Part III

Shaping the Financial Literacy Environment

Chapter 10

Financial Counseling, Financial Literacy, and Household Decision-Making

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Research suggests that consumers often make what appear to be welfare-reducing decisions. Many individuals do not hold a checking account (Hilgert et al., 2003); maintain large outstanding balances on credit cards when cheaper forms of credit are available (Gartner and Todd, 2005); take out payday loans at astronomical interest rates when cheaper forms of credit are available (Agarwal et al., 2009*c*); choose suboptimal credit contracts (Agarwal et al., 2006); fail to refinance mortgages when it would be optimal to do so (Agarwal et al., 2008*a*); and fail to plan for retirement, reaching it with little or no savings (Lusardi and Mitchell, 2006). A leading explanation for this behavior is that consumers are not financially literate—they lack sufficient information about financial concepts and instruments to make informed financial decisions.[1]

Surveys find that a large proportion of consumers, in the United States and in other countries, fail basic financial literacy tests. Many adults do not understand the difference between compound and simple interest; the characteristics of financial assets such as stocks and bonds; the benefits of portfolio diversification; or the important features of their own mortgages, Social Security, and pension plans (Lusardi and Mitchell, 2006, 2007*a*, 2007*b*).

If financial illiteracy drives suboptimal (or welfare-reducing) financial behavior, then improving literacy could increase consumer welfare. A growing literature investigates whether education programs are effective in improving financial literacy and behavior. Although the evidence is mixed, it appears that some financial education programs do improve the behavior and outcomes of their graduates. The effects appear to be strongest for the most financially vulnerable, especially those with low incomes and levels of education. However, the relationships among financial education, financial literacy, and financial behavior and outcomes are not straightforward. Some financial education programs improve financial

literacy, but not financial behavior; others lead to improved behavior and outcomes without improving financial literacy; and still others do not appear to be effective at all.

In what follows, we review the existing literature and offer a critique of studies evaluating the extent of consumer financial literacy. We then evaluate the evidence on the effectiveness of financial education programs in improving participants' financial behavior and outcomes. We do not attempt a comprehensive survey of the literature in these areas; instead, we look for the most convincing evidence, paying particular attention to study design, data limitations, and potential sources of bias. We also devote particular attention to whether the perceived impact of educational programs can be attributed to increases in financial literacy. Overall, we find some evidence of beneficial effects of education programs on outcomes, and this is partially due to improvements in financial literacy. Yet, it is difficult to differentiate the factors contributing to the improvement. Some is due to the educational programs, some to the selection of participants, and some to auxiliary influences resulting from the educational program. We also discuss how the gains from financial literacy programs may wane over time, as financial decision-making becomes more difficult with age. We conclude with a discussion of the need for future research in this area.

Review of the literature on financial literacy and financial education programs

There is considerable evidence that a large segment of the US population is not financially literate. This means that many people do not understand basic financial concepts and products well enough to make sound short- and long-term financial decisions for themselves and their families. The evidence comes from surveys administered to various groups of consumers over the past two decades to ascertain their knowledge of financial products and understanding of basic concepts. While the surveys vary significantly in content and sample populations, they generally agree on several findings:

1. A large proportion of consumers are not financially literate, even among the wealthiest and most educated.
2. Financial literacy rates vary consistently by demographic groups, tending to be higher for those with more wealth and education, for men (although results vary), and for whites (in the United States).
3. Financial illiteracy leads to welfare-reducing financial behavior and outcomes.

While there is a fairly broad consensus that financial illiteracy leads to suboptimal decisions by consumers, there is significant disagreement as to how best to combat these ill effects, as well as on the effectiveness of the approaches that have been tried to date. Research efforts to evaluate the impact of such programs encounter an array of econometric issues that could bias the findings. Similarly, changes in behavior may not result from the educational benefits of these programs, but rather from auxiliary influences associated with the program. In the discussion that follows, we first evaluate the evidence on financial literacy and the adverse effects of suboptimal financial decisions; then, we discuss the impact of educational programs aimed at improving literacy, all the while emphasizing potential problems that arise when quantifying these effects.

Financial literacy

Most research on financial literacy has been conducted in the United States and accordingly we concentrate on that literature.[2] Early studies to measure adult financial literacy were conducted during the 1990s by private firms utilizing surveys that consisted of a small number of questions covering material specific to corporate interests (Volpe et al., 2006). Similarly, early studies of high school and college students asked relatively few questions and often sampled few institutions.[3] Perhaps, the most useful early studies assessing overall financial literacy were those conducted on high school and college students. The Jump$tart Financial Literacy Survey administered the same exam to randomly selected high school seniors every two years from 1997 to 2006. The exam included thirty-one questions on income, money management, saving and investment, and spending and credit and was intended to capture financial competence in a broad set of areas. Jump$tart's findings were not encouraging: students scored an average of 57 percent in 1997 (with 60 percent being a passing score), and scores declined by several percentage points in subsequent years (2000–6).[4]

Chen and Volpe (1998) find similarly low rates of financial literacy among college students. In a sample of thirteen public and private universities, the average respondent scored only 53 percent on a thirty-six-question exam covering general financial knowledge. The sample included a high proportion of business majors, who scored higher than their peers in other fields, who barely averaged 50 percent on the exam. Importantly, students scored highest on questions covering areas in which young people are likely to have some experience (e.g., auto insurance and apartment leases) and lowest where they are likely to have the least experience (e.g., taxes, life-insurance, and investment). This suggests that financial

experience could increase financial literacy, and studies that find an effect of financial literacy on financial behavior should test for reverse causality.

