No Lean Season 2017 Pre-Analysis Plan

March 15, 2018

1 Overview

This document presents a description of the 2017-2018 No Lean Season randomized impact evaluation experiment conducted in collaboration between Evidence Action and researchers from Yale University, the London School of Economics, and the University of California, Davis. There are two main goals for this study:

1. A replication of previous findings showing positive impact of incentivized migration on seasonal migration, caloric intake, food and non-food expenditure, income, and food security. Our aim is to estimate impact of a scaled version of the No Lean Season program: intensifying program implementation within branches and expanding the provision of loans to all eligible households.
2. Investigating the program's spillover effects on workers at the migration destination who are not offered migration incentives. Given the scale of the No Lean Season program, we anticipate that there will be enough migration to noticeably affect destination labor markets. Destination workers include those who permanently reside at migration destinations as well as seasonal migrants from other areas. We aim to evaluate the effect of the program on these workers' income and location choice.

In this document we describe the design of the intervention (the No Lean Season program as it will be scaled); lay out the research goals of the study; describe the design of the experiment to investigate our research questions; describe the data we will collect; and finally how we will conduct our analysis.

Research team:

Role	
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2 Intervention

No Lean Season is a program, implemented in collaboration between Evidence Action and RDRS, seeking to address seasonal poverty in rural Bangladesh by offering households small, interest-free loans, covering the costs of a round-trip bus fare to nearby areas that do not experience the same seasonal fluctuations. Loans are provided in the Northern Rangpur region of Bangladesh to households with limited land ownership and limited job opportunities in the lean season. The loan is generally offered right before and during the lean season, so participating households can take advantage of the opportunity when it is best for them. Upon return from their migration, households are asked to pay back the loan before the next program cycle begins. Below is a description of each phase of the program.

2.1 Household Targeting

Targeting the appropriate households for the program is the first phase of activity. In this phase, the targeting (baseline) survey is administered to each household in sampled villages and branches, similar to that of a census. The targeting survey lists all households in each village and collects data on the targeting criteria. In previous years, researchers have used land ownership (50 decimals or fewer) and food security in the last lean season (whether any member of the household skipped meals in the previous lean season) to define eligibility for the program. This year, the team decided to modify the criteria slightly and used the following parameters:

- Cultivable land ownership of 50 or fewer decimals; or,
- Someone in the household did not have a sufficient amount of meals in the two weeks prior to the targeting survey.

The decision to go with the latter criteria instead of past year's criteria on food security was made because recent food insecurity may be a more relevant condition for deciding to migrate. Previous research has revealed that decisions on migration were taken considering the household's current situation and negative shocks. Moreover, recall bias is reduced when referencing a more recent time period.

Household targeting surveys are conducted by two partners - RDRS, the No Lean Season implementing partner, and Innovations for Poverty Action (IPA), the research implementing partner[1]. RDRS collected data for the survey in all program villages, while IPA collected data for the survey in all non-treated sampled villages - this includes *spillover*, *spillover-control*, and *pure-control* villages (more on this distinction below).

2.2 Information Outreach and Offers

After households have been deemed eligible for the program, they are invited to attend group meetings in their village to learn more about the program. Invited households who attend these meetings receive a standardized presentation on seasonality, the program, and migration for work. After the meeting concludes, households are asked whether they accept in the program offer. There are three possible responses: yes, no, or interested. If households respond yes, they will be added to a list for follow-up and can begin the loan application process. If households respond no or interested, they are given time to think about the offer and Migration Organizers (MOs) - RDRS' frontline of program implementation - will follow-up with the household up to three more times.

For those households unable to attend the offer meeting, they are assigned door-to-door offers and receive an offer at their household after the offer meeting has been completed. All following steps after the door-to-door offer are the same as those for offer meeting.

2.3 Disbursements

In the offer meeting, households are provided information about their respective branch offices and are instructed on the days in which they can pick up the loan. Disbursements are then provided to households that visit their RDRS branch office and successfully fill out the loan applications. The process for receiving a loan is short and only requires households to bring official identification. Applicants are cross-referenced with the eligible household lists collected in the first phase, and loans are only disbursed to valid members of the household. After the loan has been disbursed, RDRS staff must provide tips to the migrant to ensure they are aware of how to best protect themselves while migrating.

In a select group of remote villages, village-based disbursements are carried out. This accounts for approximately 1% of the treated villages. During these village-based disbursements, households are only

[1]IPA and RDRS implemented slightly different versions of the targeting survey. However, the majority of questions included in both surveys overlap, including eligibility questions. See attached survey instruments for details.

able to receive disbursements on that day and must travel to the branch office if they desire to take out the loan at a later date.

2.4 Follow-up (soft conditionality)

After households have taken out the loan, MOs follow-up at the household to remind loanees that they have taken out the loan with the intention of migrating for work. This visit is to remind the household that the loan is meant to encourage migration, and to ensure the loanee is aware that they have an obligation to pay back the loan at the end of the lean season. These visits initially occur one week after the household takes out the loan. If a member from the household has migrated at the time of the first visit, the MO will only visit the household in one month increments to see if the migrant has returned. If a member from the household has not migrated by the first visit, the MO will visit a maximum of once a week until they have/he has confirmed a household member's migration. This phase is intended to monitor the soft conditionality of the loan, but it is important to note that household members are not forced to migrate if they take out the loan.

2.5 Loan repayment and debrief

The final phase of implementation occurs when the migrant has returned to the village. Once the MO learns of the migrant's return, he visits the household to collect repayment and conduct a short migration debrief survey. Households are not required to pay on the first visit after they have returned, and are given up to six chances to repay. The migration debrief survey conducted at the first visit after the migrant's return collects data on the migrant's destination, employment, wages, living expenses, and any experiences that may affect their ability to migrate or find a job. If a household is unable to pay back the loan in full, households are given the opportunity to describe their situation. All requests for loan exemption are then reviewed by RDRS management, and decisions on exemption are later communicated to households by the MO. At the end of the program year (March), all remaining unpaid balances are written off. A household's loan repayment in a given year will never affect its ability to participate in future years of the program.

3 Experiment Design

RDRS is organized administratively into branch offices. Each branch has a set of villages in its catchment area defined by the geographic (road) distance to the branch. Branch catchment areas are non-overlapping so each village in the experiment can be allocated to a single branch.

Treatment, defined as the offer of a migration subsidy (incentivization), occurs at the village level. Every eligible household in a treated village is offered the migration subsidy. Our randomization strategy places villages into four categories:

1. *Incentivized*: Villages in which the migration subsidy offer is made.
2. *Spillover*: An untreated village geographically in the middle of a group of treated villages.
3. *Spillover-control*: An untreated village that belongs to a branch that includes treated villages, but is surrounded by other untreated villages.
4. *Pure-control*: An untreated village that belongs to a branch that has no treated villages.

