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FINANCIAL LITERACY AND FINANCIAL EDUCATION: AN OVERVIEW

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ABSTRACT

This article provides a concise narrative overview of the rapidly growing empirical literature on financial literacy and financial education. We first discuss stylized facts on the demographic correlates of financial literacy. We next cover the evidence on the effects of financial literacy on financial behaviors and outcomes. Finally, we review the evidence on the causal effects of financial education programs focusing on randomized controlled trial evaluations. The article concludes with perspectives on future research priorities for both financial literacy and financial education.

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1 Introduction

Globalization and the increasing complexity of the economic landscape have placed *financial literacy* and *financial education* on policymakers' agendas. Globally, individuals and households face a wide array of financial products and options, making the understanding of basic financial concepts increasingly important. The increase in inflation further underlines the importance of financial literacy in navigating complex financial markets. With the advent of new technologies, such as digital transactions, online banking, and crypto assets, financial literacy is also critical for scam avoidance and wise money management. Additionally, the shift away from traditional pension schemes and toward individualized retirement accounts in many countries places additional responsibility on individuals to secure their future.

In the U.S., the last decade has also seen a sharp rise in student loan debt, emphasizing the need for young people to comprehend loan terms and repayment options. Additionally, increasing healthcare costs reinforce the need for sound financial planning and a comprehensive understanding of insurance policies. Hence, fostering financial literacy (i.e., domain-specific human capital related to personal finance) through financial education is expected to empower individuals to make informed decisions and improve their financial well-being in the US and around the world.

A large and growing body of literature documents the importance of financial literacy and financial education. Over 7,000 publications in peer-reviewed academic journals have been indexed in the Social Science Citation Index (Clarivate Analytics) over the past fifteen years. In 2022 alone, there have been over 1,300 publications attracting more than twenty thousand citations. Research on financial literacy is now conducted in many countries and in a variety of disciplines. Moreover, *financial literacy* has become an established field of study in the academic economics profession, with its own *Journal of Economic Literature* code (G53).

This paper provides a concise overview of the large body of financial literacy and financial education literature. A narrative literature review of this type must be selective and limited in scope: As we cannot cover all available studies, we present the reader with a curated discussion of selected high-impact papers in this field. As such, this review complements the existing literature reviews and discussion articles on this topic (e.g., Xu and Zia 2012; Hastings et al. 2013; Lusardi and Mitchell 2014, 2023; Zia 2023) as well as the quantitative meta-analyses in this field (e.g., Fernandes et al. 2014; Miller et al. 2015; Kaiser and Menkhoff 2017, 2020; Kaiser et al. 2022). We intend to give the reader a concise summary of the state of the empirical evidence and to highlight gaps and questions worthy of future research. Using the most recent wave of the National Financial Capability Study (NFCS), a large-scale survey of over 25,000 adult U.S. respondents, we discuss some descriptive statistics that help set the stage and highlight areas for further work.

This paper has four sections: First, we document some stylized facts on the demographic correlates of financial literacy. Second, we summarize the empirical evidence on the relationship among financial literacy, financial behavior, and outcomes. Third, we summarize the evidence on the causal effects of financial education programs in impact evaluation studies. We conclude by discussing research priorities and topics to be covered in future research, highlighting areas for further exploration.

2 Measurement and demographic correlates of financial literacy

Measurement of financial literacy. Stylized facts on the level of financial literacy and its heterogeneity are based on large-scale and representative household surveys conducted in many countries (see Lusardi and Mitchell 2011a, 2014, 2023). Most of the empirical evidence is based on a short but informative three-item measure of financial literacy, i.e., what has

become known as the "Big Three."[1] These questions measure understanding of basic financial concepts (i.e., compound interest, inflation, and risk diversification) and have been mainly designed with the aim of minimal response burden as well as adequate reliability and discrimination (see Lusardi and Mitchell 2014, 2023). They were also designed to embody broad rather than context-specific concepts, allowing researchers to administer these items in numerous countries. Recent work has demonstrated the sound psychometric properties of this measurement scale, including evidence on construct validity, temporal stability, and predictive validity of the test items (see Angrisani et al. 2023; Kaiser et al. 2023a). The rapid development of financial literacy as a field of study can be largely explained by the ability to measure basic financial literacy levels with a small number of survey questions (Lusardi and Mitchell, 2023).

The following discussion of the stylized facts is based mainly on empirical evidence relying on the "Big Three." However, we will also discuss literature relying on more extensive measures of financial literacy.

Low levels of financial literacy around the world. The empirical evidence shows that financial literacy cannot be taken for granted, even in countries with well-developed financial markets or the G7 countries. Overall, only half of the population or even less, in most countries, is knowledgeable about the basic concepts covered in the "Big Three."

The gender gap in financial literacy. One of the most striking empirical regularities is the gender gap in financial literacy favoring men, i.e., women perform worse than men on financial literacy assessments (Lusardi and Mitchell, 2011a, 2014). This gender gap has been replicated on every continent and in as many as 135 of the 144 countries covered in the S&P Ratings Service Global Financial Literacy survey (Global Finlit Survey) (Grohmann 2016;

[1] See Hastings, Madrian, and Skimmyhorn (2013). In addition to these well-established and widely used items, there have been numerous other measurement scales targeted to different audiences: The National Financial Capability Study (NFCS) administers a longer battery of questions, also known as the Big Five. A global survey on financial literacy has been administered across more than 140 countries (Klapper and Lusardi, 2020). Similarly, the PISA financial literacy assessment tests domain-specific problem-solving abilities among 15-year-old students and relies on both multiple-choice and open-ended items to measure this latent trait (Lusardi, 2015). Additionally, many impact evaluation studies also rely on test instruments designed to cover specific curricula.

Klapper and Lusardi 2020). Even after controlling for differences in education, income, and other demographic characteristics, a substantial share of the gender gap remains unexplained (Fonseca et al. 2012; Grohmann 2016). Gender differences are already present among the young (Lusardi, Mitchell, and Curto, 2010), and after adjusting for gender-specific heterogeneity in test-taking effort, the gender gap in financial literacy can also be observed among 15-year-old students (Oberrauch and Kaiser 2023).

While the gender gap can be considered a stylized fact in the literature, its roots are less well understood. Among candidates are the effects of parental inputs (Bottazzi and Lusardi 2020), intra-household dynamics (Fonseca et al. 2012) as well as cultural characteristics and prevalent stereotypes (Bottazzi and Lusardi 2020; Driva et al. 2016; Tinghög et al. 2021). Another possible explanation is that women may be less confident in answering financial literacy questions, as exemplified by their much higher propensity to answer 'I do not know,' particularly when faced with complex questions (Bucher-Koenen et al. 2021). More research is needed on the reasons for these pervasive gender differences.