Although these low financial literacy scores are worrisome, these results must be interpreted with caution, as they may be biases. Both studies have low response rates—51 percent for the survey of Chen and Volpe and much lower for the Jump$tart exams—and hence could suffer from nonresponse bias.[5] Chen and Volpe had also a disproportionally high share of responses from business majors. While business majors may have been oversampled to begin with, it is plausible that they were also more likely to respond to the survey as it was less costly for them to complete in terms of time and effort. The authors did not address this concern, nor did they weight results to reflect the demographic distribution of college students. Jump$tart suffered from a different sampling problem: the study randomly selected US public high schools and asked each to administer the survey to one class of seniors. Yet only 44 percent of high schools agreed to conduct the survey in 1997, and this rate dropped below 20 percent thereafter. High schools that declined to participate most often cited the need to prepare for state and federally mandated standardized tests, suggesting that the most disadvantaged schools were the least likely to participate. It is important to note that these nonresponse biases, if they exist, would bias the results of both studies upward, leading the studies to *understate* the pervasiveness of financial illiteracy.

It is also questionable whether the exams given by Jump$tart and by Chen and Volpe accurately evaluate respondent financial competence. Both exams consisted entirely of multiple choice questions, which means some correct responses were likely guesses, which would lead to an overstatement of financial literacy. On the other hand, inaccurate responses might not reflect ability to save, plan for retirement, manage debt, and make important financial decisions. For example, some questions sought factual data (e.g., how much would a college degree affect earning power), but respondents may have answered in terms of their own personal prospects. Additionally, certain questions concerned concepts with which high school students might not be familiar (e.g., down-payments and liquidity), and a current misunderstanding of such concepts could be a poor indicator of a student's future ability to make financial decisions. The Chen and Volpe (1998) survey questions were even more difficult, and often required specific financial knowledge that a competent individual might not have (e.g., the maximum amount of money that is FDIC-insured at a member commercial bank). Some questions were less difficult but more ambiguous.

Other research evaluates financial literacy among adults in more specific contexts. For instance, there is an extensive literature on the relationship between financial literacy and planning/saving for retirement. This literature yields two broad, but important findings. First, after controlling for

a broad range of economic and demographic characteristics, more financially literate individuals are more likely to plan for retirement, and those who plan have greater net worth upon reaching retirement. Second, causation goes from literacy to planning to wealth.

Individuals accrue retirement assets both individually as well as through Social Security and employer-sponsored pensions. To figure out how much to save for retirement, individuals must know their expected dates of retirement, expected lifespan, and Social Security and/or pension entitlements. They must then calculate how much to save to maintain a certain standard of living in retirement, given the expected rate of return on saving. This planning process requires knowledge of Social Security and pension plan characteristics, as well as the ability to perform calculations involving compound interest and monthly accumulation. In practice, this is a difficult process, as illustrated by Bernheim (1988), Mitchell (1988), and Gustman and Steinmeier (2005). Many adults do not know important features of their Social Security entitlements and pensions, as found by Bernheim (1988). Using the Social Security Retirement History Survey (RHS), he showed that adults nearing retirement did not report accurate estimates of expected Social Security benefits. He compared expected benefits to realized benefits, finding that predictions were unbiased but 'noisy': indeed, expected benefits accounted for only 60 percent of the variation in realized benefits. In addition, over half of respondents provided no estimate.

Mitchell (1988) examined employee knowledge of company pensions and found that many workers were unaware of important features of the plan. She compared pension characteristics reported by individuals from the Survey of Consumer Finances (SCF) to accurate administrative data. Only half of employees who were required to contribute to their pensions reported doing so, and only half of those whose employers contributed said they did. Over one-third of respondents did not know about early retirement provisions and, among those who did, two-thirds described them inaccurately. Those who gave correct information were more likely to be white, have a higher income and level of education, and have greater firm seniority.

Gustman and Steinmeier (2005) confirm these findings using the 1992 Health and Retirement Survey (HRS), showing that a majority of those surveyed could not accurately report their Social Security or pension entitlements. Only 27 percent of respondents gave estimates within 25 percent of their true Social Security entitlements, and only 16 percent of respondents with pensions gave estimates within 25 percent of their true pension entitlements. Perhaps most surprising, over 40 percent of respondents were unable to provide any estimate. Being educated, having higher income, and being white and male predicted more accurate responses.

