Personal Finance Education Mandates and Student Loan Repayment*

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Abstract

I investigate how personal financial literacy (PFL) education during high school affects federal student loan repayment outcomes after college. I use university-level repayment outcomes to overcome a lack of viable borrower-level data. Changes to state curricula impact university cohorts differentially depending on the share of students from adopting states. Using this variation, I find that PFL mandates improve federal student loan repayment and the effects are largest for first generation and low income students at public universities. I also explore several mechanisms that might explain how PFL mandates increase repayment.

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In the United States, high school students are increasingly tasked with making consequential human capital investment decisions with a rather limited information set. Some of these decisions include whether to attend college, which college to attend, and how to finance postsecondary education. Perhaps as a result of such decision-making under uncertainty, 47% of Americans with student loan debt say they would have accepted fewer federal aid dollars if they could make the choice again. Also, over half say they've had difficulties making monthly payments (Consumer Reports, 2016). These difficulties can be compounded for first generation students who often do not have access to mentors with first-hand experience in postsecondary education. Several recent studies document these frictions and find that these students are less likely to apply for admission to selective universities, are less likely to retake standardized tests, and are more likely to under-invest in postsecondary education (Hoxby and Turner, 2015; Goodman, Gurantz, and Smith, 2018; Avery and Turner, 2012).

In this paper, I study an intervention that might improve outcomes for high school students making human capital investment decisions: mandated personal financial literacy (PFL) coursework. Specifically, I investigate whether PFL education during high school improves postsecondary finance outcomes by providing students with additional information at the critical time financial aid decisions are made. Between 1993 and 2014, 23 states adjusted high school graduation requirements to include topics covering personal financial literacy (Stoddard and Urban, 2019). The main objective for the focus on PFL is to increase the overall financial literacy of the state population, but many of the state standards include topics discussing postsecondary education and career research. Since enrollment in these courses often coincides with the timing of the federal financial aid application process, requiring personal finance education in high school can operate as a just-in-time information intervention to improve postsecondary finance outcomes.

I estimate how personal finance education mandates affect federal student loan repayment by exploiting plausibly exogenous variation in university-level exposure to state mandates. When states adjust high school graduation requirements to include PFL topics, universities become increasingly populated by students who were exposed to this course content during high school. Universities are differently affected by changes to state standards because their student bodies have different shares of incoming students from adopting states. I use this variation in exposure to state graduation requirements to identify the causal effect of required PFL education in high school.

I find that increased exposure to state adopted PFL standards improves university-level student loan repayment. The effect is largest for students at public universities and especially for first generation students and for students from households earning less than $30,000 per year. The estimates suggest a 5% increase in the probability a low income or first generation student is able to pay down some of their balance during the first year of repayment. I conduct a counterfactual exercise using these estimates which suggests that mandating PFL standards for all high school students between the 2001 and 2008 graduating classes would have resulted in around 9,000 additional students successfully repaying student loans each year.

I also present evidence in support of the identifying assumptions and I conduct a number of robustness checks. Using a flexible event study specification, I show that universities that were more exposed to PFL mandates were not trending differently than universities less exposed to PFL mandates. I confirm results from the literature that the adopted PFL mandates did not significantly shift students to select different colleges (Stoddard and Urban, 2019). Additionally, I estimate an alternative specification which holds fixed the share of students from each feeder state and relies only on the state adoption of mandates over time. The results from this specification are similar to the baseline specification. Taken together, I conclude that the findings are not driven by a compositional change in university cohorts, but via micro-level improvements in student loan repayment.

I explore several mechanisms by which mandated PFL education may improve federal student loan repayment. First, I test whether improvements in student loan repayment are due to decreases in average loan balances. I find that only high income students change borrowing behavior resulting in roughly 8% lower balances upon entering repayment. Point estimates suggest small declines (less than 3%) for first generation and middle income students but the estimates are not precise. Next, I test whether students bound by mandates are better able to correctly answer financial literacy questions. Across three different surveys, I find no evidence of improvements in financial literacy at the time of survey for those that were bound by the state mandates. I also find no evidence that students bound by personal finance mandates were any more likely to attend college or earn a degree. However, I do find that affected students were more likely to correctly answer questions pertaining to federal student loan regulations. The results suggest that, rather than reductions in borrowing or improvements in financial literacy, the personal finance education mandates studied in this paper may act as a just-in-time information intervention for students making postsecondary decisions.

The remainder of the paper is organized as follows: Section 1 reviews background details of the federal financial aid system and summarizes the previous studies discussing personal finance education. Section 2 discusses the potential mechanisms by which personal finance education in high school can influence student loan repayment after college. Section 3 details the data used in the analysis. Section 4 discusses the empirical strategy and assumptions necessary for identification and inference. Section 5 presents the empirical results, tests the identification assumptions, and presents evidence for the tested mechanisms. Section 6 concludes the paper. I also present results from various robustness checks and the estimation of alternative specifications in Appendix A.

1 Background

In order to qualify for federal student loans, students must complete a Free Application for Federal Student Aid (FAFSA) which collects details about students and their families including income and asset information. While federal subsidized loans are means-tested based on information from the FAFSA, unsubsidized loans are available to any student.[1] Students also face limits on federal borrowing based on the loan type, year of schooling, university Cost of Attendance (COA), and other financial aid received.

A wealth of evidence from the literature has largely concluded that access to financial aid increases access to higher education for low income students (Dynarski, 2003) and more recent evidence suggests increased student loan borrowing causes higher grades and more completed credits for community college students (Marx and Turner, 2019b). Despite the benefits, studies have been critical of the burdensome bureaucracy and complicated process for applying for and receiving financial aid (Dynarski and Scott-Clayton, 2006; Novak and McKinney, 2011; Bettinger, Long, Oreopoulos, and Sanbonmatsu, 2012; Dynarski and Scott-Clayton, 2013; Scott-Clayton, 2015).[2] Critics often argue that the complicated application process and the multitude of choice options tend to reduce the receipt of aid for students that would otherwise be eligible and encourages students to opt into the default option (Kofoed, 2017; Marx and Turner, 2019a). Even for students that successfully navigate the application stage, complexities surrounding the number and type of repayment plans can lead to issues during repayment (Cox, Kreisman, and Dynarski, 2018; Abraham, Filiz-Ozbay, Ozbay, and Turner, 2018). The evidence presented in this paper is consistent

[1]Subsidized loans are loans in which interest does not accrue while the student is in school while unsubsidized loans begin accruing interest after dispersement.

[2]Castleman, Schwartz, and Baum (2015) summarizes several studies that test interventions designed to improve the decision making process in investing in higher education.

with the literature that finds improvements in outcomes through reducing the barriers to federal financial aid access.

A few recent studies find that interventions that provide students with more information about college applications and federal financial aid improve outcomes for students from disadvantaged backgrounds. Bettinger, Long, Oreopoulos, and Sanbonmatsu (2012) show that providing low income families with assistance completing the FAFSA can dramatically increase the probability of applying for federal aid and increase college enrollment and persistence. Barr, Bird, and Castleman (2016) find that providing information to community college students about federal student loan options can shift borrowers away from higher cost financing which is largely driven by students with lower levels of financial literacy and higher debt balances. Gurantz, Pender, Mabel, Larson, and Bettinger (2019) find that virtual college counseling that targets low and middle income students increased the probability students chose to attend a college with a high graduation rate. Additionally, Castleman and Goodman (2018) find that college counseling can increase low income student enrollment and persistence in less expensive four-year public universities with higher graduation rates. Bettinger and Evans (2019) also find that peer advising from recent college graduates to high school students can increase enrollment in two-year colleges for low income and Hispanic students without reducing four-year enrollment.

One large scale intervention that might improve postsecondary outcomes for disadvantaged students is the addition of personal financial literacy (PFL) education during high school. A few recent articles study the effect of PFL directly, the first being Brown, Grigsby, van der Klaauw, Wen, and Zafar (2016). They investigate how changes in economics, mathematics, and personal finance requirements affect financial outcomes for young people. The results confirm findings in the previous literature that increasing math requirements increases asset levels and incomes for young adults (Goodman, 2019). Additionally, mandated PFL coursework is shown to reduce the amount of delinquent debt held by young adults. They also find the effect of course mandates on credit health grows as mandates mature. This suggests either implementation lags on the part of schools or improvements in teaching efficiency over time. Harvey (2019) and Urban, Schmeiser, Collins, and Brown (2018) also investigate how mandated PFL coursework affects financial outcomes for young people. Harvey (2019) finds that young people bound by mandates are less likely to use alternative financial services which typically carry very high interest rates with high rates of delinquency. Urban, Schmeiser, Collins, and Brown (2018) compare credit report data across mandated and non-mandated young people and find that those who were bound by personal finance education mandates during high school have fewer delinquent credit accounts and higher credit scores.

This paper is most closely related to Stoddard and Urban (2019) which studies how PFL mandates affect the receipt of federal student aid for first year college students. Using a difference-in-differences design with several waves of the National Postsecondary Study of Student Aid (NPSAS), they find that mandated college freshmen are more likely to complete the FAFSA, more likely to borrow from federal sources, more likely to receive grants or scholarships, borrow fewer private loans, and are less likely to carry a credit card balance. They also find that the impact on extensive borrowing of federal loan dollars and the lower likelihood of credit card borrowing is larger for low income students and these students are also less likely to work while enrolled in college.

This paper extends this literature in four distinct dimensions. First, to my knowledge, this is the first paper to estimate the impact of PFL mandates on student loan repayment. To this end, I employ two measures of federal student loan repayment progress that vary in sensitivity. The first measure, loan default, is a more adverse and relatively rare outcome that is difficult to affect. The second outcome, the repayment rate, measures whether loan principals are declining and is thus a more sensitive measure of repayment progress. Second, I test whether PFL mandates affect the level of student debt upon leaving college.[3] Third, I investigate the source of these changes by estimating the effect of PFL mandates on financial literacy and on knowledge of the federal student loan system. Lastly, I employ a novel identification strategy to overcome the lack of quality micro-level data that instead relies on university-level benchmarks to proxy borrower level changes in student loan outcomes. I show that, under the necessary assumptions, this identification strategy consistently estimates the micro-level effect of PFL mandates on borrower outcomes.

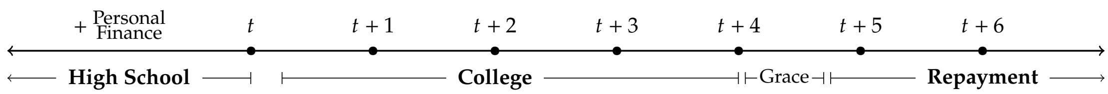
2 Potential Mechanisms

To better understand how mandated personal finance education can influence student loan repayment after college, consider the case of two otherwise identical high school students in which one student (the treated student) is required to be exposed to personal finance education during high school as in Section 2 and the second student (the untreated student) is not. There are many avenues for the intervention to affect repayment outcomes since exposure to personal finance

[3] Brown, Grigsby, van der Klaauw, Wen, and Zafar (2016) finds larger student loan balances at age 27 which is a combination of original principal and pace of repayment rather than total loan debt upon entering repayment.

education occurs during high school (t) and student loan repayment does not begin until after college ($t + 5$). The three categories of course content most likely to directly or indirectly affect student loan repayment are topics on financial literacy, financial aid, and career research.

Figure 1: Example Timeline for Personal Financial Literacy Education Intervention through to Student Loan Repayment



First, since the goal of most PFL mandates is to improve financial literacy, the standards typically focus on topics such as interest, inflation, risk tolerance, insurance, budgeting, and investing.[4] As a result, students bound by PFL mandates may be better at managing money, be more likely to understand the risks of borrowing, and be more likely to make on time loan payments. The state standards for many courses require students to "examine ways to avoid and eliminate credit card debt" (Texas[5]), "design a financial plan for earning, spending, saving, and investing" (Missouri[6]), and "evaluate the different aspects of personal finance including careers, savings and investing tools, and different forms of income generation" (Michigan[7]). However, most studies that evaluate small scale financial literacy interventions find little to no improvement in financial literacy and most improvements depreciate quickly (Huston, 2010; Fernandes, Lynch Jr, and Netemeyer, 2014). On the other hand, some evidence shows that financial literacy interventions during high school can improve more objective measures of financial health such as credit scores (Brown, Grigsby, van der Klaauw, Wen, and Zafar, 2016; Urban, Schmeiser, Collins, and Brown, 2018).

Second, the required coursework may help students navigate the federal financial aid system. In many states, the PFL standards require students to research various ways of funding postsecondary education. For example, students in Oregon must research the costs and benefits of using loans to finance higher education[8] and students in Texas should "research and evaluate various

[4]Figure A.3 shows a word cloud of the text of all PFL state standards.

[5]https://web.archive.org/web/20111107152521/http://ritter.tea.state.tx.us/rules/tac/chater118/ch118a.html

[6]https://dese.mo.gov/sites/default/files/personal_finance_competencies.pdf

[7]https://www.michigan.gov/documents/mde/SS_COMBINED_August_2015_496557_7.pdf

[8]https://www.ode.state.or.us/teachlearn/subjects/socialscience/standards/oregon-social-sciences-academic-content-standards.pdf

scholarship opportunities."[9] Some states take this a step further and require students to practice applying for federal financial aid and research the differences in various aid types. Tennessee's state standards require students to research both positive and negative aspects of borrowing federal student loans[10] while Utah students must "utilize the FAFSA4caster to explore the FAFSA process."[11] Previous research has shown that simplifications in the federal financial aid system often lead to improvements in outcomes for vulnerable groups (Dynarski and Scott-Clayton, 2006; Novak and McKinney, 2011; Bettinger, Long, Oreopoulos, and Sanbonmatsu, 2012; Dynarski and Scott-Clayton, 2013; Scott-Clayton, 2015). Therefore, requiring students to become familiar with the aid process might improve outcomes for low income and first generation students.

Third, many PFL course requirements direct students to research various careers, colleges, and majors. These exercises might alter the trajectory of the student in a number of dimensions. Many of the state standards for the required personal finance education directly address investments in human capital. Students bound by various state mandates are required to "explore potential careers and the steps needed to achieve them" (Arkansas[12]) or to "identify a career goal and develop a plan and timetable for achieving it, including educational/training requirements, costs, and possible debt" (New Jersey[13]). These activities during the personal finance coursework might cause students to be more aware of various career paths and education requirements which might better prepare students for success in college.

In this paper, I directly test a number of hypotheses that might provide supporting evidence for these mechanisms. I first test whether student loan balances upon entering repayment change as a result of PFL mandates. Next, I test whether PFL mandates improve literacy. Specifically, I test whether students bound by PFL mandates are better able to answer financial literacy questions and questions about federal student loans. As a test for one identification assumption, I investigate whether the adoption of PFL state standards alter students' college choice. Lastly, I test whether students bound by PFL mandates have a higher likelihood of attending college or earning a degree.

[9] https://web.archive.org/web/20111107152521/http://ritter.tea.state.tx.us/rules/tac/chapter118/ch118a.html

[10] https://www.tn.gov/content/dam/tn/education/ccte/cte/cte_std_personal_finance.pdf

[11] https://www.schools.utah.gov/file/6348311c-77c7-4fbd-87e7-ba3484bddb6e

[12] http://www.arkansased.gov/public/userfiles/Learning_Services/Curriculum%20and%20Instruction/Frameworks/Personal_Finance/Economics-aligned-to-PF-Standards.pdf

[13] https://www.state.nj.us/education/cccs/2014/career/91.pdf

3 Data

Since micro-level data on repayment outcomes for sequential cohorts are not available to researchers, I instead rely on university-level outcomes. The federal student loan repayment outcomes used in this paper come from the College Scorecard database. The College Scorecard was developed during the Obama Administration and debuted in 2015 as a website tool to provide more information to potential college students. The Department of Education provides the underlying university-level data dating back to the 1996-1997 academic year and updates the data frequently. The data are sourced via self-reports from universities, from various federal data sources, and from administrative data on students receiving financial aid. The data used in this paper are largely constructed using the administrative National Student Loan Data System (NSLDS) which contains records on the universe of federal aid recipients.

