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The Complementary Effects of Financial Education and Payday Lending Regulations on Financial Inclusion

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Abstract

Approximately 5.6 million U.S. households remained unbanked in 2023. We examine the effects of state-mandated high school personal finance coursework on banking outcomes. Because the unbanked population resorts to alternative financial services, such as payday loans, for their financial needs, we also examine the interplay between payday loan regulation and financial education. We find that exposure to personal finance coursework is associated with a lower likelihood of being unbanked and of unbanked adults being uninterested in opening a bank account. This finding holds regardless of whether the state has allowed or restricted payday lending, but we find larger effects in states with stronger restrictions. These results suggest that, for financial inclusion, regulatory measures and financial education are more likely complements than substitutes.

JEL classifications: D12, D14, G21, G28

Keywords: financial education, payday lending, financial inclusion, consumer finance

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I. Introduction

Financial inclusion—universal access to and usage of formal financial services—is a crucial first step toward achieving financial wellbeing and critical to economic development and individual prosperity (Demirgüç-Kunt et al. 2018). For low-income populations, a bank account can enhance wealth accumulation and provide greater financial security through savings and improved credit access while offering consumer protections (Célerier and Matray 2019). At the same time, the U.S. unbanked rate stands out compared to other developed countries: approximately 5.6 million U.S. households were unbanked in 2023 (Federal Deposit Insurance Corporation, FDIC 2024). This raises questions about how participation in formal financial services could be increased and how the share of unbanked households might be reduced in the U.S.

Two commonly discussed policy approaches to reducing the proportion of unbanked households: educate towards accounts and regulate away from alternative financial services (AFS) like payday loans. Are these two strategies substitutes or complements in improving financial inclusion?

Policymakers recognize financial education as a tool aimed at empowering individuals to make informed financial decisions, with research documenting its effectiveness (Hastings et al. 2013; Kaiser et al. 2022; Lusardi and Mitchell 2014; Lusardi et al. 2017). In this paper, we study one such intervention—state-mandated personal finance (PF) coursework requirements for high school graduation in the U.S.—to examine its causal impact on individuals' banking decisions and intentions. As of 2020, more than thirty states had some PF education required in high schools aimed at increasing financial literacy to better equip young adults with the necessary financial knowledge to make financial decisions confidently (Urban, 2024a).

Households outside of the traditional financial system heavily rely on AFS—and most commonly, payday loans—for their financial needs. The extremely high interest rates and fees commonly charged by payday lenders often place households in a long-term struggle to repay the loan, straining their financial health indicators (Bhutta et al. 2015; Melzer 2011; Skiba and Tobacman 2019). While some researchers argue that banning of payday lending is associated with positive financial outcomes (Morgan et al. 2012; Morse 2011), others view payday loans as a reasonable solution for households without access to mainstream credit (Servon 2017), and yet others found that payday lending bans or associated restrictions produce inconclusive results and

essentially crowd out borrowers to look for loans elsewhere (Bhutta et al. 2016; Desai and Elliehausen 2017).

Regardless of the documented effectiveness or lack thereof, restricting payday loans has been a common strategy among state authorities. As of 2023, nearly 20 states passed laws that either explicitly prohibit payday lending or placed strong restrictions on price caps that effectively prohibit payday lending (Consumer Finance Protection Bureau, CFPB 2020; The Pew Charitable Trust 2022; National Conference of State Legislatures, NCSL 2023). Pappalardo (1997) notes that rather than relying on a single strategy—be it regulations, information dissemination, or consumer education, the three may work together to create complementary effects rather than serve as substitutes.

Our study uses pooled data from the 2009-2023 rounds of the FDIC National Survey of Unbanked and Underbanked Households. We utilize variations in the timing and presence of state-required PF coursework to estimate its effect on the likelihood of being unbanked and on the interest in opening a bank account among unbanked individuals. Using a two-way fixed effects (TWFE) model, our findings show that mandated financial education coursework is associated with a reduced likelihood of being unbanked. Moreover, among the unbanked population, the PF education is associated with a reduced likelihood of being uninterested in opening a bank account in the future.

Using heterogeneity in state-level payday lending regulations, we examine the differential impact of mandated financial education on financial inclusion, shedding light on whether financial education programs and payday lending regulations are complementary or substitute policy practices in determining banking behavior. In the states that prohibit or effectively prohibit payday lending (henceforth, effectively restrict), PF education mandates decrease the likelihood of being unbanked as well as the likelihood of being uninterested in banking across the two regulatory environments. The decrease is slightly larger in states that effectively restrict payday lending, suggesting that the two policies are more likely to be complements than substitutes.

Our study contributes to existing literature via three avenues. First, we build on the deep and evolving literature on state-mandated financial education and downstream behaviors by studying banking behavior (Brown et al., 2016; Burke et al., 2024; Kaiser et al., 2022; Harvey, 2019; Lee et al. 2024; Mangrum 2022; Stoddard and Urban, 2020; Urban et al., 2018).

Second, our study contributes to the vast literature on financial inclusion and specifically to the limited research on the causal effects of financial education on financial inclusion, especially examining the U.S. population. Researchers have conducted field experiments in developing countries to study the effects of educational interventions on financial inclusion and subsequently on financial wellbeing (Agarwal et al. 2022; Dupas et al. 2018; Prina 2015). Examining the U.S. unbanked landscape, Barcellos and Zamarro (2021) found lower financial literacy rates among the unbanked in the U.S.[3]

Third, our study is the first to document the treatment effect of PF mandates on financial inclusion when states effectively restrict payday lending compared to those that do not, allowing us to understand if the education is a complement or substitute to regulation. Ansar et al. (2023) examined global data on unbanked and underbanked consumers, highlighting the role of financial literacy in promoting the effective use of financial services, accompanied by thoughtful product design and strong consumer protections to ensure equitable access to financial services.

The paper has six sections: section 2 provides background for our study; section 3 describes the different data sources used for our analyses; section 4 describes the empirical method used and the model specifications; section 5 presents the results of our analyses; section 6 discusses several robustness checks of our analyses; and finally, section 7 provides a discussion on the economic significance of our effect sizes, the implications of our analyses, and concludes.

2. Background

According to the 2023 FDIC survey data, nearly 96 percent of households are banked in the U.S., reflecting several concerted efforts by public and private entities to expand access to financial services[4]. An increasing number of financial institutions now offer accounts with no or low monthly fees and no minimum balance requirements, courtesy of the Bank On initiative.[5] As

[3] More recently, Calem et al. (2025) conducted a correlational analysis finding that states with lower financial literacy scores, as measured by reading and math proficiency, are more likely to have lower unbanked rates.
[4] Toh (2022) compiled a list of programs offered by Federal agencies and the U.S. Department of Treasury to support federally regulated financial institutions that has been offering depository accounts mainly to racial minority and low-to-middle-income consumers, such as, the Minority Depository Institution program run by the FDIC, the Community Development Financial Institutions Program supported by the U.S. Treasury, and the Low-Income Credit Union designation offered by the National Credit Union Administration.
[5] The Bank On initiative, led by the Cities for Financial Empowerment (CFE) Fund, is a national movement to connect unbanked and underbanked individuals to safe, affordable, and functional bank accounts (with features such

of 2023, Bank On-certified accounts have been opened in about 90 percent of all U.S. zip codes (Locke and Chalise, 2024). Among the unbanked households, the FDIC (2024) survey reports that the majority (66 percent) rely solely on cash for transactions, while a third (34 percent) use nonbank transaction accounts.[6] However, Toh (2023) found that nonbank transaction accounts have a limited capacity to serve the unbanked population, and a majority of them still resort to paper-based transaction products or rely mainly on cash. This suggests that a portion of the unbanked population still isn't served by existing financial services options.

