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Evidence and Ideology in Assessing the Effectiveness of Financial Literacy Education

Lauren E. Willis*

ABSTRACT: Financial literacy education has long been promoted as key to consumer financial well-being. It is widely embraced as an effective alternative to substantive legal regulation. Yet its effectiveness has never had more than negligible empirical support. This review (1) sets forth the model of financial literacy education underlying public support for these programs today, (2) identifies pervasive and serious limitations in existing empirical research used by policymakers as evidence of the effectiveness of this education, and (3) recommends a number of alternative public policies suggested by the existing research.

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I. INTRODUCTION

> *In considering means to improve the financial status of families, education can play a critical role by equipping consumers with the knowledge required to make wise decisions when choosing among the myriad of financial products and providers…. Financial literacy can empower consumers … to obtain goods and services at lower cost. This effectively increases their household budgets, providing more opportunity to consume and save or invest.…[E]ducation can help provide individuals with the financial knowledge necessary to create household budgets, initiate savings plans, manage debt, and make strategic investment decisions for their retirement or their children's education. Having these basic financial planning skills can help families to meet their near-term obligations and to maximize their longer-term financial well-being.*
>
> –Federal Reserve Board Chairman Alan Greenspan (2002)

Financial literacy education (hereinafter, "FLE") has enjoyed widespread and long-standing public support in the U.S. In the 1930s, General Motors Corporation Chairman Alfred P. Sloan, Jr. endowed a foundation to create a "nation of economic literates" (Gordon 1940, 403). The foundation spawned national conferences, an association, and a journal, all premised on the belief that "making consumer education"—particularly household economics or what we now call personal finance education—"effective" was within reach (Canoyer 1941, 296). Since then, support for FLE has increased. Many states require their schools to teach it, the federal government devotes financial and logistical resources to it, and financial institutions and community organizations offer it (Bush 2008, Vitt et al. 2000).

FLE is widely believed to turn consumers into "responsible" and "empowered" market players, motivated and competent to engage in financial behaviors that increase their own welfare (Greenspan 2002). Ideally, educated consumers handle their own credit, insurance, savings and investment matters by confidently navigating the marketplace. In this idealized world, substantive legal regulation of financial products is unnecessary and even counterproductive. This vision depends on the belief that FLE can not only improve financial behavior, but that it can do so to the degree necessary for consumers to protect and even increase their welfare in the modern financial marketplace.

Although this vision is seductive to conservatives and liberals alike, the necessary predicate belief in the efficacy of financial literacy education is largely based on ideology rather than evidence. The U.S. Financial Literacy and Education Commission, created to spearhead the federal government's involvement in FLE, recently issued a report that makes claim after claim about what FLE can achieve, although it simultaneously observes that "there is little research on successful methods for financial education" (2006, xi). Financial literacy program advocates make their case by presenting lists of dire statistics about how little Americans know about financial matters and how poorly they are managing their financial affairs (Jump$tart Coalition 2007). But they do not demonstrate that FLE will cure these ills. Policymakers throw mandatory financial education and counseling at problems of bankruptcy and predatory lending without good evidence that the education will help (e.g., North Carolina Predatory Home Lending Act 2000; Bankruptcy Abuse Prevention and Consumer Protection Act of 2005). Legal academics routinely suggest FLE as a solution to consumer personal finance problems—assuming its efficacy without evidence (e.g., Jackson and Anderson 2007, Barr 2002, Johnson 2004-05).

The resources spent on financial literacy education and the opportunity costs of pursuing financial literacy rather than other public policies that might improve consumer credit, insurance, savings, and investment decisions and behaviors call for empirical assessment of FLE's effectiveness. Although cited by policymakers as support for financial literacy initiatives (e.g.,

U.S. Senate Bill 2005, citing Danes 2004), research to date has yet to produce reliable, statistically significant evidence of effectiveness, encouraging findings have not been replicated, and few papers have been scientifically peer-reviewed. Past literature reviews generally have approached the subject seeking to "build[] the case for financial literacy" (Fox, Bartholomae, and Lee 2005). For example, a recent review out of the Federal Reserve Bank of Virginia, after recognizing many of the methodological problems in the existing research and without locating a single instance of replicated results, surprises the reader with the following: "Generally, we can conclude from this literature review that there is a need for financial education and that many existing approaches are effective" (Martin 2007, 22).

This article aims to fill the gap in the literature by critically examining the studies commonly cited as evidence of the effectiveness of FLE. By way of introduction, the article sets forth the model underlying public and policymaker support for financial literacy programs today. The article's critique of existing findings regarding FLE is paired with explanations of the barriers to better research. The article recommends further investigation of a number of alternative public policies suggested by the FLE studies. The concluding section asks researchers to help policymakers and the public better understand the limits of empirical findings regarding the effectiveness of FLE.

II. THE IMPLICIT MODEL OF EFFECTIVE FINANCIAL LITERACY EDUCATION

Consumer financial education is conducted through classroom teaching, self-study materials, informational websites, interactive games, and the educational component of counseling. Programs vary in content, audience, and methodology. But they all aim to achieve welfare-enhancing financial behavior engaged in by consumers as the result of acquired financial literacy. Such literacy requires both cognitive knowledge and skills and a well-calibrated degree of psychological confidence in that knowledge and those skills. Hogarth has elaborated on the cognitive components as "being knowledgeable, educated, and informed on the issues of managing money and assets, banking, investments, credit, insurance, and taxes" and "understanding the basic concepts underlying the management of money and assets (e.g., the time value of money in investments and the pooling of risks in insurance)" (2002, 15). Turning this cognitive literacy into positive action requires a particular degree of confidence—neither underconfidence (Danes 2004; Vitt et al. 2000), nor overconfidence (Barber and Odean 2000).[1]

Ultimately, FLE is only effective if it enables consumers, given their financial resource constraints, to make the decisions and take the actions necessary for financial well-being today (NEFE 2005). Effectiveness must therefore be measured against the decisions and actions our society and marketplace require.

The demands of contemporary personal financial management are prodigious and varied. A U.S. Federal Reserve consumer handbook explains that in shopping for a home mortgage, "[t]o compare two ARMs [adjustable rate mortgages] with each other or to compare an ARM with a fixed-rate mortgage, you need to know about indexes, margins, discounts, caps on rates and payments, negative amortization, payment options, and recasting (recalculating) your loan" (Board of Governors of the U.S. Federal Reserve System 2006, 4). Retirement planning skills, including the ability to predict rates of return (even if "past performance is no guarantee of future results"), are similarly complex. Directions from a U.S. Department of Labor consumer booklet exemplify the government's expectations of consumer proficiency:

> Worksheet C lets you calculate potential growth using a savings growth factor representing 3, 5, or 7 percent rates of return, depending on how much you believe each of the worksheet items will increase in value between now and … 10 years [from now]. To get 10-year totals, multiply the amount you believe you will add monthly to IRAs,

> 401(k)s and other savings instruments by the growth factor you select. The result: the value of your new savings in 10 years. (2006, 47).

Consumers with limited resources might never have a mortgage or own a 401(k), but mere budgeting requires them to perform expected value calculations because incomes in low-wage sectors can be unpredictable from one week to the next (Boushey 2007). These sectors provide fewer employee benefits (GAO 2007). Consequently, low-wage consumers need the literacy to shop for insurance and retirement savings vehicles on the open market, without the expertise or bargaining power of a human resources department.