To achieve this classification, we randomize at two levels. First, we randomly divide branches into treated and control. Branches assigned to be control contain only *pure-control* villages. Branches assigned to be treated contain the other three types of villages (See Figure 1).

Within treated branches, our randomization strategy generates a treated sector (designated as *incentivized*), a single untreated village within the treated sector (designated as *spillover*), and an untreated sector (designated as *spillover-control*). In accordance with the RDRS workplan, the treated sector comprises one third of the villages in a treated branch. For assignment, we identify the centroid of the branch catchment area and

then project each village onto a circle around the centroid. We randomly select one village on this circle and designate it as *spillover*. We then define the *incentivized* sector as the third of the circle surrounding the *spillover* village. In effect, we create a "pie slice" (designated as the *incentivized* sector), with one village in the middle left untreated as *spillover*.

This strategy stems from the fact that incentivization may generate spillovers onto nearby villages. Spillovers come from three main sources. First, we find in previous work that migrants generally travel in groups and migrants from geographically close sources tend to go to geographically similar destinations. Therefore, inducing migration in one village may lower the returns to migration from nearby villages through the destination labor market. Second, labor markets may be locally integrated. Out-migration from an *incentivized* village may lower labor supply, raise wages, and induce in-migration from nearby villages. Third, household risk sharing networks may extend beyond village boundaries. An *incentivized* household may share the benefits of migration with others in nearby villages.

Our randomization strategy creates multiple types of *non-incentivized* villages to evaluate the geographic extent of these spillovers. The *spillover* village in a treated branch is on average closest to *incentivized* villages and therefore most exposed to treatment spillovers. At the other extreme, we believe *pure-control* villages are sufficiently far from treated regions that their workers are no more exposed to treatment spillovers than workers from anywhere else in the country. *Spillover-control* villages falls between these extremes and allow us to estimate how quickly the spillovers dissipate with distance.

For evaluation, we plan to survey (record) households in only a subset of *incentivized*, *spillover-control*, and *pure-control* villages. Survey villages are selected as follows:

1. *Incentivized*: One randomly selected village in the *incentivized* sector per branch.
2. *Spillover*: The village in the middle of the *incentivized* sector, designated as *spillover* in each treated branch.
3. *Spillover-control*: The village diametrically opposite the *spillover* village on the circle projection.
4. *Pure-control*: One randomly selected village in each untreated branch.

The randomization design generates the four experimental categories while ensuring that the status of a village is uncorrelated with other geographic characteristics. In particular, treatment status is orthogonal to the geographic density of villages and their proximity to a branch's boundary. The survey design preserves orthogonality between likelihood of being surveyed and geographic characteristic as well. Unfortunately, in maintaining this orthogonality, we cannot guarantee that *spillover* villages are closer to the *incentivized* sector than *spillover-control* in every treated branch. We do not account for proximity to the centroid in randomization, meaning that a very central *spillover-control* village may be closer to the treated region than a peripheral *spillover* village. However, on average, *spillover* villages are closer to *incentivized* villages than *spillover-control* villages. Similarly, an *incentivized* village in our sample is on average closer to the *incentivized* sector than a *spillover-control* village, but slightly father on average than a *spillover* village.

The intervention and study sampling will be clustered as follows:

1. *Households*, indexed by i.
2. *Villages*, defined as the set of households, indexed by j.
3. *Branches*, defined as the set of villages served by an RDRS branch office, indexed by k.
4. *Subdistricts*[2], defined as the subdistrincts within which study villages are located, index by m.

We define $\mathcal{V}_k = \{j : k[j] = k\}$, the set of all villages in branch k.

Household data collection will be collected at three points in time: (a) a baseline/targeting survey, (b) a midline survey and (c) an endline survey. For some variables, time will be indexed with a subscript $t \in \{0, 1, 2\}$ for the three rounds, respectively.

For the study population we have the following treatment assignment indicators:

- $Z_k^p \in \{0, 1\}$ is the branch level treatment assignment indicating whether branch k was a program branch.

[2]Upazilas

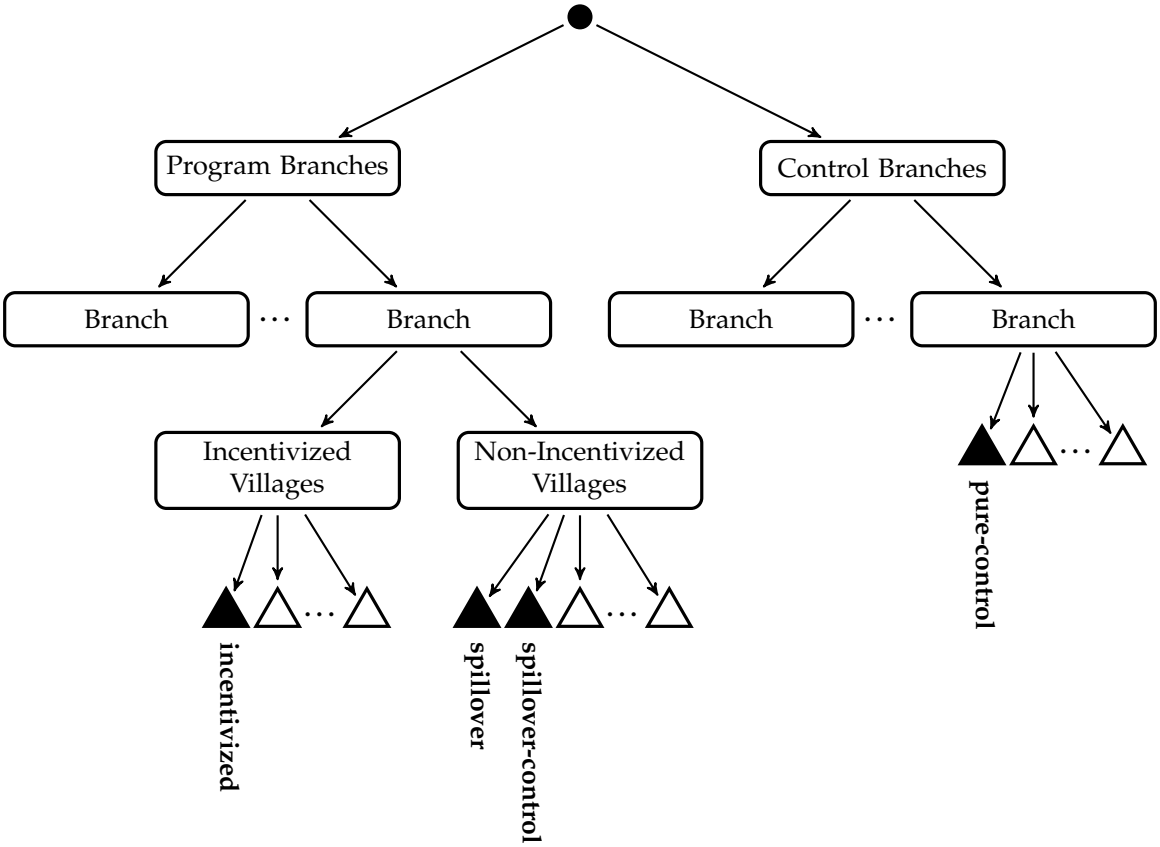

Figure 1: Tree diagram of assignment and recording mechanisms. Triangles represent villages and the solid triangles represent recorded villages.

