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ABSTRACT

The Devil Is in the Details: State-Mandated Personal Finance Education and Financial Well-Being

Research documents the positive effects of state-required financial education in high school on downstream financial outcomes but pools different types of mandates. Do state policies embedding personal finance content into another course have the same effect as those that require a standalone course? This paper considers the relative effects of the two policy levers on credit scores and subjective financial well-being, using data from states implementing these policies in similar years. Our results show that states requiring a full semester of coursework improve both credit scores and subjective measures of financial well-being, while states allowing more flexibility in implementing personal finance coursework into other classes do not.

JEL Classification: D12, D14, D15, G53

Keywords: financial education, household finance, financial well-being, credit scores

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1 INTRODUCTION

Especially since the Great Recession of 2008–2009, policymakers in the United States have viewed financial literacy education in public schools as one strategy to reduce the risk of future economic crises (Blinder, 2015; Lusardi and Mitchell, 2023). While the role of state and federal policymakers in educational curriculum varies, the majority of states have implemented financial literacy education in K-12 curriculum in the last decade, or are considering doing so (Luedtke and Urban, 2023).

States can implement a financial education mandate in a number of different ways, from mandating certain content, but not a standalone course, to requiring a specific course before graduation from high school. While mandates are nearly always unfunded, the costs of implementing a new course, including administration and teacher preparation, are potentially greater for a standalone course. While research shows that this new course requirement does not hinder graduation rates (Urban, 2023), the opportunity costs of eliminating the option of an elective course that could otherwise benefit students is also a consideration. Prior studies, however, do not answer a highly relevant question for states planning to implement a financial education policy: how much more do students benefit more when personal finance is required to be delivered as a discrete class than when financial literacy content is embedded into an existing required class?

This study estimates the causal effect of state policies requiring a standalone personal finance class compared to state policies requiring personal finance to be embedded into another course on financial behaviors. We consider two outcomes that reflect overall outcomes both objectively and subjectively: credit scores (as in Brown et al. (2016) and Urban et al. (2020)) and subjective financial well-being (as in Burke, Collins and Urban (2025)).

While there are a series of studies that show financial behaviors improve after the implementation of financial education mandates for high school graduation (Brown et al., 2016; Harvey, 2019; Urban et al., 2020; Stoddard and Urban, 2020; Mangrum, 2022; Burke, Collins and Urban, 2025; Lee et al., 2024), these studies have not explored differences in effectiveness across states that require a standalone versus an embedded financial education courses. This research is the first to estimate the causal effects of personal finance required coursework on financial behavior separately for states that required a standalone course and states that allowed personal finance material to be incorporated into another class. This study also builds upon prior literature by incorporating more states that have added education mandates in more recent years.

The last decade witnessed a large number of states with standalone course requirements—commonly called "guarantees" since they guarantee every student has access to personal finance—climbing from zero states for the graduating class of 2007 to 10 states for the graduating class of 2024 (see Table 1). This is a remarkable shift, and recognizing the distinction between these two different approaches, the remainder of this paper will refer to these two policies as "Standalone" and "Embedded."

We use nationally-representative data from two different sources paired with two-way-fixed effects strategies to determine the causal effects of each policy on two different outcomes. First, we use nationally-representative administrative credit panel data from a large credit bureau to look at the causal effects of each policy on credit scores. Second, we use nationally-representative survey data to understand how the policies affect subjective financial well-being, measured by the U.S. Consumer Financial Protection Bureau (CFPB) financial well-being scale. Given the recent econometric concerns with staggered roll-

out (Goodman-Bacon, 2021; Baker, Larcker and Wang, 2022; Callaway and Sant'Anna, 2020; Sun and Abraham, 2020), we run two separate models for our estimates. We compare states from each policy group to the 14 never treated states in our sample period.[3] We are also careful to choose treatment states that happen in the same time windows such that there are not heterogeneous effects based on economic conditions.

Burke, Collins and Urban (2025) finds that requiring financial education improves overall subjective financial well-being, using the same formal measure used in our study. However, the authors show that the greatest benefits are accrued to men and to those who attend at least some college. The education actually *lowered* the subjective financial well-being of those who ended their education with a high school diploma. This finding can be interpreted as education helped people with less human capital to re-calibrate their expectations to be more realistic about their money situation—they were much more likely to say they would "never have the things they wanted in life because of money," for example. However, that study also did not find evidence that state financial education mandates improved objective financial outcomes for this group either, at least using the states with any education mandates at that time. Our study uses more states and exploits variation in the form of the mandate being embedded or standalone coursework.