Additionally, the Consumer Financial Protection Bureau proposed a rule in 2024 aiming to reduce "junk fees," e.g., overdraft fees and non-sufficient fund fees associated with holding a bank account scheduled to go into effect on October 2025 (CFPB 2024). Di Maggio et al. (2025) found that when banks put a pause on aggressive practices concerning their account overdraft rules, the low-income populations benefit by reducing payday borrowing, increasing consumption, realizing long-term improvements in financial health, and gaining access to the mainstream financial system. In October 2024, the U.S. Department of Treasury published the National Strategy for Financial Inclusion in the United States. One of its five main objectives is to promote access to transaction accounts that meet consumer needs, with a prediction that consumers will likely gain greater access to transaction accounts through government payments and private-sector innovation.

Along with the prominence of nonbank transaction accounts, a growing body of literature documents how fintech and nonbank financial institutions cater to the credit needs of financially underserved consumers, much more than traditional financial institutions (Dolson and Jagtiani 2024; Jagtiani and Lemieux 2018). Relatedly, Jagtiani and Lemieux (2017) found that alternative sources for assessing credit readiness assist consumers in obtaining credit at a reduced cost, who would otherwise be excluded, enhancing financial inclusion. However, Di Maggio and Yao (2021) found that fintech lenders lending money to high-risk borrowers as well as low-risk borrowers integrate this risk in their pricing strategies.

Some research suggests that banks may benefit from tailoring services and products to the needs and concerns of the unbanked population in the U.S. for successful financial inclusion.

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as, low or no fees and no overdrafts). It works through local coalitions of government, financial institutions, and community groups.

[6] Nonbank transaction accounts refer to prepaid card accounts or accounts with digital payment services such as PayPal, Venmo, or Cash App or other online payment service providers.

Hayashi et al. (2023) studied two groups of unbanked households: those who are interested and previously banked and those who are uninterested and have never been banked. The study highlights the different motivations and barriers that each group faces regarding banking access. Using a similar concept, Falcettoni and Nygaard (2024) propose redefining the term unbanked to those who do not have a bank account and would like to have one (the "unbanked") and individuals who do not have a bank account and are not interested in having one (the "out of banking population") highlighting that understanding the specific reasons for non-participation between the two groups allow for more targeted and effective interventions.

Among the unbanked, the most reported reasons for being unbanked remain consistent as in the past years - "not enough money to meet minimum requirements" followed by "do not trust banks" (FDIC 2024). The final objective of the National Strategy for Financial Inclusion (U.S. Department of Treasury 2024) emphasizes the importance of financial education to build trust in the financial system and protect consumers from illegal and predatory activities, thus underscoring the need for financial education and the dissemination of information in simple and understandable language.

We suspect that the key mechanisms on how PF coursework can influence banking behavior is that financial education may increase awareness of the availability of low-cost banking options, improve understanding of financial products and services, and build confidence in managing finances. It may also reduce anxieties or misconceptions about banks and the banking process, directly increasing engagement and interest in banking services. Additionally, in states with payday lending restrictions, lower exposure to AFS institutions in one's neighborhood may help avoid familial patterns in payday lending use. At the same time, effectively restricting payday lenders could reduce the importance of financial education in schools. Thus, this is an open question empirically.

3. Data

3.1. FDIC National Survey on Unbanked and Underbanked Households

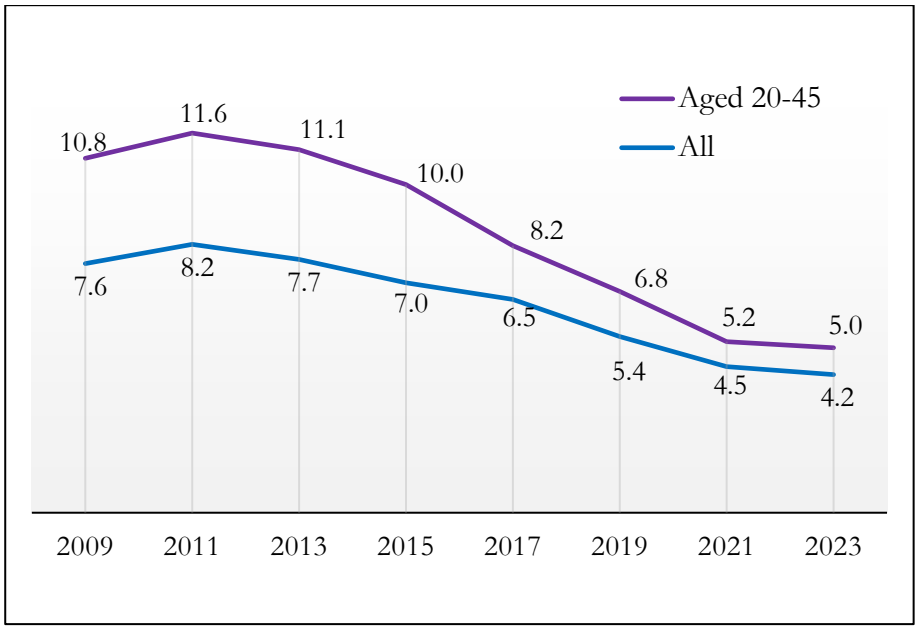
We use eight rounds of the FDIC survey data from 2009 through 2023. The FDIC survey is a biennial survey conducted since 2009 as a supplement to the U.S. Census Bureau's Current Population Survey. The FDIC survey has a large (more than 30,000 households were included each year) sample that is weighted to be nationally representative. The respondents are asked

about their banking status and use of AFS, including prepaid cards, nonbank financial transactions, and credit services.[7] The survey also gathered information on each household's characteristics from all respondents. Sociodemographic characteristics include income, education, age, race, employment status, disability status, homeownership, marital status, and more. Geographic characteristics include state, metropolitan statistical area, and proximity to urban areas. Respondents aged 15 and above are accepted to participate in this survey as long as they have some involvement in the household's financial decision-making.

The FDIC data do not have information on the state where the respondents attended high school. We restrict the sample to adults under age 45, as their likelihood of moving out of their high school state is relatively low (Brown et al., 2015; Molloy, Smith and Wozniak, 2011).[8] Our main analysis considers those who have at least graduated high school. Since many U.S. states mandated PF education starting in the 2000s (see Figures 3-4), we also factor in the age of respondents who are likely to be "treated" by the PF when designing our age restrictions. We use a pooled sample of 137,874 households from the eight rounds of the FDIC survey, with high school graduates between 20 and 45 years old.

