The Effects of State Mandated Financial Education on College Financing Behaviors

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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 and a sample of incoming freshmen at four year institutions in the National Postsecondary Student Aid Study. Our results suggest financial education graduation requirements increase aid applications and federal loans, while decreasing private loans and the likelihood of holding credit card balances. However, the federal loan increases are concentrated among students from less affluent family backgrounds, while the private loan decrease comes from a separate population of students from more affluent families. The mandates do not change institutional choice or college attendance.
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I. Introduction

Student loan debt now accounts for over $1.28 trillion in the United States, surpassing credit card debt as the second largest source of debt after mortgages (Federal Reserve Bank of New York 2016). While the popular press is full of dire news about student loan decisions, bemoaning the high and rising levels of borrowing, high levels of student loan debt alone do not suggest that these students are behaving suboptimally. However, the average young adult has limited experience in the financial market when navigating decisions about how to finance his or her college education. As Akers and Chingos (2016, p. 113) point out, "Student borrowers are being asked to make an important financial decision that

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requires the kinds of analytical abilities and critical thinking skills that may not develop until they attend college." This paper asks how education in high school that builds specific financial abilities and skills affects student postsecondary education financing.

There is evidence that many students make systematic mistakes in the financial aid process due to administrative costs or lack of information. First, many students falsely believe they are ineligible for federal aid (McKinney and Novak 2015), and a sizeable fraction of students who would be eligible for no-cost grant aid fail to apply.[1] Second, students often apply after priority deadlines and consequently fail to receive some forms of state or institutional financial aid (McKinney and Novak 2015; Callahan 2016). Third, even among students who access credit, 54% of student loan borrowers report that they did not calculate their future monthly payments prior to choosing a loan (Lusardi 2016). Only 29% of students report that they would make the same loan choices again, if given the opportunity to repeat the process (Lusardi 2016).

Some of these mistakes may be related to low levels of financial literacy among young adults. Lusardi, Mitchell and Curto (2010) show that only 27 percent of 23-28 year olds understand basic financial concepts such as inflation, interest, and risk diversification. Results from the 2015 Programme for International Student Assessment (PISA) financial literacy exam corroborate this evidence, where only 29% of 15 year olds in the U.S. are proficient at level 4, meaning they can compare loans with different interest rates and terms (National Center for Education Statistics 2017).

Over the past several deacdes, 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 students to cover personal finance topics prior to graduation, including material on interest rates, saving, credit, debt, and income. Some state personal finance standards cover financing postsecondary education as an explicit component. This paper is the first to determine the causal effect of these financial education graduation standards on the ways in which students finance their postsecondary educations.

The analysis uses a difference-in-difference approach to compare students 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 financial aid, federal and private student loan amounts, earning grants and scholarships, whether or not students carry a credit card balance, and whether or not students work while enrolled.

Our study makes sizebale contributions to existing literature. First, an increas-

[1]Kantrowitz (2009) estimates that nearly 2.9 million undergraduate students would have qualified for Federal Pell Grants in 2007-2008 but did not apply. Kofoed (2017) finds an aggregate of $24 billion in foregone aid.

ing number of studies examine the impact of financial education on knowledge, credit, and debt. Personal finance courses are most likely to affect student aid behavior if they increase financial skills and knowledge. Previous studies document that well-implemented high school courses do improve financial knowledge (Tennyson and Nguyen 2001; Walstad, Rebeck and MacDonald 2010). Urban et al. (2014) find that personal finance graduation requirements increase credit scores and decrease severe delinquencies for 18-22 year olds, and Brown et al. (2016*b*) find that financial literacy exposure reduces non-student debt, increases credit scores, and improves repayment for those under 30.[2,3] Both use data from the New York Federal Reserve Bank/Equifax Consumer Credit Panel. Brown et al. (2016*b*) further find that financial literacy reforms only modestly (and insignficantly) increase student loan debt, making it the most closely related to our study. We differ from Brown et al. (2016*b*) in several key ways. 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 insight into the mechanisms through which financial education may impact financial behaviors (e.g., applying for aid, choosing lower cost sources of credit, or seeking non-loan forms of finance). Second, our data contain individual-level demographic characteristics that are not observable in credit report data, allowing us to explore heterogenous responses. We focus on differences in family background, where we identify heterogeneous effects by race and by expected family contribution. Third, we study graduation requirements rather than more general financial education reforms.[4] Fourth, we focus on initial behaviors of students in the years immediately after high school graduation. This will give us the cleanest lens 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. Finally, we examine whether financial education influences other educational decisions, including institutional choice and college attendance.

This paper contributes to a second body of literature that investigates higher education financing interventions to address the documented complexity of student loan decisions (Dynarski and Scott-Clayton 2006). Our study is novel in both the type of policy examined and the context. The previous literature has typically examined the effects of three types of interventions on financing decisions: the provision of general information, provision of specific or tailored information

[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.

[3]For a slightly different population of new enrollees in bootcamp for the U.S. Army, Skimmyhorn (2016) finds that personal management attendance reduced credit card balances and delinquencies in the same year of the course, but did not change account behavior or credit scores in the subsequent year. The course increased retirement savings for both the current and subsequent year.

[4]We follow the distinction in Urban et al. (2014) in studying graduation requirements as opposed to more general financial literacy reforms. This is because some state mandates never resulted in a graduation requirement, some state mandates required only that an elective be offered, and some state mandates allowed localities to determine implementation.

or assistance, and prompts or nudges targeted to specific actions. In a seminal study, Bettinger et al. (2012) show that randomly assigning personalized help in filling out the FAFSA increases the likelihood of completion, the amount of federal dollars received, and the probability of enrollment. Similarly, Page, Castleman and Meyer (2017) find that customized information about the benefits of applying for aid increase FASFA aplication and college matriculation. In contrast, non-personalized information does not appear to affect enrollment or aid behavior. Bird et al. (2017) further find that sending timely prompts on FAFSA completion increases college enrollment, though information about the financial benefits of FAFSA completion and college completion do not change enrollment behavior.[5]

The policy we study differs from these interventions in three important ways. First, the nature of the intervention is qualitatively different in its focus on skills rather than information. Personal finance courses are not as customized as the individual assistance in Bettinger et al. (2012), but improved skills may be more effective than simple offers of general information, as in the low-cost treatment arm of Bettinger et al. (2012) or in Bird et al. (2017). Second, the courses are typically completed in the junior or senior year of high school but are not as tightly aligned to specific decision dates as the nudges in Bettinger et al. (2012) and Bird et al. (2017). Consequently, the enrollment effects and effects on financial aid are not easily predictable based on these previous interventions.[6] Finally, to the best of our knowledge this is the first paper to study the effects of a state-based policy on financing behaviors, as opposed to efforts at specific education institutions or on a large-scale platforms.[7]

Our main results suggest that personal finance graduation requirements increase the average student's use of federal aid: students are 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 exposed students also reduced private loan balances and were 2 percentage points less likely to carry a credit card balance. The magnitude of the increase in federal subsidized loans (+$106) is statistically similar to the decrease in private loans (-$152), while the magnitude of the increase in those who have a grant (3 percentage points) is similar to the decrease in the percentage of those carrying a credit card balance (2 percentage points).

