Journal of Economic Behavior & Organization 95 (2013) 146–158

Contents lists available at ScienceDirect

Journal of Economic Behavior & Organization

journal homepage: www.elsevier.com/locate/jebo

The impacts of mandatory financial education: Evidence from a randomized field study

J. Michael Collins*

Department of Consumer Science, University of Wisconsin-Madison, 1300 Linden Drive, Madison, WI 53706, United States

ARTICLE INFO

Article history:
Received 30 July 2010
Received in revised form 5 January 2012
Accepted 21 August 2012
Available online 23 September 2012

JEL classification:
D1
I3

Keywords:
Financial education
Asset accumulation
Financial security

ABSTRACT

Financial education is commonly assumed to affect knowledge and behavior, yet its impacts remain relatively untested. Very low-income families in a subsidized housing program were randomly assigned to a mandatory financial education program and tracked for 12 months. Financial education led to improvements in self-reported behaviors, but no measurable effects on savings or credit, except for participants in education expanding their use of credit, albeit with no evidence of problems in the study period. This study also illustrates the methodological issues that arise in social experiments with small samples, including non-compliance, attrition and self-report bias.

1. Background

The subprime mortgage meltdown and Madoff investment scandal are just two of a long string of examples of apparent failures of people to make well-informed financial choices. Surveys consistently show the extent to which consumers lack the ability to perform basic financial calculations, and this seems to be especially acute for those with low-incomes and low educational attainment (Agnew and Szykman, 2005; Bernheim, 1998; Lusardi and Mitchell, 2007). Hilgert et al. (2003) show people who report low levels of financial knowledge in surveys are less likely to report regular saving, paying their bills on time, or maintaining a budget. Similarly, Courchane et al. (2008) show low objective and subjective financial knowledge levels are correlated with poor credit behavior. Consumers' understanding of interest rates appears to be a particular area of weakness, and a major concern as policymakers attempt to regulate credit markets (Campbell, 2006; Lusardi and Tufano, 2009).

The combination of real world events and empirical research have led many observers to promote an expansion of financial education, especially for low-income and/or lesser educated populations (Kozup and Hogarth, 2008). Financial education is required for financially distressed consumers going through the bankruptcy process and for some mortgage borrowers. State-level high-school financial education mandates have also been enacted, some of which have been associated with modest improvements in financial behaviors and knowledge in past research (Bernheim and Garrett, 2003). Other programs have focused on financial education related to under saving for retirement by delivering seminars in the workplace (Duflo and Saez, 2003). Although firms often simultaneously promote retirement planning seminars and introduce new retirement savings programs, there is some evidence that workplace-based financial education promotes savings (Bernheim et al., 2001).

* Tel.: +1 608 616 0369.
E-mail address: jmcollins@wisc.edu

0167-2681/$ – see front matter
http://dx.doi.org/10.1016/j.jebo.2012.08.011

Despite the growing interest in and resources devoted to financial education initiatives, however, the effects of financial education on low-income populations or in a mandated context are relatively under-studied.

One of the limitations of existing financial education studies is simply the lack of a valid control group. Collins and O'rourke (2010) provide a review of financial literacy evaluations, finding most studies use non-random comparison groups. The problem with these studies is that highly motivated people are likely to enroll in financial education, introducing unobserved selection bias into the sample. Participants in financial education may end up being more future-oriented and patient than nonparticipants and this may in fact be more responsible for positive effects associated with financial education than the program content itself (Meier and Sprenger, 2007). The primary example of a study using a randomized design is Duflo and Saez (2003), a study focused on retirement education in the workplace. There are no studies with randomized designs focused on the population seemingly of most interest—low-income people mandated to receive education as requirement to participate in a social welfare program—a highly salient population from a public policy perspective if financial education requirements are to be replicated.

Low-income families enrolled in the Federal Housing Choice Voucher (Section 8) program receive financial assistance from the U.S. Department of Housing and Urban Development to rent housing from private landlords. Clients in the program receive vouchers based on their household income and family size. The Family Self-Sufficiency (FSS) program allows families to earn income above standard limits without losing their housing vouchers, an incentive to work and earn more income. But the FSS program also requires clients to complete a financial education course, providing a unique opportunity for a field study. The five-course sequence covers relevant basic personal finance concepts such as budgeting, credit reports and credit management, banking and financial planning delivered in classroom setting for a total of about 12 h over two months.

In 2005, the nonprofit Community Development Corporation of Long Island, New York (CDCLI) identified 181 FSS clients who needed to complete financial education by the end of 2007. In order to manage class sizes and the workload for caseworkers, the agency needed to stagger the flow of clients through its financial education classes. This created an opportunity to use random assignment of clients to educational cohorts and produced a control group without the typical selection biases. A total of 144 clients agreed to participate in the evaluation and were randomly assigned to either the treatment or control group (37 did not consent—22 left the program, six had disabilities and physically unable to take the course, nine refused to consent for the study but did take the course). Members of the control group were wait listed and were prohibited from attending classes for 12 months. CDCLI collected data at baseline and 12 months after baseline for each client, including credit reports, bank account records and self-reported surveys.

This then provides the basis for a unique field study combining administrative and survey data from a small but highly relevant population of program participants. Because the agency had a backlog of clients to move through education, a natural control group existed as one cohort completed the course and the next cohort awaited access to financial education. The results of this experiment offer insights into what role education might have on key behaviors such as credit management, savings and financial planning.

The results are consistent with clients who were offered or participated in education acquiring more debt, although this result was not robust to matching estimates. There is no evidence educated clients had problems managing their expanded use of debt, and the effects of treatment on the treated suggest an increase in credit scores. Self-reported behaviors among clients assigned to the education courses show improvements in financial planning activities such as forecasting expenses, budgeting and paying bills. Overall a relatively modest intervention with an economically distressed population does show at least modest effects on behavior.

2. Data

Data on consenting clients includes bank account balances (checking and savings accounts), credit report items (number of delinquent payments of any kind listed in the report, outstanding revolving or other debt, number of open credit cards listed, debt outstanding as a share of total credit limits, FICO score) and then questions from a self-reported survey collected at baseline and then 12 months later.