[7] Since the questions on AFS, prepaid cards, and nonbank financial transactions are not always asked or are not consistent in the way they were asked over time, we do not use them in our analysis as direct outcomes.
[8] Brown et al. (2016) found that the probability of adults living in the same state from ages 18 through 29 is 82 percent. Molloy et al. (2011) report that only 4 percent of 18–24-year-olds moved to another state, and about 3 percent of 25–44-year-olds moved to a new state over the same period. Further, Conzelmann et al. (2025) found that the share of in-state college graduates staying in state post-graduation is about 70 percent on average, depending on the type of college degree and the institution.

Figure 1: Unbanked rates in the U.S. (2009-2023)

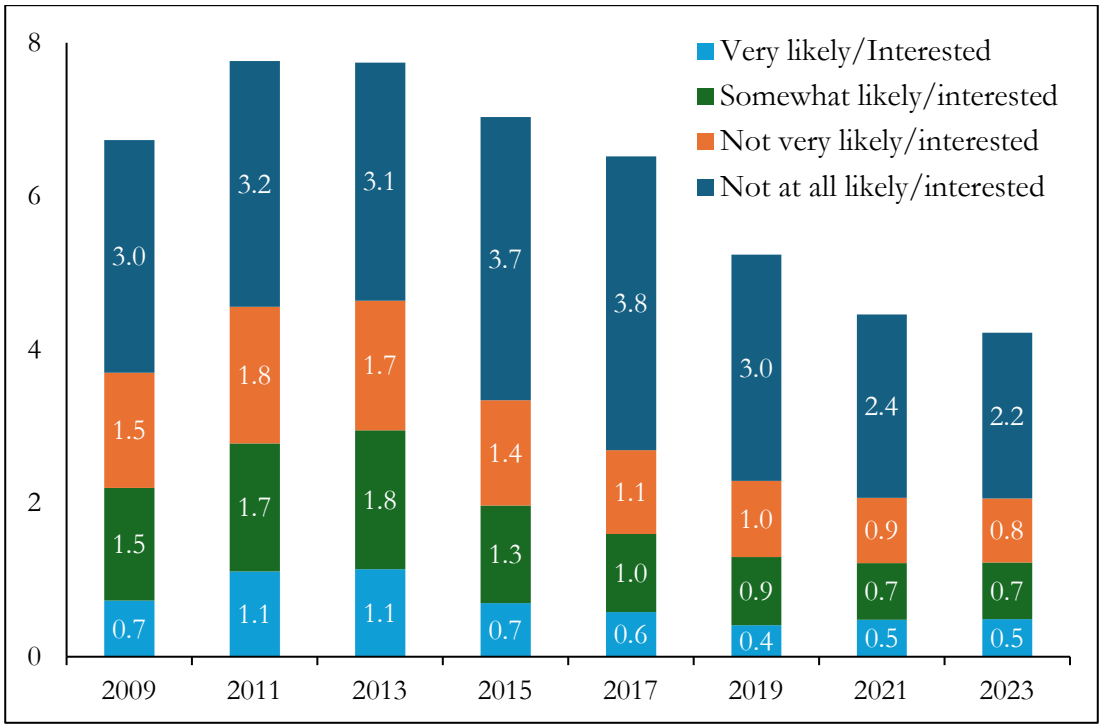


Source. FDIC Survey of Unbanked and Underbanked Households

Figure 1 plots the unbanked rate from the FDIC survey as it steadily decreased from 8.2 percent in 2011 to 4.2 percent in 2023 (blue line). When considering the respondents aged 20-45, the share of unbanked rate more than halved between 2011 and 2023 (purple line). Unbanked households in the FDIC survey were also asked about their reasons for not having a bank account, their previous banking status, and their interest in (or likelihood of) having a bank account.[9] Among the unbanked population, the shares of those uninterested in banking are shown in Figure 2. Since 2015, at least half of the unbanked population reported that they are not at all likely/interested to have a bank account. The trend remains the same for those aged 20-45. For our analysis of banking intentions, we use a pooled sample of 5,999 unbanked households (high school graduates, 20 and 45 years old) from the eight rounds of the FDIC survey.

[9] The FDIC survey question regarding bank account access intention underwent a key change in 2019. The question in the survey asks, "How likely are you or anyone in your household in having a bank account?" the respondents can choose from four choices ranging from "not at all likely" to "very likely" and were asked in the 2009 - 2017 rounds of the survey. The new version of the question (2019 – 2023) asks, "How interested are you or anyone in your household in having a bank account?" and the respondents can choose from four choices ranging from "not at all interested" to "very interested".

Figure 2: Decomposing the future banking intentions of the unbanked (2009-2023)



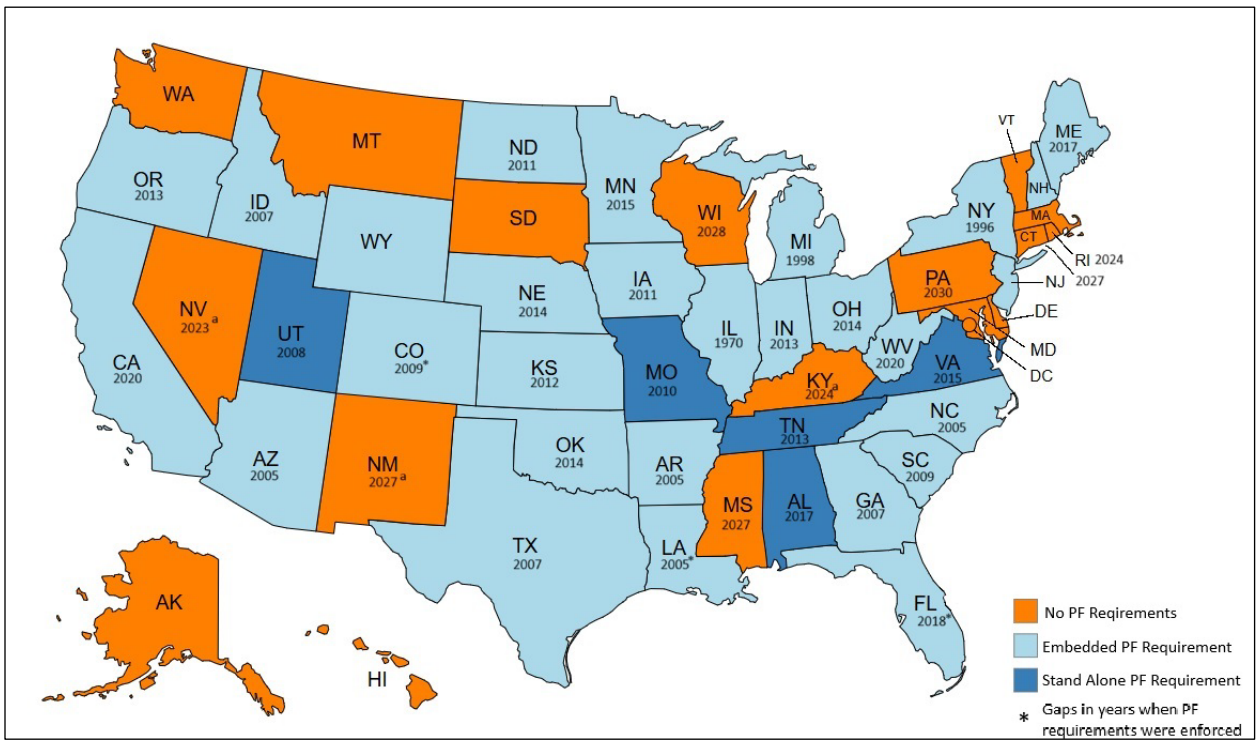
Source. Authors' calculations and FDIC Survey of Unbanked and Underbanked Households

