# Big Push for the Rural Economy
# Pre-Analysis Plan - II

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## 1. Abstract

The Big Push for the Rural Economy (BPRE) program provides vocational skills training in the agriculture and livestock sectors in four high-poverty districts of South Punjab.[1] By infusing frontier skills and practices simultaneously at multiple nodes of the value chains, BPRE presents an opportunity to rigorously evaluate the impact of "big push" intervention on village-level GDP, productivity, household income and the extent of spillovers.

This pre-analysis plan has been filed prior to analyzing results of the second post-treatment survey, which occurred between March and May of 2019 after the first post-treatment tracker in 2018 and pre-treatment surveys in 2016 and 2013.

## 2. Motivation

Agriculture, livestock and their related sectors dominate Pakistan's rural economy, but high dispersion of productivity and skills keeps income and output growth stagnant (Rasul et. al., 2012; Ahmed & Gautam, 2013; Pankaj & Ramyar, 2019). To improve productivity in these sectors, BPRE delivers trainings of frontier knowledge and skills at different stages of the production cycle while exploiting the complementarities and economies of scale that result from the "big push" model (Murphy, Shleifer, & Vishny, 1989; Kremer, 1993; Sachs, 1999; Nankhuni & Paniagua, 2013; Cheema, Khwaja, Naseer, & Shapiro, 2015; Bedoya et. al., 2019).

## 3. Research Questions

Our primary question is whether "big push" style trainings can help fight poverty. More specifically, we will examine whether the BPRE intervention improves trainees' knowledge and production skills; whether trainees update their existing production practices; and, ultimately, whether improved skills and practices enhance yields and income as well as related household socio-political and economic outcomes.

## 4. Research Strategy

### 4.1 Treatment

Based on a close examination of the agri-livestock value chains and demand for skills, the BPRE intervention delivered skills trainings in synchrony with the production cycle. The Punjab Skills

[1] PEOP's exclusive focus was on Punjab's four high-poverty districts located in the South: Bahawalpur, Bahawalnagar, Lodhran and Muzaffargarh (which will be referred to as the original PEOP districts in this report).

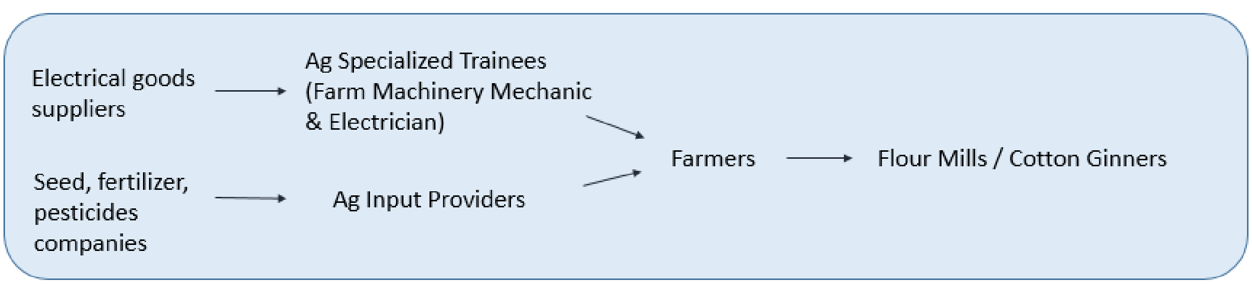
Development Fund (PSDF) recruited private companies with rich experience in the local agriculture and diary sectors to design and implement the intervention. Agriculture skills trainings focused on wheat and cotton, the most common crops in South Punjab, while livestock skills trainings concentrated on milk production.

Agriculture and livestock skills trainings rolled out on staggered and ongoing basis between late 2016 and mid-2018. Wheat training began in December 2016 and ended in May 2017, followed immediately by cotton training that concluded in November 2017. In the second half of 2017, BPRE also hosted trainings on kitchen gardening and farm food processing. Livestock training occurred between April 2017 and March 2018. Each training went through similar stages: facility setup, mobilization and invitation, application and registration, enrollment, enrollment confirmation and training. Noticeably, during the mobilization stage, CERP conducted multiple visits and distributed encouragement vouchers to households that engaged in crop or milk production (directly by owning land/animals or indirectly by working for others) because we expect these households to benefit more from the treatment. We refer to these households as “eligible” and the rest as “non-eligible”.

