

Impact of Financial Education Mandates on Economically Disadvantaged Students' Postsecondary Decisions

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ABSTRACT

Economically disadvantaged students borrow more to finance postsecondary education and attend riskier institutions than their wealthier counterparts. One oft-discussed policy solution is to mandate financial education in schools; however, previous research is mixed as to its success in improving financial capability. Employing data on institutional quality and student finances, I use difference-in-differences to exploit cross-state and student-cohort variation in financial education mandates to assess how these mandates impact postsecondary education decisions. I find that students under the mandate made better college and financing decisions than students who were not. However, these effects are heterogeneous. Both socio-economic subgroups subject to the mandate are more likely to enroll full-time and borrow fewer amounts of federal student loans conditional upon borrowing any, plausibly through reducing use of unsubsidized loans. However, later-generation and higher-income students under the mandate are particularly more likely to enroll in institutions with lower cohort default rates. This study suggests that states without mandates may want to consider establishing them to ensure that students make postsecondary education choices that maximize their benefits. However, policymakers need more information about why results pertaining to college choice are driven by later-generation and higher-income students to determine the most efficient and equitable implementation strategy.

JEL Classifications: I2, J1

Keywords: public policy, financial education, college choice, financial aid, young adults, disadvantaged students

* Harvey: Pardee RAND Graduate School, mharvey@prgs.edu. Version 12.31.2017. Paper prepared for the 2018 ASSA/AEA Annual Meeting. I am grateful for comments to date from Katie Carman, Trey Miller, Jeremy Burke, Phil Armour, Robert Sanders, Steven Raphael, Isaiah Andrews, Gery Ryan, Lara Arsinian, and all participants of the RAND Labor and Population Brown Bag Seminar. *This is a draft report containing work in progress; please do not cite or quote without the author's permission.*

Pursuing postsecondary education is often the first yet most consequential financial decision that most young adults make, and college students are increasingly relying on debt to pay for it. Total student loan debt in the United States is an estimated \$1.4 trillion (Board of Governors 2017). Approximately 2.9 million students earning undergraduate degrees in 2016 graduated with an average debt of \$37,173 (Kantrowitz in Picchi 2016; NCES 2016, Table 105.30). Currently, one out of four student loan borrowers are in default (CFPB 2016).

According to the 2015 National Financial Capability Study (NFCS), only one out of five college students can answer all three questions about compound interest. More specifically, two-thirds of college students can answer general questions about compound interest and know that loans with shorter repayment periods require higher monthly payments but charge less in interest. However, only one-third of college students can correctly calculate how much time it would take for a \$1,000 loan compounded at 20 percent per year to double if they did not pay anything off. Doing comparison shopping for any loans requires understanding what interest rates are and how compound interest works.

Specifically regarding student loans, the 2015 NFCS asked respondents: “[i]f you could go through the process of taking out loans to pay for your education all over again, would you take the same actions or make a change?” Among current college students and undergraduate completers not pursuing any graduate studies who reported borrowing for themselves, a significantly higher proportion of those borrowing any private loans (58 percent) said that they would “make a change” than those borrowing only federal student loans (49 percent). This suggests that students may be realizing ex-post that they have made suboptimal decisions. In fact, there is some underlying misunderstanding about student loans because at least half of college students and graduates report that they would “make a change.”

Many of the outcomes related to student loan debt may not only be due to lack of financial knowledge, but may also stem from where students decide to attend college. For instance, 14 percent of all first-time beginning college freshmen attended for-profit institutions in the 2011 – 2012 academic year.¹ For-profit institutions are known to be more expensive than non-profit institutions (average net price of \$17,667 versus \$14,532).² Graduates of for-profit institutions borrow significantly less than graduates of non-profit institutions (average median debt of \$15,063 versus \$18,192), but experience significantly higher unemployment rates (median rate of 3.5 percent versus 3.8 percent) and earn significantly less post-entry than graduates of non-profit institutions.³ Figure 1 below suggests that the earnings gap between for-profit graduates and non-profit graduates widens across number of years post-entry. The earnings gap between

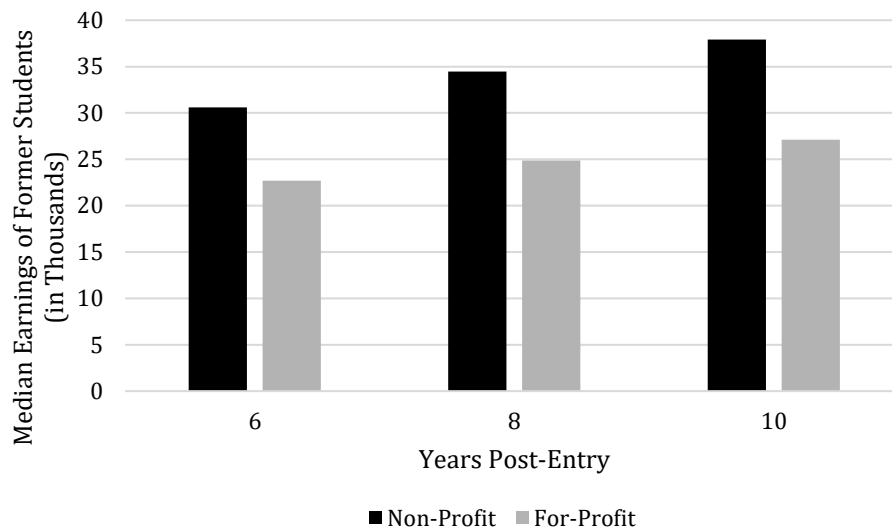
¹ Author’s calculations online using the BPS:12/14 via NCES DataLab’s QuickStats function.

² Author’s calculations of College Scorecard latest cohort data (last update provided in September 2017). All differences cited in this paragraph are statistically significant at the one percent level.

³ Ibid. The trend on median debts also align with the findings in the Federal Reserve Board’s *Report on the Economic Well-Being of U.S. Households in 2015*, under “Education Debt and Student Loans.” They attribute this to the fact that students at selective institutions borrow more than anyone.

non-profit graduates and for-profit graduates is \$7,907 six-years post-entry, \$9,606 eight-years post-entry, and \$10,805 ten-years post-entry.⁴

Figure 1. Median Earnings of Former Students from Non-Profit versus For-Profit Institutions During Fiscal Year 2016



SOURCE: U.S. Department of Education, College Scorecard (2017)

In postsecondary financing, benefits concern labor market outcomes. However, benefits received from attending a given postsecondary institution are not well-known nor advertised. To resolve this information asymmetry, there have been a few efforts to reveal graduates' median earnings by major or by institution such as The Hamilton Project, College Scorecard, and state publications on economic outcomes of college graduates; yet, these efforts are still in process of being tweaked (Akers and Chingos 2016; Brookings Institution 2017; U.S. Department of Education 2017).

Another applicable, oft-discussed policy solution is school-based financial education. Financial education mandates are state-level policies that require teaching personal finance in public schools. Policymakers thought of financial education courses to address more general issues with financial literacy and consequential decision-making. Yet when framed properly, concepts on credit, debt, comparison shopping, and investments can be applied to decisions regarding postsecondary education as well. Many argue that school-based financial education is important to ensuring that students have a basic set of information needed to make sound financial decisions. Only half of college students learn about finances and money management

⁴ Ibid.

from their parent(s) or guardian(s).⁵ Another 38 percent of college students do not learn about finances from home or school.⁶

This study focuses specifically on state mandates that require students to complete a personal finance course to graduate from high school. This policy has substantial political and stakeholder support, with recent movement to incorporate college choice and financial aid concepts in personal finance curricula among states with mandates (e.g. Pelletier 2017).⁷ Yet, it is not clear if these mandates have improved students' financial capability – whether it includes college financing as part of the curriculum or not.

Past studies on the efficacy of financial education mandates present mixed results. These studies typically examine effects of mandates on middle-aged adults' savings rates, investment behavior or wealth accumulation, and on young adults' credit behavior. The age division in the literature corresponds to the life cycle, where we would expect to see older adults investing and building wealth, and young adults borrowing to smooth out consumption. Regardless of age group and its corresponding behaviors, these studies find that mandates either improve financial decisions, or have no effects.

To the best of my knowledge, no research to date has focused on the impact of mandates on college choice and little research has focused on the impact of mandates on financial behaviors among economically vulnerable youth and young adults. Fernandes, Lynch and Netemeyer (2014, 1873) recommend “‘just-in-time’ financial education tied to a particular decision” so that the concepts are more relevant. These mandates would be “just-in-time” for high school juniors and seniors because they are deciding whether to obtain postsecondary education, where to obtain it, and how to finance it. Understanding college choice and financing decisions from a financial decision-making viewpoint is critical because finances most determine persistence, and are the most common reason that students drop out of school (e.g. Engle, Bermeo and O'Brien 2006; Stinebrickner and Stinebrickner 2008; Joo, Durband and Grabble 2008; Eitel and Martin 2009; Johnson et al. 2009). This then exacerbates the issue that the largest proportion of student loan defaulters is college dropouts (e.g. Gladieux and Perna 2005; Nguyen 2012; Dynarski 2015).

To examine impacts of financial education mandates on young adults' college choices and college financing behaviors, I obtain data on institutional characteristics from the U.S. Department of Education's College Scorecard (2015), data on students' college attendance and financing behaviors from the restricted versions of the 1996, 2004, and 2012 Beginning

⁵ According to author's calculations of the 2015 NFCS.

⁶ Ibid.

⁷ According to their state standards or legislations, Alabama, Missouri, Tennessee, Texas, and Utah explicitly cover postsecondary financing in their high school personal finance courses. However, this was amended to their standards after the mandates were officially implemented. Since these amendments fall outside of the study period, I cannot assess any policy heterogeneities based on explicit content standards. New Jersey's amendment is pending approval, effective for the graduating class of 2021 (State of New Jersey, 217th Legislature 2016; Reitmeyer 2017).

Postsecondary Students' Longitudinal Study, and data on state mandates from Urban and Schmeiser (2015). I use difference-in-differences to exploit cross-state and student-cohort variation in financial education mandates. Given that higher education is an investment, I hypothesize that financial education mandates may encourage students to attend institutions generating better outcomes for their enrollees, and may increase probabilities of students using federal financial aid products. I restrict the sample for this study across several criteria, most importantly to those under age 23 at the time of enrollment to ensure that identification is not confounded with age.

This paper is organized as follows: The literature review summarizes previous studies on the role of information in college choice, observed college financing choices among students, and on the effectiveness of financial education mandates. The methodology section explains the identification strategy and model specifications. The data section describes the survey data, administrative data, and state mandate database used in my analyses. The findings section examines the effects of financial education on college choices and college financing. General findings are presented, as well as findings by heterogeneous effects and robustness checks. The limitations section discusses data issues, and implications for current and future analyses. The concluding section highlights key findings and its implications for financial education policy to date.

Literature Review

This research draws on three literatures. The first strand considers what college choices students are making, and how information affects those decisions. The second strand investigates what financial products students use to pay for postsecondary education, and if they are optimally choosing financing options. The third strand examines the impact of financial education mandates on using various products, especially among college students and young adults. The previous literature identified on college choice and financing decisions considers the impact of individual characteristics and informational interventions, none of which includes formal financial education.

The first part of postsecondary financing is institutional choice. Some studies explore initial college choices, especially among high-achieving low-income students. Regardless of achievement status, economically disadvantaged students and underrepresented minority students tend to apply to and enroll in less selective and for-profit institutions (Deming, Goldin and Katz 2012; Hoxby and Avery 2013; Black, Cortes and Lincove 2015; Hoxby and Turner 2015). Reasons for this phenomenon may be that the less selective institution is closer to home; caters to students' religion or culture; does not feel stereotypically threatening or otherwise elitist; appears to be cheaper; or is flexible to students' work schedules and familial needs. Other reasons may include that these students did not have any resources available in high school to discuss college options and financial aid. Yet, one posit that has been formally tested in the

literature is lack of information. Using data on low-income high school seniors from a college advising program in Massachusetts, Castleman and Goodman (2018) find that participants were more likely to enroll in the more selective institutions and were more likely to enroll in institutions with lower net prices than their non-participating peers. When Hoxby and Turner (2015) gave information about applying to colleges, net costs of colleges, resources, and fee waivers to high-achieving, low-income students via a randomized intervention, these students were much more likely to apply to and attend more selective institutions than their non-receiving peers.

Note that low-income students made better college choices once they received informational interventions. They could plausibly receive such information in their personal finance courses as well, especially in cases where personal finance is integrated into career preparedness classes.⁸ Accordingly, my paper adds to this first strand of literature by assessing if another form of informational intervention (school-based financial education) can improve financial prospects (labor market and debt burden outcomes) by improving college choices. To the best of my knowledge, no study to date has examined the impact of state-mandated financial education on students' college choices.

Then, the second part of postsecondary financing is figuring out which products to use to pay for postsecondary education. Crucially, students must apply for financial aid to receive grants or loans from the federal government, many state governments, and most institutions. As of 2017, nearly half of high school seniors did not fill out a Free Application for Federal Student Aid (FAFSA) (National College Access Network 2017).⁹ The National Center for Education Statistics (2016, Figure 1) reports that 20 percent of all undergraduate students did not apply for any financial aid; of which an additional ten percent did not fill out a FAFSA. Common reasons that students do not fill it out are because the forms are too complicated and require substantial amounts of financial information (Dynarski and Scott-Clayton 2006; Baum et al. 2012; Bettinger et al. 2012). Students limit their set of financing options when they do not file a FAFSA. Economically disadvantaged students not completing this form then may be more prone to use sub-optimal options to fill the gap.

In regards to loan options, which students are more likely to use student loans or credit cards has been extensively studied. Yet, none of them look at taking required financial education courses as a potential factor. Generally, students with high levels of student loan debt are more likely to have high levels of credit card debt (Lyons 2008). This particularly applies to students of color, first-generation college students, and independent students (Wei et al. 2005; Pinto and Mansfield 2006; Lyons 2008). Another study finds that at one university, first-generation college

⁸ To date, Alabama and Tennessee integrate personal finance into career preparedness courses. Michigan and Utah include explicit discussion about career options.

⁹ According to NCEC calculations using the ELS: 2002, 90 percent of high school seniors who complete the FAFSA successfully enroll in college versus only 55 percent of those who did not fill out a FAFSA (NCES, ELS: 2002, Tables 1 – 2).

students were more likely to rely on student loans; more likely to believe that loans are the only way they can afford college; and that they are more debt averse (Lee and Mueller 2014). Other types of students at risk for misusing or mismanaging debt include students receiving need-based financial aid (Lyons 2008).¹⁰ My paper will enhance understanding of which students are more likely to borrow student loans by assessing if exposure to financial education differentially impacts those who rely more on student loans to finance postsecondary education ex-ante.

