# The Effects of Financial Education on Student Financial Aid Choices

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*Students entering college have limited financial experience while making complex borrowing decisions. This paper examines a policy lever that may improve these decisions: required personal finance education in high school. Our difference-in-difference strategy compares students experiencing requirements based on their state and year of graduation. Using data on financial aid decisions made by college freshmen at four-year institutions from the National Postsecondary Student Aid Study, the results suggest graduation requirements increase aid applications and federal loans, while decreasing private loans and the likelihood of holding credit card balances. The mandates do not change institutional choices or college attendance.*
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## I. Introduction

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

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asks how education in high school that builds specific financial abilities and skills affects student loan aid packages.

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

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

Increasingly, states have required high school students to meet graduation standards in personal finance to improve financial literacy among young adults. Currently, 25 states require students to cover personal finance topics prior to graduation, including material on interest rates, saving, credit, debt, and income. Some state personal finance standards include financing postsecondary education as an explicit component. This paper is the first to causally determine the effect of financial education graduation requirements in high school on financial aid decisions in college.

Personal finance content may improve access to credit and choices of loan packages, particularly in states where completing the FASFA is part of the curriculum. To the extent that borrowers learn to shop for interest rates, this policy could shift students from private loans and credit cards toward lower cost public loans. If the content emphasizes investing up front in searching for scholarships and grants or makes students more debt-averse, it could reduce debt in favor of grant and scholarship support. In the event that students are already optimally choosing financial aid packages, the added information in personal finance coursework would not be expected to affect student behavior.

The analysis in this paper draws on a panel of cross-state comparisons, using a difference-in-difference approach to compare students from states with personal finance graduation requirements before and after implementing the requirement to comparable students whose states lack such a mandate. We use the National

[1] Kantrowitz (2009) estimates that nearly 2.9 million undergraduate students would have qualified for Federal Pell Grants in 2007-2008 but did not apply.

Postsecondary Student Aid Study (NPSAS) 1999 through 2011 waves to draw on a rich set of outcomes, including whether students applied for financial aid, whether they accessed federal and private student loans, grants and scholarships, whether or not students carry a credit card balance, and whether or not students work while enrolled.

Our study contributes to three strands of literature. First, we contribute to a growing literature that investigates interventions surrounding financing higher education. The fact that student loan decisions are complex is not a novel concept (Dynarski and Scott-Clayton 2006). In a seminal study, Bettinger et al. (2012) show that randomly assigning personalized help in filling out the FAFSA increases the likelihood of completion, increases the amount of federal dollars received, and increases the probability of enrollment.[2] This has fueled a number of efforts designed to provide students with better information about loans and with improved architecture for making financing choices (Marx and Turner 2016; Castleman and Page 2016; Castleman and Long 2016; Stoddard, Urban and Schmeiser 2017; Darolia 2016; Bettinger and Long 2017). To the best of our knowledge, this is the first paper to study the effects of a state-based policy on financial aid behaviors, rather than one based on specific education institutions.

Second, we contribute to the literature examining the impact of financial education on knowledge, credit, and debt. This literature finds that well-implemented high school courses improve financial knowledge (Tennyson and Nguyen 2001; Walstad, Rebeck and MacDonald 2010). Urban et al. (2014) find that personal finance graduation requirements increase credit scores and decrease severe delinquencies for 18-21 year olds, and Brown et al. (2016) find that financial literacy exposure reduces non-student debt, increases credit scores, and improves repayment for those under 30.[3,4] Both use data from the New York Federal Reserve Bank/Equifax Consumer Credit Panel. Brown et al. (2016) further find that financial literacy reforms increase student loan debt, making it the most closely related to our study. We differ from Brown et al. (2016) in three key ways: (1) we focus on a broad portfolio of initial financial aid decisions, as opposed to the magnitude of student debt, (2) we study graduation requirements rather than more general financial education reforms,[5] and (3) our data contain individual-level demographic characteristics that are not observable in credit report data,

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

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

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

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

allowing us to explore heterogenous responses.

Third, we broadly contribute to a larger literature understanding the role of credit constraints in debt decisions. The last several decades have witnessed a high and rising rate of return to college degrees (Goldin and Katz 2009; Oreopoulos and Petronijevic 2013), though a sizable body of literature suggests that many students still under-invest in their schooling (Avery and Turner 2012; Cowan 2014; Lochner and Monge-Naranjo 2015, 2011).[6] However, it is unclear how financial education would shape the level of student loan amounts, since some students may be under-investing and others may be over-borrowing to finance their postsecondary educations. This paper does not tackle students' ability to optimally invest in higher education. Instead, we turn the discussion to the specific components of financial aid packages: federal loans, private loans, credit card balances, grants and scholarships, and working while enrolled. Currently, there is less attention paid to helping students identify the optimal mix of these options.

Our main 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 5.2 percent more likely to take out a direct federal Stafford loan. The education reduced private loan balances, and exposed students were 2 percent less likely to carry a credit card balance. The effects on federal aid are strongest for students attending public universities and minority students: students at public schools increased subsidized Stafford loan amounts by $150 on average, while Black and Hispanic students increased their subsidized Stafford amounts by $260 and $300, respectively. At the same time, reductions in private sector borrowing exist only for students attending private institutions and white students. These results are consistent with the fact that private sector borrowing options are most available to relatively advantaged students.

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

The change in financial aid in the NPSAS data is based on students attending a post- secondary institution. With these data, we demonstrate that graduation requirements do not affect the choice of institution conditional on enrollment. Students are no more or less likely to attend a two year as opposed to a four

[6]Some studies show that students are aware of self-control problems, which could be one reason they decline aid (Cadena and Keys 2013; Johnson 2013).

year school, or a public instead of a private school, or a school with relatively lower tuition due to the graduation requirement. We further supplement the main analysis with data from the Current Population Survey (CPS) to show that graduation requirements do not appear to change college enrollment decisions.

## II. Background

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

States often include student loan and financing postsecondary education content explictly 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. Tennessee's state standards include the following content: "Demonstrate an understanding of Free Application for Federal Student Aid (FAFSA) requirements to apply for postsecondary education financial aid by completing an application. Identify strategies for reducing the overall cost of postsecondary education, including the impact of scholarships, grants, work study, and other assistance." The Texas standards are perhaps the most obvious example of a clear channel through which financial education can affect student loan decisions. The Texas State Board of Education requires that all students "understand the various methods available to pay for college and other postsecondary education and training." The standards include requiring that students understand how to complete the FAFSA; research and evaluate scholarship opportunities; compare student grant options; analyze student loan options; evaluate work-study options; investigate nontraditional methods of paying for postsecondary education. We study each of these outcomes directly in our analysis.

Table 1 lists the states with personal finance graduation requirements, as well as the year in which the first graduating class was required to complete the material. This classification is relatively conservative, as some states will require a full stand-alone course in personal finance, while others require that the material be taught within another subject, such as social studies, math, or economics. In all cases, states have documented personal finance requirements in graduation standards.[7] Figure 1 provides a map of the requirements based on the rigor of the standards. Less rigorous states are states with requirements to offer electives

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

on personal finance, but where there is not an explicit graduation requirement.[8] It shows that there is no clear geographic pattern in either the implementation or form of these policies.

