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Financial Literacy, Financial Education, and Downstream Financial Behaviors

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Policy makers have embraced financial education as a necessary antidote to the increasing complexity of consumers' financial decisions over the last generation. We conduct a meta-analysis of the relationship of financial literacy and of financial education to financial behaviors in 168 papers covering 201 prior studies. We find that interventions to improve financial literacy explain only 0.1% of the variance in financial behaviors studied, with weaker effects in low-income samples. Like other education, financial education decays over time; even large interventions with many hours of instruction have negligible effects on behavior 20 months or more from the time of intervention. Correlational studies that measure financial literacy find stronger associations with financial behaviors. We conduct three empirical studies, and we find that the partial effects of financial literacy diminish dramatically when one controls for psychological traits that have been omitted in prior research or when one uses an instrument for financial literacy to control for omitted variables. Financial education as studied to date has serious limitations that have been masked by the apparently larger effects in correlational studies. We envisage a reduced role for financial education that is not elaborated or acted upon soon afterward. We suggest a real but narrower role for "just-in-time" financial education tied to specific behaviors it intends to help. We conclude with a discussion of the characteristics of behaviors that might affect the policy maker's mix of financial education, choice architecture, and regulation as tools to help consumer financial behavior.

Keywords: behavioral economics; household finance; consumer behavior; education systems; public policy; government programs; statistics; causal effects; design of experiments; meta-analysis; financial education; financial literacy
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1. Introduction

The financial environment that consumers face today has become dramatically more perilous just in one generation (Boshara et al. 2010). Baby boomers witnessed during their working careers the advent of exotic mortgage forms (Lacko and Pappalardo 2007; cf. Woodward and Hall 2012), much-expanded credit availability, and new borrowing options such as payday loans and debt consolidation loans. They experienced fivefold increases in bankruptcies in the United States in the last 30 years (White 2009). In the arena of retirement savings, defined benefit pensions of boomers' parents were replaced by defined contribution retirement systems, simplifying the balance sheets of employers, but requiring employees to figure out how much to save, where to invest, and how to make lump-sum payouts last throughout retirement (McKenzie and Liersch 2011).

Many experts observed the phenomena above and prescribed the same remedy: increased financial literacy and financial education (Hilgert et al. 2003, Greenspan 2005, Morton 2005, Lusardi and Mitchell 2007a, Mishkin 2008, U.S. Congress 2010, Cordray 2013). It is a solution that appeals to all political persuasions and to all geographies. For example, the Second Annual Child and Youth Finance Summit in Istanbul in May of 2013 brought together experts describing initiatives by the United States, the United Kingdom, Turkey, the Philippines, Chile, Nigeria, Egypt, Ghana, Nepal, Macedonia, Spain, and the United Nations to provide financial education to millions.[1] Worldwide, employers, nonprofits, and governments are creating educational interventions

[1] See http://www.childfinanceinternational.org/program-2013/summit-program-overview-2013 (accessed December 30, 2013).

that have real costs and create much larger opportunity costs by supplanting some other activities, such as required high school courses that replace other electives. We estimate these real and opportunity costs to be in the billions of dollars annually.

Creating financial literacy interventions is an obvious and common sense response to the increased complexity of the financial world. There are many domains of social policy where it is obvious what should work to redress a social problem. But as Watts (2011) has admonished, "everything is obvious (once you know the answer)." For example, it is obvious that incentives should matter, e.g., to improve educational performance. But sometimes effects are surprisingly weak (Gneezy et al. 2011), and rigorous scientific approaches can shed light on which "obvious" conclusions are true and which are not.

But what is financial literacy? And what capabilities might financial education improve? Financial literacy is most commonly viewed as a specialized kind of consumer expertise pertaining to how one manages one's financial affairs successfully (cf. Alba and Hutchinson 1987) or a personal finance-specific form of human capital. Remund (2010, p. 284) argues:

> Financial literacy is a measure of the degree to which one understands key financial concepts and possesses the ability and confidence to manage personal finances through appropriate short-term decision-making and sound, long-range financial planning, while mindful of life events and changing economic conditions.

Though conceptually financial literacy refers to skills, existing measures of financial literacy are dominated by measures of objective knowledge. Financial literacy is measured by the percentage correct on knowledge tests where each question has a right answer—e.g., "Suppose you had €100 in a savings account and the interest rate was 20% per year. After 5 years, how much do you think you would have in the account if you left the money to grow? (i) More than €102; (ii) Exactly €102; (iii) Less than €102; (iv) Do not know; (v) Refusal" (van Rooij et al. 2011, p. 452).

Academic work has concluded that financial literacy is an antecedent to various healthy financial behaviors. But several excellent recent literature reviews have drawn sharply different conclusions about the effects of financial literacy and financial education (Adams and Rau 2011; Collins and O'Rourke 2010; Hastings et al. 2013; Hira 2010; Thaler and Sunstein 2008; Willis 2009, 2011). Adams and Rau (2011, p. 6) conclude, "Perhaps one of the most robust findings across the literature is that financial literacy (a cognitive factor…) plays a key role in financial preparation for retirement. Both experimental and nonexperimental studies demonstrate that understanding the basic principles of saving, such as compound interest, has a direct effect on financial preparation. This effect holds after controlling for demographic characteristics." Willis (2009, p. 456) disagrees: "What degree of effectiveness should appropriately be claimed for the current model of financial literacy education? As yet, none, and the barriers to research that would soundly demonstrate effectiveness may be insurmountable."

We attribute disagreements about this literature to two factors. First, prior analysts like those just cited have conflated two kinds of studies. One type includes experimental and quasi-experimental studies of the effects of financial education *interventions*. A second type includes correlational and econometric studies that *measured* financial literacy by the percentage of correct answers on tests of financial knowledge and predicted downstream financial behaviors. We refer to these two types of studies as "manipulated financial literacy" and "measured financial literacy" below. Second, prior reviews relied on qualitative summaries rather than statistical summaries via transparent meta-analysis. Meta-analysis can test the magnitude of the average effect of an independent variable, whether there is systematic variation in effect sizes across studies beyond what would be expected by chance and, if so, what differences among the studies could explain this variation (Lipsey and Wilson 2001). "Effect size" refers to a measure of association in meta-analysis parlance and is not meant to imply a causal relation.

We report the first systematic meta-analysis of this literature. Based on reading a subset of papers in this literature, we developed the working hypothesis that we would find weak effects of financial literacy in studies of financial education *interventions* intended to improve downstream financial behaviors. In contrast, we expected to find stronger effects in econometric studies that predicted downstream financial behavior based on measured financial literacy, controlling for various demographics. We have worked in the measurement of individual differences in the arena of intertemporal choice, critical to financial decision making (Bearden et al. 2011, Lynch et al. 2010). We perceived that leading researchers working in the financial literacy arena had not considered individual difference variables that we thought were strong candidates to be correlated with financial literacy and that might plausibly cause the financial behaviors studied.

We find strong support for our hypothesis that study findings depend on study methodology, and we propose and test three explanations for the gap between the moderate effect size of measured financial literacy and the miniscule effect of interventions that were intended to improve financial literacy. We then follow up this meta-analysis with empirical

studies suggesting that the larger effect sizes for measured literacy studies may be due in part to the correlation of measured financial literacy with other traits that are omitted from prior research. These omitted variables might plausibly produce overestimates of the effect of financial literacy on the financial behaviors studied.

2. Meta-Analysis

2.1. Meta-Analysis Overview

In a traditional qualitative literature review, the authors may rely on a convenience sample of studies, and the rules for inclusion and treatment are often unstated. There is often room for interpretation, and flaws in studies are taken in a one-off fashion. In contrast, meta-analysis makes explicit the rules for inclusion and exclusion of studies, as well as the coding procedures to characterize similarities and differences among studies. Furthermore, meta-analysis examines roughly the same independent variable to dependent variable relationships. The key statistic used to summarize the findings is an effect size that varies continuously.

We examined all studies that *manipulated* financial literacy with some education intervention or that *measured* financial literacy with well-known psychometric scales. We quantified effect sizes by the (partial) r of manipulated or measured financial literacy on measures of financial behaviors: saving, planning for retirement, absence of debt, stock ownership and investment decisions, cash flow management, activity in retirement plans, and financial inertia such as choice of default options and payment of unnecessary fees.

We identified studies by a computerized bibliographic search in numerous databases for the terms "financial literacy," "financial knowledge," and "financial education." We found 10,650 articles published from 1969 to 2013; 267 from 1987 to 2013 were empirical tests of the effect of financial literacy, some reanalyzing the same data. We pursued all working papers referenced in any of our sources. We included in our analysis 168 papers covering 201 nonredundant studies. If multiple papers used the same study data, we included only the paper with the most inclusive sample. We included pretest/posttest studies only if pre- and posttests were separated by at least two weeks. We excluded studies providing insufficient statistical information to compute an effect size if authors could not provide required details.

In Web Appendix A (Web appendices available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2333898), we provide references for all papers included in our meta-analysis. Most studies reported multiple effect sizes across dependent variables. We averaged the effect sizes for each study that manipulated financial literacy and for each study that measured financial literacy. Using this approach, 201 effect sizes were available: 90 effects of interventions (manipulated financial literacy) and 111 effects of measured financial literacy. Tables WA1–WA4 in Web Appendix A present the authors of each paper included, their respective effect sizes, and relations investigated. Studies in these tables are sorted by whether the independent variables were manipulated or measured financial literacy, and within each group and by the type of design and analysis employed. We also coded all identified studies in terms of the financial behavior dependent variable examined and sample characteristics.

Among the studies that manipulated financial literacy (Tables WA1 and WA2 in the Web appendix), we coded for what type of educational intervention was conducted (high school financial education, counseling, seminar or workshop, multiple sources of education, and exposure to information such as a newsletter or a fair). In addition, when reported, we coded for the hours of instruction in the interventions and for the delay in months between the intervention and measurement of behavior. We also coded for research design. Only 15 studies in Table WA1 had better designs with randomized control trials. The majority of studies of manipulated financial literacy (75 in Table WA2) used quasi-experimental or pre-post designs. Finally, we coded for whether the decision to participate at the intervention was voluntary and for whether the study was focused exclusively on low-income students.

Among the studies that measured financial literacy, we coded for what type of analysis was performed (Tables WA3 and WA4). Only 24 studies (in Table WA3) used econometric analyses with instrumental variables to control for endogeneity on the effect of measured financial literacy on financial behaviors. The majority of studies (87 in total in Table WA4) performed only ordinary least squares (OLS) regressions to estimate the effect of financial literacy on downstream financial behavior. And we also coded for whether the study was focused exclusively on low-income respondents.

We followed common guidelines for meta-analysis to compute and integrate the effect sizes (Rosenthal 1984, Hedges and Olkin 1985, Lipsey and Wilson 2001). We selected the (partial) correlation coefficient, r, as the effect size metric because it is an easy to interpret, scale-free measure imputable from a variety of statistics. Calculation of effect sizes was made using the statistical information in the papers. Direct calculation of effect size from group mean contrasts or frequency distributions was difficult in cases in which means and standard deviations were not reported. Under those circumstances, we calculated

effects sizes through a range of statistical information (e.g., student's t, F ratios, χ^2) via the formulae given by Lipsey and Wilson (2001). When necessary, we solicited additional information from authors.

