Working paper

International Growth Centre

No Lean Season 2017 summary of results

Gharad Bryan
Mushfiq Mobarak
Karim Naguib
Maira Emy Reimao
Ahish Shenoy

September 2018

When citing this paper, please use the title and the following reference number:
C-89441-BGD-1

DIRECTED BY

FUNDED BY

NLS 2017 Summary of Results

Gharad Bryan[*]
Mushfiq Mobarak[†]
Karim Naguib[‡]
Maira Emy Reimao[§]
Ahish Shenoy[¶]

September 11, 2018

1 Overview

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

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 summary of results document, we focus on the first goal and restricting our analysis to migration, caloric intake, food expenditure, and income. At this early stage in analysis, we find no evidence that the program had an impact (positive or negative) on migration, caloric intake, food expenditure, or income.

We also include an exploratory section where we non-quantitatively consider some possible explanations for the empirical results.

Further work needs to be conducted:

1. Investigate geographic spillover and variation in response to the program.
2. Investigate how prior program years' differed from the 2017 program.
3. Investigate program/implementation quality (observational and qualitative) based on administrative data and new program staff surveys.

2 Study Design

For complete details on the study's design, refer to the pre-analysis plan posted on the AEA RCT registry.

As shown in Figure 1, the study's randomized treatment was clustered over villages and stratified by branch. A branch represents an administrative grouping of villages by the program's implementer, RDRS. Randomization was conducted over two stages:

[*]London School of Economics
[†]Yale University
[‡]Evidence Action
[§]Evidence Action/Yale University
[¶]University of California Davis

