Pre-analysis plan: Short-run effects of enhanced pre-departure training for Overseas Filipino Workers

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1 Introduction

This pre-analysis plan (PAP) outlines the analysis of the short-run effects of enhanced pre-departure trainings for temporary labor migrants from the Philippines.[1] More specifically, it concerns female Overseas Filipino Workers (OFWs) migrating to Saudi Arabia and Hong Kong as household service workers (HSWs). It focuses on short-term outcomes for which data will be collected in survey wave 2 (wave 1 is the baseline survey). It will primarily focus on the effects of the enhanced PDOS on the situation of the migrant in the destination country and financial decision-making. In a later stage of the project a second PAP will outline the analysis for the medium- and long-term effects, which will have a stronger focus on feedback effects from migrants on their families in the Philippines. This analysis will be conducted with data collected in survey waves 3 and 4.

The hypotheses we present below are at the center of our research interest and we will give most weight to the results that we find for this part of the analysis. These results have a confirmatory character and the analysis will therefore be conducted with special scrutiny and we will discuss in detail how the analysis will be conducted. The PAP will be archived before we start data collection of wave 2.

In addition to the confirmatory analysis we will use the data for additional exploratory analysis that is not detailed out beforehand. The exploratory analysis allows for more surprise findings and gives a chance to explore research questions that only become evident once the data is available. Due to the exploratory nature we will treat these findings differently and will give less weight to them. We will also separate the publication of confirmatory and exploratory findings.

2 Overview of the study

This study aims to investigate the effects of enhanced pre-departure orientation seminars (PDOS) for Filipino migrants using a randomized control trial. The study consists of two parts: the evaluation of PDOS for permanent migrants and the evaluation of PDOS for

[1] This PAP draws on similar PAPs by Almeida et al. (2012) and Finkelstein et al. (2010).

temporary migrants (OFWs). This pre-analysis plan only covers the evaluation of PDOS for OFWs.

Every Filipino labor migrant is required by law to attend a pre-departure training (PDOS). For OFWs, these trainings are provided directly by labor recruiters and NGOs. In addition to the PDOS, HSWs are required to attend a Comprehensive Pre-Departure Education Program (CPDEP), which is provided by the Overseas Workers Welfare Administration (OWWA).

The PDOS primarily aims to provide OFWs with information that help them to address the difficulties commonly encountered by OFWs in adjusting in their first months of life on a foreign jobsite. More recently, a welfare perspective was introduced to protect workers and help them achieve their objectives in seeking work abroad. However, despite its long history, many problems persist in the PDOS. The latest effort to assist OFWs in adjusting to work in a foreign country was addition of the CPDEP that consists primarily of language training and familiarization, plus additional cultural orientation and stress management modules.

This study will use a randomized control trial to evaluate the effectiveness of enhanced training modules inserted into the CPDEP, and two additional behavioral interventions to improve welfare and financial decision-making. It will survey roughly 2,000 household service workers migrating to Saudi Arabia (KSA) and Hong Kong (HK) and their families over a period of two years after the training takes place. The results of the evaluation might be used in policy formulation at OWWA and will be highly informative for deriving best practices for other migrant sending countries.

Description of the interventions

Four interventions were implemented that aim to a) improve financial decision-making and b) increase migrant wellbeing abroad. For the financial literacy domain, we focus on two interventions: a savings module and savings reminders. The two interventions in the welfare domain are a group intervention relying on spreading knowledge from HSWs with prior experience abroad and a behavioral intervention to improve the relationship between HSW and employer (gift cum photo). The savings module and the intervention relying on experienced HSWs are group interventions while the savings reminders and gift cum photo are individual interventions. Group interventions were implemented in CPDEP classes randomly assigned to these interventions.

Implementing the interventions as part of the CPDEP and not of the PDOS had also logistical reasons. About 75 accredited PDOS providers deliver the PDOS at their own locations. The CPDEP in contrast is centrally administered by OWWA for all HSWs. Arabic classes (for those bound for the Middle East) are run at 11 OWWA centers all over the Philippines; however, 91% come through the OWWA offices in Manila. For those bound for Hong Kong, Cantonese training is offered in 2 OWWA centers, but 93% take the CPDEP in the OWWA training facility in Manila (CPDEP training by OWWA is run at three locations in Manila). These centralized locations allow for a convenient venue to survey the target 2,000 OFW sample and run group interventions.

Savings Module: The savings module in the current PDOS template is a fairly comprehensive presentation that covers three aspects: setting family goals including a financial timeline, the right formula for savings (set aside savings first before expenses), and investing (in financial assets as well as in a business). In practice, however, the savings module in the PDOS given by labor recruiters, NGOs and industry associations takes the form of inviting bank representatives who give a short talk on their remittance and deposit products and assist HSWs to open accounts and give pointers to HSWs in dealing with banking offices overseas. While the information and products offered by the banks might be of practical relevance for the HSWs, objective information on financial decision-making and financial products is generally seldom provided.