Some discussions of FLE assume that poor financial outcomes evidence "bad" financial behavior and "good" decisions and behaviors always lead to good outcomes. But this is both too onerous and too weak a demand. Resource constraints, job loss, disability, discrimination, and natural disasters can prevent consumers from enjoying good financial outcomes no matter how high their literacy and how welfare-enhancing their behaviors. For example, paying bills late is generally classified as a "bad" financial behavior reflecting poor cash flow management skills. However, as Getter explains, "[a]n unanticipated negative [income] shock can still reduce solvency and trigger delinquencies even for 'financially responsible' households that have accumulated precautionary wealth" (2003, 99). Longitudinal examination of bankruptcy debtors conducted by Porter and Thorne (2006) indicates that insufficient income—not financial mismanagement—is the key barrier to long-term financial health.

Good financial outcomes standing alone are likewise not evidence of literacy or welfare-enhancing behaviors. Given sufficient resources and a professional financial advisor, even the most spendthrift financial illiterate may experience good financial outcomes.

Diagramed, the model of effective FLE implicitly endorsed by policymakers and advocates appears:

Financial Education → Financial Literacy → Good Financial Decisions & Behavior

III. THE LIMITS OF EXISTING RESEARCH ON FINANCIAL LITERACY EDUCATION

Policymakers routinely cite a number of studies for the proposition that the financial literacy education model works. However, the academics who have performed these evaluations generally do not make such a sweeping claim. Despite some resourceful data collection methods, ingenious research designs, and rigorous statistical analysis techniques, studies have been unable to overcome issues affecting data reliability, research design, measure validity, and interpretation of results. The following describes the major methodological weaknesses of well-cited research on FLE. Most studies suffer from more than one of these, but in the interest of brevity I describe only selected examples of each.

A. Data Reliability Issues

1. *Self-Reports of Knowledge, Behavior, Current Financial Condition, and Past Education*

Research on FLE often relies on surveys (Vitt et al. 2000; Lyons et al. 2006) in which consumers evaluate the education, assess their own knowledge, report their behaviors, reveal their financial condition, or recall their exposure to FLE. But responses to surveys conducted to evaluate FLE are vulnerable to social desirability, demand characteristic, and selective recall biases. Each of these errors is likely to inflate estimates of the efficacy of FLE. Their cumulative effect indicates that relying on survey data to demonstrate the effectiveness of FLE is unsound

Survey data on any topic contains errors. Sometimes errors are small and randomly distributed. Other times, errors are large and systematic. When widespread social norms favor a particular behavior, consumers over-report engaging in the behavior. When respondents believe that administrators want a particular response, results are skewed in favor of that response. Misreporting can also reflect respondents' misperceptions or unconscious biases favoring beliefs, recollections, or predictions about their own behavior consistent with self-concept (Tourangeau and Yan 2007; Mitchell and Jolley 2007, 213-215).

In surveys taken at or after completion of a personal finance course, consumers on the whole report that the course was effective and that they will or already have improved their financial behavior. Demand characteristics render these responses suspect. Consumers likely over-report the extent to which they are following their teachers' instructions. As Martin (2007, 14) notes, "after employees sit through over four hours of training, they are likely to say they learned something" and "they are likely to praise the company that provided the training as a courtesy, if nothing else." Field data supports this conjecture; although Braucher's analysis (2001) of a FLE program for bankruptcy debtors revealed a small negative effect on financial outcomes, the participants rated the classes highly.

The belief in the efficacy of FLE is propagated by the programs. One of the first items in the American Homeowner Education and Counseling Institute's "core curriculum" is "the importance of education" (Mallach 2001, 23 Table 5). When participation is voluntary, programs use "marketing techniques" to induce consumers to incur childcare and transportation costs and spend time attending class rather than earning income (Clancy et al. 2001, 3). Regardless of whether these programs improve financial decisions and behavior, they may convince participants to believe in FLE's efficacy or screen out the nonbelievers at the first class.

Misperception and over-optimism also can skew survey responses unidirectionally. For example, the U.S. Department of Agriculture reports results from its Financial Security in Later Life course: of nearly 50,000 Americans who attended the course and completed a survey, nearly 90% said they "increased their financial knowledge" (2006, 1). But comparing self-assessments to performance tests shows that consumers think they learn more from FLE than they do. In one experiment, well-educated consumers approaching retirement who were given financial training believed their financial planning skills improved. However, while they increased their factual knowledge, they did not increase their ability to make good financial decisions at a statistically significant level; they continued to overestimate retirement income by many years (Hershey et al. 1998). Thus, consumer self-assessments must be taken as a measure of confidence (or overconfidence) rather than literacy.

Society views many personal financial decisions and behaviors as having a normative valence, raising the problem of social desirability bias. In anonymous surveys unconnected with FLE, Americans overstate their good financial habits and understate their poor habits. For example, about 60% report that they pay off their credit card balances in full every month, but card issuer data shows that the number is closer to 40% (Getlen 2005). In the Survey of Consumer Finances, families report on average about a third as much credit card debt as issuers report to the Federal Reserve (Draut and Silva 2003).

Financial literacy programs attempt to reinforce social messages to consumers about the desirability of engaging in particular financial behaviors. With this in mind, consumers are likely to consciously or unconsciously exaggerate their behavioral change after FLE. Mandell, who runs the Jump$tart Coalition's annual nationwide test of high school seniors, has consistently found that students who have taken personal finance classes score lower on that test but report higher levels of thriftiness than their peers (2005). While Mandell hypothesizes that the classes increase savings without increasing literacy, an explanation at least as plausible is that the classes

affect self-reporting but not actual thriftiness. Analyses of the effectiveness of the heavily-promoted Money 2000™ (Osteen, Muske, and Jones 2007) and NEFE High School Financial Planning programs (Danes 2004) suffer from a similar reliance on self-reports of financial behaviors.

Bernheim and Garrett (2003) use self-reports of financial condition to conclude that workplace financial education stimulates savings. They concede that "education may affect reporting, rather than behavior" (1489), but they argue that (a) if education causes people to inflate their savings behavior then it should cause them to inflate their overall wealth and spouse savings behavior, and (b) their data does not show any correlation between education and reported wealth or spouse behavior. This assumes that only intentional misreporting inflates consumer responses to savings questions and that consumers possess sufficient financial literacy and forethought to inflate their savings and wealth numbers in a consistent manner. In reality, social desirability bias could cause respondents unconsciously to report their intended or wishful savings behavior.

Indeed, even if misreporting is conscious, people are not very consistent liars. Bernheim and Garrett's data itself suggests erroneous over-reporting of retirement savings—8.8% of respondents stated higher rates of saving for retirement than rates of saving for all purposes. Further, financial issues are a major source of marital discord, and spouses typically disagree on the family's income, wealth, and debt (Zagorsky 2003). Consumers exposed to FLE might inflate their own savings numbers in contrast to their spouses' numbers.

Studies that survey consumers about past participation in FLE to assess the relationship between FLE and outcomes may suffer from selective recall. Courchane and Zorn (2005) asked consumers whether they had "learned about personal finance in courses or seminars" to estimate the effect of past education on current creditworthiness. However, respondents who have experienced financial setbacks might answer that they had not "learned" from courses or seminars, and might not even recall courses or seminars they had attended. Contrariwise, respondents who have experienced financial success would be prone to believe that they learned from FLE and to have better recall of having received it. Analyses that rely on respondent recall of whether past employers offered financial education (Lusardi and Mitchell 2007) have the same weakness—selective recall is likely to distort estimates of FLE effectiveness upwards.

