Can a High School Personal Finance Course Reduce the Gender Gap in Being Unbanked?

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Abstract

Among unpartnered young adults in the United States—whose banking status reflects their own financial decisions rather than a partner's—women are significantly more likely than men to be unbanked, even after accounting for income and education. I determine whether state-mandated high school personal finance courses reduce this gender gap, and if so, through what mechanisms. Using the FDIC National Survey of Unbanked and Underbanked Households (2009–2023) and a heterogeneity-robust difference-in-differences estimator, I estimate that mandates reduce the likelihood of being unbanked for both women and men, but by significantly more for women, narrowing the gender gap by 42.9 percent. The effects are concentrated among women with no education beyond high school and among Black and Hispanic women—groups that face the greatest barriers to financial inclusion. I examine two mechanisms, and the evidence supports both. First, the mandates increase women's objective financial knowledge, although not their self-assessed knowledge. Second, the mandates increase self-employment among employed women, improve the likelihood of attaining middle-income status, and increase college completion. Because the mandates reach individuals before financial independence and do not rely on voluntary participation, mandatory high school financial education can reduce persistent gender disparities in financial inclusion that adult financial education programs have struggled to address.

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1 Introduction

Bank accounts are important gateways to participation in the formal financial system. They allow households to receive income, make payments, accumulate savings, establish credit histories, and access mainstream financial products. However, despite the widespread availability of banking services in the United States, approximately 4.2 percent of households, or about 5.6 million households, remained unbanked in 2023.[1] Among young adults without partners, the unbanked share rises to 10.3 percent. Moreover, within this subgroup, where banking decisions largely reflect individuals' own financial choices rather than those of a partner, a staggering 13.2 percent of women are unbanked, compared to 7.0 percent of men. Much of this gap reflects the fact that single women tend to be worse off financially than their male counterparts. However, even after accounting for differences in income, education, and other observable characteristics, a substantial gender gap remains, suggesting that factors beyond economic resources continue to limit women's access to the banking system. Understanding the sources of this persistent disparity is important for identifying policies that can effectively promote financial inclusion.

In this paper, I ask whether public policy can reduce the gender gap in financial inclusion, by studying the effects of mandatory high school personal finance education on the likelihood of being unbanked. Existing research documents disparities in banking participation across demographic groups, including substantial gender differences, and examines the factors associated with being unbanked (Bogan and Wolfolds, 2022; Hayashi, Routh and Toh, 2024). However, much less is known about the policies that can reduce these gaps. One reason to expect financial education to help is that opening and maintaining a bank account requires understanding financial products, comparing account options, and navigating financial institutions, so improving financial knowledge before adulthood can raise banking participation and narrow the gender gap.

The analysis combines a descriptive decomposition with a causal research design. I first

[1] Author's calculations using the 2023 FDIC National Survey of Unbanked and Underbanked Households.

examine how much of the gender gap in being unbanked can be explained by differences between unpartnered women and men in income, education, household structure, and other observable characteristics. Using a Kitagawa–Oaxaca–Blinder decomposition of the gender gap in unbanked status with data from the Federal Deposit Insurance Corporation (FDIC) survey, I quantify the contribution of each characteristic. Because this decomposition is descriptive and cannot establish whether policies can narrow the gap, I then turn to a specific policy: state mandates requiring students to complete a personal finance course before graduating from high school. Unlike voluntary financial education programs for adults, these mandates reach nearly all students before they become financially independent, avoiding concerns about selective participation. Variation in the timing of mandate adoption across states and graduating cohorts provides the basis for identifying the policy's effect. I combine information on mandate timing with data from the 2009 through 2023 waves of the FDIC National Survey of Unbanked and Underbanked Households and estimate the policy's effect using the extended two-way fixed-effects estimator of Wooldridge (2025), which accommodates heterogeneous treatment effects under staggered policy adoption.

I then examine the mechanisms through which the mandates affect banking outcomes. One possibility is that they improve financial literacy, making young adults better equipped to open and manage bank accounts. Another is that they improve socioeconomic circumstances by increasing educational attainment, employment, and income, thereby making bank account ownership more accessible and valuable. I examine the financial literacy channel using the National Financial Capability Study, which provides a direct measure of financial literacy through the Big Five questions, and the socioeconomic channel by estimating the mandates' effects on educational attainment, employment, and income using the FDIC survey.

I find that mandatory high school personal finance education narrows the gender gap in being unbanked. Exposure lowers men's probability of being unbanked by 1.8 percentage points, or 25.3 percent of the rate among men, and women's by 4.4 percentage points, or 33.5

percent of the rate among women, closing 42.9 percent of the gender gap. The reduction is even larger among groups furthest from the formal financial system to begin with. Among respondents with no education beyond high school, the mandates reduce the gender gap by 62.9 percent, while among Black and Hispanic respondents, they reduce it by 54.2 percent. These larger effects are driven by particularly strong reductions in unbanked status among women in these groups.

The evidence supports both mechanisms. Relative to men, exposure increases women's financial knowledge score by an additional 0.13 points, or 5.9 percent of the mean, with the gains concentrated in the two investment-related questions. By contrast, exposure has no differential effect on women's self-assessed financial knowledge, suggesting that the mandates change what women know rather than how knowledgeable they believe themselves to be. The evidence also points to improvements in women's socioeconomic circumstances relative to men. The mandates increase women's likelihood of engaging in unincorporated self-employment by an additional 1.3 percentage points, equivalent to a 35.7 percent increase relative to the mean, and increase their likelihood of living in a household with income between $50,000 and $75,000 by 23.5 percent. They also increase women's probability of completing a bachelor's degree by 7 percent.

These findings suggest that mandatory high school personal finance education can be an effective policy tool for expanding financial inclusion among young adults by reducing barriers to banking participation. The larger gains among women indicate that universal financial education can also narrow gender disparities in financial inclusion. At the same time, the weaker effects for men suggest that a uniform curriculum may not benefit all students equally, highlighting the importance of accounting for heterogeneous responses when designing financial education policies.

2 Related Literature

This paper draws on research on financial inclusion (specifically, unbanked status), financial literacy, and financial education to examine whether high school financial education reduces unbanked status and narrows gender disparities in financial inclusion.

The first body of research examines the causes and consequences of being unbanked. This literature has established that households without a bank account are disproportionately low-income, less educated, and members of racial and ethnic minority groups (Boel and Zimmerman, 2022; Calem, Henderson and Wang, 2026; Bogan and Wolfolds, 2022; Hayashi, Routh and Toh, 2024). Less is known about how these patterns translate into gender disparities in financial inclusion. Bogan and Wolfolds (2022) show that gender differences in being unbanked vary substantially across racial and ethnic groups: Black women are more likely to be unbanked than Black men and than any other race-gender group, Hispanic women are less likely to be unbanked than Hispanic men, and White women and men have similar rates. Because existing measures of banking status are collected at the household level, however, these comparisons may not capture differences in individual banking participation. This measurement constraint motivates a focus on unpartnered adults, for whom household banking status more closely reflects individual banking status and therefore provides a clearer basis for examining gender differences in being unbanked.

A growing literature examines the factors underlying disparities in unbanked status and potential policies to reduce them. Calem, Henderson and Wang (2026) show that the decline in the overall unbanked rate has coincided with rising incomes, but that remaining unbanked households are increasingly concentrated among single individuals. They also find that states with higher levels of financial literacy have lower unbanked rates, suggesting that financial capability may be an important determinant of banking participation. However, evidence on whether policies that improve financial knowledge can expand financial inclusion remains limited. Existing policies have largely focused on expanding access to bank accounts, but these interventions have generally produced limited long-run effects (Dupas et al., 2018; Collins, Larrimore and Urban, 2026). This limited persistence suggests that barriers to banking may also arise on the demand side. These barriers include both preferences against having a bank account, such as distrust of financial institutions, and informational constraints, such as limited knowledge about which products carry low or no minimum balances or how to open and maintain an account (Hayashi, Routh and Toh, 2024). Policies that expand access primarily address supply-side barriers and therefore may not overcome these demand-side constraints. Financial education, in contrast, is primarily suited to addressing informational barriers by improving individuals' knowledge and ability to navigate financial products. These considerations suggest that financial education may address an important demand-side barrier to banking, but whether improving financial knowledge reduces unbanked status remains less well understood.

The second body of research examines financial literacy, with particular attention to gender gaps in financial knowledge. Across countries, women score lower than men on standard measures of financial knowledge, are more likely to respond "don't know" rather than guess, and report lower confidence in their financial knowledge even conditional on measured knowledge (Lusardi and Mitchell, 2011; Bucher-Koenen et al., 2017; Aristei and Gallo, 2022). These differences are not fully explained by observable characteristics such as income, education, and demographics (Fonseca et al., 2012; Aristei and Gallo, 2022). Gender differences in financial literacy also persist among groups for whom individual financial knowledge may be particularly consequential, including single women and widows (Bucher-Koenen et al., 2017). Together, these findings suggest that financial literacy may be an important channel through which education affects financial behavior, particularly for women. Whether improvements in financial literacy translate into lower rates of being unbanked, however, remains an open empirical question.

The third body of research examines the effects of state-mandated high school financial education. Early evaluations of these mandates often found limited effects on subsequent financial knowledge and behavior (Cole, Paulson and Shastry, 2016; Fernandes, Lynch and

Netemeyer, 2014; Willis, 2011). More recent evidence, however, has produced more positive findings by leveraging the staggered adoption of mandates across states and cohorts. This literature shows that mandate exposure increases financial knowledge and improves a range of downstream financial outcomes, including credit management, borrowing decisions, debt outcomes, and reliance on alternative financial services (Walstad, Rebeck and MacDonald, 2010; Kaiser and Menkhoff, 2020; Urban et al., 2020; Stoddard and Urban, 2020; Brown et al., 2016; Harvey, 2019).[2] However, less is known about whether such policies reduce unbanked status and whether their effects differ across demographic groups.

A small but growing literature examines the relationship between financial education and banking participation directly. Harvey (2019) finds that financial education reduces young adults' reliance on alternative financial services, partly through improvements in financial literacy and financial planning. More recently, Routh and Urban (2025) use the FDIC National Survey of Unbanked and Underbanked Households to show that state financial education mandates reduce the likelihood of being unbanked, particularly in states with stronger payday lending restrictions. These findings provide evidence that financial education can affect unbanked status, but they leave open whether such effects differ systematically across demographic groups and what mechanisms may account for these differences.

I build on the existing literature in three ways. First, I examine whether high school financial education reduces gender disparities in being unbanked, rather than simply lowering the overall unbanked rate. To better capture individuals' own financial decisions, I focus on unpartnered young adults, for whom household banking status more closely corresponds to individual banking status.[3] Second, I examine which groups benefit most from the policy and show that the largest reductions in being unbanked are concentrated among women with no education beyond high school and among Black and Hispanic women, groups with the highest

[2]Evidence from randomized evaluations outside the United States similarly finds that school-based financial education improves both financial knowledge and financial behaviors (Bruhn et al., 2016, 2025; Frisancho, 2023).

[3]Because unbanked status is measured at the household level, married or cohabiting respondents share the same banking status as their partners. As a result, gender differences among partnered individuals may not reflect differences in individual financial decisions. I discuss this issue further in Section 4.

baseline rates of being unbanked. Third, I examine why the effects of financial education on being unbanked are larger among women than men. Using the National Financial Capability Study, I show that mandate exposure increases women's objective financial knowledge, but not their self-assessed knowledge, consistent with the distinction between financial knowledge and confidence emphasized by Aristei and Gallo (2022). Using the FDIC survey, I further show that mandate exposure is associated with improvements in women's socioeconomic outcomes through higher college completion, greater self-employment, and movement into the middle of the income distribution.[4] These findings extend the literature by showing how financial education can reduce unbanked status, whether its effects differ across demographic groups, and the mechanisms that may account for these differences.

