



## Financial Inclusion:

Promoting digital payments among female migrant factory workers

### Pre-Analysis Plan

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*This plan outlines the hypotheses to be tested and specifications to be used in the impact analysis of the digital payment applications training project. This pre-analysis plan (PAP) is being submitted to the AEA RCT Registry after the baseline survey and the implementation of our intervention have been completed, but before the endline survey is completed and data analysis takes place.*

## Intervention Overview

### I. Intervention Description

#### 1. Context

India has made substantial progress towards financial inclusion over the last decade. Data from the 2017 Findex survey,<sup>1</sup> which interviewed 3000 lower and middle-class participants in India, reveals that 80% of people in India have access to bank accounts, an impressive increase from the 2014 level of 53%. This increase, along with the expansion of pro-poor financial schemes such as the *Pradhan Mantri Jhan Dan Yojana* and *Pradhan Mantri Suraksha Bima Yojana* has brought millions into the fold of formal financial services. Still, much progress remains to be made. While many have access to bank accounts, a bulk of these accounts lies dormant. This prevents the development of a safety net suitable for absorbing the costs of emergencies and job loss. In this context, digital options have been increasingly seen as simple and relevant tools to bridge the financial gap in India, especially in the context of women empowerment.

Our study is also related to the dramatic rise in internal migration in low-income countries. Worldwide, internal migration affects an estimated 763 million people (WEF, 2017); most of this phenomenon is characterized as rural-to-urban. One of the primary drivers of the decision to migrate is to be able to remit money back to migrants' origin households.<sup>2</sup>

Many of the female workers at Shahi Exports, our industry partner, are rural migrants who move temporarily to the city due to financial distress in their villages. According to our exploratory research and baseline data, they tend to remit a large share (close to 50%) of their wages back home. The standard modus operandi is to cash out their salary from their bank account and transfer the money through over-the-counter agent/shopkeeper services (92% of our sample at baseline). By saving time and money, digital payment services have the potential to improve their financial wellbeing.

While these workers are aware of the existence of digital payment platforms, our scoping work and baseline data show that they do not know how to use digital payment apps and are wary of losing money on these platforms (i.e., the level of trust in digital transfer technology is low). In addition to this, workers face other issues that prevent them from signing up for and using digital payment applications. Workers' mobile phones must be registered with their bank account in order for them to use the Unified Payments Interface (UPI)<sup>3</sup> applications. Additionally, in our piloting work, technological problems, such as server issues, limit workers' ability to download and use applications.

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<sup>1</sup> <https://globalfindex.worldbank.org/>

<sup>2</sup> <https://migrationobservatory.ox.ac.uk/resources/primers/migration-and-development/>

<sup>3</sup> UPI (Unified Payments Interface) technology allows users to create a unique id (a UPI id) linked to their bank account, which, in turn, is linked to their mobile number. People can send money from one UPI id to another.

In this context, this research studies the impact of training female migrant workers in India to use digital payment applications. Our team, a collaboration between Good Business Lab and IDinsight, in conjunction with Shahi Exports, designed a randomized controlled trial that studies the effect of a workplace intervention - training sessions to use a digital payments application - on take-up and use of digital payment application. We are interested in studying the extent to which training programs of differing intensities may be effective and cost-efficient in improving take-up.

We randomize the implementation of digital payments application training sessions in 19 hostels attached to Shahi Exports factories. We vary the type of training on digital payments that workers receive. In one arm of the study, we provide individualized support, and in the other, we conduct larger group classroom training. In both treated arms, workers are taught how to download, set up, and use (send and receive money) digital payment applications.

We hypothesize that individualized training would be more effective in increasing take-up, compared to classroom training. Hence, the intensive intervention aims to overcome trust and cognitive barriers, as well as purely technological barriers.

Our intensive intervention seeks to achieve three goals:

- 1) Build knowledge of how to use digital payment applications
- 2) Develop familiarity with the applications
- 3) Develop awareness of the application's value proposition (convenience, cheapness)

We expect these three goals to build trust and increase the use of digital payment applications.

## 2. Intervention

The first part of the intervention consists of a hands-on training session on digital payment applications. The two treatment arms are:

### 1) Classroom treatment:

This treatment arm included information on digital payments with facilitated sign-up, opportunities for practice, and financial incentives. A trainer, as well as one trained assistant, conducted the training sessions, which consisted of batches between 20 and 30 participants and lasted one hour.

### 2) Individualized treatment:

In this treatment arm, the number of workers in each training batch was smaller than the classroom treatment arm (5 participants per trainer). This allowed trainers to dedicate more attention to each of the participants to help them solve technical issues such as errors while downloading the app, setting up a UPI id, and creating a password. These training sessions were more resource-intensive, as two trainers were assisted by 3-4 trained assistants who provided individual attention to each of the participants.

During both types of training sessions, participants created a UPI id and learned how to send money using the BHIM app (See Appendix: About the digital payments technology). Once participants set up their UPI accounts, we sent Rs. 50 to each participant and encouraged her to practice sending money to the person next to her. Facilitators were available to troubleshoot as needed. The second part of the intervention was SMS reminders. Just before participants received next month's salary, we sent a

reminder SMS to workers in the treatment groups. This message reminded workers to use digital payment apps to remit money, making the digital payment method salient to the respondent, around the time she would be most likely to use it.<sup>4</sup>

### 3. Study contribution

We see this study as generating direct and important new results in the following areas:

The study provides evidence in the research area of digital payment application usage in India. It may generate evidence in explaining the relatively low take-up rate of digital application technologies. We are interested in seeing to what extent and how efficient different kinds of training programs and intensities may be in improving digital application take-up. Some of the barriers that participants face during the intervention stage do not depend on the intensity of the training sessions but rather on purely technological barriers (mobile phone linked to bank account). Hence, findings could show how many people are locked out from using digital payment applications because of specific institutional matters.

