



The minimal impact of a large-scale financial education program in Mexico City[☆]

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ABSTRACT

We conduct randomized experiments around a large-scale financial literacy course in Mexico City to understand the reasons for low take-up among a general population, and to measure the impact of this financial education course. Our results suggest that reputational, logistical, and specific forms of behavioral constraints are not the main reasons for limited participation, and that people do respond to higher benefits from attending in the form of monetary incentives. Attending training results in a 9 percentage point increase in financial knowledge, and a 9 percentage point increase in some self-reported measures of saving, but in no impact on borrowing behavior. Administrative data suggests that any savings impact may be short-lived. Our findings indicate that this course which has served over 300,000 people and has expanded throughout Latin America has minimal impact on marginal participants, and that people are likely making optimal choices not to attend this financial education course.

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1. Introduction

As access to financial services expands around the world, there has been growing concern that many consumers may not have sufficient information and financial acumen to use new financial products responsibly, a concern amplified by the recent global financial crisis. In response, many governments, non-profit organizations and financial institutions have started to provide financial literacy courses. However, participation rates for non-compulsory financial education programs are typically extremely low. Willis (2011, p. 430) sums this up as “Voluntary financial education is widely available today, yet seldom used”.¹ This raises two interrelated questions which are important for research

and policy. The first is whether there are economic or behavioral constraints which prevent more individuals from participating in such programs? The second question is whether there are any benefits to marginal individuals from doing so, or whether they are rationally choosing not to participate in such training?²

We investigate these questions through the context of randomized experiments conducted in Mexico City. We evaluate a free, 4-hour financial literacy course offered by a major financial institution on a large-scale, covering savings, retirement, and credit use. We document that there is relatively low interest in such a program among the general population, and screen recruited subjects on interest in participating. Motivated by different theoretical and logistics reasons why individuals may not attend training, we randomized the treatment group into different subgroups, which received incentives designed to provide evidence on some key barriers to take-up. These incentives included monetary payments for attendance equivalent to \$36 or \$72 USD, a one-month deferred payment of \$36 USD, free cost transportation to the training location, and a video CD with positive testimonials about

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¹ For example, Brown and Gartner (2007) examine pilot experimental efforts by credit card providers in the United States to provide online financial literacy training to delinquent and at-risk credit card holders, as well as to college students who had newly received credit cards. Participation rates in the three studies were only 0.06%, 0.9% and 6.5% (the latter pilot offered a 60-minute phone card for participation).

² There is a growing literature attempting to assess the causal impacts of financial education in developing countries starting with Cole et al. (2010). Much of the existing literature has looked at training provided in a compulsory manner (e.g. Bruhn et al., 2013) or to specific populations like migrants or farmers (Cai, 2012; Doi et al., 2012; Gibson et al., 2014; Seshan and Yang, 2014). Miller et al. (2013) provide a meta-analysis. We are not aware of other studies which look at large-scale voluntary programs provided to a general population in urban areas — which are precisely the types of programs that are increasingly being adopted in many countries.

the training. We find that the latter two incentives did not increase take-up, but both current and deferred monetary payments boosted attendance rates by about 10 percentage points. There was no significant difference in attendance between those who got the payment immediately and those who received a deferred payment, suggesting that low take-up of the course we evaluate is not due to the cost of attending training being realized immediately while benefits accrue in the future combined with time-inconsistency or high discount rates.