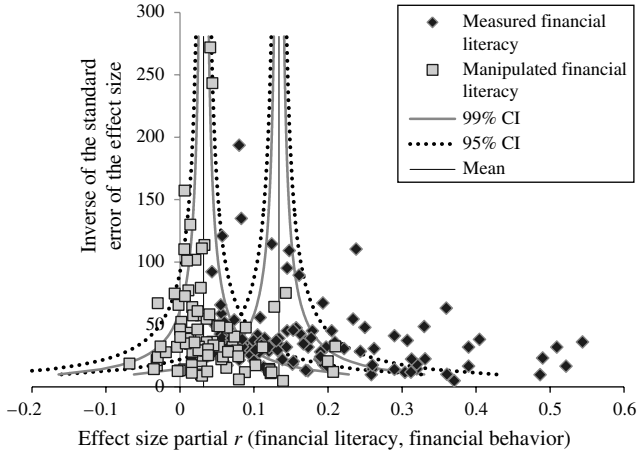
2.2. Meta-Analysis Results

Because sample size affects the correspondence between the estimated relationship between variables and true relationship, we first weighted effects by the inverse variance. Empirically in our sample, smaller studies reported larger effect sizes. Given that it requires a larger effect size to reach statistical significance with a smaller N, this might suggest a publication bias favoring significant results. We examined significance for the mean effect size by calculating the confidence intervals of the effect sizes to determine whether the confidence interval includes 0.

2.2.1. Measured Financial Literacy vs. Financial Education Interventions. Our most striking finding was that financial education interventions have statistically significant but miniscule effects: $r^2 = 0.0011$, implying that interventions explained about 0.1% of the variance in downstream financial behaviors studied (90 effect sizes, $r = 0.032$, $CI_{95} = 0.029$ to 0.035). By social science and education conventions, $r \leq 0.10$ is a small effect size, $0.10 < r < 0.40$ is medium, and $r \geq 0.40$ is large.

As hypothesized, we found a larger effect size for measured financial literacy (111 effect sizes, $r^2 = 0.0179$, $r = 0.134$, $CI_{95} = 0.130$ to 138) than for manipulated financial literacy. Figure 1 presents a "funnel plot" (Lipsey and Wilson 2001) that clearly depicts the separation of the two distributions and relates effect size to the inverse standard error (i.e., the precision of each effect size). The Q statistics confirmed that, within each type of study, there was significant variability in effect sizes beyond what would be expected by chance if all effect sizes of each type were random draws from a common distribution. If that had been true, the symbols for all manipulated literacy studies should fall within the single-peaked distribution shown and the same for measured literacy. Effect sizes clearly violate that assumption (for measured financial literacy, $Q = 1{,}464$, $p < 0.01$; for manipulated financial literacy, $Q = 310$, $p < 0.01$).

Figure 1 **Study Method Affects Average Partial r Effect Size in Meta-Analysis of the Relationship Between Manipulated or Measured Financial Literacy on Financial Behavior**



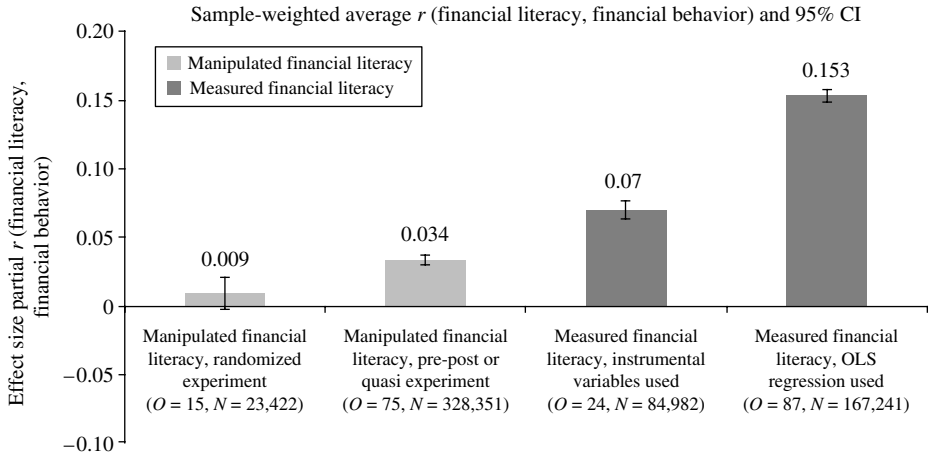
We next report a series of tests to understand what causes variation in effect sizes within each distribution. One factor is variation in the research design of the study, as shown in Figure 2. Intervention studies with randomized control group designs in Table WA1 of the Web appendix found significantly smaller effects (15 studies, sample-weighted $r = 0.009$, CI_{95} is from -0.004 to 0.022) than studies with weaker pre-post or quasi-experimental designs in Table WA2 (75 studies, sample-weighted $r = 0.034$, CI_{95} is from 0.031 to 0.037).

Among studies of measured literacy, one might distinguish studies using simple OLS from econometric studies using instrumental variables and two-stage least squares (2SLS; see Web Appendix A, Tables WA3 and WA4). Arguably, properly chosen instruments can control for reverse causation and are similar to quasi experiments when the instrument for financial literacy is not plausibly caused by the dependent variable (Angrist and Krueger 2001). A proper instrument should predict financial literacy but have no partial relationship with the financial behavior in question except through financial literacy. But it is difficult to prove the validity of an instrument (Stock and Watson 2003). Consequently many authors will use instrumental variable analyses only for robustness analysis (e.g., Morse 2011, Larcker and Rusticus 2010), or take the view that estimates using instrumental variables may, in some cases, be more rather than less biased compared to OLS estimates (Bound et al. 1995, Larcker and Rusticus 2010).

We found smaller effects for studies using instrumental variables than for OLS studies lacking those controls. For papers that used instrumental variables, the sample-weighted effect size of measured financial literacy on behavior is significantly lower (24 studies, $r = 0.070$, CI_{95} is from 0.063 to 0.077) than for papers that did not use instrumental variables (87 studies, $r = 0.153$, CI_{95} is from 0.148 to 0.158). Moreover, OLS regressions reported in the 24 studies that used instrumental variables were larger ($r = 0.094$, CI_{95} is from 0.087 to 0.101) than the effect sizes found using instrumental variables.

These results indicate that studies with putatively better designs and analyses find weaker effects of manipulated and of measured financial literacy. The best designs using randomized control groups

Figure 2 Study Method Affects Average Partial *r* Effect Size of the Relationship Between Manipulated or Measured Financial Literacy and Financial Behavior



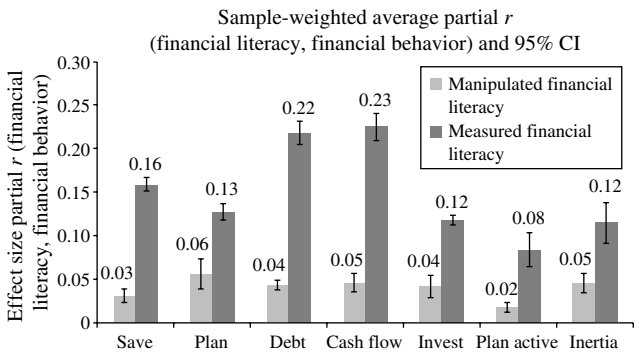
Note. O = number of studies, and N = number of participants in those studies.

showed no significant effects—and significantly lower effects than studies of any other type.

We performed a series of robustness tests of whether the conclusions from Figure 2 were due to some artifact or moderated by some background variable. Might the stronger effect size for measured financial literacy compared to manipulated financial literacy depend in some way on the financial behavior studied? Figure 3 shows that, for all seven categories of financial behavior, effect size of manipulated financial literacy is much less than that of measured financial literacy.

Might the low apparent treatment effects in the 15 randomized experiments result from using conservative "intent to treat" analyses? In such an analysis, those initially randomly assigned to the treatment and control groups are compared, regardless of whether some or many in the treatment group never took up the offered treatment. There were no differences in average effect size between eight experiments using this approach and seven experiments comparing the control group only to those who completed the treatment (so-called "treatment on treated" designs).

Figure 3 Effect Size Partial *r* of Manipulated vs. Measured Financial Literacy as a Function of Financial Behavior Studied



Might effects of interventions be lower for educational treatments not freely chosen? In 75 quasi-experimental or pre-post tests, we found no differences in average effect size between 6 studies where the students self-selected to join the intervention, 37 studies where the intervention was imposed, or 32 studies that did not specify the role of student choice in enrollment. One cannot explain the weak effects of financial education by arguing that that effect sizes are biased downward by analyses penalizing treatment groups when those assigned to treatment interventions fail to enroll or by arguing that forced education is less effective than freely chosen education.

Table 1 shows that financial literacy interventions had smaller effects on behavior in low-income samples than in general population samples; moreover, in studies of measured literacy, effects on behavior were marginally weaker in low-income samples than in general population samples. We ran a meta-regression model with financial literacy (manipulated versus measured, coded -1 and $+1$, respectively), income (low-income sample versus general population sample, coded -1 and $+1$), and their interaction. The interaction was not significant ($B = 0.003$, $t = 0.96$, $p = 0.34$), and we found the expected significant effects of manipulated versus measured financial literacy ($B = 0.047$, SE $= 0.003$, $t = 14.60$, $p < 0.001$) and an effect of income ($B = 0.008$, SE $= 0.003$, $t = 2.37$, $p = 0.02$).

Might effect sizes of manipulated financial literacy depend on the type of intervention? We found statistically significant but practically small differences among counseling, exposure to information about financial education, financial education in high school, multiple sources of financial education, participation in seminars or workshops, and participation in a program of financial education. These

Table 1 Effect of Manipulated and Measured Financial Literacy by Income of Participants

Type of intervention	Number of raw effects	Total *N*	Sample-weighted average partial *r*	95% CI Lower bound	95% CI Upper bound	% variance explained
Manipulated literacy (low-income sample)	35	80,014	0.025	0.018	0.032	0.06
Manipulated literacy (general population sample)	55	271,759	0.035	0.031	0.038	0.12
Measured literacy (low-income sample)	8	6,975	0.113	0.089	0.136	1.27
Measured literacy (general population sample)	103	245,248	0.134	0.130	0.138	1.80

intervention forms explained, respectively, 0.14%, 0.05%, 0.15%, 0.12%, 0.18%, and 0.10% of the variance in the financial behaviors studied.

Our meta-analysis so far makes three main points. First, econometric and correlational studies of measured financial literacy show significantly larger effect sizes than studies of the effects of manipulated financial education interventions. Interventions on average explain only 0.1% of the variance in the behaviors they attempt to influence.

Second, within each subset (manipulated and measured), putatively more rigorous designs lead to smaller effect sizes. True randomized experiments lead to smaller effect sizes than less rigorous quasi-experimental and pre-post designs, consistent with Collins and O'Rourke (2010). Among studies of measured literacy, studies using instrumental variables find smaller effects than studies using simple cross-sectional designs and OLS. Moreover, studies that use instrumental variables find smaller effects using that estimation strategy than when they use OLS on the same data sets. Lusardi and Mitchell (2014) claim that certain studies using instrumental variables find larger effect size estimates than were found by OLS. But our meta-analysis of the entire set of papers using instrumental variables clearly shows the opposite to be true, on average. It is true that, in some studies, unstandardized coefficients are dramatically larger with instrumental variables compared to OLS (Meier 2011), but standardized coefficients clearly show smaller average effects with instruments.

