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## Can Personal Financial Management Education Promote Asset Accumulation by the Poor?

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**Abstract:** This paper asks whether personal financial management education is an effective mechanism for helping lower-income households accumulate financial assets and improve credit histories. The paper argues that the best existing studies of the effectiveness of financial literacy initiatives suggest that such initiatives might help lower-income households build savings and improve credit records, but the results are only suggestive due to the limitations of the studies. The paper concludes that a high research priority should be to gathering more robust evidence on whether teaching personal financial management skills to lower-income households can be an effective means to improve their financial situations.

**About the Author:** John P. Caskey is a professor of economics at Swarthmore College. He received his B.A. from Harvard University and his Ph.D. from Stanford University. For over a decade, his research has focused on the use of the financial services by lower-income U.S. households and on community development financial institutions. Professor Caskey has served as a visiting scholar at the Federal Reserve Bank of Philadelphia, the Federal Reserve Bank of Kansas City, the Universidade Nova de Lisboa (Portugal), and Yale University. He has also worked as a consultant for the Filene Research Institute, the Ford Foundation, Pew Charitable Trusts, and the World Bank. In addition to his academic work, Professor Caskey serves on the Board of Directors of the Chester Community Improvement Project, a non-profit low-income housing development agency, and the Franklin Mint Federal Credit Union.

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# Can Personal Financial Management Education Promote Asset Accumulation by the Poor?

John P. Caskey

## I. Introduction

Millions of low- and moderate-income American households live from paycheck to paycheck with no, or almost no, financial savings. They are in a precarious financial situation. They have no savings to fall back on if they experience an unexpected decline in income or an unexpected expense. Furthermore, many of these households are ineligible for traditional sources of credit because they have a history of failing to meet financial obligations in a timely manner. This is not surprising. They are living with no financial margin of safety and frequently must trade off one pressing payment obligation against another.

Individuals who live from paycheck to paycheck with impaired credit histories incur a number of costs. Frequently, they obtain their payment services from check cashing outlets where they pay more for these services than individuals who are able to maintain bank accounts. If they need short term loans to bridge periodic financial crises or to meet expenditure needs, they face the embarrassment and censure associated with turning to family or friends or they have to patronize high-cost lenders or turn to equally high-cost alternatives, such as rent-to-own stores (Caskey, 2005). Finally, living with no financial margin of safety is stressful, and this in itself can take a severe toll (Caskey, 1997).

A wide variety of organizations have instituted programs to teach personal financial management (PFM) skills in an effort to help such LMI households improve their credit histories and build savings. This paper addresses the question: Is this an effective approach to the problem? In brief, the conclusion is that we don't know. Several high quality studies provide evidence that suggests that PFM education can help LMI households to build savings or improve credit histories. But, I argue, the evidence so far is only suggestive. Given the prominence of PFM educational initiatives, there is a pressing need to begin to build a stronger case that they are indeed effective.

In addition to PFM programs, there are numerous other wealth-building initiatives targeting LMI households. Many focus on making homeownership accessible to LMI households or on raising their incomes by improving job skills. Others use "individual development accounts," (IDAs) which offer dollar matches for the financial savings of lower-income households. In order to receive the match, IDAs typically require that the households leave their funds in a special deposit

account for an extended period of time and specify that withdrawals can only be used to pay for an approved set of wealth-building expenditures, such as education, a down payment on a home, or a to start a business. In many cases, these alternative approaches are combined with PFM programs. To keep this paper focused and reasonably brief, however, I examine exclusively the evidence that supports the effectiveness of PFM initiatives.

## **II. Many LMI Households Lack Savings and Have Impaired Credit Records**

Household surveys consistently find that many families have almost no financial savings. The most comprehensive survey of households' assets and liabilities is the Survey of Consumer Finances (SCF). Summary statistics from a weighted, nationally-representative sample of the 2004 SCF were recently published (Bucks et al, 2006). The data indicate that a large fraction of LMI households have almost no financial assets. Of families with incomes in the lowest quintile of the income distribution, 19.9 percent have no financial savings. This means that about 4.5 million lower-income households live with no financial savings, not even a bank account. Even among those families in the lowest quintile of the income distribution who do have financial savings, the median holding is a very modest \$1,300. If we rank families by net worth rather than income, among families in the lowest quarter of net worth, the median value of total financial assets was just \$1,000.