I restrict the sample to four-year baccalaureate universities since four-year universities are largely populated by first-time degree seeking recent high school graduates.[14] I remove universities that aggregate repayment outcomes across multiple branch campuses and universities that do not receive federal financial aid.[15] In order to construct a balanced panel, I remove universities that either enter or exit the sample during the sample window. This can occur due to a university opening or closing or a university opting into or losing access to federal aid.[16] The resulting sample contains 1,386 universities across 50 states and the District of Columbia of which 450 are public universities and 936 are private universities.[17]

The two main outcomes from the College Scorecard are the two-year cohort default rate and the one-year repayment rate. After leaving college, federal student loan borrowers are granted a six month grace period before they must begin making monthly payments. One year after entering repayment, borrowers fit into one of four mutually exclusive bins as depicted in Figure 2.[18] If the

[14] In 2016, 45% of recent high school graduates enrolled in 4-year colleges while 23.7% enrolled in 2-year schools. `https://nces.ed.gov/fastfacts/display.asp?id=372`

[15] This restriction is necessary due to the nature of the identification strategy discussed in the next section. When a university system aggregates outcome measures across multiple branches, the identifying variation on the right-hand-side of the estimating equation is aggregated at a smaller granularity than the outcome measure on the left-hand-side. The reasoning behind the varying level of aggregation is discussed in footnote 17 of *Using Federal Data to Measure and Improve the Performance of U.S. Institutions of Higher Education* found at `https://collegescorecard.ed.gov/assets/UsingFederalDataToMeasureAndImprovePerformance.pdf`

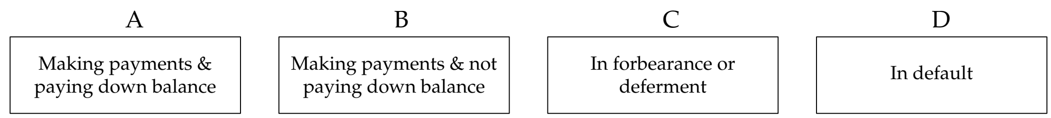
[16] This restriction helps to alleviate the concern of selection into or out of the sample. Universities may lose access to federal aid as a result of poor student loan repayment or choose to begin accepting federal aid as a result of unobservables that change over time. Looney, Yannelis, et al. (2019) notes that the majority of the variation in cohort defaults over time stem from entry into and out of the student loan market.

[17] Table A.3 details the change in sample size as a result of each of the restrictions. Figure A.2 plots the locations of the universities in the sample.

[18] The College Scorecard database also includes the repayment rate for 3, 5, and 7 years, however the data begins for all

student is making payments on her loans and the balance is declining, this student fits in bin A. If the student is making payments toward her loan but the payment is not sufficient to cover accruing interest (i.e. negative amortization), the student fits in bin B.[19] If the student has been granted forbearance or deferment of payments (and thus no payments are required) and the balance is not declining, the student fits in bin C. If the student has not made any payments for 270 days, the student enters default and fits in bin D.[20]

Figure 2: Repayment Status Bins for Repayment Cohort



Repayment Rate = $\frac{A}{A+B+C}$ Default Rate = $\frac{D}{A+B+C+D}$

The two-year **cohort default rate** is calculated in the College Scorecard by dividing the total number of students in default (bin D) at the end of the two year window by the total number of students in the repayment cohort (the sum of bins A, B, C, and D). The one-year **repayment rate** is calculated by dividing the number of students who have paid down at least one dollar of their original principal (bin A) one year after entering repayment by the total repayment cohort excluding those in default (the sum of bins A, B, and C). Those students in bin B (making payments but facing negative amortization) and in bin C (not required to make payments and facing negative amortization) both count against the repayment rate *but are not in default.* This makes the repayment rate a more sensitive measure of loan repayment health that does not require default but factors in repayment progress.

The College Scorecard reports the default rate and the repayment rate for the full repayment cohort, but the repayment rate is also reported for various subsamples of the student body. Of particular interest in this paper, these subsamples include **first generation students** (students whose parents did not have a college degree upon college entry), **low income students** (students with

variables for FY 2006 and thus these variables have very small windows of data availability.

[19]Due to income-driven repayment plans, it is possible that the monthly minimum payment is not enough to cover the interest accruing each month. In this case, it is very unlikely the borrower will have a declining balance without paying more than the monthly minimum payment.

[20]Default for students loans is atypical compared to other consumer debt. Upon default, there is no repossession of assets since the loans are unsecured. Rather, the federal government levies fines and allows loan services to garnish wages and tax refunds to collect outstanding debts.

household income less that $30,000 upon college entry), **middle income students** (students with household income between $30,000 and $75,000 upon college entry), and **high income students** (students with household income above $75,000 upon college entry).[21] Table 1 reports summary statistics for the sample of universities for the main outcome variables. I present the means and standard deviations weighted by the number of borrowers used in constructing each university outcome. The weighted moments are presented to be representative of the population of student loan borrowers.

Table 1: Descriptive Statistics from the College Scorecard

	All Universities (n=1,386)		**Public Universities** (n= 450)		**Private Universities** (n= 936)	
Outcome Variable	Mean	SD	Mean	SD	Mean	SD
Default Rate	0.042	(0.032)	0.046	(0.031)	0.036	(0.034)
Repayment Rate						
Overall	0.594	(0.170)	0.593	(0.149)	0.595	(0.199)
First Generation	0.536	(0.165)	0.551	(0.147)	0.512	(0.188)
Low Income (<$30k)	0.458	(0.170)	0.478	(0.151)	0.423	(0.194)
Middle Income ($30k to $75k)	0.631	(0.148)	0.630	(0.136)	0.632	(0.166)
High Income ($75k+)	0.749	(0.114)	0.730	(0.107)	0.777	(0.117)

Means and standard deviations for the main outcome variables are presented above for the full sample and separately by institution control. Moments are weighted by the number of borrowers used to compute each outcome in order to be representative of the population of student borrowers. Default rate is the two year cohort default rate from FY1995 to FY2013. The one year repayment rate is reported for the full repayment cohort and separately for first generation students (students whose parents did not have a college degree) and for students by household income bins.

The repayment rate is first reported in the College Scorecard beginning with the 2007-2008 academic year which includes students entering repayment in the 2006 fiscal year.[22] The most recent data in the Scorecard covers students that entered repayment in the 2013 fiscal year.[23] Beginning in FY2009, the Department of Education began grading universities on the three-year cohort default rate instead of the previous two-year cohort default rate. This change was a concerted effort to hold universities accountable for borrowers beyond two years after entering repayment. The College

[21]The College Scorecard uses nominal dollars to determine these bins. As a result, students with similar household incomes in real terms might be shifted into higher income bins over time due to inflation.

[22]The College Scorecard reports the one-year repayment rate as a two year rolling average in order to reduce variability. Although this is not ideal for identification, I match the repayment rate outcome using the first year a repayment cohort is reported in the data to match incoming college cohorts to repayment cohorts. Any bias from this rolling average will work *against* detecting an effect of mandates on repayment since it will include one untreated cohort and the first treated cohort.

[23]For repayment cohort counts smaller than 30 students, the data is suppressed and thus these small cells are omitted from the analysis.

Scorecard continued to include the two-year cohort default rate through FY2011 but deferred to only posting the three year cohort default rate in subsequent years. Due to the change in the cohort default metric, I use the two-year cohort default rate in the available years between FY1995 and FY2011.

Table 2: Implementation of Personal Financial Literacy (PFL) Mandates Since 1990

State	Coursework	First Graduating Cohort Bound
New Hampshire	Incorporated (Economics)	1993
New York	Incorporated (Economics)	1996
Michigan	Incorporated (Career Skills)	1998
Wyoming	Incorporated (Social Studies)	2002
Louisiana	Incorporated (Free Enterprise)	2005
Arkansas	Incorporated (Economics)	2005
Arizona	Incorporated (Economics)	2005
South Dakota	0.5 Credit (Economics or Personal Finance)	2006
Georgia	Incorporated (Economics)	2007
Texas	Incorporated (Economics)	2007
Idaho	Incorporated (Economics)	2007
North Carolina	Incorporated (Economics)	2007
Utah	0.5 Credit	2008
Colorado	Incorporated (Economics, Math)	2009
South Carolina	Incorporated (Math, ELA, Social Studies)	2009
Missouri	0.5 Credit	2010
Iowa	Incorporated (21st Century Skills)	2011
Tennessee	0.5 Credit	2011
New Jersey	2.5 Credits (Economics or Personal Finance)	2011
Kansas	Incorporated (Economics)	2012
Oregon	Incorporated (Social Studies)	2013
Virginia	0.5 Credit	2014
Florida	Incorporated (Economics)	2014

PFL mandate data are from Stoddard and Urban (2019). States marked Incorporated require personal finance coursework in the required course denoted in parenthesis. States with listed credit requirement require the denoted number of credits in a standalone required personal finance course. States with a choice of Economics or Personal Finance have personal finance course standards in both courses.

In addition to the College Scorecard, I use the national rollout of personal finance education mandates since 1990 from Stoddard and Urban (2019) which is detailed in Table 2. They define the effective year of PFL mandate by the first high school graduating class that is bound by a mandate. PFL standards are most often included in other required courses such as Social Studies, Economics, and Math. However, several of the more recently adopting states have started requiring students to complete a standalone course in personal finance.

Lastly, I use data from the Integrated Postsecondary Education Data System (IPEDS) which includes biannual counts of the incoming cohort of students by previous state of residence for each university. Between 1986 and 1994, these data were collected every two years from each university and after 1994, universities could voluntarily provide these data to IPEDS in odd years but were required to submit in even numbered years. I use counts of first-time degree seeking students who graduated high school within 12 months of entering college. I replace missing student counts in odd years with linearly interpolated values from neighboring even years.[24]

Additional information on these data sources along with supplemental data sources are detailed in Appendix A.2.

4 Empirical Strategy

4.1 Identification

To motivate the empirical strategy introduced in the next section suppose a researcher is able to randomize across a population of N students whether student i will be required to be exposed to PFL topics during high school where $D_i = 1$ denotes those randomly assigned to the mandate and $D_i = 0$ otherwise. After high school, the researcher is able to track an outcome, y_i, for each student i. Due to the random allocation of D_i, the researcher can estimate the causal effect of personal finance education by comparing outcomes across $D_i = 0$ and $D_i = 1$ using the regression specification

$$y_i = \alpha + \gamma^{RCT} D_i + \varepsilon_i$$

where γ^{RCT} is the estimate of the Average Treatment Effect (ATE) of mandated personal finance education on outcome y.[25] Due to the infeasibility of an intervention of this type, a second-best alternative to estimate the causal effect is to exploit a natural experiment in which state policy changes divide the population into mandated students and non-mandated students. Outcomes are then compared across the two populations. Under this difference-in-differences framework, the estimating equation then becomes

$$y_{ist} = \alpha + \gamma^{DD} D_{st} + \delta_{st} + \varepsilon_{ist}, \tag{1}$$

where D_{st} now denotes whether state s had a binding mandate for cohort t and δ_{st} is a state-by-year fixed effect. y_{ist} denotes an outcome variable for individual i belonging to graduating cohort t from

[24] In Appendix A.4.2, I instrument student counts using a combination of fixed effects, observable policy changes, and linear and quadratic trends to replace missing values with estimated values.

[25] Since we have randomization across D_i, we have $E(\varepsilon_i | D_i) = 0$.

state s.[26] The outcome variable can be rewritten using the potential outcomes framework so that

$$y_{ist} = y_{1,ist} \cdot D_{st} + y_{0,ist} \cdot (1 - D_{st})$$

where $y_{1,ist}$ denotes the outcome for an individual if they are bound by a state mandate and $y_{0,ist}$ denotes the outcome if the same individual is not bound by a state mandate. In reality, the researcher only observes either $y_{1,ist}$ or $y_{0,ist}$ for any given individual. However, if the researcher assumes students in states not bound by a state mandate evolve similarly to the unobserved non-mandated students in mandated states, γ^{DD} can be interpreted as the Average Treatment Effect on the Treated (ATT). More formally, suppose there are only two cohorts ($t = 0, 1$) and the state adopting a mandate adopts for the second cohort ($t = 1$). The requisite Parallel Trends Assumption states that

$$E[y_{0,is1} - y_{0,is0} \mid D_{s1} = 0] = E[y_{0,is1} - y_{0,is0} \mid D_{s1} = 1].$$

Under this assumption, observed outcomes for students in the non-adopting states are used as the unobserved counter-factual outcomes for students in the adopting states and the parameter γ^{DD} captures the impact of the state adopted personal finance education on the outcomes for the students who were treated.

However, since micro-level data of this type is not available, consider the case where outcomes are only observed at the university-level for university j. Suppose there exists a function G that maps each student $i \in \mathcal{I}$ to a university $j \in \mathcal{J}$.[27] The outcome $Y_{j\tau}$ is defined as

$$Y_{j\tau} \coloneqq \frac{1}{|J_{j\tau}|} \sum_{i \in J_{j\tau}} y_{ist}$$

where $J_{j\tau}$ is the set of students that attend school j, $|J_{j\tau}|$ is the number of students in the set $J_{j\tau}$, and $\tau = t + k_i$ for some k_i which defines the number of periods between graduating high school and appearing in the university-level outcome for student i. Under the enumerated assumptions below, I show in Appendix A.1 that the parameter γ^{DD} can be consistently estimated using the aggregated estimating equation:

[26] In this example, the data are repeated cross section and an individual i is unique to a cohort t and y_{ist} is only observed once per individual.

[27] For example, the universities can be indexed such that $\mathcal{J} = \{0, 1, .., J\}$ such that $j = 0$ denotes no university attendance and $j = 1, ..., J$ denotes university attendance.

$$Y_{j\tau} = \alpha + \gamma^{DD}\text{pctBound}_{j\tau} + \delta_j + \delta_\tau + e_{j\tau} \tag{2}$$

where the term attached to γ^{DD}, $\text{pctBound}_{j\tau}$, corresponds to the fraction of the cohort τ for university j that were bound by state personal finance education mandates. The necessary assumptions for identification are:

1. Parallel Trends Assumption: $E[\Delta y_{0,ist} \mid D_{s(i)t} = 0] = E[\Delta y_{0,ist} \mid D_{s(i)t} = 1] \;\; \forall t$
2. Cohort Matching Assumption: $k_i = k \;\; \forall i$
3. Stability of University Mapping: $G(\, i, D_{s(i)t} = 1) = G(\, i, D_{s(i)t} = 0)$

As discussed above, we require the Parallel Trends Assumption in order to satisfy the micro-level difference-in-differences identification strategy. Next, it should be the case that for all i, $k_i = k$. If students in the same high school cohort t enter into different university repayment cohorts τ then it is possible that PFL mandates begin affecting $Y_{j\tau}$ prior to the first mandated cohort as matched by k. This leakage of treated high school cohorts into untreated repayment cohorts will attenuate the estimate of γ^{DD}. Lastly, it must be the case that assignment of D_{st} does not change the choice of university for students. If students respond to personal finance education by altering the assignment to $J_{j\tau}$, then the estimate from the aggregated specification captures γ^{DD} *plus any potential compositional change* in $J_{j\tau}$ that might affect $Y_{j\tau}$.

Under these assumptions, we have that Equation (2) consistently estimates γ^{DD} which is the causal ATT (Average Treatment Effect on the Treated) estimated from the micro-level difference-in-differences specification. Section 5.2 presents evidence in support of these assumptions and tests the robustness of the results to a loosening of assumptions.

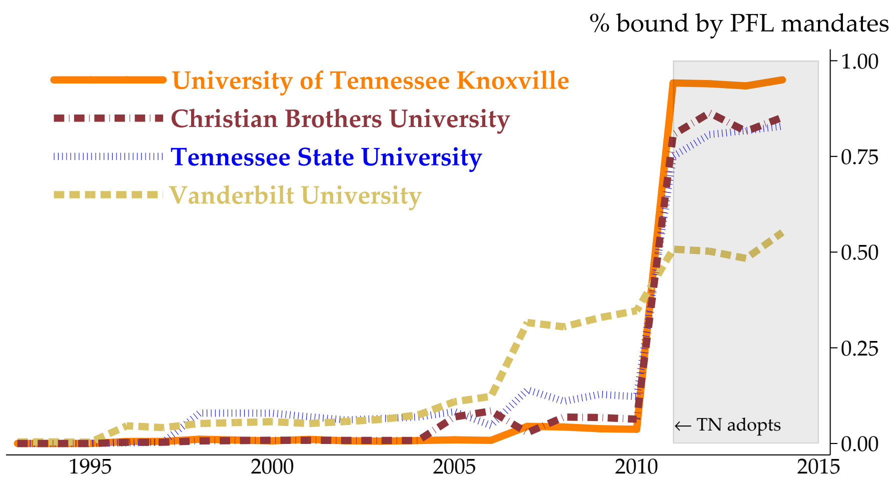
4.2 Dose Response Specification

To implement the proposed specification above, I begin by quantifying the share of each incoming cohort bound by PFL mandates in each cohort. Variation at the university-level is driven by two components. The first is through the state adoption of course mandates. When a state changes course standards for high school graduation, all future high school students within the state are affected by this change. When these students graduate high school and proceed to college, universities that receive these students are now populated by these affected students. This is most often universities within the adopting state, however across-state migration of high school students to colleges allows for spillovers from adopting states to non-adopting states. In addition, students from non-adopting states drive down the exposure at colleges in adopting states.

