance concepts into already-existing courses, such as economics.[50] While costs associated with teacher training are not negligible, states implementing financial education requirements often offer sample curricula

[49]The authors find increases in student loan debt for 25 and 27 year-olds, though this age range is more likely to have completed more years of college.

[50]Most state policies incorporated personal finance into economics. Prior to the personal finance requirement, there were no specific standards and teachers were supposed to "teach economics." Once the personal finance requirement began, specific standards for both economics and personal finance were included, likely raising the quality of instruction for both subjects for the average instructor.

and statewide webinars and trainings to minimize costs.

We emphasize that our results are for initial decisions among first-time students at four-year institutions and thus can be a lower bound estimate of the full effects of the policy. Particularly if student financial aid decisions are persistent over time, there may be a larger cumulative reduction in high-cost borrowing in favor of low-cost borrowing in the long-run, and there may be additional effects on persistence and graduation that extend beyond the timeframe we analyze. In assessing the benefits, we also note that high school personal finance is geared more towards building general skills than to the single financial aid decision. As a result, the benefits of this curriculum extend beyond those under study here, especially as previous literature finds that this type of high school education also reduces non-student debt, increases young adult credit scores, and decreases severe delinquencies (Urban et al. 2018; Brown et al. 2016*b*). The broad set of impacts of financial education mandates are suggestive of the role of financial capabilities and skills in contributing to a range of improved financial decision making among young adults.

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Tables and Figures

Figure 1: Financial Education Requirements

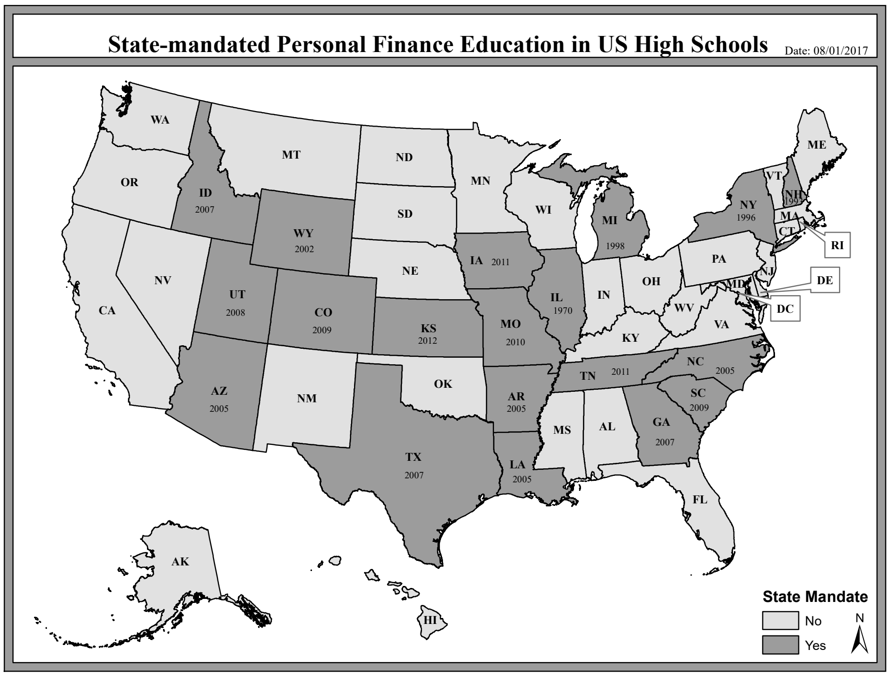

Table 1: States with Personal Finance Graduation Requirements

State	First Graduating Class Affected	State	First Graduating Class Affected
Arkansas	2005	New Hampshire	1993
Arizona	2005	New Jersey	2011
Colorado	2009	New Mexico*	2003
Georgia	2007	New York	1996
Iowa	2011	Oregon	2013
Idaho	2007	South Carolina	2009
Illinois	1970	South Dakota*	2006
Kansas	2012	Tennesse	2011
Louisiana	2005	Texas	2007
Michigan	1998	Utah	2008
Missouri	2010	Virginia	2008
North Carolina	2005	Wyoming**	2002
Nebraska*	2011		

Notes: * Denotes that the state required that a course be offered, but not that it is taken. These we denote as not having a policy. ** Denotes that the state had only one personal finance standard to be implemented in social studies. Wyoming is included as having a policy. We note that Connecticut, Oregon, Virginia, and West Virginia had local control over how to implement the policies, Louisiana's policy occurred the same year as Hurricane Katrina, and New Jersey, Kansas, and Oregon conducted pilots at the same time as their requirements were to take effect. For more on the full dataset, see `http://www.montana.edu/urban/financial-edu-database.html`.