Our findings show that requiring a standalone personal finance course improves long-run (as old as age 34 among the earliest adopter states as of 2024) subjective financial well-being by 3.4 percent as a marginal effect from the mean. State policies that embed personal finance content into another required class improve the same measure by much less than one percent, and that estimate is not statistically distinguishable from zero. The

[3]For simplicity, we refer to DC as a state.

results are also similar if we construct an alternative, less subjective measure of financial well-being—a positive and statistically significant estimate for a standalone course, and a negligible effect of an embedded course.

Standalone courses improve outcomes across educational attainment. Those who end their education with a high school diploma and those with more education see improvements in credit scores after states implement standalone course mandates.

Overall, our results suggest that when state policies allow personal finance content to be embedded in a non-personal finance required class (e.g., Economics, Math) or as standards that can be implemented in a variety of classes, takeup is not universal. Luedtke and Urban (2023) show that embedded requirements are implemented in schools less than 50 percent of the time.[4] Since our estimates on financial wellbeing are over six times as large in states with standalone than embedded states, we expect that both standalone course requirements are reaching more students and they are more effective than embedded requirements. Further, standalone course requirements are well-targeted to students who end their education with a high school diploma.

2 DATA

We pair data on state-required personal finance courses with nationally-representative individual-level data from two sources: the Ohio State University-University of Wisconsin Consumer Credit Panel (OSU-Wisc CCP) for credit scores and the FINRA Investor Education Foundation's National Financial Capability Study (NFCS) for financial well-being.

[4] In Delaware, this was substantially lower (Delaware Department of Education, 2025).

2.1 Consumer Credit Panel

The OSU-Wisc CCP includes a one percent random sample of annual administrative data (Q4) from a large credit bureau from 2015-2024. In addition to the one percent sample, individuals pulled are followed over time and every member of the household is pulled along with the first person randomly selected. Importantly, these data contain both state and age, which allow us to assign people to the policy environment. We rely upon work by Molloy, Smith and Wozniak (2011); Brown et al. (2016) that most people do not move states in making the assumption that state of residence is also state of high school attendance.

We consider both credit scores and whether the individual has 'prime' or 'subprime' credit in a given year as our outcomes of interest. This allows us to see average effects as well as the ability to avoid having poor credit in young adulthood. These measures have direct implications for the costs of consumer credit, interest rates and access to loans.

2.2 National Financial Capability Study

The NFCS is a nationally- and state-representative survey conducted every three years that captures a host of outcomes related to a household's financial situation. We use survey years 2012–2024, including each respondent's age. Beginning in 2018, the NFCS included the subjective financial well-being scale as developed by the Consumer Financial Protection Bureau (Consumer Financial Protection Bureau, 2017*b*).

Subjective financial well-being is intended to measure how well individuals are keeping up with month-to-month and day-to-day finances, as well as their ability to prepare for the future financially (Consumer Financial Protection Bureau, 2015). It captures how well individuals' goals and expectations meet their realized situations, where the mea-

sure is designed to be independent of income (Consumer Financial Protection Bureau, 2017*b*). Indeed, research has found that each income bracket has a full distribution of financial well-being (Burke, Collins and Urban, 2025; Consumer Financial Protection Bureau, 2017*b*).

We measure financial wellbeing using the 2018, 2021, and 2024 surveys using five standardized items shown in Table 2. We developed a second measure called "proxied financial well-being" scale (PFWB) based on Burke, Collins and Urban (2025) to allow us to both have two additional years of data (2012 and 2015) and to generate a more objective version of the measure. Instead of being based on attitudes and feelings, these questions include items related to emergency savings, household spending, and debt. The corresponding question for each is also listed in Table 2. Both scales are estimated using item response theory (IRT) and re-scaled such that higher items represent better outcomes with a central tendency around 50 Consumer Financial Protection Bureau (2017*a*).

We use age and state to assign individuals to the embedded or standalone course mandate as a treatment effect based on the first year a state required a graduating class to complete personal finance content (see Table 1).

2.3 State Mandates

In order to compare similar years, we begin with a dataset of states that implemented standalone or embedded course requirements beginning with the class of 2008. Before that year, no state had a standalone course requirement.

