The Effects of Financial Education on Student Financial Aid Choices

By Christiana Stoddard and Carly Urban*

Young adults entering college have little experience with acquiring debt and typically have low levels of financial literacy. However, they face complicated decisions on how to finance postsecondary education. This paper examines a policy lever that can potentially improve young adults financial knowledge around financial aid decisions: required personal finance courses in high school. While these courses teach basic principles of shopping for interest rates and calculating monthly repayments of loans, some states have directly incorporated student loans and the Federal Application for Student Aid in their standards. We use a difference-in-difference strategy that compares students living in states that did and did not require financial education prior to graduation before and after the requirement was binding and data from the National Post Secondary Aid Study. The results suggest that state-mandated personal finance education increases aid applications, federal loans, and scholarship attainment, while decreasing reliance on private loan dollars and credit card balances for college freshmen at four-year institution. However, financial education does not change individuals' institutional choices or decisions to attend college.
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I. Introduction

The last several decades have witnessed a high and rising rate of return to a college degree (Goldin and Katz 2009; Oreopoulos and Petronijevic 2013). Within this context, many students borrow to finance postsecondary education. In fact, the federal government provides both grants and loans with preferential terms in order to help students achieve these benefits. However, identifying the optimal way to finance higher education is fraught with many pitfalls for students as well as the government. There is a sizeable body of literature to suggest that many students

* Stoddard: Associate Professor of Economics, Montana State University, 306 Linfield Hall PO Box 172920 Bozeman, MT 59717, cstoddard@montana.edu. Urban: Assistant Professor of Economics, Montana State University, 208A Linfield Hall PO Box 172920 Bozeman, MT 59717, carly.urban@montana.edu. The authors acknowledge support from the Montana Office of Public Instruction's subcontract from the Statewide Longitudinal Data System Grant. Rachel Baker, Ben Cowan, Michael Collins, Chris Taber, and seminar participants at the Association of Public Policy and Management Annual Conference, the Household Finance Seminar Series at the Univeristy of Wisconsin-Madison, and the Western Economic Association all proovided valuable comments on this paper. Christian Cox, Neil Silveus, and Madison Traucht provided excellent research assistance. This paper is was deemed IRB Exempt CU051116-EX.

are still credit constrained and under-invest in their schooling (Avery and Turner 2012; Cowan 2014; Lochner and Monge-Naranjo 2015, 2011), but there is also significant policy concern around the rising level of student loan debt. Student loans now account for over $1.28 trillion in debt in the United States, surpassing credit card debt as the second largest source of debt after mortgages (Federal Reserve Bank of New York 2016).

One key concern for both over- and under-borrowing is that young adults typically lack experience in the financial market beyond a checking or savings account. They also lack basic financial literacy, and poor decisions about how to finance higher education may stem from this context of minimal information. Lusardi, Mitchell and Curto (2010) show that young adults have low financial literacy–only 27% of 23-28 year olds possess knowledge of basic financial concepts such as inflation, interest, and risk diversification. This limited information inhibits students who must compare private and public loans, decide whether to use credit cards for borrowing, assess the value of their time searching searching for scholarships and grants, and bear administrative costs associated with applying for federal and non-federal aid. Some states attempt to mitigate these information problems by requiring high school students to complete a course in personal finance before graduation. This paper is the first to causally determine the effect of financial education in high school on financial aid decisions in college.

The fact that student loan decisions are complex is not a novel concept (Dynarski and Scott-Clayton 2006). Students appear to have difficulty both with access to credit and with choosing optimal packages. Kantrowitz (2009) estimates that nearly 2.9 million undergraduate students would have qualified for Federal Pell Grants in 2007-2008 but did not apply. Part of this may be due to administrative burdens in knowing whether, when, and how to apply for aid. In a seminal study, Bettinger et al. (2012) show that randomly assigning personalized help in filling out the Federal Application for Student Aid (FAFSA) increases the likelihood of completion, increases the amount of federal dollars received, and increases the probability of enrollment for some groups.[1] Furthermore, even among students who apply for aid, the lack of financial literacy may lead to suboptimal student loan decisions. In a review of the National Financial Capability Study Lusardi (2016) shows that 54% of student loan borrowers did not calculate their future monthly payments prior to choosing a loan. Further, when asked if student loan borrowers could do the process over again, 53% said they would make a change, with only 29% reporting they would make the same borrowing choices.[2] Borrowers with private loans, which tend to have higher interest rates than public loans, were 11 percentage points more likely to say they would make a change than those with federal loans.[3]

[1] Castleman and Page (2016) further finds that students nudged to continue completing the FAFSA after the first year of college were more likely to persist.

[2] 17% noted that they were unsure about what they would do.

[3] Another strand of literature suggests that students are aware of self-control problems and could cause them to decline aid (Cadena and Keys 2013; Johnson 2013).

Providing students with better information about loans and with improved architecture for making borrowing choices is an ongoing area of policy interest (Marx and Turner 2016; Castleman and Page 2016; Castleman and Long 2016). As states increasingly consider the capabilities students need for graduation, one policy option is to mandate financial education at the high school level. Financial education in high school may help students learn how to access credit and how to make optimal borrowing decisions. Currently, 25 states require students to complete personal finance material prior to graduation, including material on interest, saving, credit, debt, and income, with emphasis on making sound individual-specific financial decisions. Some state personal finance standards include financing postsecondary education as an explicit component. For example, in Texas, the State Board of Education requires that all students "understand the various methods available to pay for college and other postsecondary education and training." This includes understanding how to complete the FAFSA, researching and evaluating scholarship opportunities, comparing grant options, comparing private and federal student loans, evaluating work-study options, and investigating any non-traditional methods of financing college or training.

These courses may impact access to credit (particularly in states where completing the FASFA is part of the curriculum) and the choices of loan packages. To the extent that borrowers learn to shop for interest rates, particularly in the context of financing postsecondary education, this statewide policy could allow students to seek out public student loans at higher rates, replacing higher interest private loans or credit card borrowing. It could also result in additional scholarship applications. However, it is unclear whether financial education would shape student loan amounts, as some students may be under-investing and others may be over-spending in making college enrollment and financing decisions.

Previous literature has examined the impact of financial education on credit outcomes (Cole, Paulson and Shastry 2013; Brown et al. 2016; Urban et al. 2014), but this paper is the first to estimate the causal effect on the formative financial decisions made when students first enroll in higher education. The analysis draws on a panel of cross-state comparisons, using a difference-in-difference approach to compare students from states before and after implementing a personal finance graduation requirement to comparable students whose states lack such a mandate. We use the National Post Secondary Aid Study (NPSAS) 1999 through 2011 waves to draw on a rich set of outcomes, including whether students applied for financial aid, their financial aid packages (including both federal and private loans), credit card debt, and work behavior.

The change in financial aid we document in the NPSAS data requires that students attend a post secondary institution. With these data, we are able to rule out that these graduation requirements affect the choice of institution conditional on enrollment. We then supplement our main analysis with data from the Current Population Survey (CPS) to show that personal finance graduation requirements do not appear to change college enrollment decisions.

To some degree, the estimates may understate the effects of financial aid as the control states include schools that voluntarily offer financial education courses as well. To explore this possibility, the final section uses administrative data from Montana to understand how personal finance education offerings affect students in a state without a mandate. Specifically, we document which high schools offer a course covering personal finance materials and the year in which the course began. We pair this with administrative student loan data from the Montana University System to show how access to these courses affects initial financial aid packages. This helps us to bound the causal effects of financial education mandates on aid packages in our NPSAS sample.

Our results suggest that personal finance graduation requirements are effective in increasing students' use of federal aid: students are 3 percent more likely to apply for aid and 5.5 percent more likely to take out a direct federal Stafford loan. These requirements also induce students to accept packages that are more likely to contain aid other than loans (including scholarships, grants, and other forms of aid). Furthermore, both private loan amounts and credit card debt are lower in these states. We find these effects are strongest in states with rigorous mandates, and that the effect on federal aid is strongest for more disadvantaged populations, while the reductions in private sector borrowing is reduced for relatively more advantages populations. We find no evidence that the choice of institution is affected by personal finance graduation requirements.

II. Background

A. State-Mandated Financial Education

In the last decade, states have increased requirements for personal finance coursework in high school with aims to reduce short-term and long-term financial distress among young adults. These courses center around principles in financial literacy, where the standard course broadly covers interest rates, saving, investing, and borrowing. Within these broad topics, each state customizes its standards to fit the demographic composition in the state. Specific topics in graduation standards cover many of the following: 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.

In Table 1, we list 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 is a relatively conservative measure, as some states will require a full standalone cousre in personal finance, while others require that the material be taught within another subject, such as social studies, math, or economics. We discuss this heterogeneity in more detail in Section IV.C. In all cases, states have

outlined personal finance requirements in graduation standards documentation.[4]

An ongoing literature studies the effect of financial education state mandates on knowledge (Tennyson and Nguyen 2001; Walstad, Rebeck and MacDonald 2010), savings (Bernheim, Garrett and Maki 2001; Cole, Paulson and Shastry 2013), and debt (Cole, Paulson and Shastry 2013; Brown et al. 2016; Urban et al. 2014). Tennyson and Nguyen (2001); Walstad, Rebeck and MacDonald (2010) find that well-implemented courses improve knowledge. The findings are mixed regarding savings and debt: Bernheim, Garrett and Maki (2001) finds that early-life financial education improves assets in middle-age, and Cole, Paulson and Shastry (2013) finds that these same mandates do not affect savings for the same time frame. These studies, however, focus on early mandates, where financial education was more focused around consumer education.[5] The findings are also mixed in terms of credit outcomes. Cole, Paulson and Shastry (2013) find that financial education has no effect on debt, though Brown et al. (2016) find that for young adults (under 30), financial literacy exposure reduces debt and increases credit scores. Urban et al. (2014) point out the importance of studying course requirements as opposed to financial literacy mandates to define students that were and were not exposed to financial education. This removes states that passed a mandate that never became a graduation requirement, as well as states that left it up to localities to determine implementation. Urban et al. (2014) find that personal finance graduation requirements increased credit scores and decreased severe delinquencies for 18-21 year olds.