## III. Data

We draw on data from the NPSAS to determine the causal effect of financial education on student aid decisions. The NPSAS is a nationally representative study of students enrolled in institutions of higher education. It contains detailed data on financial aid extracted from institutional data, along with student and parent interview responses about demographics, high school degree, family background, private loans, credit card balances, and work.[9] We use data from the 1999, 2003, 2007, and 2011 waves of this survey, since outcome measures are consistent across years and most states implemented financial education requirements after 2000.

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 they would not be exposed to the personal finance curriculum; this eliminates students with GEDs, students who were homeschooled, students who did not have a high school certificate, and students who graduated from a foreign high school. We also exclude students at for-profit institutions as there are too few in these waves to separately identify effects for this subgroup. 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. We focus on students enrolled at four-year institutions, as tuition and aid packages tend to be larger and more consistent across institutions at this level.[10]

Table 2 shows the characteristics of the sample by state personal finance requirement. Across the states, over 90 percent of students apply for some type of aid. Over half (55%) of students have a Stafford loan, which is substantially

[8] This also includes Wyoming, which had one short and vague requirement in its state standards: "Explain the roles and effect of money, banking, savings, and budgeting in personal life and society."

[9] While the federal loan data are administrative, the private loan data are based on surveys. While this survey data may contain errors in the amount of private loans, we have no reason to believe the errors will be related to our policy of interest.

[10] We find no effect of personal finance education on student loans for a sample of two-year universities. These results are available on request. Note that our sample is restricted to students directly enrolling in college after high school, and this is not a representative of two-year students (median age of 24) who tend to be older than four-year students (median age of 21).

higher than the 11 percent of students that have private loans. Average private loan amounts are also smaller than Stafford loans, $800 when compared to nearly $2,300 in total Stafford unsubsidized and subsidized loans combined, although these averages include students with no loans. Slightly less than half (45%) of students work while a college freshman in some capacity, and about 10 percent hold a balance on a credit card in their freshmen year. The NPSAS sample is roughly 55 percent female, 70 percent white, and just over 18 years of age, with 97 percent of students dependents. Expected Family Contributions (EFCs) are roughly $14,700 on average, meaning parents potentially are able to contribute roughly that amount annually.[11] About 20 percent of students have parents without any college education.

While the bulk of the analysis relies on the NPSAS, Section VII uses data from the Current Population Survey (CPS) to show that financial education requirements do not affect students' decisions to enroll in college. Finally, Section VIII employs data from the Montana University System (MUS) to examine how elective financial education courses affect financial aid decisions. These data supplement the main analysis.

## IV. Empirical Strategy

This paper uses a difference-in-difference model to determine the causal effect of financial education graduation requirements on financial aid decisions. We compare students who graduated in states before and after a financial education graduation requirement was implemented to the same difference over time for students from states without graduation requirements. Note that this initial specification classifies states that mandate personal finance be offered but do not have a graduation requiremenl as not having the policy. 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. Standard errors are clustered at the state level, as the policies under consideration are state specific. In all specifications, we include state fixed effects to account for differences in financial aid and higher education policies that are consistent within a state over time and year fixed effects to account for federal changes in higher education financing.

Our difference-in-difference specification estimates Equation 1.

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

We estimate Equation 1 for a suite of dependent variables $Y_{i,s,t}$ that capture characteristics of the financial aid package for individual $i$ with permanent res-

[11] 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.

idency in state $s$ entering college in year $t$. These outcomes include whether a student applied for financial aid, accepted any financial aid, the specific components of the federal Stafford loans obtained, whether the package only included loan aid (as opposed to also including grants and/or scholarships), private loans balances, credit card balances, and the decision to work while enrolled.

Our independent variable of interest, $\text{PF}_{i,s,t}$, equals one if individual $i$ in state $s$ graduated from high school in a year $t$ after the state mandated a personal finance graduation requirement. Thus, this variable captures a binding personal finance requirement for the specific student.

Equation 1 includes a rich set of individual-level characteristics ($\boldsymbol{X_i}$), including an indicator for male students, dummies for white, black, and hispanic demographic groups, age dummies, and dummy variables for parental education groups. We also include a dummy variable for whether or not a student is a dependent for the purposes of financial aid, although this is true for 97 percent of the sample. Our specifications also include either the expected family contribution (EFC). The EFC is based on measures related to income, assets, and the enrollment in higher education of other family members. As such, it captures family income and wealth and any correlated factors, such as preferences, depth of financial knowledge, or level of access to credit markets. It also determines eligibility for need-based aid at both the federal and state level.[12]

## V. Results

Table 3 reports the difference-in-difference effects of personal finance graduation requirements on major financial aid choices at four-year institutions. The first two columns focus on the discrete decision to apply for and to accept federal aid. The dependent variable for Column 1 is a binary variable equal to one if the student applied for aid,[13] and the dependent variable for Column 2 is a binary variable equal to one if the student accepted a direct federal Stafford loan. The dependent variables for Columns 3 and 4 are the specific dollar amounts of Stafford subsidized and unsubsidized loans, respectively, and include non-borrowers as zeros.[14] Column 5 indicates whether or not a student had only loan aid in their package, as opposed to also having grants and/or scholarships. Column 6 includes results using a binary variable equal to one if the student took out a private loan to finance their education, while Column 7 reports results for the balance on

[12]It could be that the financial crisis affected parents' ability to pay for college and happened at a time when states were implementing financial education requirements. However, if we control for state unemployment rates by year, there is no change in our coefficient on personal finance requirements. These results are available on request.

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

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

that private loan (including zeros). The dependent variables for Column 8 and 9 are binary variables equal to one for students who self-report carrying a positive credit card balance and whether or not the student worked while enrolled in school, respectively.

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

Columns 5-9 in Table 3 report the effect of personal finance graduation requirements on non-federal aid decisions. Students exposed to the graduation requirements were no more or less likely to accept a package that only contains loans.[15] This could be because students obtain scholarships that are not given directly to schools, but are paid directly to students, such as Rotary Club Scholarships. While there is no difference in holding private loans, borrowers in states with graduation requirements had $150 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 2.1 percent less likely to carry a credit card balance.[16] A survey across college campuses shows that only 9.4 percent of students with credit cards pay their balance in full each month, leaving the remainder with interest and late fees (Ludlum et al. 2012). The authors also find that there are information problems, where 75 percent of students are unaware of late fee charges on their credit cards. Our results suggest that financial education can improve decisions around credit cards for college students, suggesting this is one potential policy lever to reduce mistakes that may be due to information gaps. Finally, Column (9) reports that financial education does not change individuals' self-reported decisions to work during college.

To be sure that the specifications are not driven by our particular measure of income, we estimate models including either tuition or family income instead of

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

[16]We see no difference in the amount of credit card balances, though the means of these balances change substantially across survey waves raising concerns about consistent measurements and leading the effects to be less precisely estimated.

EFC. We further estimate the model examining only dependent students and only full-time students. All of these results are consistent with the main findings.[17]

## A. Heterogeneity by School and Demographic Characteristics

For whom are these personal finance requirements most likely to affect behavior? We begin by first looking at students at different types of institutions.[18] Table 4 reports the $\alpha_1$ coefficient from Equation 1 for the subgroup listed on the left. The coefficient estimates are bolded where the coefficient for the sub-group is statistically different from the average effect.