To infuse skills and knowledge in other nodes of the value chains, the following individuals also received trainings or attended information sessions over the course of 2017 and 2018: agriculture extension agents, agro dealers, farm machinery mechanics, electricians, farm supervisors, animal health workers, village milk collectors (VMC) and artificial insemination technicians (AIT).

**Livestock Value Chain**


**Ag Value Chain**




Following skills trainings, half of the treatment villages were also offered a market linkage component. This was implemented in the form of two separate *melas* or “fairs” per village, where all farmers and specialized trainees were invited to a central location in the village and introduced to each other. These *melas* connected trained farmers with other players in the value chains, such as farm mechanics and animal health workers. Furthermore, downstream buyers such as milk bottling companies and cotton ginning factories were also invited. The objective of this exercise was for farmers to be aware of the additional services available in their village that could potentially help increase their productivity, as well as linking them to potential buyers. Two village *melas* were conducted after the main training courses in agriculture and livestock had been completed; one *mela* focused on agriculture and the other on livestock. The two *melas* were conducted between April and August 2018. Village level participation in these *melas* was high; on average, 76% of the total trainees in a village attended at-least one *mela*.

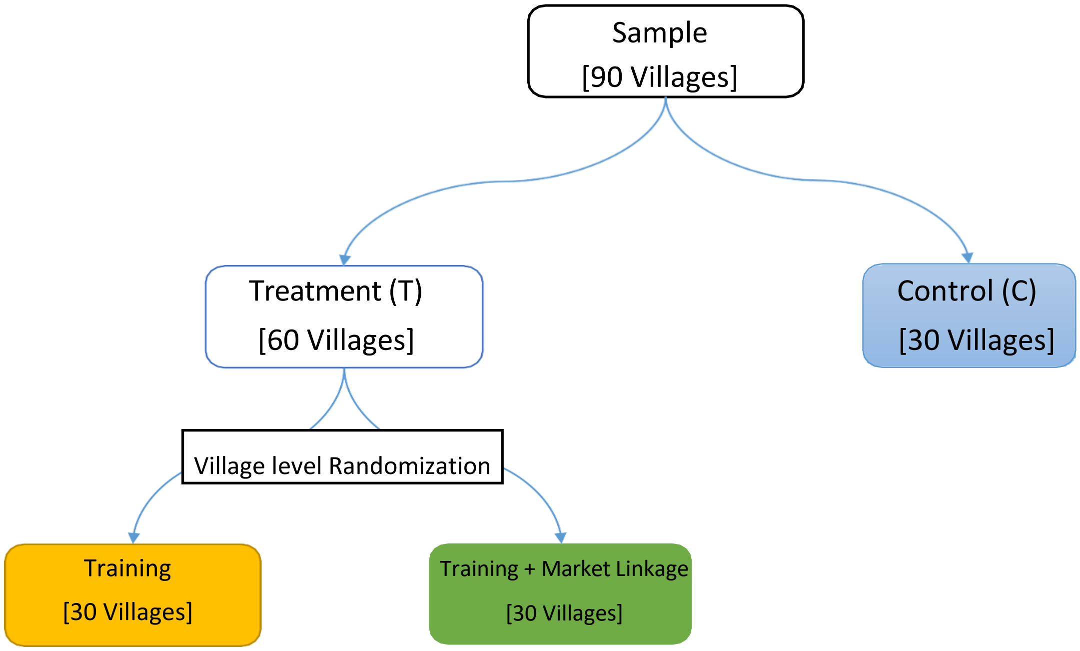
The intervention concluded in the summer of 2018. The Center for Economic Research in Pakistan (CERP) was responsible for conducting surveys and collecting program data for program monitoring and evaluation.