Even if consumers do have information about their Social Security and pension entitlements, they still have trouble performing the calculations necessary to plan for retirement. Significantly, many adults cannot correctly answer questions requiring a basic financial understanding. For instance, Lusardi and Mitchell (2006, 2007*a*) find that only 18 percent of 2004 HRS respondents thought that an account initially holding $100 and earning 20 percent compound annual interest would hold more than $200 after five years. In particular, many respondents thought the account would hold exactly $200, suggesting they did not understand compounding. An easier interest rate question from a three-question financial literacy module in the 2004 HRS yielded more correct responses, but it did not require respondents to understand the difference between compound and simple interest. Consistent with other research in this area, the probability of answering correctly was higher for those with more wealth and education, for whites, and for men. Nevertheless, mistakes persisted, even among the groups most likely to answer correctly. Lusardi and Mitchell (2007*b*) find that even in the RAND American Life Panel (ALP), a sample of educated and high-earning middle-aged adults, over a quarter of respondents could not accurately answer the more difficult HRS compound interest question.

Further research showed that facility with interest rates is only weakly related to age. Lusardi et al. (2009) found that respondents in their 20s do about as well as respondents in their 50s, with the same demographic characteristics predicting correct responses as in other studies.

These studies also revealed other forms of financial illiteracy. Many consumers answered a 'money illusion' question incorrectly, suggesting they did not understand the consequences of inflation (Lusardi and Mitchell, 2006, 2007*b*). Nearly half of HRS respondents missed a 'lottery division' question, which amounted to a simple division problem (Lusardi and Mitchell, 2007*a*). In the HRS financial literacy module, only 52 percent of respondents said investing in a mutual fund was less risky than investing in a single company's stock, indicating a misunderstanding of risk and portfolio diversification (Lusardi and Mitchell, 2006).

Additional examples of financial illiteracy are also found in mortgage markets. For example, many individuals who hold adjustable rate mortgages (ARMs) exhibit shocking ignorance of their mortgage terms. Bucks and Pence (2006) document this by comparing the distribution of household-reported mortgage characteristics in the SCF to distributions in three lender-reported datasets. They found that ARM borrowers often could not provide basic information about their own loans.[6] When ARM borrowers did report their loan characteristics, they regularly got them wrong, often underestimating their risks and potential liabilities. Agarwal et al. (2009*b*) corroborate this evidence with data from a mandatory loan counseling program for high-risk mortgage applicants in select Chicago zip codes.

Most of the applications were for ARMs. According to a summary of counselor assessments from the program, the 'overwhelming majority' of ARM applicants were unaware that their interest rate was not fixed for the life of the mortgage.[7] In addition, 9 percent of counseled borrowers gave a verbal description of the loan that was significantly different from loan documents.

These studies suggest that many consumers lack the financial knowledge and computational ability to make informed financial decisions. However, survey-based studies could still overstate the illiteracy in the population, as respondents often have little incentive to answer questions correctly. Respondents may ignore or give the wrong answer to a question they might answer accurately with more time and analysis. In contrast, if they realized there is a monetary impact resulting from a financial decision, there would be a stronger incentive to make the correct choice. Nevertheless, if higher financial literacy scores lead to positive financial behavior and outcomes, these tests will capture variables that seem to be important.

Some of the strongest evidence that the causal chain proceeds from literacy to outcomes comes from three papers by Lusardi and Mitchell (2006, 2007*a*, 2007*b*), who seek to link retirement planning, household wealth on entering retirement, and financial literacy. Using data from the 1992 and 2004 HRS and the RAND ALP they document that literacy, planning, and wealth are strongly and positively correlated, even after controlling for economic, demographic, and other characteristics. Furthermore, the authors established that causation proceeds from literacy and planning to wealth, and not from wealth to planning and literacy. They do so by testing for reverse causality using instrumental variable techniques. In particular, they regress a dummy variable for having planned for retirement on economic and demographic characteristics and on the previous year's regional change in housing prices. The last variable is thought to be a valid instrument for household wealth. The effect of the wealth instrument on planning is not significant, which suggests that individuals are not more likely to plan for retirement because they are wealthier.

Further analysis of the RAND ALP confirms the findings in the HRS. Lusardi and Mitchell (2007*b*) find that scores on a more detailed financial literacy test predict planning behavior. To eliminate endogeneity, they use answers to the following question as an instrument for financial literacy: *How much of your school's education (high school, college or higher degrees) was devoted to economics? A lot, some, little, or hardly at all?* Interestingly, the instrument produces an even larger estimate of the relationship between literacy and planning than the original test scores. The ALP asked a larger set of financial literacy questions than the HRS, many of which require more detailed knowledge of financial instruments. The authors find that after instrumenting for retirement planning, planning behavior still

predicts financial literacy. Accordingly, while it is possible that planning affects literacy but not vice versa, it is unlikely that reverse causality fully explains the relationship.[8] In the realm of retirement planning and saving, the evidence suggests that financial literacy affects financial behavior and outcomes.

The correlation between financial literacy and behavior is also corroborated by studies of the loan market. In a survey of Washington State residents, Moore (2003) found that less financially literate consumers tended to make inferior mortgage product choices. Furthermore, consumers borrowing from lenders involved in a predatory lending lawsuit tended to do worse on questions about investing and compound interest, suggesting that financial illiteracy leaves consumers open to exploitation. In related studies, Stango and Zinman (2010) documented that consumers who were unable to calculate the interest rate on a loan—given the principal and a stream of payments—borrowed more, accumulated less wealth, and paid more for credit. Campbell (2006) found that less financially sophisticated households tend to make significant financial mistakes. In particular, they are less likely to refinance their mortgages under advantageous circumstances.