Besides sheer use of debt, studies demonstrate that students are making sub-optimal trade-offs, such as substituting federal student loans with more expensive debt (e.g. credit cards) or with job earnings (working more hours) (Avery and Turner 2012; Lavecchia, Liu and Oreopoulos 2014). The main tradeoff we see first-generation college students make are working more hours instead of borrowing student loans (Engle, Bermeo and O'Brien 2006). But, the Consumer Financial Protection Bureau and U.S. Department of Education (2012) found that many private student loan borrowers do not borrow the maximum amounts allowed under federal student loans before borrowing private student loans. Students whose parents' highest education level was an associate's degree or less are significantly more likely to use a combination of federal and private student loans than students whose parents' highest education level is college graduate or beyond (Consumer Financial Protection Bureau and U.S. Department of Education 2012). While studies demonstrate that these trade-offs are occurring, no study explores if financial knowledge reduces to what extent these trade-offs occur. While this study does not explicitly examine trade-offs as an outcome, this study does examine impacts on federal student loan borrowing. In most cases, federal student loans are the cheapest loans available to students, and provide the most flexible options.¹¹ Greater use of federal student loans may suggest lesser use of private student loans or credit card debt.¹²

In this paper, I build upon this second strand of literature by examining if school-based financial education can influence likelihoods that students fill out the FAFSA. Students cannot access any federal student aid – and many state-level financial aid as well as some need-based institutional aid – without completing the FAFSA application. If financial education increases the probability that students apply for federal financial aid, then it may increase use of federal student aid products.

¹⁰ Lyons (2008, 188) defines misusing or mismanaging debt as either “having credit card balances at \$1,000 or more; being delinquent on credit card payments by at least two months; reaching credit card limits; or only [paying] off credit card balances some of the time or never.”

¹¹ There are state loan programs that sometimes offer better terms than federal student loans. For example, Texas's B-on-Time Loans charged zero percent interest and converted to a grant if a student completed a Bachelor's degree with less than 150 semester credit hours and a 3.0 GPA (THECB 2017).

¹² Post-graduation, this author will explore these trade-offs by proxying maxima according to Woo and Horn (2016) using 1996 – 2016 waves of NPSAS. I do not analyze private student loan or credit card use because this information is not available for the 1996 cohort.

The final strand of literature examines the impact of state-mandated financial education on financial behaviors, especially as they concern young adults. Two studies using the Federal Reserve Bank of New York/Equifax Consumer Credit Panel find that young adults under state-mandated financial education had higher credit scores and fewer credit delinquencies than their peers who were not required to take any financial education courses in high school (Brown et al. 2014; Brown et al. 2016).

Two studies concentrate specifically on college students. Mandell and Klein (2009) and Gutter and Copur (2011) examine the effects of high school financial education on college students' financial behaviors (e.g. paying credit cards on time, not writing bad checks, balancing checkbooks, competence in savings and investments, compulsive buying, and willing to take average financial risk) using cross-sectional datasets. It is not clear if these general financial behaviors are employed in paying for postsecondary education. Yet, Mandell and Klein (2009) find null effects while Gutter and Copur (2011) find positive effects. There could be several reasons that the results from these two studies differ from one another, including differences in when the studies were conducted and differences in their sampling frame. Mandell and Klein (2009) survey high school classes of 2001 – 2004 from only one school system with a small sample size (N = 79). Meanwhile, Gutter and Copur (2011) sample college students across 15 universities who graduated high school between 2004 and 2008 (N = 15,797). Nevertheless, the fact that at least Gutter and Copur (2011) find that financial education positively influences general financial behaviors may translate to specific college financing behaviors. For instance, when amount of expected student debt to be held at graduation is controlled for, the amount of student debt is negatively associated with paying credit cards late (Mandell and Klein 2009). This suggests that student loan debt and credit cards may be substitutes in the financial options' market.

Policymakers are now pushing to have concepts on career choice, postsecondary education, and financial aid taught in high school classrooms. A few states recently amended (or plan to amend) their personal finance content standards to cover these concepts. The State of New Jersey, for example, plans to amend the curriculum to include “instruction on existing state and federal student loan and tuition assistance programs, scholarships and grants. [Students] would also learn about student loan repayment issues and the consequences of not paying down student debt in a timely manner” amidst rising student loan debts (State of New Jersey, 217th Legislature 2016; Reitmeyer 2017). It is likely that some high school personal finance teachers already teach concepts on financial aid, award letter comparison, and student loans to their high school junior and seniors.¹³ Yet, we currently do not know if formal classroom-based financial education may help students choose less risky institutions or less costly financing. Of note, Fernandes, Lynch and Netemeyer (2014, 1873) recommend “‘just-in-time’ financial education tied to a particular

¹³ Based on personal conversations with multiple teachers during the Council on Economic Education's Annual Financial Literacy and Economic Education Conference, 2016 & 2017.

decision, enhancing perceived relevance and minimizing forgetting.” These mandates may be “just-in-time” for high school students who are making decisions about where to go to college and how to finance it during a time that personal finance courses are offered. This effect may be through the explicit mechanism of discussing financial aid with students, or through applying discussions about credit and debt, financial planning, budgeting, and investing to the higher education scenario.

Assessing the relationship between state-mandated financial education and postsecondary education outcomes is a growing concern. My paper adds to the literature on college choice by assessing if school-based financial education could improve these decisions (e.g. enroll in institutions generating better labor market outcomes for its former students, and enroll in school full-time as opposed to part-time). To the best of my knowledge, this paper is the first study to examine impacts of financial education policies on students’ college choices. Additionally, my paper builds on previous research about financial aid use by assessing if school-based financial education can influence likelihoods that students apply for financial aid, use any financial aid, or engage in any sub-optimal financing options (e.g. working while enrolled). Finally, this paper takes special care to explore socio-economic heterogeneity to assess impacts of formal financial education on groups that may more likely rely on it. Results from this study may not only pertain to school-based financial education, but also to students’ ability to apply those lessons toward postsecondary education planning – an activity not obviously realized as financial decision-making.

Theoretical Framework

Higher education is an investment good, since students (consumers) are paying for a product now in exchange for higher future income, where the extra earned had they not pursued the higher education is the dividend. Like with all investments, there are two elements that a consumer must consider: the cost of the product versus the benefits that the product will reap. Ideally, the benefit will accumulate in value over time. Hence, students should select the institution whose benefits will ultimately exceed costs. In higher education, the benefits (lifetime earnings) should exceed the total cost of attendance.

Then, students need to consider how to pay for their postsecondary education. The options available for students to finance their education are grants and scholarships, federal student loans, private student loans, other debt (e.g. credit cards, personal loans), job earnings (including work-study), or other financial supports. Most students (consumers) have limited income to finance their education; therefore, they will need loans and grants to cover all educational costs. This means that the main foci for financial decision-making in higher education are institution selection and borrowing decisions.

The life-cycle theory in economics suggests that it is rational for students to borrow against expected future income to pay for their education and to smooth out consumption over the

lifetime. Hence, the concern is not necessarily that students are borrowing money; but rather, it is whether students are selecting the cheapest bundle of financing options available to pay for an appropriate college where the student can expect to more than recoup their expenses. Economic theory suggests that consumers should finance higher education by exhausting cheaper options first, and then using more expensive options. However, this is not always the case (e.g. McSwain, Price and Cunningham 2006; Avery and Turner 2012; Consumer Financial Protection Bureau and U.S. Department of Education 2012).

I hypothesize that one reason for these sub-optimal decisions is a lack of financial literacy and financial education. Financial education may reduce search costs and mitigate other behavioral biases. Low financial literacy is more prevalent among low-income college students and their families, who may not understand the costs of higher education, and may not understand or know of all the available means to finance higher education (Long 2008). Low-income college students and their families are more hesitant to use student loans, even when that option is in their best interest (Lavecchia, Liu and Oreopoulos 2014). Debt aversion may lead to underinvesting in education, or resorting to sub-optimal solutions such as working part-time or using higher-cost debt options rather than subsidized student loans (Lavecchia, Liu and Oreopoulos 2014). Students may take on more costly forms of debt because of our tendency to choose what we already know about. For example, low-income college students and their families may not be as familiar with education loans as they are about credit cards.

Methodology

In this paper, I investigate the impact of state-mandated financial education on student college choice and financing decisions. I particularly analyze state mandates that require high school students to take personal finance courses as a core prerequisite for graduation because these are where concepts on credit and loans, investments, financial planning, and so forth are covered. I focus on the short-term postsecondary-related decisions (hence, freshman-year outcomes) because these are most proximal to the intervention.

I use a difference-in-differences approach with repeated cross-sections to assess if financial education mandates impact college choices and financing decisions. Difference-in-differences is an estimation technique that allows researchers to control for underlying differences between state trends and cohort trends to isolate the effects of the mandate. Difference-in-differences suggest causality if the trends of the studied outcomes between states with mandates and states without mandates are similar prior to mandate implementation.

My empirical strategy identifies the impact of state-mandated financial education on postsecondary education decisions by high school graduation year. In particular, my empirical approach exploits variation across college students within the same state before and after the mandate was implemented, and across college students in states with mandates and states without mandates within the same high school graduating class. We assume that high school

financial education mandates are exogenous to students.¹⁴ While treatment is exogeneous to students, the states' decision to require personal finance courses may not be random. Some states may have mandated financial education due to an economic crisis (whether that be at the state level or federal level). Note that all public high school students are required to take the course to graduate, regardless of any preferences. We can also assume exogeneity on the grounds that financial education mandates vary across states and over time.¹⁵

The outcomes I examine are all related to poor financial decisions among students pursuing postsecondary studies just after high school. Additionally, these decisions are made early in students' academic career, and are made close to mandate exposure. I intend to study outcomes that clearly constitute financing mistakes or sub-optimal decision-making. These outcomes and their financial rationale are the following:

- *Attends four-year institution*: the concern about institutional level selection is time to earning a Bachelor's degree when starting at a two-year institution versus starting at a four-year institution. For several reasons (e.g. credit loss when transferring, special requirements for all students at transfer school, time to transfer to a four-year), starting on a Bachelor's degree at a two-year institution may take longer than starting on it at a four-year institution.
- *Attends for-profit institution*: for-profit institutions are costlier, and are known to generate poorer labor market outcomes than non-profit institutions.
- *Enrolls full-time*: it is optimal for most students to enroll full-time. Failure to do so adversely affects aid eligibility (especially Pell grants), and decreases the probability of finishing school. In many cases, it ironically costs students more to attend school part-time than to attend school full-time.
- *3-year CDR of institution*: cohort default rates (CDRs) are a proxy of cost-benefit. Higher cohort default rates indicate that greater proportions of borrowers in repayment are defaulting on their loans.
- *Median earnings of former students 10-years post entry*: higher median earnings signal greater benefits of attending the particular institution.
- *Debt-to-earnings ratio*: I constructed this variable by dividing the median debt for completers expressed in 10-year monthly payments by the monthly median earnings of former students six years post-entry. Lower debt-to-earnings ratio indicate paying less to achieve higher benefits.
- *Debt-to-earnings ratio above sample median*: enrolling in institutions generating higher debt-to-earnings (DTI) ratios for students on average may signal relative financial hardship post-completion.
- *Files FAFSA*: students cannot access any federal financial aid, many states' financial aid, or most institutional aid without filling out a FAFSA. This would severely limit options available for students to finance postsecondary education.

¹⁴ Random assignment checks suggest that mandates are plausibly exogeneous to students; refer to [Appendix A](#).

¹⁵ Additionally, adults do not typically vote on these measures; rather, they are passed by state legislature or state departments of education. By law, minors cannot vote on any measures.

- *Uses any Pell grants*: it is not optimal for Pell-eligible students to turn down any Pell grants because these do not have to be repaid, and are usually enough to cover tuition at two-year institutions.
- *Uses any Stafford loans; amount borrowed*: ideally, students borrow the full amount that they can in subsidized Stafford loans before using any unsubsidized Stafford loans. Higher student loan debt is not necessarily bad if it corresponds with working fewer hours while enrolled, or with enrolling in institutions generating higher median earnings.
- *Hours worked per week while enrolled*: it is optimal to work fewer hours while enrolled to minimize likelihood of dropping out of college. According to previous studies, working 20 hours or more is considered to be detrimental to academic progress, and may lead students to drop out.

I estimate the impact of high school financial education mandates on these outcomes using four specifications where Y and the appropriate functional form vary (see Table 2). The general specification is as follows:

$$f(Y_{ics}) = \beta_0 + \theta M_{cs} + X'_i \beta + \gamma_c + \gamma_s + \varepsilon_{ics}$$

where Y_{ics} is the estimated postsecondary outcome of student i in entering cohort year c from high school state s . These estimated postsecondary outcomes are listed in Table 2, and fall under two major concerns: postsecondary institutional choice and postsecondary financing behaviors.

The independent variable of interest is M_{cs} , which denotes if the student was required to take personal finance in high school for graduation. Taken together, I use a student's high school state and high school graduation year to determine if the respondent was exposed to mandated financial education. For example, the first graduating class required to take personal finance courses in Texas is the graduating class of 2007. Therefore, any student graduating high school from Texas in 2007 or later is assumed to be treated; otherwise, those graduating prior to 2007 are assumed not to be treated. For students with missing information, I proxy state of legal residence for high school state and proxy the year at which the student was aged 18 for high school graduation year.¹⁶ Proxies are applied to 11 percent of the sample, and treatment cannot be determined for only 0.8 percent of the sample. Students whose treatment cannot be determined are naturally not included in the models because the treatment variable is registered as "missing."

$X'_i \beta$ is a vector of student i 's demographic characteristics such as annual household income per family member, race/ethnicity, gender, first-generation status, dependency status, and high school GPA. This vector also includes student i 's institutional level for regressions on all

¹⁶ 95.4 percent of included respondents' high school state is also their state of legal residence. In financial aid policy, state of legal residence is used to determine which state aid a student may qualify for. Relocating to a state strictly to attend a particular postsecondary institution does not count as a student's state of legal residence. On the FAFSA application, if a student has lived in their indicated state of legal residence for less than five years, then they are asked in which previous state they lived in. This previous state is then counted as "state of legal residence." As such, it is reasonable to assume that state of legal residence is where a student attended high school among most students ages 23 and younger.

outcomes except for institutional level. γ_c is the entering cohort year fixed effect.¹⁷ Note that younger students are more likely to be exposed to financial education in schools. This fixed effect also captures unobserved cohort factors such as real costs of postsecondary institutions, availability of financing products, or technological changes facilitating information access. γ_s is the high school state fixed effect that captures unobserved state characteristics and policies that may impact college choice or postsecondary financing decisions. I cluster standard errors at the high school state because that is where the policy variation occurs.

Table 1. Regression Models Used to Estimate Dependent Variables

Regression Model	Dependent Variable	
	Initial College Choice	Postsecondary Financing Decisions
Probit	Attended four-year institution freshman year Attended for-profit institution freshman year Enrolled full-time Debt-to-earnings ratio above sample median	Filed FAFSA Used any Pell grants Used any subsidized Stafford loans Used any unsubsidized Stafford loans Used any Stafford loans Employed while enrolled Worked 20 hours or more per week while enrolled
Fractional Probit	CDR of freshman-year institution Debt-to-earnings ratio	
Tobit	Logged median earnings of former students 10-years post-entry	Logged subsidized Stafford loans borrowed Logged unsubsidized Stafford loans borrowed Logged total Stafford loans borrowed
Robust Poisson		Hours worked/week while enrolled

I do not weight or adjust regressions according to complex survey designs because I append multiple waves of survey data together. While the survey design is largely similar across all waves, there are some important nuances between the sampling population. For example, the 1996 wave samples non-Title IV as well as Title IV institutions. The 2012 wave excludes students attending institutions in Puerto Rico. The downsides for not weighting the data is that this imposes a less flexible approach, and assumes that simply controlling for demographics is sufficient to capture the change in demographics over time. However, weighting the data without accurately accounting for the survey design is more dangerous for data inference. As such, this study is not designed to generalize the student population. This study emphasizes exploring causal mechanisms of financial education on financial behavior.