## 4.2 Sampling & Randomization

The BPRE sample consists of 90 villages in four high-poverty Punjab districts (Bahawalnagar, Bahawalpur, Lodhran and Muzaffargarh). We first manually drew 15 grids to ensure that each grid has a group of similarly sized and geographically contiguous villages. Stratifying on the grids, we randomly assigned 30 villages to be control villages ("C" - where no program is offered) and the remaining 60 villages to one of two treatment arms – T1 and T2. The 30 "T1" villages received skills trainings only while the 30 "T2" villages received, in addition to trainings, the market linkage intervention.[2] Within both T1 and T2 villages, all eligible households received encouragement in the form of home visits and vouchers to enroll in the trainings, whereas non-eligible households received no encouragement but could still access the treatment.

The number of sample households in each village varied with the village size. The budget and preliminary analysis initially assumed 140 households per village on average. Given heterogeneity in village size and logistical constraints, the final sample was adjusted down in smaller villages and up in larger villages. To do so, we split the sample villages into four size quartiles by village population. A sample of 92 households were randomly drawn from the smallest villages, whereas villages in the largest size quartile got a sample of 188 households. The average was around 40% of the village sampled, though this naturally varied across village size. Our sample eventually amounts to around 12,710 households (with almost 70% involved in either agriculture or livestock production).

[2] Logistical and budgetary constraints do not permit including a set of villages which are only linked to the market (i.e. no training is given). Moreover, PSDF is not interested in just evaluating the impact of market linkages since that is not its mandate.




## 5. Measurement

Our outcomes of interest include the following broad categories: knowledge of best practice, production practice and input, animal health and mortality, agricultural and milk production, income from agri-livestock production, financial behaviors (borrowing, lending, savings) of the households, household food and non-food expenditures, child health, and political participation. Because BPRE intervention also trained specialized technicians such as farm mechanics, electricians, VMCs, animal health workers, AITs, and farm supervisors, we will also measure village-level access to these specialized service providers and their quality of service.

As summarized in Table 1, we plan to expand each broad category of outcomes to a list of specific variables or index scores based on survey data. We consider these variables our main outcomes of interest for the analysis related to the endline survey.

TABLE 1. Main Outcomes of Interest

| Category | Outcome Variables |
|---|---|
| Financial Management / Behaviors (Borrowing/Lending/Savings) | Dummy indicators of HH engagement in borrowing/lending/savings<br>Yearly total values of borrowing/lending/savings levels (rupees)<br>Log of yearly borrowing/lending/savings<br>Ratio of borrowing/lending in agri-livestock sector to total borrowing/lending<br>Ratio of savings utilized for agri-livestock purchases to total savings spent<br>Increase in borrowing/lending/savings for agri-livestock sectors to past (ratio) |
| Agriculture: Land Allocation and Crop Production | Amount of land allocated to crops/vegetables<br>Yearly wheat, cotton and vegetables output levels (maund)<br>Log of yearly wheat, cotton and vegetables output levels<br>Yearly wheat, cotton and vegetables yields (maund per kanal)<br>Log of yearly wheat, cotton and vegetables yields<br>Dummy indicators of HH engagement in agricultural production<br>Increase in wheat, cotton and vegetables production compared to past (ratio) |
| Livestock: Animal Ownership and Milk Production | Number of animals owned<br>Daily milk output level (liter)<br>Daily milk output per animal (liter per animal)<br>Dummy indicators of HH engagement in milk production<br>Log of daily milk output level<br>Increase in milk production compared to past (ratio)<br>Log of daily milk output per animal |
| Income | Yearly revenue/profits from crop sales (level, log, & ratios)<br>Daily revenue/profits from milk sales (level, log, & ratios)<br>Total monetary value of annual crop and milk output (level, log, & ratios) |
| Knowledge of Best Practices | A weighted sum of correct responses to survey questions on knowledge about water use, fertilizer and pesticide use, animal feed amount, animal health, milk storage, awareness of and response to climate change, and preservation of soil quality (for agriculture & livestock) |
| Input Use and Practice | A weighted sum of correct responses to survey questions on actual practice of water use, fertilizer and pesticide use, animal feed, animal health care, milk storage, and preservation of soil quality (for agriculture & livestock) |
| Animal Mortality | Number of animals lost from the past 12 months prior to survey time (0 if HH owns no animal) |