We cut off states that implemented in 2020 and beyond for two reasons. First, the NFCS and the OSU-Wisc CCP data end in 2024. This means the data are not powered to

look at effects beyond age 18 for some states. Since there are fewer than 20 individuals treated in the NFCS in some of those states, we do not have enough power to estimate the effects for this group. Further, the OSU-Wisc CCP has thinner samples for 18-21 year-olds, as they are less likely to have a credit file. If there are small samples within a given ZIP code and people are at risk of being identified, ages are instead coded as ranges (e.g., 18-20). This grouping invalidates our estimation strategy, giving us a preference for removing states with only a few years post treatment. Second, since those graduating in 2020 were the first cohort to be affected by the COVID-19 pandemic and some states were affected more or less by the pandemic, we cannot isolate the causal effects of implementing personal finance graduation requirements at the same time as the pandemic. Similar concerns exist for the cohorts first changing their policy in 2021. For simplicity, we consider states implementing between 2008 and 2019. This has an added advantage of tracking outcomes ages 22 through 34 (just until they turn 35). Focusing on the population at least age 22 also captures the period after which many students have entered the workforce and completed some post-secondary training.

As shown in Table 1, we consider the following states with standalone course requirements: AL (2017), MO (2010), TN (2013), UT (2008), VA (2015). The following states implementing embedded requirements beginning in 2008 are also in the sample: IN (2013), KS (2012), ME (2017), MN (2015), ND (2011), NJ (2014), OH (2014), OK (2014), OR (2013), SC (2008). Our control states never implemented graduation requirements during the entire sample period: AK, CT, DC, DE, HI, MA, MD, MT, NM, PA, SD, VT, WA, WI.[5]

[5]While some of these states passed legislation during the sample period, none of these states required students to complete a course by the graduating class of 2025.

3 EMPIRICAL STRATEGY

We estimate the causal effect of a required standalone personal finance class or an embedded requirement on credit scores and financial well-being using the specifications show in Equations 1 and 2, respectively. $Y_{i,s,t,y}$ is an outcome for individual i in state s, born in year y, and represented in survey year t. In Equation 1, we include only states with standalone policies and no policies, and in Equation 2, we include only states with embedded policies and no policies. The main coefficients of interest (α_1 and β_1) are the estimates for the causal effect of the state policy on (alternatively) credit scores or financial well-being. The models include state fixed effects (δ_s), birth year fixed effects (η_y), and survey year fixed effects (γ_t). We cluster standard errors at the state level to account for the level of the policy variation.

$$\mathrm{Y}_{i,s,y,t} = \alpha_0 + \alpha_1 \mathrm{Standalone}_{i,s,y,t} + \delta_s + \gamma_t + \eta_y + \varepsilon_{i,s,y,t} \tag{1}$$

$$\mathrm{Y}_{i,s,y,t} = \beta_0 + \beta_1 \mathrm{Embedded}_{i,s,y,t} + \delta_s + \gamma_t + \eta_y + \varepsilon_{i,s,y,t} \tag{2}$$

These estimates identify the changes in credit scores and financial well-being among residents of a state before and after a state moves from no state financial education policy to either an embedded or standalone state mandate. The biggest assumption in a two-way fixed effects (TWFE) model is that trends in outcomes would have been parallel in the absence of the policy. This is untestable, but we can show that trends were parallel in the period leading up to the policy in Figures 1 and 2. In the event studies, we omit the period two years prior to the policy enactment. This is because school systems are incrementally

complying with these mandates—most states phase in curriculum requirements over several years. Schools in states with standalone requirements are implementing courses in the years leading up to the graduation year mandate (Luedtke and Urban, 2023).

4 RESULTS

Table 3 reports the effects of financial education policies on credit scores. We explore three outcomes: credit scores, whether someone has a subprime credit score (a score that generally will have a higher loan denial rate, or higher interest rates if approved), and whether someone has a super-prime credit score (a score that will be more likely to be approved and receive the lowest available interest rate).

Across all columns, the findings are clear: long run credit scores improve for those in states with standalone course requirements but not for those in states with embedded requirements in similar years. Being in a graduating cohort in a state with a requirement increases credit scores by roughly 2 points (0.3 percent relative to the mean). Importantly, these gains seem to come from a reduction in subprime credit scores. Those required to complete a standalone course were 1.2 percentage points (or 3.9 percent as a marginal effect) less likely to have a subprime credit score. We see less movement at the top: there is no statistically distinguishable effect from zero on having a super-prime credit score based on state's implementing a financial education course.

Across all outcomes, the effect of being in a state with an embedded requirement does not change credit scores. Given the size of the dataset, we expect that the results should not be under-powered to study our question, but the estimates for embedded states have

large standard errors and are not statistically different from the standalone states.