Age and financial literacy. Many empirical studies document an inverse u-shape relationship between age and financial literacy, i.e., financial literacy increases with age (at a decreasing rate) up to a point and then decreases (Lusardi and Mitchell 2011a; Finke et al. 2017). This fact is consistent with theoretical models endogenizing financial literacy as a form of investment in human capital, with benefits but also costs of doing so (Lusardi et al. 2017). The decline in older ages reflects the effects of less investment in financial literacy and the depreciation of knowledge.

Cognitive ability and education. Naturally, one would expect specific human capital (such as financial literacy) to be correlated with broader human capital and educational attainment (Lusardi and Mitchell 2014, 2023). Based on data from the U.S., financial literacy is especially low for those without a college degree (Lusardi and Mitchell 2011b, 2023).

Obviously, a positive correlation between education and financial literacy does not imply a causal effect of education attainment on financial literacy, as both could be a function of general cognitive ability (Callis et al. 2023). An extensive literature has studied the effects of general cognitive ability and education on financial decision-making (e.g., Christelis et al. 2010; Agarwal and Mazumder 2013; Cole et al. 2014). Overall, it seems advisable to adjust for differences in general cognitive ability. However, while studies have shown that general cognitive ability and numeracy are correlated, much of the heterogeneity in financial literacy remains unexplained (Lusardi et al. 2010).

Replicating demographic correlates in new data. We study these demographic correlates in the most recent (2021) wave of the National Financial Capability Study (NFCS) by the Financial Industry Regulatory Authority (FINRA) Investor Education Foundation. The NFCS is a large-scale data collection effort, and each wave includes data from more than 25,000 adults across all 50 U.S. states and Washington D.C. Like many other studies, we use the "Big Three" to study the correlates of financial literacy in this representative sample of U.S. adults. Table 1 reports the relevant descriptive statistics.

< Table 1 about here >

Fifty-four percent of respondents are female, 26 percent are adults belonging to minorities, and 53.6 percent report no college degree. Only about 14 percent report having received financial education in school. Other descriptive statistics regarding income, age, and marital status reported in Table 1 follow what one would expect from a representative US household survey. We now turn to studying the demographic correlates of financial literacy scores ((see Figure 1).

< Figure 1 about here >

Figure 1 shows unstandardized regression coefficients from a model regressing financial literacy (standardized to have a mean of zero and a standard deviation of one) on the

demographic correlates listed in Table 1. As documented in the previous literature, the most recent wave of the NFCS continues to document a gender gap favoring men in the order of magnitude of 0.3 SD units. Additionally, minorities score about 0.19 SD units lower than Whites in financial literacy, and those without a college degree score about 0.23 units lower than those with a college education. There is some evidence that married individuals score slightly lower than those in informal relationships or living alone, that retirees score slightly higher than those working, and that individuals who are less risk averse score higher. There also is a strong positive relationship between age and financial literacy and income and financial literacy, respectively.

Intersectionality. Given that we have such a rich and large data set, we can study in more detail the evidence for intersectionality, i.e., interaction effects between the demographic variables (tables available on request). Specifically, we do not observe varying patterns in the relationship between income levels and financial literacy when comparing genders. However, we did notice that, for minority groups and individuals without a college degree, the disparities in financial literacy become less marked at higher income levels (above $100,000). These interaction effects are statistically significant at the 10-percent level, but they do not follow a clear pattern across different income brackets and sometimes do not appear at all.

In terms of age-related differences, the gender gap in financial literacy widens with age. For working-age adults, the financial literacy gap between white individuals and minorities is narrower. Additionally, the difference in financial literacy between those with and without a college degree is most noticeable among older people (aged 65 and above). When examining the combined effects of gender with minority status and education, we found no intersectional effects between gender and minority status. However, there is a moderately strong interaction between gender and lack of college education—suggesting that women without a college degree experience a more significant gender gap in financial literacy. When examining the two-

way interaction effects of gender with minority status and education, we found no interaction effects between gender and minority status but a moderate interaction between gender and lack of college education, suggesting a more pronounced gender gap among those without a college degree.

3 The effects of financial literacy on financial behaviors and outcomes

In this section, we highlight selected research work on the effects of financial literacy on household financial behaviors and outcomes. Identifying the causal effects of financial literacy on financial behaviors, such as retirement saving or stock market participation, in observational data presents several challenges. The main concern is that the association between financial literacy and financial behavior might not reflect a causal relationship due to several potential issues, including endogeneity and selection bias. Endogeneity arises when financial literacy scores are correlated with the regression model's error term. This could happen if there are variables that affect both financial literacy and financial behaviors but are not included in the model. For instance, inherent characteristics such as cognitive ability, preferences, personal motivation, confidence, perceived skills, and interest in financial matters may influence a person's financial literacy and financial behaviors, but these characteristics are rarely present as variables in existing data sets (e.g., Allgood and Walstad 2016; Andersen et al. 2018). Endogeneity can also result from reverse causality, where the dependent variable (financial behavior) influences the explanatory variable (financial literacy). For instance, it might be that participating in the stock market or starting to save for retirement improves one's financial literacy rather than the other way around.

Using instrumental variables (IVs) estimation is a common strategy to address endogeneity (see, e.g., Angrist and Krueger 2001). An instrumental variable is a variable that is assumed to be correlated with the explanatory variable (financial literacy) but uncorrelated

with the error term in the regression model. In practice, finding a suitable instrument (or instruments) for financial literacy is challenging and requires strong assumptions: the instrument must be correlated with financial literacy but must not be correlated with the error term in the model, meaning it should not directly influence the financial behavior, except through its impact on financial literacy (i.e., the exclusion restriction). Furthermore, the interpretation of IV estimates can sometimes be difficult. In the presence of heterogeneous effects, IV estimation identifies the effect for the subpopulation of individuals whose financial literacy is influenced by the instrument. This effect may be different from the effect for the whole population, which is often what researchers and policymakers may be most interested in (see Mogstad and Torgovitsky 2018 for an excellent overview of this discussion).

Despite the strong assumptions and more nuanced interpretation required, several studies have implemented instrumental variables strategies and found compelling evidence that financial literacy has a causal effect on financial behaviors and outcomes. One obvious strategy is to rely on past (non-voluntary) exposure to mandated financial education in school or the workplace. Several studies used this type of plausibly exogenous instrument (sometimes combined with other instrumental variables) (see Fernandes et al. 2014). Other examples include the financial financial situation of the oldest sibling in comparison to the financial situation of the respondent (van Rooij et al. 2011), bank information policies (Fort et al. 2016), education policies, macroeconomic conditions, and family background (Behrman et al. 2012), as well as the cost of learning and acquiring financial knowledge (proxied by an economics degree of parents) (Fornero and Monticone 2011). Recently, studies have combined instruments such as exposure to economics education and the financial situation of the oldest sibling with heteroskedasticity-based identification, making it possible to test overidentifying restrictions in settings where researchers previously may only have included a single instrumental variable (Deuflhardt 2018).