The top panel of Table 4 divides students into two classes of institutions: public four-year and private four-year.[19] The results indicate that personal finance graduation requirements have economically and statistically significant effects on public four-year university students: nearly every point estimate is larger in magnitude in these estimates than in the baseline results. However, they are not always statistically different from the average. One exception is that public students are more responsive to financial education when it comes to carrying a credit card balance, where the point estimate is twice as large as the average. They are also less likely to be working while in school, suggesting that the additional federal aid may be used to both reduce private borrowing on credit cards and replace work for some students. Though it is not statistically different from the average effect, the decrease in private student loan borrowing appears to come entirely from private school attendees. This could be because students attending private school (or their parents) have greater access to other types of credit, include second mortgages.

The next two panels of Table 4 report significant heterogeneity in the effects across important sub-groups, including sex and race or ethnicity (white, black, or Hispanic). Male students appear to be more responsive than female students in terms of applying for aid. However, female students have higher rates of applications to begin with: 93 percent of female students and 90 percent of male students apply for aid. Females decrease the likelihood of carrying a credit card balance and decrease private loan amounts due to the education, though the latter is not statistically different from the average effect. Black students do not appear to respond to graduation requirements by increasing applications for aid, although roughly 99 percent of Black students apply for aid, leaving no scope for an increase. Black and Hispanic students increase their subsidized Stafford loan amounts more than the average due to the graduation requirement, and Black students increase their unsubsidized Stafford loan amounts. This is regardless of the fact that Black students have higher levels of subsidized and unsubsidized Stafford

[17] These results are available upon request.

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

[19] There are too few students in early waves at for-profit four year institutions to include this subgroup.

loans on average, $2,009 and $1,696, respectively. This is also consistent with the potential lack of information about these opportunities among disadvantaged students. White students, in contrast, have the largest responses for reduced private loan amounts and reduce unsubsidized Stafford loans due to the education. To the degree that white students have more access to private loan markets, this may contribute to their greater responsiveness along these margins.[20]

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

## B. Heterogeneity by State Requirements

The results presented so far are averages across students from states requiring exposure to personal finance prior to high school graduation. This masks 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 trains teachers, funds the requirement in schools where teachers are properly certified, and gives sample evaluations for teachers to use. In contrast, Wyoming requires personal finance topics be covered in the Social Studies curricula, but it does not have specific content requirements. 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 that all students take the course.[21] 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.[22]

In the main analysis, states requiring schools to offer an elective course but not requiring students to take it or specifying a graduation requirement are classified as zeros for the initial PF indicator. If there are effects on student aid choices 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 coded as having a personal finance requirement, as content is required to be completed prior to finishing high school, although the

[20] In unreported results, we also examine subgroups based on EFC, and similarly find a relatively large decrease in private loans for high EFC students, consistent with greater responsiveness for students with more access to private credit.

[21] In South Dakota, students are required to take *either* Economics or Personal Finance. We thus count this as an elective.

[22] In our baseline analysis, we count all four as having personal finance. In the event that these programs are not enforced, this would bias us against finding an effect.

content standards are minimal. This again likely biases the baseline estimates towards zero, this time because the "treatment" was minimal.

We continue by using alternative classifications, this time dividing treated states as those with more or less rigorous mandates following Urban et al. (2014). Figure 1 maps these definitions across states. Accordingly, Table 5 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 results in Table 5 indicate that more stringent laws do indeed have a larger effect on aid decisions, both in terms of the general pattern of the magnitude and significance of the coefficients. The magnitude of the effects of rigorous laws is roughly the same in Table 5 as the baseline results in Table 3, although the standard errors are smaller than the baseline effects. One exception is that states implementing less rigorous policies had a slightly larger effect on reducing private loan debt, though neither effect is statistically different from the baseline average. Students in states with less rigorous policies see no statistically significant effect on aid applications, Stafford loans, or prevalence of non-loan aid. This suggests that specific graduation standards are a more effective mechanism in influencing student aid decisions.

Most states passed personal finance mandates after 2000. Four states passed an early version of personal finance graduation requirement in 1998 or earlier,[23] 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, we confirm that our results are robust to dropping these states.[24] Our results are robust to excluding states that mandated personal finance be taught but allowed school district or county flexibility in the way the mandate was implemented, leading to variation in the timing and stringency of the requirement across the state.[25]

## VI. Threats to Internal Validity

In this section, we address two threats to internal validity: (1) testing that the trends in the treatment and the control groups are parallel in the pre-treatment period and (2) ensuring that the policy is not endogenous to other state education policies.

### A. Testing for Parallel Trends

Difference-in-difference strategies assume that the treatment and control groups would have had parallel trends in the absence of the policy. This assumption is

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

[24] These results are available upon request.

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

required for the non-treatment group to represent a proper counterfactual. This assumption is commonly tested by examining the periods prior to the implementation of the policy. However, the NPSAS data are not collected annually, but rather every three to four years. Further, the survey measures change somewhat over time, making it more challenging to provide a formal test of pre-trends.

Accordingly, we use several strategies to address the issue of parallel trends. First, we verify that students graduating from high school in the years prior to the binding financial education requirement were not affected by the future mandate. This could have been the case if either financial education content was incorporated in a wide spread manner prior to the mandate, or if financial aid packages in these states were already changing prior to the mandate. For each state, student requirements took effect between two NPSAS reporting years. Thus, the previous NPSAS waves can act as a control group: students completing high school prior to the year the graduation requirement took effect in a state with a requirement in the subsequent wave should not be affected by the policy. Put another way, those who were high school freshmen in Utah in Fall of 2007 were required to complete personal finance content before they graduated in Spring of 2011. Sophomores, juniors, and seniors in the Fall of 2007 would act as the "previous wave" for this group. Those who exited high school prior to 2007 would serve as two periods prior as they will be two waves earlier in the NPSAS sample.[26]

Table 6 reports results based on this classification. These results consider only observations without graduation requirements–that is, observations in either control states or graduates in waves prior to the state mandate. Across all specifications, we find no statistical differences in coefficients. We reiterate that this is identified from states implementing in 2011 and later.

Next, Table 7 uses data from earlier waves of the NPSAS (1989, 1992, and 1995) in addition to the later data (1999, 2003, 2007, 2011). This limits the outcome measures, as all of the variables were not collected throughout the full sample or were not collected consistently. This variation of Equation 1 includes all control states and three pre-treatment waves for nearly all states that implement, excluding two early implementers: Illinois and New Hampshire. Table 7 indicates that only one coefficient is statistically different from zero at the ten percent level. As there are fifteen tests, we expect that at least one of these will be statistically different at the ten percent level.

We further provide a placebo test in Appendix Table A.1. This analysis uses the older NPSAS sample from 1989-2003, again preserving the variables that have been consistent over time. The variable $PF_{-10}$ is equal to one if an individual was 18 in a state within the 10 years prior to the time the graduation requirement became binding in that state. Here, we estimate that the falsely placed policy

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

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 graduation requirements, further validating our results.

Finally, Appendix Table A.2 uses the 1989-2011 data with the same variables and shows that results remain consistent if we include a state-specific linear time trend to our main specification. We do not include these state-specific trends in our main analysis, as there are too few waves in the 1999-2011 period with which to consistently identify a linear time trend.

## B. Policy Endogeneity

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

First, do states that pass mandates have fundamentally different economic contexts at the time of passage? If economic conditions change in a way influences both passage of a mandate and student aid decisions, the estimated treatment will conflate economic conditions with the personal finance requirement. We formally test the correlation between state-level economic conditions and personal finance requirements using data from the University of Kentucky's Poverty Center (2016) and our personal finance requirement database. We estimate Equation 2.