The most closely related paper is aimed at determining the effect of financial literacy reforms on student loan amounts, among many other debt-related outcomes (Brown et al. 2016). They find that financial education reform increases student loan balances and the probability of ever taking out a student loan for 19-29 year olds. Our paper differs from theirs in several key ways: we focus on a broader protfolio of financial aid decisions, we link these to specific graduation requirements rather than more general financial education reforms, and our data contain individual level demographic characteristics that are not observable in credit report data. These differences allow us new insight into the mechanisms through with individuals are affected by financial education.

B. Potential Mechanisms

There are several mechanisms through which personal finance education may affect student aid choices. These include (1) a better understanding of interests rates, risk, and investment, which may change the types of aid sought (2) budgeting skills, which may affect loan amounts and the amount of other aid obtained prior to enrollment, and (3) clearer information about whether, how, and when

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

[5]See Urban and Schmeiser (2015) for more on the early mandates. The authors only document four states with these types of course mandates.

to fill out a FASFA to apply for aid.

First, the existing literature shows that that young adults lack basic knowledge on interest rates, inflation, and risk (Lusardi, Mitchell and Curto 2010). Mortgages are also covered in many states' personal finance curricula. While not exactly the same, mortgage originations draw many parallels with student loan originations. This could help students to better understand interest rates, learn to finance and pay back long-term debt, and understand how missed payments affect credit scores. Learning about mortgages could make the investment component of attaining human capital more salient. As these skills are paramount in determining the proper student loan package, learning about interest rates may allow students to properly calibrate their expected future debt burden. Students with financial education may be better equipped to choose loans with the lowest interest rates and apply for more non-loan aid that is not required to be paid back.

Second, creating a budget is a skill taught in most personal finance curricula that could influence a student's initial student loan choice. On average, the default bias may suggest that students will take out the maximum offered, and a status quo bias suggests they are likely to continue to take out the same amount in future periods. However, if students create a specific budget, they will be more likely to calculate an amount to borrow that differs from the default offering. This could either 1) seeking out additional forms of private loans, non-loan aid, or deciding to work while in school if the budget suggests expenses will exceed federally offered amounts or 2) result in taking out lower federal loan amounts than are offered based the subsidized and unsubsidized limits provided by the federal government. These calculations may also result in less use of credit cards as a stop-gap measure to meed unanticipated expenses.

Third,the literature suggests multiple errors in filling out the FASFA. As noted, many students who are eligible for aid do not apply (Kantrowitz (2009)), and furthermore, many students falsely believe they are inelegible for federal aid (McKinney and Novak 2015). Clearer information about the FASFA would then likely increase application rates. In addition, students in states requiring personal finance prior to graduation may be better equipped to overcomethe administrative burden in completing it, consistent with Bettinger et al. (2012).[6] Finally, personal finance courses may help students complete the FASFA in a timely manner: the Department of Education reports that one of the most common mistakes with the FAFSA is filling it out on time (Callahan 2016). Further, some financial aid is awarded on a first-come, first-served basis, meaning that if the FAFSA is not completed substantially ahead of the deadline, states and colleges can run out of money. McKinney and Novak (2015) show that, indeed, students who file after a priority deadline receive lower amount sof state and institutional aid than those

[6]We show that there is no response on the college attendance margin, which is different from Bettinger et al. (2012). This could be because Bettinger et al. (2012) sent personalized aid estimates and local tuition amounts as an arm of the experiment as well.

who file on time.

A number of states have explicit methods for addressing these errors by directly include student loan and financing postsecondary education content 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, and are even encouraged to seek guidance from school counselors regarding its completion. Tennesse'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 direct application of each of our tested hypotheses. In Texas, personal financial literacy education requires that students understand how to complete the FAFSA; research and evaluate scholarship opportunities; compare student grant options; analyze student loan options; evaluate work-study options; investigate nontraditional methods of paying for postsecondary education. We attempt to study each of these outcomes direclty in our analysis.

III. Data and Empirical Strategy

The National Postsecondary Student Aid Study is a nationally representative study of students enrolled in institutions of higher education. The NPSAS contains detailed data on financial aid extracted from institutional data, along with student and parent interview responses about demographics, high school degree, family background, credit card balances, and work. We use data from the 1999, 2003, 2007, and 2011 waves of this survey.

The NPSAS reports a student's legal state of residence, but not the state where the student attended high school. Many students relocate for higher education, and in some cases establish residency in another state in order to obtain tuition benefits associated with in-state status at a public institution. To reduce the mismatch of the graduation requirements to students' high school state, we restrict the sample to students in their first year of higher education who graduated in the same calendar year or one year prior to enrollment. We further restrict the sample to students between the ages of 18 and 21 and those who were born in the United States, as prior studies show that 93 percent of individuals stay in the same state from ages 18 to 22 (Brown et al. 2016). We also drop any students who did not complete a traditional high school degree as the would not be exposed to the personal finance curriculum (eliminating students with GEDs, who were homeschooled, who did not have a high school certificate, or who graduated from a foreign high school). This leaves us with a sample of 44,729 students, with 2,696 in 1999, 13,652 in 2003, 11,259 in 2007, and 17,122 in 2011. In the specifications, we mainly focus on students enrolled at four-year institutions, as

tuition and financial aid packages tend to be much larger and more consistent across institutions at this level.[7]

The results are from difference-in-difference specifications that include state and year fixed effects. Standard errors are clustered at the state level, as the high school graduation requirement policies under consideration are state policies. Note that this initial specification classifies those states with mandates but no graduation requirement as not having a personal finance requirement. This conservative approach will bias us against finding an effect if there are individuals within these states who took a course and responded by changing their aid packages. We explore this explicitly later and find that the estimates are larger in states with rigorously implemented personal finance mandates, requiring high school students to complete personal finance material prior to graduation, than in states with looser mandates without a graduation requirement. Appendix A Table A.1 provides details about the state policies. These will be discussed further in the robustness exercises.

Our initial results estimate Equation 1.

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

We estimate this for a suite of dependent variables $Y_{i,s,t}$ that capture characteristics of the full financial aid package for individual i with permanent residency in state s in year t. These include whether a student applied for financial aid, accepted any financial aid, the specific components of the federal Stafford loans obtained, non-loan aid accepted, private loans accepted, 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 and year t graduated from high school in a year after the state that passed 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 roughly 95 percent of the sample. Our specifications also include either the expected family contribution (EFC) or family income (parental income for individuals who are dependents or own/spouse income for individuals who are independent). The EFC is based on the financial information provided on the Free Application for Federal Student Aid (FAFSA), and it is calculated according to a standard formula that does not vary based on tuition and fees. The EFC includes measures related to income,

[7]Appendix B Table B.1 shows that we see no effect of personal finance education on student loans for the sample of two-year universities.

assets, and the enrollment in higher education of other family members. The EFC serves two purposes in our regressions. First, it can be thought of as a control variable that captures both family income and wealth. Higher income or wealthier students may have more information about finances and how to make appropriate financial decisions; they may also have different attitudes towards risk or different time preferences than lower income or less wealthy students. Second, the EFC determines student eligibility for financial aid, as a financial aid package are related to the difference between a student's Cost of Attendance (COA) and the EFC. The EFC determines eligibility for need-based federal aid, including both Pell grants and subsidized loans, as well as determining state and institutional aid offers in many instances.

Other factors that determine the aid package offered are tuition and whether the school is public or private institution.[8] However, these variables may be endogenous: the choice of what college to attend may itself be related to a student's level of financial literacy. The baseline results report specifications excluding tuition but including the type of institution, although specifications including tuition are reported in Appendix B Table B.4. We also explicitly examine whether the choice of institution is directly affected by personal finance mandates in the NPSAS data (Table 11) and whether enrollment is affected by mandates using data from the Current Population Survey (Appendix D).

Table 2 shows the characteristics of the sample by personal finance requirement. Across the states, nearly 88 percent of students apply for some type of aid, though this is slightly larger for those in states in years when the personal finance requirement was in place. Between 10 and 15 percent of students report having no financial aid at all, which is lower for students graduating from high school in states requiring personal finance. Roughly 60 percent of students have a Stafford loan, which is substantially higher than the 12 percent of students that have private loans. Average private loan amounts are also smaller than Stafford loans, $837 when compared to $2,826 in total Stafford unsubsidized and subsidized loans combined. A small proportion of students, around 7 percent, only have loans in aid packages, while around a third of students have some type of aid that comes from a source other than the state, federal government, or institution. (A Rotary scholarship or aid from an employer would be examples of this type of aid.) Nearly half of students work while a college freshman in some capacity, and nearly half hold a balance on a credit card in their freshmen year. The NPSAS sample we use is roughly 55% female, 65% white, and just over 18 years of age, with 95% of students dependents. EFCs are roughly $12,700 on average, meaning parents are able to contribute roughly that amount annually.

[8]We remove for-profit colleges from our analysis, though the results change only modestly if we instead include them. We see no effect of personal finance education on financial aid packages for for-profit students, though the sample is substantially smaller (roughly 5,000 students) than our other samples.