| | |
|---|---|
| | Fraction of animals lost from the past 12 months prior to survey time (0 if HH owns no animal) |
| Specialized Service Providers | At HH Level:<br>Dummy indicator of service availability in the village<br>Number of service providers in each village<br>Quality of service provided<br>At Village Level:<br>Proportion of HHs (at village level) that can reach out to these service providers<br>Proportion of HHs (at village level) who are satisfied with the quality of service |
| Child Health | Body Mass Index (BMI)<br>Stunting<br>Wasting |
| Household Expenditure | Monthly Food Expenditures of the HH (Levels, Logs)<br>Monthly Non-food Expenditures of the HH (Levels, Logs) |
| Wellbeing | Wellbeing Index (k6)<br>Financial Security/Stability Index and<br>Asset Index using Principal Component Analysis (PCA) |
| Labor Market | At HH level:<br>Net Earnings by providing laborer in agricultural and livestock related activities (Level and Log)<br>Number of members providing agricultural or livestock related laborer |
| Political Engagement | Dummy indicator of participation i.e., whether voted or not<br>A weighted sum of correct responses to survey questions on President of the country and Chief Minister of the province |

In addition to household-level analysis, we intend to examine the abovementioned outcome variables at the village level either by aggregating them or by using other sources of village-level data (e.g. administrative and/or survey data available at the village level for production, availability of specialized services, etc.)

# 6. Analysis

## 6.1 Balance Checks

We will test for balance along pre-treatment characteristics, which we collected in 2013 & 2016 through baseline surveys, between treatment (T1 & T2) and control groups (C). These characteristics will include borrowing, lending, savings, agricultural and livestock knowledge and practice index scores (2016 only); wheat, cotton and milk output levels and productivity; and household-level characteristics such as land quality, access to water, wealth index score, household head literacy, and political participation.

Because we randomly assigned villages to T1 and T2, we will also perform balance tests between these two sub-groups and expect balanced results.

The following regression summarizes our approach to the balance checks:

$$Y_{ij,t=1} = \beta_0 + \beta_1 T_j + \gamma + \varepsilon_{ij}$$

where we use $i$ to index household, $j$ village and $t$ survey round.[3] The left-hand-side variable, $Y_{ij,t=1}$, refers to the baseline value of a main outcome of interest or household-level characteristic. $T_j$ is a dummy variable indicating the village's treatment status and will be further split into T1 and T2 to specify which treatment arm the village was assigned to. $\gamma$ represents grid fixed effects.

We will control for household-level characteristics that appear imbalanced in the final statistical evaluation of BPRE's impact.

## 6.2 Main Regression Specifications

Because we randomly assigned BPRE treatment at the village level, we plan to measure the impact of BPRE in two forms: intent-to-treat (ITT) and local average treatment effects (LATE). We should note that both these effects are not really capturing the effect of a particular household being trained but rather the aggregate effect of a village (community) having (a fraction of its population) received training i.e. the impact of a "big push" in aggregate human capital.

As we have two post-treatment surveys, the shorter 2018 tracker and the 2019 endline, we will pool the two datasets for outcomes on which we have information in both the survey rounds and apply time fixed effects. We will also test separately for short-term and long-term impacts of training by interacting with a dummy that separates out the two post rounds.

As opposed to 2018 and 2016, for the endline 2019 we reached out to both male and female respondents in each HH. We plan to run the regressions at HH level as well as at individual level (2019 only). Running these specifications at individual level will allow us to look at effects separately by individual attributes such as gender, age etc. The interaction of gender dummy with treatment status would tell us the differential impact of treatment by gender. We will evaluate the impact of treating multiple members in one HH (both male and female for instance) on our main outcome variables. For the HH level regressions, we will take average of the male and female responses. For instance, HH's milk production will be constructed as the average of milk production reported by the male respondent and by the female respondent of that HH.

### 6.2.1 ITT

#### *6.2.1.1 ITT without covariates*

First, we plan to run a simple linear regression model without controlling for any pre-treatment covariates, except baseline values of the outcome:

$$Y_{ij,t=\{2,3\}} = \beta_0 + \beta_1 T_j + \beta_2 Y_{ij,t=0} + \beta_3 Y_{ij,t=1} + \gamma + \varepsilon_{ij} \qquad (1)$$

[3] To date, we have had four rounds of surveys: baseline 2013 (t=0), baseline 2016 (t=1), the shorter 2018 tracker survey (t=2), and the endline 2019 survey (t=3).

