Name: Melody Harvey

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Date of Ph.D.: 2018

Current university affiliation: University of Wisconsin–Madison

Personal website: https://www.melodyharveyphd.com/
Work website: https://humanecology.wisc.edu/staff/harvey-melody/

Short summary of current research activities:

My research interests lie within household behavior and family economics, financial economics, and public economics with an emphasis on economically vulnerable households. I particularly evaluate how consumer, social welfare, and K-12 education policies affect personal and household finances among younger adults across a range of socioeconomic groups. My research seeks to inform the heterogeneous effects of public policies that could causally improve financial health. For example, a recent paper of mine finds no improvements in retirement savings among adults ages 25-40 exposed to high school personal finance course requirements, even if state content standards explicitly include retirement concepts.

I am an economist in an interdisciplinary department and would benefit strongly from exposure to the cutting-edge economics research featured at the NBER. I regularly read top economics journals to help my research and aspire to make my own contributions through these channels. Currently, my research focuses on the effects of K-12 financial education mandates on younger adults' finances such as payday borrowing, student loan decisions, emergency savings, and retirement savings. A separate current project is methodological in nature and documents recall bias in high school financial education assessments and the implications of such bias on empirical policy evaluation. In addition to these projects, I examine:

- The effects of financial regulations on consumers' access and use of credit-based alternative financial services and safety net programs
- The consequences of parental adverse events on adult children's financial planning
- The impacts of public assistance eligibility criteria on near-poor households' saving and consumption decisions (e.g., crowd-out)

I am eager to attend the in-person conference to have a chance at building networks at this early stage of my career. Thank you for your kind consideration of my application.

Summer Institute session(s) interested in attending (July 25-29, 4 nights):

1. SI 2022 Public Economics (July 26-27, 2022)
2. SI 2022 Education (July 27, 2022)
3. SI 2022 Children (July 28-29, 2022)

MELODY A. HARVEY

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EDUCATION

2018	Ph.D., Policy Analysis, Pardee RAND Graduate School *Analytic Concentration*: Economic Analysis
2012	M.P.P., Economics and State and Local Policy, Pepperdine University
2010	B.A., Economics, Reed College

RESEARCH INTERESTS

Public Policy: consumer policy, social policy, and curricula in state education policy

Economics: household behavior and family economics, financial economics, and labor and demographic economics

EMPLOYMENT

Current Position: University of Wisconsin–Madison

2020-	Assistant Professor, Department of Consumer Science, School of Human Ecology
2020	Visiting Professor, School of Human Ecology
2018-2020	Postdoctoral Fellow, National Poverty Fellows Program, Institute for Research on Poverty, College of Letters & Science

Previous Positions

2018-2020	National Poverty Fellow, Institute for Research on Poverty, University of Wisconsin–Madison, Madison, WI *In residence*: Office of Planning, Research and Evaluation, Administration for Children and Families, U.S. Department of Health and Human Services, Washington, DC
2012-2018	Assistant Policy Researcher, RAND Corporation, Santa Monica, CA
2017	Research Fellow (Foundations-Level Research Methods), AEA Summer Program, Michigan State University, East Lansing, MI
2015	Research Consultant: Community Investment Trust Project, Mercy Corps Northwest, Portland, OR
2011-2012	Teaching Assistant (Research Methods, Macroeconomics, and Advanced Econometrics), School of Public Policy, Pepperdine University, Malibu, CA
2011	Research Assistant, Women Organizing Resources, Knowledge and Services (WORKS), Los Angeles (Highland Park), CA
2011	Interning Research Assistant, Policy Consulting Associates, Culver City, CA
2009-2013	Research Assistant, Flintridge Center, Pasadena, CA
2009-2010	Survey Researcher: Lents Community REIT Project, Mercy Corps Northwest, Portland, OR
2008	Summer Intern, Flintridge Operating Foundation, Pasadena, CA

PUBLICATIONS

Peer-Reviewed Articles

Harvey, Melody. 2019. "Impact of Financial Education Mandates on Younger Consumers' Use of Alternative Financial Services." *Journal of Consumer Affairs* 53, no. 3: 731-769.

Peer-Reviewed Reports

Hunter, Sarah B., **Melody Harvey**, Brian Briscombe, and Matt Cefalu. 2017. *Evaluation of the Housing for Health Permanent Supportive Housing Program*. Research Report, RR-1694-BC. Santa Monica, CA: RAND Corporation.

Kaufman, Julia H., Susan M. Gates, **Melody Harvey**, Yan Wang, and Mark Barrett. 2017. *What it Takes to Operate and Maintain Principal Pipelines: Costs and Other Resources*. Research Report, RR-2149-WF. Santa Monica, CA: RAND Corporation.

Martin, Laurie T., Nazleen Bharmal, Janice C. Blanchard, **Melody Harvey**, and Malcolm Williams. 2014. *Barriers to Enrollment in Health Coverage in Colorado*. Research Report, RR-782-COHF. Santa Monica, CA: RAND Corporation.

Chinman, Matthew, Sarah Beth Hunter, Jill S. Cannon, M. Rebecca Kilburn, **Melody Harvey**, and Mollie Rudnick. 2014. *Process Evaluation of the New Mexico Maternal, Infant, and Early Childhood Home Visiting Competitive Development Grant*. Research Report, RR-639-REC. Santa Monica, CA: RAND Corporation.

Gates, Susan M., Laura S. Hamilton, Paco Martorell, Susan Burkhauser, Paul Heaton, Ashley Pierson, Matthew Baird, Mirka Vuollo, Jennifer J. Li, Diana Lavery, **Melody Harvey**, and Kun Gu. 2014. *Preparing Principals to Raise Student Achievement: Implementation and Effects of New Leaders Program in Ten Districts*. Research Report, RR-507-NL. Santa Monica, CA: RAND Corporation.

Issue Briefs

Harvey, Melody, and Carly Urban. 2022. "Is High School the Right Time to Teach About Retirement?" *Trends and Issues*. New York: TIAA Institute.

Harvey, Melody. 2019. "Does State-Mandated Financial Education Affect High-Cost Borrowing?" *Insights: Financial Capability*. Washington, DC: FINRA Investor Education Foundation.

Other Publications

Harvey, Melody, Jeremy Burke, Joyce Serido, and Soyeon Shim. 2018. "What Drives Financial Overconfidence among Young Adults?" [Conference Proceeding.] *Consumer Interests Annual* 64.

Harvey, Melody. 2017. "Are Financial Education Mandates Associated with Use of Alternative Financial Services?" [Conference Proceeding.] *Consumer Interests Annual* 63.

Harvey, Melody. 2017. "Review of Game of Loans, by Beth Akers, Matthew M. Chingos." [Book Review.] *Journal of Policy Analysis and Management* 36, no. 3: 708-712.

O'Hanlon, Claire E., and **Melody Harvey**. 2017. "How Does Cuba's Health Care System Do More with Less?" [Commentary.] *Georgetown Journal of International Affairs* Online (10/2/2017).

Harvey, Melody. 2012. "Egypt Wrestles with Democracy: Expectations versus Realities." [Commentary.] *Pepperdine Policy Review* 5: 5-19.

WORKING PAPERS

Collins, J. Michael, Sarah Halpern-Meekin, **Melody Harvey**, and Jill Hoiting. Pragmatism about Financial Services among Low-income Parents.

Harvey, Melody. Do Standardized Testing Requirements Enhance the Effectiveness of Financial Education Mandates? *Revise and resubmit*.

Harvey, Melody. Does State-Mandated High School Financial Education Affect Savings among Young Low-Income Households?

Harvey, Melody. Impacts of Parental Health Shocks on Adult Children's Wealth Accumulation. CFS RDRC Working Paper No. JSIT20-02, October 2021.

Harvey, Melody, and Carly Urban. Does Financial Education in High School Affect Retirement Savings in Adulthood? Wharton Pension Research Council Working Paper No. 2022-03, February 2022.

Harvey, Melody, Cliff A. Robb, and Christopher L. Peterson. Law and Order? Associations between Payday Lending Legislation and AFS Use by Degree of Enforcement. *Reject and resubmit*.

Hunter, Sarah B., **Melody Harvey**, Ryan McBain, Brian Briscombe, Enrico G. Castillo, and Corrin Buchanan. Housing for Health Program: Evaluating a Multimodal Intervention in Los Angeles County.

Urban, Carly, and **Melody Harvey**. How Confident are Potential Personal Finance Teachers? *Under review*.

HONORS AND AWARDS

2022	ACCI-NEFE Paper Award, National Endowment for Financial Education
2020	JSIT Scholar, Center for Financial Security, University of Wisconsin–Madison
	UW-Madison's Day at the Capitol's Research and the Wisconsin Idea Showcase Presenter, Office of State Relations, University of Wisconsin–Madison
	2019-2020 Research Scholar, The Sadie Collective
2018	NFCS Research Award, FINRA Investor Education Foundation
	ACCI-NEFE Paper Award, National Endowment for Financial Education
2017	Outstanding Research Fellow, AEA Summer Program, Michigan State University
2011	Pi Alpha Alpha Honor Society (Pepperdine University Chapter), National Association of Schools of Public Affairs and Administration
	Best and Brightest Scholar, Millennium Momentum Foundation
2010	"Ten from '10", Reed College

RESEARCH FUNDING

2020	JSIT Small Grant Award, Retirement and Disability Research Center, Center for Financial Security, University of Wisconsin–Madison	$5,000
	PI: "Impacts of Parental Health Shocks on Adult Children's Wealth Accumulation"	
	Pension Research Council/TIAA Institute Partnership Grant (with Carly Urban)	$62,611
	PI: "Does Financial Education Affect Retirement Savings?"	
	BBVA Research Grant, BBVA Center for Financial Capability and Education	€10,000
	PI: "Who Recalls Personal Finance?"	
2019	IWGYP/CFPB Youth Financial Education Award, U.S. Consumer Financial Protection Bureau and U.S. Department of Health and Human Services	$25,000
	PI: "Does Standardized Testing Enhance Mandate Effectiveness?"	
2017	APLUS Research Award, School of Human Ecology, University of Wisconsin–Madison	$10,000
	PI: "Antecedents of Financial Overconfidence among Young Adults"	

INVITED SEMINARS

2022	Consumer Sciences Seminar, Department of Human Sciences, The Ohio State University, March 4

2021 ECO Seminar Series, Department of Economics, University of Wisconsin–La Crosse, December 10
Orman-Harris Lecture Series, College of Human and Environmental Sciences, The University of Alabama, March 23
2019 Krause Economics Lecture Series, Department of Economics, Reed College, December 3
FDIC Research Seminar, Division of Depositor and Consumer Protection, Federal Deposit Insurance Corporation, July 31
2018 Household Finance Research Seminar, Center for Financial Security, University of Wisconsin–Madison, April 19

INVITED PRESENTATIONS

2021 Panelist: *Building Greater Racial & Social Equity through Financial Education*, Treasury Management Training Symposium, National Association of State Treasurers, June 2
Panelist: "Systemic Barriers in the Financial System" Tweet Chat, University of Chicago Financial Education Initiative, March 30
2020 Discussant: Chen and Friedline's "Making the Invisible Underbanked Visible: Who Are the Underbanked?," *JFCP* & FINRA Foundation Workshop, AFCPE Virtual Conference, November 16
Panelist: *The Importance of PFL Mandates*, Colorado Department of Education, "MoneyWi$er: Personal Finance and How to Teach It" Workshop, Multi-day (June 2, June 6, and June 18)
Discussant: Becher and Benoliel's "'Sneak in' Contracts: An Empirical Analysis," Berkeley Center for Consumer Law and Economic Justice, UC Berkeley School of Law, Consumer Law Scholars Conference, March 6
2019 Panelist: *Defining the Problem* in "Addressing the Financial Literacy Crisis," U.S. House of Representatives Congressional Briefing. Hosted by Representatives Madeleine Dean (D-PA), David Scott (D-GA), and Steve Stivers (R-OH) in conjunction with the House Financial and Economic Literacy Caucus, October 23
Panelist: *Financial Literacy and Education Over the Life Cycle*, U.S. Financial Literacy and Education Commission, JCA/FLEC Research Symposium, May 20
Panelist: *What Works? Research Panel*, Next Gen Personal Finance, The Road to Financial Capability Summit, April 8
2018 Panelist: *Understanding Financial Behavior in America: Insights from the National Financial Capability Study*, FINRA Investor Education Foundation, ACCI Annual Conference, May 17
Guest: "Tim Talks To Financial Education Researcher Melody Harvey," Next Gen Personal Finance, "Tim Talks To…" Podcast, April 27
Panelist: *What's New in Econ and Personal Finance Ed Research?*, California Council on Economic Education, NAEE Professional Development Conference, March 1

PRESENTATIONS

2022 ACCI Annual Conference (paper forthcoming); Junior Household Finance Brownbag Seminar (intragroup presentation forthcoming); APPAM Fall Research Conference (paper); TIAA Institute/PRC Fellows Symposium (paper)
2021 IRP Seminar (internal presentation); WEAI Annual Conference (discussant); ASSA/AEA Virtual Annual Meetings (paper)
2020 APPAM Virtual Fall Research Conference (paper); CFPB Youth Financial Education Research Virtual Convening (paper); CFS Household Finance Research Seminar (internal presentation); Sadie T.M. Alexander Conference for Economics and Related Fields (poster)

2019 APPAM Fall Research Conference (paper); CFS Household Finance Research Seminar (internal presentation); IRP Seminar (internal presentation)
2018 RAND Labor and Population Brown Bag Seminar (internal presentation); ACCI Annual Conference (paper); Cherry Blossom Financial Education Institute (paper); ASSA/AEA Annual Meetings (paper)
2017 APPAM Fall Research Conference (poster); All-California Labor Economics Conference (poster); ACCI Annual Conference (paper); RAND Labor and Population Brown Bag Seminar (internal presentation)
2016 APPAM Fall Research Conference (poster); All-California Labor Economics Conference (poster); RAND Labor and Population Brown Bag Seminar (internal presentation)

MEDIA

2020 *Journalist's Resource*, "Alternative financial services in the time of coronavirus: What you need to know." May 19.
TFA.org, "Closing the Financial Literacy Gap." April 23.
CNBC, "How each US state is shaping the personal finance IQ of its students." February 5.
2019 CNBC, "A threat for payday lenders: Personal finance classes." April 5.