Table 2: Summary Statistics by Financial Education Status

	No PF	PF Required	Both
Dependent Variables			
Applied for Aid	0.907	0.934	0.915
	(0.291)	(0.248)	(0.279)
Stafford Loan	0.540	0.599	0.558
	(0.498)	(0.490)	(0.497)
Have Grant	0.865	0.664	0.748
	(0.342)	(0.472)	(0.434)
Private Loan	0.111	0.120	0.114
	(0.314)	(0.325)	(0.317)
Have CC Balance	0.096	0.094	0.095
	(0.295)	(0.292)	(0.294)
Work while Enrolled	0.468	0.420	0.454
	(0.499)	(0.494)	(0.498)
Independent Variables			
Male	0.442	0.441	0.442
	(0.497)	(0.497)	(0.497)
White	0.732	0.657	0.710
	(0.443)	(0.475)	(0.454)
Black	0.097	0.150	0.113
	(0.296)	(0.357)	(0.316)
Hispanic	0.091	0.120	0.100
	(0.288)	(0.325)	(0.299)
Age 17	0.0080	0.009	0.008
	(0.088)	(0.095)	(0.090)
Age 19	0.364	0.319	0.351
	(0.481)	(0.466)	(0.477)
Dependent	0.974	0.971	0.973
	(0.160)	(0.169)	(0.163)
EFC (000s)	14.7	14.6	14.7
	(18.7)	(19.4)	(18.9)
Parent < HS	0.024	0.027	0.025
	(0.153)	(0.162)	(0.156)
Parent HS Grad	0.182	0.184	0.182
	(0.386)	(0.388)	(0.386)
Parent Some Coll	0.204	0.222	0.209
	(0.403)	(0.416)	(0.407)

Notes: Source: NPSAS data (1999, 2003, 2007, 2011). EFC is expected family contribution.

Table 3: Personal Finance Graduation Requirements do not Change College Attendance

	(1) College At All	(2) College Full Time	(3) College Part Time
PF	-0.007	-0.006	-0.001
	(0.007)	(0.007)	(0.002)
N	510,933	510,933	510,933

Notes: Source: Current Population Survey data (1995-2013). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regression includes state, survey month, and year fixed effects and the following controls: male, age 18 and age 19 dummies, marital status, white, black, and hispanic indicators, and a dummy for whether or not the respondent lives in a city. The regressions also include CPS weights but are robust to not including these weights.

Table 4: Personal Finance Graduation Requirements and Choice of Institution Type

	(1) Private	(2) Tuition & Fees	(3) In State	(4) Four yr
PF	-0.002	-685.225	-0.020	0.017
	(0.042)	(678.172)	(0.017)	(0.045)
N	25,354	22,437	25,354	54,546

Notes: Source: NPSAS data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regression includes state and year fixed effects and all covariates listed in Table A.2. Columns 1 through 3 include only four-year students; Column 4 includes students at two- and four-year institutions.

Table 5: Federal Financial Aid Decisions at Four-Year Institutions

	(1) Applied for Aid	(2) Have Stafford	(3) Have Sub Stafford	(4) Have Grant	(5) Have Private	(6) Have CC Balance	(7) Work while Enrolled
PF	0.033*	0.053*	0.057*	0.033*	-0.003	-0.021*	-0.014
	(0.013)	(0.023)	(0.022)	(0.017)	(0.007)	(0.008)	(0.014)
N	25,354	25,354	25,354	25,354	25,354	25,354	25,354

Notes: Source: NPSAS data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. All reported results are from the α_1 coefficient in Equation (1). Each regression includes state and year fixed effects. $PF = 1$ if the student's permanent address was in a state that required personal finance prior to graduating high school and 0 otherwise. Estimated control variables are in Table A.2.