The SCF data also indicate that many LMI households live with heavy debt burdens and impaired credit histories. For example, 27 percent of the families in the lowest quintile of the income distribution had debt-to-income ratios over 40 percent. In contrast, only about two percent of families in the highest quintile of the income distribution had such heavy debt-to-income ratios. In addition, among families in the lowest quintile of the income distribution, 16 percent had been late sixty days or more in paying a bill during the previous year. Among families in the highest quintile of the income distribution, only about two percent reported being late sixty days or more.

The Freddie Mac Corporation (1999), a large government-sponsored housing enterprise, found similar results in its 1998 survey of 20,000 households with incomes under \$75,000. The survey focused on the households' credit histories and financial behaviors. It classified a household as having a "bad" credit record if the household reported that:

- it had been at least 90 days late on a payment in the previous two years,
- it had been 30 days late on a payment more than once in the previous two years, or if
- it had a record of bankruptcy or liens files on its property due to payment delinquencies.

By these criteria, at least 36 percent of the households with 1998 income below \$25,000 had bad credit histories. The percentage could actually be substantially higher

since about 13 percent of the survey respondents did not provide sufficient information for Freddie Mac to classify their credit history.

### **III. How Might a PFM Course Help?**

Most existing PFM courses targeted to lower-income households or to heavily indebted households are implicitly based on the premise that households fail to save because they underestimate the gains from acquiring savings or overestimate the costs associated with cutting current spending. Consider the content of typical budget-education courses.<sup>1</sup> Almost all require clients to begin by listing all of their household expenditures over the course of a month or longer. This practice forces the clients to sum their household expenditures over the course of a month in various categories, providing the clients and counselors a concise portrait of how the households allocate their incomes. Without this exercise, most people have little idea of the fraction of their incomes that go to purchases of gasoline, newspapers, outside meals, etc. They lack this information because such expenditures are made in small daily or weekly increments and it is easy to overlook their cumulative share of a monthly or annual budget. Counselors argue that the information frequently reveals to their clients expenditure cuts, such as bringing a homemade lunch to work rather than eating out, that might be relatively painless and yet result in significant cost savings over time. Moreover, counselors generally encourage clients to continue to record and analyze household expenditures so that the families will always have concise, accurate information on the allocation of their incomes.

Beyond helping clients to assemble information on their expenditure patterns, many personal financial management courses provide comparative cost information on various insurance and credit choices, telephone plans, energy saving strategies, and on shopping alternatives. Courses aimed at lower-income urban households, for example, might compare the cumulative annual cost of using a check-cashing outlet to the annual cost of a bank account or compare the cost of purchasing a television under an installment plan to the cost of acquiring the same television through a rent-to-own store. Most PFM courses discuss the role of annual percentage rates. Many such courses provide examples of the impact of compound interest on asset accumulation, and it is common for PFM courses to discuss the importance of maintaining sufficient levels of precautionary savings. Finally, many courses include information on social security benefits to help families make well-informed retirement savings plans.

In addition to providing information to help people make better informed spending and saving decisions, PFM education may also teach behavioral "tricks" that people use to help themselves adhere to a desired course of action. PFM courses commonly advocate, for example, that people should withdraw only a fixed amount

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<sup>1</sup> Katy Jacob et al (2000) provides a survey of PFM courses targeting lower-income families.

of cash at the beginning of each week to pay for recurrent minor expenditures and should abstain from using credit cards except where absolutely necessary. The courses also commonly recommend that people should immediately deposit a part of each paycheck in a savings account that is not used for transactions purposes (the "pay yourself first" rule) and use payroll deductions to fund retirement plans and other savings goals. Beyond teaching such pre-commitment tricks, personal financial management courses can include motivational counseling.

#### **IV. Do Personal Financial Management Courses Work?**

In this section, I review the five studies that are often cited as supporting the conclusion that PFM education can increase the savings of lower-income households or help them to improve credit histories. These studies have numerous strengths. But I argue that none provides reliable evidence that PFM education is an effective remedy for the lack of savings or impaired credit histories of LMI households.

