Impact of Financial Education Mandates on Younger Consumers' Use of Alternative Financial Services

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ABSTRACT

Financial literacy in the United States remains alarmingly low: two-thirds of Americans are unable to correctly answer more than three out of five questions regarding interest rates, inflation, risk diversification, mortgages, and bond pricing. Adequate financial knowledge is critical because consumers are increasingly being asked to take responsibility for their own financial health. One policy response has been to mandate financial education in schools; however, it remains unclear if financial education actually improves financial capability. In this study, I use data from the National Financial Capability Study (2012) to examine whether financial education impacts the use of alternative financial services (AFS). By estimating probability and count data models with state fixed effects to examine AFS use among younger adults who were mandated to take financial education in high school relative to those who were not, I find that financial education mandates significantly reduced the likelihood and frequency of payday borrowing. Furthermore, financial education mandates significantly reduced the probabilities of using AFS among underrepresented minorities and females. These findings suggest that evaluations on financial education policy should also focus on financial behaviors that younger consumers are significantly more likely to engage in, such as high-cost borrowing. Failure to do so may underestimate the benefits of school-based financial education.

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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. 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.[2] Good credit scores are not required to use these products nor do they determine the APR charged.

Table 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
RALs	$100 per loan	70 – 500%	$300 - $1,000	1 – 2 weeks	Tax refund
RTOs	N/A	57 – 230%	Up to product price	12 – 24 months	Purchased product

SOURCES: Bradley et al. 2009; McKernan, Ratcliffe and Kuehn 2013; CFPB 2013; Federal Trade Commission 2014; Galperin and Weaver 2014; Robb et al. 2015; and Butta, Goldin and Homonoff 2016.

[2] 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.

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.[3]

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, Schmeiser and Collins 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 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, financially 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 obtained data on AFS use from the restricted version of the 2012 National Financial Capability Study and data on state mandates from Urban, Schmeiser and Collins (2015). I used difference-in-differences to exploit

[3] This rule is now pending Congress's approval.

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 restricted 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.

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 was approximated. 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, Butta, 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 Butta, 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).

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). While Mandell and Klein (2009) find no effects on savings or credit payment behaviors using independently-collected cross-sectional surveys, Gutter and Copur (2011) do find positive effects on those same behaviors. These discrepancies in findings are likely due to different sample sizes, sampling frames, and study timing.[4]

Another study examines the impact of receiving high school financial education on having a bank account using a nationally representative survey. Grimes, Rogers and Smith (2010) find that taking a financial education course in high school increases the likelihood that one maintains a bank account.

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 employed 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

[4] The sample size is 79 in Mandell and Klein (2009) and is 15,797 in Gutter and Copur (2011). Mandell and Klein (2009) surveyed high school classes of 2001 – 2004 from only one school system while Gutter and Copur (2011) sampled college students across fifteen universities who graduated high school between 2004 and 2008.

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 used quota sampling from various online panels to recruit survey respondents, where quotas were established and weights were 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) in order to ensure that the quota sampling would produce similar results to a probability-based sample. I conducted 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 restricted the sample for this study to those under age 40 who have at least a regular high school diploma, leaving 7,324 observations.[5]

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 employed data on state mandates from Urban, Schmeiser and Collins (2015) in order 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.[6] The dataset distinguishes between state mandates that required schools to offer

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[5] 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.

[6] 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.

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.[7] I explicitly analyzed the policy variation that requires all students to take personal finance as a core prerequisite for graduation. I linked 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 used 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 exploited 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.[8] By nature, mandates mean that all high school students are required to take the financial education 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. We can also assume exogeneity on the grounds that financial education mandates vary across states and over time.

I examined 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 explored specifications where the dependent variable measures if a consumer used any of the five types (equaling one if yes, zero if otherwise). Further, I examined 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 examined models that simultaneously estimate the probability of using a specific type of AFS and how

[7] 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.

[8] 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.

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.