COURSES TAUGHT

CNSRSCI 275 Consumer Finance: Fall 2020-

CNSRSCI 579 Consumer Policy Analysis: Spring 2021, Fall 2021, and Spring 2022

PROFESSIONAL SERVICE

Service to Field

Events:

2021 Initiation Speaker (Invited), Phi Upsilon Omicron Honor Society, Nu Chapter, University of Wisconsin–Madison
2020 Faculty Research Mentor, AEA Summer Program, Michigan State University
2019 NEFE Paper Award Subcommittee, ACCI Annual Conference
2018 *Navigating the Ph.D. Job Market* (Panelist), AEA Summer Mentoring Pipeline Conference

Scholarly Reviews: *Journal of Behavioral and Experimental Finance*, *Journal of Economic Education, Journal of Public Policy and Marketing*

Service to School/Department

2020- Consumer Science Undergraduate Planning Committee

PROFESSIONAL AFFILIATIONS

Research Affiliations

2022- Member, Diversity Scholars Network, National Center for Institutional Diversity, College of Literature, Sciences, and the Arts, University of Michigan
2021- Research Fellow, G53 Network, Global Financial Literacy Excellence Center, School of Business, George Washington University

2020- Faculty Affiliate, Institute for Research on Poverty, University of Wisconsin–Madison
2020- Faculty Affiliate, Center for Demography and Ecology, University of Wisconsin–Madison
2020- Research Affiliate, Center for Financial Security, University of Wisconsin–Madison

Memberships

Association for Public Policy Analysis and Management, American Council on Consumer Interests, American Economic Association, National Association of Economic Educators

Does State-Mandated High School Financial Education Affect Savings among Young Low-Income Households?

Melody Harvey[1]

March 4, 2022

Abstract

As of 2019, one-third of American adults set aside *no* money in the past year for unexpected expenses. Young adults and low-income households, given their resource constraints, are even less likely to save for emergencies or "rainy days." In this study, I use pooled cross-sections from the 2014 and 2018 panels of the Survey of Income and Program Participation (SIPP) to examine if state-mandated financial education impacts young adults' liquid savings propensity and levels. I find no effects on liquid account ownership, liquid savings propensity, or levels of liquid savings for emerging adults overall. However, mandate exposure plausibly increased levels of liquid savings among those employed full time and increased liquid account ownership among those without postsecondary education credentials. These findings suggest that policymakers and educators may wish to shift curricula to focus more on liquid savings and cash flow management as opposed to longer-term savings.

Keywords: financial education, liquid savings, young adults, low- and moderate-income households, Survey of Income and Program Participation (SIPP)

JEL: A21, D14, J1

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[1] Harvey: Assistant Professor, Department of Consumer Science, University of Wisconsin-Madison, maharvey3@wisc.edu. Version last updated March 4, 2022.

1 Introduction

Typically, wealth accumulation begins with building basic liquid savings and saving for unexpected expenses, emergencies, or "rainy days." Having reserves reduces adverse effects that could come from financial shocks. Overall, one-third of American adults saved no money for these purposes in the past year (FDIC 2020, 52), 30% of American adults could not pay a $400 unexpected expense using available liquidity or available credit (to be paid off in full by the next statement) (Federal Reserve Board 2020), and 30% of American adults could not certainly or probably come up with $2,000 within 30 days of encountering an economic shock (Lin, Mottola, and Lusardi 2019, 11). While these savings rates have increased in the past decade for all, these proportions continue to be lower for young adults and low-income households.

To build emerging adults' financial capability, states are increasingly requiring their public high school students to take personal finance courses as a core prerequisite for graduation. This growth in consumer education (as it was historically called) has less emphasis on home economics and more emphasis on personal finance management. The intent is similar across states; the Texas Education Agency (2012, 1), for example, says that "the concepts of personal financial literacy are to be mastered by students in order that they may become self-supporting adults who can make informed decisions relating to personal financial matters." Personal finance courses cover areas germane to this study such as banking and financial services, and savings and investments.

Schools are a key avenue for financial education because information can be disseminated at scale to a heterogeneous population. Stakeholders (including educators) emphasize goals to improve students' long-term financial security with some states covering concepts commonly considered during late-young or middle adulthood (e.g., homeownership, investing, retirement planning, estate planning). However, most young adults just started managing their finances for the first time and have little means to immediately purchase homes or save for retirement, especially those from low- and moderate-income families.

Overall, the causal evidence for state-mandated financial education's efficacy is mixed with previous studies either finding improvements in or no effects on individuals' behaviors. However, recent literature focusing on young adult populations find improvements such as lower credit delinquencies, higher credit scores, lower likelihood of payday borrowing, and higher likelihood of applying for and using federal financial aid (Brown et al. 2016; Urban et al. 2018; Harvey 2019; Stoddard and Urban 2019). This paper augments the literature by examining causal effects of state mandates on predominately low-income young adults' introductory savings holdings. Liquid savings accounts are potential mechanisms through which consumers manage short-term cash flow, so the presence of cash in savings accounts could signal prudent financial management.

This paper estimates the impacts of state-mandated financial education on young adults' propensities to save using pooled cross-sections of the 2014 and 2018 Survey of Income and Program Participation

(SIPP). Particularly, I examine the likelihood and levels of saving. I use difference-in-differences to exploit cross-state and age cohort variation in state-mandated financial education. I restrict the sample to those ages 18-24 who have at least a regular high school diploma since these individuals are most likely to be subject to and affected by state financial education requirements. I further exclude individuals whose total liquidity and total annual individual income exceed the 95th percentile to ensure that I am not capturing otherwise well-off young adults. Overall, I find no overall effects on liquid savings for young adults, but it is associated with increased liquidity among young adults working full time and higher bank account ownership among those without postsecondary education credentials.

This paper is organized as follows: The literature review summarizes previous studies on the association between financial education and savings and on the effectiveness of financial education mandates. The data section describes the survey data used in my analyses. The methodology section explains the identification strategy and model specifications. The findings section examines the effects of financial education mandates on propensities to save. The limitations section reiterates that policy implementation is heterogeneous, financial education mandates are a new policy, and that the survey instrument imperfectly measures liquidity. The discussion section summarizes key findings and provides policy implications.

2 Literature Review

One body of literature has explored the relationship between high school financial education and banking status and savings.[2] For example, using data from a self-administered cross-sectional survey among predominately low-income adults, Grimes, Roger, and Smith (2010) determined that receiving financial education in high school is associated with a greater likelihood of being banked. However, it is unclear how or if this association with simply being banked translates into maintaining at least a minimum balance.

A few papers have examined general correlations between high school financial education and savings-related behaviors among young adults. Peng et al. (2007) examined the relationship between financial education in high school, college, or both settings using a self-administered cross-sectional survey among alumni from a single university. They find that financial education, regardless of setting, was significantly associated with higher savings rates. Gutter and Copur (2011) also find a positive relationship, albeit between high school financial education and college students' savings behaviors using cross-sectional datasets. Like Grimes, Roger, and Smith (2010), these two papers defined financial education receipt using self-reported data. My paper augments these studies by examining causal effects on propensities to save using a larger-scale survey that oversamples low-income adults (who may or may not be in college) and defines exposure to high school financial education based on

[2] I only discuss descriptive papers that directly relate to my study. Interested readers are encouraged to consult Herman et al. (2015) or Walstad et al. (2017) for others.

policy data.[3] This study extends beyond the mere ownership of a savings account to examine the amount of money held in such accounts.

Another body of literature has examined causal effects of state-mandated financial education on financial behaviors. It has found either improvements in or no changes to individuals' behaviors. Papers estimating causal effects of state-mandated financial education typically examine either middle-aged adults' wealth accumulation or young adults' debt behaviors. This corresponds to the life cycle theory of financial behavior, where we expect young adults to borrow to smooth consumption over the life cycle.

Mixed effects are concentrated among studies on middle-aged adults. Bernheim, Garrett, and Maki (2001) and Cole, Paulson, and Shastry (2016) examine impacts of state-mandated financial education on middle-aged adults' wealth accumulation. Using a self-administered cross-sectional household survey, Bernheim, Garrett, and Maki (2001) find that mandates improved middle-aged adults' saving rates and wealth accumulation. Yet, when including state fixed effects and using the Consumer Credit Panel and the pooled 1996-2008 waves of the SIPP datasets, Cole, Paulson, and Shastry (2016) find no effects. While the different effects in their studies may be due to different methodological approaches, effects may be mixed overall because only four states implemented financial education mandates prior to 2002, and because effects of high school financial education may be confounded with other financial capability interventions or "life lessons" they might have received.

Regardless of legislative data source, causal studies tend to find that mandates improved behaviors among young adults.[4] Using the Consumer Credit Panel, Brown et al. (2016) and Urban et al. (2018) find that mandates reduced credit delinquency and improve credit scores. Urban et al. (2018) find larger effects among emerging adults who were exposed to mandates that were in place longer, potentially as teachers and schools become better at the content. Pooling the 2012 and 2015 waves of the National Financial Capability Study, Harvey (2019) finds that young adults exposed to personal finance mandates were less likely to borrow using payday loans, plausibly through increased financial literacy and improved financial planning practices. Most recently, using the pooled 2000-2012 waves of the National Postsecondary Student Aid Study, Stoddard and Urban (2019) find that mandates

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[3] In determining exposure to high school financial education, self-reported data is not substitutable for policy or legislative data. Cude (2017) finds that fewer than half of incoming college freshmen at an R1 institution who attended high school in GA (a mandating state) reported taking a required course in personal finance. Harvey (2018) finds that attending high school in a mandating state, but self-reporting not receiving financial education in high school or at all, is associated with not receiving financial education beyond high school and not believing that financial education should be offered in school. This suggests that self-reported data could be measuring interest in personal finance instead of measuring courses taken.

[4] For example, Brown et al. (2016) use Council of Economic Education's (CEE) *Survey of the States* and Urban et al. (2018) use Urban and Schmeiser (2015) (now updated as Urban 2020). Urban (2020), tracks mandate implementation annually; CEE's surveys track mandates biannually, but they do not distinguish between mandate adoption and mandate implementation. In education, policies are rarely implemented the same year they are adopted.

increased federal financial aid applications and acceptance among college freshmen attending four-year institutions. An emerging consensus around positive effects of mandates among literature concentrating on young adults may be because the mandate is closest to treatment among these age groups; therefore, it is most reasonable to see effects, if any, among them.

In this paper, I augment this body of literature in two ways. First, I examine a different type of short-term behavior (basic liquid savings) using young adults, which is a different demographic within the SIPP than previous studies have employed.[5] Second, I emphasize effects among lower-income young adults without postsecondary education credentials, and hence may not have opportunities to receive formal financial education beyond high school.

The Aspen Institute's report on short-term financial stability emphasizes the importance of short-term financial stability to achieve longer-term financial stability. The four indicators of short-term financial stability include a "routinely positive cash flow (income that exceeds expenses); liquid savings (such as cash and money held in checking and savings accounts); access to high-quality credit; and strong social networks" (The Aspen Institute 2019, 3). In this study, I incorporate a variant of liquid savings, thereby supplementing the literature by examining the potential role of education in inducing these behaviors. Results from this study may show if school-based financial education provides students with a foundation to achieve short-term financial stability, a necessary condition for building longer-term wealth.

3 Theoretical Framework

Financial social work adopted Maslow's hierarchy of needs to explain a hierarchy of savings (e.g., Xiao and Anderson 1997; Friedline, Johnson, and Hughes 2014). Hierarchy of savings ideates that consumers first peruse regular savings accounts and build emergency reserves to meet short-term needs given that regular savings accounts carry low risk and are highly liquid. Once they have successfully built these basic savings, they then move on to more sophisticated levels of savings and investing, such as saving for retirement or acquiring stocks. These savings products, which are riskier and carry liquidity restrictions, are designed to meet long-term needs.

Saving, investing, and retirement planning are commonly taught in high school personal finance courses. According to states' content standards on personal savings, students are expected to understand the savings options available to them and how to develop a savings plan that would help them achieve their financial goals.[6] For example, Colorado Department of Education (2014, 17) states

[5] Like Cole, Paulson, and Shastry (2016), I also examine outcomes using the SIPP. I use the young adult population in SIPP 2014 and 2018 instead of the middle-aged adult population in SIPP 1996, 2001, 2004, and 2008 because higher proportions of individuals were exposed to school-based financial education then.