While 144 out of 181 clients consented to participate in the study, the final sample comprised of 127 clients. Seventeen of the 144 clients who initially agreed to participate were lost to attrition because they were uncooperative or were no longer in the program at follow-up. The problem of attrition is common in longitudinal evaluations, and the nature of attrition bias can be difficult to estimate. Administrative records indicate that eight of the 13 clients in the treatment group who were lost to attrition were terminated or withdrew from the program, compared to one of the four clients lost to attrition in the control group. Termination could result from noncompliance with program terms or because the client's income increased beyond program limits. One possibility is that assignment to financial education may have influenced clients to withdraw from the program. Alternatively, remaining in the program at follow-up could also signal higher levels of motivation relative to clients who were non-compliant, who withdrew, or who were terminated. Thus, the direction of attrition bias is unclear, and the estimates derived from the simple randomized comparisons require further analysis.

In addition to clients who are not observed at follow up, another challenge is variation in treatment. Among those who were assigned to treatment—the so-called intent to treat (ITT)—only 58 percent completed all five financial education sessions, as shown in Table 1. Most non-compliant clients missed one or two sessions, but a few completed just one session. The subset of clients who were assigned to education classes and completed them—the treatment on treated (TOT) represented

Table 1
Summary statistics treatment and control at baseline.

	FullSample			
	Control mean	SD	Treated mean	SD
Savings	718.94	4766.64	627.51	2249.86
Debt	12679.20	12840.43	13226.66	26107.13
No. of cards	2.23	3.98	1.82	3.79
Share limit	77.48	46.13	76.37	38.50
No. of dels	0.31	0.67	0.14	0.42
FICO Score	566.11	69.84	577.38	69.83
Net Wealth	−11909.30	13247.05	−12599.15	25916.07
No FICO	0.81	0.40	0.87	0.34
FICO lt 680	0.91	0.29	0.96	0.20
Tenant rent	358.90	246.96	427.07	294.53
Child support	0.82	0.39	0.78	0.42
Hshld size	3.94	1.87	3.92	1.88
Total rent	1648.20	315.63	1532.05	254.08
Log income	8.60	3.14	9.59	1.77
Years in FSS	3.64	1.50	3.71	1.46
White	0.27	0.45	0.32	0.47
Male	0.07	0.26	0.04	0.20
High school	0.30	0.46	0.37	0.49
Some college	0.49	0.50	0.47	0.50
Fin. knowledge	1.76	1.06	1.79	1.32
Age	39.06	7.17	39.30	7.82
Age2	1576.10	587.48	1604.95	632.09
Smoker	0.38	0.49	0.32	0.47
TOT	0.00	0.00	0.58	0.50
Attrit	0.06	0.23	0.18	0.39
Observations	144			

a select group of clients. Meanwhile 6 percent of control clients were lost by follow-up, and 18 percent of treatment assigned clients were lost to attrition.

Table 1 displays descriptive statistics for the 144 clients at baseline for treatment and control groups. The overall statistics are telling regarding the economic condition of people in this program. Savings levels averaged \$719 and \$628 for the control and treatment-assigned clients respectively, and \$12,679 and \$13,227 in outstanding debt. Net wealth is therefore solidly negative, averaging \$−11,909 for controls and \$−12,599 for those assigned to treatment. On average clients had about 2 credit cards listed in their credit report and borrowed 77 percent of available credit limits. The mean number of delinquent payments listed in the report was 0.31 for the controls and 0.14 for those assigned to the education course. FICO credit scores were 566 and 577, respectively, both below the 680 cutoff point for subprime credit. These statistics paint a picture of people with little savings, high levels of debt and poor credit histories.

Table 1 also describes characteristics related to participation in subsidized housing. The average control client paid \$359 in rent, and \$427 for those assigned to treatment. Eighty-two percent (controls) and 79 percent (treatment) of clients in each group received child support payments, reflected also in an average family size of 3.9, suggesting multiple children per household. Total contract rent ranged from \$1648 for controls to \$1532 for treatments, meaning the difference in rent paid and total rent resulted in a rental voucher worth \$1100–\$1300. Mean income at baseline was \$19,938, which is defined as very low-income for this geographic area. Less than one-third of clients were of a white race, more than 90 percent were female and nearly half had at least some college education. The average age was 39. The average time clients were in the program was 3.6–3.7 years. All this suggests that the clients in this study had a history with the program had substantial economic gains from continued participation.

Clients self-rated how much they knew about five topics: interest rates, credit ratings, financial management, investing, and credit reports with responses ranging from “nothing” (0) to “a lot” (4). The questions were combined to create a general self-assessed knowledge indicator (Cronbach’s $\alpha = .82$, see Table 15 for survey wording). Respondents also recorded self-reported competencies in personal finance such as budgeting, saving and financial planning, with scores on each question ranging from “poor” (0) to “excellent” (4). Self-reported financial knowledge scores were relatively low at 1.79 on a 0–4 point scale. Notably, about a third of clients were smokers—a potential proxy for self-control problems and/or high discount rates reflecting a present bias.

The drop in the number of study participants due to attrition and non-compliance is a concern. Table 2 shows probit regressions of attrition, treatment assignment (ITT) and completion of education (TOT). Attrition and TOT (columns 1 and 3, respectively) appear to show little systematic relationship to client characteristics. Treatment assignment (column 2)—the one mechanism that is unrelated to voluntary selection—shows significant relationships to total rent and log income. This is either a failure of random assignment or the result of chance. Regardless, a simple comparison of treatment and control groups may not be sufficient to produce causal inferences of effects.

Table 2
Attrition, treatment assignment and treatment-on-treated (probit) probabilities by baseline characteristics.