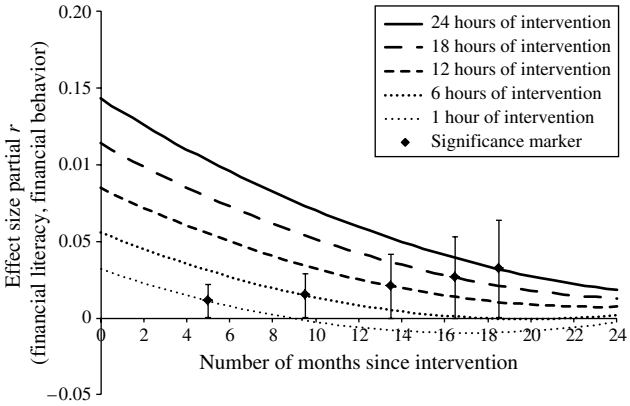
Third, one can see from Figure 2 that effect size estimates using instrumental variables are far higher than from experiments. Econometric studies using instrumental variables for financial literacy are sometimes held up as equivalent to quasi experiments in power to support causal claims, notwithstanding that these studies do not show a way to translate to effective educational interventions. But there is no overlap between the 95% confidence intervals for effects of financial literacy in these studies using instrumental variables and either the group of quasi experiments or the true randomized control experiments that remain the gold standard for causal inference.

Something is causing these studies using instrumental variables to produce larger effect sizes, even though instrumental variables seem to be partially effective in controlling for alternative explanations present in correlational studies that do not use instrumental variables. Appendix A shows that 8 of 24 studies that use instrumental variables either fail tests for nonweak instruments or do not report tests (cf. Angrist and Pischke 2009). In those 8 papers, we find no difference between average effect size with OLS (average effect size = 0.106) and instrumental variables (average effect size = 0.109). In the 16 studies with good instruments, authors report stronger effects with OLS (average effect size = 0.091) than with instrumental variables (average effect size = 0.059). Moreover, of studies that report passing tests for nonweak instruments, a subset might cause readers to wonder whether instruments used would satisfy the requirement of no partial relationship with the financial behaviors studied except via financial literacy. We return to this issue in §4 of this paper.

2.2.2. Why Does Manipulated Literacy Have Weaker Effects Than Measured Literacy? Why are effects of financial education interventions so weak, and why are effects stronger when financial literacy is measured rather than manipulated? We offer two answers to these questions pertaining to intervention studies and a third pertaining to measured literacy studies.

Explanation 1: *Intervention effects decay over time—the case for "just-in-time" financial education*. Effect sizes for interventions may be small because effects of interventions decay. We examine the effects of the intensity of the intervention and of the delay between intervention and measurement of financial behavior using meta-regression analysis. Most studies omit key information about intervention details, but 33 papers reported a mean of 9.7 hours of instruction (SD = 11.9), and 29 papers reported a mean delay of 11 months between intervention and measurement

Figure 4 Partial Correlation of Financial Education Interventions with Financial Behavior as a Function of Number of Hours of Intervention and Number of Months Since Intervention



of behavior (SD = 12.4). Our model regressed effect size (r or partial r) on linear effects of mean-centered number of hours of instructions, linear and quadratic effects of number of months between intervention and measurement of behavior, and the interaction of their linear effects. Figure 4 shows the estimated response surface from this meta-regression model.

The meta-regression analysis revealed a positive linear simple effect of the number of hours of instruction on the effect sizes at average delay ($B = 0.0032$, SE = 0.0004, $t = 8.35$, $p < 0.0001$). More hours of instruction produce larger effects on downstream behaviors. For the number of months between intervention and measurement of behavior at the average number of hours of instruction, there was a negative simple linear effect of delay ($B = -0.0033$, SE = 0.0009, $t = -3.53$, $p = 0.002$) and a positive quadratic effect ($B = 0.00014$, SE = 0.00004, $t = 3.48$, $p = 0.002$)—i.e., effect size of interventions decayed with delay, but at a decreasing rate. Finally, there is a significant interaction of the linear effects of hours of instruction and delay ($B = -0.0002$, SE = 0.00003, $t = -6.76$, $p < 0.0001$).

In Figure 4, decay over time is stronger for larger interventions. The significance marker on each hours-of-intervention curve indicates the number of months since intervention when the corresponding 95% CI on predicted effect size r no longer excludes zero. Importantly, at delays of 18.5 months or greater, there is no significant effect of even 24 hours of instruction. After 23.5 months of instruction, there are no significant effects of amount of instruction. But brief interventions at short delays have effects equal to more intensive interventions at long delays. We observe equal effects for 6 hours of intervention at no delay and 18 hours of intervention at 10 months of delay, and equal effects of 1 hour of instruction at no delay and 12 hours at 10 months of delay. We argue below that these findings militate toward "just-in-time" financial education rather than lengthy education years before the behaviors it is intended to change (cf. Mandell 2006).

Explanation 2: *Financial education produces weak effects on financial knowledge that is presumed to cause financial behavior*. Another explanation for why the effects of interventions are much weaker than the effect of measured financial literacy is that financial education yields surprisingly weak changes in financial knowledge presumed to cause financial behavior. In 12 papers reporting effects of interventions on both measured literacy (knowledge) and some downstream financial behavior, the interventions explained only 0.44% of the variance in financial knowledge. By comparison, meta-analyses in other domains of education show interventions explain 5 to 13 times as much variance in acquired knowledge from science and math instruction (2.25%), organizational and work setting interventions (5.76%), and special topic interventions from creative thinking to career counseling (5.29%) (Lipsey and Wilson 1993). Our analysis includes only effect sizes of education on knowledge gains in studies that also measure financial behavior. If others replicated our estimated effect sizes of education on knowledge gains in the larger set of studies that include no measure of financial behavior, this would suggest that something is amiss in how financial education is now being delivered.

Explanation 3: Is there omitted variable bias in studies of effects of measured financial literacy? Perhaps measured financial literacy has larger effects than manipulated financial literacy because effect size estimates for measured financial literacy in econometric studies may be inflated. Scores on financial literacy tests may predict behavior because of their correlation with other unmeasured variables. Our meta-analysis indicates that the prior research that controls for omitted variable bias with instrumental variables on average finds weaker effects than studies that use OLS regression.

In our empirical studies, we replicate results found in the 87 studies in our meta-analysis that used OLS regression to examine links between measured financial literacy and financial behavior; we show how results change with addition of certain traits that are arguably correlated with both financial literacy and with the financial behaviors predicted by financial literacy. In addition, we attempt to control for omitted variable bias by using instrumental variables, similar to the 24 studies in our meta-analysis that examined links between measured financial literacy and financial behavior using instrumental variables. Our designs are simple cross-sectional designs typical of the 87 OLS studies, and thus do not permit strong claims about causality. Our results do however serve as a concrete evidence of the possibility of omitted variable bias in a large body of measured literacy studies just analyzed.

3. Empirical OLS Studies: Apparent Effects of Financial Literacy Due to Omitted Variable Bias?

In our meta-analysis, we found larger effect sizes in the 87 studies that tested effects of measured financial literacy on behaviors using OLS regression than in studies using designs that many might consider "better." We conjecture that this may reflect omitted variable bias in the estimates from the OLS designs. To test this conjecture, we conducted three primary research studies of U.S. English-speaking adults. With Studies 1 and 2, we sought to develop a short, reliable, and valid measure of financial literacy from a pool of 26 financial literacy items used in prior research. With Studies 2 and 3, we also sought to replicate findings that financial literacy predicted financial behaviors, and test whether the effects of financial literacy diminish in the presence of four traits correlated with financial literacy and the financial behaviors studied. Study 3 was very similar to Study 2, but used a true probability sample of U.S. adults. Study 3 included measures of several traits that audience members suggested might be related to our four focal traits when we had presented this work. Appendix A shows all items of the measures included in all studies. Tables C.1, C.2, and C.3 in Appendix C show summary statistics and correlations among all variables for Studies 1, 2, and 3, respectively.

3.1. Deriving a Unidimensional, Reliable, and Valid Scale of Financial Literacy

Prior research on measured financial literacy had created measures that were not subjected to standard psychometric tests to establish unidimensionality, reliability, and validity, and to establish that the measures of financial literacy represented a construct that was distinct from other correlated traits (Bearden et al. 2011; cf. Knoll and Houts 2012). In Study 1 we developed a 13-item scale with excellent psychometric properties, distilled from 26 items used in numerous prior studies. See Appendix B for the final 13-item scale and Web Appendix B for Study 1 details. We replicated these findings supporting the excellent psychometric properties of our 13-item scale in Study 2. The primary purpose of Study 2, though, was to test whether the predictive effects of financial literacy changed when we controlled for four traits that we believed to be plausibly correlated with both financial literacy and five financial behaviors studied in papers covered in our meta-analysis. Study 3 replicates Study 2 with a probability sample. We present the methods of the two studies first and then their combined results. We also show very similar conclusions about the diminished effects of financial literacy on financial behaviors when one controls for omitted variables by use of instrumental variables—mirroring our meta-analysis.

3.2. Do Predictive Effects of Financial Literacy on Financial Behavior Change When Controlling for Correlated Traits?

3.2.1. Study 2 and Study 3 Methods. In Study 2, we surveyed 543 U.S. adults from a Qualtrics panel. We again included demographics, the 26 financial literacy items, and four traits that we had hypothesized would be correlated with both financial literacy and the financial behaviors to be described below.

- First, we conjectured that numeracy might correlate with both financial literacy and with some financial behaviors. A number of financial literacy items used in past research have numerical reasoning components. More numerate consumers have been shown to be better able to retrieve numerical principles to reason numerically, e.g., about debt repayment (Peters et al. 2006, Soll et al. 2013).
- Propensity to plan for the use of money reflects setting spending goals, thinking about subgoals and means of achieving them, thinking about constraints and interdependencies, and liking planning versus spontaneity in spending. This trait has been linked to FICO scores (Lynch et al. 2010), wealth accumulation (Ameriks et al. 2003), and financial literacy (Lusardi and Mitchell 2009).
- Confidence in information search—the degree to which an individual feels capable and assured with respect to marketplace decisions and behavior—has been linked to proactive information, processing, and consideration set formation (Bearden et al. 2001). Similar to self-efficacy, it reflects a core belief that one has the power to produced desired effects, facilitating success in tasks where approach and persistence requires the self-belief that gives one an incentive to persevere (cf. Bandura and Locke 2003). Many financial mistakes are the result of inaction and avoidance that have motivated research on various kinds of auto-enrollment (Madrian and Shea 2001).
- Willingness to take prudent investment risks is a critical antecedent of accumulating wealth by investing, for example, in saving for retirement. Jianakoplos and Bernasek (1998) have shown that lower willingness to take risks by women compared to men predicts gender differences in wealth accumulation. Iyengar and Kamenica (2010) noted that proliferation of 401(k) options leads to avoidant choices of low-risk investment at the expense of equities.

Our five dependent measures were self-reported financial behaviors representative of those most heavily used in research reviewed in our meta-analysis: (1) a yes/no measure of saving for an emergency fund (Lusardi and Mitchell 2007b); (2) a yes/no measure of figuring out how much savings is needed for retirement (Lusardi and Mitchell 2007b); (3) four yes/no questions about performing positive

savings/investment behaviors summed to form an overall score ranging from 0 to 4 (Lusardi and Tufano 2009); (4) a rating of how do you think banks or credit card companies would rate your credit? 1 = very poor, 10 = excellent, found by Lynch et al. (2010) to correlate 0.85 with FICO credit scores; and (5) three multiple choice items assessing bouncing checks and late or incomplete credit card payments (credit and checking fees). We summed these three items to form an overall score that could range from 3 to 14 (Mandell and Klein 2009).