- $Z^s_j \in \{0,1\}$ is the village level treatment assignment indicating whether village j is in a program branch and not receiving any migration loan incentives (spillover treatment).
- $Z^l_j \in \{0,1\}$ is the village level treatment assignment indicating the provision of migration loan incentives.

We also have the following recording assignment indicators:

- $W_j \in \{0,1\}$ is an indicator that village j has been selected to be surveyed.
- $W_i \in \{0,1\}$ is an indicator that household i has been selected to be surveyed (thus $W_i \implies W_{j[i]}$).

We define the following sizes:

- $N^b = N^p + N^c$ is the number of study branches: N^p is the number of program branches and N^c is the number of non-program branches.
- $N^v = \left(N^v_k\right)_k$ is an N^b-vector of the number of village in each branch.
- N^h is the total number of recorded (surveyed) households

Every village has the treatment assignment vector $Z_j = (Z^p_{k[j]}, Z^l_j, Z^s_j)$. In addition, we define two within branch assignment $\|N^v\|$-vectors Z^s and Z^l, as well as Z^p as the program assignment N^b-vector. Likewise, W is an $\|N^v\|$-vector indicating village recording status. For convenience, we also define the categorical variable

$$\widetilde{Z}_j \in \{\text{pure-control, spillover-control, spillover, incentivized}\}$$

to identify the treatment group of village j.

Treatment assignment will be split into the following groups:

Table 2: Experiment Treatment Assignment Groups

$\widetilde{Z}_j$	$Z^p_{k[j]}$	Z^s_j	Z^l_j
pure-control	0	0	0
spillover-control	1	0	0
spillover	1	1	0
incentivized	1	0	1

In the study, we randomly assigned treatment to 111 branches from the pool of available RDRS branches, such that 40 branches were assigned to be treated and 71 were assigned to be control branches. An additional 10 branches were also treated but were not randomly selected for treatment; these were selected as part of pilot run in 2016.

Update: during the midline household survey round it was discovered that one of the *pure-control* village (Mandal Para in Berubari branch) was incorrectly assigned to receive incentivization. It was incorrectly placed in two different branches. In our analysis we will drop this *pure-control* village.

3.1 Assignment Mechanism

Random assignment to treatment is designed in a manner concentrating *incentivized* villages ($Z^l_j = 1$) around the *spillover* village ($Z^s_j = 1$), in program villages ($Z^p_k[j] = 1$). This is done as follows:

1. Construct the circle-distance $N^v_k \times N^v_k$ matrices $\{\mathbf{D}_k\}_k$.

   For each branch k:

   a) Project all village onto the surface of a circle centered on the branch's centroid (based on the geographic location of all branch villages). The actual radius of the circle is irrelevant, but we set it to the smallest (geographic) distance between a branch village and the centroid. See Figure 3.

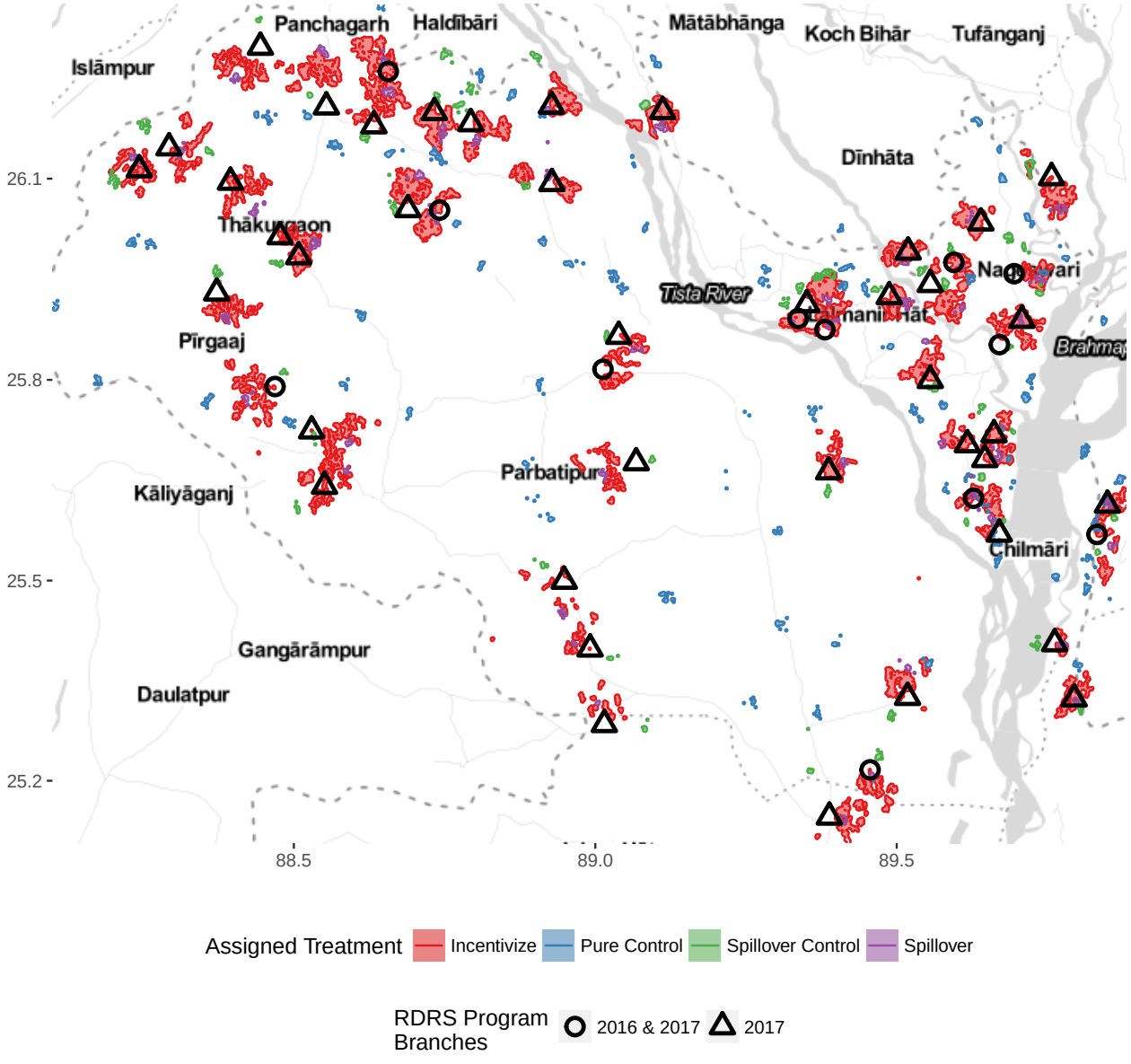

Figure 2: Household Sampling Frame Map

Black filled points show the selection of villages to record (survey).