Next, we would generate results on the effectiveness of digital payments training in the Indian context. Our study would be among the first to try to understand how to teach customers to use UPI technology – past work has focused on the impact of mobile wallets. The difference between UPI and mobile wallets is that UPI allows users to link directly to their bank account and transfer money from their bank accounts, while a mobile wallet requires users to regularly deposit money into their electronic wallets (which could create additional barriers for the adoption of this technology).

As UPI is easier to set up and use,<sup>5</sup> it is more scalable. In fact, it has overtaken mobile wallets in terms of demand in India.<sup>6</sup> Finding a scalable way to teach people how to use UPI thus has a large potential for impact. Organizations like the National Payments Corporation of India can use these results to train a large number of people to use UPI-based apps.

Our study also addresses the low take-up of digital payments in a traditionally underserved group, women. Innovations for Poverty Action's "Building Resilience Through Financial Inclusion: A Review of Existing Evidence and Knowledge Gaps"<sup>7</sup> highlights the need to "improve take-up of mobile money so that the benefits of digital transfers are more evenly distributed". In our study, we focus on women, who constitute an under-served and under-studied population. FINDEX 2017 estimates from India indicate that women are less likely than men to use digital payments - 14% of females above the age of 15 made a digital payment in the past year, compared to 26% of males above the age of 15. While Lee et al. (2018) train both female and male migrants to use digital payments, the population is not balanced –

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<sup>4</sup> The message in English is the following: <name>, thanks for attending the BHIM training session this week. We hope you can use BHIM to send money home and for other payments. Here's a video to help you remember the steps: <https://www.youtube.com/watch?v=wRTdbIn9Cdg>

<sup>5</sup> <https://www.livemint.com/Money/A1bTvyBsfMmZeNu6oSfozJ/4-reasons-why-UPI-may-overtake-mobile-wallets-soon.html>

<sup>6</sup> <https://qz.com/india/1227756/upi-vs-mobikwik-freecharge-ola-money-india-government-e-payments-gateway-is-killing-e-wallets/>

<sup>7</sup> <https://www.poverty-action.org/sites/default/files/publications/Building-Resilience-Through-Financial-Inclusion-January-2019.pdf>

70% of the migrants in their study are male. Our study would be one of few focusing on the take-up of digital payments exclusively by women.

Power calculations suggest that it is unlikely that the study is powered to detect the impact on financial strain, financial control, as well as workplace outcomes such as retention and absenteeism. However, if the effect sizes are large enough, we see our paper as contributing to the following literature:

First, we contribute to the literature that documents the effects of using digital payments. For example, Suri and Jack (2014) show that households that received remittances through mobile money were better able to cope with financial shocks;<sup>8</sup> Lee et al. (2018) show positive outcomes for households that receive remittances through digital payments – such households save more, respond better to shocks, and have improved health, education and agricultural productivity. In this paper, we will measure the effects of digital payments on migrants, focusing on a new set of outcomes (perception of financial strain and workplace outcomes), which may help create a compelling case for employers to pay for such interventions.

Second, we will add to evidence on financial strain and workplace outcomes (productivity and retention). Mani et al. (2013) show that the poor have diminished cognitive function compared to the well-off. They suggest that this is because the poor expend mental resources to address poverty-related concerns, leading to lower cognitive capacity for other tasks. Based on this work, we contend that lowering the perception of financial strain would improve people's cognitive performance, which would lead to increased retention and attendance in the workplace.

We also hypothesize that the increased cash-in-hand (due to money saved on fees) and increased transparency into and control over finances would help workers save more. If true, this would imply that workers' salaries would cover expenses for a more extended period, improving their perception of employment at Shahi, and this may encourage them to stay at their jobs longer.

Thirdly, we contribute to the literature on how behaviourally-informed interventions can lead to changes in the desired behavior. We hypothesize that having greater access to consumption and saving patterns (through checking their bank balance and seeing past transactions through the app) will make these patterns more salient to the workers, allowing them to plan better, shift consumption away from temptation goods, and save more. We thus contribute to the literature on how to build a savings habit through a behavioral intervention - making the state of finances more salient.<sup>9</sup>

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<sup>8</sup> The mechanism proposed was that mobile money reduced the transaction cost of sending and receiving remittances, which was why households facing an income shock were more easily able to receive money and smooth consumption. Additionally, given that mobile money is a safe method of saving, households may have been able to facilitate self-insurance.

<sup>9</sup> <https://www.ideas42.org/wp-content/uploads/2015/05/Applying-BE-to-Improve-Microsavings-Outcomes-1.pdf>

## II. Theory of Change

### 1. Needs assessment

Most female migrants working at Shahi Exports in Bangalore remit a large share of their wages home - the migrant workers we have surveyed at Shahi Exports, Karnataka have migrated from rural areas in Odisha, Jharkhand, and Chhattisgarh so as to earn money to send home.

The research questions emerge from the following worker needs and constraints.

First, these workers face significant behavioral barriers (lack of trust and fear of losing money) that prevent them from using digital payments apps to remit money. Instead, they opt for costlier and more inconvenient remittance options, such as sending money through agents/shopkeepers or delivering cash in person. While most workers are aware of both the digital payment platforms and their value proposition, the behavioral barriers identified above prevent them from using these platforms.

Second, in addition to the behavioral barriers mentioned above, workers face other issues that prevent them from signing up for and using digital payments applications. A major constraint is that workers' mobile phones must be linked to their bank accounts in order for them to use UPI applications. Additionally, in our piloting work, technological problems, such as server issues, have limited workers' ability to download and use applications. We anticipate that most of the technological barriers faced while downloading and signing up on the digital payment applications won't be removed with our intervention.

We are interested in seeing to what extent and how efficient different kinds of training programs and intensities may be in improving take-up, in the face of these behavioral and "last mile" technological barriers.

### 2. Theory of Change

The following figure (**Figure 1**) presents an overview of the path required for our intervention to achieve the short term effects we seek by mapping intervention inputs to outputs. Below, **Figure 2** lays out the theory of change with the long term mechanisms we expect.