	(1) Attrit b/se	(2) Treatment b/se	(3) TOT b/se
Main			
Tenant rent	−0.0008	0.0004	0.0007
	(0.001)	(0.000)	(0.000)
Child support	0.6419	−0.1168	−0.0885
	(0.538)	(0.299)	(0.311)
Hshld size	0.0515	0.0977	0.0384
	(0.107)	(0.078)	(0.078)
Total rent	−0.0006	−0.0014**	−0.0008
	(0.001)	(0.000)	(0.001)
Log income	0.0159	0.1099+	0.1040
	(0.069)	(0.058)	(0.072)
Years in FSS	−0.0099	0.0793	−0.0172
	(0.119)	(0.083)	(0.085)
White	0.0337	0.1907	−0.1341
	(0.361)	(0.262)	(0.284)
Male	0.3730	−0.6346	0.2049
	(0.554)	(0.506)	(0.540)
High school	−0.0302	0.2446	0.1595
	(0.406)	(0.326)	(0.364)
Some college	−0.2383	0.0038	0.2654
	(0.420)	(0.314)	(0.349)
Fin. knowledge	0.1082	0.0336	0.0633
	(0.126)	(0.096)	(0.102)
Age	−0.0467	−0.0133	0.2374
	(0.160)	(0.123)	(0.150)
Age2	0.0010	0.0001	−0.0030
	(0.002)	(0.002)	(0.002)
Smoker	0.1534	−0.1182	−0.2629
	(0.331)	(0.243)	(0.266)
Constant	−0.8823	0.6839	−5.3532+
	(3.307)	(2.474)	(2.984)
N	144	144	144

Statistical significance: $+p < .10$, $^{*}\ p < .05$, $^{**}p < .01$.

3. Empirical methods

The average effects of financial education on the treatment group are estimated using a difference-in-differences experimental estimator with baseline controls:

$$\Delta(Y_i) = \alpha_i + \beta_1\tau_i + \boldsymbol{\beta_2 H}_i + \boldsymbol{\beta_3\gamma}_i + \beta_4\kappa_i + \beta_5\delta_i + \epsilon_i \tag{1}$$

where the effect of treatment (τ) for person *i* is indicated by a 1 if assigned to the education course, and a 0 if not. This model tests whether the treatment, (τ), affects the outcome ΔY. Using difference-in-differences means the measured outcomes are relative to clients' status at baseline.

All clients in this study received rental housing vouchers and participated in the FSS program. However, the benefit clients received varied. **H** is a vector of factors that are related to program participation and generosity, including the client's rental payment as well as the total rent for the client's housing unit (the difference between these two variables is the subsidy from the voucher). Also included in **H** is the amount of any child support received, which measures the availability of resources outside the household. Another programmatic factor in **H** is household size, which is included as both a measure of the economic burden facing the family as well as a component of what is used to calculate rent subsidy amounts. Log income is also included as an indicator of total resources available and a factor in determining the voucher amount. The longer clients are in the FSS program, the more opportunities they have had to earn additional income without losing their voucher benefits. Therefore, **H** includes the length of their participation in the FSS program.

Race, gender and education are included in Eq. (1) as γ to capture potential demographic differences. This vector includes an indicator for the client being of white race and another indicator variable for being a male. Education is included as two indicator variables, with high school and then some college as the highest level achieved by the client, leaving less than high school for the constant. Finally age and a squared term are included to capture age cohort differences which may explain differing effects of financial education. Next, κ is a compilation of self-assessed financial competencies as a control for general financial knowledge at baseline. Finally δ is an indicator of clients who self-report smoking tobacco or related products as a proxy for time preference and self-control. Prior studies suggest that smokers are less likely to be financial planners versus engaging in impulsive financial behavior (Khwaja et al., 2007). These are all designed as controls of baseline characteristics

Table 3
Propensity score match balancing.

Variable	Sample	Mean Treated	Control	%bias	%reduct Bias	t	t-Test p
Tenant rent	Unmatched	452.54	358.3	34.4		1.94	0.055
	Matched	445.13	444.67	0.2	99.5	0.01	0.993
Child support	Unmatched	.72881	.8209	−22		−1.24	0.218
	Matched	.75472	.74844	1.5	93.2	0.07	0.941
HH size	Unmatched	3.9153	3.9851	−3.7		−0.21	0.836
	Matched	4	3.8986	5.4	−45.2	0.26	0.792
Total rent	Unmatched	1528.9	1660.3	−45.8		−2.55	0.012
	Matched	1570.6	1530.6	13.9	69.6	0.68	0.501
Log Inc.	Unmatched	9.7355	8.539	47.8		2.62	0.010
	Matched	9.6897	9.5512	5.5	88.4	0.42	0.673
Yrs FSS	Unmatched	3.7672	3.5987	11.3		0.63	0.528
	Matched	3.6288	3.3128	21.2	−87.6	1.11	0.270
White	Unmatched	.32203	.25373	15		0.84	0.401
	Matched	.30189	.23512	14.7	2.2	0.77	0.443
Male	Unmatched	.0339	.0597	−12.1		−0.67	0.501
	Matched	.03774	.0357	1	92.1	0.06	0.956
Education	Unmatched	2.678	2.4478	21.1		1.18	0.239
	Matched	2.6038	2.6584	−5	76.3	−0.26	0.795
SR knowledge	Unmatched	1.7792	1.7284	4.2		0.24	0.812
	Matched	1.6976	1.728	−2.5	40.2	−0.13	0.896
Age	Unmatched	38.78	38.463	4.4		0.25	0.806
	Matched	37.774	37.251	7.2	−64.9	0.36	0.719

and included in all models in part to address the differences between treatment and control groups in the final sample. These controls are not of primary interest and not displayed in tables for parsimony but generally perform as would be predicted. Income and rent—the variables that showed significant differences between treatment and control groups at baseline—are consistently statistically significant.

The specification above is essentially a difference-in-differences with controls for observable baseline characteristics, with the coefficient on τ offering an estimate of the effect of being assigned to treatment. This estimate of treatment (β_1) is often described as intent to treat (ITT). ITT is ideal in a randomized setting as it shows the average treatment effect across those assigned to treatment. τ is exogenous to the outcomes of interest, ΔY. The differences between clients who comply with treatment and those who defy treatment do not bias these estimates. But implicit in this approach is that the effects of treatment on compliers are large enough to overcome the (non) effects on the 'defier' clients. As the participation rate for 'complier' clients declines the effects of τ are muted.