In summary, there is overwhelming evidence that many consumers are not financially literate and, further, that these consumers tend to make poor financial decisions.

Financial education, financial literacy, and financial behavior

If financial illiteracy causes undesirable financial behaviors, then increasing financial literacy could enhance consumer welfare. An array of financial education programs have been introduced in the United States for this purpose over the past few decades. These programs range from employer-provided seminars on retirement planning, to state-mandated personal finance classes in public schools, to one-on-one mortgage counseling. Are these programs effective? If so, which types of programs are more effective?

To answer these questions, we draw on several reviews of the financial education literature, not all of which agree on the strength of the available evidence. The most comprehensive of these reviews is a recent article by Collins and O'Rourke (2010), who are cautiously optimistic that financial education can be effective. Martin (2007) shared this optimism for programs targeting saving and retirement, credit, and homeownership, and Hogarth (2006) gives an even more sanguine assessment. Yet, some reviews have less positive outlooks: Hathaway and Khatiwada (2008) and Willis (2008, 2009) find no conclusive evidence that financial education

programs are effective. In a review of five studies evaluating personal financial management courses, Caskey (2006) concludes that nonexperimental program evaluations—even ones that use instrumental variables and other modeling techniques to eliminate endogeneity—often fail to approximate results obtained under experimental conditions. This critique casts doubt on studies where treatment is not randomly assigned and, hence, on the vast majority of papers in the financial education literature.

While we concur with some of these critiques, we believe there is strong evidence that some financial education programs improve financial behavior and outcomes, with weaker evidence that these programs increase financial literacy. Yet, the link between education, literacy, and outcomes is still not clearly established. No study definitively demonstrates that a financial education program improved participant outcomes *through* financial literacy, and many studies find that the financial education programs evaluated were ineffective. We also find that the strength of evidence for financial education's effectiveness depends on the type of financial education program studied. In the next two sections, we discuss the effectiveness of financial education in the workplace and in schools, respectively, as this literature provides the strongest evidence that financial education can be effective. Then, we examine how evaluations of mortgage, bankruptcy, credit-repair, and other financial education programs augment these results. Finally, we document initial research into optimal programs and innovative study designs that could serve as models for future research.

Behavior and outcome: evidence from workplace programs

Evaluations of financial education programs may not produce credible impact estimates if the program suffers from potential selection bias: that is, when participation is voluntary, then exposure to treatment may be correlated with unobserved traits that affect outcomes. As a result, the impact may actually be attributable to these traits instead of to the treatment. To deal with this problem, studies of school and workplace financial education have looked for valid instruments for exposure to treatment. Workplace studies have used *availability* of workplace financial education programs rather than actual attendance, while school-based studies have used state financial education mandates. Overall, these studies find that financial education does affect outcomes, increasing saving rates, pension plan participation, and net worth later in life.

In a telephone survey of workers in the United States conducted by Merrill Lynch, Bernheim and Garrett (2003) show that the availability of workplace financial education predicted increases in saving rates, assets

held in 401(k) accounts and other retirement accounts, and 401(k) participation. However, the measured effect on total assets was not significant, suggesting that differences could be due to asset substitution rather than higher overall saving. The authors argued that availability of financial education could be a valid instrument for treatment because workers did not choose employers based on financial education offerings. In fact, it appeared that workplace financial education was often remedial, which could bias the estimated impact downward. In this case, a positive estimated impact could be interpreted as a lower bound on the true impact.

Offsetting these rather strong results, this study also potentially suffered from a number of possible sources of bias. First, offering financial education seminars might be correlated with employer characteristics that attracted workers, even though the seminars were not a factor in the job search process. This would lead to an overestimate of the seminar effect. Second, the authors could not control for pension plan characteristics, which could have driven differences in saving patterns. Although they cited studies finding low correlations between plan features, participation, and saving rates, other papers have found stronger relationships (Bayer et al., 2009). Yet a third concern about this study is that it relied on self-reported employee survey data. This is less reliable than employer-provided or administrative data, particularly if respondents most influenced by workplace financial education were also more likely to recall that it was offered. In this case, estimated impacts would be upwardly biased.[9]

A complementary paper by Bayer et al. (2009) corroborated Bernheim and Garrett (2003) and also addressed some of these concerns. It used a survey of employers taken over two consecutive years, providing more accurate measures of employee 401(k) contributions and the availability of workplace financial education. The study also controlled for pension plan characteristics, and its longitudinal nature allowed them to control for individual firm characteristics so as to eliminate a significant potential source of selection bias. Cross-sectional results confirm that workplace seminars have a significant and positive effect on 401(k) participation and contributions, with a greater effect for low-income employees. In addition, the authors confirmed that workplace seminars were often remedial, making cross-sectional estimates a lower bound of the true effect. While the authors could not reject the possibility that increased participation and contribution rates are driven by asset substitution and individual heterogeneity, the research helps confirm that workplace education programs can influence financial behavior.