2. *Unrepresentative Samples*

No voluntary survey receives a perfect response rate. When respondents do not systematically differ from nonrespondents, the response rate does not need to be high for the data to be valuable. However, voluntary surveys of consumers who attend FLE classes are nigh destined to reflect a nonresponse bias, in that the sample that completes the surveys is unlikely to be representative of the population surveyed.

Financial educators complain that surveys given at the completion of courses "are simply too long—unduly taxing program participants, [which] unnecessarily drives down response rates" (Lyons et al. 2006, 218). Participants who believe they learned the most are likely to complete the survey at higher rates, whether out of a sense of pride or reciprocity. The net effect is to inflate the results in favor of finding FLE to be effective.

The population that completes follow-up surveys is no doubt even less representative. Weiner and his colleagues (2005), in their experimental testing of the efficacy of personal finance training given to bankruptcy debtors, ran into this problem. For the test instrument sent three months after financial training sessions were conducted, the response rates of debtors who received the training was 34%, of debtors who did not receive the training was 56%, and of non-debtors was 71%. Those who participated in the program—who, if they felt it had helped them,

would have the most motivation to complete the survey, but who also would be the most embarrassed to report not having changed their behavior—were the least likely to respond.

The National Endowment for Financial Education (NEFE) advertises that three months after completing its ten-hour High School Financial Planning Program, over half the students improved their spending and saving habits (Danes 2004). However, only 17% of the students responded, meaning that about 10% of the students who completed the program reported improved financial habits. Because those who, accurately or inaccurately, believe that they improved their behavior have more impetus to report doing so, it is plausible that few non-respondents saw any improvement. Further, high school students who voluntarily complete and return a survey three months after a program might be unusually approval-seeking and obedient, personality traits predisposing them to exaggerate good behavior due to susceptibility to social desirability bias and demand characteristics.

Given these factors, it is not possible to conclude from these studies that FLE changed participant financial decisions and behavior.

3. *Barriers to Better Data Collection Methods*

If survey data is so unreliable, and those who complete them unrepresentative, why do academics use these sources? Why do researchers not objectively test financial knowledge and skills before and after finance courses? Why do they not track consumer decisions and behavior before and after FLE through longitudinal observation?

Educators explain that their clients do not want to be tested. The U.S. Department of Agriculture Cooperative State Research, Education, and Extension Service (n.d.) warns teachers of its Financial Security in Later Life course that if they attempt to evaluate the program by giving tests, "some adult audiences may be turned off by having to take a 'test,' and choose not to continue participation in the program." Lyons found that while programs claimed to use changes in knowledge, skills, and behavior to evaluate FLE's impact, virtually all of these changes were assessed through voluntary surveys; only 3.1% of educators used "exams, quizzes and secondary data sources" (2006, 226 Table 4). Educators are unlikely to permit testing of their students for fear of deterring participation. If researchers did test students, the manipulation might create attrition bias; consumers who withdrew to avoid testing would differ from those who continued with the course and these differences probably would correlate with factors that affect outcomes (Bloom and Ford 1979).

Tracking financial behavior is even more difficult. Because we generally do not know who will participate in FLE in the future, academics can view pre-FLE behavior only retrospectively, when the consumer's memory has faded or has been distorted by ensuing events. Direct observation of consumer decisionmaking after FLE, were it logistically possible, would alter behavior. Decisions are therefore usually observed only after they are made, in effect observing outcomes rather than behavior. However, as discussed above, outcomes are influenced by more than behavior and behavior is not always accurately reflected in outcomes. In addition, consumers frequently find surveys about their finances invasive, and will not answer the questions (Lyons et al. 2006).

To avoid the biases inherent in surveys and to obtain longitudinal data, some researchers, using exacting procedures to maintain consumer anonymity, have managed to obtain credit bureau reports and credit scores (e.g., Courchane and Zorn 2005; Elliehausen, Lundquist, and Staten 2007). But measuring FLE efficacy through credit report and score changes is problematic. Credit reports contain inaccuracies and incomplete information, and these errors are not randomly distributed but rather vary by consumer age and income and share of minorities in the consumer's home census tract (Avery et al. 2004; Staten and Cate 2005). Consumers might

object to giving direct access to their financial transaction records so as to correct any credit report errors, due to privacy concerns or worries that errors currently in their favor might be corrected. This could be an obstacle to generating a sufficiently large and random sample of subjects absent confounding incentives for subject participation.

B. Research Design Issues

1. *Confounds*

Financial education is frequently bundled with other forms of assistance. As a result, the contribution of the education to any improved financial behavior or outcomes is uncertain. Programs that provide financial assistance with their classes may lead to better outcomes due to the former rather than the latter (Mallach 2001). For example, the American Dream Demonstration Project gives its low-income participants between one to seven dollars for every dollar they save (Schreiner, Clancy, and Sherraden 2002), obscuring the effect of the FLE component of the project.

Financial counseling may improve financial outcomes due to non-educative components of the process rather than any change in literacy. "Counseling" includes actions counselors take on behalf of their clients. Some homeownership counselors admit that their interventions on behalf of consumers are more effective than their interactions with consumers (Mallach 2001). Credit counselors can negotiate payment plans that reduce interest rates, fees, and minimum payments (Staten, Elliehausen, and Lundquist 2002). Counselor intervention can alleviate consumer stress which may improve health and increase employability and productivity, potentially increasing income (Kim, Garman, and Sorhaindo 2003) without increasing financial literacy.

Merely disputing errors on a client's credit report can have an effect. About half of the 16 million credit reports sent to consumers each year result in a question or dispute, and of the formal disputes lodged, over half result in a change to the credit report (Pratt 2003). Results of studies that use credit bureau reports and credit scores (e.g., Elliehausen, Lundquist, and Staten 2007) to assess outcomes could reflect counselor-initiated changes in the credit report data rather than consumer changes in behavior, skewing results towards findings that policymakers interpret as evidence that FLE works.

Even when they do not take actions on behalf of clients, counselors and teachers in small class settings can give consumers personalized advice and written action plans. Compliance with these specific instructions might improve finances regardless of whether the consumer increases financial literacy in the process. For example, because "[t]he first requirement for credit counseling clients is to cut up all their credit cards and close the accounts" (Kim, Garman, and Sorhaindo 2003, 77), all but the noncompliant consumers who attend counseling should close these accounts. Findings of investigations that use a reduction in the number of open accounts as evidence of improved financial behavior (Elliehausen, Lundquist, and Staten 2007) could reflect counselor persuasiveness and consumer obedience rather than any effect of FLE.

A particularly frequently-cited analysis by Hirad and Zorn (2002) of participants in Freddie Mac's Affordable Gold mortgage program found that classroom-based homeownership counseling significantly reduced mortgage delinquency rates. Without testing consumer knowledge, the title of their paper attributes these good outcomes to "knowledge" gained through the counseling. But in the text, the authors also note that counselors and classroom teachers give consumers individual attention regarding their personal situations. It is therefore plausible that these consumers received direct assistance and personal financial advice, and that these non-educative components contributed to the study's findings.