Figure 3: Example of within branch treatment assignment

i. For each village $j \in \mathcal{V}_k$: Calculate the point of intersection of a ray projecting from the centroiding and intersecting j geographic location.
ii. Let $\mathcal{D}_k$ be the matrix of geographic distances between every pair of villages' projected locations.

2. For each program branch k ($Z_k^p = 1$):
   a) Randomly select one village to be the *spillover* village.
   b) Assign the $\mathsf{round}\left(\frac{N_k^v}{3}\right)$ villages with the smallest circle-distance to the *spillover* village to be *incentivized*.
   c) Assign the village with the greatest circle-distance to the *spillover* village to be the *spillover-control* village.

$$p(Z^p, Z^s, Z^l | \{\mathbf{D}_k\}_k) = p(Z^p) \cdot p(Z^s, Z^l | Z^p, \{\mathbf{D}_k\}_k)$$

where the branch level assignment mechanism is

$$p(Z^p) = \begin{cases} \binom{N^b}{N^p}^{-1} & \text{if } \|Z^p\| = N^p \\ 0 & \text{otherwise} \end{cases}$$

and the village level assignment mechanism is

$$p(Z^s, Z^l | Z^p, \{\mathbf{D_k}\}_k) = \prod_k p(Z_k^s, Z_k^l | Z_k^p, \mathbf{D}_k)$$

and[3]

$$p(Z_k^s, Z_k^l | Z_k^p, N_k^v) = \begin{cases} 1 & \text{if } Z_k^p = 0 \wedge \|Z_k^s\| = 0 \wedge \|Z_k^l\| = 0 \\ 1/N_k^v & \text{if } Z_k^p = 1 \wedge Z_k^s \cdot Z_k^l = 0 \wedge \texttt{is\_in\_incentivize\_wedge}(Z_k^s, Z_k^l, \mathbf{D}_k) \\ 0 & \text{otherwise} \end{cases}$$

Z_k^s and Z_k^l are the N_k^v-vector for village treatment assignment within branch k and $\texttt{is\_in\_incentivize\_wedge}(s, l, d)$ is an indicator function for whether all incentivized villages (Z_k^l) are within a "incentivization wedge" around the *spillover* village (Z_k^s). Figure 4 shows how the (non-zero) probability of treatment assignment of villages in program branches varies.

3.2 Recording Mechanism

$$p(W | Z^p, Z^s, Z^l, N^v, \{\mathbf{D}_k\}_k) = \prod_k p(W_k | Z_k^p, Z_k^s, Z_k^l, N_k^v, \mathbf{D}_k)$$

and

$$p(W_k | Z_k^p, Z_k^s, Z_k^l, N_k^v, \mathbf{D}_k) = \begin{cases} 1/N_k^v & \text{if } Z_k^p = 0 \wedge \|W_k\| = 1 \\ 1/\|Z_k^l\| & \text{if } Z_k^p = 1 \wedge W_k \cdot Z_k^s = 1 \wedge W_k \cdot Z_k^l = 1 \wedge W_k \cdot \texttt{spillover\_control}(Z_k^s, \mathbf{D}_k) = 1 \\ 0 & \text{otherwise} \end{cases}$$

W_k is the N_k^v-vector for village recording status within branch k and $\texttt{spillover\_control}(z, d)$ returns a vector mask indicating which villages are the "farthest" from the *spillover* village (Z_k^s).

The second case in the above recording mechanism above assigns the probability $1/\|Z_k^l\|$ when

[3] "$\wedge$" is the logical *and* operator.

i. *k* is a program branch
ii. The *spillover* village is recorded
iii. One random *incentivized* village is recorded
iv. The *spillover-control* village is recorded

Details on how distance between villages is determined is in the experimental design section.

4 Data Collection

This section is to describe the data collection protocol (e.g., samples and random selection, stratification, clusters). Data collection at the origin can be divided into three phases: baseline/targeting; midline; and endline.

Household sampling differs between treated and untreated villages due to program implementation. In *incentivized* villages, RDRS created a census of all households and then identified those eligible for the program. We will sample a random subset of eligible households. In untreated villages (comprising *spillover*, *spillover-control*, and *pure-control*), IPA selected households using a random walk. We then evaluated these households for eligibility and plan to survey a random subset of those eligible. Both sampling strategies are designed to generate a representative random sample of eligible households, but differ in their implementation.

4.1 Baseline/Targeting

For program implementation, RDRS conducted a targeting survey in every village selected for incentivization (treatment), to determine eligibility for the loans. Within *incentivized* villages, all households were surveyed through a short questionnaire in June - September 2017, through which basic information was collected on the composition of (potentially) working members in their household; ownership of land; recent migration patterns; and food security in the previous season as well as most recently.

To create a comparable group in non-incentivized villages for our study (*spillover*, *spillover-control*, and *pure-control* villages), a questionnaire containing the same set of questions was deployed by IPA in this latter group of villages. Under this activity, carried out in August - September 2017, IPA enumerators applied the survey to 50 randomly selected households in each non-incentivized villages. This is in contrast to the RDRS activity, which covered every household in a village, but was chosen as the only purpose was to generate a list of 20 randomly selected households in each non-incentivized village that would have been eligible for the loan had the program been implemented in their village.

This basic information collected prior to the lean season – and used for targeting by RDRS in treated villages and for generating a comparable group in non-treated villages – forms the baseline data for this study.

Aside from the baseline data activities, potential migrant households were also contacted by phone and asked information regarding their work plans at destination as well as employer information at destination areas. This brief activity was carried out to assist in the design of a sample frame for data collection in destination areas, as described below.