In related analysis, Lusardi (2004) found that having attended a retirement seminar (most of which were employer-provided) predicted greater overall saving, not just larger pension contributions. This finding fills an important gap in earlier work. Drawing on the 1992 HRS, Lusardi could

control for individual heterogeneity to an extent that prior work could not. She concluded that having attended a retirement seminar increased several measures of total saving and wealth by economically significant amounts. She also found that differences were greatest (proportionally) for those who saved the least and were the least educated. Importantly, Lusardi noted that her estimates decreased but remained significant when a full set of individual controls was included.

These studies make a strong case that financial education can improve financial behavior. Further, they show that the effect is greatest for the most economically vulnerable populations. However, these studies do not show that the programs were effective because they increased financial literacy. For instance, workplace seminars may increase worker's awareness of these vulnerabilities, or alternatively, they may increase saving because they make peer effects more important (Duflo and Saez, 2003). They could also increase employees' exposure to plans offered by the firm, along with strong encouragement to contribute. These could shape behavior without improving employees' understanding of the benefits of saving or the specific financial products they are using. To get at these issues, several authors test explicitly whether workplace financial education increases financial literacy. Their results are somewhat mixed.[10] For instance, Hira and Loibl (2005) find in a survey of a large US insurance company that employees who attended a half-day retirement seminar reported increased knowledge in four areas: retirement needs, investing, planning for the future, and managing credit. However, these measures of financial knowledge were employee perceptions, not objective assessments. As people can perceive knowledge gains to be greater than actual gains, those who attended a seminar may claim to have derived some benefit whether or not they actually did (Willis, 2009).

Behavior and outcome: evidence from school-based programs

Studies of school-based financial education programs also provide mixed evidence of effectiveness. Some evaluate individual school-based programs and consistently find significant improvements in student financial knowledge and behavior.[11] Nevertheless, these studies were subsequently challenged on the basis of research design flaws or data limitations (e.g., small sample issues). Another set of papers that used state financial education mandates as an instrument for exposure to in-school financial education found some evidence that school programs affect saving and investment in adulthood. An oft-cited study by Bernheim et al. (2001) found that state financial education mandates lead to greater asset accu-

mulation in adulthood. Bernheim et al. used the same Merrill Lynch survey as Bernheim and Garrett (2003), as many survey respondents were in school during the 1960s and 1970s, when the state mandates were introduced.

Several steps are required to argue that a link between state mandates and adult saving behavior is due to financial education. State mandates must be exogenous to population characteristics that might affect saving; they must lead to greater exposure to financial education; and they must be correlated with saving behavior. Bernheim et al. argued that state mandates are exogenous, usually driven by efforts from individual legislators and interest groups rather than broad public consensus. States with and without mandates do not differ significantly in income, proportion of high school graduates, or retail sales during the period studied. To demonstrate increased exposure, Bernheim et al. estimated a probit model and found that survey respondents who graduated high school after the introduction of a mandate in their state are more likely to report having been taught about household finance in school. The probability increases with the number of years in school after the mandate, suggesting mandates take time to implement. The authors checked whether a variable for 'years before mandate' affected the probability of exposure and found that the coefficient was small and statistically insignificant, suggesting that results did not reflect a general trend of increasing financial education independent of state mandates. Finally, Bernheim et al. found that more years in high school after a mandate predict higher reported saving rates and net worth.

Although the Bernheim et al.'s study is a contribution, it has been criticized in more recent studies. Cole and Shastry (2009) attempted to replicate Bernheim et al.'s results with US Census data and a more robust empirical specification, and the findings do not match. In particular, Cole and Shastry relaxed the assumption of a linear relationship between the number of years in school after imposition of a mandate and the outcome variables. They also controlled for statewide differences in economic conditions and augmented the Bernheim et al. specification to include a full set of birth-year cohort dummies and state fixed effects.[12] They find there is no clear break point at the time of mandate introduction, and perhaps most worryingly, coefficients for having graduated at least five years after a mandate are much smaller than the others; the relationship is not monotonic. One explanation for their finding is that mandates were introduced during times of high state GDP growth, which could explain why financial market participation and investment income went up both before and after the mandates. It is possible that these factors dominated any actual effects of state mandates.

Behavior and outcome: evidence from mortgage and other counseling programs

Studies of mortgage counseling programs build on the workplace- and school-based literature in three important ways. First, the studies evaluate specific financial education programs and so tell us more about the programs as well as the participants' financial circumstances before and after counseling. Second, mortgage counseling programs are very different in format from school and workplace programs, which are usually conducted in a classroom setting. Mortgage programs are usually offered in a one-on-one counseling format that addresses individual questions and needs. In addition, mortgage programs treat individuals who are at the point of making a critical financial decision. Finally, the mortgage programs discussed here are primarily targeted at low- to middle-income populations with characteristics that make them more likely to default on their mortgages. As this group has the lowest level of financial literacy, it may be most in need of counseling and is therefore an important group to study.