However, these average effects mask important heterogeneity in student responses to the education. The increases in federal aid are strongest for students

[5]Castleman and Page (2016) further find that community college students nudged to continue completing the FAFSA after the first year of college were more likely to persist, though there was no effect for those at four year institutions.

[6]Additional complementary results come from a number of other efforts at specific education institutions designed to provide students with better information about loans and with improved architecture for making financing choices (Marx and Turner 2016; Castleman and Page 2016; Castleman and Long 2016; Stoddard, Urban and Schmeiser 2017; Darolia 2016; Bettinger and Long 2017).

[7]We acknowledge a separate literature investigating the relationship between state-based aid policies on college enrollment and retention, though these studies do not closely investigate the effects of the policies on student loans or other forms of financing. They also do not provide any type of financial education around postsecondary financing decisions. See Deming and Dynarski (2009) for a review of this literature.

with less affluent family backgrounds: students with expected family contributions lower than $4,000 increased subsidized Stafford loan amounts by $183 on average, while black and hispanic students increased their subsidized Stafford amounts by $260 and $300, respectively. At the same time, reductions in private sector borrowing exist largely for students with more affluent family backgrounds, students with expected family contributions greater than $4,000 and white students. These results are consistent with the fact that private sector borrowing options are most available to relatively advantaged students. The reduction in private lending for these students with more affluent backgrounds is not paired with a comparable increase in public lending, grants, credit card balances, or working while enrolled, suggesting these students have other sources of postsecondary education financing, such as informal borrowing or home equity loans.

To some degree, the estimates may understate the effects of financial education mandates on postsecondary financing decisions, as the control states include schools that may 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 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.

Finally, we investigate the extent to which financial education requirements affect institutional choice. In the NPSAS data, which is based on students attending a post-secondary institution, we demonstrate that graduation requirements do not affect the choice of institution conditional on enrollment. 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 due to the graduation requirement. We 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.[8] This suggests that personal finance information in high school does not change students' decisions about the value of college or their choice of institution (either on average or for specific groups of interest) but may improve the financial skills and abilities needed to complete the FASFA appropriately or to evaluate the impact of higher cost borrowing.

[8]The lack of an enrollment effect is consistent with the lower touch interventions in Bettinger et al. (2012) and Bird et al. (2017) and the lack of enrollment changes in Castleman and Page (2016) for four-year institutions.On the other hand, other studies do find that increased aid or lower tuition prices increase enrollment by between 1 and 5 percentage points (Deming and Dynarski (2009) review this literature.).

II. Background

There are several channels through which learning personal finance content may improve choices of loan 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 startegies, 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 invest 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 with 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 pay-

ing 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. 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).

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 stand-alone 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.[9] 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.[10]

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. 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. 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.

Ideally, this study would test the effects of state-based financial education mandates on student knowledge to determine if an increase in skills and human capi-

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

[10]Since 2011, eight states have passed financial education graduation requirements, and four required standards be implemented into any course. Additional states have bills currently being prepared for House votes.

tal can explain the results. However, reliable testing data for 18 year olds across states and over time do not currently exist.[11] Walstad, Tharayil and Wagner (2016) point out further limitations of testing data in determining the effects of state-level policies on financial knowledge. Despite the lack of cross-state multi-year testing data, Walstad, Tharayil and Wagner (2016) discuss findings in previous literature that document increases in knowledge due to specific high school financial education curricula in specific settings.[12]

III. Data

The bulk of the analysis draws on data from the NPSAS to determine the causal effect of financial education on student aid decisions. To support 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. Further, we use data from the CPS and IPEDS to show that financial education requirements do not affect students' decisions to enroll in college. These data supplement the main analysis. We describe each below.

A. 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 data, along with student and parent interview responses about demographics, high school degree, family background, private loans, credit card balances, and work.[13] The main results use data from the 1999, 2003, 2007, and 2011 waves of this survey. We choose this period for 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.[14] Furthermore, a 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. When testing for pre-trends, we include data from the 1989, 1992, and 1995 waves using a smaller set of outcomes, and a series of robustness checks also examines heterogeneity in state policies.

[11] While the Programme for International Student Assessment (PISA) provides rigorous testing across states and over several years, these data are for 15-year olds, who would not yet be exposed to personal finance education.

[12] This is in contrast to the findings in Jacob et al. (2017), who find that a statewide broad-based mandatory college prepartory curriculum in Michigan did not change overall ACT performance in any subject except for Science.

[13] While the federal loan data are administrative, the private loan data are based on student survey responses.

[14] Only three states implemented personal finance graduation requirements between 1989 and 1999. 19 adopted between 1999 and 2011.

Important for our study is that the NPSAS reports a student's legal state of residence, which is drawn from the student's reported permanent address.[15] 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.[16] 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.[17] This restriction reduces the mismatch of the graduation requirements to students' high school states, as a student's permanent residence is most likely to be in the state he completed high school. The choice to examine incoming freshmen has two other benefits. First, it allows us to focus on students at the pivotal point when they make their initial decisions on how to finance their postsecondary education. Second, we do not confound our results with additional financial education and financial counseling offered by colleges and universities. Since 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. We thus choose to estimate our effects as cleanly as possible, by focusing only on incoming freshmen.

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).[18] 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.[19] We focus on students enrolled at four-year 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.[20]

[15]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."

[16]84% of students in our NPSAS sample go to school in the same state as their legal state of residence.

[17]11.6% of the sample are foreign born.

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

[19]The 1999 wave is smaller than in later years because of the smaller target number of students for the sample.

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

Table 2 shows the characteristics of the sample by state personal finance requirement. Across the states, over 90 percent of students apply for some type of aid, although this does not indicate whether applications were timely or completed correctly.[21] Over half (55%) of students have a Stafford loan, which is substantially higher than the 11 percent of students that have private loans. Average private loan amounts are also smaller than Stafford loans, $800 when compared to nearly $2,300 in total Stafford unsubsidized and subsidized loans combined, although these averages include students with no loans. 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.[22] About 20 percent of students have parents without any college education.

B. 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.[23] 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 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.

age of 21). We do estimate the effect of personal finance education on student loans for the full sample of two-year and four profit students with the same age restrictions. These results are in Table A.1. Not suprisingly, the results are muted relative to our baseline specifications.

[21] 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.

[22] 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.

[23] 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 less complete.

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,[24] 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. 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.

C. 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 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.[25] We assume that students remain in the same state in which they attended high school until they are age 20, which is likely to contain more error when compared to the NPSAS recorded state of legal residence.[26] Appendix C reports and discusses the results from this exercise. 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).

[24]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.

[25]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.

[26]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.

IV. Empirical Strategy

This paper uses a difference-in-difference strategy to determine the causal effect of financial education graduation requirements on postsecondary financing decisions. 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.