3 Institutional Background

Over the past several decades, states have increasingly adopted legislation requiring high school students to complete personal finance coursework before graduation. By 2030, 42 states will have implemented such requirements. These mandates were designed to improve students' financial knowledge and prepare them to manage financial decisions as they transition into adulthood. Personal finance courses are typically offered in grades 10 through 12, with most students taking them during their junior or senior year, and cover topics such as budgeting, saving, *banking*, credit, debt management, insurance, and investing. The banking component is directly relevant to the outcome studied here: it teaches students how to compare account fees, select among financial products, open accounts, and avoid costly mistakes such as overdrafts. By providing this information before students begin managing their own finances, personal finance education may influence whether young adults enter adulthood with a connection to the formal banking system.

These state mandates provide a useful setting for studying whether financial education

[4]This finding is consistent with Struckell et al. (2022), who find a stronger positive relationship between financial literacy and self-employment among women than among men.

affects banking participation because exposure is determined by state graduation requirements rather than individual decisions to enroll in a course. All students in an affected graduating cohort must complete the required instruction to graduate, including students who might not otherwise receive financial education at school or at home. This feature distinguishes mandatory financial education from voluntary programs, where participation may reflect unobserved differences in financial interest or ability.

A second feature of these mandates is their timing. Although some students may already have experience using a bank account before completing high school, the transition out of high school is when individuals increasingly begin managing finances independently, including earning income, paying bills, and making their own financial decisions. Personal finance courses are typically completed shortly before this transition, allowing the instruction to shape financial behaviors as young adults begin managing their finances independently.

States adopted these requirements at different times, generating variation in exposure across states and graduating cohorts. Illinois is the earliest adopter, with the requirement first applying to the 1970 graduating cohort. Three additional states first implemented requirements for cohorts graduating in the 1990s, followed by 11 states in the 2000s and 16 states in the 2010s, with remaining states adopting in the 2020s and beyond. A summary of the timing of adoption and the first graduating cohort exposed to each requirement is provided in Table A1.

States differ in how they implement personal finance graduation requirements, and these implementation details can affect the effectiveness of financial education (Collins and Urban, 2025). Some require students to complete a stand-alone personal finance course, while others integrate personal finance instruction into existing courses such as economics, consumer education, or civics. As shown in Table A1, stand-alone requirements are uncommon over the period I study. Among the 31 states contributing treated cohorts, only 5 require a stand-alone course by the 2019 cohort, while 26 provide instruction through an embedded curriculum. Most stand-alone requirements were adopted after the cohorts observed in my sample. I therefore define treatment as whether a state requires personal finance instruction for graduation, regardless of whether the instruction is delivered through a stand-alone course or an embedded curriculum. I measure exposure using the first graduating cohort required to complete the mandate rather than the year of legislative adoption, because implementation often begins several years after legislation is enacted.

4 Data

I use two data sources. The primary dataset is the FDIC National Survey of Unbanked and Underbanked Households, which I use to estimate the effects of state personal finance mandates, examine treatment heterogeneity, and test the socioeconomic mechanism. I supplement this analysis with the National Financial Capability Study (NFCS), which contains detailed measures of financial knowledge, including the Big Five financial literacy quiz. Although the NFCS records respondents' ages only in broad categories, making it less suitable for the main identification strategy, it is what allows me to test whether the mandates improve financial knowledge.

4.1 FDIC National Survey of Unbanked and Underbanked Households

The FDIC National Survey of Unbanked and Underbanked Households is a biennial supplement to the Current Population Survey (CPS), first fielded in 2009. I use all eight available waves, spanning 2009 through 2023. The supplement is administered to households interviewed in the relevant CPS month and collects information on household banking status together with demographic and socioeconomic characteristics. Because the supplement records each respondent's state of residence and age, I can link respondents to the timing of state personal finance mandates, under assumptions about where they attended high school, as described below.

Sample selection criteria. The supplement is administered at the household level, with one designated respondent providing information about the household's banking status. All households interviewed in the CPS during the relevant survey month are eligible for the supplement, but only households whose respondent can report whether anyone in the household has a bank account proceed to the supplement questions. Across the 2009 through 2023 waves, 296,005 households were supplement respondents, out of 570,943 households interviewed in the CPS during the months in which the supplement was fielded. I reduce these to the 24,955 respondents used in estimation by imposing five criteria in the following order. First, I drop 28,581 respondents who did not complete high school, because treatment is defined by exposure to a high school graduation requirement and those who left school before graduating were never subject to one. Second, I drop 178,613 respondents outside the 1991 through 2019 high school graduating cohorts, because earlier cohorts experienced different underlying trends and provide less comparable controls, while later cohorts are not yet observed as independent adults. Third, I drop 30,320 respondents observed outside ages 22 to 36, of whom 3,309 are younger and 27,011 older, for the reasons discussed below. Fourth, I drop 923 respondents who did not report family income, so that a common sample underlies my analyses. Fifth, I drop 32,613 respondents who live with a spouse or partner. Because banking status is measured at the household level, partnered respondents' outcomes cannot be interpreted as their own individual banking decisions.

Exposure is assigned by state and graduation cohort, so identification comes from variation across states and cohorts rather than from variation in age. The age restriction therefore serves to define a comparable observation window rather than to generate identifying variation. The two age bounds serve separate purposes. I set the lower bound at 22 because banking status is measured at the household level, and respondents younger than 22 are more likely to still be enrolled in school or living with their parents, in which case the recorded status may reflect their parents' banking rather than their own. I set the upper bound at 36 to maintain a comparable observation window across cohorts, since the cohort restriction alone would allow the earliest cohorts to be observed at substantially older ages. For example, a respondent from the 1991 graduating cohort is age 50 in the 2023 wave. Restricting the sample to age 36 ensures that all cohorts are observed over a comparable span of early adulthood.

Restricting the sample to unpartnered individuals minimizes the influence of a partner's financial decisions on household banking status. I define unpartnered individuals as those living without a spouse or cohabiting partner, including respondents who are legally married but whose spouse is absent from the household. This restriction is important because banking status is measured at the household rather than individual level. Among partnered respondents, a gender comparison may therefore reflect which partner answered the survey rather than differences in their own banking decisions, since both partners are assigned the same household banking status.

Treatment assignment. Treatment is defined as exposure to a state-mandated high school personal finance requirement. I obtain the timing of each state's requirement—defined as the first graduating cohort required to complete personal finance instruction—from Urban et al. (2020) and Urban's compilation of state financial education requirements, last updated in 2024.[5] The FDIC supplement records respondents' state of residence but not the state in which they attended high school. I therefore assign each respondent the requirement in their state of residence, assuming that this is also the state in which they attended high school, and infer their high school graduation year as the year they turned 18. A respondent is classified as treated if the assigned state had implemented a mandate by the respondent's high school graduation year. Interstate migration between high school and the survey may result in misclassification of the assigned requirement, so the estimates should be interpreted as intent-to-treat effects with respect to the assigned mandate rather than effects of coursework actually completed.

[5]The compilation is available at `https://papers.carlyurban.com/Policies_ForPosting_2024.xlsx`.

Outcome and covariates. The primary outcome is whether the respondent lives in an unbanked household, which the FDIC defines as a household in which no member has a checking or savings account at a bank or credit union.

The FDIC supplement provides information on respondents' race and ethnicity, family income, education, marital status, age, labor force status, and whether they were born in the United States. I recover respondents' gender and cohabitation status by linking the supplement to the underlying CPS files. I combine marital status, cohabitation status, and the presence of children younger than 18 to construct a measure of household structure. Educational attainment is classified into four categories: less than a high school diploma, high school graduate, some college, and college graduate. Family income is reported in five categories ranging from less than $15,000 to $75,000 or more. These are the survey's own categories, recorded in nominal dollars and not adjusted for inflation, so income enters the analysis as a set of category indicators rather than as a real amount. Family income includes the income of all household members related to the reference person by birth, marriage, or adoption. Therefore, income from an unmarried partner or an absent spouse is excluded. Because relatively few respondents identify as Asian, I combine them with other racial and ethnic groups into an "Other" category, leaving indicators for White, Black, Hispanic, and Other. A comparison of these measures by treatment status is reported in Table A2.

4.2 National Financial Capability Study

The National Financial Capability Study (NFCS) is conducted every three years by the Financial Industry Regulatory Authority (FINRA) Investor Education Foundation, and I use all six waves spanning 2009 to 2024. The NFCS provides information on financial literacy, financial attitudes, and exposure to financial education, none of which are collected in the FDIC supplement.

The NFCS differs from the FDIC supplement in two ways that limit its use for the main analysis. First, it is an online, non-probability sample rather than a nationally representative household survey. Second, it records age in broad bands rather than in single years, which prevents me from assigning respondents to the graduation cohorts used to define mandate exposure. I therefore use the NFCS to examine the financial knowledge mechanism, while relying on the FDIC survey to estimate the effect of the mandate on banking outcomes.

Exposure to financial education is measured using questions that ask whether a school, college, or workplace the respondent attended offered financial education and, if so, whether the respondent participated. Respondents who report participating are then asked whether they received the education in high school. These questions were first included in 2012, so measures of financial education exposure are available for five of the six waves.

The primary outcome is financial knowledge, measured using the standard Big Five financial literacy questions developed by Lusardi and Mitchell (2011, 2014). Although a sixth question was added in 2015, I use the original five questions to maintain a consistent measure across survey waves. I also examine self-assessed financial knowledge, given evidence of gender differences in financial confidence even conditional on measured financial literacy (Bucher-Koenen et al., 2017; Aristei and Gallo, 2022).

To make the NFCS analysis as comparable as possible to the FDIC analysis, I restrict the sample to unpartnered respondents aged 18 to 44 with at least a high school diploma, retaining the three youngest of the six age bands recorded in the survey (18–24, 25–34, and 35–44). Because age is observed only in bands, graduation cohorts can be approximated only coarsely. The lower age bound does not raise the same concern as in the FDIC analysis because the NFCS measures financial knowledge at the individual level rather than banking status at the household level. The NFCS also provides fewer covariates than the FDIC survey, with race observed only as White versus non-White. In the NFCS analysis, I control for gender, non-White status, state, graduation-cohort, and survey-year fixed effects. A similar comparison of the NFCS measures by treatment status is reported in Table A3.

5 Documenting the Gender Gap in Being Unbanked

Women in the sample are substantially more likely than men to be unbanked. Table 1 reports the unbanked rate and the female−male gap across household structure, race and ethnicity, family income, education, labor force participation, nativity, and age. Overall, 13.24 percent of women and 7.04 percent of men in the sample are unbanked, a gender gap of 6.20 percentage points. The gap is concentrated among respondents with fewer economic resources. It is 12.14 percentage points among high school graduates and 11.26 percentage points among respondents with family income below $15,000, compared with 1.05 percentage points among those with a college degree or more and essentially zero among those with family income above $30,000. The gap is also substantially larger among respondents who are unemployed or out of the labor force than among those who are employed, as well as among several groups with relatively high overall rates of being unbanked. For example, it is 10.67 percentage points among never-married respondents with children and 8.44 percentage points among Black respondents.

Women and men also differ systematically in their observed characteristics. As shown in Table 2, women are much more likely to be never married with children, to have family income below $30,000, and to be unemployed or out of the labor force. Men, in contrast, are more likely to be foreign-born, employed, and to have family income above $50,000. Differences in educational attainment are comparatively modest, and women and men are similar in age. Because several characteristics associated with being unbanked are distributed differently across women and men, the unconditional 6.20-percentage-point gap may partly reflect these compositional differences. I therefore quantify how much of the gap is associated with differences in observed characteristics in Section 7.1.

The descriptive evidence also suggests that the gender gap does not simply reflect weaker intentions to obtain bank accounts among women. Among the 1,883 unbanked respondents observed in the 2009 through 2017 waves, which include the questions on future intentions, 45.92 percent of women report wanting to open an account, compared with 37.59 percent of men. Conversely, 42.57 percent of men report having never held an account and having no plans to open one, compared with 29.32 percent of women. Thus, women are not more likely than men to report having no interest in obtaining a bank account. The survey responses suggest greater stated interest among unbanked women.

