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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 their credit card balances by larger amounts after stimulus checks were distributed and were more likely to buy homes and to refinance mortgages at low rates during the pandemic. The larger credit card balance reduction was driven by middle-income areas and subprime borrowers, while prime borrowers drove mortgage refinancing. 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

The onset of the COVID-19 pandemic brought about substantial economic and financial uncertainty for households across the globe. The sudden closure of businesses and schools in March 2020 necessitated a swift and decisive response from fiscal and monetary authorities to address the unprecedented economic challenges. Some of these responses included stimulus payments, enhanced unemployment distributions, sharp reductions in interest rate targets, and broadly targeted debt forbearance. While the most severe economic harms were relatively short-lived through the summer of 2020, the fiscal and monetary supports continued much longer. As a result, many U.S. households faced unique opportunities to better their financial situations via these economic supports, and a large share of households widely took advantage of such opportunities. Fiscal stimulus checks, combined with fewer opportunities for consumption, allowed households to decrease aggregate credit card balances by more than $300 billion from Q4 2019 to Q2 2021. 14 million homeowners refinanced mortgages at lower interest rates leading to an annual reduction of $30 billion in aggregate monthly mortgage payments for the foreseeable future. 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).

With these pandemic supports now in the rear-view, we focus attention on which households were better able to take advantage of financial opportunities during the pandemic era. More specifically, we investigate whether individuals with more financial education were better prepared to navigate the financial opportunities that were available to households. To this aim, we leverage school-cohort-level variation in exposure to state-level requirements that high school students must complete financial education in order to graduate. As of the 2018 high school graduating class, 31 states required high school students to complete coursework in personal finance (Burke et al., 2024). Several previous papers leverage similar variation in high school graduation requirements and find general improvements in financial outcomes (Urban et al., 2018; Stoddard and Urban, 2020; Brown et al., 2016; Harvey, 2019; Burke et al., 2024; Mangrum, 2022). In this paper, we apply a similar approach to the pandemic era and ask "were individuals who were required to take financial education during high school more likely to make opportune financial decisions during the pandemic era of 2020-2021?"

To answer this question, we use anonymized, administrative credit report data from the New York Fed Consumer Credit Panel (CCP), which contains a 5% representative sample of credit reports from a credit bureau, Equifax. This rich dataset allows us to track back the state of residence for an individual's high school attendance which we assign as the first appearance in credit bureau data. The CCP also contains rich details regarding balance evolutions and new originations by debt type at a quarterly frequency and contains a borrower's birth year and the address down to the Census Block.

Our analysis focuses on decisions surrounding the reduction of credit card balances, home purchases financed by new mortgages, mortgage refinances, and student loan repayment. We find that individuals who were required to complete financial education during high school reduced their credit card balances by larger amounts after the second and third stimulus payments which were distributed in December 2020 and March 2021, respectively. Those that were bound by mandates reduced their balances by 16% more than their not mandated peers. Additionally, individuals bound by a mandate were more likely to buy a new home taking advantage of historically low mortgage interest rates in the pandemic-era, and this was primarily driven by borrowers buying their first home who were 10% more likely to purchase their first house than those who were not required to take financial education. 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. Treated individuals were almost 4% more likely to execute a rate refinance than those who were from a state without a binding mandate at the time of their graduation. Lastly, we find no evidence that mandated borrowers who had defaulted on federal student loans prior to the pandemic were more likely to voluntarily rehabilitate those loans, nor do we find that borrowers who held student loans that were not covered by the federal student loan forbearance and interest waiver were more likely to consolidate or focus pay-down on their student loans that were still accruing interest.

Next, we disentangle underlying channels underpinning our findings by testing whether the increased probability of making these financial decisions was because the individuals bound by financial education were on better financial footing prior to the pandemic due to their financial education. To this aim, we control for a range of observable credit characteristics for each borrower just before the start of the pandemic. We find that the higher probability of making opportunistic financial decisions is unchanged for most of the outcomes, except for the higher probability of mortgage refinance. For mortgage refinance, we find that controlling for pre-pandemic characteristics, including the size of the outstanding mortgage, mitigates roughly three-quarters of the difference between mandated and not mandated individuals. We confirm that those bound by financial education mandates had larger mortgages prior to the pandemic, and thus they had more to benefit from refinancing.

We also explore whether the effect of mandatory financial education differed by area income and credit score. We assign each borrower into terciles of neighborhood income and into three groups of borrower credit score of approximately equal size as of the end of 2019. We find that credit card pay-down following the disbursements of stimulus checks was largely driven by those in middle income neighborhoods and those with subprime credit scores. While the effect of financial education on mortgage originations was similar across neighborhood income, we find that the effect is largely driven by those with prime credit scores. On the other hand, treated individuals with low credit scores pre-COVID were more likely to cash-out refinance than those who were not required to take financial education coursework, while financial education increased the probability of rate refinancing in higher income areas and among those with higher credit scores. We note that the increased likelihood of cash-out refinances among those with low credit score was likely driven by large improvements in credit scores among low score borrowers after the onset of the pandemic.

