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Abstract

We examine the impact of financial education on credit decisions during COVID-19. The pandemic presented economic challenges, but policy responses provided opportunities for savvy borrowers. Using variation in state-mandated financial education during high school, we find that mandated borrowers reduced revolving credit card balances by larger amounts after stimulus checks were distributed and were more likely to buy homes and refinance mortgages during times of low interest rates. Paused student loan borrowers bound by mandates originated more auto loans and mortgages while reducing growth in credit card balances. Our findings underscore the importance of financial education for economic resilience.

JEL classification: D14, G51, G53
Key words: financial education, high school curriculum, financial decisions, household debt, COVID-19 pandemic

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1 Introduction

Economic crises such as the Great Recession of 2008 and the COVID-19 pandemic of 2020-2021 expose households to profound financial disruptions with potentially long-lasting impacts. For instance, individuals who experienced greater labor market shocks during the Great Recession had lower employment rates nearly a decade later (Yagan, 2019), and those in counties more affected by the 1980-1982 recession faced permanently reduced educational attainment and income levels (Stuart, 2022). Similarly, students graduating during recessions experience persistently worse labor market outcomes, but those who were able to navigate the crises by transitioning quickly to better firms were able to mitigate these long-term harms (Oreopoulos et al., 2012; Rothstein, 2023). These findings motivate investigation into potential policy levers which might help households better navigate crises to avoid the lasting impacts of economic disruptions. While the Great Recession of 2008 caused deep economic scars in the form of high unemployment, foreclosure, and credit delinquencies, the COVID-19 pandemic and subsequent policy responses, the context of this paper, presented a different set of unique challenges and opportunities for households.

The abrupt closure of businesses and schools during the pandemic necessitated a swift and decisive response from fiscal and monetary authorities. Measures included stimulus payments, enhanced unemployment distributions, sharp reductions in interest rate targets, and broad debt forbearance measures. These supports not only softened the immediate economic impact but also create unique financial opportunities for households. For example, fiscal stimulus checks, combined with fewer opportunities for consumption, led to a \$300 billion reduction in aggregate credit card debt from Q4 2019 to Q2 2021. Additionally, 14 million homeowners refinanced their mortgages at lower interest rates, reducing aggregate monthly payments by \$30 billion. And more than three years of federal student loan payment forbearance waived an estimated \$260 billion in monthly payments for covered borrowers (Haughwout et al., 2023). In stark contrast to previous recessions, these measures resulted in historically low rates of delinquencies, bankruptcies, and foreclosures, while enabling many households to strengthen their financial positions. However, uptake of these financial opportunities varied significantly across households, and the reasons for this divide remain unclear. Uneven uptake not only undermines the effectiveness of policy interventions but also risks exacerbating inequalities during economic recovery. This raises the central question of this paper: What policy interventions can help to equip individuals to better navigate the economic and financial challenges and opportunities posed by crises to improve financial resilience?

One potential tool is financial education, which may help households respond more effectively to economic shocks, policy measures, and financial opportunities. Descriptive evidence suggests a correlation between financial literacy and resilience during economic disruptions (Lusardi and Streeter, 2023; Hasler et al., 2023), but causal evidence is limited. Moreover, the mechanisms through which financial education influences financial resilience are not well understood. This paper seeks to address these gaps by examining the role of financial education in shaping household decision-making during the COVID-19 pandemic and the subsequent policy responses in the United States.

This study examines which households were better able to leverage these financial opportunities, focusing on the role of financial education. We exploit plausibly exogenous variation in exposure to financial education across states and cohorts using state-level high school personal finance education requirements. Previous studies show that these requirements improve financial outcomes, including lower credit delinquencies, higher credit scores, reduced reliance on high-cost debt products, better student loan repayment rates, and higher subjective well-being (Brown et al., 2016; Urban et al., 2018; Stoddard and Urban, 2020; Harvey, 2018; Mangrum, 2022; Burke et al., 2024). Building on this evidence, we investigate whether individuals exposed to financial education during high school were better prepared to navigate the economic challenges and opportunities of the COVID-19 pandemic.

We begin by constructing a range of individual level decision variables from a nationally representative sample of credit reports. We focus on decisions that were particularly salient to savvy individuals during the pandemic as a result of a range of fiscal and monetary actions. First, we explore the evolution of credit card balances. Due to reduced consumption opportunities and several rounds of fiscal stimulus payments, consumers dramatically reduced credit card indebtedness between the start of the pandemic and the middle of 2021. We explore whether those bound by financial education mandates reduced outstanding credit card balances by more than those not bound by mandates. We find that individuals who were required to complete financial education during high school reduced their credit card balances by larger amounts, roughly 9% more than their not mandated peers. Further, we provide evidence that the treatment effects were larger for those living in middle income neighborhoods, where a larger share of tax-filers received stimulus payments relative to high income neighborhoods. We take this as evidence that those with more financial education were more likely to use stimulus payments to pay down debt.

Next, we investigate mortgage decisions, including new purchase mortgage originations and existing mortgage refinances. In part due to monetary policy actions, the average interest rate on a 30 year mortgage dropped to a historic low of 2.65% by January 2021 (Freddie Mac, 2024). These low rates created opportunities for new and existing home owners to purchase new homes and refinance existing mortgages with lower monthly payments. We find that individuals with more financial education were more likely to take advantage of low mortgage rates to finance a new home, and this was primarily driven by borrowers buying their first home – those bound by financial education requirements were 10% more likely to open a mortgage for the first time. Treated individuals were also more likely to refinance existing mortgages, predominantly through rate refinances, which reduced interest rates and monthly payments without increasing the outstanding balance, rather than cash-out refinances, which involves withdrawing equity while increasing outstanding mortgage balance. The higher likelihood of mortgage rate refinances due to financial education was almost 4% larger than for untreated individuals, which would translate to at least 36,000 additional rate refinances nationally. Additionally, we find that while the treatment effect was similar across neighborhood income, the difference in the rate of mortgage refinances between those with more financial education and those with less was most stark for high credit score individuals.

We then explore the financial decisions of those with federal student loans. At the onset of the pandemic, executive and congressional actions suspended federal student loan payments and set interest rates at 0% and the payment freeze and suspension of interest lasted more than three years through August 2023 resulting in more than $260 billion in waived payments (Haughwout et al., 2023). Additionally, borrowers with defaulted loans were extended an opportunity to rehabilitate their loans to current status without having to make the typically required nine months of on-time payments. We investigate whether federal student loan borrowers who had more financial education were more likely to respond to these opportunities. First, we test whether mandated borrowers with paused federal student loans reduced their non-housing balances by more than not mandated borrowers (also excluding their paused loans). For borrowers with paused federal loans who had non-zero payments prior to the pause, we find that borrowers with more financial education increased their total debt balances on net during the moratorium. But this overall increase masks differences across products, with housing and auto loan balances growing (via new originations) and credit card balances declining relative to their peers with less financial education. On the other hand, mandated borrowers who had defaulted loans at the onset of the pandemic were no more likely to rehabilitate those loan relative to their not mandated peers.

We conclude by investigating the mechanisms unpinning the higher likelihood of making opportune financial decisions by those who had more financial education in high school. Since many years passed for most individuals between the financial education intervention and the pandemic, it is possible that the treatment improved the financial situation prior to the pandemic which would likely increase financial resilience even in the absence of a higher likelihood to take-up opportune decisions. On the other hand, it is also possible that increased financial education did not materially improve the credit standing of individuals in the intervening years, but the additional financial education instead increased the likelihood of take-up of these decisions. And of course, some combination of these two is also possible. To better understand these mechanisms, we control for a range of observable credit characteristics for each borrower just before the pandemic to test whether the difference between treated and not treated individuals remains. For most outcomes, the higher likelihood of making opportunistic financial decisions remains unchanged after these controls, except in the case of mortgage refinancing. For mortgage refinance, controlling for pre-pandemic characteristics, including mortgage size, explains about three-quarters of the difference between individuals with and without mandated financial education. We also test whether treated and untreated individuals had different credit attributes prior to the pandemic. We find that individuals bound by financial education mandates were generally similar to those not bound, except they had larger mortgages prior to the pandemic, which increased the potential benefits from refinancing. Our decomposition of the total treatment effect indicates that for most outcomes, financial education directly increased the likelihood of taking advantage of financial opportunities, rather than merely putting individuals in a better position to do so.

Our findings contribute to three broad strands of the literature. First, we contribute to a broad literature that explores the drivers of heterogeneous household financial decisions during crises. Many of the papers studying the COVID-19 pandemic use rich, high frequency spending data to track household responses at the onset of the pandemic and subsequent government intervention (Chetty et al., 2024; Baker et al., 2020, 2023). Chetty et al. (2024) found heterogeneous spending responses across households by income (as proxied by median ZIP code income) at the onset of the pandemic. Notably, while higher income households pulled back substantially on non-essential spending, lower income households had smaller reductions in spending and recovered back to 2019 levels by August 2020. Baker et al. (2020) also found deeper spending cuts for those with households with children and those with low liquidity. Baker et al. (2023) use rich financial data to track household responses to the receipt of stimulus checks in April and May of 2020 and find significantly larger spending responses for those with low liquidity and very small responses for those with high levels of liquidity. Additional survey data shed light on how households used their stimulus payments. Findings from the U.S. Census Household Pulse Survey suggest that roughly 15% of households planned to use stimulus checks primarily toward paying down debt and another 15% planned to use it primarily towards savings, and that those with incomes between $50,000 and $100,000 were more likely to save or pay down debt than households with lower or higher income (United States Census Bureau, 2020). Armantier et al. (2020) and Armantier et al. (2021) find that the marginal propensity to repay debt increased from the first stimulus payment to the second stimulus payment with a corresponding decline in the marginal propensity to consume. Analysis by Koşar et al. (2023) of data from the New York Fed's Survey of Consumer Expectations indicate that households on average used a third of their transfers to pay down debt and that households with low net liquid wealth-to-income ratios were more likely to pay down debt and more likely to improve their net asset positions. Several papers also document heterogeneous take-up of government and non-government relief programs. Kim et al. (2024) found mortgage servicer-level frictions prevented many households from entering mortgage forbearance to avoid delinquency, and using the servicer assignment as an exogenous source of variation in mortgage forbearance, they found that extra cash flow from mortgage forbearance helped pay down credit cards, but only among those with financial liquidity. Those with higher credit card utilization rates appeared to direct the savings from forbearance to increased consumption instead. Hedin et al. (2020) document income and demographic heterogeneity across California as explaining differences in a potentially eligible worker's claiming unemployment benefits. Our findings in this paper provide evidence for another mechanism by which households might differ in their response to crises: having the knowledge and ability to make advantageous decisions through previously learned financial education. Skills learned during coursework may better equip households to form best responses during these crises which can help them navigate economic shocks and uncertainty.

Next, our paper contributes to a deep literature that explores the effectiveness of fiscal and monetary policy, and the extent to which differential transmission of policy across households can affect inequality. Several papers have studied the size and heterogeneity of fiscal multipliers as a result of government spending (Blanchard and Perotti, 2002; Auerbach and Gorodnichenko, 2012; Kaplan and Violante, 2014) and how fiscal policy can interact with monetary policy to affect fiscal multipliers through the interaction of spending, interest rates, and debt pay-down (Christiano et al., 2011; Koşar et al., 2023; Kaplan and Violante, 2014).[1] Notably for our context, these papers note that stimulus is often spent on debt reductions, such as paying down credit cards, which causes no immediate fiscal stimulus through consumption but may enable future consumption via newly available credit limits and reduced interest charges. Additionally, monetary policy, through lower interest rates, can support long-run future consumption by enabling favorable conditions for homeowners to refinance mortgages (Agarwal et al., 2023). The substantial surge in mortgage refinancing during the low interest rate environment of the COVID-19 pandemic and subsequent recovery, when more than 14 million borrowers refinanced their mortgages, freed-up on average over \$2,000 per year in smaller monthly payments (Haughwout et al., 2023). However, in the case of debt pay-down and mortgage refinancing, little is known about who is more likely to take up these opportunities when the situation arises and what policies might improve such take-up.