The reasons respondents give for being unbanked also differ by gender. Table 3 shows that the most common reason for both women and men is not having enough money to meet a minimum balance requirement, but women are 6.93 percentage points more likely than men to report this reason. Men, in contrast, are 6.92 percentage points more likely to report that they do not trust banks. Gender differences in the other reported reasons are smaller and generally not statistically significant. These patterns suggest that the gender gap may reflect not only differences in the prevalence of barriers to account ownership, but also differences in the type of barrier women and men perceive.

The distinction between intentions and barriers is relevant for the role of financial education. If unbanked women simply had less interest in obtaining bank accounts, information about banking products would have limited scope to change their banking status. Instead, women are more likely than men to report wanting an account while also being more likely to cite insufficient funds to meet minimum balance requirements. A high school personal finance course cannot directly eliminate a financial constraint, but it may improve knowledge about account options, including accounts with low or no minimum balance requirements, as well as other aspects of opening and maintaining a bank account. These patterns therefore motivate examining whether exposure to mandated personal finance education affects banking participation and whether its effects differ by gender. I return to financial knowledge as a potential mechanism in Section 7.4.2.

6 Empirical Strategy

My empirical analysis has two parts, each addressing a different question. The first is descriptive. I ask how much of the female–male gap in being unbanked reflects differences between unpartnered women and men in income, education, household structure, and other characteristics. To do so, I estimate a linear probability model of unbanked status and use its coefficients in a Kitagawa–Oaxaca–Blinder decomposition, which separates the gap into the contributions of these characteristics. In the second part, I ask whether the gender gap can be reduced through mandatory financial education. I answer this using a staggered difference-in-differences design, estimated with the extended two-way fixed-effects estimator of Wooldridge (2025). I carry this specification through the remainder of the analysis, using subgroup analysis to examine which women benefit most from the mandates and alternative outcomes to examine potential mechanisms.

6.1 Decomposition

I use a Kitagawa–Oaxaca–Blinder decomposition to quantify how differences in observed characteristics between women and men account for the gender gap in being unbanked (Kitagawa, 1955; Oaxaca, 1973; Blinder, 1973). The decomposition requires estimates of how these characteristics are related to unbanked status. I therefore first estimate a linear probability model in the pooled sample of women and men, where unbanked status is regressed on income, education, labor-force status, household structure, race and ethnicity, whether the respondent was born in the United States, age, and an indicator for being a woman.[6] I include the gender indicator so that the coefficients on the other characteristics

[6]Because the outcome is binary, a nonlinear decomposition such as Fairlie (2005) is also a natural choice. Fairlie's method is to estimate a logit model and decompose the difference in predicted probabilities rather than mean outcomes. In practice, however, nonlinear decompositions are more sensitive to sparse combinations of covariates because they rely on estimated predicted probabilities and repeated resampling to construct the decomposition. In my application, the extensive set of categorical covariates produce many thin cells, making the decomposition and the state-clustered bootstrap unstable across replications. Therefore, I use the standard Kitagawa–Oaxaca–Blinder decomposition based on a linear probability model, at the cost that fitted values are not constrained to lie between zero and one.

capture their relationship with unbanked status conditional on gender rather than absorbing the overall female–male difference in unbanked status.

I then use the estimated coefficients from this pooled model to calculate the explained component of the gender gap, following the pooled estimation approach of Fortin, Lemieux and Firpo (2011). For each characteristic, I multiply the difference in its mean between men and women by its pooled coefficient. A characteristic therefore contributes more to the explained gap when women and men differ substantially in that characteristic and when the characteristic is more strongly related to being unbanked. Summing these contributions across characteristics gives the total explained component: the portion of the observed gender gap associated with differences in observed characteristics, evaluated using a common set of coefficients for women and men. The unexplained component is the remaining portion of the gap. It captures differences between women and men in how strongly observed characteristics are related to unbanked status, as well as differences associated with factors that are not measured in the FDIC supplement.

The coefficients in this decomposition are descriptive rather than causal. For example, the estimated coefficient on family income captures the conditional relationship between income and the probability of being unbanked, but it may also reflect financial education received earlier in life or unobserved state, cohort, and individual factors that jointly affect income and banking status. The decomposition therefore does not imply that raising women's incomes would eliminate the portion of the gender gap attributed to income. Instead, the decomposition provides a descriptive account of which observed characteristics are most closely associated with the gender gap.

6.2 Staggered Difference-in-Differences Design

The objective of this part of the analysis is to estimate the effect of the mandate on the probability of being unbanked, separately for women and for men, together with the difference between those two effects, which is the change in the gender gap. A simple comparison of unbanked rates between exposed and unexposed respondents would not recover these effects because mandate exposure, although determined independently of individual banking outcomes, is correlated with the state and cohort environments in which respondents were exposed. States that adopted early may differ systematically from those that adopted later in factors affecting banking status, such as access to branches, ATMs, low-cost accounts, and mobile banking.[7] Such unobservables are not recorded in the survey data, and they can vary with the timing of adoption.

I estimate the effect of the mandates using the extended two-way fixed-effects (ETWFE) estimator of Wooldridge (2025). Because states adopted them in different years and the effect of the mandates can differ across adoption cohorts, the estimator must accommodate heterogeneous effects. ETWFE does so by estimating a separate treatment effect for every combination of adoption cohort and graduation cohort, drawing comparisons only against cohorts that are not yet treated or never treated, and averaging those effects into a single summary. A conventional two-way fixed-effects (TWFE) model instead allows already-treated cohorts to serve as controls for later-treated cohorts, creating the forbidden comparisons that can generate negatively weighted estimates (Goodman-Bacon, 2021).[8] However, for the mechanism analysis using NFCS data, I use conventional TWFE because the smaller sample and coarser cohort proxy make the staggered-adoption estimators less feasible. An unreported Goodman–Bacon decomposition using the FDIC sample shows that forbidden comparisons account for only a small share of the identifying variation, suggesting that conventional TWFE provides a reasonable approximation in this setting.

Let $s(i)$ denote respondent i's state and $c(i)$ their high school graduating cohort, defined as the year they turned 18, as listed in Table A1. State s first requires the mandate for

[7] Other examples include the assistance that caseworkers administering Temporary Assistance for Needy Families (TANF) and Medicaid provide to recipients in establishing direct deposit of benefits, and state and municipal account-opening initiatives such as the BankOn coalitions supported by the Cities for Financial Empowerment Fund, which have expanded at different rates across states.

[8] The Callaway–Sant'Anna and Sun–Abraham estimators also avoid them, and I report both alongside ETWFE in the event study figure in Figure A1. I prefer ETWFE for the main estimates because it recovers all cohort-specific effects within a single pooled regression, so that the difference between women's and men's effects and the standard error of that difference come from one clustered covariance matrix.

cohort g_s, with $g_s = \infty$ for never-treated states, so respondent i is exposed if $c(i) \geq g_{s(i)}$. Eleven states adopted mandates that first apply to cohorts graduating in 2019 or later, after the last cohort I observe. Respondents in these states are therefore never exposed within the sample and serve as not-yet-treated controls.

Let $D_i^{gt} = \mathbf{1}\{\, g_{s(i)} = g,\ c(i) = t,\ t \geq g \,\}$ indicate that respondent i belongs to treated cell (g, t), so that each treated respondent has $D_i^{gt} = 1$ for one cell only and untreated respondents have $D_i^{gt} = 0$ for every cell. I estimate

$$Y_i \;=\; \alpha_{g_{s(i)}} + \lambda_{c(i)} + X_i'\beta + \gamma\,\text{Female}_i + \sum_g \sum_{t \geq g} \Big(\tau_{gt} + \delta_{gt}\,\text{Female}_i\Big) D_i^{gt} + \varepsilon_i \tag{1}$$

using ordinary least squares, with standard errors clustered at the state level. The dependent variable, Y_i, is an indicator for being unbanked, α_g are adoption-cohort fixed effects, λ_t are graduation-cohort fixed effects, and Female_i is an indicator equal to one if respondent i is female. To avoid sparse treatment cells, graduation cohorts are grouped into three-year bins.

For the set of control variables, X_i, I use indicators for Black, Hispanic, and other race or ethnicity, with White as the omitted category, as well as an indicator for being born outside the United States. These are the characteristics fixed before high school that are available in the FDIC supplement. I do not include the richer set of covariates used in the decomposition, such as employment status and income, because these variables may themselves be affected by exposure to the mandate and are treated as potential mechanisms rather than controls. I specify X_i additively rather than interacting these characteristics with treatment cells and graduation cohorts, as in Wooldridge (2025), because the relatively small FDIC sample makes the fully interacted specification costly in terms of precision. Thus, coefficients on control variables are common across treatment cells, which comes at the cost of a more restrictive conditional parallel-trends assumption.

Within a treated cell (g, t), τ_{gt} captures the effect of the mandate on men, while δ_{gt} captures the difference in the effect for women relative to men. Thus, the corresponding effect on women is $\tau_{gt} + \delta_{gt}$. Because these effects may vary across treated cells, the estimates reported in the results tables aggregate the cell-specific effects using count-share weights. Specifically, I calculate the average effect for men as the weighted average of τ_{gt} across treated cells, where each cell is weighted by its share of treated men. Similarly, I calculate the average effect for women as the weighted average of $\tau_{gt} + \delta_{gt}$, where each cell is weighted by its share of treated women. The difference between these two averages then gives the additional effect of mandate exposure for women relative to men. Thus, the reported coefficient estimate on Mandate exposure is the weighted average effect for men, while the coefficient estimate on Mandate exposure $\times$ Female is the weighted difference between the average effects for women and men.

The two sets of fixed effects absorb systematic differences in outcome levels across adoption cohorts and graduation cohorts. The adoption-cohort effects α_g absorb differences that are fixed over time and shared by states that first required the course in the same year, while the graduation-cohort effects λ_t absorb differences common to all cohorts, including nationwide changes. The identifying variation therefore comes from differences across graduation cohorts in when they become exposed to the mandate, after accounting for adoption-cohort and graduation-cohort effects and predetermined individual characteristics. Figure 1 shows how the rollout translates into the number of states and respondents exposed across graduation cohorts.

The fixed effects cannot remove differences in trends. If banking participation improved faster in early-adopting than late-adopting states, the estimator could attribute that differential improvement to the mandate. Identification therefore requires that, conditional on controls and fixed effects, treated and untreated cohorts would have experienced similar changes in banking participation absent the mandate. I assess this assumption using event-study estimates. Figure 2 shows no evidence of differential pre-treatment trends, and joint tests fail to reject zero pre-treatment effects for women, men, and the female–male gap. Another requirement is that respondents do not change their banking status in anticipation of future exposure. This is plausible because the policy operates through high school coursework and cannot directly affect adult banking outcomes before exposure.

7 Findings

In this section, I first present the decomposition results to characterize the sources of the gender gap in being unbanked. I then present the difference-in-differences estimates of the effect of state personal finance mandates, examine treatment effect heterogeneity, and present evidence on two potential mechanisms.

7.1 Decomposition Findings

In Table 4, I report the results of decomposing the 6.20 percentage point gender gap in unbanked status using the pooled linear probability model reported in Table 5. Observable characteristics explain 5.05 percentage points (81 percent) of the gap, leaving only 1.15 percentage points unexplained. Thus, most of the difference in banking status between unpartnered women and men reflects differences in their observed characteristics rather than differences in how those characteristics translate into banking outcomes.

The explained component is highly concentrated in three sets of characteristics. Family income accounts for 1.98 percentage points (32 percent of the total gap), household structure contributes 1.75 percentage points (28 percent), and race and ethnicity contribute another 1.06 percentage points (17 percent). Together, these three factors explain over three-quarters of the overall gender gap.