Taken together, these results suggest that individuals who were required to complete financial education during high school were more likely to take advantage of several of the opportune financial decisions that were available to households during the pandemic, and part of these results is due to those with financial education having attained a credit standing prior to the pandemic that better enabled them to take advantage of these opportunities. For instance, mandated borrowers had larger mortgage balances prior to the pandemic which might explain why they were more likely to refinance those larger balances. We take these findings as evidence that knowledge learned through financial education may be especially effective when opportunities arise to act on this knowledge.

This paper contributes to three broad strands of the literature. First, 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 improve student loan repayment (Mangrum, 2022). This paper provides further evidence that financial education coursework can improve outcomes for those who were bound by state mandates.

Next, we contribute to a broad literature that explores the drivers of heterogeneous household financial decisions during the COVID-19 pandemic and other 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.

Lastly, 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). 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.

These unanswered questions have consequences for whether policy responses to economic disruptions worsen economic inequality. If wealthier and more affluent households are more likely to take advantage of the opportunities afforded by fiscal and monetary policy, policy-assisted recoveries from economic disruptions may disproportionately benefit those of higher affluence and exacerbate inequality. In our setting, we find that those with more financial savvy via financial education were more likely to take advantage of opportunities that were enabled due to fiscal and monetary policy in the aftermath of the COVID-19 pandemic. Thus, it is possible that improved financial savvy from required financial education during high school can help increase the uptake of opportune financial decisions in the wake of economic disruptions. However, differences in *who* among the more financially savvy are able to take advantage of these opportunities has consequences for the distribution of who benefits most from policy decisions during economic crises.

2 Background

This paper primarily builds upon a string of works studying the effect of required financial education for high school graduation on financial outcomes. An earlier string of literature found that high school requirements for "consumer education" that were binding for high school graduates between 1957 and 1982 were largely ineffective at improving financial outcomes (Bernheim et al., 2001; Cole et al., 2016). However, a more recent series of papers studying a new generation of financial education coursework for high school graduating classes after 1990 show more promising effects of these courses. We study the more recent wave, specifically those who graduated high school in 2000-2018.

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 dataset has several advantages over definitions used in some previous papers. First, the data assign mandate status according to the first graduation cohort that was bound by a mandate rather than the legislative year that the policy was adopted. Since many changes to course standards take several years to be binding for high school students, this definition improves the accuracy of the assignment of mandate status. This classification of course mandates has been used in several recent studies that find positive impacts of adoption (Urban et al., 2018; Harvey, 2019; Mangrum, 2022; Burke et al., 2024).

Figure 1 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 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.[1]

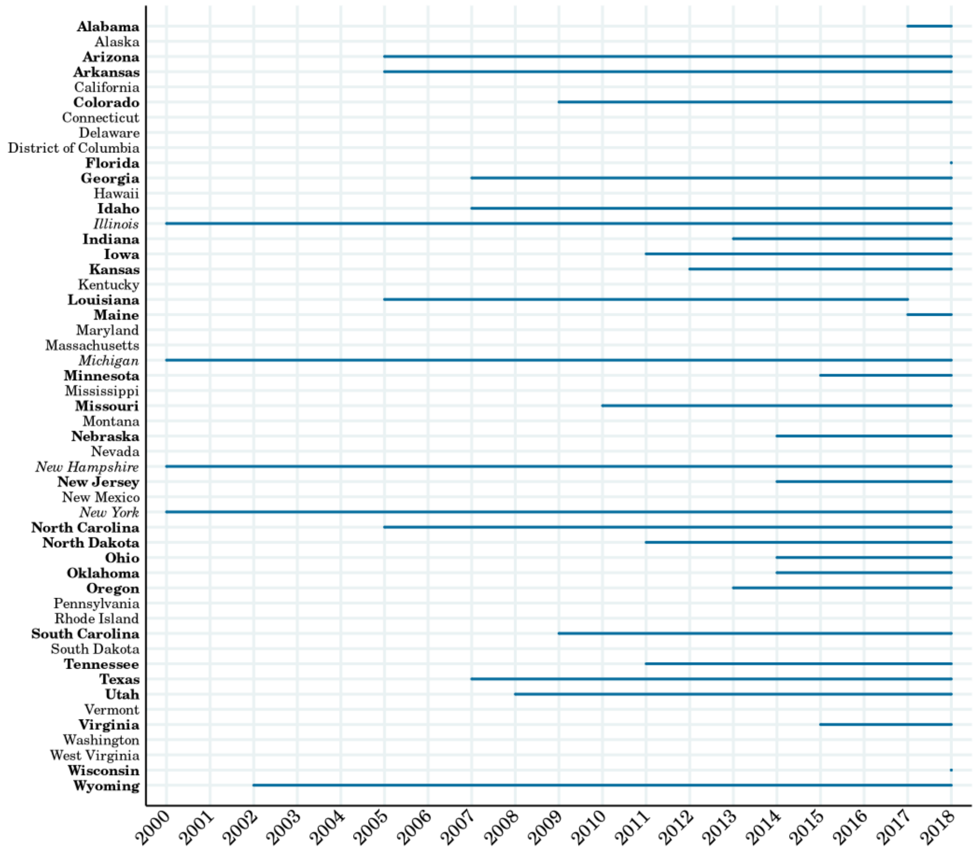
Several papers use similar variation in exposure to financial education mandates to explore the effect of financial education on financial outcomes. Bernheim et al. (2001) was the first such study that leveraged variation in course standard adoption from the late 1950s to the early 1980s, finding that exposed students had higher savings in adulthood. However, Cole et al. (2016) attempted to replicate the finding and concluded that the results were not robust to the inclusion of state fixed effects.