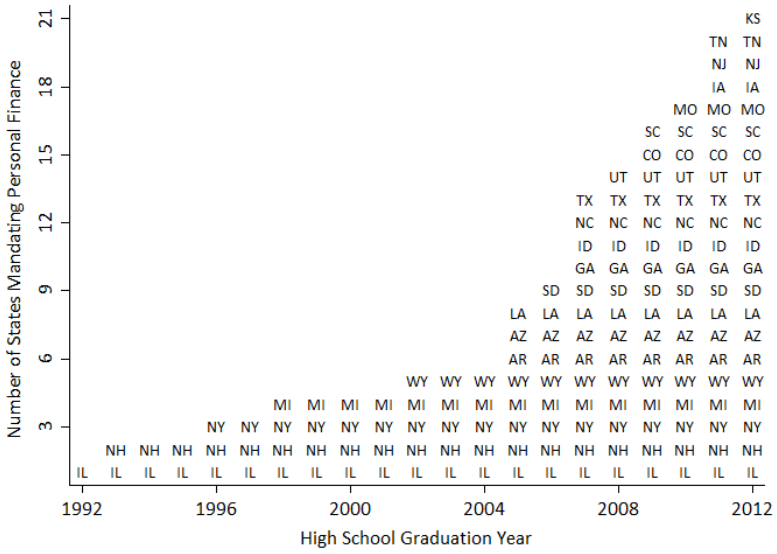
The independent variable of interest is X_{st}, which denotes if the consumer was required to take personal finance in high school for graduation. Taken together, I was able to use age and state of residence to determine if the respondent was likely to be exposed to mandated financial education as pictured in Figure 1. 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 used graduation requirement 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.[9] I tested if state of residence is a good proxy for high school state by using the ALP version of the NFCS, as shown later.

$X_i'\beta$ accounts for the consumer's financial and demographic characteristics that may be related to AFS use.[10] 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.

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

[10] 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 now examining the effects of financial education on subjective financial knowledge. A number of these are also correlated with age (e.g. credit scores, homeownership).

Figure 1. States Implementing Personal Finance Mandates, 1992 – 2012



SOURCE: Urban, Schmeiser and Collins (2015)

I weighted all estimations and clustered standard errors by state in order to account for survey design and to account for policy variation occurring at the state level. I used listwise deletion since the percent of respondents for missing observations did not exceed three percent for any given variable. I also reported average marginal effects.

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 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.

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.

Table 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%

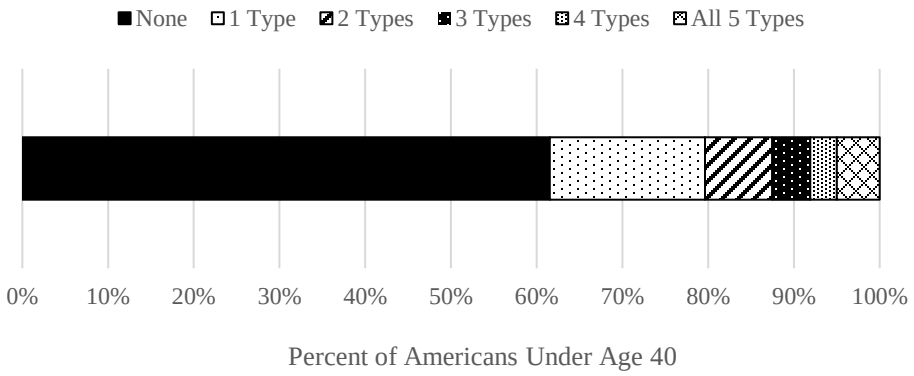
NOTE: Reported statistics are weighted.
[a] These variables are top-coded at four.
[b] Black/African-American, Hispanic/Latino, and American Indian/Alaskan Native racial groups are categorized as underrepresented minorities.

Figure 2 and Table 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 2. When looking more specifically at each type of AFS use in Table 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.[11] Furthermore, the top-

[11] 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.

coding of the AFS question prohibits combining the total number of times that consumers may have used AFS products overall.

Figure 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 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.