Another approach is to estimate β_1 not as ITT but as treatment on treated (TOT). In the latter case (τ) represents 1 if the client was assigned to treatment *and completed* all five financial education sessions and 0 if assigned to treatment and did not complete five sessions, *or* the client was assigned to the control group. These models therefore compare educated clients to the non-educated or partially educated clients. Because client choices to participate in the course and cooperate in attending all five courses are likely to be correlated with the behavioral outcomes studied, these models could have bias—that is τ is no longer exogenous. Table 2 column 3 does not show bias in TOT by observed client characteristics, but this does not rule out unobserved bias. Because most financial education programs are in fact voluntary, the TOT estimates are relevant for policy and practice, even though the mechanisms for the effects estimated will remain unclear. If another financial education program is offered in a similar manner and client population, the TOT effects suggest the predicted effects for those who take part.

3.1. Matching estimator

Both the ITT and TOT estimates include controls for program participation, demographics, knowledge levels and time preferences. As discussed above, there remains concern that the coefficient τ is not exogenous and may be related to ΔY in observable ways. Another approach is to estimate the probability of each client being in the ITT or TOT group, $\Pr(\tau)$. As an additional robustness check the estimated value of $\Pr(\tau)$ can be used as a propensity score in order to match treated and control group clients based on relative probabilities of treatment. This more precisely addresses the imbalances between treatment and control groups observed in Table 1. The propensity score is the predicted probability that a client in the control group would have been assigned to the treatment group given pretreatment characteristics (Rosenbaum and Rubin, 1983, 1985). The score is calculated using Epanechnikov kernel matching using the same controls as described in Eq. (1). Alternative matching routines such as nearest neighbor matching, caliper matching with varying magnitude caliper sizes and local linear regression matching all performed similarly with no major differences in coefficients or statistical significance. The Epanechnikov kernel matching approach has some support in prior studies as providing a rigorous estimator (Galdo et al., 2008). The results of the probit model to predict ITT and TOT are shown in Table 2 column 2. Table 3 displays the pre- and post-matching balancing tests, showing income and rent in particular are better matched with the propensity

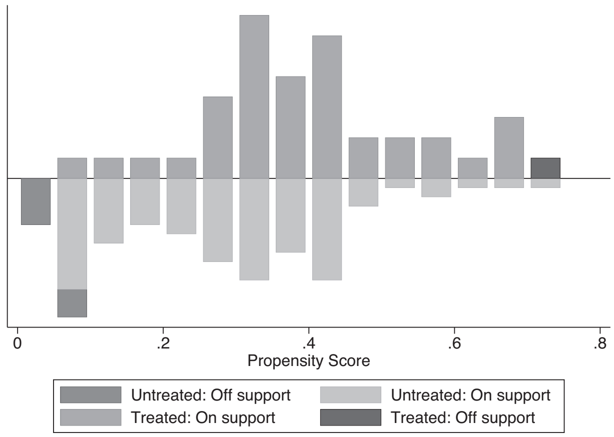

Fig. 1. Propensity score common support.

score model. Fig. 1 graphically displays the imposition of common support for treatment and control groups for the ITT estimates. Here it is apparent a small number of treatment and control clients are outliers and may skew the distribution in the naive estimates shown in Eq. (1). In order to address this problem common support is imposed, limiting the sample to a maximum of 116 observations, and further limiting the power of the data, but eliminating a small number of clients with baseline characteristics that might drive the results otherwise (Crump et al., 2009). All propensity score models use a probit for the propensity score and ordinary least squares for the estimated coefficients. Bootstrapped standard errors are not used based on some concerns in prior studies (Imbens and Abadie, 2008).

3.2. Dependent measures

The net result of these varied approaches are four estimates: (1) an ITT model with controls, (2) a TOT model with controls, (3) a propensity score matched ITT model, and (4) TOT with propensity score matching. ITT offers the average treatment effect regardless of take-up of education and is in theory least prone to selection biases. Adding propensity scores better balances the treatment and control groups due to observable differences between treatment and control groups at baseline. TOT estimates offer a more direct measure of the effect of education among those who take part—a biased estimate but one of interest for practitioners.

ΔY estimates are divided into two groups, the first being bank accounts/credit report measures, and the second being self-reported behaviors. The main bank account measure is total savings, which combines all savings and transactional accounts (these data are collected as part of the means tests required for Section 8 voucher clients). The remaining measures are as reported in credit bureau records. Debt is the total of all outstanding debt, including revolving debt, education loans, car loans, installment loans and other outstanding liabilities (by definition all clients are renters and have no mortgage debt). Number of credit cards is also pulled from credit records, and includes cards with revolving credit features including credit cards from retails stores as well as bank cards. Delinquencies are measured as the total number of late payments listed, generally this includes only payments that are 30 days or more late on any regularly scheduled payment with a due date. Fair Issacs and Company credit scores, universally called FICO scores, are also included, ranging from 350 to about 800, where higher scores suggest lower observable credit risks. Three additional dependent measures are derived from other variables, including net worth (savings-debt), an indicator of having no credit history and an indicator of having a FICO score under 680, a common cutoff for subprime/poor credit. Bank account and debt measures are estimated using a tobit specification with a left censoring of zero, while FICO scores are estimated within the bounds of scores (350–800). Net wealth is estimated using OLS. Dichotomous variables such as the no FICO score indicator and the subprime indicator are estimated using probits.

Arguably bank account and credit report data are superior in terms of measuring actual behavior in the financial marketplace. But these administrative data may be prone to lags, for example a credit report may not immediately reflect changes in payment behavior. Like a student's grade point average (GPA) a delinquency or large debt in prior periods may require an extended period of 'good' performance to shift a FICO score. Similarly, given low and constrained incomes, savings and debt may reflect client's limited capacity for changes. Because all cooperating clients completed survey questions at baseline and follow-up, this analysis also examines five self-reported behaviors that are salient to the education offered. Behaviors include controlling spending, paying bills on time, engaging in financial planning, saving for the future and using a budget. Each behavior was graded by the client on a four point scale (1 = poor, 2 = fair, 3 = good, 4 = excellent) at baseline and follow-up. Because these behaviors are measured as categorical variables, each is estimated using an ordered probit specification with four cutoffs. Like all specifications presented in this analysis, 'robust' standard error correction routines are employed to address the potential for heteroskedastic error terms.