I estimate probit models for dichotomous variables instead of linear probability models because proportions of the sample corresponding to most dichotomous variable fall out of the 30

¹⁷ The year that a student began postsecondary studies (entering cohort year) is used instead of high school graduation year fixed effects because the time dimension in the data is entering cohort year. Entering cohort years are strongly correlated with high school graduation year, where the high school graduation year would never exceed the entering cohort year.

– 70 percent range. Linear probability models are known to give similar results as probit and logit regressions when using large sample sizes and when the proportion of the sample acting on the dependent variable is within 30 – 70 percent (Greene 2003; Hellevik 2009). Estimating LPMs on dichotomous outcomes with extreme proportions may produce more predictions that fall outside of the 0 – 1 range. Moreover, using probit models instead of logit models ensures that analyses are consistent with tobit models.

Sample Restrictions

My analysis examines all students, and then examines economically disadvantaged subgroups such as first-generation college students and Pell-eligible college students. I exclude the following students from all analyses:

- *Students who are on active duty, reservists, or veterans:* Military personnel have a different set of financial options, and are more likely to have financial education or financial coaching available to them through the military than civilian students.
- *Students over age 23:* Adult students have a different set of experiences and challenges relative to younger students, including having more experiences with debt and credit. These students are also further away from the intervention that would have occurred in high school; hence, including these students would confound identification.
- *Students who attended foreign high schools or domestic non-public high schools:* The policy I am analyzing only applies to students attending public high schools in the United States. Therefore, I am taking a conservative approach because we do not know if students attending non-public high schools were certainly treated or not.¹⁸
- *Students who do not have a regular high school diploma:* Discerning if students with GEDs or alternative credentials received treatment is tricky because they do not have to meet all course requirements to obtain a GED, certificate of completion, or other. We also do not know when these students stopped attending high school.
- *Students who attended more than one institution:* There was not sufficient data in the 1996 and 2004 waves to determine which school was a student's first institution. Therefore, I exclude students attending more than one institution during their freshman year from analyses.
- *Students who did not attend a Title IV institution (applicable to 1996 cohort only):* BPS:96/01 sampled students regardless of if they were attending a Title IV or non-Title IV institution, whereas later waves only sampled students who attended Title IV institutions. Therefore, to keep the sample population consistent, I exclude students in the 1996 cohort who attended any non-Title IV institution from analyses.

¹⁸ Some economists recommend conducting a DDD, where the third dimension would be private high school versus public high school. This would not be a good falsification test because while all public schools must implement the mandate, there is nothing in the legislation that says that private schools cannot do the same. Additionally, parents' decision to enroll their child(ren) in private school is based on selection of unobservables (e.g. greater parental involvement in education, higher preference for children to be in controlled or homogenous environment, student his/herself is generally more motivated).

- *Students who are not comparable to NPSAS:87*: This restricts the sample to enrollees who are not in Puerto Rico. I impose this restriction to ensure that the sample population remained similar, especially since the BPS:12/14 did not survey students in Puerto Rico.

Furthermore, I exclude students who were missing high school state, state of legal residence, high school graduation year, and age information from the study because I cannot determine treatment without this information. Upon excluding those who fit any of the above criteria, the maximum sample sizes were 5,100 from BPS:96/01; 10,250 from BPS:04/09; and 15,250 from BPS:12/14. This means that the overall maximum sample size was 30,600 first-time beginning college freshmen.

Data

I investigate whether state-mandated financial education affects postsecondary decisions among economically disadvantaged college students using data on state-level financial education mandates (Urban and Schmeiser 2015), data on institutional-level outcomes of former students (College Scorecard), and student-level data on institution choice, financing decisions, and demographic information (restricted-use versions of the 1996, 2004, and 2012 Beginning Postsecondary Students Longitudinal Study) as further explained below:

Urban and Schmeiser (2015) is a database that contains information on when states implemented financial education mandates between 1970 and 2014.¹⁹ This dataset clearly distinguishes between state mandates that required schools to offer financial education as an elective and state mandates that required all students to take financial education for graduation. This dataset also distinguishes between course subjects (economics or personal finance), course offering (integrated into a math/social science course or standalone course), and if states require standardized testing in financial education. I specifically analyze the policy variation that required all students to take personal finance as a core prerequisite for graduation.

The U.S. Department of Education's College Scorecard contains information about a Title IV institution's net price, financial aid, debt, graduation rates, retention rates, former students' earnings post-entry, student body demographics, SAT/ACT scores, and academic programs. The College Scorecard is an administrative database that pulls its data from the Integrated Postsecondary Education Data System, the National Student Loan Data System, the Treasury Internal Revenue Service (IRS), the Operation Performance Division's (OPD) Default Management database, and FAFSA applications between FY 1997 and FY 2016. The College Scorecard was officially released in September 2015. The intent of the College Scorecard is to provide students, families, and any high school advisors with comparative, comprehensive information about institutions so that they can assess prospective institutions' costs and benefits.

¹⁹ Their dataset does not include District of Columbia – but CEE survey results and Bernheim, Garrett and Maki (2001) reveal that D.C. has never implemented any personal finance mandates.

Certain variables, including average net price of the institution, are broken down by socioeconomic status and other demographics or circumstances pertinent to higher education (e.g. first-generation college students, Pell recipients). I particularly use FY 2012 data across all students because this corresponds to the latest cohort in my sample, and there is little within variation.

An important piece of information that the College Scorecard extracts from the Default Management database is the postsecondary institutions' three-year cohort default rates (CDR). A three-year CDR is the percent of students who entered loan repayment in a given year and defaulted within three years of starting repayment. OPD calculates the three-year CDRs, which determines an institution's eligibility to participate in the federal financial aid program. Institutions with CDRs of 30 percent or higher in the last three consecutive fiscal years, or with CDRs higher than 40 percent in the most recent fiscal year may lose their eligibility to participate in the federal financial aid program for the next five fiscal years (OPD 2016).

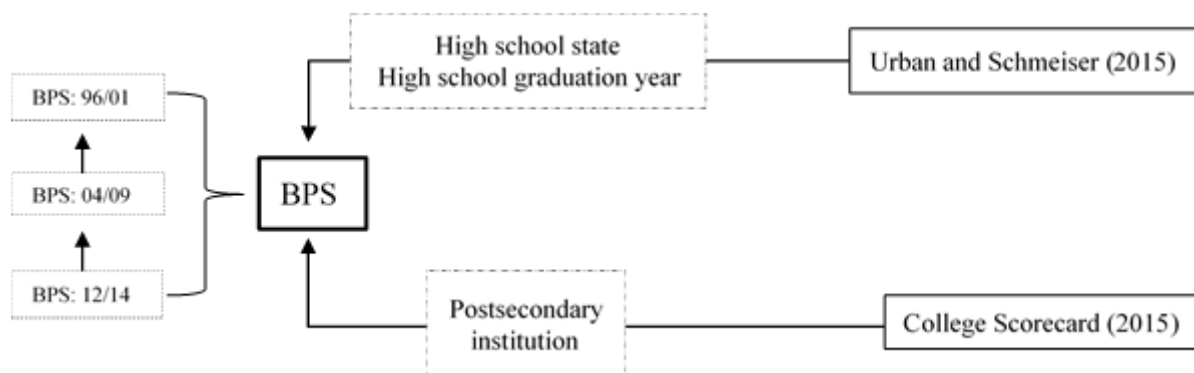
The Beginning Postsecondary Students Longitudinal Study (BPS) is a combination of survey and administrative data that examines how a nationally representative cohort of college freshmen attending Title IV institutions nationwide financed college for five years after beginning undergraduate studies. The BPS contains student-level data from postsecondary institutions, administrative databases, parent interviews, and student interviews. The BPS is the only large-scale dataset that has extensive, longitudinal information about enrolled college students' finances, financing behavior, and educational outcomes. The advantage of the BPS is that while administrative data only captures federal financial aid use and education information, the survey data also captures other forms of debt (e.g. private student loans, credit cards, loans from family/friends). The U.S. Department of Education conducts the BPS study every eight years as of 1996. I concentrate on freshman year outcomes because these decisions were made close to mandate exposure.

To employ difference-in-differences, I append the restricted-use versions of 1996, 2004, and 2012 waves of the BPS together on a common set of variables because each wave only represents a single cohort. I modify some variables to ensure comparability across waves, and I convert all dollar amounts to constant 2016 dollars using the CPI-U. I link Urban and Schmeiser (2015) to the BPS using high school state and high school graduation year. For students with missing information, I proxy state of legal residence for high school state and proxy the year at which the student was aged 18 for high school graduation year.²⁰ I then merge the FY 2012 data

²⁰ 95.4 percent of included respondents' high school state is also their state of legal residence. In financial aid policy, state of legal residence is used to determine which state aid a student may qualify for. Relocating to a state strictly to attend a particular postsecondary institution does not count as a student's state of legal residence. On the FAFSA application, if a student has lived in their indicated state of legal residence for less than five years, then they are asked in which previous state they lived in. This previous state is then counted as "state of legal residence." As such, it is reasonable to assume that state of legal residence is where a student attended high school, especially among students ages 23 and younger.

from the College Scorecard dataset to the BPS using a student's freshman-year postsecondary institution. I apply the FY 2012 data across all cohorts because there is little within variation relative to the between variation among the variables. Figure 2 visually explains how I handle the data.

Figure 2. Sketch of Data Appending and Merging



Findings

Descriptive Statistics

Approximately one-third of the students in the sample are underrepresented minorities, meaning that they racially identify as at least one of the following: Black/African-American, Latino/Hispanic, Native American, or Pacific Islander. Over half of the sample are female students. In terms of economically disadvantaged categories, roughly three in ten students in the sample are first-generation college students, meaning that neither parent has obtained any postsecondary degree or credential. There is great variation in household income. While the average annual household income is \$72,523 (in 2016 dollars), 53 percent of the students in the sample are Pell-eligible (come from households with income of \$50,000 or less). On average, the students' high school GPA is between a B- and an A-, where the mode range is a B to an A-. Yet, 28 percent of the sample has a high school GPA between an A- to an A. Overall, 28 percent of students were required to take personal finance to graduate high school due to a state mandate.

Eighty percent of students enrolled full-time during their freshman year, and 62 percent of them attended a four-year institution. Approximately 18 percent of them attended for-profit institutions. The average three-year CDR of the institutions sampled students are attending is 11 percent. This is well below the penalty threshold of 30 percent, although the maximum CDR in the dataset is at 44 percent. The average debt-to-income ratio generated at attended institutions is 6.8 percent, which is considerably manageable. However, it expands up to 29 percent for some students. The median annual earnings among former students from attended institutions range

from \$11,500 to \$250,000. Ideally, median annual earnings should be over \$25,000 if students expect to earn more than the average high school graduate (College Scorecard 2015).

In terms of college financing, over 80 percent of students filed a FAFSA, which is abnormally high.²¹ Subsequently, 44 percent of students accessed Pell grants, and sampled students borrowed an average of \$2,692 (in 2016 dollars) in federal student loans during freshman year. Approximately 54 percent of the average amount borrowed were in subsidized loans. Nearly half of students worked while enrolled, and 31 percent of students worked at least 20 hours per week while enrolled.

Table 2. Descriptive Statistics of Overall Sample Population

VARIABLES	N	Mean	SD	Min	Max
Personal Finance Required for Graduation Everywhere in State ^a	30,530	0.281	0.449	0	1
<i>Demographics^b</i>					
First-Generation Student	30,600	0.314	0.464	0	1
Independent	30,550	0.086	0.280	0	1
Income as Percent of Poverty Level	30,600	303	242	0	1,000
Annual Household Income (in 2016 Dollars)	30,600	72,523	70,044	0	1,530,000
Household Size	30,550	3.947	1.421	1	13
Pell-Eligible	30,530	0.526	0.499	0	1
Underrepresented Minority	30,600	0.339	0.474	0	1
Female	30,600	0.579	0.494	0	1
High School GPA	29,650	5.630	1.245	1	7
<i>Institutional Characteristics and Students' Enrollment Intensity^b</i>					
Attends Four-Year Institution	30,600	0.622	0.485	0	1
Attends For-Profit Institution	30,600	0.182	0.386	0	1
Enrolled Full-Time	30,600	0.801	0.399	0	1
<i>Institutional-Level Outcomes of Former Students^c</i>					
CDR (3-yr)	28,710	0.105	0.073	0	0.44
Former Students' Debt-to-Earnings Ratio	27,370	0.068	0.030	0	0.29
DTI Ratio Above Sample Median (DTI > 0.06)	27,370	0.492	0.500	0	1
Median Earnings of Former Students 10 Years Post-Entry (in 2014 dollars)	28,890	38,763	11,218	11,500	250,000
<i>Postsecondary Financing^b</i>					
Completed FAFSA	30,600	0.841	0.366	0	1
Used Any Pell Grants	30,600	0.436	0.496	0	1
Total Stafford Borrowed (in 2016 Dollars)	30,600	2,692	3,178	0	13,341
Total Subsidized Stafford Borrowed (in 2016 Dollars)	30,600	1,445	1,744	0	8,413
Total Unsubsidized Stafford Borrowed (in 2016 Dollars)	30,600	1,247	2,049	0	13,067
Employed While Enrolled	30,470	0.483	0.500	0	1
Hours Worked Per Week While Enrolled	30,470	11.41	14.55	0	120
Works 20+ Hours Per Week While Enrolled	30,470	0.313	0.464	0	1

SOURCES: ^a Urban and Schmeiser (2015), ^b BPS (1996, 2004, and 2012), and ^c U.S. Department of Education's College Scorecard (2015)

NOTES: High school GPA is placed in 7 categories, where each level is in 0.5 increments. Its mode is "B to A –", which takes a value of "6." Statistics are unweighted.

²¹ I discuss plausible reasons for this in [Study Limitations](#).

Main Findings

I initially examine the impact of mandates on institutional level selection.²² Since mandates do not affect selection into a two-year or four-year institution, I control for institutional level in remaining regressions to account for their differing environments. I present results in four major quadrants: general institutional characteristics, institutions' outcomes of former students, federal financial aid use, and employment while enrolled. I present overall results, and then heterogeneous results.

Table 3 highlights results of institution level, institution control, and students' attendance intensity. When holding student demographics, high school GPA, and institutional level (where appropriate) constant, students required to take personal finance in high school are four percentage points less likely to attend a for-profit institution and four percentage points more likely to enroll full-time than students who are not required to take the course. These results suggest that mandates influence students to select less costly institutions, and to make decisions that maximize their ability to get financial aid.