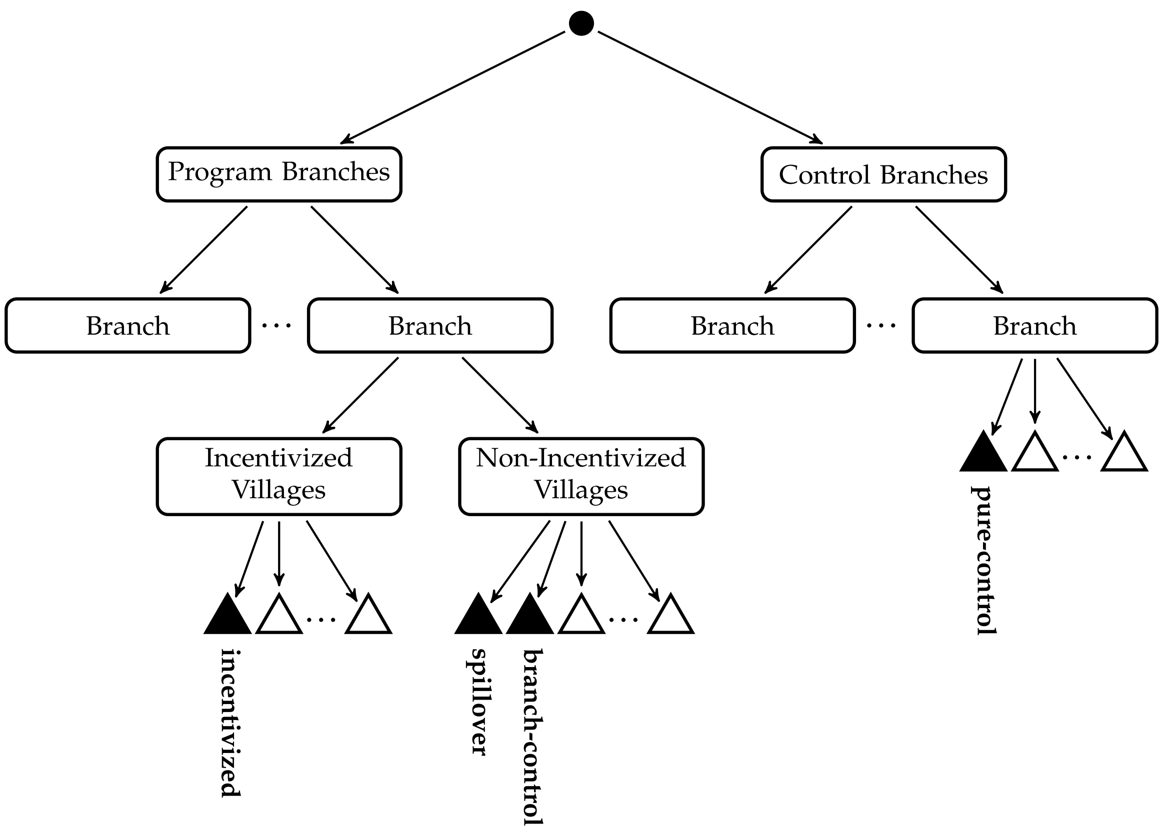

Figure 1: Experiment Design Diagram

1. Branches were randomly assigned to be treated (where the program will be implemented for some villages) or control (no program implementation).
2. Within branch randomization:
   a) For control branches, no villages were offered the migration incentive. These are designated as *pure-control*.
   b) For treated branches, a subset of villages were selected to be incentivized, and the rest are designated as *spillover* villages.
      i. The set of incentivized villages were selected to be in a tight geographic cluster.
      ii. One village in the middle of the cluster was not incentivized and designated as a *spillover* village.
      iii. The remaining un-incentivized villages served by the branch are outside the treated cluster and designated as *branch-control*.
3. We sample one village of each type from each branch for study.
   a) Among untreated branches, we randomly select one village. This set of villages comprises our *pure-control* sample.
   b) Among treated branches, we select
      i. One random *incentivized* village, included in the treated sample
      ii. The village in the middle of the treated cluster designated as *spillover*. Because of the randomization in the assignment to treatment, this can be treated as random sampling.
      iii. One random village from outside the treated cluster but still served by the branch. These villages comprise our *branch-control* group.

Thus we have four treatment arms:

1. *Incentivized* villages. This is the only arm where the program was implemented.
2. *Spillover* villages. These are control villages that are closest to incentivized villages.
3. *Branch-control* villages. These are control villages furthest from the incentivized village while still remaining in a treated branch.
4. *Pure-control* villages. These are control villages from control branches (the furthest possible from program spillover).

Furthermore, our analysis splits the study population into two subgroups:

1. The *old eligible* subgroup. This is the subpopulation of households that meet the No Lean Season program eligibility criteria as defined in prior years (pre 2017).
2. The *new eligible only* subgroup. Eligibility criteria in 2017 were relaxed and thus the eligible population was a super-set of the *old eligible* subpopulation. We define the *new eligible only* subpopulation as that is eligible in 2017 but would not have been eligible pre 2017.

3 Empirical Findings

3.1 Regression Analysis

In this section we present intention-to-treat regression analysis results for the four study arms. For each outcome we present regression tables and level plots, stratified by survey round and eligibility subgroup. Table column headers identify results for the midline and endline regressions and whether we used eligibility subgroup interactions. Reported point estimates are treatment effect estimates, except for the intercept row (pure-control). Level plots report levels for each treatment arm, split by survey round and eligibility subgroup. Level point estimates are shown along with the 90% confidence interval (testing for a zero treatment effect in comparison to the pure-control). The dotted vertical line shows the pure-control level.

