Personal Finance Education Mandates and Student Loan Repayment

Daniel Mangrum*

Abstract

This paper estimates the impact of requiring high school students to complete personal finance education on federal student loan repayment behavior after college. I merge student loan borrowing and repayment data from the College Scorecard with data from the Integrated Postsecondary Education Data System on counts of high school graduates enrolling in college from different states. I estimate the causal effect of personal finance education mandates by relating the change in the share of university students subject to a state mandate to changes in university cohort student loan outcomes. I find only students with higher-income parents respond by adjusting borrowing, reducing median balances by 7%. By contrast, first-generation and low-income borrowers bound by mandates did not significantly adjust borrowing, but were nonetheless more likely to pay down balances. Repayment improvements are in part due to better understanding of the terms governing federal student loans. State mandates that incorporate career research alongside personal finance education are associated with better student loan repayment than those focused only on personal finance education.

Keywords: financial literacy, student loans, postsecondary finance, high school curriculum
JEL: D14, G51, G53, I22, I28, J24

★The views expressed herein are those of the authors and do not represent those of the Federal Reserve Bank of New York or the Federal Reserve System.

*Federal Reserve Bank of New York, 33 Liberty Street, New York, NY 10045.
Email address: daniel.mangrum@ny.frb.org (Daniel Mangrum)

1. Introduction

In the United States, high school students are increasingly tasked with making consequential decisions, often with limited information, about whether to attend college, which college to attend, and how to finance postsecondary education. Perhaps as a result, 54% of Americans with student loan debt say their loans were not worth it (CNBC, 2022), and over 11% of student loan balances were severely delinquent at the end of 2019 (Federal Reserve Bank of New York, 2022). Difficulties can be compounded for first-generation college students who do not have family members with first-hand postsecondary education experience. These students are less likely to apply for admission to selective universities or retake standardized tests and are more likely to under-invest in postsecondary education (Hoxby and Turner, 2015; Goodman, Gurantz, and Smith, 2020; Avery and Turner, 2012). However, interventions that provide information to students when they make postsecondary decisions improve outcomes for first-generation and low-income college students (Bettinger et al., 2012; Barr et al., 2016; Castleman and Goodman, 2018; Bettinger and Evans, 2019; Gurantz et al., 2020).

In this paper, I study one such intervention that might improve outcomes for high school students making decisions regarding postsecondary education: requirements for personal finance (PF) education during high school. Between 1993 and 2014, 23 states adjusted high school graduation requirements to include topics covering personal finance education (Stoddard and Urban, 2020). The main objective for these courses is to improve general financial literacy. However, many of the state standards explicitly include topics covering 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 intervention providing information precisely when initial decisions are made resulting in improvements in downstream postsecondary finance behaviors (Fernandes et al., 2014).

More specifically, I investigate how exposure to personal finance education during high school affects federal student loan borrowing and repayment after college. I combine two data sources that allow me to link high school exposure to personal finance education to student loan debt accumulation and short-run repayment outcomes. Student loan outcome data come from the College Scorecard which uses administrative data to compute university-cohort level borrowing and repayment metrics. This data allow me to uncover important heterogeneity in the impact of personal finance education mandates across different student groups like low-, middle-, and high-income students and students whose parents did not have a college degree. Since university cohorts include students who graduated from states with and without mandates, I merge outcomes from the College Scorecard with data from the Integrated Postsecondary Education Data System (IPEDS) to calculate the share of each university cohort that attended high school in a state with a binding personal finance mandate. Similar to Billings et al. (2020, 2022), I leverage variation in this measure of exposure to personal finance education mandates across universities and over time to estimate the extent to which this coursework drives changes in student loan borrowing and repayment behavior.

Using this strategy, I find that personal finance education mandates improve federal student loan outcomes, but these improvements are heterogeneous across student groups. For borrowing, I find no significant impact of mandates on median student loan debt after college except for students from high-income backgrounds who reduced borrowing by roughly 7%. For repayment, I find that the required coursework improved short-term loan repayment rates for students who attended public universities. The improvements in repayment are largest for first-generation students and those from lower income households. These borrowers were 5% more likely to have paid down some of their original balance one year after entering repayment. I also find that the effect of personal finance mandates grows over time. Improvements in repayment are only modest for the first treated cohort but grow throughout at least the first three cohorts after the mandate is binding. The main results are robust to several functional forms and alternative constructions of the exposure variable. Biases that are typically associated with staggered treatment timing contribute to an under-estimate of the treatment effect suggesting that the estimated improvements are likely a lower bound.

Next, I investigate the mechanisms that might cause personal finance education mandates to improve student loan outcomes. First, I test whether students bound by mandates were more financially literate. I find that those bound by mandates were no more likely to correctly answer general financial literacy questions, but they were more likely to correctly answer specific questions related to federal student loans. This suggests that mandates might be more effective when teaching specific financial skills just when the information is needed rather than general financial literacy (Fernandes et al., 2014; Brown et al., 2019). I then test whether personal finance education mandates change a student's decision to attend college or the type of college they attend. I find no evidence that mandated students change college attendance or that they affect the probability of attaining a degree. Lastly, I find that personal finance education mandates focused on career research are associated with greater improvements in student loan repayment. Although this finding could be because states that implement more effective mandates also include career research, it is also plausible that exercises that ask students to research potential careers improve labor market outcomes which leads to better repayment.

This paper expands our understanding of personal finance education mandates and decisions surrounding federal student loans in several key dimensions. First, these findings provide more evidence that required personal finance education in high school improves credit outcomes for young adults. Cole et al. (2016) finds that students bound by mandates were less likely to hold delinquent debt. Urban et al. (2018) documents that mandated borrowers have fewer delinquent accounts and higher credit scores. Results from Harvey (2019) suggest that mandated students are less likely to use payday loans which typically carry high interest and delinquency rates. This paper is most similar to Stoddard and Urban (2020) which finds that students bound by mandates are more likely to take federal student loans and less likely to get private loans which tend to have higher interest rates and fewer borrower protections. This work studying PF mandates for high school students is a subset of a wider literature studying more general financial education interventions. These studies generally find financial education interventions to be effective at improving financial knowledge and financial outcomes (Kaiser et al., 2021). Personal finance education during high school might be comparably even more effective in these dimensions since it often includes coursework targeting specific financial literacy topics, such as the college application process and career research, and delivers that information just as students are making consequential decisions (Fernandes et al., 2014;

Brown et al., 2019). Barr et al. (2016) shows that providing information to prospective college students while they make postsecondary finance decisions can improve outcomes for those students, and this paper provides more evidence for this result.

Next, this paper is the first to separate the impact of personal finance education mandates on borrowing from the impact on repayment. Previous work examined either federal student loan borrowing in the first year of college (Stoddard and Urban, 2020) or total outstanding student loan balances in young adulthood (Brown et al., 2016). The former is an incomplete measure of total borrowing and the latter is a function of total borrowing, accumulated interest, and pace of repayment. In contrast, I separately estimate the impact of mandates on student borrowing and loan repayment. For borrowing, I find a heterogeneous response where high-income students are the only type to reduce borrowing as a result of mandated PF education. One potential explanation for this divergence is the higher relative cost of borrowing faced by high-income students who are ineligible for subsidized federal loans. Since PF coursework often includes assignments asking students to research the costs of borrowing for college, students with higher income parents may learn of this higher relative cost and respond by reducing borrowing.

In studying repayment, I improve on the previous literature by focusing on student loan repayment progress rather than relying on measures of delinquency and default (Brown et al., 2016; Urban et al., 2018). Federal student loans are unique among credit products in that borrowers struggling with payments can typically avoid adverse credit outcomes by enrolling in either Income-Driven Repayment (IDR) or forbearance programs. As a result, previous studies focusing on delinquency or default are less likely to detect improvements in credit outcomes associated with personal finance education mandates. Instead, I focus on the repayment rate from the College Scorecard which more accurately tracks progress toward loan repayment and is more likely to be affected by financial literacy interventions. Similar to the literature, I find no impact of mandates on loan default, but mandates do improve repayment. Borrowers bound by mandates were more likely to have declining student loan balances one year after beginning repayment. Although this is only evidence of short-term improvement, I present evidence that students achieving this short-run benchmark are far more likely to pay down their loans in the long-run.

Improvements in federal student loan performance benefit both borrowers and taxpayers. With the increased popularity of IDR plans, more borrowers are projected to be eligible for student loan forgiveness. Recent estimates from the non-partisan Congressional Budget Office suggest that student loan forgiveness for loans disbursed between 2020 and 2029 will cost the federal government roughly $200 billion (Congressional Budget Office, 2020). Hence, initiatives that improve federal student loan repayment, like requiring personal finance education in high school, could potentially reduce future public expenditures for student loan forgiveness and reduce the number of student loan borrowers "underwater" on the loans they take to finance higher education.

2. Background

To qualify for federal student loans, students must complete a Free Application for Federal Student Aid (FAFSA) that collects information about students and their families, including income and assets. Any student who completes the FAFSA is eligible for federal student loans. Students with higher need are eligible for subsidized loans which do not accrue interest until they leave college. Students also face limits on federal borrowing based on loan type, year of schooling, cost of attendance, and other financial aid they receive.

The literature has largely concluded that financial aid increases access to higher education for low-income students (Dynarski, 2003). More recent evidence suggests increased student loan borrowing by those at community colleges causes higher grades and more credits completed (Marx and Turner, 2019b). Despite the benefits, experts 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; Castleman, Schwartz, and Baum, 2015). Critics often argue that the complicated application process and multitude of choices reduce aid for otherwise eligible students encouraging some to choose suboptimal options (Kofoed, 2017; Marx and Turner, 2019a). Even for students who successfully navigate the application process , complexities created by the number and type of repayment plans can cause repayment issues (Cox, Kreisman, and Dynarski, 2020; Abraham, Filiz-Ozbay, Ozbay, and Turner, 2020).