4.2 Midline

A short origin midline survey will be deployed in January/February 2018 to all households in our study (*incentivized*, *spillover*, *spillover-control*, and *pure-control*). The purpose of this survey is to gather data on migration, consumption, and wages/income during the migration period, and questions are largely based off previous survey instruments (e.g., 2008/2011 consumption modules; 2014 high-frequency employment survey). It will be deployed separately from the endline precisely to gather information on dimensions that

cannot be reliably measured through recall (e.g., food and non-food consumption; wages in a given week) as well as to provide early information on migration patterns and decisions while these are taking place.

Definitions

Household. A household can be of a single person or a group of people living in the household, sleeping in the household, and eating from the same pot.

Household member. A member would be counted as a household member if s/he lives in a household, sleeps in the household, and eats from the same pot. If the household head or a student (household member) lives outside, however, sends to or receive money from the household regularly would be considered as household member. Please include all the individuals who were household members at any point in the last 12 months (including a household member who is born or died in last 12 months).

Respondent. Ideally the household head should be the respondent. In absence of household head, the eldest son (if adult and knowledgeable) or HH's wife/husband should be selected as respondent. If the eldest son/wife/husband of head of household is not available, any other adult person from the household can be the respondent, if available and knowledgeable about the household.

In case of information regarding migration, try to take information from the migrant him/herself, if present at the time of interview. At the time of his/her absence, please take information from a HH member who is well aware/knowledgeable about the member who migrated.

Household Head. A household member who is recognized as the head of household by the all household members, and who takes all decisions in the household.

Migration. If a person went outside of own Upazila for work within September 2017 to January 2018 and stayed for at least 7 days, will be considered as migration.

- If someone migrated before the time period and stayed there during the above time period or part of the time period, that will be accounted as well if the person does not permanently work there and/or stays most of the times in this household.
- If someone migrated more than 4 times, collect data for the latest 4 episodes. Remember to go backwards, starting from fourth migration episode.
- If someone migrated at one place, and moved to another place from there, consider that as one migration episode. Collect data for the place of migration and relevant information for the place where he stayed for longer period.
- If someone migrated before the described time period (September 2017 to January 2018), and did not come back even during survey, will not be included in the migration roster.
- If any member from the household stays permanently outside the Household for job/education purpose, will not be included in the migration roster.

Sample and replace

1. Follow the serial number and survey households till the indicated serial.
2. If a household does not exist anymore or eligible respondent is not available for a household, in the form record the reason and replace the next household as indicated in the tracking sheet.

Age

- Take months only for children below 3 years old.
- For others, take the years that is completed, such as: if someone's age is 10 years 7 months, write 10 years.

Employment

- Get information only for household members who are 10 years old or above.
- Get information only for the household members who has an income.
- If a household member works at more than one sector, include all the sectors, but take income as the average from all the sectors together.

4.3 Endline

The endline survey will be deployed at the end of the migration period, in April/May 2018. This survey is more extensive, and will again be applied to all households in our study. The primary purpose of this survey is to collect information on migration and income during the lean season, through we will also use it to investigate secondary outcomes, such as education investment and decisions, credit access and use, and intra-household effects. The instrument for this survey is again largely based off previous tools, particularly the 2011 and 2014 endline survey instruments.

5 Surveys

This part of the document will to describe the actual data to be collected from the activities outlined in the Data Collection section. Survey instruments will be attached to the pre-analysis document for a more in-depth look at collected data. In the Analysis section of the document we will describe which variables (and how) will be used in our analysis.

5.1 Census/Baseline Household Surveys

The purpose of the baseline household survey was to collect data which could be used to determine eligibility for the No Lean Season program. As such, the survey collected basic demographic data that could be used as a proxy to measure household vulnerability during the lean season. The survey was administered to an adult household member with knowledge about the working members of the household (or a working member themselves). The following pieces of data were collected in the baseline survey:

- A roster of working member names and their respective ages
- Household land ownership (on paper) in decimals - total land and cultivable land
- Amount of land cultivated by the household in the last aman season (includes land owned and rented)
- Household food security in the last 2 weeks preceding the survey (all members in household completed at least 2 meals with satisfaction each day)
- Household food security in the last lean season
- Previous work migration
- Location of previous work migration

Ultimately, cultivable land ownership and food security in the last 2 weeks were used to determine program eligibility.

The baseline survey was completed as a census in treated villages. In control villages, the baseline survey was conducted in random sample of 40 households per village. It was estimated that at least half of the households in treated and control villages would be eligible for the program, providing a sample of roughly 40 eligible households in treated villages and 20 eligible households in control villages, on average.

5.2 Midline Household Survey

The main goal of the midline household survey is to capture data that is likely to have a short recall period, for example, food consumption items within the last seven days. This is information critical to evaluating the impact of the program on consumption during the lean season, and it is not likely to be easy to recall at the endline survey 4-5 months after.

5.2.1 Household Composition (section 1)

This section of the midline module is composed of two parts, both of which will be repeated in the endline survey.

1. A roster of all household members, their relationship to other household members and general demographic characteristics.
2. Three short questions on school participation within the last seven days.

5.2.2 Baseline Eligibility (section 2)

This is a short section of the module repeating some of the baseline survey questions determining eligibility to the NLS program. We will use this data, in addition to other household demographics, to check the balance between households that were surveyed at baseline by different organizations (RDRS and IPA). This and all following survey rounds will be conducted by IPA.

5.2.3 Food Consumption (section 3)

This section of the module is composed of two parts:

1. An itemized food consumption list, replicating the data collection used previous studies, and which will mainly be used to impute the caloric intake of household members.
2. A short survey on food security, replicating the data collection conducted in the last study (2014-2015). This part of the survey will be be repeated at the endline.

5.2.4 Non-food Expenditure (section 4)

This section replicates the data collection done in previous studies, measuring household expenditure on non-food items.

5.2.5 Employment (section 5)

This section asks about the employment of household members who are 10 years old and above. This is mostly a new section with questions identifying possible inter-village employment. This section will be repeated in the endline survey.

5.2.6 Migration (section 6)

This section of the module will capture seasonal migration data for all household members. It is composed of two parts

1. A migration roster
2. In-depth questions on migration episodes leading up to the time of survey

This section will be repeated at the endline survey. The in-depth migration part of the section will be expanded with more detailed questions.

5.2.7 Endline Household Survey

The goal of the endline survey is to capture more comprehensive details about household characteristics, migration and economic activities that are unlikely to have short recall time. Some of the modules from the midline have been copied over to the endline and have been clearly identified in the documentation here and in the survey instruments themselves. We also attempted to keep the section ordering as it was in the 2011 endline survey module (we skip section numbers 10 and 15 since they are no longer applicable in this year's endline survey round).

5.2.8 Household Composition (section 1)

This section contains the same questions as in the midline survey in addition to an employment part (copied from section 5 in the midline).

5.2.9 Questions about HH Roster (2)

This section asks more in-depth questions about the household such as household construction material and access to water.