We compare our estimates to effects in other settings in the literature. Brown, Cookson and Heimer (2019) show that people growing up on Native American Reservations that are allowed to have banks compared to those without banks—a long intervention that affects most of one's formative years—increases adulthood credit scores by about 7 points (1.1 percent); our effect is about a quarter of this size. Dobkin et al. (2018) find that a hospital admission (excluding labor and delivery) for a non-elderly person with insurance, a shock that results in a 20 percent decline in labor market earnings and increases out-of-pocket medical expenses by $3,275, reduces credit scores by 1.6 points 12 months later (a 0.22 percent decrease). Our results are larger than this persistent negative health, labor market, and expenditure shock.

We next turn to the NFCS survey data. Table 4 shows causal estimates of each policy on subjective financial well-being and, following Burke, Collins and Urban (2025), a proxied measure of financial well-being that contains more objective items.

Standalone course requirements improve subjective financial well-being through age 34 by 1.9 points (4.0 percent as a marginal effect). State mandates for embedding personal finance material into other courses have an effect that is just 0.3 points, or only one-sixth of the size of the standalone course. This estimate is also not statistically different from zero. It seems that the estimates from prior studies in the financial education literature are mostly picking up the effects of standalone courses, rather than embedded courses. When we instead use the proxy measure of financial well-being based on more objective outcomes (see Table 2), the effect of standalone courses remains large: a 1.6 point (3 percent) increase.

To contextualize the magnitude of these estimates, prior studies show that job loss—a short-run and acute event—reduces a person's subjective financial well-being by 4 points. Standalone financial education requirement in high school improves longer-run financial well-being by about 2 points, or just under half of the magnitude of a major shock. We consider this to be a relatively large effect.

Tables A.1-A.2 show the results by question for each well-being scale. Standalone courses improve every measure of financial well-being, though question four—the degree to which one's finances control their life—is not statistically different from zero at the 10 percent level. Further, each effect is large in magnitude: ranging from a 4 percent increase (question 4) to an 11 percent increase (question 2, on whether one's savings will last). The only metric that improves for students in the embedded policy is the availability of emergency savings (question 1).

The difference in the estimates of effects of embedded and standalone policies could be the result of the greater intensity of standalone classes, or because embedded courses are poorly implemented. Earlier research shows that when state policies allow for embedding personal finance content in another course, fewer than half of schools adhere to the requirement and actually implement a new curriculum with fidelity to the mandate (Luedtke and Urban, 2023). Given that the estimates for standalone course policies are over six times as large as embedded policies, our results likely point to standalone courses being fully implemented and therefore more effective.

Figure 3 shows the estimates by education, comparing people with only a high school degree and those with at least some college. Standalone course requirements are lifting up both those who end their education with a high school diploma and those who go on

to more education. This is happening both with subjective and the more objective, proxy measure of financial well-being. Embedded course requirements do not improve average outcomes for either group.

5 CONCLUSION

State education policymakers are implementing financial education mandates for high school graduation with a goal of providing young people with the skills and knowledge they need to be effective at managing their personal finances as adults. Our findings suggest that these policies can have the potential to produce positive impacts on people's financial conditions, including subjective well-being and observed credit scores. However, how states implement these policies is a crucial distinction. Embedded curricula that are added to existing required courses may have lower administrative costs and teacher preparation requirements, but these curricula also show much weaker effects, or no effects at all.

We suspect this is because standalone course requirements are objectively more straightforward to monitor, holding schools accountable to state policies. A new course has to be created, clearly named, taught and recorded into student transcripts. Embedded content is less transparent, and the intensity of implementation could easily vary across schools. Indeed, there is evidence that fewer than half of schools adhere to the embedded financial education materials requirements (Luedtke and Urban, 2023).

We find that standalone course requirements generate larger gains in longer-run financial well-being than embedded courses. Our results show that the effects of standalone

courses are six times the magnitude of embedded courses. The effects of standalone courses also accrue to a wider swath of students, including those who end their education with a high school diploma. While embedded requirements show weak evidence of improving financial well-being, the effects are not always statistically different from zero.

Prior studies have not considered the wide degree of heterogeneity in how states implement financial education mandates. By estimating effects that combine embedded and standalone policies, researchers may have underestimated the potential impacts of a robust, well-implemented standalone financial education course. Prior research estimates effects that rely upon earlier (pre-2008) embedded mandates (Brown et al., 2016; Harvey, 2019; Urban et al., 2020; Stoddard and Urban, 2020; Mangrum, 2022; Burke, Collins and Urban, 2025), which could have been more well-implemented and widely used than more recent embedded mandates. Before Utah started the trend of requiring a full semester of personal finance, other states more carefully designed personal finance content that was to be included in other required classes. After Utah set the example of full semester course requirements, states that added embedded requirements could have been intentionally choosing a policy that had less teeth.