A few recent studies find that interventions that provide students with more information about college applications and federal financial aid can improve outcomes for those from disadvantaged backgrounds. Bettinger, Long, Oreopoulos, and Sanbonmatsu (2012) shows that assisting low-income families with the FAFSA can dramatically increase the probability of applying for federal aid, enrolling, and persisting in college. Barr, Bird, and Castleman (2016) finds that providing information to community college students about federal student loan options can shift borrowers away from higher-cost financing. This effect is largely driven by students with lower levels of financial literacy and higher debt balances. Gurantz, Pender, Mabel, Larson, and Bettinger (2020) finds that virtual college counseling targeting low- and middle-income students increases the probability students choose to attend colleges with higher graduation rates. Additionally, Castleman and Goodman (2018) finds that college counseling can increase low-income student enrollment and persistence in less-expensive four-year public universities that have higher graduation rates. Bettinger and Evans (2019) also finds that providing high school students peer advising from recent college graduates can increase low-income and Hispanic student enrollment in two-year colleges without reducing four-year enrollment. Last, information programs that increase enrollment in IDR plans may also improve federal student loan repayment. Herbst (2020) finds that enrollment in IDR plans can reduce delinquencies and speed the pace of student loan repayment.

One larger scale intervention that might improve the postsecondary loan performance of disadvantaged students is PF education during high school. The first strand of this literature investigates an early wave of financial literacy education that occurred between 1957 and 1982. Although Bernheim et al. (2001) finds some evidence that required financial literacy courses during this wave improved financial outcomes, Cole et al. (2016) finds these results did not hold state and year fixed effects were included. The authors conclude that students who were subject to this earlier wave of requirements had no better financial outcomes than students who were not mandated. By contrast, Brown et al. (2016) studies a more recent wave of PF education mandates and finds improvements in young borrower credit performance. The authors find that the mandated PF coursework required since 1998 reduces the delinquent debt held by young adults. They also find the effect grows as mandates mature suggesting either that schools lag in implementation or teaching effectiveness improves over time. Harvey (2019) similarly finds that young people covered by more recent mandates are less likely to use alternative financial services which typically carry very high interest rates and have high delinquency rates. Urban, Schmeiser, Collins, and Brown (2018) compares the credit performance of mandated and nonmandated young people and find that those subject to PF mandates during high school have fewer delinquent credit accounts and higher credit scores.

This paper is most closely related to Stoddard and Urban (2020) which studies how PF mandates affect receipt of federal student aid for first-year four-year college students. The authors find that mandated college freshmen are more likely to complete the FAFSA, borrow from federal sources, receive grants or scholarships, use fewer private loans, and are less likely to carry credit card balances. They also find that the change in federal loan use and the reduced reliance on credit card borrowing is greater for low-income students. These students are also less likely to work while in college. Combined, these results suggest that college freshmen subject to PF mandates in high school potentially make better borrowing decisions. However, we know little about whether these initial improvements result in persistent benefits and this paper begins to fill that gap. I provide evidence that mandated PF education in high school does improve downstream financial behaviors, in particular, by bettering federal student loan repayment performance after college.

3. Data

I combine three main data sources to relate changes in a university's exposure to state level PF mandates to changes in university level student loan outcomes. Additional information on these data sources along with supplemental data sources are detailed in Appendix A.1.

3.1. College Scorecard

First, I use the College Scorecard database to track changes in federal student loan outcomes across university cohorts. 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 federal student loan variables are constructed using the administrative National Student Loan Data System (NSLDS) which contains records on the universe of federal aid recipients.

I restrict focus to four-year baccalaureate universities since high school graduates are more likely to go immediately to a four-year than a two-year university. Although the sample of two-year university students merits attention, the identification strategy described in the next section relies on following students that move on to college within 12 months of graduating high school. I also remove universities that do not receive federal financial aid and those that aggregate student loan variables across multiple branch campuses. To construct a balanced panel, I remove universities that either enter or exit the sample during the sample window so that estimates are not driven by university-level dynamics, but reflect instead borrower-level effects. Lastly, I remove universities whose average incoming cohort is fewer than ten students. The sample contains 1,363 universities across 50 states and the District of Columbia, including 447 public and 915 private universities.

The first outcome of interest is total student loan debt at the time a borrower begins repayment. The Scorecard includes the 10th, 25th, 50th, 75th, and 90th percentile of each university repayment cohort's federal student loan debt upon entering repayment. Further, median student loan debt is reported separately for **first generation students** (students whose parents did not have a postsecondary degree when the student began college), **low-income students** (household income less than $30,000), **middle-income students** (household income between $30,000 and $75,000), and **high-income students** (household income above $75,000).

Next, the Scorecard produces two measures of student loan repayment for each university's repayment cohort. The first is the **two-year cohort default rate** which is calculated by dividing the total number of students who have defaulted on their student loans within two years of entering repayment by the total number of borrowers in the repayment cohort. Default within the first two years of repayment is relatively rare since it requires 270 consecutive dates of non-payment. Additionally, borrowers are increasingly able to avoid default through forbearance, deferment, or enrollment in an IDR plan (Mueller and Yannelis, 2019). As a result, this outcome is less sensitive to change. Alternatively, the Scorecard constructs the **one-year repayment rate**, which calculates the share of the repayment cohort that is not in negative amortization at the end of the first year of repayment. Since borrowers can enroll in deferment, forbearance, or IDR, it is possible for them to avoid default yet have a larger balance at the end of their first year of repayment than their original balance. As a result, the one-year repayment rate is a more sensitive measure of student loan repayment progress. Similar to the measures of median student loan debt, the Scorecard also reports the one-year repayment rate separately for **first generation, low income, middle income, and high income students**.

Table 1 reports summary statistics for the main outcome variables. The means and standard deviations are weighted by the number of borrowers to be representative of the population of student loan borrowers. Generally, student loan debt is larger at private than public universities, however repayment tends to be better at private than public universities (lower default rates and higher repayment rates). One important exception is that first generation and low income students have higher repayment rates at public than at private universities. Over the sample, the default rate is a relatively low 4.1% while the repayment rate shows that around 60% of student loan borrowers are able to pay down at least some of their balance after one year of repayment.

These short-run student loan repayment performance metrics are also highly correlated with long-run repayment success. Table 2 reports 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 of original balance one year after beginning repayment. Students in this cohort who were able to pay down at least a dollar of their student loan debt one year after beginning 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 achieving the benchmark still owed 81% of their original balance. Borrowers who paid down at least one dollar in the first year were also half as likely to have defaulted and were 19 percentage points more likely to have repaid their student loans. Although not causal, these comparisons suggest that improvements in the one year repayment rate caused by PF mandates could lead to larger future student loan repayment success for mandated students.

3.2. State Personal Finance Education Mandates

In addition to the College Scorecard, I use the national rollout of PF education mandates since 1990 from Stoddard and Urban (2020), shown in Table A.5. The authors define a PF mandate as binding for a student if the state high school graduation requirements include coursework on personal finance. These mandates most often specify that PF coursework be included in a required course such as Social Studies, Economics, or Math. However, several states require students to complete a standalone PF course. Stoddard and Urban (2020) also improves on the definition of state mandates from Brown et al. (2016) by more systematically defining the effective PF mandate year base on the first high school graduating class bound by a mandate instead of the year the legislation was passed.

3.3. Integrated Postsecondary Education Data System

Lastly, I use data from the Integrated Postsecondary Education Data System (IPEDS) which includes biennial counts for each university of the incoming cohort of college students by previous state of residence. Between 1986 and 1994, these data were collected every two years from each university. After 1994, universities could voluntarily provide these data to IPEDS in odd years, but were required to submit counts 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.

Table 1: Descriptive Statistics

	All Universities (n=1,363)		Public Universities (n= 447)		Private Universities (n= 915)	
Outcome Variable	Mean	SD	Mean	SD	Mean	SD
Student Loan Debt						
10th	2,558.6	(880.1)	2,408.9	(780.0)	2,815.0	(977.1)
25th	4,423.9	(2,023.5)	4,061.7	(1,597.0)	5,042.8	(2,474.9)
Median						
Overall	10,244.5	(4,573.4)	9,304.7	(3,963.8)	11,850.7	(5,070.7)
First Gen	9,847.6	(4,618.2)	9,087.2	(4,102.1)	11,213.0	(5,147.4)
Low Income	10,034.8	(4,296.6)	9,361.1	(3,703.6)	11,298.1	(4,989.5)
Middle Income	10,223.3	(5,293.8)	9,149.1	(4,629.4)	12,039.1	(5,824.0)
High Income	10,049.9	(5,286.9)	8,934.2	(4,598.4)	11,741.4	(5,789.8)
75th	18,302.6	(5,655.9)	17,742.9	(5,519.6)	19,262.4	(5,754.2)
90th	25,386.5	(7,089.2)	25,060.5	(6,769.1)	25,949.1	(7,570.3)
Default Rate	0.041	(0.032)	0.046	(0.031)	0.035	(0.033)
Repayment Rate						
Overall	0.597	(0.169)	0.595	(0.149)	0.600	(0.197)
First Gen	0.539	(0.164)	0.553	(0.147)	0.517	(0.188)
Low Income	0.461	(0.170)	0.479	(0.151)	0.429	(0.195)
Middle Income	0.634	(0.147)	0.632	(0.136)	0.635	(0.165)
High Income	0.750	(0.113)	0.731	(0.107)	0.778	(0.116)

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. Student loan debt amounts are reported upon borrower entry into repayment. Median student loan debt 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. Low income is defined as students with family income less than 30,000 upon entering college. Middle income is defined as between 30,000 and 75,000 and high income is defined as above 75,000. The 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 and for students by household income bins.

Table 2: Long-term Repayment Outcomes Conditional on One Year Repayment

	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.

4. Empirical Strategy

I estimate the causal effect of PF education mandates on federal student loan repayment performance by relating the change in the share of each university cohort that is bound by a PF mandate to changes in federal student loan outcomes from the College Scorecard. Variation in the share of each cohort bound by mandates is driven primarily by adoption of state mandates, but is augmented by state shares of incoming cohorts. When a state changes course standards for high school graduation, all future high school students within the state are affected. When these students graduate high school and go to college, the share of students who took PF coursework rises in the universities they attend. This is most often universities within the adopting state. However, high school graduate enrollment in out-of-state colleges creates spillovers from adopting states to non-adopting states. Conversely, out-of-state students from non-adopting states limit the exposure at colleges in adopting states.