In Study 3, GfK Knowledge Networks provided us a nationally representative sample of 506 English-speaking adults aged 21–65. Study 3 collected the same measures as Study 2 with the addition of four more traits—generalized self-efficacy (Chen et al. 2001), delayed gratification (Hoerger et al. 2011), restraint, and impulsivity (Maloney et al. 2012)—that have been suggested to us by other scholars as candidate "omitted variables." The rationale for generalized self-efficacy was to distinguish this presumably stable and very general trait from confidence in financial information search. Delayed gratification and impulsivity measures were included to distinguish these intertemporal traits from propensity to plan. Study 3 also collected a five-item measure of willingness to take investment risk (Weber et al. 2002) and an eight-item numeracy measure (Soll et al. 2013) that arguably had better psychometric properties than the corresponding measures used in Study 2. The financial behavior dependent variables were identical to those in Study 2.

3.2.2. Study 2 and Study 3 Hierarchical OLS Regression Results. In each study, we predicted each financial behavior with demographics, a 13-item financial literacy scale, and four traits potentially correlated with both financial literacy and the financial behaviors: confidence in financial information search, planning for money long term, willingness to take investment risk, and numeracy. We compared three hierarchical regression models: Model (1) demographics alone; Model (2) demographics + financial literacy; Model (3) demographics + financial literacy + four correlated traits. Models 1, 2, and 3 had 21, 22, and 26 model degrees of freedom, respectively, in Study 2, and 19, 20, and 24, model degrees of freedom, respectively, in Study 3.

Table 2 shows the fit of Models 1, 2, and 3 for each of the five behaviors predicted in Study 2, the test of the effect of financial literacy in Model 2, and the tests of the effect of financial literacy and four related traits in Model 3. Table 3 shows the corresponding statistics for Study 3. In Tables 2 and 3, we omit coefficients for demographic controls to save space; Tables WC1 and WC2 in Web Appendix C show the complete table including coefficients on demographics.

We used OLS regression for the scaled financial behaviors, but used logistic regression for the dichotomously-measured financial behaviors. We report in Tables 2 and 3 the unstandardized regression coefficients, the standard errors, and the Exp(β) or logistic regression. Significant Exp(β) values > 1 indicate that the odds of an outcome (being placed in the group coded as 1) increase with a unit change in the independent variable; significant Exp(β) values < 1 indicate that a unit increase in the predictor leads to a decrease in the odds of an outcome occurring. For example, in Table 2, for Study 2, "saving for an emergency fund," the predictor *planning for money–long term* has an Exp(β) value equal to 1.620. Thus, for each one-point increase in this predictor on its 1–6 scale, the odds (P(Yes)/P(No)) of having an emergency savings fund increased by a multiple of 1.620.

The reader can see three things from Tables 2 and 3. First, for all five financial behaviors in both studies, our Model 2 results replicated prior findings showing significant beneficial effects of financial literacy after controlling for demographic variables. Second, in both studies, adding four other traits to Model 3 caused R^2 values to increase by an average of 51% compared with Model 2. This was mostly due to propensity to plan, confidence in information search, and willingness to take investment risks. Confidence in financial information search had stronger effects than (single item) willingness to take investment risks in Study 2, but the reverse was true in Study 3, which used the psychometrically sounder Weber et al. (2002) scale. Propensity to plan for the use of money was a strong predictor for all five behaviors in Study 3 and for three of five behaviors in Study 2. Third, in Model 3, financial literacy remained significant only for our index of positive savings and investment behaviors in Study 2, and for that behavior and for determining how much savings is needed for retirement in Study 3.

Other robustness analyses of Study 3 compared Model 3 with a Model 4 that added the following as predictors: generalized self-efficacy (Chen et al. 2001), delayed gratification (Hoerger et al. 2011), restraint, and impulsivity (Maloney et al. 2012). For none of the five behaviors was the Model 4 R^2 significantly greater than that for Model 3, nor did one of the added traits have a significant partial effect. We concluded that predictive effects of confidence in financial information search were not plausibly caused by its correlation with general self-efficacy, and predictive effects of propensity to plan were not plausibly caused by its correlation with a delay of gratification and self-control.

Because all constructs were measured in the same surveys, it is not possible to make claims that our covariates of confidence in information search,

Table 2 Study 2 OLS/Logistic Regression for Models 2 and 3

	Saving for an emergency fund	How much is needed for retirement	Banks and credit card firms rate credit score	Credit and checking fees	Positive savings/ investment behaviors
Study 2	$N = 475$	$N = 477$	$N = 483$	$N = 373$	$N = 483$
Model 2					
Financial literacy	0.070*	0.118**	0.125**	−0.076*	0.116**
	(0.036)	(0.038)	(0.042)	(0.040)	(0.017)
	*1.073**	*1.126***			
Model 3					
Financial literacy	0.059	0.088	0.061	−0.053	0.128**
	(0.044)	(0.046)	(0.046)	(0.045)	(0.019)
	1.062	*1.092*			
Numeracy	−0.049	−0.020	0.080	0.019	−0.068**
	(0.054)	(0.055)	(0.056)	(0.055)	(0.023)
	0.952	*0.980*			
Consumer confidence investing	0.574**	0.609**	0.609**	−0.505**	0.092
	(0.144)	(0.148)	(0.140)	(0.137)	(0.058)
	*1.776***	*1.839***			
Planning for money–long term	0.482**	0.244*	0.317**	−0.138	0.045
	(0.115)	(0.114)	(0.118)	(0.115)	(0.048)
	*1.620***	*1.277**			
Willing to take investment risks	0.006	0.221*	−0.068	0.002	0.112**
	(0.088)	(0.091)	(0.090)	(0.089)	(0.037)
	1.006	*1.248**			
R^2 Model 1 (only demographics)	0.136	0.157	0.255	0.163	0.340
R^2 Model 2 (Model 1 + financial literacy)	0.143	0.175	0.269	0.166	0.400
R^2 Model 3 (Model 2 + psychol. traits)	0.248	0.267	0.330	0.227	0.443

Notes. Standard errors are in parentheses below coefficients. For "Saving for an emergency fund" and planning "How much is needed for retirement," logistic regression is used, and R^2 is the Cox and Snell R^2. Furthermore, the odds ratio coefficients of logistic regression (Exp(β)) are presented in italics for these two dependent variables. OLS regression is used for "Banks and credit card firms rate credit score," "Credit and checking fees," and "Positive savings/investment behaviors." Model 1 used just the demographic variables as predictors, Model 2 used the demographic variables and *Financial literacy* as predictors, and Model 3 used the demographic variables, *Financial literacy*, *Numeracy*, *Consumer confidence Investing*, *Planning for money–long term*, and *Willing to take investment risks* as predictors.

*$p < 0.05$; **$p < 0.01$.

propensity to plan, willingness to take financial risks, and numeracy are causes of the financial behaviors studied. The data are equally consistent with four interpretations: (1) those covariates cause financial behaviors, with financial literacy spuriously related to financial behaviors; (2) financial literacy causes those covariates, which then in turn cause the financial behaviors; (3) financial literacy causes the financial behaviors, which in turn cause the covariates; (4) the financial behaviors cause both financial literacy and the covariates. Other papers report similar cross-sectional data to our studies and assert a causal link from financial literacy to planning and from planning to financial behavior (e.g., Lusardi and Mitchell 2007a). Although we are not entirely persuaded by the evidence offered in these prior studies, our data similarly cannot sort this out. When two or more interpretations are consistent with an observed data pattern, all theories consistent with the data should logically have increased posterior probability in a Bayesian updating process (Brinberg et al. 1992).

Our aim here is simple—to highlight a negative point that prior investigations failed to control for those omitted traits. Thus, we are more interested in the coefficient on financial literacy than on the coefficients on those other traits. Still, given a potential concern for endogeneity, or reverse causality among financial literacy and financial outcomes in our primary research findings and the 87 OLS studies, we now present the results of robustness analyses based on 2SLS regression with an instrumental variable for financial literacy.

4. Controlling for Omitted Variable Bias by Use of Instrumental Variables

Others have argued that designs using instruments for financial literacy and 2SLS are superior to OLS regressions in controlling for endogeneity and omitted variable bias, and similar to quasi experiments in ability to support causal inferences. A properly

Table 3 Study 3 OLS/Logistic Regression for Models 2 and 3

	Saving for an emergency fund	How much is needed for retirement	Banks and credit card firms rate credit score	Credit and checking fees	Positive savings/ investment behaviors
Study 3	$N = 410$	$N = 410$	$N = 455$	$N = 370$	$N = 455$
Model 2					
Financial literacy	0.115**	0.239**	0.192**	−0.135**	0.134**
	(0.039)	(0.044)	(0.042)	(0.040)	(0.017)
	*1.116***	*1.269***			
Model 3					
Financial literacy	0.013	0.166**	0.089	−0.070	0.098**
	(0.055)	(0.056)	(0.049)	(0.046)	(0.020)
	1.012	*1.180***			
Numeracy	0.111	0.160*	0.134*	−0.045	0.047
	(0.078)	(0.077)	(0.069)	(0.065)	(0.028)
	1.117	*1.174**			
Consumer confidence investing	0.195	−0.245	0.237	−0.341**	−0.011
	(0.159)	(0.157)	(0.138)	(0.129)	(0.057)
	1.214	*0.783*			
Planning for money–long term	0.805**	0.650**	0.419**	−0.472**	0.124*
	(0.143)	(0.139)	(0.119)	(0.111)	(0.049)
	*2.235***	*1.915***			
Willing to take investment risks	0.672**	0.616**	0.450**	−0.139	0.283**
	(0.175)	(0.176)	(0.149)	(0.140)	(0.061)
	*1.956***	*1.851***			
R^2 Model 1 (only demographics)	0.192	0.232	0.240	0.161	0.396
R^2 Model 2 (Model 1 + financial literacy)	0.206	0.286	0.272	0.186	0.465
R^2 Model 3 (Model 2 + psychol. traits)	0.346	0.349	0.344	0.295	0.507

Notes. Standard errors are in parentheses below coefficients. For "Saving for an emergency fund" and planning "How much is needed for retirement," logistic regression is used, and R^2 is the Cox and Snell R^2. Furthermore, the odds ratio coefficients of logistic regression (Exp(β)) are presented in italics for these two dependent variables. OLS regression is used for "Banks and credit card firms rate credit score," "Credit and checking fees," and "Positive savings/investment behaviors." Model 1 used just the demographic variables as predictors, Model 2 used the demographic variables and *Financial literacy* as predictors, and Model 3 used the demographic variables, *Financial literacy*, *Numeracy*, *Consumer confidence investing*, *Planning for money–long term*, and *Willing to take investment risks* as predictors.

*$p < 0.05$; **$p < 0.01$.

chosen instrument should satisfy the "exclusion restriction"—there should be no partial effect of the instrumental variable on the financial behavior dependent variable controlling for the endogenous variable of financial literacy. Meier (2011) argued that instrumenting for financial literacy is difficult due to the possibility of not controlling completely for omitted stable traits.[2]

We used a scale of need for cognition (Epstein et al. 1996) as an instrument for financial literacy not plausibly caused by financial behaviors. Need for cognition (NFC) is a stable personality trait referring to a person's tendencies to engage in effortful thought, to entertain and evaluate ideas, and to ignore irrelevant information. Past research has shown that NFC is positively related to the Big Five personality dimensions of openness to experience and conscientiousness and negatively related to neuroticism, and positively related to rational versus experiential thinking (Sadowski and Cogburn 1997).