Even when accounting for financial education and high school GPA, differences by socioeconomic indicators remain. First-generation college students are nine percentage points less likely to attend a four-year institution, and seven percentage points more likely to enroll in a for-profit institution than later-generation students. Lower-income students are significantly less likely to attend a four-year institution, significantly more likely to enroll full-time, and are less likely to attend a for-profit institution than higher-income students. Similarly, independent students are nine percentage points less likely to attend a four-year institution, four percentage points less likely to enroll full-time, and fourteen percentage points more likely to enroll in a for-profit institution than dependent students. Since I restrict the sample to students under age 24, these independent students are likely parents, married, or are self-supporting (e.g. emancipated minors, orphans, former wards of the court, or former foster youth). These results complement existing literature, and suggests that they may be making decisions that cater to their immediate circumstances.

Table 4 highlights results of institutional-level outcomes of its former students. Median earnings signal benefits from a postsecondary institution, where higher earnings are always better. On the other hand, cohort default rates (CDRs) and debt-to-income ratios both signal cost-benefit ratios, where lower ratios are always better.

²² I combined two-year institutions and less-than-two-year institutions into one category. I refer to this category as "two-year institutions" through-out the paper.

Table 3. Average Marginal Effects of Mandates on Selected Institution's Characteristics

VARIABLES	Attends Four-Year Institution	Attends For-Profit Institution	Enrolls Full-Time
Mandated Personal Finance Courses	-0.032 (0.039)	-0.042** (0.020)	0.038*** (0.013)
First-Generation College Student	-0.092*** (0.009)	0.072*** (0.004)	-0.010 (0.007)
Per Member Household Income (in 1000s)	0.003*** (0.000)	-0.003*** (0.000)	-0.001*** (0.000)
Independent	-0.093*** (0.015)	0.139*** (0.009)	-0.035*** (0.011)
Underrepresented Minority	0.008 (0.010)	0.071*** (0.008)	-0.016** (0.007)
Female	-0.013** (0.006)	0.014*** (0.005)	0.016*** (0.004)
High School GPA: D to C-	0.022 (0.068)	0.117** (0.050)	0.056 (0.074)
High School GPA: C- to C	-0.027 (0.061)	0.127** (0.052)	0.076 (0.065)
High School GPA: C to B-	0.028 (0.063)	0.098* (0.050)	0.076 (0.065)
High School GPA: B- to B	0.108 (0.067)	0.043 (0.050)	0.106 (0.068)
High School GPA: B to A-	0.220*** (0.068)	0.013 (0.051)	0.124* (0.067)
High School GPA: A- to A	0.377*** (0.068)	-0.055 (0.050)	0.170** (0.067)
Attends Four-Year Institution	---	0.058*** (0.018)	0.150*** (0.010)
State of High School Attendance	YES	YES	YES
Entering Cohort Year	YES	YES	YES
N	29,500	29,550	29,550

NOTES: Standard errors in parentheses. Each column is a separate regression. Results are unweighted, and are estimated from probit models. Reference category for high school GPA is "below D" (below 1.0). *** p<0.01, ** p<0.05, * p<0.1

When holding student demographics, high school GPA, and institutional level constant, students exposed to the mandate enroll in institutions whose CDR is nearly one percentage points less (marginally significant) than students who were not exposed. This suggests that students subject to the mandate are enrolling in less risky institutions, complementing findings of being less likely to enroll in for-profit institutions. Mandates do not impact enrollment into institutions generating higher median earnings for their students, or enrollment into institutions resulting in lower debt-to-income ratios. This may be because students in my sample were not exposed to any public pushes to provide prospective students and their families this information.

Even when accounting for financial education and high school GPA, differences by socioeconomic indicators remain. Relative to non-economically disadvantaged students, first-generation students, lower-income students, and independent students all tend to enroll in institutions with higher CDRs, institutions generating lower median earnings for their former

students, and institutions whose students have higher debt-to-income ratios. The debt-to-income ratio, however, may be a function of disadvantaged students borrowing significantly more student loans on average. Later, I explore if mandates may particularly impact disadvantaged students' enrollments into less risky institutions.

Table 4. Average Marginal Effects of Mandates on Selected Institution's Outcomes for Former Students

VARIABLES	CDR (in Thousandths)	Logged Median Earnings 10 Years Post-Entry	Debt-to-Income Ratio	Above DTI Sample Median
Mandated Personal Finance Courses	-0.009* (0.005)	0.013 (0.013)	-0.001 (0.001)	-0.016 (0.022)
First-Generation	0.012*** (0.001)	-0.034*** (0.003)	0.003*** (0.000)	0.030*** (0.010)
Per Member Household Income (in 1000s)	-0.001*** (0.000)	0.001*** (0.000)	-0.000*** (0.000)	-0.001*** (0.000)
Independent	0.022*** (0.002)	-0.036*** (0.005)	0.002* (0.001)	0.003 (0.016)
Underrepresented Minority	0.016*** (0.003)	-0.032*** (0.005)	0.006*** (0.001)	0.029** (0.012)
Female	-0.002 (0.002)	-0.048*** (0.004)	0.002*** (0.000)	0.050*** (0.009)
High School GPA: D to C-	0.034*** (0.011)	-0.050** (0.023)	0.009* (0.005)	0.080 (0.088)
High School GPA: C- to C	0.030** (0.012)	-0.044** (0.021)	0.009* (0.005)	0.083 (0.082)
High School GPA: C to B-	0.023** (0.011)	-0.038* (0.021)	0.009* (0.005)	0.098 (0.084)
High School GPA: B- to B	0.013 (0.012)	-0.020 (0.022)	0.006 (0.005)	0.078 (0.084)
High School GPA: B to A-	0.002 (0.012)	0.008 (0.022)	0.005 (0.005)	0.066 (0.086)
High School GPA: A- to A	-0.022* (0.012)	0.090*** (0.024)	-0.002 (0.005)	-0.042 (0.087)
Attends Four-Year Institution	-0.046*** (0.007)	0.296*** (0.016)	0.034*** (0.002)	0.572*** (0.024)
Entering Cohort Year	YES	YES	YES	YES
State of High School Attendance	YES	YES	YES	YES
N	27,910	27,980	26,520	26,520

NOTES: Standard errors in parentheses. Each column is a separate regression. Results are unweighted. Logged continuous variables are estimated using tobit models, ratios are estimated using fractional probit models, and indicator variables are estimated using probit models. Reference category for high school GPA is "below D" (below 1.0). *** p<0.01, ** p<0.05, * p<0.1

Table 5 reveals results concerning federal financial aid application and use. These outcomes include filing a FAFSA, using any Pell grants, and borrowing federal student loans. All students should fill out a FAFSA to ensure that they maximize financing options available to them.²³

²³ When estimating effects on likelihood to fill out a FAFSA, I excluded for-profit students because they are significantly more likely to fill out the FAFSA than non-profit students (90 percent of for-profit civilian students

Many state governments and most postsecondary institutions also rely on the FAFSA to determine students' aid allocation. Pell-eligible students should use all Pell grants awarded to them.

When borrowing federal student loans, borrowers should at least maximize the subsidized amount available to them before borrowing any unsubsidized loans. When holding student demographics, high school GPA, and institutional level constant, students who were required to take personal finance in high school are four percentage points more likely to fill out a FAFSA. Borrowers subjected to the mandate marginally borrow five percent fewer amounts of federal student loans than borrowers not subjected to the mandate. This may be because mandated students are marginally less likely to use unsubsidized student loans, and borrow fewer amounts of unsubsidized student loans when choosing to do so. Mandates did not impact Pell grant or subsidized student loan use among Pell-eligible students. They are probably accepting Pell grants and subsidized loans regardless of any financial education.

Even when accounting for financial education and high school GPA, differences by socioeconomic indicators remain. First-generation and lower-income students are significantly more likely to fill out a FAFSA, use Pell grants, and borrow federal student loans than their non-disadvantaged counterparts. Surprisingly, however, independent students are significantly less likely to fill out a FAFSA, less likely to use Pell grants, but more likely to use federal student loans. While they are significantly more likely to use both types of loans, they borrow fewer amounts of subsidized loans and greater amounts of unsubsidized loans. This may be because independent students are substantially less likely to enroll full-time, and were most likely to enroll in a for-profit institution, as revealed in Table 3.

under age 24 versus 70 percent of non-profit peers during the 2011 – 2012 academic year according to the NPSAS:12 in NCES's DataLab TrendStats).

Table 5. Average Marginal Effects of Mandates on Federal Financial Aid Use

VARIABLES	Filed FAFSA [‡]	Used Any Pell Grants [†]	Used Any Subsidized Loans [†]	Log Amount Subsidized Borrowed (if Used Any) [†]	Used Any Unsubsidized Loans	Log Amount Unsubsidized Borrowed (if Used Any)	Used Any Stafford Loans	Log Amount Borrowed (if Used Any)
Mandated Personal Finance Courses	0.036** (0.015)	0.021 (0.020)	0.011 (0.023)	0.011 (0.023)	-0.029* (0.016)	-0.060* (0.033)	0.017 (0.016)	-0.048* (0.025)
First-Generation	0.067*** (0.006)	0.047*** (0.006)	0.047*** (0.008)	-0.024*** (0.009)	0.035*** (0.005)	-0.005 (0.015)	0.053*** (0.007)	0.001 (0.010)
Per Member Household Income (in 1000s)	-0.003*** (0.000)	-0.015*** (0.001)	0.000 (0.001)	0.001 (0.001)	-0.000 (0.000)	0.007*** (0.001)	-0.004*** (0.000)	-0.000 (0.000)
Independent	-0.029** (0.014)	-0.053*** (0.012)	0.071*** (0.012)	-0.083*** (0.013)	0.119*** (0.015)	0.308*** (0.022)	0.047*** (0.015)	0.170*** (0.018)
Underrepresented Minority	0.082*** (0.007)	0.088*** (0.007)	0.087*** (0.017)	-0.033*** (0.012)	0.069*** (0.014)	0.103*** (0.016)	0.070*** (0.017)	0.055*** (0.011)
Female	0.033*** (0.005)	0.043*** (0.005)	0.042*** (0.007)	0.014 (0.010)	0.034*** (0.005)	-0.015 (0.013)	0.037*** (0.006)	0.007 (0.006)
High School GPA: D to C-	0.135* (0.078)	0.044 (0.085)	0.166** (0.082)	0.205 (0.185)	0.111 (0.072)	0.040 (0.225)	0.165*** (0.063)	0.064 (0.137)
High School GPA: C- to C	0.083 (0.070)	0.013 (0.080)	0.135** (0.069)	0.257 (0.188)	0.124* (0.070)	0.186 (0.223)	0.161*** (0.057)	0.167 (0.134)
High School GPA: C to B-	0.103 (0.069)	0.019 (0.080)	0.117* (0.066)	0.241 (0.184)	0.094 (0.069)	0.201 (0.230)	0.130** (0.055)	0.163 (0.138)
High School GPA: B- to B	0.100 (0.072)	0.030 (0.085)	0.108 (0.068)	0.269 (0.185)	0.075 (0.069)	0.200 (0.228)	0.115** (0.056)	0.170 (0.135)
High School GPA: B to A-	0.107 (0.072)	0.018 (0.083)	0.102 (0.065)	0.271 (0.183)	0.061 (0.073)	0.189 (0.236)	0.103* (0.059)	0.157 (0.138)
High School GPA: A- to A	0.122* (0.072)	-0.010 (0.082)	0.029 (0.062)	0.238 (0.183)	0.003 (0.071)	0.165 (0.231)	0.044 (0.055)	0.106 (0.135)
Attends Four-Year Institution	0.135*** (0.010)	0.093*** (0.016)	0.367*** (0.021)	0.086*** (0.015)	0.231*** (0.015)	0.084*** (0.021)	0.340*** (0.017)	0.116*** (0.022)
Entering Cohort Year	YES	YES	YES	YES	YES	YES	YES	YES
State of High School Attendance	YES	YES	YES	YES	YES	YES	YES	YES
N	24,190	15,350	15,350	8,310	29,550	10,480	29,550	15,190

NOTES: Standard errors in parentheses. Each column is a separate regression. Results are unweighted. Indicator variables are estimated from probit models, and logged continuous variables are estimated from tobit models. [‡] denotes conditional on not attending a for-profit institution. [†] denotes conditional on Pell eligibility. Reference category for high school GPA is "below D" (below 1.0). *** p<0.01, ** p<0.05, * p<0.1

Table 6 displays results of working while enrolled in postsecondary studies. Employment during enrollment is one of the largest policy concerns in higher education, especially among economically disadvantaged students. Working substantial hours has been linked to poor academic achievement and college noncompletion.

When holding student demographics, high school GPA, and institutional level constant, mandates have no impact on the probability of working while enrolled. However, students under the mandate work approximately one fewer hours per week while enrolled than those not under the mandate. Among those who were employed, students who took personal finance in high school are marginally five percentage points less likely to work 20 hours or more per week while enrolled than students who did not.

Table 6. Average Marginal Effects of Mandates on Employment While Enrolled

VARIABLES	Employed While Enrolled	Hours Worked Per Week	Works 20+ Hours Per Week (if Employed)
Mandated Personal Finance Courses	-0.019 (0.015)	-0.898** (0.451)	-0.046* (0.024)
First-Generation	0.017* (0.009)	1.073*** (0.226)	0.060*** (0.008)
Per Member Household Income (in 1000s)	-0.001*** (0.000)	-0.032*** (0.006)	-0.001*** (0.000)
Independent	-0.033** (0.013)	1.016*** (0.362)	0.111*** (0.014)
Underrepresented Minority	-0.055*** (0.008)	-0.972*** (0.201)	0.027*** (0.008)
Female	0.024*** (0.008)	-0.277 (0.220)	-0.033*** (0.008)
High School GPA: D to C-	-0.012 (0.062)	0.384 (1.325)	0.062 (0.073)
High School GPA: C- to C	0.008 (0.072)	1.387 (1.479)	0.007 (0.069)
High School GPA: C to B-	0.031 (0.073)	1.547 (1.563)	0.012 (0.064)
High School GPA: B- to B	0.051 (0.072)	1.685 (1.593)	-0.020 (0.070)
High School GPA: B to A-	0.048 (0.070)	1.253 (1.528)	-0.055 (0.072)
High School GPA: A- to A	-0.000 (0.071)	-0.967 (1.610)	-0.148** (0.070)
Attends Four-Year Institution	-0.155*** (0.011)	-6.201*** (0.371)	-0.201*** (0.014)
Entering Cohort Year	YES	YES	YES
State of High School Attendance	YES	YES	YES
N	29,450	29,450	14,040

NOTES: Standard errors in parentheses. Each column is a separate regression. Results are unweighted. Indicator variables are estimated from probit models, and continuous variables are estimated from Poisson models. Reference category for high school GPA is "below D" (below 1.0). *** p<0.01, ** p<0.05, * p<0.1

Even when accounting for financial education and high school GPA, differences by socioeconomic indicators remain. First-generation and lower-income students are more likely to work than later-generation and higher-income students (marginally significant). First-generation and independent students work approximately one more hour per week while enrolled than their more advantaged peers. First-generation, lower-income, and independent students are also substantially more likely (six percentage points for first-generation students; eleven percentage points more likely for independent students) to work at least 20 hours per week while enrolled. First-generation students are especially known to work while enrolled due to parental pressures (e.g. parents expect that their child will still work to help support the family).