IPEDS includes counts of college enrollee previous state of residence to measure the share of students in each university cohort bound by state mandates. 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:

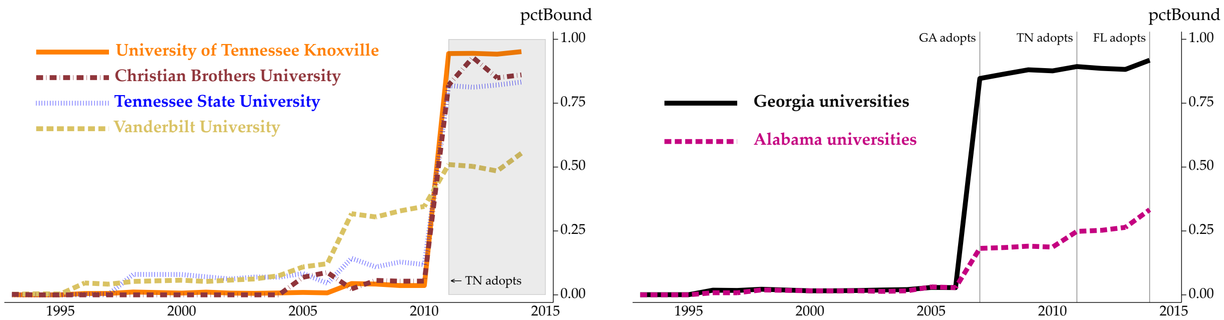
$$\text{pctBound}_{it} = \frac{\sum_{j=1}^{51} \text{pfMandate}_{jt} \times \text{enroll}_{ijt}}{\sum_{j=1}^{51} \text{enroll}_{ijt}}. \tag{1}$$

Figure 1a 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 PF mandate was the class of 2011. Typically, public universities receive large shares of their student bodies from in-state high schools. However, public universities like University of Tennessee Knoxville and Tennessee State University differ in the share of in-state students, which drives variation in pctBound after the state adopts a PF mandate. This heterogeneity is even starker for private universities like Vanderbilt University and Christian Brothers

Figure 1: Examples of Variation in pctBound

(a) Example of Within State Variation in pctBound

(b) Example of Across State Spillovers in pctBound



Notes: Figure 1a shows pctBound_{it}, as constructed in Equation (1), for four Tennessee universities over time. Data on previous state of residence come from IPES. Tennessee adopted a PF mandate first binding for the class of 2011, as shown by the shaded region in each plot. Figure 1b plots the state-level equivalent of pctBound_{it} where all universities in a state are aggregated. Georgia's mandate was first binding for the class of 2007. Alabama did not adopt a mandate during the sample, but is affected by Georgia's, Tennessee's, and Florida's adoption of students from high schools in those states attending Alabama universities.

University. 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 1b shows the state-level equivalent of pctBound_{it} where student bodies of all universities in a state are aggregated. When Georgia adopted a mandate binding on the class of 2007, Alabama universities experienced a corresponding increase in pctBound_{it}. Additionally, Alabama universities experienced subsequent increases in pctBound when Tennessee and Florida adopted mandates in 2011 and 2014, respectively.

I leverage this variation in pctBound across universities and over time to estimate the impact of state-mandated PF education on student loan outcomes. The main specification is:

$$y_{is,t+k} = \gamma \text{pctBound}_{it} + \beta \mathbf{X}_{ist} + \delta_i + \delta_t + v_{ist}, \tag{2}$$

where $y_{is,t+k}$ is an outcome for university i located in state s for high school cohort t with k being the number of periods between cohort t entering college and outcome y being observed. The coefficient of interest is γ which estimates the effect of increasing pctBound from zero to one. This estimation strategy is similar to the treatment intensity difference-in-differences strategy in Billings et al. (2020) where student debt outcomes for more-treated units are compared relative to their pre-treatment values and comparison groups who were either not treated or less treated by a shock.

A vector of control variables is 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 in math, science, English, and social studies and the total number of credit hours required. Since PF state standards might be introduced at the same time as changes in other course standards, these controls ensure γ is not also 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. I also include controls for whether cohorts had access to state merit aid scholarships since these may affect where students attend college and how much they pay. I include a vector of unemployment rates between periods t to $t+k$ to control for the local labor markets students face during college and after college. The data sources and construction of these variables are detailed in Appendix A.1. Lastly, I include both state (δ_i) and cohort (δ_t) fixed effects to control for time invariant factors within a state and common cohort factors across universities. Since pctBound_{it} varies across universities within the same state and year, γ is still identified even in the presence of state-by-year fixed effects. However, their inclusion absorbs a substantial portion of the variation in pctBound_{it} (as evidenced by Figure 1a), which causes a large loss of precision. For this reason, I do not present estimates with state-by-year fixed effects, but the results with these more granular fixed effects are broadly consistent with the main results.

In the baseline specification, I cluster standard errors at the state level to allow for correlation in the error term, v_{ist}, between universities in the same state s (Bertrand, Duflo, and Mullainathan, 2004; Cameron and Miller, 2015). However, since treated students are migrating across states to attend college, universities in different states probably also experience common unobserved shocks. If this occurs, errors might be correlated for universities *across states* and, consequently, clustering at the state level may produce excessively small standard errors (Barrios, Diamond, Imbens, and Kolesár, 2012). To address this concern, I conduct a randomization inference exercise in the spirit of MacKinnon and

Webb (2020) which produces p-values that are robust to across state correlations in v_{ist} for universities that receive students from the same states. This exercise is detailed in Appendix A.4.

The causal interpretation of γ relies on three key assumptions. First, since one dimension of variation in pctBound is driven by variation in states adopting course mandates over time, identification in this dimension is akin to a difference-in-differences identification strategy. As such, in order to infer causality from this variation we require the Parallel Trends Assumption. It must be the case that the outcomes in states who do not adopt mandates can serve as counter-factual outcomes for the states that do adopt mandates had they never adopted a mandate. Stoddard and Urban (2020) presents evidence that states adoption of PF mandates was not correlated with economic or political factors in the state. Further, I provide evidence in Section 5.3 that universities that faced larger changes in pctBound were not trending differently in student loan repayment rates before receiving more mandated students. Together, this evidence suggests that states with and without mandates were otherwise similar before passage of a PF mandate. Next, it must be the case that changes in pctBound do not lead to movements of students across repayment cohorts or to different universities. For example, if PF mandates lead students to remain in college longer, the matching of repayment cohorts to entering cohorts through k in Equation (2) would be confounded by pushing mandated students into later repayment cohorts than similar unmandated students. In Section 5.5.3, I present evidence that mandated students are no more likely to enroll in college or earn an associate's or bachelor's degree than unmandated students. I also present evidence in Section 5.3 that there are no intermediate effects from shocks to pctBound before the matched repayment cohort under this assumption. Lastly, for identification, it should also be true that PF mandates do not change student choice of universities. If exposure to PF mandates shifts college choice, it may be that changes in student loan repayment for universities more exposed to PF mandates might instead be due to changes to the sample of students attending the university rather than individual level improvements in student loan repayment. Stoddard and Urban (2020) show that PF mandates do not effect college enrollment or university choice and I present similar results in Section 5.5.2. A more complete discussion of these assumptions and a derivation of the main estimating equation from a more typical difference-in-differences specification is provided in Appendix A.2.

5. Results

5.1. Student Loan Debt

Table 3 presents the results of estimating Equation (2) for various measures of student loan borrowing before the start of repayment, with moments of the cohort original balances in Panel A and median student loan debt for subsamples of the student body in Panel B. The estimates presented in Panel A suggest minimal changes to student loan borrowing across the distribution. No estimate is statistically different from zero and each point estimate is proportionally small. The point estimates suggest small reductions in borrowing at lower student loan levels and small increases in borrowing at the upper ends of the distribution as a university moves from no students to all students bound by mandates. However none of these point estimates is statistically significant at even the 10% level.

Panel B shows the impact of PF mandates on median student loan debt for various student body subsamples. The results suggest minimal adjustments to borrowing for first-generation, low-income, and middle-income students with effect sizes smaller than 3% and no statistically significant estimates. By contrast, high-income borrowers reduce student loan balances. The estimates suggest that shifting to a fully mandated student body would lower median student loan balances for high-income students by $742, a reduction of 7.4%.

Reduced borrowing by high-income students may be because they cannot qualify for subsidized federal student loans which do not accrue interest while they are in school. Since PF coursework often teaches the difference between subsidized and unsubsidized loans, students with higher income parents might learn of this higher cost of borrowing and respond by reducing borrowing. In Oregon, students should learn to "compare and contrast the various types of loans available, how to obtain them and the function of compounding interest and explain the costs and benefits of borrowing money for post-secondary education" (State Board of Education, 2018). Similarly, students in Tennessee are expected to "explain the impact borrowing money to finance college could have on

Table 3: Dose Response Estimates: Student Loan Debt Upon Entering Repayment

A. Moments of the Cohorts Student Loan Debt Distribution

	(1) 10th	(2) 25th	(3) 50th	(4) 75th	(5) 90th
pctBound	–33.5 (76.7)	–231.0 (166.1)	–271.4 (348.4)	61.8 (317.8)	390.9 (409.8)
Outcome Mean	2,558.6	4,423.9	10,244.5	18,302.6	25,386.5
Percentage Effect	–1.3%	–5.2%	–2.6%	0.3%	1.5%

B. Median Debt by Subsamples of the Cohort Student Body

	(1) First Gen	(2) Low Income	(3) Middle Income	(4) High Income
pctBound	–124.8 (392.0)	160.6 (351.0)	–270.5 (350.9)	–742.4** (365.4)
Outcome Mean	9,847.6	10,034.8	10,223.3	10,049.9
Percentage Effect	–1.3%	1.6%	–2.6%	–7.4%

Regressions are weighted using the number of students used to compute each outcome metric. The analysis sample includes high school cohorts 1993 through 2012. 10th, 25th, 50th, 75th and 90th each represent the corresponding moment in a university's student loan debt levels for students entering repayment. Definitions of cohort subsample groups are described in Section 3.1. Fixed effects are included for universities and high school graduation years. Additional control variables are described in Section 4. Standard errors clustered at the state level are presented in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

future financial stability and security." This includes an assignment asking students to "craft an argumentative essay that either supports or opposes the use of student loan debt" (Karp, 2016). Hence, students who take these courses and complete this coursework may be more knowledgeable about student loans and understand cost differences between subsidized and unsubsidized loans. This may explain the divergent responses of low- and high-income student borrowing. An alternative explanation might be that students with higher income parents have better access to other financial resources (e.g. family loans).

By contrast, low-income borrowers may learn about the more advantageous terms of subsidized student loans, but are unable to increase their borrowing. For borrowers entering college between 2001 and 2005, over 70% took out the maximum amount of student loans (Black et al., 2020). If demand for federal student loan dollars from these students was higher than the federal limit, the allowable level of borrowing may not satisfy that demand. Additionally, experimental evidence suggests that many student loan borrowers do not make an active choice about *how much* to borrow, but only *whether* to borrow, *even after being provided with more information about borrowing options* (Marx and Turner, 2019a). As such, borrowers may be better able to adjust borrowing downward than increase borrowing.