NFC is correlated with financial literacy ($r = 0.35$ in Study 2, and $r = 0.31$ in Study 3). Second, in both Studies 2 and 3, there is no correlation between NFC and the residual of the effect of financial literacy on four of five behaviors controlling for demographics. This suggests that NFC is exogenous and a suitable instrument for 8 of 10 tests; the exceptions were for positive savings and investments in Study 2 and how much is needed for retirement in Study 3.

For all five behaviors in Studies 2 and 3, we estimated an instrumental variable equivalent to our Model 2 in our OLS estimations with only financial literacy and the demographics as independent variables. We used NFC as an instrument for financial literacy in our first stage equation, and in our second stage equation, we predicted each of the five behaviors using only the predicted value of

[2] For example, Jappelli and Padula (2011) instrumented for financial literacy with math skills at age 10. Meier (2011) asked whether it is possible that math skills might have a direct effect on outcomes due to correlation with some other stable trait such as general intelligence or intertemporal impatience.

financial literacy as an independent variable along with demographic covariates. NFC was a "nonweak" instrument, with an F-statistic from the first stage of 2SLS > 10 (Stock et al. 2002): for the first stage, $F = 48.30$, $p < 0.001$ for Study 2; $F = 51.82$, $p < 0.001$ for Study 3.

In our second-stage models, financial literacy was not significant for any of the five behaviors in Study 2. For Study 3, financial literacy was significant for two of five behaviors (figuring out how much savings are needed for retirement, and positive savings and investment behaviors), and was marginally significant for saving for an emergency fund. See Tables WC3 and WC4 in Web Appendix C.

5. Conclusion

The widely shared intuition that financial education should improve consumer decisions has led governments, businesses, and nongovernmental organizations worldwide to create interventions to improve financial literacy. These interventions cost billions of dollars in real spending and larger opportunity costs when these interventions supplant other valuable activities. Our meta-analysis revealed that financial education interventions studied explained only about 0.1% of the variance in the financial behaviors studied, with even weaker average effects of interventions directed at low-income rather than general population samples. Education effects on knowledge of material taught were also small compared to education effects on knowledge gains in other seemingly comparable domains.

5.1. Study Methodology Affects Apparent Size of Financial Literacy Effects

Our meta-analysis found much larger effects on financial behavior when financial literacy was measured rather than manipulated. We conjecture that this may reflect omitted variable bias in studies of measured financial literacy. Measured literacy studies that attempted to control for omitted variable bias using instrumental variables and 2SLS regression showed much smaller effect sizes than measured literacy studies using OLS regression, and smaller effect sizes than the OLS estimates coming from the same studies. As consumer researchers who study individual differences, we saw from our own work and the work of others that there were plausible stable traits that might be correlated with financial literacy and with the financial behaviors studied in this literature.

To make this latter possibility more concrete, we conducted our own primary research studies that attempted to replicate the most common patterns found in the literature. In Studies 2 and 3, for all five behaviors, we replicated prior findings showing that financial literacy significantly predicted financial behavior after controlling for demographics. But in both studies, for all five behaviors, adding to those models the measures of confidence in financial information search, propensity to plan, willingness to take financial risks, and numeracy caused effects of financial literacy to become nonsignificant for four of five financial behaviors in Study 2 and three of five in Study 3. When we take an alternate approach to controlling for omitted variable bias—using need for cognition as an instrument for financial literacy—we draw very similar conclusions: financial literacy remains significant in only 3 of 10 tests. Moreover, the two approaches to controlling for omitted variable bias reached the same conclusion about the significance or lack of significance of financial literacy for 8 of 10 tests. All of this suggests the presence of omitted variable bias in our Model 2 OLS estimates and those in the 87 OLS studies in our meta-analysis.

Our cross-sectional research designs do not permit positive claims that these other traits cause the financial behaviors. We instead make a negative point: past work considered to support a causal role for financial literacy might need revisiting—particularly the 87 studies in our meta-analysis that used OLS and that produced far larger effects of financial literacy on financial behavior than studies using other methods. As in our meta-analysis, effects of financial literacy diminish dramatically when one attempts to control for omitted variable bias.

Arguably, in our meta-analysis, if the instruments were successful in producing a design comparable to a quasi experiment, effect sizes should match what one finds in intervention studies that manipulate financial education. But we found that intervention studies show much smaller effects than econometric studies with instrumental variables, perhaps because the instruments used for financial literacy were not entirely successful. If so, this would imply upward bias in even the small effect sizes uncovered using instruments. It is sometimes hard to tell from published reports why a particular instrument was chosen (cf. Larcker and Rusticus 2010). Nor is it clear what other instruments might have been tried, much as it can be opaque to readers of reports of experiments what covariate combinations were tried (cf. Simmons et al. 2011).

The greater magnitude of effects of measured literacy in the 24 studies with instrumental variables compared to the 90 studies of manipulated financial literacy need not signal incomplete control for omitted variable bias. Measured literacy reflects the cumulative effects of all information over an individual's lifetime that affects financial knowledge. In contrast, the manipulated financial literacy studies test the effect of a small subset of that information contained in the educational "dose." This is analogous to the finding in marketing that a given advertisement

may have a very small effect on behavior, but the long-term effects of cumulative advertising can be strong (Mela et al. 1997). This "education is cumulative" interpretation is plausible, but it raises the question of how much education would be required for a specific initiative to have a measurable effect, and at what cost.

Our view is that one should not use the larger effect sizes in 24 instrumental variable studies of measured literacy than in the 90 studies of manipulated literacy as a justification for expenditures on financial education of the same sort tried so far. We need research that demonstrates larger effects of financial education interventions. Alternatively, we need rigorous tests of the "education is cumulative" interpretation, using appropriate designs to estimate long-term cumulative effects, as in these marketing studies. Those methods involve time series designs with panel data following a common set of individuals over time. Such designs are absent in the literature we reviewed.

5.2. Implications for Research on Financial Education

Our paper shows how much one can learn by focusing not on the individual study, but on regularities that can be meta-analyzed across a large body of studies. But one cannot meta-analyze for the effects of some study characteristic that is not reported by the authors. It is striking how many papers do not give enough details about their financial education interventions to be able to code for variables that might plausibly affect their effect sizes. For the field to accumulate knowledge more rapidly, financial education scholars must agree on a set of study characteristics that will be described in every paper. We suggest that future studies should describe in their method sections key characteristics of the instruction (e.g., contact hours, delay in measurement, curriculum details), the instructors (e.g., instructor training and financial literacy, competing sources of financial advice), and the instructed (e.g., age relative to age encountering behaviors to be influenced, income, role in family financial decision making).

In studies of measured financial literacy, there is a marked disconnect between the conceptual definition of financial literacy as a skill and form of expertise and how it has been operationalized. Alba and Hutchinson (1987, p. 411) define consumer expertise as "the ability to perform product-related tasks successfully." In contrast, the operational definition of financial literacy that has dominated the literature is knowledge of financial facts like compound interest and financial product attributes. Our meta-analysis has shown that when one attempts to control for omitted variable bias, there is a weak relationship between this "financial literacy" and behavior. Future work should develop more promising measures more connected to the conceptual definition of financial literacy as a form of consumer expertise.

5.3. Implications for Financial Education and Policy to Help Consumer Decisions

Our conclusions are about forms of financial education that have been subjected to empirical evaluation. Those wishing to draw policy conclusions from our work must understand that many innovative forms of financial education have never been studied empirically. That said, our findings for the interventions studied so far make clear that different approaches to financial education are required if one expects to produce effects on behavior larger than the very small effects we found.

What is unclear is why educational interventions investigated thus far have been unsuccessful. Perhaps teacher training and expertise are lower than with other subjects, or education is hampered by inability to clearly state normative behavior rather than "it depends" when neither teachers nor students can anticipate future circumstances. Or perhaps financial education faces unusual competition for "share of voice" versus many other sources of inexpert and biased advice.

Our findings provide hints for future directions for improving financial education. Perhaps future education should teach soft skills like propensity to plan, confidence to be proactive, and willingness to take investment risks more than content knowledge about compound interest, bonds, etc. (Hader et al. 2013). In our meta-analysis, measured knowledge of financial facts had a weak relationship to financial behavior in econometric studies controlling for omitted variable bias.

Moreover, our findings in Figure 4 showing decay of effects of financial education interventions imply that content knowledge may be better conveyed via "just-in-time" financial education tied to a particular decision, enhancing perceived relevance and minimizing forgetting. It may be difficult to retrieve and apply knowledge from education to later personal decisions with similar relevant principles but different surface details (Thompson et al. 2000), particularly decisions coming years after the education. Our findings suggest reexamining efforts at child and youth financial education, particularly if intended to affect behaviors after a significant delay. There must be some immediate opportunity to enact and put to use knowledge or it will decay. Moreover, without a ready expected use in the near future, motivation to learn and to elaborate may suffer.

Research is also needed about the effects of interventions attempting to train multiple skills and affect multiple behaviors versus interventions aimed at single behaviors. Multiple-skill, multiple-behavior programs may have certain disadvantages. One might

expect learners to perceive less relevance and give less attention with broad-based programs, and it is less likely that what is learned is "just-in-time" financial education. Moreover, insofar as an intervention attempts to change multiple behaviors rather than single behaviors, motivation to follow through may suffer (Dalton and Spiller 2012, Soman and Zhao 2011).

Thus far we have not considered alternatives to financial education. An open question is the role that financial education should play in the policy mix. Public policy tools drawn from economics point to three broad classes of interventions to help consumers make better decisions: offering more choices, providing better information to consumers about options they might consider, and providing incentives for consumers or sellers to change their behavior (Lynch and Wood 2006). Financial education is a form of information remedy. An inherent weakness of information remedies is that, for the most part, they aim to influence consumers' evaluations of options; consumer research shows that far more variance in chosen behavior is controlled by affecting the "consideration set" of actively considered options. Put another way, changing consumers' evaluation of options does not matter if consumers are not considering those options in the first place.

Others have advocated defaults, "nudges," and "choice architecture" such as opt-out retirement savings plans and "plain vanilla" financial products as less costly and more effective alternatives to financial education (Choi et al. 2003, Thaler and Sunstein 2008, Barr et al. 2009, Boshara et al. 2010). These approaches bring assistance close to the time of related decisions. They blend incentives with information, making it easy to consider and choose more desirable behaviors, and slightly more costly to consider and choose alternatives deemed less desirable by a social planner. But defaults work best when almost all consumers have similar needs. When needs are heterogeneous, one needs to know something to decide for oneself. Here, "just-in-time" financial education may have promise, alone and embedded in decision support systems that help identify a tailored consideration set of safe options.

Some might argue that the heterogeneity problem can be solved without financial education by "smart defaults" that tailor the default based on personal characteristics (Goldstein et al. 2008). Smart defaults are like recommender systems or "smart agents" in that their success depends entirely on the correlation between the order of recommendations and the ordering of options by the consumer's individual utility function if fully informed (Diehl et al. 2003). Some smart defaults like target date retirement funds customize only on one or two demographics. In marketing, it is well known that demographics may predict group behavior, but are weakly correlated with individual preferences (Bass et al. 1968). In many markets, adequate customization requires a deeper interactive conversation; consumers need some level of just-in-time financial education to inform such conversations.

