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Financial education for female foreign domestic workers in Singapore

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ABSTRACT

We evaluate a randomized field experiment to study the effect of financial workshops for domestic workers in Singapore. Groups of women met monthly with a trained mentor. Take-up rates were low and our results are inconclusive as to whether invitations to these workshops improved financial knowledge and behavior. Unexpectedly, treatment assignment had a significant, negative effect on self-reported savings. Further exploration suggests that assignment to treatment could affect participants' awareness of accumulated savings. We find a reduction in the number of savings accounts reported and an increase in the probability respondents report having disagreements with family members over finances.

1. Introduction

Financial decisions are complex and can have serious consequences for individual and social well-being. Migrant workers face additional challenges related to sending sub-national, or even international, remittances. Besides impacting the well-being of transnational households, aggregate remittance flows are substantial, making up an important part of international financial flows. Migrant workers also face greater informational asymmetries arising from being geographically separated from their families. Compounding this with gender differences in financial literacy (Lusardi & Mitchell, 2008) and intra-household control over finances, female migrant workers are especially vulnerable to making suboptimal financial decisions.

In this paper, we evaluate a savings intervention tailored to female Filipino domestic workers in Singapore. Randomly chosen women were invited to join savings clubs of 10–12 women who met with a mentor once a month for nine months. Mentors were trained and the clubs were organized by an NGO with experience in providing financial education for this specific population. The mentor covered financial material developed by the NGO, in addition to providing the participants with short-term savings goals and going over the participants' financial documents. The material focused on the importance of saving, as well as learning to say no to unnecessary expenses, either their own whims or requests from their family members in the Philippines.

We have three main findings. First, we document very low take-up. Only 16% of the women invited to join a club enrolled. This low take-up rate, along with high attrition arising from the transitory nature of employment and frequent phone number changes in this population, limit our statistical power. Consequently, our point estimates come with large standard errors. Nevertheless, while our intent-to-treat analysis reveals no statistically significant changes in financial knowledge or planning, our second finding is a statistically significant 9 percentage point *reduction* in whether the women report having any savings.[1] We find negative point estimates on savings in both Singapore and the Philippines, separately, although these estimates are not statistically significant at conventional levels. Since the financial outcomes are self-reported, it is worth considering how accurately migrants know how much savings they or their families back home have; one explanation for this unexpected finding is that assignment to treatment urges women to seek more information on how much savings they have and how their remittances are spent or saved. Consistent with this explanation, our third finding is that women invited to join a savings club

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[1] It is worth noting that the intervention we study is longer than many financial literacy workshops studied in the literature, which are often only over a few days. This intervention therefore includes better reinforcement over time.

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report having fewer savings accounts and are more likely to report disagreeing with their family members about how remittances are spent. Thus, it would be premature to conclude that assignment to treatment was harmful to participants: being more aware of how much savings one actually has could be beneficial in the long run. Similarly, there may be an optimal level of savings, and people may gain from both reducing and increasing their savings levels. We do not find support for other possible explanations that may be more harmful, such as a discouragement effect if participants decide their savings goals are unattainable.

Our primary analysis uses an intent-to-treat specification, documenting the effect of *invitations* to the savings clubs; this method addresses the endogeneity of enrollment by comparing women randomly assigned to receive an invitation to a club and women randomly assigned not to receive an invitation. One limitation of this strategy is that it does not allow us to separately identify the effect of participating in the club from the effect of receiving an invitation itself. In order to further investigate explanations for our unexpected results, we use propensity score matching methods to compare women with similar propensities to enroll in the club, following Ibarra, McKenzie and Ortega (2017). This analysis warrants caution, due to our very small sample size and limited pre-intervention data, as compared to Ibarra et al. (2017), but it provides suggestive evidence that the negative impact on savings is driven by those who received an invitation to a savings club but chose *not* to enroll. One possible explanation has to do with the course fee. The S$55 fee was paid up front by the student, but refunded over the following nine months in order to encourage regular attendance. In addition to possibly explaining the low take-up, this fee structure may have made participants more aware of their limited savings: Many women expressed concern about coming up with the money. Note that this result is not inconsistent with the explanation posited above that women might find savings more salient after filling out a survey and receiving an invitation to a financial literacy workshop; the women who enroll may then benefit from the workshop while those who do not are left simply with the realization that they have less savings than they previously thought. We are unable to provide a conclusive answer to this puzzle and must leave it to future work.[2]

The primary contribution of this paper is to the literature focused on financial literacy and financial education. A large literature documents correlations between financial literacy and a host of financial decisions, including planning for retirement, accumulating precautionary savings, and borrowing at high interest rates (see, for example, Lusardi & Mitchell, 2007; Lusardi & Tufano, 2015; van Rooij, Lusardi & Alessie, 2012). However, existing research has yet to reach a consensus on whether and what kind of financial literacy training improves financial well-being (see Fernandes, Lynch & Netemeyer, 2014; Hastings, Madrian & Skimmyhorn, 2013; Miller, Reichelstein, Pauliac & Zia, 2015; and Kaiser & Menkhoff, 2017 for reviews and meta-analyses of this literature).[3] This paper focuses on financial education provided to migrant workers. A few recent studies have evaluated financial literacy training provided to similar populations as ours and found mixed impacts. Gibson, McKenzie and Zia (2014) provide financial literacy training to migrants of both genders with a focus on remittance decisions and find a reduction in using costly remittance methods but no change in remittance frequency or amount. Doi, McKenzie and Zia (2014), in a paper very close to ours, finds that financial training for migrating women has no impact, unless family members are provided with financial literacy training as well. Seshan and Yang (2014) provide a financial workshop to male migrants and find positive impacts for households with low pre-treatment savings levels and changes in behavior for wives as well.

These papers speak to the important role that gender differences and intra-household decision-making play in understanding the impact of financial education. Accordingly, this paper also contributes to the literature on gender differences in financial decision-making (Shurchkov & Eckel, 2018) and the literature on intra-household financial decision-making. Ashraf (2009) studies the financial choices of married individuals in the Philippines and finds that individuals alter their savings choices when the choice is observed by their spouse. Ashraf, Aycinena, Claudia Martinez and Yang (2015) find that Salvadoran migrants in the U.S. (71% of whom are male) save more in the home country when offered financial products that give the migrant more control over savings. Abarcar, Barua and Yang (2017) evaluate financial education and access for transnational households, focusing on the household back home, and find reductions in borrowing from informal sources, but no effects on well-being. Consistent with these papers, our results highlight the role of spousal control over savings decisions, but the intervention we provide primarily targets female migrants.[4]

This paper proceeds as follows. In Section 2, we provide background information on the population of interest, foreign domestic workers in Singapore. Section 3 describes in detail the intervention and sample selection and presents descriptive statistics. The empirical results are presented in Section 4 and finally, Section 5 concludes.