Table 6: Heterogenous Effects of Personal Finance Graduation Requirements by EFC

	(1) Applied for Aid	(2) Have Stafford	(3) Have Sub Stafford	(4) Have Grant	(5) Have Private	(6) Have CC Balance	(7) Work while Enrolled
Below Median EFC							
PF	0.028^{**}	**0.084***	**0.087***	0.034^{*}	0.004	**-0.032****	**-0.035***
	(0.009)	(0.032)	(0.033)	(0.015)	(0.009)	(0.012)	(0.016)
N	12,678	12,678	12,678	12,678	12,678	12,678	12,678
Mean	0.9577	0.6503	0.6346	0.8837	0.1130	0.1182	0.4819
Above Median EFC							
PF	0.039^{+}	**0.028**	**0.036***	0.029	-0.009	-0.007	**0.011**
	(0.021)	(0.023)	(0.015)	(0.028)	(0.010)	(0.008)	(0.018)
N	12,676	12,676	12,676	12,676	12,676	12,676	12,676
Mean	0.8723	0.4649	0.2537	0.6127	0.1143	0.0727	0.4252

Notes: Source: NPSAS Data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. All reported results are from the α_1 coefficient in Equation (1). Each regression includes state and year fixed effect and all covariates listed in Table A.2, except for the variable corresponding to the subgroup listed. Bold indicates that the coefficient for the relevant demographic group is statistically different zero and statistically different from the average effect in Table 5.

Table 7: Effects Conditional on Applying for Aid and Borrowing Amounts Conditional on Borrowing

	Cond'l on Applying			Cond'l on Borrowing		
	Have Stafford	Have Sub Stafford	Have Grant	Sub Stafford $s	Unsub Stafford $s	Private Loan $s
Overall						
PF	0.044^{+}	0.054*	0.011	8.775	7.391	-1,307**
	(0.026)	(0.024)	(0.011)	(29.206)	(81.186)	(470.9)
N	23,199	23,199	23,199	11,261	8,518	2,882
Mean	0.6094	0.4854	0.8177	2,871	2,996	7,065
Below Median EFC						
PF	**0.073***	0.077*	0.009	20.693	93.452	**-478.5**
	(0.034)	(0.034)	(0.013)	(27.813)	(128.900)	(593.270)
N	12,142	12,142	12,142	8,045	3,988	1,433
Mean	0.6790	0.6626	0.9227	2,908	2,799	5,749
Above Median EFC						
PF	0.020	0.042*	0.009	87.738	126.152	**-2,356*****
	(0.024)	(0.016)	(0.021)	(68.689)	(81.156)	(464.949)
N	11,057	11,057	11,057	3,216	4,530	1,449
Mean	0.5330	0.2909	0.7024	2,779	3,170	8,366

Notes: Source: NPSAS Data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. All reported results are from the α_1 coefficient in Equation (1). Each regression includes state and year fixed effect and all covariates listed in Table A.2, except for the variable corresponding to the subgroup listed. Bold indicates that the coefficient for the relevant demographic group is statistically different zero and statistically different from the average effect in Table 5.

Table 8: Testing for Preexisitng Trends

	(1) Applied for Aid	(2) Have Stafford	(3) Have Sub Stafford	(4) Have Grant	(5) Have Private	(6) Have CC Balance	(7) Work while Enrolled
5-6 Yrs Pre	-0.012	0.014	0.011	-0.028	-0.017	0.012	-0.010
	(0.019)	(0.023)	(0.021)	(0.019)	(0.015)	(0.015)	(0.026)
3-4 Yrs Pre	0.020	0.031	0.004	0.079*	-0.010	-0.006	-0.027
	(0.012)	(0.030)	(0.027)	(0.030)	(0.012)	(0.011)	(0.020)
1-2 Yrs Pre	0.010	0.036	-0.001	0.047	-0.011	0.016	0.016
	(0.021)	(0.025)	(0.023)	(0.029)	(0.016)	(0.013)	(0.027)
PF	0.040**	0.072*	0.057*	0.074**	-0.008	-0.009	-0.013
	(0.012)	(0.028)	(0.028)	(0.021)	(0.016)	(0.013)	(0.018)
N	25,354	25,354	25,354	25,354	25,354	25,354	25,354

Notes: Source: NPSAS Data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The excluded group is more than 5-6 years before the requirement was binding. Each regression includes state and year fixed effect and all covariates listed in Table A.2, except for the variable corresponding to the subgroup listed.

Table 9: Offering Personal Finance and Financial Aid in Montana

	(1) Have Sub Stafford	(2) Have Unsub Stafford	(3) Have Grant
PF Offered	-0.008	-0.007	-0.001
	(0.010)	(0.012)	(0.012)
N	21,385	21,385	21,385

Notes: Robust standard errors clustered at the high school level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Source: Montana University System administrative data (2002-2014). Private student loans are not included in these data. Only loans equals one if students have loans and no grants or scholarships in their financial aid packages. Each regression includes high school and year fixed effects, sex, white and missing race dummies, age dummies (17 and 18, with 19 the excluded group), ACT (or SAT converted to ACT), and campus dummy. Have Sub Stafford and Have Unsub Stafford are dummy variables, equal to one if the individual had positive Stafford Subsidized or Unsubsidized loans, respectively. Have Grant= 1 if the given student had any form of merit, need-based, federal, or state grants and zero otherwise; it does not include external grants that were given as checks directly to the student and not through the university financial aid. PF Course Offered = 1 if the student went to high school that offered personal finance prior to the time she graduated from high school.