In the regression above, $Y_{ij,t=\{2,3\}}$ represents the pooled tracker and endline outcome of the $i^{th}$ household in the $j^{th}$ village, such as wheat production level. $T_j$ is a dummy indicator of the $j^{th}$ village's treatment status and equals one if the village is in either T1 or T2.[4] $Y_{ij,t=0}$ and $Y_{ij,t=1}$ refer to the lagged values of the left-hand-side outcome in the two baseline years, 2013 and 2016, respectively. Because some of the lagged values are missing, we plan to replace them with an arbitrary value such as zero and add dummy indicators to show their missing status. $\gamma$ stands for grid fixed effects. $\varepsilon_{ij}$ is a random error term.

In this specification, $\beta_1$ captures BPRE's impact on the average household in the village, regardless of whether it was involved in agri-livestock production and/or received training. $\beta_1$ can therefore be interpreted as the analogous of an intent-to-treat (ITT) estimate in the sense that BPRE's intention was to train individuals in the village. Alternatively, one could also consider this the causal impact of offering (and then carrying out) a range of agri-livestock trainings.

We will analyze and report impact on multiple variants of each outcome variable. For example, to measure the BPRE program's impact on crop production we plan to use not only the output level of each crop but also its log transformation, growth, yield per kanal and a dummy variable indicating whether the household grew that crop within 12 months prior to the survey time.

In all of our HH-level specifications, we will cluster standard errors at the village level to account for the level of our randomization. For the specifications at the individual level, standard errors will be clustered at the HH level. Clustering the standard errors in HH level regressions make them analogous to running village-level specifications. We prefer the household specifications since they allow us to examine heterogeneous effects more closely, while (through clustering) ensuring that we estimate our standard errors in a conservative manner. That said, in subsequent analyses, we also aim to estimate village-level outcomes not necessarily by aggregating the household-level data but by using data from alternative sources. For instance, we can estimate total production by focusing on large producers and/or administrative production data (where available), as well as conducting surveys that measure village-level access to services and other relevant outcomes.

#### *6.2.1.2 ITT with pre-treatment covariates*

In addition to the lagged values of the outcomes of interest, we also plan to control for a set of household- or village-level characteristics:

$$Y_{ij,t=\{2,3\}} = \beta_0 + \beta_1 T_j + \beta_2 Y_{ij,t=0} + \beta_3 Y_{ij,t=1} + \theta X'_{ij} + \gamma + \varepsilon_i \qquad (2)$$

where $X'_{ij}$ is a matrix of pre-treatment characteristics such as family size, an asset index, household head literacy, fertilizer usage, soil quality and access to water for the $i^{th}$ household in the $j^{th}$ village. The composition of the covariate matrix varies by our outcomes of interest. Every other variable in (2) has the same interpretation as in specification (1). $\beta_1$ remains the ITT effect of the BPRE intervention.

[4] We will separate T into T1 and T2 in later regressions.

#### 6.2.1.3 Lasso regression

Next, we also intend to explore the value of using a flexible set of controls in the hope of minimizing the standard errors on our outcomes of interest. To do so, we will first generate fake treatment randomization data, use them to replace the treatment status indicators in the aforementioned specifications, and re-run these specifications using methods such as Lasso regressions to find the set of covariates and their interactions that minimize relevant standard errors. Once we find the subset of covariates that generate the least standard errors, we will run our specifications again using real treatment randomization data and only these covariates. Because this approach uses fake treatment assignment and fixes the specification prior to running the actual treatment regressions, it will help reduce variance in the data without raising data mining concerns.