While many of these instruments pass relevance tests (and show adequate Hansen J statistics in many empirical applications), the exclusion restriction remains a strong assumption in some cases. Thus, while we deem much of this evidence to be credibly causal, we also encourage readers to exercise some caution in interpreting the findings based on observational data. Below, we describe some of the most cited findings.

Retirement planning, (retirement) savings, and investment behavior. One of the canonical findings in many countries (both advanced and emerging economies) is that financial literacy affects retirement planning and (retirement) savings both at the extensive and intensive margin (e.g., Bernheim and Garret 2003; Lusardi and Mitchell 2007a,b, 2008, 2011b; Alessie et al. 2011; Almenberg and Säve-Söderbergh 2011; Bucher-Koenen and Lusardi 2011; Cole et al. 2011; Fornero and Monticone 2011; van Rooij et al. 2012; Boisclair et al. 2017; Clark et al. 2017).

People with higher financial literacy have more wealth not just because they are able to plan and save more but also because they get better returns on their savings, even via basic financial instruments. For example, Deuflhardt et al. (2018) study the effect of financial literacy on savings account returns. They find that a one-standard-deviation increase in financial literacy scores is associated with an increase in the interest rate on the account due to greater usage of online bank accounts offering more favorable conditions.

Similarly, there is robust evidence that financial literacy is positively associated with stock market participation (van Rooij et al. 2011; Almenberg and Dreber 2015; Clark et al. 2017), portfolio diversification, and portfolio returns (Bianchi 2018; von Gaudecker 2015). Additionally, the effects of financial literacy on retirement saving behavior do not seem limited to the individual but also generate positive externalities (Haliassos et al. 2019).

Financial literacy and debt behavior. While there are many studies assessing the effects of financial literacy on assets and wealth, less attention has been paid to the effects on

household debt. Exceptions include Lusardi and Tufano (2015) and Lusardi, Mitchell, and Oggero (2020), which find that individuals with limited financial literacy face higher costs of borrowing, report concerns about excessive debt, or have difficulty assessing their debt situations and carry debt into retirement. Similarly, Disney and Gathergood (2013) and Klapper et al. (2013) find a positive correlation between financial literacy and the cost of borrowing. Gathergood (2012) finds that financial literacy and self-control measures are correlated with consumer over-indebtedness. Relatedly, Gerardi et al. (2013) find that numerical ability (an aspect closely related to financial literacy) is predictive of mortgage default.

Replicating these patterns in new data. As for the demographic correlates of financial literacy, we now test these empirical regularities in the most recent wave of the NFCS.

< Table 2 about here >

This dataset contains rich data on financial behaviors, and we show regression results on the financial outcomes considered in many studies, including "retirement planning," i.e., whether the respondent has ever tried to figure out how much they need to save for retirement; financial fragility, i.e., how confident a respondent is that they could come up with $2,000 in a month; "credit record," i.e., how the respondent would rate their credit record on a scale from 1 (very bad) to 5 (very good); and subjective debt assessment, i.e., whether the respondent agrees with the statement "I have too much debt right now" on a scale from 1 (strongly disagree) to 7 (strongly agree). To address the potential endogeneity between financial literacy and behaviors, we complement the OLS results (Columns 1 to 5 of Table 2) with IV regressions: the financial literacy score based on the "Big Three" questions is instrumented by a dummy on whether the respondent was exposed to financial education in school, 0 otherwise. This instrument is plausibly exogenous as much of the variation of this variable stems from the availability of financial education mandates at the state level (see Urban et al. 2020). Column 5 shows the first-stage results, suggesting that the instrument is relevant: those exposed to

financial education in school score about 0.15 SD units higher on the "Big Three." Accordingly, the first-stage F-statistic of about 50 is sufficiently large at conventional levels (see Andrews et al. 2019 for a discussion of weak instruments and first-stage screening).

We find that both OLS and IV estimates result in a positive relationship between financial literacy scores and outcomes such as retirement planning and the self-reported credit record. Moreover, financial literacy is negatively correlated with financial fragility and reporting "too much debt." Thus, we are able to replicate stylized facts in this more recent data as well.

We check for heterogenous effects of financial literacy on financial behaviors by splitting the sample by gender, college education, and minority status and rerunning the OLS and IV regressions for the financial behaviors discussed above.

< Table 3 about here >

Table 3 shows OLS regressions (Panel A) and the IV-set-up discussed above (Panel B) in the subsamples of females only, those without a college degree only, and non-whites (i.e., minority status). The direction and magnitudes of the effects of financial literacy on financial behaviors mirror the results in the pooled sample: there is no evidence of heterogeneity, and the equality of coefficients cannot be rejected in any sub-groups.

4 The causal effects of financial education programs

While observational data can provide useful insights into the relationship between financial literacy and financial behaviors, identifying the causal effect of financial literacy on financial behavior is fraught with complications, even when more sophisticated estimation methods than OLS are used. Thus, we turn now to another strand of the literature: experimental and quasi-experimental evaluations of the effects of financial education programs. If financial literacy is important and consequential, financial education programs should be able to affect behavior. Here, what is of interest is usually the causal effect of being offered participation in

a financial education program via random assignment (i.e., the intention to treat). Some studies also quantify the effect on the compliers (i.e., the local average treatment effect), which can be interpreted as the effect of receiving the financial education program when assuming that the mere invitation or offering itself has no effect on the outcome.

Evidence from natural experiments. Since the inception of this literature, economists have been interested in the causal effects of policy interventions designed to foster individuals' financial literacy. The fact that financial literacy is so low, as reported in many of the studies described above, calls for policy and programs to advance financial literacy, but how effective are they? It is important to look at that evidence first because, in the face of widespread illiteracy, it is evident that interventions need to be robust to be able to have some effects. Evidence from natural policy experiments, such as financial education mandates for high school students in the U.S., suggest long-term improvements in their financial literacy and behaviors. These studies use spatial and temporal variation in the timing of the mandates to identify causal effects on financial behaviors. Specifically, mandates have been found to increase financial literacy scores (Tennyson and Nguyen 2001), increase saving outcomes (Bernheim et al. 2001), reduce household debt (Brown et al. 2016), boost credit scores, and decrease default rates (Brown et al. 2016; Urban et al. 2020), and reduces the cost of student loans (Stoddard and Urban 2020). Additionally, empirical evidence shows that students from states with school mandates have higher student loan repayment rates (Mangrum 2022), reduced use of alternative financial lending among the young, and increased account ownership among individuals with low education (Harvey 2019). At the same time, these mandates do not appear to cause adverse outcomes such as a reduction in high school graduation rates (Urban 2022). However, other studies do not report such positive outcomes (see Cole et al. 2016). In addition, Harvey and Urban (2023) find no effect of the mandates on retirement planning,

suggesting that the mandates may have a larger effect on outcomes that are more immediate and relevant to students.