2. Foreign domestic workers in Singapore

Our study population is composed of Singapore-based Filipino foreign domestic workers (FDWs). The Philippines is the second largest migrant-sending country and the third largest remittance-receiving country in the world. The concentration of Filipino women as international migrants is striking; 87% of international migrants in the services sector from the Philippines in 2010 were women. Among these, 70% were domestic workers (Bell & Muhidin, 2009). Recent research in the economics of migration has documented several beneficial impacts

[2] Other possible explanations are rooted in behavioral economics, but with such a small sample size and limited survey data, we are unable to investigate further. One relevant thread from the literature on behavioral biases, focusing on nudges and reminders, most often finds that reminders improve individual's behavior (see, e.g., Karlan et al. 2016; Bhargava & Manoli 2015). There is a smaller literature on the possible costs of nudges, such as annoyance costs motivating individuals to unsubscribe from a charity's mailing list, for example (Damgaard & Gravert 2018). A related literature focuses on how scarcity may impair decision-making (Shah, Mullainathan & Shafir 2012; Mani et al. 2013); making savings or the lack thereof salient may have similar effects. Finally, this paper also relates to work on the impact of being surveyed (Zwane et al. 2011); while both the control group and the treatment group were surveyed, the treatment group received additional communication related to savings in the form of invitations to the workshop.

[3] Studies from developed countries cover a range of populations, including children (Alan & Ertac 2018), high school students (Bernheim, Garrett & Maki 2001; Cole, Paulson & Shastry 2016; Brown et al. 2016; Luhrmann, Serra-Garcia & Winter 2018; Bover, Hospido & Villanueva 2018), college students (Gartner & Todd 2005; Stoddard, Urban & Schmeiser 2017), and adults (Skimmyhorn 2016; Choi, Laibson & Madrian 2011; Agarwal & Mazumder 2013; Frisancho 2018). While some of these papers find positive impacts, many have limited or mixed findings. In developing countries, evaluations of financial literacy training for individuals or households and business training interventions for micro-entrepreneurs or farmers also find mixed effects (see Drexler, Fischer & Schoar 2014; Berge, Bjorvatn & Tungodden 2014; Karlan & Valdivia 2011; Field, Jayachandran & Pande 2011; Kaiser & Menkhoff 2018; Sayinzoga, Bulte, & Lensink, 2015 for micro-entrepreneurs and Carpena et al. 2011; Bruhn, Ibarra & McKenzie 2014; Bruhn et al. 2016; Cole, Sampson & Zia 2011; Carpena et al. 2017; Berg & Zia 2017; Calderone et al 2018 on individuals).

[4] We also draw from the general literature on intra-household decision-making in transnational households. For example, De Laat (2014) shows that male Kenyan migrants spend considerable resources monitoring their rural wives, consistent with the existence of moral hazard in wives' spending out of remittances relative to the husbands' preferences. Chen (2006) finds evidence in China that non-cooperative behavior by wives when husbands have migrated is greater for behaviors that are more difficult to monitor.

of remittance flows on household well-being and investments. For instance, households in the Philippines experiencing exogenous increases in remittances become more likely to leave poverty status, to send their children to school, and to invest in new entrepreneurial enterprises (Yang, 2006, 2008; Yang & Martinez, 2005).

Singapore is an interesting case study because it hosts a large migrant worker population, approximately one-fourth of its total population of 5 million (Singapore Department of Statistics 2011), and is also a major receiving country for female migrant labor. As of December 2010, there were 201,000 FDWs and the majority of them came from the Philippines. It has been estimated that one in five households employ a live-in maid (United Nations Development Fund for Women UNIFEM Singapore 2011).

Government regulations differentiate employment contracts of FDWs from other types of employees. FDWs, almost all of whom are women, must be between 23 and 50 years old when first entering Singapore and may work up to the age of 60. The government of Singapore also requires that the women have at least 8 years of formal education. During the period of this study, domestic workers were not covered by standard employment regulations; there were no minimum wage regulations or minimum number of days off. However, the Philippine Overseas Employment Administration stipulated a minimum salary of S$350 (approximately US$278) per month for maids with no or little experience.

3. Financial literacy intervention and experimental design

Existing evidence documents a strong correlation between financial literacy and better savings, investment, and remittance behavior. One challenge in identifying a causal relationship is that people who seek financial education may be different from those who do not. For instance, Meier and Sprenger (2013) show that individual time preference helps explain which individuals choose to become financially literate. Discount rates also explain individual saving patterns, making it difficult to separate the impact of financial literacy and time preferences.

In order to establish a causal effect, our study incorporates random assignment of Filipino domestic workers to financial literacy training. This helps ensure that those who are offered training are statistically indistinguishable from those who are not, making it easier to attribute differences between the groups after the treatment (in terms of saving, borrowing, etc.) to the financial education offer. In this section, after describing the intervention and the experimental design, we verify that the treatment and control groups were comparable at baseline.

3.1. The financial literacy program

The intervention evaluated in this study was implemented in conjunction with a non-profit organization based in Singapore dedicated to providing financial education to female migrant workers. The organization offers courses in management and entrepreneurship training, financial education, computer skills, and marketing and communication. Most of the students are female domestic workers who have migrated to support their families. In 2013–14, 600 women were enrolled in their classes.

At the time of the study, one of the NGO's core programs revolved around peer-based savings clubs. These savings clubs are organized as peer support groups. Each club consists of 10 to 12 members, who meet for three hours, once a month for nine months along with a life-planning coach to discuss savings goals and priorities. The mentors are trained by the NGO and provided with a standardized curriculum. The curriculum is structured around four main topics: (a) Importance of savings and identifying reasons to save, (b) Learning to say "no" to unnecessary expenses, both by the individual and her family members, (c) How to control remittances, and (d) Visualizing and accomplishing financial goals and business plans.

The nine sessions are organized with three keywords in mind: knowledge, goals and rewards. The mentor helps the participants acquire basic knowledge about budgeting, planning, opening bank accounts, investing in productive assets, and interest rates. The savings clubs leverage peer support groups, sustained intervention and non-monetary rewards. In each session, the women announce their goals, challenges, and successes to one another. With the goals in place, they begin saving and motivating each other to put their learning into practice. Each week, four volunteers are tasked with sending out a weekly inspirational text message to their fellow club members. Finally, when goals are achieved and saving targets met, participants are rewarded to reinforce behavioral change. The rewards are generally in the form of certificates and badges.

At every meeting, members are encouraged to show the mentor their monthly bank statement including savings deposits and total bank balance. Some women do not have a bank account and instead ask their employers to save on their behalf. In this case, they are asked to present the mentor with a letter from their employer noting how much they have saved. There is a minimum monthly saving of S$5 required for each member. Additionally, from the first session, members start tracking their expenses on a daily basis. The expense tracking notebooks are checked by the mentors each month.

3.2. Experimental set-up

Most domestic workers in Singapore get at least one Sunday off from work per month, while a majority get alternate Sundays off. On their days off, most women spend the day in religious observance or with friends in malls or parks around Singapore. In addition, some domestic workers choose to attend classes that are offered by various nonprofit organizations, schools or local community centers and churches. Domestic workers often take classes in cooking, baking, nursing, dressmaking, financial education, entrepreneurship, computer skills, and English language. Courses are usually tailored to suit migrant workers with two Sundays off per month.