Lastly, we build on a deep and evolving literature on financial education and downstream behaviors (Fernandes et al., 2014; Kaiser et al., 2022; Brown et al., 2016; Urban et al., 2018; Stoddard and Urban, 2020; Harvey, 2019; Burke et al., 2024; Mangrum, 2022). While the early literature showed little effect (Fernandes et al., 2014), several more recent works summarizing the literature find that financial education leads to improvements in downstream financial knowledge and behaviors (Kaiser et al., 2022). Specifically relating to our context, previous literature has shown that mandated financial education during high school can be effective in improving credit outcomes (Brown et al., 2016; Urban et al., 2018), post-secondary financial aid decisions (Stoddard and Urban, 2020), reduce reliance on high cost debt products (Harvey, 2019), improve financial well-being (Burke et al., 2024) and

[1] More broadly, Auclert et al. (2019) evaluate the macroeconomic impacts of government consumer debt relief programs during the Great Recession, and Demyanyk et al. (2019) finds heterogeneity in fiscal multipliers by baseline levels of consumer indebtedness prior to the Great Recession.

improve student loan repayment (Mangrum, 2022). While most of the literature focuses on financial outcomes, we pay particular attention to financial decisions, and we provide evidence that financial education increases the take-up of opportune financial decisions when opportunities arise.

The paper proceeds as follows. Section 2.1 discusses the data and describes the construction of the decision variables we use as outcomes in the analysis. Section 3 discusses our empirical strategy and how we leverage variation in financial education requirements across states and high school graduation cohorts to estimate the impact of financial education on financial decisions. Section 4 presents the main results from our analysis, beginning with the effect of financial education on each decision followed by an decomposition of the mechanisms. Section 5 presents results from a series of robustness tests. Section 6 concludes.

2 Data and Variable Construction

2.1 Data

The primary data for credit outcomes for our analysis is the New York Fed Consumer Credit Panel (CCP). The CCP is a 5% anonymized random sample of Equifax credit reports including borrower information such as age and Census Block identifiers for address along with credit and debt information such as balances, delinquencies, credit scores, and new originations. The individuals in the primary sample are selected using the randomly assigned last two digits of their social security number, producing a dynamically-updated panel dataset that is representative of the population of individuals with a social security number and credit score at every point in time.[2] Data are compiled quarterly from 1999 to the present.[3] In addition to the primary sample, the CCP also contains a household sample which includes all individuals residing at the same address as the primary sample.

[2]See Lee and Van der Klaauw (2010) for more details about the sampling design and content of the CCP.

[3]During 2020 and 2021, monthly data were made available to monitor the critical developments of household financial situation.

To match the personal finance education mandate data, we limit the sample to those who turned 18 years old, the typical age of high school graduation, between 2000 and 2018. In our baseline analysis, we follow Brown et al. (2016) and assign state of high school using the state of residence each individual first appeared in the data. In Section 5, we show that our results are robust to more stringent assignments of high school state and sample inclusion.

The CCP does not include data on borrower income, thus we also use data from the 2015-2019 American Community Survey, produced by the U.S. Census, to characterize the socioeconomic status of neighborhoods where individual CCP sample members reside. We use data on the median household income at the ZIP code level to assign a neighborhood income to each observation in the CCP. Additionally, we use the definition of binding high school mandates to include personal finance education in course material from Burke et al. (2024). We discuss these data and our empirical strategy more completely in Section 3.

2.2 Decision Variable Construction

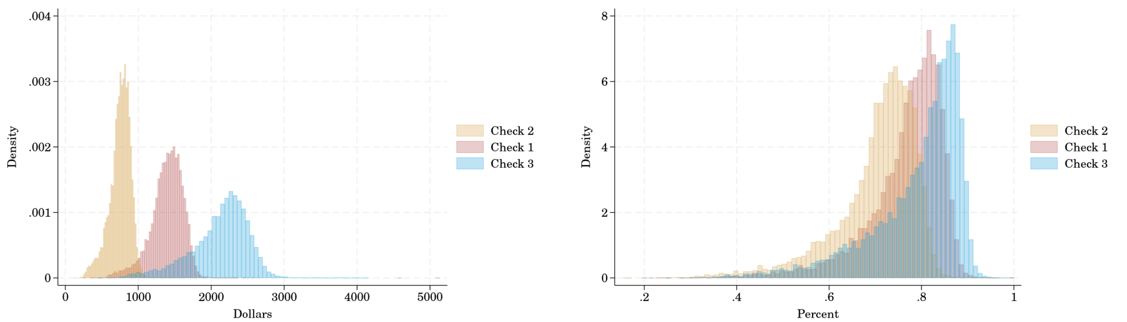
2.2.1 Credit card outcomes

Using the CCP, we measure six opportunistic financial decisions that individuals could have made during the pandemic.[4] Analyses for each outcome are conditional on borrowers' ability to make the decision (e.g. having the credit product in question). The first set of outcomes measures whether and by how much a borrower reduced their outstanding credit card balance during the months following each of the three sets of stimulus checks dispersed by the federal government in 2020 and 2021. The first economic impact payment was part of the CARES Act in March 2020. Single tax filers were eligible for up to \$1,200 while married joint tax filers were eligible for up to \$2,400. Qualifying dependents added up to \$500 in payments. Payments were means tested with eligibility phasing out beginning at \$75,000 (\$150,000 for married joint filers) and phasing out entirely at \$99,000 (\$198,000

[4]Note, we do not take a normative stance on whether these decisions were sound for each borrower. Instead, we recognize that the fiscal and monetary actions taken during this period enabled borrowers to potentially advance their financial position as a result of these decisions and we test whether borrowers were more likely to uptake these decisions.

Figure 1: Stimulus payment receipt

(a) Average payment amount per tax filing (b) Percent tax filers received payment per ZIP code



Notes: Observations are ZIP code-level and weighted by number of tax filers.
Source: Internal Revenue Service.

for married joint filers). The first stimulus checks began distribution on April 10, 2020. The second economic impact payment was paid out beginning in December 2020 for up to $600 for adults and dependents with a more strict income eligibility (full payments were set at the same maximum income but the phase out region ended at $87,000 for single filers and $174,000 for married filers). The last stimulus was the result of the American Rescue Plan Act and was the most generous in terms of maximum payments – individuals and dependents each received up to $1,400 – but the end of the phase out region again was reduced to $80,000 and $160,000 for single filers and married filers, respectively. The third payments were made beginning on March 17, 2021.

Survey evidence of households' use of the first stimulus check suggests that around 34% of the first stimulus funds were spent on reducing household debt (Armantier et al., 2020). Additionally households marginal propensity to consume declined in each round (from 29% to 26% to 25%) while the average percent saved increased each wave (Armantier et al., 2021). The findings indicate that debt pay-down was smaller in the first wave, potentially because of the substantial uncertainty at the onset of the pandemic, but grew in subsequent waves. Hence, we test whether those bound by financial education mandates reduced their credit card balances by more than those not mandated for each wave of pandemic-era stimulus. Additionally, due to the means-testing of stimulus payments, we expect the effect to be strongest for lower- and middle-income households.

We construct two periods of credit card paydown from these three payments. The first outcome is the credit card paydown after the first stimulus payment check which is the dollar difference between a person's total outstanding credit card balance in June 2020 (after the payment) relative to their outstanding balance in March 2020 (before the payment). Since the second and third economic impact payment were released in close proximity (on December 29, 2020 and March 17, 2021), we group them into one financial decision by calculating the change in outstanding credit card balances between December 2020 and May 2021. Negative values of these outcomes represent a reduction of credit card balances. We take a reduction in credit card balances to be an opportunistic decision for individuals who receive stimulus payments. Smaller credit card balances are beneficial to borrowers because it reduces their outstanding debt, increases available credit limits for future use, and reduces interest charges. However, one important consideration is the fact that reported outstanding credit balances include both new spending and revolving debt, and the credit bureau data does not disentangle these components. To mitigate these concerns, we classify borrowers into either transactors (statement balance was paid in full and no interest or fees were accumulated) or revolvers (accumulating interest on revolving balances) based on reported balances and minimum payments at the credit card account level. After classifying each credit account into either revolving or transactor, we then denote each borrower as a transactor or revolver according to whether they had any revolving accounts; those with no revolving accounts are transactors while those with at least one account charged interest are considered revolvers.[5] We estimate our main specification for the full sample before splitting the sample by revolver status and estimating again separately for each. If indeed stimulus payments led to debt reduction (and not merely changes in spending) we should see greater reductions in balances for revolvers, who were carrying balances, than for transactors.

[5] We create these classifications at the credit card account level by exploiting cluster points in the minimum payment as a share of the balance to determine whether an account is charged interest or fees on each statement. A more comprehensive discussion of this methodology will be available shortly in a companion paper. Please contact the authors for a preliminary draft.

Additionally, we do not directly observe who receives stimulus payments. We take two steps to target the analysis toward those who received stimulus checks. First, we partition our data sample into terciles of median neighborhood income using data from the ACS. We then run our analysis separately for each tercile of neighborhood income (low, middle and high income). Second, we use the IRS SOI data to compute a ZIP code level share of tax-filers who received a payment from each round of stimulus. We then use this measure to augment our treatment variable to test whether those who were mandated and more likely to receive a stimulus check paid down larger credit card balances.

2.2.2 Mortgage outcomes

The next two sets of outcomes measure activity in the mortgage market. We begin with analyzing new home purchases. We first create a binary outcome variable that characterizes whether an individual took out a new mortgage for a home purchase between the second quarter of 2020 and the last quarter of 2021, a period characterized by historically low mortgage rates. We then further refine the sample by limiting to only those people who did not previously have a mortgage, an approximation of first-time home buyers. Next we look at whether an individual refinanced an existing mortgage during the same time period. First, we measure whether an individual refinanced their mortgage, then we break mortgage refinances into either a cash-out refinance, whereby a homeowner extracts accumulated equity from their home in the form of cash, or a rate refinance, whereby a homeowner can leave equity untouched and take out a new mortgage for their existing mortgage balance at a new, prevailing (likely lower) interest rate.[6]

We take the stance that each of these decisions were potentially advantageous to borrowers, but each decision may have benefited individual borrowers differently or not at all. First, individuals who bought homes during this period were able to lock in historically low interest rates. The average 30 year fixed rate home mortgage hit a low of 2.65% in January 2021, lower than any other rate on record. By October 2023, the average 30 year fixed

[6]Further discussion of the construction of these variables is in Section Online Appendix A.

rate mortgage would hit 7.8%, almost triple the low (Freddie Mac, 2024). As a result, the monthly payment on a $500,000 mortgage with a 20% down payment would increase from roughly $1,600 per month with a 2.65% rate to $2,880 with a 7.8% rate, an increase of over 75%. As a result of the change in interest rates, new mortgage originations plummeted from $1.2 trillion in the second quarter of 2021 to $323 billion in the first quarter of 2023 (Federal Reserve Bank of New York, 2024). These lower interest rates also benefited those who already held mortgages via refinances. Homeowners could refinance their mortgage by issuing a new mortgage at prevailing rates with the option of withdrawing accumulated equity as cash. For the roughly 14 million mortgagors who refinanced during this period, monthly mortgage payment reductions averaged $178 per month (Haughwout et al., 2023).

2.2.3 Student loan forbearance

The last set of outcomes focuses on student loan borrowers. At the onset of the pandemic, Executive and Congressional actions suspended monthly payments for student loans owned by the federal government and set interest on these loans to zero percent. Subsequent extensions of this payment pause and interest waiver lasted until September 2023 before repayment ultimately resumed. This unprecedented action afforded a significant monthly savings for federal student loan borrowers, roughly $240 per month for those who had a payment due in February 2020. These savings potentially afforded federal student loan borrowers the ability to pay down other debts or to cover debt obligations on newly originated debts. For those delinquent on their student loans entering the moratorium, access to new loans was generally improved due to an average increase in credit scores associated with the removal from delinquent student loans from credit reports(Mangrum et al., 2022). Dinerstein et al. (2024) presents evidence of this mechanism by comparing paused federal borrowers to those without paused loans and found higher mortgage, auto, and credit card balances among those who were paused.

We empirically test whether borrowers with paused federal student loans and with more financial education behaved differently with regard to debt than those with less financial education. We create a range of variables which measure the change in balances between the start of the pandemic forbearance (using February 2020 as the baseline) until June 2023, the date in which the Supreme Court struck down the Biden Administrations broad student loan forgiveness plan in the Biden vs Nebraska decision, which began the process to resume student loan payments.[7] We begin by looking at the change in all balances excluding paused student loans before decomposing balances into four categories: 1) Auto, 2) Credit Card and Other, 3) Housing, and 4) Non-Direct Student Loans. In each of these outcomes, a reduction in balances is reported as a negative number. We also create binary variables denoting whether a borrower initiated a new mortgage or auto loan.