While previous evidence on workplace financial education reported relatively small or muted effects, more recent works show more promise, probably because the programs are more robust than sending employees to a benefit fair or exposing them to one retirement seminar or a retirement brochure (see the review and discussion in Lusardi and Mitchell, 2014). For example, Skimmyhorn (2016) exploits the staggered rollout of a financial education program in the U.S. Army and shows that the course reduced credit card balances and arrears and persistently increased retirement saving rates. Recently, Hvidberg (2022) studied a related treatment: Exposure to economic education in the context of higher education programs in Denmark. He finds large effects on reductions of loan defaults and arrears.

The benefit of this literature is the external validity and nature of the policies studied: As these mandates and programs are operated at scale, they are likely to reflect the true effect of financial education policies in the respective population. However, natural experiments also come with additional identifying assumptions, which are not always easy to probe. Thus, we now turn to what is sometimes referred to as the gold standard of impact evaluation and causal inference: Randomized Controlled Trials (RCTs).

Evidence of the effects of financial education from RCTs. While there have been numerous impact evaluations relying on non-random assignment of individuals into programs (for example, employing propensity score matching or other techniques to account for selection on observables), the available meta-evidence suggests that estimates of the causal effects generated in these types of studies appear inflated and not very precise (see Fernandes et al. 2014; Kaiser and Menkhoff 2017, 2020, for meta-analyses including quasi experiments). Thus, we limit discussion of the causal effects of financial education programs to those studied in RCTs. They are expected to represent the most rigorous evidence, with little debate regarding

internal validity when the random assignment protocol was followed and when post-attrition sample composition does not compromise the experiment's integrity. In recent years, RCTs have become the modal way to evaluate financial education curricula in a variety of settings. The next section discusses the evidence from the most recent meta-analysis of RCTs as well as examples of well-executed primary studies.

Evidence from Meta-Analyses. The first meta-analysis of this literature (Fernandes et al. 2014) included only 13 RCTs mainly reporting on light-touch interventions, such as information provision via brochures or workplace fairs. While the paper also studied quasi-experiments and endogeneity concerns in observational studies, it was most often cited as evidence of the general ineffectiveness of financial education in improving individual financial behavior. Following this work were three additional meta-analyses of financial education programs: one by Miller et al. (2015) and two by Kaiser and Menkhoff (2017 and 2020). These analyses provide a more nuanced interpretation of the effectiveness of financial education, contrasting Fernandes et al.'s 2014 analysis, as they integrate more studies and consider the many differences in both program implementation and results. Nonetheless, each of these successive analyses had their own limitations. For example, the 2015 study by Miller et al. conducts a statistical meta-analysis on less than twenty studies, only seven being RCTs, with an emphasis on the varying impacts across types of financial behaviors. Kaiser and Menkhoff (2017) investigate the associated factors of financial education interventions in (quasi-) experimental settings, while Kaiser and Menkhoff (2020) analyzed (quasi-) experimental studies of financial education within school settings only. Since then, the number of rigorous RCTs has grown exponentially and the most recent meta-analysis of the causal evidence relies on treatment effect estimates from as many as 76 RCTs (Kaiser et al. 2022), and the number of RCTs continues to grow each year. The main takeaways from the more recent meta-analyses relying on updated evidence and on many studies are summarized below.

First, financial education, on average, has a causal effect on financial literacy scores. The average intervention boosts financial literacy scores by about 15 to 20 percent of a standard deviation. Second, on average, interventions cause changes in financial behaviors. The average effect is estimated to be about 6 to 10 percent of a standard deviation. These results are robust enough to adjust for potential publication selection bias (i.e., authors' preference to publish estimates that lie below conventional thresholds for "statistical significance").

Third, an important insight of these meta-analyses is that treatment effects of educational interventions are highly heterogenous, as should be expected given the vast differences we have documented in the data: Any aggregation attempting to form a (precision weighted) average simply fails to accommodate the vast heterogeneity in true effects (as opposed to mere sampling error) by these interventions. The heterogeneity parameter is quantified to be around 1.2 times as large as the average standard error of the reported treatment effects, indicating that the results of programs hinge critically on contextual features. For example, treatment effects vary by outcome type studied, with treatment effects on budgeting and saving behavior being much larger than effects on outcomes concerning debt behavior (Kaiser et al. 2022, p. 265). They also vary by treatment intensity, delivery format, and age of the participants (Kaiser et al. 2022, p. 267): effect sizes increase with time spent in the classroom (see also Kaiser and Menkhoff 2020) and are much smaller with light-touch interventions, such as mere information provision (e.g., Choi et al. 2010; Goda et al. 2014). In contrast to the findings in the earlier literature about the effectiveness of classroom-based programs, the most recent evidence shows that these programs are generally effective. Treatment effects on financial literacy appear larger at younger ages, whereas treatment effects on behaviors are larger among adults.

Fourth, interventions studied in RCTs generally have low costs and thus have a very favorable cost-to-effectiveness ratio. The average intervention costs about $60 (median of

about $20) (2019 PPP) per participant for one-fifth of a standard deviation improvement in outcomes. This places financial education interventions favorably within the field of education interventions (Kraft 2020).

Meta-regression analysis. Based on the results derived from the new meta-analysis, we now turn to a re-analysis of the most recent meta dataset of financial education treatment effects estimated in RCTs. We use the data from Kaiser et al. (2020) and consider potential drivers of heterogeneity in treatment effects across sites in a meta-regression model, allowing for joint consideration of these study-level covariates (see Kaiser et al. 2020 for a formal introduction of the general meta-analysis model). We restrict the sample to classroom financial education interventions measuring changes in saving or debt behavior as an outcome. We only include studies with complete information about the study-level characteristics, which results in a reduced sample relative to the original meta-analysis (Kaiser et al. 2022). We regress the standardized financial education treatment effect estimates on measures of debt and saving behavior on study-level covariates (i.e., the intensity, the delay between treatment and measurement of outcomes, features of the target group, and the type of behavior studied). We allow for residual heterogeneity in true effects across sites by including a study-level random effect (i.e., not assuming the covariates capture the full true heterogeneity in true effects) and clustering the standard errors at the study level for inference. The weights used in the meta-analysis are a combination of the inverse of the random sampling error associated with each treatment effect estimate within each study and the heterogeneity in true effects between studies, which is estimated from the data (see Kaiser et al. 2022).