A follow-up survey conducted on clients of financial institutions six months after the course is used to measure the impacts of the training on financial knowledge, behaviors and outcomes, all relating to topics covered in the course. We find attending training results in a nine percentage point increase in financial knowledge, and a nine percentage point increase in an index on saving outcomes (i.e. whether the subject has savings or increased her savings since last year), but no impact on credit card behavior, retirement savings, or borrowing. Moreover, limited administrative data on a subset of individuals suggests that the savings impact may be relatively short-lived. Data on credit card balances and repayment rates show no systematic differences across the treatment and control groups related to the course. It therefore appears that individuals see relatively small benefits from participating and may be making rational decisions not to attend such financial literacy training programs.³

2. A large-scale financial literacy training program

The program we evaluate is a large-scale program offered to the general public by a major financial institution in Mexico City. The course was developed by banking specialists and teachers from an organization dedicated to promoting economic education. It consists of modules on saving, retirement, credit cards, and responsible use of credit, lasting about 4 h in total. Appendix 1 provides details of what is covered under each topic, and documents that the content is similar in focus to other well-known global courses, such as the Freedom from Hunger curricula. The program was first launched in 2008, trained over 300,000 individuals over its first four years in Mexico, and expanded in 2011 to five other countries in Latin America. The course received two awards from a major international development institution for innovativeness in fostering financial education.⁴ Given the scale of the program and its reputation, it is of interest to understand why more people don't participate⁵ and what the impact of participating is on those who do take the training.

3. Experimental design and the low demand for financial literacy training

Our evaluation took place between January 2010 and July 2012, with Appendix Table A2 providing a timeline. Appendix 2 details three different approaches that we used to obtain a sample for the experiment. The first was to send 40,000 invitation letters from our collaborating financial institution asking about interest in participating. However, only 42 clients (0.1%) expressed interest. The second approach was to advertise through Facebook, with an ad displayed 16 million times to individuals residing in Mexico City, receiving 119 responses. The third approach was to conduct screener surveys on streets in Mexico City and outside branches of the partner institution. Together this yielded a total sample

of 3503 people. We divided this sample into a control group of 1752 individuals, and a treatment group of 1751 individuals, using stratified randomization. A key variable used in stratification was whether or not individuals were financial institution clients.⁶ The sample of 1325 respondents who were not clients of any financial institution are used for the training program take-up experiments, but were not resurveyed. Our analysis of treatment impacts is thus based on the sample of 2178 individuals who were financial institution clients.

Our sample has average age 33, is 47% female, and 40% has at least a bachelor's degree. At baseline, 64% had made a savings deposit in the past month, and 41% had a credit card. Among those with a credit card, only half had made more than the minimum payment in all past six months, and 23% had made a late payment in the last six months. Appendix Table A3 shows randomization led to balance on baseline characteristics across the treatment and control groups.

The treatment group was contacted by telephone and invited to participate in the financial literacy training program, following a protocol outlined in Appendix 3. The overall attendance rate following these invitations for the 1751 treatment group individuals who had been screened for interest in attending a financial literacy course was only 17.8%. Theory offers a number of potential explanations for low attendance (Appendix 4). In order to investigate these barriers to take-up, the treatment group was divided further into six different groups – one group who was invited to attend once more but received no further assistance, and the following five booster treatment groups:

1. Offered 1000 pesos (US\$72) for completing the training: participants were given a Walmart gift card of 1000 pesos if they attended,⁷
2. Offered 500 pesos (US\$36) gift card for completing the training,
3. Offered 500 pesos (US\$36) gift card that they would receive one month after completing the training,
4. Offered a free taxi ride to and from the course location,
5. Provided a video CD containing positive testimonials from people who had attended the course.

These treatments enable us to examine how individuals respond to a change in the benefits of participating (treatments 1 and 2), whether high discount rates or present bias makes them sensitive to the timing of these benefits (comparing treatments 2 and 3), lowering the transport costs of training (treatment 4), and reducing informational constraints and uncertainty about benefits (treatment 5).