I use the IPEDS previous state of residence data to track within and across-state migration of high school students to colleges. For each university i and incoming cohort t, I construct pctBound_{it} by interacting the state-by-year mandate status of state j for cohort t (pfMandate_{jt}) with the number of students attending university i in cohort t from state j (enroll_{ijt}). The total number of mandated students is then divided by the total incoming cohort count from all 50 states and D.C. for cohort t:

$$\text{pctBound}_{it} = \frac{\sum_{j=1}^{51} \text{pfMandate}_{jt} \times \text{enroll}_{ijt}}{\sum_{j=1}^{51} \text{enroll}_{ijt}}. \tag{3}$$

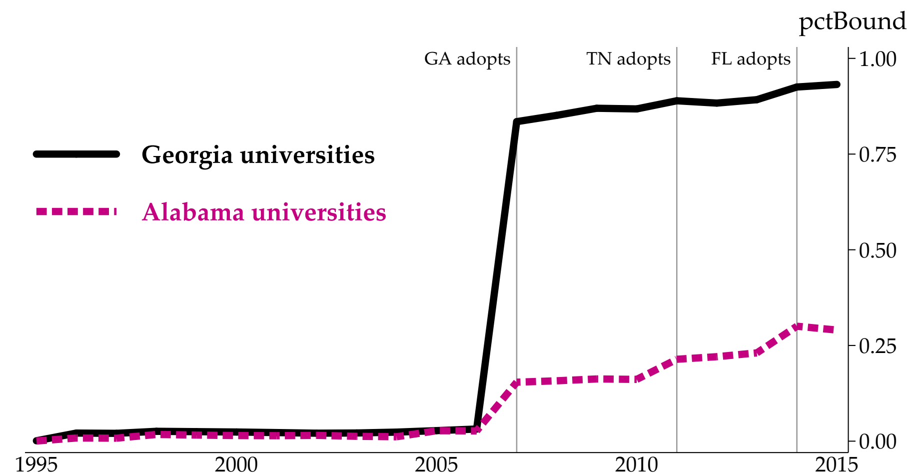
Figure 3: Examples of Within State Variation in pctBound from Tennessee



The figure above plots pctBound_{it} as constructed in Equation (3) for each university over time. Data on previous state of residence come from IPEDS. State of residence data that are not submitted to IPEDS in odd years are interpolated linearly from neighboring even years. The state of Tennessee adopted a personal finance mandate that was first binding for the class of 2011 as shown by the shaded region in each plot.

Figure 3 shows an example of how pctBound_{it} evolves over time for a select group of Tennessee universities. The first graduating class bound by Tennessee's personal finance mandate was the class of 2011. Typically public universities receive a large share of their student body from within the state. However, public universities like University of Tennessee Knoxville and Tennessee State University still differ in the share of students from within-state which drives variation in pctBound after a state adopts a PFL mandate. This heterogeneous impact is even more stark for private universities like Vanderbilt University and Christian Brothers University. Despite the private status, the impact of Tennessee's mandate adoption is larger for Christian Brothers University than for Tennessee State University while Vanderbilt University experiences a smaller shock to pctBound after 2011.

Figure 4: Example of Across State Spillovers in pctBound



The figure above plots the state level equivalent of pctBound_{it} where all universities within a state are aggregated. Data on previous state of residence come from IPEDS. Georgia's mandate was first binding for the class of 2007. Alabama did not a adopt mandate during the sample period but is affected by Georgia's adoption through students from Georgia high schools attending Alabama universities and again by Tennessee and Florida's adoption.

In addition to the within-state variation, high school students attending college in other states cause non-adopting states to be affected by mandates in adopting states. Figure 4 shows the state level equivalent of pctBound_{it} where the student bodies of all universities within a state are aggregated. When Georgia adopted a mandate binding for the class of 2007, Alabama universities experienced a corresponding increase in pctBound_{it} due to Georgia's adoption. Additionally, Alabama universities experienced subsequent increases in pctBound when Tennessee and Florida adopted in 2011 and 2014, respectively.

I exploit this unique source of exogenous variation in personal finance education exposure to estimate the effect of changes in pctBound_{it} on university-level student loan repayment outcomes.

The main specification for the dose response model as motivated by Equation (2) is:

$$y_{is,t+k} = \gamma \text{pctBound}_{it} + \beta \mathbf{X}_{ist} + \delta_i + \delta_t + v_{ist}, \tag{4}$$

where $y_{is,t+k}$ is an outcome for university i located in state s for high school cohort t and k is the number of periods between cohort t entering college and outcome y being observed. The coefficient of interest is γ which estimates the causal effect of increasing pctBound from zero to one.

A vector of control variables are also included in $\mathbf{X}_{ist}$ to control for other state level changes that might also affect federal student loan repayment. First, I include controls for the number of credit hours required for high school graduation for math, science, English, and social studies along with the total number of credit hours required. Since the introduction of PFL state standards might be introduced at the same time as changes to other course standards, these controls insure γ is not capturing the effect of changes to other course requirements. Next, I include state level counts of high school staffing for teachers, support staff, and guidance counselors. Changes to state standards might also be accompanied by other state legislation that could increase students' access to college counseling or change student-to-teacher ratios. Without these controls, γ can be biased upward as a result of omitted variable bias. I also include controls for whether cohorts had access to state merit aid scholarships since these may affect where students choose to attend college and how much students pay to attend college. Lastly, I include a vector of unemployment rates between periods t to $t + k$ to control for the local labor market students face during college and into loan repayment. The data sources and construction of these variables are detailed more thoroughly in Appendix A.2.

Following Assumption 2 above, I match high school graduating cohorts to university repayment cohorts by assuming that students enter repayment after their fourth year of college. Under this assumption, a student graduating high school in year t will enter repayment in fiscal year $t + 5$ and first enter the College Scorecard database in year $t + 6$. As such, all specifications will assume that $k = 6$ for repayment outcomes and $k = 4$ for student loan debt upon entering repayment. I present evidence in support of this assumption along with tests of the sensitivity to the assumptions in Section 5.2.

4.3 Inference

It is likely that universities within the same state experience common unobserved shocks. Therefore, in the baseline specification, I cluster standard errors at the state level to allow for correlation in the error term, ε_{ist}, between universities in the same state s. However, since treated students are migrating across states to attend college, it is likely that universities in different states also experience common unobserved shocks. If this is the case, errors might be correlated for universities *across states* and, consequently, clustering at the state level may produce standard errors that are too small (Barrios, Diamond, Imbens, and Kolesár, 2012). It is common practice in the treatment effects literature to cluster standard errors at the level of treatment (Bertrand, Duflo, and Mullainathan, 2004; Cameron and Miller, 2015). In this setting, it is not straight-forward to define the "level of treatment" because each college has a different level of exposure to treatment from various states.

To address this concern, I conduct a randomization inference exercise in the spirit of MacKinnon and Webb (ming). I estimate many "placebo" replications of Equation (4) where pfMandate_{jt} is drawn from $\{0, 1\}$ at random. In each replication, I construct placebo pctBound_{it} using the randomly drawn pfMandate_{jt} and the observed enroll_{ijt}. I then estimate Equation (4) using the placebo pctBound_{it} to generate placebo $\hat{\gamma}$ estimates. Since the states adopting mandates in the placebo replications are drawn randomly, it must be the case that the γ estimates from this exercise equal zero on average. If $\hat{\gamma}$ estimated using the observed pctBound measure is a sufficiently extreme value in the distribution of placebo estimates, the null hypothesis of no treatment effect can be rejected.

The empirical p-values generated in this algorithm use the distribution of estimated γ coefficients without regard to the standard errors or any assumptions about the correlation structure of the data generating process. Instead, the underlying data generating process of students migrating to universities is captured in the empirical distribution of γ estimates. As a result, the empirical p-values are robust to both within- and across-state correlation of universities driven by enroll_{ijt}. This algorithm is detailed in its entirety in Appendix A.3.

5 Results

5.1 Main Results

The results from the estimation of Equation (4) are presented in Table 3. Column 1 reports the estimates for the two-year cohort default rate while Columns 2 through 6 report the estimates for the various subsamples of the repayment cohort for the one-year repayment rate. The effect of personal finance mandates on defaults suggests a reduction of 0.2 percentage points (a 5% reduction from the mean) associated with a full dose treatment of the incoming cohort. The magnitude of this estimate is economically meaningful but is not statistically different from zero at conventional levels.[28]

Table 3: Dose Response Estimates: Cohort Default Rate and Repayment Rate

	Default Rate	Repayment Rate				
	(1) Overall	(2) Overall	(3) First Gen	(4) Low Income	(5) Middle Income	(6) High Income
pctBound	-0.002	0.013	0.024	0.023	0.009	0.010
	(0.447)	(0.168)	(0.029)	(0.034)	(0.344)	(0.348)
	[0.500]	[0.219]	[0.032]	[0.037]	[0.364]	[0.456]
Universities	1,386	1,384	1,340	1,354	1,319	1,317
Cohorts	1993-2006	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	0.042	0.594	0.536	0.458	0.631	0.749
Percentage Effect	-5.0%	2.2%	4.5%	5.1%	1.5%	1.3%

Regressions are weighted using the number of students used to compute each outcome metric. Each column reports a coefficient from a separate regression where the independent variable is pctBound and the outcome is denoted in the column header. The sample includes four-year universities. Default rate analysis includes high school graduating classes 1993 through 2006 and repayment rate analysis includes high school graduating classes 2001 through 2008 due to data availability. First Gen students are defined as students whose parents did not have a college degree. Low Income, Middle Income, and High Income students are defined as household income less than 30,000, between 30,000 and 75,000 and above 75,000, respectively. Controls include cohort weighted credit requirements in math, English, social studies, and science by high school graduation cohort and controls for state level high school staffing, and availability of merit aid scholarships. Also included are university and high school graduation year fixed effects. P-values using standard errors clustered at the state level are presented in parenthesis. Empirical p-values using randomization inference are presented in brackets.

Columns 3 and 4 suggest that increased exposure to personal finance education mandates improves the one-year repayment rate for both first generation and low income students. The point estimates translate to a 2.4 and 2.3 percentage point increase in the repayment rate which

[28]The results are similar for the default rate if the sample years are limited to match the data availability years of the repayment rate analysis.

corresponds to improvements of 4.5% and 5.1% for first generation and low income students, respectively. Both results are significant at the 5% level regardless of using clustered standard errors (presented in parentheses) or RI-β randomization inference (presented in brackets) to generate p-values. Further, the p-values using both methods are remarkably similar. The point estimates for the overall repayment cohort and for middle and high income students are all positive, but are not statistically different from zero using either p-values. This pattern suggests that personal finance education mandates improve the one-year repayment rate for first generation and low income students.

Table 4: Dose Response Estimates: Cohort Default Rate and Repayment Rate for Public and Private Universities

	Default Rate	Repayment Rate				
A. Public	(1) Overall	(2) Overall	(3) First Gen	(4) Low Income	(5) Middle Income	(6) High Income
pctBound	-0.003	0.017	0.025	0.022	0.011	0.020
	(0.166)	(0.098)	(0.025)	(0.031)	(0.295)	(0.097)
	[0.429]	[0.058]	[0.009]	[0.035]	[0.223]	[0.023]
Universities	450	450	449	449	445	445
Cohorts	1993-2006	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	0.046	0.593	0.551	0.478	0.630	0.730
Percentage Effect	-5.9%	2.8%	4.5%	4.7%	1.8%	2.8%
B. Private						
pctBound	0.001	-0.009	0.007	0.009	-0.007	-0.014
	(0.877)	(0.682)	(0.676)	(0.716)	(0.660)	(0.250)
	[0.820]	[0.370]	[0.981]	[0.704]	[0.242]	[0.070]
Universities	936	934	891	905	874	872
Cohorts	1993-2006	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	0.036	0.595	0.512	0.423	0.632	0.777
Percentage Effect	3.2%	-1.5%	1.3%	2.1%	-1.1%	-1.8%

Regressions are weighted using the number of students used to compute each outcome metric. Each column reports a coefficient from a separate regression where the independent variable is pctBound and the outcome is denoted in the column header. The sample includes public and private four-year universities. Default rate analysis includes high school graduating classes 1993 through 2006 and repayment rate analysis includes high school graduating classes 2001 through 2008 due to data availability. First Gen students are defined as students whose parents did not have a college degree. Low Income, Middle Income, and High Income students are defined as household income less than 30,000, between 30,000 and 75,000 and above 75,000, respectively. Controls include cohort weighted credit requirements in math, English, social studies, and science by high school graduation cohort and controls for state level high school staffing, and availability of merit aid scholarships. Also included are university and high school graduation year fixed effects. P-values using standard errors clustered at the state level are presented in parenthesis. Empirical p-values using randomization inference are presented in brackets.

Table 4 repeats the estimation separately for public and private universities in Panel A and Panel B, respectively. The qualitative results are largely unchanged when moving from the full sample in Table 3 to the public university sample in Table 4. First generation and low income students at public universities have higher repayment rates as a result of PFL mandates. The point estimate and proportional impact for the cohort default rate is again negative but the estimate remains imprecise. In contrast to the results above, high income students at public universities experience a 2.8% improvement in the repayment rate as a result of PFL mandates which is significant at the 10% level using CRVE and at the 5% using RI-β p-values. This result is contrary to several findings in the literature which find smaller or no effect of course mandates for students from more affluent backgrounds (Stoddard and Urban, 2019; Goodman, 2019). It is possible that one or more of the mechanisms that cause the improvement in repayment rates operates in a different manner for low income students than for high income students.

The results presented in Panel B suggest there is no significant impact of personal finance education mandates for the sample of private universities. It is possible that the smaller and more frequent shocks to pctBound experienced by many private schools result in a loss of precision in the estimation of the treatment effect. It may also be the case that some states do not require all private high school students to adhere to PFL mandates and these students also attend private universities. The estimated treatment effect on repayment rates is smaller than a 2.1% improvement and many point estimates actually suggest worsening outcomes. As a result, the remaining analysis will focus on the sample of public universities.

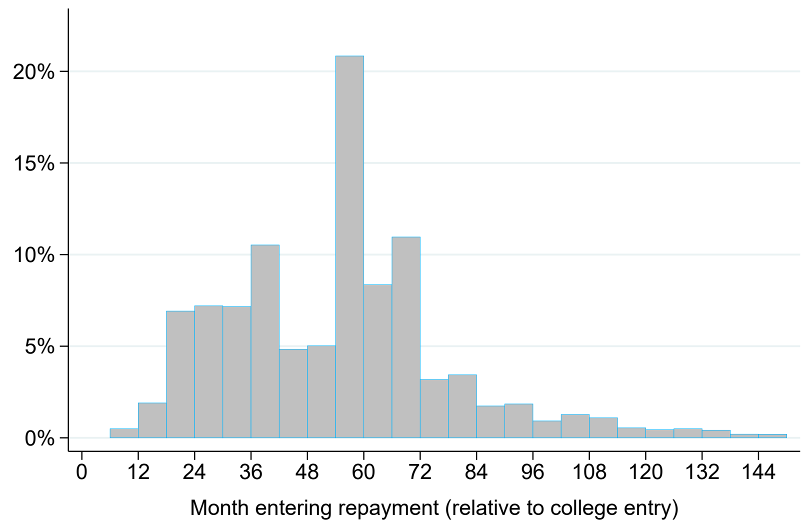
5.2 Tests of Identification Assumptions

As discussed in Section 4.1, the estimates presented above can only be interpreted as the micro-level ATT if Assumptions 1 through 3 are satisfied. This section provides evidence for each assumption.