The savings module is short and focuses only on a few messages. In terms of content, the module will offer direct guidance on: a) prioritizing and distinguishing between expenditures that are musts and those that are merely wants, b) creating a joint financial plan with their family prior to departure, including the amount and use of remittances, c) an exercise in making a budget with a template showing how much money should be saved to reach a specific savings goal after two years is also provided, d) the importance of keeping savings in a bank directly under the control of the HSW. The pedagogy is interactive, e.g. using role-play to illustrate how to deal with financial requests from relatives and neighbors, and asking HSWs who have already had experience working abroad to share the financial results of their time abroad and lessons learned. A short comics-style handout that HSWs can bring with them to use as a reminder is also provided

Savings Reminders: The literature indicates that receiving savings reminders increase the likelihood of reaching a savings goal. HSWs assigned to this intervention receive a biweekly reminder with an encouragement to save the targeted share of their income. Savings reminders are sent via text messages to mobile phones. Messages are varied to prevent them from being "tuned out". The reminders will also include factors important in maximizing remittances (e.g. the cost of different remittance avenues), and reminders to avoid financial scams and loan sharks, etc.

Sharing of Experiences: In this group intervention, those in the class who have already experienced being HSWs in the specific countries are encouraged to share their experiences, problems faced, ways of coping with problems, etc. The sharing is grouped around 3 themes: work experiences (including hours worked, specific concerns such as the large size of houses to be cleaned and number of family members served, hours of rest/sleep etc.), relationships with the employer (including cross-cultural communications, salary issues, food issues, etc.) and relationship with the families left behind. These are the areas that generally give rise to problems. To maximize the impact on HSWs, the module relies heavily on the experience of "ex-abroads" (the term generally used by the HSWs themselves to refer to those who have already worked abroad), as shared by the ex-abroads themselves. This first hand information sharing is supposed to increase the credibility of the content. This intervention tries to manage expectations regarding what the HSWs will face when they actually work abroad and provide them with strategies to cope with problems.

The limitation of the sharing intervention is that it relies heavily on the presence of ex-abroads with relevant experience in the class. Ex-abroads attend the CPDEP if it has been five years since they first went abroad or if they are going abroad to a country different from their previous deployment. There are many ex-abroads in KSA CPDEP due to the fact that there are many Arabic-speaking countries, with roughly comparable conditions of employment for HSWs. This, however, is not the case for HK CPDEP, since conditions among Asian destinations vary considerably among each other. The sharing intervention is therefore only implemented for the KSA sample.

Gift with Photo: Many former HSWs report that they are treated very poorly ~~be~~ by members of the employer's household and that they are not respected as a human being. This intervention aims to affect the relationship between HSW and the employer by changing the starting situation and thereby creating a different trajectory of the relationship. HSWs assigned to this intervention are given a pack of dried mangoes (a specialty in the Philippines) and encouraged to give them as a small introductory gift to their employer's family. They are also encouraged to show a family photo (including the household service worker) to signal to the employer's family that the HSW is a human being with a family and a personal background too. This intervention also signals the HSW's good intentions for the relationship with the employer.

Selection of the interventions

The specific interventions that will be evaluated have been chosen after extensive discussions with industry participants (PDOS providers, labor recruiters and NGOs), focus group discussions with HSWs themselves and OWWA staff and a review of the literature on OFWs. In collaboration with OWWA, the research team conducted a pre-survey among returning and prospective HSWs to assess their needs and problems encountered (for returning HSWs).

Sample selection

In 2013, 1.85 million Filipinos left the country to work abroad under temporary labor contracts. Almost 25% went abroad for the first time, while 75% had prior experience in working abroad. Of the roughly half a million new hires, one-third (164,396) were HSWs.

The decision to focus research on HSWs was based on the following reasons: HSWs are the most vulnerable of all labor migrants being almost entirely female, coming from the lowest socio-economic strata of society, young (the average age is 31 years old), with low educational attainment (usually high-school or less), working at the bottom of the salary scales (typically US$400 per month), and employed under circumstances that make them particularly vulnerable (they generally live alone in their employer's residence, and in the case of many Middle Eastern countries, have limited access to communications to call their families or interact with other Filipinos).

The research sample focuses on newly hired HSWs bound for KSA and HK, the two largest destinations for OFWs in the Middle East and Asia, respectively. KSA is by far the largest destination country for Filipino OFWs, taking in 35% of all newly hired OFWs in 2013. Hong

Kong, while the largest destination for newly hired OFWs in Asia, accounted for 5% of all new hires in 2013. Overall the sample consists of 2,000 HSWs, divided into 1,200 bounded for KSA and 800 for HK.