Table 1 presents the attendance for individuals assigned to the treatment group as well as the regression results of the impact on attendance in each incentive group. Fig. 1 shows the overall percentage attending in each incentive group. As a result of the booster interventions, we succeeded in getting an additional 114 individuals to attend the course, giving a total of 426 attendees out of 1751 treatment group individuals (24.3%). Offering a monetary incentive of \$36 increased the take-up rate from about 18% to 27% of those assigned to treatment, and the \$72 incentive increased take-up further to 33%. While the difference between the two monetary amounts is not statistically significant, they both suggest that individuals are rationally responding to higher benefits of training by being more likely to attend. The impact of the incentives on the attendance rate is exactly the same when US\$36 is offered immediately at the completion of training, or one month later. If high discount

³ It should be noted that we estimate a local average treatment effect for the effect on people induced to attend training by our invitations and incentives, and show that the impact of training is small for these individuals; we are of course unable to rule out the possibility that the benefits of training may be higher for the types of individuals who we are never able to induce to attend.

⁴ The institution received over 100 entries in the competition and announced nine winners for two types of prizes: the program we evaluate received two of these awards.

⁵ While 300,000 individuals is a substantial number, this is country-wide and over four years – in any one year it appears that less than 0.6% of Mexico City's adult population participates in training.

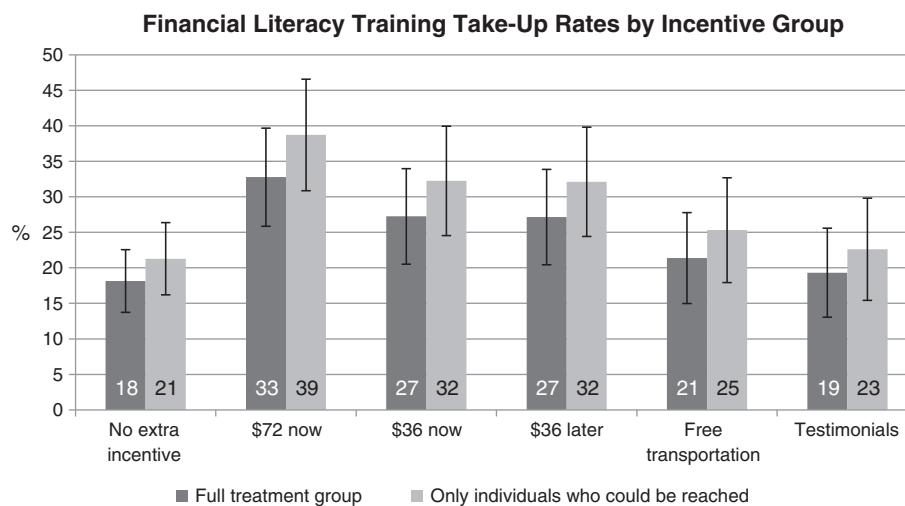
⁶ We stratified the randomization by whether we obtained the respondent through the branch vs. the mail, online or street survey, by gender, by having at least a bachelor's degree or not, and by whether the person was (i) a client of our partner financial institution, (ii) a client of another financial institution, (iii) neither. Within the sample of financial institution clients, we further stratified by whether they made a deposit into their savings account during the past month and by whether they have a credit card. For clients with a credit card, we stratified by whether they made more than the minimum payment each month during the past six months. For individuals who were not financial institution clients, we stratified by whether they lived closer than 8 km away from a training location or not. This resulted in 94 strata in total.

⁷ For comparison, at follow-up, median monthly income in our sample was about 9000 pesos (US\$650).

Table 1
Impact of incentive treatments on take-up in treatment group.

	Dependent variable: Attended course		
	(1)	(2)	(3)
Assigned to treatment group dummy	0.2431*** (0.0101)		
\$72 now dummy		0.1480*** (0.0352)	0.1758*** (0.0401)
\$36 now dummy		0.0931*** (0.0343)	0.1123*** (0.0393)
\$36 later dummy		0.0918*** (0.0343)	0.1110*** (0.0392)
Free transportation dummy		0.0330 (0.0326)	0.0407 (0.0376)
Testimonials dummy		0.0142 (0.0320)	0.0153 (0.0367)
F-test p-value: \$72 now = \$36 now		0.1435	0.1357
F-test p-value: \$36 now = \$36 later		0.9725	0.9761
Control group mean of outcome variable		0.1815	0.2129
Observations	3503	1751	1457
Sample	Full sample	Full treatment group	Only individuals who could be reached

Notes: Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10, 5 and 1% levels respectively. Column (1) presents OLS regression of a dummy of having attended the course on a dummy for assignment to the treatment group. The omitted category is the control group. Columns (2) and (3) present OLS regressions of a dummy for having attended the course on a set of dummies indicating to which incentive group the individual was randomly assigned (the omitted category is the group with no extra incentives). All regressions control for randomization strata dummies.



