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A Literature Review on the Effectiveness of Financial Education

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This survey summarizes current research on financial literacy efforts. Because most financial literacy programs are relatively new, much of the literature reviewed here is also new and part of a field that is still developing as a program of research. However, we can conclude that financial education is necessary and that many existing approaches are effective. Among the findings are that some households make mistakes with personal finance decisions; mistakes are more common for low income and less educated households; there is a causal connection between increases in financial knowledge and financial behavior; and the benefits of financial education appear to span a number of areas including retirement planning, savings, homeownership, and credit use.

JEL Codes: D12, D14

Key Words: Financial literacy, financial education, retirement, savings, homeownership

I. Introduction¹

With the exception of homeownership and mortgage counseling, the bulk of financial literacy programs are relatively new. For example, as of 1994 more than two-thirds of employer-provided financial education programs available at the time had been added after 1990. Similarly, research commissioned by the Fannie Mae foundation found that three-quarters of consumer financial literacy programs (broadly defined) were started in the late 1990s or 2000.² As a result, much of the literature included in this survey is new and part of a field that is still developing as a program of research. The relative newness of many of these programs also means that there are not as many studies to summarize as one might think, given the number of organizations and programs currently in place.

Ultimately, we are interested in household behavior and the potential effect of financial education efforts on that behavior. In order to understand the link between household financial decisions and financial literacy, we first need an understanding of the financial mistakes households make, as well as the connection between financial knowledge and behavior. These are the topics of the first two sections, after which follows a summary of the literature surrounding financial education efforts, grouped by topic.

II. Shortcomings of Household Financial Decision-Making

In comparison to the research on corporate finance, research on household financial decisions has received scant attention in the professional literature. This disparity exists because household financial decisions incorporate some unique and complex characteristics that prevent an easy application of models from corporate finance. The uniqueness of household financial decision-making almost certainly means that what economists know about corporate finance cannot be applied in a straightforward manner to households. A formal survey of household finance by Campbell (2006) addresses some of these differences, providing an excellent starting point for this survey. Much of this section is derived from this work.

A. What do we mean by good financial decision-making?

Most would answer this question using normative analysis; that is, the answer would be prescriptive in nature, describing what households *should* do to be financially successful. It would likely include things that financial planners often advocate, such as saving enough for retirement. Before prescribing what households *should* do, however, we need to understand what households actually do, preferably through the prism of economic theory. Thus, this

¹ This survey provides a summary of the current state of knowledge regarding financial literacy efforts from published research. The Federal Reserve Bank of Richmond is reviewing its involvement with financial literacy across the Fifth District. This paper is part of the review process and was initially written for staff involved in the Fed-supported financial literacy programs. A number of non-profit organizations and other groups expressed interest in the literature review, resulting in this addition to the working paper series. I would like to thank Joan Coogan for her assistance with the bibliography.

² These figures are referenced in Bernheim and Garrett (2003) and Vitt, et al. (2000).

section covers efforts to collect detailed data on household financial decisions and evaluate them relative to existing economic models of financial behavior.

Although we have several data sets that describe the collective behavior of households (the Survey of Consumer Finance, for example), these are not the same as having detailed data about individual decisions across a large sample of households. This is unfortunate as it limits our understanding to the collective behavior of groups of households, likely masking important details about the decision-making process for individuals. Some data sources that get closer to the mark include centralized registrations of stock ownership and government records (the most detailed is in Sweden).

Even if the perfect data set existed, there is still the problem of how to model household behavior when comparing what households actually *do* to what they *should do*. Although we will not address the complexities of economic modeling here, a brief, intuitive description of some of the challenges might be useful. To begin with, several researchers have noted the complexity and uniqueness of household financial decisions. One writer put it this way:

The problem of developing an appropriate personal financial plan is extraordinarily complex. Ideally, a plan should account for earnings, earnings growth, assets, current and future rates of return, pension benefits, social security benefits, special needs (e.g., college tuition, weddings, down payments on homes), household composition, current and future tax law, mortality probabilities, disability probabilities, insurance rates, risk-return trade-offs, and a host of other factors. Under these circumstances, is it reasonable to assume that the average individual makes well-informed financial decisions?

B. Douglas Bernheim (1994, p. 55)

In addition to many of Bernheim's complexities, Campbell discusses the complexities arising from human capital as an important form of wealth. Human capital is an asset that is tied to the individual and cannot be traded, even though the asset provides a stream of labor income that is similar in some ways to, say, interest payments on a bond. However, the value of the asset is subject to unique risks that cannot be hedged away as is the case with many other forms of wealth. What is more, decisions to increase human capital by undertaking higher levels of education, for example, are subject to varying rates of return due to a number of factors, including one's expected lifespan upon completion of a degree program.

Given these complications, it is probably not surprising that household financial behavior might not fit standard models of economic behavior, though it may still be welfare maximizing. Thus, economists must either find non-standard explanations, such as is found in the field of behavioral economics, or conclude that some household behavior is sub-optimal. Or, to put it in plain language, households might sometimes make mistakes.

B. Gaps between ideal and practice

Campbell attempts to reconcile observed household financial behavior with economic theory – accounting for previously mentioned issues with measurement and modeling – but reaches the eventual conclusion that some households make mistakes. That is, some households make

financial decisions that are not welfare maximizing, but there does not appear to be an adequate explanation for why that behavior is anything but a mistake. He acknowledges that future research might reconcile seemingly poor financial decisions with economic theory, but suggests that until then, the view that households make mistakes will be a key feature of financial literacy discussions.

Campbell addresses several decisions pertaining to household finance.

Participation and Asset Allocation

Participation and asset allocation are two areas of household finance that we can examine with aggregate statistics. From the Survey of Consumer Finance, for example, we know that the share of households with equity assets has been increasing over time, from 40.4 percent in 1995 to 48.6 percent in 2004. However, for most of these households, public equity holdings represent a smaller share of their total financial and property assets (as opposed to human capital) than some other category. For example, between about the 30th and 95th percentiles of households ranked by total asset value, real estate comprises the single largest component of asset holdings. At the 50th percentile, real estate holdings represent over 60 percent of all assets owned. When we account for large mortgage liabilities (and smaller home equity margins) for many of these households, it is clear that home-ownership is a key element of wealth for the middle class. By contrast, poorer households hold primarily liquid assets and vehicles, while only the wealthiest 5 percent or so hold equity assets that are greater in value than real estate ownership.