It remains important to consider the relative costs of standalone and embedded course mandates. Urban (2025) shows that there is no evidence—on average—of new teachers hired or additional educational spending for districts within states where standalone course requirements are implemented. However, costs associated with redeploying existing teachers could include teacher training efforts.[6] While we estimate larger impacts for standalone courses, we cannot directly quantify the value of these benefits. One simple ex-

[6]Non-profit philanthropic entities like Next Gen Personal Finance are training and credentialing teachers for free in both remote and in-person events.

ercise is to compare the costs of a $300,000 mortgage for a subprime versus a super-prime borrower, which could amount to more than $3,000 in additional interest costs per year (assuming the subprime borrower can borrow a mortgage at all). If standalone courses reduce the share of population with subprime scores by four percent, the relative value of improved credit scores within a state could be economically significant, especially as borrowing costs accrue over time. The extra costs of developing and administering standalone courses is likely justifiable, as mandates are almost always unfunded.

These results demonstrate the importance of implementation of legislative initiatives. Policymakers' intentions often differ from how and when policies are actually put into place. While both embedded and standalone policies have the same goal of improving people's financial situation, the impact of these policies is more complex, showing that "the devil is in the details."

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6 TABLES

Table 1: State Policies by First Post-mandate Graduation Year

Standalone		**Embedded**		**Control**
UT	2008*	SC	2009*	AK
MO	2010*	ND	2011*	CT
TN	2013*	KS	2012*	DC
VA	2015*	IN	2013*	DE
AL	2017*	OR	2013*	HI
IA	2021	NJ	2014*	MA
MS	2022	OH	2014*	MD
NC	2024	OK	2014*	MT
RI	2024	MN	2015*	NM
NE	2024	ME	2017*	PA
		CA	2020	VT
		WV	2020	WA
		NV	2023	WI
		KY	2024	

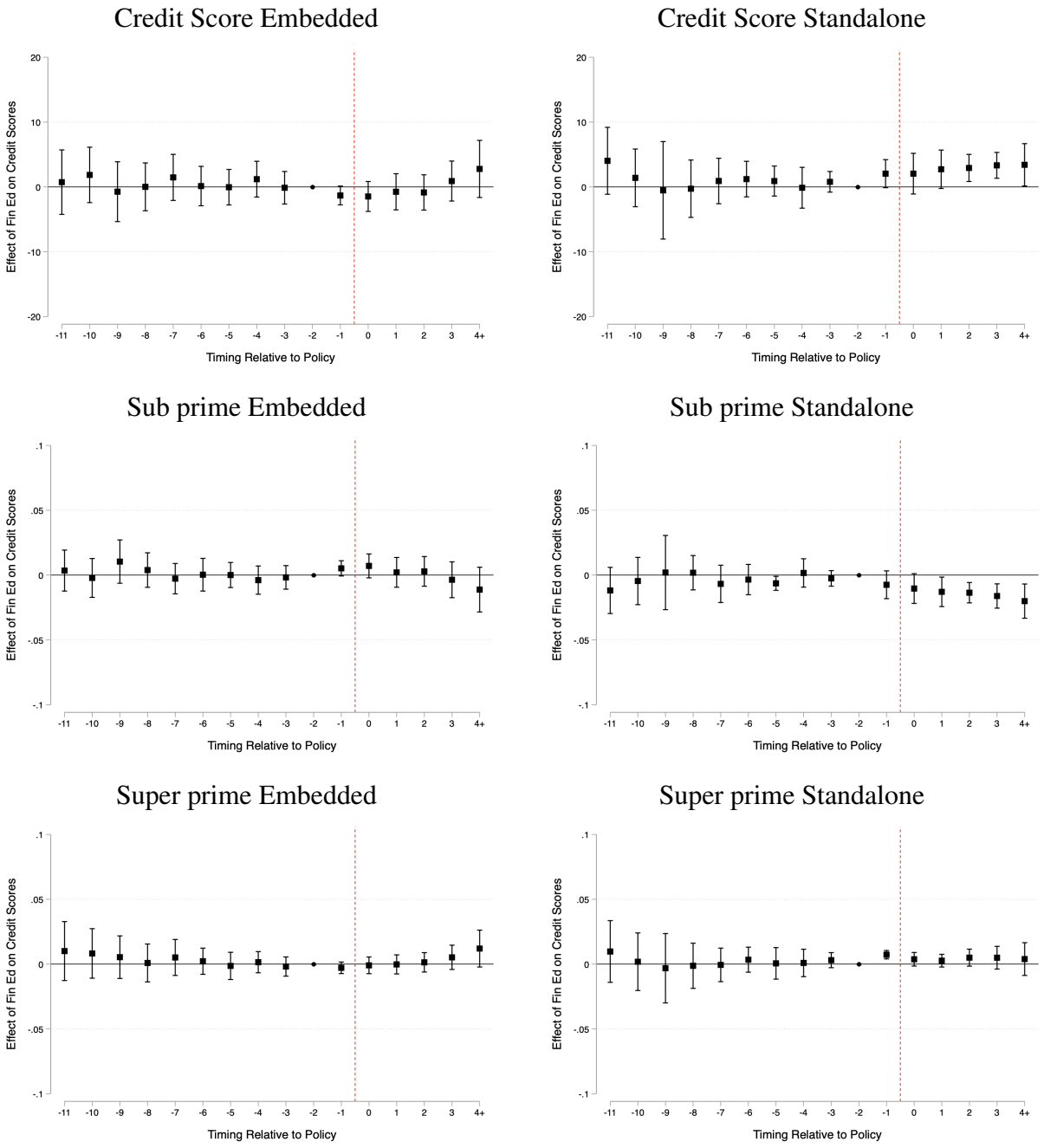
Source: Author's review of state graduation requirements. * indicates treatment states included in study. Control states never implemented graduation requirements required students to complete a course by the graduating class of 2025.