Income is the largest contributor because women are disproportionately represented at the lower end of the family income distribution. As shown in Table 5, only 15.0 percent of women report family income of at least $75,000 compared with 24.4 percent of men, while 21.7 percent of women—but only 13.1 percent of men—report family income below $15,000. Since higher-income households are substantially less likely to be unbanked, these income differences alone account for nearly one-third of the overall gender gap.

Household structure is the second largest contributor. Nearly all of this contribution reflects the much higher prevalence of never-married mothers among women (28.1 percent versus 5.4 percent among men), a group with substantially higher predicted unbanked rates than never-married adults without children. Race and ethnicity also contribute meaningfully, primarily because women are more likely than men to be Black, while Black respondents have considerably higher predicted probabilities of being unbanked.

Differences in labor force participation explain a comparatively modest 7 percent of the gap because women are somewhat more likely to be unemployed or out of the labor force. In contrast, age and nativity contribute almost nothing because women and men in the sample differ little along either dimension.

Education is the only characteristic that narrows rather than widens the gender gap. Women are more educated than men in the sample, making them less likely to possess characteristics associated with being unbanked. If women instead had the same educational distribution as men, the gender gap would be slightly larger. Thus, educational attainment offsets part of the disadvantage associated with women's lower incomes and different household circumstances.

As an experiment, I replicated the decomposition after adding a "treatment" control (an indicator of whether the person was exposed to a personal finance mandate in high school) to the regressors in the linear probability model. This factor proved to explain none of the gap because women and men in my sample have equal exposure to the treatment. This is expected, because the decomposition attributes the gap only to characteristics that differ between women and men, whereas exposure to the mandate is similar across the two groups. The decomposition therefore describes the composition of the gap rather than the effect of the policy, which I identify separately using the difference-in-differences design described in Section 6.2.

Overall, the gender gap in being unbanked is largely attributable to differences in women's socioeconomic characteristics—especially lower family income, greater prevalence of single parenthood, and racial and ethnic composition—while women's higher educational attainment partially offsets these disadvantages.

7.2 Difference-in-Differences Findings

In Table 6, I report the ETWFE estimates of the effect of high school personal finance mandates on the probability of being unbanked among unpartnered young adults. Across specifications, mandate exposure is associated with a meaningful reduction in the likelihood of being unbanked. In the simplest specification (column 1), mandate exposure is predicted to reduce the probability of being unbanked by 3.06 percentage points, equivalent to 29.77 percent of the 10.28 percent baseline unbanked rate.

The average treatment effect, however, masks substantial gender heterogeneity. Columns 2 through 4 allow the effect of mandate exposure to differ between women and men by interacting the treatment indicator with an indicator for female. In the preferred specification (column 4), mandate exposure is predicted to reduce men's probability of being unbanked by 1.78 percentage points. Women experience an additional reduction of 2.66 percentage points, implying a total decline of 4.44 percentage points. Thus, the estimated effect for women is two and a half times as large as that for men. Relative to the baseline rates reported in the first row of Table 1, the mandate is predicted to reduce men's unbanked rate by 25.28 percent (1.78/7.04), women's by 33.53 percent (4.44/13.24), and the female–male gap by 42.90 percent (2.66/6.20). Taken together, these estimates indicate that high school personal finance mandates substantially narrow the gender gap in being unbanked.

This result is robust to the inclusion of demographic controls. The estimated female differential changes little as controls for race, ethnicity, and nativity are added across columns 2 through 4, suggesting that the larger effect for women is not driven by compositional differences between women and men.

I do not include income, education, or employment status in the preferred specification because these outcomes may themselves be affected by mandate exposure and are therefore potential mechanisms rather than predetermined controls. As a sensitivity check, I add these variables as controls and report the results in Table A4. The estimated mandate effects become smaller, particularly the female differential, but remain negative and statistically significant. This attenuation is consistent with income, education, and employment capturing part of the effect of the mandates. I therefore treat these variables as outcomes when examining potential mechanisms rather than as controls in the main specification.

In Figure 2, I present event study estimates of the effect of mandate exposure by years relative to the first graduation cohort exposed to a state's personal finance requirement, separately for women and men.[9] The event study serves two purposes. First, the pre-treatment estimates are close to zero for both groups, providing no evidence of differential trends before the mandate-exposed cohorts enter the sample. Second, the decline in being unbanked emerges only after the first exposed cohort and is concentrated among women. For women, the estimated effects become negative immediately following exposure, with the first two post-treatment coefficients statistically distinguishable from zero. Although the estimates become less precise at longer horizons because fewer treated cohorts contribute to the estimation, they remain consistently negative. In contrast, the post-treatment estimates for men remain small and statistically indistinguishable from zero throughout. Overall, the event study supports the identifying assumption underlying the difference-in-differences design and reinforces the main finding that personal finance mandates primarily reduce unbanked status among women.

7.3 Subsample-Specific Difference-in-Differences Findings

The estimates in columns 1 through 4 of Table 6 summarize the average effects of mandate exposure in the full sample, allowing the effect to differ by gender. I next examine whether the additional effect for women differs across groups that may have faced different opportu-

[9]The recent difference-in-differences literature proposes several estimators for staggered treatment adoption. As a robustness check, I report event study estimates obtained using ETWFE, Callaway–Sant'Anna, Sun–Abraham, and dynamic TWFE in Figure A1.

nities to benefit from financial education during high school. If personal finance mandates are particularly valuable for women who would otherwise have fewer opportunities to receive financial education, their effects should be largest among groups that were relatively disadvantaged while in high school. To examine this prediction, I re-estimate Equation (1) separately for three subsamples defined by characteristics that plausibly proxy for disadvantage during high school: Black or Hispanic respondents, foreign-born respondents, and respondents with no education beyond high school.[10]

These subsample-specific estimates are shown in columns 5-7 of Table 6. The evidence is strongest among respondents with no education beyond high school (column 7). For this group, mandate exposure reduces women's probability of being unbanked by an additional 7.63 percentage points relative to men, nearly three times the full-sample estimate reported in column 4. This subgroup also has the highest baseline unbanked rate (22.66 percent), and because high school represents their last formal educational experience, they are the group most likely to rely on the mandate as their primary source of personal finance education.

A similar pattern emerges among Black and Hispanic respondents (column 5). Their baseline unbanked rate (18.56 percent) is substantially higher than the full-sample average of 10.28 percent, and the estimated additional effect for women is 4.10 percentage points, larger than the full-sample estimate. By contrast, the estimated differential effect among foreign-born respondents is small and statistically insignificant (column 6). This subgroup has the lowest baseline unbanked rate (9.65 percent), and the female–male gap in unbanked status is itself small (see Table 1), leaving relatively little gender gap for the mandate to reduce.

[10]Students from low-income households are another natural group to examine, since they may have the most to gain from a high-school personal finance course. I do not split the sample by income, however, because the survey measures family income at the time of the interview, when respondents are between ages 22 and 36, rather than during high school when mandate exposure occurred. Moreover, as shown in Section 7.4, mandate exposure itself affects women's income distribution, making current income a post-treatment outcome rather than an appropriate proxy for disadvantage during high school. Educational attainment is not a perfect measure either, but by these ages it largely reflects whether respondents were able to pursue education beyond high school, making it an informative proxy for disadvantage during high school.

Taken together, these results suggest that the additional benefits of high-school personal finance mandates for women are largest among groups that appear to have had fewer opportunities to acquire financial knowledge outside the required curriculum. The strongest evidence comes from respondents whose education ended with high school, for whom the estimated additional effect for women is 7.63 percentage points. Relative to this group's baseline female–male difference in unbanked status of 12.14 percentage points (Table 1), this estimate implies that the mandates would reduce the gender gap by 62.85 percent. For Black and Hispanic respondents, the estimated additional effect of 4.10 percentage points amounts to a 54.23 percent reduction relative to the subgroup's baseline gender gap of 7.56 percentage points.

7.4 Mechanisms Underlying the Gender Gap in Unbanked Rates

Several mechanisms could explain why the mandates reduce unbanked rates more for women than for men, and I examine two that the available data allow me to test. First, the mandates may improve women's financial knowledge more than men's, increasing their ability and confidence to use formal financial services. Second, the mandates may improve economic outcomes, such as educational attainment, employment, and income, thereby changing the circumstances in which banking decisions are made. These improvements can both increase the benefits of holding a bank account and reduce the barriers to opening one. I examine the second mechanism using the FDIC sample, which contains information on education, employment, and income, and the first using the NFCS sample described in Section 4, because the FDIC survey does not measure financial knowledge.

7.4.1 Human Capital and Economic Outcomes Using FDIC Data

To examine whether the larger reduction in women's unbanked rates is accompanied by improvements in education and economic outcomes that could plausibly mediate the effect, I re-estimate Equation (1) using alternative outcomes in place of the unbanked indicator while leaving the specification otherwise unchanged. I consider two education outcomes: college completion, defined as holding a bachelor's degree or higher, and some college, which includes respondents who attended a two-year or four-year college without completing a bachelor's degree. I also examine labor force status (employed, unemployed, and out of the labor force), self-employment separately for incorporated and unincorporated businesses, and four mutually exclusive family-income categories. Because the sample is restricted to unpartnered adults, family income primarily reflects respondents' own economic resources, although it may also include the income of other related household members.

The education results in columns 1–2 suggest that the mandates increase women's likelihood of earning a bachelor's degree, with little evidence that they simply increase attendance without completion. There is essentially no differential effect on attending some college without earning a bachelor's degree, with an estimated female interaction of -0.78 percentage points on a mean of 33.19 percent. By contrast, the mandates are predicted to increase men's probability of completing college by 0.90 percentage points and women's by an additional 2.92 percentage points, implying a 3.82 percentage point increase for women, or 9.20 percent of the 41.51 percent mean. Although the estimate for men is not statistically distinguishable from zero, the female differential is precisely estimated, suggesting that the educational gains are concentrated among women.

The mandates do not appear to change labor force attachment (columns 3–5). The estimated female–male differences in employment, unemployment, and labor force participation are all small and statistically insignificant, and the estimates are sufficiently informative to rule out economically meaningful changes. By contrast, among employed respondents, the mandates do appear to affect the type of work women perform. Column 6 estimates reveal that exposure increases men's probability of self-employment by 1.31 percentage points and women's by a further 1.56 percentage points, implying an increase of 2.87 percentage points for women—more than half of the 5.08 percent baseline self-employment rate. Nearly all of this additional increase comes from unincorporated self-employment. The female interaction is 1.27 percentage points on a mean of 3.56 percent, while the corresponding interaction for incorporated self-employment is small and statistically insignificant. Thus, the mandates appear to encourage women to enter self-employment primarily through unincorporated businesses.

The income results suggest that the mandates raise family income for both men and women, although the gains occur at different points in the income distribution. Men experience the largest increase in the highest income category ($75,000 or more), with an estimated gain of 3.54 percentage points (column 12). Women's gains, in contrast, are concentrated in the middle of the distribution (column 11). The female interaction is estimated at 3.93 percentage points for the $50,000–$75,000 category, while it is −2.40 percentage points for the highest income category, implying that women's estimated increase at $75,000 or more is 1.14 percentage points. Relative to the baseline, the 3.93 percentage-point increase represents a 23.52 percent increase in the share of women living in households with incomes between $50,000 and $75,000. Thus, the mandates appear to improve women's economic position primarily by moving more women into middle-income households rather than into the highest income category.

The results therefore suggest that the mandates improve women's human capital and economic circumstances along several dimensions while leaving labor force attachment largely unchanged. The largest proportional response is in self-employment, followed by movement into the middle of the income distribution, with a more modest but economically important increase in college completion. Each of these changes plausibly increases both the benefits of having a bank account and the resources available to maintain one. Although these reduced-form estimates cannot identify how much of the reduction in the gender gap in unbanked status operates through these channels, they are consistent with improvements in women's human capital and economic opportunities being one mechanism through which the mandates disproportionately benefit women.

7.4.2 Financial Knowledge Using NFCS Data

I conclude the analysis by examining whether the mandates improve financial knowledge, the outcome they are designed to affect directly. Because the FDIC survey contains no information on financial education or financial knowledge, I instead use the NFCS, which records both whether respondents received financial education and what they know, allowing me to test the policy's intended mechanism directly.