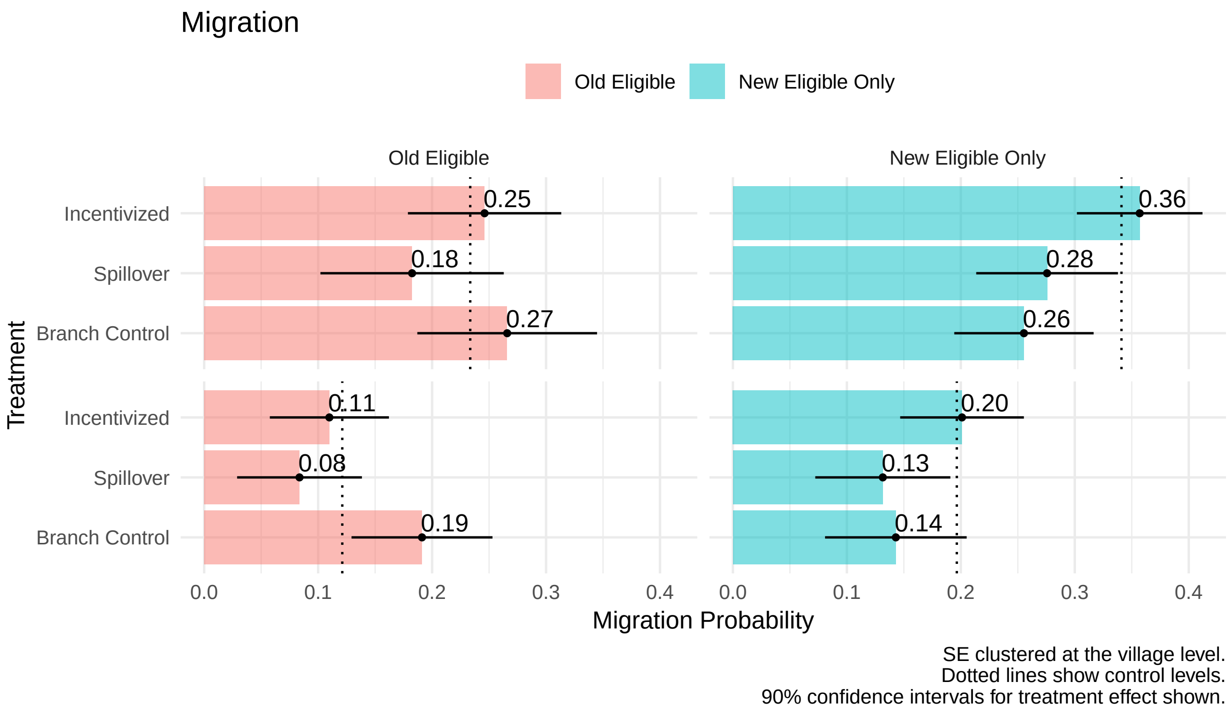

Figure 2: Impact on Migration

3.1.1 Migration

Regression results are presented in Figure 2 and Table 1.

We observe a lower average migration rate in pure-control villages compared that of previous years. Approximately 23% of subjects that were eligible for the program using the 2008 eligibility criteria migrated, as opposed to about 35% in past years.

The 2008-2011 study also allows us to observe migration rates by November of the treatment year (the closest parallel to the 2017 midline timing). In 2008, 27% percent of the control group had already migrated in the four months preceding the survey (August-November); in 2018, the migration rate of the control group using the same eligibility criteria was 23% for September through January. The difference between these two levels is significant at the 95% level, and the fact that the 2018 survey question covers a longer period means that, if the periods were the same, the difference in migration rates between the two years could only be bigger. Altogether, then, we see that base migration rates in 2018 were slightly lower than ten years before.

There is no statistically significant difference in migration rates between incentivized and control villages. This is true regardless of the how migration is measured. Table 1 presents results for migration from September to January 2017 (midline data) and having any positive migration income from roughly October to April (the six months before the endline survey; endline data).

On the other hand there appears to be some statistically significant and negative impact on the migration rate in spillover and branch-control villages. Comparing the migration rate in spillover of spillover or branch-control villages to the pure-control villages, migration rates are lower by about 4-7 percentage points. The exact effect depends on the group used, and statistical significant is not consistent. These effect sizes are stronger for new eligible population. This suggests that there might be some geographic negative spillover from incentivized villages on spillover and branch-control. Recall that both types of villages are in program (treated) branches but differ in their proximity to their branch's incentivized villages. This is a surprising result considering how little direct effect we are detecting on incentivized villages; it seems to be a program effect (responding to the program simply being present) as opposed to the intensity of migration increased in