On the other hand, Brown et al. (2016) study a more recent wave of financial education requirements for high school students that were rolled out between 1998 and 2012, subsequently focusing on financial and debt outcomes for those under 30. In addition, they also study requirements for economics and mathematics course work. For financial education, they find that those bound by the reforms were more likely to have a credit report, and conditional on having a report, were less likely to have any outstanding debts and a moderately lower share of debt in delinquency. Their results suggest higher creditworthiness of mandated students in their younger life.

Urban and Schmeiser (2015) introduce a newer and improved classification of financial education mandates. The improvements stem from a better categorization of financial education and a better identification of mandated students. Brown et al. (2016) use the categorization from the Council of Economic Education which uses the legislative year the

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

FIGURE 1: State financial education mandates, 2000-2018 high school graduating cohorts



Source: Burke et al. (2024).

reform was passed, which is often several years earlier than the first graduating cohort that was bound by the new requirements. Instead, Urban and Schmeiser (2015) identifies the first high school graduating cohort that was bound by the mandate. Although most of the states defined by Brown et al. (2016) are the same as those in Urban and Schmeiser (2015), there are several that differ, and the effective years differ for all. Urban et al. (2018) find reductions in delinquencies when they assign treatment using the first graduating class bound by the mandate, but find null results if they instead use the legislative year. For these reasons, we use the identification of effective state and year of mandated personal finance requirements from Urban and Schmeiser (2015) that were subsequently updated in Burke et al. (2024).

Urban et al. (2018) also uses credit bureau data to study the effects of financial education requirements, but they limit their analysis to two states with more rigorous financial education mandates, Texas and Georgia. They choose these two states because both introduced their mandates after 2000, better fitting the sample window of their outcome data, and because these mandates were not introduced alongside other reforms in economics or math course requirements. They also focus on outcomes for a younger set of cohorts aging between 18 and 21. They find improved credit scores (in line with Brown et al. (2016)) and a lower delinquency rate for those bound by mandates in Texas and Georgia compared to before the mandates and to those in a select pool of comparison states. Interestingly, they also find heterogeneous treatment effects across the two states despite very similar policies.

An additional set of papers use the full identifying variation of financial education mandate from either Urban and Schmeiser (2015) or Burke et al. (2024). Stoddard and Urban (2020) studies how these mandates affect the financing decisions of new college students. They find that mandated students 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. 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).

While Stoddard and Urban (2020) studies the initial interaction of college students with post-secondary finance decisions, Mangrum (2022) explores whether these result in better student loan repayment outcomes after college. He finds improvements in student loan repayment rates, particularly for borrowers from low income families and first generation college students. Although not causal, the paper also finds heterogeneous effects of financial education depending on the specific course content. More specifically, the improvement on student loan repayment is larger for states whose financial education course material also focuses on career education.

Aside from post-secondary financing, Harvey (2019) studies whether those bound by financial education mandates are less likely to use alternative financial services (AFS) like pay-day lending. They find indeed that financial education mandates reduce borrower reliance on AFS. 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. They posit that financial education policies may be more effective when covering short- and medium-term financial decisions rather than long-term decisions.

Taken collectively, the evidence suggests that required financial education during high school can lead to improvements in financial decisions and health. If this is the case, borrowers making opportune financial decisions during the pandemic could stem from at least two channels. First, it is possible that people who were exposed to financial education during high school were on better financial footing prior to the pandemic and this enabled them to take advantage of some financial opportunities that those unexposed were not able to. However, it is also possible that those exposed to financial education had the required knowledge to take advantage of unique financial opportunities during the pandemic and those unbound were less knowledgeable and thus less likely to take up these decisions. Of course, some combination of these two forces is also possible. In Section 5.1, we present the estimated combined effect of being bound by financial education on our collection of financial decisions during the pandemic and in Section 5.2 we disentangle these two channels.

3 Data and Variable Construction

3.1 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 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. 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 6, 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. We then categorize observations into neighborhood income quintiles and terciles.

[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.

3.2 Variable Construction

3.2.1 Credit card outcomes

Using the CCP, we constructed five 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. The first set of outcomes measures whether a borrower **paid down existing credit card debt** 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 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

[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.

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 November 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 that fact that reported outstanding credit balances include both new purchases and revolving debt, and the CCP data cannot disentangle these components (nor do we observe who received payments). Hence, a reduction in outstanding balances could be driven by a reduction in revolving balances (which incur interest charges), a reduction in new purchases, or a combination of both.