Appendix A: Robustness Checks in NPSAS Data

Table A.1: Federal Financial Aid Decisions at All Institutions (Includes Two- and Four- Year Institutions in Public, Private, and For- Profit Sectors)

	(1) Applied for Aid	(2) Have Stafford	(3) Have Sub Stafford	(4) Have Grant	(5) Have Private	(6) Have CC Balance	(7) Work while Enrolled
PF	0.005	0.042*	0.045**	0.006	-0.005	-0.011*	0.011
	(0.012)	(0.016)	(0.015)	(0.016)	(0.006)	(0.005)	(0.015)
N	54,546	54,546	54,546	54,546	54,546	54,546	54,546
Mean	0.9000	0.4943	0.4199	0.7044	0.0956	0.1198	0.5525

Notes: Source: NPSAS data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regression includes state and year fixed effects. $PF = 1$ if the student's permanent address was in a state that required personal finance prior to graduating high school and 0 otherwise. Additional control variables are listed in Table A.2.

Table A.2: Federal Financial Aid Decisions at Four-Year Institutions, Including Control Variables

	(1) Applied for Aid	(2) Have Stafford	(3) Have Sub Stafford	(4) Have Grant	(5) Private Loan	(6) Have CC Balance	(7) Work while Enrolled
Male	-0.016***	-0.017*	-0.007	-0.019***	0.007+	-0.015***	-0.042***
	(0.003)	(0.007)	(0.007)	(0.005)	(0.004)	(0.004)	(0.008)
White	-0.023*	0.028	0.013	-0.007	0.009	-0.016**	0.043***
	(0.010)	(0.026)	(0.022)	(0.009)	(0.010)	(0.006)	(0.012)
Black	0.034***	0.183***	0.151***	0.023*	0.022*	0.025*	-0.037+
	(0.009)	(0.021)	(0.019)	(0.011)	(0.009)	(0.010)	(0.019)
Hispanic	0.026*	0.052*	0.025	0.034*	0.010	0.013	0.047***
	(0.010)	(0.022)	(0.019)	(0.014)	(0.007)	(0.011)	(0.013)
Age 17	-0.013	-0.064*	-0.031	0.005	-0.001	-0.011	0.002
	(0.016)	(0.031)	(0.031)	(0.025)	(0.016)	(0.022)	(0.031)
Age 19	-0.024***	-0.014**	-0.005	-0.032***	0.003	0.025***	0.034***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.004)	(0.004)	(0.007)
Dependent	0.089***	0.142***	0.141***	0.112***	0.010	0.054***	-0.102***
	(0.017)	(0.017)	(0.017)	(0.018)	(0.011)	(0.011)	(0.025)
EFC Q1	0.076***	0.155***	0.424***	0.336***	-0.028**	0.040***	0.021+
	(0.007)	(0.015)	(0.019)	(0.017)	(0.008)	(0.006)	(0.012)
EFC Q2	0.080***	0.235***	0.494***	0.309***	0.010+	0.033***	0.054***
	(0.006)	(0.013)	(0.017)	(0.015)	(0.006)	(0.006)	(0.009)
EFC Q3	0.037***	0.178***	0.374***	0.091***	0.040***	0.013**	0.049***
	(0.006)	(0.010)	(0.014)	(0.011)	(0.007)	(0.004)	(0.009)
Parent	0.023*	0.023	0.019	0.045*	0.006	0.030**	0.077*
< HS	(0.009)	(0.018)	(0.017)	(0.021)	(0.010)	(0.011)	(0.031)
Parent HS	0.047***	0.098***	0.083***	0.034***	0.031***	0.030***	0.076***
	(0.005)	(0.009)	(0.008)	(0.008)	(0.006)	(0.004)	(0.008)
Parent So	0.033***	0.105***	0.086***	0.035***	0.047***	0.022***	0.055***
College	(0.005)	(0.007)	(0.008)	(0.008)	(0.006)	(0.005)	(0.007)
Public	-0.051***	-0.131***	-0.152***	-0.225***	-0.067***	0.013**	0.092***
	(0.007)	(0.016)	(0.013)	(0.013)	(0.005)	(0.004)	(0.012)
N	25,354	25,354	25,354	25,354	25,354	25,354	25,354

Notes: Source: NPSAS data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regression includes state and year fixed effects. $PF = 1$ if the student's permanent address was in a state that required personal finance prior to graduating high school and 0 otherwise. Excluded groups: Other Race, Age 18, Parent College Educated or beyond, Public colleges.