< Table 4 about here >

As expected, we found that intensity is positively correlated with larger treatment effect estimates at a rate of about +0.03 percent for each additional hour of classroom exposure, and delay between treatment and measurement of outcomes in weeks is negatively correlated with

effect sizes, assuming a linear relationship results in a fadeout of about 0.06 percent of a standard deviation per week (i.e., the average effect of interventions on saving behavior with less than one hour of intensity fades out after about four years). Thus, increasing the intensity can generate lasting effects on saving behavior, as seen in recent long-term evaluations of treatment effects in Brazilian high schools (Bruhn et al. 2016, 2022). As in the original analysis, we do not find evidence for smaller (or larger) effects based on respondent income but find smaller effects for children relative to youth or adults, likely because behavior change is more difficult to observe or measure at these early ages when children rarely interact with financial markets on their own. As in the original analysis, the effects on borrowing behavior are much more muted, with an average intervention effect of about 0.02 SD units relative to about 0.13 SD units for saving behavior (see also Kaiser et al. 2022 for an in-depth discussion of treatment effect heterogeneity along several dimensions and beyond classroom interventions).

5 What works in financial education?

While the available meta-analyses suggest effective interventions, it is important to understand the drivers behind the heterogeneity in treatment effects of financial education programs across contexts. In the following section, we discuss areas where reliable evidence of effective interventions exists. This discussion may inform policymakers interested in implementing financial education programs in the context of national strategies as well as practitioners interested in designing effective programs for new contexts or target groups.

a. Evidence on large-scale programs in schools

While there are numerous RCTs studying financial education interventions in schools (see Kaiser and Menkhoff 2020 for a meta-analysis focusing on school-based programs), they only measure short-term effects. However, there are now two large-scale RCTs studying long-term outcomes: one in Brazil and one in Peru. Bruhn et al. (2016) studied the effect of a

financial education program in a large-scale experiment with over 25,000 students in more than 890 schools in Brazil. They found that the extensive program for 16-year-old students improved financial literacy scores by a quarter of a standard deviation and had positive effects on various aspects of saving behavior in the short term. They also found strong short-term effects on students' financial autonomy and self-reported money management behavior. At the same time, however, the study also found, in the short term, adverse treatment effects on borrowing behavior, as students appeared to increase their use of expensive forms of credit to finance consumption expenditures. The authors recently conducted a long-term evaluation of the same students, following 16,000 students for nine years after the treatment, relying on administrative data to measure outcomes (Bruhn et al. 2022). This long-term follow-up provides evidence of persistent effects. In contrast to the short-term results, treated students were found to be less likely to engage in high-cost borrowing and there were fewer arrears. They also found effects on the probability of owning a micro-enterprise. These findings highlight the importance of looking at both the long-term and short-term effects of financial education.

Frisancho (2023a) studies a similar program in Peru within a large-scale experiment. She also finds immediate impacts on financial literacy scores (about 15 percent of a standard deviation) and some impacts on financial autonomy (0.02 standard deviations) and "financial savviness" (0.03 standard deviations). More importantly, three years after the program, she finds no effects on credit behavior (loan taking) at the extensive margin, but large effects on late payments at the intensive margin: treated students with loans see a reduction of arrears in the order of magnitude of about 20 percent relative to the control group.

The evidence from these programs highlights that financial education can make an important and lasting difference regarding student outcomes later in life. Since these programs have limited costs (Frisancho, 2023a, reports about 5 USD per student), it seems warranted to

advocate for personal financial education requirements in high schools. These positive results mirror the findings in the U.S. literature on mandates studied in natural experiments, indicating that programs at scale have external validity across contexts and that natural experiments appear to come close to the internal validity of RCTs in this literature.

In addition to the direct effects on students, both large-scale RCTs also study outcomes on adults exposed to the children. Bruhn et al. (2016) finds positive spillovers to parents whereas Frisancho (2023b) finds some evidence of spillovers on parent financial behavior within households of lower socio-economic status. Frisancho (2023a) also finds large effects on the financial literacy of teachers (about 0.3 standard deviations) and even savings at both the extensive and intensive margin. All of this suggests that the welfare effects of school-based financial education may be even larger, since spillover to peers is likely (see also Duflo and Saez 2003; Haliassos et al. 2019).

b. Evidence on innovative programs for adults

While school-based financial education is a natural starting point for policy intervention, experimental impact evaluations of adult financial education programs are much more common in this literature, especially in developing economies (e.g., Bruhn et al. 2014). Because the early experimental literature on the effects of classroom-based exposure to financial literacy education of adults showed relatively muted impacts, a wave of RCTs examined programs that evaluate alternatives to classroom-based settings (see also Zia 2023 for an excellent overview of the new wave of RCTs). The diverse approaches include tailoring interventions to target groups (e.g., Doi et al., 2014; Seshan and Yang 2014; Abarcar et al. 2020; Barua et al. 2020); simplifying curricula by introducing “rules of thumb” (Drexler et al. 2014); introducing personalized elements like counseling (Carpena et al. 2017); using mass media to communicate financial information and change attitudes (Berg and Zia 2017); relying on experiential learning to debias participants (Abel et al. 2020); relying on digital delivery and

gamification (Attanasio et al. 2019; Sconti 2022); adapting the teaching pedagogy to include active learning and group exercises as opposed to lecture-based formats (Kaiser and Menkhoff 2022); and relying on decentralized teaching responsibility (Hakizimfura et al. 2020). Many of these programs produced larger effects than the previous wave of evidence, and we expect more studies to follow. In summary, the evidence suggests that interventions should be designed to be (a) relevant to the life situation of those targeted by the program, (b) accessible, entertaining, and actively engaging, and (c) scalable with moderate marginal cost per participant.