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

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

Table 8 shows the results from estimating Equation 2. In Column (1), none of the variables are predictive of any personal finance graduation requirement. In Column (2) states with greater population inflows are more likely to have rigorous personal finance requirements, though in terms of economic magnitude, this is small.[27] Column (3) reports that a one unit higher unemployment rate decreases

[27] The inclusion of state fixed effects implies that increasing a state's population by 1 million residents in a given year increases the probability of having a rigorous graduation requirement by 16 percentage points. From 2010-2011, the average population growth within states was 46,509. That average increase would change the probability of adopting a rigorous personal finance requirement by a modest 0.7 percentage points.

the rate of adopting a less rigorous personal finance program by 2 percentage points. Across the remainder of Columns (2) and (3), there are no coefficients that are economically significant, as most coefficients are estimated as tight zeros.

Second, do states pass financial education graduation requirements at the same time as other graduation requirements that might also affect student borrowing decisions? We examine four such 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).[28]

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[29] 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.[30] States that have no statewide policies but rely on local school boards to determine graduation requirements are omitted from the analysis. We identify states with current policies using ACT and College Board reported data, supplemented with the Education Commission of the States (ECS) State Policy Database.[31]

We explore the sensitivity of the baseline results (reported in Table 3) to the inclusion of these policies. Table 9 indicates that when controlling for total credits required, total math credits required, the highest level of math required, and college entrance exam requirements, the coefficient on personal finance education ($\alpha_1$) remains remarkably stable.

Finally, concern may arise that states are taking on other education policies affecting higher education at the same time as personal finance education. We investigate two such policies: implementing automatic in-state scholarships and the level of state appropriations for higher education.[32] Table 9 shows that controlling for state scholarship programs does not change the effect of financial education requirements on financial aid. Similarly, accounting for changes in higher educa-

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

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

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

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

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

tion spending by state over time also does not change the overall effect of financial education on financial aid. The sample for Table 9 is restricted to public institutions as they receive the public funds. Thus, the relevant comparison table is Table 4. Overall, then, we find no evidence that the estimates are influenced by other state economic conditions, high school graduation requirements, or higher education policies.

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

The results so far suggest that personal finance graduation requirements generally increase both student applications for aid and the use of low cost borrowed funds (e.g., Stafford loans), while simultaneously 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 influence whether or not students enroll in higher education. For example, if students who complete personal finance material become more concerned about college costs, they might be more likely to attend a two-year school than a four-year institution, more likely to attend a public than a private school, more likely to chose a school with lower tuition, or less likely to attend college at all. If these choices reduce the ideal match between students and schools, these policies may have negative unintended consequences in terms of lifetime income.

Using NPSAS data, we can address the likelihood of observing an enrolled student at different types of institutions. Table 11 reports results for the effect of personal finance graduation requirements and other demographic characteristics on institutional choices: whether or not a student enrolled at a private, public or for-profit four year institutions (conditional on enrollment at a four-year institution), the tuition and fees paid at the four year institution, the likelihood that the student stays in-state for postsecondary education, and whether or not the student enrolled in a four-year, as opposed to a two-year, college. Across each of these outcomes, personal finance graduation requirements do not appear to play a role in what type of institution the student attends. The $\alpha_1$ coefficients are small and imprecisely estimated, with none of the estimates approaching statistical significance at even the 10 percent level.

Finally, in Appendix B we examine college enrollment. The NPSAS data include only enrolled students, so we turn to CPS data to examine whether personal finance education requirements change individuals' decisions to attend college. We include the sample of individuals aged 18-20 over the period 1995-2013. 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.

## VIII. Offering Financial Education Electives

Even in states where personal finance graduation requirements do not exist, high schools have the autonomy to offer a course. We seek to estimate the effect of personal finance courses when enrollment is optional. We examine this question in a state without a mandate, relying on local variation in personal finance course *offerings* to determine the intent-to-treat effect of personal finance courses on aid packages. This detailed analysis informs the previous state-based analysis in two ways. First, it indicates how a less stringent requirement for schools to offer an elective course in personal finance may influence average financial aid packages. Second, it helps us measure whether the effect found in the NPSAS analysis is likely to be a lower bound of the true effect of financial education. If courses are offered in states without mandates or in states with mandates prior to their passage, the initial analysis will understate the effect of financial education on financial aid packages.

This analysis uses administrative data from the 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 amounts of federal and state loans. However, these data do not include information on private loans.[33] It is worth noting that while Montana is a relatively low income state, average student debt levels, tuition as a fraction of state personal income, graduation rates, and Pell grant levels are similar for Montana and the nation as a whole.

Our data span the years 2002 through 2014, or 36 semesters of data. 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 high school in the state directly to determine whether or not they offered a stand-alone personal finance course and in what years. We use administrative transcript data from the Office of Public Instruction to confirm that students generally take these courses in their junior or senior year,[34] and we match students based on their age to whether the course would have been offered during their high school years or not. We only include first semester freshmen's aid packages to parallel our previous results.

We include in our analysis high school fixed effects, year fixed effects, and individual characteristics in all of our models such as a white and missing race

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

[34]Unfortunately, the transcript data are only available from 2012-present, and the higher education financing data are only available through 2012.

dummy, age dummies, a male indicator, ACT scores,[35] and campus dummies. We are careful to cluster our standard errors at the high school level as this is where policies vary.

Table 10 reports the results, where offering financial education had minimal effect on students. However, Column (3) does indicate that students are now decreasing the likelihood of only having loans; instead they are more likely to have some form of non-loan aid, in the form of grants and scholarships. This could make students slightly less reliant on non-federal loans. However, there is no statistical difference in the amount of non loan aid received. Thus, the increase in grants and scholarships is likely too small to make a substantive difference. We find no evidence that personal finance education offerings change subsidized or unsubsidized federal loan amounts.[36]

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 B.1, where we compare all of our dependent and independent variables by whether or not a school ever offered a personal finance class. Note that this does not take into consideration the timing of adding the course. Table B.1 shows that there are no clear differences in financial aid packages across the two groups. Student-level characteristics are not notably different across the two groups. Thus, it is reasonable to assume that adding personal finance as an elective is idiosyncratic across schools. We further verify that there are no pre-trends in our outcome variables based on the course offering in Table B.2, as this is an assumption of the difference-in-difference model. The results in Table B.2 suggest that there are no clear pre-trends in course offerings, where those who graduated 1 through 7 years before the course was first offered in the school have no differences in outcomes when compared to those graduating 8 or more years before the course was offered. The coefficients on PF Offering $-1$ through PF Offering $-7$ are also not statistically different from each other, confirming there are no clear trends.

## IX. Discussion

There are several mechanisms through which personal finance education may influence student aid choices, though unfortunately, our data do not allow us to test for each of these channels directly. First, the education may ameliorate gaps in financial knowledge, where learning how to compare interest rates, finance and pay back long-term debt, and calculate credit scores may influence students to rely less on credit cards and more on federal aid. As most of the education programs emphasize budgeting, students exposed to financial education may be less likely

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

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

to choose the default loan option in favor of one that is more customized to his or her spending needs. Second, literature suggests that there are many mistakes students make in filling out the FAFSA. If curricula include this component, the education may help simplify the FAFSA process, while making its benefits, when compared with the up-front costs of completing it, more salient.