5.2.1 Cohort Matching Assumption

In Figure 5, I show the distribution of the month students in the 2003 incoming college cohort entered repayment to support the choice of $k = 6$ as the appropriate lag between entering college and observing repayment outcomes. The model month students entered repayment is 51 months after entering college. This timing is consistent with a student entering college in August, graduating in May of her fourth year, and entering repayment in November (after the six month grace period). From this data, 43% of the repayment cohort enter repayment between 48 months and 72 months after entering college. However, the data show a significant percentage of students entering repayment prior to 42 months since entering college. If these are students bound by personal finance education mandates, it is possible they contribute to a repayment cohort that is inconsistent with the assumption of $k = 6$. The specification in the next section will test whether students separating from college prior to four years impact the repayment rate for a university.

Figure 5: Month Entering Repayment for 2003 High School Cohort



The figure above plots a histogram of the month a borrower enters repayment relative to the month they enter college for four-year college students who did not attend graduate school. The sample includes federal student loan borrowers from the cohort entering college in the 2003-2004 academic year. Students who attended graduate school are removed since they would mechanically enter into repayment at a later month. Total student counts are collapsed into six month bins and nationally representative weights are used to create cohort shares.
Source: U.S Department of Education, National Center for Education Statistics, 2004/2009 Beginning Postsecondary Students Longitudinal Study Restricted-Use Data File.

5.2.2 Parallel Trends Assumption

Identification of the causal effect of PFL education on student loan repayment outcomes relies on the Parallel Trends Assumption. This assumption is not directly testable since counter-factual outcomes for treated units are unobserved. Instead, I present evidence that universities more exposed to PFL mandates were not trending differentially prior to the adoption of mandates by estimating a flexible event study specification. The event study specification includes a vector of binary variables in which each variable represents a time period relative to the start of treatment for units experiencing an event. Each parameter then estimates the difference in outcomes between units experiencing an event and units not experiencing an event during the time period relative to treatment. In this context, the treatment variable is not discrete and thus care must be taken to define the first period of treatment. I define a university event as a year-over-year change in pctBound_{it} of 50 percentage points or larger:

$$\text{event}_{ist} = 1 \cdot \{\text{pctBound}_{it} - \text{pctBound}_{i,t-1} \geq 0.5\}. \tag{5}$$

I choose the 50 percentage point threshold to ensure a university can only experience one event. Table A.4 details the number of universities experiencing an event by this definition in each academic year along with the states adopting in each year. Between 1996 and 2014, 524 universities experience an academic year in which the adoption of at least one mandate changes pctBound_{it} by at least 50 percentage points. Although there are a few early adopting states, most of the events occur for the high school graduating cohorts of 2005 and later. As a result, there is more outcome data available for the periods prior to an event than for the periods after an event. The estimating equation for the event study is identical to Equation (4) aside from the event study parameters:[29]

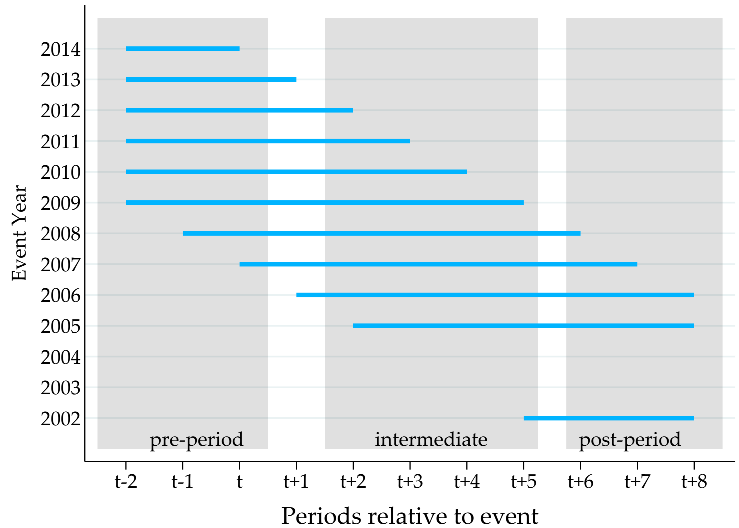
$$y_{is\tau} = \sum_{j=-2}^{0} \gamma_j \text{event}_{is,t+j} + \sum_{j=2}^{10} \gamma_j \text{event}_{is,t+j} + \beta \mathbf{X}_{ist} + \delta_i + \delta_\tau + \varepsilon_{is\tau}. \tag{6}$$

In this specification, the event occurs in period t and a separate parameter is estimated for each period relative to an event with period $t+1$ being omitted.[30] This results in ten estimated parameters across the event space. Since the College Scorecard only contains eight years of data for the repayment rate, the window for each university's outcome will not span the entirety of the range of event study parameters. Hence, the event study coefficients represent a combination of the dynamic effect of each university's change in outcomes over time plus a heterogeneous effect of universities entering and exiting the identification of the parameter space. This is more clearly shown in Figure 6 which plots the identifying variation of each event across the event

[29]The vector of control variables $\mathbf{X}_{is,t+6}$ uses the assumed matching high school cohort corresponding to repayment outcome $y_{is\tau}$.

[30]Since it takes at least one year for the repayment rate outcome to be observed, period $t+1$ is the last period before treated students can begin contributing to a university's repayment rate for each university. A treated student entering the repayment rate data in year $t+1$ would be a student who separated from college prior to the end of the first year.

Figure 6: Contribution to Event Study Parameters by University Event Year

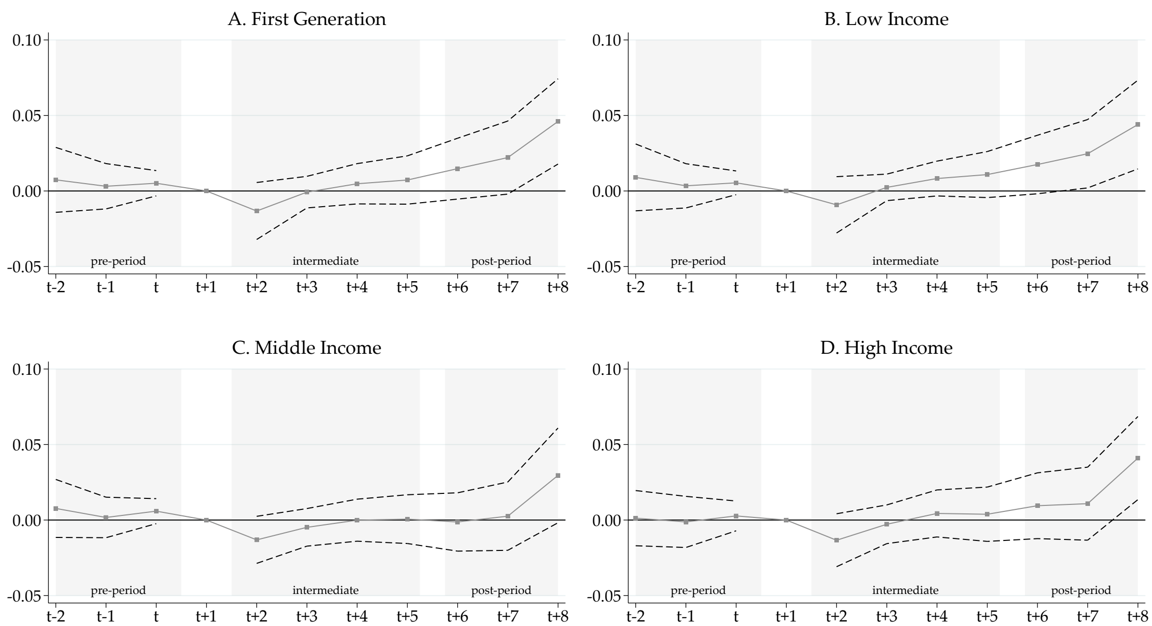


The above graph shows how each university contributes to each event study parameter by the year of university event. Universities experiencing an event in 2009 and later do not contribute to the post-event parameters while universities experiencing an event before 2007 do not contribute to the pre-event parameters. States adopting between 2006 and 2008 contribute to both the pre-period and post-period parameters.

study parameters. Since the data is both right and left censored, the universities that identify the pre-period (periods $t-2$ through period t) are largely universities located in the states adopting after the 2008 high school graduating class. On the other hand, the universities identifying the post-period (periods $t+6$ through $t+8$) are the states adopting in 2008 and prior. The parameters corresponding to periods $t+2$ through $t+5$ represent an intermediate range in which it is possible that treated students enter the Scorecard data if they leave college prior to the assumed four year spell. This intermediate range of parameters tests for changes to repayment outcomes as a result of non-completing treated students.

Figure 7 reports the coefficients and 95% confidence intervals for the estimation of Equation (6) for the repayment rate subgroups at public universities. In all four panels, there is no differential trend between universities experiencing an event and those not experiencing an event prior to treated students entering college. Further, there does not appear to be a significant effect on university repayment rates during the intermediate periods for any of the subsamples. The estimated coefficient for period $t+2$ is negative for all groups and represents the smallest parameter estimate but is rather small in magnitude. Although none of the intermediate estimates is distinguishable from zero, the upward trend suggests non-completers exposed to personal finance mandates may also be better at repaying student loans. The estimates for the post-period parameters for first generation (low income) students range from 1.5 percentage points (1.1 percentage points) in period $t + 6$ to 4.6 percentage points (4.4 percentage points) in period $t + 8$ which is consistent with the point estimates presented in the main specification. The results in Panels C and D for middle and high income students suggests no significant effect until period $t + 8$ and is largely consistent with the muted estimates presented in Table 4.

Figure 7: Event Study Coefficients for Sample of Public Universities



Each panel in the above figure presented the vector of event study parameters with period $t + 1$ omitted as the reference period. Since the repayment rate data takes at fewest one year to enter the College Scorecard, it is not possible for a member of high school cohort t to contribute to the repayment outcome in $t + 1$. However, early separators can contribute to the parameters $t + 2$ through $t + 5$. Period $t + 6$ represents students who spend four years in college and periods greater than $t + 6$ represent students from cohort t who spent longer than four years in school or students in cohorts greater than t which were also bound by PFL mandates.

In total, these results present evidence in support of the Parallel Trends Assumption and in support of the Cohort Matching Assumption. In each event study specification, the coefficients corresponding to periods prior to an event are both small and indistinguishable from zero. The same is true for each of the parameters corresponding to periods in which treated non-completers might contribute to the university repayment rate. Lastly, the parameters corresponding to the post-periods suggest that first generation and low income students have higher repayment rates.

The point estimates suggest that when the third cohort after the event has spent four years in college, repayment rates for first generation and low income students are 4.6 and 4.4 percentage points higher, respectively.

5.2.3 Stability of University Mapping Assumption

In order for university-level outcomes to appropriately proxy aggregated micro-level outcomes, it must be the case that exposure to PFL education does not alter the university a student chooses to attend. If exposure to required PFL coursework shifts the mapping of students to universities, changes in federal student loan repayment outcomes at the university-level might be due to compositional shifts in the student body as a result of PFL mandates. In this case, it is possible to detect improvements in university-level repayment outcomes without any micro-level improvements.

To test this assumption, I use the IPEDS previous state of residence data to track the flow of high school students from each state into the colleges they ultimately enroll.[31] Recall the IPEDS data includes the variable enroll_{ijt} which is the number of students in the incoming cohort for university i who previously resided in state j for incoming cohort t. Instead of aggregating student counts at the university-level, I can instead aggregate student counts at the previous state of residence level according to universities characteristics. This procedure generates the percent of high school students from each state attending universities of a given type. Equation (7) illustrates an example of this variable construction using the university characteristic Public4yr_i, which equals one if a university is a four-year public college and Seniors_{jt} is the total number of enrolled high school seniors for the graduating cohort t.[32]

$$\text{pctPublic4yr}_{jt} = \frac{\sum\limits_{i \in I} \text{Public4yr}_i \times \text{enroll}_{ijt}}{\text{Seniors}_{jt}} \tag{7}$$

The constructed variable, pctPublic4yr_{jt} measures the percent of high school seniors from state j in cohort t who enrolled in a public four-year university. I create analogous variables for any two-year and four-year college and for two-year and four-year public, private non-profit, private for-profit, and in-state universities.

I use these constructed variables as outcome measures for a state-by-year difference-in-differences

[31] Stoddard and Urban (2019) perform a similar test using the IPEDS enrollment data and counts of the number of 18 year olds in a state in a given year.

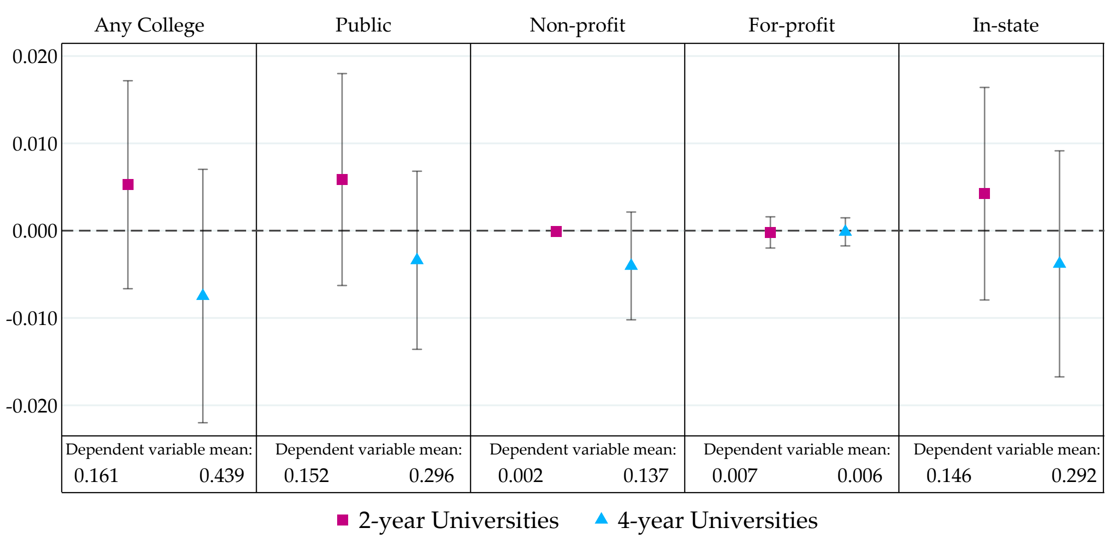
[32] The variable Seniors_{jt} comes from enrollment counts from the Department of Education's Common Core of Data.

design to test whether the state adoption of a personal finance education mandate alters the share of students attending various types of colleges. The estimating equation is similar to Equation (4):

$$y_{jt} = \gamma \text{pfMandate}_{jt} + \beta \mathbf{X}_{jt} + \delta_j + \delta_t + \varepsilon_{jt}, \tag{8}$$

where y_{jt} is a state-level variable, such as pctPublic4yr_{jt}, and pfMandate_{jt} is a binary variable denoting whether state j had a binding personal finance mandate in effect for cohort t. The vector X_{jt} is the same set of state-by-graduation year level controls as in Equation (4) and δ_j and δ_t are state and year fixed effects, respectively.[33]

Figure 8: Difference-in-Differences Estimates for Changes to College Enrollment



The figure above plots a separate difference-in-differences coefficient estimate and corresponding 95% confidence interval where the independent variable is pfMandate and the outcome variable is denoted for each column. Each outcome is reported for two-year universities with a square and four-year universities with a triangle. Control variables include state level counts of high school staffing, other high school graduation credit requirements for math, English, social studies, and science, and the availability of state merit aid scholarships.

Figure 8 presents point estimates and 95% confidence intervals for this specification.[34] All reported point estimates a smaller than a one percentage point change in either direction and no estimate is statistically different from zero at any conventional significance threshold. The results are consistent with the findings in the literature which find no changes in college attendance or the

[33]These controls include state level counts of school staffing, other course requirements for graduation, and the availability of state merit aid scholarships.

[34]The full table of results are available in Tables A.5 and A.6.

choice of college as a result of a binding personal finance education mandate (Stoddard and Urban, 2019).