IV. Results

We begin by studying the effect of personal finance requirements on financial aid for four-year institutions, where we compare states and years that require a course in personal finance prior to graduation from high school to those that do not. Table 3 reports the effects of personal finance graduation requirements on major federal aid choices. The first three columns focus on the discrete decision to apply for and accept aid: the dependent variable for Column 1 is a binary variable equal to one if the student applied for aid,[9] Column 2 is a binary variable equal to one if the student accepted no financial aid (federal or otherwise), and Column 3 is a binary variable equal to one if the student accepted a direct federal Stafford loan. The final three columns show the extent of federal loan aid. Column 4 is the amount in dollars of the subsidized Stafford loan, Column 5 is the amount of an unsubsidized Stafford loan,[10] and Column 6 is a binary variable for whether the student took out the maximum combined federal loan amount for which they were eligible.

The results in Table 3 indicate that personal finance requirements do change student behavior on important margins. Students who graduated from states in years when these requirements were in effect were 3 percent more likely to apply for aid and correspondingly 3 percent less likely to have used no aid (that is, more likely to have accepted an aid package). We expect that the increase in aid applications drives the parallel decrease in the probability of not accepting any aid.

Students required to complete personal finance education were 5.5 percent more likely to have taken out Stafford loans (Column 3). The results in Column 4 suggest that these students became more reliant on subsidized loans, though this is not statistically different from zero at the ten percent level. Column 5 shows that unsubsidized loan amounts appear unresponsive to the policy (both in terms of the small magnitude of the effect and its overall statistical insignificance). The results in Column 6 indicate that students who graduated under the personal finance requirements were nearly 5 percent more more likely to take out the maximum amount of Stafford loans for which they were eligible. Roughly 30 percent of students take the maximum offered, so this is roughly a sixth of the average rate.

The results also indicate that demographics matter in these federal choices, with male students less likely to apply for and accept federal aid, while black and Hispanic students, students with less educated parents, and students at non-public institutions were more likely to apply for and accept federal aid.

Table 4 reports the effect of personal finance graduation requirements on non-federal aid decisions: students might also finance their education by selecting

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

[10] Both Columns 4 and 5 include those without any Stafford loans.

private loans, by identifying grants or scholarships for which they may be eligible, by using credit cards, or by working. The first three columns again are binary variables. Column 1 indicates whether or not a student had only loan aid in their package. Column 2 indicates whether or not the student had any types of grants or scholarships that did not come from federal aid, state aid, or the institution. These are termed "outside aid," and this type of aid is more likely to require proactive searching on the part of the student for specific grants or scholarships for which they may be uniquely eligible; it also includes employer-sponsered aid. Column 3 includes results using a binary variable equal to one if the student took out a private loan to finance their education, while Column 4 reports results for the balance on that private loan (with students who did not take out a private loan included in the analysis). Column 5 presents results for a binary variable for a positive self-reported credit card balance. Column 6 is an indicator variable for whether or not the student worked while enrolled in school.

The results in Table 4 suggest that personal finance graduation requirements affect many different decisions about financing higher education. Students in these baseline results were 1.2 percent less likely to accept a package that only contains loans.[11] While there is no difference in having outside aid or holding private loans, borrowers in states with graduation requirements had $170 less in private loans on average. Since private loans generally have higher interest rates than public student loans, this shift is particularly interesting. Further, those exposed to financial education were 2.2 percent less likely to have a credit card balance.[12] 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). This study also finds that there are information problems, where 75 percent of students are unaware of late fee charges on their credit cards.

A. Heterogeneity by School and Demographic Characteristics

For whom are these personal finance requirements most likely to affect behavior? We begin examining this question by first looking at students at different types of institutions. Table 5 divides students into three classes of institutions: public four-year, private four-year, and public and private two-year.[13] For brevity, this table again presents only the α_1 coefficient from Equation 1.

[11] We find no evidence that this higher level of non-loan aid is correlated with grants and scholarships that tie students to a specific major. Those exposed to financial education were no more likely to decalre 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.

[12] We so no difference in credit card balances, though these data are only available for one year, and the effect is less precisely estimated.

[13] There are too few students in early waves at for-profit four year institutions to include this subgroup. We also only combine all two year students together, as about 85 percent of two year students across the waves are at public insitutions, again leaving too few at private institutions to yeild precise estimates for these subgroups.

The results in Table 5 indicate that personal finance graduation requirements have significant and substantial effects on public four-year university students: nearly every point estimate is larger in magnitude in these estimates than in the baseline results. Public four year students are 4 percent more likely to apply for (and accept) aid if their state had a graduation requirement, 6 percent more likely to have a Stafford loan, and 4 percent less likely to have a credit card balance. These students take out roughly $150 more in subsidized Stafford loans. Strikingly, they are also 4 percentage points less likely to be working while in school, suggesting that the additional federal aid is used to both reduce private borrowing on credit cards and replace work for at least some students.

For students attending private institutions, Table 5 shows that financial education only affects one margin: the dollar amount of private loans. While the sample of students attending private institutions is likely different from those attending public institutions, it is interesting that financial education reduces private student loan borrowing. There is also a modest uptake of the likelihood of working while enrolled in school among these students, though it is only statistically significant at the 10 percent level.

Table 6 and Table 7 report results based on personal demographics. Each cell in the tables is the α_1 coefficient on personal finance for the subgroup listed on the left. These subgroups include male students, female students, students of a specified race or ethnicity (white, black, or Hispanic), and low income students whose EFC is below $4,000 and higher income students whose EFC is above $4,000. We choose this cutoff, as this approximates the cutoff for Pell grant receipt.[14] For example, the first cell in the first column of Table 6 includes the coefficient on the personal finance graduation dummy variable from a regression of whether or not a student applied for financial aid for only the subsample of male students. All results in these tables pertain to students at four-year public and private insitutions, and again estimate Equation 1. The mean dependent variables by group are in Tables A.2, A.3, and A.4 for gender, race, and EFC, respectively.

These tables indicate sigificant heterogeneity in the effects across subgroups. In general, male students appear to be more responsive than female students in terms of applying for aid. However, female students appear to be more likely to reduce private borrowing, to reduce the amount of private loans, and to reduce the likelihood of having a credit card balance. Across socioeconomic groups, White and Hispanic students and higher EFC students are more likely to respond to graduation requirements by increasing applications for aid, while Black and Hispanic students are more likely to get Stafford loans and to have higher levels of subsidized federal loans, where Black students also increase their amounts of unsubsidized Stafford loans. In contrast, White and high-income students are more likely to reduce private loan amounts and to be less likely to have a balance on their credit cards. These results shed light on the mechanisms behind the

[14] If we instead expand the upper benchmark to be over $10,000, our results remain consistent.

graduation requirements: financial education increases subsidized borrowing for groups most likely to be eligible to borrow at these advantageous rates and for groups for whom credit constraints are most likely to inhibit attendance. However, graduation requirements reduce more costly forms of borrowing, particualrly for the groups that are most likely to have access to the broader options for borrowing that include private loans and credit cards.

B. Pre Trends

Difference-in-difference experimental designs require that the treatment and control groups would have had parallel trends in the absence of the policy. This allows for a proper counterfactual. We next validate our experiment by showing that students graduating from high school in the two years prior to the binding financial education requirement were not affected by the mandate in the same way as those subject to the graduation requirement in Table 8. PF_{t+1} and PF_{t+2} are equal to one if the mandate came into effect the year after and two years after the individual graduated from high school, respectively. It could be the case that since the mandate was passed, the states began piloting the course in some schools prior to the graduation requirement. This would result in some effects in the years prior to the mandate. If this were the case, it wouldl bias us against finding effects in our main analysis, since we label these individuals as untreated.

Table 8 indicates that nearly all these "placebo" effects are not statistically different from zero at the ten percent level. However, students who graduated the year before the mandate came into effect reduced their reliance on private student loans. This is a similar effect to the results from Table 9, where students in states with less rigorous requirements reduced private loans. Again, this could be from selection into the education. Students who graduated two years prior to the graduation requirement did not experience significant changes in financial aid packages.

We further provide a placebo test in Appendix B Table B.3. This analysis extends the NPSAS data to use a sample from 1989-2003, preserving the variables that have been consistent over time. The variable PF_{-10} is a placebo variable equal to one if an individual was 18 in a state within the 10 years prior to the time the course 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 only loans in an aid package, 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 gradation requirements, further validating our results.

Concern may arise that other educational reforms could be implemented in sequence with personal finance requirements and could influence financial aid decisions. We devote Appendix E to investigating four potential policies: total credit requirements, math credit requirements, maximum math level required,

and standardized college entrance exam requirements.[15] In all cases, our these robustness checks confirm our baseline effects of personal finance requirements on financial aid packages.

C. Variation across Policies

The results presented so far are averages across students from states requiring exposure to personal finance prior to high school graduation. This masks substantial heterogeneity in laws across states. 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 also trains teachers, funds the course requirement in schools where teachers are properly certified for the course, and gives sample evaluation for students. At the same time, Wyoming requires that students are proficient in personal finance topics to be covered in Social Studies curricula, though it does not mention any specific content. Further, some states, such as Nebraska, New Mexico, and South Dakota only require that schools offer a course in personal finance but do not require all students to take the course. There are also 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 the main analysis, states requiring schools to offer an elective course but not requiring students to take it are classified as zeros for the initial PF indicator. If there are effects on student aid chioces for students who did choose to enroll in these courses, the results are biased against finding an effect as the "control" states are partially treated. Additionally, a state like Wyoming, would be included in the personal finance requirement, as content is required to be completed prior to finishing high school, albeit a small amount. This again, likely biases us towards zero in our main effect, this time because the "treatment" was minimal.