[6] I present textual evidence from state content standards in Appendix A, although I frequently hear that personal finance teachers discuss saving, investing, and retirement planning with their students.

that students are expected to "analyze strategic spending, saving, and investment options to achieve the objectives of diversification, liquidity, income, and growth." Additionally, Utah State Board of Education (2016, under 3.1.3 and 4.1.3) states that students should be able to "demonstrate how to manage checking and saving accounts;" to "emphasize the importance of proactive budget priorities, such as pay yourself first, emergency/opportunity fund, insurance, and charitable or other voluntary contributions;" and to "identify and understand basic savings options such as savings accounts and CDs." Such lessons received in the classroom may propel young adults to save, even if small amounts.

However, program effects may be conditional on accessing debt per the life cycle theory of consumption, accessing public assistance benefits because eligibility rules and asset tests disincentivize savings, or other circumstances (e.g., familial responsibilities; un(der)employment; or financial independence).[7] At least a couple of states cover how resource constraints affect financial decisions. For example, Tennessee Department of Education (2016, under 9.1.12.B.4 and 9.1.12.D.8) notes that teachers may instruct students to "analyze how income and spending plans are affected by age, needs and resources." Additionally, Missouri's Department of Elementary and Secondary Education (2006, under Money Management 1) expects students to be able to "explain how limited personal finance resources affect the choices people make."

Since states directly address the importance of saving, I expect financial education mandates to directly affect propensities to save. However, the idea of young adults saving counters the life-cycle theory of consumption, which purports that consumers plan spending such that their consumption is equal in all periods. Since most young adults are transitioning into adulthood with little resources and relatively low income, they may borrow now to smooth consumption over the life cycle.[8] This suggests that although high school personal finance instruction covers saving and investing, I may not find any effects on propensities to save.

4 Data

I employ data on individuals' holdings in interest-bearing accounts and regular checking accounts from the 2014 and 2018 panels of the Survey of Income Participation and Programs (SIPP). The SIPP is a nationally representative longitudinal dataset that is used to examine income distributions and public assistance program participation to inform public assistance policies. Accordingly, it oversamples low-income households. Each wave in the 2014 and 2018 panels spans 12 months. The 2014 panel collected data on individuals from January 2013 – December 2016, consisting of a total of four waves (48 months). The newly released 2018 panel collected data on individuals from January – December 2017, currently consisting of just one wave (12 months). I specifically use the first wave from both panels (calendar year 2013 for the 2014 panel and calendar year 2017 for the 2018 panel) to strictly capture

[7] Asset limits disincentivize saving among public assistance benefits recipients (Ratcliffe et al. 2016).

[8] I acknowledge that Modigliani (1966) assumes frictionless credit markets. I discuss this further in the conclusion.

original panel members.[9] In this study, I treat each calendar year of the panel as a cross-section, where I construct annualized information per calendar year.

The SIPP contains data on numerous topics, including those most relevant to this paper such as demographics and residences, employment, benefit receipt, and financial assets. In the 2014 and 2018 panels, the asset data and demographic data are available in a single core file.

The U.S. Census Bureau collected most information monthly. However, individuals' savings amounts, regular checking account holdings, and debt holdings are only collected once a year, and amounts are only asked as of the "last day of the reference month." Both panels disaggregate amounts into accounts in one's own name, accounts jointly owned with others, and accounts jointly owned with spouse/partner. The SIPP assumes a 50% share per person in joint accounts with spouses/partners.[10] I add the amounts across individual and joint accounts to get an individual's total holdings. Unlike its previous panels, the 2014 and 2018 panels disaggregate the total amounts in interest-bearing accounts into amounts in savings accounts, money market accounts, and certificate of deposits (CDs).[11] I employ the measure that sum the amounts across all three main types of interest-bearing accounts as well as the amounts in regular checking accounts as one's total liquid savings.

I restrict the sample to individuals ages 18-24 with at least a regular high school diploma at the time of interview because they are most likely to be exposed to the mandate.[12] Furthermore, I retain individuals whose total liquidity and total annual individual income do not exceed the 95th percentile to avoid capturing well-off young adults in my analyses.[13] Once restricted, the maximum possible sample size is 9,079 individuals.

I first estimate the impact of state-mandated financial education on probabilities of having any liquid accounts. I then estimate probabilities of having any liquid savings and levels of liquid savings. I hypothesize that individuals subject to financial education requirements are more likely to have liquid savings than those who were not subject to these requirements.

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[9] One can join the SIPP (and to the eligible analytic sample) in subsequent waves for two reasons: 1) they turn 18, or 2) new sample member becomes a part of an original sample member's household (e.g., marriage/partnership, cohabitation, other family formation or caretaking, or roommate pairing). This may complicate findings since finances are naturally involved in household composition changes.

[10] Not all spouses/partners contribute 50/50 into their joint accounts. However, SIPP takes the total amount reported from one spouse/partner, and records half of that amount under each spouse/partner. Hence, I cannot discern what their precise contributions are.

[11] SIPP stopped collecting data on interest-bearing checking accounts as of the 2018 panel.

[12] I cannot conduct robustness checks using GED holders because I do not know when they stopped attending school or when they received their GED. Individuals are not required to complete all course prerequisites to receive a GED. I use respondents' ages as of November of their panel year.

[13] I use the 95th percentile to approximate who is likely well-off. I conduct robustness checks around percentile cut-offs later in the report.

5 Methodology

My empirical strategy exploits cross-state variation by cohort in a difference-in-differences (DD) framework.[14] In particular, it exploits variation across young adults residing in the same state before and after the mandate was implemented, and across same-aged young adults in states with mandates and states without mandates. These policies require all public high school students to take the personal finance course to graduate, regardless of preferences or interests. Furthermore, states' propensities to implement mandates are not associated with states' payday lending prohibitions, political party affiliation, unemployment rates, public assistance recipiencies, population size, GDP, or poverty rates (Harvey 2019; Stoddard and Urban 2019).

I assess the impact of state-mandated financial education on propensities to save by estimating standard models of the following form:

$$f(Y_{istw}) = \beta_0 + \theta MANDATED_{stw} + X_i'\beta + \gamma_s + \gamma_t + \gamma_w + \varepsilon_{istw}$$

where Y_{istw} represents the dependent variables in context of propensities to save. I first estimate propensities to have any liquid accounts and propensities to have any liquid savings. I then estimate levels of liquid savings under sensitivity analyses. I estimate all binary indicator variables using linear probability models.[15]

The independent variable of interest $MANDATED_{stw}$ is a binary indicator denoting if an individual aged t originating from state s during calendar year w was exposed to personal finance requirements as a core prerequisite for high school graduation. I determine mandate exposure exclusively based on states requiring personal finance education as a core prerequisite according to Urban (2020). I approximate treatment using the year at which the respondent was aged 18 and the state of *earliest* known residence because the SIPP does not contain data on a respondent's high school graduation year or states of high school attendance.[16] For example, the first graduating class affected by the mandate in Texas is the graduating class of 2007. Therefore, I count anyone originating from Texas who was aged 18 or younger in 2007 as treated. Otherwise, I count anyone originating from Texas who was over age 18 in 2007 as untreated.

I control for any combination of individuals' race/ethnicity, gender, marital status, annual total individual income, metropolitan area residence, employment status, financial independence status, parental status, highest education level, and enrollment status, as represented in the vector X_i. State of earliest known residence γ_s is a fixed effect that captures unobserved state characteristics and

[14] I use "cohort" and "age" interchangeably through-out the report.

[15] This paper strictly concerns parameter estimation (average marginal effects). I also run probit models and find similar results. Therefore, I report LPM results for easy interpretation.

[16] Effectively, I use their state of residence as of January. The 2014 and 2018 panels include the respondents' birthplace. Therefore, I estimate probabilities of moving out of state of birth by mandate status in the paper (excluding respondents born outside the contiguous United States).

policies, including asset tests for public assistance or policies that promote savings among low-income households. Age γ_t is a cohort fixed effect expressed in categorical form that accounts for younger adults being more likely to be exposed to financial education requirements. This fixed effect also captures unobserved cohort factors such as having more financial experience or being more likely to have the means to save. Calendar year γ_w captures any unobserved macroeconomic conditions, such as if the country was in a recession.

I run these regressions on the entire analytic sample and then among individuals ages 18-24 whose highest education credential is a regular high school diploma. Mandates are most likely to impact them because of presumably fewer opportunities to receive formal financial education beyond high school. I assess these heterogeneous effects separately instead of using interaction effects because mandates plausibly affect school attendance.[17]

I cluster standard errors at the state level because policy variation occurs at the state level. I do not weight or adjust regressions according to complex survey designs because I trim the analytic sample to great degrees. As such, this study is not designed to generalize to the emerging adult population. Instead, this study emphasizes exploring causal mechanisms of financial education on financial behavior.

5.1 Assessing Mandate Exogeneity

I assume that state mandates are exogeneous to students because these policies require all public high school students to take the personal finance course to graduate, regardless of preferences or interests. To assess if mandates are plausibly exogeneous to students (now emerging adults), I estimate the probability of mandate exposure given state of earliest known residence, panel year, estimated high school graduation year, age, and sample demographics using the following linear probability model:

$$f(MANDATED_{stw}) = \beta_0 + X_i'\beta + \gamma_s + \gamma_t + \gamma_w + \varepsilon_{istw}$$

where all notations remain from the first equation.

Results are shown in Appendix B. Sample demographics explain at most an additional 0.1 percentage point of the variation in mandate exposure, and its estimates are not jointly significant than zero. Furthermore, most of the point estimates are effectively zero. This suggests that these policies are plausibly exogeneous to youth and young adults.

5.2 Addressing Pre-Trends with an Event Study

[17] Stoddard and Urban (2019) found that state mandates do not affect school attendance, but these findings remain conditional on decisions ex-post. I also find that mandates do not affect educational attainment or school enrollment in my sample, but again, these findings remain conditional on decisions ex-post.

The DD approach strongly assumes that mandating states were not systematically different in related outcomes than non-mandating states prior to policy implementation. This means that states are not introducing mandates due to either a substantial rise or a substantial decline in savings rates, economic outcomes, or savings-promoting policies.

To empirically examine if treatment states and control states may have systematically different pre-trends, I conduct an event study using dummy variables for lead and lag treatment variables. I bound the leads up to seven years before the year that the mandate was implemented. I create categorical dummies to represent two-year intervals to reduce power issues. I include the lead and lag treatment variables and estimate the following:

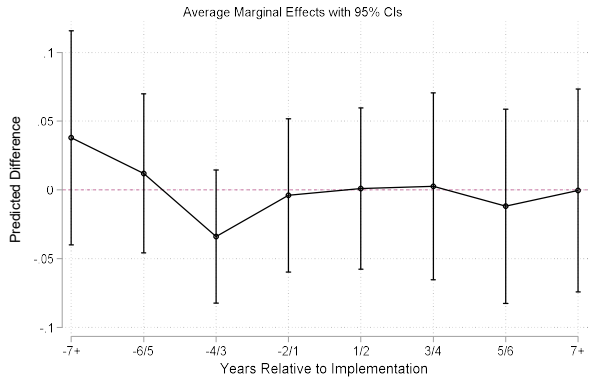
$$f(Y_{istw}) = \beta_0 + \sum_{p=-7}^{-1} \beta mandated_{stw}^{p} + \sum_{p=1}^{7} \theta MANDATED_{stw}^{p} + \gamma_s + \gamma_t + \gamma_w + \varepsilon_{istw}$$

where the omitted dummy variable is the indicator that an individual graduated during the first year of mandate implementation. Additionally, each dummy compares a state (eventually) mandating personal finance to states never mandating personal finance. As defined previously, Y_{istw} is the estimated propensity to save at the selected threshold. I use dummies so that I can see how the effects vary across implementation timing without imposing any functional form assumptions yet allowing for comparison between (eventually) mandated and non-mandated students. If pre-trends are likely common, then we should see no effect of the lead indicators.

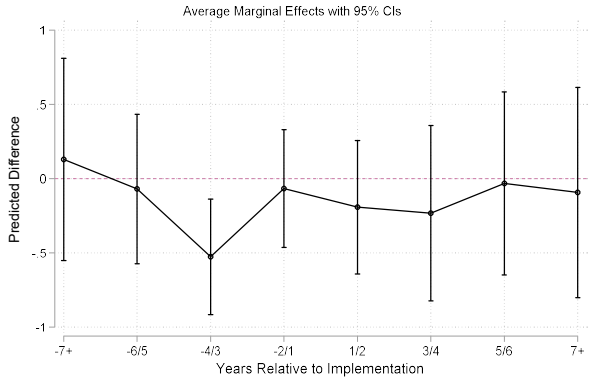
Figure 1 reveals the average marginal effects of the impacted outcomes from the event study specification for liquidity measures. All lead indicators are statistically insignificant for liquid account ownership. For propensities and levels of holding liquidity, there appears to be a particularly statistically significant low 3-4 years before mandate implementation, but otherwise remains statistically insignificant for other pre-implementation years.

Figure 1. Results from Event Study Specification: Liquid Savings

Has Any Liquid Accounts



IHS (Liquidity)



NOTES: N = 9,079. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. LPM coefficients are reported. These results, along with Pr(any liquidity) are reported in tabular form in Appendix D.