Table 4
Estimated effects of treatment assignment on change in account/credit report.

	(1) Savings b/se	(2) Debt b/se	(3) No. of cards b/se	(4) Share limit b/se	(5) No. of dels b/se	(6) FICO b/se	(7) Net b/se	(8) No FICO b/se	(9) Subprime b/se
Treatment	−353.9626	4596.1301*	0.6054	4.8039	−0.4487	−2.2546	−4361.0005*	0.0218	−0.0527
	(363.632)	(1966.049)	(0.612)	(6.094)	(0.728)	(11.545)	(1906.908)	(0.042)	(0.050)
Constant	790.8689	−13496.0018	−4.3769	−12.0798	−31.4102**	525.3061**	25133.0674	0.6701+	−0.5431
	(3505.964)	(26372.502)	(8.175)	(56.763)	(11.724)	(137.430)	(26924.995)	(0.357)	(0.569)
Sigma	1755.3087**	10243.5246**	2.6278**	31.1622**	2.6121**	52.2609**			
	(300.084)	(1306.729)	(0.298)	(2.694)	(0.447)	(3.943)			
N	125	126	126	126	126	103	125	126	103

Col 1–6 Tobit; Col 7–9 OLS. Controls include tenant rent, total rent, child support, HH size, Ln income, years in FSS, white, male, high school, college, mean knowledge, age, age2, smoker.
Statistical significance: +$p > .10$. *$p > .05$. **$p > .01$.

3.3. Two-stage estimates

As an additional robustness test, two key dependent variables, savings and debt amounts, are re-estimated using a two-stage model. Debt is zero for a small portion of clients and savings is zero for the majority of clients. Although the tobit model corrects for a left truncated distribution there may be important differences between clients who report zero and non-zero values for debt and/or savings. A two-stage model is used to first estimate the probability of reporting a non-zero value in the first stage, and then corrected conditional estimates at the second stage for savings and debt.

3.4. Assessing the validity of self-reported financial behavior

Self-reported behavioral measures are problematic in that respondents may exhibit systematic biases in their reporting. Clients may feel it is socially desirable to provide certain answers, or provide the answer they think the agency administering the housing voucher program prefers. Clients may also not know the extent that certain behaviors are desirable until they acquire financial knowledge. Yet, self-reported outcomes are often used in research on financial capability and literacy due to the potential problems (and cost) of collecting bank account and credit report data. This study offers both self-report and administrative data, collected simultaneously (see Table 15 for the verbatim questions). While self-reports are not reported as a continuous variable, directionally each should predict the magnitude of savings, debt and credit score. An additional analysis is therefore conducted using these data to take advantage of the opportunity to test the validity of self-reported values relative to actual savings, debt and FICO scores reported in financial records:

$$Y_{i,t} = \alpha + \sigma_{i,t} + \beta_1\tau_i + \beta_2\boldsymbol{X}_{i,t} + \beta_3\boldsymbol{L}_{i,t} + \beta_4\kappa_{i,t} + \epsilon_{i,t} \quad (2)$$

where Y is the actual value of savings, debt and FICO score at time t for person i and σ is the self reported value. Also included are a matrix of controls **X** which includes race, gender, age and age squared. Another set of controls, **L**, includes indicators for high school and some college as education level controls (less than high school as constant). $\kappa_{i,t}$ is the self-reported knowledge measure used in Eq. (1) and $\epsilon_{i,t}$ is the error term. The term τ_i is also included since treatment assignment may itself alter the validity of client self-reports—which would be important to observe before drawing conclusions from causal estimates. Eq. (2) is also estimated with fixed effects at the person level (i) to control for unobserved time-invariant factors (and therefore dropping all coefficients at level i). Both models are run with a long (stacked) dataset with two observations per individual, and clustered standard errors at level i using standard OLS panel regression techniques.

4. Findings

Table 4 displays the results concerning clients' bank account and credit report data. The estimated effect of the financial education program on savings account balance, number of credit cards, utilization ratio or 'share limit' (amount borrowed / amount available), number of delinquencies, overall FICO score, having no credit report and having a subprime credit score was not significant. Two outcomes were significant: holding *more* debt and a related measure, lowered net worth. Clients in the treatment group appear to have significantly more debt listed on their credit reports at follow-up using the ITT estimate with controls. Table 5 estimates the same outcomes using propensity score matching. Here none of the estimates are significant. The former results are suggestive that the education may have led some clients to expand their use of credit, possibly to the extent that the education increase awareness of how to seek out and manage loans. The latter results are not statistically significant but are directionally consistent with that finding. Given the tight economic positions of the individuals in this study, knowing how to expand the use of the liquidity available through credit markets may in fact be an appropriate response. For example, financial education may have taught clients how to finance the purchase of an automobile, which could in turn open up opportunities for work or schooling, or simply offer a more efficient use of household time relative to public transit options. There do not seem to be more delinquencies, higher utilization of credit limits or lower credit scores.

Table 5
Estimated treatment assignment effects using propensity scores.

	(1) Savings b/se	(2) Debt b/se	(3) No. of cards b/se	(4) Share limit b/se	(5) No. of dels b/se	(6) FICO b/se	(7) Net b/se	(8) No FICO b/se	(9) Subprime b/se
PS: Treatment	−286.8598	2029.7412	0.3954	−4.4404	−0.1847	9.2543	−2625.4386	0.0367	−0.0669
	(845.150)	(2625.791)	(0.740)	(6.786)	(0.148)	(13.740)	(2615.337)	(0.051)	(0.056)
Constant	1057.1818+	11966.7164**	1.7910**	76.6269**	0.3881**	576.0926**	−10600.6970**	0.8955**	0.9444**
	(580.637)	(1796.805)	(0.506)	(4.644)	(0.101)	(9.477)	(1796.796)	(0.035)	(0.039)

Propensity Score Estimates for Avg Treatment Effect matched on controls include tenant rent, total rent, child support, HH size, Ln income, years in FSS, white, male, high school, college, mean knowledge, age, age2, smoker.
Statistical significance: +$p > .10$. *$p > .05$. **$p > .01$.