Before discussing the five studies, it is useful to consider three issues that make determining the effectiveness of PFM education difficult. First, although financial-management courses share many common features, they also differ in many ways. The topics that they cover differ. They can be delivered in a variety of ways: purely via written material, through course lectures, through computer instruction, through one-on-one study and counseling sessions, etc. The clients differ. They can be high school students, adult welfare recipients, church members, mortgage applicants, etc. The implication is that any test of the effect of PFM education on subsequent behavior will only be a test of the particular course. Only further testing will determine the broader applicability of the findings.

A second issue in any evaluation of the impact of financial-management education courses is the time period over which to conduct the evaluation. The effects of the course on behavior may be very different three months after the termination of the course as compared to ten years. Ideally one would like to test for both short-term and long-term impacts, but until that is done one should be careful not to assume that the impacts are the same.

The third issue is the difficulty created by selection issues. The organization that leads a PFM educational effort may select individuals for the course who the organization believes are the most motivated to change their financial practices or have the aptitude necessary to do so. If so, any observed changes could be the result of the motivation or aptitude of the clients, not the impact of the PFM instruction. Similarly, individuals who enroll in and complete a PFM course may be individuals who are already self motivated to change their financial practices. If so, once again, any observed changes in behavior may reflect their motivation and not the course content.

The problems created by the selection issues lead me to ignore two types of studies in this paper. First, this paper ignores studies that examine whether people with better financial situations (more savings, better credit records, etc) have more knowledge about good personal financial management practices. Such studies do not demonstrate that the knowledge caused the better financial situations. It could well be that some third factor, such as motivation or general intellectual aptitude, led individuals to be financially more successful as well as to learn about good financial management practices. In other words, the correlation does not reflect causation. The second type of study that this paper ignores is the simple before-and-after study. In such studies, the researcher collects information on the financial characteristics of individuals taking a PFM course prior to the course and again sometime after completing the course. Any change in the characteristics, such as an increase in savings rates, is attributed to the course.<sup>2</sup> While this approach has been common, it has two serious problems. First, one does not know how the financial characteristics of the individuals taking the course would have changed over the same period of time even without the course. Moreover, as noted above, selection issues created by the organization running the course or by the individuals choosing to complete the course mean that the researcher using this approach cannot identify the independent impact of the PFM course.

The common strength of the five studies that I examine below is that they all make a serious effort to avoid the contamination created by selection issues. Four of the five do so by contrasting the financial characteristics of those exposed to PFM education (the “treatment group”) to the characteristics of a similar group of people who were not exposed PFM education (the “comparison group”). This approach attempts to approximate the ideal approach --- a random assignment study. In the random assignment study, one would randomly assign people to either receive PFM education or not (the “control group”). One would then track the two groups over time. The difference in their financial characteristics should reflect the impact of the PFM education since the random assignment ensures that the two groups have, on average, similar observable and unobservable characteristics. They differ only in their exposure to the treatment.

In two of the four studies below that use comparison groups, the authors acknowledge that the individuals in the comparison groups may differ from those in the treatment groups due to unobservable characteristics. This could contaminate the results since differences in the subsequent characteristics of the treatment and comparison groups could be due to the effects of PFM education or due to the differences between the individuals in the treatment and comparison groups. In these studies, the researchers attempt to isolate that part of the difference in financial characteristics that is due to PFM education and that part that is due to differences between the unobserved characteristics of the treatment and comparison groups.

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<sup>2</sup> Two examples of such studies focusing on LMI individuals are the studies by Gladys Shelton and Octavia Hill (1995) and by Julia Marlowe, Deborah Godwin, and Esther Maddux (1995).

In the other two studies that use comparison groups, the researchers argue that exposure to the treatment (PFM education) was essentially random since it depended on the company that one worked for or the state where one attended high school. If people who worked for companies that did not offer PFM education, or who attended high schools in states that did not, have relevant unobservable characteristics, such as attitudes toward risk or levels of motivation or aptitude, that are equivalent to those of individuals who were exposed to the treatment, then these quasi-experimental approaches should provide results that are nearly equivalent to an experimental random assignment study.

