Dissertation

Financial Education among Financially Vulnerable Populations in the United States

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This document was submitted as a dissertation in April 2018 in partial fulfillment of the requirements of the doctoral degree in public policy analysis at the Pardee RAND Graduate School. The faculty committee that supervised and approved the dissertation consisted of Grace Carman Grace (Chair), Trey Miller, and Jeremy Burke.

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Abstract

My dissertation examines if state-mandated financial education improves debt-related and college-going behaviors among economically vulnerable young adults. Young adults are more likely to be exposed to state-mandated financial education, yet are more likely to engage in adverse behaviors such as payday borrowing and sub-optimally financing postsecondary education. I employ a difference-in-differences approach to exploit cross-state and consumer-age (or student-cohort) variation in financial education mandates to detect causal effects in the aforementioned behaviors. Overall, I find that exposure to personal finance course requirements reduces engaging in adverse behaviors – particularly, reduces payday borrowing, increases full-time college attendance, and promotes selecting less risky institutions. Reductions in high-cost borrowing specifically occurred among subpopulations that are more likely to use AFS. However, in context of higher education, stronger improvements in college financing were seen among non-disadvantaged students. My findings reveal that financial education evaluations should account for all financial behaviors that are relevant to young adults. Otherwise, we may underestimate the impacts of school-based financial education; thereby, discourage policymakers from adopting these policies. Overall, policymakers should consider establishing these mandates to ensure that youth enter adulthood with a basic set of information to make sound financial decisions. They may wish to emphasize their efforts in underserved districts.

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Summary

Only one-third of Americans can correctly answer more than three out of five questions about compound interest, inflation, and risk diversification. Adequate financial knowledge is critical because consumers are becoming increasingly responsible for their own financial health. An emerging policy response has been to mandate financial education in schools; however, it remains unclear if financial education actually improves financial capability. Regardless of age groups studied, research on long-term effects of school-based financial education find either improvement in financial decisions, or no effects.

Previous studies have not examined the impact of these mandates on the practical behaviors or decisions that young adults engage in. Two examples of such behaviors I examine are pursuing postsecondary education and high-cost borrowing. Young adults are more likely to use alternative financial services yet are more likely to be exposed to state-mandated financial education. Furthermore, state-mandated personal finance courses are typically offered when young adults are deciding if and where to pursue postsecondary studies.

My first paper examines the impact of state-mandated personal finance education on younger consumers' use of alternative financial services (e.g. payday loans, auto title loans, tax refund anticipation loans, pawn shop services, and rent-to-own transactions). I exploit cross-state variation in financial education mandates by age to detect effects. I find that overall, consumers exposed to the mandate are six percentage points less likely to use any alternative financial services than individuals who were not subject to this requirement. However, mandates primarily affect AFS use among underrepresented minorities.

My second paper studies the impact of state-mandated personal finance education on college freshmen's initial college choices and federal financial aid use. Like the first paper, I exploit cross-state and student-cohort variation in financial education mandates to detect effects. I find that relative to non-mandated students, students who were exposed to the mandate enroll in institutions whose cohort default rates are one percentage point lower, are four percentage points more likely to enroll full-time, and borrow five percent less in federal student loans. Additionally, state-mandated financial education is associated with a four-percentage point decrease in the likelihood of attending a for-profit institution and is associated with a four-percentage point increase in applying for federal financial aid. Mandates have no differential effects on college choice outcomes by economically disadvantaged statuses. However, mandate exposure particularly increased likelihoods to borrow federal student loans among later-generation and higher-income students rather than first-generation and lower-income students.

My third chapter explores the presence of recall bias in financial education and its potential implications for accurately detecting effects of mandates. I compare measures of "mandated" (where legislative data indicates that an individual received high school financial education) with

measures of "recalled" (where the individual self-reports that they did not receive high school financial education). First, I find that age is positively associated with noncompliance, where self-reported responses do not match legislative accordance. I also find that being exposed to standalone course requirements, taking financial education in other settings, and believing that financial education should be offered in schools are negatively associated with noncompliance. Then, I find that measures of "mandated" are statistically different than measures of "recalled." This suggests that self-reported measures are not substitutes for legislative or administrative data.

Overall, my findings suggest that state-mandated financial education improves practical decisions among young adults, especially for economically vulnerable subgroups in debt-related cases. However, the mandates are more effective for non-economically disadvantaged students in college choice outcomes. Policymakers may find it beneficial to adopt these policies while augmenting resources to underserved districts. My dissertation reveals that researchers should examine all financial behaviors that are relevant to young adults when evaluating financial education mandates. Otherwise, its benefits may be underestimated; thereby, discouraging policymakers from adopting these policies.

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disadvantaged youth of promise being held back from accessing the best of the best when they deserve the best because they are not deemed the best when in fact they are the best.

Abbreviations

AFS	Alternative financial services
ALP	RAND American Life Panel
APR	Annual percentage rate
BPS	Beginning Postsecondary Students Longitudinal Study
COA	Cost of attendance
CCP	[Federal Reserve Bank of New York] Consumer Credit Panel
CDR	Cohort default rate
CEE	[National] Council for Economic Education
CFPB	Consumer Financial Protection Bureau
CPI-U	Consumer Price Index for all Urban Consumers
DD	Difference-in-differences
ELS	Education Longitudinal Study of 2002
FAFSA	Free Application for Federal Student Aid
FPL	Federal poverty line
GED	General Educational Diploma
GPA	Grade point average
HSLS	High School Longitudinal Study [of 2009]
IRS	United States Department of the Treasury, Internal Revenue Services
NB	Negative binomial
NCES	United States Department of Education, Institute of Education Sciences, National Center for Education Statistics
NCSL	National Conference of State Legislatures
NFCS	National Financial Capability Study
NPSAS	National Postsecondary Student Aid Study
OPD	United States Department of Education, Office of Federal Student Aid, Operations Performance Division
RAL	Refund anticipation loan
RDD	Random digit dialing
ROI	Return on investment
RTO	Rent-to-own financing or rent-to-own transaction
ZIP	Zero-inflated Poisson

Introduction

As of 2015, only 37 percent of Americans were able to correctly answer more than three out of five questions about interest rates, inflation, bond pricing, mortgages, and risk diversification (Lin et al. 2016). Low financial literacy, which is defined as one's ability to understand financial concepts and principles, is associated with a host of adverse behaviors including, but not limited to, being less likely to plan for retirement, using alternative financial services more frequently, feeling overindebted, being more food insecure, and being more averse to borrowing student loans (Lusardi and Mitchell 2011; Lusardi and de Bassa Scheresberg 2013; Lusardi and Tufano 2015; Carman and Zamarro 2016; Boatman and Evans 2017). Financial education requirements in high school provide a possible antidote to low financial literacy.

Financial decision making is difficult, especially when it comes to determining how much borrowing will ultimately cost. Economic theory suggests that consumers should exhaust cheaper options first, and then use more expensive options. For example, this would mean maximizing credit cards before borrowing payday loans, or maximizing subsidized loans before borrowing any unsubsidized loans. However, consumers often make mistakes.

If consumers are making mistakes because they lack knowledge and understanding of key financial concepts, or lack experience, then financial education may improve the ease of finding the information that is needed to make optimal decisions, and may decrease biases known to induce sub-optimal decisions (e.g. sticking to the status quo or to what is familiar). In financial education courses, students are expected to *explain* the characteristics of financial products, to *explain how to engage* in financial practices, to *analyze and compare* the advantages and disadvantages of financial products, to *evaluate* costs and benefits of financial products, and to *develop strategies* to become a creditworthy consumer.[1] Students would gain an understanding of financial concepts and principles as well as learn how to make and execute financial decisions that are appropriate given their constraints.

Beginning with Illinois in 1970, states have been introducing financial education requirements for high school graduation. This dissertation investigates whether these requirements improve young adults' financial choices. The first chapter considers the impact of financial education mandates on the use of alternative financial services. The second chapter examines the impact of mandates on college financing and enrollment decisions.

[1] These expectations are common across states; based on reviewing ten states' standards.

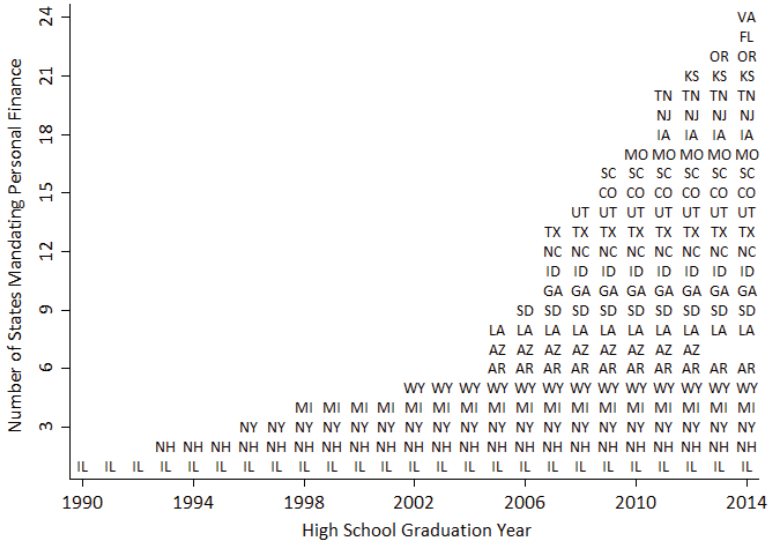
Financial Education Mandates

Financial education mandates are state policies that require teaching personal finance in public schools. Schools are one avenue through which information can be disseminated at scale to a heterogeneous population. The intent is similar across states: to improve financial literacy of student populations. The Texas Education Agency (2012, 1), for example, says that "the concepts of personal financial literacy are to be mastered by students in order that they may become self-supporting adults who can make informed decisions relating to personal financial matters."

Mandates vary from state to state in the content that is emphasized (CEE 2018). All state curricula require covering credit and debt management, understanding interest rates and compound interest, insurance, and financial planning (e.g. budgeting, savings and investment). Most states also require content on career management, and banking and financial services. While less common, some states explicitly cover concepts on alternative financial services and postsecondary education. Mandates also vary in how they are implemented. Some states offer standalone courses, but most of them integrate personal finance into economics, math or social science courses. Some states require these courses to be offered as a core requirement whereas some only require they be offered as an elective. Most states require students to take these courses during junior or senior year of high school.

I analyze mandates that require personal finance courses as a core prerequisite for high school graduation. As shown in Figure 1 below, these mandates have expanded across the United States at an exponential rate. As of 2014, 23 states required personal finance for graduation (Urban and Schmeiser 2015). According to CEE, 17 states required personal finance for graduation in 2018.

Figure 1. States Implementing Financial Education Mandates, 1990 – 2014



SOURCE: Urban and Schmeiser (2015). Graph shows when states required personal finance courses in high school as a core prerequisite for graduation. Prior to 1993, IL was the only state to do so.

Previous Studies Examining Impacts of High School Financial Education

This research adds to the literature on the impact of state-mandated financial education. Overall, studies on long-term effects of high school financial education mandates find mixed results. Accordingly, it is unclear to what extent financial education should be prioritized or emphasized in public policy. Government entities responsible for financial policy such as Consumer Financial Protection Bureau, the Federal Reserve System, and the Treasury are pushing for financial education in schools. State governments implementing financial education have cited reasons ranging from simply ensuring students' eventual financial well-being to addressing certain economic ills that the state has experienced. Academic scholars suggest that either financial education be used in tandem with other public policies, or we forego financial education altogether and consider other solutions (e.g. Willis 2011; Hastings, Madrian and Skimmyhorn 2013; Fernandes, Lynch and Netemeyer 2014).

A few literature reviews and one meta-analysis discuss the effects of financial education more broadly, which include financial education curricula, financial education programs, and financial education mandates (e.g. Hastings, Madrian and Skimmyhorn 2013; Fernandes, Lynch and Netemeyer 2014; Herman et al. 2015; Walstad et al. 2017). Below, I specifically discuss studies that examine impacts of *state-mandated* financial education.

State-Mandated Financial Education and Financial Knowledge

Financial education mandates are intended to improve financial knowledge and behaviors. Mandell (1997) and Tennyson and Nguyen (2001) are the only papers that look at the relationship between personal finance course *mandates* and financial knowledge. Both papers use the same dataset, but Mandell (1997) finds no association with financial knowledge while Tennyson and Nguyen (2001) find a positive association with financial knowledge. This is likely because Tennyson and Nguyen (2001) consider the heterogeneities of state mandates – particularly if students were required to take personal finance courses, whereas Mandell (1997) did not separately assess states who required all students to take personal finance courses versus states who required that schools offer the course as an elective.

From these two studies, it is not clear if financial education may improve financial knowledge or financial literacy.[2] Further studies on the relationship between mandates and financial literacy have not been pursued. This is likely because data on youth or young adults'

[2] Even an Marketwatch article arguing for financial education mandates failed to realize that three of the five states with the lowest financial literacy scores (according to their calculations) have financial education mandates in place (according to their list).

financial literacy is not available at a sufficient sample size or on an individually longitudinal basis. Instead, more recent studies examine the impact of mandates on financial behaviors.

State-Mandated Financial Education and Financial Behaviors

This literature contributes more directly to a larger literature that investigates the impact of financial education mandates on financial behaviors. These studies typically examine its effects on middle-aged adults' savings rates, investment behavior or wealth accumulation, and on young adults' credit behavior. As Table 1 shows, the literature finds either improvement in behavior or no effect on behavior.

Table 1. Summary of Impact of Mandates on Financial Behaviors

Authors	Research Design	Population	Behavior	Finding on Behavior
Bernheim, Garrett and Maki (2001)	Natural experiment	Middle-aged adults	Savings Wealth accumulation	Improves
Mandell and Klein (2009)	Descriptive	Young adults	Savings Credit payments	N/A
Gutter, Copur and Garrison (2011)	Descriptive	Young adults	Savings Credit payments	Improves
Brown et al. (2014)	Natural experiment	Young adults	Credit delinquency Credit scores	Improves
Cole, Paulson and Shastry (2015)	Natural experiment	Middle-aged adults	Savings Wealth accumulation	N/A
Brown et al. (2016)	Natural experiment	Young adults	Credit delinquency Credit scores	Improves

Previous literature began with retirement savings. One of the earliest – and most cited – papers to assess the effects of financial education mandates on financial behaviors is Bernheim, Garrett and Maki (2001). Using a self-administered cross-sectional survey, Bernheim, Garrett and Maki (2001) find that middle-aged individuals from states that mandated financial education had higher savings rates and higher levels of wealth than individuals from states that did not. One weakness of this paper is that it does not include state fixed effects. State fixed effects are needed to control for any differences in unobservable or unmeasurable factors occurring across states so that estimates are not picking up effects of these factors. When Cole, Paulson and Shastry (2015) do so the using U.S. Census, and the Survey of Income and Program Participation (SIPP), they find no effect of mandates on savings rates. These papers suggest that it is critical to include state-fixed effects, and that state-mandated financial education may not affect middle-aged adults' decisions.

Most of the literature, however, has focused on credit and debt. Using the FRBNY Consumer Credit Panel (CCP), Cole, Paulson and Shastry (2015) find that financial education mandates did not have an effect on middle-aged consumers' credit behavior. When studying debt in young

adults using the same dataset, Brown et al. (2014) and Brown et al. (2016) find that consumers from states that required financial education displayed better credit behavior (i.e. less likely to be delinquent and higher credit scores) than peers from states not requiring financial education. These papers suggest that state-mandated financial education may impact younger adults' decisions because they are most proximal to mandate exposure. Hence, it is worthwhile to study the impact of mandates on young adults' financial behaviors.

Mandell and Klein (2009) and Gutter, Copur and Garrison (2011) examine relationships between high school financial education and 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. Mandell and Klein (2009) find no relationship while Gutter, Copur and Garrison (2011) find a positive relationship. 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) surveyed high school classes of 2001 – 2004 from only one school system with a small sample size (N = 79). Meanwhile, Gutter, Copur and Garrison (2011) sampled college students across 15 universities who graduated high school between 2004 and 2008 (N = 15,797). It is not clear if these general financial behaviors are employed in paying for postsecondary education, an outcome that I examine in my dissertation.

The above are all examples of more mainstream financial behaviors, but it leaves out fringe banking, college choices, and college financing. My dissertation augments current literature about the effects of financial education mandates by investigating short-term impacts on these understudied behaviors. Effects of formal education diminishes over time. A large reason that the literature finds mixed results is because most of these papers examine outcomes occurring later in life, which may confound identification because it is not clear if the mandates or other circumstances (which the mandate may or may not have impacted) caused the financial outcome in question. Hence, assessing the short-run impacts of financial education is essential in determining if school-based financial education is worthwhile, especially given that the goal of financial education is to provide young adults with a foundation for making sound financial decisions.

Self-Reported Financial Education and Financial Behaviors

Many studies on financial education rely on self-reports of exposure to financial education. One example studied banking. Using a nationally representative cross-sectional dataset, Grimes, Rogers and Smith (2010) found that taking a financial education course in high school is positively correlated with the likelihood that one maintains a bank account. They found that the correlation between taking a high school financial education course and being unbanked was similar in magnitude to that of attending college (Grimes, Rogers and Smith 2010). One

weakness of this paper is that it relies on self-reports of exposure to school-based financial education, which may not always align with legislative or policy data.

Hence, previous studies may find mixed results due to recall bias, an error where respondents cannot accurately recall past events or experiences. Recall bias in education is likely due to spacing effects, where information is better recalled if it is applied more often than not, or due to recency effects, where most recent occurrences are more likely to be remembered than past occurrences. Hence, this bias may be complicated in older adults or in adults with higher educational attainment. Other reasons respondents exhibit the bias may be because they did not learn anything from the course; they genuinely did not take the course; or they do not know that they in fact took the course.

Using the RAND American Life Panel, I am able to detect if there is a recall bias when respondents self-report not taking financial education courses when legislative data suggests otherwise. This chapter briefly addresses the prevalence of remembering taking personal finance courses in high school, and its implications for policy evaluation.

Policy Applications Explored: Alternative Financial Services and Postsecondary Education

This dissertation focuses on two major financial products that are disproportionately used by young people: alternative financial services (AFS) and postsecondary education. Fernandes, Lynch and Netemeyer (2014, 1873) recommend "'just-in-time' financial education tied to a particular decision, enhancing perceived relevance and minimizing forgetting." While high school financial education may not occur immediately before the decision to use AFS, the decisions to pursue and finance college education may be particularly salient during high school.

Alternative Financial Services

Alternative financial services are short-term products characterized by high costs. Examples include payday loans, auto title loans, pawn shop loans, and rent-to-own transactions. The typical APR for borrowing an AFS loan is at least ten times the maximum APR charged on a credit card. Therefore, these products should be used as loans of last resort, but consumers, especially those who are low-income or credit-constrained, use them quite regularly. Credit-constrained and low-income consumers' inability to pay the prices of payday loans is prompting the Consumer Financial Protection Bureau (CFPB) to consider limits on this product. CFPB proposed to curtail payday lending either through requiring that lenders assess if borrowers' incomes are high enough to ensure repayment, or through requiring lenders to follow a set of restrictions ensuring that borrowers can repay loans on time. CFPB issued their proposal as a final rule in October 2017; it is now pending Congress approval. At the time of writing, the new acting director of CFPB is proposing to scale back any new regulations of payday lenders.