Note: Full treatment group includes 1,751 individuals. The sub-sample who could be reached to offer them the treatment includes 1,457 individuals. Vertical lines indicate a 95% confidence interval for the point estimates of take up rates across different incentive groups.

Fig. 1. Financial literacy training take-up rates by incentive group.

rates or present bias, combined with the cost of training occurring immediately and the benefits accruing in the future, were the main barrier to participating, we would have expected differential responses to offering the benefit immediately after training versus in one month. The fact that we don't therefore suggests that the timing of benefits combined with high discount rates or present-bias are not the main barriers to participation in training.⁸ In contrast to the monetary incentives, we find that transportation assistance and the testimonials did not

significantly increase attendance.⁹ Finally note that even when participants were offered US\$72 to attend, it is still the case that the majority of individuals who had initially expressed interest in attending financial literacy training do not attend.

The attendance rate after these efforts was 28.1% for clients of financial institutions, and 18.1% for non-clients. Given budget constraints and low power to detect impacts on the non-clients, our follow-up survey to measure impact was only done on the 2178 individuals who were clients of a financial institution (recall the randomization was stratified by this variable). We have two sources of follow-up data. The first is a survey which took place between February and July 2012, averaging 6 months post-training. This succeeded in interviewing 72.8% of the sample. The second is limited administrative data provided by our partner institution: it was able to match 470 individuals from our sample to

⁸ Note that an alternative is that high discount rates or present bias may actually affect the effectiveness of training – either because these individuals will never save regardless of whether they received the training and have especially low benefits; or because the training teaches techniques to overcome these behavioral constraints, resulting in especially high benefits for this group. We do not have baseline measures of discount rates or time inconsistency, so are unable to investigate whether there is treatment heterogeneity of this type.

⁹ One reason for the lack of impact of transportation assistance were security concerns in Mexico City, with people reluctant to take a taxi cab that came to their home.

the institution's records and provided summary statistics on saving account balances and credit card outcomes from this data. Appendix 5 includes more detail on this data collection, while Appendix Table A3 shows the baseline data is balanced by treatment status for both the subset re-interviewed at follow-up, and for the subsample that were found in the administrative data.

4. Impacts of financial literacy training

We estimate the impact of the financial literacy training with the following intention-to-treat (ITT) equation:

$$Y_{i,s,m} = \alpha + \beta \text{TrainingInvite}_{i,s,m} + \sum \gamma_s d_s + \sum \delta_m d_m + \varepsilon_{i,s,m} \quad (1)$$

where $Y_{i,s,m}$ is a follow-up survey measure of the financial knowledge, behavior, or outcome of individual i , in randomization strata s , who was surveyed in month m . The variable $\text{TrainingInvite}_{i,s,m}$ indicates whether an individual was invited to the course or not and is thus equal to one for the treatment group and equal to zero for the control group. We control for randomization strata dummies d_s , as well as month of follow-up interview dummies d_m . The main coefficient of interest is β , which represents the treatment effect of being invited to a financial literacy course. In addition, we also instrument training attendance with invitation to training to measure the local average treatment effect (LATE). This is the impact of the financial literacy course on the individuals who attended a course as a result of being invited to the course but who would not have otherwise attended.

Our follow-up survey contains a number of questions intended to measure financial knowledge, as well as savings and credit behaviors and outcomes. In order to efficiently summarize a number of different measures and reduce concerns about multiple hypotheses testing, our approach is to form aggregate index measures. When our outcomes are all binary variables, we take the simple average of the component questions, while an average of standardized z-scores is used when we have a mix of binary and continuous outcomes (Kling et al, 2007). Appendix Tables A4–A11 provide the specific measures that go into each index and impacts on each component.