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

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. These outcomes include whether a student applied for financial aid, accepted any financial aid, the specific components of the federal Stafford loans obtained, whether the package only included grants and/or scholarships, private loans balances, credit card balances, and the decision to work while enrolled.

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 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 the EFC, which is based on a measures related to income, assets, 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.

V. Results

Table 3 reports the causal effects of personal finance graduation requirements on financing behaviors at four-year institutions (α_1 from Equation 1). The first two 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,[27] and the dependent variable for Column 2 is a binary variable equal to one if the student accepted a direct federal Stafford loan. The dependent variables for Columns 3 and 4 are the specific dollar amounts of Stafford subsidized and unsubsidized loans, respectively, and include non-borrowers as zeros.[28] Column 5 indicates whether or not a student had grants and/or scholarships in his/her aid package. Column 6 includes results using a binary variable equal to one if the student took out a private loan to finance their education, while Column 7 reports results for the balance on that private loan (including zeros). The dependent variables for Column 8 and 9 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.

The results in Table 3 indicate that personal finance requirements do change student behavior on important margins. Students subject to these requirements were 3 percentage points more likely to apply for aid and consequently 5.3 percentage points more likely to have taken out a Stafford loan. The results in Column 3 suggest that these students took out roughly $106 more in subsidized Stafford loans, which is statistically different from zero at the ten percent level. Column 4 shows that unsubsidized loan amounts appear unresponsive to the policy both in terms of the near zero magnitude of the effect and its overall statistical insignificance.[29]

Columns 5-9 in Table 3 report the effect of personal finance graduation requirements on non-federal aid decisions. Students exposed to the graduation requirements were 3.1 percentage points more likely to have aid packages with grants or scholarships, with this estimate statistically different from zero at the 10 percent level.[30] We acknowledge that this measure does not include scholarships that are given directly to students, as opposed to the school, such as Rotary Club Scholarships.[31]

Table 3 further shows that those exposed to financial education were 2.1 percentage points less likely to carry a credit card balance, a relatively large effect, given that the sample average is 10 percent.[32] This finding is interesting, as credit cards are one way that college freshmen can smooth consumption to pur-

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

[28]Both Columns 3 and 4 include those without any Stafford loans. Since so many students do not have loans, one possibility would be to use a Tobit specification. We follow the advice of Angrist and Pischke (2009) in reporting results from linear models. If we use a Tobit specification, our results remain consistent. Tobit results are in Table A.2.

[29]Estimates of control variables for Table 3 are in Table A.3.

[30]We find no evidence of non-loan aid being correlated with grants and scholarships that tie students to a specific major. Those exposed to financial education were no more likely to declare a major at the time of entrance than students who were not. Conditional on declaring a major, students exposed to education were no more likely to declare STEM majors, which typically house the most scholarship aid.

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

[32]We see no difference in the amount of credit card balances, though students report bunched measures at even numbers when answering this question, suggesting that they do not know the precise amount.

chase books and food when they can no longer access student loans. Brown et al. (2016*a*) report that 2015 average credit card balances for 20 year olds were $176. 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). The authors also find that there are information problems, where 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, where average amounts increased by roughly $106, or they may be increasing their use of grant at (3 percentage point increase). 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. Finally, Column 9 reports that financial education does not change individuals' self-reported decisions to work during college on average.

To be sure that the specifications are not driven by our particular measure of income and that EFC might be affected by the policy, we remove all controls in Table A.4. We further estimate the model examining only dependent students and only full-time students in Table A.5. All of these results are consistent with our main findings. If we instead just remove EFC or replace it with family income or tuition, the results are unchanged. 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.6.

A. Heterogeneity by Demographic Characteristics

For whom are these personal finance requirements most likely to affect behavior? We focus on heterogeneity by family background including EFC and race or ethnicity.[33] Table 4 reports the α_1 coefficient from Equation 1 for the subgroup listed on the left. The coefficient estimates are bolded where the coefficient for the sub-group is statistically different from zero and from the average effect.

The top panel of Table 4 divides students into two groups: those with EFCs above and below $4,000.[34] The results indicate that the decrease in private loan amounts come from those with higher EFCs (above $4,000), while the increase in subsidized Stafford amounts come from students with lower EFCs (below $4,000). These effects are both statistically different from the average in Table 3. Students from families with lower EFCs are also less likely to be working while in school, suggesting that the additional federal aid may be used to replace work for these students. This is an important finding given that Stinebrickner and Stinebrickner (2003) and Darolia (2014) find that for full-time students at four-year universities,

[33]For brevity, the results here exclude the extensive margin decisions to take out Stafford or private loans.

[34]The EFC formula depends on many factors, including the state of resident and family assets (including home equity). We choose the $4,000 cutoff, as this generally reflects families with above median income and assets.

working is detrimental to academic performance. The fact that private lending decreases for families with greater EFCs suggests that the courses may encourage these students to identify alternative methods of payment not included in our analysis, (e.g., scholarships from parents' places of employment, informal networks for lending from extended family members, or home equity loans). [35] 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 subsitute for private loans.

The next panels of Table 4 report significant heterogeneity across race or ethnicity (white, black, or hispanic). The findings for minority populations are quite similar to patterns in low EFC students: black and hispanic students increase their subsidized Stafford loan amounts more than the average due to the graduation requirement. Black students also increase their unsubsidized Stafford loan amounts. This is regardless of the fact that black students have higher levels of subsidized and unsubsidized Stafford loans on average, $2,009 and $1,696, respectively. This is also consistent with the potential lack of information about these opportunities among disadvantaged students. In contrast to the results for low EFC students, black and hispanic students do not simultaneously decrease the likelihood of working while enrolled.

White students, in contrast, see responses similar to the relatively higher EFC students: white students decrease their private loans by roughly $230 on average. To the degree that white students have more access to private loan markets, this may contribute to their greater responsiveness along these margins.

These results shed light on the mechanisms behind the graduation requirements: financial education increases subsidized borrowing for groups most likely to be eligible to borrow 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, may not have applied in a timely manner, or may have made mistakes that limited their subsidized loan offers. At the same time, graduation requirements reduce more costly forms of borrowing for groups that are most likely to have access to the broader options for borrowing that include private loans and credit cards. These students may have access to more forms of informal and lower-cost borrowing that are used instead of higher cost private loans.

VI. Threats to Internal Validity

In this section, we address two threats to internal validity: (1) testing that the trends in the treatment and the control groups are parallel in the pre-treatment

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

period and (2) ensuring that the policy is not endogenous to other state education policies.

A. Testing for 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 and is commonly tested by examining the periods prior to the implementation of the policy. 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, it is challenging to provide a traditional test of parallel pre-trends.