3.2. State-mandated PF coursework

Our study builds upon previous research examining the impact of high school PF education requirements on several financial decisions (Brown et al. 2016; Harvey 2019; Stoddard and Urban 2020; Urban et al. 2020; Mangrum 2022; Harvey and Urban 2023; Lee et al. 2024). We use a similar methodological approach as prior work and incorporate updated mandate data (see Figures 3-4). As of 2020, more than 30 U.S. states had some form of PF education as a graduation requirement, with most policies implemented after 2000. Conversely, 17 states and the District of Columbia did not any have such requirements (Urban 2024a). These requirements can be met through standalone courses covering PF topics, embedded courses that integrate topics within other subjects (e.g., economics, math), or designated standards embedded in broader curricula (e.g., social studies).

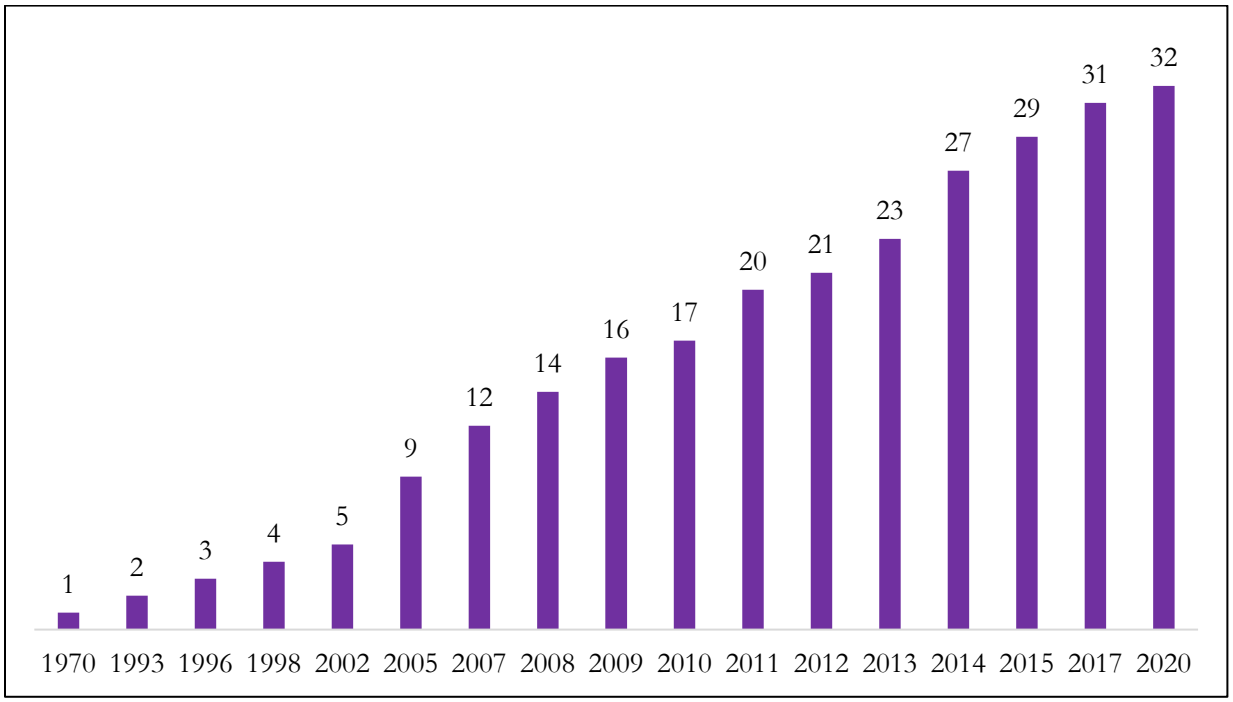
Topics covered in PF sources range from providing an understanding of credit scores, using credit cards, maintaining monthly budgets, understanding debt and interest rates, the importance of longer-term financial planning, investing, and associated risks, and related information needed to be a knowledgeable consumer in the U.S.

Figure 3: U.S. states with any PF requirement for high school graduation



Source. Urban (2024a).

Figure 4: Number of states with PF requirement for high school graduation



Source. Urban (2024a) and authors' calculations.

3.3.State Payday Lending Regulations

State authorities in the U.S. have taken the responsibility of safeguarding consumers from predatory payday lenders. Triangulating data from several sources such as the Federal Register on the CFPB's proposed rule on payday and auto title loans (2020), the Pew Charitable Trust (2022), Leal (2018), and the NCSL (2023), we list the three broad types of regulations surrounding payday lending in the U.S. (shown in Table 1). The three types are - first, states that explicitly prohibit payday lending. Second, they are states that allow for payday lending but have caps on interest rates which effectively result in a ban on payday lending as it is not profitable for the lender to operate in these states. The first and the second category together account for 18 states along with DC that have placed straight strictions on payday lending. Finally, there are thirty-two states that allow payday lending without any strict regulation.[10]

Table 1: Payday lending regulations across U.S. states

Legality	States
Prohibited and small loan interest rates capped	AR, AZ, CT, DC, GA, MD, NJ, NM, NY, VT, WV
Payday lending allowed with interest rates capped	IL, MA, ME, MT, NC, NE, NH, OR, PA, SD
Payday lending allowed without regulations	AL, AK, CA, CO, DE, HI, FL, ID, IN, IA, KS, KY, LA, MI, MN, MO, MS, NV, ND, OK, OH, RI, SC, TN, TX, UT, VA, WA, WI, WY

Source. Leal (2018), The Pew Charitable Trust (2022); NCSL (2023), and Rhodes et al. (2024)

For our analysis, we consider states that have consistently maintained strong payday lending restrictions over the period of 1998 to 2020 and the states that did not (Rhodes et al. 2024). Eight states had effectively restricted payday lending by 1998, and this number rose to

[10] Payday lending restrictions do not necessarily prevent borrowers from accessing payday loans as there are online payday lenders who would cater to their borrowing needs, and state restrictions can be particularly difficult to enforce on these lenders (Harvey et al. 2024). Further, Constantine and Farahi (2025) note that online payday lending outpaced storefront payday lending during COVID-19 pandemic and afterward.

twelve states by 2010, we also consider these twelve states as part of our robustness checks.[11] This criterion allows us to leverage heterogeneity in state-level payday lending restrictions for examining the differential impact of mandated financial education on banking behavior.

4. Empirical Methods

Over time, states have implemented policies requiring students to complete a PF course before graduation from high school. We exploit the variation in state-mandated PF education coursework requirements in high schools to estimate the causal effect of financial education mandates on banking behaviors.