3.2.2 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. 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. We then 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.[5]

We take the stance that each of these decisions were potentially advantageous to borrowers, but each decision benefited 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 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 mortgagers who refinanced during this period, monthly mortgage payment reductions averaged $178 per month (Haughwout et al., 2023).

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

3.2.3 Student loan outcomes

The last set of outcomes focuses on student loan debt. 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. In addition to the forbearance and interest waiver, borrowers with defaulted federal student loans had an opportunity to rehabilitate their loans by submitting paperwork and using accumulated months of forbearance as "quality payments" towards rehabilitation. We create two sets of opportunistic student loan decisions. First, we measure whether defaulted borrowers rehabilitated their loans to current status during the period of administrative forbearance. Rehabilitation of student loans benefits borrowers by removing the defaulted loan flag from their credit report thus improving their credit standing. Also, rehabilitation removes several penalties of student loan default including wage and tax return garnishment and it grants borrowers access to income-driven repayment (IDR) plans and access to take out new federal student loans. However, in April 2022, the Biden Administration announced that all existing defaulted borrowers would have their loans brought to current status without needing to complete the formal rehabilitation process as part of the "Fresh Start" program. As such, we measure whether a defaulted borrower rehabilitated their loan either before the Fresh Start announcement or before the Fresh Start implementation.

We create a second set of student loan outcomes by exploring the actions of borrowers who held student loans that were not covered by the federal student loan forbearance and interest waiver during the pandemic. These loans include either purely private student loans or federal student loans from the legacy Federal Family Education Loans (FFEL) which are federal loans owned by commercial banks but guaranteed by the federal government. Due to the favorable treatment of federal loans owned by the federal government during this period, borrowers with both types of loans were best served by focusing repayment on loans that were accruing interest and for which payments were due. Further, the Biden Administration announced two new waiver programs that extended loan forgiveness benefits to borrowers with federal student loans owned by commercial banks. As part of the waiver, borrowers could consolidate their ineligible loans into federal loans in order to qualify for federal student loan forgiveness under the Public Service Loan Forgiveness (PSLF) program or through enrollment in an IDR plan.

We construct two measures to capture these two decisions. First, we test whether borrowers with outstanding FFEL loans consolidated those loans into the federal Direct program to take advantage of opportunities such as the payment pause, interest waiver, or the IDR and/or PSLF waivers. We do this by categorizing loan debt by type according to lender portfolio attributes and tracking whether balances move from one portfolio during another during the payment pause. If so, we denote these as borrowers who consolidated FFEL into Direct loans. Next, we explore whether borrowers who had multiple types of loans focused their balance reductions on loans that were accruing interest. We do this by exploring the balance reduction by portfolio type for each borrower. We mark a borrower as prioritizing the reduction of private loans as one whose aggregate balance reduction for private and FFEL made up more than 80% of their total aggregate student debt balance reduction during the pandemic forbearance period.

4 Empirical Strategy

4.1 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 to estimate a difference-in-differences model, 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 Urban et al. (2018). 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.[6] 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 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.

In addition to Equation (1), we also estimate a variant that expands upon X_{isc}, to X'_{isc},

[6] 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.

to control for pre-pandemic credit variables:

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

We estimate this specification to test whether the impact of financial education mandates operates primarily through mandated individuals having higher credit-worthiness prior to the pandemic or through an increased probability of making these opportune decisions independent of pre-pandemic credit standing. For this specification, we expand the vector of controls, to X'_{isc}, to include 2019 levels of several credit variables which include: a) fixed effects for quintiles of median neighborhood income, b) fixed effects for credit risk score buckets, c) a binary variable for Direct federal student loans, d) a set of binary variables denoting whether the borrower held each type of debt product, e) how much outstanding debt each borrower had for each loan type, f) and a set of binary variables denoting whether the borrower has an outstanding delinquent loans by loan type. If, after controlling for 2019 measures of credit-worthiness, γ' still represents a higher probability of taking up the opportune financial decision, we can conclude that these decisions were not the result of improvements in credit-worthiness between high school graduation and the onset of the pandemic but instead due to a higher probability of making the opportune decision independent of credit standing relative to the individuals who were not mandated. We interpret the difference from γ to γ' between specifications (1) and (2) to be the effect of financial education attributable to pre-pandemic financial standing.

5 Results

5.1 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 4, beginning with credit card balance reductions after stimulus check payments were distributed. Table 1 shows that those bound by mandatory financial education decreased their credit card balances by an additional $21, about 16% more than untreated borrowers, after the second and third pandemic stimulus checks. These results are consistent with predictions discussed in Section 3.1 whereby the first stimulus check induced a higher MPC, perhaps due to the greater labor market shocks and general economic 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.

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

	First Stimulus	Second and Third Stimulus
Treated	2.485	–20.501*
	(13.844)	(11.311)
Observations	2,249,552	2,251,631
Untreated Mean	–408.071	–131.912

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 4) 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 November 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. 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.