5.3 Determining the Source of Variation

Recent econometric literature finds that variation within difference-in-difference approaches may be coming from always treated or units already treated as control (e.g., Baker, Larcher, and Wang 2021; Callaway and Sant'Anna 2020; Goodman-Bacon 2021; Sun and Abraham 2020). With financial education mandates, Illinois is the only state that is "always treated." Therefore, I drop IL from some analysis to ensure that no bias is derived from the always treated. I additionally run a check excluding other states that implemented mandates prior to 2000 (Michigan, New York, and New Hampshire) as they may have differential emphases than newer-implemented states. As Table A shows, results change minimally when excluding IL or pre-2000 implementers.

Table A. Results by State Exclusion

	(1)	(2)	(3)	(4)	(5)	(6)
		All			High School Subsample	
			Panel A: Liquid Account Ownership			
Mandated	0.025	0.023	0.027	0.091**	0.090**	0.094**
	(0.026)	(0.027)	(0.027)	(0.043)	(0.044)	(0.044)
N	9,079	8,662	7,968	3,157	3,010	2,801
R^2	0.039	0.040	0.038	0.040	0.043	0.042
DV Mean	0.74	0.74	0.74	0.64	0.64	0.64
			Panel B: Liquidity > 0			
Mandated	0.018	0.016	0.021	0.067**	0.066**	0.064**
	(0.018)	(0.019)	(0.019)	(0.027)	(0.027)	(0.028)
N	9,079	8,662	7,968	3,157	3,010	2,801
R^2	0.040	0.041	0.039	0.042	0.044	0.045
DV Mean	0.73	0.73	0.73	0.62	0.62	0.61
Exclusion	None	IL	OG	None	IL	OG

NOTES: All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. "OG" refers to IL, NY, and NH. Unweighted. LPM coefficients are reported.

6 Findings

6.1 Descriptive Statistics

Individuals ages 18-24 (mean age = 21) with at least a regular high school diploma and whose total annual individual income and total liquidity is less than or equal to the 95th percentile form the analytic sample. The average annual individual income is $10,889 per year, but this is skewed because one-fifth of the sample had zero total annual income. This suggests that a nontrivial proportion of the sample has no resources to build basic savings.

When looking at the sample demographics, 92% is unmarried, and more than three-quarters reside in metropolitan areas. An overwhelming majority of the sample are U.S. citizens. Slightly over half of the sample is female, and 44% of the sample is nonwhite. Nearly 30% of the sample is in poverty, and 12% of the sample received any of the five major types of federal public assistance (General Assistance, SSI, SNAP, TANF, or WIC) in the past year. Approximately 8% of the sample has a disability (e.g., difficulty hearing, difficulty running errands, difficulty seeing, difficulty walking/climbing stairs, or difficulty with cognition) and 1% of the sample is affiliated with the military.

Overall, almost half of the sample was exposed to financial education requirements in high school. Note that financial education mandates were actively rolling out during this period, meaning that younger individuals in later panels are more likely to be exposed to required personal finance courses. When breaking down ages, 55% of the 18-year-olds were exposed to the requirement versus two-fifths of 24-year-olds.

In analyses, I specifically control for any combinations of individuals' race/ethnicity, gender, marital status, annual total individual income, metropolitan area residence, employment status, financial independence status, parental status, highest education level, and enrollment status. These variables proxy potential resource (un)availability to build savings or mark characteristics where differences in net wealth are due to persistent systemic barriers. While important, I do not control for citizenship status, disability status, or military affiliation due to low variation. Regardless of low variation, I control for marital status to capture instances where an individual has higher liquidity due to joint accounts with significant others.

Table 1. Descriptive Statistics of Sample Characteristics

VARIABLES	Mean
Annual Individual Income (SD=$11,469; Median=$7,452)	$10,889
Highest Education Level (SD=1.4; Median=41)	40.5
Age of Respondent (SD=1.9; Median=21)	21
U.S. Citizen	94%
Single	92%
Lives in Metropolitan Area	79%
Employed	70%
Financially Dependent[a]	54%
Female	51%
Mandated Personal Finance Instruction	48%
Nonwhite	44%

VARIABLES	Mean
Has Any Children in Family	38%
Income ≤ 100% Federal Poverty Line	28%
Speaks Language Other than English	18%
Received Any Federal Public Assistance	12%
Has Disabilities	8%
Affiliated with Military	1%

NOTES: N = 9,079. All statistics are unweighted and use the 95% sample. Maximum annual income possible is $46,884. Minima are zero for all demographics except for age and highest education level. Highest education levels range from 39 (regular high school diploma) to 47 (doctorate degree), where the median education level is 41 ("l or more years of college, no degree). [a] Respondents are "financially dependent" if they are a "child," "grandchild," or "foster child" of the reference person.

Table 2 displays summary statistics of dependent variables overall and by mandate status. Overall, 48% of the sample has an interest-bearing account while 72% of the sample has a liquid account. Correspondingly, 71% of the sample has any liquidity (whether in any savings or regular checking accounts). The average amount held in interest-bearing accounts are $764 and the amount held across any liquid account is $1,303. This suggests that on average, most of their liquidity is in some type of savings account.

Surprisingly, young adults exposed to mandate requirements fare lower than emerging adults not exposed to mandate requirements on all measures. Age may explain some of this because younger individuals are more likely to be exposed to state-mandated financial education and older individuals are naturally more likely to have savings. Yet, the differences between proportions among savings at selected thresholds do not exceed three percentage points. This suggests that we may not see at least substantially significant effects of this policy on propensities to save among this sample.

Table 2. Means of Liquid Savings and Credit Card Debt Held, Overall and by Mandate Status

	Overall (N = 9,079)	Not Mandated (n = 4,757)	Mandated (n = 4,322)
VARIABLES	Mean	Mean	Mean
Has Any Liquid Accounts	72%	73%	72%
Has Any Interest-Bearing Accounts	48%	48%	46%
Has Any Liquidity	71%	72%	69%
Has Any Savings	46%	47%	44%
Total Liquidity (SD=$2,314; Max=$12,400)	$1,303	$1,330	$1,273
Total: Interest-Bearing Accounts (SD=$1,820; Max=$12,300)	$764	$789	$737
Has Credit Card Debt	15%	16%	14%
Total Credit Card Balance (SD=$1,546; Max=$42,800)	$290	$313	$265

NOTES: All statistics are unweighted and use the 95% sample. Interest-bearing accounts include regular savings accounts, certificates of deposits (CDs), and money market accounts (MMAs). Liquid accounts additionally include regular checking accounts. Amounts are unconditional and are rounded to the nearest dollar and

percentages are rounded to the nearest integer. Minima for all variables are zero. Maximum liquidity possible is $12,400. Differences in means by mandate status are statistically significant at the 5% level for any liquidity and any savings and are statistically significant at the 1% level for any credit card debt. Reported standard deviations and maxima concern the overall sample.

6.2 Regression Analysis

Below, I present the main results among all young adults. In all tables, each column represents a separate regression, all concerning total liquid savings. Column (1) only includes fixed effects; column (2) additionally controls for race/ethnicity, gender, marital status, parental status, total annual individual income, metropolitan area residence, and employment status; and column (3) additionally controls for any postsecondary education attainment. Below, I interpret results from Column (1). I show that results are robust to various controls in Columns (2-3).

Table 3 provides the effect of financial education mandates on probabilities of having any type of liquid account. Young adults who were mandated to take personal finance classes in high school were no more or less likely to have any liquid accounts than those who were not mandated to take personal finance classes in high school.

All estimates for probabilities of having any type of liquid account have a positive sign, which is the direction of effects intended by the mandates. While none of these results are statistically significant at the 5% level, I am unable to be precise about this "zero" effect. In the case of having any liquid accounts, the "true" value is going to be between -2.2 percentage points (-3.1%) and 6.1 percentage points (8.5%) 95% of the time.

Table 3. Effect of Mandates on Having Any Liquid Accounts from LPM Regressions

	(1)	(2)	(3)
VARIABLES	Has Any Liquid Accounts		
Mandated	0.019	0.018	0.021
	(0.021)	(0.017)	(0.018)
	[-0.022 - 0.061]	[-0.017 - 0.053]	[-0.016 - 0.058]
Fixed Effects	YES	YES	YES
Demographics	NO	YES	YES
Education Level	NO	NO	YES
Unconditional Mean	0.72	0.72	0.72
N	9,079	9,079	9,079
R^2	0.039	0.139	0.157

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Robust 95% confidence intervals are reported in brackets. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. LPM

coefficients are reported. Results are robust to controlling for financial independence status. Full regression results are shown in Appendix C.

Table 4 provides the effect of financial education mandates on having any liquid *savings*. Each column represents a separate regression. All results are unconditional on having any liquid accounts.

Young adults who were mandated to take personal finance classes in high school were no more or less likely to have any liquidity than those who were not mandated to take personal finance classes in high school. All estimates for propensities of holding any liquid savings have a positive sign, which is the direction of effects intended by the mandates. While none of these results are statistically significant at the 5% level, I am unable to be precise about this "zero" effect. According to the 95% confidence intervals, the "true" probability of having any liquid savings is going to be between -0.8 percentage points (-1.1%) and 5 percentage points (7%).

Table 4. Effect of Mandates on Having Any Liquid Savings from LPM Regressions

	(1)	(2)	(3)
VARIABLES		Liquidity > $0	
Mandated	0.021	0.020	0.023*
	(0.014)	(0.013)	(0.014)
	[-0.008 - 0.050]	[-0.007 - 0.046]	[-0.004 - 0.051]
Fixed Effects	YES	YES	YES
Demographics	NO	YES	YES
Education Level	NO	NO	YES
Unconditional Mean	0.71	0.71	0.71
N	9,079	9,079	9,079
R^2	0.040	0.137	0.159

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Robust 95% confidence intervals are reported in brackets. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. LPM coefficients are reported. Results are robust to controlling for financial independence status. Full regression results are shown in Appendix C. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

6.3 What Gives?

Given the null findings on young adults' propensities to save, I re-examine impacts of financial education mandates conditional on full-time employment. Consistent with the U.S. Census Bureau (2019b, 38), I define full-time employment as working at least 35 hours per week. One must have sufficient resources to save, and many young adults obtain their discretionary income from employment – especially those from lower-income families.

Table 5 provides the effect of financial education mandates on the probability of having any liquid accounts, the probability of having any liquid savings, and the levels of liquid savings. Each column represents a separate regression with each outcome placed in separate panels. All results are unconditional of having any liquid savings. Column (1) only includes fixed effects; column (2) additionally controls for race/ethnicity, gender, marital status, parental status, total annual individual income, metropolitan area residence, and employment status; and column (3) additionally controls for current enrollment status. Below, I interpret results from Column (2). I show that results are robust to various controls in Columns (3).

Among young adults employed full time and when controlling for demographics, those who were exposed to personal finance mandates were 4 percentage points more likely to have any liquid savings than those who were not exposed to these requirements. Given that 66% of working young adults had any savings, this translates to a 6.4% increase in the propensity to have nonzero savings. I additionally find that mandates also increase overall level of liquid savings among fully employed young adults. I continually see no effects of mandates on general propensities to have any liquid accounts.

Table 5. Effect of Mandates on Propensities and Levels of Liquid Savings among Young Adults Employed Full Time, from LPM Regressions

	(1)	(2)	(3)
VARIABLES	**Panel A: Has Any Liquid Accounts**		
Mandated	0.028	0.029	0.034
	(0.034)	(0.028)	(0.028)
	[-0.039 - 0.096]	[-0.026 - 0.085]	[-0.023 - 0.091]
Unconditional Mean	0.67	0.67	0.67
R^2	0.052	0.161	0.178
	Panel B: Liquidity > $0		
Mandated	0.040	0.042**	0.047**
	(0.024)	(0.020)	(0.021)
	[-0.009 - 0.089]	[0.002 - 0.082]	[0.004 - 0.090]
Unconditional Mean	0.66	0.66	0.66
R^2	0.052	0.157	0.179
	Panel C: IHS (Liquidity)		
Mandated	0.312*	0.335**	0.382**
	(0.183)	(0.149)	(0.149)
	[-0.055 - 0.678]	[0.035 - 0.635]	[0.083 - 0.681]
Unconditional Mean	4.642	4.642	4.642
R^2	0.060	0.183	0.212

NOTES: N = 5,377. Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Robust 95% confidence intervals are reported in brackets. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. LPM coefficients are reported. Results are robust to controlling for financial independence status. Unconditional mean for liquid savings levels: $1,207. Full regression results are shown in Appendix C. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

6.4 Heterogeneous Effects

The main results are based on a heterogeneous group of young adults transitioning into adulthood in varied ways. Hence, I rerun analyses concentrating on young adults whose highest credential is a regular high school diploma.[18] Mandates are likely to have strongest effects among individuals with no postsecondary credentials because they are most likely to employ the learned concepts immediately after graduation (since they are entering the workforce right away) and they may not receive formalized instruction elsewhere.

Table 6 provides the effect of financial education mandates on the probability of having any type of liquid account. Each column represents a separate regression. Column (1) only includes fixed effects; column (2) additionally controls for race/ethnicity, gender, marital status, parental status, total annual individual income, metropolitan area residence, and employment status; and column (3) additionally controls for current postsecondary enrollment status. Below, I interpret results from Column (1). I show that results are robust to various controls in Columns (2-3).

Among those whose highest credential is a regular high school diploma, mandate exposure increases the probability of having any liquid accounts by 8 percentage points. Given that 63% of the subsample has any type of interest-bearing or regular checking account, this translates to a plausibly 12% increase in the probability of having an account. These effects, however, are only statistically significant at the 10% level, and disappear once controlling for current postsecondary enrollment status.