Just-in-time financial education might be embedded in more deeply customized recommender systems and decision support systems for financial decisions (Lynch and Woodward 2009) or in the form of coaching (Collins and O'Rourke 2010). Coaching has the advantage of high relevance, low propensity for forgetting between information receipt and behavior, and opportunities to learn from feedback. Recommender systems are encouraged by "smart disclosures" that require sellers of financial products to disclose their features in a machine-readable form that can then be packaged by trustworthy "infomediaries" to develop recommender systems (Thaler 2012, White House National Science and Technology Council 2013). Future research should focus on these kinds of tools and on the problem of how to reach consumers at a point in time close to their decision when they are impatient for closure.

It is also important in future work to consider how the specific behavior affects the tools in the policy mix that are likely to be more effective. Four key dimensions are whether the mistakes reflect behaviors under intentional control versus the control of habit or emotion; the frequency of the behavior that leads to the mistake; whether the mistakes pertain to situations that can or cannot be readily foreseen by the consumer and any policy maker trying to help improve the decision; and whether the consumer mistakes to be corrected are errors of omission or commission.

Lynch and Wood (2006) argued that information remedies such as financial education or disclosures can be effective when behavior is under cognitive and intentional control, but not for behaviors with weak intention–behavior links. We found that financial literacy has less effect in low-income samples; the financial behavior of the poor is arguably more controlled by circumstances independent of intention (Bertrand et al. 2006). The poor have little financial slack and low fungiblility of money across periods to deal with financial shocks (cf. Zauberman and Lynch 2005). Lynch and Wood (2006) further note that information will have little effect on behaviors controlled by emotion or habit. Here, people are unlikely to retrieve the knowledge and apply it to the situation at hand. With emotionally controlled decisions such as compulsive spending, the primary role for education is in calling attention to the problematic behavior and pointing to precommitment tools for self-control coming out of behavioral economics. Behaviors under the control of habit are improved by changing habits, which research

has shown can occur much more readily when a consumer is in a transition from one environment to another (Verplanken and Wood 2006). For example, education aimed at changing college students' budgeting habits may be more effective at times of transition than midsemester. Arguably, both just-in-time financial education and nudges have more potential in redressing financial errors that are associated with very infrequent decisions, whose timing can be predicted by consumers and those trying to help them. If a mistake comes from an infrequent but legal behavior and the consumer cannot anticipate the occasion, cooling off laws are an appropriate remedy (Lynch and Zauberman 2006). If the mistake is falling prey to financial scams or frauds, unfortunately, cooling off laws provide little protection, nudges cannot help, and it is difficult for preventative educational interventions to be timely enough to be salient (cf. FINRA Investor Education Foundation 2013).

On the other hand, certain decisions are more predictable, such signing up for a 401(k) plan, buying a more expensive house than one can afford, or choosing a risky mortgage. For such decisions, the consumer perceives there is a large and important decision, and the policy maker has signals to predict its timing. If the decision is one where consumers are heterogeneous, just-in-time financial education may find a receptive audience. If consumers are homogeneous or if heterogeneity is of the sort that can be adequately addressed by "smart defaults," the policy maker can nudge at an appropriate time.

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Appendix A. Details of Studies Using Instrumental Variables in Meta-Analysis

Table A.1 Effect Sizes of Econometric Studies with Nonweak and Exogenous Instruments

Study	OLS regression effect size	Instrumental variables effect size	Instrument used	Weak instrument test used	Passed test?	Exogenous instrument test used	Passed test?
Alessie et al. (2011)	0.17	0.12	Financial experiences of siblings and parents	First-stage *F*-value	Yes	Hansen *J*	Yes
Behrman et al. (2010)	0.12	0.09	Macroeconomic conditions and family background	First-stage *F*-value	Yes	Hansen *J*	Yes
Calcagno and Monticone (2011)	0.11	0.06	Average financial literacy at regional level	First-stage *F*-value	Yes	Hansen *J*	Yes
Duca and Kumar (2012)	0.05	0.01	Whether respondent worked in a managerial or professional occupation	First-stage *F*-value	Yes	Hansen *J*	Yes
Fornero and Monticone (2011) sample 1	0.06	0.05	Cost of learning financial knowledge	First-stage *F*-value	Yes	Hansen *J*	Yes
Fornero and Monticone (2011) sample 2	0.05	0.08	Cost of learning financial knowledge	First-stage *F*-value	Yes	Hansen *J*	Yes
Jappelli and Padula (2011) sample 1	0.06	0.04	Math performance in school	First-stage *F*-value	Yes	Sargan test	Yes
Jappelli and Padula (2011) sample 2	0.08	0.06	Math performance in school	First-stage *F*-value	Yes	Sargan test	Yes
Kimball and Shumway (2007)	0.31	0.11	Financial education and demographics	First-stage *F*-value	Yes	Correlation with residuals	Yes
Klapper et al. (2011)	0.17	0.07	Number of newspapers in circulation and of universities per region	Kleibergen–Paap	Yes	Hansen *J*	Yes
Lusardi and Mitchell (2007a)	0.12	0.09	Background training in economics	First-stage *F*-value	Yes	Hansen *J*	Yes
Sekita (2011)	0.07	0.04	Individual and average regional Japanese skills	First-stage *F*-value	Yes	Hansen *J*	Yes
van Rooij et al. (2008)	0.12	0.06	Economic education, financial condition of siblings, and knowledge of parents	First-stage *F*-value	Yes	Hansen *J*	Yes
van Rooij et al. (2011)	0.18	0.06	Background training in economics	First-stage *F*-value	Yes	Hansen *J*	Yes
Yoong (2010)	0.19	0.08	Bond pricing knowledge	First-stage *F*-value	Yes	Hansen *J*	Yes
Zanghieri (2013)	0.06	0.04	Having a mortgage, risk aversion, and living in a region with a high bank density	First-stage *F*-value	Yes	Hansen *J*	Yes
Sample-weighted average effect size	0.091	0.059					

Table A.2 Effect Sizes of Studies with Weak, Endogenous, or Untested Instruments

Study	OLS regression effect size	Instrumental variables effect size	Instrument used	Weak instrument test used	Passed test?	Exogenous instrument test used	Passed test?
Bucher-Koenen and Lusardi (2011)	0.08	0.08	Voting share for the Libertarian party	First-stage *F*-value	No	Hansen *J*	Yes
Disney and Gathergood (2011)	0.05	0.08	Self-reported mathematical ability in school	None	—	None	—
Giofré (2012)	0.06	0.11	Lagged values of financial literacy	None	—	None	—
Kotlikoff and Bernheim (2001)	0.09	0.08	Macroeconomic knowledge	None	—	None	—
Lusardi and Mitchell (2009)	0.08	0.05	Lived in a state with mandated financial education at age 17	First-stage *F*-value	No	Hansen *J*	No
Lusardi and Mitchell (2011)	0.07	0.06	Exposure to mandate and length of time to mandate	None	—	None	—
Monticone (2010)	0.08	0.04	Trust in advisors and trust in banks	First-stage *F*-value	No	Hansen *J*	Yes
Mullock and Turcotte (2012)	0.15	0.15	Whether first language is English or French and is the one in charge of financial management at home	None	—	None	—
Sample-weighted average effect size	0.106	0.109					

Appendix B. Measures Used in Studies 1–3

Thirteen-Item Financial Literacy Measure (correct responses in italics)

(1) Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, would you be able to buy:

- More than today with the money in this account
- Exactly the same as today with the money in this account
- *Less than today with the money in this account*
- Don't know
- Refuse to answer

(2) Do you think that the following statement is true or false? "Bonds are normally riskier than stocks."

- True
- *False*
- Don't know
- Refuse to answer

(3) Considering a long time period (for example, 10 or 20 years), which asset described below normally gives the highest return?

- Savings accounts
- *Stocks*
- Bonds
- Don't know
- Refuse to answer

(4) Normally, which asset described below displays the highest fluctuations over time?

- Savings accounts
- *Stocks*
- Bonds
- Don't know
- Refuse to answer

(5) When an investor spreads his money among different assets, does the risk of losing a lot of money:

- Increase
- *Decrease*
- Stay the same
- Don't know
- Refuse to answer

(6) Do you think that the following statement is true or false? "If you were to invest \$1,000 in a stock mutual fund, it would be possible to have less than \$1,000 when you withdraw your money."

- *True*
- False
- Don't know
- Refuse to answer

(7) Do you think that the following statement is true or false? "A stock mutual fund combines the money of many investors to buy a variety of stocks."

- *True*
- False
- Don't know
- Refuse to answer

(8) Do you think that the following statement is true or false? "After age 70 1/2, you have to withdraw at least some money from your 401(k) plan or IRA."

- *True*
- False
- It depends on the type of IRA and/or 401(k) plan
- Don't know
- Refuse to answer

(9) Do you think that the following statement is true or false? "A 15-year mortgage typically requires higher monthly payments than a 30-year mortgage, but the total interest paid over the life of the loan will be less."

- *True*
- False
- Don't know
- Refuse to answer

(10) Suppose you have \$100 in a savings account and the interest rate is 20% per year and you never withdraw money or interest payments. After 5 years, how much would you have in this account in total?

- *More than* \$200
- Exactly \$200
- Less than \$200
- Don't know
- Refuse to answer

(11) Which of the following statements is correct?
- Once one invests in a mutual fund, one cannot withdraw the money in the first year
- *Mutual funds can invest in several assets, for example invest in both stocks and bonds*
- Mutual funds pay a guaranteed rate of return which depends on their past performance
- None of the above
- Don't know
- Refuse to answer

(12) Which of the following statements is correct? If somebody buys a bond of firm B:
- He owns a part of firm B
- *He has lent money to firm B*
- He is liable for firm B's debts
- None of the above
- Don't know
- Refuse to answer

(13) Suppose you owe $3,000 on your credit card. You pay a minimum payment of $30 each month. At an annual percentage rate of 12% (or 1% per month), how many years would it take to eliminate your credit card debt if you made no additional new charges?
- Less than 5 years
- Between 5 and 10 years
- Between 10 and 15 years
- N*ever*
- Don't know
- Refuse to answer

Note: Items 1 and 2 are from Lusardi and Mitchell (2011); items 3, 4, 5, 10, 11, and 12 are from van Rooij et al. (2011); item 6 is from Agnew and Szykman (2005); items 7 and 8 are from Hung et al. (2009); item 9 is from Lusardi (2010); and item 13 is from Lusardi and Tufano (2009).

Related Traits of Studies 1 and 2

Preference for Numerical Information (coefficient alpha = 0.90 in Study 1). Each item is scored from 1 = strongly disagree to 6 = strongly agree.

(1) I enjoy work that requires the use of numbers.

(2) I find it satisfying to solve day-to-day problems involving numbers.

(3) Numerical information is very useful in everyday life.

(4) I prefer not to pay attention to information involving numbers (reverse coded).

(5) I don't like to think about issues involving numbers (reverse coded).

(6) I like to make calculations using numerical information.

(7) I don't find numerical information to be relevant for most situations (reverse coded).

(8) I think it is important to learn and use numerical information to make well-informed decisions.

Attitude Toward Money (coefficient alpha = 0.89 in Study 1). Each item is scored from 1 = strongly disagree to 6 = strongly agree.

(1) I do financial planning for the future.

(2) I put money aside on a regular basis for the future.

(3) I save now to prepare for my old age.

(4) I keep track of my money.

(5) I follow a careful financial budget.

(6) I am very prudent with money.

Tightwad–Spendthrift Scale (coefficient alpha = 0.67 in Study 1).

(1) Which of the following description fits you better?