The first two columns of Table 2 shows 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.

TABLE 2: 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	3,434,790	2,669,108	849,755	849,755	849,755
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 4) 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.

Finally, Table 3 shows the effect of financial education mandates on student loan decisions. First, we see no impact of treatment on the likelihood of proactively rehabilitating defaulted direct loans. Although the point estimate is positive, neither rehabilitating before the Fresh Start announcement nor the implementation are statistically different from zero. This may be due to the relatively small sample size of defaulted borrowers (roughly 117,000 in our sample).

Additionally, we find that mandated borrowers were not more likely to consolidate FFEL into Direct loans nor were they more likely to prioritize decreasing interest-accruing balances, that is, balances on accounts that were not covered by the federal interest waive or the administrative forbearance, relative to Direct loans, for which payments were not required and interest was not accumulating. Again, the point estimates are positive but not statistically significant for this outcome.

TABLE 3: Effect of financial education on student loan decisions

	Rehabilitated Before Fresh Start...			
	Announcement	Implementation	Consolidated FFEL	Prioritized Private
Treated	0.008	0.002	0.003	0.011
	(0.008)	(0.008)	(0.008)	(0.009)
Observations	117,357	117,357	117,346	176,142
Untreated Mean	0.486	0.669	0.106	0.407

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 4) 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 Fresh Start program was announced in the second quarter of 2022 and implemented in the fourth quarter of 2022. Private loans include both private loans and FFEL loans owned by commercial banks but guaranteed by the federal government (but not federally owned). Each outcome is a binary indicator equal to one if the individual rehabilitated their defaulted federal loans, consolidated their FFEL loans to federal loans, or reduced their private loan balances by at least 80% of their total loan reductions. More information on variable construction can be found in Section 3.2. 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.

5.2 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 4. 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. 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[7], 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 row of Table 4 shows that despite controls, we still have no statistically significant difference between mandated and not mandated individuals in the amount of credit card paydown after the first stimulus. The second row shows that controlling for 2019 baseline credit characteristics increases the precision and point estimate for the credit card paydown after the second and third stimulus payment, but including a control for pre-pandemic credit card balance reduces the point estimate back toward the baseline estimate

[7]We exclude the outstanding debt balance for the product type being examined for column (2). We add this variable for column (3).

and the estimate is no longer statistically significant (and also not significantly different from the estimate in columns 1). We take this as evidence that borrowers who were subject to financial education mandates in high school paid down more of their credit card balance after the second and third stimulus, but some of that decision may have been due to some pre-pandemic differences in credit card borrowing between those who were mandated and those who were not.

Next, we explore our mortgage decisions. First, we see that the effect of financial education on new purchase mortgages and new purchases for those who did not previously hold a mortgage both survive the inclusion of pre-pandemic controls, suggesting that the increased probability of taking on a new mortgage during the period of historically low mortgage rates during the pandemic-era was not driven by pre-pandemic credit differences. On the other hand, the findings for mortgage refinancing may have been driven by credit differences prior to the pandemic. The estimate for the impact on rate refinancing is unchanged when we add the large set of pre-pandemic controls from column (1) to column (2), however when we control for outstanding mortgage balance prior to the pandemic, we find that there is no remaining difference between mandated and not mandated individuals. 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, and thus had more to gain from refinancing.

Lastly, we do not see any difference in point estimates for the student loan outcomes after controlling for the pre-pandemic credit outcomes in columns (2) or (3).

Table 4: Mediation analysis using pre-pandemic credit characteristics

	(1)	(2)	(3)	N
Credit card paydown after stimulus 1 ($)	2.485	4.020	14.921	2,249,552
	(13.844)	(13.641)	(12.526)	
Credit card paydown after stimuli 2 and 3 ($)	–20.501*	–30.750**	–22.444	2,251,631
	(11.311)	(13.770)	(14.036)	
Any purchase mortgage	0.006**	0.007**	0.008***	3,434,790
	(0.003)	(0.003)	(0.003)	
First-time purchase mortgage	0.007***	0.006***	0.006***	2,669,108
	(0.002)	(0.002)	(0.002)	
Any mortgage refinance	0.009*	0.008	0.001	849,755
	(0.005)	(0.005)	(0.004)	
Cash-out refinance	0.002	0.001	–0.000	849,755
	(0.003)	(0.003)	(0.002)	
Rate refinance	0.008*	0.008*	0.001	849,755
	(0.004)	(0.005)	(0.004)	
Rehabilitated student loan by 2022Q1	0.008	0.008	0.008	117,357
	(0.008)	(0.008)	(0.008)	
Rehabilitated student loan by 2022Q3	0.002	0.002	0.003	117,357
	(0.008)	(0.008)	(0.008)	
Consolidated FFEL to Direct	0.003	–0.001	0.001	117,346
	(0.008)	(0.008)	(0.008)	
Prioritized reducing private student loan balance	0.011	0.010	0.010	176,142
	(0.009)	(0.009)	(0.009)	
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 4) 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 in Column (2). More information on variable construction can be found in Section 3.2. 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 5. 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. 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 4, 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. Finally, treated individuals were statistically more likely to be delinquent on a student loan, but the difference is proportionally small.