5.2.10 Land and Agricultural Wealth (section 3)

This section's questions are aimed at measuring land and agricultural assets, similar to what was done in the baseline survey but more in-depth. This is repeat of the section used in the 2011 study.

5.2.11 Health (section 4)

This section's questions are aimed at health shocks within the last year. It is a repeat of the section used in the 2011 study.

5.2.12 Economic Activity (section 5)

These are detailed questions about economic activity within the last 12 months. There are similar employment questions in section 1 that focus on the last week.

5.2.13 Non-agricultural Enterprises (section 6)

These questions on non-agricultural enterprises were taken from the 2011 study instruments.

5.2.14 Production (section 7)

This section is split into three parts on

1. Agricultural production
2. Livestock and birds
3. Fishing

4. Forestry

It is a repeat of a module used in the 2011 study.

5.2.15 Other Assets and Income (section 8)

This section cover any assets or income not already recorded in other sections. It is a repeat of a module section used in the 2011 study.

5.2.16 Food Consumption (section 9)

This section in the endline survey focuses on food security during the past 12 months and the past 7 days. This is an expanded version of the food security part used in the midline survey.

5.2.17 Non-agricultural Household Assets (section 11)

This sections collects data on such household assets as appliances, furniture, televisions, radios, etc. This is a repeat of a module section used in the 2011 study.

5.2.18 Financial Assistance (section 12)

This section collects data on

1. Financial assistance received
2. Financial assistance given
3. Membership in any MFIs

This is a repeat of a module section used in the 2011 study.

5.2.19 Savings (section 13)

This is a short section on savings, copied from the 2011 study endline module.

5.2.20 Risk, Coping and Shocks (section 14)

This section focuses on natural and economic shocks that households confronted and how they were dealt with. This is a repeat of a module section used in the 2011 study.

5.2.21 Migration (section 16)

This is an expanded version of the migration section used in the midline survey (for example, with questions on remittances).

6 Analysis

6.1 Primary Outcomes

- $Y_{it}^{\text{mig}} \in \{0, 1, 2, \ldots\}$ is a measure of the number of migration episodes for individuals in household i, as recorded at data collection round $t > 0$.
- Y_{it}^{con} is a vector of different consumption outcomes measured at round $t > 0$ (not all outcomes will be measured every round):
  - Food expenditure (Taka per person per month), measured at $t = 1$.
  - Non-food expenditure (Taka per person per month), measured at $t = 1$.
  - Caloric intake (calories per person per day), measured at $t = 1$.
  - Income, measured at $t > 0$.
  - Food security, measured at $t = 2$.

We denote the potential outcomes in response to assigned treatment z as $Y_{it}^{mig}(z)$ and $Y_{it}^{con}(z)$. We index outcomes in the Y^{con} vector by d.

6.1.1 Food expenditure

- Based on data collected through the midline survey, Section 3 Part A.
- Reports of items consumed in the last 7 days will be multiplied by 4 to convert to monthly expenditure; and those consumed in the last 14 days will be multiplied by 2 to convert to monthly expenditure.
- We will add Taka amount spent on each item consumed (`Q10`) as well as convert the amount consumed from own production (`Q12`) and other sources (`Q13` and `Q14`) into a Taka value using the mean price of the item.
- Denominator ("per person") will be calculated using information from `FC_2`, which give the number of household members present in the last 7.

6.1.2 Total expenditure

- Based on data collected through the midline survey, Section 3 Part A and Section 4.
- It is the sum of food and non-food expenditures. Food expenditures will be calculated as described above.
- For non-food expenditures with 1 week recall period, we will multiply expenditures by 4 to convert to monthly; for those with 12 month recall periods, we will divide by 12 to convert to monthly. Those with a recall period of one month will be left as is.
- We will add the Taka amount spent on each item consumed (`Q3`) as well as convert the amount consumed from other sources (`Q4`) into a Taka value using the mean price of the item.
- Denominator ("per person") will be calculated using information from `FC_2`, which give the number of household members present in the last 7.

6.1.3 Caloric intake

- Based on data collected through the midline survey, Section 3 Part A.
- Each item reported as consumed in the last 7 or 14 days will be converted to its caloric value using the amount (`Q5`) and unit (`Q6`) consumed, divided by 7 or 14, respectively. These calories will then be added up over all the food items in the survey. We will use the same caloric values for food items as was done in 2008 and 2011. The script used to calculate caloric intake in previous studies will be attached to this pre-analysis plan. As shown, we will calculate `tqtykglcal` and divide it by the number of household members present in the household over the last 7 days.

- Denominator ("per person") will be calculated using information from `FC_2`, which give the number of household members present in the last 7.

6.1.4 Income

- Income will be measured at two points, and there are three versions of this outcome - one from the midline survey and three from the endline. **We will use the third measure from the endline survey as the primary income outcome (4 below)**.

1. For the midline, the data comes from Section 5 (employment). In particular, we will add the Taka amount earned (`Q10`) from each member over the previous week.
2. The same questions and data will be collected in the endline, generating a second measurement of labor income over the previous week.
3. A third measurement of income will come from the data from the endline survey, based on income earned over the previous 30 days, and which includes income from household enterprises and agriculture (Section 5). We will add to this any non-labor income received in the last 12 months (divided by 12; Section 8 Part B).
4. From the endline household survey section 5 part C, we will sum - The gross income from migration - As described in Akram, Chowdhury, and Mobarak (2017) "all income and profits earned at home (all income from household's enterprises, and both ag and non-ag wages minus the household's costs in the income-generating activities)" (Table 5, ITT effects on migration income, etc.).

6.1.5 Food security

- Food security questions will be asked at the endline, and will be based on a standardized set of questions, also employed in the 2016 follow-up survey (section 8).
- In particular, as is standard when dealing with this data, the responses to these questions (2-10) will be summed to create a "food security index", where a higher score indicates a higher level of food insecurity[4].
- Any further analysis of each food security variable individually will be considered exploratory. Food security questions in present in the midline survey but will not be analyzed as a primary outcome.

6.2 Primary Impact Estimands

For all the primary outcomes describe above, we are interested in estimating the impact (average treatment effect) between the following treatment groups:

1. **spillover-control vs. pure-control**: This identifies the impact of program spillover on non-incentivized villages, excepting spillover from neighboring incentivized villages (i.e., impact on non-incentivized villages that are as distant from incentivized villages in their branches as possible). This captures any impact program implementation might have on villages while limiting inter-village spillover.
2. **incentivized vs. pure-control**: This identifies the total impact of the program as it would be scaled, combining the effects of incentivization, program, and inter-village spillovers, by comparing *incentivized* villages to the counterfactual of having no program at all. If we find estimand (1) to be lower than a specific threshold we will combine the *pure-control* and *spillover-control* groups and estimate **incentivized vs. (pure-control and spillover-control)**. This threshold will be a 5% increase in both total expenditure and migration, statistically significant at the 10% level using a Fisher exact test.
3. **spillover vs. spillover-control**: This identifies the impact of inter-village spillover from neighboring *incentivized* villages on non-incentivized villages. This could be driven by a number of mechanisms:
    a) Spillover at destination labor markets, due to an increase in migration take-up from neighboring *incentivized* villages leading to an increase an in labor supply in destination markets.