AFS consumers are usually credit-constrained, meaning that they have relatively poor or no credit. AFS institutions are likely to be located in areas where there are fewer banks, areas with higher poverty rates, areas with less educational attainment on average, and in areas that are predominately minority (Prager 2014; Barth et al. 2016). AFS products may be good if there are no other options available, but they are clearly worse than more traditional sources of credit. Bertrand and Morse (2011) show that AFS consumers do not understand how much it actually costs to use these products. Those who are less financially literate, have less income, are underrepresented minorities, or are young adults are more likely to use payday loans (Chatterjee 2013; Lusardi and de Bassa Scheresberg 2013; Barth et al. 2016). In fact, consumers aged 25 – 34 were more than twice as likely to use payday loans than their senior counterparts (Chatterjee 2013, 183). Determining if high school financial education impacts how consumers are using AFS is considerable given the demographics of this subpopulation.

Postsecondary Education

Within postsecondary education, I explore college choice and financing behaviors. Paying for college is a primary concern afflicting a majority of first year college attendees, especially economically disadvantaged college students. Over time, greater percentages of financial aid consist of loans that cannot be dispelled in bankruptcy. Tuition continues to rise, and with it debt burdens rise as well. The aggregate student loan debt rose by 30 percent between 2013 and 2017, where it is currently $1.5 trillion (Board of Governors 2018). Debt burdens may be increasing not only because of rising tuition, but also because students misuse student loans or because they attend expensive institutions that generate lower returns on investment (e.g. lower earnings for former students, higher likelihoods to default on student loans, higher unemployment rates). Pursuing postsecondary education is one of the first investments that most young adults make. Ideally, young adults would select an institution whose benefits exceed costs.

High school financial education mandates may be "just-in-time" for disadvantaged college students. As more people go to college, more high school students' first major financial decision will be whether to pursue postsecondary education or immediate full-time employment. For most students, their primary resource for 1) selecting a postsecondary institution, and 2) financing their postsecondary education is their parents. However, one in five college students under age 24 do not receive parents' or guardians' help with covering education and living expenses at the start of college.[3] Most college students are deciding where to go to college and how to pay for it when they are in high school. Therefore, determining if state-mandated financial education impacts where students are going to college and how they are paying for it makes sense because of the relative timing that they are making those decisions.

[3] 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.

Organization of the Dissertation

My dissertation is organized as follows. The next two chapters are papers devoted to quantitatively addressing the following research questions:

1. Do high school financial education mandates impact younger consumers' use of alternative financial services?
2. Do high school financial education mandates impact college students' postsecondary education decisions?

In both research questions, I use the same policy lever (state-mandated financial education) and same methodological approach (difference-in-differences). In the third chapter, I explore the presence of recall bias in financial education, and posit that this could be an endogenous reason that studies on financial education mandates find mixed results. In the final chapter, I conclude with overall findings and provide policy recommendations. This dissertation is not designed to be read from beginning to end. It is a collection of three standalone essays where the introduction and conclusion tie them altogether.

1. State-Mandated Financial Education and Younger Consumers' Use of Alternative Financial Services[4]

Alternative financial services (AFS) include check-cashing, rent-to-own financing, pawn shop services, auto title loans, tax refund anticipation loans, and payday loans. They are among the costliest financial services available and are predominantly used by the most vulnerable populations, including the poor and the young. Although such services offer credit to those who would otherwise be unable to secure it from traditional banking institutions and credit unions, they can be harmful to the overall financial well-being of consumers. Exorbitant fees and interest rates make it difficult for borrowers to pay off these loans in a timely manner. For payday loans in particular, almost half of borrowers roll over their loan at least once (Burke et al. 2014). Despite these high fees and interest rates, three out of ten Americans reported using these services. Of the consumers who use AFS, at least 45 percent are young adults between the ages of 18 and 34. To mitigate the negative impact that using credit may have in general, some states have established a variety of financial education mandates on high school students. This paper examines whether or not such mandates have reduced the use of AFS among young consumers.

On average, the annual percentage rate (APR) for all AFS products is substantially higher than for traditional credit methods as illustrated in Table 1.1. While the maximum APR on unsecured credit cards is 30 percent, the typical APR on AFS loans is approximately 300 percent, of which the mean APR for payday loans is nearly 400 percent (Robb et al. 2015). For traditional credit, the APR charged depends on a consumer's credit score, and their credit limit is typically based on some combination of household income and creditworthiness. For AFS products, however, the APR charged depends on the loan fee and amount borrowed.[5] Good credit scores are not required to use these products nor do they determine the APR charged.

[4] An earlier summary of this paper appears in: Harvey, Melody. "Are Financial Education Mandates Associated with Use of Alternative Financial Services?" *Consumer Interests Annual* 63 (2017).

[5] Some credit unions provide payday loans (sometimes known as payday alternative loans) but at rates substantially less than payday lending institutions (National Federation of Community Development Credit Unions 2015; National Credit Union Association 2017). To qualify, consumers must have been a member of the issuing credit union for at least one month; loan terms cannot be less than one month; and no rollovers are allowed. Additionally, some credit unions require a credit check.

Table 1.1. Overview of Fees and Costs by Type of AFS Loan versus Traditional Credit

Product Type	Loan Fees	APR Range	Amount Borrowed	Loan Period	Collateral
Unsecured credit cards	N/A	13 – 30%	Up to credit limit	Greatly variable	Credit score
Auto title loans	$25 per $100 borrowed	300%	$100 - $5,500	4 weeks	Vehicle
Pawn shop loans	$2 - $25 per $100 borrowed	12 – 300%	Up to $150	4 weeks	Physical collateral
Payday loans	$10 - $20 per $100 borrowed	up to 1,950% mean = 391%	$100 – $500 mean = $375	2 weeks	Future paycheck
Refund anticipation loans	$100 per loan	70 – 500%	$300 - $1,000	1 – 2 weeks	Tax refund
Rent-to-own financing	N/A	57 – 230%	Up to product price	12 – 24 months	Purchased product

SOURCES: Bradley et al. 2009; McKernan, Ratcliffe and Kuehn 2013; Consumer Financial Protection Bureau 2013; Federal Trade Commission 2014; Galperin and Weaver 2014; Robb et al. 2015; and Bhutta, Goldin and Homonoff 2016.

The AFS industry is regulated at the state level, and payday loans have been a particular product of interest to policymakers. Currently, 35 states allow some form of payday lending (Morton 2016; Consumer Federation of America 2017). In some of these states, however, local jurisdictions are allowed to prohibit payday lending (e.g. Mayer and Martin 2017). In June 2016, the Consumer Financial Protection Bureau (CFPB) proposed federal regulations that would curtail payday lending and in October 2017, they issued a final rule requiring payday and auto title lenders across the nation to assess a borrower's ability to repay the loan based on the borrower's income and expenses.[6]

In addition to regulatory interventions, formal, classroom-based financial education may also be a way to reduce the use of AFS. Financial education mandates are state-level policies that require teaching personal finance in public schools. Personal finance courses typically cover concepts such as credit and loans, debt, savings, insurance, and help students learn about financial planning, budgeting and investments. Additionally, a few states also cover topics on postsecondary financing and alternative financial services.

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. As of 2012, 21 states required high school students to take personal finance courses for graduation (Urban and Schmeiser 2015). Yet, it is unclear to what degree financial education mandates have improved students' financial capability as they age.

There are conflicting findings about the efficacy of financial education mandates. 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

[6] However, the new acting director of CFPB is proposing to scale this proposal back.

group and its corresponding behaviors, these studies find that mandates either improve financial decisions, or have no effects.

Little research has focused on the impact of mandates on AFS in particular. Moreover, studies on the impact of financial education mandates do not consider whether mandating financial education in high school is particularly effective for improving decision making of younger, economically vulnerable 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. Even though the mandates may not be "just-in-time" for AFS consumers, the course may have salience for them because younger consumers are both more likely to use these products than older consumers and are more likely to have been exposed to financial education in school.

To assess how these mandates impact younger consumers' AFS use, I obtain data on AFS use from the restricted version of the 2012 National Financial Capability Study and data on state mandates from Urban and Schmeiser (2015). I use difference-in-differences to exploit cross-state and age variation in financial education mandates. Given that AFS are one of the costliest financial options and are difficult to repay, my hypothesis is that financial education deters individuals from using them. I restrict the sample for this study to those under age 40 who have at least a regular high school diploma because younger respondents are both more likely to use AFS and are more likely to have received school-based financial education, increasing my power to detect effects of the mandate.

Overall, I find that individuals who were exposed to the mandate were six percentage points less likely to use any alternative financial services (marginally significant) than individuals who were not exposed to the mandate. Particularly, I find that they were seven percentage points less likely to borrow payday loans and were four percentage points less likely to use rent-to-own financing. These effects are primarily driven by women and underrepresented minorities. Underrepresented minorities under the mandate were 13 percentage points less likely to use any alternative financial services, and women under the mandate were seven percentage points less likely to use any alternative financial services than their peers who were not subject to the mandate.

This paper is organized as follows: The literature review summarizes previous studies on AFS use and on the effectiveness of financial education mandates. The data section describes the survey data and state mandate database used in my analyses. The methodology section explains the identification strategy and model specifications. The findings section examines the effects of financial education mandates on the likelihood and frequency of AFS borrowing. General findings are presented, as well as findings by heterogeneous effects and robustness checks. The limitations section reiterates that treatment assignment is approximated and that identification is solely contingent upon time-constant factors. The concluding section highlights key findings and its implications for financial education evaluation and for financial education policy.

Literature Review

This research draws on three literatures. The first strand investigates who uses AFS. The second strand considers how local policies and characteristics impact the use of AFS. The third strand examines the impact of financial education mandates on using various products, including mainstream financial services. However, there has not yet been a focus on AFS use.

The first strand of literature investigates who uses AFS. Several studies demonstrate that consumers with lower financial literacy or less financial knowledge are more likely to use AFS. Lusardi and de Bassa Scheresberg (2013) explore the correlation between financial literacy and use of high-interest loans from the AFS industry using the 2009 National Financial Capability Study (NFCS). Financial illiteracy is highly and positively correlated with the use of high-interest loan products, even when controlling for banking status, having savings, and basic demographic and socioeconomic characteristics (Lusardi and de Bassa Scheresberg 2013). Robb et al. (2015) pooled the 2009 and 2012 NFCS to examine AFS use in relation to objective financial knowledge versus subjective financial knowledge. These researchers find that objective financial knowledge decreases the likelihood of using AFS and that overconfident consumers are more likely to use AFS. Bertrand and Morse (2011) conducted a randomized field experiment where they distributed information about costs of payday loans to consumers in various formats. Consumers receiving the information in any format had a decrease in payday loan use relative to consumers who received no information (Bertrand and Morse 2011).

Research demonstrates that young adults ages 25 – 34 are more than twice as likely to use payday loans as senior citizens, according to the 2009 NFCS (Chatterjee 2013, 183). Within this key demographic, the propensity to use AFS is greater among those with lower education levels (Lusardi and de Bassa Scheresberg 2013). The fact that demographic subgroups already associated with lower financial literacy are most likely to use AFS suggest that a lack of financial education may be one of several factors driving consumers to use these products. Accordingly, my paper adds to the first strand of literature by assessing if another form of information intervention (school-based financial education) may influence the use of AFS. This line of inquiry is especially pertinent when we consider that young adults are more likely to use AFS and that they are also most likely to be exposed to financial education in schools. My paper will also enhance understanding of who uses AFS by assessing if exposure to financial education differentially impacts those who are more likely to use AFS ex-ante.

The second set of the relevant literature considers how local policies or characteristics that shape proximity and access to AFS impact their use. For instance, Stegman and Faris (2003) find that while the number of traditional banking institutions in a given neighborhood has a small but negative impact on use of payday loans, the number of payday loan stores in a given neighborhood has a positive impact on use of payday loans whose absolute magnitude is greater than that of traditional banking institutions. Similarly, Friedline and Kepple (2017) use data from the 2012 NFCS and find that a larger concentration of AFS institutions in a given zip code is

associated with greater AFS use. Payday lending institutions tend to concentrate in poorer and predominantly minority areas, and in areas whose populations have lower credit scores (Prager 2014; Barth et al. 2016). These studies match findings that on average, AFS consumers are low or moderate income, underrepresented minorities with dependents (Chatterjee 2013; Lusardi and de Bassa Scheresberg 2013; Friedline and Kepple 2017). However, the direction of causality for this dynamic is unclear.

Other important studies examine the impacts of state-level AFS regulations on AFS use. Using the 2009 NFCS, McKernan, Ratcliffe and Kuehn (2013) find that banning payday loans is significantly associated with lower likelihoods of using payday loans. McKernan, Ratcliffe and Kuehn (2013) also find that placing a 36 percent APR cap ceiling on auto title loans is significantly associated with a decreased likelihood of using auto title loans. Yet, there is conflicting evidence on how payday lending regulation impacts other AFS use. Using IRS SPEC Return Databases, Galperin and Weaver (2014) finds that payday lending regulation is associated with a decrease in using all other AFS products such as pawn shop loans, refund anticipation loans, and auto title loans. But when using a panel on consumer credit behaviors, Bhutta, Goldin and Homonoff (2016) find that payday loan legislation increases use of other AFS products and of bank overdrafts.

In this paper, I build upon this second strand of literature by examining if another state-level policy (school-based financial education) can influence the use of AFS regardless of AFS state laws. While prohibiting payday loans drastically decreases the opportunity to use them, it does not necessarily prohibit consumers from using other AFS per Bhutta, Goldin and Homonoff (2016). This research will first assess if financial education impacts AFS use among consumers in all states, and then will examine AFS use only in states that permit payday borrowing.

The third strand examines the impact of financial education mandates on the use of various products, including mainstream financial services. Overall, these studies find that high school financial education mandates either improve financial outcomes or have no effects. One of the earliest papers to study the effects of financial education mandates is Bernheim, Garrett and Maki (2001). Their paper finds that middle-aged adults who were required to take financial education courses in high school had higher savings rates and accumulated more wealth than their peers who were not required to take these courses (Bernheim, Garrett and Maki 2001). However, they do not include state-fixed effects, which would control for any unobservable or unmeasurable differences across states. When Cole, Paulson and Shastry (2015) do so, they find no effect of mandates on middle-aged consumers' levels of wealth or credit behavior.

When looking particularly at the effect of financial education mandates on young adults' financial behaviors, two studies using the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP) dataset find that young adults who took mandatory classes had higher credit scores and fewer credit delinquencies than their non-mandated peers (Brown et al. 2014; Brown et al. 2016). Another study examines the relationship between receiving high school financial education and having a bank account using a nationally representative survey. Grimes, Rogers

and Smith (2010) find that taking a financial education course in high school is positively associated with being banked.

Academics and policymakers alike have advocated for financial education as a way to help consumers make more informed choices about using high-cost credit, but no study has examined if formal classroom-based financial education may help decrease the use of AFS products (e.g. Bertrand and Morse 2011; Lusardi and de Bassa Scheresberg 2013). In particular, Fernandes, Lynch and Netemeyer (2014, 1873) recommend "'just-in-time' financial education tied to a particular decision, enhancing perceived relevance and minimizing forgetting." Even though the mandates may not be "just-in-time" for AFS consumers, the course may have salience for them because younger consumers are less financially literate and are more likely to use these products than their older counterparts. Results from this study may not only pertain to school-based financial education, but also to students' ability to apply lessons about loans and credit to non-mainstream loans or credit.

Data

I employ data on AFS use from the restricted version of the 2012 National Financial Capability Study (NFCS). The NFCS is a nationally representative, triennial cross-sectional survey that examines consumers' finances. Specifically, the NFCS contains data about consumers' financial attitudes, financial behaviors, financial education background, financial literacy, money management, retirement accounts, income sources, homeownership and mortgages, insurance, financial advisor use, credit card use, and other debt use. Most notably for this paper, the "other debt use" section includes information about using auto title loans, payday loans, refund anticipation loans, pawn shop services, or rent-to-own financing. The 2012 survey contains consumer-level information on 25,509 Americans, with roughly 500 respondents per state. The NFCS uses quota sampling from various online panels to recruit survey respondents, where quotas are established and weights are calculated according to the American Community Survey distribution for age, gender, race, education attainment, and Census Division. In 2012, the NFCS was also administered in the RAND American Life Panel (ALP) to ensure that the quota sampling would produce similar results to a probability-based sample. I conduct some sensitivity analyses around state assignments using the ALP version of the NFCS, as explained later. Younger respondents are both more likely to use AFS and are more likely to have received school-based financial education. Therefore, I restrict the sample for this study to those under age 40 who have at least a regular high school diploma, leaving 7,324 observations.[7]

[7] Many AFS consumers have lower educational attainment. However, the mandate specifies that personal finance courses are a core requirement for high school graduation. A regular high school diploma notes that the holder has completed all course requirements. One does not need to meet all course requirements in order to receive a GED, certificate of completion, or other alternative credential. The NFCS does not contain any information about when the GED was awarded or when the respondent stopped attending high school; hence, I could not approximate treatment for these respondents.

The NFCS retrospectively asks the following questions pertaining to AFS use:
"In the past 5 years, how many times have you…

1. Taken out an auto title loan? Auto title loans are loans where a car title is used to borrow money for a short period of time. They are NOT loans used to purchase an automobile.
2. Taken out a short term "payday" loan?
3. Gotten an advance on your tax refund? This is sometimes called a "refund anticipation check" or "Rapid Refund" (Not the same as e-filing)
4. Used a pawn shop?
5. Used a rent-to-own store?"

where the answer options are "Never," "1 time," "2 times," "3 times," "4 or more times," "Don't Know," and "Prefer Not to Say" (Applied Research & Consulting LLC 2012, 27).

I employ data on state mandates from Urban and Schmeiser (2015) to determine who was required to receive financial education in high school. This database contains information on the exact years that states implemented financial education mandates between 1970 and 2014.[8] The dataset 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 high school graduation. It 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.[9] I explicitly analyze the policy variation that requires all students to take personal finance as a core prerequisite for graduation. I link this dataset to the NFCS as I explain below in order to address impacts of state mandates for education policy on financially vulnerable populations' AFS use.

Methodology

I use an approach akin to Bernheim, Garrett and Maki (2001) to assess if financial education mandates impact AFS use. My empirical strategy identified the impact of high school financial education mandates on AFS use by age. In particular, my empirical approach exploits variation across consumers within the same state before and after the mandate was implemented, and across consumers in states with mandates and states without mandates within the same age. We assume that high school financial education mandates are exogenous to the consumer.[10] By nature, mandates mean that all high school students are required to take the financial education

[8] Their dataset does not include the District of Columbia – but Council for Economic Education (1998; 2000; 2002; 2004; 2007; 2009; 2011; and 2014) and Bernheim, Garrett and Maki (2001) reveal that D.C. has never implemented any personal finance mandates.

[9] The proportions of the sample subjected to either policy (3.5 percent in standalone courses and 4.2 percent in testing requirements) are not large enough to derive expected results or meaningful conclusions. Therefore, I do not explore policy heterogeneities in this paper.