As explained in the next section, the number of HSWs interviewed in each class varies due to the specifics of the randomization. Sampling of participants is based on registration lists for the CPDEP that were available to the research team beforehand. For each class, a random sample equal to the targeted number of interviews (plus a backup list in case a HSW on the primary list could not be interviewed) is drawn. HSWs from one recruiter are assigned to the same class. Thus intra-class correlation could be high if the sample collected in a given class consists primarily of HSWs sent by one recruiter. The sampling process is designed to limit the number of HSWs from a particular recruiter sampled in a given class.

Baseline interviews started on 26 May 2014 and were completed on 15 August 2014.

Randomization

In order to establish causality, HSWs are randomly assigned to the different treatments. Treatment assignment takes places at the group level for the savings module and the sharing intervention and the individual level for the gift and photo treatment and the savings reminder. This section explains the considerations behind the randomization for the various treatments.

Group-level interventions

Randomization of the savings module and sharing interventions are randomized at the group level. Groups either receive no intervention, the savings module, or the sharing intervention (only KSA sample). No group receives both interventions. Randomization of group level interventions is mainly based on the following statistical and logistical considerations:

- Each treatment arm should have the same sample size to maximize statistical power. For the KSA sample we have three treatment cells (control, sharing, savings module). For the Hong Kong sample we have two treatment cells (control, savings module). We divide the total number of interviews equally to these five treatment cells, which results in 390 observations per treatment cell.
- Maximize the number of sessions from which observations are drawn to avoid loss in statistical power due to a small number of clusters. Six parallel classes take place per cohort for the KSA sample, 4 parallel classes take place for the Hong Kong sample.
- Interviews can only be conducted in the morning before classes start and have to take place before the interventions. The time frame and the number of surveyors available, limits the number of interviews that can be conducted by cohort to 40. Surveyors work in teams of five. The number of interviews per class should therefore be multiples of five.
- The number of sessions affected by group level interventions should be limited due to a) constraints in the number of trainers conducting the interventions, and b) limiting the disturbance of regular CPDEP sessions. We therefore interview 15

OFWs in treated sessions and 5 or 10 OFWs in control sessions. The time of trainers should also be efficiently allocated by allowing trainers to conduct more than one training at a given location on a given day.

- The CPDEP is mandatory for all HSWs. Hence, non-compliance with treatment assignment is of no concern.
- Spillover effects and control group contamination may arise if HSWs in a treatment session share information with migrants in a control session. This is of limited concern in our setting as HSWs interact almost exclusively with other HSWs from the same recruiter. Within cohorts, HSWs sent by one recruiter are generally assigned to the same class.

To create a randomization design that incorporates these considerations, we define three cohort types for both subsamples. Each cohort type is characterized by the assignment of classes to group-level treatments (see Appendix). For example, in Type F cohorts in the KSA sample, one class is assigned to the savings module, one class to the sharing intervention and 2 classes to the control group. Each cohort will be assigned a cohort type. Within cohorts, we randomize interventions to classrooms. For example, in two cohorts of Type F, a specific classroom can first be assigned to the savings module and the second time to the control group. Group level interventions are therefore randomized by date and by classroom. Neither HSWs nor the recruiters can influence the assignment of HSWs to classrooms and therefore specific treatments.

The following table shows the total number of interviews and clusters by treatment arm. For logistical reasons, there are slight deviations from 390 in the number of interviews conducted by treatment arm. As fewer interviews are conducted in control classes, the number of clusters is higher in this arm.

Table 1: Number of interviews and clusters by subsample and treatment arm

	KSA		Hong Kong	
	Interviews	Clusters	Interviews	Clusters
Control group	403	69	423	54
Savings module	384	26	384	26
Sharing	397	27		
Total	**1184**	**122**	**807**	**80**

Individual-level interventions

The assignment for the gift treatment takes place during the interview. These interventions are randomly assigned at the individual level and the randomization is independent from the randomization at the group level. For every classroom, our RA creates an assignment sheet based on the registration list for this classroom. Every HSW has a 50% chance of be assigned

to the gift treatment. Interviewers carry out the interventions as indicated on the assignment sheet.

The savings reminder is the only intervention that is assigned and implemented after the CPDEP session. A prerequisite for the savings reminder is the availability of a mobile phone number that can be attributed to the HSW. In February 2015, 375 phone numbers were available to the research team and the randomization was carried out among these individuals. The availability of baseline data at the time of randomization allowed a blocked randomization design, where randomization was carried out within 16 cells formed by the variables a) HSW received savings module training, b) HSW has a child, c) HSW has a college degree, d) HSW has a bank account.