Heterogeneous Effects

Interactions with Economically Disadvantaged Subgroups

Financial education mandates may disproportionately impact economically disadvantaged students because they rely on more debt and working more hours per week to pay for their costlier undergraduate education which generates lower payoff. Consistent with previous literature, Table 8 reveals that disadvantaged students borrow significantly more Stafford loans, accrue significantly higher amounts in credit card debt, and work significantly more hours per week while enrolled. These significantly higher amounts of debt are considerable when realizing that significantly higher proportions of disadvantaged students attend riskier institutions (institutions with higher three-year cohort default rates and with lower median earnings for its former students), and attend either for-profit institutions or two-year institutions. Students beginning their postsecondary studies at a two-year institution with the intent to complete a Bachelor's degree are less likely to do so than students beginning their postsecondary studies at a four-year institution, primarily through being less likely to successfully transfer (e.g. Doyle 2009; Melguizo, Kienzl and Alfonso 2011).²⁴

Non-disadvantaged students borrow significantly more private student loans. This may be because non-disadvantaged students qualify for less financial aid, and have someone able and willing to co-sign for these loans. Overall, these statistics reflect the fewer resources that disadvantaged students may have to help pay for their education, or to be guided in their postsecondary decisions. These differences warrant examining heterogeneous effects by economically disadvantaged subgroups to assess if financial education mandates may improve their investment decision-making.

²⁴ Melguizo, Kienzl and Alfonso (2011) do not find significant differences in earning Bachelor's degrees within eight years between college juniors whom started their education at a four-year institution and transfer students. However, they point out that there is a larger issue of transfer rates among community college students.

Table 7. Descriptive Statistics by Economically Disadvantaged Subgroup

VARIABLES	Not First-Generation (N = 21,010)	First-Generation (N = 9,600)	Not Pell-Eligible (N = 14,480)	Pell-Eligible (N = 16,050)	Dependent (N = 27,920)	Independent (N = 2,630)
Underrepresented Minority	0.276	0.479	0.190	0.474	0.321	0.527
Female	0.564	0.612	0.538	0.616	0.561	0.765
Attends Four-Year Institution	0.680	0.497	0.717	0.537	0.645	0.379
Attends For-Profit Institution	0.134	0.288	0.097	0.259	0.155	0.467
Enrolled Full-Time	0.816	0.766	0.808	0.795	0.809	0.713
CDR (3-yr)	0.094	0.130	0.084	0.125	0.101	0.157
Median Earnings of Former Students 10 Years Post-Entry (in 2014 dollars)	40,350	35,197	41,708	36,040	39,354	32,190
Completed FAFSA	0.812	0.904	0.771	0.908	0.835	0.900
Used Any Pell Grants	0.345	0.635	0.079	0.760	0.401	0.800
Total Stafford Borrowed (in 2016 Dollars)	2,521	3,066	2,322	3,037	2,579	3,899
Employed While Enrolled	0.473	0.506	0.478	0.489	0.484	0.466
Hours Worked Per Week While Enrolled	10.63	13.12	10.44	12.32	11.14	14.20
Works 20+ Hours Per Week While Enrolled	0.287	0.370	0.280	0.343	0.305	0.395
Has Credit Card	0.294	0.296	0.307	0.282	0.303	0.218
Credit Card Balance Exceeds \$1,000	0.040	0.066	0.035	0.060	0.045	0.072
Credit Card Balance (in 2016 Dollars)	181	302	157	272	202	380
Total Private Student Loans Borrowed (in 2016 Dollars)	650	547	815	439	638	421

NOTES: Statistics are unweighted, and are statistically significant at the five percent level or less for all variables except for "has credit card" by first-generation status.

Higher education policy defines three types of economically disadvantaged subgroups: first-generation college students, Pell-eligible students (low-income students), and independent students. As seen in Table 8, approximately sixty percent of all students in the sample fall into at least one of these groups. It is worth reviewing results for each economically disadvantaged subgroup because their different definitions will suggest which policies should be targeted, or will help frame how public policies should focus efforts. However, Table 8 reveals that these categories are not mutually exclusive.

Table 8. Percentage of College Students Classified as Economically Disadvantaged

Cohort	Total Students	Number of Economically Disadvantaged Categories			
		None	1	2	3
1996	5,100	36.3	36.6	24.6	2.5
2004	10,250	45.0	32.1	19.7	3.1
2012	15,250	36.7	32.6	25.7	5.7
Total	30,600	39.4	33.1	23.2	4.3

NOTES: Statistics are unweighted. Percentages may not add up to 100 due to rounding.

The set of regressions below reveal heterogeneous effects of financial education mandates on the following economically disadvantaged subgroups: first-generation college students and Pell-eligible students (defined as having an annual household income of \$50,000 or less).²⁵ Each economically disadvantaged subgroup tends to have fewer resources to help pay for college, or to consult about postsecondary financing. Accordingly, high school personal finance courses could especially provide information about financing postsecondary education for these students.

First-Generation College Students

First-generation college students are the first in their families to ever attend college (meaning, the highest education level either parent has is high school graduate or less). In the United States, parents are the most important financial resource a student has to finance their postsecondary education. Yet for many first-generation college students, their parents are not providing the same financial resources that other students may receive, both in terms of actual monetary contributions and financial guidance. First-generation college students may receive limited financial support for the following reasons:

- They are making decisions that their parents have never made. Therefore, their parents cannot guide them through the financial aid process or in how to best pay for college.

²⁵ OPE (2004; 2013) notes that up to 95 percent of Pell Grant recipients come from families whose annual household income is \$50,000 or less. Pell grant eligibility is based on household income, family size, attendance status, and institutions' COA. I do not examine heterogeneous effects by dependency status because only 8.6 percent of students in the sample are independent. Furthermore, 98.7 percent of independent students in the sample are eligible to receive Pell grants; hence, heterogeneous effects by Pell eligibility would also explain heterogeneous effects by dependency status.

- They are likely to have parents with low financial literacy, which could translate to making sub-optimal financial decisions. By definition, their parents have lower educational attainment. Financial literacy is positively correlated with educational attainment (e.g. Lusardi, Mitchell and Curto 2010; Hastings, Madrian and Skimmyhorn 2013).
- 58 percent of non-adult, civilian first-generation college students come from households whose income is at 150 percent of the poverty level or less, compared to 25 percent of the overall college population.²⁶ Their parents may not be a financial resource available to pay for college.
- 17 percent of non-adult, civilian first-generation college students are financially independent, compared to six percent of the overall college population.²⁷ Financial aid policy assumes and does not expect independent students to have parental resources to pay for college.

Many studies concerning first-generation college students are about persistence. However, financial standing is one of the main factors that determine first-generation college students' persistence (Lyons 2004; Engle, Bermeo and O'Brien 2006; Eitel and Martin 2009), and impacts academic performance (Schmeiser et al. 2015; Schmeiser et al. 2016).

Pell-Eligible Students

Pell-eligible students come from families whose annual household income is \$50,000 or less.²⁸ While up to 95 percent of students receiving Pell grants come from families whose annual household income is \$50,000 or less, a majority of students come from families whose annual household income is \$20,000 or less (OPE 1996; 2004; 2012). I set Pell eligibility according to the 95th percentile to ensure that I capture the greatest extent to which a student may be eligible for Pell grants.

First, I examine heterogeneous effects of financial education mandates on attending four-year institutions, attending for-profit institutions, and enrolling full-time. Mandates have no impact on institutional level selection for either subgroup.

Table 9 shows heterogeneous effects of the mandate on institutional enrollment by first-generation status and by Pell eligibility. These policies marginally increase the likelihood of first-generation students enrolling full-time by three percentage points, but significantly increased the likelihood of later-generation students enrolling full-time by four percentage points. A similar trend persists when examining differential effects on full-time enrollment by

²⁶ Author's calculations of BPS:12/14 using NCES PowerStats in DataLab. Excludes military personnel, veterans, and students over age 23 as of December 31, 2011.

²⁷ Ibid.

²⁸ Other financial aid programs use different definitions. For example, TRIO participants come from families whose annual household income is no more than 150 percent of the federal poverty line (FPL). The definition of Pell eligibility is broadly defined because it is complicatedly based on annual household income, family size, attendance intensity, and cost of attendance.

Pell eligibility. Mandates equally decrease the probability of attending for-profit institutions for both first-generation and later-generation students. However, stronger effects persist between Pell-eligible and Pell-ineligible students.

Table 9. Average Marginal Effects of Mandates on Selected Institution's Characteristics by Economically Disadvantaged Subgroup

VARIABLES	Attends Four-Year Institution	Attends For-Profit Institution	Enrolls Full-Time
Overall	-0.032 (0.039)	-0.042** (0.020)	0.038*** (0.013)
A. Interactions by First-Generation Status			
Not First-Generation	-0.045 (0.039)	-0.040** (0.019)	0.042*** (0.013)
First-Generation	-0.007 (0.045)	-0.048** (0.024)	0.030* (0.018)
N	29,500	29,550	29,550
B. Interactions by Pell Eligibility			
Not Pell-Eligible	-0.047 (0.040)	-0.052** (0.020)	0.043*** (0.016)
Pell-Eligible	-0.020 (0.043)	-0.039* (0.021)	0.033** (0.015)
N	29,470	29,530	29,530

NOTES: Standard errors in parentheses. Reference category: not mandated. Each column under each panel is a separate regression. Results are unweighted, and are estimated from probit models where first-generation status or income is interacted with the mandate indicator. Overall effects reported from Table 3. *** p<0.01, ** p<0.05, * p<0.1

Then I examine heterogeneous effects on enrolling in beneficial institutions as indicated in the selected institution's CDR, former students' median earnings, and former students' debt-to-income ratio. As shown in Table 11, first-generation students subject to the mandate are significantly more likely to enroll in institutions with debt-to-income ratios lower than the sample median than their first-generation peers not required to take the mandate. When considering the null findings for higher median earnings, this result may be because students exposed to the mandate are borrowing less. However, later-generation students and higher-income students who were required to take personal finance tend to enroll in institutions with lower CDRs. This implies that mandated financial education not only helps more advantaged students enroll in less risky institutions, but helps them enroll in institutions that are more likely to have financial aid options available to its students. Surprisingly, mandates have no impact on economically disadvantaged students' enrollment into institutions with lower CDRs when these students enroll in riskier institutions than their non-disadvantaged peers. Additionally, I find no other heterogeneous effects among remaining outcomes.

Table 10. Average Marginal Effects of Mandates on Selected Institution's Outcomes for Former Students by Economically Disadvantaged Subgroup

VARIABLES	CDR (in Thousandths)	Logged Median Earnings 10 Years Post-Entry	Debt-to- Income Ratio	Above DTI Sample Median
Overall	-0.009* (0.005)	0.013 (0.013)	-0.001 (0.001)	-0.016 (0.022)
A. Interactions by First-Generation Status				
Not First-Generation	-0.009** (0.004)	0.014 (0.013)	-0.000 (0.001)	-0.004 (0.023)
First-Generation	-0.009 (0.006)	0.012 (0.016)	-0.002 (0.002)	-0.045** (0.023)
N	27,910	27,980	26,520	26,520
B. Interactions by Pell Eligibility				
Not Pell-Eligible	-0.011** (0.004)	0.016 (0.013)	-0.001 (0.001)	-0.001 (0.024)
Pell-Eligible	-0.008 (0.005)	0.012 (0.015)	-0.001 (0.002)	-0.029 (0.022)
N	27,890	27,960	26,500	26,500

NOTES: Standard errors in parentheses. Reference category: not mandated. Each column under each panel is a separate regression. Results are unweighted. Logged continuous variables are estimated using tobit models, ratios are estimated using fractional probit models, and indicator variables are estimated using probit models where first-generation status or income is interacted with the mandate indicator. Overall effects reported from Table 4. *** p<0.01, ** p<0.05, * p<0.1

Third, I examine heterogeneous effects of financial education mandates on financial aid use. When it comes to federal financial aid decisions, the mandates primarily affect later-generation and Pell-ineligible students (see Table 12). Later-generation students and higher-income students who were exposed to the mandate are five percentage points more likely to fill out a FAFSA than their socioeconomically similar peers. While they are 4 – 6 percentage points more likely to borrow any federal student loans than their non-mandated peers, they borrow fewer amounts. Specifically, later-generation college students who were required to take personal finance are four percentage points more likely to use Pell grants than their non-mandated peers.

First-generation college students under the mandate are five percentage points less likely to use any unsubsidized loans, and marginally borrow fewer amounts in unsubsidized loans when doing so. Similar trends persist among Pell-eligible students, but in a slightly different way. Pell-eligible students are marginally three percentage points less likely to use any unsubsidized loans, and borrow fewer amounts of unsubsidized loans when choosing to do so. Mandates may not impact other financial aid use among economically disadvantaged students if they are already resorting to financial aid due to resource constraints.

Table 11. Average Marginal Effects of Mandates on Federal Financial Aid Use by Economically Disadvantaged Subgroup

VARIABLES	Filed FAFSA [‡]	Used Any Pell Grants [†]	Used Any Subsidized Loans [†]	Log Amount Subsidized Borrowed (if Used Any) [†]	Used Any Unsubsidized Loans	Log Amount Unsubsidized Borrowed (if Used Any)	Used Any Stafford Loans	Log Amount Borrowed (if Used Any)
Overall	0.036** (0.015)	0.021 (0.020)	0.011 (0.023)	0.011 (0.023)	-0.029* (0.016)	-0.060* (0.033)	0.017 (0.016)	-0.048* (0.025)
A. Interactions by First-Generation Status								
Not First-Generation	0.046*** (0.016)	0.040** (0.020)	0.028 (0.025)	0.022 (0.018)	-0.021 (0.017)	-0.048 (0.032)	0.037** (0.016)	-0.042* (0.022)
First-Generation	0.008 (0.015)	-0.006 (0.021)	-0.011 (0.022)	-0.002 (0.032)	-0.048*** (0.016)	-0.083* (0.043)	-0.025 (0.020)	-0.061* (0.034)
N	24,190	15,350	15,350	8,310	29,550	10,480	29,550	15,190
B. Interactions by Pell Eligibility								
Not Pell-Eligible	0.048*** (0.017)	---	---	----	-0.033 (0.026)	-0.022 (0.030)	0.057*** (0.019)	-0.044* (0.023)
Pell-Eligible	0.019 (0.014)	---	---	---	-0.025* (0.015)	-0.086** (0.039)	-0.018 (0.023)	-0.051* (0.028)
N	24,170	---	---	---	29,530	10,480	29,530	15,190

NOTES: Standard errors in parentheses. Each column under each panel is a separate regression. Reference category: not mandated. Results are unweighted. Indicator variables are estimated from probit models, and logged continuous variables are estimated from tobit models where first-generation status or income is interacted with the mandate indicator. [‡] denotes conditional on not attending a for-profit institution. [†] denotes conditional on Pell eligibility. Overall effects reported from Table 5. *** p<0.01, ** p<0.05, * p<0.1

Last, I examine heterogeneous effects of financial education mandates on employment while enrolled. Table 12 reveals that the mandates primarily affect economically disadvantaged students when it comes to employment decisions. Among these students, the mandates reduce the probability of being employed while enrolled by 3 – 4 percentage points. Additionally, economically disadvantaged students exposed to the mandate work 1.1 – 1.4 fewer hours per week while enrolled than their peers who were not required to take the course. Mandates reduce the probability of working at least 20 hours per week for most employed students – more strongly so for first-generation college students.