Table 1: Migration Regression Results

	Survey Round			
	Midline	Endline	Midline	Endline
	By Subgroup		Combined	
	(1)	(2)	(3)	(4)
Intercept	0.233	0.121	0.316	0.158
	(0.074)	(0.044)	(0.026)	(0.047)
Branch Control	0.032	0.070	−0.046	0.003
	(0.048)	(0.038)	(0.038)	(0.030)
Spillover	−0.051	−0.038	−0.073	−0.049
	(0.049)	(0.033)	(0.040)	(0.028)
Incentivized	0.013	−0.011	0.009	0.007
	(0.041)	(0.032)	(0.036)	(0.026)
New Eligible	0.108	0.075		
	(0.031)	(0.027)		
Branch Control × New Eligible	−0.118	−0.123		
	(0.051)	(0.045)		
Spillover × New Eligible	−0.014	−0.027		
	(0.050)	(0.043)		
Incentivized × New Eligible	0.003	0.016		
	(0.046)	(0.038)		
Observations	4,553	4,434	4,553	4,434
R^2	0.106	0.107	0.005	0.102
Adjusted R^2	0.096	0.096	0.004	0.091

Midline migration is based on self-reported seasonal migration, while endline migration is imputed based on having any non-zero migration income. A linear probability model was used for this analysis. All treatment effects are in comparison with the pure-control arm. Standard errors were clustered at the village level and are reported between parenthesis below the point estimates. All specifications include upazila fixed effects. There are two types of regressions: (i) by subgroup, reporting treatment effects by eligibility groups; (ii) combined, without subgroup interaction terms.

Table 2: 2008 Data: Migration Rates by November (Round 2)

Migrated by Nov 2008	Control/Info	Cash/Credit	Total
No	72.86%	57.12%	62.16%
Yes	27.14%	42.88%	37.84%
Total	608	1,292	1,900

