3ie Registry

Can gender- and nutrition-sensitive agricultural programs improve resilience?

Pre-Analysis Plan: Addenda

This note includes additional information on our pre-analysis plan not included in the fields required for the RIDIE registration.

Sample

The original ANGeL study aimed to assess interventions that can leverage agricultural growth to increase farm household incomes, improve nutrition, and enhance women's empowerment in Bangladesh. There were three types of training interventions:

1) *Agriculture Production:* Facilitating the production of high-value food commodities that are rich in essential nutrients.

2) *Nutrition Knowledge:* Conducting high-quality BCC to improve nutrition knowledge of women and men.

3) *Gender Sensitization:* Undertaking gender sensitization training that leads to an improvement in the status/empowerment of women and gender parity between women and men.

Accordingly, ANGeL was implemented as a clustered randomized controlled trial with the following arms:[1]

T-A: Agricultural Production training

T-N: Nutrition BCC

T-AN: Agricultural Production training and Nutrition BCC

T-ANG: Agricultural Production training, Nutrition BCC, and Gender Sensitization

C: Control

Because training would be conducted by government agricultural officers called SAAOs, and each SAAO was assigned to a geographic area called a "block," it was determined that cluster-randomization would be conducted at the block level, using blocks as clusters. Working with the Ministry of Agriculture, the original study team identified all rural upazilas (sub-districts) that were agro-

[1] The RCT included one additional nutrition BCC treatment arm, in which community women delivered the nutrition intervention rather than agricultural extension agents. This arm was not re-surveyed in 2022 and is not used in this analysis.

ecologically suitable for agricultural diversification and had good market connectivity, thus considered appropriate for the ANGeL interventions. From a list of 484 such upazilas, 16 upazilas were purposively selected, such that each of the eight administrative divisions of Bangladesh was represented. From the list of all 525 blocks in 16 upazilas, we randomly selected 10 blocks from each upazila, yielding 160 blocks. These were randomly assigned as follows: 25 blocks to each treatment arm (T-A, T-N, T-AN, T-ANG, as well as the additional treatment described in footnote 1), and 35 blocks to the control group. One village from each block was randomly selected. Within each of these villages, 25 farm households with at least one child under 24 months were randomly selected to participate. This yielded 625 households in each treatment arm (2,500 households in total) and 875 households in the control group, for a total sample of 3,375 households.

In the 2022 post-program survey, we will trace and interview households in the following treatment arms: **T-A**; **T-AN**; **T-ANG**; and the control group, **C**. We will not re-survey arms **T-N;** this treatment arms showed the smallest impacts at endline and we did not have sufficient budget to include them.

Analysis of the baseline data has shown that the sample was balanced across treatment arms; where balance was assessed using the control variables that appear in the treatment equation (see below). Given that we have multiple treatment arms, we will estimate a multinomial logit where the outcome variables are the treatment arms, the base category is the control group, and the covariates are the control variables that appear in the treatment equation (see below). We will assess balance primarily by looking at the prob-value of the F statistic for the null hypothesis that the estimated parameters are jointly zero; we will also look at the t statistics associated with the individual covariates.

Data

Baseline data were collected between November 2015 and January 2016. Endline data were collected between January and March 2018, ensuring minimal seasonal difference between baseline and endline surveys. In each household, both the primary female beneficiary and primary male beneficiary were interviewed. Although the male and female beneficiaries were interviewed separately, some modules were answered by only the male (e.g., household demographics, assets and wealth, agricultural production), some were answered by only the female (e.g., food consumption and food security, diet data, anthropometry, women's status, and decision-making autonomy) and some were answered separately by each (e.g., data needed to construct measures of empowerment, gender attitudes, time preferences, agency).

In February-April 2022, we traced and re-surveyed households in the **T-A**; **T-AN**; **T-ANG** treatment arms and in the control group, **C.** The survey instruments were slightly modified (for example, it includes questions on shocks such as Covid-19 and Cyclone Fani; recall dates were updated) but largely followed the same approach used in 2015-16 and 2018.

Primary outcomes to be constructed

Our study has three primary outcomes:

- Assets solely and jointly owned by women and men. We include this as a primary outcome because, as described in the main registry document, we seek to assess whether these treatment arms enabled households to be more resilient to shocks, including Cyclone Fani and Covid-19
- Pro-WEAI. We include this as a primary outcome because, as described in the main registry document, we seek to assess whether the effects of these treatment arms on women's empowerment persisted after the intervention ended.
- Household Global Diet Quality Score. We include this as a primary outcome because, as described in the main registry document, we seek to assess whether the effects of these treatment arms on the quality of household diets persisted after the intervention ended.