[10] Adults do not usually vote on these mandates; rather, they are passed by state legislature or state departments of education. By law, minors cannot vote on any measures.

course. 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). At least in respects to AFS, there is no evidence suggesting that AFS regulations are correlated with financial education mandates.

I examine the impact of high school financial education mandates on AFS use by estimating models of the following form:

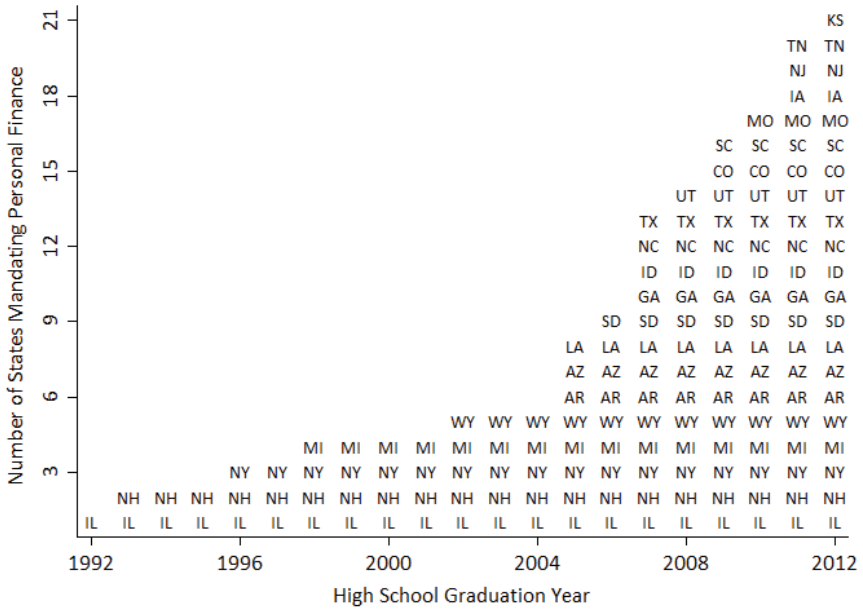
$$f(Y_{ist}) = \beta_0 + \theta X_{st} + X_i'\beta + \gamma_t + \lambda_s + \varepsilon_{ist}$$

where Y_{ist} represents the dependent variables in context of auto title loans, payday loans, refund anticipation loans (RALs), pawn shop services, and rent-to-own financing (RTOs). The first set of dependent variables are binary indicators equaling one if a consumer used the specific type of AFS in the past five years, and zero if otherwise. I also explore specifications where the dependent variable measures if a consumer used any of the five types (equaling one if yes, zero if otherwise). Further, I examine models where the dependent variables are counts indicating how many times a consumer used a specific type of AFS in the past five years. Finally, I examine models that simultaneously estimate the probability of never using a specific type of AFS and how often a consumer would use a specific type of AFS. These simultaneously-estimated models serve as a robustness check and as a discussion for where financial education may be more effective, if at all.

The independent variable of interest is X_{st}, which denotes if the consumer was required to take personal finance in high school for graduation. Taken together, I was able to use age and state of residence to determine if the respondent was likely to be exposed to mandated financial education as pictured in Figure 1.1. For example, the first graduating class required to take personal finance courses in South Carolina was the graduating class of 2009. Therefore, anyone currently living in South Carolina who was aged 18 and younger in 2009 is assumed to be treated; otherwise, those over age 18 in 2009 are assumed to not be treated. A limitation is that the NCFS does not contain the state where the respondent attended high school. Instead, I use graduation requirements for the current state of residence. According to my own calculations from the ALP, 79 percent of Americans under age 40 lived in the same state where they attended high school.[11] I test if state of residence is a good proxy for high school state by using the ALP version of the NFCS, as shown later.

[11] Calculated using MS 432 and MS 284 (the ALP subset of NFCS); weighted with clustering at state level.

Figure 1.1. States Implementing Personal Finance Mandates, 1992 – 2012



SOURCE: Urban and Schmeiser (2015)

$X_i'\beta$ accounts for the consumer's financial and demographic characteristics that may be related to AFS use.[12] Financial characteristics include annual household income and credit card holding status. Demographic characteristics include gender, race/ethnicity, marital status, and number of dependents. Age γ_t is a cohort fixed effect expressed in continuous form. Note that younger consumers were more likely to be exposed to financial education in schools. This fixed effect also captures unobserved cohort factors such as having more financial experience or accumulating more wealth. State of residence λ_s is a fixed effect that captures unobserved state characteristics and policies, including state-level AFS regulations affecting AFS use.

I weight all estimations and cluster standard errors by state to account for survey design and for policy variation occurring at the state level. I use listwise deletion since the percent of respondents for missing observations do not exceed three percent for any given variable. I also report average marginal effects.

[12] I cannot control for subjective or wealth-related variables that personal finance courses may influence, such as credit scores, financial literacy, educational attainment, subjective measures of financial knowledge, feeling overburdened in debt, having a retirement account, having emergency savings, having a bank account, or owning a home. The author is currently researching the effects of financial education on obtaining higher education, and is examining the effects of financial education on subjective financial knowledge. A number of these are also correlated with age (e.g. credit scores, homeownership).

Findings

Descriptive Statistics

The analytic sample is comprised of American adults ages 18 – 39 (mean age = 28.8) who have regular high school diplomas. Of these, nearly half are female, and slightly more than two-fifths are underrepresented minorities. The majority of consumers are married or living with a partner. Nearly half of consumers have no financially dependent children. Forty-five percent of consumers report an annual household income over $50,000, and nearly three-quarters have at least one credit card. Overall, nearly two in five consumers used at least one of the AFS loans in the past five years, with pawn shops and payday loans being the most commonly used products. The descriptive statistics in Table 1.2 also reveal that significantly higher proportions of younger consumers, underrepresented minorities, and parents of dependent children reported using any of the five AFS loans. Significantly higher proportions of those earning between $15,000 and $50,000 reported using any AFS than those in other income groups. These observations are in line with the findings of existing literature in the field. Furthermore, significantly lower proportions of those with credit cards reported using any AFS. This is not surprising, considering that credit cards are substitutes for AFS.

Table 1.2. Descriptive Statistics of Sample Characteristics and AFS Use

	Total (N = 7,324)	**Used No AFS** (n = 4,594)	**Used Any AFS** (n = 2,631)
Mandated Personal Finance in High School	16.6%	16.4%	16.8%
Used Any AFS Loan in Past Five Years[a]:	38.5%	0.0%	100.0%
Used auto title loans	13.4%	0.0%	35.0%
Used payday loans	17.8%	0.0%	46.7%
Used refund anticipation loans (RALs)	13.5%	0.0%	35.3%
Used pawn shops	24.9%	0.0%	65.0%
Used rent-to-own financing (RTOs)	15.5%	0.0%	40.5%
Underrepresented Minority[b]	40.7%	35.4%	49.2%
Female	48.5%	49.7%	46.6%
Marital Status			
Married	42.9%	43.0%	43.2%
Living with partner	11.1%	9.4%	13.9%
Single	46.1%	47.7%	42.9%
Has Any Dependent Children[a]	47.7%	40.6%	59.7%
Mean	0.9	0.8	1.2
SE	0.03	0.03	0.04
Age			
18 – 24	30.0%	28.9%	30.8%
25 – 34	48.2%	47.2%	50.0%
35 – 39	21.9%	23.9%	19.2%
Mean	28.8	29.0	28.5
SE	0.1	0.1	0.1
Income			
< $15K	16.0%	16.2%	15.5%
$15K - $50K	39.0%	34.5%	46.2%
> $50K	45.0%	49.4%	38.3%
Has Credit Card	72.1%	74.7%	68.3%

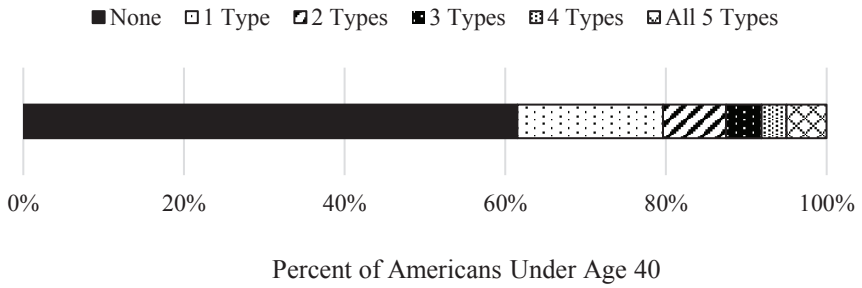
NOTES: Reported statistics are weighted. [a] These variables are top-coded at four. [b] Black/African-American,

Hispanic/Latino, and American Indian/Alaskan Native racial groups are categorized as underrepresented minorities. Approximately 1.4 percent of the analytic sample did not answer any AFS questions.

Overall, 16.6 percent of American adults ages 18 – 39 were mandated to take personal finance courses for graduation. It appears that a slightly higher proportion of those mandated to take personal finance in high school reported using any AFS, but this fact could be a function of age because higher proportions of younger consumers were subject to the mandate than older consumers.

Figure 1.2 and Table 1.3 indicate that nearly 40 percent of younger Americans used at least one AFS product in the past five years, but the proportion of these individuals using each specific product ranges from 13 – 25 percent. The reason for this is because the highest proportion of AFS consumers used only one type of AFS product, as demonstrated in Figure 1.2. When looking more specifically at each type of AFS use in Table 1.3, a majority of those using an AFS product reported using it only once in the past five years. It is unclear whether respondents considered rolling over a loan when answering the survey question on AFS use.[13] Furthermore, the top-coding of the AFS question prohibits combining the total number of times that consumers may have used AFS products overall.

Figure 1.2. Number of Different Types of AFS Products Used in the Past Five Years



SOURCE: NFCS (2012). Reported statistics are weighted percentages. N = 7,225.

Table 1.3. Frequency of AFS Use in the Past Five Years

	N	Mean Frequency	Frequency Used Particular Product				
Type of AFS Products:			0	1	2	3	4 or more
Auto Title Loans	7,186	0.25	86.6%	6.7%	3.3%	2.1%	1.3%
Payday Loans	7,185	0.42	82.2%	6.1%	4.3%	2.8%	4.7%
RALs	7,178	0.28	86.5%	5.2%	3.8%	2.4%	2.1%
Pawn Shop Services	7,176	0.58	75.1%	8.8%	5.4%	4.2%	6.4%
RTOs	7,198	0.30	84.5%	7.2%	3.9%	2.4%	2.1%

NOTES: Reported statistics are weighted. Percentages may not add up to 100 due to rounding.

[13] Burke et al. (2014, 4 – 5) found that within a 12-month study period, four out of five payday loans were rolled over within two weeks, where 48 percent of borrowers have rolled over at least one payday loan.

Main Findings

When holding demographic and financial characteristics constant, mandatory financial education classes as a core requirement for high school graduation reduced the probability of using any of the five AFS loans by six percentage points (marginally significant; see Table 1.4). Even when holding financial education constant, significant differences among certain subgroups remained. Consumers who racially identified as Black, Latino, or other were significantly more likely to use AFS than consumers who racially identified as white. Females were significantly less likely to use any AFS than males. Consumers with an annual household income of $50,000 or less were significantly more likely to use AFS than consumers with annual household income exceeding $50,000. Moreover, consumers living with a partner were significantly more likely to use AFS than married consumers, and consumers who were financially responsible for more children were significantly more likely to use AFS. Older consumers were significantly less likely to use any AFS. These differences are all consistent with findings from previous studies.

Table 1.4. Likelihood of Using Any AFS Given Mandated Financial Education and Select Characteristics

Independent Variables:	**Use Any AFS**
Mandated personal finance	-0.059*
	(0.033)
Female	-0.078***
	(0.014)
Living with partner	0.094***
	(0.031)
Single	-0.009
	(0.020)
Black/African-American	0.128***
	(0.021)
Latino/Hispanic	0.081***
	(0.018)
Asian/Pacific Islander	-0.053
	(0.038)
Other	0.081*
	(0.044)
Income: < $15,000	0.046**
	(0.021)
Income: $15,000 - $50,000	0.107***
	(0.014)
Has credit card	-0.035
	(0.022)
Number of dependent children	0.079***
	(0.007)
Age	-0.007***
	(0.001)
N	7,136

NOTES: Standard errors are reported in parentheses. Average marginal effects (AME) are reported. Regression includes state of residence fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.5 provides the effect of the mandates for each AFS product when holding demographics, credit card holding status, state of residence, and age constant. Each cell represents a separate regression. Individuals who were mandated to take personal finance classes in high school were four percentage points less likely to use RTOs in the past five years and were seven percentage points less likely to use payday loans than their peers who were not mandated to take these courses. Additionally, the negative binomial regression results in Table 1.5 suggest that individuals who were mandated to take personal finance classes in high school used 0.21 fewer payday loans in the past five years than individuals who were not required to take financial education.

Financial education mandates could have different effects across the different types of AFS for several reasons. This difference could be due to how people interpreted the AFS question or if consumers even know about some of these products. Pawn shop services can include purchasing a product, selling a product, or pawning a product, of which only the last activity constitutes taking out a loan. Payday loans are extremely well-known and readily available. Even some credit unions provide payday loans, albeit at a lower APR. RALs, on the other hand, may not be as well-known as some of the other products because they are only available once a year during the tax season.

Table 1.5. Average Marginal Effects of Mandates from Logit and Negative Binomial Regressions

	N	Logit	Negative Binomial
Dependent Variable:			
Used Any AFS	7,136	-0.059*	---
		(0.033)	---
Auto Title Loans	7,102	-0.024	-0.080
		(0.029)	(0.063)
Payday Loans	7,099	-0.072**	-0.211***
		(0.030)	(0.072)
RALs	7,091	-0.032	-0.089
		(0.030)	(0.071)
Pawn Shop Services	7,088	-0.044	-0.111
		(0.030)	(0.077)
RTOs	7,112	-0.040**	-0.048
		(0.019)	(0.055)

NOTES: Standard errors are reported in parentheses. Each cell denotes a separate regression. Each column denotes the type of specification employed. All regressions control for gender, race/ethnicity, marital status, number of dependent children, income, credit card holding status, age of respondent, and state of residence. Negative binomial regressions are not available nor appropriate for "Used Any AFS" because it is not a count variable. Average marginal effects (AME) are reported. Complete tables for both regression specifications are provided in Supplement 1A. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Heterogeneous Effects by Race/Ethnicity and Gender

The next set of results show heterogeneous effects by race and gender. As Table 1.6 reveals, 47 percent of underrepresented minorities report using AFS in the past five years versus one-third of non-underrepresented minorities. Higher proportions of minorities report using each type

of AFS than non-minorities. The largest discrepancies occur among pawn shop services (31 percent of minorities versus 21 percent of non-minorities) and payday loans (23 percent of minorities versus 14 percent of non-minorities).

However, a slightly higher proportion of men (40 percent) report using AFS in the past five years versus 38 percent of women. Greater proportions of men report using each type of AFS than women, although the differences are not as pronounced when examining differences by gender than when examining differences by race/ethnicity. In this case, the largest discrepancies occur among auto title loans and RALs (11 percent of women versus 16 percent of men for both).

Table 1.6. Descriptive Statistics of AFS Use by Race/Ethnicity and Gender

		Underrepresented Minority[a]		Gender	
	Total (n = 7,225)	Yes (n = 2,323)	No (n = 4,902)	Female (n = 4,178)	Male (n = 3,047)
Used Any AFS Loan in Past Five Years[a]:	38.5%	46.5%	33.0%	37.0%	39.9%
Used auto title loans	13.4%	15.1%	12.2%	10.6%	16.0%
Used payday loans	17.8%	23.1%	14.3%	16.2%	19.4%
Used refund anticipation loans (RALs)	13.5%	16.1%	11.7%	10.8%	16.1%
Used pawn shops	24.9%	30.7%	20.9%	22.8%	26.8%
Used rent-to-own financing (RTOs)	15.5%	18.2%	13.7%	13.7%	17.3%

NOTES: Reported statistics are weighted. [a] Black/African-American, Hispanic/Latino, and American Indian/Alaskan Native racial groups are categorized as underrepresented minorities.

Financial education could disproportionally impact consumers by race or gender in one of the following ways:

1. High school financial education would have less impact on a female consumer or a consumer identifying as an underrepresented minority (Black/African-American, Latino/Hispanic, or American Indian/Alaskan Native) because of some inherent way that the course is taught.
2. High school financial education would have more impact on a female consumer or a consumer identifying as an underrepresented minority because they may be less likely to learn about financial decisions and products elsewhere. For gender, this could be due to cultural norms, if families are less likely to discuss money with daughters than with sons. For race, underrepresented minorities are more likely to come from financially vulnerable backgrounds and therefore less likely to have familial, social or institutional sources to obtain financial information.

Table 1.7 shows that when interacting variables for financial education mandates by race and ethnicity, the mandates had stronger effects for underrepresented minorities than for non-underrepresented minorities. Underrepresented minorities who were mandated to take personal finance in high school were six percentage points less likely to use auto title loans; 12 percentage points less likely to use payday loans; eight percentage points less likely to use RALs; eight percentage points less likely to use pawn shop services; and 10 percentage points less likely to use RTOs than their peers who were not required to take personal finance classes. These results

also align with the fact that underrepresented minorities are significantly more likely to use AFS than non-underrepresented minorities.

When interacting financial education mandates by gender, the mandates had stronger effects for women than for men. Women who were mandated to take personal finance classes in high school were four percentage points less likely to use auto title loans; nine percentage points less likely to use payday loans; three percentage points less likely to use RALs (marginally significant); five percentage points less likely to use pawn shop loans (marginally significant); and five percentage points less likely to use RTOs than women not required to take personal finance classes in high school. Financial education might have a bigger impact on female students than male students because they have fewer alternative sources of information.

These gender and ethnicity effects are significant among both demographics across all AFS products. However, the effects are not as strong when looking at gender heterogeneity than they are when looking at racial heterogeneity. This is likely because women already use fewer AFS on average than men, yet underrepresented minorities use more AFS on average than non-underrepresented minorities. Nevertheless, these findings suggest that financial education mandated in high school may be more helpful for disadvantaged subgroups that are more likely to be economically vulnerable.

Table 1.7. Average Marginal Effects of Mandates by Race/Ethnicity and Gender

	Overall	Interactions by Underrepresented Minority Status		Interactions by Gender	
Dependent Variable:		Yes	No	Female	Male
Use Any AFS	-0.059*	-0.134***	0.015	-0.065**	-0.051
	(0.033)	(0.034)	(0.043)	(0.030)	(0.051)
Auto Title Loans	-0.024	-0.056**	0.013	-0.038**	-0.008
	(0.029)	(0.024)	(0.047)	(0.016)	(0.049)
Payday Loans	-0.072**	-0.120***	-0.026	-0.086***	-0.056
	(0.030)	(0.030)	(0.044)	(0.017)	(0.049)
RALs	-0.032	-0.075***	0.016	-0.034*	-0.028
	(0.030)	(0.025)	(0.041)	(0.018)	(0.044)
Pawn Shop Services	-0.044	-0.081**	-0.004	-0.047*	-0.040
	(0.030)	(0.031)	(0.040)	(0.024)	(0.043)
RTOs	-0.040**	-0.102***	0.031	-0.048**	-0.030
	(0.019)	(0.016)	(0.027)	(0.019)	(0.031)

NOTES: Reference categories: not mandated. Standard errors are in parentheses. Each row and primary column is a separate regression. Average marginal effects were calculated from logit regressions similar to Table 1.4 where race/ethnicity and gender were interacted with mandate indicator. N = 7,136. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Robustness Checks

For robustness checks, I consider an alternative specification, test the sensitivity of defining mandates according to state types, and conduct the main analyses excluding teenagers and excluding states that banned payday loans.