5.2. Student Loan Repayment

The results from the estimation of Equation (2) for the cohort default rate and the repayment rate are presented in Table 4. Column 1 reports the estimates for the two-year cohort default rate while Columns 2 through 6 report estimates for the one-year repayment rate for the overall cohort and various cohort subsamples. The effect of PF mandates on defaults suggests a reduction of 0.2 percentage points (a 4.8% 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. This result is likely unsurprising since student loan borrowers can often avoid default by enrolling in an IDR planning or seeking out forbearance.

Table 4: 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	0.015	0.028**	0.025*	0.011	0.012
	(0.003)	(0.011)	(0.013)	(0.013)	(0.011)	(0.011)
Outcome Mean	0.041	0.597	0.539	0.461	0.634	0.750
Percentage Effect	–4.8%	2.5%	5.2%	5.4%	1.7%	1.6%

Regressions are weighted using the number of students used to compute each outcome metric. 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. Definitions of cohort subsample groups are described in Section 3.1. Fixed effects are included for universities and high school graduation years. Additional control variables are described in Section 4. Standard errors clustered at the state level are presented in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

By contrast, Columns 3 and 4 suggest that increased exposure to PF education mandates improves the one-year repayment rate for both first-generation and low-income students. The point estimates translate to a 2.8 and 2.5 percentage point increase in the repayment rate which corresponds to improvements of 5.2% and 5.4% for first-generation and low-income students, respectively, with both results significant at least at the 10% level. The point estimates for middle- and high-income students are positive, but are not statistically different from zero. This pattern suggests that PF education mandates improve the one-year repayment rate for first-generation and low-income students.

Table 5: 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	–0.002	0.020**	0.029**	0.025**	0.015	0.026**
	(0.002)	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)
Outcome Mean	0.046	0.595	0.553	0.479	0.632	0.731
Percentage Effect	–5.4%	3.4%	5.3%	5.3%	2.4%	3.5%
B. Private						
pctBound	–0.000	–0.006	0.013	0.015	–0.007	–0.019
	(0.007)	(0.025)	(0.022)	(0.030)	(0.019)	(0.016)
Outcome Mean	0.035	0.600	0.517	0.429	0.635	0.778
Percentage Effect	–0.2%	–1.0%	2.5%	3.5%	–1.1%	–2.4%

Regressions are weighted using the number of students used to compute each outcome metric. 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. Definitions of cohort subsample groups are described in Section 3.1. Fixed effects are included for universities and high school graduation years. Additional control variables are described in Section 4. Standard errors clustered at the state level are presented in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 5 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 4 to the public university sample in Table 5. First-generation and low-income students at public universities have higher repayment rates as a result of PF 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 2.6 percentage point improvement in repayment rate as a result of PF mandates, which is significant at the 5% level. This level increase is similar to that of first-generation and low-income students, but the proportional effect is smaller since high-income borrowers have a higher baseline repayment rate. However, an alternative framing could be to cast the effect as a reduction in non-repayment. With this framing, high-income students have the largest proportional reduction in non-repayment. To better solidify the economic magnitude of these effects, applying the point estimate improvements to the average repayment cohort would result in additional 6,817 low-income students and 4,447 high-income students with a smaller balance after one year in repayment.

The results presented in Panel B suggest no significant impact of PF education mandates for the sample of private universities. The estimates for low-income and first-generation students suggest improving repayment behavior, but the effect is smaller than the public school and are not statistically significant. Further, the estimates for middle- and high-income students are actually negative but remain imprecise. This divergence could be due to the identification strategy as smaller and more frequent shocks to pctBound experienced by many private schools could result in a loss of precision or it may be the case that students who attend private universities are different from those who attend public universities.

In total, the results suggest that personal finance education mandates improve federal student loan performance after college. The effect is largest for low-income and first-generation students and for students at public universities. This is consistent with other studies that find course mandates are more effective in improving outcomes for vulnerable populations (Stoddard and Urban, 2020; Goodman, 2019). However, I also find an improvement in student loan repayment for high-income students at public universities. Since high-income students bound by PF mandates are also shown to reduce student loan borrowing, it is likely the improvement in repayment stems from smaller balances. However, this mechanism does not explain why first-generation and low-income borrowers are better able to repay their student loans. Other mechanisms may cause improvements for these borrowers which I investigate further in Section 5.5.

5.3. Event Study Specification

In addition to the dose response specification from Equation (2), I also estimate an event study specification. This specification presents evidence in support of the Parallel Trends Assumption, tests whether mandated students affect repayment cohorts other than their assigned cohort, and tests whether mandates become more effective over time. A typical event study flexibly estimates how units experiencing an event differ from those not experiencing an event in each period relative to the event. However, since pctBound is a continuous exposure measure, care must be taken to define an event. I define a university event as a year-over-year change in pctBound_{it} of 50 percentage points or larger:

$$\text{event}_{ist} = 1 \cdot \{\text{pctBound}_{it} - \text{pctBound}_{i,t-1} \geq 0.5\}. \tag{3}$$

I choose the 50 percentage point threshold to ensure a university can only experience a single event. The estimating equation for the event study is identical to Equation (2) aside from the event study parameters:

$$y_{is,t+k} = \sum_{j=-2}^{0} \gamma_j \text{event}_{is,t+j} + \sum_{j=2}^{10} \gamma_j \text{event}_{is,t+j} + \beta \mathbf{X}_{ist} + \delta_i + \delta_t + \varepsilon_{ist}. \tag{4}$$

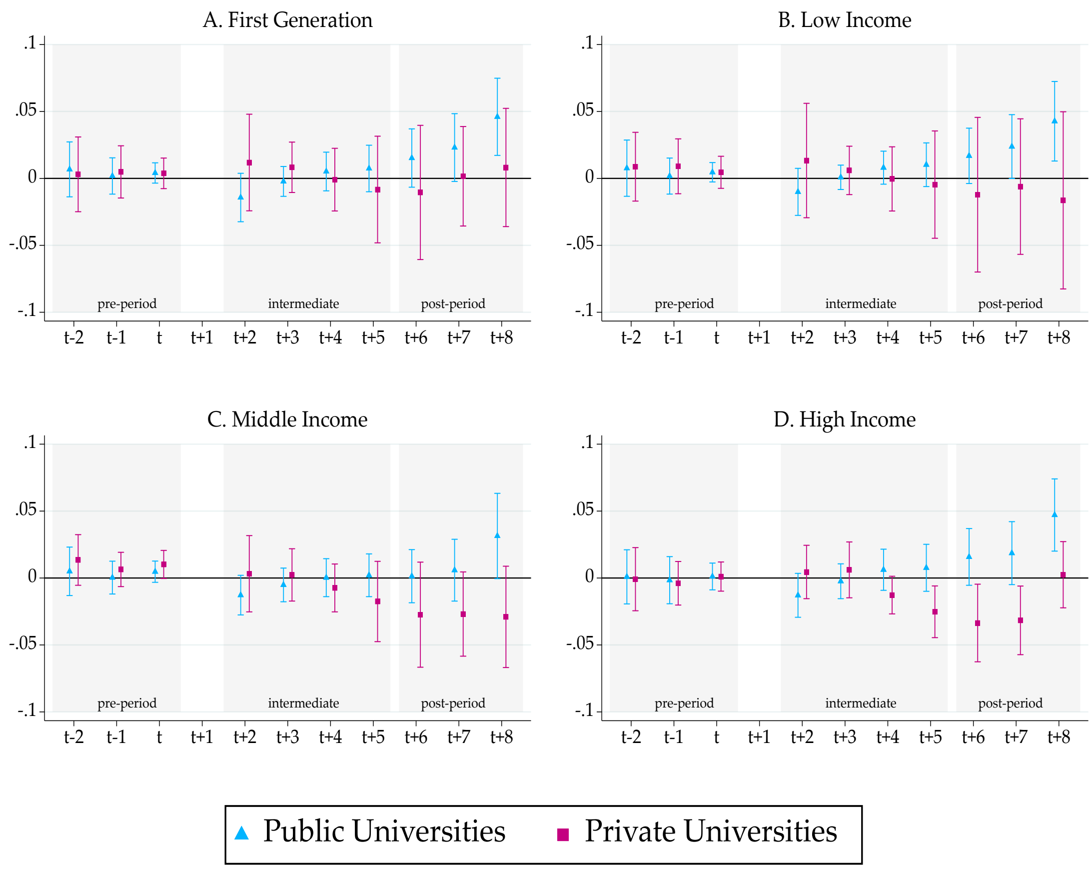
In this specification, the event occurs in period t and a separate parameter, γ_j is estimated for each period relative to an event with period $t+1$ acting as the reference period. This results in ten estimated parameters across the event space.

I present the results of estimating Equation (4) in Figure 2 which reports the coefficients and 95% confidence intervals for the repayment rate subgroups separately for public and private universities. In all four panels and for each sample, there is no differential trend between universities experiencing and not experiencing an event before treated students enter college. Further, there does not appear to be a significant effect on university repayment rates during the intermediate periods for any of the student groups or samples. The estimates for the post-period parameters for low-income (first-generation) students at public universities range from 1.7 percentage point (1.5 percentage points) in period $t+6$

to 4.3 percentage points (4.6 percentage points) in period $t + 8$, which is consistent with the point estimates presented in the main specification. The growth in the point estimates from period $t + 6$ to $t + 8$ suggests either improvements in the treatment effect over time or a "catching-up" effect coming from students taking longer than four years to graduate college. The results in Panels C and D for middle- and high-income students suggests no significant effect and is largely consistent with the muted estimates presented in Table 4. Consistent with the main results, the estimates for the private school sample are muted for low-income and first-generation students and suggest noisy but negative estimates for middle- and high-income students.

In total, these results present evidence in support of the Parallel Trends Assumption and suggest little concern of spillover across repayment cohorts. In each event study specification, the coefficients corresponding to periods before 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 first-generation and low-income students have higher repayment rates at public universities. The point estimates suggest 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 4.3 percentage points higher, respectively, which are larger than the effects reported in Table 5.

Figure 2: Event Study Coefficients for Repayment Rate



Each panel in the above figure presents the vector of event study parameters separately for public and private universities 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 PF mandates.