Assets. Assets include both land and nonland assets. Information on assets was collected in the questionnaire administered to the male household head, based on an extensive listing of assets (including agricultural productive assets, nonagricultural productive assets, consumer durables, jewelry—an important asset for women—and housing). The survey also collected information on land and water bodies, which are important for aquaculture. The land and assets modules allowed the respondent to name the three owners of the asset, allowing us to classify all assets as male-owned, female-owned, or jointly-owned assets. The respondent also gave information on the current market value of each asset; however, we note that because land sales are relatively uncommon, data on land values may be subject to error.

Given all this, we do the following. Our basic measure of asset ownership will be the value of the following assets: agricultural productive assets, nonagricultural productive assets, consumer durables, and jewelry. We will assess robustness of our results to alternative asset measures that will include the value of land and the quality of housing stock. We will also assess whether our results are sensitive to outliers by log (or Inverse Hyperbolic Sine) transforms or by winsorizing at the 2^{nd} and 98^{th} percentiles.

Pro-WEAI. Our measure of women's empowerment is the pro-WEAI, an additive and decomposable index based on the Alkire-Foster methodology adapted from the WEAI (Alkire et al. 2013) for use in agricultural development projects (Malapit et al. 2019). Pro-WEAI is based on a weighted adequacy count across 12 indicators. The 12 indicators attempt to measure three types of agency corresponding to the domains of intrinsic agency, instrumental agency, and collective agency. The indicators of intrinsic agency comprise: autonomy in income, self-efficacy, attitudes about IPV against women, and respect among household members. Instrumental agency indicators include: input in productive decisions, ownership of land and other assets, access to and decisions on financial services, control over use of income, work balance, and visiting important locations. Finally, the collective agency domain includes two indicators: group membership and membership in influential groups. For each of these indicators, individuals are classified as adequate or inadequate based on pre-determined thresholds used in the pro-WEAI. The pro-WEAI is composed of the 3DE sub-index (three domains of empowerment, the pro-WEAI analogue of the five domains of empowerment (5DE) in the WEAI), which measures the extent and depth of empowerment, and the Gender Parity sub-index, which measures gender parity between women and men in the same household. To assess overall empowerment, we use: (1) the individual empowerment score, defined as the weighted sum of the 12 pro-WEAI indicators; this score ranges from 0-1; and (2) and the individual's empowerment status, which classifies an individual as empowered if his or her empowerment score is greater than or equal to 75% of the weighted sum of the 12 binary pro-WEAI indicators.

Household Global Diet Quality Score. This is an adaptation of a recently developed indicator of diet quality (Bromage et al. 2021), the GDQS. The GDQS consists of 25 food groups: 16 healthy food groups, 7 unhealthy food groups, and 2 food groups (red meat, high-fat dairy) that are unhealthy when consumed in excessive amounts. For 24 of the GDQS food groups, three ranges of quantity of consumption are defined (in grams/day) and used in scoring the metric: low, medium, and high. For one food group (high-fat dairy), four ranges of quantity of consumption are used: low, medium, high, and very high. The points associated with the healthy GDQS food groups increase for each higher quantity of consumption category. The points associated with the unhealthy GDQS food groups decrease for each higher quantity of consumption category. For the two food groups that are unhealthy in excessive consumption (red meat, high-fat dairy), the points associated with the GDQS food group increase up to a certain threshold of quantity of consumption, after which the points decrease. The overall GDQS is a sum of the points across all 25 GDQS food groups.

The definition of GDQS is at the individual-level, wherein each respondent receives points for each GDQS food group, according to the quantity of consumption consumed for that food group during the 24-hour reference period. We will compute a version of it using the survey's detailed food consumption module. Because our analysis of ANGeL is at the household level, and our household-level food consumption data are based on data from the seven-day recall of household food consumption. we construct a variation of the GDQS at the household level, which we refer to as the household-level GDQS (hGDQS). Specifically, we analyze each household's consumption of the various food groups over the 7-day recall, then convert these to a daily adult equivalent. The GDQS has a range from 0 to 49. In our empirical analysis, we will log transform this outcome; we will also assess whether our results are sensitive to expressing this outcome in logs or levels.