Table 6
Estimated treatment effects on credit report outcomes using propensity scores.

	(1) Savings b/se	(2) Debt b/se	(3) No. of cards b/se	(4) Share limit b/se	(5) No. of dels b/se	(6) FICO b/se	(7) Net b/se	(8) No FICO b/se	(9) Subprime b/se
TOT	399.3296	4331.0892*	1.0607+	4.4020	0.6303	15.0174	−3954.4933+	0.0198	−0.0929
	(434.058)	(2175.248)	(0.591)	(6.913)	(0.775)	(11.269)	(2181.967)	(0.050)	(0.065)
Constant	1125.9767	−3344.3821	−2.4125	−2.3495	−31.1550**	546.3589**	15359.1625	0.7140*	−0.6952
	(3580.967)	(26058.017)	(7.999)	(56.489)	(11.702)	(130.246)	(27198.616)	(0.343)	(0.563)
sigma	1746.5287**	10261.5867**	2.6211**	31.1911**	2.6150**	51.8489**			
	(291.220)	(1329.941)	(0.302)	(2.696)	(0.441)	(4.016)			
N	125	126	126	126	126	103	125	126	103

Col 1–6 Tobit; Col 7–9 OLS. Propensity Score Estimates for Avg Treatment Effect.
Matched on controls include tenant rent, total rent, child support, HH size, Ln income, years in FSS, white, male, high school, college, mean knowledge, age, age2, smoker.
Statistical significance: +$p > .10$. *$p > .05$. **$p > .01$.

Table 7
Estimated treatment on treated effects using propensity scores.

	(1) Savings b/se	(2) Debt b/se	(3) No. of cards b/se	(4) Share limit b/se	(5) No. of dels b/se	(6) FICO b/se	(7) Net b/se	(8) No FICO b/se	(9) Subprime b/se
PS: Treatment	−74.3357	4281.7763	0.7029	−5.8435	−0.0283	26.9811+	−4597.4860	0.0150	−0.0999+
	(911.049)	(2814.887)	(0.797)	(7.319)	(0.161)	(14.616)	(2799.010)	(0.055)	(0.060)
Constant	944.9767+	11591.8391**	1.7586**	76.3563**	0.3103**	572.1127**	−10405.4884**	0.9080**	0.9437**
	(508.884)	(1566.059)	(0.443)	(4.072)	(0.090)	(8.147)	(1563.442)	(0.030)	(0.033)

Propensity Score Estimates for Avg Treatment Effect matched on controls include tenant rent, total rent, child support, HH size, Ln income, years in FSS, white, male, high school, college, mean knowledge, age, age2, smoker.
Statistical significance: +$p > .10$. *$p > .05$. **$p > .01$.

However, how well these clients manage significant expansions in debt service commitments is only observed 12 months after baseline. It is possible a longer study period could reveal problems making timely payments.

Table 6 estimates TOT effects for clients completing all five financial education classes. Higher levels of debt and lower net worth continue to be significant. Treated clients also show an increase in the number of credit cards held, but not problems with payments. Using propensity scores in Table 7 the TOT effects are non-significant, except FICO score and subprime are now statistically significant (at the 10 percent level). These results suggest clients who took part in education had improvements in credit scores of nearly 27 points and on average a 10 percent lower likelihood of having a subprime

Table 8
Estimated effects of treatment assignment on self reported behavior.

	(1) Control spend b/se	(2) Pay bills b/se	(3) Planning b/se	(4) Saving b/se	(5) Budgeting b/se
Treatment	0.4826*	0.7674**	0.7045**	0.2994	0.5406*
	(0.208)	(0.220)	(0.210)	(0.209)	(0.226)
N	127	127	127	126	126

Ordered Probit (4 cuts: poor, fair, good excellent).
Controls include tenant rent, total rent, child support, HH size, Ln income, years in FSS, white, male, high school, college, mean knowledge, age, age2, smoker.
Statistical significance: +$p > .10$. *$p > .05$. **$p > .01$.

Table 9
Estimated treatment effects on self-reported behavior using propensity scores.

	(1) Control spend b/se	(2) Pay bills b/se	(3) Planning b/se	(4) Saving b/se	(5) Budgeting b/se
PS: Treatment	0.3378+	0.5853**	0.8326**	0.1712	0.3061
	(0.190)	(0.218)	(0.212)	(0.185)	(0.211)
Constant	1.8955**	1.7313**	0.8507**	0.7121**	1.5606**
	(0.131)	(0.150)	(0.146)	(0.128)	(0.145)

Propensity Score Estimates for Avg Treatment Effect matched on controls include tenant rent, total rent, child support, HH size, Ln income, years in FSS, white, male, high school, college, mean knowledge, age, age2, smoker.
Statistical significance: $+p > .10$. $^{*}p > .05$. $^{**}p > .01$.

Table 10
Estimated effects: treatment on treated.

	(1) Control spend b/se	(2) Pay bills b/se	(3) Planning b/se	(4) Saving b/se	(5) Budgeting b/se
TOT	0.3242	0.7035**	0.7120**	0.5351*	0.5586+
	(0.216)	(0.216)	(0.231)	(0.218)	(0.226)
N	127	127	127	126	126

Ordered Probit (4 cuts). Controls include tenant rent, total rent, child support, HH size, Ln income, years in FSS, white, male, high school, college, mean knowledge, age, age2, smoker.
+ $p > .10$.
* $p > .05$.
** $p > .01$.

Table 11
Estimated treatment on treated effects using propensity scores.

	(1) Control spend b/se	(2) Pay bills b/se	(3) Planning b/se	(4) Saving b/se	(5) Budgeting b/se
PS: Treatment	0.3378+	0.5853**	0.8326**	0.1712	0.3061
	(0.190)	(0.218)	(0.212)	(0.185)	(0.211)
Constant	1.8955**	1.7313**	0.8507**	0.7121**	1.5606**
	(0.131)	(0.150)	(0.146)	(0.128)	(0.145)

Propensity Score Estimates for Avg Treatment Effect matched on controls include tenant rent, total rent, child support, HH size, Ln income, years in FSS, white, male, high school, college, mean knowledge, age, age2, smoker.
Statistical significance: $+p > .10$. $^{*}p > .05$. $^{**}p > .01$.