Once again, we find mixed evidence that mortgage counseling improves behavior and outcomes, with the same potential for sample selection bias. A program evaluation that sought to rigorously correct for the selection problem by Hirad and Zorn (2002) analyzed a large sample of high-risk borrowers whose mortgages were purchased by the Fannie Mae Affordable Gold program. Most borrowers were required to go through mortgage counseling before Fannie Mae would buy their loans from the original servicers, but some borrowers were exempt. Controlling for observable characteristics, counseled borrowers were less likely to become ninety days delinquent on their mortgages. The authors estimate a complex four-stage model for selection into treatment, type of organization providing treatment, and type of treatment received. Once selection was accounted for, certain types of treatment were still effective, but of unreasonably large magnitudes. In particular, one-on-one counseling was found to reduce delinquency rates by over 90 percent, while other forms of treatment had no effect. The authors noted that their selection model was a poor fit and included variables that were likely to be correlated with the error term in the original regression.

In follow-up studies by Hartarska and Gonzalez-Vega (2005, 2006) and Quercia and Spader (2008), pre-mortgage counseling was again related to loan outcomes, although the results are somewhat contradictory. Hartarska and Gonzalez-Vega found that counseled borrowers had lower default rates and exercised default more optimally, but prepayment behavior was not affected. In contrast, Quercia and Spader found better prepayment behavior, but no effect on default rates. These differences may reflect the period

studied in each case; Hartarska and Gonzalez-Vega used the 1990s, whereas Quercia and Spader followed borrowers through 2006, a time of unusually low interest rates, which gave many borrowers the opportunity to refinance. There are also additional concerns with these studies. Quercia and Spader did not test whether there was selection into different types of counseling, nor did they control for selection into treatment—arguing instead that as treatment requirements were determined by lenders, riskier borrowers were more likely to have received treatment (they were unable to test this claim directly). In the Hartarska and Gonzalez-Vega analysis, counseled borrowers were not allowed to apply for a loan until they achieved nonnegative cash flow, defined as income net of expenses, mortgage, and other debt payments. Thus, counseling may have acted as a filter, preventing less financially able borrowers from taking out loans, which would upwardly bias their results. The authors did not discuss what happened to individuals who did not 'graduate' from counseling, leaving it unclear as to whether counseling led to better loan outcomes by improving financial management or by weeding out the less credit-worthy.

By contrast, Agarwal et al. (2009*b*) found little evidence that a state-mandated pre-mortgage counseling program for high-risk borrowers in select Chicago zip codes led to better mortgage choices. At the same time, their study shows how a financial education program can affect outcomes without necessarily improving literacy. The authors showed a significant drop in default rates of mortgages originated in the treated zip codes during the period of mandatory counseling, but it appeared to occur because the riskiest lenders and borrowers left the market, not because the remaining borrowers chose better mortgage products. The threat to lenders of increased oversight and potential fraud detection, as well as the perceived cost to borrowers of attending counseling sessions, dramatically reduced both the supply and demand for credit. Borrowers who were able to choose less risky products to avoid counseling did so, and lenders rejected far more loan applications and originated fewer low-documentation loans during the treatment period (activity resumed to normal levels when the program ended). While some borrowers followed the advice provided by counselors, many modified their loans in ways that were contrary to counselor recommendations, and others took out loans they had been told they could not afford.

Mortgage and credit counseling programs often include services apart from financial education, such as client advocacy and proactive intervention, which also can make it difficult to disentangle the effects of financial education. One such program is the Indianapolis Neighborhood Housing Partnership (INHP), a voluntary mortgage counseling program evaluated by Agarwal et al. (2010). The study found that, controlling for loan characteristics, borrowers who participated in INHP (some of whom had

mortgages originated and serviced by INHP itself) had significantly lower default rates twelve and eighteen months after origination. This result was robust to several econometric specifications and to a matched propensity score model. Although it seemed that INHP's services improved outcomes, it was not clear how much of the effect was due to better loan terms, to INHP's proactive interventions when loans became delinquent, or to improved financial management on the part of borrowers.

The strongest evidence for the effectiveness of mortgage counseling comes from a study of a post-mortgage counseling program by Ding et al. (2008). That program treated over 25,000 borrowers with high-risk characteristics but low-risk mortgages. All loans were fixed-rate and 99 percent had thirty-year amortization periods, but borrowers had low credit scores, and loans had high loan-to-value ratios (three-quarters were over 95 percent). The authors found that telephone counseling delivered to forty-five-day delinquent mortgage borrowers led to a higher cure rate and a lower foreclosure frequency for those particular loans. The authors controlled for selection into treatment with a well-fitting model, noting that their estimates decreased in magnitude as a result, but remained positive and statistically significant. Apart from exemplifying the importance of controlling for unobserved borrower characteristics, this paper suggested that mortgage counseling can be effective if provided at a critical point in the decision process.