We continue by using alternative classifications, this time dividing treated states as those with more of less rigorous mandates following Urban et al. (2014). Appendix A Table A.1 reports our classification of states' policies. Accordingly, Table 9 reports estimates that disaggregate the policy variable into two separate indicator variables: one for students exposed to more stringent laws and a second indicator variable for students exposed to more relaxed standards. The table reports only the coefficients on the policy variables, but we continue to estimate Equation 1, separating dummies for rigorous and less rigorous policies. Panel A presents the results related to federal aid that correspond to 3, while Panel B corresponds to the additional measures of financial aid packages included in 4. The results in Table 9 indicate that more stringent laws do indeed have a larger effect on aid decisions, both in terms of the general pattern of the magnitude of coefficients and their significance. The magnitude of the effects of rigorous

[15] We also discuss the lack of confounding effects with Common Core requirements.

laws is roughly the same in Table 9 as the baseline results in Tables 3 and 4, although the standard errors are somewhat smaller than the baseline effect across specifications.

Table 9 shows that states implemented less rigorous policies have no statistically significant effect on aid application, the acceptance of aid, Stafford loans, or outside aid. However, these less rigorous programs seem to reduce the prevalence of private loans and the amount of private loans among students. This could be because when a class is required to be offered, a certain sample of students select into the course. For example, risk averse parents may push their children to take a course in personal finance while in high school. These students, after becoming informed, might be most likely to substitute away from private loans.

Most states passed personal finance mandates after 2002. Four states passed an early version of personal finance graduation requirement in 1998 or earlier (IL in 1970, MI in 1998, NH in 1993, and NY in 1996), although these states 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 and from laws passed by later adopters, we also run specifications that exclude these four states altogether. These results are in Appendix B Table B.2. This table also reports a second set of estimates that exclude states who mandated personal finance be taught but allowed school districts or counties to implement the mandate as they saw fit, leading to variation in the timing and the level of the requirement across the state. We also remove Lousiana 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). We find that results for both sets of regressions, those excluding the early adopters and alternatively those excluding states with ambiguous policies, yield results that are quantitatively very similar to our baseline results.

Taken together, Table 9 and Appendix B Table B.2 indicate that the policy variation that drives the effects on students' behaviors is from states with rigorous, clear policies that were implemented after 2002.

V. Offering Financial Education Electives

Finally, we explore in more detail the effects of personal finance courses in a state without a mandates, relying on local variation in personal finance course *offerings* to determine the intent-to-treat (ITT) effect of personal finance courses on aid packages. This detailed analysis informs our state-based analysis in two ways. First, it allows us to understand how requirements to offer an elective course in personal finance may influence financial aid packages. Second, it helps us to understand whether or not our effect found in the NPSAS sample is a lower bound of the true effect of financial education. If courses are offered prior to being mandatory for graduation in a state, our initial analysis will understate

the effect of financial education on financial aid packages. However, if simply offering a course does not change financial aid packages for students, on average, our estimates are likely closer to the average effect.

To conduct this analysis, we use administrative data from the Montana University System (MUS). These data include students' high schools, demographic information, the Montana postsecondary campus attended, and the degree pursued. The MUS data are novel for the detailed individual-level college funding information provided. 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 fraction of tuition covered by the loans. These data do not include any information on private loans,[16] but they do contain semester-by-semester enrollment, credits, major, GPA, courses taken, and retention. To our knowledge, we are among the first researchers to use administrative individual student loan data to examine the effects of financial education on borrowing and aid packages.

Our data span the years 2002 through 2014, or 36 semesters of data. For the purpose of this study, we limit our analysis to 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. We also limit our analysis to in-state undergraduate students so we are able to identify their high school attended. We contact each school directly to determine whether or not they offered a stand-alone personal finance course and in what years. We confirm that students generally take these courses in their junior or senior year, 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 pacakges, as this is when we expect the effect of education to be largest and to parallel our previous results.

We include in our analysis high school fixed effects, year fixed effects, and individual characterstics in all of our models such as a white and missing race dummy, age dummies, a male indicator, ACT scores,[17] and Census block group characteristics for area education, population density, and median household income. We are careful to cluster our standard errors at the high school level as this is where the policy variation stems from.

Table 10 reports the results. In general, the offering of financial education had minimal effect on students. However, Column (5) does indicate that students are now obtaining additional non-loan aid, including grants and scholarships. This could make students slightly less reliant on non-federal loans. While we do not have information on private loans or credit card balances in these data, it is plausible to think that might be one way in which students could substitute towards scholarships and away from other debt forms. We find no evidence that personal

[16]Private student loans are only a small fraction (roughly 7%) of student debt at the undergraduate level (National Center for Education Statistics 2013).

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

finance education offerings change federal loans, work studies, or the likelihood that students have only federal loans (as opposed to loans and scholarships) in their aid packages.

Another added 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 C.1 in Appendix C, 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 C.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. In terms of Census block group characteristics, schools that offer personal finance at some point are slightly less densely populated with slightly lower median household incomes. These differences are relatively small in magnitude. Thus, it is reasonable to assume that adding personal finance as an elective is idiosyncratic across schools. However, it could still be that schools in districts where parents value personal finance push for this agenda, and, at the same time, areas with low financial literacy offer the course as well. This would result in no difference in average characterstic, though the connection between financial education and financial literacy may still be endogenous.

VI. Effect of Financial Education on Institutional Choice

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 (e.g., Stafford loans), while decreasing the use of higher cost private loans or credit cards. However, if these requirements make students particularly averse to borrowing, there might be a concern that these requirements change the type of institutions students attend or even change whether or not students enroll in school. For example, if students who take these courses become more concerned about college costs, they might be more likely to attend a two-year school than a four-year instition, or more likely to attend a public than a private school, or more likely to chose a school with lower tuition, or less likely to attend college at all. If these choices reduce the ideal match between students and schools, these policies may have negative unintended consequences in terms of life-time income.

Using these data, we can address the likelihood of observing an enrolled student at different types of institutions. Table 5 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 prive, 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, and whether or not the student enrolled in a four-year, as opposed to a two-year, college. While demographic characteristics are significantly related to institutional choice in predictable ways,

personal finance graduation requirements do not appear to play a role in what type of institution the student attends. The coefficient on graduation requirements in all cases is typically small and imprecisely estimated, with none of the estimates approaching statistical significance at even the 10 percent level.

To be sure that the specifications are not driven by our particular measure of income, we also estimate with tuition included and using income instead of EFC. These results are reported in Appendix B Table B.4. If we look at only dependent students or only full-time students, our results are consistent (Appendix B Table B.5).

Finally, in Appendix D we examine college enrollment. The NPSAS includes 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. 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. Appendix D provides a detailed discussion of the data, methods, and results for this finding.

VII. Conclusions

While student loan reform has been a pressing policy topic for the last few years, the academic literature has largely focused on reforms that take place at specific higher-education instutitions. These studies have been suggestive of the importance of information in borrowing decisions: asking students to make an active choice on their initial student loan packages and offering financial counseling decreased initial loan amounts for community college high-risk borrowers (Barr, Bird and Castleman 2016), and there are some promising results of information-based interventions on borrowing behavior after freshman year (Stoddard, Urban and Schmeiser 2017; Darolia 2016).

We contribute to this new but growing strand of literature that seeks to inform student loan decisions as they are initially being made. Rather than focusing on a reforms that require institutional participation, we examine broad statewide policies that affect all high school students. Our results show that high school financial education course graduation mandates can significantly impact key student financial aid behaviors. These course mandates inrease the likelihood that students apply for aid, the likelihood students that accept aid, and the amount of federal student loan aid students receive. At the same time, these mandates are also associated with decreased private loan amounts, a decreased probability of having only loans in aid packages, and a lower likelihood of carrying a credit card balance. This complements previous findings that this type of high school education also improves young adults' credit scores and lowers default risks.

The results are consistent with several plausible mechanisms: course may result

in a better understanding of how borrow to make investments, and they may help students craft a budget to determine loans as well as to determine the need for other college funding sources. Finally, they may assist with determining whether, when, and how to apply for aid. Recent policies aimed at simplifying the FAFSA and syncing the process more closely with IRS tax data may also lower the administrative burdens of applying for aid, but the broad set of impacts of this policy are suggestive of the role of informational policies in contributing to improved financial decision making among young adults.

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VIII. Tables and Figures

Table 1—: States with Personal Finance Graduation Requirements

State	First Graduating Class Affected	State	First Graduating Class Affected
AR	2005	NH	1993
AZ	2005	NJ	2011
CO	2009	NM	2003
GA	2007	NY	1996
IA	2011	OR	2013
ID	2007	SC	2009
IL	1970	SD	2006
KS	2012	TN	2011
LA	2005	TX	2007
MI	1998	UT	2008
MO	2010	VA	2008
NC	2005	WY	2002
NE	2011		