Our study finds results that are complementary to other. First, Brown et al. (2016) find that personal finance coursework is associated with an increase in total student debt of roughly $161 for 22 year-olds, though this is not statistically different from zero.[37] While they study debt at age 22, which is when students are likely to exit school, we study financial aid decisions at age 18, upon entering college. However, our results flesh out this finding. In our data, there is an uptick of federal loans, though it is in conjunction with a reduction of private loans. This combination is consistent with the Brown et al. (2016) statistically insignificant effect for total student loans. We are also able to add to this by further understanding how financial education affects specific subgroups of interest, where our data indicate that increases in public loans come from students attending public universities and racial minorities, while decreases in private loans come from students attending private schools and white students.

The findings also complement those of Bettinger et al. (2012). Bettinger et al. (2012) study two types of interventions: provision of information about costs and one-on-one FAFSA assistance. While financial education in high school also provides general information and in some cases assistance for FASFA completion, these courses focus more on teaching a broader suite of financial concepts and skills. Though some states incorporate the FAFSA directly into the standards, one-on-one assistance is not part of the curricula. Our findings of increased applications and federal aid, especially for populations that might have the most difficulties with the FAFSA, parallels the results of Bettinger et al. (2012). However, while their study finds that one-on-one assistance increases enrollment for dependent high school seniors by 8 percent, we do not find that broader financial education changes enrollment. This lack of impact is similar to the results from the alternative intervention in Bettinger et al. (2012), where providing more information on the costs of college also did not change enrollment. Together, these studies imply greater responsiveness in terms of applications and aid packages, but suggest that changing enrollment may require more targeted (and costly) attention.

## X. Conclusions

Student loan reform has been a pressing policy topic for the last few years. The reforms and interventions motivated by this have largely taken place at specific higher-education institutions after acceptance or enrollment in college. These

[37]The authors find increases in student loan debt for 25 and 27 year-olds.

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 decrease 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).

Rather than focusing on reforms that require institutional participation, we examine broad state-wide policies that affect all high school students and inform student aid decisions in the interface between high school and college. Our results show that high school financial education graduation requirements can significantly impact key student financial aid behaviors. These mandates increase the likelihood that students apply for aid and the amount of federal student loan aid students receive. At the same time, these requirements decrease private loan amounts, and decrease the likelihood of carrying a credit card balance.

Current research does not allow for an exact comparison of costs and benefits of this policy with other interventions. In Bettinger et al. (2012), costs per participant were roughly $87.50. In contrast, the costs of financial education requirements primarily stem from the opportunity cost of displacing other courses or content. We argue that these costs are likely to be low, as in many states it appears that schools incorporate personal finance concepts into already-existing courses. However, we leave it to future research to determine what other courses or content might be displaced by explicit requirements. In assessing the benefits, we note that high school personal finance is geared more towards building general skills than to the single financial aid decision. As a result, the benefits of this curriculum extend beyond those under study here, especially as previous literature finds that this type of high school education also reduces non-student debt, increases young adult credit scores, and decreases severe delinquencies (Urban et al. 2014; Brown et al. 2016).

Recent policies have focused primarily on the financial aid application process, either by simplifying the FAFSA, syncing the process more closely with IRS tax data, or requiring FASFA completion prior to graduation for all students.[38] The broad set of impacts of financial education mandates are suggestive of the role of informational policies in contributing to a range of improved financial decision making among young adults. As states continue to experiment with ways to decrease financial barriers to college attendance, future research should continue to study the comparative costs and benefits of competing policies that aim to simplify financial aid access and strengthen students' decisions.

[38]Louisiana plans to mandate this for 2018 graduations. See `https://www.usnews.com/news/articles/2015-12-22/louisiana-to-require-students-to-fill-out-fafsa`.

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Tables and Figures

Figure 1. : Financial Education Requirements

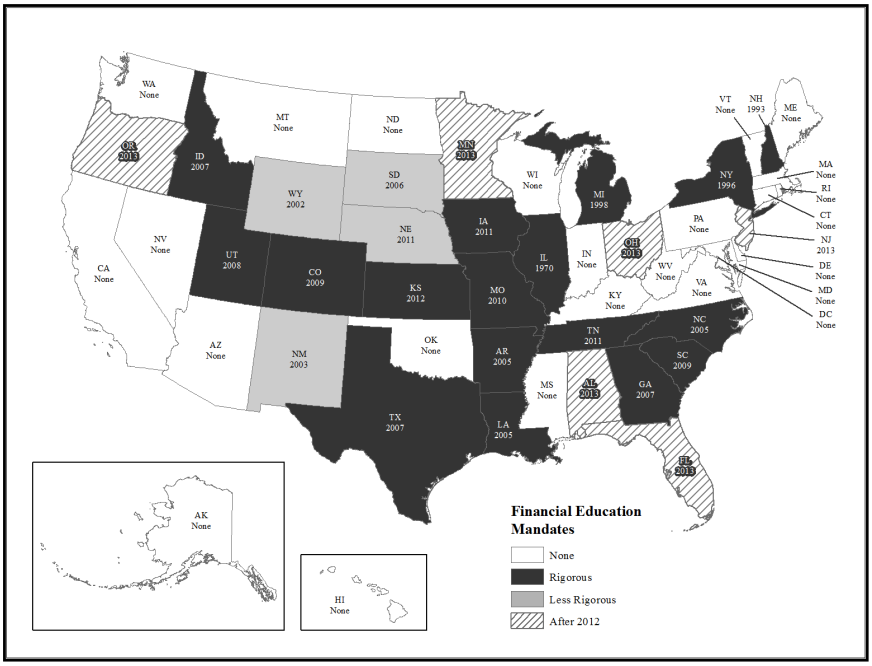

Table 1—: States with Personal Finance Graduation Requirements

| State | First Graduating Class Affected | State | First Graduating Class Affected |
|---|---|---|---|
| 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 | | |

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

Table 2—: Summary Statistics by Financial Education Status

| | No PF | PF Required | Both |
|---|---|---|---|
| Dependent Variables | | | |
| Applied for Aid | 0.907 | 0.934 | 0.915 |
| | (0.291) | (0.248) | (0.279) |
| Stafford Loan | 0.540 | 0.599 | 0.558 |
| | (0.498) | (0.490) | (0.497) |
| Subsidized Stafford $s | 1,195 | 1,464 | 1,275 |
| | (1,488) | (1,598) | (1,526) |
| Unsubsidized Stafford $s | 912 | 1,232 | 1,007 |
| | (1,601) | (1,839) | (1,681) |
| Only Loans | 0.062 | 0.048 | 0.058 |
| | (0.242) | (0.213) | (0.234) |
| Private Loan | 0.111 | 0.120 | 0.114 |
| | (0.314) | (0.325) | (0.317) |
| Private Loan $s | 782 | 852 | 803 |
| | (3,034) | (3,133) | (3,064) |
| Have CC Balance | 0.096 | 0.094 | 0.095 |
| | (0.295) | (0.292) | (0.294) |
| Work while Enrolled | 0.468 | 0.420 | 0.454 |
| | (0.499) | (0.494) | (0.498) |
| Independent Variables | | | |
| Male | 0.442 | 0.441 | 0.442 |
| | (0.497) | (0.497) | (0.497) |
| White | 0.732 | 0.657 | 0.710 |
| | (0.443) | (0.475) | (0.454) |
| Black | 0.097 | 0.150 | 0.113 |
| | (0.296) | (0.357) | (0.316) |
| Hispanic | 0.091 | 0.120 | 0.100 |
| | (0.288) | (0.325) | (0.299) |
| Age 17 | 0.0080 | 0.009 | 0.008 |
| | (0.088) | (0.095) | (0.090) |
| Age 19 | 0.364 | 0.319 | 0.351 |
| | (0.481) | (0.466) | (0.477) |
| Dependent | 0.974 | 0.971 | 0.973 |
| | (0.160) | (0.169) | (0.163) |
| EFC (000s) | 14.7 | 14.6 | 14.7 |
| | (18.7) | (19.4) | (18.9) |
| Parent < HS | 0.024 | 0.027 | 0.025 |
| | (0.153) | (0.162) | (0.156) |
| Parent HS Grad | 0.182 | 0.184 | 0.182 |
| | (0.386) | (0.388) | (0.386) |
| Parent Some Coll | 0.204 | 0.222 | 0.209 |
| | (0.403) | (0.416) | (0.407) |