Accordingly, we use five complementary strategies to validate the parallel trends assumption in these data. First, we verify that students graduating from high school in the years prior to the binding financial education requirement were not affected by the future mandate. This could have been the case if either financial education content was incorporated in a wide spread manner prior to the mandate, or if financing patterns in these states were already changing prior to the mandate. For each state, student requirements took effect between two NPSAS reporting years. Thus, the previous NPSAS waves can act as a control group: students completing high school prior to the year the graduation requirement took effect in a state with a requirement in the subsequent wave should not be affected by the policy. Put another way, those who were high school freshmen in Utah in Fall of 2007 were required to complete personal finance content before they graduated in Spring of 2011. Sophomores, juniors, and seniors in the Fall of 2007 would act as the "previous wave" for this group. Those who exited high school prior to 2007 would serve as two periods prior as they will be two waves earlier in the NPSAS sample.[36]

Table 5 reports results based on this classification. These results consider only observations without graduation requirements–that is, observations in either control states or graduates in waves prior to the state mandate. Across all specifications, we find one coefficient statistically different from zero at the ten percent level. With fourteen tests, this is likely to happen by chance at least once. We reiterate that this is identified from states implementing in 2011 and later.

Second, Table 6 uses data from earlier waves of the NPSAS (1989, 1992, and 1995) in addition to the later data (1999, 2003, 2007, 2011). This limits the outcome measures, as all of the variables were not collected throughout the full sample or were not collected consistently. This variant of Equation 1 includes all control states and three pre-treatment waves for nearly all states that implement, excluding two early implementers: Illinois and New Hampshire. Table 6 indicates

[36]One might also look at trends by the year before the requirement. However, this would require that different states identify each pre-trend (e.g., those with requirements just after a wave year would identify wave $t + 1$ and those further out would identify wave $t + 2$, etc.). We thus prefer our model specifications.

that only one coefficient is statistically different from zero at the ten percent level. As there are fifteen tests, at least one of these is likely to be statistically different at the ten percent level.

Third, we provide a placebo test in Appendix Table A.8. This analysis uses the older NPSAS sample from 1989-2003, again preserving the variables that have been consistent over time. The variable $PF_{\text{year } t+10}$ is equal to one if an individual was 18 in a state within the 10 years prior to the time the graduation requirement became binding in that state. Here, we estimate that the falsely placed policy has no effect on having any aid, having a Stafford loan, having grants or scholarships, or working while enrolled. This indicates that the states where these policies were passed did not have different uses of financial aid prior to these graduation requirements, further validating our results.

Fourth, Appendix Table A.9 uses the 1989-2011 data with the same variables and shows that results remain consistent if we include a state-specific linear time trend to our main specification. We do not include these state-specific trends in our main analysis, as there are too few waves in the 1999-2011 period with which to consistently identify a linear time trend.

Fifth, we use our supplemental data from the MUS and the CPS to confirm that there are 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 pre-trends in college enrollment, full-time college enrollment, and part-time college-enrollment in Table C.2.[37] In both cases, the evidence supports the parallel trends assumption.

B. Policy Endogeneity

A final threat to 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) that state changes other education policies or (2) when the states' economic condition warrants these graduation requirements. This might be a particular concern if the financial crises influenced states' decisions to require personal finance and the economic conditions simultanously changed student aid decisions. To explore this threat, 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.

First, do states that pass mandates have fundamentally different economic contexts at the time of passage? We formally test the correlation between state-level

[37] We show this with IPEDS data as well in Table C.4.

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 2.

$$PF_{s,t} = \alpha + \boldsymbol{\beta X_{s,t}} + \delta_s + \gamma_t + \epsilon_{s,t} \tag{2}$$

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 7 shows the results from estimating Equation 2. None of the variables 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 million residents in a given year increases the probability of having a rigorous graduation requirement by 9 percentage points. From 2010-2011, the average population growth within states was 46,509. That average increase would change the probability of adopting a rigorous personal finance requirement by a modest 0.4 percentage points. Appendix Table A.10 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 3.

Second, 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.[38] 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 (Appendix Table A.7). 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.[39]

Third, 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

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

[39] 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).

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).[40,41]

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[42] 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.[43] 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.[44]

We explore the sensitivity of the baseline results (reported in Table 3) to the inclusion of these policies. Table 8 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.

Finally, 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.[45] Table 8 shows that controlling 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 8 is restricted to public institutions as they receive the public funds. Thus, the relevant comparison table is Table 4. Overall, we find no evidence that the estimates are influenced by other state economic conditions, high school graduation requirements, or higher education policies.

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

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

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

[43]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).

[44]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.

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

VII. Effect of Financial Education on Institutional Choice and College Enrollment

The results so far suggest that personal finance graduation requirements generally increase both student applications for aid and the use of low cost borrowed funds (for students with less affluent family backgrounds), while simultaneously decreasing the use of higher cost private loans or credit cards (for students with more affluent family backgrounds). However, if these 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 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 chose a school with lower tuition, or less likely to attend college at all. In contrast, if students may find that borrowing is less intimidating then they previously thought, they may be more inclined to attend a private school than a public school. To the degree that these choices reduce the ideal match between students and schools, these policies may have unintended consequences that reduce lifetime income.

Using NPSAS data, we can address the likelihood of observing an enrolled student at different types of institutions. Table 9 reports results for the effect of personal finance graduation requirements and other demographic characteristics on institutional choices: whether or not a student enrolled at a private, public or for-profit four year institutions (conditional on enrollment at a four-year institution), the tuition and fees paid at the four year institution, the likelihood that the student stays 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 are small and imprecisely estimated, with none of the estimates approaching statistical significance at even the 10 percent level.

Finally, in Appendix C we examine college enrollment. 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.[46] Using a difference-in-difference approach, we find that there is no difference in college enrollment in states and years where personal finance graduation requirement were in place. We confirm these tightly estimated zeros with estimates of the fraction of students enrolling in four-year institutions from each permanent resident state in IPEDS (Table C.4). These results suggest that students' enrollment and institutional preferences are not affected by financial education.

[46] If we restrict the sample to 18 year olds, we obtain the same result.

VIII. Offering Financial Education Electives

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 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. 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 is idiosyncratic across schools. Table B.2 verifies that difference-in-difference assumption that there are parallel trends in our outcome variables based on the course offering. 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 10 reports the results, where offering financial education has virtually no effect on students. Across Columns (1)-(4), there is no statistical difference in the amount of subsidized Stafford loans, unsubsidized Stafford loans, likelihood of having a grant, and the amount of non-loan aid received. Columns (1) and (3) are precisely estimated zeros, while the 95% confidence intervals in Columns (2) and

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

(4) are larger with ranges of -71 to 31 and -41 to 101, respectively. We take this as evidence that personal finance education offerings do not change subsidized or unsubsidized federal loan amounts or grant payments.[48] This gives us confidence that the effects we estimate in the NPSAS are not a lower bound due to the presence of elective courses when mandates are absence.

IX. 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 the amount of federal student loan aid students receive. At the same time, these requirements decrease private loan amounts and decrease the likelihood of carrying a credit card balance. The costs of financial education requirements primarily stem from the opportunity cost of displacing other courses or content. These costs are likely to be low, as in many states schools incorporate personal finance concepts into already-existing courses, such as economics.[49] In assessing the benefits, we 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. 2014; 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.