We estimate a two-way fixed effects (TWFE) model to analyze the effect of state-mandated financial education on individuals' banking behavior. The approach compares the changes in banking trends over time across states that implemented financial education mandates (treatment group) and states that did not (control group). The TWFE model accounts for unobserved time-invariant characteristics at both the state and year levels, strengthening the identification of the causal effect.[12]

$$Y_{isyt} = \alpha_0 + \alpha_1 FinEd_{sy} + \alpha_2 X_i + \beta_t + \gamma_s + \delta_y + \epsilon_{isyt} \quad (1)$$

We estimate equation 1. *i* refers individual *i* in state *s* who was 18 in year *y* and responding in survey year *t*. Our dependent variable Y_{isyt} reflects whether the respondent is unbanked (household has no checking or savings account at a bank). When the unbanked population is considered in particular, Y_{isyt} reflects whether the respondent is uninterested in opening a bank account in the future. In the FDIC survey, the question asked to the unbanked respondents regarding their future banking intentions changed in the 2019 round of the survey. $FinEd_{sy}$ equals one if respondents residing in the given state *s* were required to complete PF content in a standalone course, an embedded course, or as part of their state standards and zero if

[11] According to Rhodes et al. (2024), CT, GA, MD, NJ, NY, PA, VT, WV were the 8 states that effectively restricted payday lending since 1998, followed by NC, DC, AR, and AZ by 2010.

[12] Recent research has brought to light potential issues with the two-way fixed-effects (TWFE) model (Goodman-Bacon 2021; Sun and Abraham 2021; Callaway and Sant'Anna 2021; Baker, Larcker and Wang 2022). However, our analysis addresses these concerns. A Bacon decomposition, as outlined by Goodman-Bacon (2021), reveals that 70 percent of our observed variation originates from comparisons between newly treated states and states that were never treated, largely mitigating the identified problems. Furthermore, we neither observe nor anticipate heterogeneity in the effect sizes. To further validate our findings, in section 5.2, we constructed a natural experiment using four states whose requirements were initiated in 2005, and our results remained consistent with those reported in Section 3.

otherwise. We consider two individual specific control variables—race and whether born in the U.S. as these were determined before the policy went into place.[13] All models include state fixed effects γ_s, survey year fixed effects β_t, and δ_y captures the graduating year fixed effects in which an individual *i* turns 18. To examine the heterogeneity in the treatment effects stemming from states that effectively restrict payday lending versus those that do not, we estimate equation (1) in two instances: for the states that have always prohibited payday lending from 1998 through 2020 and then for the states that did not.

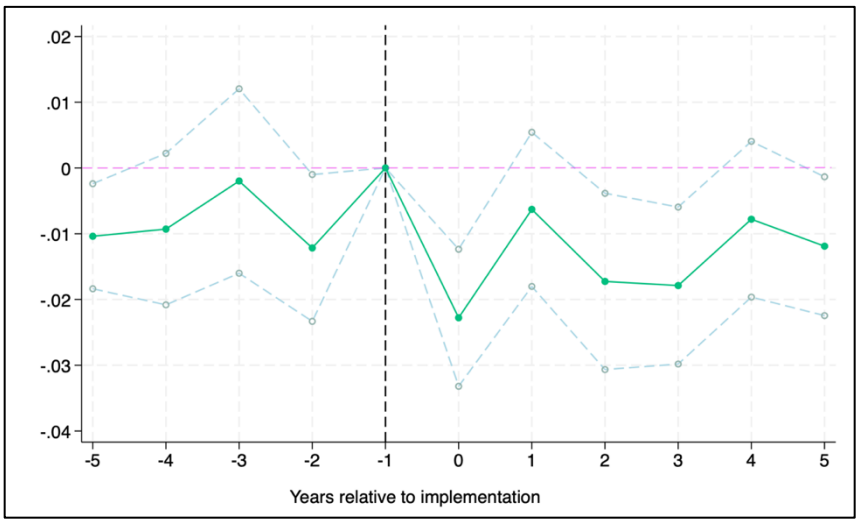
We validate the parallel trends assumption, crucial for difference-in-difference (DD) empirical strategies through event studies. Difference-in-difference (and TWFE) models require that the trends of the treatment and control groups would have been parallel in the absence of the policy. While not directly testable, we use event study specifications to show that there is no clear evidence of a non-parallel trend in anticipation of the policy. An event study approach requires that no other factors systematically changed while states implemented PF graduation requirements. The legislative process often delays implementation, many states have pursued similar education policies regardless of political affiliation, and previous research indicates no significant economic conditions are linked to the adoption of financial education requirements (Stoddard and Urban 2020).

The event study plots are shown in the Figures 5-6. We plot the event study graph showing the coefficients for the difference between the treatment and control in each period, excluding the period just before the policy went into effect, as well as 95 percent confidence intervals. The event study plot also presents evidence supporting the assumption of parallel trends, showing no differential trend before the implementation of the PF education mandates. Figure 5 shows that the effect of mandatory financial education at high school reduces the probability of being unbanked among all the cohorts post-implementation of PF requirements. The event study in Figure 6 indicates that among the unbanked, the effect of mandatory financial education at high school reduces the probability of being uninterested in opening a bank account in the future in all the cohorts post-implementation of PF requirements.[14]

[13] Gender in the FDIC survey data is tied to household composition, and thus we do not include it.

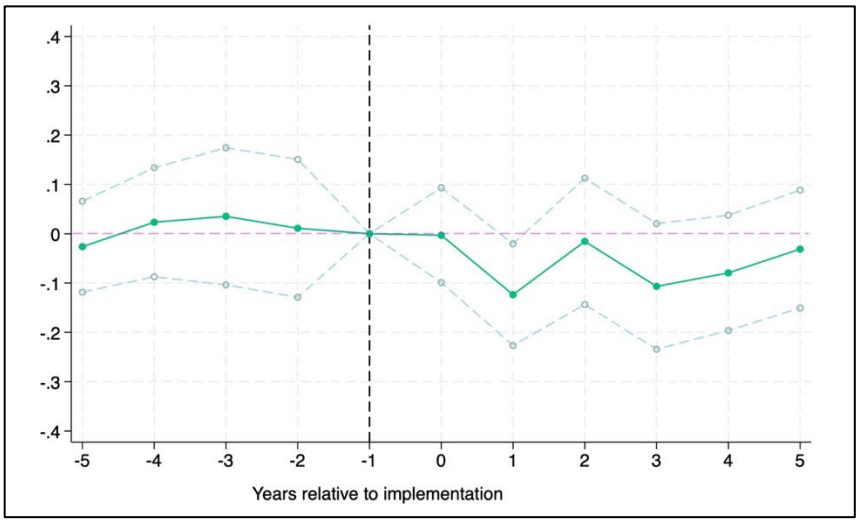
[14] Among the unbanked, the effects of PF education on being not at all "likely" or "interested" in banking considering the change in question in the FDIC survey is provided in the Appendix. Table A1 provides the effects of financial education separately for 2009-17 (unlikely) and 2019-23 (uninterested) in opening a bank account. Figures

Figure 5 - Event study graph – being unbanked



Note. Event study for the outcome - being unbanked. Coefficients with 95% confidence errors depicted, where robust standard errors are clustered at the state level. All models are linear probability models that estimate an event study specification. The models omit t-1 and include all controls from equation (1).