Table 2—: Summary Statistics by Financial Education Status

	No PF	PF Required	Both
Dependent Variables			
Applied for any Aid	0.8745	0.9011	0.8829
	(0.3313)	(0.2985)	(0.3215)
No Financial Aid	0.2262	0.1868	0.2137
	(0.4184)	(0.3898)	(0.4099)
Stafford Loan	0.4117	0.4302	0.4176
	(0.4921)	(0.4951)	(0.4932)
Subsidized Stafford $s	886	1,009	925
	(1,369)	(1,473)	(1,403)
Unsubsidized Stafford $s	667	853	726
	(1,414)	(1,616)	(1,484)
Max Stafford Loan	0.2958	0.2891	0.2937
	(0.4564)	(0.4534)	(0.4554)
Only loans in Aid Pkg	0.0802	0.0561	0.0725
	(0.2716)	(0.2302)	(0.2594)
Outside aid	0.2792	0.2607	0.2733
	(0.4486)	(0.4390)	(0.4457)
Private loan	0.0807	0.0785	0.0800
	(0.2724)	(0.2689)	(0.2713)
Private loan $s	519	510	516
	(2,424)	(2,404)	(2,418)
Have CC Balance	0.1129	0.1108	0.1122
	(0.3165)	(0.3139)	(0.3157)
Work while Enrolled	0.5640	0.5377	0.5556
	(0.4959)	(0.4986)	(0.4969)
Independent Variables			
Male	0.4506	0.4536	0.4516
	(0.4976)	(0.4979)	(0.4977)
White	0.6835	0.6223	0.6640
	(0.4651)	(0.4848)	(0.4723)
Black	0.1143	0.1709	0.1323
	(0.3181)	(0.3764)	(0.3388)
Hispanic	0.1221	0.1413	0.1282
	(0.3274)	(0.3484)	(0.3343)
Age	18.40	18.36	18.39
	(0.509)	(0.503)	(0.508)
Dependent	0.9498	0.9469	0.9489
	(0.2184)	(0.2242)	(0.2203)
EFC (000s)	11.9595	11.3573	11.7681
	(16.7384)	(16.9532)	(16.8091)
Parent < HS	0.0382	0.0395	0.0386
	(0.1916)	(0.1948)	(0.1926)
Parent HS Grad	0.2379	0.2432	0.2396
	(0.4258)	(0.4290)	(0.4268)
Parent Some Coll	0.2322	0.2518	0.2384
	(0.4222)	(0.4341)	(0.4261)

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) No Aid	(3) Have Stafford	(4) Sub Stafford $s	(5) Unsub Stafford $s	(6) Took Max
PF	0.033*	-0.033*	0.055*	100.082	16.478	0.047*
	(0.013)	(0.013)	(0.022)	(62.054)	(77.316)	(0.021)
Male	-0.018***	0.021***	-0.020**	-58.932**	-57.812**	-0.017*
	(0.004)	(0.005)	(0.007)	(20.966)	(20.750)	(0.007)
White	-0.029**	0.019*	0.032	2.430	104.778+	0.038+
	(0.011)	(0.009)	(0.027)	(66.071)	(53.763)	(0.021)
Black	0.042***	-0.062***	0.180***	488.194***	781.652***	0.175***
	(0.008)	(0.009)	(0.020)	(59.653)	(63.823)	(0.020)
Hispanic	0.028*	-0.035*	0.051*	45.278	88.079+	0.036*
	(0.011)	(0.014)	(0.021)	(62.052)	(48.893)	(0.016)
Age 17	-0.013	0.008	-0.073*	-130.072	-301.367**	-0.086*
	(0.016)	(0.019)	(0.032)	(105.275)	(103.536)	(0.041)
Age 19	-0.024***	0.027***	-0.014*	-34.994+	-68.215**	-0.028***
	(0.004)	(0.005)	(0.005)	(18.235)	(20.292)	(0.005)
Dependent	0.075***	-0.087***	0.172***	413.169***	-298.940**	0.308***
	(0.015)	(0.017)	(0.019)	(64.386)	(94.705)	(0.023)
EFC (000s)	-0.001***	0.004***	-0.005***	-30.361***	7.235***	-0.004***
	(0.000)	(0.000)	(0.000)	(1.239)	(0.733)	(0.000)
Parent < HS	0.038***	-0.059**	0.005	83.665	-75.760	-0.029
	(0.010)	(0.018)	(0.018)	(50.977)	(76.817)	(0.017)
Parent HS Grad	0.058***	-0.068***	0.093***	252.371***	127.789**	0.052***
	(0.004)	(0.006)	(0.010)	(24.233)	(36.744)	(0.008)
Parent some Coll	0.041***	-0.051***	0.100***	261.436***	166.843***	0.069***
	(0.004)	(0.005)	(0.008)	(22.552)	(22.032)	(0.007)
Private	0.052***	-0.123***	0.137***	497.026***	149.917**	0.140***
	(0.007)	(0.008)	(0.016)	(42.382)	(48.613)	(0.015)
Year FE	YES	YES	YES	YES	YES	YES
State FE	YES	YES	YES	YES	YES	YES
Observations	25,354	25,354	25,354	25,354	25,354	25,301
Adjusted R^2	0.046	0.098	0.110	0.253	0.135	0.069

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 regressions 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 4—: Other Financial Decisions at Four Year Institutions

	(1) Only Loans	(2) Outside Aid	(3) Private Loan	(4) Private Loan $s	(5) Have CC Balance	(6) Work while Enrolled
PF	-0.012^{+}	0.015	-0.004	-169.663*	-0.022*	-0.012
	(0.007)	(0.012)	(0.007)	(66.880)	(0.008)	(0.014)
Male	-0.001	-0.027***	0.008^{+}	65.580*	-0.016***	-0.043***
	(0.003)	(0.006)	(0.004)	(28.585)	(0.004)	(0.008)
White	0.023***	0.031*	0.016	116.636	-0.019**	0.045***
	(0.005)	(0.013)	(0.011)	(89.285)	(0.006)	(0.012)
Black	0.017**	0.029^{+}	0.013	20.972	0.029**	-0.039^{+}
	(0.005)	(0.015)	(0.008)	(89.228)	(0.010)	(0.019)
Hispanic	0.017*	0.007	0.008	92.187	0.014	0.047***
	(0.007)	(0.014)	(0.007)	(108.505)	(0.012)	(0.013)
Age 17	-0.004	0.001	-0.005	54.940	-0.010	-0.001
	(0.012)	(0.035)	(0.016)	(192.136)	(0.022)	(0.031)
Age 19	0.000	-0.009	0.003	47.004	0.025***	0.034***
	(0.003)	(0.007)	(0.004)	(34.628)	(0.004)	(0.007)
Dependent	0.017*	0.076***	0.040***	311.718**	0.043***	-0.088***
	(0.007)	(0.018)	(0.011)	(93.853)	(0.011)	(0.024)
EFC (000s)	0.001***	-0.002***	-0.001***	-1.180	-0.000***	-0.001***
	(0.000)	(0.000)	(0.000)	(1.401)	(0.000)	(0.000)
Parent < HS	-0.024*	-0.048*	-0.016	-222.630***	0.039***	0.070*
	(0.012)	(0.020)	(0.010)	(55.655)	(0.011)	(0.031)
Parent HS grad	-0.004	-0.019*	0.020***	167.023*	0.036***	0.074***
	(0.004)	(0.008)	(0.006)	(64.940)	(0.004)	(0.009)
Parent some coll	-0.004	0.038***	0.041***	265.053***	0.025***	0.054***
	(0.004)	(0.010)	(0.006)	(67.292)	(0.005)	(0.007)
Private	-0.069***	0.131***	0.068***	706.969***	-0.013**	-0.091***
	(0.008)	(0.011)	(0.006)	(62.240)	(0.004)	(0.012)
Year FE	YES	YES	YES	YES	YES	YES
State FE	YES	YES	YES	YES	YES	YES
Observations	25,354	25,354	25,354	25,354	25,354	25,354
Adjusted R^2	0.059	0.027	0.024	0.024	0.015	0.040

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 regressions 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 5—: Comparison of Effects at Different Types of Institutions

Panel A

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Sub Stafford $s	(5) Unsub Stafford $s	(6) Took Max
Public 4 year Institutions						
PF	0.040**	-0.040*	0.066*	153.505*	74.302	0.043^{+}
	(0.015)	(0.017)	(0.025)	(69.768)	(94.273)	(0.023)
N	14,714	14,714	14,714	14,714	14,714	14,684
Private 4 year Institutions						
PF	0.013	-0.011	0.021	2.129	-36.115	0.041
	(0.014)	(0.013)	(0.029)	(90.103)	(101.667)	(0.032)
N	10,640	10,640	10,640	10,640	10,640	10,617

Panel B

	(1) Only Loans	(2) Outside Aid	(3) Private Loan	(4) Private Loan $s	(5) Have CC Balance	(6) Work while Enrolled
Public 4 year Institutions						
PF	-0.015	0.018	-0.016	-117.616	-0.041**	-0.044**
	(0.010)	(0.014)	(0.011)	(70.223)	(0.012)	(0.014)
N	14,714	14,714	14,714	14,714	14,714	14,714
Private 4 year Institutions						
PF	-0.010	-0.003	0.009	-271.543*	0.008	0.038^{+}
	(0.009)	(0.020)	(0.011)	(129.025)	(0.013)	(0.022)
N	10,640	10,640	10,640	10,640	10,640	10,640

Notes: Source: NPSAS data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. $PF = 1$ if the student's permanent address was in a state that required personal finance prior to graduating high school and 0 otherwise. Each regressions includes state and year fixed effect and all covariates listed in Table 3.

Table 6—: Heterogenous Effects of Personal Finance Graduation Requirements, Part A

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Sub Stafford $s	(5) Unsub Stafford $s	(6) Took Max
Gender Subgroups						
Male	0.062***	-0.055**	0.056+	137.924+	-59.571	0.046
	(0.016)	(0.018)	(0.030)	(73.146)	(81.118)	(0.028)
N	11,204	11,204	11,204	11,204	11,204	11,184
Female	0.009	-0.015	0.056**	73.669	81.807	0.048*
	(0.014)	(0.014)	(0.019)	(67.723)	(84.892)	(0.018)
N	14,150	14,150	14,150	14,150	14,150	14,117
Racial and Ethnic Subgroups						
White	0.034*	-0.033*	0.031	3.870	-96.271	0.033
	(0.013)	(0.015)	(0.023)	(53.348)	(76.148)	(0.023)
N	17,996	17,996	17,996	17,996	17,996	17,961
Black	-0.002	0.003	0.106*	253.536*	445.626**	0.064+
	(0.008)	(0.016)	(0.044)	(124.272)	(157.706)	(0.037)
N	2,859	2,859	2,859	2,859	2,,859	2,849
Hispanic	0.041**	-0.050**	0.167***	340.138**	173.809	0.109*
	(0.015)	(0.016)	(0.046)	(117.884)	(125.972)	(0.042)
N	2,524	2,524	2,524	2,524	2,524	2,520
Low and High EFC Subgroups						
EFC<$4k	0.020*	-0.028**	0.063+	177.189	74.504	0.042
	(0.008)	(0.010)	(0.035)	(107.305)	(102.961)	(0.033)
N	9,224	9,224	9,224	9,224	9,224	9,208
EFC>$4K	0.040*	-0.036+	0.047**	27.581	-17.583	0.053**
	(0.017)	(0.018)	(0.017)	(51.418)	(80.345)	(0.018)
N	16,130	16,130	16,130	16,130	16,130	16,093

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 regressions includes state and year fixed effect and all covariates listed in Table 3, except for the variable corresponding to the subgroup listed.