Elliehausen, Lundquist and Staten compared credit reports of consumers who had received counseling and consumers who had not, controlling for a number of other factors, and found that counseled consumers experienced comparatively more improvement in creditworthiness during the three years subsequent to the counseling dates. They published a monograph preliminarily attributing their results to "financial education conducted in a one-on-one setting" (Staten, Elliehausen, and Lundquist 2002, 8). This monograph is widely cited as evidence of the effectiveness of FLE (e.g., Bernanke 2006; Fox, Bartholomae, and Lee 2005; Kim, Garman, and Sorhaindo 2003). But because the study does not limit the treatment to education, the more recent, peer-reviewed version of this paper concludes only that any greater improvement in credit standing experienced by counseled consumers is "associated with" counseling (Elliehausen, Staten, and Lundquist 2007, 26).

A variant on this problem was spotted by Braucher, who analyzed the effects of mandatory credit counseling on bankruptcy debtors. Braucher compared debtors in bankruptcy districts that required counseling to those in districts that did not, controlling for a variety of other ways in which the populations of consumer bankruptcy debtors might differ between districts. At first blush the data indicated that consumers receiving credit counseling had better outcomes—the districts requiring consumer counseling had better bankruptcy payment plan completion rates. But on closer analysis it became clear that the districts requiring counseling also created conditions under which it was easier for debtors to complete their plans. Judges in these districts approved more lenient payment plans and more frequently required payments under those plans to be deducted directly from the consumers' paychecks—a self-control mechanism rather than education. Once these differences between districts were controlled for, "[e]ducation resulted in a small, significant and negative effect" on outcomes (2001, 578).

Braucher's unreplicated analysis of FLE provided in the particular setting of bankruptcy does not prove that FLE is ineffective, but it demonstrates the susceptibility of analyses of the effectiveness of FLE to an upward bias due to confounds.

2. *Inadequate Controls*

The biggest methodological problem that undermines the results of FLE efficacy studies has been the lack of adequate controls that would be needed to demonstrate the causal links from education to literacy to financial decisions and behaviors. Typically, academics use as the treatment group consumers who chose to receive FLE. Common sense suggests many unobserved ways in which these consumers differ from those who do not participate. In addition to being better informed or more motivated (Bernanke 2006), those who attend may have more free time for researching and making financial decisions or less embarrassment and denial about having personal finance problems or having made "bad" decisions in the past.

A number of studies support this intuition. College students who completed an on-line version of VISA's Practical Money Skills for Life course had better financial behaviors than students who were offered but declined the course, but the former were on average already wealthier, more educated, and more creditworthy (Gartner and Todd 2005). In the Elliehausen, Lundquist, and Staten (2007) study raw data, credit counseling produced a 66 point average increase in credit scores of debtors who had low scores prior to the counseling—a large change amounting to a 30% reduction in predicted likelihood of bankruptcy. However, after applying statistical techniques to reduce selection effects, the authors discovered that counseling produced less than a single point increase in credit score, indicating that selection, not FLE, probably was responsible for the higher credit scores of counseled consumers.

Most data sets containing information on FLE and financial outcomes provide no reliable way to control for selection effects. One academic used extensive Health and Retirement Study

data on households whose head is between 50 and 61 years of age to evaluate FLE (Lusardi 2004). Approximately 13% of the sample stated that they had attended an employer-sponsored "meeting on retirement planning," although they gave no indication of when this meeting took place. The author found that these households had saved more for retirement, controlling for variables such as demographics and income, and concluded that "financial education can boost saving, particularly for those with low financial literacy" (157).

But consumers who attended these meetings might have saved just as much without attending. Perhaps they accumulated their savings prior to the meeting and attended to learn how to plan their draw-down rates during retirement. Although the study used an impressive number of controls, without an instrumental variable that affects attendance at FLE but does not affect the dependent variable (here, savings), controlling for selection when subjects attend FLE voluntarily may not be possible. Similarly, the method of analysis used by Elliehausen, Lundquist and Staten (2007), a two-stage least squares regression, can significantly reduce, but can not eliminate, selection effects (Heckman et al. 1998).

Supporting their conclusion that increased financial literacy positively affects financial behavior, Lusardi and Mitchell (2007) find that consumers who score higher on tests of financial knowledge report having thought more about retirement. To address selection effects, the study uses as an instrumental variable consumer self-reports on how much of their education, including high school, college, or higher degrees, was devoted to economics. Having devoted "a lot" of their coursework to economics has a strong correlation with financial literacy in this sample of relatively highly-educated consumers.

However, the authors do not demonstrate that having devoted a lot of schooling to economics is not correlated with omitted variables that affect retirement planning, suggesting only that economics education is an "arguably exogenous instrument[]" (15). Yet it is at least plausible that the same personality traits that lead consumers in their youth to choose "a lot" of economics coursework independently lead consumers in adulthood to plan more for retirement. Preliminary research on determinants of success in economics courses indicates that students who are goal-oriented and make detailed plans (the "judging" personality type indicator of the Meyers-Briggs scale) perform better (Ziegert 2000; Swope and Schmitt 2006). It seems plausible that success leads to enrollment in more economics courses, and that the same personality traits that lead to classroom success lead to planning more for retirement.

In what appears to be the only direct examination of self-selection in this context, Meier and Sprenger (2007) found that consumers who chose to participate in FLE differed sharply from those who did not. Their experiment offered over 800 low-to-moderate income consumers waiting in line for volunteer income tax assistance programs a free 15 minute credit counseling session. Those who accepted the offer had significantly lower financial discount rates—meaning they were more willing to wait for a larger financial reward in the future rather than taking a smaller reward sooner—than those who declined. Consumers who accepted also had more education and prior financial knowledge, although even controlling for these, discount rates remained significant. Consumer decisions were surely influenced by other unmeasured factors, such as a preference for privacy rather than allowing a stranger to see one's credit report or a desire not to be confronted with one's own past credit problems. But the authors' conclusion—that consumers who choose to participate in FLE concern themselves more with the future and so are likely to engage in relatively more welfare-enhancing financial behavior even absent any participation in FLE—remains plausible. Given that the "class" in the experiment required so little time and effort, a real FLE course might be expected to show stronger selection effects.

In sum, self-selection poses a significant problem for much FLE research, one that likely biases estimates of the effectiveness of FLE upwards.

3. *Barriers to Better Research Design*

A number of researchers have ingeniously eliminated much of the bias created by individual self-selection by focusing on settings where FLE is exogenously set. Bernheim and Garrett (2003), in their workplace financial education research described above, did not compare employees who participated in FLE programs and those who did not. Instead, they attempted to compare employees at workplaces that offered retirement education and assistance and employees at workplaces that did not. In their investigation of the effects of high school FLE, described further below, Bernheim, Garrett and Maki (2001) examined differences between consumers who had attended high school in states that mandated financial education and those who had lived in states that did not. Setting aside the methodological problems discussed above and below, that some employees and students in workplaces (schools) that did not offer (require) FLE must have received FLE, and that some employees in workplaces that offered FLE certainly did not receive FLE, should drive their estimates of the effectiveness of FLE downward.

However, while this methodology can eliminate self-selection problems, it does not create true controls. States with financial education mandates can not be kept identical in all other relevant respects to states without mandates. Workplaces that do not offer financial education and advice are not controlled settings in which employees are otherwise exposed to the same conditions as employees in workplaces not offering these services. Given differing economic conditions across states and firms, omitted variable bias is likely, although the magnitude and direction in which this would skew results is uncertain.