5.4. Robustness

In this section, I briefly summarize a series of robustness exercises that demonstrate the results presented above are not sensitive to various assumptions and functional forms of the estimating equation. A more complete discussion of each exercise is detailed in Appendix A.

5.4.1. Randomization inference to account for potential correlation of university-level errors across states

First, as discussed in Section 4, I cluster all standard errors in the main analysis at the state level to allow for correlations in the error term for universities in the same state.

However, it is possible that errors can be correlated for universities across state lines if universities in different states receive similar sets of students from other states. In this case, standard errors would be too small and I would over-reject the null hypothesis. To test for this potential over-rejection, I conduct a randomization inference exercise as described in MacKinnon and Webb (2020). I run 3,000 placebo replications of Equation (2) in which I randomly assign pfMandate_{jt} and use these randomly generated values to compute pctBound_{it} using the observed enroll_{ijt}. For randomly drawn mandates, the estimated γ should be equal to zero. I compute empirical p-values to test for rejection of the null hypothesis by comparing the estimate for γ using the true values of pfMandate_{jt} with the distribution of placebo estimates from the randomization inference algorithm. I compute two-sided p-values by calculating the share of placebo estimates larger in absolute value than the estimated γ. Using this process, I find that p-values using the RI algorithm are similar to p-values using state-clustered standard errors. Appendix A.4 compares these p-values. Each of the statistically significant results discussed above remains statistically significant using the RI p-values.

5.4.2. *Difference-in-differences analogue and potential biases associated with Two-Way Fixed Effects estimators*

Next, I test whether the results above are sensitive to the dose response specification in Equation (2) by estimating a more typical difference-in-differences specification. I restrict the sample to universities with more than 70% of students attending from in-state high schools and use the state level pfMandate_{jt} to assign treatment status. This sample contains 380 public universities and 367 private universities. The results are reported in Table A.2. They are remarkably similar to the results presented in Table 4, which suggests that most of the variation in university exposure to PF mandates stems from within-state.

One additional advantage of estimating a more typical differences-in-differences model is the ability to test whether these results are sensitive to critiques that have recently been raised with two-way fixed effects (TWFE) models due to staggered treatment timing and/or heterogeneous treatment effects (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2020; Sun and Abraham, 2020). This particular setting may be subject to these critiques since (1) mandates are adopted by different states at different times, (2) treatment effects are likely to be heterogeneous across states since mandates differ across states, and (3) treatment effects are heterogeneous over time since the effect of mandates grow over time. Goodman-Bacon (2021) shows that the TWFE estimator is a weighted average of each possible two-by-two DD comparison combination, some of which use previously treated units as "control" units for later treated units. If the impact of the mandate grows over time as is found in the literature and in Section 5.3, these comparisons will bias the TWFE estimator *toward zero* since the treatment effect for previously treated units will continue to grow over time.

To test whether the results are sensitive to these biases and, if so, in what direction, I replicate the high in-state DD strategy above, but I remove both the early and late adopting states between 2001 and 2008 cohorts retaining only the three states who implemented a mandate binding first for the 2005 graduating cohort. For this exercise, there is no variation in treatment timing for adopting states and the repayment rate for the treated units are observed for four periods before the binding mandate and four periods after the binding mandate which allows for estimating the effect of a "more mature" mandate.

In Table 6, I report three variations of this exercise to test whether the treatment effect is stable across the composition of comparison units. The first estimate compares the three 2005 adopting states to the sample of never treated states. The second estimate compares the 2005 adopting states to the sample of not-yet-treated states which include states who adopted a mandate after the outcome data window (cohorts 2009 and beyond). The third estimate includes both sets of comparison units. For brevity, I present these estimates for the first-generation student repayment rate at public universities, but the results are similar (albeit smaller) for the other student subsamples at public universities. The point estimates across these three comparison groups are all similar suggesting that the selection of comparison groups does not significantly affect the estimates. However, each of the estimates is significantly larger than the main estimates from Table 5 and the estimates from Table A.2. This is likely due to both heterogeneous treatment effects and staggered treatment timing causing the more typical difference-in-differences specification to be biased *toward zero*. If repayment rate data were available for subsequent years for the late-adopting states, we would expect for the estimated effect to be larger since mandates improve over time. Additionally, if a state adopting a mandate is able to extract best practices from states that previously adopted, we would expect earlier adopting states to have less effective mandates than later adopting states. Hence, biases caused by heterogeneous treatment effects and staggered treatment time in this setting should underestimate the effect of PF mandates on student loan repayment.

Table 6: High In-state Difference-in-difference estimates for 2005 Adopting States and Various Control States for First Generation Repayment Rate

	(1)	(2)	(3)
pctBound	0.061***	0.073***	0.073***
	(0.008)	(0.007)	(0.007)
Comparison	Never treat	Not-yet treat	Never + Not-yet
Sample	Public	Public	Public
Outcome	First Generation	First Generation	First Generation

Sample is restricted to only universities with more than 70% of students attending from within the state during the sample. pfMandate equals one if the university's state has a binding PF mandate. Regressions are weighted using the number of students used to compute each outcome metric. The outcome variable is the repayment rate for first generation students at public universities. The sample of treated states include those who adopted PF mandates binding first for the 2005 cohort: Louisiana, Arizona, and Arkansas. The first column includes the treated states and all states who never adopted a mandate. The second column includes the treated states and states that adopted a mandate binding for the class of 2009 and beyond (see Table A.5 for full list). The last column includes both never adopters and those adopting in 2009 and later. Fixed effects are included for universities and high school graduation years. Additional control variables are described in Section 4. Standard errors clustered at the state level are presented in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

5.4.3. Alternate construction of variables

Next, I consider two alternative constructions of pctBound_{it}. In Appendix A.2, I derive the necessary assumptions to interpret γ as the difference-in-difference estimate that would be estimated using micro-level data. One of these assumptions requires that university students do not change their choice of college as a result of a binding PF mandate. I estimate a specification that is robust to this assumption by constructing an alternative pctBound_{it} using fixed shares of students from each state with enrollment data from before 1994. Hence, variation in this alternative measure, $\widehat{\text{pctBound}}_{it}$, is unaffected by potential changes in enrollment patterns due to PF mandates. I discuss this specification more completely in Appendix A.5.2 and present the point estimates in Table A.3. The estimates from the baseline specification are similar to the estimates using $\widehat{\text{pctBound}}_{it}$

which suggests changes in college choice are not driving the main estimates. The next construction of pctBound_{it} uses estimated values to replace missing values of enroll_{ijt} rather than linearly interpolating between observed values. Appendix A.5.3 details this process more completely and Table A.4 shows that the estimates are similar between linear interpolation and instrumentation.

5.5. Mechanisms

In this section, I investigate the channels that might improve repayment outcomes.

5.5.1. Information Intervention

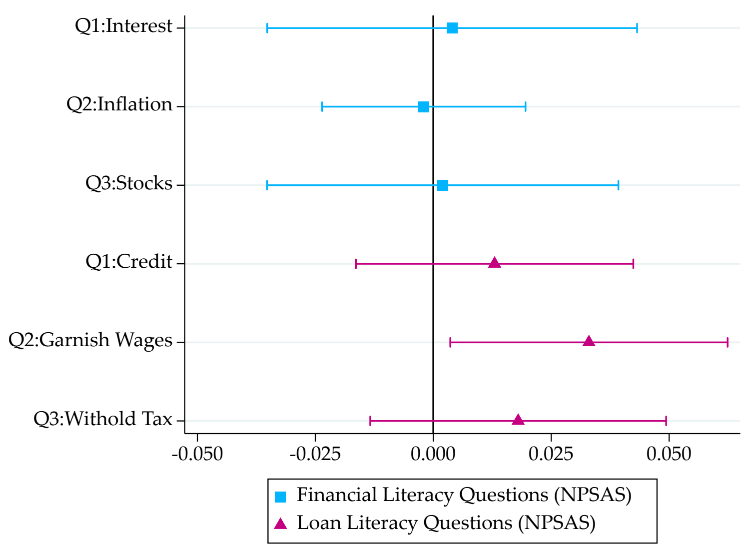
First, I test whether students bound by PF education mandates in high school are better able to answer financial literacy and federal student loan questions. I use micro-level data from a nationally representative survey of college students, the National Postsecondary Student Aid Survey (NPSAS). The 2016 NPSAS wave began asking college students three financial literacy questions and three questions about federal student loan repayment, detailed in Table A.8. These new data provide previously unavailable insights into the financial literacy of current college students. However, since these questions were only included in one survey wave, comparisons of mandated and non-mandated students from the same state largely rely on students surveyed at different ages. Regardless, the novelty of the questions asked in this survey necessitate its use. The estimating equation takes the form

$$y_{ist} = \gamma \text{pfMandate}_{st} + \beta X_{ist} + \delta_s + \delta_t + \varepsilon_{ist}, \tag{5}$$

where y_{ist} is a binary variable for whether respondent i from state s graduating from high school in year t correctly answered a particular question. X_{ist} includes a vector of binary control variables that includes race, gender, and year in college and the vector of state-by-graduation year controls for merit aid, high school staffing, and credit requirements as in Equation (2). γ is the parameter of interest, which captures the difference in the probability of mandated and non-mandated students correctly answering a particular question. Last, I include state and high school graduation year fixed effects and use the included survey weights so the analysis is nationally representative. Standard errors are clustered at the high school state level.

Figure 3 plots the coefficient estimates and 95% confidence intervals from the estimation of Equation (5). Table A.7 reports the full slate of coefficients. The point estimate 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 PF mandate. Although this result may suggest PF mandates are ineffective in improving financial knowledge, the literature suggests that correct answers to the "Big Five" financial literacy questions have low predictive power of positive financial behaviors (Anderson et al., 2017). Moreover, correct answers can be invalided through small changes in question phrasing (Van Rooij et al., 2011). Still, each of the estimates for the three federal student loan literacy questions is positive, with increases ranging from 1.3 to 3.3 percentage point in the probability of a correct answer. The largest effect is for the question asking whether the federal government can garnish wages for non-payment of federal student loans.

Figure 3: 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.Question text is available in Table A.8 and a full table of coefficient estimates and standard errors is available in Table A.7.
Source: U.S Department of Education, National Center for Education Statistics, Restricted-use National Postsecondary Student Aid Study 2016.