Table 2: Number of HSWs assigned to individual treatments

	KSA		Hong Kong	
	Yes	no	yes	No
Savings reminder	96	100	86	93
Gift treatment	597	587	372	435

Data sources

The measurement of all outcomes of interest will be based on surveys conducted with HSWs and their families remaining in the Philippines. All survey instruments will be extensively pretested before the actual survey. In total, there will be four rounds of data collection. All of them will be computer-assisted to facilitate tracking over time and improve accuracy through automated routing and error checks.

OWWA administrative data

Administrative data from OWWA and POEA contain basic demographic information on all OFWs. POEA collects data relating to deployment of OFWs. OWWA data consists of basic demographic information on OWWA members plus indications of the skill/position of OFWs, relationship and municipality of beneficiaries, contract duration, city/country of destination and salary. These data has been used to plan the evaluation and obtain a general picture of the situation. In addition to membership data, OWWA also has data resulting from operations, e.g. data on those applying to attend the language training, including classroom assignments. As explained above, these registration lists have been used for sample selection and treatment randomization.

Baseline surveys with migrants and their families

During round one of data collection, prospective HSWs have been surveyed in personal interviews immediately before attending the PDOS. In order not to disrupt the CPDEP classes, all interviews had to be collected prior to the start of classes at 8 am. HSWs typically

arrive at the training site from 5:30 am onwards. This survey collects baseline information about the prospective HSW. Conducting the interview before the training ensures that the willingness to participate in the survey is not influenced by the treatment and that responses are not primed by the interventions. To ensure high re-contact rates for the future rounds of data collection, HSWs will be asked to provide a phone number, permanent email address and other contact information as well as contact information of family members who remain in the Philippines.

The family member, who is identified as the main contact person in the Philippines, will be contacted by phone at the end of the interview to ensure the correctness of the contact information and to schedule a baseline personal interview with family members to obtain information regarding the situation of the household. These baseline interviews with the family members take place shortly after the interview with the migrant. Some households can only be interviewed with some delay due to the remoteness of their location.

Follow-up survey with HSWs

Round 2 will measure the short-run effects of the intervention. It will take place around eight months after the departure of each migrant. Since direct contact with the migrants in the destination countries is extremely difficult (no permission to make calls by employer, no cellphone available, roaming cost incurred for the migrant), a knowledgeable family member in the Philippines will be interviewed instead to provide proxy information about the most important indicators. While these indicators are likely to be measured with error, the proxy interviews help to keep attrition as low as possible. In addition, we will reach out to a subsample of HSWs in Hong Kong who have generally better communication opportunities. Obtaining direct information at least for a subsample allows us to judge the quality of the proxy interviews and detect potential systematic biases. The main analysis will be carried out with data collect by the proxy interviews. The comparison between directly and indirectly obtained information will be used to interpret the results.

The interviews will mainly focus on a) communication with peers in the Philippines, b) wellbeing and treatment by the employer, c) saving and remittance sending behavior.

Future follow-up surveys

Round 3 and 4 of data collection will take place roughly 12 and 24 months after the departure of the HSW and will be conducted with both HSWs and their families in the Philippines. The current PAP focuses only on the short-term impact and does thus not cover round 3 and 4.

3 Hypotheses

The various interventions might influence different outcome dimensions. We will collect a rich dataset that will allow us to test a number of hypotheses. Most importantly the evaluation does not only seek to understand the overall impact on the various outcome dimensions but also to understand the causal chain that leads to these effects. The causal chains that we have in mind vary with the type of intervention. Interventions aim at improving knowledge, managing expectations, increasing motivation, and influencing the behavior of the employer.

We can group our hypotheses along two dimensions. First, we can group them along the causal chain:

- Impact on knowledge, motivation, and expectations
- Impact on behavior
- Impact on outcomes

Second, we can group our hypotheses along outcome domains. In general HSWs seek work overseas largely for financial reasons:

- The family has no adequate sources of income – the spouses may not be able to find jobs that pay sufficiently to feed, educate and improve the family's standard of living, this is sometimes expressed as having too many dependents, and not having enough income to save
- To save – the family wants to accumulate funds for investment to augment income in the future, or to ensure the future educational needs of dependents so they can complete highschool or go to college
- To repay debts or to fund extraordinary family needs like the illness of a parent or a child.

Financial outcomes are therefore an important outcome dimension. The outcomes in the welfare domain are not themselves reason for seeking to work overseas. They are, however, a necessary condition to achieve financial outcomes and maintain human wellbeing. Many problems stand in the way of a "successful" stint as HSW overseas including expectations regarding the work to be done by an HSW, relationships with employers and relationships with the family left behind. Many issues results from cross-cultural differences, including religion, and socialization (norms, customs, ideologies, values).

The interventions that are evaluated attempt to raise the impact and hence the effectivity of the PDOS. We can frame the hypotheses along thematic dimensions of financial outcomes and individual wellbeing, and map these against the interventions.