Below, I argue that there are good reasons to doubt the reliability of the results produced by these non-experimental studies. Before plunging into the details, however, I should note that evaluations of a wide variety of programs, in such fields as job training, education, and health care, have used comparison groups to measure the effects of the programs. Numerous researchers have investigated whether good non-experimental approaches can approximate the results produced by random assignment studies. Steven Glazermand, Dan Levy, and David Myers (2002) recently surveyed the literature produced by these studies. They identified sixteen high quality random assignment studies where researchers used comparison groups to try to replicate the results produced by the experimental methods. The authors report that in five of the studies the non-experimental methods performed reasonably well when the researchers had extensive background data on the observable differences in the trends and levels across the treatment and comparison groups prior to the treatment. In three of the studies, some of the non-experimental methods produced results that were reasonably close to the random assignment studies but others did not. In eight of the studies, the non-experimental approaches did not produce results that approximated those of the random assignment studies. In my mind, 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 PFM education.

#### *Hirad and Zorn*

Abdighani Hirad and Peter Zorn (2002) used Freddie Mac data on almost 40,000 mortgages originated under the Affordable Gold program to study the effect of pre-purchase counseling on mortgage delinquency rates. The Affordable Gold (AG) program is intended to facilitate homeownership for borrowers who earn 100 percent or less of area median income. Since 1993, Freddie Mac has stipulated that it will only purchase a mortgage from a lender under the AG program if the lender certifies that the borrower received pre-purchase homeownership counseling. Such counseling almost always includes coverage of good budgeting and financial management practices. However, Freddie Mac would exempt borrowers from this counseling requirement if (1) at least one co-borrower previously owned a home, (2) the loan-to value ratio was below 95%, or (3) the borrowers had cash reserves

exceeding two months of mortgage payments. But for reasons that Hiram and Zorn do not explain, some of the borrowers who met at least one of these criteria either decided to obtain counseling themselves or were required by their lenders to obtain counseling. In any case, about three percent of the borrowers in the data set were not required to seek pre-purchase counseling and did not do so. This subset of borrowers serves as the comparison group.

In the Hiram and Zorn study, the dependent variable is whether a mortgage was ever 90-days delinquent. The researchers initially included as explanatory variables whether the borrower received counseling, what type of counseling the borrower received (individual, classroom, home-study, or telephone instruction), and what type of organization delivered the instruction (the lender, a non-profit, a government entity, or a mortgage insurer). They also included a variety of variables in the loan files that might affect the risk of the borrower or the property. Hiram and Zorn recognized, however, that the individuals who selected, or who were required by lenders, to receive counseling of a particular type or no counseling might differ systematically from others in the data set based on a wide range of potentially relevant information: motivation, aptitude, parents who might step in to provide help in a financial crisis, etc. Hiram and Zorn can't control for such differences in the regression since these data are not in the data set. Consequently, the estimated impact of receiving counseling, of any type, could be biased. They try to address this problem by using two stage least squares. In the first stage, they use a logistic regression to estimate the probability that one will obtain counseling of a particular type or no counseling. In the second stage regression, the dependent variable is once again whether or not the mortgage was ever 90-days delinquent but the counseling variables have been replaced by the fitted probability of obtaining counseling from the first stage regression.

In theory, this approach will provide unbiased estimates as long as the explanatory variables in the first regression are not correlated with the relevant factors omitted from the second stage regression. But for the estimates in the second stage to be sufficiently precise to be useful, the variables included in the first stage must also have a reasonably strong correlation with the type of counseling obtained.

In their first stage regression, Hiram and Zorn used five variables to explain the likelihood of obtaining counseling and the type of counseling obtained: the type of lender that sold the mortgage to Freddie Mac, the borrower's age, the borrower's gender, the loan-to-value ratio, and the population of the MSA in which the property was located. Not surprisingly, these five variables did a fairly poor job of predicting whether an individual would be counseled and, if so, what type of counseling he or she might obtain. In the second stage regression, Hiram and Zorn found that the effects of 15 of 16 types of counseling were so imprecisely estimated that the researchers could not reject the hypothesis that they had no impact on delinquency rates. Only classroom counseling by lenders was statistically significant. Hiram and Zorn rejected the hypothesis that no form of counseling reduced delinquency rates.