These results are complementary to those in Brown et al. (2016*b*), but add significant nuance to their interpretation. Brown et al. (2016*b*) finds that personal finance coursework is associated with a modest and statistically insignificant average increase in total student debt (roughly $161 for 22 year-olds).[50] In our data, the uptick of federal loans combined with the reduction private loans is consistent with this result of a total effect that is statistically indistinguishable from zero. We further flesh out this average effect by showing that it obscures differential responses by demographic group. Our data indicate that the increases in public loans are from students with lower EFCs and racial minorities, while decreases in private loans stem from students with typically greater access to multiple forms

[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.

[49] 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.

[50] 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.

of credit, that is, from students with higher EFCs and white students.[51] 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.

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[51] Note that there are several other distinctions between our approach and that in Brown et al. (2016*b*). Brown et al. (2016*b*) study debt at age 22 when students are likely to exit school, while we study financial aid decisions upon entering college and before students are exposed in any college-sponsored financial counseling. In addition, we examine high school graduation requirements in financial education, where Brown et al. (2016*b*) study broader and less intensive financial literacy requirements.

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Tables and Figures

Figure 1. : Financial Education Requirements

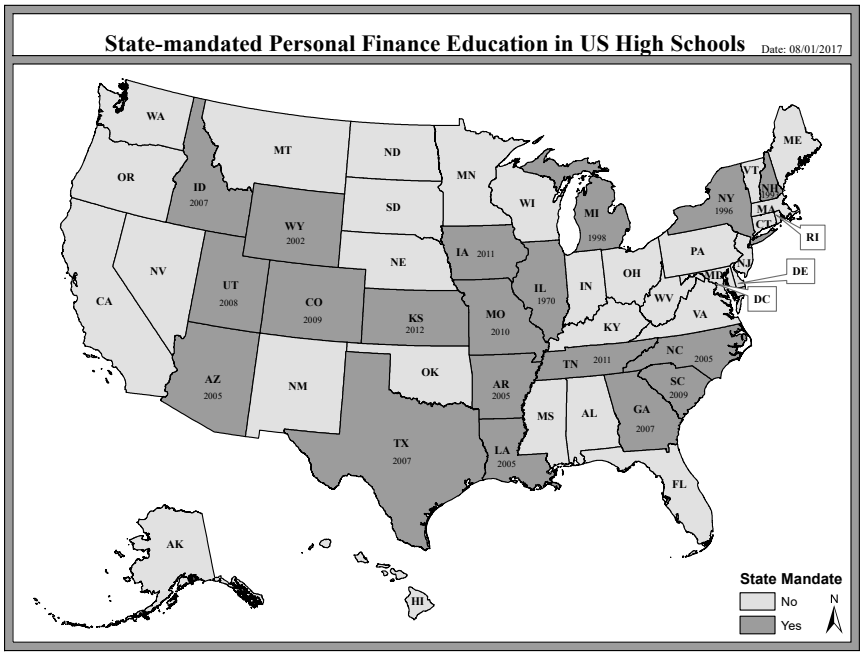

Table 1—: States with Personal Finance Graduation Requirements

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)
Subsidized Stafford $s	1,195	1,464	1,275
	(1,488)	(1,598)	(1,526)
Unsubsidized Stafford $s	912	1,232	1,007
	(1,601)	(1,839)	(1,681)
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)
Private Loan $s	782	852	803
	(3,034)	(3,133)	(3,064)
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—: Federal Financial Aid Decisions at Four Year Institutions

	(1) Applied for Aid	(2) Have Stafford	(3) Sub Stafford $s	(4) Unsub Stafford $s	(5) Have Grant
PF	0.033*	0.053*	106.25^{+}	0.024	0.031^{+}
	(0.013)	(0.022)	(61.95)	(76.92)	(0.017)
N	25,354	25,354	25,354	25,354	25,354
	(6) Private Loan	(7) Private Loan $s	(8) Have CC Balance	(9) Work while Enrolled	
PF	-0.003	-151.99*	-0.021*	-0.014	
	(0.007)	(65.83)	(0.008)	(0.014)	
N	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. Estimated control variables are in Table A.3.

Table 4—: Heterogenous Effects of Personal Finance Graduation Requirements by Race

	(1) Applied for Aid	(2) Sub Stafford $s	(3) Unsub Stafford $s	(4) Have Grant	(5) Private Loan $s	(6) Have CC Balance	(7) Work while Enrolled
Expected Family Contribution (EFC) Subgroups							
$> \$4,000$	0.039*	29.242	-41.603	0.028	**-272.740****	-0.021*	0.011
	(0.017)	(51.608)	(76.717)	(0.027)	(86.435)	(0.008)	(0.018)
N	16,307	16,307	16,307	16,307	16,307	16,307	16,307
Mean	0.884	924	1,053	0.640	958	0.079	0.439
$< \$4,000$	0.021*	**183.151**$^{+}$	77.248	0.032**	42.802	-0.019	**-0.046***
	(0.009)	(107.465)	(98.965)	(0.010)	(72.076)	(0.015)	(0.022)
N	9,400	9,400	9,400	9,400	9,400	9,400	9,400
Mean	0.968	1,878	922	0.935	529	0.125	0.479
Racial and Ethnic Subgroups							
White	0.034*	9.95	-121.06	0.031	**-230.70****	-0.022*	-0.014
	(0.015)	(63.75)	(76.55)	(0.026)	(68.35)	(0.010)	(0.023)
N	17,996	17,996	17,996	17,996	17,996	17,996	17,996
Mean	0.898	1,176	983	0.723	898	0.078	0.452
Black	-0.001	**261.21***	**452.14****	**-0.029**	-51.36	-0.010	0.008
	(0.008)	(127.30)	(161.98)	(0.023)	(123.05)	(0.029)	(0.032)
N	2,859	2,859	2,,859	2,859	2,859	2,859	2,859
Mean	0.986	2,009	1,696	0.875	712	0.130	0.388
Hispanic	0.040*	**301.55***	151.18	0.037	-162.78	0.031	0.038
	(0.015)	(127.38)	(130.55)	(0.024)	(269.15)	(0.022)	(0.040)
N	2,524	2,524	2,524	2,524	2,524	2,524	2,524
Mean	0.959	1,461	953	0.822	704	0.131	0.465

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.3, 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 3.

Table 5—: Testing the Pre-trends in Financial Education

	(1) Applied for Aid	(2) Sub Stafford $s	(3) Unsub Stafford $s	(4) Have Grant	(5) Private Loan $s	(6) Have CC Balance	(7) Work while Enrolled
$PF_{\text{wave } t+1}$	0.007	94.185	7.912	0.076^{+}	45.020	0.012	-0.014
	(0.017)	(74.955)	(97.362)	(0.044)	(188.144)	(0.022)	(0.026)
$PF_{\text{wave } t+2}$	-0.012	86.455	-102.685	-0.017	43.349	0.029	-0.003
	(0.016)	(58.394)	(68.318)	(0.035)	(138.451)	(0.020)	(0.020)
N	12,822	12,822	12,822	12,822	12,822	12,822	12,822

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.3. The model drops all states after their requirement is in effect. $PF = 1$ if the individual was 18 or younger in a state where personal finance was required in his high school for those 18 and younger. $\text{PF}_{\text{wave } t+1} = 1$ if the graduation requirement took place in an individual's state the subsequent wave after he appeared in the data. The excluded group is those in states who implemented two waves or more prior to the mandate. $\text{PF}_{\text{wave } t+2} = 1$ if the individual appeared in the data two waves before a graduation requirement was in place in his state (e.g., the graduation requirement came into place in two waves of data). The excluded group is those who implemented three waves or more prior to the requirement. The states identifying this variation are those implementing just before the 2011 wave or later: UT, VA (2008); CO, SC (2009); MO (2010); IA, NE, NJ, TN (2011); KS (2012); OR (2013) .