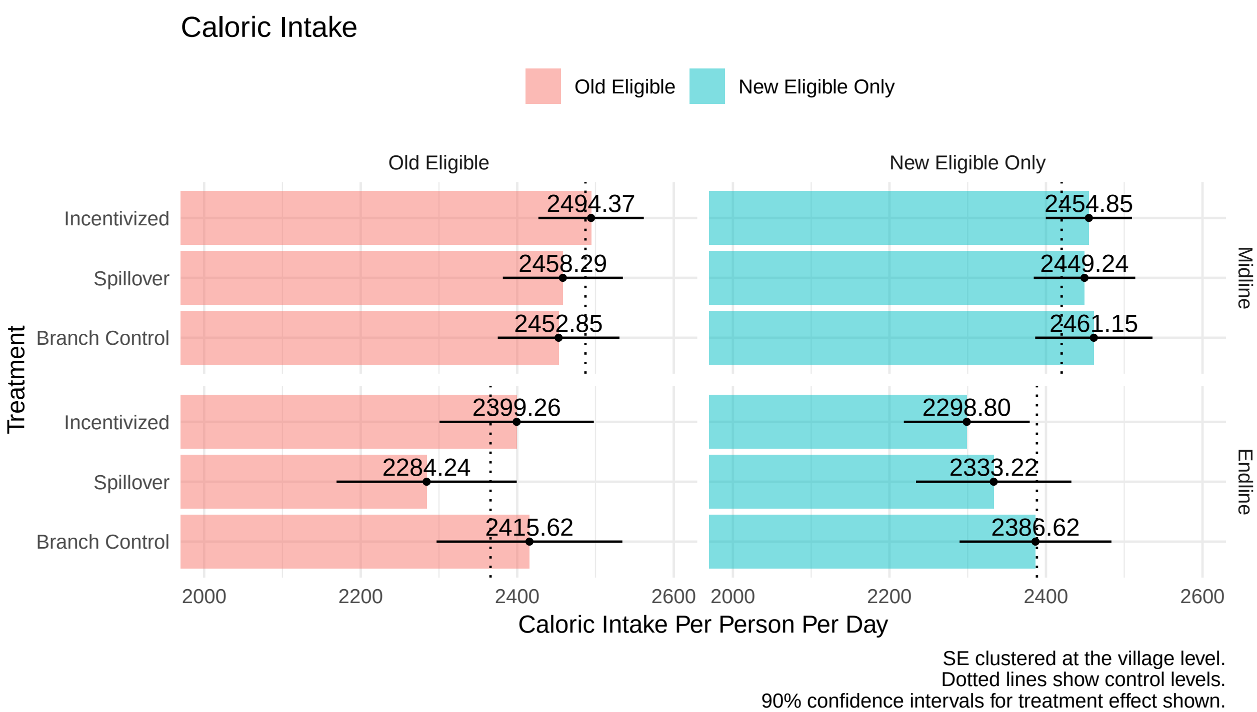

Figure 3: Impact on Caloric Intake

incentivized villages. The potential for such a negative spillover between incentivized and not incentivized villages needs to be further investigated; it could be a sign that the non-interference assumption (or SUTVA) maintained in previous studies does not hold.

3.1.2 Caloric Intake

Regression results are reported in Figure 3 and Table 3.

We find no consistent effect on caloric intake, but we observed generally higher caloric intake per person per day estimates for all arms in comparison to the 2008 study. However, this could be due to the survey timing differences between studies; we did not conduct the survey rounds at the same time across studies.

3.1.3 Food Expenditure

Results results are presented in Figure 4 and Table 4.

Similar to caloric intake, we observe no consistent effect on food expenditure and we find the estimates are generally higher than observed in 2008. For the old eligible subgroup we also see a (not statistically significant) increasing trend going from branch control to incentivized villages. This is pattern worth investigating related to possible spillover or interference.

3.1.4 Income

Results results are presented in Figure 5 and Table 5.

None of the effects estimated are significant but there appears to be qualitatively decreasing trend in net income level from pure-control to branch-control, to spillover, and finally to incentivized villages.

Table 3: Caloric Intake Per Person Per Day Regression Results

	Survey Round			
	Midline	Endline	Midline	Endline
	By Subgroup		Combined	
	(1)	(2)	(3)	(4)
Intercept	2,487.174	2,365.869	2,454.277	2,383.749
	(43.220)	(117.143)	(36.449)	(112.359)
Branch Control	−34.324	49.751	5.505	16.807
	(47.252)	(72.121)	(38.046)	(45.481)
Spillover	−28.887	−81.631	0.978	−67.862
	(46.601)	(69.956)	(34.671)	(45.510)
Incentivized	7.195	33.392	17.355	−43.967
	(40.935)	(59.897)	(29.695)	(37.857)
New Eligible	−67.148	22.576		
	(35.441)	(60.308)		
Branch Control × New Eligible	75.450	−51.574		
	(55.076)	(94.941)		
Spillover × New Eligible	58.102	26.406		
	(52.528)	(95.418)		
Incentivized × New Eligible	27.632	−123.033		
	(46.859)	(79.863)		
Observations	4,380	1,410	4,380	1,410
R^2	0.058	0.134	0.057	0.131
Adjusted R^2	0.046	0.099	0.046	0.100

All treatment effects are in comparison with the pure-control arm. Standard errors were clustered at the village level and are reported between parenthesis below the point estimates. All specifications include upazila fixed effects. There are two types of regressions: (i) by subgroup, reporting treatment effects by eligibility groups; (ii) combined, without subgroup interaction terms. Outlier observations were excluded from analysis where an outlier has an outcome outside the interval $[Q_1 - 1.5 \times IQR, Q_3 + 1.5 \times IQR]$.