4.1. Impact on financial knowledge

We measure financial knowledge through eight follow-up survey questions based on the material that was taught in the course. Our financial knowledge index is the average of eight dummy variables and represents the fraction of questions that the respondent answered correctly. We also asked individuals to rate their own level of financial education on a scale from one to five (1 = excellent, 2 = good, 3 = satisfactory, 4 = unsatisfactory, 5 = no knowledge of the topic). Based on the responses to this question, we coded a dummy variable indicating whether the self-assessed level of financial literacy is satisfactory or higher.

The first two rows of Table 2 show that the course had a positive and statistically significant impact on financial knowledge. The knowledge index increased from 0.31 in the control group to 0.34 in the treatment group due to the training. The training also increased the self-assessed level of financial literacy by five percentage points from a base of 58%.

Table 2
Impact on financial knowledge, behavior and outcomes.

	Sample size	Control mean	ITT treatment difference	LATE treatment difference
<i>Panel A: Knowledge</i>				
Knowledge index (average of 8 components in Table A4)	1586	0.31	0.0307*** (0.0094)	0.0871*** (0.0261)
Says their financial knowledge is satisfactory or higher	1550	0.58	0.0537** (0.0248)	0.1500** (0.0683)
<i>Panel B: Savings</i>				
Savings behavior index (avg. of 5 components in Table A5)	1586	0.68	0.0133 (0.0124)	0.0376 (0.0348)
Savings outcomes index (avg. of 3 components below)	1586	0.65	0.0335** (0.0147)	0.0948** (0.0414)
(1) Has any type of savings ¹	1586	0.80	0.0288 (0.0200)	0.0814 (0.0566)
(2) Saved more than zero during past 6 months	1413	0.83	0.0293 (0.0192)	0.0800 (0.0524)
(3) Saves more each month than a year ago	1547	0.36	0.0408 (0.0250)	0.1151* (0.0697)
Has a pension fund	1471	0.57	−0.0031 (0.0263)	−0.0088 (0.0739)
Retirement savings behavior index (avg. of 3 components in Table A7) ²	1471	0.20	0.0114 (0.0144)	0.0320 (0.0404)
Is saving money for retirement ²	1470	0.17	0.0122 (0.0199)	0.0343 (0.0559)
<i>Panel C: Credit cards and loans</i>				
Currently has at least one credit card	1560	0.48	−0.0287 (0.0210)	−0.0811 (0.0596)
Credit card behavior index (avg. of z-scores of 6 components in Table A8) ³	1556	0.00	−0.0233 (0.0229)	−0.0660 (0.0652)
Credit card outcomes index (avg. of z-scores of 3 components in Table A9) ³	1554	0.00	0.0434 (0.0416)	0.1228 (0.1184)
Loan behavior index (avg. of 3 components in Table A10)	1570	0.15	0.0075 (0.0118)	0.0212 (0.0334)
Loan outcomes index (avg. of z-scores of 2 components in Table A11)	1560	−0.01	−0.0132 (0.0427)	−0.0372 (0.1206)

Notes: Robust standard errors in parentheses. *, **, and *** indicate statistically different from control mean at the 10, 5 and 1% levels respectively, after controlling for randomization strata and month of follow-up interview dummies.

¹ Includes savings account, caja de ahorro, tanda and other non-retirement savings.

² Based on questions that were only answered by individuals who have a pension fund. To account for potential selection bias, we fill these variables in with “0” for individuals who do not have a pension fund.

³ Based on questions that were only answered by individuals who have a credit card and refer to the most frequently used card. To account for potential selection bias, we fill these variables in with “0” for individuals who do not have a card.