Figure 1: From inputs to outputs

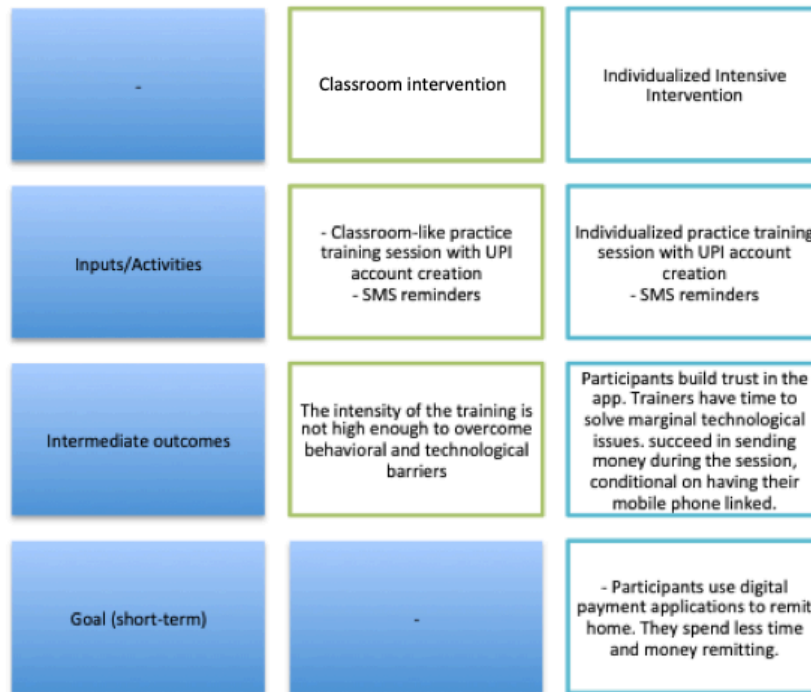
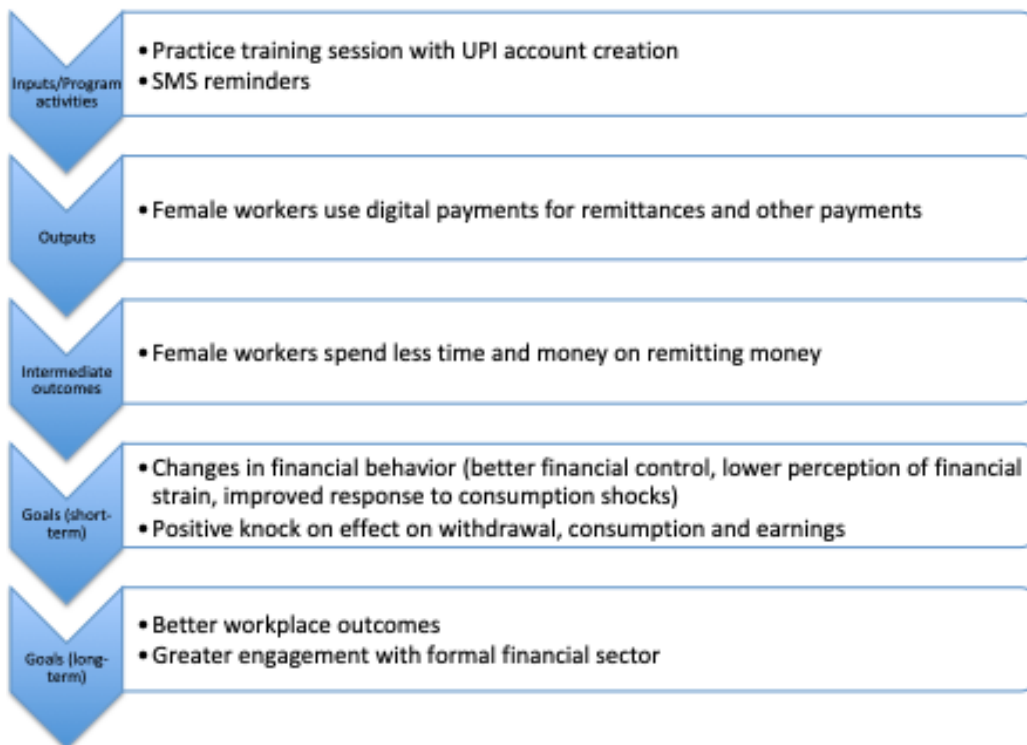


Figure 2: Theory of Change





### 3. Proposed mechanisms and hypotheses

Our proposed mechanisms for short-term effects are:

#### *Removing purely technological barriers*

We hypothesize that the intensive training session treatment arm will increase the take-up rate relative to the control and large-group intervention arm. More resources and time per participant will allow participants to solve purely technical issues, and hence will increase take-up and usage.

#### *Removing trust and cognitive barriers*

We hypothesize that conditional on success (sending and receiving money through the application during the session), the intensive training treatment will remove trust and cognitive barriers to using digital payment applications for remittance better than classroom intervention. The mechanism here is that participants involved will have more dedicated time to practice during the session, and hence will feel more confident in using this method when remitting money on their own.

#### *Coping with institutional barriers*

We hypothesize that none of the treatment arms will be effective at improving the take-up rate dramatically, given that we expect the bulk of the obstacles to stem from institutional issues. The proposed mechanism is that participants whose mobile phone is not linked to their bank account will be locked out of the treatment, regardless of the intensity of the training session. However, treatment can incentivize participants whose mobile phone is not registered. They may try to link their account to their mobile number following the training sessions.

Further down the theory of change, we also expect the following to happen, although our study is constrained by limited power to detect effects in these areas:

#### *Perception of financial strain and financial control*

We hypothesize that using digital payments to send money reduces the sender's perception of financial strain and improves financial agency, through four mechanisms: saving money on remittance fees, being able to send money easily and as-needed, and having greater access to consumption and saving patterns.