Table A.3: Treating Controls as Outcomes

	Male	White	Black	Hispanic	Age 17	Age 19
PF	0.000	-0.030	0.034	0.006	0.007*	-0.046
	(0.020)	(0.028)	(0.028)	(0.017)	(0.003)	(0.028)
N	25,354	25,354	25,354	25,354	25,354	25,354

	Dependent	Parent $< HS$	Parent HS Grad	Parent So Coll	Public
PF	0.003	-0.004	0.001	0.000	0.002
	(0.007)	(0.006)	(0.014)	(0.009)	(0.042)
N	25,354	25,354	25,354	25,354	25,354

Notes: Source: NPSAS data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regression includes state and year fixed effects.

Table A.4: Robustness Checks

	(1) Applied for Aid	(2) Have Stafford	(3) Have Sub Stafford	(4) Have Grant	(5) Have Private	(6) Have CC Balance	(7) Work while Enrolled
No Controls							
PF	0.035*	0.060*	0.066*	0.035	-0.003	-0.020*	-0.016
	(0.016)	(0.026)	(0.026)	(0.025)	(0.008)	(0.008)	(0.017)
N	25,354	25,354	25,354	25,354	25,354	25,354	25,354
Dependent Students Only							
PF	0.031*	0.052*	0.056*	0.032+	-0.005	-0.022*	-0.012
	(0.013)	(0.022)	(0.021)	(0.017)	(0.007)	(0.009)	(0.014)
N	24,664	24,664	24,664	24,664	24,664	24,664	24,664
Controlling for State-level Unemployment							
PF	0.022**	0.033+	0.040*	0.030*	-0.004	-0.005	-0.013
	(0.007)	(0.019)	(0.019)	(0.013)	(0.009)	(0.003)	(0.011)
N	25,354	25,354	25,354	25,354	25,354	25,354	25,354
No early policy states–Drop states implementing pre-1996							
PF	0.029*	0.038	0.041+	0.028+	-0.009	-0.025**	-0.017
	(0.013)	(0.023)	(0.021)	(0.016)	(0.007)	(0.008)	(0.015)
N	21,063	21,063	21,063	21,063	21,063	21,063	21,063
No locally determined policy states							
PF	0.036*	0.059*	0.060*	0.037*	-0.005	-0.018*	-0.024+
	(0.014)	(0.024)	(0.023)	(0.018)	(0.007)	(0.009)	(0.014)
N	22,942	22,942	22,942	22,942	22,942	22,942	22,942
Drop "gray area" policy states							
PF	0.035*	0.053*	0.059*	0.034*	-0.002	-0.021*	-0.018
	(0.013)	(0.023)	(0.022)	(0.017)	(0.007)	(0.009)	(0.014)
N	24,217	24,217	24,217	24,217	24,217	24,217	24,217
Results from Probits							
PF	0.033*	0.053*	0.057*	0.033*	-0.003	-0.021*	-0.014
	(0.013)	(0.023)	(0.022)	(0.017)	(0.007)	(0.008)	(0.014)
N	25,354	25,354	25,354	25,354	25,354	25,354	25,354

Notes: Source: NPSAS Data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. All reported results are from the α_1 coefficient in Equation (1). Each regression includes state and year fixed effect with control variables from table A.2, except for the top panel that only includes state, year, and age fixed effects.

Table A.5: Baseline Results when using State of College Instead of State of Residence

	(1) Applied for Aid	(2) Have Stafford	(3) Have Sub Stafford	(4) Have Grant	(5) Have Private	(6) Have CC Balance	(7) Work while Enrolled
PF	0.011	-0.006	0.006	0.020	-0.014^{+}	-0.011^{+}	-0.007
	(0.010)	(0.014)	(0.014)	(0.017)	(0.008)	(0.006)	(0.019)
N	25,243	25,243	25,243	25,243	25,243	25,243	25,243

Notes: Source: NPSAS data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regression includes state and year fixed effects. $PF = 1$ if the student's permanent address was in a state that required personal finance prior to graduating high school and 0 otherwise. Estimated control variables are in Table A.2.