The evidence suggests mandated students are more knowledgeable about regulations governing federal student loans. If so, greater familiarity with the rules and regulations make student loan borrowers better able to repay loans. These students might be more aware of IDR plans, which have been shown to improve repayment progress and reduce delinquencies (Herbst, 2020), and various forbearance or deferment options. This is consistent with the mechanism that specific financial literacy instruction delivered "just-in-time" may be more effective than general financial literacy instruction (Brown et al., 2019). On the other hand, mandated students might also be more aware of various options for loan forgiveness which might incentivize these borrowers to not repay their loans in hopes of loan cancellation. However, this is unlikely to be advantageous for most borrowers. While the Public Service Loan Forgiveness (PSLF) program allows for loan forgiveness after ten years, IDR plans require 20-25 years of on-time payments before forgiveness. These students would need to make 240 to 300 on-time monthly IDR payments before forgiveness. IDR plans calculate the monthly payment by using the Adjusted Gross Income from a borrower's tax filings, so a borrower can only reduce their monthly payment with IDR by reducing or misreporting income. In addition, since mandated students are more likely to know the federal government can garnish wages and tax returns for non-payment, they may be more likely to repay this debt since they know the government can go to extreme means to recuperate payments. Still, mandated students may be more likely to learn about the PSLF program. In that case, previously PF mandated borrowers with "underwater" student loans might be more likely to work in public or non-profit sectors in exchange for eventual loan forgiveness which may increase the cost of that program. However, these borrowers would need to remain in the public or non-profit sector for ten years while making 120 on-time IDR payments and only those with sufficiently high student loan balances and low-income would receive forgiveness after ten years of payments.

5.5.2. College Attendance

In this section, I test whether PF mandates change the college attendance decision of high school students which helps inform tests whether PF mandates shift college choice. I use the IPEDS previous state of residence data to track the flow of high school students from each state into different colleges. The IPEDS data includes the variable enroll_{ijt} which is the number of students in the incoming cohort for university i that 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 university characteristics. This procedure generates a variable detailing the percentage of high school students from each state attending universities of a given type. Equation (6) illustrates an example of the variable construction using the university characteristic Public4yr_i, which equals one if university i is a four-year public college and Seniors_{jt} is the total number of enrolled high school seniors for the graduating cohort t from the Department of Education's Common Core of Data.

$$\text{pctPublic4yr}_{jt} = \frac{\sum_{i \in I} \text{Public4yr}_i \times \text{enroll}_{ijt}}{\text{Seniors}_{jt}} \tag{6}$$

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

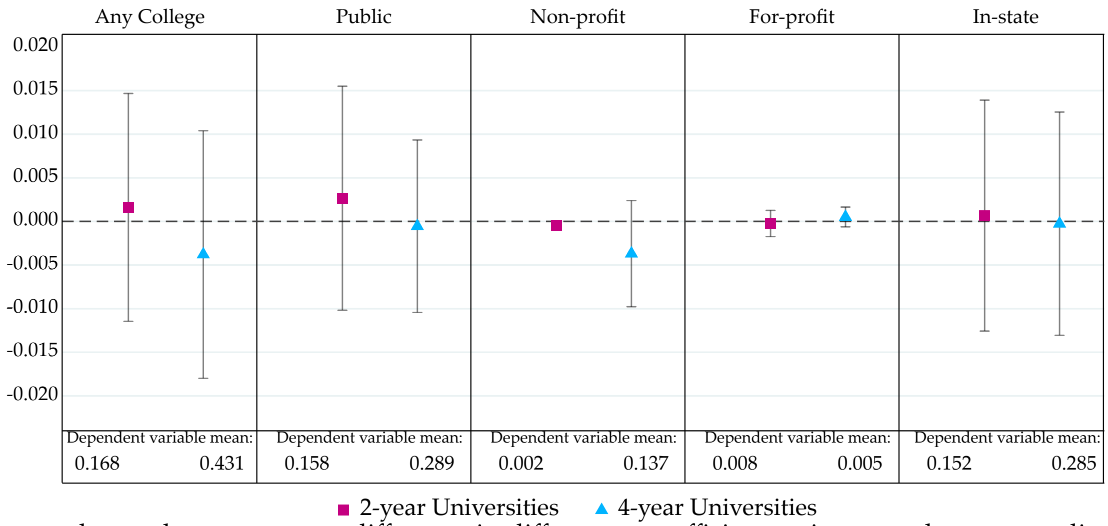
I use these constructed variables as outcome measures for a state-by-year difference-in-differences specification to test whether the state adoption of a PF education mandate alters the share of students attending various types of colleges. The estimating equation is similar to Equation (2):

$$y_{jt} = \gamma \text{pfMandate}_{jt} + \beta \mathbf{X}_{jt} + \delta_j + \delta_t + \varepsilon_{jt}, \tag{7}$$

where y_{jt} is a state-level variable, such as pctPublic4yr_{jt}, and pfMandate_{jt} is a binary variable indicating whether state j had a binding PF mandate in effect for cohort t. The vector X_{jt} is the same set of state-by-graduation year level controls as in Equation (2). δ_j and δ_t are state and year fixed effects, respectively.

Figure 4 presents point estimates and 95% confidence intervals for this specification. All reported point estimates are smaller than a one percentage point change in either

Figure 4: 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.

direction. No estimate is statistically different from zero at any conventional significance threshold. The results are consistent with the literature which finds no changes in college attendance or choice of college as a result of a binding PF education mandate (Stoddard and Urban, 2020).

5.5.3. Degree Completion

Next, I test whether PF education mandates have an impact on the probability a student earns a degree. If PF education leads to better matching of students with colleges or degree programs, students may be more successful in college. As a result of graduation, students should have better labor market prospects, which would lead to better repayment rates and lower chances of default. I use the American Community Survey (ACS) one-year samples from 2005 to 2017 to test whether students subject to PF mandates were more likely to hold college degrees or have ever attended college. The estimating equation for these tests is identical to Equation (5). However, I remove students younger than 22 because they are unlikely to have earned a bachelor's degree. Since the ACS also does not ask respondents for the year of high school graduation, I assume respondents graduated from high school in their 18th birthday year. I report results in which 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 7 reports the results of this estimation on the linear probabilities of earning a bachelor's degree or an associates degree, and the probability a respondent ever attended college. Odd-numbered columns identify mandate status by birth state. Even-numbered columns use state of residence. Estimates across all columns suggest no effect of PF education mandates on the probabilities of earning a bachelor's degree, an associates degree, or having ever attended college. In sum, I find little compelling evidence to suggest that PF education mandates shift a high school student's decision to attend college, the type of college they choose to attend, or the probability of degree completion.

5.5.4. Career-oriented Mandates

Last, I test whether PF mandates specifically designating career research in their standards are more effective in improving federal student loan repayment. Many states require students to research various careers and study the requirements, income and benefits, and risks associated with the chosen career. For example, students in Arkansas are asked to "explore potential careers (including an employment forecast) and the steps needed to achieve them based on interests and/or talents." Students in Utah are instructed to "evaluate and compare career opportunities based on individual interests, skills, and educational requirements; the value of work to society; income potential; and the supply and demand of the workforce, including unemployment." In researching potential careers, students might learn that careers that previously interested them have low earning capacity or high unemployment rates. As a result, students may choose a different career path causing greater earning potential which may improve student loan repayment ability after leaving college.

I test this mechanism by selecting the subsample of states with PF mandates that specifically include material or exercises addressing career research. Of the 24 states that adopted mandates binding prior to 2014, 15 have standards addressing career research - Arizona, Arkansas, Florida, Georgia, Iowa, Louisiana, Michigan, Missouri, New Jersey, North Carolina, South Carolina, South Dakota, Tennessee, Utah, and Virginia. I then

Table 7: 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	-0.000	0.000	0.001	-0.002	-0.006	-0.006
	(0.002)	(0.002)	(0.001)	(0.002)	(0.004)	(0.005)
Observations	2,670,765	2,670,765	2,670,765	2,670,765	2,670,765	2,670,765
Cohorts	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008	2001-2008
High School State	Birthplace	Residence	Birthplace	Residence	Birthplace	Residence
Outcome Mean	0.196	0.196	0.083	0.083	0.688	0.688
Percentage Effect	-0.2%	0.1%	0.7%	-1.9%	-0.9%	-0.9%

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. *** p<0.01, ** p<0.05, * p<0.1

construct $\text{pctBound}^{\text{career}}$ using Equation (1) with these states set to pfMandate equal to one. I re-estimate Equation (2) using $\text{pctBound}^{\text{career}}$ and compare these estimates to the baseline estimates.

Table 8 reports the results of this exercise. Each of the coefficients from the public school sample of Table 5 remain statistically significant when only considering the limited subsample of PF mandates focusing on career research. Further, each of these point estimates increases in magnitude with the largest increases for first-generation and low-income students and the smallest increase for high-income students. As in Table 5, each of the coefficients for the private school sample remain small and statistically insignificant. The results from the public university sample suggest that high school instruction and exercises surrounding career research and career readiness may improve financial conditions after college. However, we cannot conclude that this effect is casual. Since states that include career research as a topic might also include other additional topics or better implement their mandates, these unobserved factors might be driving the larger point estimates. Regardless, these results provide at least some new evidence that PF mandates incorporating career research may be more effective at improving student loan repayment.

Table 8: Dose Response Estimates for Career-oriented Mandates: 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^{career}	–0.003	0.023*	0.034**	0.030**	0.019	0.026*
	(0.003)	(0.013)	(0.015)	(0.014)	(0.014)	(0.014)
Outcome Mean	0.046	0.595	0.553	0.479	0.632	0.731
Percentage Effect	–6.3%	3.9%	6.2%	6.2%	3.1%	3.6%
B. Private						
pctBound^{career}	0.003	–0.019	0.002	0.007	–0.020	–0.026
	(0.015)	(0.031)	(0.026)	(0.038)	(0.022)	(0.019)
Outcome Mean	0.035	0.600	0.517	0.429	0.635	0.778
Percentage Effect	8.2%	–3.1%	0.3%	1.7%	–3.1%	–3.3%

Notes: pctBound^{career} is created using the subsample of PF mandate states that have specific standards focused on career research as described in Section 5.5.4 . Regressions are weighted using the number of students used to compute each outcome metric. 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. Definitions of cohort subsample groups are described in Section 3.1. Fixed effects are included for universities and high school graduation years. Additional control variables are described in Section 4. Standard errors clustered at the state level are presented in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

6. Discussion

This paper expands our understanding of personal finance education mandates and federal financial aid in several key dimensions. First, I find heterogeneity in the impact of PF mandates on federal student loan borrowing. Although I find no impact on borrowing for first-generation, low-income, or middle-income students, I do find a reduction in borrowing for high-income students. This divergence may be due to means testing for federal subsidized student loans. Since high-income students are ineligible for subsidized loans, they face a higher cost of borrowing than lower- and middle-income students. Many PF course requirements include assignments that require students to research the costs and weigh the pros and cons of borrowing for college.High-income students may learn their cost of borrowing is higher and take on less debt due to improved literacy regarding federal student loans. By contrast, lower-income students may learn of the more advantageous terms of subsidized federal student loans and wish to increase borrowing, but are unable to do so. Over 70% of student loan borrowers borrow at the maximum allowed loan amount, which means the federal limit is likely binding for most of these students. This would prevent low- and middle-income students from increasing borrowing, which is consistent with these findings.