In August 2010, we began a pilot study, where trained enumerators approached women congregating at malls and parks around the commercial center of Singapore on a Sunday. Women who identified as FDWs were asked to fill out a short baseline survey in exchange for a S $10 top-up phone voucher; 127 women completed the baseline survey. Approximately half of these women were chosen to be invited to join a savings club. At the time of the pilot, we stratified by which Sunday these women were free (the 2nd or the 3rd of the month), interest in a financial education course, whether the respondent reported having financial disagreements with family members and whether she had been living in Singapore for more than 7 years. Due to unevenness in the stratification blocks, 46.5% of the women were assigned to the treatment group. Due to low take-up (12%, 7 students out of 59 invited), only one club was started during the pilot, beginning in October 2010 and running until July 2011.

Incorporating lessons learned during the pilot, we changed our recruitment procedure for the main experiment, primarily approaching women attending computer or cooking classes in two different locations. The baseline survey occurred in January/February 2011. We went to the location of the classes and gave a brief presentation that explained the financial literacy classes. Women who filled out the survey were entered into a lottery for a S$10 phone top-up voucher.

During the main experiment, a total of 281 women were identified and randomized to be invited to a savings club, although only 243 of them completed the baseline survey. Given the low take-up from the pilot, we randomized 60% of respondents to the treatment group, stratifying just by day of interview and preferred Sunday for club meetings. There was sufficient enrollment to start three clubs, all of which began meeting in April or early May of 2011. Twenty-nine women enrolled out of 169 invited (17%). Due to the limited sample size, we analyze the results pooling both pilot and main experiment, controlling for recruitment round.

The initial registration fee for the class, S$55, was paid by the student. However, we offered a full refund if they attended all 9 sessions. To encourage regular attendance, we followed a staggered reimbursement scheme; $10 was refunded after three sessions, another $20 was refunded after the sixth session and the remaining $25 were given back at the end of the last session. The partner NGO has many different classes that are offered simultaneously. In order to avoid confounding the treatment of the savings club from participation in other NGO activities, we held the study classes in a separate location. Besides the location, the clubs involved in the study were no different than the other clubs run by the organization: mentors were chosen from their pool of experienced mentors.

In September 2011, we hired a survey firm to survey all 408 women who had been randomized into either the treatment or control group, from both the pilot and main experiment. These surveys were conducted by telephone, unlike the baseline surveys that had been conducted in person. Respondents were given S$40 grocery vouchers as an incentive to complete the survey. We managed to complete 256 surveys, yielding a relatively high attrition rate. We find no evidence of differential attrition by treatment status (described in detail below); we attribute this high level of attrition to the transitory nature of employment for many of these women and the high rate of changing phone numbers. Attrition was slightly higher from the pilot sample (48%) than the main sample (32%), possibly because more time had passed between surveys.

In both baseline and follow-up surveys, information was collected on individual and household characteristics, employment attributes, asset ownership, decision-making, expenditures, borrowing, savings, and remittances. In addition, following Lusardi and Mitchell (2011), the questionnaire included several simple math-based and problem-solving questions to measure financial literacy as well as a question measuring risk aversion. All survey instruments are available in the online appendix.

3.3. Summary statistics and attrition

Baseline summary statistics are reported in Table 1. Columns 1–3 present means and standard deviations for all women surveyed at baseline (during the pilot and main experiments) and then broken down by control and treatment. Column 4 shows the difference between the treatment and control groups. Column 5 reports the difference conditional on stratification block while Column 6 reports this conditional difference restricting the sample to women who responded to the follow-up survey. While there are a few statistically significant differences between the treatment and control group among women who responded to the endline survey when using robust standard errors, Romano-Wolf step down p-values (not shown in the interest of space) confirm that none of the differences are statistically significant when accounting for multiple hypothesis testing (Clarke, 2016; Romano & Wolf, 2005). The differences in the total amount of savings, while not statistically significant, are worth mentioning. The treatment group reports approximately S$500 more savings than the control group at baseline. Further investigation into the distributions of savings reveals that these differences are driven by three outliers in the treatment group.[5] Our preferred measure of savings, "any reported savings," is not affected by these outliers. In addition, we account for possible differences at baseline by estimating lagged dependent variable models where we control for the baseline measure of the outcome variable, and also provide robustness checks that exclude outliers or control for more baseline characteristics.

The average FDW in our sample is about 36 years old and has spent 7.5 years in Singapore. Since FDW salaries increase with experience, this explains the relatively higher mean monthly salary of S$489 among this group. Their monthly expenses, excluding remittances, are about 40% of their average monthly salary. About half the women have children, averaging 2 children each, and about a third of the women are currently married.

Despite having almost a high school education on average (11.8 years of schooling), the average woman answered less than half of the financial literacy questions correctly. We measure numeric skills through a series of 4 mathematics questions on multiplication, division, and interest rates. We also included a question on probabilities to measure risk aversion: "Suppose we had a jar with three blue balls and one red ball. You are playing a game and you have two choices. You can receive $200 for certain. Or you can pick a ball from this jar with your eyes shut, and if you choose a blue ball you will receive $400. Do you want $200 for certain, or do you want to have a chance of getting $400?" Sixty percent of women chose the option of $200 with certainty.

Approximately 80% of women reported having any savings. We also consider savings in Singapore and savings in the Philippines, but do not report the breakdowns in the table, in the interest of space. Half of the women hold savings in Singapore, and half of the women hold savings in the Philippines (with a quarter of women reporting holding savings in both places). When asked about their control over remittances, 44 percent of women felt that they had no control over how remittances were spent by their families back home and 52 percent of women reported disagreeing with their family members about how to spend remittances within the last 12 months.

Table 2 presents an analysis of attrition. Since we had high attrition, it is important to note that attrition is not different between the treatment and control groups (Column 1) and that it does not appear to be related to demographic indicators (Column 2). The characteristics of those who attrited also seem similar between the treatment and control groups (Column 3). The p-values at the bottom of the table indicate that F-tests testing the joint significance of all covariates in Column 2 and all interaction terms in Column 3 both fail to reject the null hypothesis that all coefficients are 0. As described below, we also estimate Lee (2009) bounds to account for attrition.

4. Results

To estimate the impact of financial education, we focus on intent-to-treat (ITT) estimates. That is, we compare those invited to join a savings club (the treatment group) to those not invited (the comparison group), regardless of whether they enrolled or attended the club. This accounts for the endogeneity of enrollment: comparing those who participated in a club to those who chose not to participate would yield a biased result. Since receiving an invitation to join a club is uncorrelated with participants' characteristics, the ITT estimate gives us the causal impact of the offer to join a club. In Appendix Tables A1–A3 available online, we estimate treatment-on-the-treated (TOT) estimates of the impact of participating in the club for interested readers. The TOT strategy requires the assumption that the instrument (assignment to treatment, randomly assigned) is unrelated to outcomes other than through enrollment in the club. Since being offered financial education may have its own effect, we prefer the ITT estimates to the TOT estimates. We also note that in settings where financial education is voluntary, the ITT estimate may be of greater policy interest. In Section 4.4, we use propensity score matching to estimate the effect of enrolling in a club without having to make the assumption that the invitation itself had no effect.