#### 6.2.1.4 Interaction between treatment status and indicator of (non-)eligibility

Since take-up rates of skills trainings are likely to be higher for eligible households who were already engaged in agri-livestock production,[5] we will estimate the impact separately for eligible and non-eligible households in a village. We do so by interacting village treatment dummy with an indicator of eligibility status:

$$Y_{ij,t=\{2,3\}} = \beta_0 + \beta_1 T_j + \beta_2 Y_{ij,t=0} + \beta_3 Y_{ij,t=1} + \beta_4 NonEligible_{ij} + \beta_5 NonEligible_{ij} * T_j + \theta X'_{ij} + \gamma + \varepsilon_i \quad (3)$$

where $NonEligible_{ij}$ reflects whether the $i^{th}$ household in the $j^{th}$ village performs agri-livestock production (at baseline). In specification (3), $\beta_1$ provides the ITT effect of BPRE on the eligible population.

#### 6.2.1.5 Heterogeneous effects

Analogous to the heterogeneity of impact by each household's eligibility status, we intend to interact the treatment status indicator with household- and village-level characteristics of interest to explore BPRE's potential heterogeneous effects:

$$Y_{ij,t=\{2,3\}} = \beta_0 + \beta_1 T_j + \beta_2 Y_{ij,t=0} + \beta_3 Y_{ij,t=1} + \beta_4 X_{ij} * T_j + \theta X'_{ij} + \gamma + \varepsilon_i \quad (4)$$

where $X_{ij}$ represents a pre-treatment characteristic at either village or household level. We focus on pre-treatment characteristics that could act as complements or substitutes of the training, while acknowledging that we may not have enough statistical power to detect village-level heterogeneous effects. Table 2 summarizes some of the potential sources of heterogeneity. To avoid losing observations, we will replace missing values of these variables with some arbitrary number and then include dummy indicators to show whether the values are missing.

TABLE 2. Potential Sources of Heterogeneous Effects

| Village Level | Household Level |
|---|---|
| Village size | Prior level of production/engagement |
| Village development index | Land quality and water access |
| Overall interest in BPRE training | Average skill index |

[5] The training was also open to the non-eligible households, who did not take part in agri-livestock production at the time of the baseline survey in 2016.

| | |
|---|---|
| Quality of training (e.g. trainer turnover rates)<br>Access to agri-livestock market in other villages<br>Access to agricultural extension services (proportion of HHs in village having access to these services) | Labor availability<br>Density of social network<br>Wealth and production-related asset indices<br>Access to credit<br>Access to agricultural extension services<br>Acceptance to change and adopting new ideas |

#### *6.2.1.6 Additional specifications*

One natural feature of several of our outcome variables, such as production, is that a sizeable fraction of surveyed households (and hence the village) contribute zeros or missing values to the data because they do not directly produce crop or milk. While one could simply include these values as production outcomes, we intend to explore functional forms and specifications that naturally account for the presence of many zeros. If this is censored data, specifications such as tobit are effective, and we intend to use them as appropriate. In other cases such as production, however, reporting zeros could be a result of a nested choice—a household may first choose to produce or not and then, conditional on that choice, realize production. In such cases, we will apply models—such as the Heckman selection model—that explicitly account for both the selection and realized outcomes. Specifically, we would run the main specifications given previously (equations 1 to 5 above) and include the relevant selection equation. For example, in the case of equation (1), we can estimate:

$$Y_{ij,t=\{2,3\}} = \beta_0 + \beta_1 T_j + \beta_2 Y_{ij,t=0} + \beta_3 Y_{ij,t=1} + \gamma + \varepsilon_{ij}^1 \qquad (1')$$

while recognizing that non-missing and non-zero values of $Y_{ij,t=\{2,3\}}$ are only observed when

$$\beta T_j + \gamma Z_{ij} + \varepsilon_{ij}^2 > 0 \qquad (S)$$

where corr($\varepsilon_{ij}^1$, $\varepsilon_{ij}^2$)=ρ.

The above formulation acknowledges that the training treatment can influence both the selection and outcome (i.e. production) margins. To the extent that we find no impact on selection, however, we could also simply regress the outcome variables conditioning on non-zero (non-missing) values of the variables.

#### *6.2.1.7 Individual Level Impacts*

In the 2019 endline survey, we collected data from both male and female respondents. All HH-level specifications listed above can thus be run at the individual level too. This will allow us to estimate treatment effects by individual attributes such as age, gender, education, etc.