Table 6—: Testing the Pre-trends in Financial Education Using 1989-2011 Data

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Have Grant	(5) Work while Enrolled
$PF_{wave\ t+1}$	0.136	-0.143	-0.104	0.227	0.151
	(0.113)	(0.154)	(0.123)	(0.180)	(0.105)
$PF_{wave\ t+2}$	0.069	-0.046	-0.110	0.110	0.107
	(0.086)	(0.115)	(0.090)	(0.133)	(0.068)
$PF_{wave\ t+3}$	0.026	-0.035	-0.058^{+}	0.064	0.029
	(0.040)	(0.050)	(0.034)	(0.070)	(0.043)
N	6,113	6,462	6,462	6,462	5,309

Notes: Source: NPSAS Data (1989, 1992, 1995, 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.3. We drop all states after their requirement is in effect, and include each state that passes for only three years prior to the requirement. $PF_{wave\ t+1} = 1$ if the graduation requirement took place in an individual's state the subsequent wave after he appeared in the data. $PF_{wave\ t+2} = 1$ if the individual appeared in the data two waves before a graduation requirement was in place in his state (e.g., the graduation requirement came into place in two waves of data). $PF_{wave\ t+3} = 1$ if the individual appeared in the data three waves before a graduation requirement was in place in his state (e.g., the graduation requirement came into place in three waves of data). The excluded group is those who never implemented the requirement. This is identified off all all states that implement except for New Hampshire and Illinois in Table 1. These variables are different from our main specifications, since the data span from a longer time period (1989-2011). No Aid equals one if the individual does not have any financial aid and zero otherwise. Have Stafford equals one if the individual has a Subsidized or Unsubsidized Stafford loan and zero otherwise. Have Grant equals one if the individual has grants and/or scholarships in his aid package and zero otherwise.

Table 7—: State Characteristics and Personal Finance Requirements

	PF
Governor is Democrat	0.00125
	(0.035)
Unemployment rate	-0.02940
	(0.021)
Medicaid beneficiaries	-0.00006
	(0.000)
SSI recipients	-0.00123
	(0.002)
Gross State Product	0.00004
	(0.001)
Poverty Rate	-0.00078
	(0.007)
Population	0.09944
	(0.098)
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 8—: Results Robust to Controlling for Other Educational Policies

	(1) Applied for Aid	(2) Sub Stafford $s	(3) Unsub Stafford $s	(4) Have Grant	(5) Private Loan $s	(6) Have CC Balance	(7) Work while Enrolled
Controls for Total Credits Required for Graduation							
PF	0.028^{+}	96.125	-15.378	0.015	-52.097	-0.023*	-0.030^{+}
	(0.014)	(69.925)	(87.032)	(0.017)	(68.425)	(0.009)	(0.017)
N	20,018	20,018	20,018	20,018	20,018	20,018	20,018
Controls for Total Math Credits							
PF	0.030^{+}	94.128	1.161	0.018	-45.086	-0.019*	-0.024
	(0.015)	(69.120)	(88.814)	(0.016)	(70.680)	(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	0.032*	99.237	-38.609	0.021	-110.516	-0.019*	-0.025
	(0.013)	(59.749)	(77.923)	(0.016)	(70.224)	(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*	101.509	1.412	0.029^{+}	-148.824*	-0.023**	-0.014
	(0.013)	(63.489)	(78.590)	(0.017)	(69.157)	(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*	108.147^{+}	-3.965	0.030^{+}	-154.067*	-0.021*	-0.014
	(0.013)	(61.058)	(74.425)	(0.018)	(65.663)	(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*	180.013**	56.936	0.029	-61.193	-0.046**	-0.054***
	(0.016)	(64.703)	(88.026)	(0.028)	(64.783)	(0.014)	(0.014)
N	14,714	14,714	14,714	14,714	14,714	14,714	14,714

Notes: 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.3. 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.

Table 9—: Personal Finance Graduation Requirements and Choice of Institution

	(1) Private	(2) Tuition & Fees	(3) In State	(4) Four yr
PF	-0.002	-680.349	-0.020	-0.010
	(0.042)	(669.963)	(0.016)	(0.051)
N	25,354	22,437	25,354	44,729

Notes: 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.3. Columns 1 through 3 include only four-year students; Column 4 includes students at two- and four-year institutions.

Table 10—: Offering Personal Finance and Financial Aid in Montana

	(1) Sub Stafford $s	(2) Unsub Stafford $s	(3) Have Grant	(4) Non-loan Aid $s
PF Offered	-0.469	-20.067	-0.001	29.843
	(15.561)	(25.851)	(0.012)	(35.836)
N	21,385	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. 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. Subsidized and Unsubsidized Stafford amounts are in dollars and include zeros. 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.

Appendix A: Robustness Checks in NPSAS Data

Table A.1—: Federal Financial Aid Decisions at All Institutions

	(1) Applied for Aid	(2) Have Stafford	(3) Sub Stafford $s	(4) Unsub Stafford $s	(5) Have Grant
PF	0.008	0.037*	73.195	-44.023	0.006
	(0.014)	(0.018)	(51.595)	(41.441)	(0.018)
N	52,489	52,489	52,489	52,489	52,489
	(6) Private Loan	(7) Private Loan $s	(8) Have CC Balance	(9) Work while Enrolled	
PF	-0.007	-22.887	-0.012*	0.012	
	(0.006)	(41.773)	(0.005)	(0.014)	
N	52,489	52,489	52,489	52,489	

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.3.

Table A.2—: Robustness check: Loan Amount Results Using a Tobit

	(1) Sub Stafford $s	(2) Unsub Stafford $s	(3) Private Loan $s
PF	360.878*	33.821	-410.874
	(142.055)	(174.995)	(666.662)
N	25,354	25,354	25,354

Notes: 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.3.