Let T_i be an indicator variable for whether an individual was invited to join a savings club, i.e. assigned to treatment. Y_i is an outcome of

[5] The distributions of savings for the two groups are very similar when we ignore the three outliers: 25th percentile S$50 to S$50; 50th percentile S$390 to S$400; 75% percentile S$936 to S$1031 and 99th percentile S$6250 to S $5814 for the control group and treatment group, respectively. The three outliers reported S$8314, S$11,494 and S$30,030 in savings at baseline but only the first two report savings at endline and report S$200 and S$1778, respectively. This seems to us to be measurement error.

Table 1
Summary statistics from baseline interview and balance.

	All (1)	Control (2)	Treatment (3)	Difference (4)	Difference conditional on stratification block (5)	Conditional difference among non-attriters (6)
Age	35.98	36.06	35.91	−0.142	0.881	0.854
	(7.910)	(7.526)	(8.217)	(0.836)	(0.820)	(1.064)
Years of Schooling	11.79	11.95	11.67	−0.277	−0.279	−0.111
	(2.071)	(2.122)	(2.028)	(0.220)	(0.228)	(0.301)
Married	0.348	0.371	0.330	−0.0410	−0.0287	−0.0570
	(0.477)	(0.485)	(0.471)	(0.0507)	(0.0542)	(0.0698)
No. of Children	1.025	1.109	0.961	−0.148	−0.0845	−0.140
	(1.280)	(1.310)	(1.257)	(0.146)	(0.156)	(0.205)
Years in Singapore	7.574	7.717	7.468	−0.249	0.227	0.177
	(6.076)	(6.453)	(5.799)	(0.693)	(0.633)	(0.843)
No. of Days Off Each Month	3.259	3.192	3.313	0.120	0.0681	0.256
	(1.966)	(1.632)	(2.196)	(0.207)	(0.195)	(0.304)
Earnings (in SGD)	488.5	526.9	457.7	−69.17	−4.481	−0.0809
	(533.7)	(771.7)	(186.2)	(63.20)	(16.68)	(26.28)
Monthly Expenses (in SGD)	194.6	145.8	235.0	89.19	101.0	10.30
	(673.7)	(130.7)	(901.8)	(74.84)	(71.72)	(27.82)
Fin Lit Questions Correct	0.482	0.491	0.476	−0.0149	−0.00642	0.0445
	(0.333)	(0.340)	(0.328)	(0.0351)	(0.0354)	(0.0453)
Fin Lit Questions Attempted	0.754	0.779	0.734	−0.0450	−0.0424	−0.0231
	(0.338)	(0.328)	(0.345)	(0.0352)	(0.0354)	(0.0452)
Risk Aversion	0.597	0.649	0.550	−0.0990	−0.0848	−0.0480
	(0.492)	(0.480)	(0.500)	(0.0683)	(0.0714)	(0.0922)
Happy with Savings	0.739	0.741	0.737	−0.00389	−0.0139	−0.0397
	(0.440)	(0.440)	(0.442)	(0.0546)	(0.0587)	(0.0756)
Has a Pension Plan	0.396	0.432	0.368	−0.0649	−0.0694	−0.0715
	(0.490)	(0.497)	(0.483)	(0.0542)	(0.0555)	(0.0731)
Control over Remittances	0.556	0.718	0.443	−0.275***	−0.221***	−0.209**
	(0.498)	(0.452)	(0.498)	(0.0604)	(0.0669)	(0.0810)
Disagreements	0.517	0.575	0.472	−0.103*	−0.0605	−0.133*
	(0.501)	(0.496)	(0.501)	(0.0591)	(0.0517)	(0.0721)
Any Savings	0.801	0.807	0.795	−0.0119	−0.0568	−0.0666
	(0.400)	(0.396)	(0.405)	(0.0518)	(0.0524)	(0.0675)
Total Amount of Savings	1053.1	802.5	1250.1	447.6	589.3	978.6
	(2455.1)	(1290.8)	(3067.0)	(296.7)	(418.3)	(595.2)
Any Assets	0.621	0.699	0.563	−0.136***	−0.0745	−0.0629
	(0.486)	(0.460)	(0.497)	(0.0523)	(0.0550)	(0.0703)
N	369	162	207	369	369	239

Note: This table shows baseline characteristics for the individuals in the sample. Each cell of columns 1–3 provides the mean and standard deviation for the listed variable for the entire sample, the control group, and the treatment group, respectively. Column 4 shows the difference between the treatment and control groups with robust standard errors in parentheses. Column 5 shows the difference between the treatment and control groups, conditional on stratification block. Column 6 shows the difference between the treatment and control groups among those who responded to the follow-up survey. *10% ** 5% ***1%.

interest, such as savings, remittances, financial knowledge or behavior. We estimate the following ITT regression:

$$Y_i = \alpha_0 + \alpha_1 T_i + \alpha_2 X_i + \varepsilon_i \tag{1}$$

where α_1 is the parameter of interest, the conditional difference in outcomes for individuals assigned to the treatment and control groups. X_i is a vector of control variables. Many specifications include the baseline level of the outcome variable; we assign this to 0 if it is missing and include a dummy variable indicating missing baseline information. We also include a fixed effect for stratification block, which controls for baseline round as well, and use robust standard errors. In robustness checks, we include additional characteristics measured at baseline. Note that randomization was at the individual level, hence we do not cluster our standard errors.

4.1. Take-up

Table 3 studies take-up of the invitation to join a savings club, using a dummy variable for whether the individual chose to enroll in a club as the measure of take-up in Columns (1) to (4) and the number of classes attended in Columns (5) to (8). The number of classes attended does not condition on enrollment, which explains the average of less than 1 class. Conditional on enrollment, the average is about 5 classes. The first two of each set of columns presents results from regressing take-up on demographic characteristics and survey responses at baseline, conditional on being offered treatment. The next two of each set of columns present a more traditional first-stage regression, including those in the control group and an indicator variable for being invited to treatment. Columns (1) and (5) include a restricted set of control variables. Even numbered columns add baseline income, financial literacy, risk aversion and savings. These control variables are set to zero when missing and indicators for missing observations are included. All columns include fixed effects for stratification block.

The most robust predictor of take-up is years of schooling: an increase in one year of schooling increases take-up by approximately 1–2 percentage points. On a base of 16%, this is an economically significant increase. This finding is consistent with previous research that has focused on the decision to invest in financial literacy (see, for example, Lusardi, Michaud & Mitchell, 2017). Women with more education also attend more classes. We find that women with lower amounts of self-reported savings are more likely to enroll and attend classes. Recall that foreign domestic workers in Singapore have few days off from work each month, a potential explanation for low take-up; however, we find no indication that those with more days off from work were more likely to enroll. Focusing on the first-stage regressions, we confirm the low take-up rates and find that very few individuals from the control group enrolled or attended classes (see the constant terms in Columns 3 and 7).

Table 2
Attrition across treatment and control groups.