3 Empirical Strategy

3.1 Identification Strategy

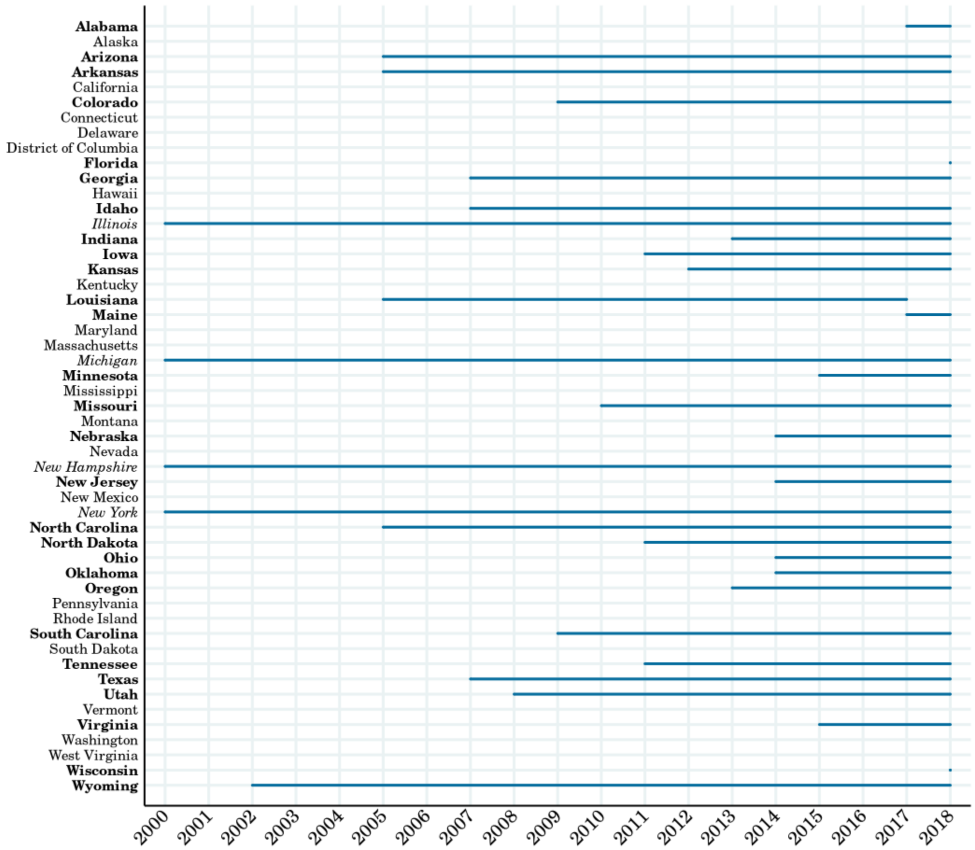
To estimate the causal effect of additional financial education on the probability of making certain financial decisions, we leverage plausibly exogenous variation in state level mandates that required certain cohorts to be exposed to financial education during high school. We use the definitions of a binding mandate initially from Urban and Schmeiser (2015) and updated in Burke et al. (2024) which have subsequently been updated through the graduating class of 2018. This data assigned each high school cohort for each state to a binary mandate status denoting whether that cohort was required to be exposed to financial education coursework according to state standards.[8]

Figure 2 reports the mandate status for all 50 states and D.C. for each high school graduating class. At the start of our sample (the 2000 graduating class), only four states

[7] We do not measure the change at the end of the forbearance (September 2023) because some borrowers began making large, payments toward their covered student loans in the period between the Supreme Court Biden v Nebraska decision and the resumption of interest (Chakrabarti et al., 2023).

[8] We use the assignment from Burke et al. (2024) instead of the data from the Council for Economic Education (as in Brown et al. (2016)) because the former assigns mandate status by the high school graduation cohort while the later uses the legislative year the mandate was passed. This assignment should reduce the attenuation bias caused by misidentification of first mandated cohort. Additionally, the states with personal finance education mandates differs somewhat across these two sources. A more comprehensive discussion of the differences can be found in Urban and Schmeiser (2015).

Figure 2: State financial education mandates, 2000-2018 high school graduating cohorts



Notes: States in bold adopted a mandate during our sample period. Italicized states had mandated financial literacy education during our entire sample period.
Source: Burke et al. (2024).

had mandatory course standards for financial education, however by 2010 that total grew to 16 and by 2018, 32 states had adopted standards to require financial education within course material for high school graduation.[9]

Several papers use this identifying variation to test whether required financial education improves outcomes for those mandated. Stoddard and Urban (2020) finds that new college students who were bound by mandates were more likely to apply for federal student aid, were more likely to take out a federal student loan, and were more likely to receive grant aid.

[9]Louisiana adopted state standards for the 2005 graduating class but removed them beginning with the 2018 graduating class.

They also find mandated students shifted away from higher cost borrowing (credit cards and private student loans) and toward lower cost borrowing (subsidized federal student loans). Mangrum (2022) finds improvements in student loan repayment rates, particularly for borrowers from low income families and first generation college students. Harvey (2019) finds that financial education mandates reduce borrower reliance on Alternative Financial Services like payday and auto title loans. Burke et al. (2024) finds that financial education mandates improve subjective financial well-being, particularly for men and college graduates. They also find improvements in objective financial situations. On the other hand, (Harvey and Urban, 2023) find no effect of the mandates on planning or saving for retirement.

Collectively, these papers suggest that mandated financial education can improve a range of financial outcomes for those who were bound by mandates when compared to those who were not bound. However, it is important to test whether adoption of these mandates is correlated with other outcomes or policies occurring at the same time in the same states and to test whether states that ever adopted these mandates are similar to those who never adopted. Several papers in the previous literature present evidence that the states who adopt are similar in economic characteristics to those who do not adopt, and that the timing of adoption is not correlated with other outcomes. For instance, Stoddard and Urban (2020) test whether state economic and governance measures predict the adoption of a state personal finance education mandate and find that none of their included regressors statistically predict adoption and all of the estimates are near-zero. They also test to whether mandate adoption occurred alongside other changes in course standards or reforms and find no correlation for the state for which they collect comprehensive data. We present further evidence of similarity between adopting and non-adopting states prior to adoption in Section Online Appendix A where we estimate event studies for states in the years near the graduation of the first adopting cohort as well as testing for pre-trends in our main outcome variables.

3.2 Estimating Equation

Similar to Stoddard and Urban (2020); Brown et al. (2016); Mangrum (2022); Harvey (2019); Burke et al. (2024) and others, we leverage variation in financial education mandate adoption across states and over time, comparing outcome variables across those bound by a state mandate against those who were not bound by a mandate. The specification takes the following form,

$$Y_{isc} = \gamma D_{sc} + \beta X_{isc} + \alpha_s + \delta_c + \varepsilon_{isc} \tag{1}$$

where Y_{isc} is an outcome for individual i whose state of residence for high school was state s and belonged to graduation cohort c. D_{sc} is a binary variable equal to one if state s had a binding financial education mandate for cohort c as defined in Burke et al. (2024). For the main specification, we include controls, X_{isc}, for neighborhood income and credit risk score in the quarter they are first observed. These include dummy variables for bins of credit risk score and for quintiles of neighborhood incomes.[10] α_s and δ_c are state and cohort fixed effects, respectively, and ε is an idiosyncratic error term which we allow to be correlated with respondents from the same high school graduation state via clustering.

In order for us to interpret γ as the average causal effect of being bound by a financial education mandate, we require that those who were not bound by a financial education mandate, either because they graduate in a mandate state before the mandate was adopted or they graduated from a high school in a state who did not adopt a mandate, serve as a suitable counter-factual for those who were bound by a financial education mandate in the absence of treatment. Several previous papers present evidence that states that adopt mandates are similar in prevailing economic conditions to those that did not adopt mandates (Stoddard and Urban, 2020) and that cohorts prior to a binding mandate are similar to

[10]The bins for credit risk score are: no credit risk score, less than 620, 620-659, 660 to 719, 720 to 759, and 760 and above. Neighborhood income quintiles are computed using the American Community Survey's measure of median household income for ZIP codes. We create quintiles by sorting ZIP codes from lowest median household income to highest and using total population counts to split the population into 5 bins.

those in states without a mandate (Brown et al., 2016; Harvey, 2019; Mangrum, 2022; Burke et al., 2024). If, after controlling for state and cohort fixed effects, those bound by a mandate are otherwise similar to those not bound by a mandate except in their exposure to financial education during high school, then we interpret γ as the causal effect of exposure to this financial education coursework.

4 Results

4.1 Effect of Financial Education on Pandemic-Era Household Decisions

We begin by presenting the results of estimating Equation (1) on the full set of pandemic-era financial decisions listed in Section 3, beginning with credit card balance reductions after stimulus check payments were distributed in Table 1. The first three columns show the effect of being bound by a financial education mandate on the change in credit card balance in the months after the first stimulus payment. Overall, we do not see a significant difference between those bound and those not bound by mandates (first column), nor do we find a difference when we break the sample into those with a revolving credit card balance (second column) versus those who are not revolving balances (third column). However, when we look at the period after the second and third stimulus checks were dispersed, we see a statistically significant larger balance reduction among those who were bound by financial education mandates than those who were not. Overall, those bound by mandates reduced their credit card balances by roughly \$22 more than those not bound by mandates. When we split the sample between revolvers and non-revolvers, we find that the treatment effect is entirely driven by revolvers who were accumulating interest on purchases at the start of the look-back period. Those bound by mandates reduced outstanding card balances by \$42 more than those not bound by mandates, or 6% larger balance reductions.

Table 1: Effect of financial education on credit card balance reductions

	First Stimulus			Second and Third Stimulus		
	Overall	Revolvers	Transactors	Overall	Revolvers	Transactors
Treated	2.49	8.52	–2.38	–21.56**	–42.22***	–2.03
	(13.85)	(17.81)	(8.08)	(9.50)	(13.36)	(6.82)
Observations	2,249,556	1,006,360	2,266,835	2,281,444	958,942	2,298,936
Untreated Mean	–408.06	–694.02	–46.86	–252.82	–654.13	102.34

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 3) where the column header denotes the outcome of interest. The period of analysis is the change in total outstanding credit card balance between March and June 2020 for the first stimulus payment and between December 2020 and May 2021 for the second stimulus payment. Credit card borrowers are considered revolvers if they have at least one credit card account that carries over a balance from month to month; otherwise, they are transactors. Negative values denote a reduction in credit card balances during the time period. The first stimulus checks began distribution on April 10, 2020, the second on December 29, 2020, and the third on March 17, 2021. Standard errors are clustered at the state of first appearance level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes < 0.01.
Source: New York Fed Consumer Credit Panel/Equifax; Internal Revenue Service; authors' calculations.

In addition, we parse individuals into three bins of neighborhood income to separately test for treatment effects within these bins. Those in low and middle income neighborhoods were more likely to receive stimulus checks and those in high income neighborhoods were less likely. We find that the results for the overall sample are generally driven by those living in middle income areas. We again find no significant effect of credit card paydown after the first stimulus payment. Meanwhile, the treatment effect after the second and third stimulus payment is driven by revolvers in middle income areas, who reduced card balances by $64 more than their untreated peers. Lastly, in Section Online Appendix A we present Table A3 which includes an interaction between the treatment variable and a measure of the share of tax filers in the borrower's ZIP code who received a stimulus check. Although this is a loose proxy for stimulus receipt and the average value across all ZIP codes is above 50% we find some evidence that those that were mandated and had high levels of ZIP-level stimulus receipt paid down larger revolving balances.

These results are consistent with predictions discussed in Section 2.1 whereby the first stimulus check induced a higher MPC, perhaps due to the greater labor market shocks and supply chain uncertainty at the time, while more of the second and third stimulus checks went toward debt pay-down and savings. Since the survey evidence suggests the second and third stimulus payments allowed for more discretion in use than the first (Armantier et al., 2021), our results suggest that those bound by mandates used more of their stimulus checks toward paying down credit card balances during the last two rounds. Further, we find that the entire result is driven by borrowers who had at least one card with a revolving balance carrying interest charges.