This is a careful empirical study with much to recommend it, but the results are hardly resounding for my purposes. First, the effects of counseling are very imprecisely estimated, which is to be expected given the poor fit of the first stage model. Even in the case of classroom counseling delivered by lenders (the one statistically significant coefficient), the standard error on the estimate indicates that the positive effect on delinquency rates could be either very minor to very large. In addition, one can argue that the estimated coefficients in the second stage regression might still be biased. Hiram and Zorn assume, for example, that age and gender are correlated with obtaining counseling and counseling type, but not correlated with unobservable variables (motivation, knowledge, parental support in a crisis, etc) that that might also be associated with delinquency outcomes. This is a strong assumption. Finally, the Hiram and Zorn results apply narrowly to mortgage delinquency rates, not to the impact of PFM education on the savings or credit histories of LMI households. I conclude that the results are suggestive for my purposes, but far from conclusive.

#### *Staten, Elliehausen, and Lundquist*

Michael Staten, Gregory Elliehausen, and Christopher Lundquist (2002) studied whether individuals who sought and received credit counseling from affiliates (known as “CCCS” counseling agencies) of the National Foundation for Credit Counseling in 1997 experienced larger improvements by 2000 in their Trans Union credit scores than similar individuals who did not seek this assistance. The researchers created a comparison group by using 1997 Trans Union credit reports to find individuals who lived in the same geographic regions as the counseled “treatment” group, and who had similar credit scores and similar incomes and ages. They also verified that the members of comparison group had not obtained counseling in 1997 from a CCCS agency. Since Trans Union’s credit bureau files have almost no demographic information on borrowers, the researchers had to geocode the address of the borrowers and assume that the median age and income of the census tract block group represented the age and income of the matched files. After cleaning their data, the final sample has 5,973 individuals in the treatment group and 5,514 in comparison group.

The researchers were well aware of the potential bias that might exist in simply comparing the change in credit scores between the treatment group and the matched group. After all, it could be that individuals who were much more highly motivated to improve their financial practices sought the counseling. The researchers addressed this issue by using a two-stage least squares regression. In applying this approach, the researchers use a variety of indicators of credit status, such as total debt, debt-to-household-income ratios, number of new credit inquiries, etc, to predict the probability that an individual sought counseling in 1997. In the second stage, they used the fitted probability of being counseled as an explanatory variable for the change in credit scores. In this regression, they found a statistically

significant and positive link between the probability of being counseled and a subsequent improvement in credit score.

This study clearly suggests that PFM courses might change the behavior of LMI households with impaired credit histories, but the link is once again only suggestive. It is far from conclusive for two principal reasons. First, the study does not specifically focus on LMI households. Second, and more importantly, it is doubtful whether the explanatory variables that the researchers use in the first stage regression are uncorrelated with the unobserved factors, such as personal motivation, that might lead people to improve their credit situations. It is reasonable to think, in fact, that the indicators of financial distress that the researchers put on the right hand side of the first stage regression are positively correlated with the desire to change behavior. If so, the estimate of the positive coefficient on the probability of being counseled in the second stage regression could be seriously biased.

### *Bernheim and Garrett*

Douglas Bernheim and Daniel Garrett (2003) used data from a household survey to investigate whether exposure to employer-provided financial education focusing on retirement increased household savings. The survey, conducted by telephone in 1994, covered 2,055 nationally representative individuals between the ages of 30 and 48. The survey gathered a variety of socioeconomic data on the households and included questions on retirement education in the workplace and financial knowledge. Bernheim and Garrett contrasted the savings behavior and wealth of individuals who had worked at firms that provide financial education (the treatment group) to those of individuals who had not (the comparison group). They argue that whether an individual works for the former type of firm or the latter should be essentially random, i.e. one does not choose a workplace based on whether it provides financial education, so that the individuals in the treatment and comparison groups should have similar unobservable characteristics.