Estimating Effects Using Zero-Inflated Poisson Regressions

One potential concern is that the main specifications may not appropriately account for the high proportion of zeroes. Hence, I also estimate effects using zero-inflated Poisson (ZIP) regressions because at least three-quarters of respondents did not use any AFS (see Tables 1.2 and 1.3), and it represents a reasonable assumption that those using AFS loans may be systematically different than those that never use AFS loans.

Table 1.8 reveals that the economic conclusions from the ZIP regressions are similar to those of the negative binomial regressions presented in Table 1.5.[14] Particularly, individuals who were mandated to take personal finance classes in high school significantly used 0.17 fewer payday loans and used 0.08 fewer auto title loans (though this effect is only marginally significant). Among the inflate model coefficients, only the payday loans and RTO estimates are statistically significant. This suggests that being mandated to take personal finance classes primarily discourages using any payday loans or RTOs to begin with.

Table 1.8. Average Marginal Effects of Mandates from Zero-Inflated Poisson Regressions

	N	Inflate Model [Logit]: Coefficient	Count Model [Robust Poisson]: Coefficient	AME
Dependent Variable:				
Auto Title Loans	7,102	-0.152	-0.485**	-0.084*
		(0.414)	(0.210)	(0.050)
Payday Loans	7,099	0.601*	-0.049	-0.177***
		(0.341)	(0.173)	(0.066)
RALs	7,091	0.246	-0.122	-0.080
		(0.275)	(0.151)	(0.075)
Pawn Shop Services	7,088	0.266	-0.009	-0.099
		(0.213)	(0.061)	(0.074)
RTOs	7,112	0.403**	0.080	-0.061
		(0.178)	(0.171)	(0.054)

NOTES: Linearized standard errors are reported in parentheses under coefficients, and delta-method standard errors are reported in parenthesis under AMEs. Each row denotes a separate regression. All regressions control for gender, race/ethnicity, marital status, number of dependent children, income, credit card holding status, age of respondent, and state of residence. ZIP regressions are not available nor appropriate for "Used Any AFS" because it is strictly binary. Average marginal effects (AME) are reported. Complete tables are provided in Supplement 1A. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Comparing Results from State Type Variables Using the RAND American Life Panel

Another potential concern is that respondents' states of current residence may not be a good proxy for respondents' high school states. To assess the sensitivity of state assignments, I use the ALP version of the 2012 NFCS. The ALP contains respondents' high school state as well as their state of current residence. While treatment assignment is more precise in the ALP, the NFCS has

[14] Economic conclusions are also similar across specification types for heterogeneous effects (see Supplement 1B).

more power.[15] The intent in using the ALP is to demonstrate that absolute results change very little according to which state information one uses. This is expected when considering that 76 percent of the sample under age 40 lived in the same state where they attended high school according the ALP subset of the 2012 NFCS.[16] To ensure comparability, I weight regressions with standard errors clustered at the state of residence. I run logit and negative binomial regressions simply on the outcome variables and mandate indicator variable among those with high school information due to small sample sizes.

Table 1.9 shows that the point estimates are nearly identical for all AFS products except for pawn shop services, regardless of which state variable is used. The absolute difference in estimates range from 0 – 3.4 percentage points for all variables except for pawn shop results, whose absolute difference in estimates range from 4.9 – 11.3 percentage points. Yet, the 95 percent confidence intervals overlap across all results. Therefore, the estimates derived from respondents' high school states are not statistically significantly different than the estimates derived from respondents' state of current residence. This means that current state of residence is a good proxy in assigning treatment.

Table 1.9. Average Marginal Effects of Mandates from Logit and Negative Binomial Regressions by State Variation in ALP

	N	State Attended High School			State of Current Residence		
		AME	$\underline{\text{AME}}$	$\overline{\text{AME}}$	AME	$\underline{\text{AME}}$	$\overline{\text{AME}}$
Logit:							
Auto Title Loans	475	0.040	-0.071	0.152	0.032	-0.097	0.161
		(0.055)			(0.064)		
Payday Loans	474	-0.029	-0.115	0.056	-0.029	-0.102	0.044
		(0.043)			(0.036)		
RALs	471	-0.061***	-0.098	-0.023	-0.060***	-0.097	-0.024
		(0.019)			(0.018)		
Pawn Shop Services	475	0.090**	0.002	0.177	0.041	-0.052	0.135
		(0.043)			(0.046)		
RTOs	475	0.016	-0.089	0.121	0.007	-0.113	0.128
		(0.052)			(0.060)		
Negative Binomial:							
Auto Title Loans	475	0.059	-0.177	0.295	0.042	-0.217	0.3
		(0.117)			(0.128)		
Payday Loans	474	0.002	-0.256	0.26	0.004	-0.214	0.222
		(0.128)			(0.108)		
RALs	471	-0.145***	-0.216	-0.075	-0.145***	-0.213	-0.077
		(0.035)			(0.034)		
Pawn Shop Services	475	0.237	-0.066	0.54	0.124	-0.247	0.494
		(0.150)			(0.184)		
RTOs	475	-0.021	-0.135	0.093	-0.055	-0.155	0.044
		(0.056)			(0.049)		

NOTES: Standard errors are reported in parentheses. Each row and primary column denotes a separate regression. Lower bars and upper bars over the subheaders "AME" denote lower bound and upper bounds of the 95% confidence interval, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

[15] The maximum analytic sample size is 7,324 in the NFCS and 683 in the ALP.

[16] Calculated without weights using MS 432 and MS 284.

Excluding Teenagers

While the sensitivity analysis addresses measurement concerns among older adults (since they are more likely to live in a different state than where they grew up), the next set of robustness checks addresses concerns pertaining to the youngest respondents. Consumers must be aged 18 to borrow any credit, including AFS. Including teenage respondents (ages 18 – 19) may impact results because while they are most likely to be required to take personal finance in high school, they do not have a sufficiently large window to use AFS. This indicates that the estimated effects of financial education may be picking up the fact that 18 and 19 years old have not had opportunities to use AFS. Therefore, we should expect to see some decline in magnitude and significance, but the impacts of state-mandated financial education should remain significant if it actually decreases high-cost borrowing.

To examine the extent to which teenagers drive results, I repeat main analyses without the respondents aged 18 and 19. As shown in Table 1.10, financial education mandates continues to particularly impact payday borrowing, with declines in significance.[17] These set of results suggests that respondents who were required to take personal finance in high school were six percentage points less likely to borrow payday loans, and used 0.19 fewer payday loans than their non-mandated counterparts. Overall, these results suggest that financial education may truly explain the declines in payday borrowing.

Table 1.10. Average Marginal Effects of Mandates from Logit and Negative Binomial Regressions When Excluding Respondents Ages 18 and 19

	N	Logit	Negative Binomial
Dependent Variable:			
Used Any AFS	6,704	-0.050	---
		(0.032)	---
Auto Title Loans	6,671	-0.010	-0.047
		(0.031)	(0.074)
Payday Loans	6,671	-0.061*	-0.191**
		(0.030)	(0.080)
RALs	6,664	-0.016	-0.074
		(0.032)	(0.081)
Pawn Shop Services	6,660	-0.034	-0.077
		(0.033)	(0.078)
RTOs	6,682	-0.028	-0.040
		(0.021)	(0.059)

NOTES: Standard errors are reported in parentheses. Each cell denotes a separate regression. Each column denotes the type of specification employed. All regressions control for gender, race/ethnicity, marital status, number of dependent children, income, credit card holding status, age of respondent, and state of residence. Negative binomial regressions are not available nor appropriate for "Used Any AFS" because it is not a count variable. Average marginal effects (AME) are reported. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

[17] Economic conclusions are also similar across estimation results for heterogeneous effects (see Supplement 1C).

Excluding States that Banned Payday Loans as of 2007 versus as of 2012

Another potential concern is that AFS regulations and financial education may be correlated since both policies represent attempts to improve financial behaviors. To assess whether this concern impacts results, I test whether there are differences in my results among states that did or did not ban payday loans.

There is no clear connection between AFS regulation and financial education mandates.[18] As of 2012, at least two-thirds of states that had ever implemented personal finance mandates (fourteen states) permitted payday lending (Urban and Schmeiser 2015; Consumer Federation of America 2017). Of the remaining one-third, all but one state prohibited payday loans before requiring financial education classes in schools. For these states, the lowest time lapse between banning payday lending and requiring high school personal finance courses is five years.[19] Finally, there are eight states that banned payday lending but did not mandate financial education. Three-quarters of these states had never permitted payday lending.

To examine whether state policy on payday loan regulation might bias my results, I first conduct analyses excluding the following states that banned payday loans as of 2007.[20] I consider states that may have banned payday loans after 2007 as "permissive" for these purposes since the survey question asks respondents about AFS use over the past five years. Table 1.11 reveals that the economic conclusions remain and the magnitudes are similar to the main regression results in Table 1.5. Even when excluding states that prohibited payday lending during the entire time period, Table 1.11 reveals that financial education mandates are still associated with reductions in payday lending. However, statistical significance disappears when examining how financial education mandates are associated with the probability of using RTOs. This is likely due to power issues because sample sizes decline when excluding states. Nevertheless, this suggests that financial education may be driving payday loan results.

Second, I conduct analyses excluding the following states that banned payday loans as of 2012, the entire five-year period covered by the NFCS survey question.[21] Table 1.12 reveals that the validity of the previous economic conclusions remain, and the magnitudes are similar to the main regression results in Table 1.5. Even when excluding states that prohibited payday lending anytime up to 2012, financial education mandates are still associated with reductions in payday borrowing. This further suggests that financial education may be driving the declines in payday borrowing.

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[18] For more information, see Supplement 1D.

[19] In absolute terms.

[20] These states are Connecticut, Georgia, Maryland, Massachusetts, New Jersey, New York, North Carolina, Pennsylvania, Vermont, and West Virginia (Consumer Federation of America 2017).

[21] The additional states excluded from analyses are Arizona, Arkansas, District of Columbia, Montana, and New Hampshire (Consumer Federation of America 2017).

Table 1.11. Average Marginal Effects of Mandates from Logit and Negative Binomial Regressions When Excluding States Banning Payday Loans by 2007

	N	Logit	Negative Binomial
Dependent Variable:			
Used Any AFS	5,800	-0.051	---
		(0.044)	---
Auto Title Loans	5,781	-0.039	-0.085
		(0.031)	(0.063)
Payday Loans	5,773	-0.081**	-0.213***
		(0.034)	(0.069)
RALs	5,768	-0.043	-0.106
		(0.028)	(0.070)
Pawn Shop Services	5,762	-0.046	-0.124
		(0.036)	(0.088)
RTOs	5,783	-0.031	0.000
		(0.026)	(0.082)

NOTES: Standard errors are reported in parentheses. Each cell denotes a separate regression. Each column denotes the type of specification employed. All regressions control for gender, race/ethnicity, marital status, number of dependent children, income, credit card holding status, age of respondent, and state of residence. Negative binomial regressions are not available nor appropriate for "Used Any AFS" because it is not a count variable. Average marginal effects (AME) are reported. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.12. Average Marginal Effects of Mandates from Logit and Negative Binomial Regressions When Excluding States Banning Payday Loans by 2012

	N	Logit	Negative Binomial
Dependent Variable:			
Used Any AFS	5,060	-0.049	---
		(0.054)	---
Auto Title Loans	5,042	-0.044	-0.073
		(0.039)	(0.083)
Payday Loans	5,036	-0.076*	-0.157*
		(0.041)	(0.078)
RALs	5,032	-0.046	-0.105
		(0.035)	(0.089)
Pawn Shop Services	5,024	-0.040	-0.075
		(0.047)	(0.111)
RTOs	5,045	-0.027	0.020
		(0.034)	(0.115)

NOTES: Standard errors are reported in parentheses. Each cell denotes a separate regression. Each column denotes the type of specification employed. All regressions control for gender, race/ethnicity, marital status, number of dependent children, income, credit card holding status, age of respondent, and state of residence. Negative binomial regressions are not available nor appropriate for "Used Any AFS" because it is not a count variable. Average marginal effects (AME) are reported. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Limitations

Treatment Assignment is Approximated

As previously mentioned, I approximate treatment assignment using state of residence and age. Roughly 79 percent of all Americans under age 40 resided in the same state that they

attended high school according to the ALP. Other studies quoted 70 – 93.7 percent of their samples residing in the same state where they attended high school (Bernheim, Garrett and Maki 2001, 448; Brown et al. 2016, 2502). These proportions tended to be higher among younger populations. While treatment was not precise, robustness checks ensured that the results change only trivially.

Some misclassification may result from simply adding eighteen years to a respondent's presumed birth year. This misclassification can occur in three ways: 1) Respondent skipped a grade or started school early, 2) Respondent was held back a grade or started school late, or 3) Respondents' birthday is before or after the cut-off date for school enrollment as set by states' compulsory school attendance laws. Previous literature also used age 18 because this is when many students graduate high school.

Additionally, these mandates only apply to public school students in the United States. The NFCS does not contain data about which type of high school a respondent attended (e.g. public, private, or homeschooled) or if a respondent attended a foreign high school. Between school years 1991 – 2012, eight to 9.1 percent of U.S. high school students attended private school (NCES 2016, Table 105.30). Hence it is likely that students attending non-public or foreign high schools were counted as certainly treated when we do not know if they were actually treated.

Identification is Solely Contingent Upon Time-Constant Factors

Given that I only use one wave of the NFCS, my identification strategy addresses time-constant factors at the state level without addressing time-specific shocks. Hence, if financial education mandates passed at the same time as other policies or other factors that could influence AFS use, then the identification assumption that conditional upon controls, all differences in states and when mandates passed are captured in state and age fixed effects is violated. One way that I check for some of this was by excluding states passing payday loan prohibitions. However, there may be more policies or major occurrences that impact AFS use. In a future study, I may analyze the impact of mandates on AFS use with a longitudinal survey or pooled cross-sections. This would allow for time-specific shocks to be captured.

Conclusion

The findings from this work demonstrate that financial education can lower AFS use among younger consumers. Overall, individuals who were mandated to receive financial education in high school were six percentage points less likely to use any AFS than individuals who were not mandated to receive such education (marginally significant). More specifically, young adults mandated to take such courses were seven percentage points less likely to take out payday loans at all. Further, these effects varied by significantly by race/ethnicity and gender. Underrepresented minorities and women were significantly less likely to take out payday loans if they had been in states that mandated financial education.

The effects of these mandates on these economically vulnerable subgroups were rather large. For instance, underrepresented minorities who were required to take personal finance courses in high school were 13 percentage points less likely to use any AFS than underrepresented minorities who were not required to do so. Women who were required to take personal finance courses in high school were seven percentage points less likely to use any AFS than women who were not required to do so.

Some argue that we should inquire if individuals are actually making rational decisions when borrowing high-cost credit. However, if exposure to financial education lowers the likelihood of young consumers using AFS – especially among consumers who are more likely to use AFS – then this suggests there is at least some sub-optimality in using these products.

These findings complement the existing literature in its findings that financial knowledge (an assumed result of financial education) reduces the likelihoods and frequencies of using AFS (e.g. Bertrand and Morse 2011; Lusardi and de Bassa Scheresberg 2013). These results also support the existing literature in its findings that school-based financial education is another form of informational intervention that influences AFS use (e.g. Grimes, Rogers and Smith 2010; Bertrand and Morse 2011; Brown et al. 2014; Brown et al. 2016). Robustness checks augment the existing literature in showing that the mandates are a state policy that reduces payday borrowing independently of payday lending legislation. Finally, this study adds new insights that financial education mandates may heterogeneously impact certain subgroups. In this case, the mandates differentially impact subgroups that are more likely to use AFS (e.g. underrepresented minorities).

This work also has implications for how to best evaluate financial education mandates. In addition to tracking the effects of such mandates on the use of traditional credit (e.g. Brown et al. 2014; Brown et al. 2016; Cole, Paulson and Shastry 2015), evaluations should also track the effects of such mandates on non-traditional sources of credit such as AFS. Failure to do so may underestimate the benefits of school-based financial education; thereby, discourage policymakers from providing financial education in schools. The finding that financial education mandates may have particularly strong positive effects on economically vulnerable young adults has a number of policy-relevant implications. Policymakers in states that already have established financial education mandates may wish to focus additional resources in underserved districts to augment the impact of such mandates. Policymakers in states that permit payday lending but do not have any financial education mandates may want to consider establishing financial education mandates to counter some of the negative social consequences of payday borrowing.[22] Alternatively, they may want to think about how to disseminate information to financially

[22] According to their current state standards or legislations, Alabama, Tennessee, and Utah explicitly cover payday loans, rent-to-own agreements, loan-sharking, and other predatory lending in their high school personal finance courses. In their implementation study, Roberts and Joyce (2016) found that high school students in a large urban school district in IL did not feel comfortable with concepts on payday loans. IL does not explicitly mention covering payday loans in their personal finance course standards.

vulnerable youth that explicitly discuss alternative financial services in tandem with other consumer credit products, and how to assess which credit products to use. This could be collaboratively done with credit unions, local minority-owned banking institutions, social media outlets, or community-based nonprofit organizations.