The reason for examining equity ownership more closely is that, although ownership incurs additional risk (dampened by mutual fund diversification), even less wealthy households would benefit from holding a portion of their portfolios in stocks so long as equities receive a higher average return over time compared with other asset classes (the equity premium). Even among the more wealthy households, however, there is still a significant fraction that does not participate in equity markets at all (almost 20 percent of households at the 80th percentile), implying that either some households make investment mistakes or some underlying assumption in the economic theory does not hold.

Campbell offers several possible explanations for why some households do not own equities. One is a lack of information about the existence of equity markets. A second is the presence of fixed costs as a barrier to market entry. Campbell suggests that households with higher education levels (high school, college, graduate school) overcome these barriers. In support, he presents regression evidence showing that higher education levels are significantly related to equity ownership by households. He also presents evidence that educated Swedish households diversify their portfolios more efficiently than less educated households.

Diversification

The issue here is how households develop their portfolios within each asset class. For stock ownership, the ideal practice would be to hold many different equities, either through index mutual funds or exchange-traded funds made up of stacks from a broad equity index. Campbell presents a number of conclusions from the literature related to this point, which are summarized as follows:

1. Many households own relatively few assets.
2. There tends to be a local bias in stock ownership, both with respect to domestic vs. foreign stocks and regionally within a country.
3. Many households have large holdings in the stock of their employer.
4. Discount brokerage customers trade intensively.
5. These effects (1-4) tend to vary across households.

In an effort to address these issues, Campbell and others studied Swedish households, where data on individual stock ownership is more detailed. Using one measure of risk, they found that roughly half of the median household's portfolio risk is unsystematic (idiosyncratic). With some further analysis, Campbell finds that the median Swedish household loses no more than 1.2 percent return relative to a currency-hedged world index, equal to about \$130 per year. A related calculation estimated the total to be about one-fourth this amount.

In either case, the loss is not an especially large figure, suggesting that most households do not pay much of a price for the lack of diversification in their stock holdings. Additionally, this line of research shows that there is a wide variation in outcomes across households, essentially reinforcing conclusion number 5 from above. Taken together, these suggest that a lack of diversification leading to a significant loss of portfolio return is a problem for only a small portion of households, at least in Sweden.

Mortgage Decisions

Campbell devotes considerable effort to diagnosing household mortgage decisions, largely because of the importance and complexity involved. He gives his conclusion up front, stating that "some households appear to choose between FRMs (fixed rate mortgages) and ARMs (adjustable rate mortgages) as if they irrationally believe that long-term interest rates are mean-reverting." According to Campbell, what should matter for this decision is not the level of long-term interest rates, but the spread between short- and long-term ones. The larger this spread, the more attractive are adjustable rate mortgages.

The idea that long-term interest rates alone should not affect mortgage decisions relies on the fact that changes in long-term rates are not highly correlated with past changes of the same. Thus, past changes in long-term rates have very little predictive ability for future rate changes. That is, just because long-term rates happen to be low today does not imply that they will return to some "average" level in the future, especially the near future, since most mortgages are paid off in less than ten years.

Campbell argues that many households are slow to take advantage of beneficial mortgage refinancing opportunities. He presents data showing the distribution of mortgage spreads in four different years, where the mortgage spread is the difference between the rate on the household's current mortgage rate and the current market rate. In 2003, he shows that, in comparison to the other years, a larger share of households held mortgages that were far enough above the market rate to make refinancing financially attractive, after accounting for closing costs. The year 2003 was roughly two years into a generally declining interest rate

environment, producing what Campbell calls “sluggishness” on the part of household refinancing decisions.

In other research, Campbell looks for explanations for sluggishness in mortgage refinancing. He finds that some households are prevented from refinancing because of declining or limited equity in the house. He also finds that some households do not have characteristics that are associated with more frequent refinancing. Households that are most likely to refinance when financially attractive tend to be smaller, younger, better-educated and affluent white households who own more expensive homes. Campbell addresses the possibility that households with these characteristics may simply be less mobile and can evaluate refinancing more frequently because they do not expect to move. Further regression results show that most of the characteristics that influence refinancing activity also influence mobility in the same manner, reinforcing the notion that less-educated and minority households are more likely to miss beneficial refinancing opportunities.

Equilibrium in Retail Financial Markets

For various reasons, financial innovation in retail markets often appears to proceed slowly. The existence of financially unsophisticated households is the key to explaining this and other phenomena. To begin with, these households tend to use standard financial contracts in their country. The standard for mortgage contracts varies with each country; most in the US use fixed rate mortgages, while variable rate mortgages are more common in the UK. By contrast, financially sophisticated households are more likely to evaluate alternatives.

Campbell discusses a report by David Miles in the UK that addresses the tendency of some households to use standard contracts. Miles was asked by the Chancellor of the Exchequer (equivalent to the Treasury Secretary in the US) to explain why fixed rate mortgages were so much less popular in Britain than in the US. The report explains that adjustable-rate mortgages in the UK are often sold at heavily discounted initial rates that automatically adjust to the much higher “standard variable rate” after two years. Though borrowers have the right to refinance without penalty after two years, a large fraction never do (nearly one-third in 2003). Miles argues that these naïve households subsidize rates for more sophisticated households that do refinance and would not be interested in an unsubsidized fixed market rate.

Hidden costs are another means through which naïve households subsidize more sophisticated households. Campbell refers to one study where variations in mortgage broker fees were tied to “points” on the loan. In a sample of 2700 mortgage loans with average mortgage broker fees of nearly \$2500, the author found that a college education was associated with a \$1500 reduction in broker fees. If the market for mortgage loans is competitive, a difference of this size is strong evidence that naïve households are subsidizing more sophisticated households.

Campbell references another study showing that some households are uninformed about their own home value and mortgage liability. While some households accurately know the market value of their homes and the terms of their current mortgage, some adjustable rate borrowers do not know the extent to which their interest rate could change, especially those with below-median income. An implicit link between household income and education levels further reinforces the notion that financial education might improve knowledge for these individuals.

By his own admission, Campbell does not address all aspects of household financial decision-making, so his list of potential mistakes made by households is not complete. Below are the conclusions Campbell offers:

1. It appears that poorer and less educated households are more likely to make mistakes than wealthier and better educated households.
2. Some mistakes may result from efforts to avoid others (for example, those who invest poorly may avoid riskier asset classes).
3. The presence of households who make investment mistakes may inhibit financial innovation.