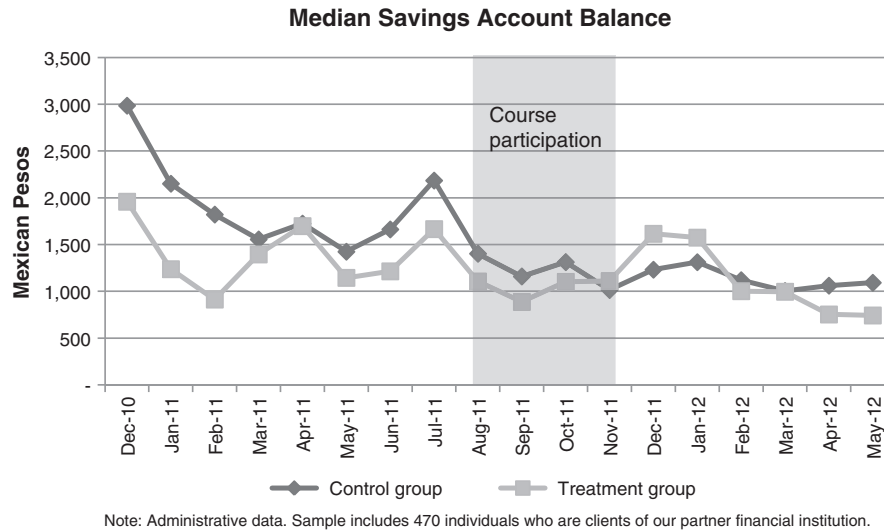


Fig. 2. Median savings account balance.

Recall that less than 30% of the treatment group attended the financial literacy course. The LATE estimates in the second column take this into account, and show larger impacts on individuals who actually went to the training as a result of being invited through the intervention: the knowledge index increased by 8.7 percentage points for these individuals, while self-assessed knowledge rose by 15 percentage points.

4.2. Impact on savings behavior and outcomes

The financial literacy course emphasized specific behaviors that may help individuals save more money, including: checking financial institution transactions regularly and keeping track of expenses, making a budget and setting a savings goal, identifying necessary and unnecessary expenses to reduce overall expenditures. The follow-up survey asked whether individuals engage in five of these behaviors, which are aggregated into a savings behavior index. The ITT impact on the overall index is small (0.01) and not statistically significant. Looking at the individual questions in Appendix Table A5, the LATE estimate suggests that participating in the course led to a 12 percentage point increase in whether individuals have cut expenses in the past three months, but this is only statistically significant at the 10% level, and does not survive any correction for multiple testing.

We then aggregate three measures of personal savings outcomes: (i) whether the respondent has any type of savings; (ii) whether they say they have saved during the past six months; and (iii) whether they say they save more each month than they did a year ago. We see each individual measure has a positive, but typically not significant effect, and that there is a positive and significant impact on the aggregate savings measure: control group individuals average 65% of the three savings outcomes, which increases to 68% in the treatment group. The LATE estimate gives a 9.5 percentage point increase in savings outcomes, significant at the 5% level. Appendix Table A6 shows this increase is robust to controlling for a direct effect of the monetary incentives used to induce participation in training.¹⁰

¹⁰ The effects on savings outcomes may be overestimated if individuals believed they were expected to give “correct” answers (i.e. they are saving more today) and answered accordingly. This could happen if, for example, individuals perceived we provided the training and did not want to disappoint us. While caution was taken to prevent this (via scripts referring to different organizations at different stages of the project and allowing an average of six months between training and the follow up survey), we cannot rule out this possibility. This suggests that the true effect of the training on savings outcomes may be smaller than 9 percentage points, adding to our overall finding of small effects of the course studied.

Fig. 2 plots median savings account balances over time in the treatment and control groups for the 470 individuals that could be matched to administrative data. The median savings account balance in the treatment and control group followed a more or less parallel trend in the few months prior to our intervention, and then, starting in October 2011, the savings balance starts rising in the treatment group, going from about 900 pesos in September 2011 to 1600 pesos in December 2011, but then starts falling again. Although the aggregate data provided to us does not allow us to conduct a test of statistical significance in the difference in medians, the observed pattern is consistent with the course having at most a temporary positive impact on savings.