Supplement 1A. Full Results for AFS Use

Table 1A.1. Average Marginal Effects for All Variables from Logit Regression

VARIABLES	Use Any AFS	Auto Title Loans	Payday Loans	RALs	Pawn Shop Services	RTOs
Mandated personal finance	-0.059*	-0.024	-0.072**	-0.032	-0.044	-0.040**
	(0.033)	(0.029)	(0.030)	(0.030)	(0.030)	(0.019)
Female	-0.078***	-0.065***	-0.059***	-0.067***	-0.066***	-0.066***
	(0.014)	(0.010)	(0.013)	(0.008)	(0.012)	(0.012)
Living with partner	0.094***	0.025	0.058**	0.065**	0.072***	0.034
	(0.031)	(0.019)	(0.026)	(0.025)	(0.026)	(0.022)
Single	-0.009	-0.011	0.005	0.020	0.030*	-0.034*
	(0.020)	(0.014)	(0.020)	(0.017)	(0.017)	(0.018)
Black	0.128***	0.031	0.108***	0.036**	0.049***	0.062***
	(0.021)	(0.021)	(0.025)	(0.017)	(0.017)	(0.019)
Latino	0.081***	0.003	0.045***	0.027*	0.053**	0.014
	(0.018)	(0.016)	(0.014)	(0.015)	(0.021)	(0.015)
Asian	-0.053	0.019	0.023	0.016	-0.062*	-0.016
	(0.038)	(0.026)	(0.031)	(0.021)	(0.031)	(0.019)
Other	0.081*	-0.001	0.060*	0.020	0.043	-0.004
	(0.044)	(0.025)	(0.031)	(0.031)	(0.032)	(0.023)
Income: < $15,000	0.046**	-0.017	-0.025	-0.020	0.083***	0.014
	(0.021)	(0.015)	(0.016)	(0.015)	(0.021)	(0.021)
Income: $15,000 - $50,000	0.107***	0.016	0.029***	0.010	0.099***	0.034***
	(0.014)	(0.014)	(0.011)	(0.010)	(0.016)	(0.012)
Has credit card	-0.035	0.061***	0.042**	0.046***	-0.012	0.003
	(0.022)	(0.014)	(0.017)	(0.015)	(0.019)	(0.015)
Number of dependent children	0.079***	0.045***	0.054***	0.052***	0.053***	0.052***
	(0.007)	(0.004)	(0.005)	(0.005)	(0.007)	(0.006)
Age	-0.007***	-0.006***	-0.006***	-0.005***	-0.007***	-0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
State of current residence	YES	YES	YES	YES	YES	YES
N	7,136	7,102	7,099	7,091	7,088	7,112

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1A.2. Average Marginal Effects for All Variables from Negative Binomial Regression

VARIABLES	Auto Title Loans	Payday Loans	RALs	Pawn Shop Services	RTOs
Mandated personal finance	-0.080	-0.211***	-0.089	-0.111	-0.048
	(0.063)	(0.072)	(0.071)	(0.077)	(0.055)
Female	-0.153***	-0.119***	-0.172***	-0.184***	-0.161***
	(0.022)	(0.032)	(0.032)	(0.032)	(0.029)
Living with partner	0.064	0.231***	0.131**	0.214***	0.056
	(0.047)	(0.075)	(0.061)	(0.069)	(0.047)
Single	-0.024	0.015	0.007	0.034	-0.097**
	(0.029)	(0.043)	(0.040)	(0.042)	(0.037)
Black	0.073	0.326***	0.078*	0.159**	0.144***
	(0.048)	(0.079)	(0.042)	(0.063)	(0.047)
Latino	0.003	0.086*	0.050	0.141**	0.038
	(0.025)	(0.047)	(0.047)	(0.054)	(0.034)
Asian	-0.005	0.038	0.023	-0.205***	-0.044
	(0.046)	(0.081)	(0.051)	(0.072)	(0.052)
Other	0.031	0.144*	-0.005	0.155	0.015
	(0.054)	(0.081)	(0.074)	(0.100)	(0.050)
Income: < $15,000	-0.033	-0.098**	-0.049	0.224***	0.027
	(0.037)	(0.046)	(0.039)	(0.058)	(0.042)
Income: $15,000 - $50,000	0.020	0.088**	0.024	0.278***	0.064**
	(0.030)	(0.034)	(0.031)	(0.034)	(0.028)
Has credit card	0.136***	0.051	0.037	-0.124**	0.007
	(0.032)	(0.049)	(0.042)	(0.057)	(0.035)
Number of dependent children	0.101***	0.151***	0.125***	0.149***	0.121***
	(0.012)	(0.018)	(0.012)	(0.023)	(0.021)
Age	-0.013***	-0.010**	-0.008***	-0.016***	-0.015***
	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)
State of current residence	YES	YES	YES	YES	YES
N	7,102	7,099	7,091	7,088	7,112

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1A.3. Average Marginal Effects for All Variables from Zero-Inflated Poisson Regression

VARIABLES	Auto Title Loans	Payday Loans	RALs	Pawn Shop Services	RTOs
Mandated personal finance	-0.084*	-0.177***	-0.080	-0.099	-0.061
	(0.050)	(0.066)	(0.075)	(0.074)	(0.054)
Female	-0.148***	-0.128***	-0.168***	-0.171***	-0.148***
	(0.018)	(0.034)	(0.026)	(0.031)	(0.023)
Living with partner	0.076	0.177**	0.142**	0.207***	0.077
	(0.050)	(0.068)	(0.059)	(0.063)	(0.050)
Single	-0.013	0.015	0.025	0.037	-0.088**
	(0.028)	(0.047)	(0.039)	(0.041)	(0.037)
Black	0.068	0.266***	0.073*	0.146***	0.140***
	(0.045)	(0.072)	(0.038)	(0.052)	(0.041)
Latino	0.019	0.078*	0.050	0.137**	0.044
	(0.023)	(0.041)	(0.039)	(0.053)	(0.033)
Asian	-0.013	0.039	0.012	-0.171**	-0.039
	(0.044)	(0.072)	(0.046)	(0.070)	(0.055)
Other	0.024	0.139	-0.004	0.110	0.013
	(0.060)	(0.093)	(0.065)	(0.087)	(0.048)
Income: < $15,000	-0.036	-0.073	-0.037	0.196***	0.023
	(0.035)	(0.043)	(0.037)	(0.052)	(0.043)
Income: $15,000 - $50,000	0.012	0.068**	0.013	0.261***	0.054*
	(0.033)	(0.033)	(0.025)	(0.035)	(0.029)
Has credit card	0.137***	0.064	0.083**	-0.088	0.019
	(0.028)	(0.039)	(0.040)	(0.055)	(0.034)
Number of dependent children	0.091***	0.132***	0.115***	0.139***	0.107***
	(0.009)	(0.015)	(0.011)	(0.020)	(0.016)
Age	-0.012***	-0.010**	-0.009***	-0.013***	-0.015***
	(0.002)	(0.004)	(0.002)	(0.003)	(0.003)
State of current residence	YES	YES	YES	YES	YES
N	7,102	7,099	7,091	7,088	7,112

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 1A.4. Full Results for All Variables from Both Models in Zero-Inflated Poisson Regression

	Auto Title Loans		Payday Loans		RALs		Pawn Shop Services		RTOs	
VARIABLES	Inflate	Count	Inflate	Count	Inflate	Count	Inflate	Count	Inflate	Count
Mandated personal finance	-0.152	-0.485**	0.601*	-0.049	0.246	-0.122	0.266	-0.009	0.403**	0.080
	(0.414)	(0.210)	(0.341)	(0.173)	(0.275)	(0.151)	(0.213)	(0.061)	(0.178)	(0.171)
Female	0.489***	-0.265***	0.449***	0.014	0.568***	-0.173	0.378***	-0.055	0.534***	-0.110
	(0.154)	(0.098)	(0.107)	(0.070)	(0.097)	(0.108)	(0.075)	(0.041)	(0.129)	(0.076)
Living with partner	-0.059	0.229	-0.367*	0.116*	-0.540**	0.039	-0.392**	0.078	-0.239	0.042
	(0.183)	(0.152)	(0.185)	(0.066)	(0.219)	(0.121)	(0.164)	(0.077)	(0.184)	(0.103)
Single	0.188	0.080	-0.031	0.014	-0.229	-0.082	-0.219**	-0.075	0.237	-0.133
	(0.145)	(0.084)	(0.157)	(0.054)	(0.159)	(0.072)	(0.109)	(0.045)	(0.174)	(0.095)
Black	-0.238	0.089	-0.766***	0.020	-0.340**	-0.010	-0.264**	0.073	-0.439**	0.108
	(0.215)	(0.128)	(0.162)	(0.089)	(0.158)	(0.083)	(0.099)	(0.064)	(0.174)	(0.112)
Latino	0.066	0.126	-0.398***	-0.094	-0.283*	-0.039	-0.311**	0.031	-0.058	0.105
	(0.240)	(0.136)	(0.118)	(0.087)	(0.141)	(0.075)	(0.137)	(0.062)	(0.158)	(0.108)
Asian	-0.377	-0.319	-0.195	-0.043	-0.222	-0.127	0.388	-0.116	0.140	-0.048
	(0.325)	(0.234)	(0.270)	(0.145)	(0.230)	(0.153)	(0.287)	(0.205)	(0.209)	(0.255)
Other	-0.044	0.066	-0.465**	-0.010	-0.368	-0.293	-0.254	0.025	0.041	0.078
	(0.327)	(0.216)	(0.210)	(0.124)	(0.354)	(0.238)	(0.204)	(0.119)	(0.279)	(0.155)
Income: < $15,000	0.168	-0.031	0.172	-0.071	0.201	0.016	-0.556***	-0.002	-0.172	-0.045
	(0.200)	(0.151)	(0.140)	(0.094)	(0.183)	(0.133)	(0.145)	(0.089)	(0.218)	(0.143)
Income: $15,000 - $50,000	-0.254*	-0.135	-0.240***	-0.014	-0.142	-0.061	-0.586***	0.077	-0.349***	-0.071
	(0.144)	(0.119)	(0.076)	(0.060)	(0.116)	(0.093)	(0.109)	(0.058)	(0.115)	(0.085)
Has credit card	-0.457**	0.325**	-0.387**	-0.124*	-0.493***	-0.068	0.003	-0.147***	0.012	0.073
	(0.211)	(0.147)	(0.152)	(0.064)	(0.161)	(0.098)	(0.113)	(0.049)	(0.156)	(0.109)
Number of dependent children	-0.437***	0.058*	-0.410***	0.022	-0.485***	0.039	-0.306***	0.045**	-0.456***	0.025
	(0.057)	(0.033)	(0.040)	(0.022)	(0.049)	(0.028)	(0.048)	(0.022)	(0.066)	(0.045)
Age	0.060***	-0.007	0.047***	0.011	0.051***	0.006	0.046***	0.008**	0.067***	-0.000
	(0.018)	(0.010)	(0.012)	(0.006)	(0.011)	(0.006)	(0.008)	(0.004)	(0.010)	(0.008)
Constant	0.818***	0.317*	1.288***	0.744***	1.727***	0.909***	0.720***	0.795***	0.495**	0.518***
	(0.276)	(0.178)	(0.214)	(0.118)	(0.207)	(0.134)	(0.132)	(0.085)	(0.222)	(0.157)
State of current residence	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
N	7,102	7,102	7,099	7,099	7,091	7,091	7,088	7,088	7,112	7,112

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Supplement 1B. Estimating Heterogeneous Effects Using Zero-Inflated Poisson Regressions

Tables 1B.1 and 1B.2 show estimations of heterogeneous effects when using zero-inflated Poisson regressions. Note that the average marginal effects generated from the zero-inflated Poisson regressions are similar to those generated from negative binomial models. This suggests that heterogeneous effects are also robust to model specifications.

Table 1B.1. Average Marginal Effects of Mandates by Race/Ethnicity from Negative Binomial versus Zero-Inflated Poisson Regressions

	Auto Title Loans		Payday Loans		RALs		Pawn Shop Services		RTOs	
	NB	ZIP	NB	ZIP	NB	ZIP	NB	ZIP	NB	ZIP
URM:										
Yes	-0.139**	-0.138***	-0.340***	-0.293***	-0.178**	-0.164**	-0.199**	-0.165**	-0.176***	-0.198***
	(0.055)	(0.049)	(0.070)	(0.056)	(0.075)	(0.067)	(0.080)	(0.080)	(0.054)	(0.048)
No	-0.013	-0.042	-0.101	-0.059	0.003	0.020	-0.035	-0.047	0.086	0.086
	(0.080)	(0.048)	(0.101)	(0.119)	(0.080)	(0.091)	(0.089)	(0.083)	(0.067)	(0.060)
N	7,102	7,102	7,099	7,099	7,091	7,091	7,088	7,088	7,112	7,112

NOTES: Reference categories: not mandated. Standard errors are in parentheses. Each secondary column is a separate regression. Average marginal effects were calculated from negative binomial (NB) and zero-inflated Poisson (ZIP) regressions similar to Table 1.7 where race/ethnicity was interacted with mandate indicator. Average marginal effects (AMEs) are reported. *** p < 0.01, ** p < 0.05, * p < 0.1

Table 1B.2. Average Marginal Effects of Mandates by Gender from Negative Binomial versus Zero-Inflated Poisson Regressions

	Auto Title Loans		Payday Loans		RALs		Pawn Shop Services		RTOs	
	NB	ZIP	NB	ZIP	NB	ZIP	NB	ZIP	NB	ZIP
Gender:										
Female	-0.089***	-0.092***	-0.221***	-0.214***	-0.088**	-0.095**	-0.117**	-0.110**	-0.039	-0.082*
	(0.033)	(0.030)	(0.049)	(0.035)	(0.042)	(0.046)	(0.050)	(0.051)	(0.051)	(0.046)
Male	-0.056	-0.067	-0.189	-0.132	-0.078	-0.052	-0.099	-0.085	-0.058	-0.028
	(0.111)	(0.074)	(0.124)	(0.118)	(0.121)	(0.115)	(0.127)	(0.117)	(0.091)	(0.084)
N	7,102	7,102	7,099	7,099	7,091	7,091	7,088	7,088	7,112	7,112

NOTES: Reference categories: not mandated. Standard errors are in parentheses. Each secondary column is a separate regression. Average marginal effects were calculated from negative binomial (NB) and zero-inflated Poisson (ZIP) regressions similar to Table 1.7 where gender was interacted with mandate indicator. Average marginal effects (AMEs) are reported. *** p < 0.01, ** p < 0.05, * p < 0.1

Supplement 1C. Estimating Heterogeneous Effects Excluding Respondents Aged 18 and 19

Tables 1C.1 and 1C.2 show estimations of heterogeneous effects when excluding respondents ages 18 and 19. Note that the average marginal effects generated when excluding respondents aged 18 and 19 are similar to those generated in Table 1.6. This suggests that heterogeneous effects are also robust to age restrictions.

Table 1C.1. Average Marginal Effects of Mandates by Race/Ethnicity When Excluding Teenagers

	Auto Title Loans		Payday Loans		RALs		Pawn Shop Services		RTOs	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
URM:										
Yes	-0.056**	-0.049**	-0.120***	-0.112***	-0.075***	-0.071***	-0.081**	-0.076**	-0.102***	-0.103***
	(0.024)	(0.023)	(0.030)	(0.028)	(0.025)	(0.025)	(0.031)	(0.035)	(0.016)	(0.018)
No	0.013	0.039	-0.026	-0.007	0.016	0.048	-0.004	0.011	0.031	0.063*
	(0.047)	(0.056)	(0.044)	(0.048)	(0.041)	(0.044)	(0.040)	(0.043)	(0.027)	(0.033)
N	7,102	6,671	7,099	6,671	7,091	6,664	7,088	6,660	7,112	6,682

NOTES: Reference categories: not mandated. Standard errors are in parentheses. Each secondary column is a separate regression. Average marginal effects were calculated from logit and negative binomial (NB) regressions similar to Table 1.6 where race/ethnicity was interacted with mandate indicator. (1) denotes results from Table 1.6, and (2) denotes results from excluding teenagers. Average marginal effects (AMEs) are reported. *** p < 0.01, ** p < 0.05, * p < 0.1

Table 1C.2. Average Marginal Effects of Mandates by Gender When Excluding Teenagers

	Auto Title Loans		Payday Loans		RALs		Pawn Shop Services		RTOs	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Gender:										
Female	-0.038**	-0.036**	-0.086***	-0.081***	-0.034*	-0.034*	-0.047*	-0.041*	-0.048**	-0.050***
	(0.016)	(0.016)	(0.017)	(0.017)	(0.018)	(0.019)	(0.024)	(0.025)	(0.019)	(0.016)
Male	-0.008	0.020	-0.056	-0.036	-0.028	0.005	-0.040	-0.026	-0.030	-0.002
	(0.049)	(0.054)	(0.049)	(0.052)	(0.044)	(0.047)	(0.043)	(0.048)	(0.031)	(0.036)
N	7,102	6,671	7,099	6,671	7,091	6,664	7,088	6,660	7,112	6,682

NOTES: Reference categories: not mandated. Standard errors are in parentheses. Each secondary column is a separate regression. Average marginal effects were calculated from logit and negative binomial (NB) regressions similar to Table 1.6 where gender was interacted with mandate indicator. (1) denotes results from Table 1.6, and (2) denotes results from excluding teenagers. Average marginal effects (AMEs) are reported. *** p < 0.01, ** p < 0.05, * p < 0.1

Supplement 1D. Table of State Characteristics in Respects to Payday Lending Prohibitions and Financial Education Mandates

State	Personal Finance Implemented	Discusses AFS in Courses	Year Payday Loans Banned
Arizona	2005		2010
Arkansas	2005		2011
Colorado	2009		
Connecticut			A
District of Columbia			2008
Georgia	2007		A
Idaho	2007		
Illinois	1970		
Iowa	2011		
Kansas	2012		
Louisiana	2005		
Maryland			A
Massachusetts			A
Michigan	1998		
Missouri	2010		
Montana			2011
New Hampshire	1993		2009
New Jersey	2011		A
New York	1996		A
North Carolina	2007		2001
Pennsylvania			A
South Carolina	2009		
South Dakota	2006		
Tennessee	2011	Yes	
Texas	2007		
Utah	2008	Yes	
Vermont			A
West Virginia			A
Wyoming	2002		

NOTES: “A” means that the state never permitted payday loans. As of 2012.
SOURCES: Urban and Schmeiser (2015); and Consumer Federation of America (2017)

2. State-Mandated Financial Education and College Students' Postsecondary Decisions

Pursuing postsecondary education is often the first yet most consequential financial decision that most young adults make and they are increasingly relying on debt to pay for it. Total student loan debt in the United States is an estimated $1.5 trillion (Board of Governors 2018). 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.[23] 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 interest. However, only one-third of college students can correctly calculate how much time it takes for a $1,000 loan compounded at 20 percent per year to double if they did not pay anything off. Consumers need to understand what interest rates are and how compound interest works to effectively borrow loans.

In addition to lack of financial knowledge, poor selection of colleges may also contribute to high student loan debt and default rates. 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.[24] As 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. These benefits will accumulate over time. Hence, students should select the institution whose benefits will ultimately exceed costs. In higher education, the benefits (discounted present value of the increment to lifetime earnings) should exceed the total cost of attendance. However, students with less financial knowledge may be less capable of comparing the costs and benefits of colleges. In particular, high student loan default rates at a particular college may suggest that the benefits of the school do not outweigh the costs. Thus, financial knowledge may influence both borrowing decisions and institution selection.

One 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

[23] To review these questions, refer to Supplement 2A.

[24] This author acknowledges that higher education is an investment good and a consumption good. However, the investment perspective is more important among economically disadvantaged students than the consumption perspective. Students presumably also care about the characteristics of the jobs but the goal of the paper is to explore the financial implications of college decisions.

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 from their parent(s) or guardian(s).[25] Another 38 percent of college students do not learn about finances from home or school.[26]

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).[27] 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).

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[25] According to author's calculations of the 2015 NFCS.

[26] Ibid.

[27] According to their state standards or legislation, 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).

To examine impacts of personal finance requirements 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 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 state-mandated financial education 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 how information affects college choice, how students finance college, 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 and specifies how robust these results are. The limitations section discusses data issues, and implications for current and future analyses. The concluding section highlights key findings and their 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.

Postsecondary Institutional Choice

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; Dillon and Smith 2017). Reasons for this phenomenon include that the less selective institution is closer to home; caters to students' religion or culture; caters to students whom had less academic preparation; 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 hypothesis 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.[28] 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.

Postsecondary Financing Behaviors

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).[29] 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.