As described in Section 4, the NFCS sample consists of unpartnered high school graduates ages 18–44. Because the survey reports age only in broad bands rather than exact birth years, graduation cohorts can be identified only approximately. I therefore estimate conventional TWFE models with state, graduation-cohort, and survey-year fixed effects, interacting mandate exposure with the female indicator and clustering standard errors at the state level.

Table 8 reports the results. Columns 1–3 examine whether mandate exposure increases reported financial education, while columns 4–10 examine whether those increases translate into greater financial knowledge.

Columns 1–3 show that the mandates increase reported exposure to financial education. Mandate exposure raises the probability that respondents report receiving any financial education and financial education specifically in high school. The estimated female interaction terms are small and statistically insignificant throughout. The mandates therefore appear to increase access to financial education similarly across genders.

Columns 4–10 then examine whether this additional exposure translates into greater financial knowledge. For men, there is little evidence that it does. The estimated mandate effects are small and statistically insignificant across all knowledge measures. Women, however, exhibit larger gains. On the five-question financial knowledge score in column 4, mandate exposure increases women's score by an additional 0.13 points relative to men. Because the estimated effect for men is small and negative, this corresponds to a net increase of 0.08 points for women. The largest improvements occur on the two investment questions.

Women become 4.35 percentage points more likely than men to answer the bond-price question correctly (column 7) and 4.28 percentage points more likely to correctly identify that a single stock is riskier than a stock mutual fund (column 9), representing one-fifth and one-eighth of the respective baseline rates. The differential effect on the compound-interest question is smaller, at 2.61 percentage points (column 5). By contrast, self-assessed financial knowledge does not change for either gender (column 10).

Taken together, these results suggest that the larger effects of the mandates for women do not arise because women are more likely than men to receive financial education. Rather, women appear to acquire more financial knowledge from the same increase in educational exposure. Although the NFCS cannot identify why the same coursework generates larger knowledge gains for women, the pattern is consistent with the larger reductions in women's unbanked rates documented earlier and supports the interpretation that improvements in financial knowledge are one channel through which the mandates reduce gender disparities.

8 Concluding Comments

Among unpartnered young adults, whose banking status reflects their own decisions rather than a partner's, women are substantially more likely than men to be unbanked. In this study, I ask whether high school personal finance mandates reduce that gap. Using the FDIC National Survey of Unbanked and Underbanked Households and a heterogeneity robust difference-in-differences design, I find that exposure to the mandate reduces the probability of being unbanked by 3.1 percentage points overall, equivalent to roughly 30 percent of the baseline unbanked rate. The effects are substantially larger for women than for men. Women experience a reduction two and a half times as large, closing 42.9 percent of the gender gap in unbanked status among unpartnered young adults. The largest gains occur among women with the lowest baseline banking participation rates, particularly those with no education beyond high school and Black and Hispanic women.

The evidence points to both financial knowledge and subsequent economic outcomes as plausible mechanisms for why the mandates have larger effects for women than for men. Using NFCS data, I find that mandates increase women's measured financial knowledge more than men's, with the largest improvements occurring on questions about investment concepts while leaving women's self assessed financial knowledge unchanged. The FDIC evidence also indicates improvements in subsequent economic circumstances. For women, the mandates are associated with higher rates of college completion, unincorporated self employment, and movement into the middle of the income distribution.

These findings contribute to the broader discussion of how to improve financial inclusion. Many adult financial education programs (Fernandes, Lynch and Netemeyer, 2014) and policies that expand access to bank accounts (Dupas et al., 2018; Collins, Larrimore and Urban, 2026) have produced limited effects. One reason may be that voluntary financial education primarily reaches individuals who are already engaged with the financial system, whereas high school personal finance mandates reach every student in an affected cohort before they begin making independent financial decisions. The FDIC data also provide little evidence that lack of interest in banking is the primary barrier for these women. Unbanked women express a greater desire to open an account than unbanked men. Together, these findings imply that financial education and account access policies are complements rather than substitutes. Expanding the availability of low cost accounts can reduce supply side barriers, but equipping young adults with financial knowledge before they enter adulthood addresses informational barriers that access alone cannot overcome. Doing so can play an important role in improving financial inclusion and reducing gender disparities.

This study also has several limitations that point to directions for future research. Because the FDIC records banking status at the household level, the analysis is restricted to unpartnered young adults whose banking status reflects their own decisions. Richer data that measure banking participation at the individual level within households would make it possible to examine how personal finance mandates affect joint banking decisions among partnered adults. In addition, while the mechanism analyses indicate that mandates improve both financial knowledge and subsequent economic outcomes, they rely on different surveys and therefore cannot identify the relative importance of each channel. Richer longitudinal data that jointly measure financial knowledge, economic outcomes, and banking behavior would help disentangle these mechanisms. The results also raise an important question about why women experience larger improvements in financial knowledge than men, despite similar exposure to the mandates. Understanding the mechanisms underlying these heterogeneous learning effects represents an important direction for future research. Despite these limitations, the results indicate that high school personal finance mandates can play an important role in improving financial inclusion and narrowing gender disparities early in adulthood.

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Tables

Table 1: Gender differences in unbanked rates by respondent characteristics

	Female	Male	Difference	N (F)	N (M)
Characteristic	(1)	(2)	(3)	(4)	(5)
All unpartnered respondents	13.24 (0.66)	7.04 (0.50)	+6.20***	13,213	11,742
Household structure[a]					
never married, with children	26.19 (1.09)	15.52 (2.14)	+10.67***	3,480	579
never married, no children	5.70 (0.43)	6.09 (0.47)	−0.39	6,612	9,469
formerly married, with children	15.14 (0.94)	8.55 (1.47)	+6.59***	2,008	367
formerly married, no children	9.22 (1.42)	9.77 (1.44)	−0.55	696	1,036
married (spouse absent), with children	19.57 (3.00)	7.72 (4.49)	+11.85***	254	27
married (spouse absent), no children	13.42 (4.15)	9.06 (1.48)	+4.36	163	264
Race and ethnicity					
White	6.81 (0.62)	3.74 (0.29)	+3.06***	7,445	7,874
Black	25.44 (1.39)	17.00 (1.27)	+8.44***	2,784	1,238
Hispanic	15.42 (1.60)	11.12 (1.04)	+4.30***	1,805	1,403
other race or ethnicity	7.71 (1.70)	3.73 (0.66)	+3.98**	1,179	1,227
Family income					
under $15,000	35.13 (1.26)	23.87 (1.71)	+11.26***	2,961	1,540
$15,000 to $30,000	17.15 (0.98)	12.32 (1.08)	+4.83***	3,028	1,962
$30,000 to $50,000	4.63 (0.44)	4.59 (0.44)	+0.05	3,246	3,016
$50,000 to $75,000	2.77 (0.44)	2.29 (0.36)	+0.48	2,085	2,503
$75,000 or more	1.39 (0.36)	1.18 (0.34)	+0.20	1,893	2,721
Education					
high school graduate	28.84 (1.24)	16.69 (1.21)	+12.14***	3,134	3,157
some college	15.61 (1.00)	6.73 (0.62)	+8.87***	4,578	3,645
college or more	2.15 (0.32)	1.10 (0.14)	+1.05***	5,501	4,940
Labor force participation					
employed	8.40 (0.56)	5.03 (0.45)	+3.38***	10,698	10,091
unemployed	35.25 (2.27)	21.34 (2.61)	+13.91***	844	605
not in the labor force	32.29 (1.29)	16.95 (1.71)	+15.33***	1,671	1,046
Foreign-born (born outside the U.S.)	10.14 (1.05)	9.23 (1.22)	+0.91	1,350	1,472
Age					
22 to 26	12.45 (0.89)	6.58 (0.60)	+5.87***	4,064	3,839
27 to 31	13.61 (0.87)	6.65 (0.61)	+6.95***	4,738	4,253
32 to 36	13.63 (0.78)	8.05 (0.68)	+5.58***	4,411	3,650

Note: Survey-weighted shares unbanked by gender are reported. The Difference column shows the female share minus the male share, in percentage points. N denotes the unweighted sample size; F and M denote female and male, respectively. Standard errors are in parentheses and clustered at the state level. Stars denote statistical significance from a weighted, state-clustered t-test of the gender difference. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[a] Household structure is defined by combining marital status and the presence of a co-resident child aged 18 or younger. Formerly married includes divorced, separated, and widowed respondents.

Table 2: Sample means of banking status and covariates by gender

	Total	Female	Male	Difference
Variable	(1)	(2)	(3)	(4)
Banking status				
1 if unbanked	10.28 (0.54)	13.24 (0.66)	7.04 (0.50)	+6.20***
Household structure[a]				
1 if never married, with children	17.24 (0.55)	28.10 (0.92)	5.38 (0.49)	+22.72***
1 if never married, no children	65.09 (0.95)	50.47 (1.25)	81.06 (0.73)	−30.59***
1 if formerly married, with children	8.34 (0.44)	13.50 (0.70)	2.71 (0.23)	+10.79***
1 if formerly married, no children	6.54 (0.30)	5.02 (0.28)	8.19 (0.50)	−3.17***
1 if married (spouse absent), with children	1.07 (0.06)	1.81 (0.11)	0.25 (0.05)	+1.56***
1 if married (spouse absent), no children	1.73 (0.15)	1.10 (0.14)	2.41 (0.19)	−1.31***
Race and ethnicity				
1 if White	52.06 (2.65)	47.94 (2.37)	56.56 (3.07)	−8.63***
1 if Black	21.52 (2.21)	26.56 (2.64)	16.02 (1.70)	+10.54***
1 if Hispanic	16.14 (2.74)	16.32 (2.81)	15.95 (2.70)	+0.37
1 if other race or ethnicity	10.27 (1.32)	9.19 (1.18)	11.46 (1.51)	−2.28***
Family income				
1 if family income is under $15,000	17.61 (0.70)	21.74 (0.97)	13.10 (0.51)	+8.65***
1 if family income is $15,000 to $30,000	19.35 (0.75)	22.20 (0.75)	16.23 (0.89)	+5.97***
1 if family income is $30,000 to $50,000	24.55 (0.68)	24.35 (0.67)	24.77 (0.82)	−0.42
1 if family income is $50,000 to $75,000	18.99 (0.42)	16.71 (0.51)	21.48 (0.54)	−4.77***
1 if family income is $75,000 or more	19.51 (1.63)	15.00 (1.45)	24.43 (1.86)	−9.42***
Education				
1 if high school graduate	25.30 (0.96)	23.79 (0.94)	26.94 (1.19)	−3.15***
1 if some college	33.19 (0.98)	35.27 (1.17)	30.91 (0.96)	+4.36***
1 if college or more	41.51 (1.55)	40.94 (1.71)	42.15 (1.57)	−1.21
Labor force participation				
1 if employed	82.69 (0.58)	80.56 (0.64)	85.02 (0.67)	−4.46***
1 if unemployed	6.00 (0.27)	6.67 (0.31)	5.26 (0.31)	+1.40***
1 if not in the labor force	11.31 (0.40)	12.77 (0.48)	9.72 (0.48)	+3.06***
Foreign-born (1 if born outside the U.S.)	13.47 (1.51)	11.78 (1.51)	15.32 (1.53)	−3.54***
Age in years	28.90 (0.05)	29.02 (0.06)	28.77 (0.06)	+0.25***
Observations	24,955	13,213	11,742	

Note: Survey-weighted means of respondent characteristics by gender are reported. The Difference column shows the female mean minus the male mean. Standard errors are in parentheses and clustered at the state level. Stars denote statistical significance from a weighted, state-clustered t-test of the gender difference. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[a] Household structure is defined by combining marital status and the presence of a co-resident child aged 18 or younger. Formerly married includes divorced, separated, and widowed respondents.