TABLE 5: Effect of financial education on pre-pandemic credit characteristics

	(1)	Untreated Mean	N
Riskscore	–0.615	664.346	3,444,429
	(0.694)		
Credit Card Balance	78.559	4,041.380	2,479,929
	(51.086)		
Credit Card Limit	649.927**	13,900.487	2,479,929
	(274.940)		
Had a Delinquent Credit Card	0.006***	0.145	2,479,929
	(0.002)		
Had a Mortgage	–0.010	0.210	3,444,429
	(0.010)		
Mortgage Balance	4112.336***	149,761.438	619,803
	(1153.818)		
Had a Delinquent Mortgage	0.000	0.031	619,803
	(0.001)		
Had a Delinquent Student Loan	0.006*	0.162	1,227,653
	(0.003)		

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 4) 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.

5.3 Heterogeneous effects by neighborhood income and borrower credit score

In this last section of results, we explore whether there were heterogeneous impacts of required financial education on pandemic-era financial decisions by neighborhood income and by credit score. Since we do not directly observe individual income in the CCP data, we use ZIP code median household income from the American Community Survey as a proxy for individual income similar to Chetty et al. (2024). We categorize each borrower into terciles (low, middle, high) of median neighborhood income and separately estimate Equation (1) for each tercile for each of our pandemic financial decisions discussed in the last section.

Table 6 reports the heterogeneous results by neighborhood income as measured in the fourth quarter of 2019 for each of our pandemic decisions. First, we find that treated individuals in the lower and middle income neighborhoods had small reductions in credit card balances relative to the untreated while those in higher income neighborhoods saw increases in credit card balances relative to those not bound by mandates after the first stimulus payment. Although these estimates are not statistically different from zero, this pattern is consistent with the means testing of stimulus payments - those in middle and lower income areas were more likely to receive payments and either lowered or did not increase their credit card balances while those in higher income areas were less likely to receive payments and increased their balances. We see a similar pattern for the evolution of credit card balances after the second and third economic impact payments. Although there was no statistically significant decline for the mandated individuals in lowest income areas compared to not mandated individuals, those in middle income areas who were bound by mandates reduced their credit card balances after the second and third payments by more than not mandated individuals living in middle income areas. And again, we see no impact on the highest income areas which were less likely to qualify for stimulus payments. The further reduction in credit card balances is roughly twice the size for middle income areas than for the overall estimate we reported in Table 1. In total, these results generally suggest that credit card pay-down after the economic stimulus payments was larger for those who were bound by financial education mandates and the results were driven by those living in middle income areas.

TABLE 6: Treatment effect heterogeneity, by ZIP code median income categories

	1st	2nd	3rd
Credit card paydown after stimulus 1 ($)	–6.697	–2.914	36.755
	(16.361)	(9.615)	(26.071)
Credit card paydown after stimuli 2 and 3 ($)	–9.594	–41.211***	–4.353
	(23.128)	(13.573)	(23.156)
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)	(0.003)	(0.007)
Rehabilitated student loan by 2022Q1	0.012	–0.002	0.019*
	(0.009)	(0.012)	(0.010)
Rehabilitated student loan by 2022Q3	0.004	–0.004	0.008
	(0.012)	(0.012)	(0.015)
Consolidated FFEL to Direct	0.000	–0.000	0.006
	(0.012)	(0.010)	(0.006)
Prioritized reducing private student loan balance	0.007	0.018	0.006
	(0.011)	(0.013)	(0.009)
Maximum observations	1,064,226	1,184,052	1,196,151

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 4) where the row denotes the outcome of interest. The first, second, and third terciles of ZIP income split the population of US households into three categories ordered by median household income using the 2015-2019 5-year American Commuty Survey. The bounds for the second tercile are $52.5k and $73.7k with the first tercile below and the third tercile above these bounds. More information on variable construction can be found in Section 3.2. 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.

Next, we explore heterogeneous effects of financial education on mortgage originations and refinance. Generally, we find similar positive significant treatment effects across area income for mortgage origination. While the point estimates are similar for any purchase mortgage and for first-time purchase mortgage across income area, lower and middle income areas had a smaller probability of these decisions for the untreated groups, thus the estimated point estimates represent larger proportional increases from the untreated mean relative to the increases for higher income areas. For low income areas, the 0.5 percentage point increase in first-time purchase mortgage represents a 9% increase from the untreated mean while mandated individuals in higher income areas were 7% more likely to open their first mortgage compared to not mandated individuals living in higher income areas.

Again, we see no effect of financial education on cash-out refinancing in any of the income terciles, however, the effect of required financial education on rate refinance varies across income areas and is largest in higher income areas. While there seems to be no effect of financial education course requirements on lower and middle income areas, the treatment effect in the highest income areas is relatively large and statistically significantly different between those bound by financial education mandates and those not bound, suggesting that the higher likelihood of rate refinancing for those bound by financial education mandates is driven by those living in higher income ZIP codes.