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

[29] According to NCES 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).

Economically disadvantaged students not completing this form then may be more prone to use sub-optimal options to fill the gap.

In regard 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 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).[30] These findings are not surprising given differences between household income and family wealth between disadvantaged and non-disadvantaged students, but it is also not clear to what extent this is due to not pursuing or not knowing about grants and scholarships. My paper will enhance understanding of which students are more likely to borrow student loans by assessing if exposure to financial education differentially impacts subgroups that are already more likely to borrow student loans due to fewer family resources.

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,

[30] 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."

and provide the most flexible options.[31] Greater use of federal student loans suggests lesser use of private student loans or credit card debt.[32]

In this paper, I build upon this second strand of literature by examining if school-based financial education can influence the likelihood that students fill out the FAFSA. Students cannot access any federal student aid – or state student 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.

Impacts of State-Mandated Financial Education on Young Adults' Behaviors

The third strand 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, Copur and Garrison (2011) examine the relationship between high school financial education and 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 willingness 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 no associations while Gutter, Copur and Garrison (2011) find positive associations. 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, Copur and Garrison (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, Copur and Garrison (2011) find that financial education is positively correlated with general financial behaviors may translate to specific college financing behaviors. In fact, Mandell and Klein (2009) find that when the 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. This

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[31] 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).

[32] 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.

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.[33] Yet, we currently do not know if formal classroom-based financial education may help students choose less risky institutions or less costly financing. This effect may be through the explicit mechanism of discussing financial aid with students, or though 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, enroll in institutions whose former students are less likely to default, 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 more than half-time while enrolled). Finally, this paper takes special care to explore socio-economic heterogeneity to assess impacts of formal financial education on groups that may rely on it more. Results from this study 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.

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

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

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.[34]

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 level differences across cohorts and states. States introduce and implement these mandates at different years. Difference-in-differences are causal if the trends of the studied outcomes between states with mandates and states without mandates are equivalent 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. I assume that high school financial education mandates are exogenous to students. Balance tests suggest that mandates are plausibly exogeneous to students; refer to Supplement 2B. The key here is that states are not introducing mandates due to either a substantial increase or a substantial decline in college-related behaviors. Note that all public high school students are required to take the course to graduate, regardless of any preferences.

The outcomes I examine and reasons I choose to study them are listed in Table 2.1. These outcomes 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 careers, and are made close to mandate exposure. I intend to study outcomes that clearly constitute financing mistakes or sub-optimal decision-making.

Table 2.1. List of College Choice and Postsecondary Financing Outcomes

Outcome	Financial Rationale
Postsecondary Institutional Choice:	
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.
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.

[34] College selection occurs prior to matriculation. Students may transfer, but high school courses are unlikely to drive this. Similarly, outcomes occurring beyond freshman year may be because of other policies or programs instead of high school courses.

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.
Postsecondary Financing Behaviors:	
Filed 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.
Used 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.
Used 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 the outcomes and the appropriate functional form vary (see Table 2.2). 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 observed postsecondary outcome of student i in entering cohort year c from high school state s. These estimated postsecondary outcomes are listed in Table 2.1, and fall under two major headings: postsecondary institutional choice and postsecondary financing behaviors.

The independent variable of interest M_{cs} 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 they were 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.[35]

[35] 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 alter 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,

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 estimation 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 outcomes except for institutional level. γ_c is the entering cohort year fixed effect.[36] 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.[37]

Table 2.2. Regression Models Used to Estimate Dependent Variables

Regression Model	Dependent Variable	
	Postsecondary Institutional 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

it is reasonable to assume that state of legal residence is where a student attended high school among most students ages 23 and younger.

[36] 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.

[37] More information on how the data was handled and regression justifications are in Supplement 2D.

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.[38] 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 requires all students to take personal finance as a core prerequisite for graduation.[39]

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 high school advisors with comparative, comprehensive information about institutions so that they can assess prospective institutions' costs and benefits. 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

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[38] Their dataset does not include District of Columbia – but CEE (1998; 2000; 2002; 2004; 2007; 2009; 2011; and 2014) and Bernheim, Garrett and Maki (2001) reveal that D.C. has never implemented any personal finance mandates.

[39] I do not consider the policy variation that requires all public high schools to offer personal finance as an elective course because this would introduce endogeneity into the model. Students in these states who elect to take these courses are more likely to be interested in personal finance – or value financial preparation more highly – at the outset than students who did not take the course.

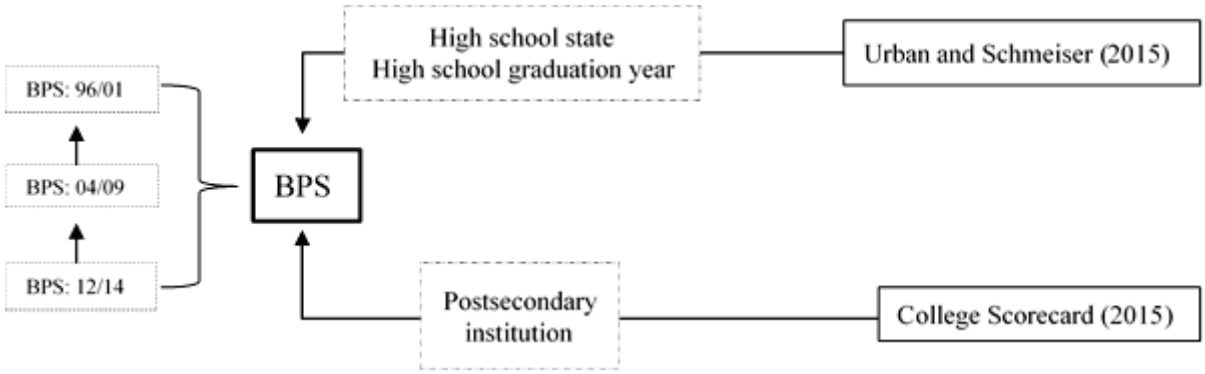
three-year CDR is the percent of students who entered loan repayment in a given year and defaulted within three years of starting repayment. OPD calculates the three-year CDRs, which determine an institution's eligibility to participate in the federal financial aid program. Institutions with CDRs of 30 percent or higher in the last three consecutive fiscal years, or with a CDR higher than 40 percent in the most recent fiscal year 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. Since the sample consists of individuals who attend college, all analyses are conditional on enrollment.

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.[40] I then merge the FY 2012 data from the College Scorecard dataset to the BPS using a student's freshman-year postsecondary institution. Figure 2.1 illustrates how I handle the data.

[40] 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.

Figure 2.1. Sketch of Data Appending and Merging



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.
- *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.[41]
- *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 postsecondary 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

[41] 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.

institutions. Therefore, to keep the sample population consistent, I exclude students in the 1996 cohort who attended any non-Title IV institution from analyses.

- *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.

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 considered manageable. However, it expands up to 29 percent for some students. The median annual earnings among former students from attended institutions ranges from $11,500 to $250,000. 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.[42] Subsequently, 44 percent of students accessed Pell grants, and sampled

[42] I discuss plausible reasons for this in Study Limitations.

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 2.3. 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
Demographics[b]					
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
Institutional Characteristics and Students' Enrollment Intensity[b]					
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
Institutional-Level Outcomes of Former Students[c]					
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
Postsecondary Financing[b]					
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 category is in a 0.5 increment. Its mode is "B to A – ", which takes a value of "6." Statistics are unweighted.

Main Findings

I initially examine the impact of mandates on institutional level selection.[43] 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

[43] I combined two-year institutions and less-than-two-year institutions into one category. I refer to this category as "two-year institutions" throughout the paper.

financial aid use, and employment while enrolled. I present overall results, and then heterogeneous results. All results reported herein are conditional upon matriculating because the sample consists of freshmen who successfully enrolled in college.

Table 2.4 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 less likely to attend a four-year institution, 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 suggest that they may be making decisions that cater to their immediate circumstances.

Table 2.4. 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	-0.042**	0.038***
	(0.039)	(0.020)	(0.013)
First-Generation College Student	-0.092***	0.072***	-0.010
	(0.009)	(0.004)	(0.007)
Per Member Household Income (in 1000s)	0.003***	-0.003***	-0.001***
	(0.000)	(0.000)	(0.000)
Independent	-0.093***	0.139***	-0.035***
	(0.015)	(0.009)	(0.011)
Underrepresented Minority	0.008	0.071***	-0.016**
	(0.010)	(0.008)	(0.007)
Female	-0.013**	0.014***	0.016***
	(0.006)	(0.005)	(0.004)
High School GPA: D to C-	0.022	0.117**	0.056
	(0.068)	(0.050)	(0.074)
High School GPA: C- to C	-0.027	0.127**	0.076
	(0.061)	(0.052)	(0.065)
High School GPA: C to B-	0.028	0.098*	0.076
	(0.063)	(0.050)	(0.065)
High School GPA: B- to B	0.108	0.043	0.106
	(0.067)	(0.050)	(0.068)

High School GPA: B to A-	0.220***	0.013	0.124*
	(0.068)	(0.051)	(0.067)
High School GPA: A- to A	0.377***	-0.055	0.170**
	(0.068)	(0.050)	(0.067)
Attends Four-Year Institution	---	0.058***	0.150***
	---	(0.018)	(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

Table 2.5 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.

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 more student loans on average. Later, I explore if mandates may particularly impact disadvantaged students' enrollments into less risky institutions.

Table 2.5. 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*	0.013	-0.001	-0.016
	(0.005)	(0.013)	(0.001)	(0.022)
First-Generation	0.012***	-0.034***	0.003***	0.030***
	(0.001)	(0.003)	(0.000)	(0.010)

Per Member Household Income (in 1000s)	-0.001***	0.001***	-0.000***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Independent	0.022***	-0.036***	0.002*	0.003
	(0.002)	(0.005)	(0.001)	(0.016)
Underrepresented Minority	0.016***	-0.032***	0.006***	0.029**
	(0.003)	(0.005)	(0.001)	(0.012)
Female	-0.002	-0.048***	0.002***	0.050***
	(0.002)	(0.004)	(0.000)	(0.009)
High School GPA: D to C-	0.034***	-0.050**	0.009*	0.080
	(0.011)	(0.023)	(0.005)	(0.088)
High School GPA: C- to C	0.030**	-0.044**	0.009*	0.083
	(0.012)	(0.021)	(0.005)	(0.082)
High School GPA: C to B-	0.023**	-0.038*	0.009*	0.098
	(0.011)	(0.021)	(0.005)	(0.084)
High School GPA: B- to B	0.013	-0.020	0.006	0.078
	(0.012)	(0.022)	(0.005)	(0.084)
High School GPA: B to A-	0.002	0.008	0.005	0.066
	(0.012)	(0.022)	(0.005)	(0.086)
High School GPA: A- to A	-0.022*	0.090***	-0.002	-0.042
	(0.012)	(0.024)	(0.005)	(0.087)
Attends Four-Year Institution	-0.046***	0.296***	0.034***	0.572***
	(0.007)	(0.016)	(0.002)	(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 2.6 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.[44] 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 borrow five percent less in federal student loans than borrowers not subjected to the mandate (marginally significant). This may be because mandated students are less likely to use unsubsidized student loans, and borrow smaller amounts of

[44] 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 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).

unsubsidized student loans when choosing to do so (marginally significant). 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 status remain. First-generation and lower-income students are 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 less likely to fill out a FAFSA, less likely to use Pell grants, but more likely to use federal student loans. While they are more likely to use both types of loans, they borrow smaller 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 2.4.

Table 2.6. 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**	0.021	0.011	0.011	-0.029*	-0.060*	0.017	-0.048*
	(0.015)	(0.020)	(0.023)	(0.023)	(0.016)	(0.033)	(0.016)	(0.025)
First-Generation	0.067***	0.047***	0.047***	-0.024***	0.035***	-0.005	0.053***	0.001
	(0.006)	(0.006)	(0.008)	(0.009)	(0.005)	(0.015)	(0.007)	(0.010)
Per Member Household Income (in 1000s)	-0.003***	-0.015***	0.000	0.001	-0.000	0.007***	-0.004***	-0.000
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)
Independent	-0.029**	-0.053***	0.071***	-0.083***	0.119***	0.308***	0.047***	0.170***
	(0.014)	(0.012)	(0.012)	(0.013)	(0.015)	(0.022)	(0.015)	(0.018)
Underrepresented Minority	0.082***	0.088***	0.087***	-0.033***	0.069***	0.103***	0.070***	0.055***
	(0.007)	(0.007)	(0.017)	(0.012)	(0.014)	(0.016)	(0.017)	(0.011)
Female	0.033***	0.043***	0.042***	0.014	0.034***	-0.015	0.037***	0.007
	(0.005)	(0.005)	(0.007)	(0.010)	(0.005)	(0.013)	(0.006)	(0.006)
High School GPA: D to C-	0.135*	0.044	0.166**	0.205	0.111	0.040	0.168***	0.064
	(0.078)	(0.085)	(0.082)	(0.185)	(0.072)	(0.225)	(0.063)	(0.137)
High School GPA: C- to C	0.083	0.013	0.135**	0.257	0.124*	0.186	0.161***	0.167
	(0.070)	(0.080)	(0.069)	(0.188)	(0.070)	(0.223)	(0.057)	(0.134)
High School GPA: C to B-	0.103	0.019	0.117*	0.241	0.094	0.201	0.130**	0.163
	(0.069)	(0.080)	(0.066)	(0.184)	(0.069)	(0.230)	(0.055)	(0.138)
High School GPA: B- to B	0.100	0.030	0.108	0.269	0.075	0.200	0.115**	0.170
	(0.072)	(0.085)	(0.068)	(0.185)	(0.069)	(0.228)	(0.056)	(0.135)
High School GPA: B to A-	0.107	0.018	0.102	0.271	0.061	0.189	0.103*	0.157
	(0.072)	(0.083)	(0.065)	(0.183)	(0.073)	(0.236)	(0.059)	(0.138)
High School GPA: A- to A	0.122*	-0.010	0.029	0.238	0.003	0.165	0.044	0.106
	(0.072)	(0.082)	(0.062)	(0.183)	(0.071)	(0.231)	(0.055)	(0.135)
Attends Four-Year Institution	0.135***	0.093***	0.367***	0.086***	0.231***	0.084***	0.340***	0.116***
	(0.010)	(0.016)	(0.021)	(0.015)	(0.015)	(0.021)	(0.017)	(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 2.7 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 five percentage points less likely to work 20 hours or more per week while enrolled than students who did not (marginally significant).

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) (Engle, Bermeo and O'Brien 2006).

Table 2.7. 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	-0.898**	-0.046*
	(0.015)	(0.451)	(0.024)
First-Generation	0.017*	1.073***	0.060***
	(0.009)	(0.226)	(0.008)
Per Member Household Income (in 1000s)	-0.001***	-0.032***	-0.001***
	(0.000)	(0.006)	(0.000)
Independent	-0.033**	1.016***	0.111***
	(0.013)	(0.362)	(0.014)
Underrepresented Minority	-0.055***	-0.972***	0.027***
	(0.008)	(0.201)	(0.008)
Female	0.024***	-0.277	-0.033***
	(0.008)	(0.220)	(0.008)
High School GPA: D to C-	-0.012	0.384	0.062
	(0.062)	(1.325)	(0.073)
High School GPA: C- to C	0.008	1.387	0.007
	(0.072)	(1.479)	(0.069)
High School GPA: C to B-	0.031	1.547	0.012
	(0.073)	(1.563)	(0.064)
High School GPA: B- to B	0.051	1.685	-0.020
	(0.072)	(1.593)	(0.070)
High School GPA: B to A-	0.048	1.253	-0.055
	(0.070)	(1.528)	(0.072)
High School GPA: A- to A	-0.000	-0.967	-0.148**

	(0.071)	(1.610)	(0.070)
Attends Four-Year Institution	-0.155***	-6.201***	-0.201***
	(0.011)	(0.371)	(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

Heterogeneous Effects by Economically Disadvantaged Subgroups

Financial education mandates may disproportionally 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. In this section, I consider whether economically disadvantaged students are differentially affected by personal finance mandates.

Consistent with previous literature, Table 2.8 reveals that disadvantaged students borrow statistically significantly more Stafford loans, accrue statistically significantly higher amounts in credit card debt, and work significantly more hours per week while enrolled. These higher amounts of debt are considerable when realizing that statistically 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; Reynolds 2012).[45]

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 disadvantaged students may have fewer resources 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.

[45] Melguizo, Kienzl and Alfonso (2011) do not find statistically 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 2.8. Descriptive Statistics by Economically Disadvantaged Subgroup

VARIABLES	Later-Generation (N = 21,010)	First-Generation (N = 9,600)	Pell-Ineligible (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. Each economically disadvantaged subgroup tends to have fewer resources to help pay for college, or to consult about postsecondary education. As seen in Table 2.9, approximately sixty percent of all students in the sample fall into at least one of these groups. I review results for first-generation college students and Pell-eligible students because their different definitions will frame how public policies should focus efforts.[46] However, Table 2.9 reveals that these categories are not mutually exclusive.

Table 2.9. Percentage of College Students Classified as Economically Disadvantaged

		Number of Economically Disadvantaged Categories			
Cohort	**Total Students**	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.					

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:

- Their parents did not attend college. Therefore, their parents cannot guide them through selecting an institution, applying for financial aid, or in how to best pay for college.
- 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

[46] 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' cost of attendance (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.

overall college population.[47] 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.[48] Financial aid policy does not expect independent students to have parental resources to pay for college.

Pell-Eligible Students

Pell-eligible students come from families whose annual household income is $50,000 or less.[49] 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 approximate Pell eligibility according to the 95th percentile because I cannot fully implement the Pell eligibility rules in the data.

Results

First, I examine heterogeneous effects of financial education mandates on attending four-year institutions, attending for-profit institutions, and enrolling full-time. Table 2.10 shows heterogeneous effects of the mandate on institutional enrollment by first-generation status and by Pell eligibility. Mandates have no impact on institutional level selection for either subgroup. Among first-generation students, mandate exposure increased the probability of enrolling full-time by three percentage points. Among later-generation students, however, mandate exposure significantly increased the likelihood of enrolling full-time by four percentage points. A similar trend persists when examining differential effects on full-time enrollment by Pell eligibility.

Mandates equally decrease the probability of attending for-profit institutions for all students. While there are some differential effects when looking within subgroups, the mandates do not have statistically significantly different impacts on enrollment by institutional characteristics across subgroups.

Table 2.10. 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	-0.042**	0.038***
	(0.039)	(0.020)	(0.013)

[47] 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.

[48] Ibid.

[49] 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.

A. Interactions by First-Generation Status			
Later-Generation	-0.045	-0.040**	0.042***
	(0.039)	(0.019)	(0.013)
First-Generation	-0.007	-0.048**	0.030*
	(0.045)	(0.024)	(0.018)
Difference	-0.037*	0.008	0.011
	(0.021)	(0.011)	(0.014)
N	29,500	29,550	29,550
B. Interactions by Pell Eligibility			
Pell-Ineligible	-0.047	-0.052**	0.043***
	(0.040)	(0.020)	(0.016)
Pell-Eligible	-0.020	-0.039*	0.033**
	(0.043)	(0.021)	(0.015)
Difference	-0.027	-0.013	0.011
	(0.021)	(0.010)	(0.016)
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. "Difference" is the difference between the non-disadvantaged subgroup and the disadvantaged group. Overall effects reported from Table 2.4. *** 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 2.11, 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 exposed to the mandate. When considering the null findings for higher median earnings, this result may be because students exposed to the mandate are borrowing less. Among later-generation and higher-income students, mandate exposure led to enrolling in institutions with lower CDRs. Surprisingly, mandates have no significant 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.