Within each domain we test whether a specific treatment has an impact. We condense the information from various indicators related to a specific hypothesis in order to reduce the number of hypotheses to be tested and therefore to increase statistical power. Where feasible we will create meaningful indicators based on various questions in the questionnaire. Where the creation of such an indicator is not possible we construct standardized treatment effects as suggested by Kling, Katz, and Liebman (2007) and employed by Finkelstein et al. (2010) and Almeida et al. (2012) (see description in Section 4: Power calculations and multiple hypotheses testing).

Table 3: Overview of effects on outcomes

Outcomes / Modules	Financial outcomes	Individual wellbeing
Savings Module	Primary impact: increase in savings, involvement of the family in financial planning, and other indicators of favorable financial outcomes	Secondary impact: Increase in sense of wellbeing by increasing financial resources
Savings Reminders	Primary impact: increase in savings, and other indicators of favorable financial outcomes	Secondary impact: Increase in sense of wellbeing by increasing financial resources
Sharing Module	Secondary impact: Better treatment in the area of remuneration (payment on time, following salary agreed upon in the contract, assistance in remitting salary) that in turn translates into better financial outcomes	Primary Impact: Managing expectations regarding conditions abroad leads to faster adjustment, better work performance, higher employer satisfaction, better treatment, higher sense of wellbeing
Gift cum Photo	Secondary impact: Favorable initial treatment may extend to better financial outcomes	Primary impact: Better first impression, leading to favorable initial treatment and different trajectory of the relationship

Impact on outcomes

Communication

Hypothesis 1.1: Being exposed to the **sharing intervention** increases the communication intensity between HSW and the family in the Philippines.

Hypothesis 1.2: Being exposed to the **gift intervention** increases the communication intensity between HSW and the family in the Philippines.

Indicators:

- B.2 – frequency of text messaging
- B.3 – number of calls in the past four weeks
- B.4 – length of last call
- Missing answers will be treated as "Missing". In case of a high share of missing values for one variable, we will only base the outcome on the others.

Specific controls from baseline survey:

- Planned communication frequency (CO3)

In order to increase statistical power we aggregate the various outcomes in this domain into a standardized treatment effect. This aggregation allows us to investigate whether the intervention has any effect on communication intensity.

Treatment by employer

Hypothesis 2.1: Being exposed to the **sharing intervention** leads to better treatment by the employer.

Hypothesis 2.2: Being exposed to the **gift intervention** leads to better treatment by the employer.

Indicators:

- C.2 – subcategories: a) shouting, b) physical violence, c) threats, d) sexual harassment
- C.4 – employer provides enough food
- Daily working hours (calculated as C.8 – C.7 – C.10)
- Create indicator if C.11>1 (no weekly rest day)
- D.4 – receives salary on time
- Deductions from salary (D.5 <=2)
- C.12 – is allowed to leave house on her own
- C.1.1=yes – Employer is described as "good" or "okay" person
- "Don't know" and "Refused" will be treated as "Missing"

In order to increase statistical power we aggregate the various outcomes in this domain into a standardized treatment effect. We will furthermore look whether there is an effect on the subjective description of the employer (C.1.1). However, due to the more subjective nature of this outcome, we will place less weight on this outcome.

Specific controls from baseline survey:

- Has talked to employer (E10)

Financial outcomes

Hypothesis 3.1: Being exposed to the **savings module** has a positive average impact on the amount of savings accumulated. Effects will be small in the short run but larger in the longer run.

Hypothesis 3.2: Receiving text messages with **savings reminders** has a positive average impact on the amount of savings accumulated (Note: Given that savings reminders only started shortly before wave 2, we expect them to have a large impact for outcomes measured in wave 3 and 4.)

Indicators:

- Savings set aside since arrival in destination
  - D.17 – create continuous variable by using the mean of each category
  - All reported values will be converted to PHP
  - Values of 60,001 PHP and more will be treated as 60,001 PHP
- Savings set aside since arrival by family

- D.18 – create continuous variable by using the mean of each category
- All reported values will be converted to PHP
- Values of 60,001 PHP and more will be treated as 60,001 PHP

Specific controls from baseline survey:

- Savings target (F37)

We will also estimate the effect on combined savings to look at the overall amount of savings and use the two individual variables to test whether the distribution of savings (by the HSW vs. the family) has changed. An important caveat of this analysis is that we use savings as reported by the family. Theoretically, HSWs could systematically underreport the amount of savings to the family, in order to decrease financial demands. Such underreporting might be a function of the tested interventions. For the subsample that we will interview directly, we will investigate whether systematic differences exist between the answers given by a family member and answers given by the HSW herself.

Hypothesis 3.3: Being exposed to the **savings module** has an ambiguous average impact on the amount of remittances since migrants are encouraged to save more themselves.