Table A.6: State Characteristics and Personal Finance Requirements

	PF
Governor is Democrat	0.00967
	(0.033)
Unemployment rate	-0.02872
	(0.019)
Medicaid beneficiaries	-0.00003
	(0.000)
SSI recipients	0.00030
	(0.001)
ln(Gross State Product)	0.14930
	(0.359)
Poverty Rate	-0.00217
	(0.007)
ln(Population)	0.76734
	(0.749)
Food Stamp/SNAP Recipients	0.00008
	(0.000)
N	1,145

Notes: Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. This regression includes state and year fixed effects. Gross state product is in billions; population is in millions; Medicaid beneficiaries, SSI recipients, and SNAP recipients are in thousands. Governor is Democrat is a dummy variable equal to one if the governor is a Democrat in the given state for the given year.

Table A.7: Results Robust to Controlling for Other Educational Policies

	(1) Applied for Aid	(2) Have Stafford	(3) Have Sub Stafford	(4) Have Grant	(5) Have Private	(6) Have CC Balance	(7) Work while Enrolled
Controls for Total Credits Required for Graduation							
PF	0.056***	0.141***	0.153***	0.049**	0.023*	-0.009	-0.082***
	(0.014)	(0.021)	(0.019)	(0.017)	(0.010)	(0.007)	(0.020)
N	20,018	20,018	20,018	20,018	20,018	20,018	20,018
Controls for Total Math Credits							
PF	0.029+	0.041+	0.049*	0.019	0.007	-0.019*	-0.024
	(0.015)	(0.024)	(0.023)	(0.016)	(0.007)	(0.008)	(0.016)
N	19,557	19,557	19,557	19,557	19,557	19,557	19,557
Controls for Highest Math Required							
PF	0.033*	0.046+	0.057*	0.024	0.003	-0.019*	-0.025
	(0.014)	(0.025)	(0.022)	(0.016)	(0.007)	(0.007)	(0.015)
N	23,093	23,093	23,093	23,093	23,093	23,093	23,093
Controls for ACT or SAT Required							
PF	0.031*	0.052*	0.055*	0.031+	-0.004	-0.023**	-0.014
	(0.013)	(0.023)	(0.022)	(0.017)	(0.007)	(0.008)	(0.014)
N	25,354	25,354	25,354	25,354	25,354	25,354	25,354
Controls for State Scholarship Programs							
PF	0.033*	0.053*	0.058**	0.032+	-0.003	-0.021*	-0.014
	(0.013)	(0.023)	(0.021)	(0.017)	(0.007)	(0.009)	(0.014)
N	25,354	25,354	25,354	25,354	25,354	25,354	25,354
Controls for Higher Ed Spending							
PF	0.037*	0.064*	0.073**	0.027	-0.010	-0.046**	-0.055***
	(0.015)	(0.025)	(0.022)	(0.026)	(0.010)	(0.014)	(0.014)
N	14,714	14,714	14,714	14,714	14,714	14,714	14,714

Notes: Source: NPSAS data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regression includes state and year fixed effect and all covariates listed in Table A.2. Highest Math equals 1 if Algebra or equiv, 2 if Geometry, 3 if Algebra II, and 4 if higher than Algebra II. Scholarship equals one if the state has a scholarship policy for attendance within state in the given year and zero otherwise. Spending is the state and local appropriations for public higher education institutions, measured in thousands of per pupil 2016 dollars. Spending regressions only include students attending public institutions.