[4]See http://www.fao.org/3/a-i7835e.pdf

   b) Spillover between villages as migration in neighboring incentivized villages leads to
      i. Greater employment opportunities in *incentivized* villages
      ii. Better networking and/or information sharing about seasonal migration
      iii. Changes in prices
4. **incentivized vs. spillover**: This identifies the impact of incentivization net spillover effects. This hinges on spillover effects being equal between *incentivized* and *spillover* villages which might not be true since *incentivized* villages were clustered by design around *spillover* villages and recorded *incentivized* villages were randomly selected from the pool of *incentivized* villages in each branch. Thus, recorded *incentivized* villages might not be exposed to the same intensity of spillover as the *spillover* villages.

In particular, for migration we are interested in (a) the average difference in the probability of seasonal migration and (b) the average number of seasonal migration episodes. For the other welfare outcomes (expenditure, caloric intake, income, etc.) we want to estimate the average scalar difference in measured outcomes.

6.3 Covariates

Our analysis will control for pre-treatment household characteristics:

- Household education
- Percentage expenditure on food
- Number of adult males
- Number of children
- Lacked access to credit
- Borrowing

These are same endline covariates used in Bryan, Chowdhury, and Mobarak (2014).

6.4 Subgroups

Criteria for program eligibility in this study have changed from those used in prior studies. In the current study, a household is eligible to receive an incentive loan if (i) ownership of cultivable land is less than or equal to 50 decimals *or* (ii) any household member has missed a meals in the previous week. In prior studies, a household was eligible if (i) ownership of cultivable land was less than or equal to 50 decimals *and* (ii) any household member had missed a meal in the previous year. Since prior findings were based on the second eligibility criteria, we plan to conduct our analysis on

1. Household eligible according to the current criteria
2. Household eligible according to the previous criteria

6.5 Implication of the Assignment and Recording Mechanisms

Since the assignment and recording mechanisms rely on the circle-distance between villages in selecting *incentivized* and *spillover-control* villages probabilities of assignment and recording vary within branches, as shown in Figure 4. Thus, while the experimental mechanisms are known, the study's design is only ignorable conditional on the circle-distance characteristics that affect assignment and recording probabilities. For that reason, we will condition our analysis on villages' propensity scores (probability of assignment and recording).

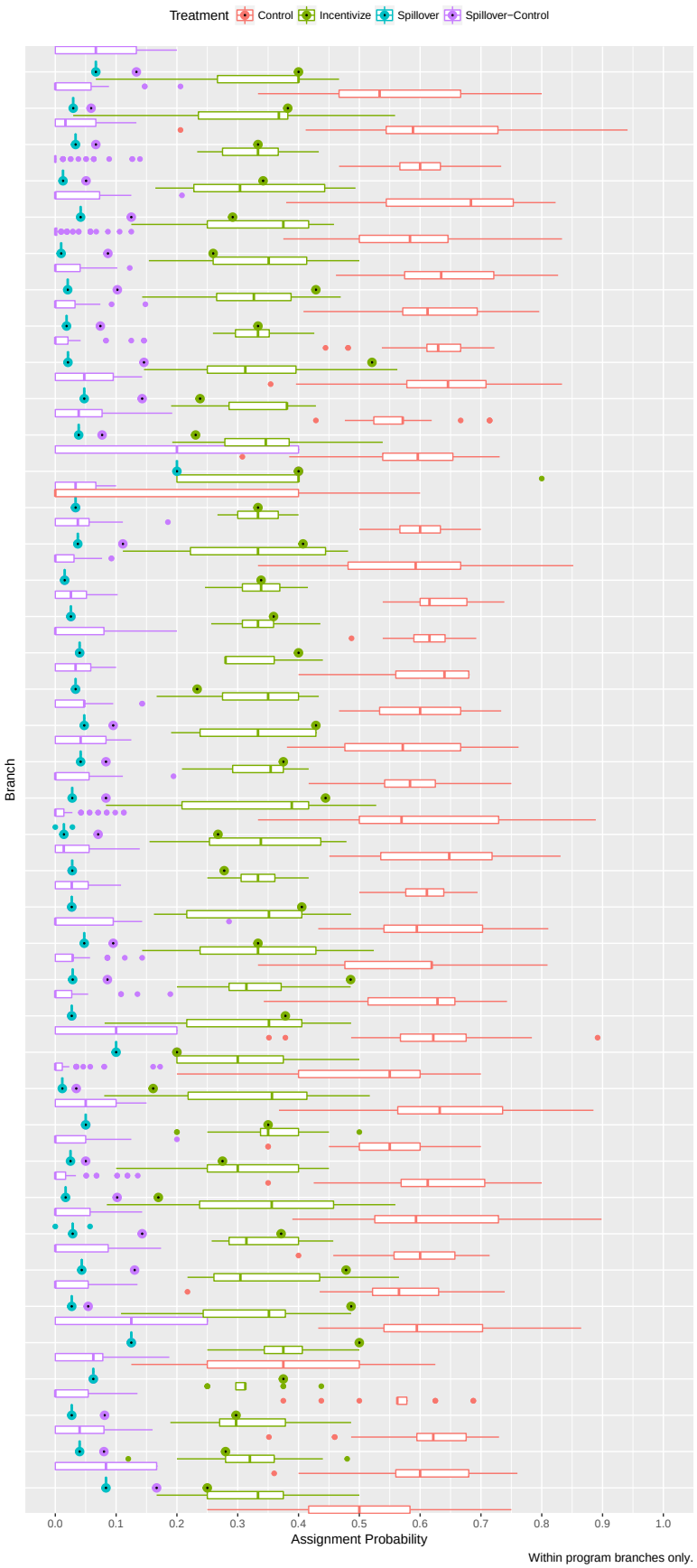

Figure 4: Treatment Assignment Probability

6.6 Empirical Strategy

We are committing to analyze the outcomes of this study using the three approaches described below, each with a slightly different motivation but altogether presenting a more robust inference.

6.6.1 Regression Analysis

For this analysis we will use the following linear model specifications to estimate and test the statistical significance of our primary estimands described above.