Table 6. Effect of Mandates on Having Any Liquid Accounts among Individuals with Highest Credential: Regular High School Diploma

	(1)	(2)	(3)
VARIABLES		Has Any Liquid Accounts	
Mandated	0.075*	0.061*	0.055

[18] I assume that current students in this group would be in their first term if they are enrolled because the SIPP asks individuals to indicate their highest educational attainment under the question text "What is the highest level of school … completed or the highest degree received by [end of reference period]" by the following detailed categories as they apply to the analytic sample: high school graduate (diploma or GED or equivalent); some college credit, but less than 1 year; 1 or more years of college (no degree); Associate's degree; Bachelor's degree; Master's degree; professional degree (e.g. MD, DDS, JD); or doctorate degree (e.g. Ph.D., Ed.D.).

	(1)	(2)	(3)
VARIABLES		Has Any Liquid Accounts	
	(0.040)	(0.033)	(0.036)
	[-0.005 - 0.155]	[-0.005 - 0.126]	[-0.017 - 0.127]
Fixed Effects	YES	YES	YES
Demographics	NO	YES	YES
Enrollment Status	NO	NO	YES
Unconditional Mean	0.63	0.63	0.63
N	3,157	3,157	3,157
R^2	0.040	0.148	0.167

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Robust 95% confidence intervals are reported in brackets. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. LPM coefficients are reported. Results are robust to controlling for financial independence status. Full regression results are shown in Appendix C. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 7 provides the effect of financial education mandates on propensities to save any amount, unconditional on having any accounts. Among those whose highest credential is a regular high school diploma, mandate exposure increases the probability of having any liquidity by 7 percentage points. Given that 60% of the subsample has any liquidity, this translates to a 12% increase in the probability of having any liquidity.

Table 7. Effect of Mandates on Having Any Liquid Savings among Individuals with Highest Credential: Regular High School Diploma

	(1)	(2)	(3)
VARIABLES		Liquidity > $0	
Mandated	0.071**	0.058**	0.052*
	(0.031)	(0.025)	(0.027)
	[0.010 - 0.133]	[0.008 - 0.108]	[-0.002 - 0.106]
Fixed Effects	YES	YES	YES
Demographics	NO	YES	YES
Enrollment Status	NO	NO	YES
Unconditional Mean	0.6	0.6	0.6
N	3,157	3,157	3,157
R^2	0.042	0.137	0.160

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Robust 95% confidence intervals are reported in brackets. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. LPM coefficients are reported. Results are robust to controlling for financial independence status. Full regression results are shown in Appendix C. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

6.4.1 Revisiting Full-Time Employment Moderation among Individuals with Only High School Diplomas

Once more, I re-run analyses restricting to individuals working full time. Like with the overall sample, one may see larger effects of the mandate among those with sufficient resources to save. Two-thirds of young adults without any postsecondary education attainment work full time compared to 59% overall.

Among these young adults employed full time, those who were exposed to personal finance mandates were 12 percentage points (20%) more likely to have any liquid accounts and 12 percentage points (21%) more likely to have any liquid savings than those not subject to any personal finance course requirements. I additionally find higher levels of liquid savings among those subject to mandates.

Table 8. Effect of Mandates on Propensities and Levels of Liquid Savings among Young Adults Without Postsecondary Education Credentials Employed Full Time

	(1)	(2)	(3)
VARIABLES	**Panel A: Has Any Liquid Accounts**		
Mandated	0.118**	0.097**	0.088*
	(0.049)	(0.044)	(0.049)
	[0.021 - 0.216]	[0.009 - 0.186]	[-0.011 - 0.186]
Unconditional Mean	0.59	0.59	0.59
R^2	0.051	0.174	0.188
	Panel B: Liquidity > $0		
Mandated	0.118**	0.099**	0.088*
	(0.045)	(0.042)	(0.046)
	[0.029 - 0.207]	[0.013 - 0.184]	[-0.005 - 0.181]
Unconditional Mean	0.56	0.56	0.56
R^2	0.052	0.159	0.175
	Panel C: IHS (Liquidity)		
Mandated	0.831**	0.712**	0.627**
	(0.318)	(0.275)	(0.299)
	[0.192 - 1.470]	[0.159 - 1.265]	[0.025 - 1.228]
Unconditional Mean	3.807	3.807	3.807
R^2	0.056	0.164	0.184

NOTES: N = 2,107. Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Robust 95% confidence intervals are reported in brackets. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. LPM coefficients are reported. Results are robust to controlling for financial independence status. Unconditional

mean for liquid savings levels: $783. Full regression results are shown in Appendix C. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

6.4.2 Further Assessments: Do Mandates Impact Subgroup's Total Liquid Savings at Extensive or Intensive Margins?

Since mandates increase the likelihood of having any liquid accounts (regular savings account, CDs, MMAs, or regular checking account) among young adults without postsecondary credentials, I conduct a final exercise assessing the impacts of mandates on account holdings conditional on having a liquid account. I approach this in two ways: first, I run Tobit models left-censored at zero for a continuous measure of IHS-transformed total liquid savings.[19] Second, I run two-part models for both the continuous measure and the binary measure. Both types of models permit me to account for potential self-selection in holdings – where one could have zero holdings because they lack an account, or because they have an account but choose not to use it.[20]

Tobit models assume that zeroes represent a corner solution – where in this instance, the corner solution is not to save any amount. However, Tobit models assume that deciding whether to save is determined by the same mechanism as deciding the amount to save.[21]

Table 9 displays estimated impacts on IHS-transformed total liquid savings in two ways: Panel A contains estimates unconditional on having any liquid accounts and Panel B contains estimates conditional on having any liquid accounts. For both panels, Columns (1-2) contain results from Tobit regressions and Columns (3-4) contain results from OLS regressions. The odd columns only include fixed effects, and the even columns additionally control for race/ethnicity, gender, marital status, parental status, total annual individual income, metropolitan area residence, and employment status.

Overall, results suggest mandates increase expected levels of total liquid savings, unconditional on having any liquid accounts. However, this effect disappears once results are conditioned on having any liquid accounts. This suggests that the significant effects seen on unconditioned estimates are primarily picking up a higher propensity to have bank accounts. Therefore, mandates are operating on the extensive rather than intensive margin.

Table 9. Mandates' Impact on Total Liquid Savings at Extensive vs Intensive Margins, by Conditionality on Having Any Liquid Accounts

[19] Using inverse hyperbolic sines (IHS) instead of natural logs permits "log-normality" while keeping zeroes as zeroes.

[20] Those who have an account may have zero holdings because 1) they have insufficient income, or 2) they have enough income but choose not to deposit in the account. I do not explore further branching in selection to maintain parsimony.

[21] This is certainly true when one first opens a bank account, but not necessarily thereafter.

	(1)	(2)	(3)	(4)
	Tobit		OLS	
VARIABLES	**Panel A: Unconditional**			
Mandated	0.816**	0.661**	0.494*	0.400*
	(0.395)	(0.336)	(0.264)	(0.217)
Fixed Effects	YES	YES	YES	YES
Demographics	NO	YES	NO	YES
N	3,157	3,157	3,157	3,157
R^2	---	---	0.050	0.144
	Panel B: Conditional			
Mandated	0.174	0.146	0.166	0.138
	(0.230)	(0.230)	(0.223)	(0.222)
Fixed Effects	YES	YES	YES	YES
Demographics	NO	YES	NO	YES
N	1,975	1,975	1,975	1,975
R^2	---	---	0.051	0.071

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. Coefficients are reported. Results are robust to conditioning on full-time employment. Full regression results are shown in Appendix C. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Two-part models also assume that zeroes represent a corner solution – where in this instance, the corner solution is not to save any amount. However, two-part models assume that the decision to save and the amount saved are independently determined.

Table 10 displays estimated impacts on IHS-transformed total liquid savings in two ways: Panel A contains estimates with no demographic controls and Panel B contains estimates with demographic controls. For both panels, Column (1) shows results from the regressions in Table 6 estimating the probability of having any liquid accounts across all respondents, and Columns (2-3) show results from OLS regressions only among those who have an account.

Overall, results suggest mandates do not independently impact propensities to save or levels of liquid savings. Hence, mandates have effects primarily on the extensive margin. They are likely to have bank accounts but having a bank account does not induce building savings.

Table 10. Mandates' Impact on Savings at Extensive vs Intensive Margins from Two-Part Models, by Conditionality on Having Any Liquid Accounts

VARIABLES	(1) Has Any Liquid Accounts	(2) Liquidity > 0	(3) IHS (Liquidity)
	Panel A: Without Demographic Controls		
Mandated	0.075*	0.008	0.166
	(0.040)	(0.025)	(0.223)
N	3,157	1,975	1,975
R^2	0.040	0.030	0.051
	Panel B: With Demographic Controls		
Mandated	0.061*	0.008	0.138
	(0.033)	(0.025)	(0.222)
N	3,157	1,975	1,975
R^2	0.137	0.031	0.071

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. Coefficients are reported. Results are robust to conditioning on full-time employment. Full regression results are shown in Appendix C. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

6.5 Robustness Checks

For robustness checks, I conduct sensitivity analyses around percentile cut-offs and estimate the probabilities of moving out of one's state of birth.

6.5.1 Sensitivity Analyses by Percentile Cut-offs

One potential concern is that these results may be sensitive to the percentile cut-offs employed. As shown in Table 11, liquidity levels and annual incomes substantially vary among the young adults in the sample. Young adults with the liquidity and incomes amounts displayed at the 99th and 100th percentiles more likely acquired them through family members or because of higher socioeconomic status. Financial education policies are unlikely to affect well-off emerging adults. Throughout the paper, I use the 95th percentile cut-off, which excludes young people whose liquidity level exceeds $12,400 or whose annual individual income exceeds $46,884.

Table 11. Percentile Distributions of Total Liquid Savings and Total Annual Individual Income among Full Analytic Sample

Percentile	N	Total Liquidity	Total Annual Individual Income
90th	8,284	$7,000	$35,100
95th	9,079	$12,400	$46,884
99th	9,799	$42,800	$83,645

100th	9,993	$922,300	$842,652

NOTES: Unweighted. Figures are rounded to the nearest dollar. Value thresholds reported. Full analytic sample is comprised of individuals ages 18-24 who do not have a GED and do not reside in Puerto Rico.

When repeating analyses using the 90th percentile cut-off, the 99th percentile cut-off, and the full analytic sample, I find similar results, as exemplified for probabilities of having any liquid accounts among individuals without postsecondary education credentials in Table 12. The effects of the mandates tend to exhibit minutely stronger effects among lower percentiles for extensive measures yet exhibit minutely stronger effects among higher percentiles for intensive measures (likely due to greater resources). This suggests that mandates may have more of a universal effect than previously thought.

Table 12. Sensitivity Analyses of Effects Among Individuals without Postsecondary Education Credentials by Percentile Cut-Offs

Percentile	90th	95th	99th	100th
VARIABLES	**Panel A: Has Any Liquid Accounts**			
Mandated	0.077**	0.075*	0.074*	0.076*
	(0.038)	(0.040)	(0.039)	(0.039)
	[0.000 - 0.154]	[-0.005 - 0.155]	[-0.004 - 0.153]	[-0.003 - 0.154]
R^2	0.038	0.040	0.040	0.041
	Panel B: Liquidity > 0			
Mandated	0.072**	0.071**	0.071**	0.067**
	(0.030)	(0.031)	(0.032)	(0.032)
	[0.012 - 0.133]	[0.010 - 0.133]	[0.007 - 0.134]	[0.002 - 0.131]
R^2	0.038	0.042	0.041	0.042
	Panel C: IHS (Liquidity)			
Mandated	0.470*	0.494*	0.593**	0.604**
	(0.253)	(0.264)	(0.280)	(0.276)
	[-0.038 - 0.978]	[-0.036 - 1.024]	[0.031 - 1.154]	[0.049 - 1.158]
R^2	0.042	0.050	0.049	0.049
N	2,930	3,157	3,327	3,374

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Robust 95% confidence intervals are reported in brackets. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. LPM coefficients are reported. Sensitivity analyses for the other results are shown in Appendix D. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

6.5.2 Estimating Probabilities of Moving Out of Birth State by Mandate Status

Another potential concern is approximating high school graduation state based on state of current residence versus some other state (e.g., state of birth). Most household surveys do not collect respondents' state(s) of high school attendance, but nearly always collect state of current residence and may collect state of birth. Neither state is a perfect proxy for high school state, but previous studies suggest that state of current residence may be a good enough proxy for high school state (e.g., Brown et al. 2016; Harvey 2019).

Both panels of the SIPP collect respondents' place of birth as well as states of current residence. An individual does not choose their place of birth but fully chooses their states of current residence. Overall, 10% of the analytic sample was born outside the United States. Among the native-born sample members, 74% lived in the same state where they were born.

Table 13 displays estimated probabilities of moving out of one's birth state: particularly, it displays estimated probabilities that native-born sample members are currently residing in their state of birth. Columns (1-2) reveal results for the full universe and Columns (3-4) reveal results for individuals without postsecondary education credentials. The odd columns only include fixed effects, and the even columns additionally control for race/ethnicity, gender, marital status, parental status, total annual individual income, metropolitan area residence, and employment status.

Overall, mandates do not impact probabilities of moving for either sample group. All coefficients have a negative sign, suggesting that young people exposed to mandates might tend to stay.