5.2.4 Robustness to Changes in College Enrollment

In this section, I present evidence that the results presented above are robust even in the case of changes to student enrollment decisions. I estimate a variation of Equation (4) using an alternate construction of pctBound. This specification is motivated by a "shift-share" framework in which exposure levels are held constant at initial levels and the variation in the identifying variable is driven by an interaction of the constant shares and an aggregate trend (Bartik, 1987).[35] Hence, the identifying variation in this model does not rely on transitory changes in high school students' college choice but rather each university's exposure to each state's potential adoption of PFL mandates in the period before PFL mandate adoption. Applying this framework to the construction of pctBound, I construct $\overline{\text{enroll}}_{ij,S}$ which is the mean enrollment of the students from state j at university i for a set of academic years S. The construction of $\widehat{\text{pctBound}}_{it}$ takes the form:

$$\widehat{\text{pctBound}}_{it} = \sum_{j=1}^{51} \left[\frac{\overline{\text{enroll}}_{ij,S}}{\overline{\text{enroll}}_{i,S}} \right] \times \text{pfMandate}_{jt} \quad . \tag{9}$$

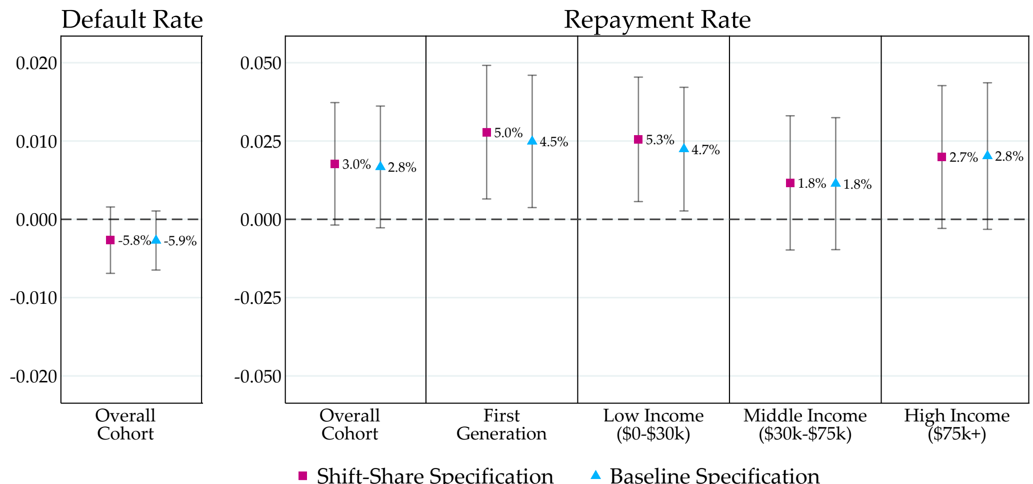
For this analysis, the set S contains the IPEDS state of residence counts from 1986 through 1994 as this period largely contains state composition before the rollout of personal finance mandates. Summing over all states and D.C. yields the mean total enrollment $\overline{\text{enroll}}_{i,S}$ for university i during the set of years S. Hence, the fixed share of students from university i from state j can be derived as the ratio of $\overline{\text{enroll}}_{ij,S}$ to $\overline{\text{enroll}}_{i,S}$. When there is no change in PFL mandate adoption from year t to year $t+1$, there is no change in $\widehat{\text{pctBound}}_{it}$ to $\widehat{\text{pctBound}}_{i,t+1}$. However, if state J adopts a mandate between graduating cohort t and graduating cohort $t+1$, the difference in $\widehat{\text{pctBound}}_{i,t+1}$ and $\widehat{\text{pctBound}}_{it}$ is exactly equal to the ratio $\overline{\text{enroll}}_{iJ,S}/\overline{\text{enroll}}_{i,S}$, or state J's historical composition for university i.

Figure 9 plots the estimates from this alternative specification relative to the estimates from the baseline specification for the sample of public universities. For all outcome variables, the point estimates are largely unchanged between the baseline specification and the shift-share specification.

[35] Recall in Appendix A.1 that identification of γ^{DD} requires that D_{st} does not affect college choice and that the reweighted state fixed effects by the state share but be included in the error term. However, this specification holds state shares fixed which allows for γ^{DD} to be consistently estimated without this assumption.

In fact, the point estimates using the shift-share specification represent larger impacts for both first generation and low income students. These results confirm that the improvements in PFL mandates stem from a university's exposure to PFL mandates rather than from transitory changes in student enrollment induced by PFL mandates.

Figure 9: Dose Response Shift-Share Estimates Relative to Baseline Specification for Public Universities



The figure above plots a separate coefficient estimate and corresponding 95% confidence interval where the independent variable is pfMandate and the outcome variable is denoted for each column. The estimates from the Shift-Share specification are reported with a square and the estimates from the baseline specification are replicated with a triangle. Proportional impacts are also printed to the right of each marker. The vector of control variables remains unchanged from the main specification. 95% confidence intervals are constructed using standard errors clustered at the state level.

5.3 Application: Universal PFL Mandates

To illustrate the magnitude of these estimates, I construct a hypothetical counter-factual using the results from the baseline specification. Consider a hypothetical case in which all high school students graduating between 2001 and 2008 were bound by state PFL mandates. Table 5 shows the total pctBound across all public universities for the incoming cohort matched to each cohort's one-year repayment rate. I also present the total number of students in each repayment cohort that successfully repaid at least one dollar of their loan balance one year after entering repayment.[36] The next panel shows how the one-year repayment rate changes when I apply the estimated ATT for the overall cohort from Table 4 scaled by the share of the student body not bound by mandates.

[36] Since the repayment rate is calculated using two cohorts, I divide the repayment cohort in half.

I also report the total number of students successfully repaying federal students loans under this assumption along with the estimated increase in the number of students meeting this metric. On average, around 9,000 additional students would have paid down at least one dollar of original principal after one year for a total of over 72,000 additional borrowers making progress on their loans.

Table 5: Hypothetical Effect of Universal Personal Finance Mandate

	Observed			Hypothetical		
HS Cohort	pctBound	Repayment Rate	Students Repaying	Repayment Rate	Students Repaying	Δ Students Repaying
2001	12.0%	71.7%	418,262	73.1%	426,868	+ 8,606
2002	12.0%	65.1%	333,385	66.6%	341,076	+ 7,691
2003	12.2%	60.9%	325,790	62.4%	333,815	+ 8,025
2004	12.6%	58.5%	342,921	60.0%	351,702	+ 8,781
2005	16.5%	56.5%	372,848	57.9%	382,312	+ 9,464
2006	17.0%	54.6%	413,066	56.0%	423,862	+ 10,796
2007	32.2%	53.4%	442,683	54.5%	452,285	+ 9,602
2008	33.7%	53.9%	460,313	55.0%	469,992	+ 9,679
						+ 72,644

The table above details a hypthetical exercise which assumes the estimated effect of PFL mandates is applied to all unmandated students in each entering cohort. The estimated treatment effect from Table 4 for the overall cohort is 0.017. The hypothetical repayment rate is computed by adding $(1 - \text{pctBound}_{ist})0.017$ to the observed repayment rate.

One important consideration is whether one-year repayment outcomes are predictive of long-term repayment success. Table 6 shows a summary of various long term repayment outcomes for a nationally representative sample of college students entering college in 2003. The sample is split by whether a student had paid down at least one dollar in principal one year after entering repayment. Students in this cohort who were able to pay down at least a dollar of their student loan debt one year after entering repayment were significantly better at repaying their loans 12 years after entering repayment. These students paid down nearly half of their loans while those not hitting the benchmark still owed 81% of their original balance. They were also half as likely to have defaulted on a student loan and were 19 percentage points more likely to have ever repair their student loans. Although not causal estimates, these comparisons suggest that improvements in the one-year repayment rate caused by PFL mandates could also lead to large future student loan repayment success for mandated students.

Table 6: Long-term Repayment Outcomes Conditional on One Year Repayment (Beginning Postsecondary Students 2004)

	Paid down principal after one year	
Outcome 12 years after entering college	Yes	No
Percent owed on balance	0.51	0.82
Ever defaulted on loan	0.12	0.24
Ever paid off loan	0.58	0.37
Remaining balance	$22,086	$36,814
Total Weighted Population	537,990	425,930

Source: U.S Department of Education, National Center for Education Statistics, 2004/2009 Beginning Postsecondary Students Longitudinal Study Restricted-Use Data File with the 2015 FSA Supplement. Estimates come from author's calculations. One year repayment rate metric is constructed by calculating the outstanding student loan balance one year after entering repayment and comparing to the outstanding balance upon entering repayment. Only borrowers who had entered repayment by the end of 2009 are considered to maintain consistent end dates.

5.4 Evidence of Mechanisms

5.4.1 Student Loan Debt

To test for changes in federal student loan borrowing, I estimate Equation (4) on moments from the student loan debt distribution and the median debt for subsamples of the student body from the College Scorecard. Table 7 reports the estimated effect of personal finance mandates on student loan debt upon entering repayment for public universities. The sample is restricted to the high school graduation years 2001 through 2008 to match the results presented above. The effect of increased exposure to personal finance education on student loan debt is proportionally small for the 10th, 75th, and 90th percentiles of the debt distribution for public universities. However, the estimates at the 25th percentile and the median represent imprecise reductions in student loan debt around 3%.

This effect can be further explored by tracing the effect on the same subsamples of the student body discussed above for the one-year repayment rate. The improvements in repayment rates were largest for low income and first generation students with smaller effects for high income students. Table 8 reports the effect of personal finance mandates on the median loan debt for subgroups of the public university cohorts. The point estimates are negative and imprecise for first generation and middle income students. On the other hand, the estimate for high income students is sizable

Table 7: Dose Response Estimates: Moments from Student Loan Debt Distribution for Public University Sample

	(1) 10th	(2) 25th	(3) 50th	(4) 75th	(5) 90th
pctBound	-2.8	-122.0	-305.2	-312.7	-144.3
	(50.1)	(127.3)	(234.6)	(269.9)	(377.1)
Universities	449	450	450	450	449
Cohorts	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	2480.3	4181.4	9516.4	18172.4	25959.9
Percentage Effect	-0.1%	-2.9%	-3.2%	-1.7%	-0.6%

Regressions are weighted using the number of students used to compute each outcome metric. Each column reports a coefficient from a separate regression where the independent variable is pctBound and the outcome is denoted in the column header. The sample includes public four-year universities. 10th, 25th, 50th, 75th and 90th each represent the correspondent moment in a unviersity's student loan debt levels for students entering repayment. Controls include cohort weighted credit requirements in math, English, social studies, and science by high school graduation cohort and controls for state level high school staffing, and availability of merit aid scholarships. Also included are university and high school graduation year fixed effects. P-values using standard errors clustered at the state level are presented in parenthesis.

and statistically significant, representing a decline in borrowing of around 8%. This heterogeneous response from personal finance education may help to explain the improvements in repayment for high income students. While there is little evidence of changes to borrowing patterns for first generation and low income students who saw the largest effects on repayment rates, it may be the case that high income students have better repayment rates as a result of lower student loan balances upon entering repayment. This could be due to a decision to borrow less or due to increases in grant or scholarship receipt as found in Stoddard and Urban (2019).

5.4.2 Information Intervention

In this section, I test whether students who were bound by personal finance education mandates in high school are better able to answer questions about financial literacy and federal student loans. I employ a difference-in-differences design with micro-level data from three nationally representative surveys. The estimating equation for each data source is similar and takes the form

$$y_{isjt} = \gamma \text{pfMandate}_{st} + \beta X_{isjt} + \delta_s + \delta_t + \delta_j + \varepsilon_{isjt}, \tag{10}$$

where y_{isjt} is a binary variable for whether respondent i from state s observed in survey wave j graduating from high school in year t correctly answered a particular question. I drop any respondent with a GED or no high school diploma since these students were not bound by state

Table 8: Dose Response Estimates: Median Student Loan Debt by Student Subsample for Public University Sample

	(1) First Gen	(2) Low Income	(3) Middle Income	(4) High Income
pctBound	-272.3	68.2	-281.9	-783.4
	(247.9)	(210.1)	(232.6)	(317.6)
Universities	449	450	446	446
Cohorts	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	9288.9	9289.7	9911.5	9602.7
Percentage Effect	-2.9%	0.7%	-2.8%	-8.2%

Regressions are weighted using the number of students used to compute each outcome metric. Each column reports a coefficient from a separate regression where the independent variable is pctBound and the outcome is denoted in the column header. The sample includes public four-year universities. First Gen students are defined as students whose parents did not have a college degree. Low Income, Middle Income, and High Income students are defined as household income less than 30,000, between 30,000 and 75,000 and above 75,000, respectively. Controls include cohort weighted credit requirements in math, English, social studies, and science by high school graduation cohort and controls for state level high school staffing, and availability of merit aid scholarships. Also included are university and high school graduation year fixed effects. P-values using standard errors clustered at the state level are presented in parenthesis.

graduation mandates. X_{isjt} includes a vector of binary control variables which includes race, gender, and education and the vector of state-by-graduation year controls for merit aid, high school staffing, and credit requirements as in Equation (4). γ is the parameter of interest which estimates the impact of a binding personal finance education mandate on the (linear) probability of correctly answering the question. Lastly, I include state, survey wave, and high school graduation year fixed effects and I use the included state-level survey weights so the analysis is representative of each state's population.[37] Standard errors are clustered at the high school state level.

The first survey is the National Postsecondary Student Aid Study (NPSAS) which includes a nationally representative sample of college students every two years. The 2016 wave of the NPSAS began asking college students three financial literacy questions and three questions pertaining to knowledge of federal student loan repayment.[38] These new data provide insights into the financial literacy of current college students that was previously not available. However, since these questions are only included in one survey wave, comparisons of mandated students to not mandated students from the same state largely rely on students surveyed at different ages. As a result, the estimate for γ potentially includes the effect of personal finance education mandates plus a bias term. Additionally, the 2016 wave of the NPSAS largely contains students that graduated high school after the period

[37]The weights included in the NPSAS:16 are nationally representative instead of state representative.

[38]The questions are detailed in Table A.11.

between 2001 and 2008 studied above. Regardless, the novelty of the questions asked in this survey necessitate its use.

In addition to the NPSAS, I also use data from the National Financial Capability Study (NFCS) and the Survey of Household Economics and Decisionmaking (SHED). The NFCS data contain waves from 2012, 2015, and 2018 while the SHED data contain waves from 2017 and 2018. Both surveys are nationally representative and each asks five financial literacy questions that largely overlap in content.[39] Since each survey contains multiple waves, it is possible to compare respondents surveyed from the same state and at the same age but with different values for pfMandate. However, neither survey includes data on state or year of high school graduation. Rather, I follow the convention in Urban, Schmeiser, Collins, and Brown (2018) and Harvey (2019) and assign mandate status by state of residence and year of 18th birthday. Additionally, for these two surveys, I restrict the sample to only those students whose (inferred) high school graduation year is between 2001 and 2008 (inclusive) in order to match the data years for the improvements in the one-year repayment rate discussed above.

Figure 10 plots the coefficient estimates and 95% confidence intervals from the estimation of Equation (10) for the three surveys.[40] Panel A plots the coefficient estimates for the three financial literacy and three loan literacy questions from the NPSAS:16. The point estimates for each of the three financial literacy questions is less than a half a percentage point and the null hypothesis cannot be rejected for any estimate. Further, there is no significant change in the total number of correct answers as a result of a binding personal finance mandate.[41] On the other hand, each of the estimates for the three loan literacy questions is positive ranging from 1.3 to 3.3 percentage point increases in the probability of a correct answer. The largest effect is for the question asking borrowers whether the federal government can garnish wages for non-payment of federal student loans. Additionally, respondents answered 3% more questions correctly if they were bound by a personal finance mandate.

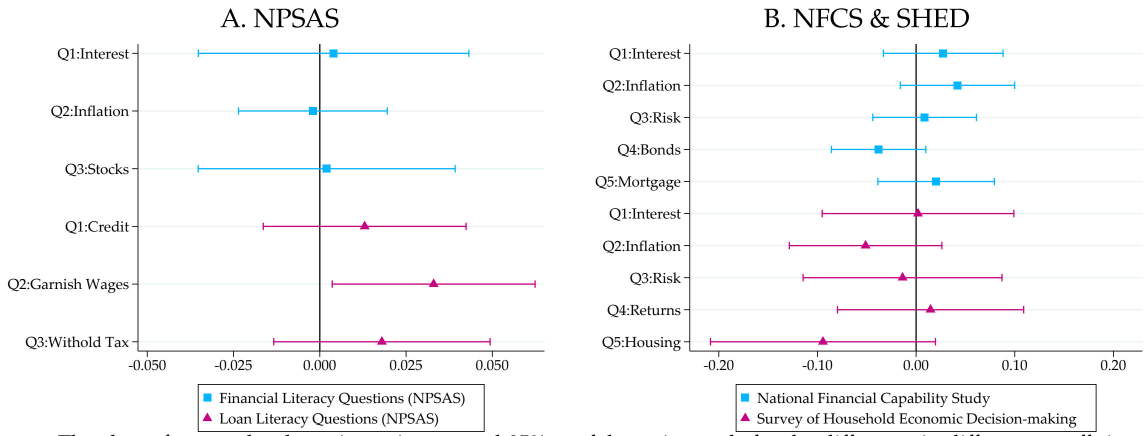
Panel B plots the point estimates for the five financial literacy questions in both the NFCS and the SHED surveys. Across all ten questions, no null hypothesis can be rejected at any conventional level. In total, the evidence suggests no difference in the probability of correctly answering financial

[39]Survey question text can be found in Table A.11.