Treatment effect equation

We will estimate intent-to-treat (ITT) impacts. For all outcomes of interest for which we have baseline values, we use an ANCOVA specification (McKenzie 2012):

$$Y_{ibt} = \alpha_t + \beta_Y Y_{ibt-1} + \beta_A TA_b + \beta_{AN} TAN_b + \beta_{ANG} TANG_b + \beta_X X_{ibt-1} + \varepsilon_{ibt} \quad (1)$$

where Y_{ibt} is the outcome of interest for individual *i* residing in block *b* at time *t*; Y_{ibt-1} is the outcome in the prior period (baseline); TA_b, TAN_b, and $TANG_b$ are dummy variables that take the value of 1 if block *b* was assigned to T-A, T-AN, and T-ANG, respectively, and takes the value of 0 otherwise; X_{ibt-1} is a vector of baseline covariates; and ε_{ibt} is an error term. β_A, β_{AN}, and β_{ANG} represent the impact estimators for T-A, T-AN, and T-ANG, respectively. For outcomes of interest that were not collected at baseline (such as the Pro-WEAI) only at endline (such as knowledge of correct agricultural practices), our estimation relies on single-difference estimates that do not include baseline values of the outcome variables.

We include the following baseline covariates, intended to capture demographic and socioeconomic characteristics, human capital, land and labor availability, as well as access to information prior to intervention: age of household head, sex of household head, mean education level of males age 18 and older, mean education level of females age 18 and older, number of adults in the household, dependency ratio, wealth index, whether the household had access to electricity, amount of land was owned at baseline, whether any fishponds were owned at baseline, the number of mobile phones owned, whether the household owned a television, whether the household had recently received an extension visit for crop production, whether the household had recently received an extension visit for livestock or fish production, and dummies for location (upazila). As a robustness check, we will also estimate equation (1) excluding baseline control variables.

We estimate ordinary-least-squares regressions for outcome variables that are continuous and linear probability models where the outcomes are dichotomous. Some of our secondary outcomes relating to levels of specific types of foods produced and consumed (homestead vegetables, homestead fruits, eggs, dairy, fish) contain both many zero values as well as many very large values. For these outcomes, we use the inverse hyperbolic sine (IHS) transformation and report marginal effects following Bellemare and Wichman (2020). In all cases, standard errors are clustered at the block level, which is the level at which the randomization was conducted. We conduct Wald tests to assess whether the difference in impacts estimated from T-SAAO and T-APK are statistically significant.

We will undertake exploratory sub-group analysis, assessing whether impacts differ by household characteristics such as education of household head and land holdings. We will also disaggregate by exposure to shocks such as Cyclone Fani and the Covid-19 pandemic; the exact way in which we will do so will depend on the extent to which these shocks affected the localities where our sample resides.

Statistical power

Our power calculations draw on the 2015/16 baseline data for asset levels and for hGDQS. For Pro-WEAI women's empowerment, we use the 2018 values for the control group (recall that Pro-WEAI is not available in the 2015/16 baseline). We set 80% power and 0.05 level of significance. Accounting for intra-cluster correlations, we are powered to detect relative to the control group, for any given treatment arm, a 30 percent increase in asset holdings, a 10 percent increase in women's empowerment (as measured by Pro-WEAI), and a 10 percent increase in hGDQS.

Multiple hypothesis testing

We have a single outcome for each of the three domains we will assess in this study. For this reason, following Leroy et al (2022), in our initial estimates, we will not adjust our results for multiple hypothesis testing. However, our analysis also includes secondary outcomes which provide a broader picture of the post-program impacts of ANGeL. For example, in addition to the hGDQS, we will have measures of household diets that include caloric acquisition per capita and the household Dietary Diversity Score.

Consequently, when assessing the impacts of ANGeL across a range of primary and secondary outcomes (such as those relating to household diet), we will calculate Romano-Wolf stepdown adjusted p-values using the Stata rwolf2 routine (Clarke, Romano, and Wolf, 2020).

Survey attrition

We will estimate correlates of attrition, paying close attention to whether attrition is associated with treatment status and the baseline covariates described above. We will construct an F test where the null hypothesis is that the coefficients on the treatment variables are jointly zero. Inclusion of the baseline covariates will assist in remedying selection attrition by these characteristics. If attrition exceeds 15 percent (ie more than two percent per year since the baseline survey) we will assess whether our results are robust to corrections for selective attrition such as Lee bounds.