Table 2: Credit card outcome heterogeneity, by pre-pandemic ZIP code median income categories

	Low	Middle	High
Credit card balance change after stimulus 1 ($)	–6.65	–2.92	36.75
	(16.36)	(9.61)	(26.11)
Revolvers' credit card balance change ($)	9.19	6.89	31.40
	(26.59)	(16.93)	(33.05)
Transactors' credit card balance change ($)	–5.81	–4.99	19.11
	(5.26)	(6.23)	(15.74)
Credit card balance change after stimuli 2 and 3 ($)	–10.49	–27.60**	–14.75
	(11.98)	(10.31)	(23.12)
Revolvers' credit card balance change ($)	–15.93	–63.79***	–14.59
	(19.67)	(19.67)	(37.67)
Transactors' credit card balance change ($)	–0.96	1.23	–12.76
	(6.63)	(6.80)	(18.35)
Maximum observations	1,061,988	1,182,044	1,194,359
Mean stimulus receipt	0.827	0.806	0.708

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 3) where the row denotes the outcome of interest. The low, middle, and high categories of ZIP income split the population of US households into three categories of equal size ordered by median household income using the 2015-2019 5-year American Community Survey. The bounds for the middle income category are $52.5k and $73.7k with the low income category below and the high income category above these bounds. The last row depicts the mean ZIP code-level stimulus payment receipt of individuals in each ZIP income group. More information on variable construction can be found in Section 2.2. Standard errors are clustered at the state of first appearance level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes < 0.01.
Source: New York Fed Consumer Credit Panel/Equifax; American Community Survey; Internal Revenue Service; authors' calculations.

Next, we discuss the effect of financial education on mortgage decisions. The first two columns of Table 3 show those bound by financial education mandates were more likely to take out new mortgages during a time of historically low interest rates. This was led mostly by people without previous mortgages—treatment led to a 0.7 percentage point increase in first-time mortgageship which translates to 10% higher uptake relative to the untreated average. The second set of columns shows that, among those who were already mortgagors, mandated borrowers saw a 0.8 percentage point increase in rate refinances compared to those who were not bound by mandates. Notably, these refinances were not driven by equity extraction, but only by the opportunities to reduce their interest rates at the prevailing lower rates. The increased probability of a rate refinance represents a 4% higher likelihood relative to those not bound by a financial education mandate. Scaling our observations up to the national level, our sample indicates that there were 4.5 million individuals who had taken mandatory financial literacy courses and owned a mortgage during this time period, of which 760,000 rate refinanced. Combining these totals with our point estimate suggests that mandatory financial literacy courses contributed to at least 36,000 of these refinances. Additionally, if we apply our estimated treatment effect of 0.008 to the total stock of mortgages held by borrowers not bound by mandates (around 12.5 million individuals), we would expect roughly 100,000 more refinances if borrowers not bound by mandates were instead mandated and saw the same treatment effect.

Table 3: Effect of financial education on new mortgages and refinance

	Purchase		Refinance		
	Any	First-time	Any	Cash-out	Rate
Treated	0.006**	0.007***	0.009*	0.002	0.008*
	(0.003)	(0.002)	(0.005)	(0.003)	(0.004)
Observations	3,434,796	2,669,112	849,757	849,757	849,757
Untreated Mean	0.091	0.073	0.295	0.089	0.218

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 3) where the column header denotes the outcome of interest. The period of analysis is the second quarter of 2020 through the fourth quarter of 2021. Each outcome is a binary indicator equal to one if the individual either took out a new purchase mortgage or a new refinanced mortgage. Standard errors are clustered at the state of first appearance level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes < 0.01.
Source: New York Fed Consumer Credit Panel/Equifax; American Community Survey; authors' calculations.

We further explore the effect on mortgageship in Table 4 by parsing our sample into three bins each by ZIP code median income (henceforth, neighborhood income) and by credit score bins prior to the pandemic, each of roughly equal size. The results from this exercise suggests that while the additional home purchases induced by financial education were roughly evenly distributed across neighborhood income, the effect on mortgage refinances were largely concentrated in higher income neighborhoods. Meanwhile, when we separate the sample by credit score bins, we find that the treatment effect for both purchases and refinances largely materializes for those with higher credit scores, with the exception of cash-out refinances. While we find no treatment effect of cash-out refinances in the general population, those who had subprime pre-pandemic credit scores and had more financial education were more likely to execute a cash-out refinance. This may have been driven by credit score inflation which could have allowed those with previously low credit scores to take advantage of improvements to their credit rating to extract equity (Mangrum et al., 2022; Sánchaz and Mori, 2023).

Table 4: Mortgage outcome heterogeneity

A. By pre-pandemic ZIP code median income	Low	Middle	High
Any purchase mortgage	0.005***	0.004	0.005*
	(0.001)	(0.003)	(0.003)
First-time purchase mortgage	0.005***	0.005**	0.006**
	(0.002)	(0.002)	(0.003)
Any mortgage refinance	–0.004	0.003	0.014*
	(0.004)	(0.005)	(0.008)
Cash-out refinance	–0.000	–0.000	0.003
	(0.002)	(0.003)	(0.004)
Rate refinance	–0.004	0.002	0.014**
	(0.004)	(0.004)	(0.007)
Maximum observations	1,060,670	1,180,921	1,193,248
B. By pre-pandemic credit risk score	**< 620**	**620–719**	**720+**
Any purchase mortgage	0.001*	0.005**	0.017**
	(0.001)	(0.002)	(0.006)
First-time purchase mortgage	0.001*	0.004	0.017***
	(0.001)	(0.002)	(0.004)
Any mortgage refinance	0.008*	–0.001	0.010**
	(0.004)	(0.005)	(0.005)
Cash-out refinance	0.007***	–0.002	0.002
	(0.002)	(0.003)	(0.003)
Rate refinance	0.000	0.000	0.009**
	(0.004)	(0.003)	(0.005)
Maximum observations	1,114,177	1,178,000	1,142,662

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 3) where the row denotes the outcome of interest. The low, middle, and high categories of ZIP income split the population of US households into three categories of equal size ordered by median household income using the 2015-2019 5-year American Community Survey. The bounds for the middle income category are $52.5k and $73.7k with the low income category below and the high income category above these bounds. Riskscores are Equifax Risk Score 3.0. More information on variable construction can be found in Section 2.2. Standard errors are clustered at the state of first appearance level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes < 0.01.
Source: New York Fed Consumer Credit Panel/Equifax; American Community Survey; authors' calculations.

Finally, Table 5 shows the effect of financial education mandates on student loan borrowers' decisions. First, we look at differences in the evolution of credit balances over the course of the forbearance on federal student loans (from the second quarter of 2020 to the second quarter quarter in 2023) between mandated and not mandated borrowers. We limit this analysis only to those borrowers who were in repayment on their federally-owned student loan with a non-zero payment due prior to the payment pause. First, we estimate a roughly $3,300 larger increase in total balances relative to untreated borrowers, whose balances rose on average $37,000. In the next four columns, we decompose this increase by credit product type and find that the overwhelming majority of this increase is driven by housing, suggesting that treated borrowers covered by the pandemic forbearance originated more or larger mortgages than borrowers not bound by state mandates. Additionally, we see moderately larger growth in auto balances and smaller growth in credit card balances relative to untreated peers, and we do not find a statistical difference in student loan debt that was not covered by the pandemic forbearance.

Table 5: Effect of financial education on student loan borrower decisions during student loan forbearance

	Balance Changes					Loan Originations	
	Total	Auto	Credit Card and Other	Housing	Non-Direct Student	Auto	Mortgage
Treated	3313.03*	179.97*	–140.38**	3251.63*	21.82	0.011**	0.011*
	(1722.21)	(89.86)	(61.24)	(1681.71)	(70.19)	(0.005)	(0.007)
Observations	553,157	553,157	553,157	553,157	553,157	553,124	553,124
Untreated Mean	37177.06	2016.34	2660.11	33504.92	–1004.30	0.472	0.206

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 3) where the column header denotes the outcome of interest. The student loan moratorium was in effect from the second quarter of 2020 through the third quarter of 2023 and applied only to federal student loans owned by the federal government (largely Direct federal loans). The balance reduction outcomes denote the change in balance from February 2020 to June 2023 for individuals in repayment for a federal loan in February 2020. Negative values denote a reduction in balances during the time period. More information on variable construction can be found in Section 2.2. Standard errors are clustered at the state of first appearance level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes < 0.01.
Source: New York Fed Consumer Credit Panel/Equifax; authors' calculations.

In the last two columns, we look at new auto loan and purchase mortgage originations during the student loan forbearance. We find a positive 1.1 percentage point treatment effect on both outcomes. Compared to student loan borrowers who did not undergo mandatory financial education, treated borrowers were 2% more likely to originate an auto loan and 5% more likely to originate a mortgage. In total, these results suggest that those bound by mandates used the pandemic forbearance on student loans to initiate more or larger mortgages and auto loans and grew their credit card balances by smaller amounts over the course of the pause on federal student loans. These findings are generally consistent with those in Dinerstein et al. (2024), who found that student loan forbearance lead to an increase in the demand for credit. However, we find that those with more financial education were more likely to open new auto loans and mortgages, but saw smaller growth in credit card balances.

4.2 Treatment effect decomposition

In this section, we aim to decompose the effect of mandatory financial education on pandemic financial decisions into two components: a) the increased probability of making an opportune financial decision conditional on pre-pandemic credit-worthiness and b) the increased ability to make an opportune financial decision due to pre-pandemic financial standing. To this aim, we present the results of the main specification for each financial decision presented above again in column (1) of Table 6. Next, in column (2), we add a set of credit variables observed at the end of 2019 for each individual to account for potential differences across mandated and not mandated individuals prior to the onset of the pandemic. This specification is a variant of Equation (1) that expands upon X_{isc}, to X'_{isc}, to control for pre-pandemic credit variables:

$$Y_{isc} = \gamma' D_{sc} + \psi X'_{isc} + \alpha'_s + \delta'_c + \epsilon_{isc} \tag{2}$$

These include fixed effects for quintiles of median neighborhood income, fixed effects for credit risk score buckets, a binary variable for whether the borrower had Direct federal student loans (and thus was covered by the forbearance), a set of binary variables denoting whether the borrower held each type of debt product, how much outstanding debt each borrower had for each loan type (except the outstanding debt balance for the product type being examined), and a set of binary variables denoting whether the borrower has an outstanding delinquent loans by loan type. These variables represent a comprehensive set of credit controls to categorize whether mandated and not mandated individuals might have differed in credit standing prior to the pandemic. Thus, the difference between these two columns helps us to gauge how much of the difference between mandated and not mandated individuals in their decision making came about due to differences in pre-pandemic financial status. Hence, if the results of column (1) and (2) are similar, then mandate individuals were more likely to make the opportune financial decisions despite being otherwise similar prior to the pandemic. On the other hand, if the inclusion of these pre-pandemic controls mitigates the differences between mandated and not mandated individuals, then it is likely the case that financial education caused mandated individuals to be in a better credit position prior to the pandemic. Lastly, in column (2), we omitted controls for the outstanding balance as of the end of 2019 for the credit product type associated with the decision at hands because the outstanding balance also measures how much someone might benefit from making that decision. For example, those with a larger credit card balance or a larger mortgage balance would benefit more from paying down their credit card balance or from refinancing to a lower interest rate. Thus controlling for these variables also controls for the potential gains of each decision. In column (3), we add this control to each specification to further disentangle differences between mandated and not mandated individuals.

The first set of results in Table 6 surrounding credit card balance changes shows that the inclusion of comprehensive controls prior to the pandemic does not qualitatively change the results from the baseline specification, suggesting that the effect of financial education operated through the proclivity to pay down debt and not due to differences between mandated and not-mandated borrowers prior to the pandemic. The same holds for home purchases, however the increased probability of mortgage refinance is of similar magnitude but is no longer statistically significant after including 2019 controls. So while mandated individuals were more likely to refinance their mortgages at a lower interest rate when mortgage rates were low, they did so largely because they had larger mortgages prior to the pandemic (consistent with owning more expensive homes), and thus had more to gain from refinancing.