Table 2: Financial Well-being (FWB) Scale and Proxy FWB Measure

Number	CFPB FWB Question	NFCS Proxy Question
Q1	I am just getting by financially	How confident are you that you could come up with $2,000 if an unexpected need arose within the next month?
Q2	I am concerned that the money I have or will save won't last	Over the past year, would you say your household's spending was less than, more than, or equal to your household's income?
Q3	Because of my money situation, I feel like I will never have the things I want in life	Overall, thinking of your assets, debts and savings, how satisfied are you with your current personal financial condition?
Q4	My finances control my life	I have too much debt right now
Q5	I have money left over at the end of the month	In a typical month, how difficult is it for you to cover your expenses and pay all your bills?

Notes: Each question is re-scaled such that higher values represent positive outcomes. For example, "I am just getting by financially" is the specific question asked, but we recode the answers such that those who strongly agree with that statement would have the lowest values.

Figure 1: Credit Score Event Studies



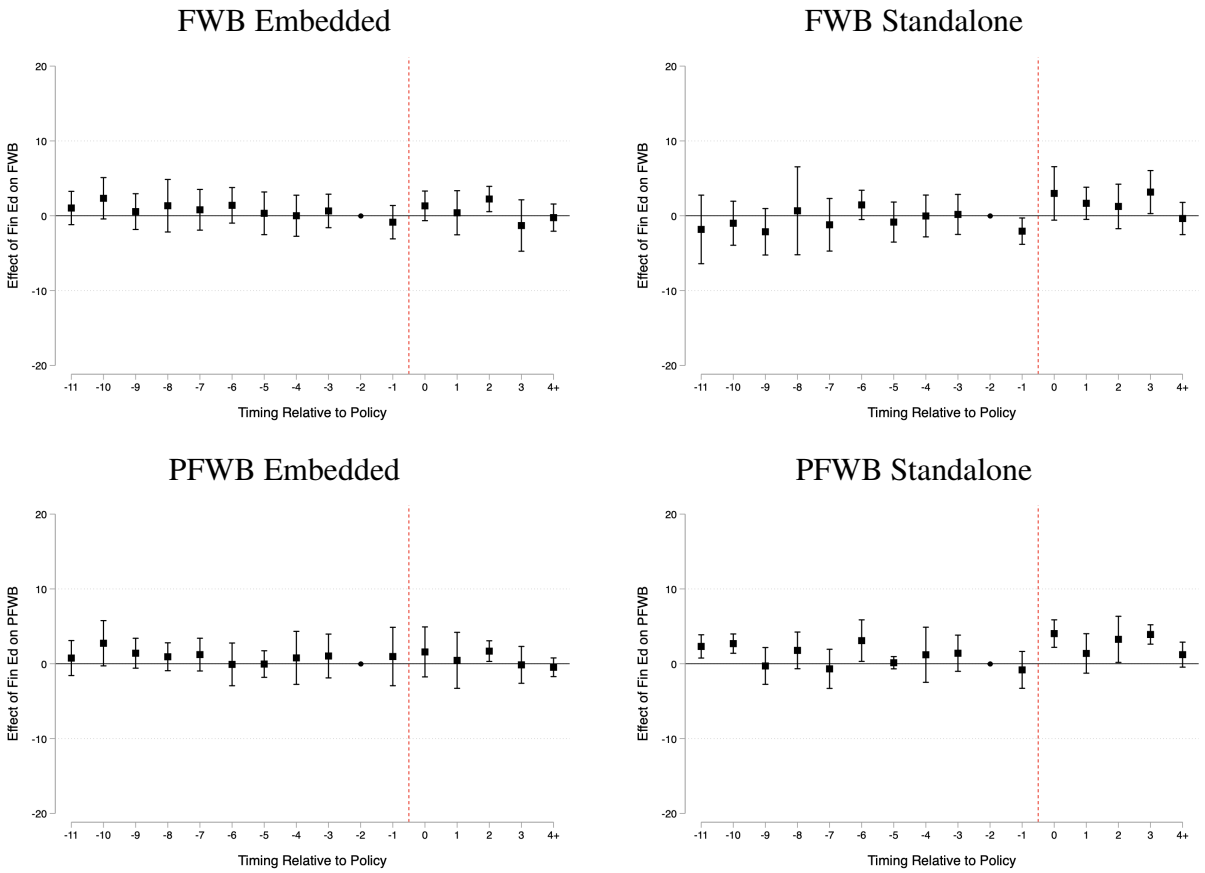
Notes: 95% confidence intervals displayed from robust standard errors clustered at the state-level. The data come from the nationally-representative sample in the OSU-Wisconsin Consumer Credit Panel from 2015-2024. The sample includes all individuals 22 through 34 years old in treatment states (implementing between 2008 and 2019) and individuals the same ages in control states. The y-axis represents the difference in the scales across the treatment and control groups in each period, compared to two periods before the policy took effect.