Nonexperimental designs cannot prove causality in the social sciences (Mitchell and Jolley 2007, 212). Randomized experiments are not always possible, forcing researchers to rely on observational or survey data. But comparisons between results of nonexperimental and experimental research consistently demonstrate that findings from the former deviate—sometimes quite substantially—from the latter.

For example, a recent article compared the findings of three different research designs used to determine the effect of get-out-the-vote phone calls on voter turnout. The randomized experiment found no increase in voter turnout, whereas regression analysis and matching estimation found a large and significant increase (Arceneaux et al. 2006). Another article compared experimental and nonexperimental analyses of the impact of, e.g., job training programs, and found that nonexperimental studies often produced results that differed from experimental results by "policy-relevant margins" (Glazerman, Levy, and Myers 2003). As Caskey has argued, "these results alone should raise doubts about the ability of non-experimental studies, even when conducted in a very conscientious manner, to measure accurately the impact of [financial] education" (2006, 5).

Societal pressures and institutional review boards beyond the control of the researchers prevent most from engaging in randomized experiments that could eliminate confounds and establish a proper control group for comparison. Analyses of consumer education have long wrestled with the problem that "program administrators find it socially unacceptable to withhold an educational treatment from any individuals" (Bloom and Ford 1979). For example, if a high school offers a personal finance course, educators are unlikely to allow researchers to randomly assign one group of students to receive the treatment (i.e., make the course mandatory for these students) and assign another group of students to be in the control group (i.e., prohibited from taking the course). For these reasons, it is extremely difficult to find naturally-occurring FLE settings that can be used to determine the efficacy of FLE.[2]

C. Measurement Issues

Recall the model of effective FLE:

Financial Education → Financial Literacy → Good Financial Decisions & Behavior

To empirically evaluate the model, researchers must employ measures of the following: exposure to financial education, financial literacy levels, and the quality of financial decisions and behavior. To validate this particular model, they must demonstrate that any link between FLE and improved behavior is moderated by increased literacy. But locating reliable measures of each stage of the model and of the model overall has proven challenging.

1. *Precision and Accuracy of Measures of Exposure to Financial Literacy Education*

To avoid the ubiquitous self-selection problem discussed above, Bernheim, Garrett and Maki (2001) use a proxy measure for exposure to FLE in their investigation of the effect of high school financial education on adult retirement savings. This proxy is whether the state in which the respondent attended high school had a mandate requiring students to receive instruction in personal finance at the time the respondent was in high school. The authors find that residents schooled in states with mandates had more retirement savings. Their proxy measure appears to have some validity, in that adults who attended high school in "mandate" states were more likely to report having attended a class that included personal finance topics. However, only about half of those in "mandate" states recalled taking such a course, and over a quarter of those in "nonmandate" states recalled taking such a course. Further, state education departments admit that "mandates" can mean as few as 25-50% of students are receiving FLE (Scott 1990, 64-65). These mandates thus do not appear to be a particularly accurate measure of exposure to FLE.

Depending on the high school state distribution of respondents, the misclassification of even a state or two could substantially undermine the Bernheim, Garrett and Maki results. As they note, there is conflicting evidence as to which states had mandates during the relevant time period. Officials in New Mexico and Oklahoma, which the authors classify as "mandate" states (440 Table 1), responded to surveys conducted in 1985 (Brennan 1985, 14-15) and 1990 (Scott 1990, 49) that they had no consumer education mandates and that (Scott 1990, 65-66) fewer than 25% of their students took such a class by graduation. In the 1985 survey, officials in Pennsylvania, classified by the authors as a "nonnmandate" state, responded that they had a consumer education mandate (Brennan 3, 16). Given that only 200 of the respondents in the study attended a high school the authors designated as covered by a mandate, results could be spurious, perhaps reflecting economic conditions in the state (70% of respondents still lived in their high school state) rather than any effect of FLE.[3]

A similar problem is at play in the research of Bernheim and Garrett on workplace "education" and saving for retirement. As their measure of exposure to education, the authors use self-reports as to whether the respondent's employer offered "seminars, professional assistance, or informative materials to assist with retirement planning" (2003, 1494). Even accurate survey responses to this question would be a poor measure of FLE because they do not distinguish between seminars and professional assistance. The authors further report that during this time period, of employees offered workplace "education," just over half were eligible to attend seminars and slightly higher percentages were given access to a financial planner or investment advice. Thus, while the study is frequently cited as demonstrating that workplace FLE boosts savings, the results might reflect the efficacy of professional retirement planning assistance in boosting savings. This would not support the model of FLE that its promoters have in mind.

2. *Validity of Measures of Financial Knowledge and Confidence*

A number of studies are cited as evidence for the first causal link of the model, between FLE and financial knowledge and skills. One investigation, for example, finds that taking a college course covering personal finance topics increases a consumer's score on the National Association of Securities Dealers (NASD) investment knowledge test by three-quarters of a point out of ten possible points (Peng et al. 2007). However, the NASD test asks about basic facts—for example, whether "a reasonable average annual return that can be expected from a broadly diversified US stock mutual fund over the long run" is 5%, 10%, 15%, 20%, or 25% (283). The Jump$tart survey asks similar factual questions, although it also asks questions requiring simple addition, subtraction, and multiplication (Jump$tart 2006). Even the questions that consumers find most challenging and are least likely to answer correctly—e.g., when interest rates fall, what should happen to bond prices? or if you put $100 in an account bearing interest at 20% per year and make no withdrawals, will you have more or less than $200 at the end of 5 years? (Lusardi and Mitchell 2007)—are factual or require mathematical calculations based on specified dollar figures, interest rates, and time periods.

But the knowledge and skills evidenced by these tests are not what a consumer would need to compare two adjustable rate mortgages or calculate the amount they need to save for retirement—knowledge and skills the Federal Reserve and the Department of Labor implicitly believe consumers should have. For low-wage sector consumers, the literacy needed to score highly on these tests would not help them establish and follow household budgets; the income side of these consumers' budget equations is too uncertain to be determined without probability calculations based on forecasts of macroeconomic factors that will affect their employers' labor needs. Moreover, consumers who answer "correctly" that the stock mutual fund annual return that can be expected is 10% are not knowledgeable so much as impressionable; past performance of the stock market can not be used to predict future performance over long horizons, and a consumer who calculates retirement savings by assuming a 10% return is not engaging in good financial behavior as some economists see it (e.g., Bodie 2003).

Further, as Lusardi and Mitchell (2007) have demonstrated, scores on these knowledge and skills tests are not true indicators of even the knowledge and skills tested because, to varying degrees, consumer answers are guesses. Given that most of these test instruments use a multiple choice format, a sizeable proportion of correct answers might be due to chance.

As for the link between FLE and confidence, a number of sources note that participants self-report increased financial confidence after taking a personal finance class. One report quotes a consumer who had just completed the Financial Security in Later Life course: "'It is amazing how a few changes made me feel empowered. I made a 'to do' list and I am determined to get them all checked off.'" (U.S. Department of Agriculture 2006).

This is an incomplete measure of the element of "confidence" in the FLE model, however, because it could indicate over-confidence rather than appropriate confidence. In one study, half of the respondents who reported that their financial literacy was at the highest end of the scale did not objectively test within the highest quartile of the sample, and over 15% were in the bottom quartile (Lusardi and Mitchell 2007). For FLE to be effective, consumers' confidence in the knowledge and skills they believe they have gained must be justified. To estimate whether a personal finance course produced an improved degree of confidence, we would need to compare how well-calibrated consumers' self-assessments of knowledge and skills were before and after participation in FLE.