One non-mortgage counseling study produces further evidence that financial counseling can affect outcomes. Elliehausen et al. (2007) examine credit counseling programs that five agencies, approved by the National Foundation for Credit Counseling (NFCC), provided to 8,000 borrowers during the summer of 1997; a matched comparison group did not receive NFCC counseling. The study followed credit and payment histories until 2000 and revealed that credit scores, debt levels, and bank account usage improved for counseled individuals. Counseling was most effective for those with the worst initial credit scores and debt behaviors. Differences were much smaller but statistically significant after correcting for selection. This study benefited from uniformity in treatment; the NFCC had specific standards for its counseling providers, and treated borrowers did not receive other NFCC services in 1997. Unobserved financial services received by members of the comparison group would bias results downward, lending more credibility to the estimated effect of counseling.

Optimal program structure

A few papers have investigated whether different *types* of programs vary in effectiveness, and others have estimated the marginal impact of extra hours

of treatment. Evidence on delivery methods is inconclusive, but it appears that extra hours of education or counseling have a positive impact on outcomes. For instance, Hirad and Zorn (2002), Quercia and Spader (2008), and Barron and Staten (2009) compare four types of treatment: home-study, telephone/Internet instruction, classroom education, and one-on-one counseling. Their findings do not consistently support one type of treatment over others. The first two studies evaluate mortgage counseling programs and find that classroom and one-on-one treatment—which tend to be more intensive than other forms—had larger impacts than telephone and home study, which had no significant effects. By contrast, Barron and Staten found that, in a credit counseling program, one-on-one counseling was not more effective than telephone or Internet counseling when clients were allowed to choose the type of treatment. The conflicting studies evaluate two different types of programs, which could explain their diverse findings. It is also possible that the limitations of the mortgage studies biased their results upward, or that Barron and Staten's results do not reflect true impacts due to selection bias.[13] In contrast to Hirad and Zorn (2002) and Quercia and Spader (2008), Ding et al. (2008) found that telephone counseling was effective for forty-five-day delinquent mortgage holders. Again, the different results may stem from program differences (or perhaps pre-mortgage counseling is simply less effective than post-mortgage counseling).

A program designer may also ask when diminishing returns may set in and cause additional counseling to be ineffective. There is evidence that more treatment leads to better outcomes in Clancy et al. (2001) and Collins (2007). The former study reports that for delinquent borrowers, extra hours of counseling (up to 5 hours) did reduce the probability of moving to a more serious stage of foreclosure, so the marginal effect of extra counseling was positive.[14] Clancy et al. (2001) study financial education classes for low-income participants in Individual Development Account (IDA) programs which involved matched savings accounts. Extra hours of class were positively correlated with saving behavior through 18 hours of treatment; someone receiving 12 hours of education saved over $100 more per year than someone receiving no education. The fourteen programs studied all had financial education requirements, but specific content varied. In general, education provided both financial information and strategies for effective saving, as well as instruction on more specific topics, such as home purchase. Although the authors used a two-stage selection model for leaving the program, they did not control for selection into hours of treatment. Thus, we do not know whether the results are due to endogeneity with characteristics of the borrowers or of the particular requirements of each IDA program.

To generate more robust information on treatment types and hours of treatment, it is necessary to correct for selection into treatment types and hours. At the very least, given that some financial education programs appear to be effective, this research agenda is worth pursuing.

Innovative research study designs

Several papers on financial education are notable for the design of their program evaluations. These papers provide instructive examples of randomization techniques that do not require denying treatment with a specific demographic focus, and using unconventional forms of treatment. This work is important, as many program evaluations suffer from endogeneity—that is, selection into a treatment, or into a type and intensity of treatment, is often not random. This problem is difficult to overcome when randomization requires denying treatment to some who want and need it. A few studies have delayed treatment for the control group (rather than denying it entirely), or offered an extra incentive to the treatment group to participate. These studies have had varying success in implementation, but their methods are instructive.

Two studies by Collins (2008) and Servon and Kaestner (2008) delay treatment. In the first, women in both treatment and control groups received the same financial education curriculum, but the control group received it a year after the treatment group. If similar changes in credit scores and saving behavior are observed for the two groups roughly a year apart, then the differences may be attributed to financial education. In the second paper, Servon and Kaestner used the same strategy to test whether access to what they called 'information and communications technologies' (including Internet and online banking services) could be a pathway to financial literacy. They studied a program that gave participants a computer, taught them how to use the Internet and online banking services, and provided financial literacy training. A treatment group was provided computers and instruction immediately, while a control group received the same services nine months later. The study is instructive for its experimental design and for its isolation of access to and facility with technology, although it suffered from implementation problems (imperfect randomization) and produced insignificant statistical results.

In Duflo and Saez (2003), the incentive strategy was illustrated by studying enrollment in a tax-deferred account program by employees at a large university. The authors offered a $20 incentive to attend a university-sponsored benefits information fair, randomly selecting departments within the university and then randomly selecting employees within the selected departments to receive the offer. This setup permitted the authors to

compare behavior of treated employees to that of untreated employees in the same department and also to that of employees in untreated departments. The effect of financial education per se was not evaluated, but their design could be used to develop incentive-based financial literacy programs, thus providing exogenous difference in treatment take-up.