Lastly, each of the student loan related outcomes remains qualitatively similar as a result of the inclusion of pre-pandemic controls, except for the larger growth in auto loan balances by those bound by financial education mandates. After including controls for the amount of auto loans outstanding in the fourth quarter of 2019, there is no longer a statistically significant difference between mandated and not-mandated individuals, but the estimated effect with the controls in column 3 is not statistically significantly different from that without the 2019 controls in column 1.

Table 6: Mediation analysis using pre-pandemic credit characteristics

	(1)	(2)	(3)	N
Credit card balance change after stimulus 1 ($)	2.49	4.03	14.94	2,249,556
	(13.85)	(13.65)	(12.54)	
Revolvers' credit card balance change ($)	8.52	11.75	26.38	1,006,360
	(17.81)	(18.61)	(19.73)	
Transactors' credit card balance change ($)	–2.38	0.70	4.14	2,266,835
	(8.08)	(7.53)	(6.42)	
Credit card balance change after stimuli 2 and 3 ($)	–21.56**	–29.31**	–22.38*	2,281,444
	(9.50)	(10.98)	(11.49)	
Revolvers' credit card balance change ($)	–42.22***	–51.36***	–44.26***	958,942
	(13.36)	(14.76)	(13.81)	
Transactors' credit card balance change ($)	–2.03	–4.33	–2.84	2,298,936
	(6.82)	(6.57)	(6.40)	
Any purchase mortgage	0.006**	0.007**	0.008***	3,434,796
	(0.003)	(0.003)	(0.003)	
First-time purchase mortgage	0.007***	0.006***	0.006***	2,669,112
	(0.002)	(0.002)	(0.002)	
Any mortgage refinance	0.009*	0.008	0.001	849,757
	(0.005)	(0.005)	(0.004)	
Cash-out refinance	0.002	0.001	–0.000	849,757
	(0.003)	(0.003)	(0.002)	
Rate refinance	0.008*	0.008*	0.001	849,757
	(0.004)	(0.005)	(0.004)	
Total balance change during forbearance ($)	3313.03*	3766.01**	3741.62**	553,157
	(1722.21)	(1578.97)	(1529.34)	
Auto balance change ($)	179.97*	199.15**	74.66	553,157
	(89.86)	(96.98)	(102.99)	
Credit card and other balance change ($)	–140.38**	–135.17**	–114.55*	553,157
	(61.24)	(57.93)	(60.23)	
Housing balance change ($)	3251.63*	3785.12**	3759.61**	553,157
	(1681.71)	(1547.72)	(1492.75)	
Non-Direct student loan balance change ($)	21.82	19.35	21.90	553,157
	(70.19)	(71.54)	(65.56)	
Originated auto loan during forbearance	0.011**	0.011**	0.011**	553,124
	(0.005)	(0.005)	(0.005)	
Originated purchase mortgage	0.011*	0.013**	0.013**	553,124
	(0.007)	(0.006)	(0.006)	
First Appearance ZIP Income Quintile FE	X	X	X	
First Appearance Riskscore Group FE	X	X	X	
2019 ZIP Income Quintile FE		X	X	
2019 Riskscore Group FE		X	X	
Student Loan Forbearance		X	X	
2019 Has Loan by Loan Type		X	X	
2019 Balance by Loan Type		X	X	
2019 Has Delinquency by Loan Type		X	X	
2019 Balance of Outcome Loan Type			X	

Notes: The table above reports the estimate for γ from Equation (1) in Column (1) and the estimate from two variants of Equation (2) (as described in Section 3) where the row denotes the outcome of interest. All results for student loan outcomes omit the forbearance control. The loan types used are auto loans, credit card, mortgage, HELOC, student, and other loans. For each outcome, the Has Loan variable for its corresponding type is omitted as this is already conditioned on for the construction of the outcome variable. 2019 Balance by Loan Type does not include the Balance variable for each outcome's corresponding loan type(s) in Column (2). More information on variable construction can be found in Section 2.2. Standard errors are clustered at the state of first appearance level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes < 0.01.
Source: New York Fed Consumer Credit Panel/Equifax; American Community Survey; authors' calculations.

To further explore pre-pandemic differences between mandated and not mandated individuals, we also estimate Equation (1) with some relevant pre-pandemic variables as outcomes to formally test for differences. We present these results in Table 7. First, we show that credit scores prior to the pandemic are not different between mandated and not mandated individuals. Second, we show that, among those with credit cards, credit card balance was not statistically different between mandated and not mandated individuals, although the point estimate suggests potentially larger balances for those who were mandated. Mandated borrowers were also not more likely to have revolving balances. On the other hand, these individuals have higher credit card limits and delinquencies (0.6 percentage points off a baseline of 14.5%) that are statistically significant. As implied by our mediation analysis in Table 6, mandated individuals had larger mortgage balances. However, they did not differ from not mandated individuals in their probability of having a mortgage or their mortgage delinquency. Treated individuals were statistically more likely to be delinquent on a student loan, but the difference is proportionally small. Finally, they were more likely to have an auto loan in 2019 but had a lower conditional balance on those loans.

Table 7: Effect of financial education on pre-pandemic credit characteristics

	(1)	Untreated Mean	N
Riskscore	–0.614	664.346	3,444,435
	(0.694)		
Credit Card Balance	78.65	4,041.370	2,479,934
	(51.04)		
Credit Card Limit	649.95**	13,900.450	2,479,934
	(274.98)		
Had a Delinquent Credit Card	0.006***	0.145	2,479,934
	(0.002)		
Had a Revolving Credit Card Balance	0.001	0.464	2,418,060
	(0.005)		
Had a Mortgage	–0.010	0.210	3,444,435
	(0.010)		
Mortgage Balance	4114.75***	149,760.830	619,802
	(1154.96)		
Had a Delinquent Mortgage	0.000	0.031	619,802
	(0.001)		
Had a Student Loan	–0.011	0.337	3,444,435
	(0.011)		
Student Loan Balance	–321.97	33,839.940	1,227,662
	(752.06)		
Had a Delinquent Student Loan	0.006*	0.162	1,227,662
	(0.003)		
Had an Auto Loan	0.011*	0.443	3,444,435
	(0.006)		
Auto Loan Balance	–355.08**	13,814.290	1,457,622
	(133.20)		
Had a Delinquent Auto Loan	0.002	0.140	1,457,622
	(0.002)		

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 3) where the row denotes the outcome of interest. Each balance, limit, and delinquency outcome is conditional on having a loan of that type. Standard errors are clustered at the state of first appearance level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes < 0.01.
Source: New York Fed Consumer Credit Panel/Equifax; American Community Survey; authors' calculations.

5 Robustness

In this section, we explore whether our main results are robust to different sample inclusion criteria, different assumptions regarding the state of high school graduation, or the choice of specification. We also consider whether treatment states were trending differently prior to the adoption of a mandate and whether the health and economic effects of the pandemic affected treated states differently. As mentioned above, incorrect assignment of individuals in the data to the appropriate mandate status will result in attenuation bias, causing our estimates to be biased toward zero. Hence, this mis-identification of treatment status will bias our estimates against finding treatment effects. As a result, the estimates we present above likely represent lower-bound estimates of the true treatment effects. Nonetheless, we explore several robustness checks against our baseline specification to test the extent to which our treatment identification may bias our estimates.

Table 8 reports the main results of our analysis first for our main specification in the first column, and again for many different assignments of borrowers to treatment status and for different sample windows. Across all of these various changes to our main specification, our main results remain similar. First, we limit the full sample to only those who are observed in the CCP by the time they are 21 years old. Since we assume the state of residence upon first appearance in the CCP is the state of high school graduation, those appearing for the first time in the CCP at an older age are somewhat more likely to live in a different state than the state from which they graduated high school. If this is true, limiting to a subsample that appear in the CCP at younger ages is likely to have less error in mandate status. Next, we explore a more comprehensive assignment of state of high school graduation by employing the household sample of the CCP. In addition to the primary sample, the CCP includes a larger sample of credit reports which also includes all individuals who reside in the same household as a primary member. As such, we can observe someone who, for example, entered the CCP for the first time at age 25, match them to a household member who is likely their guardian, and track that household member's state of residence in the year that the primary member was 18. This assignment algorithm is described in more detail in Section Online Appendix A. Next, we limit the CCP sample to only those individuals who have the same state of residence throughout their existence in the CCP sample. Although this sample is likely different on observable characteristics since people who are more mobile are different on important attributes than those who are less mobile, this serves as another check on the sensitivity of our assignment of high school graduation state.

Lastly, we conduct another robustness check in addition to the previous checks on treatment assignment. For this exercise, we limit the sample of individuals in states that adopt a mandate during our sample window to only include those individuals who graduated high school in the five years before and five years after mandate adoption. Since our baseline analysis essentially leverages variation in older and younger cohorts within each adopting state, we conduct this exercise to ensure that the treatment effects we detect are not driven by contemporaneous differences across cohorts. Limiting to this smaller sample reduces the cohort difference within the adopting states but still includes all observations in the never-adopting states and the states that adopted before our analysis window to better estimate the cohort fixed effects.

Table 8: Main results, by robustness sample

	Full	Under 21	Household Match	Never Moved	5-Year
Credit card balance change after stimulus 1 ($)	2.49	–4.60	3.85	–1.51	–3.07
	(13.85)	(18.99)	(16.47)	(20.78)	(11.68)
Revolvers' credit card balance change ($)	8.52	–2.68	8.92	10.40	–6.84
	(17.81)	(21.29)	(20.16)	(28.21)	(14.53)
Transactors' credit card balance change ($)	–2.38	–0.89	–0.20	–6.21	–1.95
	(8.08)	(10.97)	(8.99)	(9.61)	(8.31)
Credit card balance change after stimuli 2 and 3 ($)	–21.56**	–24.78***	–20.88**	–26.25**	–18.64**
	(9.50)	(9.13)	(9.60)	(12.75)	(7.49)
Revolvers' credit card balance change ($)	–42.22***	–36.67*	–36.76**	–48.81***	–33.26*
	(13.36)	(19.16)	(16.53)	(13.31)	(16.79)
Transactors' credit card balance change ($)	–2.03	–4.03	–3.73	–4.04	–2.60
	(6.82)	(7.97)	(7.38)	(7.42)	(6.32)
Any purchase mortgage	0.006**	0.006*	0.006**	0.007*	0.004**
	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)
First-time purchase mortgage	0.007***	0.007**	0.007***	0.007**	0.006***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)
Any mortgage refinance	0.009*	0.010*	0.010*	0.010	0.007*
	(0.005)	(0.005)	(0.005)	(0.007)	(0.003)
Cash-out refinance	0.002	0.002	0.002	0.000	–0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)
Rate refinance	0.008*	0.009*	0.009**	0.010*	0.007*
	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)
Total balance change during forbearance ($)	3313.03*	3821.81**	3788.97**	4405.08*	2522.95**
	(1722.21)	(1825.21)	(1794.34)	(2311.76)	(1220.27)
Auto balance change ($)	179.97*	210.81**	185.02**	192.06**	171.86*
	(89.86)	(90.26)	(89.44)	(92.54)	(95.12)
Credit card and other balance change ($)	–140.38**	–122.12*	–157.05**	–182.31**	–88.11*
	(61.24)	(66.45)	(60.67)	(74.09)	(51.10)
Housing balance change ($)	3251.63*	3742.31**	3750.20**	4377.92*	2463.50**
	(1681.71)	(1774.27)	(1749.71)	(2276.09)	(1185.87)
Non-Direct student loan balance change ($)	21.82	–9.20	10.80	17.41	–24.29
	(70.19)	(78.14)	(74.23)	(71.14)	(80.64)
Originated auto loan during forbearance	0.011**	0.014**	0.012**	0.009	0.011**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Originated purchase mortgage	0.011*	0.012*	0.012*	0.014	0.009*
	(0.007)	(0.007)	(0.007)	(0.008)	(0.005)
Maximum observations	3,444,435	2,477,275	2,957,357	2,467,641	2,053,066

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 3) where the row denotes the outcome of interest. Each column represents an adjustment to the sample as described in Section 5 with further details in Section Online Appendix A. More information on variable construction can be found in Section 2.2. Standard errors are clustered at the state of first appearance level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes < 0.01. Source: New York Fed Consumer Credit Panel/Equifax; American Community Survey; authors' calculations.