Controlling for 401(k) eligibility and a variety of other household and employer characteristics, the survey participants' reported higher rates of saving, both in general and for the purposes of retirement, when they had worked for firms that offered retirement education programs. This difference was statistically and economically significant. The median reported overall savings rate increase from about eight percent of income for households without employer-based retirement education to about 9.6 percent for households with employer-based retirement education. Somewhat paradoxically, Bernheim and Garrett found no evidence that working for an employer that offers financial education raises household wealth. When the dependent variable was the household's reported net wealth, the estimated coefficients on the availability of financial education at the workplace was negative, although not statically significant. The authors explain this result by suggesting that firms tend to introduce financial education when they believe that their workers are

not saving sufficiently. They hypothesize that the education raises savings rates but the increased savings rates will not raise depressed wealth levels for many years.

These results are certainly consistent with the notion that PFM education might raise savings rates among LMI households, but the results are far from decisive. For one, the study did not focus on LMI households. Moreover, the results depend on the strong assumption that exposure to financial education was essentially random. But this is certainly open to doubt. It could be, for example, that stable firms tend to offer financial education and that people who are most future-oriented in their thinking are attracted to stable firms. If so, exposure to financial education would be associated with higher savings rates but would not cause the higher savings rates. Moreover, if people who seek employment at stable firms are more risk adverse than average and hold conservative investment portfolios, then their higher savings may not result in higher levels of wealth. This is just one possibility among many that could account for the Bernheim and Garrett results where there would be a possible correlation between savings and exposure to financial education in the workplace without there being any causal link.

*Bernheim, Garrett and Maki (2001)*

Douglas Bernheim, Daniel Garrett, and Dean Maki (2001) use data from a household survey to study the effects of high school financial education courses on subsequent adult savings rates and wealth accumulation. The BGM study is based on a 1995 telephone survey of 2,000 nationally representative households headed by individuals between the ages of 30 and 49. The survey collected information on a wide range of socioeconomic characteristics of the individuals and asked the participants to identify the state in which they attended high school. The authors matched the identified state to a list of fourteen states that had mandated, at the time the survey participants were in high school, that all students take a course covering topics in personal finance. Most of the states with such mandates imposed them in the 1970s. About 200 people in the survey attended high schools in such states. As in the study based on financial education in the workplace, the authors argue that exposure to the financial education mandate was essentially random. No one selected the state in which to attend high school based on the presence or absence of the mandate and, the authors argue, the states that imposed the financial education requirement did not differ systematically from those that did not.

Using regression analysis to control for a variety of other relevant factors, BGM find that individuals who attended high school in one of the states with mandated financial education courses reported statistically significant higher rates of savings and higher levels of asset accumulation than those who did not. But the effect appears only for those who went to high school a few years after the mandate was initiated. The authors explain this finding by hypothesizing that the school systems took a few years to implement and perfect the mandated courses. In any case, the results suggest that self-reported savings rates were 1.5 percentage points

higher for those entering the affected high school grade five years after the imposition of the education mandate. 910 individuals provided net worth data in the survey, about ten percent of whom were exposed to the mandate. Those exposed to mandate reported net worth levels that were higher than those who were not exposed. The difference was approximately equivalent to one year's earnings.

These results suggest that PFM education can raise savings rates and wealth levels. But since the study did not focus on LMI individuals we can't be sure of their applicability to this population group. More importantly, however, the reliability of the results depends critically on the assumption that any relevant unobserved characteristics are equally distributed across the treatment and comparison groups. This assumption is open to serious doubt. Household wealth levels differ substantially across the states as do attitudes about politics, religion, and, presumably, proper financial behavior.<sup>3</sup> In fact, a footnote in the study by Staten, Elliehausen, and Lundquist (p.16) notes that there are significant differences across the states in consumers' borrowing and payment behavior. For any fourteen randomly selected states, it would not be surprising to find that reported individual wealth levels or saving rates differed from those in other states, even after controlling for the incomes, ages, and some other readily available demographic variables.<sup>4</sup> These considerations lead me to conclude that the BGM results are suggestive of a possible impact of financial education on behavior, but far from conclusive.

### *Schreiner and Sherraden*

Mark Schreiner and Michael Sherraden (forthcoming) tested the effectiveness of PFM education in the context of 14 organizations that offered individual development accounts between 1997 and 2001. These 14 organizations, located in different regions of the country, tried to encourage LMI individuals to build savings

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<sup>3</sup> The striking difference in wealth levels across the states is indicated in the raw data of the study. Individuals who attended high school in the fourteen states with the mandate reported a median net worth of \$93,000. Individuals in the comparison group reported a median net worth of \$75,250.