			Main effect	Interactions with treatment indicator
	(1)	(2)	(3)	
Assigned to treatment	−0.029	0.0071	0.66	
	(0.049)	(0.056)	(0.64)	
Age		−0.00057	−0.0024	0.00048
		(0.0046)	(0.0100)	(0.011)
Years of Schooling		0.0068	0.025	−0.030
		(0.014)	(0.022)	(0.031)
Married		−0.038	−0.057	0.050
		(0.068)	(0.099)	(0.14)
No. of Children		−0.017	−0.047	0.066
		(0.027)	(0.039)	(0.054)
Years in Singapore		−0.0046	0.00036	−0.0047
		(0.0059)	(0.0098)	(0.012)
No. of Days Off Each Month		−0.00097	0.011	−0.021
		(0.013)	(0.036)	(0.037)
Earnings (in SGD)		−0.000013	0.00081*	−0.00062
		(0.000097)	(0.00048)	(0.00052)
Monthly Expenses (in SGD)		−0.000018	−0.00062	0.00038
		(0.000094)	(0.00045)	(0.00055)
Fin Lit Questions Correct		−0.13	0.073	−0.52
		(0.17)	(0.26)	(0.36)
Fin Lit Questions Attempted		−0.015	−0.20	0.45
		(0.19)	(0.28)	(0.40)
Risk Aversion		0.084	0.20	−0.16
		(0.085)	(0.12)	(0.16)
Happy with Savings		0.072	0.11	−0.035
		(0.069)	(0.12)	(0.16)
Has a Pension Plan		−0.060	−0.030	−0.012
		(0.063)	(0.11)	(0.14)
Control over Remittances		0.084	0.13	−0.039
		(0.072)	(0.11)	(0.16)
Disagreements		−0.043	−0.072	0.052
		(0.077)	(0.12)	(0.14)
Any Savings		−0.0015	−0.10	0.12
		(0.10)	(0.16)	(0.20)
Total Amount of Savings		0.0000036	0.000051	−0.000059
		(0.000018)	(0.000047)	(0.000050)
Any Assets		0.026	−0.039	0.055
		(0.067)	(0.11)	(0.14)
F-test (*p*-value)		0.84		0.61
Observations	408	408		408
R-Squared	0.18	0.26		0.32

Note: This table displays the results from a regression of whether the individual attrited from the sample on a treatment indicator and survey responses at baseline. All columns include stratification block fixed effects. Columns 2 and 3 also include indicators for missing observations for each of the covariates (values of the original variable are set to zero). The p-values at the bottom of the table are from the F-tests of joint significance of all covariates in Column 2 and of all interaction terms in Column 3. Robust standard errors are in parentheses. * 10% ** 5% *** 1%.

4.2 Effects on financial knowledge and behavior

Our survey instrument included questions on financial knowledge, attitudes, and preferences. Table 4 presents OLS estimates from estimating Eq. (1) with these outcome variables. We report both robust standard errors corrected for heteroscedasticity (in parentheses) as well as Romano-Wolf step down *p*-values (in brackets), adjusting for multiple hypothesis testing for all outcome variables in this table (Clarke, 2016; Romano & Wolf, 2005).

Looking at the dependent variable means for individuals in the control group surveyed at endline, presented in the last row of Table 4, we note that 59% of women reported having gathered together their financial information, reviewed it in detail, and put together a specific financial plan in the past 6 months. A majority of the women (72%) also had plans to continue making financial plans in future. On average, participants could answer 65% of the financial literacy questions correctly. Only 55% of respondents could answer a simple question about budgeting but 93% knew what a pension plan was (although only 1% of women had a pension plan). About 30% of the sample regretted making a purchase in the past month.

The intent-to-treat results indicate that assignment to treatment had no statistically significant effect on financial knowledge or behavior for any of these variables, using either the robust standard errors or the Romano-Wolf p-values. In fact, most of the coefficients are negative. That said, it is important to point out that take-up is very low and the standard errors are quite large. While we can calculate the smallest positive effect we can reject (3.64 percentage points for the fraction of financial literacy questions participants answer correctly, for example), the fact that we have only about 16% take-up implies that the treatment-on-the-treated effects this rules out are large (22.7 percentage points). As noted above, estimating treatment-on-the-treated effects also requires assuming that assignment had no effect on non-compliers. Thus, we conclude that these results are inconclusive: we find no evidence that invitations to these workshops had any impact on financial knowledge, but cannot conclude that there was no effect.

A number of robustness checks (available online) confirm this lack of results: Appendix Tables A4 and A5 use probit and logit models for the dummy dependent variables; Appendix Table A6 omits the lagged dependent variable control;[6] Appendix Table A7 includes the baseline demographic variables listed in Table 1 as controls (with indicators for whether the variable is missing at baseline). Finally, Appendix Table A8 presents Lee (2009) bounds to account for attrition. We first use the exact trimming procedure described in Lee (2009), using a Stata command described in Tauchmann (2014). Specifically, the sample is 'trimmed' to achieve equal attrition between the treatment and control groups. Since there is (slightly) more attrition in the control group than in the treatment group, we calculate lower bounds by dropping the participants in the treatment group with the highest values of the outcome variable and upper bounds by dropping participants in the treatment group with the lowest values of the outcome variable. Since the method in Lee (2009) is described for specifications with no control variables, we also use a method similar to Lee (2009) that allows for the inclusion of controls. The outcome variable is regressed on the lagged dependent variable and stratification block fixed effects and then the trimming is done with the residuals. We find no evidence to suggest that the lack of results on financial knowledge or behavior is due to attrition.

4.2. Effects on savings

Next, we study the effect of an invitation to join a savings club on savings. Table 5 displays OLS estimates from estimating Eq. (1) for savings outcomes. Column (1) looks at the impact on the probability of reporting any savings, Column (2) looks at the impact on the natural log of the total amount of savings (adding 1 to avoid dropping 0 s) and Column (3) looks at the impact on the level of savings (in S$). Columns (4)–(6) use the corresponding outcome variables focusing on savings in Singapore (in S$) and Columns (7)–(9) focus on savings in the Philippines (in PhP). Stars in this table indicate significance based on robust standard errors shown in parentheses. In addition, we report Romano-Wolf step down p-values (in brackets) to adjust for multiple hypothesis testing in Columns (2)–(9). We do not include Column (1) in the Romano-Wolf estimation because "Any savings" is an aggregate indicator of the other savings outcomes in the table. All columns control for the lagged dependent variable and a dummy variable indicating missing baseline information.

The results are counterintuitive. We find a negative and statistically significant impact of the invitation to join a savings club on reporting

[6] In the interest of brevity, most of these financial knowledge and behavior outcomes were not asked in the baseline survey; thus only the financial literacy measures, risk aversion, and whether or not participants had a pension plan include lagged dependent variable controls.

Table 3
Predictors of take-up and attendance among the treatment group.