As many countries have implemented or are implementing national strategies for financial literacy (Lusardi and Mitchell 2023), impact evaluations of financial education campaigns operated at scale are especially important. Recently, we pre-registered an impact evaluation of a national financial education campaign delivered via television, radio, print, and social media in Italy in the AEA RCT registry (Kaiser et al. 2022). We employ a randomized encouragement design to study the short-term and long-term effects of the campaign on financial attitudes, awareness, and behaviors. We also elicit predictions from experts about the treatment effects of such a campaign and pre-registered heterogeneity analyses based on baseline gender, baseline financial literacy, and socio-economic background. The large-scale evaluation with representative household survey data is expected to inform evidence-based policies regarding national campaigns with low marginal costs.

c. Evidence on causal mechanisms

While it is informative to know *what* works, it is even more important to understand *why* interventions work. While there is now robust evidence that financial education, on average, is successful in changing financial behavior, the causal mechanism translating educational inputs into action is less well understood. The extant literature has discussed potential causal mediators of the treatment effects (Sayinzoga et al. 2016; Carpena and Zia

2020; Horn et al. 2020; Kaiser and Menkhoff 2022), but no experiment has been designed to identify mechanisms. An obvious candidate is a cognitive mechanism, i.e., improved financial knowledge leads to potential correction of financial mistakes and/or to better financial outcomes. Some studies provide evidence for such a mechanism (e.g., Sayinzoga et al. 2016), but there remain unexplained direct effects of the treatment on outcomes. Horn et al. (2020) show that lasting behavior changes among treated youth do not depend on persistent gains in financial knowledge. Similarly, Kaiser and Menkhoff (2022) find that financial literacy scores do not appear to mediate the observed treatment effects in a financial education intervention directed at adults and which was found to impact behavior persistently, i.e., until four years after the treatment. Changes in measures of self-control have a good deal of explanatory power, but a large share of treatment effects appear to be unexplained by the considered mediators. Recently, Carpena and Zia (2020) employed more formal mediation analysis methods to study the importance of different mechanisms. While such an exercise comes with strong assumptions and should be interpreted with caution, they provide evidence that suggests that the treatment effects are not mediated by increased financial numeracy (i.e., a cognitive mechanism) but instead by changes in financial awareness and especially attitudes. More research is clearly needed to better understand what drives behavior change.

6 Outlook and research priorities in financial literacy and financial education

The academic research of the past fifteen years has generated an abundance of empirical evidence on financial literacy and financial education that can inform evidence-based policy and inspire future work. Despite this remarkable advance in research and what can be learned from the research, we see three broad areas where more work is needed.

i. Financial literacy and causal mechanisms from financial education to behavior

Why do we observe a gender gap in financial literacy at early ages? What is the role of the social environment in shaping financial literacy and behaviors? How does intergenerational transmission of financial literacy work? How does heterogeneity in financial literacy contribute to persistent inequality across generations? These questions are related to the generation and growth of financial literacy in different societies (e.g., Grohmann et al. 2015). While selected studies in the extant literature try to address these questions, there is substantial room for additional empirical inquiry and theoretical modeling in this area. More work on the measurement of financial literacy and related constructs is also welcome. While the "Big Three" serve as a reliable and concise measure of basic financial knowledge, measurement instruments that capture a broader range of knowledge, such as the TIAA-Institute-GFLEC Personal Finance Index, are needed (Yakoboski, Hasler, and Lusardi, 2023).

Related to measurement and the latent construct of financial literacy's psychometric structure is the question of how financial literacy relates to the nascent literature on heterogeneity in individuals' mental models of different aspects of financial markets and how financial literacy may interact with belief and expectation formation (e.g., Andre et al. 2022, 2023; Heiss et al. 2022).

The link between financial literacy and economic preferences is also an area of active debate, but empirical work has shown some regularities worth noting. First, financial literacy and time preferences (i.e., individual-level discount factors) appear positively correlated in many empirical inquiries. On average, individuals with more patience appear to be more financially literate (Bianchi 2018; Oberrauch and Kaiser 2022). One mechanism may be that patient individuals are more likely to acquire financial information, for example, by participating in voluntary financial education programs (Meier and Sprenger 2013). In general, the field lacks psychometric studies that consider the relationship among financial literacy, preferences, and other variables related to human capital and financial decision-making.

Cognitive biases can also be important. Stango and Zinman (2023) and Chapman et al. (2023) have recently studied the dimensionality of behavioral biases and derived a taxonomy of consumer financial decision-making. They found that financial literacy is negatively correlated with many of the considered biases. We envision work that extends this observation and studies whether financial education can help to mitigate biases, i.e., whether education interventions may help with debiasing individuals.

Regarding the mechanisms underlying behavior change in response to financial education interventions, several evaluations of financial education programs have been found to have causal effects on time preferences (i.e., patience and time-inconsistency) and the quality of intertemporal decision-making (i.e., choice consistency), especially on the young (Alan and Ertac 2018; Bover et al. 2018; Lührmann et al. 2018; Sutter et al. 2020; Kaiser et al. 2023b). Thus, changes in time preferences and self-control caused by educational interventions could play an important role in explaining, for example, the treatment effects on saving behavior observed in the literature (Kaiser et al. 2022). Supporting this hypothesis, in a program with adults in Uganda, Kaiser et al. (2023b) found evidence for causal effects on patience in incentivized tasks. These effects are heterogeneous by age, with large effects for youth and zero effects for adults. Interestingly, these effects also carry over to saving behavior.

While the identification of causal effects on preference parameters is an intricate issue (see Lührmann et al. 2018 for a discussion of concerns that financial education treatments cause respondents to engage in intertemporal arbitrage, violating identifying assumptions of utility parameters), the literature on the malleability of preferences provides promising results on the study of mechanisms behind financial behavior change.

ii. Long-term effects of financial education and overcoming the limitations of survey data in impact evaluations

While there is currently high-quality evidence from RCTs that considers relatively long-term outcomes and relies on administrative data (Bruhn et al. 2016, 2022; Frisancho 2023a), we

envision a wave of new evidence emerging from behavior change studies that rely on transaction data and other forms of administrative data. In the extant literature, concerns that survey response behaviors drive elements of the treatment effects on behaviors are warranted. However, results from the limited set of studies relying on administrative data paint a picture similar to that of studies based on household surveys (Attanasio et al. 2019; Bruhn et al. 2022; Frisancho 2023a). We expect more evidence in this direction, especially for advanced and emerging economies where the importance of digital finance is increasing.

iii. The effect of financial education beyond directional changes in financial behavior

Finally, it seems important to focus less on directional changes in financial behavior (which are often assumed to improve well-being) and more on welfare assessments and the quality of decision-making; after all, the individual's objective is not just to save more or borrow less, but to increase their well-being. De Beckker et al. (2023) study changes in students' decision-making in hypothetical choice experiments and find that financial education does not automatically improve choices. Ambuehl et al. (2022) have developed a method to evaluate the success of financial education interventions by considering choice errors in framed decision situations. Similarly, Boyer et al. (2022) have evaluated welfare loss in hypothetical choice experiments. This approach has the appeal of not depending on normative assumptions about the optimality of consumer behavior change (for the average consumer) in a certain direction. Similarly, theoretical work by Lusardi et al. (2017, 2020) suggests that it may sometimes be optimal for consumers to do nothing. This highlights the need for approaches that evaluate treatment effects on heterogeneous consumers within a theoretical framework. So far, theoretical work and empirical impact evaluations have not been well connected.