[40]Tables A.8 to A.10 report the full results for the NPSAS, SHED, and NFCS, respectively.

[41]The full table of coefficients and standard errors along with the effect on the number of correct answers can be found in Table A.8.

Figure 10: Difference-in-Difference Estimates for Financial Literacy and Loan Literacy from NPSAS:16



The above figures plot the point estimates and 95% confidence intervals for the difference-in-difference coefficients using the NPSAS, NFCS, and SHED surveys. Panel A plots the effect of PFL mandates on the linear probability for three financial literacy and three loan literacy questions. Panel B plots the effect of PFL mandates on the linear probability of five financial literacy questions for each survey. Question text is available in Table A.11 and a full table of coefficient estimates and standard errors is available in Tables A.9 to A.10.
Sources: U.S Department of Education, National Center for Education Statistics, Restricted-use National Postsecondary Student Aid Study 2016. Also National Financial Capability Study and Survey of Household Economics and Decisionmaking

literacy questions between mandated and not mandated respondents *at the time of survey*. However, these results should not be taken as evidence that personal finance education mandates do not improve financial literacy. The evidence does not preclude the case where PFL mandates improve financial literacy during and immediately after high school and either financial literacy depreciates quickly or non-mandated peers catch up to mandated peers after high school. If this is the case, PFL mandates may still improve downstream outcomes due to decisions made during high school while financial literacy was higher than non-mandated peers.

On the other hand, the evidence does suggest that mandated students are more knowledgeable about regulations governing federal student loans. If this is the case, student loan borrowers may be better able to repay loans due to this increased familiarity with the rules and regulations for their loans. These students might be aware of income-driven repayment plans or deferment or forbearance options. In addition, Stoddard and Urban (2019) find that students bound by personal finance education mandates are more likely to borrow from federal sources. It could also be the case that the increase in knowledge about the federal student loan system is due to an increase in the probability of federal borrowing. Anderson, Conzelmann, and Lacy (2018) find that federal borrowers have higher student loan literacy which might be a result of more experience with the federal loan system.

5.4.3 Completion

Lastly, I test whether personal finance education mandates have any impact on the probability a student earns a degree. If personal finance education leads to better matching of students to colleges or degree programs, students may be more successful in college. As a result of graduation, students will likely have better labor market outcomes which would lead to better repayment rates and a lower chance of default. I use the American Community Survey (ACS) one-year samples from 2005-2017 to test whether students bound by personal finance mandates were more likely to hold a college degree or have ever attended college. The estimating equation for these tests is identical to Equation (10). However, I remove students younger than 22 as these students are unlikely to have earned a bachelor's degree yet. Since the ACS also does not ask respondents for the year of high school graduation, I assume respondents graduate from high school in the year of their 18th birthday. I report results where either the state of birth or the state of residence is used in place of state of high school since this is also unobserved.

Table 9 reports the results of this estimation on the (linear) probability of earning a bachelor's degree, the probability of earning an associate's degree, and the probability a respondent ever attended college. Odd numbered columns identify mandate status by birth state while even numbered columns use state of residence. The estimate in Column 1 suggests a potential negative relationship between personal finance mandates and bachelor's degree receipt although the point estimate is quite small. However, when using the state of residence instead of birth state in Column 2, the estimate is not statistically significant. The estimates in Columns 3 through 6 suggest there is also no effect of personal finance education mandates on the probability of earning an associates degree or having ever attended college. In total, I find little compelling evidence to suggest that the improvement in student loan repayment is due to an increase in degree completion.

Table 9: Difference-in-Differences Estimates for Degree Completion from ACS

	(1) Bachelor's Earned	(2) Bachelor's Earned	(3) Assoc Earned	(4) Assoc Earned	(5) Ever College	(6) Ever College
PF Mandate	-0.005	-0.004	0.001	-0.002	-0.007	-0.006
	(0.003)	(0.003)	(0.001)	(0.002)	(0.004)	(0.005)
Observations	2,236,990	2,236,990	2,236,990	2,236,990	2,236,990	2,236,990
Cohorts	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
High School State	Birthplace	Residence	Birthplace	Residence	Birthplace	Residence
Outcome Mean	0.240	0.240	0.097	0.097	0.692	0.692
Percentage Effect	-1.9%	-1.8%	0.9%	-2.4%	-1.0%	-0.8%

Notes: Sample includes respondents from the 2005-2017 American Community Survey with a high school diploma or higher that were born in the U.S. and 22 years of age or older. Controls include binary variables for gender and race along with credit requirements in math, English, social studies, and science by high school graduation year and state of residence, controls for state level high school staffing, and availability of merit aid scholarships at the state level. Also included are state and high school graduation year fixed effects. Standard errors are clustered at the state level.

6 Conclusion

The findings in this paper extend the literature on personal finance education mandates and federal financial aid in several key dimensions. I find that students who were bound by PFL mandates in high school were better at repaying student loan balances. The impact is largest and most precisely estimated for low income and first generation students at public universities which is consistent with other findings in the literature (Stoddard and Urban, 2019; Goodman, 2019). The results suggest that low income and first generation students are 5% more likely to have paid down some of their original balance one year after entering repayment. Despite some suggestive evidence of improvements, I cannot conclude that mandates have any meaningful impact on the cohort default rate. However, this result is likely not surprising since student loan default is a more rare and adverse outcome while repayment progress is a more sensitive measure.

I conduct a counter-factual exercise to estimate how many additional students would have been able to successfully pay down some of their student loan balance if PFL mandate were universal. If all high school graduating cohorts between 2001 and 2008 were bound by PFL mandates, an additional 72,000 students would have paid down at least a dollar of their balance one year after entering repayment. I show evidence that repayment progress after one year is correlated with long term repayment outcomes. Students who had made progress on their loans one year after entering repayment were half as likely to default and were 36% more likely to have paid off their full balance.

I find that median student loan balances are not significantly declining as a result of personal finance education mandates for first generation or low income students who are better at repaying loans. However, improvements in repayment rates for high income students might be a result of decreased borrowing. The results suggest the median high income student loan debt is 8% lower as a result of PFL mandates.

I use correct answers on financial literacy questions as a proxy for general financial literacy. I find no evidence that students bound by PFL mandates are more financial literate when surveyed. Across 13 questions asked in three surveys, I find no evidence that students bound by a personal finance mandate have a higher probability of correctly answering these questions. This does not necessarily imply that PFL mandates are ineffective at improving financial literacy. Rather, it is possible that improvements in financial literacy depreciate quickly after high school and/or non-mandated peers quickly catch up. In this case, personal finance education in high school may still operate as a just-in-time intervention in which financial literacy is temporarily improved at the same time postsecondary financing decisions are made.

On the other hand, I present evidence that students bound by personal finance mandates are more knowledgeable about the federal financial aid system. Students bound by mandates are more likely to correctly answer one of the three questions about federal student loans and answer more of these questions correctly. This suggests that students bound by the mandates may be better able to repay student loans in part due to increased familiarity with the federal student loan system. If this is the case, personal finance mandates might not be necessary to improve student loan outcomes if the federal loan system were to be simplified. The results from this paper lend further evidence to the string of literature that shows potential benefits to a more streamlined federal financial aid system with fewer complexities that borrowers must learn before making postsecondary financing decisions (Dynarski and Scott-Clayton, 2006; Bettinger, Long, Oreopoulos, and Sanbonmatsu, 2012; Novak and McKinney, 2011; Dynarski and Scott-Clayton, 2013; Castleman, Schwartz, and Baum, 2015; Kofoed, 2017). However, the benefits to personal finance education mandates highlighted in this paper and in the related literature indeed suggest that mandating personal finance education in high school can improve financial outcomes for those students exposed to course material.

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A Appendix

A.1 Derivation of Motivating Specification

In this section, I show that when the three assumptions are satisfied, the aggregated estimating equation, Equation (2), consistently estimates the difference-in-differences parameter, γ^{DD}. First, assume the following assumptions hold:

1. Parallel Trends Assumption: $E[\Delta y_{0,ist} \mid D_{s(i)t} = 0] = E[\Delta y_{0,ist} \mid D_{s(i)t} = 1] \ \forall t$
2. Cohort Matching Assumption: $k_i = k \ \forall i$
3. Stability of University Mapping: $G(\ i, D_{s(i)t} = 1) = G(\ i, D_{s(i)t} = 0)$

where $s(i)$ is the state of high school for student i. First, define the function $G : \mathcal{I} \times \mathcal{D} \rightarrow \mathcal{J}$ where $\mathcal{I} = \{1, ..., I\}$, $\mathcal{D} = \{0, 1\}$, and $\mathcal{J} = \{0, 1, ..., J\}$. By Assumption 3, $G(i, D_{s(i)t} = 1) = G(i, D_{s(i)t} = 0)$ so we can simplify this function to G' which maps $\mathcal{I} \rightarrow \mathcal{J}$ such that $G'(i) = j$ is independent of $D_{s(i)t}$. Recall the difference-in-differences specification using micro-level data is:

$$y_{ist} = \alpha + \gamma^{DD} D_{s(i)t} + \delta_{s(i)t} + \varepsilon_{ist}, \tag{11}$$

Using the assumption that $k_i = k$ for all i, we can define $\tau \coloneqq t + k$. Define $J_{j\tau}$ equal to $\{i : G'(i) = j, t = \tau - k\}$ and define $|J_{j\tau}|$ as the number of students in $J_{j\tau}$. The aggregated outcome, $Y_{j\tau}$, is defined by

$$Y_{j\tau} \coloneqq \frac{1}{|J_{j\tau}|} \sum_{i \in J_{j\tau}} y_{ist}$$

which constructs the average of y for all students in the set $J_{j\tau}$. Similarly, the same transformation can be applied to the RHS of Equation (11):

$$\begin{aligned}
Y_{j\tau} &= \frac{1}{|J_{j\tau}|} \sum_{i \in J_{j\tau}} \left[\alpha + \gamma^{DD} D_{s(i)t} + \delta_{s(i)t} + \varepsilon_{ist}\right] \\
&= \frac{1}{|J_{j\tau}|} \sum_{i \in J_{j\tau}} \alpha + \frac{1}{|J_{j\tau}|} \sum_{i \in J_{j\tau}} \left[\gamma^{DD} D_{s(i)t}\right] + \frac{1}{|J_{j\tau}|} \sum_{i \in J_{j\tau}} \delta_{s(i)t} + \frac{1}{|J_{j\tau}|} \sum_{i \in J_{j\tau}} \varepsilon_{ist} \\
&= \alpha + \gamma^{DD} \left[\frac{\sum\limits_{i \in J_{j\tau}} D_{s(i)t}}{|J_{j\tau}|}\right] + \frac{1}{|J_{j\tau}|} \sum_{i \in J_{j\tau}} \delta_{s(i)t} + \frac{1}{|J_{j\tau}|} \sum_{i \in J_{j\tau}} \varepsilon_{ist}
\end{aligned}$$

The first term in the specification trivially reduces to α. The second term reduces to the share of students in $J_{j\tau}$ for which $D_{s(i)t} = 1$ which we will define as $\text{pctBound}_{j\tau}$. Additionally, since the error term is assumed mean-zero in the micro-level case conditional on observables and the Parallel Trends Assumption, the aggregated university-level error draws will also be conditionally mean-zero since the allocation of students to universities is unchanged by $D_{s(i)t}$. As a result, the university error term can be rewritten as an arbitrary mean-zero error term $e_{j\tau}$.

$$Y_{j\tau} = \alpha + \gamma^{DD}\text{pctBound}_{j\tau} + \frac{1}{|J_{j\tau}|}\sum_{i\in J_{j\tau}} \delta_{s(i)t} + e_{j\tau}$$

By the Parallel Trends Assumption, we can rewrite $\delta_{s(i)t} = \delta_{s(i)} + \delta_t$. Further, $\delta_{s(i)}$ can be rewritten as $\sum\limits_{s=1}^{S} \delta_s \cdot 1\{s(i) = s\}$ and the specification becomes

$$Y_{j\tau} = \alpha + \gamma^{DD}\text{pctBound}_{j\tau} + \sum_{i\in J_{j\tau}}\sum_{s=1}^{S} \delta_s \frac{1\cdot\left\{s(i) = s, i \in J_{j\tau}\right\}}{|J_{j\tau}|} + \frac{1}{|J_{j\tau}|}\sum_{i\in J_{j\tau}} \delta_t + e_{j\tau}$$
$$Y_{j\tau} = \alpha + \gamma^{DD}\text{pctBound}_{j\tau} + \delta_t + \sum_{s=1}^{S} \delta_s \left[\sum_{i\in J_{j\tau}} \frac{1\cdot\left\{s(i) = s, i \in J_{j\tau}\right\}}{|J_{j\tau}|}\right] + e_{j\tau}$$

Since $\tau = t + k$ by assumption, the time fixed effect is unchanged and δ_τ is just a change in notation. However, the last remaining term is more nuanced. Note that this term is a reweighting of the feeder-state fixed effect in accordance with the share of the cohort from each feeder state. For ease of interpretation, define the following terms

$$\text{StateShare}_{sj\tau} := \sum_{i\in J_{j\tau}} \frac{1\cdot\left\{s(i) = s, i \in J_{j\tau}\right\}}{|J_{j\tau}|}, \quad \delta_j := \sum_{\tau=k}^{T+k}\sum_{s=1}^{S} \delta_s \text{StateShare}_{sj\tau}$$

Adding and subtracting δ_j yields:

$$\begin{aligned} Y_{j\tau} &= \alpha + \gamma^{DD}\text{pctBound}_{j\tau} + \delta_t + \delta_j - \sum_{\tau=k}^{T+k}\sum_{s=1}^{S} \delta_s\text{StateShare}_{sj\tau} + \sum_{s=1}^{S} \delta_s\text{StateShare}_{sj\tau} + e_{j\tau} \\ &= \alpha + \gamma^{DD}\text{pctBound}_{j\tau} + \delta_t + \delta_j + \left[\sum_{s=1}^{S} \delta_s \left(\text{StateShare}_{sj\tau} - \sum_{\tau=k}^{T+k}\text{StateShare}_{sj\tau}\right)\right] + e_{j\tau} \end{aligned}$$

The remaining term in brackets represents the sum of transitory deviations from the university's mean share of students from each state multiplied by the fixed effect for each state. By the Stability of University Mapping assumption, this term is independent of the components of $\text{pctBound}_{j\tau}$. Collecting this transitory enrollment deviations term with $e_{j\tau}$, we can rewrite the estimating equation as:

$$Y_{j\tau} = \alpha + \gamma^{DD}\text{pctBound}_{j\tau} + \delta_j + \delta_t + v_{j\tau}. \tag{12}$$

Hence, under the three aforementioned assumptions, the aggregate university-level specification consistently estimates the micro-level difference-in-differences specification. Additionally, in Section 5.2.4, I estimate an alternative university-level specification that holds $\text{StateShare}_{sj\tau}$ fixed at initial levels. In this specification, the identification of γ^{DD} in the university-level specification is consistent *even in the case where students alter their college choice* as a result of $D_{s(i)t}$.

A.2 Data Appendix

A.2.1 College Scorecard

All data used in the analysis was pulled from the College Scorecard website using the October 30, 2018 update. The subsequent updates (as of July 20, 2020) did not affect the measures from the NSLDS used in this paper .

A.2.2 Integrated Postsecondary Education Data System

I collect previous state of residence data from the Integrated Postsecondary Education Data System (IPEDS). This data comes from the Compare Institutions tool on the IPEDS website. The data include years 1986 through 2016 for all universities in the Scorecard sample. From the Fall Enrollment category, I use the "State of residence when student was first admitted" and counts of "First-time degree/certificate-seeking undergraduate students who graduated from high school in the past 12 months." These data were required to be submitted in: 2016, 2014, 2012, 2010, 2008, 2006, 2004, 2002, 2000, 1998, 1996, 1994, 1992, 1988, and 1986. Universities could voluntarily provide this data in: 2015, 2013, 2011, 2009, 2007, 2005, 2003, and 2001. I impute missing values by linearly interpolating between the nearest non-missing years. In addition, Appendix A.4.2 estimates Equation (4) by using instrumented values of enrollment counts rather than linear interpolation.