Next, I find that students bound by PF mandates in high school were better at repaying student loan balances in the short-run. I also present evidence that short-run repayment improvements are strongly correlated with long-run repayment success. The impact on repayment is largest and most precisely estimated for low-income and first-generation students, which is consistent with the literature showing course mandates have larger effects on more vulnerable populations (Stoddard and Urban, 2020; Goodman, 2019). The results suggest low-income and first-generation students are roughly 5% more likely to have paid down some of their original balance one year after beginning repayment. I also present evidence similar to Brown et al. (2016) that mandates become more effective over time. I find a roughly 7% improvement in federal student loan repayment for low-income and first-generation students subject to PF mandates at least three years old. By contrast, I find no statistically significant impact of PF mandates on repayment by middle-income students. I do find improvements for high-income students at public universities, which may be because they have smaller balances when they begin repayment. Despite some suggestive evidence of improvements, I cannot conclude that mandates have any meaningful impact on default. This result should not be surprising since student loan default is a rarer and more adverse outcome that can be avoided through forbearance or IDR, while repayment progress is a more sensitive measure that is more difficult to manipulate.

Last, I also investigate the primary channel through which these improvements operate. I find no evidence that students bound by personal finance education mandates are better able to answer typical financial literacy questions when they are in college. This does not necessarily imply that PF mandates are ineffective at improving financial literacy or financial behavior. It is possible that improvements in financial literacy depreciate quickly after high school or that non-mandated peers quickly catch up. It is also possible that the ability to answer financial literacy questions is a poor proxy for financial skills that tangibly improve financial outcomes (Anderson et al., 2017). Several findings in the literature show PF mandates improve financial outcomes other than student loan repayment(Urban et al., 2018; Stoddard and Urban, 2020; Brown et al., 2016; Harvey, 2019).

I present evidence that students bound by personal finance mandates are more knowledgeable about the federal financial aid system. Students subject to mandates are more likely to correctly answer questions about federal student loans. This suggests mandated students may be better able to repay student loans in part because they are more familiar with the federal student loan system. Given the increased public cost of the federal student loan system (Congressional Budget Office, 2020), PF education in high school may be a cost-effective intervention for improving federal student loan repayment, reducing the volume of student loans going into default or forgiven. The magnitude of this potential public cost savings should be studied further with better data on long-term student loan outcomes. I also find that personal finance education mandates that include career research in the state standards are associated with larger student loan repayment improvements. This suggests that mandated students may be better at repaying loans due to better labor market outcomes. Whether mandated personal finance education improves labor market outcomes after college is another potentially fruitful topic for future research.

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A. Appendix

A.1. Data Appendix

A.1.1. College Scorecard

All data used in the analysis was pulled from the College Scorecard website using the February 7, 2022 update.

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 A.1.[2] If the 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.[3] 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.[4]

Figure A.1: Repayment Status Bins for Repayment Cohort

A	B	C	D
Making payments & paying down balance	Making payments & not paying down balance	In forbearance or deferment	In default

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

[2]The College Scorecard database also includes the repayment rate for 3, 5, and 7 years. However, the data begins for all variables for FY 2006 and thus these variables have very small windows of data availability.

[3]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.

[4]Default for students loans is atypical compared with other consumer debt. Upon default, there is no repossession of assets since the loans are unsecured. Rather, the federal government levies fines and allows

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.*

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.[5] The most recent data in the Scorecard covers students that entered repayment in the 2013 fiscal year.[6] 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 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.

A.1.2. 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,

loan services to garnish wages and tax refunds to collect outstanding debts.

[5]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.

[6]For repayment cohort counts smaller than 30 students, the data is suppressed and thus these small

2011, 2009, 2007, 2005, 2003, and 2001. I impute missing values by linearly interpolating between the nearest non-missing years. In addition, AppendixA.5.3 estimates Equation (2) by using instrumented values of enrollment counts rather than linear interpolation. In fewer than 3% of observations in the sample, the total number of students enrolled within 12 months of graduating high school are missing despite data for the overall cohort being reported. For these observations, I calculate the historical share of the incoming cohort for each university-feeder state that graduated high school within the last 12 months and I apply that share to the university's reported full incoming cohort for that feeder state-year in instances of misreports.

A.1.3. 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.1.4. 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 these 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

cells are omitted from the analysis.

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 tracks 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.1.5. 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.1.6. 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 (1), 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}:

$$\text{(A.1)} \qquad \mathrm{X}_{it} = \frac{\sum_{j=1}^{51} \mathrm{x}_{jt} \times \mathrm{enroll}_{ijt}}{\sum_{j=1}^{51} \mathrm{enroll}_{ijt}},$$

This vector includes the state level measures of high school staffing and high school graduation requirements.[7] In addition to these state weighted controls, $\mathbf{X}_{it}$ also includes binary variables for whether the state offered a merit aid scholarship along with unemployment rates for periods t through $t + k$.

A.2. Proof of Consistency of Dose Response Specification

In this section, I show that the γ estimate from Equation (2) consistently estimates the micro-level difference-in-differences estimate under three assumptions. To begin, suppose state-level graduation requirements divide students into those in states that require high school students to complete PF coursework and those not required. Using a difference-in-differences framework, the effect of the required coursework on an outcome is estimated using the equation

$$\text{(A.2)} \qquad y_{ist} = \alpha + \gamma^{DD} D_{st} + \delta_s + \delta_t + \varepsilon_{ist},$$

where D_{st} denotes whether state s had a binding mandate for cohort t and δ_s and δ_t are state and year fixed effects. y_{ist} denotes an outcome variable for individual i belonging to graduating cohort t from state s. 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 were not bound by a state mandate. In

[7]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

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, consider the case where outcomes are 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 $j \in \mathcal{J}$. The outcome $Y_{j\tau}$ is defined as

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

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 next that the parameter γ^{DD} can be consistently estimated using the aggregated estimating equation:

$$Y_{j\tau} = \alpha + \gamma^{DD}\text{pctBound}_{j\tau} + \delta_j + \delta_\tau + e_{j\tau} \tag{A.3}$$

where the variable attached to γ^{DD}, $\text{pctBound}_{j\tau}$, corresponds to the fraction of the cohort τ for university j that were bound by the state-level policy. The necessary assumptions

of high school graduation standards.

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: $G(\, i, D_{s(i)t} = 1) = G(\, i, D_{s(i)t} = 0)$

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:

$$y_{ist} = \alpha + \gamma^{DD} D_{s(i)t} + \delta_{s(i)t} + \varepsilon_{ist}, \tag{A.4}$$

Using the assumption that $k_i = k$ for all i, we can define $\tau := t + k$. 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

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

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 (A.4):

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

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 $\sum_{s=1}^{S} \delta_s \cdot 1\{s(i) = s\}$ 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:

$$Y_{j\tau} = \alpha + \gamma^{DD}\text{pctBound}_{j\tau} + \delta_j + \delta_t + v_{j\tau}. \tag{A.5}$$

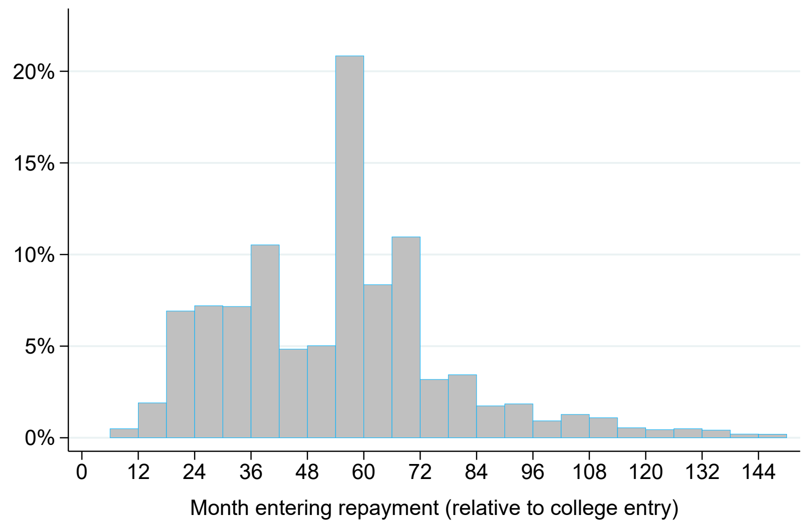
Hence, under the three aforementioned assumptions, the aggregate university-level specification consistently estimates the micro-level difference-in-differences specification.

A.3. Discussion of Identifying Assumptions

Stoddard and Urban (2020) present evidence in support of both the Parallel Trends Assumption and the Stability of University Mapping Assumption. First, they present evidence that states that adopt mandates are not observably different nor are they trending differently than states that did not adopt a mandate in the years surrounding adoption. Further, they present evidence that students from states that ultimately adopted a mandate did not have significantly different financial aid outcomes for cohorts prior to mandates adoption. These results suggest that mandated states and non-mandated states were trending similarly prior to mandate option which lends evidence toward the Parallel Trends Assumption. I also present evidence in Section 5.3 that universities that received more mandated students were not trending differently in student loan repayment outcomes.

To help inform the Cohort Matching Assumption, Figure A.2 shows the distribution of the month students in the 2003 incoming college cohort entered repayment. The modal 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. This suggests that a plurality of students will enter the College Scorecard data six years after high school graduation. As such, I set $k = 4$ for student loan debt upon entering repayment and $k = 6$ for student loan repayment outcomes to match this trend. While these assumptions match the modal student loan borrower, the data show a significant share 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 $k = 6$. To test sensitivity of this assumption to the main results, I present the results of a flexible event study specification in Section 5.3 which tests whether there are intermediate effects of treated students potentially entering repayment prior to $k = 6$.