1. Saving money on remittance transaction fees: We hypothesize that the money saved from remittance fees (even though it is a small amount) will increase worker savings and reduce their perceived financial strain. Additionally, we expect the fact that they saved money would make participants feel as though they have greater control over their finances, thereby reducing perceived financial strain.
2. Being able to send money easily and as-needed: We hypothesize that knowing that they can use digital payments to send money to their families at any time (instead of spending time to go to an agent/shopkeeper) would also reduce workers' perception of financial strain, as they can provide assistance to their families at short notice.
3. Having greater access to consumption and saving patterns: Digital payment apps allow you to check your bank balance and look into past digital payment transactions. We hypothesize this will improve workers' understanding of their consumption and saving patterns, allowing them to

plan their finances better and so reduce perceived financial strain (both through saving more money and feeling like they have more control over their finances).

#### *Safety net and response to consumption shocks*

We hypothesize that using digital payments to send money expands the safety net and improves workers' capacity to respond to consumption shocks, as they are able to receive money easily and as-needed to reach a network that they could not have relied on otherwise.

#### *Workplace outcomes*

We hypothesize that using digital payments to send money would affect workplace outcomes (retention, attendance), because saving money increases the utility of employment. By using digital payments, workers see their salary's overall added value increase. This phenomenon may contribute to a higher incentive to stay employed at Shahi, as it may increase the retention and attendance rates.

## Impact Evaluation Design

### I. Research Questions

#### 1. Primary Research Questions:

Q1.1 Does classroom training lead to an increase in the number of study participants completing at least one successful digital payment transaction compared to the control group?

Q1.2 Does individual training lead to an increase in the number of study participants completing at least one successful digital payment transaction compared to the control group?

Q1.3 Does classroom training lead to an increase in the number of study participants using digital payment apps for sending remittances as measured at endline compared to the control group?

Q1.4 Does individual training lead to an increase in the number of study participants using digital payment apps for sending remittances as measured at endline compared to the control group?

Q1.5 Does individual training lead to a higher number of study participants completing at least one successful digital payment transaction compared to classroom training?

Q1.6 Does individual training lead to a higher number of study participants using digital payment applications for sending remittances as measured at endline compared to classroom training?

#### 2. Secondary Research Questions:

Q2.1 Does classroom training lead to an increase in the number of study participants using digital payments for non-remittance purposes, compared to the control group?

Q2.2 Does individual training lead to an increase in the number of study participants using digital payments for non-remittance purposes, compared to the control group?

Q2.3 Does individual training lead to an increase in the number of study participants using digital payments for non-remittance purposes, compared to classroom training?

*In the next set of questions, we want to quantify the barriers faced by the study participants and how effective the different training interventions are in addressing them.*

Q2.4 What percentage of participants invited for each of the training session types drop-off at each of the necessary steps that are required for study participants to successfully complete a digital payment transaction at the end of the session?

Q2.5 Do individual training session participants have a lower percentage of drop-off for each of the necessary steps required for study participants to successfully complete a digital payment transaction at the end of the session, compared to classroom training participants?

The steps referenced in the two research questions above are:

- 1) Attended the training
- 2) Had her mobile phone linked to her bank account
- 3) Was able to send SMS (for verification) from the phone i.e. had mobile phone balance
- 4) Had all necessary components during training (ATM card, mobile phone, and the correct SIM card)
- 5) Was able to download the application on the phone
- 6) Was able to set up a UPI id

### 3. Exploratory analysis

For the expected effect size, we are not powered enough to test the impact of the interventions on the second stage outcomes such as financial stress, financial control, remittance amount, and workplace outcomes such as retention and absenteeism. In the exploratory analysis, we will look at the following questions -

Q3.1 Does classroom training lead to an increase in the remittance amount participants send, compared to the control group?

Q3.2 Does individual training lead to an increase in the remittance amount participants send, compared to the control group?

Q3.3 Does classroom training lead to a reduction in perceived financial stress, compared to the control group?

Q3.4 Does individual training lead to a reduction in perceived financial stress, compared to the control group?

Q3.5 Does classroom training lead to an increase in perceived financial control, compared to the control group?

Q3.6 Does individual training lead to an increase in perceived financial control, compared to the control group?

Q3.7 Does classroom training lead to an increase in the retention rate at workplace of participants, compared to the control group?

Q3.8 Does individual training lead to an increase in the retention rate at workplace of participants, compared to the control group?

Q3.9 Does classroom training lead to a reduction in absenteeism at workplace of participants, compared to the control group?

Q3.10 Does individual training lead to a reduction in absenteeism at workplace of participants compared, to the control group?

## II. Evaluation Methodology

### 1. Sampling Criteria

The eligible population for the study is female garment workers at Shahi Exports Private Limited, Karnataka, India, who live in 19 selected Janodaya hostels (Janodaya is the NGO managing migrant hostels linked to Shahi factories) and own a smartphone. These hostels were sampled out of convenience and availability but reflected the overall population of migrant workers in Shahi garment industry in the Karnataka region.

### 2. Randomization

Randomization took place using the statistical software package Stata. We conducted randomization using the following procedure:

- 1) **Collect baseline data from hostels:** Prior to randomization, we conducted baseline data collection in 19 hostels, saturating each hostel
- 2) **Stratified random assignment by unit and mobile registration dummies:** Randomization was carried out at the individual level, stratified by units and reported mobile phone registration at

baseline. Within each stratum, a third of the sample was randomized into the classroom training intervention treatment group (n=273), a third of the sample was randomized into the intensive intervention treatment group (n=273), while the remaining third of the sample in a control group (n=273) did not receive any intervention. We conducted randomization once, after baseline was completed.

### 3. Unit of Analysis and Unit of Treatment

The unit of **analysis** is the individual/respondent. The primary unit of **treatment** is also the individual. We chose to conduct randomization at the individual-level as we believe spillover risks are very limited, considering the barriers at stake.