Table 13. Probability of Moving Out of State of Birth by Mandate Status

	(1)	(2)	(3)	(4)
VARIABLES	All		High School Subsample	
Mandated	-0.043	-0.041	-0.017	-0.015
	(0.028)	(0.028)	(0.040)	(0.039)
Fixed Effects	YES	YES	YES	YES
Demographics	NO	YES	NO	YES
N	8,141	8,141	2,844	2,844
R^2	0.063	0.069	0.078	0.087

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. LPM coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

6.5.3 Effects of Mandates on Liquid Account Ownership from Other Data Sources

Finally, a third potential concerns regards if null findings among the overall young adult sample are a function of using a dataset that oversamples low-income populations.[22] As previously demonstrated, SIPP panel members possess little, if any, liquidity. Hence, the null findings could be a function of liquidity and resource constraints naturally occurring among this sample. To address this issue, I assess effects on savings propensities using two other nationally representative pooled cross-sectional datasets that capture banking status among a wider population across more periods.

I specifically employ FINRA Investor Education Foundation's 2012-2018 National Financial Capability Study (NFCS) and FDIC's 2009-2019 Survey of Household Use of Banking and Financial Services (FDIC).[23] While not as detailed as in the two most recent SIPP panels, both surveys still generally capture if one has a checking or savings account. Consistent with main analyses, I define liquid account ownership as having a checking or savings account. Neither survey captures detailed information amount of liquidity held; hence, I only examine propensities of liquid account ownership. Same sample restrictions apply, except for the 95th percentile given that neither the FDIC nor the NFCS provides exact total liquidity or total individual or household incomes.

Table 14 shows estimated probabilities of having any liquid accounts from SIPP versus FDIC and NFCS data. Column (1) shows regression results from using SIPP data, and Columns (2-3) show regression results from using FDIC data, and Column (4) shows regression results from using NFCS data. Overall, most estimates except for the fully employed FDIC continually show no effects on liquid account ownership among young adults at large. When restricted to the exact same years as employed in the SIPP, the FDIC shows no effects. Additionally, all results reveal overlapping confidence intervals, suggesting that the general finding likely holds.

Table 14. Effect of Mandates on Having Any Liquid Savings from LPM Regressions, by Dataset

Datasets:	SIPP	FDIC[1]	FDIC	NFCS
Mandated	0.019	0.026	0.033***	0.011
	(0.021)	(0.017)	(0.009)	(0.017)
	[-0.022 - 0.061]	[-0.007 - 0.060]	[0.015 - 0.050]	[-0.023 - 0.044]
Unconditional Mean	0.74	0.87	0.94	0.9
N	9,079	4,049	12,383	6,927
R^2	0.039	0.028	0.023	0.023

NOTES: Unweighted. Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Robust 95% confidence intervals are reported in brackets. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence.

[22] It could also be a technical function of using only two periods.

[23] Formerly known as FDIC National Survey of Unbanked and Underbanked Households. For more information about FDIC, visit https://economicinclusion.gov/. For more information about NFCS, visit https://www.usfinancialcapability.org/.

[1] Denotes results when restricted to 2013 and 2017 waves. LPM coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

7 Study Limitations

7.1 State-Mandated Financial Education is a Young Policy and is Heterogeneously Implemented

State-mandated financial education is a relatively new policy. Given that individuals are relatively young at exposure, behaviors at age 18 are difficult to approximate among individuals older than 21 years of age. This greatly affects generalizability beyond young adults.

Different students are exposed to these mandates in differing years of exposure, as well as the varying ways in which this policy is implemented and in how well this policy is implemented and overseen. Financial education policy implementation is an active area of research. One element of this heterogeneity concerns how long the mandate was implemented when the individual took the course. For example, Urban et al. (2018), found that individuals exposed to the requirement in its third year or later were even less likely to have delinquencies than those exposed to the requirement in its first or second year. Other elements of this heterogeneity concern teacher training, where Way and Holden (2009) found that K-12 teachers are often unprepared to teach personal finance courses, and course content mismatch, where Roberts and Joyce (2016) found that students may not necessarily connect concepts of later-occurring mortgages with soon-to-occur concepts of student loans.

Additionally, some schools or districts already offer (or require) personal finance education regardless of state order. For example, up to 70% of Wisconsin's school districts required personal finance instruction for graduation prior to 2018 (Wisconsin Department of Financial Institutions 2018). This means that treatment may be shifting a proportion of students exposed to mandatory personal finance instruction rather than all students. I cannot examine more accurate effects of mandates on respondents' financial outcomes since I lack data on the high school(s) respondents attended as well as any school-level data on schools mandating personal finance courses or state-level data on proportions of districts requiring personal finance instruction.

7.2 Liquidity is Poorly Measured

The SIPP captures holdings once annually, as of the last day of the reference period (December 31st). This date coincides with major holidays and other financially taxing events. An individual plausibly could have had nonzero amounts in their savings or checking accounts earlier in the 12-month period. Hence, this study may underestimate effects.

Additionally, the SIPP captures how much credit card debt individuals have, but does not capture information on credit card behaviors, credit card limits, or on available lines of credit (including if a

consumer even has a credit card). One cannot assess financial health based on sheer levels of debt alone, especially in young adulthood when borrowing is largely expected. Furthermore, liquidity results may be conservative estimates when considering emergency savings since some household surveys include available credit sources when assessing adequacy to meet unexpected expenses.

8 Discussion

The findings from this work demonstrate that financial education has no effect on savings on emerging adults overall. Emerging adults exposed to financial education requirements were no more or less likely to have any liquid accounts, or to have nonzero liquid savings than their peers who were not exposed to these requirements. These findings are consistent with Cole, Paulson, and Shastry's (2016) finding that state-mandated financial education has no effect on savings. While this study employed a similar dataset (the SIPP), the papers differ in survey years employed, ages examined, savings product considered, and state mandate database accessed. Yet, both studies find no impact of financial education policies in schools on savings on their respective general populations. While this largely could be due to data limitations, Harvey (2019) did not find emergency savings to be a plausible mechanism through which mandates reduced probabilities of payday borrowing among young adults. This implies that there may be some disconnects between which savings instruments are emphasized in these courses relative to decisions youth will make in their near futures.

However, some associations appear when examining levels of liquid savings among young adults who are employed full time. This reflects likelihood to have discretionary income for saving, a necessary condition to begin accumulating assets.

This work poses broader questions about the "common thresholds" used in measuring adequate emergency savings. Analyses consistently revealed no differences in savings among emerging adults overall. This is unsurprising given the sample is predominately low-income with correspondingly little liquidity. Experts recommend 3-6 months of living expenses because the average unemployment period is 3 months. Realistically, this is difficult for anyone to meet, especially low- and moderate-income households. Future research may consider estimating how much of a cushion consumers genuinely need and the optimal amounts needed to successfully navigate a hardship. Poverty thresholds are well-established, but this has yet to be completed for savings thresholds.

Nevertheless, this study adds new insights that financial education policies in schools particularly affect propensities to save among emerging adults without any postsecondary credentials. Among them, on average, individuals who were exposed to financial education requirements were more likely to have any liquid accounts (marginally significant) and were more likely to have nonzero liquid savings. However, the findings on nonzero thresholds were unconditional on having an account, and subsequent analyses suggests that financial education policies in schools may impact liquid savings only on the extensive margin. This heterogeneous finding could be because this group is more financially independent during this life stage than their peers pursuing higher education and have

some financial resources to devote toward liquid savings. Additionally, this group might not have been exposed to more formal forms of financial education after high school, whereas their peers may be receiving more personal finance instruction in college.

This work has policy implications for structuring youth financial education curricula. This paper largely finds no statistically significant movements in propensities to save among young adults. This suggests that policymakers may consider consumer economic theories when constructing youth financial education curricula. This involves examining if the time allocation and emphases devoted to saving versus investing and retirement planning generates the greatest effects for the cost. Per the life-cycle hypothesis, young adults likely need to know about cash flow management. This is not to say that these courses should never cover savings; the life-cycle hypothesis assumes that credit markets are frictionless, which is especially not the case among young adults post-Credit Card Accountability Responsibility and Disclosure Act of 2009. Rather, policymakers may wish to shift curricula to focus more on liquid savings and cash flow management as opposed to longer-term savings. Liquid savings is a critical piece to successfully managing cash flow.

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Appendix A. Examples of States' Personal Finance Content Standards on Savings and Investments[24]

Alabama State Department of Education:

Saving and Investing

17. Distinguish differences between the purpose of saving and the objectives associated with investing.

a. Explain how using the principles of compound interest and the Rule of 72 in investing builds wealth to meet financial goals.

b. Evaluate various ways to buy and sell investments, including mutual funds, exchange-traded funds (ETFs), stocks, bonds, certificates of deposit (CDs), real estate, and commodities.

Banking and Financial Institutions

18. Analyze various types of financial institutions.

a. Evaluate services and related costs associated with financial institutions in terms of personal banking needs. Examples: checking and savings accounts, personal checks, cashier checks, overdraft fees

19. Demonstrate how to manage checking and savings accounts, balance bank statements, and use online financial services.

Colorado Department of Education:

Analyze strategic spending, saving, and investment options to achieve the objectives of diversification, liquidity, income, and growth (PFL)

Students can:

a. Compare and contrast the variety of investments available for a diversified portfolio (DOK 2-3)

b. Evaluate factors to consider when managing savings and investment accounts (DOK 2-3)

c. Explain how economic cycles affect personal financial decisions (DOK 1-3)

d. Describe the appropriate types of investments to achieve the objectives of liquidity, income and growth (DOK 1-2)

[24] These are copy and pasted verbatim from states' standards.

Georgia Department of Education:

SSEPF1 The student will apply rational decision making to personal spending and saving choices.

a. Explain that people respond to positive and negative incentives in predictable ways.

b. Use a rational decision making model to select one option over another.

c. Create a savings or financial investment plan for a future goal.

SSEPF2 The student will explain that banks and other financial institutions are businesses that channel funds from savers to investors.

a. Compare services offered by different financial institutions.

b. Explain reasons for the spread between interest charged and interest earned.

d. Evaluate a variety of savings and investment options; include stocks, bonds, and mutual funds.

Idaho State Department of Education:

Goal 3.4: Explain the concepts of personal finance.

Objective(s): By the end of Economics, the student will be able to:

9-12.E.3.4.1 Examine and apply the elements of responsible personal fiscal management, such as budgets, interest, investment, savings, credit, and debt.

Missouri Department of Elementary and Secondary Education:

MONEY MANAGEMENT

1. Explain how limited personal financial resources affect the choices people make.

2. Interpret the opportunity costs of financial decisions.

3. Evaluate the consequences of personal financial decisions.

4. Apply a decision-making process to personal financial choices.

7. Design a financial plan (budget) for earning, spending, saving, and investing.

SAVING AND INVESTING

1. Compare consumer choices for saving and investing.

2. Explain the relationship between saving and investing.

3. Examine reasons for saving and investing, e.g., time value of money.

Tennessee Department of Education:

Saving and Investing

16) Explain how saving and investing contribute to financial well-being, building wealth, and helping meet personal financial goals. Compare and contrast saving and investment strategies, such as savings accounts, certificates of deposit, stocks, bonds, mutual funds, employer sponsored savings plans, physical assets, and commodities. Design a diversified saving and investment plan that includes strategies compatible with personal goals. Include time value of money and compound interest calculations in analysis.

Texas Education Agency:

(17) Personal financial literacy. The student understands the role of financial markets/institutions in saving, borrowing, and capital formation. The student is expected to:

(A) explain the functions of financial institutions and how they affect households and businesses;

(B) explain how the amount of savings in an economy is the basis of capital formation;

(C) analyze the role of interest and risk in allocating savings to its most productive use; and

(D) examine the types of accounts available to consumers from financial institutions and the risks, monetary costs, and benefits of maintaining these accounts.

(18) Personal financial literacy. The student understands the role of individuals in financial markets. The student is expected to:

(A) assess ways to be a wise investor in the stock market and in other personal investment options;

(B) explain how to begin a savings program;

(C) examine investment options available in a personal retirement plan;

(D) demonstrate how to maintain a checking account, including reconciling a bank statement;

Utah State Board of Education:

Standard 3.1: Describe and discuss financial institutions, and demonstrate how to manage personal financial accounts.

3.1.2 Compare the roles of financial institutions and their services, such as banks, credit unions, investment or brokerage firms, insurance companies, and loan agencies.

3.1.3 Demonstrate how to manage checking/debit and saving accounts, both manually and/or electronically, including reconciliation.

Standard 3.2: Discuss the dynamics of saving and investing.

3.2.1 Explain how paying yourself first (PYF) early and often influences positive progress toward long-term financial goals.

3.2.2 Identify and understand basic saving options such as savings accounts and Certificates of Deposit. Understand that savings are designed to preserve principal.

3.2.3 Identify and understand investment options, including retirement planning, long- and short-term investments, and dividend re-investment plans. Understand that investments put principal at risk.

Standard 4.1: Identify and explain the process of budgeting based on calculated income.

4.1.1 Develop a budget.

4.1.2 Identify and prioritize fixed, variable, and periodic budget categories.

4.1.3 Emphasize the importance of proactive budget priorities, such as pay yourself first, emergency/opportunity fund, insurance, and charitable or other voluntary contributions.