The only outcome where mandates have a differential effect between disadvantaged students and non-disadvantaged students when it comes to institution selection based on outcomes for former students is debt-to-income ratio (above sample median). This suggests that mandates primarily improve debt-to-income ratios for first-generation and Pell-eligible students.

Table 2.11. 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*	0.013	-0.001	-0.016
	(0.005)	(0.013)	(0.001)	(0.022)
A. Interactions by First-Generation Status				
Later-Generation	-0.009**	0.014	-0.000	-0.004

	(0.004)	(0.013)	(0.001)	(0.023)
First-Generation	-0.009	0.012	-0.002	-0.045**
	(0.006)	(0.016)	(0.002)	(0.023)
Difference	0.001	0.002	0.001	0.041**
	(0.003)	(0.009)	(0.001)	(0.017)
N	27,910	27,980	26,520	26,520
		B. Interactions by Pell Eligibility		
Pell-Ineligible	-0.011**	0.016	-0.001	-0.001
	(0.004)	(0.013)	(0.001)	(0.024)
Pell-Eligible	-0.008	0.012	-0.001	-0.029
	(0.005)	(0.015)	(0.002)	(0.022)
Difference	-0.003	0.004	0.0004	0.028**
	(0.003)	(0.008)	(0.001)	(0.013)
N	27,890	27,960	26,500	26,500

NOTES: Standard errors in parentheses. Reference category: not mandated. 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 where first-generation status or income is interacted with the mandate indicator. Overall effects reported from Table 2.5. *** 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 2.12). 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 smaller 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. The differential effects of mandates on later-generation versus first-generation students when it comes to completing a FAFSA, using any Pell grants, and borrowing any student loans are statistically significant.

First-generation college students under the mandate are five percentage points less likely to use any unsubsidized loans, and borrow smaller amounts in unsubsidized loans when doing so (marginally significant). Mandates have a significantly stronger effect on first-generation college students than later-generation college students when it comes to reduced likelihoods of borrowing any unsubsidized loans.

Similar trends persist among Pell-eligible students, but in a slightly different way. Pell-eligible students are three percentage points less likely to use any unsubsidized loans (marginally significant), 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 2.12. 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**	0.021	0.011	0.011	-0.029*	-0.060*	0.017	-0.048*
	(0.015)	(0.020)	(0.023)	(0.023)	(0.016)	(0.033)	(0.016)	(0.025)
A. Interactions by First-Generation Status								
Later-Generation	0.046***	0.040**	0.028	0.022	-0.021	-0.048	0.037**	-0.042*
	(0.016)	(0.020)	(0.025)	(0.018)	(0.017)	(0.032)	(0.016)	(0.022)
First-Generation	0.008	-0.006	-0.011	-0.002	-0.048***	-0.083*	-0.025	-0.061*
	(0.015)	(0.021)	(0.022)	(0.032)	(0.016)	(0.043)	(0.020)	(0.034)
Difference	0.038***	0.046***	0.039**	0.023	0.027**	0.034	0.062***	0.020
	(0.009)	(0.008)	(0.016)	(0.021)	(0.011)	(0.030)	(0.012)	(0.023)
N	24,190	15,350	15,350	8,310	29,550	10,480	29,550	15,190
B. Interactions by Pell Eligibility								
Pell-Ineligible	0.048***	---	---	----	-0.033	-0.022	0.057***	-0.044*
	(0.017)				(0.026)	(0.030)	(0.019)	(0.023)
Pell-Eligible	0.019	---	---	---	-0.025*	-0.086**	-0.018	-0.051*
	(0.014)				(0.015)	(0.039)	(0.023)	(0.028)
Difference	0.029**	---	---	---	-0.007	0.064*	0.075***	0.008
	(0.012)				(0.028)	(0.033)	(0.023)	(0.019)
N	24,170	---	---	---	29,530	10,480	29,530	15,190

NOTES: Standard errors in parentheses. Each column 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. "Difference" is the difference between the non-disadvantaged subgroup and the disadvantaged group. ‡ denotes conditional on not attending a for-profit institution. † denotes conditional on Pell eligibility. Overall effects reported from Table 2.6. *** p<0.01, ** p<0.05, * p<0.1

Last, I examine heterogeneous effects of financial education mandates on employment while enrolled. Table 2.13 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 statistically equally reduce the probability of working at least 20 hours per week for all employed students except for Pell-eligible students.

Table 2.13. 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	-0.898**	-0.046*
	(0.015)	(0.451)	(0.024)
		A. Interactions by First-Generation Status	
Later-Generation	-0.010	-0.619	-0.040*
	(0.015)	(0.408)	(0.023)
First-Generation	-0.038*	-1.435**	-0.060**
	(0.022)	(0.682)	(0.029)
Difference	0.029	0.816	0.020
	(0.021)	(0.541)	(0.015)
N	29,450	29,450	14,040
		B. Interactions by Pell Eligibility	
Pell-Ineligible	-0.002	-0.650	-0.059**
	(0.019)	(0.527)	(0.024)
Pell-Eligible	-0.033**	-1.082**	-0.036
	(0.016)	(0.502)	(0.025)
Difference	0.032*	0.432	-0.023*
	(0.019)	(0.499)	(0.012)
N	29,430	29,430	14,040

NOTES: Standard errors in parentheses. Reference category: not mandated. Each column 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. "Difference" is the difference between the non-disadvantaged subgroup and the disadvantaged group. Overall effects reported from Table 2.7 *** p<0.01, ** p<0.05, * p<0.1

Robustness Checks

I conduct three robustness checks. First, I visually assess pre-trends. Second, I alternate the mandate specification to vary by pre-treatment and post-treatment. Third, I conduct a falsification test where I arbitrarily set years of mandate implementation five years back. I

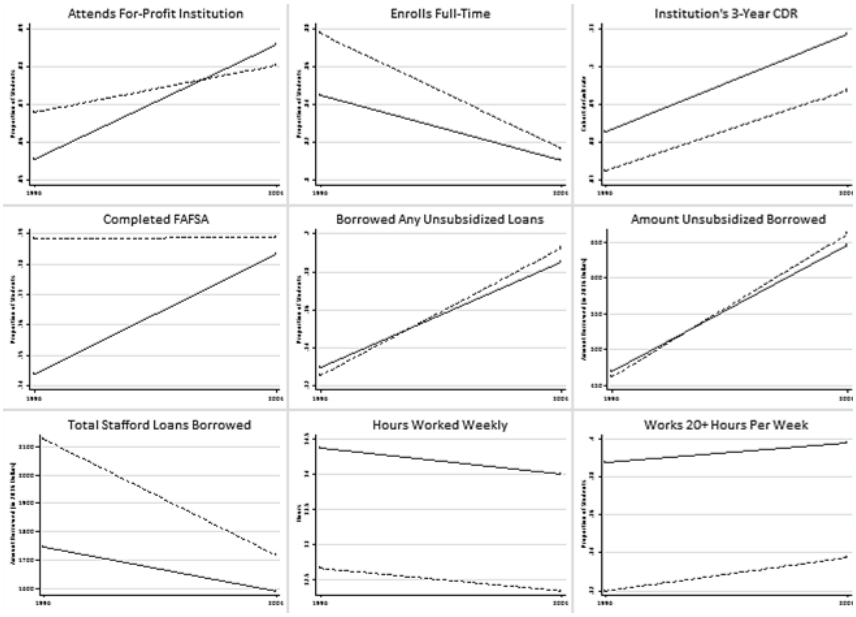
perform robustness checks among outcomes where financial education mandates had any overall effect in the main results.[50]

Visual Representation of Pre-Trends Among the 2012 Entering Cohort

Figure 2.2 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 2.2 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 2.2. 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. Solid lines represent states that began implementing personal finance courses any time after 2004; dashed lines represent control states.

[50] 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.

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.[51] 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 2.14 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 2.14 in the "Post – Pre" row.

I find that the lead treatment dummy variables are not jointly statistically different from 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 the 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.

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

Table 2.14. 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	-0.035*	0.002	-0.108***	0.012	-0.031	0.022	0.036	1.095*
	(0.046)	(0.019)	(0.009)	(0.038)	(0.023)	(0.059)	(0.056)	(0.039)	(0.650)
-6 Years	0.106	-0.075	0.014	-0.063	0.022	-0.162**	-0.013	0.085	1.447
	(0.082)	(0.046)	(0.017)	(0.045)	(0.046)	(0.077)	(0.098)	(0.074)	(1.441)
-5 Years	0.142**	-0.086***	0.019	-0.203***	0.033	-0.141**	0.016	0.042	1.762
	(0.067)	(0.026)	(0.013)	(0.057)	(0.031)	(0.068)	(0.075)	(0.068)	(1.496)
-4 Years	0.044	-0.008	0.014	-0.069	-0.018	-0.031	-0.007	-0.040	-1.137*
	(0.067)	(0.024)	(0.012)	(0.049)	(0.034)	(0.061)	(0.060)	(0.045)	(0.619)
-3 Years	-0.081**	-0.001	-0.001	-0.035	-0.122***	-0.109**	-0.033	0.015	0.315
	(0.035)	(0.018)	(0.009)	(0.038)	(0.047)	(0.050)	(0.055)	(0.043)	(0.708)
-2 Years	0.054	0.021	-0.004	-0.092	-0.009	0.098	0.119*	-0.045	0.305
	(0.083)	(0.025)	(0.010)	(0.058)	(0.027)	(0.068)	(0.062)	(0.053)	(0.741)
-1 Year	0.086	-0.029	0.008	-0.028	-0.038	0.148**	0.026	-0.051	-0.893
	(0.076)	(0.022)	(0.012)	(0.048)	(0.042)	(0.059)	(0.072)	(0.078)	(1.035)
1 Year	0.045	-0.043**	0.002	-0.026	-0.002	-0.008	0.008	-0.029	1.310
	(0.052)	(0.019)	(0.010)	(0.040)	(0.040)	(0.057)	(0.057)	(0.055)	(1.252)
2 Years	0.058	0.006	0.003	-0.030	-0.041	-0.065	-0.004	-0.029	0.188
	(0.067)	(0.033)	(0.010)	(0.049)	(0.035)	(0.058)	(0.058)	(0.065)	(0.759)
3 Years	0.081	-0.006	0.007	-0.060	-0.021	-0.085	0.003	-0.052	-1.647
	(0.063)	(0.035)	(0.011)	(0.044)	(0.050)	(0.093)	(0.060)	(0.070)	(1.033)
4 Years	-0.051	0.037**	-0.008	-0.022	-0.080***	-0.143**	-0.068	-0.105**	-1.636***
	(0.048)	(0.015)	(0.008)	(0.042)	(0.030)	(0.071)	(0.073)	(0.051)	(0.507)
5 Years	0.064	0.052*	0.005	-0.078*	-0.032	-0.083	-0.013	-0.065	-0.831
	(0.066)	(0.029)	(0.010)	(0.046)	(0.042)	(0.080)	(0.058)	(0.050)	(0.876)
6 Years	-0.027	0.029	-0.008	-0.022	-0.085***	-0.089	-0.014	-0.005	-0.577
	(0.048)	(0.022)	(0.012)	(0.040)	(0.026)	(0.071)	(0.056)	(0.074)	(0.842)
7+ Years	-0.013	0.041**	0.001	-0.057	-0.141***	-0.032	-0.109*	-0.029	-1.952**
	(0.059)	(0.018)	(0.010)	(0.042)	(0.031)	(0.080)	(0.062)	(0.060)	(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	0.047***	-0.007	0.043***	-0.040*	-0.040	-0.047**	-0.051***	-1.148**
	(0.024)	(0.015)	(0.005)	(0.015)	(0.021)	(0.033)	(0.022)	(0.018)	(0.542)
Main Results	-0.042**	0.038***	-0.009*	0.036**	-0.029*	-0.059*	-0.048*	-0.046*	-0.888**
	(0.020)	(0.013)	(0.005)	(0.016)	(0.016)	(0.033)	(0.025)	(0.024)	(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 suggests 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.[52] 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 2.15 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 2.15. Average Marginal Effects of Mandates on Impacted Outcomes Using a Falsification Test

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

[52] 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 actors may carry out the policy 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

Table 2.16 summarizes which variables passed robustness checks.[53] 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 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 2.16. 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 2.17 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 statistically 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

[53] 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.

sampled successfully 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 2.17. 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 Supplement 2C). Accordingly, I report results from derived data, which is also per NCES's strong recommendations.

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 respect to choosing the BPS, my study changed in scope throughout the research process. 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 earliest 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.

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

These results do not account for any composition effects. One of the biggest questions is if state-mandated financial education increases the likelihood that students go to college at all. Mandates may increase the likelihood that students attend college because these courses teach them how to assess the correlation between education, training, and earnings. As a result, a student may realize that they need to obtain postsecondary education or training to achieve their desired career or earn their ideal income. Students exposed to any element of career preparation in these courses are more likely to be informed about all aspects of college.

However, I cannot address this question now because I use a dataset of successfully matriculated college freshmen. In future research, 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.[54] 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 the 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.

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.

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[54] 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, conditional on enrollment, individuals who were exposed to personal finance course requirements 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 subject to the mandate.

However, these effects vary by first-generation status and by Pell eligibility. Conditional on enrollment, 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 significantly 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 non-disadvantaged students. Mandates have no statistically significant different impacts on enrolling into less risky institutions by economically disadvantaged status. However, mandate exposure primarily increases probabilities of borrowing any student loans among later-generation and higher-income students.

Conditional on enrollment, 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 a reduced probability of attending for-profit institutions for all students who were required to take 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 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 significantly 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 selection 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).

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 the 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 large 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.

Supplement 2A. 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

Supplement 2B. Confirming Exogeneity of Financial Education Mandates with Balance Tests

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		0.002
		(0.005)		(0.005)
Per Member Household Income (in 1000s)		0.00003		0.00001
		(0.0001)		(0.0001)
Independent		-0.031**		-0.003
		(0.015)		(0.008)
Underrepresented Minority		0.010		0.010
		(0.007)		(0.007)
Female		0.002		0.001
		(0.003)		(0.004)
D to C-		0.047		0.040
		(0.054)		(0.051)
C- to C		0.027		0.022
		(0.051)		(0.047)
C to B-		0.033		0.024
		(0.053)		(0.049)
B- to B		0.034		0.023
		(0.056)		(0.051)
B to A-		0.033		0.020
		(0.058)		(0.053)
A- to A		0.031		0.017
		(0.058)		(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***	-0.280***	-0.336***	-0.381***
	(0.066)	(0.067)	(0.120)	(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

Supplement 2C. Sensitivity Analysis by Data File Source

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

Supplement 2D. Data Appendix

Economists typically prefer linear probability models due to easy interpretation. Statisticians strongly advise using weights and survey design adjustments so that results are generalizable to all college freshmen attending Title IV institutions in the United States. Below, I provide more details about technical restrictions and rationale for running censored and non-linear regressions, and a note on other missing data that may be useful in understanding financing behavior.

Technical Restrictions and Regression Justification

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 downside to 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 to the student population. Instead, 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 – 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.

Other Missing Data on Student Financing

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.

3. Considering Recall Bias in Analyzing Impacts of Financial Education Mandates

Previous research on financial education has used both mandates and self-reports of financial education. Self-reports may be inaccurate because survey respondents are asked about high school courses many years later. This is a form of recall bias, and may contribute to mixed results in the literature because self-reported data is not the same as legislative or policy data. Self-reported data might be capturing something other than if they took financial education in high school. In this chapter, I investigate the frequency of recall bias and the characteristics of individuals most likely to exhibit recall bias. I then provide implications for future research.

I use data from a panel that has self-reports of financial education as well as information on the state of residence during high school (or at the time of high school graduation). This allows me to investigate whether self-reports are consistent with the mandates. Figure 3.1 illustrates four possible scenarios. Survey respondents may provide answers that are consistent. For example, they lived in a state that had a mandate and they report taking financial education, or they lived in a state that did not have a mandate and report not taking financial education. Alternatively, they may report that they had financial education, but did not live in a state that mandated it. These responses could be accurate because their school may have required students to take personal finance even if the state does not. This can also be because their state required schools to offer personal finance as an option for electives. However, those in the green box provide answers that are not consistent: they lived in states where they were required to take financial education but report that they did not take it. We refer to these respondents as non-compliers. These non-compliers may have actually not taken a financial education course or they may not recall the class.

The important caveat here is that comparing these measures of "mandated" and measures of "recalled" is not equivalent to saying that this is a measure of what they actually did. This is simply testing the reliability of self-reported information in relation to policy or legislative information.

Figure 3.1. Defining Misremembrance of Taking Personal Finance in High School

	Self-Reported:	
	Yes	No
Mandated		
Not Mandated		

In particular, the green area in Figure 3.1 represents where we know there is a discrepancy or potential recall issue. A discrepancy occurs where a respondent was instructed to take personal finance in high school as a core requirement for graduation, but self-reported one of the following:

- Not being offered any financial education in high school
- Being offered financial education but did not participate
- Took financial education but not in high school

Using the NFCS as administered in the ALP, I calculate that 70.2 percent of younger respondents mandated to take personal finance in high school self-reported not taking financial education in high school.[55]

Data

I use data on respondents' demographics and self-reported financial education from the RAND American Life Panel (ALP). The ALP is a nationally representative internet panel made of over 6,000 adults ages 18 and over in the United States. The ALP uses address-based sampling and random digit dialing (RDD) to recruit panel participants and supplies the necessary equipment to panel participants without computers or internet so that they can participate in the survey and maintain the representativeness of the ALP. The ALP consists of longitudinal data dating back to its inception in 2006 that has covered a variety of topics relating to financial behavior, financial literacy, labor market participation, health behavior, and political viewpoints. Researchers can link surveys from these various topics to explore any theoretical relationships between them. All ALP data is publicly available at no cost to researchers to use in their analyses. More information about the RAND American Life Panel is available at https://alpdata.rand.org/.

Several institutions including the Federal Reserve Bank of Boston, FINRA Investor Education Foundation, and the National Institutes of Aging have administered their surveys through the ALP. In particular, the 2012 NFCS was also administered in the ALP. The full NFCS has more respondents allowing for analysis at the state level. However, the ALP subset can be linked to longitudinal information about respondents' financial behaviors and has more precision in treatment assignment.