Since consumer educational needs vary widely, financial education programs are often targeted towards specific demographic and socioeconomic groups. As one example, Sanders et al. (2007) studied battered women at four emergency shelters. Two of the shelters had implemented a financial education curriculum tailored to the needs of these women, while the other two shelters had not yet implemented the program. The study suffered from having a small sample, and it only measured subjective measures of financial knowledge and 'self-efficacy'. Further, there was significant attrition before the follow-up exam. Nevertheless, such a research model is promising for situations with more observations, and objective knowledge and behavioral data if more effective follow-up could be done.

All these studies evaluated conventional forms of financial education, whereas more recently, innovative delivery methods have been adopted. Spader et al. (2009) analyzed one such case, a Spanish-language soap opera entitled 'Nuestro Barrio' (Our Neighborhood) that targeted low-income Hispanic immigrants. Information and instruction about financial products such as banking services and credit behavior were incorporated into the plot line to reach audiences that otherwise would not be exposed to financial education. The creators also hoped to overcome traditional barriers to participation in financial education programs, such as time and monetary costs, as well as mistrust of organizations providing the education. Quantifying the impact of this intervention will be difficult, although the cost of delivering the information is relatively low.

Conclusion

In this chapter, we review the literature on financial counseling, financial literacy, and consumer decision-making. Summarily, we found many consumers lack basic financial literacy and are ill prepared to meet their financial goals. In some cases, financial education appears to improve financial literacy and behavior, and is most effective for those who have the least financial knowledge, income, and savings. However, it is not clear that effective programs improve behavior through increased literacy, whether programs are cost-effective, or which types of programs are most effective. Answering these questions will require a great deal more research.

Fortunately, the recent proliferation of financial education programs provides ample opportunity to conduct such research. However, the designs of existing programs are rarely conducive to robust impact evaluations. In their review, Hathaway and Khatiwada (2008) call for the introduction of formal program evaluation methods into the design of financial education programs. This recommendation seems to us to be most appropriate.

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Endnotes

[1] Others have linked financial literacy to cognitive ability. For example, Agarwal et al. (2008*b*, 2009*a*) find that some consumers are more likely to make suboptimal financial decisions by paying higher fees and interest rates, and they are less likely to learn from their mistakes. Agarwal and Mazumder (2010) explicitly link these mistakes to cognitive abilities.

[2] Several surveys conducted in other countries generally confirm the US findings (ANZ, 2003; Miles, 2004; OECD, 2005).

[3] Additional early studies by private firms include CFA/AMEX (1991), EBRI (1995), KPMG (1996), Oppenheimer Funds/Girls Inc. (1997), PSRA (1996, 1997), and Vanguard Group/Money Magazine (1997). Early studies of high-school programs include Bakken (1967), Langrehr (1979), Danes and Hira (1987), and Volpe et al. (1996).

[4] Mandell (2008) analyzes the results in detail and notes that income, parental education, and race are strong predictors of scores.

[5] It is not clear what the direction of this bias may be.

[6] Thirty-five percent did not know the per-period cap on interest rate changes; 41 percent did not know the maximum interest rate allowed; and 20 percent did not know the initial interest rate.

[7] Counseling information was provided by Housing Action Illinois (2007). For loans for which the counseling was aimed at protecting against predatory lending, they also found that 9 percent of loans had 'indications of fraud', 22 percent had interests rates over 300 basis points above the market rate, and half of all borrowers were deemed unable or nearly unable to afford the loan.

[8] The NLSY sample of young adults also performed just as well as the HRS cohort on the same measures of financial literacy, even though the HRS sample population was more likely to have thought about retirement (Lusardi and Mitchell, 2006; Lusardi et al., 2009).

[9] Though this bias could be potentially serious, if we assume that workplace financial education did not convince anyone to save less, then it would imply that financial education did have a positive effect, albeit a smaller one than estimated.

[10] See Kim et al. (1998), Garman et al. (1999), Clark and D'Ambrosio (2002), Kim (2007), and Holland et al. (2008).

[11] See Boyce and Danes (1998), Danes (2004), Peng et al. (2007), and Mandell (2008).

[12] One would expect to see no effect on outcomes for dummy variables indicating the number of years a respondent graduated before a mandate, and monotonically increasing (positive) estimates for each extra year in school after a mandate. Instead, the coefficients on all dummies are large and positive and most are statistically significant. This holds for both participation rates and investment income.

[13] As discussed previously, the selection model of Hirad and Zorn was a rather poor fit and included regressors that may have been positively correlated with the error term in the main specification. Quercia and Spader did not model selection into treatment at all, let alone selection into treatment types. Thus, the results in both papers may be upwardly biased. Barron and Staten do not model selection, but they do find evidence of selection that may have driven their results. In their credit counseling program, Internet clients had seen larger reductions in their credit scores during the year preceding counseling, which could indicate that these clients were particularly motivated to learn and change their behavior.

[14] Yet, the study suffers from a small sample, a short follow-up period of six months, and the possibility that borrowers were simultaneously exposed to other treatments. Furthermore, the instrument for hours of instruction—marketing efforts in the borrower's metropolitan area—leaves open the possibility that more motivated individuals received more counseling.

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