Figure 6 - Event study graph – being uninterested in banking



Note. Event study for the outcome - being uninterested in banking. Coefficients with 95% confidence errors depicted, where robust standard errors are clustered at the state level. All models are linear probability models that estimate an event study specification. The models omit t-1 and include all controls from equation (1).

A1a-b show the event study plots for the same. We also relax the definition of disinterest in banking by including the unbanked population "not very" interested in banking in addition to the "not at all" interested. Table A2 shows the same effects for the unbanked population who are "somewhat" uninterested in banking.

5. Results

Our analysis reveals a statistically significant impact of state-mandated PF requirements on individuals' banking decisions. Exposure to mandated high school PF coursework reduces the likelihood of being unbanked by 0.6 percentage points, or a 14 percent reduction (Table 2, Column (1)). The estimate is statistically different from zero at the 1 percent level.

Among the unbanked population, the mandated PF coursework requirements decrease the probability of being uninterested in opening a bank account in the future by about five percentage points (Table 2, Column (2)). Although the magnitude of this estimate is economically meaningful, it is not statistically different from zero.

Table 2: Effects of PF education on being unbanked and uninterested in banking

	(1) Unbanked	(2) Uninterested
Financial education	-0.006***	-0.050
	(0.002)	(0.035)
Outcome mean	0.044	0.370
Percentage effect	-13.6%	-13.5%
Observations	137,874	5,999
R-squared	0.040	0.057

Note. Robust standard errors clustered at the state level in parentheses. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. We estimate equation (1). Dependent variables reflect whether the respondent is unbanked or not in Column (1) and if unbanked, uninterested to open a bank account or not in Column (2). Financial Education equals one if the respondent lives in state where financial education was required for high school graduation at the time they were 18 and zero if otherwise. Fixed effects are included for states and graduation years. Race, citizenship at birth, and survey years are the control variables included in the regression model.

5.1. Heterogeneous treatment effects: State restrictions on payday lending

Do our results vary by payday policy environment? Exposure to mandated high school PF coursework reduces the likelihood of being unbanked for respondents in states that effectively restrict payday lending since 1998 and in the remaining states that allow for payday lending (Table 3 Columns (1) and (2)). The effect is modestly larger in states that effectively restrict payday lending (a 0.9 percentage point, or 20 percent decrease) than in states allowing payday lending (a 0.7 percentage point, or 16 percent decrease). This provides evidence that consumer protection and financial education are more likely to be complements than substitutes for young adults.[15]

Among the unbanked population, the mandated PF coursework requirements decrease the probability of being uninterested in opening a bank account in the future, both in the presence and absence of strict payday lending regulations (Table 4 Columns (1) and (2)). Again, the negative effect is larger for states that effectively restrict payday lending than in the case of those that do not. Although the magnitudes of these estimates in Columns (1) and (2) are economically meaningful, the sample size gets smaller when we consider only the states with strict payday lending restrictions to produce statistically reliable estimates in Column (1) and the estimate in Column (2) is not statistically different from zero at the 1 percent level.

[15] The results in Table 2 remain qualitatively same when we restrict the sample to high school graduates 1998 onwards as shown in Table A3. Further, Tables A4-5 in the Appendix show the heterogenous treatment effects on unbanked and disinterest in banking considering the states that effectively restricted payday lending since 2010.

Table 3: Heterogeneous treatment effects on being unbanked by payday lending restrictions

	(1) Unbanked, in states with payday lending effectively restricted	(2) Unbanked, in states allowing payday lending
Financial education	-0.009*	-0.007**
	(0.004)	(0.003)
Outcome mean	0.046	0.044
Percentage effect	-19.6%	-15.9%
Observations	12,304	67,605
R-squared	0.045	0.043

Note. Robust standard errors clustered at the state level in parentheses. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. We estimate equation (1). The dependent variables reflect if a respondent is unbanked or not, based on whether they live in states with effective restrictions on payday lending (Column (1)) or in states allowing it (Column (2)). Financial education equals one if the respondent lives in state where financial education was required for high school graduation at the time they were 18 and zero if otherwise. Fixed effects are included for states and graduation years. Race, citizenship at birth, and survey years are the control variables included in the regression model.

Table 4: Heterogeneous treatment effects on being uninterested in banking by payday lending restrictions

	(1) Uninterested, in states with payday lending effectively restricted	(2) Uninterested, in states allowing payday lending
Financial education	-0.101**	-0.073
	(0.051)	(0.045)
Outcome mean	0.354	0.372
Percentage effect	-28.5%	-19.6%
Observations	569	2,959
R-squared	0.129	0.070

Note. Robust standard errors clustered at the state level in parentheses. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. We estimate equation (1). The dependent variables reflect if a respondent is uninterested in banking or not, based on whether they live in states with effective restrictions on payday lending (Column (1)) or in states allowing it (Column (2)). Financial education equals one if the respondent lives in state where financial education was required for high school graduation at the time they were 18 and zero if otherwise. Fixed effects are included for states and graduation years. Race, citizenship at birth, and survey years are the control variables included in the regression model.

6. Robustness Checks

6.1. Alternative measure of PF education

As a robustness check of our original analyses in section 4, we reanalyze eq. (1) using a continuous measure of PF requirements ranging from 0 to 1 considering the variations in the PF education requirements as reported in Urban and Luedtke (2024). The continuous measure of PF education requirements is calculated based on a hand-collected dataset of over 19,000 high school PF courses for over 7,400 high schools across the U.S. (for more details, see Luedtke and Urban 2024). Although access to PF courses has increased in recent years, not all schools in the mandated states require their students to take these courses. Urban (2024b) reported that about 43 percent of students receive PF education through a required course, either as a standalone subject (27 percent) or integrated into another course (16 percent). However, the largest group of students (37 percent) attends schools where personal finance is only available as an elective. A small minority (6 percent) of students are in schools offering no personal finance content.

We observe similar results as in Table 2 using the continuous measure as with our primary specification. Exposure to mandated high school PF coursework reduces the likelihood of being unbanked by 0.8 percentage points, or an 18 percent reduction (Table 5, Column (1)). The estimate is statistically different from zero at the 1 percent level. Among the unbanked population, the alternative measure of mandated PF coursework requirements decreases the probability of being uninterested in banking (see Table 5, Column (2)) by 13 percentage points, amounting to a 35 percent reduction in uninterested rate. This estimate is statistically significant at a 5 percent level. The estimates in Table 5 are consistent with those in Tables 2-3, though much more precisely estimated.

Table 5: Effects of PF education (alternative measure) on being unbanked and uninterested in banking

	(1) Unbanked	(2) Uninterested
Financial education	-0.008***	-0.131**
	(0.003)	(0.050)
Outcome mean	0.044	0.370
Percentage effect	-18.2%	-35.4%
Observations	137,874	5,999
R-squared	0.040	0.058

Note. Robust standard errors clustered at the state level in parentheses. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. We estimate equation 1. Dependent variables reflect whether the respondent is unbanked or not in Column (1) and if unbanked, uninterested to open a bank account or not in Column (2). Financial education ranges from 0 to 1 based on the extent of the PF requirements for high school graduation in a particular state for respondents when they were 18. Fixed effects are included for states and graduation years. Race, citizenship at birth, and survey years are the control variables included in the regression model.