Hypothesis 3.4: Being exposed to the **text messages** has an ambiguous average impact on the amount of remittances since migrants are encouraged to save more themselves.

Indicators:

- Frequency of remittances
  - D.10 – number of times HSW has sent remittances
- Amount sent last time
  - D.11 – create continuous variable by using the mean of each category
  - All reported values will be converted to PHP
  - Values of 60,001 PHP and more will be treated as 60,001 PHP

Specific controls from baseline survey:

- Has discussed amount of remittances (F43)
- Has discussed use of remittances (F45)
- Different opinions on the use of remittances (F46)

We have no priors about the effects on remittances sent. The encouragement to agree with the family on a financial plan and to save more personally, makes the expected effects ambiguous.

Mechanisms

Any effects of the treatments on the various outcome domains must result from a change in behavior either on the side of the HSW, the family in the Philippines, or the employer. The second set of hypotheses investigates whether such changes in behavior can be observed in order to learn more about the mechanisms behind potential effects on the outcomes. Due to the use of proxy interviews, we restrict our attention to behaviors that are likely to be known to family members.

Hypothesis M.1: Being exposed to the **savings module** increases the probability that HSWs coordinate on a financial plan with their family before departure

Hypothesis M.2: Receiving text messages with **savings reminders** increases the probability that HSWs coordinate on a financial plan with their family

Indicators:

- Financial coordination
- D.1 – HSW and family members discussed and agreed on a budget (treated as ordinal variable)
- D.2 – HSW and family members discussed and agreed on the amount of remittances (treated as ordinal variable)
- D.3 – HSW and family members discussed and agreed on the use of remittances (treated as ordinal variable)
- Missing answers will be treated as "No"

Specific controls from baseline survey:

1. Has discussed amount of remittances (F43)
2. Has discussed use of remittances (F45)
3. Different opinions on the use of remittances (F46)

We will create two indices of financial coordination. The first one is an index of successful coordination and is the sum of "1…discussed and agreed" answers to questions D.1-D.3. The second one is only an index for the coordination attempt and is the sum of "1…discussed and agreed" and "2…discussed but could not agree" answers to questions D.1-D.3

Hypothesis M.2: Being exposed to the **savings module** decreases demands for extra money from family members

Indicators:

- Demand for extra remittances
  - D.13 – Family members asked for extra money (yes/no)
  - Missing answers will be treated as "Missing"

Specific controls from baseline survey:

- Has discussed amount of remittances (F43)
- Has discussed use of remittances (F45)
- Different opinions on the use of remittances (F46)

Hypothesis M.3: Being exposed to the **sharing intervention** leads to more accurate expectations.

Indicators:

- Expectations regarding work to be performed
  - C.5 =1 (Type of work as expected)
  - C.6 = 1 (Amount of work as expected)

Specific controls from baseline survey:

- Expected daily hours of work (E14)
- Has worked as domestic helper in the Philippines (E3)
- Has talked to employer (E10)

We will create an index that is based on the sum of answers "1" (as expected) on questions C.5 and C.6.

4 Estimation

Estimation of main effects

We estimate all equations in the joint sample of KSA and HK. We will then also look into the effects for the two subsamples separately. To obtain the effect of the savings module intervention, we estimate equation (1) and to obtain the effect of the sharing intervention we estimate equation (2):

$$\boldsymbol{Y_{i,t=2} = \beta_0 + \beta_1 TSM_i + X_S'\theta + \varepsilon_i \quad if\ TSI_i = 0} \quad (\mathbf{1})$$

$$\boldsymbol{Y_{i,t=2} = \beta_0 + \beta_1 TSI_i + X_S'\theta + \varepsilon_i \quad if\ TSM_i = 0} \quad (\mathbf{2})$$

$Y_{i,t=2}$ is the outcome measured in wave 2. TSM_i is an indicator for being exposed to the savings module and TSI_i is an indicator for being exposed to the sharing intervention. β_1 will thus provide the treatment effects of interest. Importantly, we only use the sample that was not assigned to the other group intervention. Thus, the control group consists of HSWs that did not receive any group intervention. X_S' is a vector of pre-treatment covariates that are expected to be strongly correlated with the outcome. For analysis in the joint sample, the covariates include a dummy for the HK subgroup. Their inclusion in the model should reduce the error variance and improve balance. We include in this vector age, education, an indicator for having a child, time since arrival in the destination (log days), an indicator for frequent Internet use, an indicator for having a bank account. The inclusion of hypothesis-specific

control variables – in particular pre-treatment measures of the respective outcomes – is indicated at the respective hypothesis description.