Main Findings

When holding demographic and financial characteristics constant, mandatory financial education classes as a core requirement for high school graduation marginally reduced the probability of using any of the five AFS loans by six percentage points (see Table 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 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 5 lists 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 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 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Heterogeneous Effects

Interactions with Race and Gender

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 6 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 seven percentage points less likely to use auto title loans; fifteen percentage points less likely to use payday loans; eight percentage points less likely to use RALs; six percentage points less likely to use pawn shop services; and eleven 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 females than for males. Female consumers who were mandated to take personal finance classes in high school were five percentage points less likely to use auto title loans; eleven percentage points less likely to use payday loans; four percentage points less likely to use RALs; and six percentage points less likely to use RTOs than female consumers 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 except for pawn shop services. Consumers access pawn shops for a variety of reasons, and pawn shops provide multiple services that sometimes includes tax preparation (McKernan and Compton 2010).[12] Furthermore, the effects are not as strong when looking at gender heterogeneity than they are when looking at racial heterogeneity. This is likely because females already use fewer AFS on average than males, 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 6. 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*	-0.150***	-0.002	-0.080**	-0.067
	(0.033)	(0.040)	(0.049)	(0.034)	(0.057)
Auto Title Loans	-0.024	-0.069***	-0.005	-0.049***	-0.029
	(0.029)	(0.022)	(0.041)	(0.014)	(0.041)
Payday Loans	-0.072**	-0.147***	-0.053	-0.106***	-0.088*
	(0.030)	(0.029)	(0.042)	(0.018)	(0.047)
RALs	-0.032	-0.079***	0.012	-0.038**	-0.034
	(0.030)	(0.026)	(0.044)	(0.019)	(0.045)
Pawn Shop Services	-0.044	-0.064*	0.012	-0.033	-0.022
	(0.030)	(0.038)	(0.048)	(0.030)	(0.051)
RTOs	-0.040**	-0.111***	0.018	-0.056***	-0.043
	(0.019)	(0.017)	(0.028)	(0.019)	(0.030)

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 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 considered an alternative specification, tested the sensitivity of defining mandates according to state types, and conducted the main analyses excluding states that banned payday loans.

[12] It is not clear if pawn shops tend to serve as "one-stop shops" in certain areas more so than others.

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 estimated effects using zero-inflated Poisson (ZIP) regressions because at least three-quarters of respondents did not use any AFS (see Tables 2 and 3), and it represents a reasonable assumption that those using AFS loans may be systematically different than those that never use AFS loans.

Table 7 reveals that the economic conclusions from the ZIP regressions are similar to those of the negative binomial regressions presented in Table 5.[13] 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 7. 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. Full regression results are available in Appendix C. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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 used 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

[13] Economic conclusions are also similar across specification types for heterogeneous effects (see Appendix D).

NFCS has more power.[14] 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.[15] To ensure comparability, I weighted 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 8 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 8. 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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

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

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 tested 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 (see Appendix B). As of 2012, at least two-thirds of states that had ever implemented personal finance mandates (fourteen states) permitted payday lending (Urban, Schmeiser and Collins 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.[16] 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 conducted analyses excluding the following states that banned payday loans as of 2007.[17] I considered 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 9 reveals that the economic conclusions remain and the magnitudes are similar to the main regression results in Table 5. Even when excluding states that prohibited payday lending during the entire time period, Table 9 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 conducted analyses excluding the following states that banned payday loans as of 2012, the entire five-year period covered by the NFCS survey question.[18] Table 10 reveals that the validity of the previous economic conclusions remain, and the magnitudes are similar to the main regression results in Table 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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[16] In absolute terms.

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

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

Table 9. 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 10. 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 approximated treatment assignment using state of residence and age. Roughly 77 percent of all Americans under age 40 resided in the same state that they

attended high school according to the ALP.[19] 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 may have been counted as certainly treated when we do not know if they actually were treated.

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. More specifically, youth 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 ethnicity and gender. Underrepresented minorities and females 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 fifteen 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 eight percentage points less likely to use any AFS than women who were not required to do so.

[19] Statistic is as of October 2015. Calculated using MS 432 among those with non-missing high school information; weighted.

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; Gutter and Copur 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. Mandell and Klein 2009; Gutter and Copur 2011; Brown et al. 2014; Brown et al. 2016), evaluations should also include tracking 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.[20] Alternatively, they may want to think about how to disseminate information to financially vulnerable youth that explicitly discuss alternative financial services in tandem with other 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.

[20] 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.