6.2. Placebo test

To further assess the robustness of our estimates, we conducted a placebo test by considering the respondents who reported that they did not graduate high school, as they are least likely to have had the coursework. Column (1) of Table 6 shows that PF education mandates positively affect the likelihood of being unbanked. Among the unbanked population, PF education mandates also positively affect the likelihood of being uninterested in banking. However, neither of these estimates from the placebo tests, in Columns (1) and (2) of Table 6, are statistically different from zero. The placebo tests reinforce our primary analyses, with the caveat that sample sizes are smaller than for the full analysis.

Table 6: Effects of PF education on being unbanked (1) and uninterested (2) in banking for those who did not complete high school

	(1) Unbanked	(2) Uninterested
Financial education	0.022	0.013
	(0.018)	(0.042)
Outcome mean	0.248	0.449
Percentage effect	8.9%	2.9%
Observations	12,122	3,007
R-squared	0.071	0.072

Note. Robust standard errors clustered at the state level in parentheses. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. We estimate equation (1). Dependent variables reflect whether the respondent is unbanked or not in Column (1) and if unbanked, uninterested to open a bank account or not in Column (2). Financial education equals one if the respondent lives in state where financial education was required for high school graduation at the time they were 18 and zero if otherwise. Fixed effects are included for states and graduation years. Race, citizenship at birth, and survey years are the control variables included in the regression model.

6.3. PF education mandates in 2005 only

For our main analyses, we use the TWFE model which has faced criticism due to issues related to staggered treatment timing and/or heterogeneous treatment effects (Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Sun and Abraham 2021; Baker, Larcker and Wang 2022). The critiques are the following – first, different states adopt mandates at different times. Second, treatment effects are likely to vary across states since the mandates differ. And third, treatment effects change over time, as the impact of mandates tends to grow. To assess whether our results are sensitive to these biases, and to what extent, we replicate the analysis from section 4 but focus exclusively on the states that implemented a mandate for students graduating in 2005.

We create a natural experiment with four states whose requirements began in 2005 (Arizona, Arkansas, Louisiana, North Carolina) as the treatment group compared to the states that never implemented PF education mandates, our results remain consistent. We note that the PF education mandates have a negative effect on the likelihood of being unbanked. Among the unbanked population, the PF education mandates have a negative impact on the likelihood of

being uninterested in banking. Both these results are qualitatively as well as quantitatively similar to what we obtained in section 5, thus supporting our results and the methodology used.[16]

Goodman-Bacon (2021) shows that the TWFE estimator is a weighted average of each possible two-by-two DD comparison combination, some use previously treated units as "control" units for later treated units. A Bacon decomposition in our case reveals that 70% of our observed variation originates from comparisons between newly treated states and states that were never treated, largely mitigating the identified problems.

Table 7: Effects of PF education on being unbanked (1) and uninterested (2) in banking considering only states that mandated PF education in 2005 vs. never treated

	(1) Unbanked	(2) Uninterested
Financial education	-0.007**	-0.020
	(0.003)	(0.083)
Outcome mean	0.041	0.380
Percentage effect	-17.1%	-5.3%
Observations	52,461	2,147
R-squared	0.043	0.091

Note. Robust standard errors clustered at the state level in parentheses. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. We estimate equation (1). Dependent variables reflect whether the respondent is unbanked or not in Column (1) and if unbanked, uninterested to open a bank account or not in Column (2). Financial education equals one if the respondent lives in state where financial education was required for high school graduation at the time they were 18 and zero if otherwise. Fixed effects are included for states and graduation years. Race, citizenship at birth, and survey years are the control variables included in the regression model.

7. Conclusion

This study finds that state-mandated PF education in high school is associated with greater financial inclusion. Further, it highlights that regardless of the regulatory environment—whether a state does or does not effectively ban payday lending—financial education reduces the likelihood of being unbanked. In fact, the magnitude of the effect is larger in states with bans,

[16] Following Mangrum (2022), we also conduct the same analyses using the four states whose PF requirements began in 2005 compared to the never treated and the not yet treated states. Our results remain consistent.

providing evidence that financial education and consumer protection can work in tandem for young adults gaining their financial independence.

How big is our documented effect? Financial education reduces the likelihood of being unbanked by 0.6 percentage points among 20–45-year-olds from 2009-2023. Thus, we estimate that graduation requirements reduced the number of unbanked people by roughly 134,000.[17] With the unbanked rate among this population at the beginning of our sample at 10.8% and falling to 5.0% at the end of our sample, the education accounts for 12 percent of the trend.

While many efforts to increase financial inclusion require a strategy to reach the unbanked, high school is one place to reach everyone where they are before they gain their financial independence. Thus, its effects may differ from other targeted interventions later in life.

[17] We calculate this by first taking the population of 20–45-year-olds and the percent of unbanked people in that age range from 2009 and then reducing the percentage of unbanked people by 0.6 percentage points. We scale the decrease by the fraction of treated individuals in the sample (20 percent).

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Appendix

A.1. Robustness Check: Addressing the change in the FDIC survey question about interest in banking

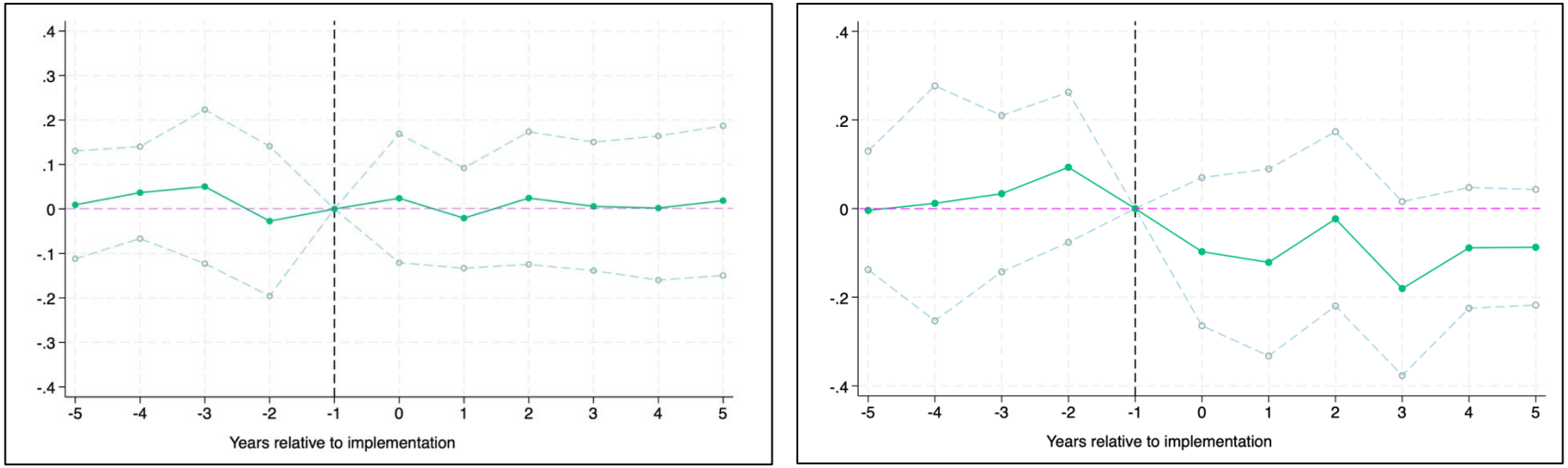
We address the change in the wording of the FDIC survey question asking about the unbanked respondents' intention to open a bank account. In our primary analyses, we consider both “not at all likely” and “not at all interested” to be uninterested in banking. In this section, we conduct the same analyses as specified in section 4, first, for the 2009 - 2017 survey waves as these rounds ask the unbanked respondents how “likely” they are to have a bank account, and then for the 2019 - 2023 survey waves, asking the unbanked respondents how “interested” they are to have a bank account We note that exposure to PF education decreases the likelihood of being uninterested in both cases and is consistent with our findings in section 5.