Table 12. Average Marginal Effects of Mandates on Employment While Enrolled by Economically Disadvantaged Subgroup

VARIABLES	Employed While Enrolled	Hours Worked Per Week	Works 20+ Hours Per Week (if Employed)
Overall	-0.019 (0.015)	-0.898** (0.451)	-0.046* (0.024)
A. Interactions by First-Generation Status			
Not First-Generation	-0.010 (0.015)	-0.619 (0.408)	-0.040* (0.023)
First-Generation	-0.038* (0.022)	-1.435** (0.682)	-0.060** (0.029)
N	29,450	29,450	14,040
B. Interactions by Pell Eligibility			
Not Pell-Eligible	-0.002 (0.019)	-0.650 (0.527)	-0.059** (0.024)
Pell-Eligible	-0.033** (0.016)	-1.082** (0.502)	-0.036 (0.025)
N	29,430	29,430	14,040
NOTES: Standard errors in parentheses. Reference category: not mandated. Each column under each panel is a separate regression. Results are unweighted. Indicator variables are estimated from probit models, and continuous variables are estimated from Poisson models where first-generation status or income is interacted with the mandate indicator. Overall effects reported from Table 6. *** p<0.01, ** p<0.05, * p<0.1			

Robustness Checks

For robustness checks to date, I visually assess pre-trends, I alternate the mandate specification to vary by pre-treatment and post-treatment, and I conduct a falsification test where I arbitrarily set years of mandate implementation five years back. I perform robustness checks among outcomes where financial education mandates had any overall effect in the main results.²⁹

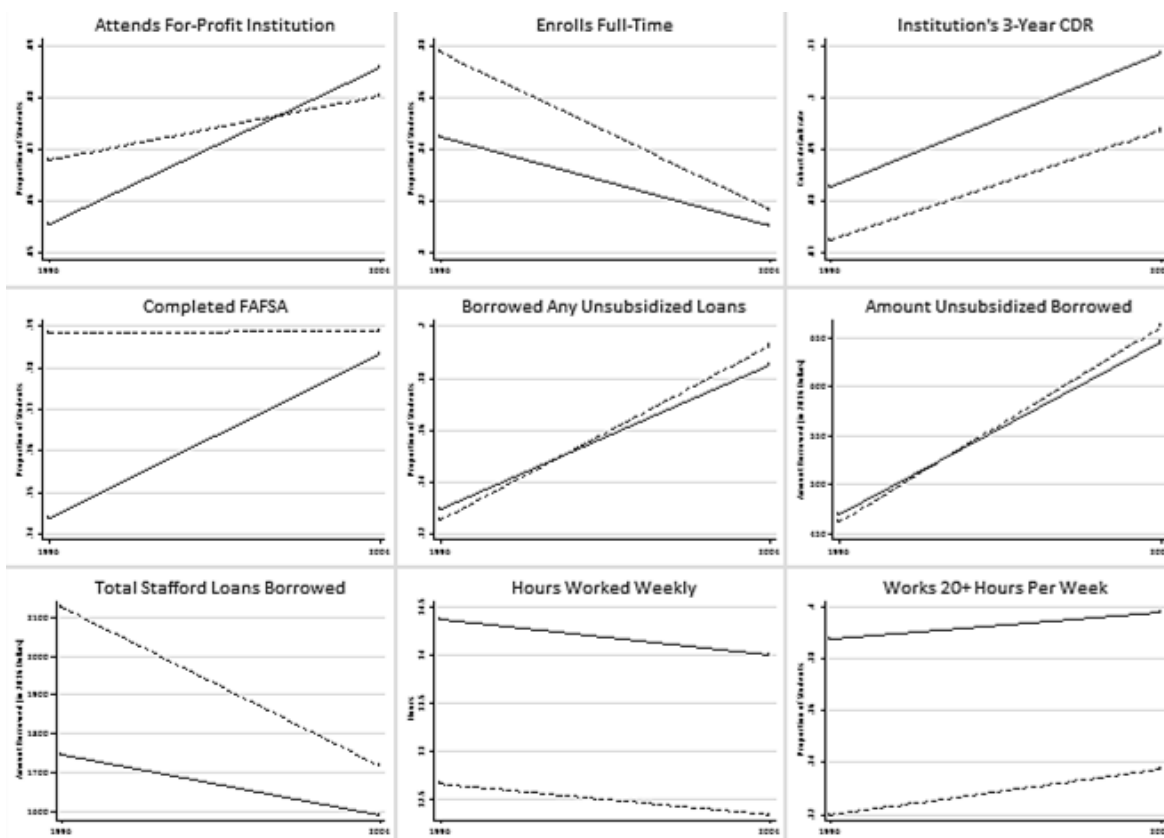
²⁹ These outcomes include attending a for-profit institution, enrolling full-time, CDR of attended institution, filling out a FAFSA, using any unsubsidized loans, amount unsubsidized borrowed, amount total borrowed, hours worked per week, and working 20 hours or more if employed.

Visual Representation of Pre-Trends Among the 2012 Entering Cohort

Figure 3 displays the visuals for pre-trends for each impacted outcome. The two groups displayed are the control states (states that have never implemented a mandate), and treatment states (states that have implemented a mandate any time after 2004). Since there are only three cohorts, the trends below are mean outcomes within 1996 and 2004, where I compare states who never implemented the mandate with states that implemented the mandate after 2004. This means that Figure 3 excludes states that implemented mandates prior to 2005.

Overall, the visuals suggest that enrolling in for-profit institutions violates the parallel trends assumption, as does unsubsidized loan borrowing. For these outcomes, differences began to appear prior to mandate implementation. Completing a FAFSA also violates the parallel trends assumption because application rates remain consistent in the control states while application rates increase in the treatment states. I further examine pre-trends in the event study explained next.

Figure 3. Pre-Trends of Impacted Outcomes



SOURCE: Author's calculations of the BPS:96/01 and BPS:04/09, unweighted. N = 30,460. "Amount Unsubsidized Borrowed" and "Total Stafford Borrowed" are in 2016 dollars. FAFSA trend excludes students attending for-profit institutions. Solid lines represent states that began implementing personal finance courses any time after 2004; dashed lines represent control states.

Event Study

To empirically examine if treatment states and control states had systematically different pre-trends, I conduct an event study using dummy variables for lead and lag treatment variables.³⁰ I bound the leads up to seven years before the year that the mandate was implemented and bound the lags up to seven years after the mandate was implemented. In event study specifications, I replace the general mandate indicator M_{ics} with the lead and lag treatment variables and estimate the following:

$$f(Y_{ics}) = \beta_0 + \sum_{p=-7}^{-1} \beta m_{ics}^p + \sum_{p=1}^7 \beta m_{ics}^p + X_i' \beta + \gamma_c + \gamma_s + \varepsilon_{ics}$$

where the omitted dummy variable is the year that the mandate is first implemented (m_{ics}^0). Additionally, states who have never implemented a mandate are the reference categories. As defined previously, Y_{ics} is the estimated postsecondary outcome, $X_i' \beta$ is a vector of student i 's demographic characteristics and institutional level attended, γ_c is the entering cohort year fixed effect, and γ_s is the high school state fixed effect. I use dummies so that I can see how the effects vary across implementation timing without imposing any functional form assumptions yet allowing for comparison between mandated and non-mandated students. If pre-trends are genuinely common, then we should see no effect of the lead indicators. Yet, this specification can also provide useful information about when effects of mandates may occur.

Table 13 reveals the average marginal effects of the impacted outcomes from the event study specification. I also provide information on F-test results when jointly testing pre-implementation dummy variables. I test the difference between the average effects across all post-implementation dummy variables and the average effects across all pre-implementation dummy variables. In the testing differences exercise, I aim to demonstrate obtaining similar estimates as those from the main results. I report these results in Table 13 in the “Post – Pre” row.

I find that the lead treatment dummy variables are not jointly significant than zero for all outcomes except for cohort default rate and using any unsubsidized loans. This suggests that there are some systematic differences in outcomes between treated and control states prior to implementing the mandate. The variables where there are insignificant or marginally significant variables through-out the entire pre-period are cohort default rates, total student loans borrowed, probability of working at least 20 hours per week while enrolled, and hours worked per week. Enrolled full-time contains one significant pre-effect in the fifth year prior to mandate implementation, but otherwise shows no or marginal effects during the pre-trend. These findings are rather consistent with the visual representation of the pre-trends.

³⁰ Such event studies were also carried out in Brown et al. (2016) and Cole, Paulson and Shastry (2015).

Table 13. Average Marginal Effects from Event Study Specification

Event Timing	Attends For-Profit Institution	Enrolls Full-Time	CDR	Filed FAFSA [‡]	Used Any Unsubsidized Loans	Log Amount Unsubsidized Borrowed (if Used Any)	Log Amount Borrowed (if Used Any)	Works 20+ Hours Per Week (if Employed)	Hours Worked Per Week
-7+ Years	0.039 (0.046)	-0.035* (0.019)	0.002 (0.009)	-0.108*** (0.038)	0.012 (0.023)	-0.031 (0.059)	0.022 (0.056)	0.036 (0.039)	1.095* (0.650)
-6 Years	0.106 (0.082)	-0.075 (0.046)	0.014 (0.017)	-0.063 (0.045)	0.022 (0.046)	-0.162** (0.077)	-0.013 (0.098)	0.085 (0.074)	1.447 (1.441)
-5 Years	0.142** (0.067)	-0.086*** (0.026)	0.019 (0.013)	-0.203*** (0.057)	0.033 (0.031)	-0.141** (0.068)	0.016 (0.075)	0.042 (0.068)	1.762 (1.496)
-4 Years	0.044 (0.067)	-0.008 (0.024)	0.014 (0.012)	-0.069 (0.049)	-0.018 (0.034)	-0.031 (0.061)	-0.007 (0.060)	-0.040 (0.045)	-1.137* (0.619)
-3 Years	-0.081** (0.035)	-0.001 (0.018)	-0.001 (0.009)	-0.035 (0.038)	-0.122*** (0.047)	-0.109** (0.050)	-0.033 (0.055)	0.015 (0.043)	0.315 (0.708)
-2 Years	0.054 (0.083)	0.021 (0.025)	-0.004 (0.010)	-0.092 (0.058)	-0.009 (0.027)	0.098 (0.068)	0.119* (0.062)	-0.045 (0.053)	0.305 (0.741)
-1 Year	0.086 (0.076)	-0.029 (0.022)	0.008 (0.012)	-0.028 (0.048)	-0.038 (0.042)	0.148** (0.059)	0.026 (0.072)	-0.051 (0.078)	-0.893 (1.035)
1 Year	0.045 (0.052)	-0.043** (0.019)	0.002 (0.010)	-0.026 (0.040)	-0.002 (0.040)	-0.008 (0.057)	0.008 (0.057)	-0.029 (0.055)	1.310 (1.252)
2 Years	0.058 (0.067)	0.006 (0.033)	0.003 (0.010)	-0.030 (0.049)	-0.041 (0.035)	-0.065 (0.058)	-0.004 (0.058)	-0.029 (0.065)	0.188 (0.759)
3 Years	0.081 (0.063)	-0.006 (0.035)	0.007 (0.011)	-0.060 (0.044)	-0.021 (0.050)	-0.085 (0.093)	0.003 (0.060)	-0.052 (0.070)	-1.647 (1.033)
4 Years	-0.051 (0.048)	0.037** (0.015)	-0.008 (0.008)	-0.022 (0.042)	-0.080*** (0.030)	-0.143** (0.071)	-0.068 (0.073)	-0.105** (0.051)	-1.636*** (0.507)
5 Years	0.064 (0.066)	0.052* (0.029)	0.005 (0.010)	-0.078* (0.046)	-0.032 (0.042)	-0.083 (0.080)	-0.013 (0.058)	-0.065 (0.050)	-0.831 (0.876)
6 Years	-0.027 (0.048)	0.029 (0.022)	-0.008 (0.012)	-0.022 (0.040)	-0.085*** (0.026)	-0.089 (0.071)	-0.014 (0.056)	-0.005 (0.074)	-0.577 (0.842)
7+ Years	-0.013 (0.059)	0.041** (0.018)	0.001 (0.010)	-0.057 (0.042)	-0.141*** (0.031)	-0.032 (0.080)	-0.109* (0.062)	-0.029 (0.060)	-1.952** (0.791)
N	29,550	29,550	27,910	24,190	29,550	10,480	15,190	14,040	29,450
F-Test									
Pre-Mandate	153.61	22.28	10.95	60.70	9.70	12.50	2.45	17.62	51.36
Prob > F	0.000	0.002	0.141	0.000	0.206	0.000	0.017	0.014	0.000
Post - Pre	-0.033 (0.024)	0.047*** (0.015)	-0.007 (0.005)	0.043*** (0.015)	-0.040* (0.021)	-0.040 (0.033)	-0.047** (0.022)	-0.051*** (0.018)	-1.148** (0.542)
Main Results	-0.042** (0.020)	0.038*** (0.013)	-0.009* (0.005)	0.036** (0.016)	-0.029* (0.016)	-0.059* (0.033)	-0.048* (0.025)	-0.046* (0.024)	-0.888** (0.452)

NOTES: Standard errors in parentheses. Regression includes high school state and entering cohort year fixed effects. ‡ denotes conditional on not attending a for-profit institution. *** p<0.01, ** p<0.05, * p<0.1

Falsification Test

To further address concerns of whether the mandates causally impact outcomes, I conduct a falsification test. This falsification test consists of randomly changing the treatment dates as a sanity test to see if the results persist. If results are still present, then this may suggest that there was another event or intervention taking place prior to mandate implementation or that

something questionable is occurring in the DD framework. For the test, I arbitrarily set the year of mandate implementation five years prior to the actual year to ensure that no residual effects of adoption are being captured during testing.³¹ With any policy – especially education policy – there is likely some small-scale implementation or pilot testing occurring prior to the official rollout across all affected units. However, some states may drop the adopted policies before they are ever implemented (Urban and Schmeiser 2015; Morton 2016).

As Table 14 reveals, I find no effects of the pseudo-mandate on enrolling full-time, selecting into institutions with lower cohort default rates, or on federal student loan borrowing. However, the outcomes that do not pass the test are attending for-profit institutions, completing a FAFSA, and employment outcomes. This suggests that some other occurrence or framework may explain the decreases in deciding to attend for-profit institutions, to complete a FAFSA, and to work while enrolled.