Table 3: Reasons for being unbanked, by gender (2013–2023)

	Total	Female	Male	Difference
Reason	(1)	(2)	(3)	(4)
Don't have enough money to meet minimum balance	49.92	52.20	45.26	+6.93**
Don't trust banks	34.99	32.72	39.64	−6.92**
Bank account fees too high or unpredictable	34.24	32.88	37.01	−4.13
Avoiding a bank gives more privacy	30.47	29.76	31.93	−2.17
Don't have required personal identification	15.53	14.59	17.45	−2.86
Banks don't offer needed products or services	15.25	14.93	15.91	−0.98
Inconvenient bank hours or locations	12.27	11.21	14.44	−3.23
Observations (unbanked)	1,559	1,072	487	

Note: Survey-weighted shares of unbanked respondents citing each reason, by gender, are reported. The Difference column shows the female share minus the male share, in percentage points. Respondents can cite more than one reason, so the shares do not sum to 100. Stars denote statistical significance from a weighted, state-clustered t-test of the gender difference. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Kitagawa–Oaxaca–Blinder decomposition of the gender gap in unbanked status

	Contribution	Share
	(1)	(2)
Total gap (Female − Male)	**+6.20**	**100%**
	(0.63)	
Explained	**+5.05**	**+81%**
	(0.42)	
Family income	+1.98	+32%
	(0.15)	
Household structure	+1.75	+28%
	(0.22)	
Race and ethnicity	+1.06	+17%
	(0.12)	
Labor force participation	+0.42	+7%
	(0.07)	
Age	+0.06	+1%
	(0.01)	
Foreign-born	+0.01	+0%
	(0.02)	
Education	−0.23	−4%
	(0.10)	
Unexplained	**+1.15**	**+19%**
	(0.40)	

Note: Contributions to the female−male gap in being unbanked are reported. The decomposition is based on a linear probability model; the underlying means and estimated coefficients from that model are in Table 5. The Contribution column is in percentage points; the Share column shows each row's contribution as a share of the total gap. Standard errors are in parentheses and computed from 400 state-clustered bootstrap resamples.

Table 5: Sample means and estimated coefficients underlying the decomposition in Table 4

	Sample mean		Difference	Estimated	Contribution
	Women	Men	(Women − Men)	coefficient	to the gap
	(1)	(2)	(3)	(4)	(5)
Family income					**+1.98 (0.15)**
1 if family income is under $15,000 (reference category)	0.217 (0.010)	0.131 (0.005)	+0.086 (0.008)		
1 if family income is $15,000 to $30,000	0.222 (0.007)	0.162 (0.008)	+0.060 (0.007)	−12.03 (1.06)	−0.72 (0.10)
1 if family income is $30,000 to $50,000	0.243 (0.006)	0.248 (0.008)	−0.004 (0.006)	−19.33 (0.93)	+0.08 (0.12)
1 if family income is $50,000 to $75,000	0.167 (0.005)	0.215 (0.006)	−0.048 (0.006)	−18.70 (0.95)	+0.89 (0.12)
1 if family income is $75,000 or more	0.150 (0.014)	0.244 (0.018)	−0.094 (0.008)	−18.32 (0.86)	+1.73 (0.15)
Household structure					**+1.75 (0.22)**
1 if never married, no children (reference category)	0.505 (0.012)	0.811 (0.007)	−0.306 (0.010)		
1 if never married, with children	0.281 (0.009)	0.054 (0.005)	+0.227 (0.011)	+7.10 (0.78)	+1.61 (0.22)
1 if formerly married, with children	0.135 (0.007)	0.027 (0.002)	+0.108 (0.006)	+1.79 (0.82)	+0.19 (0.09)
1 if formerly married, no children	0.050 (0.003)	0.082 (0.005)	−0.032 (0.005)	+1.92 (0.85)	−0.06 (0.03)
1 if married (spouse absent), with children	0.018 (0.001)	0.003 (0.001)	+0.016 (0.001)	+3.43 (2.60)	+0.05 (0.04)
1 if married (spouse absent), no children	0.011 (0.001)	0.024 (0.002)	−0.013 (0.002)	+3.65 (1.46)	−0.05 (0.02)
Race and ethnicity					**+1.06 (0.12)**
1 if White (reference category)	0.479 (0.024)	0.566 (0.031)	−0.086 (0.012)		
1 if Black	0.266 (0.025)	0.160 (0.016)	+0.105 (0.012)	+10.08 (0.90)	+1.06 (0.13)
1 if Hispanic	0.163 (0.028)	0.160 (0.027)	+0.004 (0.006)	+4.11 (0.73)	+0.02 (0.03)
1 if other race or ethnicity	0.092 (0.011)	0.115 (0.014)	−0.023 (0.006)	+0.96 (0.71)	−0.02 (0.01)
Labor force participation					**+0.42 (0.07)**
1 if employed (reference category)	0.806 (0.006)	0.850 (0.007)	−0.045 (0.006)		
1 if unemployed	0.067 (0.003)	0.053 (0.003)	+0.014 (0.003)	+11.42 (1.26)	+0.16 (0.04)
1 if not in the labor force	0.128 (0.005)	0.097 (0.005)	+0.031 (0.005)	+8.59 (0.75)	+0.26 (0.05)
Age (in years)	29.019 (0.064)	28.767 (0.056)	+0.252 (0.050)	+0.22 (0.05)	**+0.06 (0.01)**
Foreign-born (1 if born outside the U.S.)	0.118 (0.014)	0.153 (0.015)	−0.035 (0.004)	−0.32 (0.64)	**+0.01 (0.02)**
Education					**−0.23 (0.10)**
1 if high school graduate	0.238 (0.009)	0.269 (0.012)	−0.031 (0.008)	+11.49 (0.81)	−0.36 (0.11)
1 if some college	0.353 (0.011)	0.309 (0.009)	+0.044 (0.008)	+2.98 (0.47)	+0.13 (0.03)
1 if college or more (reference category)	0.409 (0.016)	0.421 (0.015)	−0.012 (0.010)		

Note: The components underlying the decomposition in Table 4 are reported. The Estimated coefficient column reports estimated coefficients, in percentage points, from a linear probability model of unbanked status estimated on the pooled sample and including an indicator for female. For categorical variables, estimated coefficients are relative to the reference category. The Contribution to the gap column is the product of the female−male difference in the characteristic and its estimated coefficient. Standard errors are in parentheses and computed from 400 state-clustered bootstrap resamples.

Table 6: ETWFE estimates of the effect of high school personal finance mandates on the probability of being unbanked

	Dependent variable: 1 if unbanked						
	Full sample				Subgroups		
					Black/Hisp	Foreign born	HS only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mandate exposure × Female	–	−2.78***	−2.68***	−2.66***	−4.10**	0.19	−7.63***
		(0.80)	(0.69)	(0.69)	(1.85)	(1.91)	(1.77)
Mandate exposure	−3.06***	−1.76**	−1.78**	−1.78**	−4.63**	−6.77***	−1.64
	(0.86)	(0.87)	(0.76)	(0.76)	(1.87)	(2.19)	(1.66)
1 if female	–	6.72***	4.76***	4.71***	7.25***	−1.00	10.48***
		(0.63)	(0.52)	(0.52)	(1.10)	(1.03)	(1.37)
1 if Black	–	–	17.80***	17.89***	8.11***	3.95*	25.22***
			(1.19)	(1.18)	(1.52)	(2.03)	(1.81)
1 if Hispanic	–	–	9.56***	9.95***	–	18.04***	12.56***
			(0.94)	(0.93)		(2.07)	(1.58)
1 if other race or ethnicity	–	–	2.56**	3.12***	–	−0.57	9.17***
			(1.10)	(1.20)		(1.26)	(2.68)
1 if foreign-born	–	–	–	−1.53**	1.51	–	1.86
				(0.66)	(1.00)		(1.95)
Mean dep. var. (%)	10.28	10.28	10.28	10.28	18.56	9.65	22.66
Observations	24,955	24,955	24,955	24,955	7,230	2,822	6,291
Adjusted R^2	0.007	0.018	0.066	0.066	0.029	0.097	0.095

Note: All columns include adoption-cohort and graduation-cohort fixed effects; estimated coefficients are in percentage points. In column 1, the estimated coefficient on mandate exposure is the overall effect. In the remaining columns, mandate exposure is interacted with female, so this coefficient is the effect for men and the interaction is the female−male differential. Columns 5–7 restrict the sample to Black or Hispanic respondents, foreign-born respondents, and respondents with no education beyond high school. Standard errors are in parentheses and clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: ETWFE estimates of the effect of high school personal finance mandates on human capital and economic outcomes

	Education		Labor force status			Self-employment among employed			Income			
	Some college	College+	Employed	Unemployed	Not in LF	Self-emp.	SE inc.	SE uninc.	< 30k	30-50k	50-75k	75k+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Mandate exposure × Female	−0.78	2.92***	1.22	0.04	−1.26	1.56***	0.29	1.27***	−0.78	−0.75	3.93***	−2.40**
	(1.04)	(1.07)	(1.31)	(0.43)	(1.05)	(0.49)	(0.29)	(0.44)	(1.52)	(1.04)	(1.03)	(1.02)
Mandate exposure	−0.14	0.90	−0.57	0.48	0.10	1.31**	1.08***	0.23	−1.80	−1.58	−0.17	3.54***
	(1.65)	(2.27)	(1.18)	(0.61)	(1.11)	(0.65)	(0.36)	(0.56)	(1.33)	(1.29)	(1.45)	(1.34)
1 if female	2.63***	1.94**	−4.15***	0.43	3.72***	−3.16***	−1.43***	−1.73***	13.66***	−1.04	−5.62***	−7.01***
	(0.80)	(0.88)	(0.69)	(0.29)	(0.58)	(0.28)	(0.15)	(0.24)	(1.19)	(0.75)	(0.82)	(0.72)
1 if Black	6.97***	−19.77***	−10.23***	5.92***	4.32***	−1.19***	−0.46**	−0.74*	16.43***	−2.44**	−5.91***	−8.08***
	(1.18)	(1.46)	(1.04)	(0.49)	(0.75)	(0.42)	(0.22)	(0.39)	(1.53)	(0.98)	(0.86)	(1.15)
1 if Hispanic	9.19***	−24.62***	−2.53**	1.92***	0.62	−0.59	−0.38	−0.21	8.77***	0.54	−3.13***	−6.17***
	(1.48)	(1.70)	(1.00)	(0.47)	(0.75)	(0.61)	(0.23)	(0.50)	(1.49)	(0.98)	(1.03)	(1.73)
1 if other race or ethnicity	−0.89	4.74	−6.80***	1.28*	5.52***	−0.27	−0.44	0.18	2.05	−6.34***	−2.19**	6.49***
	(1.73)	(3.07)	(1.37)	(0.71)	(1.03)	(0.65)	(0.30)	(0.56)	(1.97)	(1.11)	(0.86)	(1.92)
1 if foreign-born	−10.65***	13.70***	0.46	−2.30***	1.85**	0.39	−0.01	0.40	−1.00	−2.16**	0.64	2.52***
	(0.94)	(1.99)	(1.05)	(0.54)	(0.78)	(0.85)	(0.27)	(0.76)	(1.27)	(1.04)	(1.24)	(0.87)
Mean dep. var. (%)	33.19	41.51	82.69	6.00	11.31	5.08	1.52	3.56	36.96	24.55	18.99	19.51
Observations	24,955	24,955	24,955	24,955	24,955	20,789	20,789	20,789	24,955	24,955	24,955	24,955
Adjusted R^2	0.019	0.062	0.018	0.012	0.011	0.007	0.003	0.003	0.049	0.006	0.009	0.044

Note: Each column is a separate regression of a binary outcome and includes adoption-cohort and graduation-cohort fixed effects; estimated coefficients are in percentage points. Mandate exposure is interacted with female in every column, so the estimated coefficient on mandate exposure is the effect for men and the interaction is the female−male differential. The four income categories are mutually exclusive. Standard errors are in parentheses and clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