Dependent variable	Enrolled				Number of classes attended			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Assigned to treatment			0.15***	0.15***			0.73***	0.80***
			(0.025)	(0.029)			(0.14)	(0.17)
Age	−0.00076	−0.0019		−0.000032	0.0090	−0.0070		0.0043
	(0.0031)	(0.0028)		(0.0018)	(0.016)	(0.015)		(0.010)
Years of Schooling	0.022*	0.020		0.013*	0.18**	0.16**		0.10***
	(0.012)	(0.013)		(0.0070)	(0.071)	(0.072)		(0.039)
Married	−0.022	−0.076		−0.037	−0.20	−0.52		−0.23
	(0.065)	(0.073)		(0.035)	(0.33)	(0.41)		(0.19)
No. of Children	0.018	0.021		0.0039	0.020	0.031		−0.027
	(0.027)	(0.027)		(0.013)	(0.12)	(0.14)		(0.064)
Years in Singapore	0.00063	0.0027		0.00014	−0.0066	0.016		−0.0012
	(0.0049)	(0.0053)		(0.0025)	(0.030)	(0.030)		(0.014)
No. of Days Off Each Month	0.014	0.012		0.015	0.071	0.074		0.078
	(0.013)	(0.016)		(0.012)	(0.067)	(0.089)		(0.060)
Earnings (in SGD)		−0.00013		0.000012		−0.00091		0.00023
		(0.00014)		(0.000063)		(0.00085)		(0.00043)
Monthly Expenses (in SGD)		0.00031		−0.0000081		0.0026*		0.00017
		(0.00020)		(0.000059)		(0.0015)		(0.00041)
Fin Lit Questions Correct		0.18		0.080		0.66		0.17
		(0.17)		(0.094)		(0.87)		(0.49)
Fin Lit Questions Attempted		−0.012		0.031		−0.086		0.16
		(0.16)		(0.095)		(0.91)		(0.53)
Risk Aversion		−0.12		−0.068		−0.92		−0.43
		(0.10)		(0.047)		(0.57)		(0.27)
Happy with Savings		0.058		−0.012		0.65*		0.12
		(0.077)		(0.043)		(0.39)		(0.22)
Has a Pension Plan		0.057		0.0059		0.48		0.15
		(0.056)		(0.032)		(0.33)		(0.19)
Control over Remittances		0.0067		0.033		0.43		0.38*
		(0.071)		(0.035)		(0.36)		(0.19)
Disagreements		0.026		0.0086		0.055		−0.010
		(0.071)		(0.044)		(0.45)		(0.28)
Any Savings		0.0069		0.039		−0.095		0.17
		(0.089)		(0.049)		(0.47)		(0.30)
Total Amount of Savings		−0.000017*		−0.000012*		−0.000088*		−0.000075**
		(0.0000093)		(0.0000064)		(0.000053)		(0.000038)
Any Assets		0.0067		0.040		−0.15		0.13
		(0.054)		(0.037)		(0.31)		(0.19)
Constant	−0.12	−0.16	0.0084	−0.28*	−1.73	−1.60	0.037	−2.18**
	(0.19)	(0.25)	(0.011)	(0.15)	(1.08)	(1.33)	(0.060)	(0.84)
Observations	228	228	408	408	228	228	408	408
R-Squared	0.39	0.50	0.26	0.36	0.46	0.57	0.30	0.40

Note: This table displays the results from a regression of whether the individual chose to enroll (Columns 1–4) or the number of sessions attended (Columns 5–8) on demographic characteristics and survey responses at baseline. Columns (1), (2), (5), and (6) condition on being offered treatment while the remaining columns include the control group. All columns also include indicators for missing observations for each of the covariates (values of the original variable are set to zero) and fixed effects for stratification block. Robust standard errors are in parentheses. * 10% ** 5% *** 1%.

any savings and on the amount of savings in logs. The magnitudes are meaningful: the probability of reporting any savings falls by 9 percentage points on a base of 89%. The magnitudes for the effect on the amount of savings (either in logs or levels) are quite large – an 88 percent decline or a $422 decrease, respectively, relative to the control group. As noted above, the standard errors are quite large, allowing for

Table 4
Intent-to-treat effect on financial knowledge and behavior.

	Made Financial Plan Past 6 Months (1)	Will Make Financial Plan Next 12 Months (2)	Fin Lit Questions Correct (3)	Fin Lit Questions Attempted (4)	Knowledgable about Pension (5)	Knowledgable about Budget (6)	Risk Aversion (7)	Regret Purchase in Past Month (8)	Has a Pension Plan (9)
Assigned to treatment	−0.0128	−0.0227	−0.0305	0.00193	−0.0486	−0.0291	−0.0451	0.0520	0.00837
	(0.0682)	(0.0639)	(0.0341)	(0.0275)	(0.0460)	(0.0712)	(0.0759)	(0.0660)	(0.00648)
	[0.980]	[0.980]	[0.921]	[0.980]	[0.891]	[0.941]	[0.941]	[0.941]	[0.802]
Observations	253	256	239	239	219	254	215	253	254
R-Squared	0.18	0.14	0.21	0.21	0.18	0.18	0.22	0.14	0.75
Dep var mean (control)	0.59	0.72	0.65	0.92	0.93	0.55	0.66	0.29	0.01

Note: This table displays the results from a regression of financial knowledge and behavior outcomes from the endline survey on whether the individual was assigned to treatment. All regressions include the lagged dependent variable, when available from the baseline surveys (Columns 3, 4, 7 and 9), and a dummy variable indicating whether the baseline response is missing (the variable itself is set to 0), as well as fixed effects for stratification block. Stars (* 10% ** 5% *** 1%) are based on robust standard errors shown in parentheses and Romano-Wolf step down p-values, adjusting for multiple hypothesis testing, are shown in square brackets.

Table 5
Intent-to-treat effect on savings.

	Any Savings (1)	Ln (Total Amount of Savings + 1) (2)	Total Amount of Savings (3)	Any Savings in Singapore (4)	Ln (Savings Amount in Singapore + 1) (5)	Savings Amount in Singapore (6)	Any Savings in Philippines (7)	Ln (Savings Amount in Philippines + 1) (8)	Savings Amount in Philippines (9)
Assigned to treatment	−0.0914**	−0.880**	−422.7*	−0.105	−0.739	−130.6	−0.0394	−0.481	−10,265.6
	(0.0455)	(0.359)	(223.9)	(0.0740)	(0.451)	(138.2)	(0.0736)	(0.757)	(6536.9)
		[0.050]	[0.238]	[0.426]	[0.396]	[0.594]	[0.653]	[0.624]	[0.426]
Observations	256	231	231	231	231	231	231	231	231
R-Squared	0.24	0.29	0.25	0.23	0.21	0.13	0.19	0.19	0.27
Dep var mean (control)	0.89	5.70	1280.39	0.51	3.06	429.38	0.56	5.56	28,721.65

Note: This table displays the results from a regression of savings outcomes from the endline survey on whether the individual was assigned to treatment. All regressions include the lagged dependent variable and a dummy variable indicating whether the baseline response is missing (the variable itself is set to 0), as well as fixed effects for stratification block. Stars (* 10% ** 5% *** 1%) are based on robust standard errors shown in parentheses and Romano-Wolf step down p-values, adjusting for multiple hypothesis testing, are shown in square brackets.

much smaller effects; but the size of these point estimates also helps motivate further investigation into the mechanisms behind this impact. The point estimates for savings in Singapore and savings in the Philippines are always negative, but not statistically significant.

A number of robustness checks support these findings. Probit and logit models for the dummy dependent variables are presented in Appendix Tables A9 and A10. Appendix Table A11 omits the lagged dependent variable control, while Appendix Table A12 includes baseline demographic controls. The results are also robust to dropping the three outliers in the treatment group discussed above (Appendix Table A13). Appendix Table A14 presents Lee (2009) bounds to account for attrition, using both methods described above. Lower bound estimates are generally significant, and for more savings outcomes than in Table 5. Upper bound estimates are usually negative, as in the main results, but not statistically significant.