III. Connecting Knowledge to Behavior

Most of the studies in this survey examine the link between financial education efforts and personal financial behaviors. Many find a relationship, but they take it as given that there is a casual chain from knowledge to behavior. A key issue here is the difference between correlation and causation. Although there is a clear correlation between knowledge and behavior in personal finance, behavior differences may be the causal factor. For example, those who are predisposed to save more may seek out training to help them do so in an informed manner.

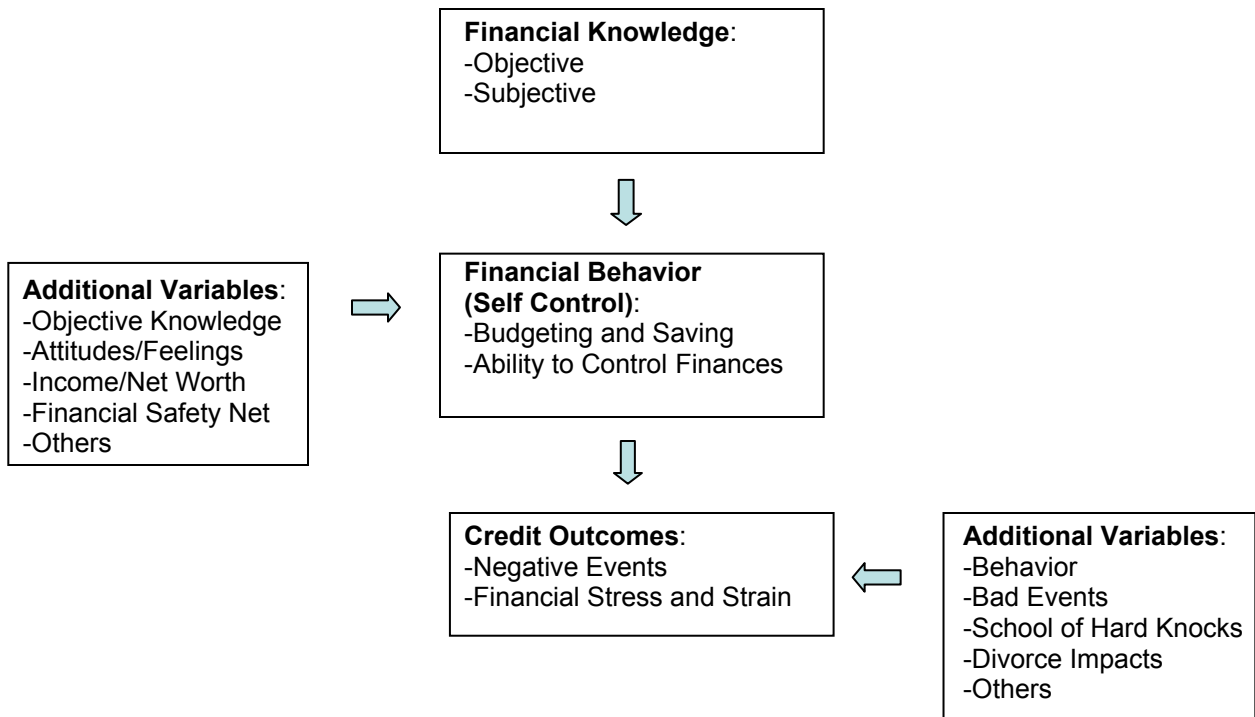
A few papers examine the link between knowledge and behavior. One of these is a Federal Reserve Bulletin article by Hilgert, Hogarth and Beverly (2003). This article looks at the link between knowledge and behavior for four categories of financial activity. These are cash-flow management, credit management, savings, and investment. For three of these areas, the authors find strong links between knowledge and behavior, using the results of a financial knowledge test in combination with questions about financial practices from the University of Michigan's Survey of Consumers.

Hilgert, Hogarth and Beverly (2003) use the Survey of Consumers results on 18 financial-management behaviors to construct a financial practice index for each of the four areas of financial activity. The three index categories are low, medium, and high, based on the percentage of financial behaviors practiced by each household. The authors then compare the indices to scores from a true/false financial knowledge test from the Survey of Consumers in a two month period. They confirm positive and significant correlations between knowledge and behavior across the range of personal finance activities.

Additionally, the authors discuss survey questions about learning experiences and effective ways to learn personal financial management skills. Among methods that respondents said were the most effective ways to learn to manage money, media and video presentations scored the highest, while informational seminars and formal courses scored the lowest (a majority of respondents indicated these would also be effective, however). Personal experience was the most frequently cited source of knowledge, with friends and family also scoring highly. Courses in high school and educational sessions either through an employer or anywhere outside of a school setting scored much lower across all financial practices and skill levels.

Although the Hilgert, Hogarth and Beverly (2003) paper provides evidence of a link between financial education and personal finance behaviors, it does not provide conclusive evidence that the financial education leads to sound personal finance decisions. A plausible alternative is that intrinsic characteristics of individuals cause them to seek financial education because they want to improve their financial results. A more recent paper by Courchane and Zorn (2005) addresses the issue with more direct results. The authors develop a recursive model with links from financial knowledge to financial behaviors to credit outcomes, as shown in the figure below. The flow of the model runs from knowledge to behavior to outcomes, with allowances for other factors potentially affecting this flow at various stages.

Model of Creditworthiness



The data Courchane and Zorn (2005) use for this study come from three sources. The first is a lengthy 12-page consumer credit survey, the second is a large collection of demographic data kept by two private marketing firms, and the third is individual credit data from Experian. Together, the three sources provide extensive (confidential) data on each individual, providing a large sample of over 12,000 respondents from which to conduct the study. The respondents were specifically selected in the age range of 20-40 years and with incomes under \$75,000 in order to represent a population for which homeownership and credit issues are important. Because of the way the authors picked the sample, it contains a large share of respondents with impaired credit.

The authors then present regression analyses that follow the line of reasoning outlined in the figure above. Overall, they find that their equations fit the model well. Knowledge is a key explanatory variable for behavior, while behavior, in turn, is a significant determinate of credit outcomes. These results alone provide significant support for the range of financial education programs that are examined in more detail in the sections that follow. In other words, this research provides strong evidence that the causal connection runs from knowledge to behavior to outcomes, as previously assumed by those involved in financial education.

Beyond the overall results, the details provide some other useful insights. In the first step – analyzing the influences on financial knowledge – the authors find that in addition to personal experience and education levels, other variables play an important role. One is “learning from bad times,” which the authors also call the school of hard knocks. Credit card use is another significant factor, with a corollary outcome that those acquiring a credit card at a younger age gain a little more financial knowledge earlier in life than those without credit cards.