Figure A.2: 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.

Lastly, identification relies on the third assumption, Stability of University Mapping. In essence, the estimate of γ would be biased if students exposed to PF mandates changed their college enrollment decision. Using a difference-in-difference strategy with micro-level data, Stoddard and Urban (2020) present evidence that students bound by mandates were no more likely to choose to attend college nor were they any more likely to attend college full- or part-time. Further, these students were no more likely to attend a private university, a university with lower tuition, an in-state university, or a four-year (versus two-year) university. In addition to these findings, I present evidence in Section 5.5.2 which suggests that states adopting mandates did not send their college seniors to different types of schools after the mandate was binding. Additionally, in Appendix A.5.2, 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.4. Randomization Inference Algorithm

I estimate many "placebo" replications of Equation (2) 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 apply the observed enroll_{ijt}. I then estimate Equation (2) 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 and a comparison of p-values using cluster robust standard errors and p-values using the randomization algorithm are detailed in Appendix A.4.

The randomization inference algorithm used to compute the empirical p-values is based off the RI-β algorithm in MacKinnon and Webb (2020). I conduct 3000 replications of Equation (2) 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.[8] In each of these replications, it should be the case that the

[8] 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."

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[9]

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 (2) 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:

$$\bar{p} = \frac{1}{3000} \sum_{n=1}^{3000} 1 \cdot \left\{ |\hat{\gamma}_n| \geq |\hat{\gamma}_{true}| \right\} \tag{A.6}$$

Table A.1 replicates Table 4 and includes the p-values calculated from this algorithm. Across each outcome, the p-values using CRVE are always smaller than the p-values using the RI-β algorithm, suggesting that there is indeed some correlation in error terms across states. However, these larger p-values do not change the level of significance for the main

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

results, namely the improvement in the one-year repayment rate for low income and first generation borrowers.

Table A.1: Dose Response Estimates: Cohort Default Rate and Repayment Rate - CRVE and Empirical P-Values

	Default Rate	Repayment Rate				
	(1) Overall	(2) Overall	(3) First Gen	(4) Low Income	(5) Middle Income	(6) High Income
pctBound	–0.002	0.015	0.028	0.025	0.011	0.012
	(0.489)	(0.200)	(0.033)**	(0.063)*	(0.340)	(0.286)
	[0.525]	[0.156]	[0.020]**	[0.043]**	[0.232]	[0.188]
Outcome Mean	0.041	0.597	0.539	0.461	0.634	0.750
Percentage Effect	–4.8%	2.5%	5.2%	5.4%	1.7%	1.6%

Regressions are weighted using the number of students used to compute each outcome metric. 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. Definitions of cohort subsample groups are described in Section 3.1. Fixed effects are included for universities and high school graduation years. Additional control variables are described in Section 4. P-values using standard errors clustered at the state level are presented in parenthesis. Empirical p-values using the randomization inference described in Appendix A.4 are presented in brackets. *** p<0.01, ** p<0.05, * p<0.1

A.5. Alternative Specifications

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

A.5.1. State-Level Difference-in-Differences for High In-state Universities

In this section, 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.[10] 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 (2)

$$y_{is,t+6} = \gamma \text{pfMandate}_{st} + \beta \mathbf{X}_{st} + \delta_s + \delta_t + \varepsilon_{ist}, \tag{A.7}$$

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 (2), pfMandate_{st} is equal to one if the 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.2 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 show improvements in the one-year repayment rate for first generation and low income students similar to those found in Table 4. This suggests that the main findings are not sensitive to the continuous construction of pctBound.

adopting states in 2007.

[10] 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.2: Robustness: Difference-in-Differences Estimates for High In-state Universities

	Default Rate	Repayment Rate				
	(1) Overall	(2) Overall	(3) First Gen	(4) Low Income	(5) Middle Income	(6) High Income
pfMandate	–0.002	0.019*	0.028**	0.025**	0.012	0.017
	(0.002)	(0.010)	(0.011)	(0.010)	(0.010)	(0.010)
Outcome Mean	0.045	0.586	0.546	0.469	0.627	0.728
Percentage Effect	–5.4%	3.2%	5.1%	5.2%	2.0%	2.3%

Sample is restricted to only universities with more than 70% of students attending from within the state during the sample. pfMandate equals one if the university's state has a binding PF mandate. Regressions are weighted using the number of students used to compute each outcome metric. 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. Definitions of cohort subsample groups are described in Section 3.1. Fixed effects are included for universities and high school graduation years. Additional control variables are described in Section 4. Standard errors clustered at the state level are presented in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

A.5.2. Robustness to Changes in College Enrollment

In this section, I present evidence that the results presented above are robust even if the Stability of University Mapping Assumption is violated. Note that in Appendix A.2, it was necessary to assume that the term

$$\left[\sum_{s=1}^{S} \delta_s \left(\text{StateShare}_{sj\tau} - \sum_{\tau=k}^{T+k} \text{StateShare}_{sj\tau}\right)\right]$$

was independent of pctBound. For this specification, instead of assuming pctBound is independent of the StateShare terms, I construct an alternate pctBound that holds the StateShare terms constant over the sample years. As such, Appendix A.5.2 now equals zero since there can be no deviation from current year state shares to the mean state share of students over the panel. 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). 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 PF mandates in the period before PF 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:

$$\widehat{\text{pctBound}}_{it} = \sum_{j=1}^{51} \left[\frac{\overline{\text{enroll}}_{ij,S}}{\overline{\text{enroll}}_{i,S}} \right] \times \text{pfMandate}_{jt} \quad . \tag{A.8}$$

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 PF 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.

Table A.3 reports the point estimates from this robustness exercise. For all outcome variables, the point estimates are largely unchanged between the baseline specification and the shift-share specification. 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 PF mandates stem from a university's exposure to PF mandates rather than from transitory changes in student enrollment induced by PF mandates.

A.5.3. Instrumenting for Enrollment Counts

As noted in Section 3, 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

Table A.3: Robustness: Dose Response Estimates with Fixed Enrollment Shares

	Default Rate	Repayment Rate				
	(1) Overall	(2) Overall	(3) First Gen	(4) Low Income	(5) Middle Income	(6) High Income
$\widehat{\text{pctBound}}$	–0.002	0.016	0.029**	0.025*	0.011	0.013
	(0.003)	(0.012)	(0.013)	(0.014)	(0.011)	(0.011)
Outcome Mean	0.041	0.597	0.539	0.461	0.634	0.750
Percentage Effect	–4.6%	2.6%	5.4%	5.5%	1.8%	1.8%

$\widehat{\text{pctBound}}$ is created using fixed shares of students from feeder states as described in Appendix A.5.2. Regressions are weighted using the number of students used to compute each outcome metric. 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. Definitions of cohort subsample groups are described in Section 3.1. Fixed effects are included for universities and high school graduation years. Additional control variables are described in Section 4. Standard errors clustered at the state level are presented in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

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 (A.9) details the specification for this strategy:

$$\begin{aligned}\text{enroll}_{isjt} =& \delta_i + t \cdot \delta_i + t^2 \cdot \delta_i + t \cdot \delta_s + t^2 \cdot \delta_s \\ & + t \cdot \delta_{ij} + t^2 \cdot \delta_{ij} + \text{meritAid}_{jt} + \text{meritAid}_{jt} \cdot \{s = j\} \\ & + \delta_{jt} + \varepsilon_{isjt}\end{aligned} \tag{A.9}$$

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 (1) to re-estimate Equation (2). Table A.4 presents the results from this exercise. The estimates are largely consistent with the estimates presented in Table 4.

Table A.4: Robustness: Dose Response Estimates with Instrumented Enrollment

	Default Rate	Repayment Rate				
	(1)	(2)	(3)	(4)	(5)	(6)
$\widetilde{\text{pctBound}}$	–0.002	0.014	0.026**	0.023*	0.010	0.010
	(0.003)	(0.010)	(0.011)	(0.012)	(0.010)	(0.010)
Outcome Mean	0.041	0.597	0.539	0.461	0.634	0.750
Percentage Effect	–3.9%	2.3%	4.8%	5.0%	1.5%	1.3%

$\widetilde{\text{pctBound}}$ is created using instrumented values of enroll_{ijt} as described in Appendix A.5.3. Regressions are weighted using the number of students used to compute each outcome metric. 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. Definitions of cohort subsample groups are described in Section 3.1. Fixed effects are included for universities and high school graduation years. Additional control variables are described in Section 4. Standard errors clustered at the state level are presented in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

A.6. Additional Tables and Figures

Table A.5: Implementation of Personal Finance (PF) Mandates Since 1990

State	Coursework	First Cohort Bound
New Hampshire	Incorporated (Economics)	1993
New York	Incorporated (Economics)	1996
Michigan	Incorporated (Career Skills)	1998
Wyoming	Incorporated (Social Studies)	2002
Arizona	Incorporated (Economics)	2005
Arkansas	Incorporated (Economics)	2005
Louisiana	Incorporated (Free Enterprise)	2005
South Dakota	0.5 Credit (Economics or Personal Finance)	2006
Georgia	Incorporated (Economics)	2007
Idaho	Incorporated (Economics)	2007
North Carolina	Incorporated (Economics)	2007
Texas	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
New Jersey	2.5 Credits (Economics or Personal Finance)	2011
Tennessee	0.5 Credit	2011
Kansas	Incorporated (Economics)	2012
Oregon	Incorporated (Social Studies)	2013
Florida	Incorporated (Economics)	2014
Virginia	0.5 Credit	2014

PF mandate data are from Stoddard and Urban (2020). 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.

Table A.6: Events per Academic Year

Academic Year	Events	Adopting States
1996	87	New York
1997	0	
1998	35	Michigan
1999	0	
2000	0	
2001	0	
2002	2	Wyoming
2003	0	
2004	0	
2005	36	Louisiana, Arizona, Arkansas
2006	10	South Dakota
2007	133	Texas, Idaho, Georgia, North Carolina
2008	7	Utah
2009	39	South Carolina, Colorado
2010	30	Missouri
2011	65	Tennessee, New Jersey, Iowa
2012	12	Kansas
2013	13	Oregon
2014	60	Virginia, Florida
Total	529	

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.7: 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. *** p<0.01, ** p<0.05, * p<0.1

Table A.8: Question Text for Financial Literacy and Student Loan Literacy Questions

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

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