Table 12
Estimated treatment assignment effects using Heckman 2-stage specification for non-zero values of savings and debt.

	(1) Saving b/se	(2) Debt b/se
Treatment	−206.7317	3915.4089*
	(262.335)	(1936.136)
Constant	1648.5244	−9826.3084
	(50681.789)	(20427.662)
select	2.4118**	2.3433**
	(0.363)	(0.380)
athrho	0.0000	0.6775
	(1732.804)	(0.581)
lnsigma	7.1936**	9.1936**
	(0.063)	(0.065)
N	126	127

Heckman 2-stage predicting non-zero values in 1st stage.
Controls include tenant rent, total rent, child support, HH size, Ln income, years in FSS, white, male, high school, college, mean knowledge, age, age2, smoker.
Statistical significance: $+p > .10$. $^{*}p > .05$. $^{**}p > .01$.

Table 13
Treatment on treated effects using Heckman 2-stage specification for non-zero values of savings and debt.

	(1) Saving b/se	(2) Debt b/se
TOT	382.4182	4113.2271*
	(274.969)	(2016.056)
Constant	1781.4412	−959.1824
	(218633.243)	(174542.423)
select	2.4118**	2.4147**
	(0.363)	(0.363)
athrho	0.0000	−0.0000
	(7525.790)	(816.001)
lnsigma	7.1884**	9.1850**
	(0.063)	(0.063)
N	126	127

Heckman 2-stage predicting non-zero values in 1st stage.
Controls include tenant rent, total rent, child support, HH size, Ln income, years in FSS, white, male, high school, college, mean knowledge, age, age2, smoker.
Statistical significance: +$p > .10$. *$p > .05$. **$p > .01$.

credit score. The imposition of common support and the smaller number of treatment-on-treated clients reduces statistical power, but these results are consistent with positive credit management practices, even if the average level of indebtedness among educated clients expanded.

Table 8 shows changes in self-reports of positive financial behavior, with positive coefficients suggesting movement to a higher category and improved self-reported behavior. Self-reported behaviors show strong responses to the treatment with the exception of being able to save money. Given the low-incomes of the people in this study, savings may in fact not be a reasonable expectation regardless of the rigor of the education involved. Table 9 shows the same dependent variables using propensity score matching. Here the coefficient for controlling spending is smaller in magnitude than without matching, and weaker in terms of statistical significance. Paying bills on time is also smaller in magnitude. Financial planning shows larger impacts, while saving and budgeting are not significant with matching. These results suggest effects focused on forward

Table 14
Actual savings, debt and credit rating predicted by self report.

	(1) Actual savings b/se	(2) Actual savings, FE b/se	(3) Actual debt b/se	(4) Actual debt, FE b/se	(5) Actual FICO b/se	(6) Actual FICO, FE b/se
White	0.7160+		−0.0190		10.8102	
	(0.423)		(0.200)		(9.032)	
Male	0.6788		0.4419		68.5852	
	(1.152)		(0.345)		(42.299)	
High school	0.3105		0.2107		−8.6760	
	(0.506)		(0.273)		(12.944)	
Some College	0.4529		0.5692*		4.7036	
	(0.482)		(0.252)		(12.158)	
Mean Fin. knowledge	0.3949*		−0.0007		1.9429	
	(0.170)		(0.083)		(3.858)	
Age	0.1712		−0.0948		−10.7525**	
	(0.206)		(0.102)		(4.142)	
Age2	−0.0017		0.0012		0.1422**	
	(0.003)		(0.001)		(0.049)	
Treatment group	0.0562		−0.1160		1.9290	
	(0.377)		(0.188)		(8.953)	
Self report saving	0.1255	0.0952				
	(0.139)	(0.182)				
Self report debt			0.0912*	0.0144		
			(0.036)	(0.034)		
Self report credit					29.9582**	17.7526**
					(4.593)	(6.281)
Constant	−4.7709	1.4172**	9.9405**	8.9395**	623.0604**	536.7593**
	(4.167)	(0.082)	(2.237)	(0.073)	(97.721)	(13.850)
N	265	270	241	246	229	234

2-period dataset using clustered Std. Err. at respondent level or Fixed Effects (FE).
+ $p > .10$.
* $p > .05$.
** $p > .01$.

thinking financial activities such as planning and timely bill payment, but weaker on areas perhaps with limited capacity for change given the low-incomes of clients in the study such as controlling spending, saving and budgeting (all three activities imply some discretionary income and expenses which may not be as applicable for people with tight budget constraints).

Table 10 shows similar effects for clients who actually completed the courses (TOT). Controlling spending is not significant but paying bills, financial planning, saving and budgeting are. The coefficients are also larger than in Table 8, consistent with stronger positive effects for the TOT group as would be predicted if exposure to more education has stronger effects overall. Table 11 shows the same dependent variables using propensity score matching to estimate TOT effects. Timely bill payment and financial planning remain the most robust results, with weaker results for controlling spending. Together these models provide modest support for improved self-reported behaviors related to these forward-thinking tasks.

The next set of estimates are in Tables 12 and 13 which present a Heckman 2-stage model for intent to treat (ITT) and treatment on treated (TOT) coefficients on savings and debt. This is primarily a robustness test since both variables have values of zero which might be correlated with certain client characteristics. In these models treatment as TOT and ITT both show accumulation of debt of $3900 and $4100 respectively during the 12 month study period. These results are slightly smaller in magnitude but similar to the non-matching results in Tables 4 and 6.