Previous literature has often found that individuals with low baseline levels of financial literacy exhibit larger increases in knowledge as well as larger changes in behavior (see, e.g. Cole, Sampson & Zia, 2011). In Appendix Table A15, we find no evidence of differential effects on financial knowledge or behavior when we break up the sample by initial levels of financial literacy, but in Appendix Table A16, we find that the negative effect on savings is driven by individuals with below median levels of baseline financial literacy. In the next subsections, we explore several other outcome variables to shed more light on these findings.

4.3. Effects on other outcome variables

Recall that these outcomes, including the savings outcomes, are self-reported, making it difficult to determine whether treatment assignment affected actual savings or whether it simply affected whether women *report* that they have savings. Determining whether actual savings decreased is almost an impossible task absent bank account information. Nonetheless, we begin by considering that actual savings may have fallen if participants had greater monthly expenses or sent more money home in remittances. We find small, often negative, statistically insignificant changes in these variables at endline (Columns 1 and 2 in Table 6). We find no change in whether participants report that remittances were spent on particular budget items, such as education, food, entertainment, mobile phone bills, etc. (results left out in the interest of conciseness, but available upon request). We also find no evidence that individuals report less (liquid) savings because they are substituting to other forms of investments. Specifically, we find no change in whether they report any assets (Column 3 in Table 6) or whether they report specific types of assets, such as a house, land, farm, livestock, vehicles, machinery or other assets (results available upon request).

Without corroborating evidence that actual savings fell, we next consider whether women are simply reporting less savings, either because they now believe that they have less savings or because they are more cautious about reporting savings to strangers in a survey. We cannot rule out the latter possibility, but believe it is unlikely since we find no differences in how much income they report (Column 4 in Table 6). It is not obvious whether they would believe assets in Singapore to be safer (given the legal structure in Singapore) or whether they would believe assets in Singapore to be less safe (since the surveyor is in Singapore), but it is worth noting that we found similar effects of financial education on savings in Singapore and in the Philippines in Table 5.

We are left with treatment assignment leading women to report they have less savings, believing it to be true. One possible explanation is that financial education makes these women pessimistic about being able to achieve their savings goals and they give up. Specifically, anecdotal evidence indicates that these women often save with the goal of returning to the Philippines and starting a small business, a goal that will require a fairly substantial amount of capital. Again, we cannot fully rule out this explanation, but we find no evidence that they report different savings goals (results available upon request).

The last explanation that we investigate is whether the treatment assignment led women to seek more accurate information about whether they have any savings and how much they have, in their own bank accounts in Singapore or held with family members in the Philippines. Recall that the class fee was paid upfront by participants who chose to enroll and then reimbursed in a staggered manner over the nine meetings. The need to come up with the S$55 fee may have made women invited to join a club more aware of their own financial situation. Anecdotally, many women expressed concern about coming up with the S$55 since this amounted to more than 10% of their monthly earnings. We find two pieces of evidence that support this explanation, although we acknowledge that these results are, at best, suggestive. First, we see a marginally significant decrease in the number of bank accounts these women report having, including accounts other people have on their behalf (Column 6 in Table 6), even though there is no decline in whether women have any bank accounts. While this could be a financially-motived decision (to consolidate bank accounts and/or minimize account fees), this seems unlikely since the average woman in the control group has only 1 account (see last row of Table 6). Only one respondent out of 212 at endline reported more than 2 accounts. An alternate explanation is that women realized that they had one account less than they originally believed. For example, this could be because they had no balance in an old account or because their family members did not have savings in their accounts in the Philippines. Supporting this speculation, we find that women invited to a savings club are marginally more likely to report disagreeing with their family members

Table 6
Intent-to-treat effect on other outcomes.

	Monthly Expenses Not including Remittances (1)	Monthly Remittances (2)	Any Assets (3)	Earnings (4)	Any accounts (5)	Number of accounts (6)	Has Full Control Over Remittances (7)	Has Disagreements Over Spending (8)
Assigned to treatment	−1.950	−5.994	−0.0112	−15.49	−0.0371	−0.171*	−0.0265	0.0371*
	(8.558)	(17.06)	(0.0598)	(11.88)	(0.0600)	(0.101)	(0.0699)	(0.0221)
	[0.941]	[0.941]	[0.941]	[0.604]	[0.941]	[0.356]	[0.941]	[0.386]
Observations	246	244	255	253	247	212	248	254
R-Squared	0.15	0.14	0.28	0.26	0.20	0.23	0.25	0.18
Dep var mean (control)	101.76	273.22	0.66	489.21	0.80	1.13	0.60	0.01

Note: This table displays the results from a regression of additional outcomes from the endline survey on whether the individual was assigned to treatment. All regressions include the lagged dependent variable, when available from the baseline surveys (all except Column 2), and a dummy variable indicating whether the baseline response is missing (the variable itself is set to 0), as well as fixed effects for stratification block. Stars (* 10% ** 5% *** 1%) are based on robust standard errors shown in parentheses and Romano-Wolf step down p-values, adjusting for multiple hypothesis testing, are shown in square brackets.

about how to spend remittances they send back (Column 8 in Table 6).[7]

4.4. Separating out the effect of the club and the invitation

Our intent-to-treat analysis described above gives us the causal effect of the invitation to join a savings club, with the fee reimbursed in the manner described above. Since enrollment is endogeneous, estimating the causal impact of the course itself requires making additional assumptions either about who chooses to enroll or about the effect of the invitation itself on those who choose not to enroll. For example, two stage least squares (treatment-on-the-treated) estimates of participating in a savings club, using treatment assignment as an instrument requires assuming that the invitation itself had no effect on behavior (other than through whether the participant enrolled). The mechanism for which we find the most support – that treatment assignment motivates women to seek out information about their own financial situation – could be a result of the course, but it could also come from having filled out a detailed survey, followed by an invitation to a financial literacy program, even if the participant ultimately decides not to enroll (perhaps because of the fee). Thus, in this section, we attempt to separately identify the effect of enrolling in the course from the effect of being invited to the course but not enrolling, using propensity score matching methods similar to Ibarra et al. (2017).[8] Ibarra, McKenzie, and Ortega (IMO, hereafter) study a financial literacy program offered to almost 75,000 randomly chosen credit card clients of a Mexican bank. To deal with a very low take-up rate of 0.8% leading to very imprecise intent-to-treat estimates, IMO use their rich administrative data to predict take-up for those in the control group using propensity score matching and then compare participants from the treatment group who enrolled in the course with similar participants from the control group who were not offered the course. The experimental variation from the randomization helps satisfy the concern with propensity score matching about why participants with similar propensity scores did not enroll – those in the control group were not invited.