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

Table 3—: Federal Financial Aid Decisions at Four Year Institutions

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

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

Table 4—: Heterogenous Effects of Personal Finance Graduation Requirements

| | (1) Applied for Aid | (2) Sub Stafford $s | (3) Unsub Stafford $s | (4) Only Loans | (5) Private Loan $s | (6) Have CC Balance | (7) Work while Enrolled |
|---|---|---|---|---|---|---|---|
| Four-year Institution Type | | | | | | | |
| Public | 0.040* | 158.041* | 52.375 | 0.000 | -115.319 | **-0.041**** | **-0.046**** |
| | (0.015) | (69.814) | (94.075) | (0.011) | (70.954) | (0.012) | (0.014) |
| N | 14,714 | 14,714 | 14,714 | 14,714 | 14,714 | 14,714 | 14,714 |
| Mean | 0.895 | 1,076 | 905 | 0.084 | 473 | 0.083 | 0.502 |
| Private | 0.014 | 10.63 | -41.61 | -0.001 | -233.47$^{+}$ | **0.009** | **0.035** |
| | (0.015) | (90.20) | (102.82) | (0.007) | (123.86) | (0.013) | (0.023) |
| N | 10,640 | 10,640 | 10,640 | 10,640 | 10,640 | 10,640 | 10,640 |
| Mean | 0.942 | 1,550 | 1,147 | 0.023 | 1,259 | 0.105 | 0.386 |
| Gender Subgroups | | | | | | | |
| Male | **0.062***** | 146.82$^{+}$ | **-70.88** | 0.004 | -50.393 | **-0.007** | -0.020 |
| | (0.018) | (78.96) | (84.16) | (0.010) | (99.057) | (0.011) | (0.022) |
| N | 11,204 | 11,204 | 11,204 | 11,204 | 11,204 | 11,204 | 11,204 |
| Mean | 0.901 | 1,197 | 958 | 0.062 | 827 | 0.087 | 0.432 |
| Female | **0.009** | 75.96 | **60.19** | -0.003 | -237.11** | **-0.035**** | -0.009 |
| | (0.015) | (70.623) | (87.29) | (0.008) | (85.25) | (0.010) | (0.019) |
| N | 14,150 | 14,150 | 14,150 | 14,150 | 14,150 | 14,150 | 14,150 |
| Mean | 0.927 | 1,337 | 1,045 | 0.055 | 784 | 0.102 | 0.470 |
| Racial and Ethnic Subgroups | | | | | | | |
| White | 0.034* | **9.95** | **-121.06** | -0.001 | **-230.70**** | -0.022* | -0.014 |
| | (0.015) | (63.75) | (76.55) | (0.009) | (68.35) | (0.010) | (0.023) |
| N | 17,996 | 17,996 | 17,996 | 17,996 | 17,996 | 17,996 | 17,996 |
| Mean | 0.898 | 1,176 | 983 | 0.062 | 898 | 0.078 | 0.452 |
| Black | **-0.001** | **261.21*** | **452.14**** | **0.022**$^{+}$ | -51.36 | -0.010 | 0.008 |
| | (0.008) | (127.30) | (161.98) | (0.011) | (123.05) | (0.029) | (0.032) |
| N | 2,859 | 2,859 | 2,,859 | 2,859 | 2,859 | 2,859 | 2,859 |
| Mean | 0.986 | 2,009 | 1,696 | 0.048 | 712 | 0.130 | 0.388 |
| Hispanic | 0.040* | **301.55*** | 151.18 | -0.012 | -162.78 | **0.031** | 0.038 |
| | (0.015) | (127.38) | (130.55) | (0.015) | (269.15) | (0.022) | (0.040) |
| N | 2,524 | 2,524 | 2,524 | 2,524 | 2,524 | 2,524 | 2,524 |
| Mean | 0.959 | 1,461 | 953 | 0.046 | 704 | 0.131 | 0.465 |

Notes: Source: NPSAS Data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. All reported results are from the $\alpha_1$ coefficient in Equation (1). Each regression includes state and year fixed effect and all covariates listed in Table 3, except for the variable corresponding to the subgroup listed. Bold indicates that the coefficient for the relevant demographic group is statistically different from the average effect.

Table 5—: Comparison of Rigorous and Less Rigorous Requirements

| | (1) Applied for Aid | (2) Sub Stafford $s | (3) Unsub Stafford $s | (4) Only Loans | (5) Private Loan $s | (6) Have CC Balance | (7) Work while Enrolled |
|---|---|---|---|---|---|---|---|
| Rigorous | 0.035* | 120.92+ | 29.87 | -0.017** | -163.93* | -0.019* | -0.021 |
| | (0.014) | (63.78) | (78.94) | (0.006) | (67.77) | (0.008) | (0.014) |
| Less Rig. | 0.006 | -33.58 | 211.50 | -0.049 | -435.36* | -0.040+ | 0.080 |
| | (0.011) | (106.03) | (211.25) | (0.030) | (194.19) | (0.023) | (0.048) |
| N | 25,354 | 25,354 | 25,354 | 25,354 | 25,354 | 25,354 | 25,354 |

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

Table 6—: Testing the Pre-trends in Financial Education

| | (1) Applied for Aid | (2) Sub Stafford $s | (3) Unsub Stafford $s | (4) Only Loans | (5) Private Loan $s | (6) Have CC Balance | (7) Work while Enrolled |
|---|---|---|---|---|---|---|---|
| $PF_{t+1}$ | 0.007 | 94.185 | 7.912 | -0.010 | 45.020 | 0.012 | -0.014 |
| | (0.017) | (74.955) | (97.362) | (0.015) | (188.144) | (0.022) | (0.026) |
| $PF_{t+2}$ | -0.012 | 86.455 | -102.685 | 0.018 | 43.349 | 0.029 | -0.003 |
| | (0.016) | (58.394) | (68.318) | (0.013) | (138.451) | (0.020) | (0.020) |
| N | 12,822 | 12,822 | 12,822 | 12,822 | 12,822 | 12,822 | 12,822 |

Notes: Source: NPSAS Data (1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$. Each regression includes state and year fixed effect and all covariates listed in Table 3. The model drops all states after their requirement is in effect. $PF = 1$ if the individual was 18 or younger in a state where personal finance was required in his high school for those 18 and younger. $PF_{t+1} = 1$ if the graduation requirement took place in an individual's state the subsequent wave after he appeared in the data. The excluded group is those in states who implemented two waves or more prior to the mandate. $PF_{t+2} = 1$ if the individual appeared in the data two waves before a graduation requirement was in place in his state (e.g., the graduation requirement came into place in two waves of data). The excluded group is those who implemented three waves or more prior to the requirement. The states identifying this variation are those implementing just before the 2011 wave or later: UT, VA (2008); CO, SC (2009); MO (2010); IA, NE, NJ, TN (2011); KS (2012); OR (2013) .

