PRE-ANALYSIS PLAN

FOR

How does the extended react to better jobs opportunities for one member? Evidence from a large-scale public work program in Egypt

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Abstract

In the Developing World, Public Work Programs (PWPs) are an increasingly used tool to protect households against shocks to their income. To date, most of the evidence on this kind of program comes from one country (India) and uses quasi-experimental methods. An important question with regards to the evaluation of PWPs is how the labor supply at the household level reacts to the participation of one member to one of these programs. There might be crowding-out of family labor supply due to the additional resources brought in by the program; it is also possible that other members, in particular children, have to perform more household chores or unpaid family work to compensate for the previously inactive household member. This paper uses a large-scale randomized control trial of a PWP based on community social services in Egypt in order to investigate how these programs affect labor supply as well as various welfare indicators at the household level. The treatment is an offer of a good-quality jobs in community social services, for a duration of one year, targeted at disadvantaged youth, in particular young women. The randomization is done at the individual as well as the community level, in order to take into account possible crowding-out effects of the program on the local labor supply. In addition to answering the questions outlined above, the paper will allow to shed light on the precise nature of high youth unemployment in Egypt (ie., due to insufficient labor demand, or to constraints on the supply side).

1 CONTENTS

1/ INTRODUCTION

The Government of Egypt has been implementing, through Social Fund for Development (SFD), the Emergency Labor-Intensive Investment Project (ELIIP) financed by the World Bank. The project is a cash-for-work program that provides a social safety net to millions of beneficiaries. The program aim is "to contribute to the reduction of the negative impact of crisis that may lead to food insecurity and unemployment of the poor and vulnerable in selected areas, and support the protection and building of community assets in poor communities." It does so by providing short-term employment opportunities for unemployed unskilled and semi-skilled workers by supporting locally generated subprojects such as community level infrastructure construction and rehabilitation that is proposed by the local government.

PWPs such as Egypt's ELIIP are widespread in low- and middle-income countries and have been carried out in a variety of settings, including Argentina, Ethiopia, India and South Africa, among others. Together with cash transfer programs constitute the core of many developing countries' social safety nets (Camfield, 2014) (Grosh, del Ninno, Tesliuc, & Ourghi, 2008). Despite the pervasiveness of PWPs across poor and developing countries, rigorous evidence about their impact and effectiveness is still scarce. The empirical literature consists primarily of non-experimental studies evaluating long-running PWPs in India, including the Maharashtra Employment Guarantee Scheme (MEGS) and the Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS). Studies using randomized interventions and in other geographical contexts are still rare.

The short-term positive effects of PWPs on earnings and income are well documented, as are their effects on consumption, except in the case of Malawi where the value of the transfers and the amount of workdays allotted to beneficiaries seems to have been too small to have a meaningful impact on welfare indicators. At the same time, the evidence on any additional impacts beyond short term increases in consumption is still scarce. This is regrettable, as any assessment of the cost-efficiency (or cost-benefit) of such programs depend crucially on such impacts, given the high costs of public work programs.

Another concern with PWPs is that their generalization may have perverse effects or "hidden costs" for participants and non-participants alike. Indeed, some recent papers have found that participation in NREHS in India is linked with lower schooling outcomes and a higher burden of domestic work for school age children. On the other hand, if the spillovers from PWPs are sufficient to increase wages of all low-skill workers in the treatment area (not only program participants), the benefits of the PWP might be severely underestimated by looking only at participants.

This paper aims at disentangling the various interactions at the household level that occur after one of its members participates in the PWP. As evidenced by the example above, depending on the specifics of the case, interactions at the household level might counteract as well as increase the potential of the public work programs. Hence the relevance of putting this topic front and center of our investigation.

The rest of this note proceeds as follow. Part 2 consists in a brief presentation of the ELIIP community social services program. Part 3 details our research questions, the theory of change, as well as our main hypotheses. Part 4 presents the evaluation design and the data used in this project. The next part present the econometrics aspects of our study, while part 6 details the outcome variables used.

2/ INTERVENTION OVERVIEW

The Community Social Services component of the ELIIP evaluated in this IE focuses on social services and youth employment activities that are fostered through grants to non-governmental and/or community-based organizations that employ youth, especially females, to provide social services such as cleanliness, maternal health and environmental awareness campaigns in local communities. A distinguishing feature is that sub-projects are lasting relatively long between 12-18 months and thus provide employment and security for a longer period. Further, sub-projects are required to be labor-intensive: at least 60% of project costs must be on labor. Other criteria are that 80% are between 18 and 29 years old, at least 70% is female, and the beneficiaries should be considered the “poorest of the poor” within their community. The projects are implemented through NGOs, with which the SFD has worked with in the past. To give an example: community health care projects will create job opportunities for girls from the age of 19, who will be trained to provide health education programs and administer home visits to expand access to women, thus contributing to improved maternal and child health. Other NGO projects include: (a) cleanliness and environmental awareness campaigns; (b) early childhood education; (c) mother and child health awareness home visit programs; (d) illiteracy eradication activities; and (e) youth engagement in community initiatives in rural and urban areas, among others.