The course also included material on retirement savings and pension funds. We ask about three behaviors (choosing their pension fund administrator based on fees or returns, checking their pension fund statement, and calculating how much they need for retirement), and one retirement outcome (whether they are saving money for retirement). Tables 1 and A7 show small and not statistically significant impacts on these measures.

4.3. Impact on borrowing behavior and outcomes

The course then discussed responsible use of credit cards and healthy borrowing behavior. Panel C in Table 1 shows 48% of the control group had a credit card at follow-up, with a small and not statistically significant impact of treatment on this. We examine six credit card behaviors, measuring whether individuals know their credit limits and interest rate, check their statements monthly, and the fraction of months where they pay their balance in full, pay only the minimum balance, or took a cash advance on their card. We construct a credit card behavior index based on these behaviors, converting all variables to z-scores.¹¹ We also measure three credit card outcomes – having their card blocked by the lender, being charged late payment fees, and being charged overdraft fees. Table 1 and Appendix Tables A8 and A9 show that training had a small and not statistically significant effect on the overall index measures of credit card behaviors and outcomes. The effects were not statistically significant on each individual component measure either. Likewise we find a small and not statistically significant effect on broader loan

¹¹ Individuals without a credit card had these variables coded as 0. Making only the minimum payment or withdrawing cash through the credit card were coded with negative signs in forming the average.

behaviors¹² and loan outcomes.¹³ Appendix Figs. A1 and A2 also show no discernible impact on median credit card balances or the percentage of debt paid off each month for the subsample which could be matched to administrative data.

With insignificant impacts, there is always a question of how precise the estimates are. Our ITT estimates typically are close to zero in magnitude, with standard errors in Table 2 averaging 0.02 for the statistically insignificant outcomes — so we can typically rule out increases that are larger than 5 percentage points.

5. Conclusions

We find very little interest in participating in a financial education course among samples of financial institution clients and Facebook users, and that even among a sample of those screened for interest in attending training the majority of individuals do not attend. Experiments to induce take-up suggest that the low participation rate for this course is not mainly due to high discount rates, time-inconsistency, or lack of information, but rather appears to be due to individuals thinking the benefits of attending are not high enough. Monetary incentives which increase the benefits lead to more attendance. A follow-up survey conducted approximately six months after the course shows that the benefits appear relatively minimal. The training does lead to some increase in financial knowledge, but this knowledge has limited impact on outcomes. There is an increase in self-reported savings, but this increase in savings appears to dissipate quickly, and there is no evidence that the training changed credit card usage or borrowing behavior. The reluctance of individuals to attend training therefore appears likely to be a rational choice given the relatively minimal benefits that come from attending.

We caution that our study measures local treatment effects of one particular course, showing the effect of training for people who are induced to attend as a result of our interventions, but would not attend without it. The impacts for those who self-select into not attending a program available to anyone who wants it may differ from the impacts for those who choose to participate of their own accord,¹⁴ and likewise

might be higher (or lower) for other financial literacy courses around the world. Our study does suggest that the benefits of encouraging more people to participate in this particular training course are likely to be slight, adding to recent skepticism about the value of such types of training (The Economist, 2013; Willis, 2011).

Appendix A. Supplementary data

Supplementary data to this article can be found online at <http://dx.doi.org/10.1016/j.jdeveco.2014.02.009>.

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¹² Measured in terms of applying for a loan from any source in the past six months, getting credit from a pawn shop, and stopping servicing outstanding debt (see Table A10).

¹³ Measured by whether individuals currently have any form of loan, and their debt outstanding as a fraction of income (see Table A11).

¹⁴ Revealed preference would suggest the impact should be larger for those who voluntarily attend; but there is also evidence from business training to suggest those who are initially least interested might benefit the most (Karlan and Valdivia, 2011).