Finally, to test the effect of the individual interventions, the gift intervention and the savings reminder, we estimate the following equations. In these specifications, we control for receiving the group interventions:

$$\boldsymbol{Y_{i,t=2} = \beta_0 + \beta_1 SR_i + \beta_2 TSM_i + \beta_3 TSI_i + X_S' \theta + \varepsilon_i} \qquad (3)$$

$$\boldsymbol{Y_{i,t=2} = \beta_0 + \beta_1 GI_i + \beta_2 TSM_i + \beta_3 TSI_i + X_S' \theta + \varepsilon_i} \qquad (4)$$

SR_i is an indicator whether an individual received the savings reminder; GI_i is an indicator whether an individual received the gift intervention.

Calculation of standard errors

We will use Huber-White standard errors clustered at the class level.

Estimation of heterogeneous impacts

All analysis will be done separately for each destination country.

Power Calculations and Multiple Hypotheses Testing

The availability of preliminary baseline data allows us to estimate intra-class correlations (ICC) for important baseline characteristics. Intra-class correlation is below 0.05 for all covariates and not significantly different from zero in many cases. We see the highest values for "Salary deduction" (0.038) and "Has already talked to employer" (0.041), which are most likely the result of the clustering by recruitment agencies.

We use an ICC of 0.2 for our power calculations, which is more conservative than the baseline covariates suggest. As the size of our clusters vary, we use a coefficient of variation in cluster size of 0.5. We furthermore assume 30% attrition. We provide the power calculations using the sample size of the two subsamples. Only for the savings reminder, where we have to operate with a much smaller sample, we provide power calculations based on the joint sample.

The power calculations suggest that we will be able to detect medium (0.5 sd) and large (0.8 sd) effects for all interventions with a probability of close to one at the 10 and 5% significance level (see Cohen, 1988 for a discussion on effect sizes). Power to detect small effects (0.2 sd) is about 0.4 at the 10% significance level for group level interventions. Power to detect small effects for the gift treatment and the savings reminder is 0.76 and 0.48 respectively.

Table 4: Intra-class correlation of baseline covariates

	Hong Kong	KSA
Age	0	.027
Married (0/1)	0	.018
Has children (0/1)	0	0
Speaks Tagalog at home (0/1)	.012	.023
Max. highschool degree (0/1)	.019	0
College degree (0/1)	.008	0
Worked 6 months ago (0/1)	.014	0
Worked as domestic helper in PH (0/1)	0	.036
Born in Pangasinan prov. (0/1)	.005	0
Born in NCR (0/1)	0	.017
Does NOT use internet (0/1)	.023	0
Has personal savings (0/1)	.029	0
Salary deduction (0/1)	.038	0
Has talked to employer (0/1)	.041	.005
Knows someone at destination (0/1)	0	0
Knows language spoken at destination (0/1)	0	.003
Destination city is Riyadh (0/1)	-	.030

For some variables we collect pre-treatment information and expectations already at baseline. Including these pre-treatment measures in the estimation reduces error variance and therefore increases statistical power. For other covariates where no pre-treatment information is available (as those variables are not defined for non-migrants), we will include a set of covariates that are likely predictors of the outcome to increase statistical power. We provide additional estimates that assume the availability of pre-treatment covariates with a predictive power of 0.3. Including those covariates increases statistical power by roughly 0.03.

Table 5: Power-calculations for group interventions

Group-level intervention (Treatment arm size 390)				
	10% significance level		5% significance level	
	Normal	With pre-treatment	Normal	With pre-treatment
0.2 st.dev.	0.42	0.44	0.30	0.33
0.5 st.dev.	0.97	0.98	0.95	0.97
0.8 st.dev.	0.99	0.99	0.99	0.99

Note: Intra-class correlation: 0.2, coefficient of variation of cluster size 0.5, predictive power of pre-treatment measures 0.3, and 30% attrition.

Table 6: Power-calculations for gift treatment

Individual-level intervention (Control and treatment group size 390)				
	10% significance level		5% significance level	
	Normal	With pre-treatment	Normal	With pre-treatment
0.2 st.dev.	0.76	0.79	0.65	0.69
0.5 st.dev.	0.99	0.99	0.99	0.99
0.8 st.dev.	0.99	0.99	0.99	0.99

Note: Assumed predictive power of pre-treatment measures 0.3, and 30% attrition.

Table 7: Power-calculations for savings reminder (joint sample)

Individual-level intervention (Control and treatment group size 185)				
	10% significance level		5% significance level	
	Normal	With pre-treatment	Normal	With pre-treatment
0.2 st.dev.	0.48	0.51	0.35	0.38
0.5 st.dev.	0.99	0.99	0.98	0.99
0.8 st.dev.	0.99	0.99	0.99	0.99

Note: Assumed predictive power of pre-treatment measures 0.3, and 30% attrition. The sample size is based on the joint sample of HSWs in HK and KSA for whom we have a cellphone number.