Additionally, we test whether states that adopted a mandate fared differently during the pandemic than those who had not adopted a mandate by the 2018 graduating class. We split adopting states into four bins by the first effective year of their mandate. In Figure A1, we show that those states that mandated states fared similarly in new COVID-19 cases per capita, new COVID-19 attributed deaths per capita, and the unemployment rate relative to those the never adopted a mandate. This suggests that health and economic differences across states by mandate status are unlikely to drive our main results.

We also test for pre-trends in state-level outcomes relative to the adoption of financial education mandates using outcomes from the credit bureau data. For this analysis, we use Callaway and Sant'Anna (2020)'s stacked event study framework to compare states that had not yet adopted and states who never adopted a mandate by the 2018 graduating cohort to those that adopt a mandate for each cohort relative to the first adopting cohort. We find that credit scores, total balances, delinquency, and the rate of new bankruptcy are all similar in adopted versus not-adopting states for the cohorts prior to the first treated cohort. Additionally, after the first mandated cohort, we find no statistically different outcomes in the years after the first treated cohort turns 18, suggesting that the inflow of mandated young people are not spilling over to state level credit trends.

Lastly, we apply the Callaway and Sant'Anna (2020) stacked event study framework to our main results of pandemic financial decisions. We do not adopt this framework for our main specification since this framework is typically used with either panel or repeated cross-sectional data, while we level cross sectional data at fixed points in calendar time. However, this framework allows us to estimate a separate treatment effect for each cohort relative to the first cohort that was mandated. Figures A3 to A5 show the point estimates and 95 percent confidence intervals for each of our main financial decision results using the Callaway and Sant'Anna (2020) framework. For each outcome, we do not see any consistent evidence of pre-trends in outcomes prior to the first mandated cohort. However, we begin to see differences between treated and untreated cohorts after the first few treated cohorts with many outcomes showing rising trends as more cohorts are treated. The largest (in absolute value) point estimates in the direction of improving outcomes come from the cohorts that were exposed to the most mature mandates. This is consistent with evidence in Mangrum (2022) showing that the effectiveness of mandated financial education increases over time, perhaps as educators become more skilled at teaching the materials or if adherence to the mandates improves over time.

6 Discussion

In this paper, we explore the impact of financial education on household debt decisions during the COVID-19 pandemic-era. During this time, fiscal and monetary policy decisions created the foundation for several advantageous financial decisions through economic stimulus payments, low interest rates, and broad forbearance provisions. However, not all households took advantage of these opportunities. We found that one driver of differences among households in their responses to these policies was exposure to financial education. We leverage variation in state-level mandates that require high school students be exposed to financial education across states and graduating cohorts. We find that those who were exposed to financial education during high school were more likely to pay down revolving credit card balances after stimulus checks were issued, were more likely to originate new mortgages while interest rates were low (driven by first-time home-buyers), and were more likely to refinance higher interest mortgages at lower rates. Altogether, financial literacy led to about 36,000 more rate refinances among borrowers nationwide. Additionally, student loan borrowers bound by mandates who benefited from the pause in federal student loan payments had smaller increases in credit card balances, accumulated larger mortgage balances, and originated more auto loans and mortgages over the course of the payment freeze.

Next, we disentangle whether the effect of financial education occurred primarily prior to the pandemic by increasing the credit worthiness of individuals to take advantage of these decisions. We find that, generally speaking, differences in the probability of making opportune financial decisions were largely due to differences in behavior during the pandemic rather than differences in credit standing leading up to the pandemic. The notable exception to this pattern is the effect of financial education on mortgage rate refinances - once we account for differences in outstanding mortgage balances, those who were required to be exposed to financial education were no more likely to refinance their mortgage at a lower rate. Their higher refinance rate is entirely attributable to the fact that mandated borrowers had larger mortgages prior to the pandemic and thus they had a larger benefit to refinancing to lower rates.

Our findings help us better understand the effectiveness of financial education. First, we contribute to the literature studying financial education requirements for high school students. We show that this coursework can be effective in helping households make advantageous financial decisions when opportunities come about. Additionally, our findings help reconcile seemingly contradictory results in the literature. Namely, some findings in the literature suggest that financial behaviors and financial well-being can be improved by requiring financial education in high school (Harvey, 2019; Mangrum, 2022; Burke et al., 2024; Stoddard and Urban, 2020; Kaiser et al., 2022) while having only small impacts on outcomes like credit scores measured in credit bureau data (Cole et al., 2016; Brown et al., 2016). We also found only very small differences in outcomes like credit scores and delinquencies as a result of required financial education. However, our results also show that individuals bound by these mandates may still have the knowledge and acumen to act on advantageous financial decisions even if they appear similar in financial health as those not bound by mandates. We would like to stress that even if one disagrees with our notion of measured decisions and outcomes being advantageous ones, our results show that those subject to financial education mandates make different choices, which could have aggregate economic impacts.

Perhaps our most important contribution to the literature is that financial education can help us better understand the transmission of fiscal and monetary policy and their impacts on inequality. We find that those bound by financial education mandates were more likely to use fiscal stimulus to pay down their credit card balances. While this likely improved their individual financial footing, a reduction in credit card balances does not contribute to consumption and thus this mechanism would weaken the short-run fiscal multiplier of the fiscal policy. However, these individuals who were exposed to more financial education were more likely to take advantage of low interest rates to initiate new mortgages and refinance existing mortgages. Thus, financial education helped to increase the proportion of borrowers who increased spending as a result of low interest rates, highlighting the importance of financial education for the transmission of both monetary and fiscal policy to household consumption and credit decisions. However, our heterogeneity analysis suggests that financial education did not increase the probability of these decisions uniformly across neighborhood income and credit scores. In particular, financial education induced rate refinancing of existing mortgages primarily in high income neighborhoods and increased new mortgages for those with prime credit scores. While strong underwriting is important to maintaining a healthy stock of mortgages and promoting stability in the housing market, this increased probability of advantageous mortgage decisions by those in higher income areas and by those with high credit scores has consequences for equitable recoveries from economic crises.

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Online Appendix A Appendix

Online Appendix A.1 Financial Decisions

Each financial decision's regression includes a different subsample of borrowers due to differing eligibility requirements.

Credit card paydown:

Credit card paydown after stimulus is the dollar difference between a person's credit card balance in a given period 2 and their balance in period 1. A negative value indicates their balance decreased. To be eligible, a borrower must have a credit card balance in period 1. For stimulus check 1, which was released on April 10, 2020, periods 1 and 2 are March 2020 and June 2020. Because checks 2 and 3 were released within a quarter of each other on December 29, 2020 and March 17, 2021, we group them into one financial decision. For this decision, periods 1 and 2 are November 2020 and May 2021.

Mortgage:

Mortgage originations in the CCP are not explicitly denoted as either a purchase or a refinance. We distinguish them using an algorithm that looks at the timing of the previous closed mortgage and the newly originated mortgage along with the change in the address. If there was a primary mortgage that was replaced by a new mortgage within a year and the address stays the same, we classify them as refinance originations, otherwise, purchase originations. For a detailed description of the algorithm and a code, see Gibbs et al. (2014).

Our four mortgage-related financial decisions focus on the period of historically low rates 2020Q2-2021Q4, inclusive. Any purchase mortgage indicates that a borrower had a purchase origination during 2020Q2-2021Q4. To be eligible, a borrower must have at least one observation in this period. First-time purchase mortgage also indicates that a borrower had a purchase origination during 2020Q2-2021Q4. To be eligible, a borrower must have at least one observation in the period and must not have a recorded mortgage at any point in 1999Q1-2020Q1.

Any mortgage refinance indicates that a borrower refinanced a mortgage during 2020Q2-2021Q4. This is broken out into cash-out and rate refinances. A refinance is an cash-out refinance if the mortgage balance for that account increased by at least 5% afterward. To be eligible, a borrower must have a mortgage at any point in 2020Q2-2021Q4. Rate refinance indicates that the mortgage account's balance decreased, remained the same, or increased by at most 5% afterwards. To account for smaller mortgages, we also classify a refinance as rate if the balance increased by at most $5,000.[11] To be eligible, a borrower must have a mortgage at any point in 2020Q2-2021Q4.

Student loan:

The federal student loan moratorium lasted from 2020Q1-2023Q3 and applied to Direct government loans but not FFEL or private loans. Student loans in the CCP are not explicitly denoted as Direct, FFEL, or private. We first use loan-level student loan data from the CCP to determine each student loan servicer's type. A servicer is considered private if at least 50% of its loans are jointly-held. For remaining servicers, we assign them as Direct if a) they have less than 1%

[11] Thus, a small percentage (1.2%) of refinances are classified as both rate and cash-out.

balance delinquent[12] in 2020, b) they have at least 99.9% balance delinquent in 2019 and at least 99% delinquent in 2020, c) they have at least 95% balance delinquent in 2019 and no loans in 2020, or d) they contain a code associated with direct servicers. Otherwise, they are FFEL. Afterward, we can find the balance and number of defaulted accounts by loan type for borrowers.

Balance change during moratorium indicates the change in balance of a given loan type from February 2020 to June 2023. To be eligible, a borrower must have a Direct loan with a nonzero payment in February 2020. "Total" refers to all debt besides Direct student loans, meaning auto, credit card, HELOC, mortgage, FFEL and private student, and other, which includes retail and consumer credit. "Housing" refers to HELOC and mortgage.

Originated loan during moratorium indicates that a borrower originated a loan of a given type (auto or purchase mortgage) in any quarter from 2020Q1 to 2023Q2, inclusive. It has the same eligibility requirements as the balance change outcomes.

Online Appendix A.2 Robustness Samples

Full indicates our main sample, which includes borrowers in the 5% non-household CCP who were born between 1982 and 2000, inclusive. We exclude borrowers whose first appearance is in a US territory or who lack a ZIP code in their first appearance. Furthermore, they must have an observation in 2019Q4 that includes a ZIP code and credit score. This sample thus contains 3.4 million observations. We construct several subsets of Full to test the validity of our results.

Identification of treatment status: Our first concern is that we assigned treatment incorrectly by using a borrower's first appearance in the CCP as their state of high school education, even if they were older than 18 in that observation. The first three robustness samples address this issue. We compare them to Full in Table A1.

Under 21 only includes borrowers whose first appearance in the CCP was at or below age 21. We use 21 as a cutoff because 93% of borrowers in the CCP who appear at ages 18 and 21 are in the same state in both. It contains 2.5 million observations.

Household Match first includes all members of Under 21. For those whose first appearance is at an older age, we use the household sample of the CCP to find a probable parent for them. For each borrower, we begin with the pool of people who have ever been recorded to share an address with them. We then restrict to cohabitants who are 15-50 years older. We assign a parental likelihood score to each potential parent-child pair via the following:

- 1 point if the parent is ≤ 40 years older
- 1 additional point if the parent is 18-30 years older
- 1 point if the pair's first quarter of cohabitation occurred when the borrower was ≤ 30 years old
- 1 additional point if the pair's first quarter of cohabitation occurred when the borrower was ≤ 23 years old

[12] "Delinquent" excludes defaulted loans.

- An additional min(floor(total quarters of cohabitation/4), 5) points to reward longer cohabitation time

We keep the likeliest parent for each child as long as they have fewer than 2 likelihood points. We then find a CCP observation for the parent when the borrower was 18, or barring that, the age closest to 18 in the range 13-21. The parent's state in that observation is then assumed as the borrower's high school state and treatment is reassigned. Moreover, the parent's ZIP code in that observation is then assumed as the borrower's high school ZIP code in the income control in Equation (1). This sample includes 3.0 million observations.

Finally, Never Moved only includes borrowers who remained in the same state for all their CCP observations from the first to 2023Q2, inclusive. It contains 2.5 million observations.