An additional analysis is presented in Table 14 to show the regression results combining client responses over the two surveys to estimate how well actual savings, debt and credit levels are predicted using self-reported measures. Within subjects comparisons are estimated correcting for errors at the individual level and controlling for race, gender, education level, self-reported financial knowledge, age, and treatment status. Overall, self-reported debt and credit measures are relatively predictive of actual debt levels and credit scores as shown in the credit record. Controlling for fixed effects only credit self reports show a correlation with actual levels. Self-reported savings levels, which is more than 80 percent of clients reported as zero, had little or no predictive value concerning actual savings. Only actual bank account balances are observed, while the survey question refers to total savings and investments; this difference may at least partially account for this result. Other than education or self-assessed financial knowledge, no other factors predict the difference between self-reported

Table 15
Survey questions.

How do you grade yourself in the following areas in the last 12 months?					
	Poor	Fair	Okay	Good	Excellent
Controlling my spending	[]	[]	[]	[]	[]
Paying my bills on time	[]	[]	[]	[]	[]
Planning for my financial future	[]	[]	[]	[]	[]
Saving money	[]	[]	[]	[]	[]
Following a budget	[]	[]	[]	[]	[]
How much do you know about...					
	Nothing	Very little	Some	A fair amount	A lot
Credit ratings and credit files	[]	[]	[]	[]	[]
Managing finances	[]	[]	[]	[]	[]
Investing money	[]	[]	[]	[]	[]
What is on your credit report	[]	[]	[]	[]	[]
Interest rates, finance charges and credit terms	[]	[]	[]	[]	[]
How much would you estimate you and your spouse/partner have in combined total savings and investments?					
No assets	[]				
Less than $1000	[]				
$1000 to $4999	[]				
$5000 to $9999	[]				
$10,000 to $14,999	[]				
$15,000 to $19,999	[]				
$20,000 or more	[]				
How much would you estimate you and your spouse/partner have in combined total debts?					
No debts	[]				
Less than $1000	[]				
$1000 to $4999	[]				
$5000 to $9999	[]				
$10,000 to $14,999	[]				
$15,000 to $19,999	[]				
$20,000 or more	[]				
How would you rate your current credit record?					
Very bad	[]				
Bad	[]				
About average	[]				
Good	[]				
Very good	[]				

and actual measures. These comparisons suggest that self-reports are directional indicators, but they fall short of capturing actual behavior. Nonetheless they do no exhibit systematic bias by treatment nor by observed characteristics.

5. Discussion and conclusions

This field study is unique in that clients were randomly assigned to financial education, while a control group was not offered education until after the study period. Because participants in the study were enrolled in a public means-tested program, data could be collected regularly, facilitating a longitudinal study. The program was only 12 h in length and was delivered over five evenings in the course of a month and a half—a relatively weak intervention provided to a financially distressed population. Administrative notes provided by case managers working with the participants in this study show that many clients experienced problems with domestic violence, unstable employment, drug and alcohol abuse, and inadequate daycare, all of which may have made financial management a low priority. This also helps explain why nearly 2 out of 5 treated clients did not attend all the courses required.

While diminished by low statistical power due to a small sample, this study is unique for including objective measures of behavior from bank accounts and credit reports, as well as self-reported data. Nevertheless, the effects of attrition are only partially observable and the propensity score model cannot fully account for a client's decision to attend classes or leave the program. Moreover, because blinding clients of their assignment into the treatment or control group was impossible, the consent process may have alerted clients that they needed financial education or to attend to financial matters. Clients who were assigned to the control group and subsequently told that they were not permitted to participate in the course for 12 months may have reacted to this information in ways that affected their survey responses or even their behavior. It should also be noted that clients in this study were enrolled in a self-sufficiency program and as such may have responded differently to financial education than individuals not involved in a housing subsidy or self-sufficiency program.

Overall the effects of this educational program are illustrative, albeit a bit muddled. Without matching it appears clients who were offered or participated in education took on more debt and experienced lower net worth. One perspective could be this is a negative outcome if increased savings is viewed as a positive behavior and debt a negative one. But given the limited economic conditions of the people in this study, expanding debt in order to finance expanded consumption might be a positive behavior, if that debt is well managed. With propensity score matching the effects of treatment on debt are not statistically significant, however matching estimators of the effects of treatment on the treated suggest an increase in credit scores, potentially an unambiguously positive outcome. Self-reported behaviors are consistent with a focus on financial planning in the sense of taking an active role in forecasting expenses, budgeting and paying bills.

The idea that credit constrained consumers might use information gained from a course to access more credit is plausible, although it remains to be seen if this is in fact welfare enhancing. In the short run consumers may benefit from consumption or investment in durable goods or even human capital that offers positive returns over time. But if borrowers struggle to repay the debt, this may lower overall household well-being. To the extent people have self control problems related to consumption, ignorance of additional credit options may serve as a welfare-enhancing constraint (Thaler and Shefrin, 1981). If education exacerbates the self control problem it could be welfare reducing. Caution is warranted before generalizing these results to other populations, but the role of education/information in expanding use of credit deserves further study. If education supports consumers to use more credit without more defaults this could imply lenders would benefit from an expansion of financial education efforts.

Mandating financial education, at least in the form of this program, is not costless. Providing workshops incurred direct costs of at least $100 per participant. There is also an opportunity cost for the participant of the time and effort required to attend sessions. To the extent financial education can be delivered at or below its marginal benefit, public policies mandating financial education may be a good investment of public and private resources if improving the financial status of low-income families is a policy goal.

Methodologically, this study demonstrates the implementation of a randomized design in a field-based setting. The study did not deny services to any client but rather exploited an education requirement within an existing pool of clients during a two-year period. By randomizing the timing of service delivery and using multi-wave data collection, a comparison group could be created. This study also suggests that self-reported credit and debt measures provide at least directional indicators of actual personal financial behavior.

Overall, it appears that information, even when conveyed in a group setting over a relatively short time period, can result in modest behavior changes. If debt is viewed as a negative behavior there could be an aspect of moral hazard in this program—that is showing people how to manage money increases debt and lowers net wealth. But there are no indications of problems managing debt, and savings may not be a reasonable expectation for a family of four with an income of $1300 per month even with a subsidized rent of only $450. Previous literature suggests strong effects might be achieved by leveraging financial knowledge with innovations in financial product access and design (Jones, 2008), but even in the absence of such interventions the provision of even simple financial information has effects.

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