### 4. Treatment Arms

Our study has two intervention arms and a pure control arm. The SMS reminders were sent to both the intervention arms. With only two treatment arms, this impact evaluation will not provide evidence on the relative effects of each intervention type (training sessions and SMS reminders). We expect the bundle (training plus SMS reminder) to have a stronger effect than smaller combinations (training sessions and SMS reminders alone). We do not expect any negative complementarities between the interventions.

### 5. Outcome indicators

#### 1. *Use of digital payment applications for remittances*

In the primary surveys, we ask whether the respondent uses digital payment applications to remit money. The variable is coded as 1 if the respondent reports using any of the following channels to remit money: mobile money, UPI-based digital payments app, net banking/bank application, and 0 otherwise.

#### 2. *Effectiveness of training sessions*

During the training session, we collect data on whether the participant can send money to someone's UPI id. The variable is coded as 1 if she can send money to someone's UPI id during the session, and 0 otherwise.

During the training sessions, we also collect data on whether the participant can send money to someone's bank account similar to the modus operandi to remit home through digital payment platforms. The variable is coded as 1 if she can send money with someone's bank account during the session, and 0 otherwise. We will use this as a robustness check.

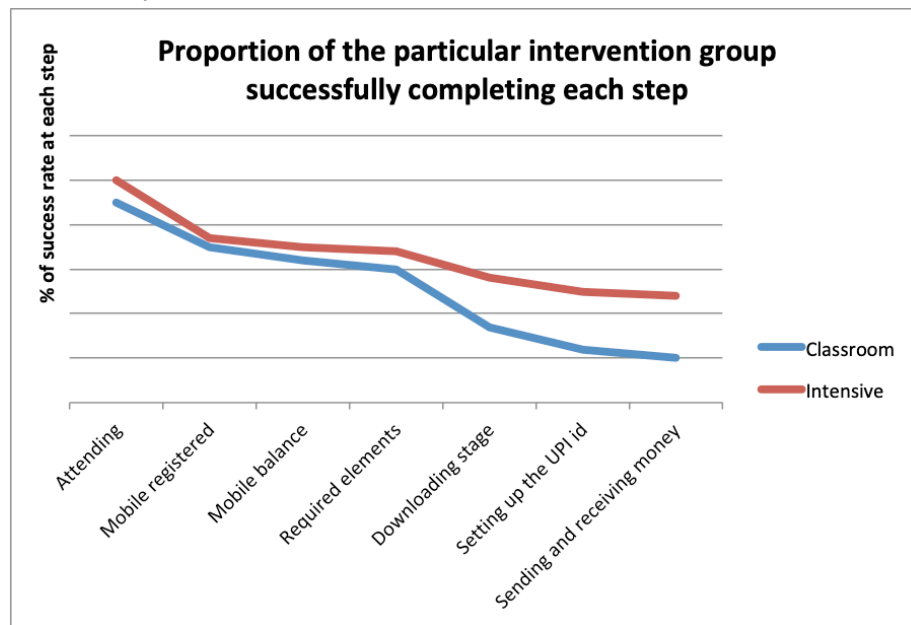
#### 3. *Quantification of the extent of the different barriers*

We collect the following indicators from the participants during the training sessions, which are all the necessary steps enabling participants to send and receive money:

- 1) Whether the participant attended the training

- 2) Whether the participant’s mobile phone was linked to her bank account
- 3) Whether the participant had mobile balance to send verification SMS from her phone
- 4) Whether the participant had the required elements (ATM card, mobile phone and SIM card)
- 5) Whether the participant was able to download the app
- 6) Whether the participant was able to set up her UPI id

These indicators will enable us to conduct a step-by-step analysis. The following illustrative graph shows how the barriers will be quantified.



#### 4. Use of digital payment applications for non-remittance purposes

We collect data on whether the respondent uses her phone for the following tasks:

- 1) to make bill payments, not for goods
- 2) to buy goods
- 3) to receive money from someone living in the city
- 4) to receive money from someone living outside of the city
- 5) to repay money she had borrowed
- 6) to lend money to someone

We will create an outcome variable equal to one if any of the above indicators is 1 and 0 otherwise.

To test for the extent of the usage of the digital payment channel, we will create an outcome variable using a weighted average of the indicator variables above. We will aggregate the individual variables

into an index, constructing weights following the methodology developed by Anderson (2008).<sup>10</sup> We will also analyze each variable separately to see if one of them is more affected than the other. For all such regressions, where we evaluate the effect of the treatment on the outcomes individually, we will report both unadjusted p-values as well as p-values corrected for multiple comparisons using the Family-Wise Error Rate.

#### 5. *Other outcomes for exploratory analysis*

We do not expect to have enough power to detect changes in the following outcomes. However, in case in which we are powered enough, our exploratory analysis would rely on the following variables.

##### *Perception of financial strain*

We collect the following indicators for each worker:

1. How often she has felt nervous and stressed about not having enough to spend in the last month. The variable is coded as 0 if it rarely to never happens, 1 if it happens sometimes, and 2 if it happens most of the time to all the time.
2. How often she has felt nervous and stressed about not remitting enough money to her family in the last month. The variable is coded as 0 if it rarely to never happens, 1 if it happens sometimes, and 2 if it happens most of the time to all the time.
3. Whether she was able to make small purchases (cooking oil, cosmetics, snacks) without worrying about her finances in the last month. The variable is coded as 0 if it rarely to never happens, 1 if it happens sometimes, and 2 if it happens most of the time to all the time.

Our regression specification will be an indicator variable created using a weighted average of the three indicator variables above. We will aggregate the individual variables into an index, constructing weights following the methodology developed by Anderson (2008).<sup>11</sup>

##### *Financial control*

We collect an indicator which measures whether the respondent is aware of the current balance of her bank account. It is coded as 1 if the respondent reports knowing the current balance of her bank account; 0 otherwise.