Table A1: Borrower characteristics, by treatment and robustness sample

	Untreated				Treated			
	Full	Under 21	HH Match	Never Moved	Full	Under 21	HH Match	Never Moved
Graduation Cohort	2007	2007	2007	2007	2011	2012	2011	2011
Age of First Loan	21.1	19.3	20.3	21.4	20.5	19.2	19.9	20.6
Student Loan Forbearance	31.5%	38.8%	35%	29.5%	37.9%	45.3%	41.6%	36.8%
2019Q4:								
Number Credit Cards	1.8	1.84	1.83	1.65	1.47	1.42	1.46	1.31
Number Mortgages	0.22	0.23	0.23	0.2	0.13	0.12	0.13	0.11
Riskscore	664	662	662	659	653	649	651	649
Had a Delinquent Account	22%	23.5%	23.1%	22.6%	20.6%	21.6%	21.4%	20.8%
Median ZIP Code Income	69,862	69,770	69,883	68,346	67,084	67,142	67,218	65,729

Notes: The table above reports borrower-level means by group. Riskscores are Equifax Risk Score 3.0.
Source: New York Fed Consumer Credit Panel/Equifax; American Community Survey; authors' calculations.

Lifecycle differences: Although our model includes age and state fixed effects separately, we have yet to account for the lifecycle differences between treated (usually younger) and untreated (usually older) borrowers in the same state. To make these groups more comparable, 5-Year only includes borrowers who turned 18 within five years of the first graduating class to face a financial literacy mandate; for example, in Virginia, a mandate first applied to the class of 2015, so this sample includes the 2010-2018 (inclusive) Virginia graduation cohorts. It also includes all borrowers in Full who graduated from states that either had or did not have a mandate during the entirety of 2000-2018. It contains 2.1 million observations. Table A2 shows that the untreated and treated groups in this sample are more similar in various pre-pandemic characteristics than they are in Full.

Table A2: Borrower characteristics, by treatment, 5-year sample vs full

	Full		5-Year	
	Untreated	Treated	Untreated	Treated
Graduation Cohort	2007	2011	2009	2010
Age of First Loan	21.1	20.5	20.8	20.7
Student Loan Forbearance	31.5%	37.9%	34.9%	37.4%
2019Q4:				
Number Credit Cards	1.8	1.47	1.62	1.6
Number Mortgages	0.22	0.13	0.19	0.15
Riskscore	664	653	660	657
Had a Delinquent Account	22%	20.6%	21.2%	21.1%
Median ZIP Code Income	69,862	67,084	70,124	68,160

Notes: The table above reports borrower-level means by group. Riskscores are Equifax Risk Score 3.0.
Source: New York Fed Consumer Credit Panel/Equifax; American Community Survey; authors' calculations.

Online Appendix A.3 Mandate Adoption and COVID Outcomes

We check if states in different waves of financial literacy mandate adoption were differentially affected by COVID by estimating each γ in

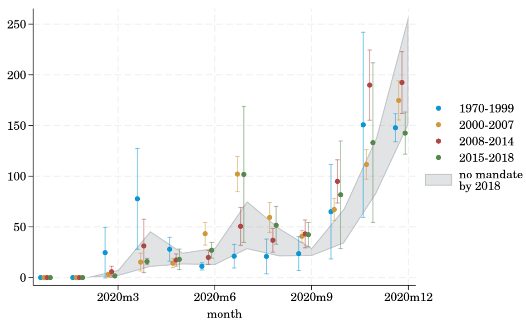
$$Y_{swm} = \sum_{w=1}^{5} \sum_{m=1}^{12} \gamma_{wm} D_{wm} + \varepsilon_{swm} \tag{A.1}$$

where Y_{swm} is a COVID-19 outcome for state s in mandate adoption wave w during month m in 2020 and D_{wm} is a binary variable equal to 1 if an observation pertains to wave w and month m.

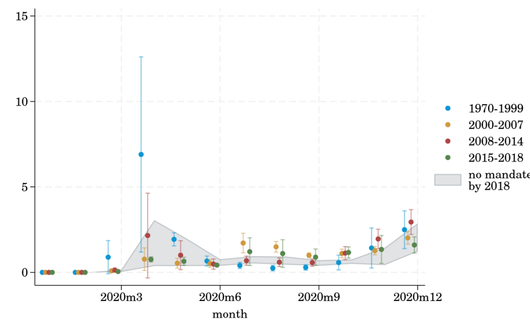
We find that states in different waves of mandate adoption experienced similar effects from the pandemic.

Figure A1: COVID-19 effects and financial literacy mandate adoption

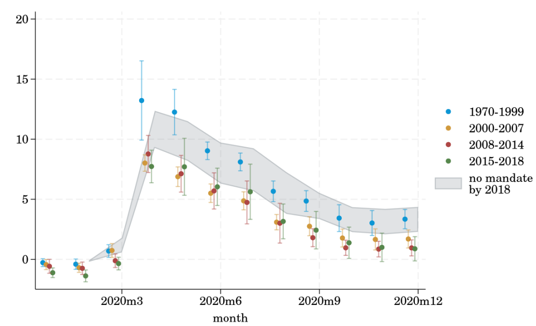
(a) New COVID-19 Cases per Capita



(b) New COVID-19 Deaths per Capita



(c) Unemployment Rate



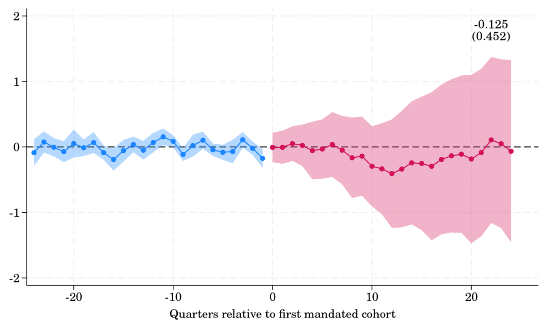
Notes: The figure above reports the estimates of γ_{wm} and their 95% confidence intervals from Equation (A.1) for the state-month outcomes of new COVID cases per capita, new COVID deaths per capita, and unemployment rate during 2020. The estimations are weighted by state population in 2019. Source: Centers for Disease Control and Prevention; Bureau of Labor Statistics; American Community Survey; author's calculations.

Online Appendix A.4 Mandate Adoption and Credit Trends

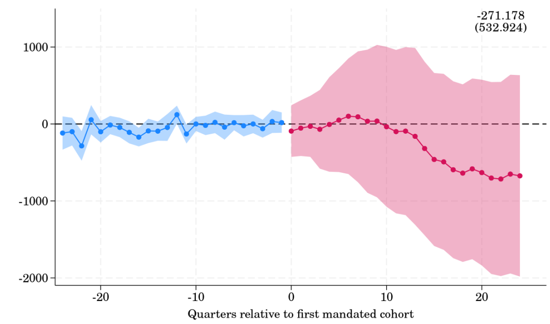
Next, we examine the possibility that the passage of a financial literacy mandate causes aggregate credit characteristics of a state's population to change. We use the Callaway and Sant'Anna (2020) difference-in-differences with multiple periods estimator on state-quarter panel data. We find no significant effects of mandatory financial education on state credit outcomes.

Figure A2: State credit outcomes and financial literacy mandate adoption

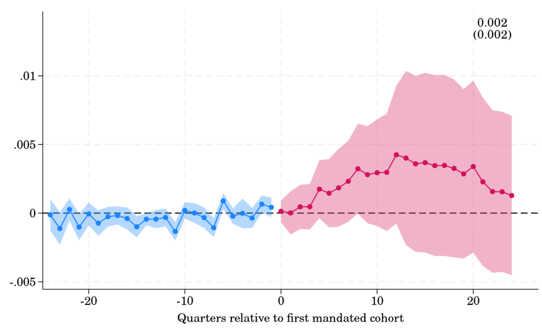
(a) Mean Riskscore



(b) Mean Balance



(c) Stock Delinquency



(d) New Bankruptcy Rate

Notes: The figures above depict Callaway and Sant'Anna (2020) event studies for state-quarter aggregate statistics calculated over the state's entire population from 2000Q1 to 2024Q4 using both never treated and not yet treated states as controls. To maintain a balanced sample, we remove states who passed a financial literacy mandate from 2019-2024. Results are censored from $t-24$ to $t+24$. The 95% confidence intervals are shaded. The upper right-hand corner of each figure includes the average treatment on the treated from period $t+0$ to $t+24$ on the first line and the standard error in parentheses on the second. * denotes $p < 0.1$. Source: New York Fed Consumer Credit Panel/Equifax.

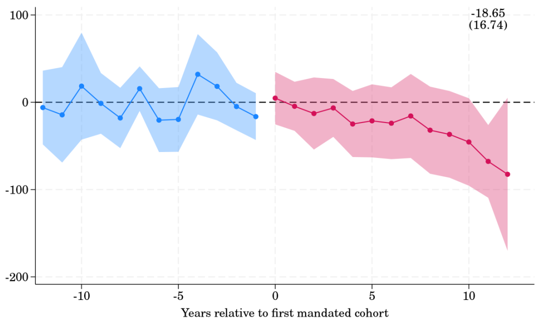
Online Appendix A.5 Pandemic-Era Household Decisions Event Study

We re-estimate our model in Equation (1) using the Callaway and Sant'Anna (2020) difference-in-differences with multiple periods estimator. Due to the model's assumptions regarding crosssection data, we omit first observation neighborhood income and credit risk score as covariates.

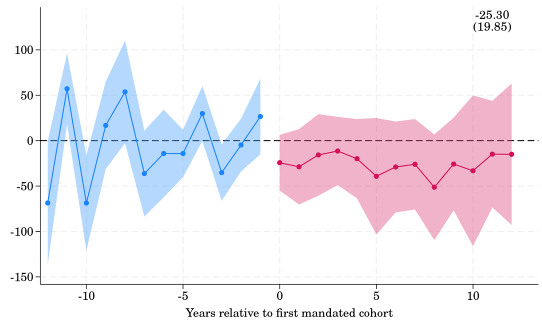
The point estimates on each event study are similar to those seen in our two-way fixed effect model. The results on mortgage purchases and total, auto, and housing balance increases during the student loan moratorium are statistically significant.

Figure A3: Effect of financial education on credit card balance reductions

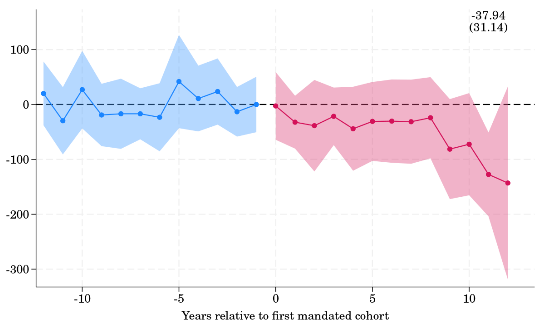
(a) Credit card balance change after stimulus 1:



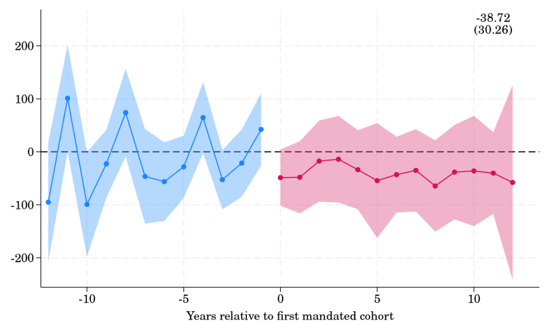
(b) Credit card balance change after stimuli 2 and 3:



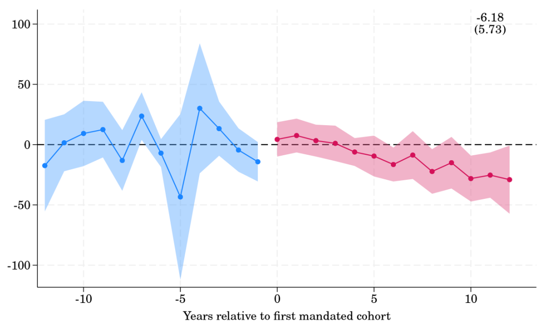
(c) Revolvers' credit card balance change



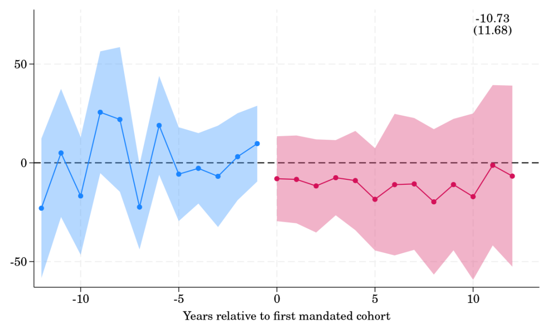
(d) Revolvers' credit card balance change