In the next step, where the authors assess factors affecting behavior, knowledge is by far the most significant. A set of psychological variables is also important, with measures such as optimism, risk aversion, and ability to cope, all leading to “better” financial behaviors. The authors define “better” financial behaviors as an index from behaviors such as budgeting, saving regularly and paying bills on time.

The final step in the analysis connects behavior to credit outcomes, measured by the degree to which credit is impaired. Courchane and Zorn (2005) find that behavior is the second most important variable for explaining credit outcomes after race. That is, the variable for behavior, which was significantly affected by knowledge in the previous step, is a significant determinant of credit outcomes. This finding is important as it establishes a causal link from financial knowledge to financial behaviors and then to credit outcomes. Furthermore, the links are significant after allowing for a range of other influences that includes demographic and financial characteristics.

When considered along with Campbell’s (2006) work, the research by Courchane and Zorn (2005) provides the analytical basis for research into the effectiveness of financial literacy programs themselves. Their research establishes that, though explanations for apparent mistakes in financial decision-making by households might yet surface, current economic theory and common sense suggest that these mistakes are real. A lack of knowledge about key personal finance issues contributes to these mistakes, therefore increasing individual knowledge will yield “better” financial outcomes.

The remaining task, then, is to evaluate the effectiveness of various programs in the differing areas of personal finance, to the extent that there is available research on each of these.

IV. Financial Education Programs on Retirement and Savings

This section covers financial education programs offered by employers or presented within secondary school curriculums, as well as other general financial education topics that do not fit into these two categories but tend to cover retirement and savings topics. Overall, this segment of the literature supports both the need for and the positive impact from financial

literacy programs with the general goal of increasing savings, especially for retirement purposes.

There are a number of approaches and topics of interest within this branch of research. Many rely on survey methods that are subject to reporting error, especially in cases where individual surveys are key data sources (as opposed to employer surveys or employer records). Some of the studies address related issues of importance, including the idea that increased savings as a result of financial education represent a net increase in overall savings and not a shifting of assets. Other studies address variations in the effectiveness of financial education across methods ranging from individual counseling to printed materials and the need to tailor these efforts to individual needs. Most studies find a positive and significant relationship between financial education programs and financial outcomes.

A. Employer-based programs

The dramatic shift away from defined benefit pension programs toward defined contribution plans such as 401(k)s has been most pronounced in the last 15 years or so. Given that a large share of the existing workforce has changed from an environment where the responsibility for planning has shifted from employer to employee in a short period of time, it is perhaps not surprising that most employer-based programs for retirement planning are still relatively new. By 1994, 88% of large employers offered some sort of financial education (including both printed materials and seminars), with more than two-thirds of these programs initiated after 1990 (Bernheim and Garrett, 1996). Today, nearly all large employers and many smaller ones offer programs, but the relative newness of these efforts means that the number of useful studies investigating their effectiveness is not large, though additional studies will be published in the near future.

The earliest studies in this field were limited to either noting the correlations between financial literacy and decision-making or using case studies that inferred a connection between the two. Neither approach attempted to control for other factors that might influence behavior, such as changes in an employer's 401(k) match or increases in employee income over time that increased disposable income.

As the growing popularity of 401(k) and similar programs has increased both the interest in these plans and the need for financial planning, more recent research has introduced additional sources of data and formal econometric techniques to examine these problems. One early study by Bayer, Bernheim and Scholz (1996) makes use of a KPMG retirement benefits survey conducted in 1993 and 1994. The survey includes characteristics about the firms, their retirement plan and their 401(k) plan characteristics. The data also include details about the types of financial education offered to educate employees about these benefits.

Bayer, Bernheim and Scholz (1996) rely on the assumption that employers tend to offer training on a "remedial" basis; that is, when participation in the 401(k) plan is determined to be too low. The alternative explanation is that employers offer financial education in response to employee demand, which would make employee characteristics the driver of changes in employer-sponsored educational offerings and, eventually, savings patterns. The timing supports the authors' conclusion that retirement seminars are remedial in nature.

The regression results in this paper provide limited support for the effects of financial education on retirement savings. Specifically, the authors find that frequent seminars (based on survey responses) increase both participation rates and contribution rates for non-highly compensated individuals (defined in various ways, one of which is annual income under \$100,000). Other forms of financial education accounted for in this study were insignificant, including infrequent seminars, newsletters, and printed plan descriptions.

In a related study by Bernheim and Garrett (1996), the authors present evidence on workplace financial education by means of a survey of households. Although survey data from individuals might be considered less accurate than those from firms, the survey approach provides the authors more detailed data about other asset holdings and factors not available in the firm survey that may influence individual decisions about retirement savings. The trade-off is that the household survey lacks detail about the composition of the retirement plan itself, which was a key data element of an earlier paper with a similar focus.

The data in this study come from a survey sponsored by Merrill Lynch in 1994. The key section of the paper examines the impact of employer-provided education on four variables. Two of these are stock variables (total net worth and total value of retirement assets) and two are flow variables (total savings and savings for retirement purposes). Since the former of these represent the accumulation of savings over time, financial education would be expected to have less of an impact relative to the savings variables. Additionally, given the authors' belief that employers tend to provide financial education in remedial situations, it may even be the case that the stock variables are lower for the group that receives financial education.

The regression evidence presented generally conforms to the expected pattern. Median regressions, in which the measures for each of the four variables listed above is the dependent variable, show that employer-provided financial education is a significant and positive influence on retirement wealth, total savings and retirement savings. The effect on total wealth is positive, as expected, but not significant. These results are consistent for two different measures of financial education. The first is simply the presence of an employer program, regardless of whether the employee made use of that resource. The second variable tallies actual participation in employer financial education programs.

The authors use both of these measures – the presence of an employer program and actual participation – as a means of addressing the possibility that participation in a program is actually dependent on other factors that represent an underlying tendency toward savings. One of these could be the authors' previous assertion that employers tend to offer financial education in remedial situations. There may also be characteristics about individuals that predispose them toward savings. The use of two different explanatory variables for financial education is an imperfect means of addressing this issue, but data limitations prevent further investigation. The authors suggest that the consistency of results across the two measures indicates these biases are small. For example, in the regression the point estimate for the impact of financial education on retirement wealth is \$2488 using the availability of education and \$2176 using participation in a program. In both cases, the variable is highly significant.