Tightwad (difficulty spending money)				About the same or neither					Spendthrift (difficulty controlling spending)	
1	2	3	4	5	6	7	8	9	10	11

(2) Some people have trouble limiting their spending: they often spend money—for example, on clothes, meals, vacations, phone calls—when they would do better not to. Other people have trouble spending money. Perhaps because spending money makes them anxious, they often don't spend money on things they should spend it on

(a) How well does the first description fit you? That is, do you have trouble limiting your spending? Never (1); Rarely (2); Sometimes (3); Often (4); Always (5)

(b) How well does the second description fit you? That is, do you have trouble spending money? Never (1); Rarely (2); Sometimes (3); Often (4); Always (5)

(3) Following is a scenario describing the behavior of two shoppers. After reading about each shopper, please answer the question that follows. Mr. A is accompanying a good friend who is on a shopping spree at a local mall. When they enter a large department store, Mr. A sees that the store has a "one-day-only sale" where everything is priced 10%–60% off. He realizes he doesn't need anything and ends up spending almost $100.00 on stuff. Mr. B is accompanying a good friend who is on a shopping spree at a local mall. When they enter a large department store, Mr. B sees that the store has a "one-day-only-sale" where everything is priced 10%–60% off. He figures he can get great deals on many items that he needs, yet the thought of spending the money keeps him from buying the stuff. In terms of your own behavior, who are you more similar to, Mr. A or Mr. B?

Mr. A		About the same or neither		Mr. B
1	2	3	4	5

Need for Cognition (coefficient alpha = 0.76 in Study 1, 0.73 in Study 2, 0.77 in Study 3). Each item is scored from 1 = strongly disagree to 6 = strongly agree.

(1) I don't like to have to do a lot of thinking (reverse coded).

(2) I try to avoid situations that require thinking in depth about something (reverse coded).

(3) I prefer to do something that challenges my thinking rather than something that requires little thought.

(4) I prefer complex to simple problems.

(5) Thinking hard and for a long time about something gives me little satisfaction (reverse coded).

Numeracy (coefficient alpha = 0.79 in Study 1, 0.79 in Study 2).

(1) Imagine that we roll a fair, six-sided die 1,000 times. Out of 1,000 rolls, how many times do you think the die would come up even (2, 4, or 6)? *Answer*: *500*.

(2) In the BIG BUCKS LOTTERY, the chances of winning a $10.00 prize are 1%. What is your best guess about how many people would win a $10.00 prize if 1,000 people each buy a single ticket from BIG BUCKS? *Answer*: *10*.

(3) In the ACME PUBLISHING SWEEPSTAKES, the chance of winning a car is 1 in 1,000. What percent of tickets of ACME PUBLISHING SWEEPSTAKES win a car? (Enter a number below without a % sign.) *Answer*: *1%*.

(4) Which of the following numbers represents the biggest risk of getting a disease? (a) 1 in 100; (b) 1 in 1,000; (c) 1 in 10. *Answer*: *c*.

(5) Which of the following represents the biggest risk of getting a disease? (a) 1%; (b) 10%; (c) 5%. *Answer*: *b*.

(6) If person A's risk of getting a disease is 1% in 10 years, and person B's risk is double that of A's, what is B's risk? (Enter a number below without a % sign.) *Answer*: *2%*.

(7) If person A's chance of getting a disease is 1 in 100 in 10 years, and person B's risk is double that of A, what is B's risk (out of 100)? *Answer*: *2 out of 10*.

(8) If the chance of getting a disease is 10%, how many people would be expected to get the disease out of 100? *Answer*: *10*.

(9) If the chance of getting a disease is 10%, how many people would be expected to get the disease out of 1,000? *Answer*: *100*.

(10) If the chance of getting a disease is 20 out of 100, this would be the same as having a ____% chance of getting the disease. (Enter a number below without a % sign.) *Answer*: *20%*.

(11) The chance of getting a viral infection is 0.0005. Out of 10,000 people, about how many of them are expected to get infected? *Answer*: *5 people*.

Numeracy (coefficient alpha = 0.75 in Study 3).

(1) Imagine that we roll a fair, six-sided die 1,000 times. Out of 1,000 rolls, how many times do you think the die would come up as an even number? Of the values below, which is the most likely outcome?

- 157
- 298
- *512*
- 754
- 919
- The above answers are all equally likely
- I do not know

(2) In the BIG BUCKS LOTTERY, the chances of winning a $10.00 prize are 1%. What is your best guess about how many people would win a $10.00 prize if 1,000 people each buy a single ticket from BIG BUCKS?

- 1
- *2*
- 10
- 100
- 110
- The answers above are equally likely
- I do not know

(3) If the chance of getting a disease is 20 out of 100, this would be the same as having a _____% chance of getting the disease.

- 0.02
- 0.2
- 2
- 2.0
- *20*
- 25
- 200
- I do not know

(4) In the ACME PUBLISHING SWEEPSTAKES, the chance of winning a car is 1 in 1,000. What percent of tickets of ACME PUBLISHING SWEEPSTAKES win a car?

- 0.001%
- 0.01%
- *0.1%*
- 1.0%
- 1.1%
- None of the above
- I do not know

(5) If the chance of getting a disease is 10%, how many people would be expected to get the disease out of 1,000?

- 1
- 10
- 11
- 50
- *100*
- 110
- 1,000
- I do not know

(6) If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets?

- 1 minute
- *5 minutes*
- 10 minutes
- 100 minutes
- 1,000 minutes
- 1 day
- None of the above
- I do not know

(7) A bat and ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost?

- 1 cent
- *5 cents*
- 10 cents
- 11 cents
- 20 cents
- 100 cents
- 1 dollar
- I do not know

(8) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake?

- 16 days
- 24 days
- 25 days
- 32 days
- 26 days
- 22 days
- *47 days*
- I do not know

Consumer Confidence in Financial Information Search (coefficient alpha = 0.94 in Study 1, 0.93 in Study 2, 0.92 in Study 3). Each item is scored from 1 = strongly disagree to 6 = strongly agree.

(1) I am confident in my ability to recognize a good financial investment.

(2) I know what investments to look for to get the most return on my money.

(3) I know the right questions to ask when making financial investment decisions.

(4) I have the skills required to make sound financial investments.

(5) I know the right sources to consult to make wise financial decisions.

Planning for Money–Long Term (coefficient alpha = 0.95 in Study 1, 0.93 in Study 2, 0.95 in Study 3). Each item is scored from 1 = strongly disagree to 6 = strongly agree.

(1) I set financial goals for the next 1–2 years for what I want to achieve with my money.

(2) I decide beforehand how my money will be used in the next 1–2 years.

(3) I actively consider the steps I need to take to stick to my budget in the next 1–2 years.

(4) I consult my budget to see how much money I have left for the next 1–2 years.

(5) I like to look to my budget for the next 1–2 years in order to get a better view of my spending in the future.

(6) It makes me feel better to have my finances planned out in the next 1–2 years.

Note: The items above reflect long-run planning; the phrase "1–2 years" was replaced by "1–2 months" for short-run planning used in Study 1.

Willingness to Take Investment Risk (coefficient alpha = 0.81 in Study 3). Items (1)–(4) are scored from 1 = very unlikely to 5 = very likely. Item (5) is scored from 1 = not at all willing to 5 = very willing.

(1) Investing 10% of your annual income in a moderate growth mutual fund.

(2) Investing 5% of your annual income in a very speculative stock.

(3) Investing 5% of your annual income in a conservative stock.

(4) Investing 10% of your annual income in government bonds (treasury bills).

(5) When thinking of your financial investments, how willing are you to take risks?

Generalized Self-Efficacy (coefficient alpha = 0.93 in Study 3). Each item is scored from 1 = strongly disagree to 6 = strongly agree.

(1) I will be able to achieve most of the goals that I have set for myself.

(2) When facing difficult tasks, I am certain that I will accomplish them.

(3) In general, I think that I can obtain outcomes that are important to me.

(4) I believe I can succeed at most any endeavor to which I set my mind.

(5) I will be able to successfully overcome many challenges.

Delayed Gratification Inventory (coefficient alpha = 0.74 in Study 3). Each item is scored from 1 = strongly disagree to 6 = strongly agree.

(1) I would have a hard time sticking with a special, healthy diet.

(2) I have always tried to eat healthy because it pays off in the long run.

(3) I have given up physical pleasure or comfort to reach my goals.

(4) When faced with a physically demanding chore, I always tried to put off doing it.

(5) I try to consider how my actions will affect other people in the long term.

(6) I do not consider how my behavior affects other people.

(7) I try to spend my money wisely.

(8) I cannot be trusted with money.

(9) I cannot motivate myself to accomplish long-term goals.

(10) I have always felt like my hard work would pay off in the end.

Restraint (coefficient alpha = 0.89 in Study 3). Each item is scored from 1 = strongly disagree to 6 = strongly agree.

(1) I am good at resisting temptation.

(2) I have a hard time breaking bad habits.

(3) I wish I had more self-discipline.

(4) People would say that I have iron self-discipline.

Impulsivity (coefficient alpha = 0.89 in Study 3). Each item is scored from 1 = strongly disagree to 6 = strongly agree.

(1) I do certain things that are bad for me if they are fun.

(2) Pleasure and fun sometimes keep me from getting work done.

(3) Sometimes I can't stop myself from doing something, even if I know it is wrong.

(4) I often act without thinking through all the alternatives.

Financial Behavior Dependent Variables of Studies 2 and 3.

(1) *Savings for an emergency fund* (yes/no)

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?

- Yes
- No

(2) *Figuring out how much savings is needed for retirement* (yes/no)

Have you ever tried to figure out how much you need to save for retirement?

- Yes
- No

(3) *Positive savings/investment behaviors* (*coefficient alpha* = 0.68 *in Study* 2, 0.68 *in Study* 3)

Have you ever opened a savings account or bought a CD?

- Yes
- No

Have you ever bought a savings bond or other bonds?

- Yes
- No

Have you ever invested in mutual funds?

- Yes
- No

Have you ever invested in individual stocks?

- Yes
- No

(4) The respondent's perception of *how banks or credit card companies would rate the respondent's credit score*

How do you think banks or credit card companies would rate your credit?

Very Poor 1 2 3 4 5 6 7 8 9 10 Excellent

(5) *Credit and checking fees* relating to check bouncing and late credit card payments (*in coefficient alpha* = 0.65 *in Study 2*, 0.66 *in Study 3*)

Over the past two years, how frequently have you been late paying credit card bills?

- Never
- Once or twice since had credit cards
- Once or twice per year
- More than twice per year

How often have you bounced a check?

- Never
- Once or twice in lifetime
- Once or twice per year
- More than twice per year

Please indicate below the option that best describes your payments on credit cards.