Table 14. Average Marginal Effects of Mandates on Impacted Outcomes Using a Falsification Test

VARIABLES	Main Results	Falsification Test Results
Attends For-Profit Institution	-0.042** (0.020)	-0.063*** (0.020)
Enrolls Full-Time	0.038*** (0.013)	0.015 (0.011)
CDR (in Thousandths)	-0.009* (0.005)	-0.001 (0.005)
Filed FAFSA	0.036** (0.016)	0.042* (0.021)
Used Any Unsubsidized Loans	-0.029* (0.016)	-0.023 (0.025)
Log Amount Unsubsidized Borrowed (if Used Any)	-0.059* (0.033)	-0.036 (0.036)
Log Amount Total Borrowed (if Used Any)	-0.048* (0.025)	-0.018 (0.027)
Works ≥ 20 Hours Per Week While Enrolled (if Employed)	-0.046* (0.024)	-0.054*** (0.019)
Hours Worked Per Week While Enrolled	-0.888** (0.452)	-1.000** (0.426)

NOTES: N = 29,550. Standard errors in parentheses. Reference category: not mandated. Each cell is a separate regression. All regressions control for student demographics, high school GPA, institutional level, and include entering cohort year and high school state fixed effects. Results are unweighted. Numbers in bold indicate that the specific outcome passed the placebo test. *** p<0.01, ** p<0.05, * p<0.1

³¹ Subsequent work will control for year of adoption as well as year of implementation. This could help with eliminating some of the bias from the effects found when controlling strictly for year of implementation in cases where policies are known to acted upon prior to official implementation date. Urban and Schmeiser (2015) and CEE (1998; 2000; 2002; 2004; 2007; 2009; 2011; and 2014) do not include years of adoption in their data. For high school graduation years 2008 – 2011, I approximated placebo treatments using Pelletier (2015; 2017) in relation to Urban and Schmeiser (2015).

Summary of Robustness Checks and Implications for Findings (to Date)

Table 15 summarizes which variables passed robustness checks.³² The outcomes that passed all three checks are enrolling full-time, selecting institutions with lower cohort default rates, and total amount of federal student loans borrowed. Hence, the mandates may casually increase probabilities of enrolling full-time, increase enrollments into institutions with lower cohort default rates, and decrease the amount of federal student loans borrowed.

Table 15. List of Outcomes Passing Which Robustness Checks

Outcome	Visuals of Pre-Trends	Event Study	Placebo Tests
Attends for-profit institution			
Enrolls full-time	X	X	X
3-year CDR of institution	X	X	X
Filed FAFSA			
Using any unsubsidized loans			X
Amount unsubsidized borrowed (logged)			X
Total Stafford borrowed (logged)	X	X	X
Hours worked per week	X	X	
Worked 20 hours or more per week (if employed)	X	X	

Study Limitations

Estimation Bias in Filling Out the FAFSA

Some bias may have been inadvertently introduced during data imputation, or when restricting the sample for analyses. Particularly, even when accounting for survey design and weights within each cohort, an abnormally high proportion of students filled out the FAFSA relative to what other studies highlight. Table 16 contains information about these proportions across sample subsets. During the 2011 – 2012 school year, 45 percent of high school seniors filled out a FAFSA (National College Access Network 2017). Some of this bias is due to sample restriction. A significantly higher proportion of my analytic sample filled out the FAFSA than respondents who were not included in the sample. Additionally, this upward bias may be because some derived variables were created using readily available data from the FAFSA applications. Another plausible reason for this upward bias may be because they sampled successfully

³² I will conduct further robustness checks once data are available. I may explicitly control for state-level financial aid policies, control for the number of for-profit institutions, control for variation in state lotteries, control for policies that govern college campuses, and may conduct placebo tests where treatment is completely randomized across students. In these placebo tests, we should not see impacts of the placebo treatment on outcomes more than five percent of the time.

matriculated students. According to NCES calculations using the ELS:2002, 90 percent of high school seniors who completed the FAFSA successfully enrolled in college versus only 55 percent of those who did not fill out a FAFSA (NCES, ELS:2002, Tables 1 – 2).

Table 16. Proportions of Students Applying for Federal Aid by Sample Population

	BPS Universe: Codebook	BPS Universe: Calculated			Analytic Sample: Calculated		
	Weighted	Max N	Weighted	Unweighted	N	Weighted	Unweighted
Cohort:							
1996	0.587	11,980	0.593	0.733	5,100	0.635	0.767
2004	0.721	16,680	0.718	0.776	10,250	0.741	0.790
2012	0.824	24,770	0.824	0.896	15,250	0.837	0.900
Total	---	53,430	0.722	0.822	30,530	0.756	0.841

NOTES: "Max N" refers to the larger number out of the weighted and unweighted proportions. I weighted estimates using Taylor series approximation, and the weighted estimates in the codebook are generated from bootstrapping. For my analytic sample, unweighted N is reported. Differences in proportions between the calculated analytic sample and the calculated universal sample are statistically significant at the one percent level.

This then raised concerns that the effects of the mandate would attenuate to zero given that some variables were imputed using the FAFSA. Therefore, for sensitivity analyses, I ran a set of results using student-reported versus derived information for the sample that had student-reported data where appropriate for household size, parents' education level, and gender variables except income since most of the 1996 cohort was missing that information. I find that the magnitudes were similar across results regardless of if student-reported or derived data were used (see [Appendix D](#)). Accordingly, I report results from derived data, which is also per NCES's strong recommendations.

Currently, this study only employs three cohorts of college students. I initially chose the BPS over the National Postsecondary Student Aid Study (NPSAS) because the BPS allows researchers to track outcomes for individuals over time. Yet most importantly for this study, the BPS only captures first-time beginners whereas not all freshmen in the NPSAS are first-time beginners. NCES took great measures to ensure that the BPS only captured first-time beginners. These procedures involved triangulating administrative data from postsecondary institutions and several other sources (e.g. the National Student Loan Data System, Central Processing System, and National Student Clearinghouse) to see if potential sample members had any financial aid use or any enrollment at a postsecondary institution prior to the entering cohort year (Wine et al. 2002; Wine et al. 2011; Hill et al. 2016). However, with respects to choosing the BPS, my study changed in scope relatively late to timeline requirements. In subsequent work, I will further explore between and within variation of college choices using the NPSAS, which has (or will soon have) up to nine cohorts of data available (1987 – 2016). For this, I will restrict the minimum year to 1996 because Illinois was the only state to mandate personal finance was prior to 1993. The lower bound would still permit sufficient variation in mandate changes while not decreasing power to detect effects.

Decomposition Effects Are Currently Not Accounted for Due to Sample Population Limitation

These results do not account for any decomposition effects. One of the biggest questions is if financial education mandates increase the likelihood that students go to college at all. However, I cannot address this question now because I use a dataset of successfully matriculated college freshmen. In next steps, I will explore if mandates change the likelihood of pursuing postsecondary education using the Education Longitudinal Study of 2002 (ELS:2002) and the High School Longitudinal Study of 2009 (HSLs:09). The ELS:2002 follows a nationally representative sample of 10th graders through postsecondary education, and the HSLs:09 follows a nationally representative sample of 9th graders through postsecondary education.³³ The goals of both surveys are to examine students' trajectories into postsecondary education and post-graduate employment. Hence, these datasets are ideal for examining if mandates impact the decision to go to college at all.

Missing Data for Subsets of Cohorts that Would Provide More Insights into Results

Information on high school characteristics is not available for 2004 cohort. This is a year when states switch; therefore, I could not control for high school characteristics in analyses. This information is key to understanding reasons we see heterogeneous effects by economically disadvantaged status. It may be that disadvantaged students need more guidance in college choice, or that the mandates are implemented better in wealthier districts. At a minimum, controlling for high school characteristics can shed light on this, although it would be more fruitful to interact high school characteristics with mandate status.

There is no information on other non-educational loans students may be using to pay for their education (e.g. personal loans or alternative financial services). There is also no information on how much aid the student was actually awarded; therefore, examining financing outcomes relative to individual maxima could not be completed. It is not reasonable to assume that students are being offered the federal maxima of options unless students are below a certain income threshold and do not receive any non-federal aid, a scenario that is highly unlikely for most students. In a subsequent study, I will obtain the dataset that will allow me to at least approximate if students are approaching their individual maxima.

Information available in the 1990s is far more limited because many items or situations did not exist then (e.g. private student loans), or were not measured (e.g. credit card balances). To keep populations and variation in mandates consistent when examining outcomes, I did not analyze outcomes that were not available for the 1996 cohort.

³³ This means that ELS:2002 students were 12th graders in 2004, and HSLs:09 students were 12th graders in 2012. While not perfectly, this would complement the BPS: 04/09 and the BPS:12/14 studies. An update of the HSLs:09 study will be available to researchers in early 2018.

Discussion

The findings from this study suggest that financial education mandates may improve institution selection and college financing decisions among all students. Overall, individuals who were required to take a personal finance course for high school graduation were 3.8 percentage points more likely to enroll full-time, enrolled into institutions whose cohort default rates were 0.9 percentage points lower (marginally significant), and borrowed 4.8 percent lower amounts of federal student loans (marginally significant) than their peers who were not required to take the course as a core prerequisite.

However, these effects vary significantly by first-generation status and by Pell eligibility. School-based financial education increases the likelihood of enrolling full-time for both economically disadvantaged students (by 3.0 – 3.3 percentage points relative to non-mandated peers) and their wealthier counterparts (by 4.2 – 4.3 percentage points relative to non-mandated peers). Additionally, these mandates marginally reduce the amount of total federal student loans borrowed for both economically disadvantaged students (by 5.1 – 6.1 percent relative to peers not exposed to mandate) and their more advantaged counterparts (by 4.2 – 4.4 percent relative to peers not exposed to mandate). This may be due to borrowing less unsubsidized loans among economically disadvantaged students, or due to receiving any grants for higher-income students. However, later-generation and Pell-ineligible students who were exposed to the mandate are the ones who are enrolling into institutions with lower CDRs. Relative to later-generation and Pell-ineligible peers whom were not required to take personal finance, they enrolled into institutions whose CDRs were 0.9 – 1.1 percentage points lower on average. Mandates have no impact on enrolling into less risky institutions for economically disadvantaged students. For outcomes that impact both groups, we see slightly larger effects for later-generation and higher-income students than for first-generation and lower-income students.

State-mandated financial education is *associated* with a 4.2 percentage point decrease in the probability of attending a for-profit institution, a 3.6 percentage point increase in the likelihood of applying for federal financial aid, a 2.9 percentage point decrease in the probability of borrowing unsubsidized loans (marginally significant) (and a six percent decrease in the amount borrowed among borrowers), a 0.9 hour reduction in working while enrolled, and a 4.6 percentage point decrease in the likelihood of working 20 hours or more when employed (marginally significant). These policies are associated with the reduced probability in attending for-profit institutions for all students who were required personal finance courses. Mandates are specifically associated with increased likelihoods of filling out a FAFSA among later-generation and higher-income students, and are specifically associated with reduced hours worked per week and decreased unsubsidized loan borrowing among first-generation and lower-income students. We cannot infer that these set of results are causal at this time because they were not robust to event study specifications or falsification tests. Future studies incorporating more cohorts may

help assess causal implications for these outcomes because more cohorts would better capture variations in mandate implementation and postsecondary education decisions.

These findings complement the existing literature in its findings that information interventions (in this case, school-based financial education) increases the likelihood that students select into more promising institutions (e.g. Hoxby and Turner 2015; Castleman and Goodman 2018). These results also support the literature in its previous findings that state-mandated financial education may promote better use of credit products among young adults (e.g. Gutter and Copur 2011; Brown et al. 2014; Brown et al. 2016). Yet, while Brown et al. (2016, 2508) find that financial education courses have a positive yet insignificant effect on student loan borrowing, I find that personal finance courses have a marginally negative effect on student loan borrowing. When looking separately at subsidized versus unsubsidized student loans, my findings suggest that the decrease in total student loan borrowing may be through decreased use of unsubsidized loans.

This study provides a new perspective in thinking about college choice in conjunction with high school financial education, as evidenced in findings that mandates increase the likelihood to enroll full-time and increase selecting into institutions with lower CDRs. Finally, this study advances new insights that financial education heterogeneously impacts certain subgroups. In this context, mandates primarily impact college choices of non-disadvantaged students, and is *associated* with improving behaviors that respective subgroups are known to avoid or to engage in (e.g. higher-income students not filling out the FAFSA and lower-income students working more hours per week while enrolled).

This work also has implications for evaluating financial education policy. In addition to tracking effects of the mandates on use of traditional credit products, evaluations should also include tracking effects of the mandates on any decisions that impact young adults' eventual financial well-being, or on any decisions made in young adulthood that are implicitly financial such as postsecondary education decisions and postsecondary financing. Excluding the full range of young adults' financial behaviors may understate the benefits of state-mandated financial education; hence, dissuade policymakers from providing personal finance courses to high school students.

The finding that financial education mandates impact economically disadvantaged students and non-economically disadvantaged students in ways that we could reasonably expect has a few policy-relevant implications. Policymakers in states that already implemented financial education mandates should consider incorporating concepts concerning college choice and college financing into their curricula. Students would be very receptive to these lessons not only because it is immediately applicable during junior and senior years of high school, but because they are likely aware that they need to know how to borrow student loans wisely, and how to plan for college or a career. For example, in their implementation study of personal finance courses in Chicago Public Schools, Roberts and Joyce (2017) quoted a student mentioning that “a lot of assignments are related to mortgages; right now, I am interested in student loans” during a focus

group session. For policymakers in states with mandates and are already moving toward incorporating postsecondary education concepts into the curricula, they should be careful to emphasize any aspect of postsecondary education that impacts their financing options, such as the decision to enroll in school full-time or part-time. Additionally, they may want to incorporate lessons on assessing other characteristics of an institution now that such information is readily available on the College Scorecard (e.g. look up cohort default rates and median earnings of former students). States without mandates which have a significant percentage of students pursuing postsecondary studies may want to consider establishing financial education mandates – or minimally, offer seminars on college choice and financial aid – to ensure that students make choices that maximize their benefits.

Yet, more studies using alternative datasets are needed before we can definitively say what the impacts of financial education mandates are on any activities that extend beyond the simple decision to attend college. We also need to further investigate why results on college choice are driven by later-generation and higher-income students, especially given that first-generation and lower-income students are significantly more likely to attend riskier institutions to begin with. This may be due to disadvantaged students already maximizing financial aid options, but needing more guidance on college choice; or, these mandates have been implemented better in wealthier or higher-resourced districts. If the former, then policy recommendations would center around strengthening curricula on college choice in underserved districts. If the latter, then policy recommendations would center around focusing more resources in underserved districts or augmenting other accessible yet effective interventions.

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Appendix A. Confirming Exogeneity of Financial Education Mandates with Random Assignment Checks

I run linear probability models assessing the impact of high school state, entering cohort year, high school graduation year, and students' characteristics on exposure to financial education mandates to test if they are exogeneous to students' pre-determined characteristics. Results are shown in the table below. Students' demographics and high school GPA explain an additional one percentage point of the variation in the mandates, and its estimates are not jointly significant than zero. This suggests that these policies are plausibly exogeneous to students.