<sup>4</sup> In fact the fourteen states are not randomly distributed geographically. They are clustered in five contiguous groups: one stretching from Florida to North Carolina; one encompassing Texas and New Mexico; one consisting of Oregon, Idaho, and Nevada; one with Illinois and Wisconsin; and one with Pennsylvania and Delaware. When the study was circulating as a working paper, I thought that the magnitudes of the estimated results were implausible, and I asked Douglas Bernheim to provide me with the raw data so I could investigate the sensitivity of the results to other specifications. He told me that the data set was the property of Merrill Lynch and referred me to an individual in that company. That person, in turn, told me that the data were no longer available, so BGM's results cannot be duplicated or tested for sensitivity to alternative specifications by other researchers.

for home purchases, education, to start a small business, or for retirement by offering to match the dollars that the individuals put into an account for these purposes. The match rates varied across the organizations as did many of the details of their operations, but all of the organizations required individuals participating in the IDA programs to complete PFM courses. These courses differed in their exact content and structure as well as the number of hours of required instruction. One organization required six hours of instruction, one required eight, four required ten hours, three required twelve, one require sixteen, and one required 45. One organization did not provide sufficient data to be part of the study. All of the organizations, except the last one, generally enforced their requirements about hours of instruction. In the site where the requirement was not enforced, the typical IDA participant completed 9.2 hours of instruction. In the twelve other sites, 80 to 90 percent of the almost 1,000 IDA participants completed all of the required hours of instruction. Those who fell short, usually did so by only an hour or two.

Schreiner and Sherraden test for the effects of financial education on IDA savings balances by running a cross sectional regression where individual's observed savings is the dependent variable. The explanatory variables are seven characteristics of the programs (match rates, imposed caps on matches, the number of months during which a participant can make a matchable deposit, etc), 35 socioeconomic characteristics of the individuals (age, gender, education, income, etc) and the number of hours of completed PFM education. Since Schreiner and Sherraden want to allow for a nonlinear relationship between hours of education and savings, they use a spline function for the hours of education. They argue that the motivation of the individuals should not bias the estimated coefficients on the hours of education since the organizations, not the individuals, determined the number of instructional hours. The regression results indicate that each additional hour of PFM education, up to ten hours, was associated with a \$1.16 increase in monthly net IDA savings and this relationship was statistically significant. Hours of instruction beyond ten did not, however, have a statistically significant impact on saving rates.

As in the case of the other studies, Schreiner and Sherraden's results support the notion that PFM education can raise saving rates among LMI individuals, but they are far from definitive. Since only four organizations required fewer than ten hours of PFM instruction, the results must be driven almost entirely by the observed savings of the participants in these four sites. Saving rates and required hours of instruction could have been lower in these four sites for a variety of reasons. It could be, for example, that the organizations in these sites required fewer hours of PFM education because their staff could not spare the time for a more intensive effort. It could be that savings rates were lower because the staff in these sites did not use as much one-on-one moral suasion encouraging participants to save. One can imagine a variety of such explanations that could account for Schreiner and Sherraden's results. Moreover, strictly speaking, their results apply only to participants in IDA programs, not to LMI individuals who are not also offered a match for their savings.

## **V. Conclusion**

I conclude that the best empirical studies to date suggest that personal financial management education can help low-and moderate-income households to build savings and improve credit histories. But the findings are only suggestive. The studies reviewed in this paper use non-experimental approaches and must rely on strong, but unverifiable, assumptions to identify the impact of financial education. Furthermore, some of the studies reviewed in this paper address the link between financial education and mortgage delinquencies or the savings of middle-income households. Even highly reliable results from such studies would only be suggestive of a link between PFM education and the financial behavior of LMI individuals. Clearly, there is a need for high quality random assignment studies that can provide much stronger evidence for the impact that PFM education can have on the savings and credit histories of low- and moderate-income households. Given the prominence of personal financial management courses as a means of addressing the financial problems of LMI households, such studies should be a priority.

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