A.2.3 High School Staffing Variables

I collect counts of state level high school staffing to use as controls in all specifications. These data come from the Common Core of Data (CCD) and are accessed using the `educationdata` Stata package from the Urban Institute. I pull these data for the years 1993 through 2015 at the school district level. Counts for each of the following are collected and aggregated to the state level: total staff, full-time equivalent total teachers, full-time equivalent total school support staff, total school guidance counselors, and total student support staff.

A.2.4 High School Graduation Requirements

I create a panel dataset of credit requirements for high school graduation at the state-by-graduation-year level. These data are primarily sourced from the National Center for Education Statistics (NCES) Digest of Education Statistics Chapter 2. These tables present snapshots in time of state credit requirements for each state along with the first effective graduating cohort bound by the requirements. The first table is from 1995 and I use these snapshots to track changes in graduation requirements in: **Total Credits, English/Language Arts, Social Studies, Math, and Science**. The creation of this data required some decisions in which I try to follow objective rules. First, not all states have state requirements for high school graduation. States like Colorado deferred requirements to the district level. For these states, I impute the state requirements by substituting the national average for each graduating cohort for states with requirements and I include a binary variable denoting local control. Second, many states have multiple tracts students can select with different credit requirements for each track. When possible, I select the vector of graduating requirements that had the minimum standards. These are typically obvious when the choice is between a "standard" diploma and an "honors" diploma, however the definition can be more subjective when states allow students a technical career path. In these cases, I choose the standard diploma requirements as the technical career path students are less likely to attend a four-year college after high school graduation.

I supplement and cross reference the NCES data with data from the Education Commission of the States 50-State Comparison: High School Graduation Requirements (Macdonald, Dounay-Zinth, and Pompelia, 2019). When conflicts between the sources arose, I tracked the course standards using state Department of Education websites to resolve discrepancies. This data is available upon request.

A.2.5 State Merit Aid

I use the definition of state merit aid availability at the state-graduation-year level as defined by Sjoquist and Winters (2015). They define merit aid scholarships as "strong" and "weak" merit aid programs and I follow their convention. I include a binary indicator variable at the high school graduating cohort by year level for the presence of weak and strong merit aid in each specification.

A.2.6 Constructing University Cohort Controls from State-by-Graduation-Year Data

A vector of incoming cohort level controls are included in $\mathbf{X}_{it}$. In a similar manner to Equation (3), I create a vector of control variables for each incoming university cohort that is weighted by the state composition of the incoming cohort. I use high school graduation state j by high school graduation year t variables, x_{jt}, combined with previous state of residence data, enroll_{ijt}, for university i from state j in year t to construct an incoming university cohort measure for each variable in X_{it}:

$$\mathrm{X}_{it} = \frac{\sum_{j=1}^{51} \mathrm{x}_{jt} \times \text{enroll}_{ijt}}{\sum_{j=1}^{51} \text{enroll}_{ijt}}, \tag{13}$$

This vector includes the state level measures of high school staffing and high school graduation requirements.[42] In addition to these state weighted controls, $\mathbf{X}_{it}$ also includes binary variables for whether the state of university offered a merit aid scholarship along with unemployment rates for periods t through $t + k$.

A.3 Randomization Inference Algorithm

The randomization inference algorithm used to compute the empirical p-values is based off the RI-β algorithm in MacKinnon and Webb (ming). I conduct 3000 replications of Equation (4) for each outcome variable where the identifying variation in the replication is randomly generated by supposing that the adopting states do not adopt and the non-adopting states do adopt.[43] In each of

[42]Not all states have high school graduation standards set at the state level. For states with no state standards, the mean value across all states is used and a binary variable is included denoting local control of high school graduation standards.

[43]I have also repeated this algorithm without taking into account observed adopting states and instead drawing states and implementation years unconditionally. The results are similar for both which further suggests the states and years of adoption are "as good as random."

these replications, it should be the case that the estimated treatment effect for the placebo replications is zero on average. Further, the estimated treatment effect using the observed pctBound measure should be a sufficiently extreme value in the distribution of placebo replications. The algorithm proceeds as follows for each replication:

1. Split the sample of 50 states plus D.C. into two groups

   Group A: States adopting a mandate binding for the class of 2008 and prior (13 states)

   Group B: States adopting a mandate binding for the class of 2009 and later and states that never adopt a mandate.

2. Choose 13 states at random from Group B to slot into the mandate adoption slots observed in the true data[44]

3. Use this selection of states and adoption years to generate placebo pfMandate_{jt}.

4. Compute $\text{pctBound}_{it} = \frac{\sum_{j=1}^{51} \text{pfMandate}_{jt} \times \text{enroll}_{ijt}}{\sum_{j=1}^{51} \text{enroll}_{ijt}}$ using placebo pfMandate_{jt}.

5. Estimate Equation (4) using the placebo pctBound_{it}.

6. Store $\hat{\gamma}_n$.

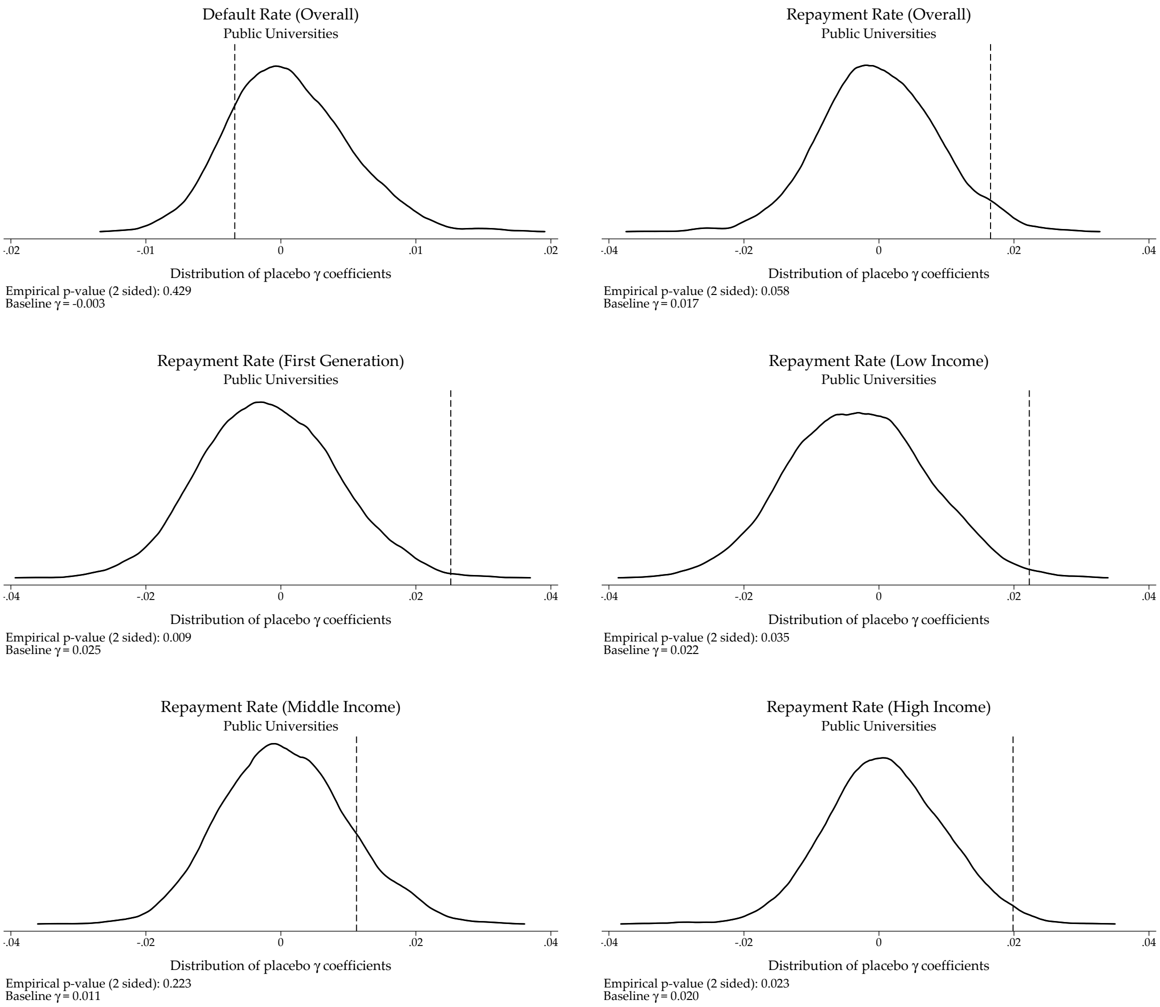
Once all $\hat{\gamma}_n$ for $n = 1, ..., 3000$ are collected, the empirical p-value is computed using:

$$\bar{p} = \frac{1}{3000} \sum_{n=1}^{3000} 1 \cdot \left\{ |\hat{\gamma}_n| \geq |\hat{\gamma}_{true}| \right\} \tag{14}$$

Figure A.1 presents the distributions of the $\hat{\gamma}_n$ estimates for each outcome for the public university sample with the $\hat{\gamma}$ from the baseline specification marked in each distribution.

[44]One adopting state in 1993, 1996, 1998, 2002, 2006 and 2008. Three adopting states in 2005. Four adopting states in 2007.

Figure A.1: Distribution of Placebo Estimates for pctBound for Repayment Outcomes for Public Universities



A.4 Alternative Specifications

In this section, I explore whether the results presented above are sensitive to the choice of specification and the use of the continuous treatment measure.

A.4.1 State-Level Difference-in-Differences for High In-state Universities

First, I abandon the use of the continuous treatment measure to estimate a more straight-forward difference-in-differences specification in which treatment status is assigned to each university at the state level. To isolate the sample to only those that are most affected by the within-state adoption of a personal finance education mandate, I restrict the sample to public and private universities with a historically high in-state percentage of students. For each university, I calculate the mean percentage of students who resided in the state in the previous year over the sample years and only include a university in this analysis if the mean percentage of in-state students is 70% or higher.[45] The result is a sample of 656 universities of which 370 are public and 286 are private. This subsample represents over 75% of the public universities in the sample but less than one-third of the private universities. In this specification, each university is assumed to only be affected by its own state's mandate adoption (if any) and universities in states that do not adopt a mandate act as controls. The specification is similar to Equation (4)

$$y_{is,t+6} = \gamma \text{pfMandate}_{st} + \beta \mathbf{X}_{st} + \delta_s + \delta_t + \varepsilon_{ist}, \tag{15}$$

where $y_{is,t+6}$ is the same student loan repayment outcome for university i located in state s for the repayment cohort matched to high school graduating class t. Rather than pctBound_{it} as in Equation (4), pfMandate_{st} is equal to one if the university state s has a binding mandate for high school graduating cohort t. Also included are the vector of control variables $\mathbf{X}_{st}$ at the state level which include other course credit requirements, high school staffing levels, availability of state merit aid scholarships, and a vector of the state unemployment rates between periods t and $t + 6$. Fixed effects for state (δ_s) and high school graduating cohorts (δ_t) are also included and standard errors are clustered at the state level.

Table A.1 reports the estimated γ coefficients for this specification for all universities with 70% or higher historical in-state percentage for the main outcome variable split by public and private universities. Columns 3 and 4 in Panel A show improvements in the one-year repayment rate for first generation and low income students similar to those found in Table 4. However, the estimates presented in Panel B suggest that private universities who receive a large share of students from in-state high schools indeed see improvements in student loan repayment at least for low income students. In fact, the point estimates for private universities are larger than for public universities. This divergence in the sample of private universities across specifications may be due to the type of student attending out-of-state private universities or the type of private universities that attract

[45]For the sample of universities, the median historical in-state percentage is 64% so this is roughly half the universities in the main analysis

Table A.1: Robustness: Difference-in-Differences for High In-state University Sample for Public and Private Universities

	Default Rate	Repayment Rate				
A. Public	(1) Overall	(2) Overall	(3) First Gen	(4) Low Income	(5) Middle Income	(6) High Income
pfMandate	-0.002	0.015	0.023	0.021	0.009	0.017
	(0.002)	(0.010)	(0.011)	(0.009)	(0.011)	(0.011)
Universities	370	370	370	370	366	366
Cohorts	1993-2006	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	0.046	0.589	0.552	0.479	0.628	0.723
Percentage Effect	-3.9%	2.6%	4.2%	4.4%	1.5%	2.4%
B. Private						
pfMandate	-0.005	0.016	0.027	0.031	0.014	0.002
	(0.003)	(0.017)	(0.018)	(0.018)	(0.016)	(0.016)
Universities	286	286	284	285	271	268
Cohorts	1993-2006	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	0.040	0.571	0.522	0.421	0.618	0.747
Percentage Effect	-11.5%	2.8%	5.1%	7.3%	2.2%	0.2%

Regressions are weighted using the number of students used to compute each outcome metric. Each column reports a coefficient from a separate regression where the independent variable is pfMandate_{jt} and the outcome is denoted in the column header. The sample includes public and private four-year universities with 70% or higher in-state share of students during the sample period. Default rate analysis includes high school graduating classes 1993 through 2006 and repayment rate analysis includes high school graduating classes 2001 through 2008 due to data availability. First Gen students are defined as students whose parents did not have a college degree. Low Income, Middle Income, and High Income students are defined as household income less than 30,000, between 30,000 and 75,000 and above 75,000, respectively. Controls include cohort weighted credit requirements in math, English, social studies, and science by high school graduation cohort and controls for state level high school staffing, and availability of merit aid scholarships. Also included are university and high school graduation year fixed effects. Standard errors clustered at the state level are presented in parenthesis.

largely in-state students.

A.4.2 Instrumenting for Enrollment Counts

As noted above, universities are required to send data on the previous state of residence for each incoming cohort only in even numbered years. Universities may elect to also provide this information in odd years but are not required. As a result, the data contain many missing values over the sample. Further, investigation of the data reveal numerous transcription errors in which the cohort is coded as including only students who graduated from high school longer than 12 months prior when this is highly unlikely given previous years' data. In the main analysis, I linearly interpolate missing values using the neighboring non-missing years. However, trends in attendance may not be linear in years and transitory shocks and time trends in attendance numbers may occur which deviate from linearly interpolated values.

In this section, I conduct a more thorough exercise to replace missing data that uses more information to predict missing values by instrumenting enroll_{isjt} with linear and quadratic trends, a series of fixed effects, and the availability of state merit aid scholarships. Equation (16) details the specification for this strategy:

$$\begin{aligned}\text{enroll}_{isjt} =&\delta_i + t \cdot \delta_i + t^2 \cdot \delta_i + t \cdot \delta_s + t^2 \cdot \delta_s \\ &+ t \cdot \delta_{ij} + t^2 \cdot \delta_{ij} + \text{meritAid}_{jt} + \text{meritAid}_{jt} \cdot \{s = j\} \\ &+ \delta_{jt} + \varepsilon_{isjt}\end{aligned} \tag{16}$$

In this specification, predicted values of enroll_{isjt} are estimated by regressing enroll_{isjt} on university fixed effects and state fixed effects both of which are interacted with linear and quadratic time trends. In addition, linear and quadratic trends for each university-by-feeder state are also included. I include an indicator for whether the feeder state offered a state merit aid scholarship for cohort t. Since state merit aid scholarships provide an added incentive to attend an in-state school, the addition of a scholarship might cause students to be less likely to attend an out-of-state school (Fitzpatrick and Jones, 2016). For this reason, I also include an interaction of meritAid_{jt} with an indicator for whether the feeder state is an in-state university since this effect would be opposite-signed. Lastly, I include feeder-state-specific year fixed effects to capture transitory shocks to feeder state level college enrollment.

I use the estimated coefficients and fixed effects to predict enroll_{isjt} ($\widetilde{\text{enroll}}_{isjt}$) for both non-missing values included in the regression as well as missing observations not included in the regression. I then use $\widetilde{\text{enroll}}_{isjt}$ to construct $\widetilde{\text{pctBound}}_{ist}$ as in Equation (3) to re-estimate Equation (4). Table A.2 presents the results from this exercise. The estimates in Panel A for public universities are largely consistent with the estimates presented in Table 4. However, the results in Panel B potentially suggest better outcomes for private university students as a result of changes in $\widetilde{\text{enroll}}_{isjt}$. This result suggests that private universities may be more likely to choose not to report enrollment data in odd years resulting in more missing data and thus less precise results during linear interpolation in the main results. While the point estimates are larger and suggest improvements in the repayment rate for first generation and low income students, these estimates are not statistically significant at conventional levels. Further, this estimation does not take into account the fact that $\widetilde{\text{enroll}}_{isjt}$ is a generated regressor and thus standard errors are likely under-estimated as is.