For instance, to estimate the treatment effects by gender we will run the specifications with interaction of village treatment dummy and indicator of individual's gender:

$$Y_{ijk,t=\{2,3\}} = \beta_0 + \beta_1 T_j + \beta_2 Y_{ijk,t=0} + \beta_3 Y_{ijk,t=1} + \beta_4 Gender_{ijk} + \beta_5 Gender_{ijk} * T_j + \theta X'_{ij} + \gamma + \varepsilon_i \qquad (5)$$

where $Gender_{ijk}$ reflects the gender of $k^{th}$ individual in $i^{th}$ household in the $j^{th}$ village.

### 6.2.2 LATE

While our primary specification could be thought of as an ITT/reduced form estimate (i.e. the impact of offering training in a village), it is plausible that this impact works through the fact that individuals are actually trained in the village (i.e. it is not just the offer that has impact but the fact that villagers avail of it). This is analogous to saying that if an individual were (randomly) offered a training opportunity, any observed impact was due to the training itself rather than other factors (such as simply being selected).

If the observed impact indeed works through the training, however, then much in the same way that one would use an instrumental variable specification (where actual training take-up is instrumented by the randomized offer) to capture the local average treatment effect (LATE), one would want to estimate the analogous village LATE.

In our case, we can do so by replicating the specifications from Section 6.2.1 but use village treatment status as an instrument for each household's participation status. Specification (6) provides an example:

$$Y_{ij,t=\{2,3\}} = \beta_0 + \beta_1 Training_{ij} + \beta_2 Y_{ij,t=0} + \beta_3 Y_{ij,t=1} + \theta X'_{ij} + \gamma + \varepsilon_i \tag{6}$$

where $Training_{ij}$, the participation status of the $i^{\text{th}}$ household in the $j^{th}$ village, is instrumented by $T_j$, the village treatment status.

We should caution that this estimate is not the impact of *any one* person receiving training. Since everyone in a treatment village enjoyed access to (different types of) training around the same time, we cannot isolate the impact of any *one* individual being trained. Rather, what the "LATE" would capture in our case is the contrast between no one being trained in the village to (at least one member of) *all* households in the village receiving (at least one of) the trainings. We believe this "scaling up" of the ITT estimates is useful to give a sense of how large these effects could be under the given level of program exposure and take-up.

We will evaluate two approaches to estimating spillovers: (1) leveraging exogenous variation in the number of trainees in a village and (2) using observables to construct a valid counterfactual for the group exposed to potential spillover effects (either through propensity score matching or synthetic trends). For (1), there are two sources of exogenous variation in the number of trainees in each village: (i) Timing of course offering in the case of livestock training in a village was randomly determined and would affect village-level participation differently depending on when it fell relative to other crop cycles. (ii) The number of training slots offered in a village was a discrete function of the optimal class size and total applications received (as in Lavy-Angrist 1999), thus leading to exogenous variation in training supply in otherwise similar villages. The feasibility of option (1) depends on the distribution of courses and impact of the capacity constraints, which we have not yet investigated. The feasibility of option (2) depends on the quality of the match on observables across non-participants in Treatment and Control villages, which we have not yet investigated. We will check both and if there is a viable first-stage given how the program rolled out in practice, we will file an amended PAP with first stage results and second stage estimating equations before doing any analysis of spillovers.

#### 6.2.3 Analysis of T1 and T2

Each treatment village in the BPRE program received either T1 (skills trainings) or T2 (skills trainings and market linkage). To measure the impact of each treatment arm, we will separate $T_j$—the village-level treatment status indicator—into $T1_j$ and $T2_j$ and re-run the ITT specifications in Section 6.2.1, as illustrated by Specification (7):

$$Y_{ij,t=\{2,3\}} = \beta_0 + \beta_1 T1_j + \beta_2 T2_j + \beta_3 Y_{ij,t=0} + \beta_4 Y_{ij,t=1} + \theta X'_{ij} + \gamma + \varepsilon_i \quad (7)$$

To estimate the analogous LATE specification, we will replicate the instrumental variable model in Section 6.2.2 using $T1_j$ and $T2_j$ as instruments for participation status in the two types of villages.

For both the ITT and LATE models, we will also compare the effects of T1 and T2 by performing a test between $\beta_1$ and $\beta_2$.

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