Bernheim and Garrett (1996) also present results showing that workplace financial education is an important factor for the total savings rate, but not total wealth. The result is a potentially important one as it raises the possibility that financial education that promotes retirement savings may simply cause individuals to shift assets rather than increase overall savings. The authors state that the relative newness of most employer financial education efforts implies that an insufficient amount of time has passed for there to be a significant impact on overall wealth. They additionally state that their finding that financial education raises the total savings rate suggests positive spillover to other types of savings.³

Bernheim and Garrett (1996) present additional analysis that looks at outcomes at different percentiles based on income distribution. The outcomes test the robustness of the initial results and identify population segments that benefit the most from financial education efforts. The authors find that the effect of employer financial education on the total savings rate is larger and more significant for those at the 25th percentile than those at the 75th percentile. The evidence is less clear for the rate of retirement savings, with nearly identical results for both the 25th and 75th percentiles when employees participate in a financial education program.

One interesting side note is that this study also finds that employer financial education tends to displace authoritative sources of financial advice (financial planners and print media) as well as more doubtful sources (friends and family) as the primary source of information on retirement planning. The authors find that the latter tend to be displaced more frequently. Moreover, the displacement of authoritative sources is not necessarily detrimental if one assumes employers are offering equally authoritative advice.

A version of this study published in 2003 provides much of the same information as the 1996 paper, although the authors present additional results that reinforce the conclusions from the earlier paper. Specifically, the authors find that after accounting for a wide range of observable characteristics, nearly all measures of retirement accumulation (both stocks and flows) are “significantly higher on average and at the 25th and 50th percentiles when the respondent’s employer offers financial education. There is also some evidence that the increased savings represent a net increase and not simply a shifting of assets.

Interestingly, the expanded results all use the availability of financial education in the workplace as the education variable instead of actual participation in these programs. As the authors note in a discussion of this point, the employer determines the availability of workplace financial education, making financial education independent of individual predilections toward savings. The authors’ method biases their results against a finding that financial education is important, which only serves to emphasize the significant outcomes presented in the paper.

As previously mentioned, the finding that financial education improves retirement savings relies on the assumption that employers tend to introduce workplace financial education in remedial circumstances. A study by Clark and Schieber (1998) published after Bernheim and Garrett (1996) lends some support for this assumption. The study found that firms that offered

³ In related research, Poterba, Venti and Wise (1996) find that the presence of retirement savings plans such as 401(k)s increase overall savings, which would increase wealth over time.

a broader range of written communications about their 401(k) plans had better participation rates.

Using employer records from 1994 for 19 firms of various sizes, Clark and Schieber (1998) found that a firm using both “generic and specifically tailored information can increase participation rates by 36 percentage points” without changing the company’s match rate. The notion that increased communication leads to higher participation illuminates the deficiencies that can arise from incomplete information about the plan itself. The idea that financial education will also increase participation seems to be an extension of this concept, especially as a lack of personal finance skills can also be viewed as a lack of information.

TIAA-CREF sponsored a study by Clark and D’Ambrosio (2003) that also uses a survey approach. In this case, the authors gave three surveys to groups that attended a one-hour session on retirement planning. The first was given prior to the session, the second immediately after, and the third three months later. The surveys covered retirement intentions, including at what age the respondents intended to retire and what their income goals were as a percentage of their working income. The surveys also asked respondents if they contributed to retirement accounts and by how much (first survey), how they planned to change their savings habits (second survey) and if they did indeed follow up with those commitments, if there were any (third survey).

Clark and D’Ambrosio (2003) find that there was a substantial change in retirement goals in response to the one-hour sessions, but there was limited follow-up three months down the road. In some regards, this finding is not surprising, although the background characteristics of those surveyed suggest that the inertia would be even greater for a more representative sample. Because TIAA-CREF provides retirement plan services primarily for colleges and universities, the educational characteristics of the survey are heavily skewed. The percentage of those surveyed with doctoral degrees is 27 percent, for example, far above the general population. In fact, a clear majority of respondents had a graduate degree and mean household earnings across the entire group was \$102,677.

In addition to the skewed sample, another difficulty that limits the usefulness of the study is the response rate. There are over 600 responses for the first two parts of the survey, but just 110 for the third part three months later. This third portion is the most critical as it attempts to measure actual changes in behavior and not just perceptions or knowledge levels. The small response rate for the third part is even more problematic in that the authors indirectly hint that the responses to this final survey were even more skewed towards college professors than the first two surveys.

Finally, a recent paper by Hira and Loibl (2005) uses data gathered from a national sample of employees of a large insurance company to test a range of hypotheses. Chief among these is the authors’ presumption that financial education, by improving financial literacy, improves workers’ expectations about their future financial situation, resulting in increased workplace satisfaction. This result was based on employee responses such as “proud to work” and “sincere interest in my well-being” when talking about the employer.

To evaluate results on specific areas covered in educational sessions (retirement, investment, financial future, and credit use), the authors mainly rely on a self-assessment of financial literacy. The potential problem with the validity of self assessments is that after employees sit through over four hours of training, they are likely to say they learned something. Additionally, if asked, they are likely to praise the company that provided the training as a courtesy, if nothing else. Put another way, these sentiments may have value, but they do not measure changes in behavior, which limits the usefulness of the research.

B. School-based programs

A couple of studies have looked into the impact of financial education in the classroom. They are predicated on the growing number of curriculum mandates requiring high schools, in particular, to include elements of personal finance.

The first of these studies, by Bernheim, Garrett and Maki (1997), is an extension of the work by Bernheim and Garrett (2003) on employer financial education. Specifically, the authors use the same household survey sponsored by Merrill Lynch to investigate differences in savings rates and wealth due to differences in state mandates for financial education. The effort requires the authors to catalogue the curriculum requirements for all 50 states, 37 of which had something in place at the time of the study.

Because their survey covers only one year, the study has some unique challenges. The main one is that those who participated in a high school financial education program did so at varying lengths of time after the imposition of the mandate. As a result, the authors construct a variable that accounts for the number of years that have passed since the mandate was imposed and include these in any regressions. Bernheim and Garrett (2003) reason that this variable is necessary because of the time needed for schools to implement the mandate after it is imposed, including securing qualified teachers and other logistical concerns. The variable they construct accounts for the time lag between the initial legislative mandate and the presence of a fully effective school financial education program.

As with the related study using the Merrill Lynch survey, the authors examine the impact of financial education on both savings (flow) and wealth (stock) measures. They find evidence that, in states with a mandated financial education curriculum, students on average had savings rates that were 1.5 percent higher five years down the road than those students not exposed to a mandate. The effect was even larger for children of non-frugal parents, which supports the notion that financial education efforts are most effective in remedial cases.