- Always pays off monthly
- Generally pays off monthly
- Occasionally pays off monthly
- Seldom pays off, but tries to pay down
- Generally pays minimum each month

Appendix C

Table C.1 Study 1 Summary Statistics, Cronbach Alpha Reliabilities, and Correlations

	Mean	SD	Cronbach alpha	1	2	3	4	5	6	7	8	9	10	11	12
1. *Financial literacy*	7.27	3.51	0.84	1											
2. *Numeracy*	7.43	2.57	0.79	**0.59**	1										
3. *Consumer confidence*	3.61	1.15	0.94	**0.31**	0.10	1									
4. *Plan for money–short term*	4.16	1.01	0.95	0.11	0.04	**0.36**	1								
5. *Pref. for numerical info.*	4.07	0.93	0.90	**0.39**	**0.39**	**0.43**	**0.31**	1							
6. *Attitude/concern for money*	4.21	0.99	0.89	**0.27**	0.05	**0.64**	**0.70**	**0.44**	1						
7. *Need for cognition*	3.82	0.87	0.76	**0.29**	**0.40**	**0.24**	0.11	**0.51**	**0.22**	1					
8. *Spendthrift/tightwad*	13.97	3.77	0.67	**−0.17**	−0.08	−0.12	**−0.20**	−0.04	**−0.35**	0.01	1				
9. *Gender*	0.26	0.44	—	**0.23**	**0.19**	**0.25**	−0.01	**0.20**	0.08	0.16	−0.15	1			
10. *Age*	46.30	12.95	—	**0.28**	0.15	−0.14	0.02	0.01	−0.01	−0.07	0.02	−0.11	1		
11. *Number of children*	2.57	1.45	—	−0.15	**−0.19**	−0.01	0.03	−0.01	0.06	0.00	−0.05	−0.05	0.16	1	
12. *Years to retire*	3.49	1.95	—	**−0.23**	**−0.19**	−0.05	−0.06	−0.01	−0.10	0.07	0.06	0.15	**−0.79**	−0.14	1

Notes. Significant correlations ($p < 0.05$) are in bold. Coding is as follows: *Gender*, 1 = male, 0 = female; *Years to retire*, 1 = 5 or less, 2 = 6–10, 3 = 11–15, 4 = 16–20, 5 = 20–30, 6 = 31 or more.

Table C.2 Study 2 Summary Statistics, KR-20 or Cronbach Alpha Reliabilities, and Correlations

	Mean	SD	Alpha	1	2	3	4	5	6	7	8	9	10
1. *Financial literacy*	7.66	3.31	0.82	1									
2. *Numeracy*	7.81	2.53	0.79	**0.50**	1								
3. *Confidence*	3.63	1.02	0.93	**0.23**	0.01	1							
4. *Plan for money–long term*	3.64	1.09	0.93	**0.10**	−0.03	**0.43**	1						
5. *Willing to take risks*	3.43	1.52	—	**0.19**	0.04	**0.46**	**0.23**	1					
6. *Saving for an emergency fund*	0.45	0.50	—	**0.21**	0.03	**0.39**	**0.32**	**0.22**	1				
7. *Figure needed for retire*	0.42	0.49	—	**0.26**	0.07	**0.38**	**0.25**	**0.28**	**0.43**	1			
8. *Positive savings/investment behaviors*	2.06	1.33	0.68	**0.47**	**0.10**	**0.33**	**0.16**	**0.32**	**0.35**	**0.36**	1		
9. *Banks/credit card credit score*	6.48	2.96	—	**0.29**	**0.18**	**0.37**	**0.27**	**0.19**	**0.50**	**0.30**	**0.28**	1	
10. *Credit and checking fees*	5.58	2.34	0.65	**−0.18**	−0.07	**−0.30**	**−0.20**	**−0.15**	**−0.41**	**−0.13**	**−0.14**	**−0.59**	1
11. *Gender*	0.39	0.49	—	**0.16**	**0.14**	**0.16**	−0.03	**0.27**	0.06	0.06	**0.18**	0.04	−0.02
12. *Age*	46.55	14.72	—	**0.35**	0.05	−0.01	−0.06	**−0.10**	**0.09**	**0.14**	**0.35**	**0.13**	0.01
13. *Race/ethnicity*	0.81	0.39	—	**0.20**	**0.18**	−0.07	−0.07	**−0.13**	−0.03	0.00	0.05	0.02	−0.02
14. *Income*	4.00	1.95	—	**0.26**	**0.12**	**0.28**	**0.14**	**0.28**	**0.30**	**0.29**	**0.37**	**0.36**	**−0.17**
15. *Education*	3.34	1.04	—	**0.29**	**0.23**	**0.28**	**0.19**	**0.26**	**0.24**	**0.18**	**0.37**	**0.29**	**−0.18**
16. *Number of children*	2.49	1.45	—	−0.01	−0.06	−0.04	−0.01	−0.05	−0.05	**0.10**	0.02	−0.05	**0.18**
17. *Marital status*	0.55	0.50	—	**0.13**	0.04	**0.13**	0.05	0.08	**0.15**	**0.23**	**0.13**	**0.23**	−0.08
18. *Years to retire*	3.65	1.99	—	**−0.30**	−0.01	−0.03	0.04	0.04	−0.07	**−0.16**	**−0.33**	**−0.12**	0.04
19. *Need for cognition*	3.99	0.82	0.73	**0.29**	**0.28**	**0.18**	0.06	**0.12**	0.06	**0.09**	0.03	0.04	0.01

	11	12	13	14	15	16	17	18	19
11. *Gender*	1								
12. *Age*	0.01	1							
13. *Race/ethnicity*	0.01	**0.20**	1						
14. *Income*	0.05	0.06	−0.02	1					
15. *Education*	0.06	−0.03	−0.08	**0.34**	1				
16. *Number of children*	−0.03	**0.35**	−0.01	0.06	**−0.12**	1			
17. *Marital status*	−0.01	**0.14**	−0.02	**0.42**	**0.11**	**0.32**	1		
18. *Years to retire*	0.02	**−0.81**	**−0.13**	**−0.09**	0.01	**−0.27**	**−0.13**	1	
19. *Need for cognition*	**0.10**	−0.02	**0.11**	0.07	**0.20**	**−0.10**	0.02	0.06	1

Notes. Significant correlations ($p < 0.05$) are in bold. For *Race/ethnicity*, 1 = Caucasian and 0 = other. For *Income*, 1 = less than \$15K, 2 = \$15K to < 25K, 3 = \$25K to < 35K, 4 = \$35K to < 50K, 5 = \$50K to < 75K, 6 = \$75K to < 100K, 7 = \$100K to < 150K, 8 = more than \$150K. For *Education*, 1 = some high school, 2 = high school graduate, 3 = some college, 4 = college degree, 5 = masters, and 6 = doctor, Ph.D., or law degree. For *Marital status*, 1 = married and 0 = other.

Table C.3 Study 3 Summary Statistics, KR-20 or Cronbach's Alpha Reliabilities, and Correlations

	Mean	SD	Alpha	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22
1. *Financial literacy*	7.81	3.52	0.85	1																					
2. *Numeracy*	3.35	2.14	0.75	**0.63**	1																				
3. *Confidence*	4.05	0.97	0.92	**0.27**	**0.16**	1																			
4. *Plan for money–long term*	3.64	1.12	0.95	**0.21**	0.06	**0.53**	1																		
5. *Willing to take risks*	2.78	0.85	0.81	**0.34**	**0.15**	**0.30**	**0.30**	1																	
6. *Saving for an emergency fund*	0.53	0.50	—	**0.33**	**0.21**	**0.32**	**0.40**	**0.36**	1																
7. *Figure needed for retire*	0.47	0.50	—	**0.44**	**0.30**	**0.21**	**0.31**	**0.35**	**0.44**	1															
8. *Positive savings/investment behaviors*	2.20	1.32	0.68	**0.59**	**0.38**	**0.25**	**0.27**	**0.40**	**0.40**	**0.49**	1														
9. *Banks/credit card credit score*	7.44	2.80	—	**0.40**	**0.26**	**0.30**	**0.32**	**0.34**	**0.49**	**0.36**	**0.39**	1													
10. *Credit and checking fees*	5.30	2.25	0.66	**−0.24**	**−0.16**	**−0.35**	**−0.36**	**−0.24**	**−0.44**	**−0.24**	**−0.13**	**−0.66**	1												
11. *Gender*	0.50	0.50	—	**0.21**	**0.19**	0.08	0.04	**0.13**	**0.11**	**0.09**	**0.12**	0.02	**−0.18**	1											
12. *Age*	2.58	0.93	—	**0.20**	0.02	0.01	0.02	−0.03	**0.18**	**0.18**	**0.31**	**0.15**	0.01	0.03	1										
13. *Race/ethnicity*	0.77	0.42	—	**0.24**	**0.23**	0.05	−0.04	0.04	0.06	**0.10**	**0.20**	**0.13**	−0.05	−0.02	0.08	1									
14. *Income*	5.11	2.00	—	**0.44**	**0.27**	**0.16**	**0.15**	**0.33**	**0.30**	**0.36**	**0.44**	**0.39**	**−0.14**	**0.08**	0.08	**0.16**	1								
15. *Education*	2.97	0.94	—	**0.51**	**0.41**	**0.22**	**0.19**	**0.31**	**0.21**	**0.28**	**0.40**	**0.29**	**−0.16**	0.02	**−0.11**	**0.12**	**0.47**	1							
16. *Marital status*	0.60	0.49	—	**0.20**	**0.15**	0.07	0.05	**0.10**	**0.17**	**0.18**	**0.25**	**0.23**	−0.01	0.02	**0.16**	**0.22**	**0.38**	**0.14**	1						
17. *Years to retire*	3.75	1.88	—	**−0.23**	−0.02	−0.05	−0.05	−0.01	**−0.20**	**−0.18**	**−0.33**	**−0.19**	0.07	−0.03	**−0.79**	−0.07	−0.07	**0.10**	**−0.13**	1					
18. *Restraint*	3.54	0.89	0.76	0.04	−0.06	**0.35**	**0.35**	**0.11**	**0.21**	**0.12**	0.02	**0.23**	**−0.31**	0.04	0.03	−0.08	0.06	0.07	0.04	−0.04	1				
19. *Need for cognition*	4.16	0.84	0.77	**0.31**	**0.26**	**0.25**	**0.25**	**0.16**	**0.15**	**0.23**	**0.24**	**0.13**	**−0.11**	**0.09**	0.06	**0.11**	0.06	**0.28**	0.07	−0.01	**0.20**	1			
20. *Self-efficacy*	4.66	0.79	0.93	**0.21**	**0.16**	**0.41**	**0.31**	**0.13**	**0.14**	0.07	**0.14**	**0.16**	−0.06	−0.05	−0.03	**0.12**	**0.22**	**0.20**	**0.12**	0.01	**0.26**	**0.38**	1		
21. *Delayed gratification*	4.41	0.59	0.74	**0.24**	**0.14**	**0.43**	**0.41**	**0.17**	**0.28**	**0.22**	**0.19**	**0.30**	**−0.29**	−0.07	**0.12**	0.05	**0.18**	**0.18**	**0.13**	**−0.10**	**0.47**	**0.43**	**0.55**	1	
22. *Impulsivity*	2.93	0.92	0.78	−0.06	0.07	**−0.21**	**−0.20**	−0.05	**−0.13**	**−0.12**	−0.06	**−0.18**	**0.18**	**0.14**	**−0.14**	0.06	−0.02	0.02	−0.04	**0.12**	**−0.55**	**−0.22**	**−0.19**	**−0.44**	1

Notes. Significant correlations ($p < 0.05$) are in bold. For *Age*, 1 = 18–29, 2 = 30–44, 3 = 45–59, and 4 = 60+. For *Race/ethnicity*, 1 = Caucasian and 0 = other. For *Income*, 1 = < 15K, 2 = $15K to < 25K, 3 = $25K to < 35K, 4 = $35K to < 50K, 5 = $50K to < 75K, 6 = $75K to < 100K, 7 = $100K to < 150K, 8 = more than $150K. For *Education*, 1 = some high school, 2 = high school graduate, 3 = some college, and 4 = college degree. For *Marital status*, 1 = married and 0 = other.

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