Likewise, there is no evidence about likely general equilibrium effects of financial literacy expansion. Kosfeld and Schuewer (2017) consider financial markets with shrouded add-on pricing and argue that financial education may not achieve an unshrouded equilibrium

but will shift markets to an equilibrium in which financial institutions discriminate between consumer types. In this instance, consumers who remained financially illiterate would pay higher prices. It is possible that the welfare effects of education can be ambiguous or negative (resulting from the negative externality on naïve consumers). While this is an intriguing theoretical argument, there is no empirical work considering the equilibrium effects of financial education provision. Few studies investigate the issue of spillover to untreated peers (Hamdan et al. 2021), and much remains to be learned about the impact of financial education on supply-side decisions.

Empirical evidence on likely general equilibrium effects is especially important in anticipating how the financial industry will react to greater sophistication among its clients. If financial education reaches enough naïve clients, an unshrouded equilibrium with lower prices may be achieved in the long run. However, if a relatively large share of clients remains naïve, there could be higher prices for those with low literacy. Currently, few studies investigate financial education provided by the banking sector. One exception is Fort et al. (2016), who studied the effect of bank information policies in Italy on financial knowledge. They find that the policies are effective for about five to ten percent of the population, particularly among those who are elderly and have low levels of education.

Lastly, an interesting avenue of research is the effect of financial education on mental models of the economy and economic policy preferences. Stantcheva's 2021 work has shown remarkable heterogeneity in laypersons' understanding of economic policy. An important area of research would be to study the relationship between financial literacy and the understanding of economic policy as well as normative attitudes, as summarized in Fornero and Lo Prete (2023). Additionally, the exploration of whether financial education interventions affect policy, especially monetary policy, seems worthwhile.

Recently, a global network of financial literacy and personal finance researchers (the G53 Network) and a field journal dedicated to this topic (*Journal of Financial Literacy and Wellbeing*) have been formed. One role of the network and the journal is to empower emerging researchers to work on open questions and advance knowledge about what works in financial literacy and financial education, and why. We hope that this paper will provide interested readers with a concise overview of the existing empirical work and will inspire future work that advances knowledge about financial literacy and financial education and their effects.

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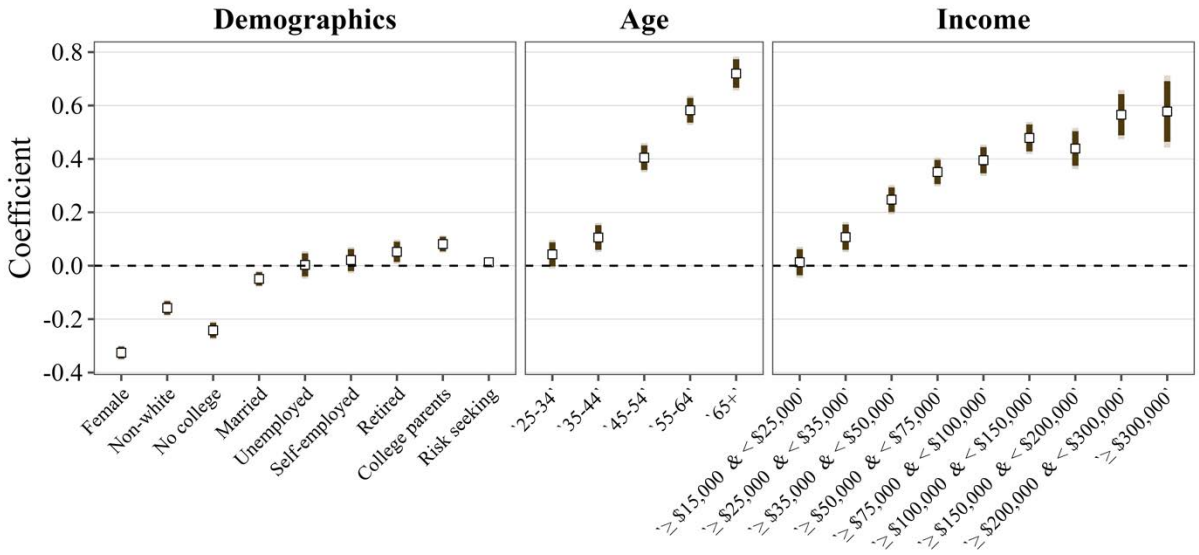
Table 1: Sample descriptive statistics of the 2021 NFCS

Variable	N	Mean	SD	Min	Max
Individual characteristics					
Female (1/0)	27,118	0.54		0	1
Non-white ethnicity (1/0)	27,118	0.26		0	1
No college (1/0)	27,118	0.536		0	1
Couple (1/0)	27,118	0.09		0	1
Married (1/0)	27,118	0.49		0	1
Unemployed (1/0)	27,118	0.081		0	1
Self-employed (1/0)	27,118	0.079		0	1
Retired (1/0)	27,118	0.216		0	1
College edu parents (1/0)	26,637	0.428		0	1
Risk seeking	26,458	5.054	2.71	1	10
Fin., edcuation in school	24,563	0.136		0	1
Age groups					
18-24	27,118	0.111		0	1
25-34	27,118	0.173		0	1
35-44	27,118	0.168		0	1
45-54	27,118	0.170		0	1
55-64	27,118	0.174		0	1
65+	27,118	0.203		0	1
Income levels					
< $15,000	27,118	0.123		0	1
≥ $15,000 & < $25,000	27,118	0.108		0	1
≥ $25,000 & < $35,000	27,118	0.108		0	1
≥ $35,000 & < $50,000	27,118	0.142		0	1
≥ $50,000 & < $75,000	27,118	0.185		0	1
≥ $75,000 & < $100,000	27,118	0.132		0	1
≥ $100,000 & < $150,000	27,118	0.128		0	1
≥ $150,000 & < $200,000	27,118	0.045		0	1
≥ $200,000 & < $300,000	27,118	0.021		0	1
≥ $300,000	27,118	0.010		0	1

Notes: Data from the 2021 wave of the National Financial Capability Study (NFCS) across all 50 U.S. states, and Washington D.C, with about 500 respondents per state, on average. California and Oregon were oversampled with 1,250 respondents in each state. All analyses include survey weights to be representative of Census distributions for the variables age, gender, ethnicity, education, and state, based on data from the American Community Survey (with adjustments to oversampling of the two states) (see www.FINRAFoundation.org/NFCS).