The result of improved savings by students exposed to financial education was based on the variable used to measure the number of years that had elapsed between the imposition of a mandate and the time of the survey. Although this variable is statistically significant in their regressions, a second variable accounting for exposure to a mandate in the first place is not. The authors explain that this is an indication that systematic behavior differences did not exist prior to the introduction of curriculum mandates in some states. This pattern holds up in the statistical evidence for net worth as well. Here, however, the variable for the number of years since the mandate was imposed is not significant, but an interactive term combining exposure

to the mandate and non-frugal parents is significant, further supporting the notion that these mandates are most significant for those who did not learn to save from their parents.

In the second study, Tennyson and Nguyen (2001) specifically address the effectiveness of school mandates regarding personal finance on knowledge levels. They state up front that there are other links along the way to improved financial behaviors, but limit the scope of their study based on the information content of the survey they have at hand. Interestingly, they document just 20 states as having articulated some form of policy in the specific area of personal finance. This is nine more than the study by Bernheim, Garrett and Maki, which used a broader definition that included more general consumer education topics.

The survey data come from the Jump\$tart 1997 survey of high school seniors. The survey contained 31 multiple choice questions covering four personal finance topics and also included questions covering individual demographic and family characteristics. The authors find that financial education efforts are more likely to improve knowledge if the mandate requires the teaching of personal finance concepts within a specific course. By contrast, “generic” educational standards and testing mandates were found to be insignificantly related to personal finance knowledge.

C. Other savings and retirement related research

There are a number of related research studies that do not fall explicitly into the two groups already discussed in this section, though many cover some of the topics, either directly or tangentially.

A study by Muller (2002) using data from the 1992 Health and Retirement Survey finds that retirement education increases the probability that persons under age 40 will save a lump sum distribution from a retirement account by 27 percent. However, retirement education does not increase the likelihood that financially vulnerable groups (women, non-college grads and those with lower incomes) will save their distributions. In fact, Muller finds that these groups are significantly less likely to save a lump sum distribution if they have been exposed to retirement education.

Other research expands on Muller’s (2002) finding that different demographic groups may respond differently to financial education. Schriener and colleagues (2001) note that a successful financial literacy program (meaning one that leads to improved financial behaviors) must translate the “complex language of finance into concrete and simple terms.” Additionally, it must be tailored to the recipient, accounting for differences in education skills, cultural differences and unique market segments such as children, women and single parents, for example. The natural extension of this work is that community programs most familiar with the backgrounds of those they work with might be most suited to providing financial education that is appropriately tailored to the recipient.

A unique study by Clancy, Weiss and Schreiner (2001) focuses on the impact of financial education on the use of Individual Development Accounts (IDAs). The authors find that each additional hour of financial education in the range of 1-6 hours led to an increase in monthly deposits in an IDA account of \$1.24 and an increase of \$0.56 for each additional hour in the

range of 7-12 total hours of education. Thus, a participant with 12 hours of financial education deposited \$10.80 per month more than one with no financial education. Since IDAs are targeted to the poor, the resulting increase in savings per month is significant.

Like Muller, other researchers have taken advantage of the wealth of information contained in the Health and Retirement Survey (HRS). Lusardi (2003) uses this data set to delve more deeply into the effect of retirement seminars on savings and wealth. In this study, she uses an HRS sample of households close to retirement at the time of the survey (50-61 years old). The survey is extensive, with questions on retirement planning, financial history, and risk preferences in addition to the standard demographic characteristics. Furthermore, the respondents can be linked with their Social Security histories, allowing Lusardi to investigate the role of both Social Security and pensions in respondents' retirement plans.

Lusardi's methods also differ from other retirement research in that she accounts for differences in permanent income across households. Permanent income can be thought of as the expected average income over time for a household based on its ownership of the inputs to production (primarily as wage earners, in this case).⁴ The idea is related to the life-cycle hypothesis that states households aim to consume a steady amount that is related to this longer view of income over time. One example is the worker at peak earning years putting away money for retirement. Later, this same person retires and consumes entirely out of savings and earns little or no labor income.

In this study, Lusardi estimates permanent income for each household based on a wide range of characteristics that might explain lifetime income (e.g., education levels) and possible life events (e.g., illness). She then takes household liquid wealth (savings) or net worth as a ratio to permanent income. Lastly, she examines the importance of retirement seminars to see if these are a significant factor in explaining these ratios across households.

The author finds that retirement education increases liquid wealth (savings) by approximately 18 percent overall. Most of the impact comes from those at the bottom of the income distribution, where liquid wealth in the bottom quartile was 70 percent higher because of financial education. A similar pattern holds when the sample is broken down by educational attainment, where those with the least education see increases near 100 percent. Lusardi (2003) performs a similar analysis using the ratio of net worth to permanent income and finds significant results once again for lower income households, though the overall impact is necessarily smaller when using this measure of accumulated savings.

Finally, Lusardi performs the analysis again accounting for the presence of pension and Social Security wealth. Because of data limitations, other studies had not been able to account for these measures, which comprise a significant portion of retirement wealth for lower income households. In this case, she finds that the effect of retirement education on household wealth is still significant, but is now more even across households of varying income levels. Interestingly, the overall impact on the sample as a whole is only modestly lower at 16

⁴More precisely, the permanent income hypothesis states that an individual consumes a constant proportion of the present value of the income flow from work and wealth without altering wealth holdings.

percent than the initial result of 18 percent when pension and Social Security wealth were omitted.

V. Homeownership and Credit Counseling Programs

Most of the papers in this segment of the literature recount the history of homeownership education and counseling (HEC) and the ties this industry has to Housing and Urban Development (HUD) legislation from more than thirty years ago. Unfortunately, research on outcomes from these efforts is sparse despite the long history of these programs.⁵

The primary reasons for the dearth of studies evaluating the usefulness of HEC and other credit counseling efforts are the lack of appropriately detailed data and study design flaws. A more useful program of research has only begun to bear fruit; any conclusions drawn from the literature should be viewed as tentative until enough studies are published to add to the weight of evidence. Even with this background, a summary of the results shows that HEC and credit counseling appears to lower default rates and limit other adverse financial outcomes, especially for lower income individuals. However, by making some individuals more financially sophisticated, HEC may also make defaults more likely under circumstances that increase wealth against alternative actions.