Table 7—: Heterogenous Effects of Personal Finance Graduation Requirements, Part B

	(1) Only Loans	(2) Outside Aid	(3) Private Loan	(4) Private Loan $s	(5) Have CC Balance	(6) Work while Enrolled
Gender Subgroups						
Male	-0.009	0.025	0.014	-92.621	-0.009	-0.017
	(0.008)	(0.021)	(0.013)	(109.621)	(0.011)	(0.019)
N	11,204	11,204	11,204	11,204	11,204	11,204
Female	-0.016	0.007	-0.019^{+}	-230.613**	-0.035**	-0.009
	(0.010)	(0.018)	(0.010)	(73.629)	(0.010)	(0.018)
N	14,150	14,150	14,150	14,150	14,150	14,150
Racial and Ethnic Subgroups						
White	-0.016*	0.014	-0.013	-253.017***	-0.023*	-0.010
	(0.008)	(0.012)	(0.008)	(68.771)	(0.010)	(0.020)
N	17,996	17,996	17,996	17,996	17,996	17,996
Black	0.011	-0.042	0.005	-68.035	-0.010	0.010
	(0.013)	(0.035)	(0.019)	(139.797)	(0.028)	(0.032)
N	2,859	2,859	2,859	2,859	2,859	2,859
Hispanic	-0.028	0.058^{+}	0.005	-112.678	0.030	0.032
	(0.017)	(0.032)	(0.019)	(260.357)	(0.022)	(0.038)
N	2,524	2,524	2,524	2,524	2,524	2,524
Low and High EFC Subgroups						
EFC<$4K	-0.017^{+}	0.023	0.006	22.170	-0.020	-0.046*
	(0.009)	(0.027)	(0.008)	(70.384)	(0.015)	(0.021)
N	9,224	9,224	9,224	9,224	9,224	9,224
EFC>$4K	-0.011	0.008	-0.013	-295.395**	-0.021**	0.011
	(0.010)	(0.016)	(0.010)	(89.308)	(0.008)	(0.018)
N	16,130	16,130	16,130	16,130	16,130	16,130

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 regressions includes state and year fixed effect and all covariates listed in Table 3, except for the variable corresponding to the subgroup listed.

Table 8—: Testing the Pre-trends in Financial Education

Panel A

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Sub Stafford $s	(5) Unsub Stafford $s	(6) Took Max
PF_{t+1}	-0.048	0.121^{+}	-0.134^{+}	-198.851	-125.943	-0.076
	(0.057)	(0.063)	(0.079)	(198.260)	(227.082)	(0.069)
PF_{t+2}	-0.026	0.001	-0.015	-132.870	120.365^{+}	-0.011
	(0.038)	(0.039)	(0.026)	(87.509)	(62.368)	(0.026)
N	14,714	14,714	14,714	14,714	14,714	14,684

Panel B

	(1) Only Loans	(2) Outside Aid	(3) Private Loan	(4) Private Loan $s	(5) Have CC Balance	(6) Work while Enrolled
PF_{t+1}	-0.003	-0.034	-0.034^{*}	-234.448^{*}	0.033	0.143^{*}
	(0.021)	(0.036)	(0.015)	(112.482)	(0.042)	(0.069)
PF_{t+2}	-0.031^{+}	-0.021	-0.012	-94.128	0.002	-0.001
	(0.017)	(0.020)	(0.009)	(69.366)	(0.015)	(0.015)
N	14,714	14,714	14,714	14,714	14,714	14,714

Notes: Source: NPSAS Data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regressions includes state and year fixed effect and all covariates listed in Table 3. $PF_{t+1} = 1$ if the individual was 19 in a state where personal finance was required in his high school for those 18 and younger. $PF_{t+2} = 1$ if the individual was 20 in a state where personal finance was required in his high school for those 18 and younger.

Table 9—: Comparison of Loose and Rigorous Requirements, Four Year Institutions

Panel A

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Sub Stafford $s	(5) Unsub Stafford $s	(6) Took Max
Rigorous	0.035*	-0.036*	0.061**	120.920^{+}	29.869	0.054*
	(0.014)	(0.014)	(0.022)	(63.778)	(78.938)	(0.021)
Less Rigorous	0.006	-0.013	-0.015	-33.580	211.504	-0.006
	(0.011)	(0.018)	(0.042)	(106.026)	(211.246)	(0.024)
N	25,354	25,354	25,354	25,354	25,354	25,301

Panel B

	(1) Only Loans	(2) Outside Aid	(3) Private Loan	(4) Private Loan $s	(5) Have CC Balance	(6) Work while Enrolled
Rigorous	-0.017**	0.017	-0.005	-163.930*	-0.019*	-0.021
	(0.006)	(0.013)	(0.007)	(67.773)	(0.008)	(0.014)
Less Rigorous	-0.049	0.026	-0.052***	-435.357*	-0.040^{+}	0.080
	(0.030)	(0.056)	(0.012)	(194.192)	(0.023)	(0.048)
N	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 regressions includes state and year fixed effects and all covariates listed in Table 3.

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

	(1) Have Stafford	(2) Sub Stafford $s	(3) Unsub Stafford $s	(4) Only Loans	(5) Non-loan Aid $s	(6) Work Study
PF Offered	-0.010	0.687	-5.062	-0.003	75.206*	0.004
	(0.013)	(18.637)	(22.755)	(0.008)	(30.709)	(0.003)
N	21,313	21,313	21,313	21,313	21,313	21,313

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 regressions 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), median household income and education at the Census block group-level, and campus dummy. Have Stafford= 1 if student received a Stafford loan of any kind, and zero otherwise. 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). Work study only includes federal work study, not work outside the university. PF Course Offered = 1 if the student went to high school that offered personal finance prior to the time she graduated from high school.

Table 11—: Personal Finance Graduation Requirements and Choice of Institution

	(1) Private	(2) Public	(3) Tuition & Fees	(4) Four yr
PF	-0.003	0.003	-740.970	-0.010
	(0.042)	(0.042)	(665.612)	(0.050)
Year FE	YES	YES	YES	YES
State FE	YES	YES	YES	YES
Observations	25,354	25,354	22,437	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 regressions includes state and year fixed effects and all covariates listed in Table 3. Columns 1 through 3 include only four-year students; Column 4 includes students at two- and four-year institutions.

IX. Appendix A: Financial Education Requirements

Table A.1—: State Personal Finance Graduation Requirements by Year or Implementation

Rigorous: Personal Finance Graduation Requirement, 11 states
Arkansas (2005), Colorado (2009), Georgia (2007), Idaho (2007),
Iowa (2011), Missouri (2010), North Carolina (2005), South Carolina (2009),
Tennessee (2011), Texas (2007), Utah (2008)
States with Long Existing Personal Finance Graduation Requirements, 4 states
Illinois (1970), Michigan (1998), New Hampshire (1993), New York (1996)
Less Rigorous, 4 states
Nebraska (2011), New Mexico (2003), South Dakota (2006), Wyoming (2002)
Excluded for Local Control, Large-Scale Pilots, or Natural Disaster, 8 states
Arizona (2005), Connecticut (various), Kansas (2012) Louisiana (2005)
New Jersey (2011), Oregon (2013), Virginia (2008), West Virginia (various)
No Personal Finance Graduation Requirement or Standard Course, 23 states
Alabama, Alaska, California, Delaware, Florida, Hawaii, Indiana, Kentucky, Maine, Maryland,
Massachusetts, Minnesota, Mississippi, Montana, Nevada, North Dakota, Ohio,
Oklahoma, Oregon, Pennsylvania, Rhode Island, Vermont, Washington, Wisconsin

Notes: Data as of 2013. For full data, see `http://www.montana.edu/urban/financial-edu-database.html`.