Appendix B. Balance Test Results from LPM Regressions

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
IHS (Income)		0.000			0.000	
		(0.001)			(0.001)	
Nonwhite		0.001	0.000		0.001	-0.000
		(0.006)	(0.006)		(0.006)	(0.006)
Female		-0.004	-0.004		-0.004	-0.004
		(0.003)	(0.003)		(0.003)	(0.003)
Single		-0.007	-0.003		-0.007	-0.003
		(0.008)	(0.008)		(0.008)	(0.008)
Lives in Metro Area		-0.018**			-0.018**	
		(0.008)			(0.008)	
Employed		-0.002	0.000		-0.002	0.000
		(0.007)	(0.005)		(0.007)	(0.005)
Has Children		-0.002	-0.000		-0.002	-0.000
		(0.002)	(0.002)		(0.002)	(0.002)
Financially Dependent			-0.011			-0.011
			(0.007)			(0.007)
$<$ 1 Year College		-0.008	-0.008		-0.007	-0.007
		(0.010)	(0.010)		(0.010)	(0.010)
1+ Years College - No Degree		-0.002	-0.003		-0.003	-0.004
		(0.005)	(0.006)		(0.005)	(0.006)
Trade/Vocational						
Diploma/Cert		-0.005	-0.005		-0.004	-0.004
		(0.006)	(0.006)		(0.006)	(0.006)
Associate's		-0.011	-0.012		-0.010	-0.011
		(0.011)	(0.011)		(0.011)	(0.011)
Bachelor's		-0.008	-0.011		-0.007	-0.010
		(0.024)	(0.024)		(0.024)	(0.025)
Master's		0.084*	0.084		0.084*	0.083*
		(0.050)	(0.051)		(0.044)	(0.045)
Professional's Degree		-0.022	-0.013		-0.012	-0.003
		(0.019)	(0.019)		(0.012)	(0.014)
Student			0.001			0.000
			(0.005)			(0.005)
Constant	0.033***	0.059***	0.048***	-0.117***	-0.092**	-0.103**
	(0.010)	(0.018)	(0.017)	(0.040)	(0.040)	(0.041)
State		YES	YES		YES	YES
Panel Year		YES	YES		YES	YES
Age/Cohort		YES	YES		NO	NO
Grad Year		NO	NO		YES	YES

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
N	9,079	9,079	9,079	9,079	9,079	9,079
R^2	0.834	0.834	0.834	0.834	0.835	0.834
F-test		1.028	0.888		1.027	0.876
Prob > F		0.442	0.576		0.443	0.588

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. Coefficients from linear probability models are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Appendix C. Additional Regression Results

Table C1. Full Regression Results for Table 3, Effects of Mandates on Having Any Liquid Accounts from LPM Regressions

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Has Any Liquid Accounts				
Mandated	0.019	0.018	0.021	0.016	0.013
	(0.021)	(0.017)	(0.018)	(0.018)	(0.019)
IHS (Income)		0.024***	0.025***		
		(0.002)	(0.002)		
Nonwhite		-0.102***	-0.091***	-0.091***	-0.102***
		(0.011)	(0.011)	(0.011)	(0.011)
Female		0.022**	0.010	0.002	0.002
		(0.011)	(0.011)	(0.011)	(0.010)
Single		-0.022	-0.035*	0.010	0.008
		(0.020)	(0.020)	(0.020)	(0.019)
Lives in Metro Area		0.047***	0.038**		
		(0.017)	(0.016)		
Employed		0.048**	0.044**	0.224***	0.236***
		(0.020)	(0.019)	(0.012)	(0.012)
Has Children		-0.044***	-0.037***	-0.026***	
		(0.005)	(0.005)	(0.006)	
Has Any PSE			0.129***	0.127***	
			(0.010)	(0.010)	
Financially Dependent				-0.096***	-0.112***
				(0.011)	(0.011)
Student					0.139***
					(0.013)
Constant	0.574***	0.481***	0.417***	0.504***	0.442***
	(0.016)	(0.023)	(0.025)	(0.020)	(0.024)
State	YES	YES	YES	YES	YES
Panel Year	YES	YES	YES	YES	YES
Age/Cohort	YES	YES	YES	YES	YES
N	9,079	9,079	9,079	9,079	9,079
R^2	0.039	0.139	0.157	0.148	0.145

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. Coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table C2. Full Regression Results for Table 4, Effects of Mandates on Having Any Liquid Savings from LPM Regressions (Threshold: > $0)

	(1)	(2)	(3)	(4)	(5)
VARIABLES			Liquidity > $0		
Mandated	0.021	0.020	0.023*	0.018	0.015
	(0.014)	(0.013)	(0.014)	(0.014)	(0.014)
IHS (Income)		0.024***	0.025***		
		(0.002)	(0.002)		
Nonwhite		-0.113***	-0.099***	-0.100***	-0.111***
		(0.011)	(0.012)	(0.012)	(0.011)
Female		0.011	-0.002	-0.010	-0.010
		(0.011)	(0.012)	(0.011)	(0.011)
Single		-0.027	-0.042**	0.002	-0.002
		(0.020)	(0.021)	(0.022)	(0.020)
Lives in Metro Area		0.054***	0.043**		
		(0.018)	(0.017)		
Employed		0.042**	0.038**	0.219***	0.232***
		(0.018)	(0.017)	(0.012)	(0.012)
Has Children		-0.045***	-0.037***	-0.027***	
		(0.006)	(0.006)	(0.007)	
Has Any PSE			0.148***	0.146***	
			(0.011)	(0.011)	
Financially Dependent				-0.092***	-0.109***
				(0.012)	(0.012)
Student					0.159***
					(0.012)
Constant	0.528***	0.449***	0.376***	0.467***	0.401***
	(0.015)	(0.027)	(0.030)	(0.024)	(0.023)
State	YES	YES	YES	YES	YES
Panel Year	YES	YES	YES	YES	YES
Age/Cohort	YES	YES	YES	YES	YES
N	9,079	9,079	9,079	9,079	9,079
R^2	0.040	0.137	0.159	0.150	0.148

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. Coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table C4. Full Regression Results for Table 6, Impacts of Mandates on Having Any Liquid Accounts among Individuals with Highest Credential: Regular High School Diploma

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Has Any Liquid Accounts				
Mandated	0.075*	0.061*	0.055	0.055	0.055
	(0.040)	(0.033)	(0.036)	(0.035)	(0.035)
IHS (Income)		0.017***	0.019***		
		(0.003)	(0.002)		
Nonwhite		-0.133***	-0.132***	-0.132***	-0.134***
		(0.020)	(0.020)	(0.019)	(0.018)
Female		0.014	0.009	-0.004	-0.007
		(0.020)	(0.020)	(0.019)	(0.019)
Single		-0.077**	-0.093**	-0.055	-0.051
		(0.037)	(0.036)	(0.037)	(0.035)
Lives in Metro Area		0.061***	0.052**		
		(0.023)	(0.022)		
Employed		0.159***	0.155***	0.293***	0.295***
		(0.030)	(0.028)	(0.021)	(0.021)
Has Children		-0.018**	-0.015**	-0.008	
		(0.007)	(0.007)	(0.008)	
Financially Dependent				-0.066***	-0.071***
				(0.019)	(0.017)
Student			0.166***	0.161***	0.162***
			(0.031)	(0.030)	(0.030)
Constant	0.493***	0.382***	0.265***	0.347***	0.337***
	(0.022)	(0.041)	(0.044)	(0.041)	(0.039)
State	YES	YES	YES	YES	YES
Panel Year	YES	YES	YES	YES	YES
Age/Cohort	YES	YES	YES	YES	YES
N	3,157	3,157	3,157	3,157	3,157
R^2	0.040	0.148	0.167	0.161	0.161

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. Coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table C5. Full Regression Results for Table 7, Effects of Mandates on Having Any Liquid Savings among Individuals with Highest Credential: Regular High School Diploma (Threshold: > $0)

	(1)	(2)	(3)	(4)	(5)
VARIABLES			Liquidity > $0		
Mandated	0.071**	0.058**	0.052*	0.053**	0.053**
	(0.031)	(0.025)	(0.027)	(0.026)	(0.026)
IHS (Income)		0.017***	0.019***		
		(0.003)	(0.003)		
Nonwhite		-0.122***	-0.122***	-0.123***	-0.124***
		(0.018)	(0.017)	(0.017)	(0.016)
Female		0.005	0.000	-0.012	-0.013
		(0.021)	(0.021)	(0.020)	(0.020)
Single		-0.076**	-0.093**	-0.059	-0.056
		(0.037)	(0.036)	(0.037)	(0.035)
Lives in Metro Area		0.056**	0.046*		
		(0.024)	(0.023)		
Employed		0.148***	0.144***	0.283***	0.284***
		(0.028)	(0.026)	(0.019)	(0.019)
Has Children		-0.015*	-0.012	-0.005	
		(0.009)	(0.009)	(0.009)	
Financially Dependent				-0.058***	-0.061***
				(0.019)	(0.019)
Student			0.183***	0.177***	0.178***
			(0.028)	(0.028)	(0.028)
Constant	0.430***	0.324***	0.196***	0.272***	0.266***
	(0.024)	(0.043)	(0.051)	(0.048)	(0.042)
State	YES	YES	YES	YES	YES
Panel Year	YES	YES	YES	YES	YES
Age/Cohort	YES	YES	YES	YES	YES
N	3,157	3,157	3,157	3,157	3,157
R^2	0.042	0.137	0.160	0.153	0.153

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. Coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table C6. Full Regression Results for Table 8, Mandates' Impact on Total Liquid Savings at Intensive vs Extensive Margins, by Conditionality on Having Any Liquid Accounts

	Unconditional				Conditional			
	Tobit		OLS		Tobit		OLS	
VARIABLES	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Mandated	0.816**	0.661**	0.494*	0.400*	0.174	0.146	0.166	0.138
	(0.395)	(0.336)	(0.264)	(0.217)	(0.230)	(0.230)	(0.223)	(0.222)
IHS (Income)		0.250***		0.141***		0.069**		0.067**
		(0.040)		(0.023)		(0.028)		(0.027)
Nonwhite		-1.548***		-0.922***		-0.153		-0.157
		(0.219)		(0.137)		(0.142)		(0.136)
Female		-0.145		-0.152		-0.416***		-0.403***
		(0.271)		(0.169)		(0.114)		(0.111)
Single		-0.846*		-0.465*		0.001		-0.000
		(0.437)		(0.274)		(0.229)		(0.219)
Lives in Metro Area		0.848***		0.511***		0.219		0.217
		(0.289)		(0.168)		(0.135)		(0.132)
Employed		1.504***		0.798***		-0.337		-0.328
		(0.362)		(0.221)		(0.205)		(0.201)
Has Children		-0.283**		-0.194***		-0.154**		-0.153**
		(0.114)		(0.067)		(0.062)		(0.059)
N	3,157	3,157	3,157	3,157	1,975	1,975	1,975	1,975
R^2	—	—	0.050	0.144	—	—	0.051	0.071

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. Coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table C6.2. Full Regression Results for Table 8, Mandates' Impact on Total Liquid Savings at Intensive vs Extensive Margins, by Conditionality on Having Any Liquid Accounts among Full Time Employed

	Unconditional				Conditional			
	Tobit		OLS		Tobit		OLS	
VARIABLES	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Mandated	1.512***	1.236**	0.831**	0.712**	0.381	0.338	0.349	0.309
	(0.553)	(0.517)	(0.318)	(0.275)	(0.318)	(0.310)	(0.303)	(0.295)
IHS (Income)		0.244***		0.134***		0.085***		0.082**
		(0.043)		(0.023)		(0.032)		(0.031)
Nonwhite		-1.256***		-0.653***		0.121		0.111
		(0.262)		(0.162)		(0.163)		(0.158)
Female		-0.188		-0.190		-0.500***		-0.484***
		(0.320)		(0.181)		(0.129)		(0.125)
Single		-0.979**		-0.529*		-0.082		-0.075
		(0.454)		(0.284)		(0.269)		(0.264)
Lives in Metro Area		1.220***		0.699***		0.312*		0.309*
		(0.354)		(0.191)		(0.175)		(0.171)
Employed		1.809***		0.983***		-0.503*		-0.477*
		(0.414)		(0.244)		(0.269)		(0.262)
Has Children		-0.278*		-0.174**		-0.118		-0.117
		(0.145)		(0.082)		(0.078)		(0.075)
N	2,107	2,107	2,107	2,107	1,250	1,250	1,250	1,250
R^2	—	—	0.056	0.164	—	—	0.056	0.081

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. Coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table C7. Full Regression Results for Table 10, Mandates' Impact on Savings at Extensive vs Intensive Margins from Two-Part Models, by Conditionality on Having Any Liquid Accounts

	(1)	(2)	(1)	(2)	(1)	(2)
VARIABLES	Has Any Liquid Account		Liquidity > $0		IHS (Total Liquid Savings)	
Mandated	0.075*	0.061*	0.008	0.008	0.166	0.138
	(0.040)	(0.033)	(0.025)	(0.025)	(0.223)	(0.222)
IHS (Income)		0.017***		0.002		0.067**
		(0.003)		(0.002)		(0.027)
Nonwhite		-0.133***		0.005		-0.157
		(0.020)		(0.011)		(0.136)
Female		0.014		-0.015*		-0.403***
		(0.020)		(0.008)		(0.111)
Single		-0.077**		0.001		-0.000
		(0.037)		(0.021)		(0.219)
Lives in Metro Area		0.061***		0.002		0.217
		(0.023)		(0.012)		(0.132)
Employed		0.159***		-0.008		-0.328
		(0.030)		(0.016)		(0.201)
Has Children		-0.018**		-0.001		-0.153**
		(0.007)		(0.005)		(0.059)
N	3,157	3,146	1,975	1,975	1,975	1,975
R^2	0.078	0.187	0.030	0.031	0.051	0.071