Table 3: Effects of Standalone and Embedded Financial Education Requirements on Credit Scores

	Credit Score		Subprime Credit Score		Super-prime Credit Score	
	(1)	(2)	(3)	(4)	(5)	(6)
Standalone	2.036*		-0.012**		0.002	
	(1.157)		(0.004)		(0.005)	
Embedded		-0.170		-0.000		0.002
		(1.602)		(0.007)		(0.005)
N	2,971,100	3,749,405	2,971,100	3,749,405	2,971,100	3,749,405
Mean DV	660	660	0.31	0.31	0.14	0.14

Notes: Robust standard errors clustered at the state-level are in parentheses. This table reports estimates of α_1 in Equation 1. The data come from the nationally-representative sample in the OSU-Wisconsin Consumer Credit Panel from 2015-2024. The sample includes all individuals 22–34 years old in treatment states (those implementing between 2008 and 2019) and individuals the same ages in control states. Subprime Credit Score equals one if the individual has a credit score below 600 and zero otherwise. Super-prime Credit Score equals one if the individual has a credit score above 780 and zero otherwise.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2: Financial Well-being and Pseudo-Financial Well-being Event Study



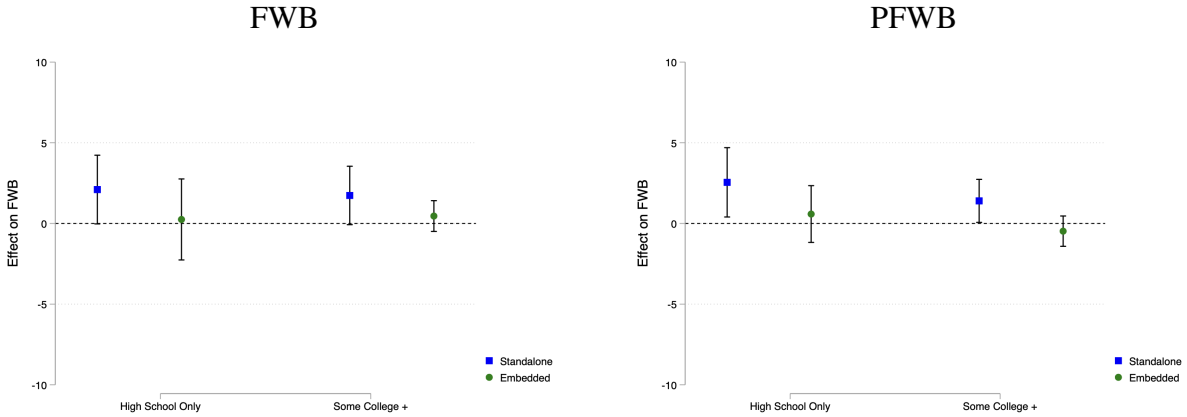
Notes: 95% confidence intervals displayed from robust standard errors clustered at the state-level. Data come from the 2018, 2021, and 2024 NFCS (Top Panel) and 2012, 2015, 2018, 2021, and 2024 NFCS (Bottom Panel). The y-axis represents the difference in the scales across the treatment and control groups in each period, compared to two periods before the policy took effect. The sample includes all individuals 22 through 34 in treatment and control states.