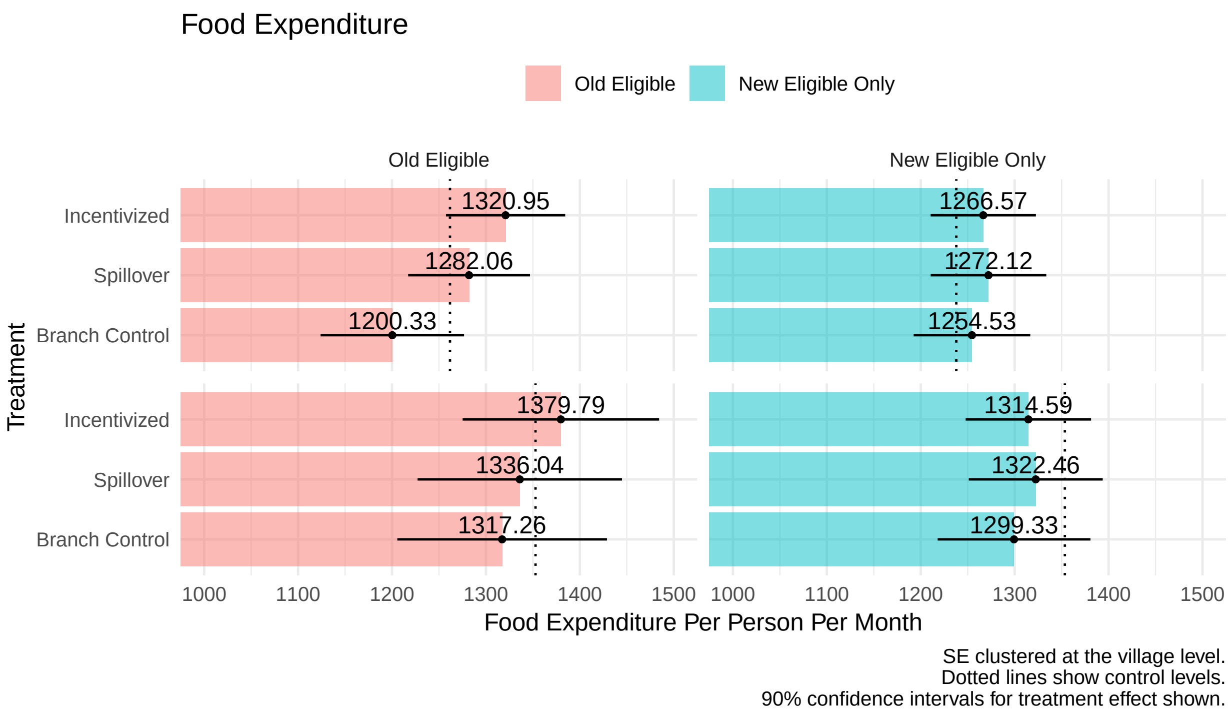

Figure 4: Impact on Food Expenditure

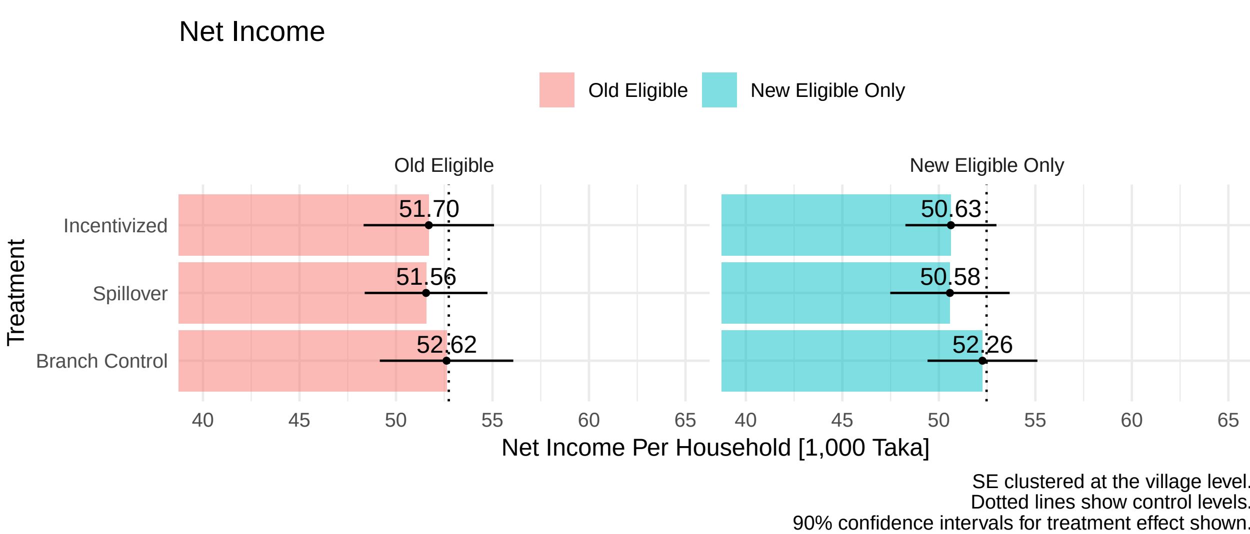

Figure 5: Impact on Net Income

Table 4: Food Expenditure Per Person Per Month Regression Results

	Survey Round			
	Midline	Endline	Midline	Endline
	By Subgroup		Combined	
	(1)	(2)	(3)	(4)
Intercept	1,261.726	1,352.807	1,249.067	1,359.410
	(64.012)	(99.677)	(60.358)	(92.692)
Branch Control	−61.399	−35.550	−18.068	−48.507
	(46.422)	(67.834)	(34.108)	(41.203)
Spillover	20.336	−16.763	26.727	−26.898
	(39.426)	(66.117)	(31.158)	(38.335)
Incentivized	59.220	26.986	36.529	−16.056
	(38.578)	(63.538)	(28.745)	(37.812)
New Eligible	−23.849	0.613		
	(31.275)	(54.874)		
Branch Control × New Eligible	78.055	−18.536		
	(49.664)	(82.835)		
Spillover × New Eligible	13.910	−14.197		
	(44.865)	(77.067)		
Incentivized × New Eligible	−30.530	−65.820		
	(43.503)	(71.090)		
Observations	4,371	1,415	4,371	1,415
R^2	0.061	0.093	0.059	0.092
Adjusted R^2	0.049	0.057	0.048	0.059

All treatment effects are in comparison with the pure-control arm. Standard errors were clustered at the village level and are reported between parenthesis below the point estimates. All specifications include upazila fixed effects. There are two types of regressions: (i) by subgroup, reporting