3. *Validity of Measures of Financial Behavior*

Studies cited as evidence for the proposition that FLE improves behavior use rough measures of behavior quality. Most commonly, this research takes change in or amount of savings as its indicator of FLE's efficacy (e.g., Lusardi 2004; Bernheim, Garrett, and Maki 2001; Bernheim and Garrett 2003). Other measures used to assess the quality of financial behavior derive from data in credit reports, such as number of credit cards, late payments, bankruptcies, foreclosures, and credit score (e.g., Elliehausen, Lundquist, and Staten 2007; Courchane and Zorn 2005). For studies of particular FLE programs, progress toward the goals of the program is the measure by which researchers typically measure behavior quality (e.g., Braucher 2001; Hirad and Zorn 2002).

However, these measures may not reflect the quality of financial behavior accurately. What appears to be good financial behavior may be caused by inertia rather than literacy. Savings and insurance may reflect employer decisions about default or matching contribution rates. Credit reports are not only riddled with error, but their contents and resulting credit scores may not be good measures of behavior. Credit scores discount excellent financial behaviors by individuals who pay rent rather than a mortgage, place great weight on poor financial outcomes that may not be a result of poor financial behavior, and reflect good financial outcomes unrelated to literacy (Avery et al. 2004; Staten and Cate 2005).

Goals of some FLE programs, such as increasing savings, avoiding bankruptcy, or reducing mortgage default, are not always financially wise. Savings is not invariably the best use of money; investment in education or job training, housing, or a business might generate higher long-term returns. Declaring bankruptcy and getting a fresh start might be optimal financial behavior for consumers who have lost their homes or jobs in a natural disaster. Defaulting on an underwater mortgage—meaning the debt exceeds the market value of the house—can be financially advantageous, depending, e.g., on whether the negative effect on the consumer's credit report is smaller or greater than the positive effect on her balance sheet from the divestment of the debt (e.g., Hartarska and Gonzalez-Vega 2005).

Further, because an individual's financial well-being is determined by meeting a web of interrelated goals and along many metrics, meeting one goal or improving one metric may have little effect on a consumer's financial situation overall. If FLE increases savings, this will be of no help in retirement if the money is poorly invested, diverted to crises precipitated by underinsurance, or spent to meet subsequent credit payment obligations (Lerman and Bell 2005). If FLE causes a reduction in late payments sufficient to increase credit scores, this may reduce prices paid for insurance and credit, but only if consumers understand different pricing structures for insurance and credit products and shop for the best price (Willis 2006).

4. *Completeness of Measures of the Financial Literacy Education Model*

Policymakers cite academic work as support for the entire FLE model, although most work examines only one link, either between FLE and literacy or between literacy and behavior. Tests of knowledge and skills such as the Jump$tart survey can demonstrate changed literacy, but not whether that literacy will change behavior. On the other hand, observations of behavior change can not demonstrate that changed financial literacy was the moderator.

Investigations that look at both links falter if they measure the effect of FLE on financial literacy separately from the effect of financial literacy on financial behavior. For example, research that demonstrates that economics training and workplace retirement education are associated with increased literacy and that increased literacy is associated with better financial behavior (Lusardi and Mitchell 2007), does not demonstrate causation from FLE to literacy to behavior. Likewise, a finding that consumers increased their financial knowledge on average and

improved their reported behaviors on average after participating in financial training (Weiner et al. 2005) does not show that these consumers used gained knowledge to improve their financial behavior.

Instead, a model in which financial literacy is the moderator between FLE and financial behavior must be empirically validated. Courchane and Zorn's recent study (2005) is designed to test the entire model. If better data were available, selection effects eliminated, and valid measures of good financial behavior established, this design could form the basis for more complete research designs in the future.

5. *Barriers to the Development of Better Measures*

Why have researchers used financial education, literacy, and behavior measures of questionable validity? Demonstrating the effectiveness of the FLE model requires valid measures of each of these, as well evidence of causal links among them. But the FLE model is underspecified in both technical and normative respects. We lack consensus as to both "what should be measured" and "how it should be measured" (Lyons et al. 2006, 216). As researchers at the Federal Reserve have conceded, despite that agency's own efforts to promote FLE, "[i]n analyzing the efficacy of financial literacy programs, the primary challenge is defining and quantifying 'success'" (Braunstein and Welch 2002, 449).

We have consensus on general principles of good financial behavior: where cost-effective, consumers should perform an adequate search for information about alternatives, should expend the needed resources to analyze those alternatives objectively, should base decisions on that analysis by trading off incommensurate costs and benefits where needed, should plan for the future and implement those plans through budgeting where necessary, should select financial products that meet needs without paying excessive prices or incurring excessive risk, should have a personal financial safety net through insurance and/or precautionary savings, and, once a safety net is established, should accept those risks that present a positive probability of higher returns rather than only low-risk low-return alternatives.

But assessing whether a particular consumer has followed these principles before and after receiving FLE would require us to more concretely operationalize each of these principles. Here measurement instruments flounder, because we have few benchmarks for evaluating financial decisions and behavior.

First, we lack technical agreement about which financial decisions and behaviors are good ones. Ask three different planning software programs how much to save for retirement and they will give you three different results (Darlin 2007). The common wisdom dispensed to consumers about investing for retirement is that they should invest in stocks when young and gradually shift to lower-risk investments as they age, but respected economists disagree (e.g., Bodie 2003). In 2004, Federal Reserve Board Chairman Alan Greenspan publicly stated that consumers who took out fixed rate mortgages were leaving money on the table (Greenspan 2004), but today we know that many consumers would be better off if they had declined adjustable rate mortgages they can no longer afford (Bajaj 2007).

Second, because consumer circumstances vary, behaviors that are welfare-enhancing for some are not for others. Consumers who are not "in a financial position to maintain a healthy [bank] account" might be better off without one (Lyons and Scherpf 2004, 229). In addition to paying overdraft charges, these consumers could suffer lowered credit scores that result in higher credit and insurance costs over the long term. For low-income families, reducing current consumption to accumulate savings may do more harm than good (Scholz and Seshadri 2007). Homeownership also can have a down side; it appears to lead to poorer neighborhood conditions, on average, for low-income consumers who were previously renters (Van Zandt 2007).

Even a credit card over-the-limit fee does not necessarily reflect a poor decision. On financial grounds alone, paying for medical treatment and incurring the fee might be wiser than foregoing treatment and suffering health consequences that reduce earning potential. These contextual factors mean that operationalized measures used to evaluate FLE must vary with the circumstances.

Third and more fundamentally, we lack normative consensus about the quality of financial decisions and behavior (e.g., Braucher 2001). If financial decisions were just about money, if they were part of a game without real life consequences, we could develop decision rules that maximize wealth. But financial decisions do have consequences, requiring tradeoffs among costs and benefits that are valued very differently by different consumers.

Normatively, the decision to purchase anything from Neiman Marcus might be poor. Normatively, paying for a daughter's wedding dress and incurring a credit card over-the-limit fee might be good. The quality of any financial decision will depend on the values held by the consumer and a host of other unobserved situational, psychological, and social factors.