Appendix B: MUS Data

Figure 2: Financial Education Course Offerings

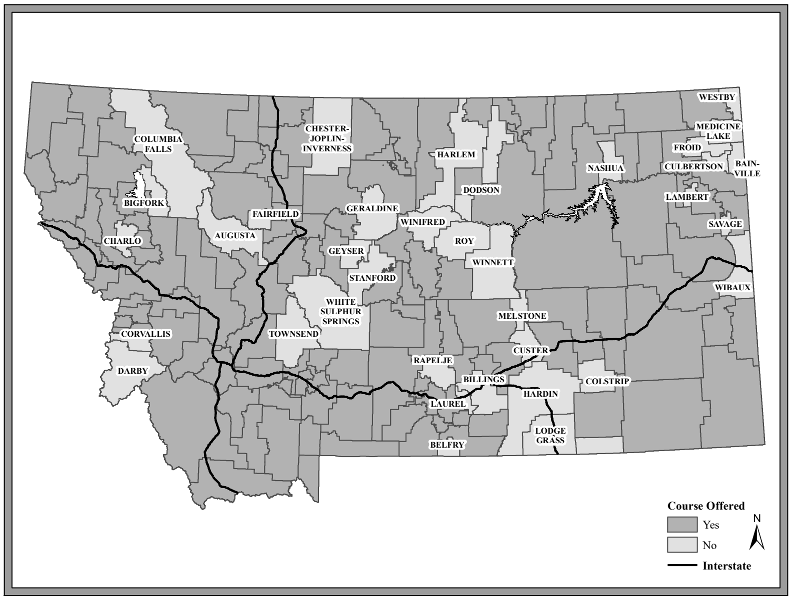

Table B.1: Summary Statistics by Financial Education Offering Status

	No PF	PF Offered	Both
Dependent Variables			
Have Subsidized Stafford	0.394	0.387	0.390
	(0.489)	(0.487)	(0.488)
Have Unsubsidized Stafford	0.258	0.248	0.251
	(0.438)	(0.432)	(0.434)
Stafford Subsidized $s	559.8	547.7	550.7
	(725.8)	(720.6)	(721.9)
Stafford Unsubsidized $s	398.4	386.8	389.6
	(775.4)	(779.5)	(778.5)
Have Grant	0.653	0.623	0.637
	(0.476)	(0.485)	(0.481)
Individual-level Variables			
ACT	22.96	22.86	22.88
	(4.053)	(4.130)	(4.112)
White	0.907	0.907	0.907
	(0.291)	(0.290)	(0.290)
Race Missing	0.0265	0.0242	0.0248
	(0.161)	(0.154)	(0.155)
Male	0.468	0.468	0.468
	(0.499)	(0.499)	(0.499)
Age	18.53	18.50	18.51
	(0.505)	(0.509)	(0.508)
Montana State	0.502	0.564	0.548
	(0.500)	(0.496)	(0.498)

Notes: Robust standard errors clustered at the high school level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Source: Montana University System administrative data (2002-2014). Private student loans are not included in these data. Have Sub Stafford and Have Unsub Stafford are dummy variables, equal to one if the individual had positive Stafford Subsidized or Unsubsidized loans, respectively. Have Grant= 1 if the given student had any form of merit, need-based, federal, or state grants and zero otherwise; it does not include external grants that were given as checks directly to the student and not through the university financial aid. Non-loan aid is the amount of scholarships, grants, awards, and exemptions the student received in dollars. It does not include Pell grants, or other grants received directly by the student that were not awarded through the institution (i.e., private work grants). PF Course Offered = 1 if the student went to high school that offered personal finance prior to the time she graduated from high school.

Table B.2: Pre-trends in MUS Data

	(1) Have Sub Stafford	(2) Have Unsub Stafford	(3) Have Grant
PF Offered	-0.017	-0.007	0.003
	(0.011)	(0.012)	(0.012)
PF Offered −1	-0.019	0.009	0.004
	(0.016)	(0.021)	(0.015)
PF Offered −2	-0.028*	-0.012	0.005
	(0.014)	(0.022)	(0.018)
PF Offered −3	0.009	-0.006	0.016
	(0.015)	(0.020)	(0.018)
PF Offered −4	-0.023	0.008	0.011
	(0.016)	(0.016)	(0.017)
PF Offered −5	-0.037	0.013	-0.012
	(0.024)	(0.017)	(0.019)
PF Offered −6	0.012	0.014	-0.004
	(0.017)	(0.023)	(0.017)
PF Offered −7	-0.009	-0.005	0.044*
	(0.015)	(0.015)	(0.020)
N	21,385	21,385	21,385

Notes: Robust standard errors clustered at the high school level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Data come from the Montana University System administrative data (2002-2014). Private student loans are not included in these data. Only loans equals one if students have loans and no grants or scholarships in their financial aid packages. Each regression includes high school and year fixed effects, sex, white and missing race dummies, age dummies (17 and 18, with 19 the excluded group), ACT (or SAT converted to ACT), and campus dummy. Have Sub Stafford and Have Unsub Stafford are dummy variables, equal to one if the individual had positive Stafford Subsidized or Unsubsidized loans, respectively. Have grant equals one for individuals who had grants or scholarships and zero otherwise. PF Course Offered = 1 if the student went to high school that offered personal finance prior to the time she graduated from high school. PF Offered $-i$ equals one if the course was offered i years after an individual graduated from high school. The excluded group is those who graduated high school more than 7 years before a course was offered in his or her high school.