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. Coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table C7.2. Full Regression Results for Table 10, Mandates' Impact on Savings at Extensive vs Intensive Margins from Two-Part Models, by Conditionality on Having Any Liquid Accounts among Full Time Employed

	(1)	(2)	(1)	(2)	(1)	(2)
VARIABLES	Has Any Liquid Account		Liquidity > $0		IHS (Total Liquid Savings)	
Mandated	0.118**	0.097**	0.030	0.029	0.349	0.309
	(0.049)	(0.044)	(0.035)	(0.035)	(0.303)	(0.295)
IHS (Income)		0.015***		0.003		0.082**
		(0.003)		(0.003)		(0.031)
Nonwhite		-0.116***		0.013		0.111
		(0.023)		(0.014)		(0.158)
Female		0.009		-0.018		-0.484***
		(0.024)		(0.013)		(0.125)
Single		-0.092**		-0.008		-0.075
		(0.039)		(0.021)		(0.264)
Lives in Metro Area		0.082***		0.004		0.309*
		(0.028)		(0.018)		(0.171)
Employed		0.189***		-0.029		-0.477*
		(0.029)		(0.026)		(0.262)
Has Children		-0.020**		-0.001		-0.117
		(0.009)		(0.007)		(0.075)
N	2,107	2,107	1,250	1,250	1,250	1,250
R^2	0.051	0.174	0.048	0.051	0.056	0.081

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. Coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Appendix D. Additional Regression Results

Table D1. Sensitivity Analyses on Study Results, by Percentile Cut-Offs

Subsample	All				Non-PSE Subsample			
Percentile	90th	95th	99th	100th	90th	95th	99th	100th
VARIABLES	**Panel A: Has Any Liquid Accounts**							
Mandated	0.019	0.019	0.017	0.017	0.077**	0.075*	0.074*	0.076*
	(0.022)	(0.021)	(0.018)	(0.018)	(0.038)	(0.040)	(0.039)	(0.039)
	[-0.026 - 0.064]	[-0.022 - 0.061]	[-0.020 - 0.053]	[-0.019 - 0.054]	[0.000 - 0.154]	[-0.005 - 0.155]	[-0.004 - 0.153]	[-0.003 - 0.154]
Constant	0.571***	0.574***	0.581***	0.577***	0.491***	0.493***	0.490***	0.478***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.021)	(0.022)	(0.021)	(0.021)
R^2	0.037	0.039	0.042	0.043	0.038	0.040	0.040	0.041
	Panel B: Liquidity > 0							
Mandated	0.020	0.021	0.018	0.017	0.072**	0.071**	0.071**	0.067**
	(0.017)	(0.014)	(0.014)	(0.014)	(0.030)	(0.031)	(0.032)	(0.032)
	[-0.014 - 0.054]	[-0.008 - 0.050]	[-0.010 - 0.046]	[-0.011 - 0.045]	[0.012 - 0.133]	[0.010 - 0.133]	[0.007 - 0.134]	[0.002 - 0.131]
Constant	0.524***	0.528***	0.536***	0.533***	0.425***	0.430***	0.425***	0.415***
	(0.015)	(0.015)	(0.015)	(0.015)	(0.025)	(0.024)	(0.024)	(0.024)
R^2	0.038	0.040	0.042	0.043	0.038	0.042	0.041	0.042
	Panel C: IHS (Liquidity)							
Mandated	0.086	0.111	0.109	0.095	0.470*	0.494*	0.593**	0.604**
	(0.156)	(0.132)	(0.130)	(0.120)	(0.253)	(0.264)	(0.280)	(0.276)
	[-0.228 - 0.399]	[-0.154 - 0.377]	[-0.152 - 0.371]	[-0.146 - 0.335]	[-0.038 - 0.978]	[-0.036 - 1.024]	[0.031 - 1.154]	[0.049 - 1.158]
Constant	3.549***	3.602***	3.669***	3.636***	2.650***	2.674***	2.580***	2.523***
	(0.110)	(0.115)	(0.117)	(0.117)	(0.186)	(0.199)	(0.202)	(0.203)
R^2	0.046	0.050	0.057	0.059	0.042	0.050	0.049	0.049
N	8,284	9,079	9,799	9,993	2,930	3,157	3,327	3,374

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Robust 95% confidence intervals are reported in brackets. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. Coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table D2. Full Regression Results for Table 13, Mandates' Impact on Moving out of State of Birth by Subsample

	(1)	(2)	(1)	(2)
Subsample	All		Non-PSE Subsample	
Mandated	-0.043	-0.041	-0.017	-0.015
	(0.028)	(0.028)	(0.040)	(0.039)
Nonwhite		-0.006		-0.010
		(0.020)		(0.024)
Female		0.003		-0.002
		(0.009)		(0.016)
Single		-0.079***		-0.102***
		(0.018)		(0.038)
Has Children		-0.015***		-0.012
		(0.005)		(0.007)
IHS (Income)		0.001		0.001
		(0.002)		(0.003)
Lives in Metro Area		0.065***		0.066***
		(0.018)		(0.024)
Employed		-0.016		-0.024
		(0.021)		(0.033)
Constant	0.275***	0.317***	0.249***	0.324***
	(0.014)	(0.026)	(0.017)	(0.040)
N	8,141	8,141	2,844	2,844
R^2	0.063	0.069	0.078	0.087

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. LPM coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table D3. Event Study Specification Results

	(1)	(2)	(3)	(4)	(5)	(1a)	(2a)	(3a)	(4a)	(5a)
	All					All (Set Post-2018 States to Zero for All Values)				
VARIABLES	Has Liquid Account	Liquidity $>$ \$0	IHS (Liquidity)	Has CC Debt	IHS (CC Debt)	Has Liquid Account	Liquidity $>$ \$0	IHS (Liquidity)	Has CC Debt	IHS (CC Debt)
7+ Years Pre	0.038	0.010	0.130	0.066*	0.492*	0.010	-0.006	-0.159	0.026	0.195
	(0.039)	(0.041)	(0.339)	(0.036)	(0.267)	(0.047)	(0.045)	(0.385)	(0.030)	(0.231)
5-6 Years Pre	0.012	0.002	-0.069	0.049	0.349	0.002	-0.004	-0.150	0.044	0.289
	(0.029)	(0.031)	(0.250)	(0.030)	(0.230)	(0.031)	(0.032)	(0.277)	(0.033)	(0.261)
4-3 Years Pre	-0.034	-0.055**	-0.526***	0.071***	0.536***	-0.039	-0.060**	-0.568***	0.075***	0.583***
	(0.024)	(0.024)	(0.194)	(0.026)	(0.181)	(0.024)	(0.024)	(0.198)	(0.026)	(0.185)
2-1 Years Pre	-0.004	-0.008	-0.066	0.054**	0.418**	-0.008	-0.010	-0.100	0.050*	0.385**
	(0.028)	(0.026)	(0.197)	(0.025)	(0.177)	(0.028)	(0.026)	(0.199)	(0.025)	(0.177)
1-2 Years Post	0.001	-0.004	-0.192	0.016	0.141	0.005	-0.001	-0.158	0.017	0.154
	(0.029)	(0.031)	(0.224)	(0.022)	(0.176)	(0.029)	(0.030)	(0.224)	(0.022)	(0.176)
3-4 Years Post	0.003	-0.009	-0.233	-0.010	0.003	0.010	-0.003	-0.160	-0.006	0.037
	(0.034)	(0.037)	(0.294)	(0.018)	(0.138)	(0.034)	(0.037)	(0.295)	(0.018)	(0.138)
5-6 Years Post	-0.012	0.012	-0.033	-0.023	-0.089	0.000	0.021	0.080	-0.017	-0.037
	(0.035)	(0.038)	(0.307)	(0.020)	(0.165)	(0.036)	(0.038)	(0.312)	(0.019)	(0.163)
7+ Years Post	-0.000	0.009	-0.093	-0.032	-0.149	0.018	0.022	0.082	-0.022	-0.071
	(0.037)	(0.039)	(0.352)	(0.027)	(0.218)	(0.039)	(0.038)	(0.365)	(0.026)	(0.211)
Constant	0.566***	0.537***	3.704***	-0.051*	-0.481**	0.577***	0.542***	3.806***	-0.037	-0.367*
	(0.032)	(0.032)	(0.266)	(0.028)	(0.208)	(0.035)	(0.033)	(0.284)	(0.028)	(0.196)
N	9,079	9,079	9,079	9,079	9,079	9,079	9,079	9,079	9,079	9,079
R^2	0.04	0.041	0.051	0.059	0.06	0.04	0.04	0.051	0.059	0.06
F-Test: Pre-Mandate	6.507	12.512	6.195	1.992	2.364	3.677	7.130	3.987	2.581	3.328
Prob $>$ F	0.000	0.000	0.000	0.110	0.066	0.011	0.000	0.007	0.048	0.017
Post - Pre	-0.021	0.059	-0.019	-0.289***	-1.889**	0.068	0.118	0.821	-0.223**	-1.369*
SE	(0.136)	(0.132)	(1.089)	(0.112)	(0.795)	(0.15)	(0.126)	(1.194)	(0.106)	(0.754)

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. LPM coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Appendix E. Robustness Checks using FDIC and NFCS Data

Survey of Income and Program Participation (SIPP 2014-2018) (Administered quadrennially)

SIPP captures liquid account ownership on a per account basis:

- Owned any [regular (non-interest-earning)] checking accounts during the reference period.
- Owned any certificates of deposit during the reference period.
- Owned any money market deposit accounts or money market funds during the reference period.
- Owned any savings accounts during the reference period.
- Owned any interest-earning checking accounts during the reference period. (pre-2018 only)

All analyses exclude interest-earning accounts since this is no longer collected as of 2018. Levels of liquid savings are captured for each liquid account type as of SIPP 2014. In prior panels, these levels were combined into one variable.

National Financial Capability Study (NFCS 2012-2018) (Administered triennially)

NFCS captures liquid account ownership in two questions:

> B1) Do you/Does your household have a checking account?
>
> B2) Do you/Does your household have a savings account, money market account, or CDs?

Levels of liquid savings are not captured, although the closest variables to proxying holding any liquidity are captured in the following questions:

> J41.3) I am concerned that the money I have or will save won't last. (2018 only)
>
> J5) Have you set aside emergency or rainy day funds that would cover your expenses for 3 months, in case of sickness, job loss, economic downturn, or other emergencies?

FDIC Survey of Household Use of Banking and Financial Services (formerly National Survey of Unbanked and Underbanked Households) (FDIC 2009-2019) (Administered biannually)

FDIC captures liquid account ownership as follows:

> 2) Do you [or anyone else in your household] have a checking or savings account now?
>
> 4) Have you [or anyone else in your household] had a checking or savings account in the past 12 months, that is since June 20YY?

In the case of the FDIC, I use the banked measure in Question 2 since the SIPP only captures banking status as of the end of the reference period (their equivalent of "now").

Levels of liquid savings are not captured, although the closest variables to proxying holding any liquidity are captured in the following questions:

> 170) Even if you later spent it, did you [or anyone in your household] set aside any money in the past 12 months that could be used for unexpected expenses or emergencies? I'm only asking about funds that could be easily spent if necessary, and am not asking about retirement or other long-term savings.
>
> 171) Where did you keep this money? [IF 170 = Yes] [*Plausible options for study purposes*: checking account; savings account; in other accounts such as CDs, brokerage accounts, or savings bonds]

In either case, I avoid proxies because asking if one saved for a specific purpose is inequivalent to asking how much one has in liquid accounts.

Table E1. Regression Results for Table 14, Effects of Mandates on Having Any Liquid Accounts from LPM Regressions, by Dataset

Dataset	SIPP	SIPP	FDIC[1]	FDIC[1]	FDIC	FDIC	NFCS	NFCS
Mandated	0.019	0.018	0.026	0.034**	0.033***	0.030***	0.011	0.004
	(0.021)	(0.020)	(0.017)	(0.015)	(0.009)	(0.011)	(0.017)	(0.016)
	[-0.022 - 0.061]	[-0.021 - 0.058]	[-0.007 - 0.060]	[0.003 - 0.065]	[0.015 - 0.050]	[0.009 - 0.052]	[-0.023 - 0.044]	[-0.029 - 0.037]
Demographics	NO	YES	NO	YES	NO	YES	NO	YES
Constant	0.574***	0.421***	0.852***	0.881***	0.857***	0.886***	0.780***	0.708***
	(0.016)	(0.024)	(0.025)	(0.034)	(0.019)	(0.027)	(0.013)	(0.022)
Unconditional Mean	0.74	0.74	0.94	0.94	0.87	0.87	0.9	0.9
N	9,079	9,079	4,049	4,049	12,383	10,080	6,927	6,927
R^2	0.039	0.159	0.028	0.135	0.023	0.116	0.023	0.075

NOTES: Clustered standard errors are reported in parentheses, accounting for 50 states and the District of Columbia. Robust 95% confidence intervals are reported in brackets. Each column denotes a separate regression. All regressions include fixed effects for panel year, age-specific, and state of earliest known residence. [1] Denotes results when restricted to 2013 and 2017 waves of FDIC. LPM coefficients are reported. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$