Lastly, we explore differences in the effect of financial education for student loan decisions. Although our main results found no overall effect, we find that treated borrowers in high income areas were more likely to rehabilitate defaulted loans before the Fresh Start announcement. We see no higher probability of consolidating FFEL to Direct loans for lower or middle income areas, but a positive but insignificant estimate for high income areas. Meanwhile we find that all income terciles have positive point estimates for prioritizing interest-accruing student loans, and while none of the estimates are statistically different from zero, treated individuals in middle income areas have the largest estimated difference from untreated borrowers.

In Table 7, we report heterogeneous effects of financial education by credit score as measured in the fourth quarter of 2019. We create three bins for credit score of roughly equal size – those with scores below 620, those with scores between 620 and 719, and those with scores above 720. We find that the effect of financial education on credit card paydown after the second and third stimulus payments is driven by the lowest credit score bin for the second and third stimulus payments.[8]

For mortgage originations, we find increases across each of the credit score bins, but the largest and most precise estimates are for the highest credit score bin. Since mortgage applications go through significant underwriting and since those with higher scores receive better terms, it is sensible that the positive effect of financial education on mortgage origination is more pronounced for borrowers with high credit scores. For untreated borrowers, only 2.9% of the lowest credit score borrowers originated a mortgage during the period of low rates as compared to 15.0% of the highest score borrowers.

For mortgage refinance, we find that the null main result on cash-out refinance masks heterogeneous impacts by credit score. Those bound by financial education mandates with the lowest credit scores in 2019 were actually more likely to extract equity via a refinance than those not bound by mandates. These borrowers may have been able to take advantage of lower interest rates while also accessing equity through credit they may have not had access to through other means due to low credit scores. Additionally, those with lower credit scores were more likely to see their credit scores increase during this time since many borrowers saw rising credit scores in the beginning of the pandemic (Mangrum et al., 2022; Sánchaz and Mori, 2023). We confirm these patterns in our data. We find that homeowners in the lowest credit score group saw an average increase in credit score of 45 points between the last quarter of 2019 to the last quarter of 2021 (the end of the mortgage boom).[9] Moreover, among borrowers in the lowest credit score group who extracted equity from their mortgages, the mean credit score change between the fourth quarter of 2019 and the quarter before the borrower's cash-out refinance was an increase of 87 points. Taken together, this suggests that many homeowners saw significant enough increases in their credit scores during the pandemic that they became creditworthy enough to qualify for cash-out refinancing with reasonable interest rates.

[8]When we analyze credit card pay-down by 2019 credit card utilization, we similarly find that the pay-down after the second and third stimulus payments is driven by those with 80-100% credit card utilization in 2019.

[9]Both the treated and untreated group saw similar increases.

Table 7: Treatment effect heterogeneity, by pre-pandemic credit risk score

	<620	620-719	720+
Credit card paydown after stimulus 1 ($)	–10.230	18.692	9.915
	(8.956)	(12.218)	(28.130)
Credit card paydown after stimuli 2 and 3 ($)	–50.078***	–15.215	3.327
	(11.547)	(18.956)	(26.872)
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)
Rehabilitated student loan by 2022Q1	0.008	0.007	
	(0.008)	(0.043)	
Rehabilitated student loan by 2022Q3	0.002	0.013	
	(0.008)	(0.034)	
Consolidated FFEL to Direct	–0.009	–0.003	–0.001
	(0.018)	(0.008)	(0.008)
Prioritized reducing private student loan balance	0.007	0.014	–0.002
	(0.015)	(0.012)	(0.011)
Maximum observations	1,117,218	1,182,620	1,144,591

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 4) where the row denotes the outcome of interest. Riskscores are Equifax Risk Score 3.0. More information on variable construction can be found in Section 3.2. 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.

On the other hand, the effect of rate refinances were predominantly driven by those with prime credit scores above 720. For the rehabilitation of student loans, the point estimates for treatment effects are largely driven by the higher score bin of those with defaulted loans, however neither point estimate is statistically different from zero. Lastly, the prioritization of reducing interest-accruing student loans is largest for those with 2019 credit risk scores of 620-719, but none of the estimates are statistically different from zero.

6 Robustness

In this section, we explore whether our main results are robust to different sample inclusion criteria and different assumptions regarding the state of high school graduation. 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 may be an underestimate 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.

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. Like the full sample, the under 21 sample shows that treatment increases borrowers' likelihood to pay down credit cards after the second and third stimulus checks, take out a new mortgage, and rate refinance. The point estimates are similar between the samples except for a marked increase for credit card pay-down amounts, opening up the possibility that our estimates might be a lower bound (in absolute value) on the actual treatment effect.