Table A.3—: Federal Financial Aid Decisions at Four Year Institutions, Including Control Variables

	(1) Applied for Aid	(2) Have Stafford	(3) Sub Stafford $s	(4) Unsub Stafford $s	(5) Have Grant	(6) Private Loan	(7) Private Loan $s	(8) Have CC Balance	(9) Work while Enrolled
PF	0.033*	0.053*	106.25+	0.024	0.031+	-0.003	-151.99*	-0.021*	-0.014
	(0.013)	(0.022)	(61.95)	(76.92)	(0.017)	(0.007)	(65.83)	(0.008)	(0.014)
Male	-0.018***	-0.020**	-58.97**	-57.80**	-0.026***	0.008+	65.60*	-0.016***	-0.043***
	(0.004)	(0.007)	(20.97)	(20.77)	(0.005)	(0.004)	(28.59)	(0.004)	(0.008)
White	-0.029**	0.032	2.35	104.84+	-0.032**	0.016	116.64	-0.019**	0.045***
	(0.011)	(0.027)	(66.06)	(53.79)	(0.009)	(0.011)	(89.25)	(0.006)	(0.012)
Black	0.042***	0.180***	487.98***	781.99***	0.053***	0.013	20.73	0.029**	-0.039+
	(0.008)	(0.020)	(59.64)	(63.85)	(0.011)	(0.008)	(89.21)	(0.010)	(0.019)
Hispanic	0.028*	0.051*	45.13	88.24+	0.041*	0.008	92.146	0.014	0.047***
	(0.011)	(0.021)	(62.02)	(48.91)	(0.016)	(0.007)	(108.57)	(0.012)	(0.013)
Age 17	-0.013	-0.073*	-130.55	-300.89**	0.007	-0.005	54.85	-0.010	-0.000
	(0.016)	(0.032)	(105.21)	(103.41)	(0.025)	(0.016)	(192.11)	(0.022)	(0.031)
Age 19	-0.024***	-0.014**	-34.98+	-68.37**	-0.030***	0.003	47.24	0.025***	0.034***
	(0.004)	(0.005)	(18.24)	(20.33)	(0.006)	(0.004)	(34.66)	(0.004)	(0.007)
Dependent	0.075***	0.172***	413.22***	-298.84**	0.039*	0.040***	311.48**	0.043***	-0.088***
	(0.015)	(0.019)	(64.40)	(94.72)	(0.016)	(0.011)	(93.82)	(0.011)	(0.024)
EFC (000s)	-0.001***	-0.005***	-30.36***	7.24***	-0.005***	-0.001***	-1.18	-0.000***	-0.001***
	(0.000)	(0.000)	(1.24)	(0.73)	(0.000)	(0.000)	(1.40)	(0.000)	(0.000)
Private	0.052***	0.137***	497.00***	149.91**	0.231***	0.068***	707.03***	-0.013**	-0.091***
	(0.007)	(0.016)	(42.37)	(48.61)	(0.013)	(0.006)	(62.25)	(0.004)	(0.012)
Parent<HS	0.038***	0.005	83.72	-75.92	0.106***	-0.016	-222.44***	0.039***	0.070*
	(0.010)	(0.018)	(50.99)	(76.84)	(0.026)	(0.010)	(55.70)	(0.011)	(0.031)
Parent HS	0.058***	0.093***	252.36***	127.77**	0.070***	0.020***	167.07*	0.036***	0.074***
	(0.004)	(0.010)	(24.23)	(36.77)	(0.008)	(0.006)	(64.93)	(0.004)	(0.009)
Parent So	0.041***	0.100***	261.48***	166.83***	0.053***	0.041***	265.02***	0.025***	0.054***
College	(0.004)	(0.008)	(22.55)	(22.04)	(0.007)	(0.006)	(67.29)	(0.005)	(0.007)
N	25,354	25,354	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 are: Other Race, Age 18, Parent College Educated or beyond, Public colleges.

Table A.4—: Results with No Controls

	(1) Applied for Aid	(2) Sub Stafford $s	(3) Unsub Stafford $s	(4) Have Grant	(5) Private Loan $s	(6) Have CC Balance	(7) Work while Enrolled
PF	0.037*	133.803^{+}	22.006	0.037	-155.241*	-0.021*	-0.018
	(0.016)	(73.989)	(85.364)	(0.024)	(69.287)	(0.008)	(0.017)
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 no control variables.

Table A.5—: Robustness check: Full-time and Dependent Students

	(1) Applied for Aid	(2) Sub Stafford $s	(3) Unsub Stafford $s	(4) Have Grant	(5) Private Loan $s	(6) Have CC Balance	(7) Work while Enrolled
Full-time Students Only							
PF	0.029*	81.877	22.198	0.021	-189.05*	-0.020*	-0.010
	(0.013)	(59.538)	(80.622)	(0.019)	(72.47)	(0.010)	(0.017)
N	23,419	23,419	23,419	23,419	23,419	23,419	23,419
Dependent Students Only							
PF	0.031*	94.17	23.33	0.029	-195.63**	-0.025**	-0.009
	(0.013)	(60.58)	(76.55)	(0.017)	(69.56)	(0.009)	(0.014)
N	24,664	24,664	24,664	24,664	24,664	24,664	24,664

Notes: 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.3.

Table A.6—: 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	EFC ($000s)	Parent $< HS$	Parent HS Grad	Parent So Coll
PF	0.003	-0.332	-0.004	0.001	0.000
	(0.007)	(0.728)	(0.006)	(0.014)	(0.009)
N	25,354	25,354	25,354	25,354	25,354

Notes: 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.7—: Robustness check: Alternative policy specifications

	(1) Applied for Aid	(2) Sub Stafford $s	(3) Unsub Stafford $s	(4) Have Grant	(5) Private Loan $s	(6) Have CC Balance	(7) Work while Enrolled
No early states–Drop states implementing pre-1996							
PF	0.028*	62.56	15.50	0.026	-179.99*	-0.026**	-0.014
	(0.013)	(65.35)	(84.64)	(0.016)	(73.00)	(0.008)	(0.014)
N	21,063	21,063	21,063	21,063	21,063	21,063	21,063
No locally determined policies							
PF	0.035*	117.19^{+}	32.57	0.035^{+}	-175.14*	-0.019*	-0.021
	(0.014)	(63.62)	(84.78)	(0.019)	(71.42)	(0.008)	(0.014)
N	22,942	22,942	22,942	22,942	22,942	22,942	22,942

Notes: 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.3.

Table A.8—: Placebo Financial Education did not Affect Outcomes

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Have Grant	(5) Work while Enrolled
$PF_{\text{year } t+10}$	-0.000	0.004	-0.005	0.001	-0.019
	(0.016)	(0.020)	(0.028)	(0.024)	(0.029)
N	7,843	8,655	8,655	8,655	6,207

Notes: 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.3. $PF_{\text{year } t+10} = 1$ if the individual was 18 in a state where personal finance was required in his high school within the 10 years before the graduation requirement was binding. This variable essentially just falsely moves the policy back ten yers. The sample includes data from 1989-2003.

Table A.9—: State Linear Time Trends in Financial Education, 1989-2011

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Have Grant	(5) Work while Enrolled
PF	0.028^{+}	-0.033	0.057*	-0.012	-0.066**
	(0.017)	(0.020)	(0.025)	(0.036)	(0.019)
N	35,976	38,268	38,268	38,268	33,224

Notes: Source: NPSAS Data (1989, 1992, 1995, 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.3, as well as state linear time trends. These variables are different from our main specifications, since the data span from a longer time period (1989-2011). No Aid equals one if the individual does not have any financial aid and zero otherwise. Have Stafford equals one if the individual has a Subsidized or Unsubsidized Stafford loan and zero otherwise. Only Loans equals one if the individual only has loans in his aid package (no grants or scholarships) and zero otherwise.