A. Homeownership and mortgage counseling

A couple of definitional challenges have to be addressed in this segment of the literature before tackling the more typical problems associated with applied research. One of these is the difference between pre- and post-purchase counseling. In this context, pre- and post- refer to the timing of counseling for households who become homeowners and take on a mortgage commitment. Post-purchase counseling by nature tends to be crisis counseling after a household becomes delinquent. By contrast, pre-purchase counseling offers a wider range of options with increased chances for alternative outcomes.

Another definitional issue is the inherent difficulty in defining “improved” or “successful” outcomes for households, especially as foreclosure may be the preferred exit in situations where counseling is offered. Additionally, degrees of counseling intensity vary across programs so that it is not enough to simply divide mortgages into those for which counseling was received and those where there was no counseling. Consider, for example, the differences between a homeowner who received one hour of counseling in a classroom setting and one that received extensive individual counseling. The former is closer to “no counseling,” but could be included in the group noted as receiving counseling if the data are not specific.

Quercia and Wachter (1996) focus on the problem of identifying success across the wide range of counseling programs offered. The definition they provide suggests a heavy weight towards pre-mortgage counseling. In their own words, a successful program is “defined as one that assists a household with a low long-term probability of ownership in buying a home and reducing its default risk.” By their definition, high foreclosure rates are seen as an indicator of failure for pre-mortgage counseling.

⁵ See Hornburg (2004) for a good summary of this discussion and the literature published up to that point.

The authors look at the existing research at the time of their paper and suggest that it fell short of providing useful conclusions. There had been a previous body of research on counseling efforts tied to government programs funded through HUD. Unfortunately, these earlier studies had substantial flaws while newer research efforts had not yet produced many results. Effectively, their paper is the first with useful research, finally addressing many of the key data and methodology issues raised by the authors a decade ago.

A number of issues limited the effectiveness of previous research, as noted by both Quercia and Wachter and Mallach (2001). Data collection had been a key problem, as was emphasized by initial research by Pricewaterhouse. This preliminary study noted that lenders have little incentive in collecting detailed data for research on the effectiveness of counseling. Moreover, efforts to get more detailed data are complicated by the frequent sale of mortgage assets to other lenders, making it difficult to track a given mortgage over time, which is a necessary component if preventing foreclosures is a measure of success.

Second, study design presents an array of other issues, including the need for standard definitions of homeownership counseling, the need to form matching control and experiment groups (without introducing ethical issues by the direct denial of potentially helpful counseling to control group members), and the need to track results over a longer period of time, given the longevity of the mortgage decision. Together with Quercia and Wachter (1996), the Mallach (2001) paper lays out what is needed to get useful research results.

A more recent study by Hirad and Zorn (2001), published shortly after Mallach's assessment, addresses these data and methodology issues. The authors, employed at Freddie Mac, use data from that organization's Affordable Gold program. The program is designed to increase homeownership among borrowers who earn less than 100 percent of a region's median income (i.e., cost-of-living differences are accounted for). Most of those using the program go through some form of HEC. Certain individuals can opt out of counseling if they are perceived as lower risk, providing a natural control group for the study and addressing one of the key study issues raised by Mallach.

The data from the Affordable Gold program provides a large sample with good detail on the type of counseling each applicant received. The sample includes almost 40,000 mortgages that originated between 1993 and 1998, which were tracked over at least eighteen months. The sample also includes the form of HEC provided to each applicant, which is available as classroom study, home study, individual counseling and telephone counseling. This addresses a key methodological issue previously mentioned by allowing for differentiation of results by type of counseling. The sample also has data on the source of the HEC, differentiating between government agencies, lenders, mortgage issuers and non-profit organizations.

Hirad and Zorn (2001) researched three questions in the study. First, does pre-purchase counseling reduce delinquency, measured as the probability that borrowers ever become delinquent at the 90-day mark or longer? The authors find that it does, with the key metric being that borrowers, on average, have a 19 percent lower delinquency rate after counseling. The authors are quick to point out that this is the first study to find an empirical connection

between counseling and borrower behavior. This is especially relevant given the previously stated shortcomings of earlier studies.

Second, the authors ask if different types of counseling have disparate effects on the probability of delinquency. They find that borrowers receiving counseling through individual programs experienced a 34 percent reduction in delinquency rates, significantly more than the 26 percent and 21 percent reductions for classroom and home study counseling, respectively. They could not find a significant difference in outcomes for telephone counseling. The third question expands on the second by looking across counseling providers, but no significant difference is found.

The authors run a final test to see if their results show the actual impact of counseling or simply reflect unobserved characteristics not accounted for in their model. They address another issue discussed in both Mallach (2001) and Quercia and Wachter (1996) – the lack of detailed data that controls for other characteristics potentially affecting the decision to default on a mortgage. Even when the authors get into more detail, they still find that counseling itself has a significant impact. What is more, this approach more clearly shows the benefits of classroom counseling in particular, while the effectiveness on other forms of counseling becomes less certain.

In more recent years, several papers on related research have addressed counseling and mortgage loan default by low-income households. Three papers by Hartarska and Gonzalez-Vega (2002, 2005, and 2006) use the same approach and the same data source, with similar results that are summarized in the following paragraphs.⁶

The data for these studies comes from the Community Mortgage Loan Program (CML), which was organized by a bank in Ohio to fulfill Community Reinvestment Act (CRA) requirements and offer financial services to underserved communities. In the CML program's early years, counseling was neither universally available nor mandatory. In 1996, Fannie Mae became a partner in the program and from that date forward, counseling services were provided by Consumer Credit Counseling Services.

One unique aspect of this data set is that each borrower provided some initial information that was used to determine the amount of counseling provided (including the number of counseling sessions, for example). All of the counseling was pre-purchase, but it included topics such as tracking living expenses and cash flow. Only applicants who could demonstrate positive cash flow for a given interest rate and loan amount were allowed to receive mortgage loans through this program.

The modeling approach provides another unique feature, where default behavior is modeled as the exercise of a put option. Thus, default is a choice that is driven by the comparative values of the associated default and prepayment options as borrowers seek to maximize wealth. In this setup, defaulting on the mortgage loan is a purely financial decision independent of the initial decision to purchase a house. Furthermore, the decision is actually a

⁶ The 2002 study was not published as a refereed article, but forms the basis for the 2005 and 2006 articles, which are the focus of this section.

joint one, where the borrower considers the present value of the option to terminate the mortgage by giving up the house in exchange for debt termination, while also considering the value of the refinance option.