1. **spillover-control vs. pure-control**: For all observations i such that $\widetilde{Z}_{j[i]} \in$ {pure-control, spillover-control} we estimate the model

$$Y_{it}^{q} = \alpha_1^q + \beta_1^q \cdot Z_{k[i]}^{p} + \delta_{1,m[i]}^{q} + \varepsilon_{1,it}^{q}, \tag{1}$$

   and test

$$\begin{aligned} \mathrm{H_o} &: \beta_1^q = 0 \\ \mathrm{H_a} &: \beta_1^q \neq 0 \end{aligned}$$

2. **incentivized vs. pure-control**: For all observations i such that $\widetilde{Z}_{j[i]} \in$ {pure-control, incentivized} we estimate the model

$$Y_{it}^{q} = \alpha_2^q + \beta_2^q \cdot Z_{j[i]}^{l} + \delta_{2,m[i]}^{q} + \varepsilon_{2,it}^{q}, \tag{2}$$

   and test

$$\begin{aligned} \mathrm{H_o} &: \beta_2^q = 0 \\ \mathrm{H_a} &: \beta_2^q \neq 0 \end{aligned}$$

3. **spillover vs. spillover-control**: For all observations i such that $\widetilde{Z}_{j[i]} \in$ {spillover, spillover-control} we estimate the model

$$Y_{it}^{q} = \alpha_3^q + \beta_3^q \cdot Z_{j[i]}^{s} + \delta_{3,m[i]}^{q} + \varepsilon_{3,it}^{q}, \tag{3}$$

   and test

$$\begin{aligned} \mathrm{H_o} &: \beta_3^q = 0 \\ \mathrm{H_a} &: \beta_3^q \neq 0 \end{aligned}$$

4. **incentivized vs. spillover**: For all observations i such that $\widetilde{Z}_{j[i]} \in$ {spillover, incentivized} we estimate the model

$$Y_{it}^{q} = \alpha_4^q + \beta_4^q \cdot Z_{j[i]}^{l} + \delta_{4,m[i]}^{q} + \varepsilon_{4,it}^{q}, \tag{4}$$

   and test

$$\begin{aligned} \mathrm{H_o} &: \beta_4^q = 0 \\ \mathrm{H_a} &: \beta_4^q \neq 0 \end{aligned}$$

Where $q \in \{\text{mig, con}\}$, the primary outcomes studies, and δ^q is a subdistrict fixed effect.

We will conduct the following statistical tests:

1. For each individual hypothesis above and for each primary outcome, we will test the null hypothesis of *zero average treatment effect*:
   a) Using conventional cluster robust standard errors
   b) Multiple hypotheses corrected procedures (e.g. List, Shaikh, and Xu (2016), Westfall and Young (1993))
2. For each individual hypothesis above and for each primary outcome, we will test the null hypothesis of *zero treatment effect*, using a Fisher exact test (A. Young 2017).
3. For each primary outcome, we will test the null joint hypothesis of *zero treatment effect* for all experimental treatments using a cross equation joint test as proposed by A. Young (2017).
4. For all primary outcomes and all experimental treatments, we will conduct a omnibus test of no effect (A. Young 2017).

6.6.2 Model-Based Hierarchical Analysis

We will build a hierarchical Bayesian model (Imbens and Rubin 2015; Gelman et al. 2013) to estimate the posterior distribution of the estimands in the Primary Impact Estimands section. This allows us to study the heterogeneity in treatment effect between branches and villages (the experiment clusters). Such a model also enables us to address multiple hypothesis concerns (multiple treatments and outcomes) using regularizing priors, as well as use information more efficiently across model levels.

To calculate the finite sample posterior distribution of treatment effect, we will use Markov Chain Monte Carlo simulation (Carpenter et al. 2017) to estimate the prediction function

$$p(\mathbf{Y}^{mis}|\mathbf{Y}^{obs}, \widetilde{\mathbf{Z}}) = \int_{\theta} p(\mathbf{Y}^{mis}|\mathbf{Y}^{obs}, \widetilde{\mathbf{Z}}, \theta) \cdot p(\theta|\mathbf{Y}^{obs}, \widetilde{\mathbf{Z}})\, \mathrm{d}\theta \tag{5}$$

$$= \int_{\theta} \prod_{j} p(\mathbf{Y}_j^{mis}|\mathbf{Y}_j^{obs}, \widetilde{Z}_j, \theta_j) \cdot p(\theta|\mathbf{Y}^{obs}, \widetilde{\mathbf{Z}})\, \mathrm{d}\theta \tag{6}$$

where θ is a vector of all model hyper parameters and hierarchical parameters; $\mathbf{Y}^{mis}$ is a matrix of unobservable outcomes (columns) for all observations (rows); $\mathbf{Y}^{obs}$ is a matrix of observed outcomes (columns) for all observations (rows); and $\widetilde{\mathbf{Z}}$ is a vector of assigned treatments. This estimated distribution will allow us to make multiple imputations of unobservable counterfactuals. Thus we will be able to calculate the finite sample posterior distribution of treatment effects, $Y_{it}(z) - Y_{it}(z')$, for any treatments z, z'.

Posterior probability functions for all parameters will be estimated using observed data and prior probabilities. Model levels will be comprised of villages (indexed by j), branches (indexed by k), and subdistricts (indexed m).

The posterior probability of the model parameters is

$$p(\theta|\mathbf{Y}^{obs}, \widetilde{\mathbf{Z}}) = \prod_{j} p(\theta_j|\mathbf{Y}^{obs}, \widetilde{\mathbf{Z}}, \theta_{k[j]}) \prod_{k} p(\theta_k|\mathbf{Y}^{obs}, \widetilde{\mathbf{Z}}, \theta_{m[k]}) \prod_{m} p(\theta_m|\mathbf{Y}^{obs}, \widetilde{\mathbf{Z}}, \theta_o) \cdot p(\theta_o|\mathbf{Y}^{obs}, \widetilde{\mathbf{Z}}), \tag{7}$$

where model parameters are linear (and generalized linear) location, scale and correlation parameters for each level (parameters θ_o are the top-level model hyper parameters). Such model parameters will also allow us to calculate finite population and super-population estimands (the same as we do above with finite sample estimands).

7 Document History

Date	
Jan 16, 2018	IPA notified of data embargo (restriction)
Jan 31, 2018	Describing the empirical strategies we plan to use
Feb 1, 2018	Clarifying how primary outcomes will be calculated
Feb 6, 2018	Specifying analysis subgroups
Feb 19, 2018	Plot showing village assignment mechanism, within program branches
Mar 15, 2018	Remove at destination sections (moved to separate document). Update on overlapped pure-control village. Study households map. Covariates. Assignment/recording mechanism implication for analysis.

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