Table A.10—: Robustness check: Controlling for State-level Unemployment

	(1) Applied for Aid	(2) Sub Stafford $s	(3) Unsub Stafford $s	(4) Have Grant	(5) Private Loan $s	(6) Have CC Balance	(7) Work while Enrolled
PF	0.033*	107.609^{+}	-7.358	0.031^{+}	-147.663*	-0.021*	-0.016
	(0.013)	(60.193)	(86.760)	(0.017)	(66.086)	(0.008)	(0.013)
N	25,354	25,354	25,354	25,354	25,354	25,354	25,354

Notes: 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.3.

Appendix B: MUS Data

Figure 2. : Financial Education Course Offerings

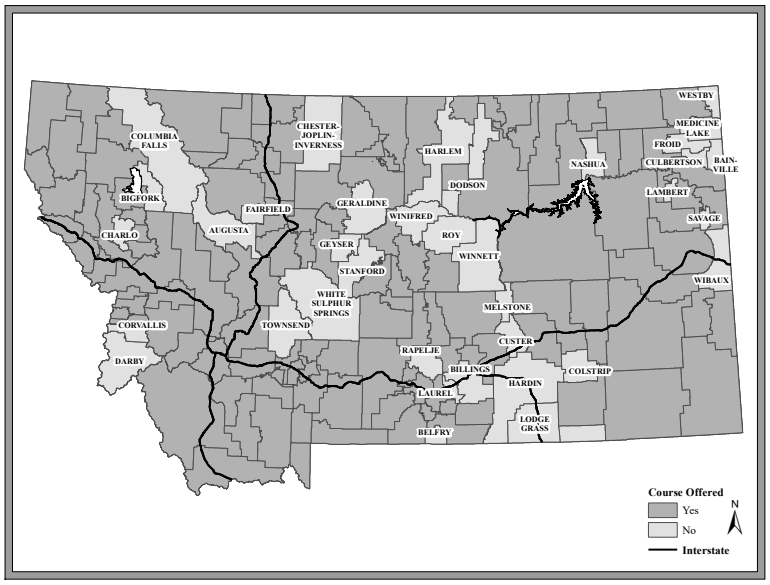

Table B.1—: Summary Statistics by Financial Education Offering Status

	No PF	PF Offered	Both
Dependent Variables			
Get Stafford	0.498	0.482	0.486
	(0.500)	(0.500)	(0.500)
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)
Non Loan Aid	985.2	984.9	985.0
	(1583.5)	(1602.4)	(1597.8)
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)

Table B.2—: Pre-trends in MUS Data

	(1) Sub Stafford $s	(2) Unsub Stafford $s	(3) Have Grant	(4) Non-loan Aid $s
PF Offered	-10.82	-14.62	0.003	16.48
	(15.90)	(30.69)	(0.012)	(38.87)
PF Offered −1	-26.62	62.95	0.004	-61.04
	(24.15)	(43.64)	(0.015)	(66.76)
PF Offered −2	-40.67	-8.81	0.005	0.55
	(25.97)	(44.84)	(0.018)	(59.82)
PF Offered −3	11.55	-21.08	0.016	-40.02
	(24.38)	(36.13)	(0.018)	(50.64)
PF Offered −4	-21.52	5.07	0.011	6.32
	(25.08)	(22.20)	(0.017)	(43.92)
PF Offered −5	-34.91	14.8	-0.012	-25.61
	(31.20)	(33.10)	(0.019)	(57.63)
PF Offered −6	30.44	-13.87	-0.004	-54.87
	(23.38)	(36.57)	(0.017)	(57.33)
PF Offered −7	0.083	-19.11	0.044*	79.17
	(22.71)	(23.01)	(0.020)	(88.24)
N	21,385	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. 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. Subsidized and Unsubsidized Stafford amounts are in dollars and include zeros. Only loans= 1 if the given student had a loan and no other form of non-loan 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. 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

In this section, we seek to understand how personal finance graduation requirements affect college attendance using data from the Current Population Survey (CPS). 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.[52] There are no significant differences across the two sets of states, and the average college attendance rate is roughly 54%, with 48% attending postsecondary education full time and only 6% going to school part-time. There are no notable differences across the two samples in terms of demographic characteristics of individuals within those states either.

$$Y_{i,s,t} = \alpha_0 + \alpha_1 \text{PF Required}_{i,s,t} + \boldsymbol{\beta X_i} + \delta_s + \gamma_t + \zeta_m + + \epsilon_{i,s,t} \tag{3}$$

Next, we estimate the effect of personal finance education on college attendance using Equation 3. Our dependent variable, $Y_{i,s,t}$, equals one if individual i in state s at time t attends college and zero otherwise. Our independent variable of interest, PF Required$_{i,s,t}$, 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 C.3 reports the results from Equation 3. Our baseline specification shows that personal finance graduation requirements do not change college attendance rates, where these effects are precisely estimated zeros. We then replicate our policy heterogeneity from Table **??** and find only one coefficient statistically different from zero at the 10 percent level, which we expect to see by chance one in every ten times.[53] In all specifications, the results are nearly zero in magnitude. Finally, we replicate the heterogeneity exercises by gender and race and still find no effect of personal finance education on college attendance for these groups. Thus, we think we have tightly estimated a null effect of financial education on college attendance.

Table C.2 confirms that the parallel trends assumption required for the difference-in-difference estimation strategy is 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

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

[53] 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.

year before the requirement. The coefficients on PF Requirement −1 through PF Requirement −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 and divide this by the number of 18 year olds in the state in that year. While we 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, as changes in four-year enrollment could be due to either shifts toward two-year enrollment or lack of attendance. Table C.4 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 both measures, we show that there is no pre-trend in financial education (Columns (3)-(4)).

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)

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: 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: CPS

	(1) College At All	(2) College Full Time	(3) College Part Time
Baseline			
PF	-0.007	-0.006	-0.001
	(0.007)	(0.007)	(0.002)
N	510,933	510,933	510,933
Male			
PF	-0.002	-0.000	-0.002
	(0.008)	(0.009)	(0.005)
N	244,833	244,833	244,833
Female			
PF	-0.011	-0.011	-0.000
	(0.010)	(0.010)	(0.004)
N	266,100	266,100	266,100
White			
PF	-0.012	-0.010	-0.002
	(0.008)	(0.009)	(0.003)
N	421,865	421,865	421,865
Black			
PF	0.016	0.012	0.004
	(0.017)	(0.016)	(0.004)
N	56,204	56,204	56,204
Hispanic			
PF	0.009	0.007	0.002
	(0.014)	(0.013)	(0.005)
N	50,723	50,723	50,723

Notes: 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.

Table C.4—: 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: 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.