The authors find that financial education has a strong impact on all borrowing decisions, even the most complex, for no other reason than it makes borrowers more fully aware of the choices available. An example is the notion that default is always financially bad for the borrower and should be avoided at all costs. By contrast, the approach in this group of papers involves a constant reassessment of the option value of default in comparison to these other values. It is a decision that changes with personal and market values, but necessarily treats the decision to buy a house in the first place as irrelevant because it is in the past (a sunk cost).

The empirical methods used for these studies include factors previously identified as important for explaining borrowers' likelihood of default. The 2006 study uses a sample of 233 loans from the portfolio of 909 that were originated between 1992 and 1995. The 2005 paper uses 1338 loans originated between 1992 and 2000, allowing them to evaluate differences between the pre- and post-1996 periods. Although large by most statistical measures, these samples are considerably smaller than other studies, including the approximately 40,000 used in the Hira and Zorn (2001) paper.

Despite the differences in study design, the results provide a consistent story. The authors find that counseled borrowers were less likely to default than non-counseled. That is, the variable representing counseling in the regressions is significant and negative. Those who received counseling were less likely to default, after accounting for other variables that also impact the chances of defaulting. Additionally, counseled borrowers were more likely to default when it made financial sense; that is, when the default option had value but their refinance option did not.

B. Credit counseling more generally

There is small branch of study related to credit counseling more generally that is, nevertheless, similar in nature to that of HEC. In particular, the paper by Elliehausen, Lundquist and Staten (2007) looks at the impact of credit counseling on a broad range of metrics describing an individual's credit profile.

The study uses data on clients of five credit counseling agencies affiliated with the National Foundation for Credit Counseling (NFCC). The final sample consisted of more than 11,000 individuals and contained Empirica credit scores and other measures at two time periods. The first was in June 1997, with all counseling occurring in a five month period centered on this date. The second was three years later, with the deliberate aim of determining the long-term impact of financial counseling. All of the counseling in this study was one-on-one, with much of it in person, though there is no distinction between in-person and telephone counseling.

One methodology issue the authors address is the potential self-selection bias arising from the fact that counseling was voluntary for individuals in this sample. Self-selection bias might arise because those most likely to seek counseling are also those most likely to have better financial results over time, since they are actively trying to correct their past mistakes.

Without correcting for this issue, it would appear that counseling alone was the sole reason for improved financial outcomes, ignoring the initiative and desire to improve shown by some individuals in the sample. To correct for this potential problem, the authors employ an appropriate econometric technique (two-stage, instrumental variable). The first part models the decision to seek counseling, while the second part models the impact of counseling itself, given a decision to seek counseling in the first place.

Elliehausen, Lundquist and Staten (2007) find that credit counseling improves the performance of those counseled across a wide array of performance measures, including debt levels, number of accounts, and delinquency rates. However, they find very little difference in credit scores three years later for the counseled group relative to the comparison group, indicating that the decision to seek credit counseling in the first place was more important than the counseling itself. For those with low initial credit scores (the lowest quintile), the predicted change in credit scores from the model are only 0.63 percent higher than those in the comparison group. For those with the highest initial credit scores (top quintile), the counseled group's predicted credit scores are 0.80 percent lower after counseling when compared to the group not receiving counseling.

According to the authors, it makes sense for credit scores to drop for some after counseling. After all, those with good credit at the start are likely to seek counseling when they face an event that is about to suppress their credit score. In a previous version of their paper, the authors note that life events such as job loss, divorce and uninsured health problems may cause some to seek counseling before the event has impacted their credit score.

The table below contains many of the results presented in the study, including those for credit scores. The results are presented as percentage differences in predicted outcomes for the counseled and comparison groups in the later time period (three years later). The results are presented by quintiles based on initial Empirica credit scores. For example, the predicted outcome for Empirica credit scores described in the previous paragraph is presented in the first row. It shows only a 0.63 percent difference in predicted outcomes for counseled individuals in the lowest quintile relative to the comparison group.

In the table, the remaining credit measures (below the Empirica score) show a larger percentage difference in outcomes for the counseling group with regard to credit card usage and various measures of debt levels. After we account for self-selection, the largest impact from counseling appears to be related to the use of revolving debt, with the counseled group having dollar levels that are 12.37 percent lower than the comparison group. It may be the case that counseling has a larger impact on these other measures of credit performance because they can change more rapidly than credit scores, which reflect financial behavior over an extended time period. If so, it suggests that a study covering a longer time period than the three years used in this study may show a more significant result for credit scores.

Percent Difference in Predicted Changes Between Counseled and Comparison Groups					
	Initial Empirica Score Group				
	Lowest	Second	Third	Fourth	Highest
Empirica Score	0.63	0.11	-0.25	-0.78	-0.80
Bank card utilization (% of credit limit)	-5.98	-3.11	-0.99	1.56	3.92
Revolving debt (dollars)	-12.37	-10.36	-6.26	3.70	27.62
Total accounts with positive balances	-9.54	-8.34	-5.15	-2.90	0.67
Total debt (dollars)	-9.71	-7.19	-4.23	-0.17	6.81
Consumer debt (dollars)	-10.59	-7.32	-2.20	5.97	23.32

Source: Elliehausen, Lundquist and Staten (2007)⁷

VI. Conclusion

Generally, we can conclude from this literature review that there is a need for financial education and that many existing approaches are effective. More specific conclusions might be best described as tentative given the current scarcity of research in some areas. A summary of these include the following:

- 1) Some households make mistakes with personal finance decisions.
- 2) Mistakes are more common for low income and less educated households.
- 3) There is a connection between knowledge and behavior, with increases in knowledge having a positive impact on personal finance behaviors (i.e., the causality runs from knowledge to behavior).
- 4) Because low-income and less educated households tend to make more mistakes, they also tend to benefit the most from financial education. Other groups that appear to benefit disproportionately include minorities, single parents, and women.
- 5) The benefits of financial education appear to span a number of areas including retirement planning, savings, homeownership, and credit use.
- 6) Financial education programs are most effective when they are tailored to the needs of the recipient and include face-to-face time, either with a counselor or in a classroom setting.
- 7) Financial education programs that cover specific topics and teach skills are better than those covering more general subjects.
- 8) The outcomes of financial education efforts are often described as “improved” or “better” results for households, though increased financial knowledge may also result in seemingly worse outcomes, such as the increased use of mortgage default in certain circumstances.

⁷ The table data are derived from the results presented in Table 8 of Elliehausen, Lundquist and Staten (2007) and presented with permission of the authors.

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