Table 7—: Testing the Pre-trends in Financial Education Using 1989-2011 Data

| | (1) Applied for Aid | (2) No Aid | (3) Have Stafford | (4) Only Loans | (5) Work while Enrolled |
|---|---|---|---|---|---|
| $PF_{t+1}$ | 0.136 | -0.143 | -0.104 | -0.094 | 0.151 |
| | (0.113) | (0.154) | (0.123) | (0.056) | (0.105) |
| $PF_{t+2}$ | 0.069 | -0.046 | -0.110 | -0.058 | 0.107 |
| | (0.086) | (0.115) | (0.090) | (0.040) | (0.068) |
| $PF_{t+3}$ | 0.026 | -0.035 | -0.058$^{+}$ | -0.023 | 0.029 |
| | (0.040) | (0.050) | (0.034) | (0.020) | (0.043) |
| N | 6,113 | 6,462 | 6,462 | 6,100 | 5,309 |

Notes: Source: NPSAS Data (1989, 1992, 1995, 1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$. Each regression includes state and year fixed effect and all covariates listed in Table 3. We drop all states after their requirement is in effect, and include each state that passes for only three years prior to the requirement. $PF_{t+1} = 1$ if the graduation requirement took place in an individual's state the subsequent wave after he appeared in the data. $PF_{t+2} = 1$ if the individual appeared in the data two waves before a graduation requirement was in place in his state (e.g., the graduation requirement came into place in two waves of data). $PF_{t+3} = 1$ if the individual appeared in the data three waves before a graduation requirement was in place in his state (e.g., the graduation requirement came into place in three waves of data). The excluded group is those who never implemented the requirement. This is identified off all all states that implement except for New Hampshire and Illinois in Table 1. These variables are different from our main specifications, since the data span from a longer time period (1989-2011). No Aid equals one if the individual does not have any financial aid and zero otherwise. Have Stafford equals one if the individual has a Subsidized or Unsubsidized Stafford loan and zero otherwise. Only Loans equals one if the individual only has loans in his aid package (no grants or scholarships) and zero otherwise.

Table 8—: State Characteristics and Personal Finance Requirements

| | (1) PF | (2) Rigorous | (3) Less Rigorous |
|---|---|---|---|
| Governor is Democrat | 0.00125 | -0.00381 | 0.01931 |
| | (0.035) | (0.018) | (0.017) |
| Unemployment rate | -0.02940 | 0.00280 | -0.02145$^{+}$ |
| | (0.021) | (0.012) | (0.012) |
| Medicaid beneficiaries | -0.00006 | -0.00009$^{*}$ | 0.00002 |
| | (0.000) | (0.000) | (0.000) |
| SSI recipients | -0.00123 | -0.00147 | 0.00065 |
| | (0.002) | (0.001) | (0.000) |
| Gross State Product | 0.00004 | -0.00014 | -0.00053 |
| | (0.001) | (0.000) | (0.000) |
| Poverty Rate | -0.00078 | 0.00122 | -0.00529 |
| | (0.007) | (0.004) | (0.004) |
| Population | 0.09944 | 0.15813$^{**}$ | 0.01606 |
| | (0.098) | (0.056) | (0.014) |
| Food Stamp/SNAP Recipients | 0.00008 | 0.00010 | -0.00006$^{+}$ |
| | (0.000) | (0.000) | (0.000) |
| N | 1,145 | 1,145 | 1,145 |

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

Table 9—: Results Robust to Controlling for Other Educational Policies

| | (1) Applied for Aid | (2) Sub Stafford $s | (3) Unsub Stafford $s | (4) Only Loans | (5) Private Loan $s | (6) Have CC Balance | (7) Work while Enrolled |
|---|---|---|---|---|---|---|---|
| Controls for Total Credits Required for Graduation | | | | | | | |
| PF | 0.028$^{+}$ | 96.125 | -15.378 | 0.002 | -52.097 | -0.023* | -0.030$^{+}$ |
| | (0.014) | (69.925) | (87.032) | (0.007) | (68.425) | (0.009) | (0.017) |
| N | 20,018 | 20,018 | 20,018 | 20,018 | 20,018 | 20,018 | 20,018 |
| Controls for Total Math Credits | | | | | | | |
| PF | 0.030$^{+}$ | 94.128 | 1.161 | -0.000 | -45.086 | -0.019* | -0.024 |
| | (0.015) | (69.120) | (88.814) | (0.006) | (70.680) | (0.008) | (0.016) |
| N | 19,557 | 19,557 | 19,557 | 19,557 | 19,557 | 19,557 | 19,557 |
| Controls for Highest Math Required | | | | | | | |
| PF | 0.032* | 99.237 | -38.609 | 0.001 | -110.516 | -0.019* | -0.025 |
| | (0.013) | (59.749) | (77.923) | (0.006) | (70.224) | (0.007) | (0.015) |
| N | 23,093 | 23,093 | 23,093 | 23,093 | 23,093 | 23,093 | 23,093 |
| Controls for ACT or SAT Required | | | | | | | |
| PF | 0.031* | 101.509 | 1.412 | 0.001 | -148.824* | -0.023** | -0.014 |
| | (0.013) | (63.489) | (78.590) | (0.007) | (69.157) | (0.008) | (0.014) |
| N | 25,354 | 25,354 | 25,354 | 25,354 | 25,354 | 25,354 | 25,354 |
| Controls for State Scholarship Programs | | | | | | | |
| PF | 0.033* | 108.147$^{+}$ | -3.965 | 0.000 | -154.067* | -0.021* | -0.014 |
| | (0.013) | (61.058) | (74.425) | (0.007) | (65.663) | (0.009) | (0.014) |
| N | 25,354 | 25,354 | 25,354 | 25,354 | 25,354 | 25,354 | 25,354 |
| Controls for Higher Ed Spending | | | | | | | |
| PF | 0.037* | 180.013** | 56.936 | 0.001 | -61.193 | -0.046** | -0.054*** |
| | (0.016) | (64.703) | (88.026) | (0.011) | (64.783) | (0.014) | (0.014) |
| N | 14,714 | 14,714 | 14,714 | 14,714 | 14,714 | 14,714 | 14,714 |

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

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

| | (1) Sub Stafford $s | (2) Unsub Stafford $s | (3) Only Loans | (4) Non-loan Aid $s |
|---|---|---|---|---|
| PF Offered | -0.469 | -20.067 | -0.017* | 29.843 |
| | (15.561) | (25.851) | (0.008) | (35.836) |
| N | 21,385 | 21,385 | 21,385 | 21,385 |

Notes: Robust standard errors clustered at the high school level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$. Data come from the Montana University System administrative data. Private student loans are not included in these data. Only loans equals one if students have loans and no grants or scholarships in their financial aid packages. Each regression includes high school and year fixed effects, sex, white and missing race dummies, age dummies (17 and 18, with 19 the excluded group), ACT (or SAT converted to ACT), and campus dummy. Subsidized and Unsubsidized Stafford amounts are in dollars and include zeros. Only loans= 1 if the given student had a loan and no other form of non-loan aid. Non-loan aid is the amount of scholarships, grants, awards, and exemptions the student received in dollars. It does not include Pell grants, or other grants received directly by the student that were not awarded through the institution (i.e., private work grants). PF Course Offered = 1 if the student went to high school that offered personal finance prior to the time she graduated from high school.