##### *Safety net and response to consumption shocks*

We collect the following indicators:

1. Whether the respondent is able to come up with 4000 INR<sup>12</sup> within the next month if she is facing a personal emergency. The variable is coded as 0 if “quite easy”, 1 if “Possible, but not easy”, and 2 if “very difficult, or not possible”.

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<sup>10</sup> Anderson, Michael L. "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American Statistical Association*, 103.484 (2008): 1481-1495.

<sup>11</sup> Anderson, Michael L. "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American Statistical Association*, 103.484 (2008): 1481-1495.

<sup>12</sup> We came up with 4000 INR from piloting. Workers earn on average 7000 INR a month.

2. Whether the respondent was able to send or receive money from someone new thanks to digital payment applications. The variable is coded as 1 if the respondent reports sending or receiving money from someone new thanks to digital payment applications, and 0 otherwise.

Our regression specification will be an indicator variable based on a weighted average of the two indicator variables above. We will aggregate the four individual variables into an index, constructing weights following the methodology developed by Anderson (2008).<sup>13</sup>

## 6. Control Variables

### *Stratification variables*

We are stratifying on two variables:

- 1) Unit
- 2) Mobile phone registration dummy at baseline

### *Individual-level control variables*

We will include the following individual-level control variables, which are measured at baseline or available in the administrative data:

- 1) Age
- 2) Native state
- 3) Marital Status
- 4) Perception of financial strain at baseline
- 5) Remittance dummy at baseline
- 6) Monthly remittance quantity at baseline
- 7) Use of digital payment applications at baseline

*Additionally, we will include baseline values of the outcome variables.*

## 7. Treatment Effects

### *Intent-to-Treat*

The treatment effect of the interventions will be estimated using an ANCOVA specification. We have two main specifications for estimation. In the first, we estimate the treatment effect for each of the treatment arms compared to the control group.

$$(1) \quad y_{ih} = \beta_1 T_{i1} + \beta_2 T_{i2} + \delta y_{ih}^B + \gamma X_i + h_h + \varepsilon_{ih}$$

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<sup>13</sup> Anderson, Michael L. "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American Statistical Association*, 103.484 (2008): 1481-1495.



For an individual  $i$  in stratum  $h$ , the variables are defined as follows:

- $y_{ih}$ : the outcome variable at endline
- $T_{i1}$ : Treatment dummy that takes the value of 1 for participants in the treatment group 1 (classroom) and 0 for those in the control group or in treatment group 2
- $T_{i2}$ : Treatment dummy that takes the value of 1 for participants in the treatment group 2 (intensive) and 0 for those in the control group or in treatment group 1
- $y_{ih}^B$ : the outcome variable at baseline
- $Xi$ : a vector of individual-level controls (mentioned in the section above)
- $h_h$ : strata-level fixed effects
- $\varepsilon_{hi}$ : standard errors at the individual-level, accounting for multiple-arm adjustments

$\beta_1$  identifies the (intent to treat) treatment effect of the treatment 1 with respect to the control group, and  $\beta_2$  identifies the (intent to treat) treatment effect of the treatment group 2 with respect to the control group. For testing the effectiveness of the intensive training with respect to the classroom training, we test the statistical significance of the linear combination ( $\beta_2 - \beta_1$ ).

To test the effectiveness of the intensive training with respect to the classroom training for the training session outcomes, we use the following specification for the subsample of participants who are in a treatment arm.

$$(2) \quad y_{ih} = \beta_3 T_{i2} + \delta y_{ih}^B + \gamma Xi + h_h + \varepsilon_{ih}$$

$\beta_3$  identifies the (intent to treat) treatment effect of the treatment group 2 with respect to the classroom training group.

We will also include a regression equation where we estimate the effect of pooled treatment on the outcome variables (combining T1 and T2).

### *Treatment on the Treated*

The complier causal effect of the intervention on the treated will be estimated for the main outcomes using a two-stage least squares (2SLS) regression with the initial treatment assignment as the instrumental variable. The initial treatment assignment is a relevant instrument as it has explanatory power for program take-up, and is correlated to outcomes of interest through, and only through, the program take-up variable. Note that as it is a one-sided compliance setting, we define the compliers as the treated (invited) participants who attended the workshop.

## 8. Heterogeneous Effects

We will look for heterogeneity for following variables:

1. Baseline financial stress
2. Workers who have joined recently

We expect higher adoption among new workers (defined as joined in October and November 2019) because they would likely not have default remittance habits to fall back on.

### III. Sample Size and baseline tests

#### 1. Power calculations

**Table 1** below provides the parameters we used for power calculations, as well as our rationale for making these assumptions before conducting baseline. We also provide the updated parameter values after baseline. Our study is powered to detect a change in 10 percentage points for the use of digital payment applications for remittances.

Parameter	Pre-baseline estimates	Baseline Value	Source
$\alpha$	0.05	0.05	Statistical convention
Power (1- $\beta$ )	0.8	0.8	Statistical convention
Correlation between follow-up measurements	0.5	0.5	Correlation between follow-up measurements was based on our estimation (and then actual) baseline take-up rates, as well as our estimated size effects for each treatment arms. We found a correlation of 0.51
Minimum detectable effect size	15pp	10pp	Lee et al., 2019 found an effect size of 48 percentage points from training to use mobile money. We choose a conservative effect size of 15 percentage points.
Attrition rate	20%	24%	The monthly attrition rate in Shahi factories is about 8% per month (which comes to about 24% over three months). We assume the study attrition rate to be similar to Shahi's attrition rate, as migrant women usually stay in the same hostel and room as long as they work in the factory.  Methods to prevent attrition are rather limited as study attrition is directly linked to workplace attrition.
Sample size per arm	1/3 each	1/3 each	Optimum ratio for power
Total sample size	1,300	900	Pre-baseline: Expected sample size

			based on hostel maximum capacity
			Baseline: actual sample size reduced to smartphone users only

**Table 1:** Power parameters

The baseline sample was 1367 workers. After applying the inclusion criterion of workers with smartphones, the sample size is 900 workers.