Table 4: Effects of Standalone and Embedded Financial Education Requirements on Subjective and Objective Financial Well-being

	(1) Subjective FWB	(2) Subjective FWB	(3) Proxy FWB	(4) Proxy FWB
Standalone	1.918**		1.596***	
	(0.848)		(0.519)	
Embedded		0.282		-0.158
		(0.514)		(0.391)
N	6321	8096	10643	13408
Mean DV	46.9	46.7	51.3	51.0

Notes: Robust standard errors clustered at the state-level are in parentheses. This table reports estimates of α_1 in Equation 1 and β_1 in Equation 2. The outcomes in Columns (1) and (2) are the US CFPB's measure of Financial Well-being (FWB), and the outcomes in Columns (3) and (4) are the pseudo-financial well-being measure used for a more objective scale from Burke, Collins and Urban (2025). Both range from 0 to 100. Data come from the 2018, 2021, and 2024 NFCS (Columns (1)-(2)) and 2012, 2015, 2018, 2021, and 2024 NFCS (Columns (3)-(4)). The sample includes all individuals 22 through 34 in treatment and control states. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3: Effects of Financial Education Types on Financial Well-being and Pseudo-Financial Well-being Educational Attainment



Notes: 90% confidence intervals displayed from robust standard errors clustered at the state-level. Data come from the 2018, 2021, and 2024 NFCS (Left Panel) and 2012, 2015, 2018, 2021, and 2024 NFCS (Right Panel). The y-axis represents the difference in the scales across the treatment and control groups in each period, compared to the period just before the policy took effect. The sample includes all individuals 22 through 34 in treatment and control states.

7 APPENDIX

Table A.1: Effects of Standalone and Embedded Financial Education Requirements on Financial Well-being, Question by Question

	(1) Q1	(2) Q2	(3) Q3	(4) Q4	(5) Q5
Standalone	0.100**	0.162*	0.199**	0.070	0.185**
	(0.039)	(0.085)	(0.091)	(0.100)	(0.077)
N	6197	6187	6195	6226	6238
Mean DV	1.71	1.51	1.81	1.65	2.11
	(1) Q1	(2) Q2	(3) Q3	(4) Q4	(5) Q5
Embedded	0.078**	0.008	0.038	-0.003	-0.052
	(0.034)	(0.054)	(0.056)	(0.049)	(0.047)
N	7942	7925	7948	7966	8002
Mean DV	1.70	1.50	1.80	1.64	2.09

Notes: Robust standard errors clustered at the state-level are in parentheses. This table reports estimates of α_1 in Equation 1 and β_1 in Equation 2. The outcomes in Columns (1) and (2) are the US CFPB's measure of Financial Well-being (FWB), and the outcomes in Columns (3) and (4) are the pseudo-financial well-being measure used for a more objective scale from Burke, Collins and Urban (2025). Both range from 0 to 100. Data come from the 2018, 2021, and 2024 NFCS (Columns (1)-(2)) and 2012, 2015, 2018, 2021, and 2024 NFCS (Columns (3)-(4)). The sample includes all individuals under 34 in treatment and control states.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Effects of Standalone and Embedded Financial Education Requirements on Proxied Financial Well-being, Question by Question

	(1) Q1	(2) Q2	(3) Q3	(4) Q4	(5) Q5
Standalone	0.195**	0.005	0.096	0.111	0.163*
	(0.082)	(0.063)	(0.100)	(0.105)	(0.084)
N	10240	10388	10520	10486	10230
Mean DV	2.32	2.50	1.90	1.82	2.25
	(1) Q1	(2) Q2	(3) Q3	(4) Q4	(5) Q5
Embedded	-0.072	0.027	-0.026	-0.006	0.077
	(0.058)	(0.055)	(0.052)	(0.051)	(0.051)
N	12882	13067	13232	13194	12864
Mean DV	2.28	2.49	1.88	1.79	2.23

Notes: Robust standard errors clustered at the state-level are in parentheses. This table reports estimates of α_1 in Equation 1 and β_1 in Equation 2. Data come from the 2012, 2015, 2018, 2021, and 2024 NFCS. The sample includes all individuals 22 through 34 in treatment and control states.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$