VARIABLES	(1)	(2)	(3)	(4)
First-Generation College Student		-0.002 (0.005)		0.002 (0.005)
Per Member Household Income (in 1000s)		0.00003 (0.0001)		0.00001 (0.0001)
Independent		-0.031** (0.015)		-0.003 (0.008)
Underrepresented Minority		0.010 (0.007)		0.010 (0.007)
Female		0.002 (0.003)		0.001 (0.004)
D to C-		0.047 (0.054)		0.040 (0.051)
C- to C		0.027 (0.051)		0.022 (0.047)
C to B-		0.033 (0.053)		0.024 (0.049)
B- to B		0.034 (0.056)		0.023 (0.051)
B to A-		0.033 (0.058)		0.020 (0.053)
A- to A		0.031 (0.058)		0.017 (0.054)
Entering Cohort Year	YES	YES	NO	NO
High School Graduation Year	NO	NO	YES	YES
State of High School Attendance	YES	YES	YES	YES
Constant	-0.233*** (0.066)	-0.280*** (0.067)	-0.336*** (0.120)	-0.381*** (0.115)
N	30,530	29,550	30,530	29,550
R ²	0.701	0.711	0.705	0.715
F-test		1.398		0.989
Prob > F		0.204		0.469

NOTES: Robust standard errors (clustered at high school state) in parentheses. Each column is a separate regression. Results are unweighted, and are estimated from linear probability models. Reference category for high school GPA is "below D" (below 1.0). *** p<0.01, ** p<0.05, * p<0.1

Appendix B. Financial Literacy Questions on Compound Interest in the NFCS 2015

Questions M6, M7, and M31 from the 2015 NFCS were used in discussing compound interest. These questions are listed below, exactly as they are written in the 2015 NFCS questionnaire (with correct answers in bold):

M6) Suppose you had \$100 in a savings account and the interest rate was 2% per year. After 5 years, how much do you think you would have in the account if you left the money to grow?

More than \$102

Exactly \$102

Less than \$102

Don't know

Prefer not to say

M7) 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

Don't know

Prefer not to say

M31) Suppose you owe \$1,000 on a loan and the interest rate you are charged is 20% per year compounded annually. If you didn't pay anything off, at this interest rate, how many years would it take for the amount you owe to double?

Less than 2 years

At least 2 years but less than 5 years

At least 5 years but less than 10 years

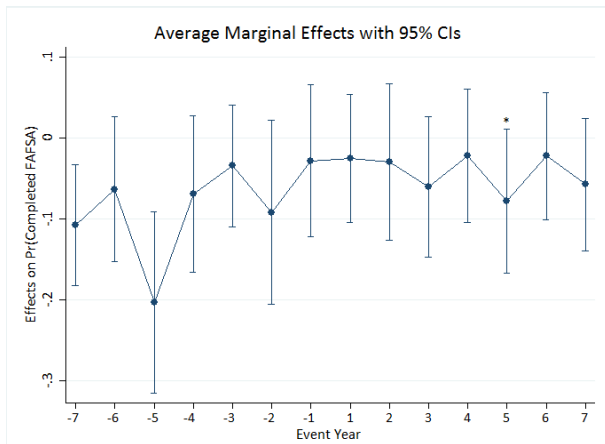
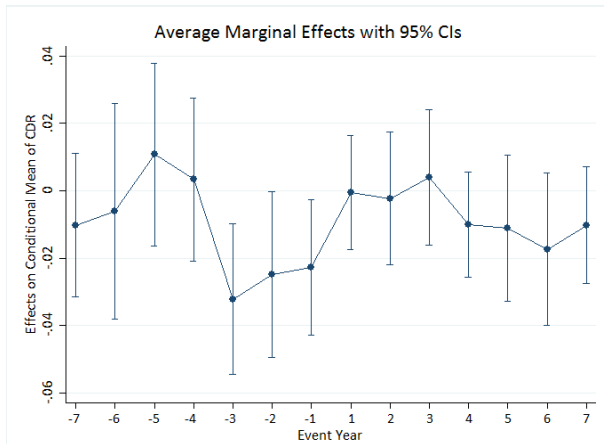
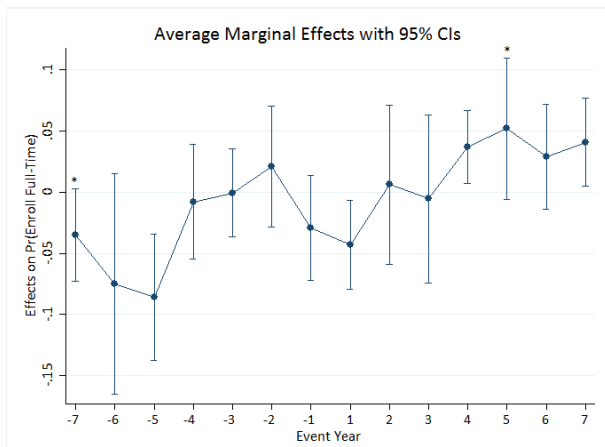
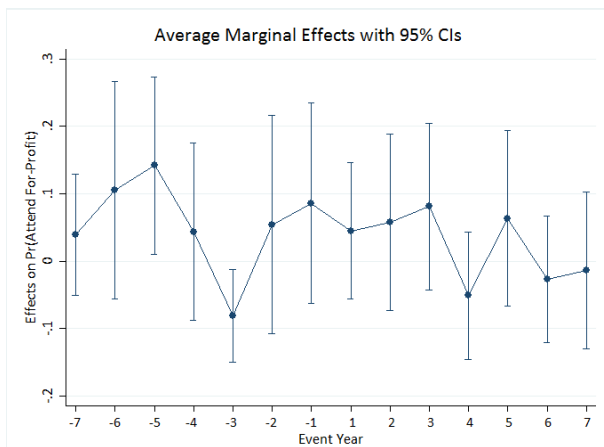
At least 10 years

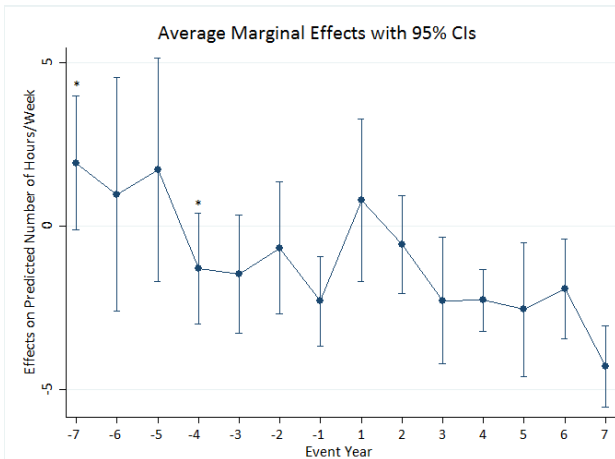
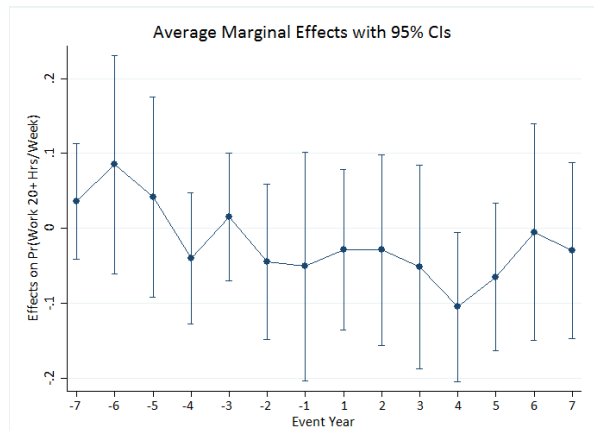
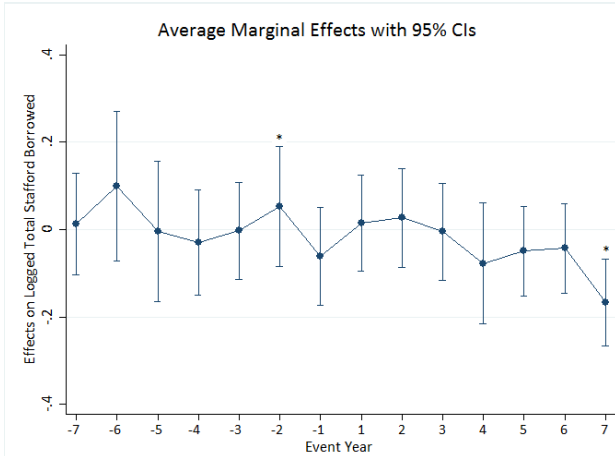
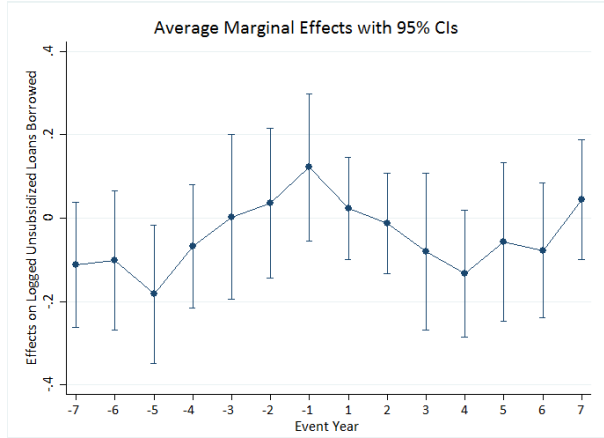
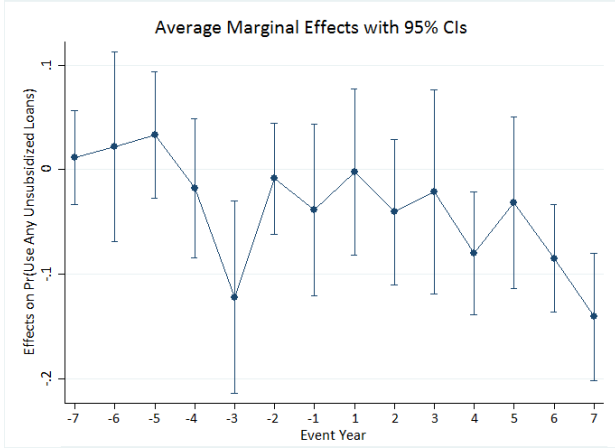
Don't know

Prefer not to say

Appendix C. Graphs of AMEs from Event Study Specifications

Below are plots of average marginal effects with 95 percent confidence intervals for the following variables: 1) attending for-profit institution, 2) enrolled full-time, 3) three-year cohort default rates of attended institutions, 4) completed a FAFSA (excluding students attending for-profit institutions), 5) if used any unsubsidized loans, 6) logged amount of unsubsidized loans borrowed (if borrowed any), 7) logged amount of total Stafford loans borrowed, 8) work at least 20 hours per week while enrolled, and 9) hours worked per week while enrolled.





Appendix D. Sensitivity Analysis by Data File Source

VARIABLES	Amount Unsubsidized									
	Enrolls Full-Time		CDR (in Thousandths)		Borrowed (if Used Any)		Amount Borrowed (if Used Any)		Hours Worked Per Week	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Mandated Personal Finance Courses	0.038*** (0.013)	0.038*** (0.013)	-0.008* (0.005)	-0.008* (0.005)	-0.067* (0.039)	-0.066* (0.038)	-0.054** (0.025)	-0.055** (0.025)	-1.126** (0.511)	-1.141** (0.507)
Independent	-0.037** (0.015)	-0.037** (0.015)	0.026*** (0.003)	0.027*** (0.003)	0.261*** (0.027)	0.243*** (0.026)	0.195*** (0.020)	0.195*** (0.020)	0.986** (0.465)	1.144** (0.455)
Underrepresented Minority	-0.012 (0.008)	-0.012 (0.008)	0.014*** (0.003)	0.014*** (0.003)	0.121*** (0.015)	0.109*** (0.015)	0.075*** (0.010)	0.075*** (0.010)	-1.123*** (0.230)	-1.043*** (0.223)
Per Member HH Income (in 1000s)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.000*** (0.000)	0.007*** (0.001)	0.005*** (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.037*** (0.008)	-0.022*** (0.006)
First-Generation College Student	-0.010 (0.006)	-0.011 (0.007)	0.011*** (0.001)	0.011*** (0.001)	0.005 (0.017)	0.001 (0.016)	0.002 (0.010)	0.010 (0.010)	1.052*** (0.247)	1.093*** (0.254)
Female	0.011*** (0.004)	0.011*** (0.004)	-0.002 (0.002)	-0.002 (0.002)	-0.006 (0.012)	-0.006 (0.012)	0.005 (0.006)	0.005 (0.006)	-0.163 (0.207)	-0.142 (0.210)
High School GPA: D to C-	0.026 (0.079)	0.026 (0.079)	0.023** (0.012)	0.023* (0.012)	-0.045 (0.236)	-0.024 (0.227)	-0.014 (0.129)	-0.016 (0.130)	-0.001 (1.899)	0.019 (1.904)
High School GPA: C- to C	0.016 (0.071)	0.015 (0.070)	0.017 (0.013)	0.017 (0.013)	0.109 (0.233)	0.134 (0.223)	0.072 (0.123)	0.070 (0.123)	1.182 (1.990)	1.200 (1.998)
High School GPA: C to B-	0.026 (0.069)	0.025 (0.069)	0.009 (0.012)	0.009 (0.013)	0.140 (0.236)	0.168 (0.228)	0.071 (0.123)	0.069 (0.123)	1.105 (2.078)	1.107 (2.083)
High School GPA: B- to B	0.065 (0.074)	0.063 (0.073)	-0.001 (0.013)	-0.001 (0.013)	0.139 (0.238)	0.168 (0.228)	0.078 (0.123)	0.077 (0.124)	1.172 (2.047)	1.178 (2.052)
High School GPA: B to A-	0.081 (0.073)	0.079 (0.073)	-0.013 (0.012)	-0.012 (0.013)	0.138 (0.242)	0.167 (0.232)	0.069 (0.127)	0.067 (0.128)	0.792 (2.023)	0.797 (2.027)
High School GPA: A- to A	0.133* (0.072)	0.131* (0.072)	-0.038*** (0.013)	-0.038*** (0.013)	0.116 (0.239)	0.153 (0.230)	0.021 (0.125)	0.020 (0.125)	-1.601 (2.106)	-1.607 (2.109)
Attends Four-Year Institution	0.150*** (0.010)	0.150*** (0.010)	-0.049*** (0.006)	-0.049*** (0.006)	0.083*** (0.021)	0.092*** (0.022)	0.114*** (0.021)	0.115*** (0.021)	-6.578*** (0.373)	-6.640*** (0.377)
N	25,020	25,020	23,740	23,740	8,460	8,460	12,700	12,700	24,960	24,960

Standard errors in parentheses. I show sensitivity analyses for a subset of outcomes for parsimony, but the same pattern persists across all outcomes. (1) denotes specifications from derived data, and (2) denotes specifications from student-reported data. Includes high school state and entering cohort year fixed effects. Impacted variables are household size, first-generation status, and gender. Results are unweighted. *** p<0.01, ** p<0.05, * p<0.1