We modify IMO's procedure to take into account our substantially smaller sample size and our limited pre-intervention data (self-reported financial behavior from the baseline survey). Specifically, we regress outcomes at endline on indicators for enrollment and treatment assignment, and use propensity score matching methods to account for the endogeneity of enrollment. The results are presented in Table 7 for the outcomes that have statistically significant results in our main regressions (see Appendix Tables 21–23 for rest of the outcome variables from Tables 4 to 6). In Panel A, we begin with benchmark regressions that do not include propensity scores, but instead control for all the baseline demographic and financial behavior measures used in the take-up regressions in Table 3. For Panels B and C, we use the coefficients from Column 2 in Table 3 to predict enrollment for all individuals, regardless of whether they were invited to a workshop and then control for this propensity score in the regression. Panel B controls for this propensity score linearly, while Panel C includes indicators for 10 percentage point ranges of the propensity score distribution. In Panels B and C, we bootstrap the standard errors.

Before discussing the results, it is important to be clear that while suggestive, these results are speculative. Our very small sample size and limited pre-intervention data make it difficult to fully believe the identifying assumptions for this strategy – selection into enrollment is likely not going to be determined by only these observable characteristics. Any omitted characteristics correlated with both the outcome variables and an individual's propensity to enroll, conditional on these observable characteristics, will bias these estimates. That said, the unobservable characteristics that spring to mind are likely positively related to both enrollment and savings behavior, leading to a positive bias for the coefficient on enrollment. As before, assignment to treatment should not suffer from these biases as it was randomly assigned.

The results suggest that the impact we estimated in Tables 4–6 are driven by the invitation to the club and not the club itself. The coefficient on being assigned to treatment is consistently of the same sign as our intent-to-treat effects (negative for the savings outcomes and the number of accounts and usually significant; positive for intra-household disagreements and only marginally significant in one specification). Relative to the negative impact on savings of the invitation, enrolling in the course has a positive (and sometimes marginally statistically significant) effect. The magnitudes of these coefficients are such that the two effects would cancel each other out: being invited to a savings club but not enrolling appears to have a negative effect on savings, while being invited and enrolling in it has no effect. This is consistent with the results from our exploration into mechanisms in Section 4.3: the invitation may have made women more aware of their financial situation, but those who paid the S$55 fee to join the class report no more or no less savings while those who did not join the class report having less savings.

[7] We conduct the same robustness checks for Table 6 as we did with Tables 4 and 5. Appendix Tables A9 and A10 use probit and logit models for the dummy dependent variables in Table 6. We lose many observations due to the stratification block fixed effects (and the low take-up), but the impact on intra-household disagreements is robust to the probit specification. All columns in Table 6 include lagged dependent variable controls except for the monthly remittances (Column 2) which was asked in a different way at baseline. These results are robust to omitting the lagged dependent variable (see Appendix Table A17), but we have insufficient power when we include baseline characteristics (see Appendix Table A18). In addition, Appendix Table A19 breaks up Table 6 by initial level of financial literacy and finds no evidence of differential effects. Finally, Appendix Table A20 presents Lee (2009) bounds to account for attrition. As in the savings results, one of the bounds is statistically significant but the other is not for the number of accounts and intra-household disagreements outcomes.

[8] We thank an anonymous referee for this suggestion.

Table 7
Propensity score matching estimates.

	Any Savings (1)	Ln (Total Amount of Savings + 1) (2)	Total Amount of Savings (3)	Number of accounts (4)	Has Disagreements Over Spending (5)
Panel A					
Enrolled	0.0961	0.359	−320.6	0.239	−0.0181
	(0.0611)	(0.569)	(516.4)	(0.200)	(0.0354)
Assigned to treatment	−0.159***	−1.066**	−210.4	−0.134	0.0364
	(0.0595)	(0.452)	(267.1)	(0.112)	(0.0296)
Panel B					
Enrolled	0.0997*	0.619	−286.6	0.289	−0.0159
	(0.06)	(0.608)	(576.33)	(0.21)	(0.03)
Assigned to treatment	−0.113*	−1.016**	−378.4	−0.231**	0.0388*
	(0.06)	(0.436)	(212.6)	(0.098)	(0.02)
Propensity score	0.161	1.419	565.4	0.0580	0.0407
	(0.132)	(1.081)	(854.564)	(0.244)	(0.063)
Panel C					
Enrolled	0.0898*	0.954	10.71	0.251	0.000257
	(0.067)	(0.723)	(650.391)	(0.251)	(0.04)
Assigned to treatment	−0.110*	−1.112**	−465.3	−0.252**	0.0371
	(0.06)	(0.478)	(261.05)	(0.103)	(0.023)
Propensity score controls	Yes	Yes	Yes	Yes	Yes

Note: This table displays the results from a regression of various outcomes from the endline survey on whether the individual enrolled in the course and whether the individual was randomly assigned to treatment. All regressions include the lagged dependent variable and a dummy variable indicating whether the baseline response is missing (the variable itself is set to 0), as well as fixed effects for stratification block. Panel A includes controls for baseline characteristics used in the even columns of Table 3 (along with dummies indicating missing values). Panel B includes, as a control variable, a propensity score estimated using the coefficients in Column 2, Table 3, while Panel C includes dummy variables indicating 10 percentage point ranges of the propensity score. Stars (* 10% ** 5% *** 1%) are based on robust standard errors shown in parentheses in Panels A and on bootstrapped standard errors shown in parentheses in Panels B and C.

These results are also consistent with qualitative information from the savings club attendance logs. The women who enrolled were motivated, attending 82% of class meetings, with 65% attending all nine meetings. At each meeting, the mentor would record how much savings the participant had accumulated, usually from viewing bank statements. Comparing recorded savings from one meeting to the next, we find that on average savings are increasing and reported savings amounts are highly correlated one meeting to the next, but the median change is 0 and the increase is not statistically significant. Many women report lower savings over time; it would not be out of the question for the intervention to lead to a reduction in actual savings. However, we also find little correlation between savings reported in the baseline survey and savings reported at the first meeting participants attended; the change from baseline to first meeting report averages -$317 and is negative for 75% of the women for whom we can match this information, even though there are on average 3 months between these two reports. While speculative, since we do not have similar data from the control group, this suggests a role played by differences between the survey responses and reports to mentors which required bank statements.

5. Conclusion

In this paper, we evaluated the impact of offering a financial education program to female Filipino foreign domestic workers in Singapore. The program focused on the importance of saving and controlling spending and remittances. We documented three main findings. First, we find very low take-up for the course and that women with more years of schooling are more likely to enroll. Second, we find that assignment to treatment has a negative effect on whether women report having any savings and on the amount of savings they report. We explore several channels that may drive this result, finding no evidence for many explanations. We find some support for the invitation to the course having increased awareness of savings and ones' own financial situation. We find that women invited to the course report having fewer savings accounts and disagreeing more with family members about how remittances are spent. We also find suggestive evidence that the effects are driven by those women who chose not to enroll in the class.

These results have two implications worth noting. First, invitations to workshops can impact behavior even for those who do not attend. The invitations were not particularly intrusive (a few text messages), but may still have had an effect, possibly because they were combined with a detailed survey on finances and the consideration of where to find S$55 for the fee. The second implication relates to intra-household bargaining. It is somewhat surprising for limited bargaining power to have an effect on savings for these women since they are the primary earners in these households and have full immediate control over the income (it is paid to them in Singapore). That said, intra-household dynamics are likely to change slowly. We conclude that intra-household bargaining norms can limit the impact of financial education programs.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.econedurev.2019.101920.

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