treatment effects by eligibility groups; (ii) combined, without subgroup interaction terms. Outlier observations were excluded from analysis where an outlier has an outcome outside the interval $[Q_1 - 1.5 \times IQR, Q_3 + 1.5 \times IQR]$.

Table 5: Net Income Per Household Regression Results

	By Subgroup	Combined
	(1)	(2)
Intercept	52.735	52.723
	(2.349)	(2.098)
Branch Control	−0.116	−0.213
	(2.102)	(1.345)
Spillover	−1.176	−1.632
	(1.933)	(1.444)
Incentivized	−1.034	−1.589
	(2.054)	(1.214)
New Eligible	−0.263	
	(1.686)	
Branch Control × New Eligible	−0.097	
	(2.706)	
Spillover × New Eligible	−0.720	
	(2.519)	
Incentivized × New Eligible	−0.811	
	(2.432)	
Observations	4,201	4,201
R^2	0.047	0.047
Adjusted R^2	0.034	0.035

All treatment effects are in comparison with the pure-control arm. Standard errors were clustered at the village level and are reported between parenthesis below the point estimates. All specifications include upazila fixed effects. There are two types of regressions: (i) by subgroup, reporting treatment effects by eligibility groups; (ii) combined, without subgroup interaction terms. Outlier observations were excluded from analysis where an outlier has an outcome outside the interval $[Q_1 - 1.5 \times IQR, Q_3 + 1.5 \times IQR]$.