Table A1: Effects on being unlikely (2009-17) or uninterested (2019-23) in banking

	(1) Unlikely (2009-2017)	(2) Uninterested (2019-2023)
Financial education	-0.034	-0.104*
	(0.035)	(0.056)
Outcome mean	0.346	0.468
Percentage effect	-9.8%	-22.2%
Observations	4,829	1,170
R-squared	0.055	0.090

Note. Robust standard errors clustered at the state level in parentheses. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. We estimate equation (1). Dependent variables reflect whether the respondent is uninterested in banking or not using 2009-2017 rounds of the FDIC survey in Column (1) and using 2019-2023 rounds of the survey in Column (2). Financial education equals one if the respondent lives in state where financial education was required for high school graduation at the time they were 18 and zero if otherwise. Fixed effects are included for states and graduation years. Race, citizenship at birth, and survey years are the control variables included in the regression model.

Figure A1a-b: Event study graph - being unlikely (2009- 2017) or uninterested to open a bank account (2019- 2023)



Note. Event study for the outcome - being uninterested in banking. Coefficients with 95% confidence errors depicted, where robust standard errors are clustered at the state level. All models are linear probability models that estimate an event study specification. The models omit t-1 and include all controls from Eq. (1).

A.2. Robustness Check: Alternative measure of "uninterested" in banking

We also relax the definition of being uninterested in banking. In our primary analyses, we consider "not at all' likely/interested to be uninterested in banking to reflect those who are not considering having a bank account at all. We considered an extreme level of disinterest in banking, and this segment of the population may be too disengaged to be effectively banked. As a robustness check, we consider "not very" as well as "not at all' likely or interested as our adjusted definition of uninterested in banking. We note that exposure to PF education decreases the likelihood of being somewhat uninterested in banking by 2.5 percentage points. However, the estimate is not statistically different from zero. This result is also consistent with our findings in section 5.

Table A2: Effects of financial education on being "somewhat" uninterested in banking

	Somewhat uninterested
Financial education	-0.025
	(0.028)
Outcome mean	0.566
Percentage effect	-4.4%
Observations	5,999
R-squared	0.052

Note. Robust standard errors clustered at the state level in parentheses. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. We estimate equation (1). Dependent variables reflect whether the respondent is somewhat uninterested in banking or not. Financial education equals one if the respondent lives in state where financial education was required for high school graduation at the time they were 18 and zero if otherwise. Fixed effects are included for states and graduation years. Race, citizenship at birth, and survey years are the control variables included in the regression model.

A.3. Robustness Check: Effects of PF education restricting sample to 1998 onwards

Since we examine the heterogenous treatment effects using the differences between states that have strict payday lending restrictions since 1998 versus those that allow payday lending, we conduct a robustness check running the same analyses as shown in Table 2 by restricting the sample to only include high school graduates between 25-40 years who graduated 1998 onwards. The estimates shown in Table A3 are consistent with the estimates in Table 2.

Table A3: Effects of PF education on being unbanked and uninterested in banking (1998 onwards)

	(1) Unbanked	(2) Uninterested
Financial education	-0.007***	-0.073*
	(0.003)	(0.041)
Outcome mean	0.044	0.369
Percentage effect	-15.8%	-19.8%
Observations	79,909	3,530
R-squared	0.043	0.069

Note. Robust standard errors clustered at the state level in parentheses. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. We estimate equation (1). Dependent variables reflect whether the respondent is unbanked or not in Column (1) and if unbanked, uninterested to open a bank account or not in Column (2). Financial Education equals one if the respondent lives in state where financial education was required for high school graduation at the time they were 18 and zero if otherwise. Fixed effects are included for states and graduation years. Race, citizenship at birth, and survey years are the control variables included in the regression model.

A.4. Robustness Check: State restrictions on payday lending since 2010

For our main analysis as shown in Tables 3-4, we consider states that have consistently maintained strong payday lending restrictions over the period of 1998 to 2020 and the states that did not (Rhodes et al. 2024). Eight states had effectively restricted payday lending by 1998, and this number rose to twelve states by 2010, we also consider these twelve states as part of our additional robustness checks. The estimates shown in Tables A4-5 are similar to the estimates in Tables 3-4 though the sample size gets quite small when analyzing the uninterested in banking or to produce statistically reliable estimates.

Table A4: Heterogeneous treatment effects on being unbanked by payday lending restrictions since 2010

	(1) Unbanked, in states with payday lending effectively restricted	(2) Unbanked, in states allowing payday lending
Financial education	-0.021**	-0.005
	(0.009)	(0.008)
Outcome mean	0.036	0.034
Percentage effect	-58.3%	-14.7%
Observations	3,722	12,704
R-squared	0.048	0.038

Note. Robust standard errors clustered at the state level in parentheses. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. We estimate equation (1). The dependent variables reflect if a respondent is unbanked or not, based on whether they live in states with effective restrictions on payday lending (Column (1)) or in states allowing it (Column (2)). Financial education equals one if the respondent lives in state where financial education was required for high school graduation at the time they were 18 and zero if otherwise. Fixed effects are included for states and graduation years. Race, citizenship at birth, and survey years are the control variables included in the regression model.

Table A5: Heterogeneous treatment effects on being uninterested in banking by payday lending restrictions since 2010

	(1) Uninterested, in states with payday lending effectively restricted	(2) Uninterested, in states allowing payday lending
Financial education	-0.577**	-0.192*
	(0.231)	(0.102)
Observations	135	432
R-squared	0.231	0.176

Note. Robust standard errors clustered at the state level in parentheses. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. We estimate equation (1). The dependent variables reflect if a respondent is uninterested in banking or not, based on whether they live in states with effective restrictions on payday lending (Column (1)) or in states allowing it (Column (2)). Financial education equals one if the respondent lives in state where financial education was required for high school graduation at the time they were 18 and zero if otherwise. Fixed effects are included for states and graduation years. Race, citizenship at birth, and survey years are the control variables included in the regression model.