Using the actual sample size and baseline values to calculate the minimum detectable effect size, we find that the minimum effect that can be detected with a 95% confidence interval and power of 0.8 is 10 percentage points.

## 2. Balance Tests

We conducted t-tests for differences in means on relevant baseline variables to determine whether balance between the two treatment and control arms had been successfully achieved.

Our balance tests show no significant differences at conventional levels between the treatment and control groups across any variables of interest of first importance. Balance tests show a 10% significance level difference in Odia language distribution between the two treatment arms. **Table 2** below displays these results. The final columns in this table report the difference in means between the two treatment and control groups.

**Table 2: Balance tests**

Variable	N	(1) Control		(2) Classroom Treatment		(3) Invidualized Treatment		(4) Total Mean/SE	t-test Difference (1)-(2)	t-test Difference (1)-(3)	t-test Difference (2)-(3)
		Mean/SE	N	Mean/SE	N	Mean/SE	N				
Digital payment application use dummy	273	0.055 [0.014]	273	0.044 [0.012]	273	0.062 [0.015]	819	0.054 [0.008]	0.011	-0.007	-0.018
Mobile phone registration dummy	261	0.648 [0.030]	265	0.638 [0.030]	263	0.650 [0.029]	789	0.645 [0.017]	0.010	-0.003	-0.012
Remittance dummy	273	0.857 [0.021]	273	0.864 [0.021]	272	0.893 [0.019]	818	0.872 [0.012]	-0.007	-0.036	-0.029
Monthly remittance quantity	235	4200.000 [205.649]	236	4415.678 [305.637]	243	4204.938 [221.848]	714	4272.969 [142.983]	-215.678	-4.938	210.740
Hindi speaking dummy	273	0.245 [0.026]	273	0.212 [0.025]	273	0.205 [0.024]	819	0.221 [0.015]	0.033	0.040	0.007
Odia speaking dummy	273	0.502 [0.030]	273	0.458 [0.030]	273	0.535 [0.030]	819	0.498 [0.017]	0.044	-0.033	-0.077*
Marital Status	273	0.055 [0.016]	273	0.066 [0.018]	273	0.059 [0.017]	819	0.060 [0.010]	-0.011	-0.004	0.007
Mostly stressed about their finances	273	0.081 [0.017]	273	0.070 [0.015]	273	0.081 [0.017]	819	0.077 [0.009]	0.011	0.000	-0.011
Rarely stressed about their finances	273	0.337 [0.029]	273	0.355 [0.029]	273	0.418 [0.030]	819	0.370 [0.017]	-0.018	-0.081*	-0.062
Mostly stressed about being able to remit money	273	0.099 [0.018]	273	0.136 [0.021]	273	0.095 [0.018]	819	0.110 [0.011]	-0.037	0.004	0.040
Rarely stressed about being able to remit money	273	0.297 [0.028]	273	0.333 [0.029]	273	0.377 [0.029]	819	0.336 [0.017]	-0.037	-0.081**	-0.044

The value displayed for t-tests are the differences in the means across the groups.  
\*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.

## IV. Data Collection and Validation

### 1. Types of Data

#### *Data collected during the training sessions*

We ask trainers and surveyors to collect data during the training sessions on tracking sheets that are digitized later on. This data gathers the necessary information for some of the research questions, such as the effectiveness of training sessions and the extent of different barriers to a successful session.

#### *Survey Data*

We built survey instruments based on external survey questionnaires (Findex, EMERGE) and scoping work conducted in May 2019 and September 2019. Survey instruments cover remittance behavior and financial indicators. The data is grouped as follows:

- 1) Individual characteristics including phone number, household address, language and native state
- 2) Digital literacy and current financial behavior
- 3) Remittance means and use, to understand amount and frequency patterns
- 4) Consumption and withdrawal habits, to understand consumption habits and bank account use
- 5) Digital payments, which includes use of digital payment applications beyond remittances
- 6) Financial monitoring, response to shocks and perception of financial strain, to understand mechanisms which might occur throughout the study
- 7) Engagement in formal services such as borrowing and saving money
- 8) Feedback from training sessions

Additionally, we will also collect data by conducting phone-based surveys for workers who are either on long leave or have left the firm. These surveys will include:

- 1) Questions related to the main outcomes: whether or not they remitted, whether or not they remitted through digital payment applications.
- 2) Information related to their new working conditions: where they are currently working and living, and how much they earn.

#### *Administrative Data*

We will use the following data from Shahi: worker attendance (daily), productivity (daily), and retention data.

### 2. Data Quality and Validation

Our field-management staff members are experts at recruiting and training well-performing survey teams and administering surveys, which assures high data quality. We also take the following steps to ensure data is accurate and of high-quality:

- 1) **Survey administration** makes use of SurveyCTO, a software that allows surveyors to conduct surveys using their smartphones. This ensures data collection can be easily monitored by

research teams. We program the survey according to best practices to prevent surveyor mistakes and malpractice.

- 2) **High frequency data quality checks and backchecks** are performed every day and every three days respectively for both rounds of data collection to identify issues in data quality and inform us as to which surveyors to monitor.

## V. Technical Risks

We are confident in the ability of this study design to produce internally valid estimates of the effects of the intervention. Nonetheless, we have identified various areas where the study design is at risk of producing biased results. The following sections discuss the risks our project may face, each of which poses challenges to our ability to recover unbiased estimates of the effects of our intervention. In addition, we highlight the steps we will take to minimize these risks and ensure that our analysis produces rigorous causal impact estimates.

### 1. Attrition

Workers may drop out from the study between baseline and endline. We consider three cases where attrition would be a concern.

- 1) First, attrition could be random, i.e. there are no systematic differences between those we can and cannot find at endline. This does not affect internal validity, but reduces the statistical power of the study.
- 2) Second, attrition may be non-random, i.e. the attriters may differ significantly from the non-attriters, changing the composition of our sample. If this attrition is balanced across treatment and control groups, internal validity is preserved, though the generalizability of our results diminishes.
- 3) Finally, most damaging for internal validity, we may have differential attrition between the treatment and control groups.