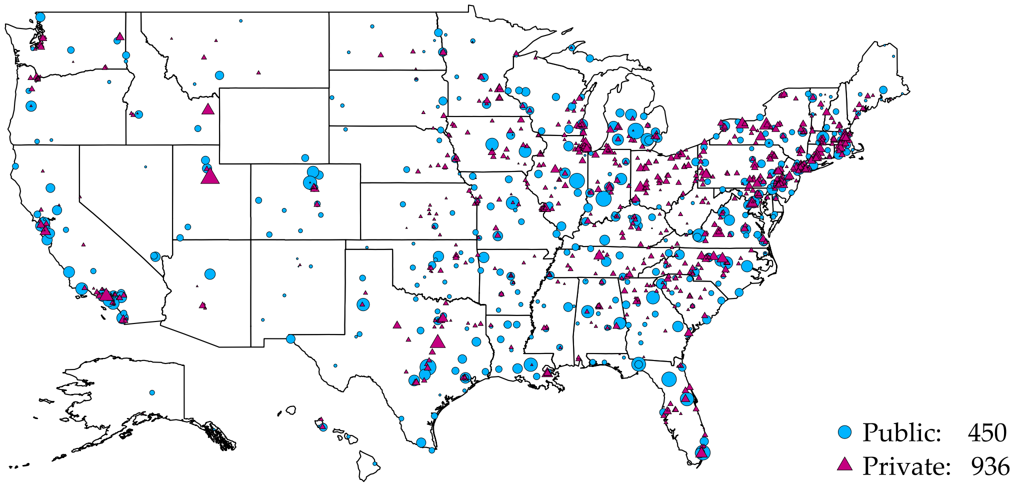
Table A.2: Robustness: Dose Response Estimates with Instrumented Enrollment for Public and Private Universities

	Default Rate	Repayment Rate				
	(1)	(2)	(3)	(4)	(5)	(6)
A. Public	Overall	Overall	First Gen	Low Income	Middle Income	High Income
pctBound	-0.003	0.016	0.025	0.022	0.010	0.018
	(0.002)	(0.009)	(0.010)	(0.010)	(0.010)	(0.011)
Universities	444	446	446	445	442	442
Cohorts	1993-2006	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	0.046	0.594	0.551	0.478	0.631	0.730
Percentage Effect	-5.6%	2.6%	4.5%	4.6%	1.6%	2.5%
B. Private						
pctBound	0.002	-0.000	0.012	0.017	-0.002	-0.009
	(0.005)	(0.014)	(0.011)	(0.017)	(0.009)	(0.010)
Universities	823	821	800	804	796	799
Cohorts	1993-2006	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	0.035	0.600	0.516	0.426	0.636	0.779
Percentage Effect	6.0%	-0.1%	2.4%	4.1%	-0.4%	-1.2%

Regressions are weighted using the number of students used to compute each outcome metric. Each column reports a coefficient from a separate regression where the independent variable is pctBound and the outcome is denoted in the column header. The sample includes public and private four-year universities. Default rate analysis includes high school graduating classes 1993 through 2006 and repayment rate analysis includes high school graduating classes 2001 through 2008 due to data availability. First Gen students are defined as students whose parents did not have a college degree. Low Income, Middle Income, and High Income students are defined as household income less than 30,000, between 30,000 and 75,000 and above 75,000, respectively. Controls include cohort weighted credit requirements in math, English, social studies, and science by high school graduation cohort and controls for state level high school staffing, and availability of merit aid scholarships. Also included are university and high school graduation year fixed effects. Standard errors clustered at the state level are presented in parenthesis.

A.5 Additional Tables and Figures

Figure A.2: Map of University Sample from College Scorecard



The map above shows the locations and relative cohort sizes of the sample of public and private universities from the college scorecard. Each marker is weighted by the mean cohort size over the sample period. Public universities are denoted with a blue circle while private universities are denoted by a pink triangle. Some universities in the sample have missing GPS coordinates and are not plotted despite inclusion in the sample.

Figure A.3: Word Cloud of PFL State Standard Text

The image above shows a word cloud of the text for the state PFL standards for the following states: Arizona, Arkansas, Colorado, Florida, Georgia, Idaho, Iowa, Kansas, Louisana, Michigan, Missouri, New Hampshire, New Jersey, New York, North Carolina, Oregon, South Carolina, South Dakota, Tennessee, Texas, Utah, and Virginia.

Table A.3: Sample Construction

Restriction	All Universities	Public	Private
Full Sample	3,563	708	2,855
Balanced Sample	1,844	590	1,254
Single Branch	1,498	470	1,028
Non-missing Outcomes	1,386	450	936

The table above describes the number of universities that survive each iterative step of creating the sample. The first row is the full sample of four-year universities from the College Scorecard database. The second row is the result of removing universities that opened or closed during the sample period. The third row is the result of removing universities with outcome data aggregated across multiple branches. The fourth row is the result of removing universities with missing outcome data due to non-Title IV status or all cell sizes smaller than 30 students and thus suppressed.

Table A.4: Events per Academic Year

Academic Year	Events	Adopting States
1996	89	New York
1997	0	
1998	34	Michigan
1999	0	
2000	0	
2001	0	
2002	1	Wyoming
2003	0	
2004	0	
2005	41	Arizona, Arkansas, Louisiana
2006	10	South Dakota
2007	130	Georgia, Idaho, North Carolina, Texas
2008	6	Utah
2009	38	Colorado, South Carolina
2010	29	Missouri
2011	68	Iowa, New Jersey, Tennessee
2012	12	Kansas
2013	13	Oregon
2014	53	Florida, Virginia
Total	524	

The table above details the number of university events in each academic year where an event is defined as a year-over-year change in pctBound$_{ist}$ of 50 percentage points or larger. In addition, the last column summarizes the states that adopt a personal finance mandate in each academic year. Events induced by New Hampshire's 1993 mandate occur before the sample period for outcome data.

Table A.5: Difference-in-Differences Estimates for Changes to College-going for Two Year Universities (2001-2008)

	(1) pctAny	(2) pctPublic	(3) pctNonProfit	(4) pctForProfit	(5) pctInstate
PF Mandate	0.005	0.006	-0.000	-0.000	0.004
	(0.006)	(0.006)	(0.000)	(0.001)	(0.006)
Observations	408	408	408	408	408
Cohorts	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	0.161	0.152	0.002	0.007	0.146
Percentage Effect	3.3%	3.9%	-5.0%	-2.9%	2.9%

Each column above reports the Difference-in-Differences estimate for the outcome in each column header. Each outcome measures the percentage of recent high school graduates from a state who chose to attend a two year university of the given charactistic. Each observation is a state-year cell. The sample is restricted to high school graduation years 2001 through 2008 to match the results for the one year repayment rates. pctPublic and pctPrivate sum to one and thus the results are inverses of each other. Controls include credit requirements in math, English, social studies, and science by high school graduation year and state of university, controls for state level high school staffing, and availability of merit aid scholarships at the state level. Also included are state and high school graduation year fixed effects.

Table A.6: Difference-in-Differences Estimates for Changes to College-going for Four Year Universities (2001-2008)

	(1) pctAny	(2) pctPublic	(3) pctNonProfit	(4) pctForProfit	(5) pctInstate
PF Mandate	-0.007	-0.003	-0.004	-0.000	-0.004
	(0.007)	(0.005)	(0.003)	(0.001)	(0.007)
Observations	408	408	408	408	408
Cohorts	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	0.439	0.296	0.137	0.006	0.292
Percentage Effect	-1.7%	-1.1%	-2.9%	-2.3%	-1.3%

Each column above reports the Difference-in-Differences estimate for the outcome in each column header. Each outcome measures the percentage of recent high school graduates from a state who chose to attend a four year university of the given charactistic. Each observation is a state-year cell. The sample is restricted to high school graduation years 2001 through 2008 to match the results for the one year repayment rates. pctPublic and pctPrivate sum to one and thus the results are inverses of each other. Controls include credit requirements in math, English, social studies, and science by high school graduation year and state of university, controls for state level high school staffing, and availability of merit aid scholarships at the state level. Also included are state and high school graduation year fixed effects.

Table A.7: Robustness: Stable State Composition Estimates for Public and Private Universities

	Default Rate	Repayment Rate				
A. Public	(1) Overall	(2) Overall	(3) First Gen	(4) Low Income	(5) Middle Income	(6) High Income
$\widehat{\text{pctBound}}$	-0.003	0.018	0.028	0.026	0.012	0.020
	(0.002)	(0.010)	(0.011)	(0.010)	(0.011)	(0.012)
Universities	450	450	449	449	445	445
Cohorts	1993-2006	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	0.046	0.593	0.551	0.478	0.630	0.730
Percentage Effect	-5.8%	3.0%	5.0%	5.3%	1.8%	2.7%
B. Private						
$\widehat{\text{pctBound}}$	-0.003	0.003	0.025	0.024	0.002	-0.011
	(0.004)	(0.017)	(0.012)	(0.020)	(0.012)	(0.013)
Universities	936	934	891	905	875	873
Cohorts	1993-2006	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	0.036	0.595	0.512	0.423	0.632	0.777
Percentage Effect	-9.3%	0.6%	5.0%	5.6%	0.3%	-1.4%

Regressions are weighted using the number of students used to compute each outcome metric. Each column reports a coefficient from a separate regression where the independent variable is $\widehat{\text{pctBound}}$ and the outcome is denoted in the column header. The sample includes public and private four-year universities. Default rate analysis includes high school graduating classes 1993 through 2006 and repayment rate analysis includes high school graduating classes 2001 through 2008 due to data availability. First Gen students are defined as students whose parents did not have a college degree. Low Income, Middle Income, and High Income students are defined as household income less than 30,000, between 30,000 and 75,000 and above 75,000, respectively. Controls include cohort weighted credit requirements in math, English, social studies, and science by high school graduation cohort and controls for state level high school staffing, and availability of merit aid scholarships. Also included are university and high school graduation year fixed effects. Standard errors clustered at the state level are presented in parenthesis.

Table A.8: Difference-in-Differences Estimates for Financial Literacy and Loan Literacy from NPSAS

	(1) FL1: Interest	(2) FL2: Inflation	(3) FL3: Risk	(4) FL: Num Correct	(5) LL1: Credit	(6) LL2: Garnish Wages	(7) LL3: Tax Returns	(8) LL: Num Correct
pfMandate	0.004	-0.002	0.002	0.004	0.013	0.033	0.018	0.064
	(0.020)	(0.011)	(0.019)	(0.040)	(0.015)	(0.015)	(0.016)	(0.030)
Observations	45,230	45,230	45,230	45,230	45,230	45,230	45,230	45,230
Cohorts	2009-2016	2009-2016	2009-2016	2009-2016	2009-2016	2009-2016	2009-2016	2009-2016
Outcome Mean	0.662	0.856	0.466	1.944	0.753	0.558	0.660	1.97
Percentage Effect	0.5%	-0.4%	0.2%	0.1%	1.7%	6.0%	2.7%	3.2%

Source: U.S Department of Education, National Center for Education Statistics, Restricted-use National Postsecondary Student Aid Study 2016. Each column reports the coefficient of the binary personal finance mandate variable from a separate linear regression. Columns 1-3 and 5-7 report the impact of a personal finance mandate on an indicator variable for whether the respondent correctly answered the question from a linear probability model. Columns 4 and 8 show the impact of the mandate on the total number of correct questions answered. Each regression includes controls for race, gender, Expected Family Contribution, year of schooling, and public or private high school attended. Also included are state of high school attendance and high school graduation year fixed effects. Standard errors are clustered at the state level.

Table A.9: Difference-in-Difference Estimates for Financial Literacy from NFCS

	(1) Q1: Interest	(2) Q2: Inflation	(3) Q3: Risk	(4) Q4: Bonds	(5) Q5: Mortgage	(6) Number Correct
pfMandate	0.027	0.042	0.009	-0.038	0.020	0.060
	(0.031)	(0.030)	(0.027)	(0.024)	(0.030)	(0.110)
Observations	10,020	10,020	10,020	10,020	10,020	10,020
Cohorts	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	0.714	0.420	0.363	0.197	0.711	2.405
Percentage Effect	3.9%	10.0%	2.4%	-19.3%	2.8%	2.5%

Notes: Sample includes respondents from the 2012, 2015, and 2018 waves of the restricted use National Financial Capabilities Survey with a high school diploma or higher. Controls include binary variables for gender, race, and education along with credit requirements in math, English, social studies, and science by high school graduation year and state of residence, controls for state level high school staffing, and availability of merit aid scholarships at the state level. Also included are state and high school graduation year fixed effects and state-level survey weights. Standard errors are clustered at the state level.

Table A.10: Difference-in-Differences Estimates for Financial Literacy from the Survey of Household Economic Decision-making

	(1) Q1: Interest	(2) Q2: Inflation	(3) Q3: Risk	(4) Q4: Returns	(5) Q5: Housing	(6) Number Correct
pfMandate	0.002	-0.051	-0.014	0.015	-0.094	-0.143
	(0.050)	(0.039)	(0.051)	(0.048)	(0.058)	(0.109)
Observations	2,604	2,604	2,604	2,604	2,604	2,604
Cohorts	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
Outcome Mean	0.653	0.478	0.425	0.367	0.579	2.502
Percentage Effect	0.3%	-10.7%	-3.2%	4.0%	-16.3%	-5.7%

Notes: Sample includes respondents from the 2017 and 2018 waves of the Survey of Household Economic Decision-making with a high school diploma or higher that graduated high school between 2001 and 2008. Controls include binary variables for gender, race, and education along with credit requirements in math, English, social studies, and science by high school graduation year and state of residence, controls for state level high school staffing, and availability of merit aid scholarships at the state level. Also included are state, survey wave, and high school graduation year fixed effects. Standard errors are clustered at the state level.

Table A.11: Question Text for Financial Literacy Questions

A. NPSAS:16

Label	Question Text	Choices
FL1: Interest	Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, how much would you be able to buy with the money in this account?	More than today Exactly the same **Less than today**
FL2: Inflation	Suppose you had $100 in a savings account and the interest was 2% per year. After 5 years, how much do you think you would have in the account if you left the money to grow?	**More than $102** Exactly $102 Less than $102
FL3: Risk	Buying a single company's stock usually provides a safer return than a stock mutual fund.	True **False**
LL1: Credit	If a borrower is unable to repay his or her federal student loan, the government can report that the student debt is past due to the credit bureaus	**True** False
LL2: Garnish Wages	If a borrower is unable to repay his or her federal student loan, the government can have the student's employer withhold money from his or her pay (garnish wages) until the debt, plus any interest and fees, is repaid	**True** False
LL3: Tax Returns	If a borrower is unable to repay his or her federal student loan, the government can retain tax refunds and Social Security payments until the debt, plus any interest and fees, is repaid	**True** False

B. NFCS

Label	Question Text	Choices
Q1: Interest	Suppose you had $100 in a savings account and the interest rate was 2% per year. After 5 years, how much do you think you would have in the account if you left the money to grow?	**More than $102** Exactly $102 Less than $102
Q2: Inflation	Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, how much would you be able to buy with the money in this account?	More than today Exactly the same **Less than today**
Q3: Risk	Buying a single company's stock usually provides a safer return than a stock mutual fund.	True **False**
Q4: Bonds	If interest rates rise, what will typically happen to bond prices?	They will rise **They will fall** They will stay the same No relationship
Q5: Mortgage	A 15-year mortgage typically requires higher monthly payments than a 30-year mortgage, but the total interest paid over the life of the loan will be less.	**True** False

C. SHED

Label	Question Text	Choices
Q1: Interest	Suppose you had $100 in a savings account and the interest rate was 2% per year. After 5 years, how much do you think you would have in the account if you left the money to grow?	**More than $102** Exactly $102 Less than $102
Q2: Inflation	Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, how much would you be able to buy with the money in this account?	More than today Exactly the same **Less than today**
Q3: Risk	Buying a single company's stock usually provides a safer return than a stock mutual fund.	True **False**
Q4: Returns	Considering a long time period (for example 10 or 20 years), which asset described below normally gives the highest returns?	**Stocks** Bonds Savings accounts Precious metals
Q5: Housing	Housing prices in the US can never go down.	True **False**