Figure 1: Demographic correlates of financial literacy



Notes: The dependent variable is financial literacy (measured with the "Big Three") standardized to have a mean of zero and an SD of 1 in the full sample. This figure shows unstandardized OLS regression coefficients with 90% and 95% CIs at an estimated intercept of -0.349. Number of observations is N=26,007. Adjusted R^2 is 0.22. Base cohort in age is the group of 18-24-year-old respondents. Coefficient estimates in category "Income" are relative to the group with less than $15,000 annual income. All variables are defined in Table 1.

Table 2: The effect of financial literacy on financial behaviors

	OLS				IV				
	(1) Retirement planning	(2) Financial fragility	(3) Credit record	(4) Too much debt	(5) 1st stage	(6) Retirement planning	(7) Financial fragility	(8) Credit record	(9) Too much debt
Fin. Literacy (Big 3)	0.045*** [0.004]	-0.038*** [0.004]	0.084*** [0.008]	-0.086*** [0.008]		0.642*** [0.100]	-0.362*** [0.065]	0.643*** [0.128]	-0.196* [0.116]
Fin. education					0.156*** [0.022]				
Mean (SD) of Dep. Var.	0.42	0.30	0.000 (1.000)	0.000 (1.000)	0.000 (1.000)				
Controls	✓	✓	✓	✓		✓	✓	✓	✓
N	19,209	25,251	24,679	25,737	24,563	18,240	23,880	23,334	24,338
Adj.R^2	0.167	0.239	0.290	0.108					

Notes: Dependent variables are whether the respondent has ever tried to find out how much she needs to save for retirement (columns 1 and 6), a dummy indicatingwhether the respondent "probably" or "certainly" could not come up with $2,000 if an unexpected need arose within the next month (columns 2 and 7), how the respondent rated her credit record on a scale from 1 (very bad) to 5 (very good) (columns 3 and 8), and whether the respondent agrees with the statement "I have too much debt right now" on a scale from 1 (strongly disagree) to 7 (strongly agree) (columns 4 and 9). The dependent variables "Credit record" and "Too much debt" are standardized to have mean of 0 and standard deviation of 1. Regressions with the binary dependent variables "Retirement planning" and "Financial fragility" are based on linear probability models.Columns 6-9 show instrumental variable estimations where the financial literacy score based on the "Big 3" questions is instrumented by a dummy indicating whether the respondent received financial education in school. The first stage F-Statistic (column 5) is 49.572. Standard errors in brackets. ***p<0.01, **p<0.05, *p<0.1.

Table 3: Heterogenous effects of financial literacy on behaviors

	(a) Female				(b) *No college education*				*(c) Non-white ethnicity*			
	(1) Retirement planning	(2) Financial fragility	(3) Credit record	(4) Too much debt	(5) Retirement planning	(6) Financial fragility	(7) Credit record	(8) Too much debt	(9) Retirement planning	(10) Financial fragility	(11) Credit record	(12) Too much debt
Panel A: Ordinary Least Squares (OLS)												
Fin. Literacy (Big 3)	0.052*** [0.006]	-0.044*** [0.005]	0.081*** [0.01]	-0.046*** [0.011]	0.051*** [0.006]	-0.033*** [0.005]	0.061*** [0.01]	-0.037*** [0.011]	0.037*** [0.008]	-0.038*** [0.007]	0.092*** [0.015]	-0.078*** [0.015]
Intercept	0.117*** [0.026]	0.711*** [0.024]	-0.47*** [0.048]	0.109** [0.05]	0.045** [0.02]	0.726*** [0.021]	-0.328*** [0.041]	-0.143*** [0.043]	0.205*** [0.036]	0.597*** [0.034]	-0.234*** [0.07]	-0.116 [0.072]
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	10,155	13,482	13,165	13,795	10,262	13,141	12,678	13,497	5,458	64,68	6,292	6,643
Adj. R^2	0.157	0.237	0.295	0.108	0.114	0.204	0.256	0.081	0.119	0.183	0.23	0.071
Panel B: Instrumental variable estimation (IV)												
Fin. Literacy. (Big 3)	0.449*** [0.08]	-0.273*** [0.063]	0.477*** [0.121]	-0.316** [0.127]	0.418*** [0.074]	-0.356*** [0.07]	0.669*** [0.127]	-0.340*** [0.116]	0.592*** [0.166]	-0.487*** [0.148]	0.876*** [0.293]	-0.629*** [0.24]
Intercept	0.414*** [0.024]	0.379*** [0.023]	-0.281*** [0.047]	0.3*** [0.047]	0.309*** [0.02]	0.427*** [0.022]	-0.347*** [0.043]	0.148*** [0.038]	0.457*** [0.037]	0.224*** [0.026]	0.069 [0.051]	0.076 [0.046]
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	9,629	12,745	12,451	13,052	9,861	12,586	12,140	12,930	5,235	6,174	5,994	6,345

Notes: Panel A shows results from OLS regressions. Dependent variables are whether the respondent has ever tried to figure out how much she needs to save for retirement , a dummy on whether the respondent "probably" or "certainly" could not come up with $2,000 if an unexpected need arose within the next month, how the respondent would rate her credit record on a scale from 1 (very bad) to 5 (very good), and whether the respondent agrees with the statement "I have too much debt right now" on a scale from 1 (strongly disagree) to 7 (strongly agree) (columns 4 and 9). Dependent variables "Credit record" and "Too much debt" are z-standardized to have mean of 0 and standard deviation of 1 in the pooled sample. Regressions with the binary dependent variable "Retirement planning" and "Financial fragility" are based on a linear probability model (LPM). Panel B shows instrumental variable estimations where the financial literacy score based on the "Big 3" questions is instrumented by a dummy on whether the respondent received financial education in school, 0 otherwise. Standard errors in brackets. ***p<0.01, **p<0.05, *p<0.1.

Table 4: Meta-regression analysis of classroom financial education treatment effects

	(1) Std. treatment effect on financial behavior
Classroom intervention characteristics	
Intensity of intervention (h)	0.0003* [0.0001]
Delay (weeks)	-0.0006*** [0.0002]
Target group characteristics	
Low-income student (=1)	0.017 [0.0371]
Children (=1)	-0.078* [0.041]
Outcome type	
Borrowing behavior (base category: saving behavior)	-0.110*** [0.006]
Intercept (meta-estimate)	0.126*** [0.319]
No. of treatment effect estimates	253
No. of studies	41

Notes: Meta-regression based on data and method described in Kaiser et al. (2022). Standard errors in brackets. ***p<0.01, **p<0.05, *p<0.1.