D. Issues in the Interpretations of Results

1. *"Findings" Suggestive of Bias*

The ideological belief that FLE is effective runs so deep that even well-respected researchers—or perhaps the editors who publish their work—at times misinterpret null results from their own studies as providing support for the FLE model.

For example, a National Association of Securities Dealers investigation found that elderly consumer fraud victims were more financially literate, on average, than elderly nonvictims:

> A major hypothesis going into the survey was that investment fraud victims do not know as much about investing concepts as non-victims and would therefore score lower on financial literacy questions. In fact, the study found the exact opposite: investment fraud victims scored higher than non-victims on eight financial literacy questions. Additionally, a subgroup of "likely active investors" was created within the larger group of non-victims to determine if the difference in financial literacy scores had to do with the number of active investors in the non-victim group. The investment victims outscored even this subgroup of likely active investors on the financial literacy questions (2006, 5).

Perhaps the experience of being victimized led to increased knowledge about investing. Perhaps pre-existing knowledge led to overconfidence which led to victimization. Perhaps the personality characteristics that lead some to seek and retain knowledge about investing also make them prone to falling for investment fraud schemes. The causal mechanism is unclear, so all one can deduce from the data is that literacy is correlated with the incidence of fraud. Instead, the study asserts that "[t]his finding suggests that financial literacy programs are necessary but probably not sufficient to prevent fraud" (ibid.).

A study commissioned by the State of Washington to examine the financial literacy of victims of predatory home lending shows the same pattern, with the author erroneously claiming that the study "strongly supports the need for an education program that teaches financial concepts to consumers" (Moore 2003, 15). The author tested consumers who had taken loans from a predatory home lender ("victim group") against a sample from the general population. Test results indicated that the former knew relatively more about home mortgages, but less about investments (27 Table 2). The author concludes that the group who had taken loans with the predatory lender had "lower financial knowledge" and would benefit from a literacy program (60, 61).

The author does not explain how increasing knowledge of investments would help consumers avoid predatory lenders. More plausible explanations of the data would be that either victims were made more vulnerable by their knowledge, perhaps due to overconfidence, or victims became more financially literate in the area of mortgages through their bad experience.

The conclusion in an evaluation of the Money 2000™ program that its "data lend support for the efficacy of financial literacy training in promoting improved financial behaviors" is similarly unfounded (Osteen, Muske, and Jones 2007). The only behavior changes reported in the study are decreased debt for some participants, increased savings for some, and increased debt for some. Overall, the study reports that increased debt exceeded decreased debt. Increased debt could be a good financial behavior, depending on the surrounding circumstances, but it could also be a poor financial behavior. The data here does not support the conclusion that FLE is effective.

2. *Low Statistical Significance*

Some findings cited as support for the FLE model lack statistical significance at the 0.05 level. Gartner and Todd's research (2005) on web-based FLE based on VISA's MoneyChoices and Practical Money Skills for Life programs produced no statistically significant results. A more complex picture emerges in the Hirad and Zorn (2002) examination of the effects of homeowner counseling and education given to Freddie Mac's Affordable Gold borrowers. The authors explain that after adding controls for selection effects, their estimates for the efficacy of one-on-one, self-study, and telephone homeownership counseling are not statistically significant. With controls, their only statistically significant finding is that classroom counseling is effective. Without a plausible account of why classroom counseling would be more effective than one-on-one counseling, even the strength of the classroom counseling finding is questionable.

The preliminary results of the Courchane and Zorn (2005) study designed to test the entire FLE model suffer from low statistical significance as well. They compiled a large data set to test whether there is a link between FLE, financial knowledge and confidence, financial behavior, and creditworthiness (the study's financial outcomes measure). With elaborate controls, the results show that respondents who reported learning more from financial seminars or classes (the study's FLE measures) were relatively more confident in their financial knowledge (Table 9). But preliminary analysis of the data does not demonstrate at the 0.05 level the relationships postulated in the FLE model between education and knowledge (Table 10), nor, given high financial confidence, between various levels of financial knowledge and financial behavior (Table 11). Further, FLE through seminars does not have a relationship to financial behavior demonstrable at the 0.05 level (Table 11). Although the data indicates that better financial behavior is associated with better outcomes (Table 12), FLE has no direct relationship to outcomes at the 0.05 level (Tables 12 and 13).

Admittedly, 5% is not some holy grail; "... surely, God loves the .06 nearly as much as the .05" (Rosnow and Rosenthal 1989, 1277). But some of the results here were obtained at the 0.12 to 0.99 level (e.g., Hirad and Zorn 2002, Table 5A-2; Courchane and Zorn 2005, Tables 10, 11, 12, and 13). The researchers explain that their results cannot reject the possibility that FLE is not effective (the null hypothesis) at conventional confidence levels. But policymakers and others who cite their results are not always so careful (e.g., Bernanke 2006; Martin 2007).

3. *Limited Value of Reported Positive Effects*

A number of studies have reported positive effects that, methodological issues aside, might be caused by FLE. However, some of these improvements are very small.

Elliehausen, Lundquist and Staten (2007) present evidence of a small negative relationship between credit counseling (which they assert involves a FLE component) and debt. For consumers in middle and lower income groups, counseling was associated with a reduction in debt between 2% and 12%, although at higher incomes counseling was associated with an increase in debt. However, while a 12% reduction in debt for low income consumers appears to be cause for celebration, it is apparently not important enough to affect these consumers' credit scores; the authors found counseling was associated with virtually no increase in credit scores.

Tennyson and Nguyen (2001), after contacting state education officials to confirm their state classifications, find improved financial literacy when particular FLE coursework is mandated for high school students. They divide data from the Jump$tart test of high school seniors into results from states with no FLE curriculum mandates, states with general mandates but no specific required course content, and the three states that require students to take specific coursework in personal finance. Controlling for other variables that they determined affect scores, they find no differences in scores between students in the first two types of states, but that students in the states with specific coursework mandates scored an average 2.3 points higher. Examining particular questions, they find that these students score no higher on the questions about spending, debt, or money management, but outperform students in other states on questions about savings, investing, and income.

Unfortunately, financial gains accrued through knowledge about savings, investing, and income can be quickly lost through welfare-reducing spending, debt, or money management decisions. Further, the gain in scores associated with mandated personal finance coursework amounts to a difference of less than 1 of the 31 questions on the test. On average, students who attended schools with financial coursework mandates answered fewer than 60% of the questions correctly.

Weiner and his colleagues (2005) assessed the effectiveness of a voluntary program offered to consumers in bankruptcy in part by comparing the financial literacy levels of debtors who received financial training, debtors who did not, and non-debtors. All subjects were tested for their financial knowledge using 12 identical questions before the training date and three months later. The group that received training received course materials to bring home. It also was the only group to increase their average score at a statistically significant level. The authors conclude that "[a]lthough the gains are modest, these data show strong quasi-experimental evidence that the financial literacy training program improves knowledge of appropriate saving, spending, and credit use" (363).

The score increase for trained debtors was 4%, equivalent to less than half a question. Given that the response rates of trained debtors to the post-test was 34% and of untrained debtors and non-debtors were 56% and 71% respectively, and that when self-administering the post-test some of the trained debtors likely consulted their course materials, the 4% increase in the average trained debtor's score might not reflect even a modest gain in financial literacy.