TABLE 8: Main results, by robustness sample

	Full	Under 21	Household Match	Never Moved	5-Year
Credit card paydown after stimulus 1 ($)	2.485	–4.609	3.848	–1.527	–3.033
	(13.844)	(18.996)	(16.466)	(20.785)	(11.681)
Credit card paydown after stimuli 2 and 3 ($)	–20.501*	–29.576**	–23.260**	–19.541	–13.113
	(11.311)	(11.584)	(10.464)	(15.094)	(9.738)
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)
Rehabilitated student loan by 2022Q1	0.008	0.006	0.007	0.003	0.007
	(0.008)	(0.008)	(0.008)	(0.010)	(0.009)
Rehabilitated student loan by 2022Q3	0.002	0.003	0.002	0.001	–0.003
	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
Consolidated FFEL to Direct	0.003	0.005	0.003	0.005	–0.000
	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)
Prioritized reducing private student loan balance	0.011	0.010	0.010	0.013	0.014
	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)
Maximum observations	3,444,429	2,477,262	2,957,346	2,467,629	2,053,069

Notes: The table above reports the estimate for γ from Equation (1) (as described in Section 4) where the row denotes the outcome of interest. Each column represents an adjustment to the sample as described in Section 6 with further details in Section Online Appendix A. More information on variable construction can be found in Section 3.2. 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.

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. Similar to the last robustness check, this assignment should reduce the potential error in the assignment of high school graduation state. Similar to the previous robustness, we find somewhat larger point estimates for the credit card pay-down results, and similar point estimates for the other outcomes.

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. Again, we find quantitatively similar results across our main outcomes of interest, but credit card pay-down is no longer statistically significant.

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. Again, the estimated coefficients are all qualitatively similar to the baseline specification that includes the full identifying variation. Besides credit card pay-down, each of the estimated effects remains statistically significant, with coefficient estimates all similar to baseline. We take this as evidence that our estimates are not spuriously estimated via differences in life-cycle profiles but instead evidence of the causal effect of financial education mandates.

7 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 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. In contrast, we did not detect a difference in the student loan decisions we tracked between those bound by a financial education mandate and those not bound. For the decisions surrounding rehabilitating defaulted loans, this may be because we condition on having defaulted loans and thus we are conditioning on a negatively selected sample for which the financial education intervention may have not been effective. In this case, it is likely not surprising we do not find a difference.

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 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.

In exploring heterogeneity in the effect of financial education across neighborhood income and credit score, we find that credit card pay-down was driven more by treated borrowers in middle income neighborhoods and those with lower credit scores, consistent with the means testing behind these stimulus payments. The effect of financial education on mortgage origination was similar across all income neighborhoods but was largely taken advantage of by those with credit scores above 720. Although we did not detect an increased probability of those bound by financial education to extract equity via refinancing existing mortgages in the general sample, we did find that borrowers with credit scores below 620 were more likely to cash-out refinance if they were bound by state financial education mandates. We also find that many homeowners who had credit scores below 620 prior to the pandemic saw dramatic increases in their credit scores which may have improved their credit worthiness, unlocking their ability to extract equity through refinance at historically low interest rates. Meanwhile the higher probability of rate reduction refinances by treated individuals was driven by those in higher income neighborhoods and those with higher credit scores. Lastly, we found no statistical effect of financial education for the student loan outcomes we explored, however that may be due to the relatively small sample size for those who could have made the opportune decisions we study.

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.

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. 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.[10] 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

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

1% balance delinquent[11] 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.

The student loan moratorium allowed borrowers to easily rehabilitate their defaulted Direct loans through a nine-month process. Then, the Fresh Start program, which would rehabilitate all defaulted federal loans, was announced 2022Q2 and implemented 2022Q4. *Rehabilitated student loan by 2022Q1* indicates that a borrower with at least one defaulted federal student loan in 2019Q4 has zero defaulted federal student loans in 2022Q1, before the Fresh Start announcement. *Rehabilitated student loan by 2022Q3* indicates that a borrower with at least one defaulted federal student loan in 2019Q4 has zero in 2022Q3, before Fresh Start began. To be eligible for either decision, a borrower must have a defaulted federal student loan in 2019Q4.

Consolidated FFEL to Direct indicates that a student loan borrower transferred balances from FFEL to a Direct Consolidation Loan. To be eligible, a borrower must have a FFEL loan in February 2020. Then, we use quarterly data to look at the quarter-to-quarter change in a borrower's FFEL and Direct balances. If their FFEL balance decreased in one quarter and their Direct balance increased by 80-120% of that amount the quarter before, the same quarter, or up to three quarters after the FFEL decrease, this is considered a consolidation.

Prioritized reducing private student loan balance indicates that a borrower's aggregate FFEL and private student loan balance was lower in June 2023 than in February 2020. Moreover, the decrease must be at least four times greater than the decrease in their Direct balance over the same period, meaning that they directed roughly 80% of their student loan payments to interest-accruing accounts during the pause. To be eligible, a borrower must have both a Direct and a non-Direct (FFEL or private) loan in February 2020. Moreover, a borrower cannot have consolidated FFEL to Direct.

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 and 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

[11] "Delinquent" excludes defaulted loans.

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

We keep the likeliest parent for each child, then remove it if it has 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.