We have taken or will take the following steps to reduce attrition in general:

- 1) We have excluded from randomization workers who left Shahi in December (after baseline but before the intervention started).
- 2) We limit our study to two survey rounds and conduct the endline survey cycle shortly after treatment workers receive the intervention.
- 3) We take contact information for each participant at baseline, such as their mobile phone number, their family's phone number, their household address, so that we can contact them easily during the endline surveys to conduct phone call surveys.
- 4) We will make multiple attempts to reach each respondent from our baseline survey at endline.
- 5) We powered our study conservatively to guard against loss of statistical power due to attrition.

We expect an attrition rate from intervention to endline cycle of around 24%, which derives from the Shahi 8% average monthly attrition rate.

- 1) We will conduct brief phone surveys for the attrited sample (see “Survey Data” section).
- 2) For the main outcomes, we will include data from the phone survey in the analysis specifications.
- 3) For other variables, we will check whether results are stable when including phone survey data.

To test whether attriters are statistically different from non-attriters, we will compare baseline characteristics between the three groups. These characteristics include the outcome variables and all control variables used in our analysis. In case attrition is non-random, we will employ inverse probability weighting to address this, using all available information on the attriters from baseline.

Finally, we will test whether attrition is differential between the treatment and control groups, a major risk to the internal validity of our estimates. We will conduct this analysis by regressing a binary outcome variable equal to 1 if the worker is surveyed during both baseline and endline, and 0 otherwise, on treatment status, and baseline values of our covariates. We will additionally test for differential attrition by regressing attrition on a dummy variable for treatment, all baseline covariates, and the treatment dummy interacted with baseline covariates.

In case attrition is significantly different across treatment and control groups, we will follow the approach suggested in Wooldridge (2010), which comprises the following:

- 1) Estimate a probit specification for the probability of being present, which is a dummy variable that takes the value 1 if we can survey the worker at endline, and 0 otherwise. From this specification, we will use the inverse of the predicted probability as the probability weights or sample weights in all the regressions.
- 2) We then re-estimate the equation using the other outcome variables on the left-hand side and these estimated weights.

## 2. Spillovers

Participants who don't receive the intervention may be aware of, discuss, or even share aspects of the interventions within hostels or units, leading to spillovers. For example, workers in the control group may hear of the training session some other workers had in the factory, or hear a group of treated workers discussing the session content. If the case, any aspects of the intervention that affect workers' behaviour in treatment groups, either positively or negatively, would prevent us from recovering unbiased estimates of the treatment effect. Such a control group whose behavior is affected by the intervention given to treatment individuals is no longer truly a control group – it does not provide a good comparison of what would have happened had the treatment group not received the behavioral intervention.

Spillovers could occur through two channels. **The first channel is visibility and discussion.** Individuals in the control group may see that workers in the treatment group are using digital payment applications for remittances, or may see/hear the workers in the treatment groups discussing the interventions. **The second channel through which spillovers could occur is actual usage.** This would occur if a member of the control group asks someone in the treatment group to explain how to use digital payment applications, and/or ask them to send money through their applications. This use could be sporadic or regular, depending on the relationships between individuals in the treatment and control groups.

After conducting qualitative work and pilot sessions in the migrant hostels during the design of the interventions, we believe spillovers are unlikely to pose major issues to our study for the following reasons:

- 1) **The cognitive and technological barriers to adoption are likely high enough to curb spillovers.** As key barriers to taking-up and using digital payment applications are those of trust and technology, we believe attending the training sessions is a requirement to effectively take-up the new method to remit money.
- 2) **The interventions are “soft-touch”.** The interventions consist of a short training session as well as an SMS reminder.

Based on the above considerations, we do not expect the interventions to change control workers’ remittance behavior appreciably. Nonetheless, spillovers are a threat to our ability to estimate an unbiased treatment effect of the treatment. Despite our expectations, control workers may borrow, copy, or be influenced by the intervention, and these actions may impact control workers’ remittance behavior.

The survey design captures whether and how workers in the control group learned about digital payments. In the endline survey, we ask workers how they learned to use digital payments, and in which context they first used digital payments. If workers in the control group reported at baseline that they did not use digital payment applications, and report using them at endline, we can find out whether the workers learned about digital payments from other workers in their hostel/unit.

### 3. Evaluation-Driven Effects

Evaluation-driven effects refer to effects on the outcome variable driven by the evaluation itself. These effects are related to evaluation activities, and are independent of the actual intervention. For example, the fact that survey asks questions related to digital payment applications may have an effect on remittance behavior, regardless of whether the purpose of the intervention is ever implemented. If there are evaluation-driven effects in our evaluation, then we would be worried that the treatment effects we estimate are biased due to the inclusion of these effects in the estimate.

Evaluation-driven effects are likely to be limited in our project, yet may exist. Study protocols will intentionally minimize the potential for evaluation-driven effects by specifying clear actions and protocols for all steps of the evaluation.

## Appendix

### About the digital payments technology

UPI<sup>14</sup> (Unified Payments Interface) technology allows users to create a unique id (a UPI id) linked to their bank account, which, in turn, is linked to their mobile number. People can send money from one UPI id to another. Money is moved directly and immediately from bank accounts. The sender authenticates all transactions using a UPI pin.

BHIM (Bharat Interface for Money)<sup>15</sup> is an app that leverages UPI technology to allow people to send and receive money from their bank accounts. It requires an internet connection.

While the sender needs to have a UPI id, the receiver need not. The sender can send money to:

1. A UPI id/mobile number linked to UPI id
2. A bank account (using account number and IFSC code)

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<sup>14</sup> <https://www.npci.org.in/product-overview/upi-product-overview>

<sup>15</sup> <https://www.bhimupi.org.in/>