To account for problems with multiple hypothesis testing we follow the approaches by Finkelstein et al. (2010) and Almeida et al. (2012). As described above, we group our outcomes into domains and estimate the effects on an overall index or we estimate standardized treatment effects within each domain.

To estimate the standardized treatment effects we follow the procedure of Kling et al. (2007). We normalize each outcome within a domain by subtracting the mean of the control group

and dividing by the standard deviation of the control group. Let Y_k be the kth of K outcomes, let μ_k be the control group mean, and let σ_k be the control group standard deviation. The normalized outcome is $Y_k^* = (Y_k - \mu_k)/\sigma_k$. The summary index is $Y^* = 1/K \sum_K Y_k^*$. We reverse the signs for adverse outcomes, so that a higher value means a more beneficial outcome. These estimates show us whether there is an overall effect of an intervention on an outcome domain.

We will look at the effects on the individual indicators to examine which dimensions are driving a potential overall effect. We will treat the results with extra care if we do not find an overall effect but an effect on an individual indicator. In order to account for multiple hypotheses testing, we will apply the Westfall and Young step-down resampling methods for the hypotheses tests for the effects on individual indicators.

For the investigation of heterogeneous treatment effects we will follow the recommendations of Fink et al. (2010) and employ the Benjamin and Hochberg step-down procedure. We will only investigate treatment effect heterogeneity for the overall effects and not for the individual indicators to reduce the number of hypotheses.

Strategies to deal with attrition

Attrition is a serious concern in any longitudinal study and for a study that seeks to track migrants over space and time in particular. To keep attrition as low as possible we employ four strategies. First, in the baseline interviews migrants will be asked to provide their contact details in the destination country, a permanent email address, contacts in social networks, as well as contact details of family members who remain in the Philippines. In case we fail to re-contact a migrant in the destination country directly, the survey company will contact her family members to obtain updated contact information. The survey company will update the contact information of the HSW and household every 2 months. Also, the respondent (household or HSW) will be in contact with only one enumerator to avoid confusion and build trust. Second, to ensure a sustained willingness to participate in all survey rounds, migrants will already be informed before the baseline interview that their participation in future rounds is highly desired. Third, an incentive to take part in the various rounds of the survey, interviewed migrants and their family will be given a token after the interview. Fourth, in case we still fail to re-contact a migrant in the destination country, a knowledgeable family member of that migrant in the Philippines will be interviewed instead to provide proxy information on the most important indicators. For this round, the analysis will be based on these proxy interviews and direct interviews with HSWs will only be conducted with a subset of HSWs in Hong Kong.

Nevertheless, selective attrition remains a serious concern. In a first step, we will estimate whether attrition itself is a function of one of the interventions. This might in particular be true for the sharing and the gift intervention. If an F-test of joint significance of all treatment indicators does not reject the null of no effect on the probability to successfully conduct an interview with the household at the 5% level, we will conduct the analysis without adjustments for attrition and assume that attrition is random conditional on the covariates

included in equation (1). If we find a significant relationship between treatment status and attrition we will construct non-parametric bounds on our treatment estimates as suggested by Behaghel et al. (2014). For this purpose, we will collect information on all contact attempts.

5 Literature

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6 Appendix

Table 8: Cohort types

Number of Type F1 cohorts:	26
Number of Type G1 cohorts:	1
Number of Type C1 cohorts:	3

30 cohorts

10 weeks

possible interview days: mo, we, fr

KSA sample (3 cohorts/week)	Number of classes per cohort			Number of interviews per class			Total number of interviews per cohort				Total number of classes (clusters)			Total number of interviews		
	Savings	Sharing	Control	Savings	Sharing	Control	Savings	Sharing	Control	Total	Savings	Sharing	Control	Savings	Sharing	Control
Type F1: 1 class Savings, 1 class Sharing, 2 classes control	1	1	2	15	15	5	15	15	10	40	26	26	52	390	390	260
Type G1: 1 class Sharing, 5 classes control	0	1	5	0	15	5	0	15	25	40	0	1	5	0	15	25
Type C1: 4 classes control	0	0	4	0	0	10	0	0	40	40	0	0	12	0	0	120
											26	**27**	**69**	**390**	**405**	**405**

Number of Type F2 cohorts:	13
Number of Type C2 cohorts:	7

20 cohorts

10 weeks

possible interview days: mo, tu, th, fr

HK sample (2 cohorts/week)	Number of classes per cohort			Number of interviews per class			Total number of interviews per cohort				Total number of classes (clusters)			Total number of interviews		
	Savings		Control	Savings		Control	Savings		Control	Total	Savings		Control	Savings		Control
Type F2: 2 classes Savings, 2 classes control	2		2	15		5	30		10	40	26		26	390		130
Type C2: 4 classes control	0		4	0		10	0		40	40	0		28	0		280
											26		**54**	**390**		**410**