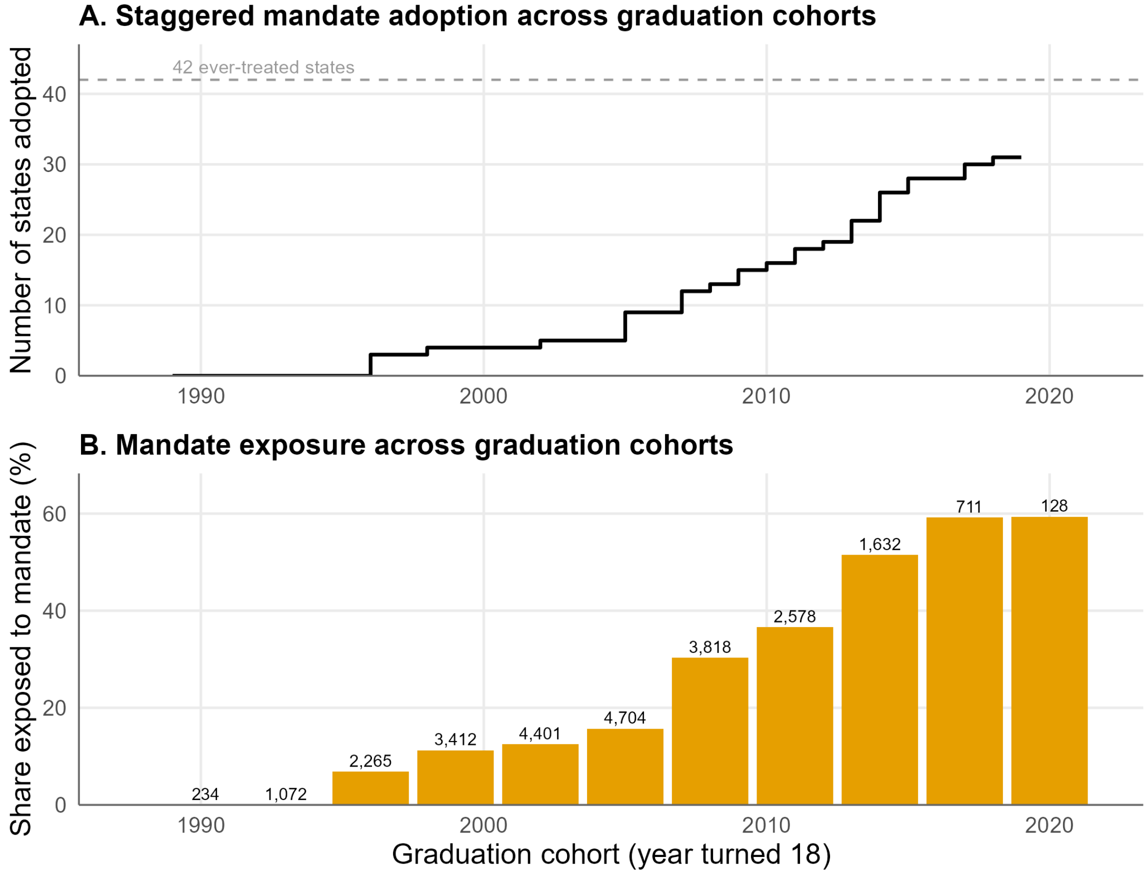
Table 8: TWFE estimates of the effect of high school personal finance mandates on financial education and financial knowledge

	Financial education			Financial knowledge						Self-rated
	Offered	Received	Received in HS	Score (0–5)	Comp. interest	Inflation	Bonds	Mortgage	Stocks	Score (1–7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mandate exposure × Female	0.54	0.47	0.29	0.13***	2.61*	1.73	4.35***	0.13	4.28***	−0.03
	(1.26)	(0.92)	(0.74)	(0.04)	(1.49)	(1.59)	(0.81)	(1.04)	(1.30)	(0.04)
Mandate exposure	0.34	2.09*	2.57**	−0.05	−1.32	−0.26	−2.74***	0.35	−1.14	0.02
	(1.42)	(1.16)	(1.10)	(0.04)	(1.45)	(1.40)	(0.72)	(1.04)	(1.13)	(0.04)
1 if female	−9.10***	−4.57***	−3.97***	−0.40***	−5.93***	−11.67***	−7.08***	−3.26***	−12.11***	−0.43***
	(0.76)	(0.48)	(0.56)	(0.03)	(0.62)	(1.05)	(0.45)	(0.70)	(0.73)	(0.02)
1 if non-White	3.12***	2.12***	0.15	−0.25***	−5.28***	−7.57***	−0.53	−8.37***	−3.39***	0.09***
	(0.85)	(0.67)	(0.45)	(0.03)	(0.81)	(0.92)	(0.48)	(0.86)	(1.24)	(0.02)
Mean dep. var.	37.86	24.08	14.85	2.22	65.20	42.15	20.32	59.14	35.07	4.80
Observations	26,421	26,421	26,421	31,659	31,659	31,659	31,659	31,659	31,659	30,624
Adjusted R^2	0.037	0.022	0.030	0.076	0.032	0.051	0.011	0.042	0.040	0.049

Note: All columns include state, graduation-cohort, and survey-year fixed effects and are estimated on the NFCS sample of unpartnered respondents. The dependent variables are, in columns 1–3, indicators for financial education offered, received in school, college, or at work, and received in high school; in column 4, the number of the Big Five financial literacy questions answered correctly; in columns 5–9, those questions taken one at a time, each an indicator equal to one for a correct answer; and in column 10, respondents' assessment of their own financial knowledge. For the binary outcomes in columns 1–3 and 5–9, estimated coefficients are in percentage points and mean dependent variables in percent; for columns 4 and 10, both are in points of the underlying scale. Observations differ across columns because the outcomes are not asked in every wave; the financial education questions in columns 1–3 are asked only from 2012 onward. Standard errors are in parentheses and clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

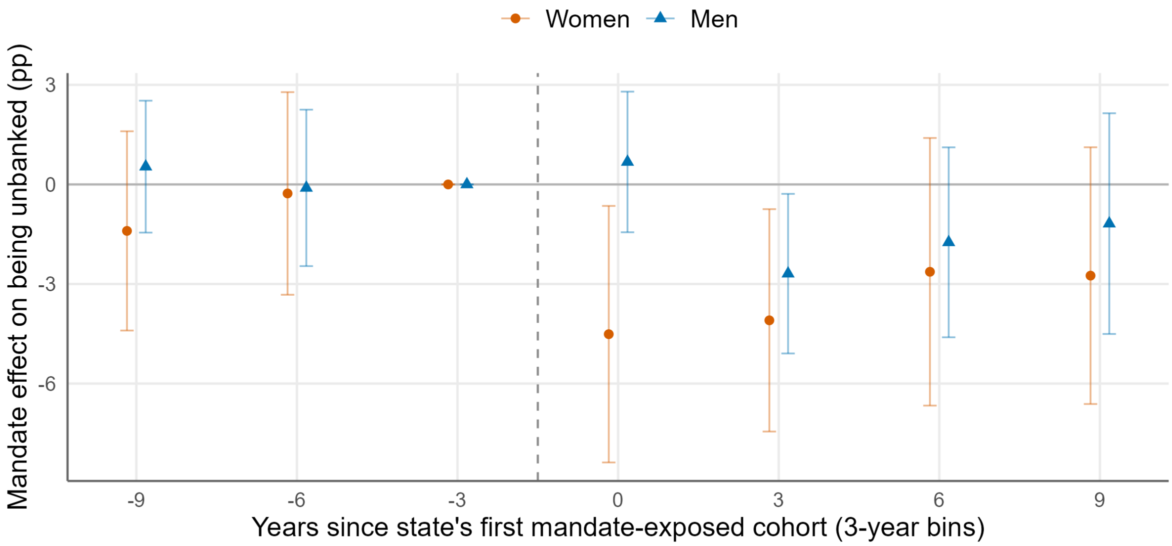
Figures

Figure 1: Staggered adoption of high school personal finance graduation requirements



Note: Panel A shows the cumulative number of states whose requirement binds by each graduation cohort, and the dashed line marks the 42 ever-treated states. The count reaches 31 by the 2019 cohort because the remaining 11 states first bind on cohorts graduating in 2020 or later, outside the sample; they enter the estimation as not-yet-treated controls. Panel B shows the share of the analysis sample exposed to a mandate in each three-year graduation-cohort bin, with the number of observations above the bars.

Figure 2: Event study estimates of the effect of personal finance mandates on being unbanked by gender



Note: Estimates are from an additive extended two-way fixed-effects (ETWFE) specification with never-treated states as the comparison group, separately for women and men. Graduation cohorts are grouped into three-year bins, and estimates are normalized to the bin three years before the first exposed cohort. The horizontal axis is years since the state's first mandate-exposed cohort. Points are estimates in percentage points, and bars are 95 percent confidence intervals from state-clustered standard errors.

Appendix

Table A1: State personal finance mandates and first exposed graduating cohorts

State	First exposed cohort	Stand-alone course	In estimation sample
States with a personal finance graduation requirement (42)			
Illinois	1970	—	Yes
New Hampshire	1993	2027	Yes
New York	1996	—	Yes
Michigan	1998	2028	Yes
Wyoming	2002	—	Yes
Arizona	2005	—	Yes
Arkansas	2005	—	Yes
Louisiana	2005	2027	Yes
North Carolina	2005	2024	Yes
Georgia	2007	2028	Yes
Idaho	2007	—	Yes
Texas	2007	2030	Yes
Utah	2008	2008	Yes
Colorado	2009	2030	Yes
South Carolina	2009	2027	Yes
Missouri	2010	2010	Yes
Iowa	2011	2021	Yes
North Dakota	2011	—	Yes
Kansas	2012	2027	Yes
Indiana	2013	2028	Yes
Oregon	2013	2027	Yes
Tennessee	2013	2013	Yes
Nebraska	2014	2024	Yes
New Jersey	2014	—	Yes
Ohio	2014	2026	Yes
Oklahoma	2014	—	Yes
Minnesota	2015	2028	Yes
Virginia	2015	2015	Yes
Alabama	2017	2017	Yes
Maine	2017	—	Yes
Florida	2018	2027	Yes
California	2020	2031	No
West Virginia	2020	2028	No
Mississippi	2022	2022	No
Nevada	2023	—	No
Kentucky	2024	2030	No
Rhode Island	2024	2024	No
Connecticut	2027	2027	No
New Mexico	2027	—	No
Wisconsin	2028	2028	No
Delaware	2030	2030	No
Pennsylvania	2030	2030	No
States with no requirement (9)			
Alaska	—	—	—
District of Columbia	—	—	—
Hawaii	—	—	—
Maryland	—	—	—
Massachusetts	—	—	—
Montana	—	—	—
South Dakota	—	—	—
Vermont	—	—	—
Washington	—	—	—

Note: The first exposed cohort is the earliest high school graduating cohort required to complete personal finance instruction; a dash in the Stand-alone course column marks a state that meets the requirement through an embedded course instead. The last graduating cohort observed in the sample is 2019, so a state enters the estimation sample only if its first exposed cohort is that cohort or earlier. Adoption dates are from Urban et al. (2020) and Urban's compilation of state financial education requirements, last updated in 2024.