4 Explanation

In this section, we summarize some possible explanations under consideration for why the empirical results above differ from results found in previous rounds of research. All these hypotheses are post hoc hypotheses; they were not part of the pre-analysis plan and were only considered in response to the above results. Some of the motivation behind them was based on direct communication with implementation officers while others were based on the PI's or the organizational domain knowledge. While these are plausible scenarios, the evidence behind them is weak:

- Inference does not adjust for the large number of hypotheses considered or the "forking paths" of causality explored.
- This is a purely observational investigation:
  - The results presented in this section should not be interpreted as causal because they are all retrospective.
  - The experiment was not designed with these comparisons in mind, so we cannot rule out omitted variables or spurious correlations.
- We do not exhaustively explore all possible mechanisms nor do we conduct a model selection analysis over a set of plausible models.

With this caveat, we describe three 'types' of explanations we have identified below. For each type of explanation, are working to outline the channels through which program effectiveness may have been altered. We will then seek trends in the data we have collected that would be consistent with each channel and give some insight into how important the channel could potentially be. Data and results uncovered in this way would be designed to guide future implementation, by focusing on factors that may have limited program effectiveness, and to generate hypotheses to test in future evaluation rounds.

The first explanation focuses on an intentional program design change from previous years. This year, we expanded the program eligibility criteria to include more potential loan recipients. In the past, the program had excluded those with recent previous migration experience because they were most likely to be able to migrate even without a loan. It does not appear that this change alone accounts for the difference between current and past results. If we break down treatment effects between those who would have been eligible only under the old criteria and those who are newly eligible, effects among both subgroups mirror the pooled results.

The first explanation focuses on an intentional program design change from previous years. This year, we expanded the program eligibility criteria to include more potential loan recipients. It does not appear that this change alone accounts for the difference between current and past results. If we break down treatment effects between those who would have been eligible only under the old criteria and those who are newly eligible, effects among both subgroups mirror the pooled results.

The second set of explanations focus on unintentional implementation changes caused by the change in eligibility, the vastly expanded scope of the program, or other factors. In the most recent round, it is possible that Migration Officers (MOs) focused their efforts on those households who were most likely to migrate even without a loan to the exclusion of the target population households who need a loan to afford migration. Such behavior may have even been encouraged by policies such as loan targets set by the NGO to manage implementation at such a large scale. We have implemented a qualitative survey to understand the incentives and actions of MOs last year, and are revising our instructions to avoid any possibility of this issue this year.

A second point of implementation failure may have been in the loan conditionality. Migration loans were intended to include migration as a condition of loan receipt. However, it is possible that this conditionality was poorly enforced, leading many to accept the loan and report migrating without any actual migration. We are working on linking our independent survey data to the program administrative data to determine how potentially large the issue of misreporting may be.

Finally, the third set of explanations focuses on true program effects. It is possible that what we observe this year may be the true effect of the No Lean Season program when implemented at scale. This may be because conditions in rural Bangladesh have changed since the initial years of success, spillovers at scale cancel out any gains observed in small-scale pilots, or pilot villages were selected because they were most likely to be receptive to the program. The experimental design, both last year and this year, is explicitly intended to capture spillover effects so we can try to evaluate this explanation. We are still working on devising research strategies to address other possible explanations if the true effect at scale is diminished from the pilot.

Alternately, it may be the case that we observe the true effect this year, but it is diminished due to temporary factors in 2017 alone. Most notably, the program was affected by severe flooding in many regions, and implementation was subsequently delayed as well. We are still evaluating whether these regions are the ones with the most diminished effects, although we lack the data in control areas to conduct an experimental comparison.

The International Growth Centre (IGC) aims to promote sustainable growth in developing countries by providing demand-led policy advice based on frontier research.

Find out more about
our work on our website
www.theigc.org

For media or communications
enquiries, please contact
mail@theigc.org

Subscribe to our newsletter
and topic updates
www.theigc.org/newsletter

Follow us on Twitter
@the_igc

Contact us
International Growth Centre,
London School of Economic
and Political Science,
Houghton Street,
London WC2A 2AE

International
Growth Centre

DIRECTED BY

FUNDED BY

Designed by soapbox.co.uk