(e) Transactors' credit card balance change



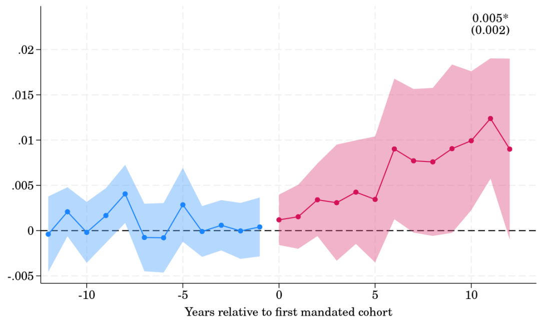
(f) Transactors' credit card balance change



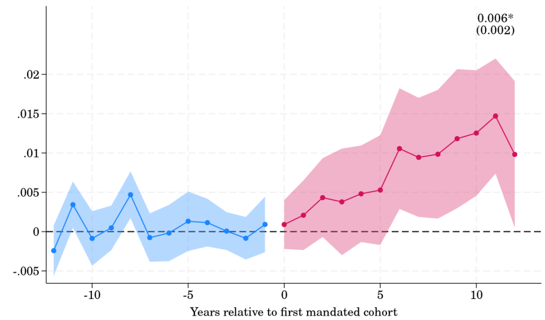
Notes: The figures above depict Callaway and Sant'Anna (2020) event studies using both never treated and not yet treated individuals as controls. "Years relative to first mandated cohort" refers to the number of years before or after a state financial literacy mandate's passage an individual graduated high school. The period of analysis is the change in total outstanding credit card balance between March and June 2020 for the first stimulus payment (column 1) and between December 2020 and May 2021 for the second and third stimulus payment (column 2). Negative values denote a reduction in credit card balances during the time period. The first stimulus checks began distribution on April 10, 2020, the second on December 29, 2020, and the third on March 17, 2021. Results are censored from $t-12$ to $t+12$. The 95% confidence intervals are shaded. The upper right-hand corner of each figure includes the average treatment on the treated from period $t+0$ to $t+12$ on the first line and the standard error in parentheses on the second. Standard errors are clustered at the state of first appearance level. * denotes $p < 0.1$.
Source: New York Fed Consumer Credit Panel/Equifax.

Figure A4: Effect of financial education on new mortgages and refinance

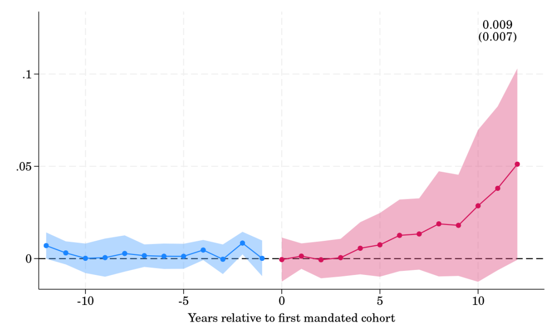
(a) Any purchase mortgage



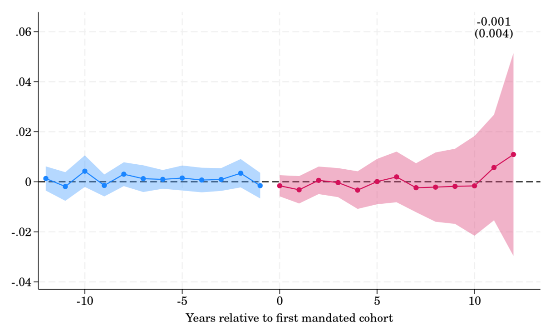
(b) First-time purchase mortgage



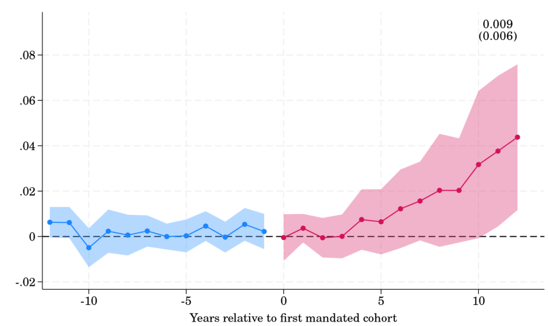
(c) Any mortgage refinance



(d) Cash-out refinance



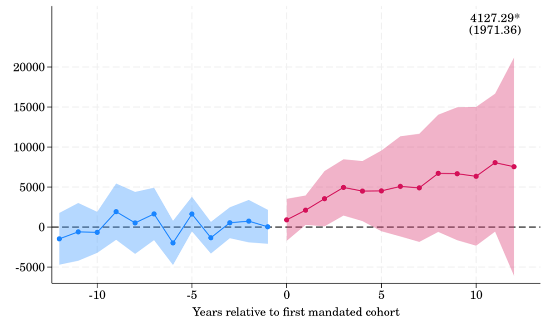
(e) Rate refinance



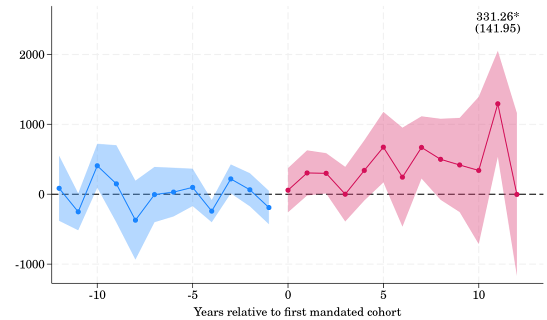
Notes: The figures above depict Callaway and Sant'Anna (2020) event studies where "Years relative to first mandated cohort" refers to the number of years before or after a state financial literacy mandate's passage an individual graduated high school. The period of analysis is the second quarter of 2020 through the fourth quarter of 2021. Each outcome is a binary indicator equal to one if the individual either took out a new purchase mortgage or a new refinanced mortgage. Results are censored from $t - 12$ to $t + 12$. The 95% confidence intervals are shaded. The upper right-hand corner of each figure includes the average treatment on the treated from period $t + 0$ to $t + 12$ on the first line and the standard error in parentheses on the second. Standard errors are clustered at the state of first appearance level. * denotes $p < 0.1$.
Source: New York Fed Consumer Credit Panel/Equifax.

Figure A5: Effect of financial education on student loan decisions

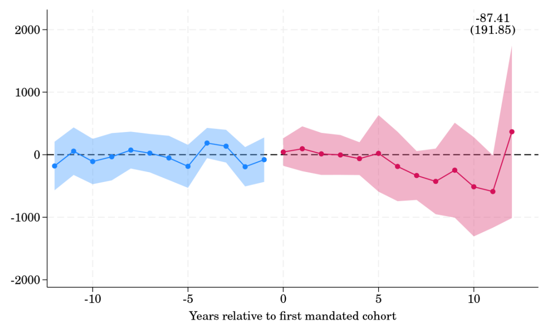
(a) Total balance change during moratorium



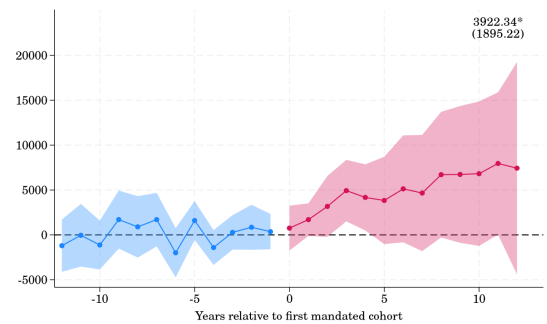
(b) Auto balance change



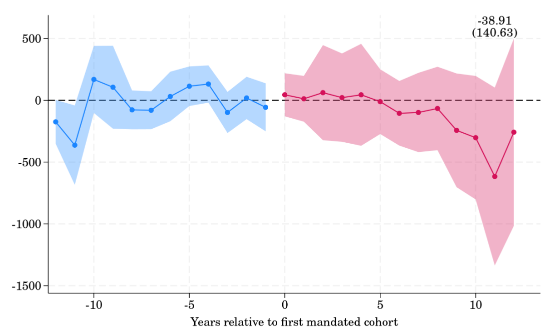
(c) Credit card and other balance change



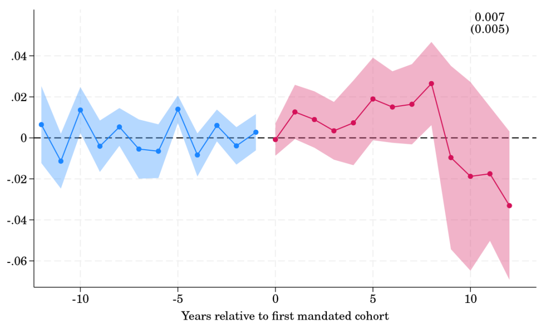
(d) Housing balance change



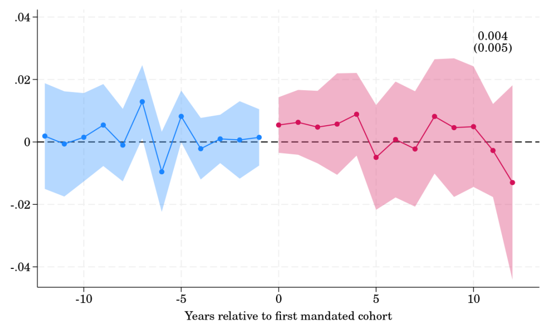
(e) Non-Direct student loan balance change



(f) Originated auto loan



(g) Originated mortgage purchase



Notes: The figures above depict Callaway and Sant'Anna (2020) event studies using both never treated and not yet treated individuals as controls. "Years relative to first mandated cohort" refers to the number of years before or after a state financial literacy mandate's passage an individual graduated high school. The student loan moratorium was in effect from the second quarter of 2020 through the third quarter of 2023 and applied only to federal student loans owned by the federal government (largely Direct federal loans). The Fresh Start program was announced in the second quarter of 2022 and implemented in the fourth quarter of 2022. The rehabilitation outcome is a binary indicator equal to one if the individual rehabilitated their defaulted federal loans before the Fresh Start announcement. The balance reduction outcomes denote the change in balance from February 2020 to June 2023 for individuals in repayment for a federal loan in February 2020. Negative values denote a reduction in balances during the time period. Results are censored from $t - 12$ to $t + 12$. The 95% confidence intervals are shaded. The upper right-hand corner of each figure includes the average treatment on the treated from period $t + 0$ to $t + 12$ on the first line and the standard error in parentheses on the second. Standard errors are clustered at the state of first appearance level. * denotes $p < 0.1$.
Source: New York Fed Consumer Credit Panel/Equifax.

Online Appendix A.6 Additional Tables and Figures

Table A3: Effect of financial education on credit card balance reductions, interacted with stimulus receipt

	First Stimulus			Second and Third Stimulus		
	Overall	Revolvers	Transactors	Overall	Revolvers	Transactors
Treated	63.04	154.28	97.23	–17.81	83.22	–44.24
	(167.89)	(212.75)	(97.67)	(62.27)	(147.68)	(41.11)
% Stimulus Receipt	1024.80***	1917.50***	601.61***	–477.76***	359.35**	–378.90***
	(142.60)	(174.75)	(62.67)	(65.30)	(153.32)	(35.20)
Treated × % Stimulus Receipt	–79.82	–189.07	–128.77	–4.80	–158.54	53.35
	(208.87)	(265.83)	(119.77)	(75.99)	(176.40)	(47.71)
Observations	2,240,746	1,002,248	2,256,751	2,262,982	951,186	2,275,702
Untreated Mean	–408.06	–694.02	–46.86	–252.82	–654.13	102.34

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 3) where the column header denotes the outcome of interest. The period of analysis is the change in total outstanding credit card balance between March and June 2020 for the first stimulus payment and between December 2020 and May 2021 for the second stimulus payment. Negative values denote a reduction in credit card balances during the time period. The first stimulus checks began distribution on April 10, 2020, the second on December 29, 2020, and the third on March 17, 2021. % stimulus receipt is a continuous value from 0 to 1. In columns 1-3, it is given by the percentage of households in the borrower's March 2020 ZIP code of residence that received the first stimulus check. In columns 4-6, it is given by the percentage of households in the borrower's March 2021 ZIP code of residence that received the third stimulus check. Standard errors are clustered at the state of first appearance level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes < 0.01.
Source: New York Fed Consumer Credit Panel/Equifax; Internal Revenue Service; authors' calculations.