Table A2: Sample means of key variables by treatment status, FDIC survey sample

Variable	Total (1)	Ever-treated (2)	Never-treated (3)	Difference (4)
Banking status				
1 if unbanked	10.28 (0.54)	10.65 (0.56)	6.10 (0.94)	+4.54***
Household structure[a]				
1 if never married, with children	17.24 (0.55)	17.39 (0.58)	15.48 (2.00)	+1.91
1 if never married, no children	65.09 (0.95)	64.65 (1.03)	70.07 (1.42)	−5.42***
1 if formerly married, with children	8.34 (0.44)	8.51 (0.48)	6.44 (0.54)	+2.07***
1 if formerly married, no children	6.54 (0.30)	6.64 (0.32)	5.32 (0.83)	+1.33
1 if married (spouse absent), with children	1.07 (0.06)	1.08 (0.06)	0.92 (0.14)	+0.16
1 if married (spouse absent), no children	1.73 (0.15)	1.72 (0.16)	1.77 (0.31)	−0.05
Race and ethnicity				
1 if White	52.06 (2.65)	51.84 (2.83)	54.54 (4.91)	−2.70
1 if Black	21.52 (2.21)	21.90 (2.34)	17.28 (7.38)	+4.61
1 if Hispanic	16.14 (2.74)	16.51 (2.94)	12.04 (1.74)	+4.46
1 if other race or ethnicity	10.27 (1.32)	9.76 (1.44)	16.13 (2.89)	−6.37**
Family income				
1 if family income is under $15,000	17.61 (0.70)	17.90 (0.78)	14.38 (0.56)	+3.52***
1 if family income is $15,000 to $30,000	19.35 (0.75)	19.64 (0.81)	16.01 (1.40)	+3.64**
1 if family income is $30,000 to $50,000	24.55 (0.68)	24.76 (0.75)	22.11 (1.12)	+2.66**
1 if family income is $50,000 to $75,000	18.99 (0.42)	18.97 (0.46)	19.21 (0.83)	−0.24
1 if family income is $75,000 or more	19.51 (1.63)	18.73 (1.80)	28.29 (1.94)	−9.57***
Education				
1 if high school graduate	25.30 (0.96)	25.66 (1.05)	21.17 (1.36)	+4.49***
1 if some college	33.19 (0.98)	33.54 (1.04)	29.19 (2.93)	+4.35
1 if college or more	41.51 (1.55)	40.80 (1.67)	49.64 (3.66)	−8.84**
Labor force participation				
1 if employed	82.69 (0.58)	82.50 (0.61)	84.92 (0.90)	−2.42**
1 if unemployed	6.00 (0.27)	6.10 (0.29)	4.87 (0.23)	+1.23***
1 if not in the labor force	11.31 (0.40)	11.41 (0.42)	10.22 (0.72)	+1.19
Foreign-born (1 if born outside the U.S.)	13.47 (1.51)	13.29 (1.65)	15.52 (1.57)	−2.24
Age in years	28.90 (0.05)	28.88 (0.06)	29.06 (0.15)	−0.18
Observations	24,955	20,843	4,112	

Note: Survey-weighted means of respondent characteristics by treatment status are reported. The Difference column shows the ever-treated mean minus the never-treated mean. Standard errors are in parentheses and clustered at the state level. Stars denote statistical significance from a weighted, state-clustered t-test of the difference. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
[a] Household structure is defined by combining marital status and the presence of a co-resident child aged 18 or younger. Formerly married includes divorced, separated, and widowed respondents.

Table A3: Sample means of key variables by treatment status, NFCS sample

Variable	Total (1)	Ever-treated (2)	Never-treated (3)	Difference (4)
Financial knowledge				
Quiz score (0–5)	2.22 (0.02)	2.19 (0.02)	2.38 (0.04)	−0.19***
1 if compound interest correct	65.20 (0.74)	64.14 (0.66)	70.79 (1.05)	−6.65***
1 if inflation correct	42.15 (0.71)	41.27 (0.70)	46.73 (1.19)	−5.46***
1 if bond prices correct	20.32 (0.39)	20.39 (0.44)	19.92 (0.73)	+0.47
1 if mortgage interest correct	59.14 (0.69)	58.65 (0.74)	61.70 (1.39)	−3.05**
1 if stock-vs-fund risk correct	35.07 (0.59)	34.32 (0.52)	39.01 (1.61)	−4.68***
Self-assessed knowledge				
Self-rated knowledge (1–7)	4.80 (0.02)	4.81 (0.02)	4.74 (0.05)	+0.07
Financial education				
1 if offered financial education (school, college, or workplace)	37.86 (0.44)	37.77 (0.46)	38.36 (1.26)	−0.60
1 if received financial education (school, college, or workplace)	24.08 (0.50)	24.06 (0.52)	24.15 (1.63)	−0.09
1 if received financial education in high school	14.85 (0.39)	15.06 (0.43)	13.79 (0.99)	+1.27
Demographics and education				
1 if non-White	55.06 (3.16)	54.47 (3.54)	58.18 (7.21)	−3.71
1 if aged 18–24	38.98 (0.63)	39.93 (0.45)	34.00 (2.01)	+5.93***
1 if aged 25–34	34.89 (0.48)	34.46 (0.44)	37.15 (1.72)	−2.69
1 if aged 35–44	26.13 (0.49)	25.61 (0.44)	28.85 (1.42)	−3.24**
1 if high school graduate	34.18 (0.83)	35.32 (0.72)	28.24 (2.27)	+7.08***
1 if some college	41.90 (0.79)	42.37 (0.65)	39.49 (3.55)	+2.87
1 if college or more	23.91 (1.30)	22.32 (0.88)	32.27 (5.65)	−9.95*
Observations	31,659	26,218	5,441	

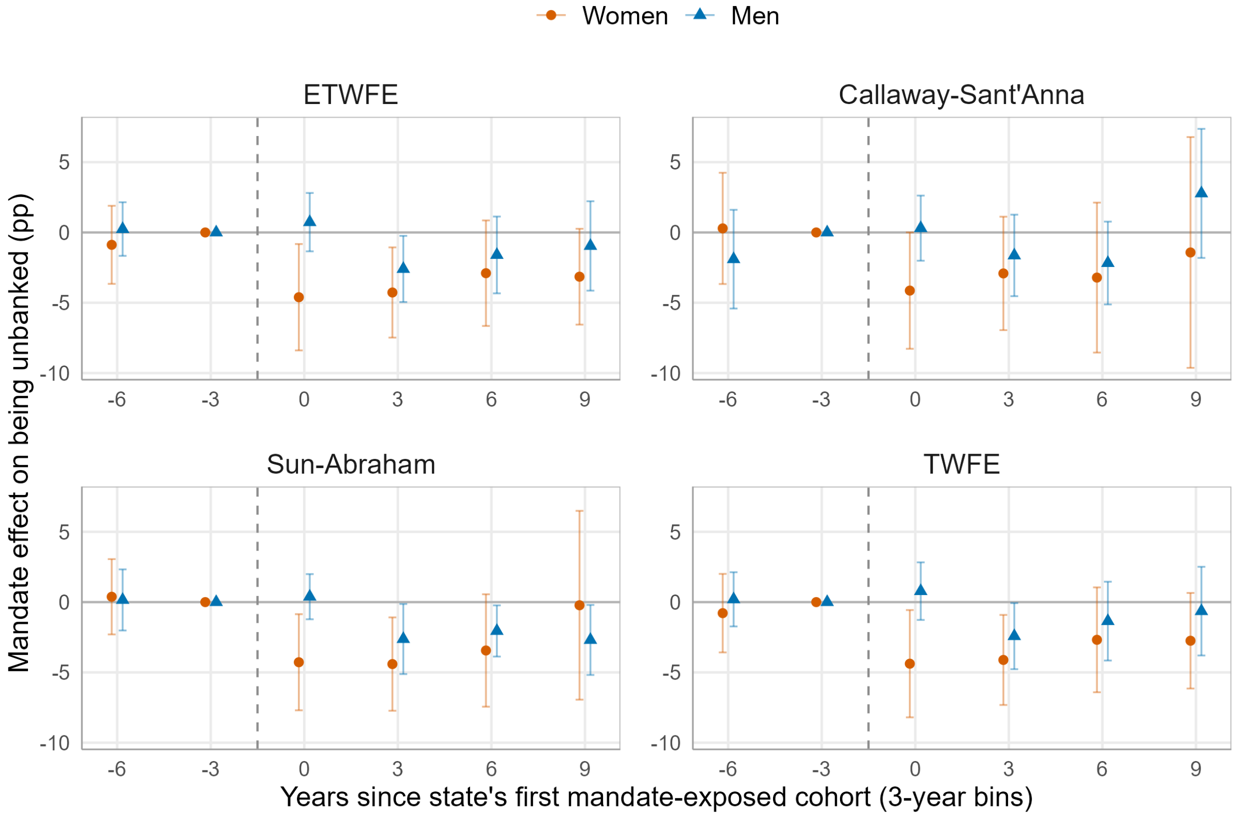
Note: Survey-weighted means of NFCS respondent characteristics by treatment status are reported. The Difference column shows the ever-treated mean minus the never-treated mean. The financial education questions are asked only in NFCS waves from 2012 onward. Standard errors are in parentheses and clustered at the state level. Stars denote statistical significance from a weighted, state-clustered t-test of the difference. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: ETWFE estimates of the effect on the probability of being unbanked, adding post-treatment mediators as controls

	Dependent variable: 1 if unbanked				
	(1)	(2)	(3)	(4)	(5)
Mandate exposure × Female	−2.66***	−2.17***	−2.21***	−2.44***	−1.86***
	(0.69)	(0.57)	(0.70)	(0.59)	(0.51)
Mandate exposure	−1.78**	−1.53**	−1.63**	−1.89***	−1.64***
	(0.76)	(0.75)	(0.66)	(0.70)	(0.62)
1 if female	4.71***	2.04***	5.35***	3.95***	3.03***
	(0.52)	(0.37)	(0.51)	(0.44)	(0.36)
1 if Black	17.89***	14.54***	14.97***	15.98***	12.65***
	(1.18)	(0.97)	(1.16)	(1.12)	(0.98)
1 if Hispanic	9.95***	8.62***	6.37***	9.47***	6.52***
	(0.93)	(0.85)	(0.86)	(0.84)	(0.77)
1 if other race or ethnicity	3.12***	2.31**	3.90***	1.87*	2.26***
	(1.20)	(0.92)	(1.00)	(1.04)	(0.81)
1 if foreign-born	−1.53**	−1.43**	−0.12	−1.44**	−0.60
	(0.66)	(0.61)	(0.58)	(0.61)	(0.56)
1 if family income \$15,000 to \$30,000	–	−15.77***	–	–	−12.44***
		(1.11)			(0.96)
1 if family income \$30,000 to \$50,000	–	−24.09***	–	–	−18.47***
		(1.13)			(0.92)
1 if family income \$50,000 to \$75,000	–	−25.25***	–	–	−18.00***
		(1.13)			(0.88)
1 if family income \$75,000 or more	–	−25.65***	–	–	−17.18***
		(1.13)			(0.87)
1 if Some college	–	–	7.97***	–	4.09***
			(0.50)		(0.37)
1 if High school graduate	–	–	18.44***	–	12.82***
			(0.94)		(0.74)
1 if unemployed	–	–	–	18.92***	11.25***
				(1.46)	(1.08)
1 if not in the labor force	–	–	–	18.25***	8.79***
				(1.01)	(0.79)
Mean dep. var. (%)	10.28	10.28	10.28	10.28	10.28
Observations	24,955	24,955	24,955	24,955	24,955
Adjusted R^2	0.066	0.162	0.124	0.119	0.201

Note: To test how sensitive the estimates are to income, education, and employment, I include them as control variables. Because these variables are measured only after high school, the mandate may already have affected them, so conditioning on them absorbs part of the treatment effect and attenuates the estimated gender differential. I study them as outcomes in Table 7 instead. All columns include adoption-cohort and graduation-cohort fixed effects; estimated coefficients are in percentage points. Mandate exposure is interacted with female in every column, so the estimated coefficient on mandate exposure is the effect for men and the interaction is the female−male differential. Standard errors are in parentheses and clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A1: Event study estimates of the effect of personal finance mandates on being unbanked by gender across estimators



Note: Each panel presents a different estimator: extended two-way fixed effects (ETWFE), Callaway–Sant'Anna, Sun–Abraham, and two-way fixed effects (TWFE). The first three use never-treated states as the comparison group; TWFE uses the standard TWFE comparisons. Points are estimates in percentage points, and bars are 95 percent confidence intervals from state-clustered standard errors.