Table A.2—: Summary Statistics by Gender

	Female	Male	Both
Applied for any aid	0.9005	0.8616	0.8829
	(0.2993)	(0.3454)	(0.3215)
No Financial Aid	0.1900	0.2425	0.2137
	(0.3923)	(0.4286)	(0.4099)
Stafford Loan	0.4321	0.3999	0.4176
	(0.4954)	(0.4899)	(0.4932)
Subsidized Stafford $s	975	864	925
	(1429)	(1370)	(1404)
Unsubsidized Stafford $s	755	692	726
	(1511)	(1449)	(1484)
Max Stafford Loan	0.3042	0.2805	0.2935
	(0.4601)	(0.4493)	(0.4554)
Only Loans in Aid Pkg	0.0712	0.0741	0.0725
	(0.2572)	(0.2620)	(0.2594)
Outside Aid	0.2866	0.2572	0.2733
	(0.4522)	(0.4371)	(0.4457)
Private Loan	0.0799	0.0802	0.0800
	(0.2711)	(0.2715)	(0.2713)
Private Loan $s	512	521	516
	(2390)	(2451)	(2418)
Have CC Balance	0.4611	0.4424	0.4526
	(0.4985)	(0.4967)	(0.4978)
Work while Enrolled	0.5722	0.5354	0.5556
	(0.4948)	(0.4988)	(0.4969)

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

Table A.3—: Summary Statistics by Race

	White	Black	Hispanic	Other	All
Applied for any Aid	0.8622	0.9600	0.9204	0.8716	0.8829
	(0.3447)	(0.1960)	(0.2707)	(0.3346)	(0.3215)
No Financial Aid	0.2369	0.1016	0.1809	0.2425	0.2137
	(0.4252)	(0.3022)	(0.3850)	(0.4286)	(0.4099)
Stafford Loan	0.4233	0.5112	0.3228	0.3897	0.4176
	(0.4941)	(0.4999)	(0.4676)	(0.4877)	(0.4932)
Subsidized Stafford $s	888	1328	804	829	925
	(1389)	(1537)	(1369)	(1302)	(1404)
Unsubsidized Stafford $s	743	1047	541	519	726
	(1480)	(1787)	(1315)	(1252)	(1484)
Max Stafford Loan	0.3044	0.3440	0.2055	0.2765	0.2935
	(0.4602)	(0.4751)	(0.4041)	(0.4473)	(0.4554)
Only Loans in Aid Pkg	0.0702	0.0441	0.0392	0.1416	0.0725
	(0.2555)	(0.2053)	(0.1940)	(0.3487)	(0.2594)
Outside Aid	0.2943	0.2431	0.2010	0.2713	0.2733
	(0.4558)	(0.4290)	(0.4008)	(0.4446)	(0.4457)
Private Loan	0.0893	0.0696	0.0549	0.0698	0.0800
	(0.2852)	(0.2545)	(0.2278)	(0.2548)	(0.2713)
Private Loan $s	602	401	352	378	516
	(2626)	(2063)	(2079)	(1942)	(2417)
Have CC Balance	0.4311	0.5112	0.6081	0.3525	0.4526
	(0.4952)	(0.4999)	(0.4882)	(0.4778)	(0.4978)
Work while Enrolled	0.5690	0.4918	0.5378	0.5706	0.5556
	(0.4952)	(0.5000)	(0.4986)	(0.4950)	(0.4969)

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

Table A.4—: Summary Statistics by EFC

	EFC<4K	EFC<4K	Both
Applied for any Aid	0.9525	0.8281	0.8829
	(0.2128)	(0.3773)	(0.3215)
No Financial Aid	0.0929	0.3090	0.2137
	(0.2903)	(0.4621)	(0.4099)
Have Stafford Loan	0.4275	0.4098	0.4176
	(0.4947)	(0.4918)	(0.4932)
Subsidized Stafford $s	1190	716	925
	(1497)	(1288)	(1404)
Unsubsidized Stafford $s	568	851	726
	(1325)	(1587)	(1484)
Max Stafford	0.2724	0.3102	0.2935
	(0.4452)	(0.4626)	(0.4554)
Only Loans in Aid Pkg	0.0215	0.1128	0.0725
	(0.1452)	(0.3163)	(0.2594)
Outside Aid	0.2449	0.2957	0.2733
	(0.4301)	(0.4564)	(0.4457)
Private Loan	0.0608	0.0951	0.0800
	(0.2390)	(0.2934)	(0.2713)
Private Loan $s	298	688	516
	(1583)	(2900)	(2418)
Have CC Balance	0.5097	0.4076	0.4526
	(0.4999)	(0.4914)	(0.4978)
Work while Enrolled	0.5647	0.5484	0.5556
	(0.4958)	(0.4977)	(0.4969)

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

X. Appendix B: NPSAS Robustness Exercises

Table B.1—: Effects at Two-Year Institutions

Panel A

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Sub Stafford $s	(5) Unsub Stafford $s	(6) Took Max
PF	-0.037*	0.029	0.009	31.500	4.659	-0.005
	(0.016)	(0.031)	(0.036)	(80.966)	(72.918)	(0.016)
N	18,052	18,052	18,052	18,052	18,052	18,041

Panel B

	(1) Only Loans	(2) Outside Aid	(3) Private Loan	(4) Private Loan $s	(5) Have CC Balance	(6) Work while Enrolled
PF	-0.020	0.011	-0.010	-8.967	-0.013	0.034
	(0.015)	(0.019)	(0.006)	(24.889)	(0.009)	(0.028)
N	18,052	18,052	18,052	18,052	18,052	18,052

Notes: Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regressions includes state and year fixed effect and all covariates listed in Table 3.

Table B.2—: Robustness check: Alternative policy specifications

Panel A

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Sub Stafford $s	(5) Unsub Stafford $s	(6) Took Max
No early states–Drop states implementing pre-1996						
PF	0.028*	-0.026*	0.040^{+}	62.556	15.496	0.032
	(0.013)	(0.012)	(0.022)	(65.346)	(84.635)	(0.020)
N	21063	21063	21063	21063	21063	21016
No locally determined policies						
PF	0.035*	-0.036*	0.063**	117.193^{+}	32.573	0.055*
	(0.014)	(0.014)	(0.022)	(63.624)	(84.779)	(0.021)
N	22942	22942	22942	22942	22942	22897

Panel B

	(1) Only Loans	(2) Outside Aid	(3) Private Loan	(4) Private Loan $s	(5) Have CC Balance	(6) Work while Enrolled
No early states–Drop states implementing pre-1996						
PF	-0.013^{+}	0.009	-0.010	-179.985*	-0.026**	-0.014
	(0.008)	(0.013)	(0.007)	(73.002)	(0.008)	(0.014)
N	21063	21063	21063	21063	21063	21063
No locally determined policies						
PF	-0.015*	0.019	-0.005	-175.140*	-0.019*	-0.021
	(0.007)	(0.013)	(0.007)	(71.418)	(0.008)	(0.014)
N	22942	22942	22942	22942	22942	22942

Notes: Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regressions includes state and year fixed effects and all covariates listed in Table 3.

Table B.3—: Placebo Financial Education did not Affect Outcomes in the Pre-Period

	(1) No Aid	(2) Have Stafford	(3) Only Loans	(4) Work while Enrolled
PF_{-10}	0.002	-0.007	0.002	-0.017
	(0.020)	(0.021)	(0.013)	(0.029)
N	8,655	8,655	7,876	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 regressions includes state and year fixed effect and all covariates listed in Table 3. $PF_{-10} = 1$ if the individual was 18 in a state where personal finance was required in his high school within the 10 years before the course requirement was binding. This variable essentially just falsely moves the policy back ten yers. The sample includes data from 1989-2003.

Table B.4—: Robustness check: Alternative affordability controls

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Sub Stafford $s	(5) Unsub Stafford $s	(6) Took Max
Include Tuition and EFC						
PF	-0.018**	0.010	-0.002	-109.280^{+}	-0.021*	-0.023
	(0.007)	(0.013)	(0.007)	(56.515)	(0.008)	(0.016)
N	28355	28355	28355	28355	28355	28355
Income Instead of EFC						
PF	0.033*	-0.033*	0.055*	100.247	16.457	0.047*
	(0.013)	(0.013)	(0.022)	(64.934)	(76.750)	(0.022)
N	25,354	25,354	25,354	25,354	25,354	25,301

Notes: Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regressions includes state and year fixed effects and all covariates listed in Table 3.

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

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Sub Stafford $s	(5) Unsub Stafford $s	(6) Took Max
Full-time Students Only						
PF	0.029*	-0.025^{+}	0.052*	81.877	22.198	0.048*
	(0.013)	(0.012)	(0.022)	(59.538)	(80.622)	(0.022)
N	23,419	23,419	23,419	23,419	23,419	23,373
Dependent Students Only						
PF	0.031*	-0.031*	0.054*	94.166	23.330	0.051*
	(0.013)	(0.013)	(0.021)	(60.579)	(76.547)	(0.021)
N	24,664	24,664	24,664	24,664	24,664	24,612

Notes: Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regressions includes state and year fixed effects and all covariates listed in Table 3.

XI. Appendix C: MUS Data

Table C.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)
Only Loans in Aid Pkg	0.153	0.134	0.139
	(0.360)	(0.340)	(0.345)
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)
Census Block Group Variables			
Population Density	1656.0	1561.9	1585.0
	(2125.6)	(2079.0)	(2090.9)
% No High School	5.676	5.941	5.876
	(3.453)	(3.186)	(3.255)
% High School Ed	22.92	23.11	23.06
	(8.194)	(8.075)	(8.104)
% Some College	30.56	30.85	30.78
	(3.160)	(3.061)	(3.088)
% Non White	7.252	7.084	7.125
	(6.948)	(5.980)	(6.232)
Median HH Income	44402.2	43471.0	43699.5
	(12329.8)	(10558.5)	(11026.5)

XII. Appendix D: CPS Data

In this sectio, we seek to understand how personal finance graduation requirements affect college attendence using data from the Current Population Survey (CPS). These data span from 1995-2013, and we trim the sample to match our 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.[18] We assume that students remain in the same state in which they attended high school until they are age 20.

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 attedance, as well as the combination of the two. Table D.1 shows the average dependent variables by whether or not the state ever required personal finance prior to graduation, using the CPS sample weights.[19] 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.

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

Next, we estimate the effect of personal finance education on college attendance using Equation 2. Our dependent variable, $Y_{i,s,t}$, equals one if individual i in state 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 D.2 reports the results from Equation 2. Our baseline specification shows that personal finance course requirements do not change college attendance rates. We then replicate our policy heterogeneity from Table 9 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. Further, when we perform additional robustness tests to drop early adopters or those with locally-controlled policies,

[18] 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.