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Appendix A. Full Results for AFS Use

Table A1. 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 A2. 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 A3. 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 A4. 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix B. 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, Schmeiser and Collins (2015); and Consumer Federation of America (2017)

Appendix C. Notes on Full versus Reduced Specifications

A potential concern is that many of the variables in the full specification (e.g. household income, marital status, number of dependent children, and credit card holding status) are correlated with age when age is a key component of the identification strategy. However, another potential concern is that failure to account for these characteristics would inflate the effects of the mandate. In the NFCS, the only predetermined variables available are race and gender.

In Table C1, the reduced specification only accounts for the treatment and all available predetermined variables (race and gender). Table C1 below reveals that there is little change in the effects of financial education mandates regardless of using the full or reduced variable specifications. As expected, there are some differences in the estimated effects of age between the reduced and full specifications. While parsimony is preferred, the adjusted Wald test results indicate that household income, marital status, number of dependent children, and credit card holding status significantly improve the fit of the models. These observations also hold for negative binomial and zero-inflated Poisson regressions (not included for parsimony). Therefore, results from the full specifications are presented through-out the report.

Table C1. Average Marginal Effects from Full versus Reduced Specifications in Logit Regressions

VARIABLES	Use Any AFS		Auto Title Loans		Payday Loans		RALs		Pawn Shop Services		RTOs	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Mandated personal finance	-0.059*	-0.067**	-0.024	-0.036	-0.072**	-0.081***	-0.032	-0.039	-0.044	-0.046*	-0.040**	-0.045**
	(0.033)	(0.031)	(0.029)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.027)	(0.019)	(0.019)
Female	-0.078***	-0.039***	-0.065***	-0.053***	-0.059***	-0.040***	-0.067***	-0.053***	-0.066***	-0.042***	-0.066***	-0.040***
	(0.014)	(0.015)	(0.010)	(0.011)	(0.013)	(0.013)	(0.008)	(0.008)	(0.012)	(0.012)	(0.012)	(0.013)
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***	0.156***	0.031	0.034	0.108***	0.118***	0.036**	0.046***	0.049***	0.081***	0.062***	0.068***
	(0.021)	(0.022)	(0.021)	(0.021)	(0.025)	(0.025)	(0.017)	(0.017)	(0.017)	(0.016)	(0.019)	(0.018)
Latino	0.081***	0.106***	0.003	0.016	0.045***	0.058***	0.027*	0.037**	0.053**	0.071***	0.014	0.028*
	(0.018)	(0.017)	(0.016)	(0.016)	(0.014)	(0.015)	(0.015)	(0.015)	(0.021)	(0.021)	(0.015)	(0.015)
Asian	-0.053	-0.085**	0.019	0.016	0.023	0.013	0.016	0.006	-0.062*	-0.079***	-0.016	-0.030*
	(0.038)	(0.037)	(0.026)	(0.024)	(0.031)	(0.030)	(0.021)	(0.021)	(0.031)	(0.029)	(0.019)	(0.017)
Other	0.081*	0.111**	-0.001	0.002	0.060*	0.069**	0.020	0.024	0.043	0.068**	-0.004	0.007
	(0.044)	(0.042)	(0.025)	(0.024)	(0.031)	(0.031)	(0.031)	(0.032)	(0.032)	(0.034)	(0.023)	(0.025)
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***	-0.003***	-0.006***	-0.002**	-0.006***	-0.001	-0.005***	-0.001	-0.007***	-0.006***	-0.008***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
State of current residence	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
N	7,136	7,225	7,102	7,186	7,099	7,185	7,091	7,178	7,088	7,176	7,112	7,198

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. (1) indicates full, and (2) indicates reduced specifications.

Appendix D. Estimating Heterogeneous Effects Using Zero-Inflated Poisson Regressions

Tables D1 and D2 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 D1. 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 7 where race/ethnicity was interacted with mandate indicator. Average marginal effects (AMEs) are reported. N = 7,112. *** p < 0.01, ** p < 0.05, * p < 0.1

Table D2. 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 7 where gender was interacted with mandate indicator. Average marginal effects (AMEs) are reported. N = 7,112. *** p < 0.01, ** p < 0.05, * p < 0.1