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

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

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

## Appendix A: Validating Pre Trends

Table A.1—: Placebo Financial Education did not Affect Outcomes

| | (1) Applied for Aid | (2) No Aid | (3) Have Stafford | (4) Only Loans | (5) Work while Enrolled |
|---|---|---|---|---|---|
| $PF_{-10}$ | -0.000 | 0.004 | -0.005 | -0.006 | -0.019 |
| | (0.016) | (0.020) | (0.028) | (0.013) | (0.029) |
| N | 7,843 | 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 regression 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 graduation requirement was binding. This variable essentially just falsely moves the policy back ten yers. The sample includes data from 1989-2003.

Table A.2—: State Linear Time Trends in Financial Education, 1989-2011

| | (1) Applied for Aid | (2) No Aid | (3) Have Stafford | (4) Only Loans | (5) Work while Enrolled |
|---|---|---|---|---|---|
| PF | 0.028$^{+}$ | -0.033 | 0.057$^{*}$ | 0.002 | -0.066$^{**}$ |
| | (0.017) | (0.020) | (0.025) | (0.010) | (0.019) |
| N | 21,334 | 22,144 | 22,144 | 21,365 | 19,696 |

Notes: Source: NPSAS Data (1989, 1992, 1995, 1999, 2003, 2007, 2011). Robust standard errors clustered at the state level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$. Each regression includes state and year fixed effect and all covariates listed in Table 3, as well as state linear time trends. These variables are different from our main specifications, since the data span from a longer time period (1989-2011). No Aid equals one if the individual does not have any financial aid and zero otherwise. Have Stafford equals one if the individual has a Subsidized or Unsubsidized Stafford loan and zero otherwise. Only Loans equals one if the individual only has loans in his aid package (no grants or scholarships) and zero otherwise.

Appendix B: MUS Data

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

| | No PF | PF Offered | Both |
|---|---|---|---|
| Dependent Variables | | | |
| Get Stafford | 0.498 | 0.482 | 0.486 |
| | (0.500) | (0.500) | (0.500) |
| Stafford Subsidized $s | 559.8 | 547.7 | 550.7 |
| | (725.8) | (720.6) | (721.9) |
| Stafford Unsubsidized $s | 398.4 | 386.8 | 389.6 |
| | (775.4) | (779.5) | (778.5) |
| 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) |

Table B.2—: Pre-trends in MUS Data

| | (1) Sub Stafford $s | (2) Unsub Stafford $s | (3) Only Loans | (4) Non-loan Aid $s |
|---|---|---|---|---|
| PF Offered | -10.82 | -14.62 | -0.018* | 16.48 |
| | (15.90) | (30.69) | (0.008) | (38.87) |
| PF Offered −1 | -26.62 | 62.95 | 0.007 | -61.04 |
| | (24.15) | (43.64) | (0.015) | (66.76) |
| PF Offered −2 | -40.67 | -8.81 | -0.008 | 0.55 |
| | (25.97) | (44.84) | (0.016) | (59.82) |
| PF Offered −3 | 11.55 | -21.08 | -0.008 | -40.02 |
| | (24.38) | (36.13) | (0.013) | (50.64) |
| PF Offered −4 | -21.52 | 5.07 | -0.008 | 6.32 |
| | (25.08) | (22.20) | (0.01) | (43.92) |
| PF Offered −5 | -34.91 | 14.8 | 0.002 | -25.61 |
| | (31.20) | (33.10) | (0.015) | (57.63) |
| PF Offered −6 | 30.44 | -13.87 | 0.010 | -54.87 |
| | (23.38) | (36.57) | (0.015) | (57.33) |
| PF Offered −7 | 0.083 | -19.11 | -0.008 | 79.17 |
| | (22.71) | (23.01) | (0.017) | (88.24) |
| N | 21,385 | 21,385 | 21,385 | 21,385 |

Notes: Robust standard errors clustered at the high school level in parentheses. $^{+}$ $p < 0.10$, $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$. Data come from the Montana University System administrative data. Private student loans are not included in these data. Only loans equals one if students have loans and no grants or scholarships in their financial aid packages. Each regression includes high school and year fixed effects, sex, white and missing race dummies, age dummies (17 and 18, with 19 the excluded group), ACT (or SAT converted to ACT), and campus dummy. Subsidized and Unsubsidized Stafford amounts are in dollars and include zeros. Only loans= 1 if the given student had a loan and no other form of non-loan aid. Non-loan aid is the amount of scholarships, grants, awards, and exemptions the student received in dollars. It does not include Pell grants, or other grants received directly by the student that were not awarded through the institution (i.e., private work grants). PF Course Offered = 1 if the student went to high school that offered personal finance prior to the time she graduated from high school. PF Offered $-i$ equals one if the course was offered i years after an individual graduated from high school. The excluded group is those who graduated high school more than 7 years before a course was offered in his or her high school.

Appendix C: CPS Data

In this section, we seek to understand how personal finance graduation requirements affect college attendance 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.[39] 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 C.1 shows the average dependent variables by whether or not the state ever required personal finance prior to graduation, using the CPS sample weights.[40] There are no significant differences across the two sets of states, and the average college attendance rate is roughly 54%, with 48% attending postsecondary education full time and only 6% going to school part-time. There are no notable differences across the two samples in terms of demographic characteristics of individuals within those states either.

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

Next, we estimate the effect of personal finance education on college attendance using Equation 3. Our dependent variable, $Y_{i,s,t}$, equals one if individual $i$ in state $s$ at time $t$ attends college and zero otherwise. Our independent variable of interest, PF Required$_{i,s,t}$, equals one if individual $i$ living in state $s$ with a personal finance requirement in place prior to the time that individual graduated from high school. We include state fixed effects ($\delta_s$), year fixed effects ($\gamma_t$), and CPS survey month fixed effects ($\zeta_m$), as well as individual-level characteristics ($\boldsymbol{X_i}$) that include male, white, black, hispanic, married, a metropolitan-resident dummy, and age dummies.

Table C.2 reports the results from Equation 3. Our baseline specification shows that personal finance graduation requirements do not change college attendance rates. We then replicate our policy heterogeneity from Table 5 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.[41] In all specifications, the results

[39] 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.

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

[41] When we perform additional robustness tests to drop early adopters or those with locally-controlled policies, we again find no effects of personal finance on postsecondary education attendance.

are nearly zero in magnitude. Finally, we replicate the heterogeneity exercises by gender and race (as in Table 4) and still find no effect of personal finance education on college attendance for these groups. Thus, we think we have tightly estimated a null effect of financial education on college attendance.

Table C.3 confirms that the parallel trends assumption required for the difference-in-difference estimation strategy is satisfied, as the years before the requirement in states with personal finance requirements show no difference in the outcome variables. There are no clear trends from the excluded group, those who graduated more than 13 years before a graduation requirement came into effect, and each year before the requirement. The coefficients on PF Requirement −1 through PF Requirement −13 are not statistically different from one another. This gives us confidence that there are no differences across states with and without personal finance requirements in college enrollment in the pre- or post- policy change years.

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

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

Table C.2—: Pre-trends in CPS Data

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

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

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

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

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