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

as in Table B.2, we again find no effects of personal finance on postsecondary education attendance. In all specifications, the results are nearly zero in magnitude. Thus, we think we have tightly estimated a null effect of financial education on college attendance.

Table D.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	0.0402	0.0520	0.0454
	(0.196)	(0.222)	(0.208)
Age	19.37	19.38	19.37
	(0.664)	(0.663)	(0.664)

Table D.2—: Financial Education does not Change College Attendance

	(1) College At All	(2) College Full Time	(3) College Part Time
Baseline Results			
PF	-0.007	-0.006	-0.001
	(0.007)	(0.007)	(0.002)
N	510,933	510,933	510,933
Policy Heterogeneity			
Rigorous	-0.010	-0.012	0.003
	(0.008)	(0.008)	(0.002)
Less Rigorous	0.012	0.009	0.003^{+}
	(0.031)	(0.031)	(0.002)
N	510,933	510,933	510,933
Dropping Early Adopters			
PF	-0.010	-0.010	0.001
	(0.008)	(0.008)	(0.002)
N	437,959	437,959	437,959
Dropping Local Control			
PF	-0.006	-0.005	-0.001
	(0.007)	(0.007)	(0.002)
N	457,580	457,580	457,580

Notes: Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regressions includes state, survey month, and year fixed effects and the following controls: male, age 18 and age 19 dummies, marrital 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.

XIII. Appendix E: Other Educational Policies

One potential concern with these results arises if states pass financial education graduation requirements at the same time they implement other education reforms that simultaneously affect student borrowing decisions. We examine four such policy changes that have taken place over this period: changes in the total number of Carnegie units required for graduation, changes in the number of math courses students are required to take in high school for graduation, changes in the highest level of math classes required for graduation, and the requirement that all students take a college placement exam (SAT or ACT). We explore each of these to rule out that these policies do not confound our results, and we explicitly show that they do not change the magnitude of our coefficient of interest α_1.

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 (`http://ecs.force.com/mbdata/mbprofall?Rep=HS01`).[20] This reports policy as of 2007 and documents any changes since that date for each graduating class. 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. Where these sources differ, we refer to state statues. 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). States that have no statewide policies but rely on local school boards to determine graduation requirements are omitted from the analysis.

If state legisluatures change multiple course mandates at the same time, we may actually be picking up the result of another course change. One coures requirement that experiences the most changes is mathematics, which could plausibly change financial education decisions as well. In our data, the correlation between personal finance graduation requirements and math credits required is only 0.17. It is, however, still important to show that our main effects do not change if we control for this math education reforms. Table E.1 reports that increasing required math courses does not affect aid applications or federal aid amouts, though it does slightly decrease the likelihood of only having loans in one's aid package, potentially increasing students' scholarship awards. Across the board, including this variable does not affect the stability of the coefficient on personal finance education (α_1). The results in Table E.2 further confirm that math education does not confound the effect we find on personal finance education: α_1 coefficients

[20]This was retrieved on December 20, 2016.

are not statistically different from our main effect in Table 3. Requiring higher levels of math only affects aid packages in one meaningful way: a decrease in the likelihood of having outside aid.

We next explore the possibility that adding personal finance standards results in a simultaneous change in total credits, and therefore, can affect our main estimates. In practice, the correlation between personal finance graduation requirements and total credit requirements is low, 0.08. Table E.3 reports these findings, where our main effects of personal finance education remain consistent. However, increasing total credits required decreases overall borrowing for postsecondary education: aid applications, federal loans, and private loans all decrease as a result of an increase in total credits.

Many states now require all students to take a college entrance exam (either the ACT or SAT) for graduation, as used in (Hyman 2016; Bulman 2015). These are typically taken in a student's junior year. We identify states with current policies using ACT and College Board reported data (See `http://www.edweek.org/ew/articles/2014/10/29/10satact.h34.html` for the 2014 map of participating states). We then consult the Education Commission of the States (ECS) State Policy Database,[21] to identify the data each state passed the college entrance exam policy. This policy has the highest correlation with passages of financial education graduation requirements (0.33).

Table E.4 reports the effects of ACT (or SAT) requirements and personal finance education on financial aid packages. Requiring college entrance exams does not change finanancial aid packages, with the exception of lowering aid applications and subsequently decreasing the probability of having aid. Including this added policy variable does not change our main coefficient of interest (α_1).

A final policy that may be confounded with personal finance graduation requirements could be adding the Common Core. However, all of these adoptions have been post 2010, with most early adopters fully adopting after 2012, giving us only one year of post-policy implementation to study in the NPSAS data. We do not think these policies will be confounding our effects.

[21] This was retrieved on December 22, 2016.

Table E.1—: Effects of Math Requirements on Financial Aid

Panel A

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Sub Stafford $s	(5) Unsub Stafford $s	(6) Took Max
Math Credits	-0.007	0.005	-0.025	-22.052	-28.306	-0.018
	(0.007)	(0.010)	(0.022)	(59.885)	(66.848)	(0.025)
PF	0.030*	-0.025^{+}	0.044^{+}	88.430	20.788	0.040^{+}
	(0.015)	(0.015)	(0.023)	(68.330)	(87.999)	(0.023)
N	19,557	19,557	19,557	19,557	19,557	19,526

Panel B

	(1) Only Loans	(2) Outside Aid	(3) Private Loan	(4) Private Loan $s	(5) Have CC Balance	(6) Work while Enrolled
Math Credits	-0.011**	0.002	-0.009	16.202	-0.007	-0.001
	(0.003)	(0.010)	(0.007)	(45.403)	(0.005)	(0.010)
PF	-0.001	0.025^{+}	0.005	-71.193	-0.023**	-0.021
	(0.006)	(0.013)	(0.007)	(71.085)	(0.008)	(0.016)
N	19,557	19,557	19,557	19,557	19,557	19,557

Notes: Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regressions includes state and year fixed effect and all covariates listed in Table 3.

Table E.2—: Effects of Highest Math Requirements on Financial Aid

Panel A

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Sub Stafford $s	(5) Unsub Stafford $s	(6) Took Max
Highest Math	0.002	-0.002	-0.009	-65.312	-27.133	-0.008
	(0.005)	(0.007)	(0.015)	(42.662)	(75.033)	(0.014)
PF	0.032*	-0.026+	0.048*	95.467	-21.501	0.041+
	(0.013)	(0.014)	(0.024)	(58.904)	(78.237)	(0.022)
N	23,093	23,093	23,093	23,093	23,093	23,058

Panel B

	(1) Only Loans	(2) Outside Aid	(3) Private Loan	(4) Private Loan $s	(5) Have CC Balance	(6) Work while Enrolled
Highest Math	-0.004	-0.024***	-0.007	-24.586	-0.008*	-0.007
	(0.005)	(0.006)	(0.005)	(46.995)	(0.003)	(0.011)
PF	0.000	0.018+	0.001	-132.497+	-0.021**	-0.022
	(0.006)	(0.010)	(0.007)	(70.859)	(0.008)	(0.015)
N	23,093	23,093	23,093	23,093	23,093	23,093

Notes: Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regressions includes state and year fixed effect and all covariates listed in Table 3.

Table E.3—: Effects of Total Course Requirements on Financial Aid

Panel A

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Sub Stafford $s	(5) Unsub Stafford $s	(6) Took Max
Total Credits	-0.003***	0.004***	-0.011***	-17.299*	-20.006**	-0.013***
	(0.001)	(0.001)	(0.002)	(7.317)	(6.477)	(0.002)
PF	0.028^{+}	-0.023	0.043^{+}	90.927	3.838	0.033
	(0.014)	(0.014)	(0.023)	(68.916)	(86.568)	(0.021)
N	20,018	20,018	20,018	20,018	20,018	19,987

Panel B

	(1) Only Loans	(2) Outside Aid	(3) Private Loan	(4) Private Loan $s	(5) Have CC Balance	(6) Work while Enrolled
Total Credits	-0.002***	-0.002	-0.003***	-9.333^{+}	-0.001^{+}	-0.001
	(0.001)	(0.001)	(0.001)	(4.981)	(0.000)	(0.001)
PF	0.001	0.021^{+}	0.003	-76.742	-0.023**	-0.026
	(0.007)	(0.012)	(0.007)	(68.956)	(0.008)	(0.016)
N	20,018	20,018	20,018	20,018	20,018	20,018

Notes: Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each regressions includes state and year fixed effect and all covariates listed in Table 3.

Table E.4—: Effects of ACT or SAT Requirements on Financial Aid

Panel A

	(1) Applied for Aid	(2) No Aid	(3) Have Stafford	(4) Sub Stafford $s	(5) Unsub Stafford $s	(6) Took Max
ACT Reqd	-0.041***	0.037**	-0.028	-111.548	42.660	-0.039
	(0.007)	(0.013)	(0.069)	(168.892)	(127.063)	(0.062)
PF	0.031*	-0.031*	0.054*	94.420	18.644	0.045*
	(0.013)	(0.013)	(0.022)	(63.162)	(78.597)	(0.021)
N	25,354	25,354	25,354	25,354	25,354	25,319

Panel B

	(1) Only Loans	(2) Outside Aid	(3) Private Loan	(4) Private Loan $s	(5) Have CC Balance	(6) Work while Enrolled
ACT Reqd	0.001	-0.060	-0.013	46.153	-0.012	-0.002
	(0.015)	(0.047)	(0.029)	(111.189)	(0.015)	(0.018)
PF	-0.012^{+}	0.012	-0.005	-167.320*	-0.024*	-0.012
	(0.007)	(0.012)	(0.007)	(69.740)	(0.009)	(0.014)
N	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 regressions includes state and year fixed effect and all covariates listed in Table 3.