The American Dream Demonstration Project is routinely cited for its findings that "financial education has positive effects on savings and … courses need not be long to take advantage of the potential benefits" (Schreiner, Clancy, and Sherraden 2002, 51). For "savers" who attended personal finance classes in this program, each hour of FLE up to eight hours was associated with a statistically significant average increase in monthly savings. Eight hours of education may have added about $125 to annual savings of "savers" during the program, in which the average participation lasted two years.

However, these results are reported only for the 56% of participants who saved a net of at least $100 in the program (iv and 32). For these "savers," an additional $125 each year could create a buffer that would help them cope with small financial shocks—e.g., missing a couple of

days of work. But many events that require consumers to dip into savings are much more expensive. Further, the time spent in class and the increase in annual savings had to come from somewhere, and so results must be balanced against the reduction in hours available for work and in monthly spending experienced by these low-income consumers (Scholz and Seshadri 2007). On the whole, this FLE program may not have improved participant financial welfare.

4. *Barriers to Better Interpretation of Results*

Data and financial support for FLE research is not easy to come by. FLE advocates want to spend every dollar on programs. Government agencies have limited resources. The financial services industry has no interest in discovering that FLE is ineffective. These programs help them promote goodwill, penetrate new markets, cull out unqualified home loan applicants (McCarthy and Quercia 2000), and, ideally, increase retirement savings under their management. If FLE is not effective, industry loses its most potent argument against regulation—that consumers are better off making their own financial choices and that impediments to good consumer financial decisionmaking are better addressed through FLE. When industry is supplying the data and/or funding, it cannot help but have an effect on the publicized research.

On the other hand, none of the above studies hid their weaknesses. Most include a substantial discussion of research limitations. But editors may obscure these caveats. For example, one table of data on consumers' intentions to change their financial behavior is titled "Estimates of Changes in Retirement Savings Behavior" (Clark et al. 2006, 62 Table 6), even though the authors make plain in the text that few consumers followed up on their intentions. The "A Little Knowledge Is a Good Thing" title of the Affordable Gold homeownership counseling study similarly may reflect a publisher's choice; as noted above, the authors neither tested participant knowledge nor claim participants gained any (Hirad and Zorn 2002). Even without these sorts of invitations, FLE advocates and policymakers tend to see what they want to see in empirical work, and researchers have little ability to control that.

IV. CONCLUSION

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 or ineffectiveness may be insurmountable. But the conclusion is not that we must accept or reject FLE on ideological grounds alone and move on. Rather, we should search for alternative public policy models that recognize what financial education might realistically achieve. At least two such models are suggested by the studies critiqued above.

The first alternative policy model is suggested by Bernheim, Garrett, and Maki (2001) regarding their finding that high school curriculum mandates increase savings. They conjecture that this is not due to increased financial knowledge or skills, but "increased comfort with financial transactions and concepts" (450). Putting aside methodological issues, this conjecture would explain why they found increased savings across all FLE "mandate" states, whereas Tennyson and Nguyen (2001) found that students in the majority of these states had scores on tests of financial knowledge and skills no greater than students in nonmandate states.

Mandell's (2005) examination of self-reported thrift among high school students found that students who took financial classes did not improve their scores on the Jump$tart exam, but did report higher levels of thrift, and so he too conjectures, assuming students self-report accurately, a causal link between FLE and improved financial behavior unrelated to financial literacy. Courchane and Zorn (2005) come to a similar conclusion.

These academics present an intriguing possibility that it is not financial literacy, but a norm or rule-of-thumb of thrift that mediates between FLE and savings rates. Rather than providing support for the current FLE model, these results suggest an alternative model of norms training leading to changed behaviors. Financial norms education (FNE) would encounter the same challenges in developing appropriate norms that FLE faces in developing appropriate measures of good financial behavior—technical disagreement, appropriateness varying with context, and normative disagreement. But FNE would be forced to address these head on in establishing the norms to be taught. Weiner and his colleagues (2005), among others, have been developing a financial education program that explicitly seeks to change participants' attitudes or norms. These sorts of programs should be developed and examined further.

For decisions and behavior that do not require a high degree of financial literacy, financial norms training could be effective. But adopting a norm can benefit consumers only when they can determine how to apply the norm to the context at hand. Sometimes this will be easy—a norm of not investing a 401k in an employer's stock is one that most consumers, if they have access to a 401k, could follow. But knowing a rule of thumb to "diversify assets" in which that 401k is invested is not enough if a consumer does not understand the basics about how assets differ. Further, once a consumer decides how much to allocate to a mutual fund class, a norm of comparison price shopping will not be enough for her to determine which fund within the class has the lowest fees and expenses.

The demands of society and the marketplace today require more than knowledge of and desire to follow financial norms. Thus, a norms model of financial education would be effective in improving consumer welfare only if concurrent public policies simplified the decisions and actions our society and marketplace require so as to facilitate application of these norms.

The second public policy model suggested by the research above stems from the studies of credit, retirement, and homeowner counseling. It is plausible that intervention by the counselor and individualized financial advice could improve consumer financial welfare. This raises the possibility that rather than education, a better public policy response to consumer finance problems might be a national system of licensed pro bono financial advisors. Consumers would need sufficient education to select a trustworthy and qualified advisor, but would not need to perform difficult calculations, judge the value of information sources, or perform economic forecasting themselves.

The two proposals dovetail in some respects. FNE might inculcate a norm of skepticism about claims made by sellers of financial products by explaining financial scams and sellers' financial incentives to steer consumers to products that generate the most revenue for the seller. Many "consumer rights education" programs include this in their curricula already, as do some debtor education programs. A skepticism norm might be sufficient for simple financial matters, but for complex decisions the skepticism standing alone could lead to consumer fear without a way of distinguishing between scams and good financial products. In turn, skepticism about the market's offerings could lead consumers to seek out financial advisors for assistance. Provided that these advisors are trustworthy, qualified, and affordable, the best FNE might espouse a norm of relying on these advisors for help.

These policies and others should be tried and tested. But until and unless stronger evidence emerges that the current model of financial literacy education is effective, policymakers should be circumspect in their use of it as a public policy response to consumer financial problems. Researchers should be particularly cautious in the presentation of their findings, so that academic work will contribute to the public policy discussion empirical, rather than ideological, assessments of consumer financial education.

ENDNOTES

1. Although little-discussed in the FLE literature, research in behavioral economics indicates that even knowledge, skills, and well-calibrated confidence will not necessarily produce good financial decisions. As I have discussed at length elsewhere (Willis 2008), heuristics, biases, and emotional coping mechanisms that interfere with good decisionmaking are ubiquitous in personal finance. Debiasing training is not suited to the personal finance context. Policy measures designed to ameliorate consumer finance problems must address these behavioral barriers to better financial decisionmaking.

2. One on-going study has the potential to surmount these barriers to better research design. The Federal Reserve Board is conducting a multi-year study to determine the effectiveness of FLE given to members of the armed services chosen at random (Bernanke 2006; Fox, Hoffman, and Welch 2004). The military is in a unique position to require its members to participate in FLE without giving them incentives to participate that might confound results. It is likewise uniquely able to randomly assign its members to control and treatment groups, eliminating selection problems. Conditions, although not fully controlled, can be roughly controlled in that the treatment and control groups can be selected so as to represent equivalent distributions of incomes, ages, seniority, familial status, geographical location, general education level, etc. Participation in both the education and all follow-up presumably can be mandated, so as to virtually eliminate the potential for nonresponse bias.

3. For further discussion of this study, see Caskey 2006.

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