Appendix C: Enrollment Data

Table C.1: Summary Statistics by Financial Education Requiring Status

	No PF	PF Required	Both
Dependent Variables			
College At All	0.550	0.530	0.541
	(0.497)	(0.499)	(0.498)
College Full Time	0.488	0.472	0.481
	(0.500)	(0.499)	(0.500)
College Part Time	0.0625	0.0579	0.0605
	(0.242)	(0.234)	(0.238)
Individual-level Variables			
Lives in Central City	0.353	0.396	0.372
	(0.478)	(0.489)	(0.483)
Male	0.487	0.486	0.487
	(0.500)	(0.500)	(0.500)
White	0.787	0.784	0.785
	(0.409)	(0.412)	(0.411)
Black	0.124	0.161	0.140
	(0.330)	(0.367)	(0.347)
Hispanic	0.150	0.139	0.145
	(0.357)	(0.346)	(0.352)
Married	0.040	0.052	0.045
	(0.196)	(0.222)	(0.208)
Age	19.37	19.38	19.37
	(0.664)	(0.663)	(0.664)

Notes: Source: Current Population Survey data (1995-2013).

Table C.2: Pre-trends in CPS Data

	(1) College At All	(2) College Full Time	(3) College Part Time
PF	-0.009	-0.008	-0.001
	(0.017)	(0.018)	(0.005)
PF −1	-0.006	-0.010	0.004
	(0.019)	(0.021)	(0.005)
PF −2	-0.000	-0.007	0.007
	(0.019)	(0.020)	(0.006)
PF −3	-0.001	-0.003	0.001
	(0.016)	(0.018)	(0.005)
PF −4	0.001	0.009	-0.008
	(0.015)	(0.018)	(0.005)
PF −5	0.008	0.015	-0.007
	(0.017)	(0.018)	(0.005)
PF −6	-0.018	-0.014	-0.004
	(0.017)	(0.018)	(0.005)
PF −7	-0.013	-0.011	-0.002
	(0.017)	(0.019)	(0.004)
PF −8	-0.007	-0.006	-0.002
	(0.014)	(0.017)	(0.005)
PF −8	-0.004	-0.005	0.001
	(0.016)	(0.017)	(0.007)
PF −10	0.009	0.012	-0.003
	(0.016)	(0.019)	(0.005)
PF −11	0.007	0.015	-0.007
	(0.014)	(0.017)	(0.005)
PF −12	-0.003	0.001	-0.004
	(0.020)	(0.019)	(0.007)
PF −13	0.014	0.010	0.004
	(0.015)	(0.016)	(0.003)
N	510,933	510,933	510,933

Notes: Source: Current Population Survey data (1995-2013). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regression includes state, survey month, and year fixed effects and the following controls: male, age 18 and age 19 dummies, marital status, white, black, and hispanic indicators, and a dummy for whether or not the respondent lives in a city. PF Requirement $-i$ equals one if a personal finance requirement began i years after an individual graduated from high school. The excluded category are individuals who graduated more than 13 years before a PF requirement began. The regressions also include CPS weights.

Table C.3: Personal Finance Graduation Requirements and College Attendance: IPEDS

	Dependent Varaible = Fraction Enrolled in 4-year School			
	(1)	(2)	(3)	(4)
	Resident State	Current State	Resident State	Current State
PF	0.003	0.003	0.006	0.010
	(0.008)	(0.008)	(0.023)	(0.022)
PF −1			0.009	0.014
			(0.022)	(0.022)
PF −2			0.013	0.018
			(0.021)	(0.021)
PF −3			-0.011	-0.006
			(0.024)	(0.023)
PF −4			0.002	0.005
			(0.019)	(0.019)
PF −5			0.003	0.007
			(0.022)	(0.021)
PF−6			-0.004	-0.003
			(0.019)	(0.018)
PF −7			-0.030	-0.027
			(0.024)	(0.023)
PF −8			0.014	0.018
			(0.016)	(0.016)
PF −9			0.013	0.014
			(0.019)	(0.019)
N	765	765	765	765

Notes: Source: Integrated Postsecondary Education Data Sytem (1995-2013). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regression includes state and year fixed effects. The regressions divide total 4-year enrollment from IPEDS by CPS population totals of 18 year-olds in the given state and year. Columns (1) and (3) use the resident state from IPEDS, and Columns (2) and (4) use the state of the postsecondary institution to calculate the numerator.