Specifically, I pull data from the following surveys: MS 432 ("Pilot Surveys 2015") to obtain high school state data and MS 284 ("National Financial Capability Study") to obtain the main demographic and self-reported financial education data. Overall, 2,094 individuals participated in MS 284, and 5,280 individuals participated in MS 432, with 1,713 participating in both. As in

[55] One immediate concern is to what extent respondents who attended private high schools drive this discrepancy. Given that up to nine percent of high school students attend private schools in a given academic year between 1991 and 2012, a discrepancy rate of 70 percent suggests that there is some other underlying issue in recalling course receipt (NCES 2016, Table 105.30).

previous chapters, I limit the sample to those under 40, leaving 545 individuals. Older cohorts would not have been subject to the financial education mandates in any state. I exclude respondents with unknown high school state information and respondents who attended foreign high schools from the analyses. Within NFCS respondents, high school state is missing for nine percent of the sample under 40. This leaves an eligible sample of 503 individuals. However, only 13 percent of the sample was required to take personal finance courses, leaving an analytic sample of 66 individuals.

Respondents' state in which they attended high school is derived from the ALP question "What state did you live in when you were in high school? If you lived in more than one state, please choose the last state you lived in while in high school," which was included in MS 432. An option for respondents attending high school outside the country ("none of the below") was added after 650 respondents already answered the question without that option.

I use mandate data from Urban and Schmeiser (2015), where the specific policy I analyze is mandating personal finance courses as a core requirement for graduation. I weight all estimations with standard errors clustered at the high school state via the *svyset* command in Stata. Together, I use state of high school attendance and age to determine if the respondent was exposed to mandate. I calculate high school graduation year from the sum of 18 and the respondent's presumed birth year.

Descriptive Statistics

Table 3.1 presents descriptive statistics for the full sample, those not mandated to take personal finance, and those mandated to take personal finance. Generally, the mean age of the eligible sample is 29.7, and two-thirds of them have received at least some college education. The mean score on the "Big Five" financial literacy quiz is 2.7 out of five. Slightly more than one-thirds of the sample was able to answer more than three out of the five questions correctly. Thirteen percent of the sample was required to take personal finance for high school graduation, of which the mandate has been implemented for at least three years among three-quarters of the sample. The majority of those exposed to the personal finance course requirement took personal finance integrated into another course (e.g. economics or math) instead of a standalone course. Nearly one in five eligible sample members reported taking financial education in college, workplace, or military. An overwhelming 97 percent of them believe that financial education should be offered in schools.

Table 3.1. Descriptive Statistics of Characteristics Tested for Associations

VARIABLES	Overall (N = 503)		Not Mandated (N = 437)		Mandated (N = 66)	
	Mean	SE	Mean	SE	Mean	SE
Age	29.69	0.27	30.15	0.34	26.72	1.12
Proportion, 18 – 24	0.20	0.03	0.17	0.03	0.37	0.12
Proportion, 25 – 34	0.58	0.04	0.57	0.04	0.62	0.12
Proportion, 35 – 39	0.23	0.02	0.26	0.03	0.01	0.01
Number of Financial Literacy Questions Answered Correctly (Max = 5)	2.68	0.07	2.76	0.06	2.20	0.23
Financially Literate (Answered More than 3 Correctly)	0.37	0.02	0.39	0.02	0.24	0.04
Education: At Least Some College	0.67	0.03	0.67	0.04	0.62	0.05
Received Financial Education in College, Workplace, or Military	0.18	0.01	0.17	0.02	0.24	0.07
Believes that Financial Education Should Be Offered in Schools	0.97	0.01	0.98	0.01	0.95	0.02
Year Mandate Was Implemented at Exposure:						
None	0.87	0.07	1	0	0	0
1st Year	0.01	0.01	0	0	0.10	0.03
2nd Year	0.02	0.01	0	0	0.15	0.05
3rd Year/Beyond	0.10	0.05	0	0	0.75	0.06
Course Administration:						
None	0.87	0.07	1	0	0	0
Integrated	0.11	0.06	0	0	0.84	0.16
Standalone	0.02	0.02	0	0	0.16	0.16
Required to Take Standardized Tests on Personal Finance Concepts	0.06	0.04	0.02	0.02	0.28	0.20

NOTES: Summary statistics for all eligible sample members are reported under “Overall,” and summary statistics for analytic sample members are reported under “Mandated.” Statistics are weighted and standard errors clustered at high school state.

First, I run simple linear regressions to assess if the following characteristics were associated with noncompliance: if respondent self-reported taking personal finance courses somewhere other than high school, highest education level, if respondent believes that financial education should be offered in schools, number of years state has implemented mandate, course administration, if state required standardized testing on personal finance concepts, age, and financial literacy (where respondent is financially literate if answered more than three out of five correctly). I run each variable as a separate regression because I only have 66 observations. I report these results in Table 3.2.

As expected, higher proportions of noncompliance occurred among respondents who did not report taking financial education elsewhere and respondents who do not believe that financial education should be offered in schools. Among respondents not reporting financial education elsewhere, this may indicate some sort of disinterest; therefore, they decided not to take other financial education courses. Respondents who do not believe that financial education should be offered in school may have had an adverse experience in their high school course, or come from

a political background that hails financial education to be parents' responsibility. Contrary to expectation, a higher proportion of noncompliance occurred among respondents mandated to take a standalone course than among respondents mandated to take an integrated course. This may be plausible if they in fact did not receive a standalone course (e.g. Roberts and Joyce 2017). Education attainment, financial literacy, years since implementation, and being mandated to be tested on personal finance does not explain the discrepancy between the legislative and self-reported data. When continuous, age does not explain the discrepancy between the legislative and self-reported data. Yet, when placed into separate categories (ages 18 – 24, ages 25 – 34, and ages 35 – 39), I find that young adults are significantly less likely to be non-compliers than older adults. This is expected because older adults would be further from exposure, and may have had other financial education classes since.

Overall, financial education dispositions and type of course administration explain the discrepancy between self-reported and legislative information. Those who were mandated to take a standalone course had a significantly higher proportion of noncompliance than those who were mandated to take an integrated course. Those who did not take financial education in other settings (e.g. college, workplace, or military) had a significantly higher proportion of noncompliance than those who did. This could reflect respondents' general interest in personal finance. Finally, those who do *not* believe that financial education should be offered in schools had a significantly higher proportion of noncompliance than those who thought otherwise. This could reflect respondents' interest in personal finance as well as their recollection of the course.

Table 3.2. Results of Associated Characteristics Using Simple Linear Regressions Among Mandated Respondents

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age	0.022									
	(0.016)									
Age 25 – 34		0.214								
		(0.164)								
Age 35 – 39		0.435**								
		(0.155)								
Number of "Big Five" Questions Correct			0.039							
			(0.067)							
Answered > 3 Questions Correctly				-0.009						
				(0.110)						
2nd Year					0.321					
					(0.300)					
3rd Year +					0.214					
					(0.206)					
Some College						0.038				
						(0.153)				
College Graduate						0.049				
						(0.186)				
Post-Graduate Education						-0.084				
						(0.232)				
Took Financial Education in College, Workplace, or Military							-0.394**			
							(0.154)			
Believes that Personal Finance Should Be Offered in Schools								-0.244**		
								(0.0788)		
Required Standalone Course									0.257**	
									(0.103)	
Required Standardized Testing										-0.042
										(0.237)
Mandated Personal Finance										

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Constant	0.124	0.565***	0.615**	0.704***	0.492*	0.687***	0.796***	1.000***	0.661***	0.713***
	(0.487)	(0.155)	(0.216)	(0.107)	(0.246)	(0.187)	(0.0684)	(0.000)	(0.103)	(0.0730)
N	66	66	66	66	66	66	66	56	66	66
R^2	0.055	0.054	0.018	0.000	0.029	0.007	0.135	0.017	0.042	0.002

NOTES: Standard errors in parentheses. Each column denotes a separate regression. Linear probability models were run, and are weighted with standard errors clustered at high school state. *** p<0.01, ** p<0.05, * p<0.1.

Hypothesis Testing

In the NFCS administered in the ALP, I test the hypothesis that measures of "recalled" are not equivalent to measures of "mandated" using two-tailed t-tests. Under this t-test, the null hypothesis is that the self-reported measures are equivalent to the legislative measures:

$$H_o: PF_{legislated} = PF_{self-reported}$$

$$H_A: PF_{legislated} \neq PF_{self-reported}$$

To conduct this t-test, I run a simple difference of means among those required to take the course where the dependent variable is self-reporting taking any financial education and the independent variable is being mandated personal finance courses. As empirically highlighted in Table 3.3, I find that the measures of "recalled" are not the same as the measures of "mandated." This is not surprising, given that only 29.8 percent of those required to take personal finance in high school self-reported taking any financial education.

Table 3.3. Two-Tailed T-Tests Results for Measures of "Recalled" versus Measures of "Mandated" Among Mandated Respondents

Variable	N	Mean	SE	SD	95% CI
		A. Self-Reported Taking Any Financial Education			
Legislative	55	1	0	0	[1, 1]
Self-Reported	55	0.291	0.062	0.458	[0.167, 0.415]
Difference	55	0.709	0.062	0.458	[0.585, 0.833]
t = 11.47, df = 54					
		B. Self-Reported Taking Financial Education in High School			
Legislative	55	1	0	0	[1, 1]
Self-Reported	55	0.145	0.048	0.356	[0.049, 0.242]
Difference	55	0.855	0.048	0.356	[0.758, 0.951]
t = 17.81, df = 54					

Data Sources: Urban and Schmeiser (2015), and NFCS 2012 (ALP Version)

Additionally, I run simple linear regressions among the entire sample where the dependent variable is self-reporting taking any financial education and the independent variable is being required to take personal finance in high school. In this case, if measures of "recalled" does not equal measures of "mandated", then the personal finance requirement indicator should not be statistically significant than zero. Otherwise, it would suggest that the measure of "recalled" is a somewhat reliable measure of "mandated." As shown in Table 3.4, the personal finance indicator is not statistically significantly different than zero, and does not explain any of the variation in self-reporting financial education. Therefore, both measures are distinct and should not be substituted for one another.

Table 3.4. Regression Results for Measures of "Recalled" versus Measures of "Mandated" Among All Respondents

VARIABLES	Self-Reported Measures	
	Taking Any Financial Education	Taking Financial Education in High School
Required Personal Finance in High School	0.0131	0.0401
	(0.0883)	(0.0892)
Constant	0.271***	0.130***
	(0.0241)	(0.0195)
N	418	416
R^2	0.000	0.002

NOTES: Standard errors in parentheses. Each column denotes a separate regression. Linear probability models were run, and are weighted with standard errors clustered at high school state. *** p<0.01, ** p<0.05, * p<0.1.

My findings produce two major implications. First, this means that caution should be warranted among studies using legislative data versus studies using self-reported data in finding results. Studies using self-reported data may be picking up effects of interest in personal finance instead of effects of taking personal finance courses. On the other hand, these findings suggest that it may take more than being exposed to personal finance requirements for it to be effective. This could be because students do not find the material to be relevant; or the courses were implemented poorly if at all (e.g. Roberts and Joyce 2017; Cude 2017).[56] To get an accurate effect of the mandate, it may be worth instrumenting school-based financial education indicators on subjective measures such as subjective financial knowledge, how interested they are in personal finance, or how helpful they found their courses when such information is available. Mandates may unlikely affect uninterested students, or students who did not find the course helpful.[57]

[56] One of the author's students revealed to her that he did take personal finance in high school, but he felt like it was a joke because it was implemented in a civics' course that vaguely covered personal finance in a couple of classes. This student is a 20-year-old economics major who already owns two businesses – both of which he founded himself. On the contrary, a Lyft driver explained that he found his personal finance class to be very helpful. His teacher took the time to explain how investments work, and encouraged students to find out more about how to invest in a product that interested them. The driver actually executed the investment plan he developed in his high school class, and found it has paid off very well for him. He might be more reliant on his high school course because he did not pursue any postsecondary studies.

[57] Results here are very preliminary. I would prefer to re-run this test using the ALP at a later date, when I can acquire funds to administer the same questions in the NFCS 2012 and inquire about high school type across the entire ALP sample under age 40 so that I can gain more precision.

Key Findings and Policy Recommendations

This dissertation has investigated the impact of financial education on younger adults' financial outcomes. Across the two main chapters, I find that financial education supports better financial decisions. This is in contrast to some of the previous literature which has found mixed effects. One of the most important implications of my dissertation is that evaluations of state-mandated financial education should include the financial behaviors that are relevant to young adults. This ensures that the impact of the policy is not underestimated, and tests if general concepts learned in required personal finance courses are transferable to young adults' realized scenarios. Without considering all plausible outcomes, research may discourage policymakers from adopting an otherwise effective mandate. I further discuss key findings for two examples of these understudied financial behaviors and suggested next steps for public policy.

Alternative Financial Services

Overall, individuals who were subject to personal finance requirements were less likely to use any type of alternative financial services than those who were not. Particularly, they were statistically significantly less likely to borrow payday loans. Underrepresented minorities and women who were exposed to personal finance education in high school were less likely to borrow payday loans, auto title loans, tax refund anticipation loans, were less likely to use pawn shop services, and were less likely to purchase items via rent-to-own transactions than their respective peers not exposed to the mandate.

Financially educated consumers may shop around for cheaper credit options with the understanding that AFS are one of the most expensive options on the market. Additionally, they may accumulate savings in case of emergencies allowing them to avoid AFS. Alternatively, if there are implications for college attendance and financing as discussed in Chapter 2, then this may reduce the need to use AFS.

Postsecondary Education

Overall, first-time beginning college freshmen who were exposed to state-mandated financial education were more likely to enroll in college full-time and enrolled in institutions with lower cohort default rates (marginally significant). Mandates do not have significantly heterogeneous effects on college choice outcomes by economically disadvantaged statuses. Additionally, financial education mandates are equally associated with reduced enrollments in for-profit institutions among all students.

Overall among student loan borrowers, first-time beginning college freshmen who were exposed to personal finance requirements borrowed less than their peers who were not required to take the class (marginally significant). This may be due to borrowing less unsubsidized loans,

especially among first-generation and lower-income students. Mandates particularly increased the likelihood of borrowing any federal student loans among higher-income and later-generation students. While mandates are associated with greater likelihoods to fill out a FAFSA, this association strictly applies to non-disadvantaged students.

Policy Recommendations

Based on my key findings above, I provide some next steps that policymakers and other relevant stakeholders can take today. Slightly different steps apply to states without mandates versus states with mandates, but the challenges and needs to address them apply to all states.

Among States without Mandates:

The first set of recommendations apply to states that have not established any financial education mandates. The two recommendations are to assess the feasibility of requiring financial education in high school and to focus on disseminating information to underserved communities. Both recommendations consider ways to improve financial capability among economically vulnerable youth and young adults.

Examine Political and Financial Feasibilities of Mandating Personal Finance Course Requirements in Public High Schools

Policymakers should look into the history and current efforts in establishing financial education mandates. There are three major ways that policymakers can establish financial education mandates for their high school juniors or seniors:

1. Require personal finance courses in a standalone course
2. Require personal finance courses as integrated into economics, mathematics, or other social science courses
3. Require schools to offer personal finance courses as an elective

These ways are listed from most to least rigorous, but policymakers will have to determine the resources their states have to establish one of these variants. This will include talking to state budget divisions or finance departments about any available funds to institute these requirements.

Focus on Disseminating Information Associated with This Research, Especially in Underserved Communities

Policymakers should focus on disseminating information to financially vulnerable youth in partnership with relevant organizations. Alternative financial services should be discussed in tandem with other credit products. It will be important to emphasize alternative financial services as loans of last resort. In this case, policymakers may collaborate with credit unions, local minority-owned banking institutions, community-based nonprofit organizations, city-level offices of financial empowerment (if available), and social media outlets.

Common approaches to disseminating information about college choice and financial aid has been through seminars and college fairs. Here, policymakers may also want to make sure that a general representative is available to discuss other important aspects of choosing a postsecondary institution with students and their parents or guardians (e.g. alumni employment and attendance intensity). This can easily be done in partnership with local colleges and universities, high school guidance counselors, knowledgeable teachers, city-level offices of financial empowerment (if available), and community-based nonprofit organizations (especially those who work on college access and youth development).

Among States with Mandates:

The second set of recommendations apply to states that have established financial education requirements. The recommendation is to incorporate content on postsecondary education in personal finance courses. This recommendation will ensure that students can immediately apply their personal finance education into their first major investment (themselves). As previously discussed, this might mean better use of credit overall.

Incorporate and Prioritize Concepts on Postsecondary Education in Personal Finance Courses

Alabama, Missouri, Tennessee, Texas, and Utah have included concepts on postsecondary financing, college selection, and career choices. New Jersey will likely include these concepts as well, legislation pending. At the end of the personal finance courses, these states explicitly expect students to:[58]

- Recognize and explore the correlation between education, training, and potential lifetime income (UT).
- Investigate the postsecondary/higher education admissions process, including completing admission and financial aid applications (AL).
- Understand how to complete the FAFSA (TX).
- Analyze and compare student loan options, including private and federal loans (TX).
- Research multiple viewpoints that support or question the use of student loan debt in paying for postsecondary education. Assess the extent to which the reasoning and evidence presented support the author's claim (TN).
- Compare and contrast the various sources and types of consumer credit, such as student loans, auto loans, store credit cards, and payday loans (MO & TN). Draw conclusions about the types of credit best suited for financing and/or purchasing various goods and services, defending claims with specific textual evidence (TN).
- Calculate the costs of post-high school training options and analyze the return on investment (ROI) based on career choices, including understanding the cost differences between public and private, and between nonprofit and for-profit education and training (UT).

[58] Copied precisely from state standard documents to show extent of expectations in understanding postsecondary education concepts.

Their respective agencies, Alabama Department of Education, Missouri Department of Education, Tennessee Department of Education, Texas Education Agency, and Utah State Board of Education may provide good examples of states that have incorporated postsecondary education and career management content into their personal finance curriculum.

Additionally, policymakers in departments of education, school district personnel, and high school personal finance teachers should consult developed curricula from the Council for Economic Education (CEE), state-level councils for economic education, the Federal Reserve System, the Treasury, among other advocacy and government sources to garner suggestions on how to present postsecondary education concepts to high school students. For example, CEE and the Fed recently published their "Investing in College Education" module which includes information on deciding if to go to college, where to go to college, how to calculate the costs of college, and how to finance college. This allows policymakers and department of education staff tasked with developing curriculum and instruction to determine what pieces will best serve their students.

Given limited time and scarce resources, policymakers in remaining states should also consider which areas in personal finance may not need to be emphasized as much to youth because most students will not encounter them for at least several more years (e.g. homeownership, retirement planning, estate planning). Policymakers should solicit feedback from personal finance teachers and high school students when deciding which areas are less important. Substituting these concepts for content on postsecondary education will enhance the mandate's effectiveness because it will be "just-in-time" for students making the crucial decision to invest in their futures. While no cost-benefit studies have been conducted on school-based financial education, my finding that students exposed to personal finance mandates enroll in institutions with lower cohort default rates suggest that this substitution might be socially worth the effort.

Among All States:

Evidence from my work and previous studies suggest that financial education improves future decision-making among youth and young adults. The challenge, however, is how to deliver personal finance education at the right time and in the right manner to the individuals who need it most (e.g. economically disadvantaged students). This requires coordinated efforts of states' departments of education; states' finance/budget departments; higher education institutions; city-level offices of financial empowerment (if available); community-based nonprofit organizations (especially those who work on credit counseling, college access, and youth development); councils for economic education; school districts; high school personnel; and high school students.

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