3/ RESEARCH QUESTIONS AND THEORY OF CHANGE

The ELIIP public work program can be conceptualized as an exogenous increase of labor demand from the equilibrium point – an outward shift of the labor demand curve. In a classic, competitive labor market setting, such a shift is predicted to increase equilibrium wage on the market (i.e., for participants as well as nonparticipants of the program), but to reduce the total amount of labor demanded by the private sector. However, such an effect will take place only if (a) the size of the program is large enough (see Beegle et al., 2017 for a counter example) and (b) if there is no market power of the employers on the market. If the size of the program is too small in terms of the amount of additional labor demand created, the equilibrium wage effects will fail to materialize. If the labor market is non-competitive (i.e. in the presence of monopsony power in the employers), there will be no crowding-out of labor demand, but a “double benefit”: higher wages as well as higher employment (Muralidharan et al., 2017).

The “net” effect of a PWP on people’s livelihood depends on interactions within the recipients’ households. Additional wage employment by previously “inactive” person may induce remaining household members to work more, or less. On the one hand, increased income might reduce labor supply, due to classic income effects; on the other hand, this could mean an increase in the burden of domestic work, if the PWP participants was previously performing a large share of these chores (Rosas & Sabarwal, 2017). In some extreme cases, this might even cause a decrease in schooling for school-age children, who have to perform activities previously performed by adults (Li & Sekhri, 2019).

Social protection programs such as PWP serve an insurance function against shocks. In the absence of such programs, this function is often performed by self-insurance by income pooling at the household level, or by contingent transfers through extended family. If these transfers are mainly motivated by altruism, they might diminish if the household has access to other sources of income and/or protection in case of shock (Fafchamps, 2011). This is one explanation as to why the impact of the PWP program at the household level may be smaller than at the individual level

The additional character of public work programs can also be ascertained by looking at the use that the individuals and household make of the funds they receive through the program. An increase in food consumption or healthcare-related expenditure would be indicative of previously unmet needs. An increase in savings will indicate that the main need of the household was related to self-insurance against unexpected shocks. The increase in "luxury" purchases or temptation goods (cigarettes, alcohol) is also worthy of an investigation.

Finally, the operation of a public-work program in a local labor market is likely to have important distributional consequences (Imbert & Papp, 2015; Muralidharan et al., 2017). While low-skilled workers are likely to gain as a result of the program, small business owners or farmers that hire low-wage work are likely to be worse-off after the implementation of the PWP, and the corresponding increase in wages.

In the following section, we detail the research questions that the study intends to examine, the theory of change, as well as the main outcomes under consideration.

3.1. Research questions

This paper is concerned with three main research questions related to the impact of the ELIIP public work program. The first set of questions is linked to the overall labor market effect of the program:

(a) What is the causal impact of participation in the ELIIP PWP on labor market outcomes?

This research question can be decomposed into the following sub-questions:

a.1. What is the causal effect of ELIIP PWP on individual labor supply at the intensive and extensive margin?
a.2. What is the causal effect of participation of ELIIP PWP on *net* earnings for the participants? (ie, exclusive of foregone earnings)
a.3. What is the effect of ELIIP on the earnings of non-participants (spillover effects)

The second research question focuses on intra-household distribution of labor and possible hidden costs:

(b) What is the causal impact on ELIIP on intra-household labor distribution?

b.1. what is the effect of ELIIP on paid work of other household members?
b.2. what is the effect of ELIIP on the household work of other household members?
b.3. what is the effect of ELIIP on the schooling of children in the household of participants?

Finally, the third research question studies the use of ELIIP funds at the household level

(c) How is the consumption decision of households modified by ELIIP?

c.1. Does the participation in ELIIP translate in more consumption, savings, or both?
c.2. How is the structure of consumption modified by ELIIP?
c.3. Is there an increase in the consumption of temptation goods?

3.2. Theory of change and hypotheses

The theory of change linking the public work program to labor market outcomes is mediated by intra-household interactions. In low and medium income country context, the household performs a role of insurance against various shocks, through income pooling as well as contingent transfers (Dercon 2005).

We hypothesize the following causal chain, with the direction of the impacts at each level

- Direct impact of the PWP for beneficiary, at the individual level:
  - Increase in hours spent in wage work. The increase may be less than the total amount of hours spent of the program, if the individual was previously working
  - Increase in the individual income
  - Decrease in the hours of leisure (if the individual was previously unemployed and/or inactive)
  - Decrease in hours spent on household tasks / nonpaid family work. This may also be less than the full amount of the time previously spent on household chores if there is a "double shift" phenomenon, especially for women

- Indirect impacts, at the household level
  - Increase (if crowding-in) or decrease (if crowding out) of paid work effort by other people
  - Increase of total work income. The increase at the household level may be more (if crowding in) or less (if there are crowding out / substitution effects) than the increase at the individual level.
  - Increase of transfers sent / decrease of transfers received
  - Increase of total household income
  - Increase of household work / nonpaid family work for other members
  - Decrease of leisure time of other household members
  - Decrease of schooling or time doing housework of school-age children

- Impact on consumption
    - Increase in the general level of consumption by less than the additional amount earned
    - Increase in household durables and/or business assets
    - Increase in spending on health care
    - Uncertain impact of spending on schooling
    - Impact on luxury & temptation goods: uncertain

4/ EVALUATION DESIGN & DATA

4.1. Randomization

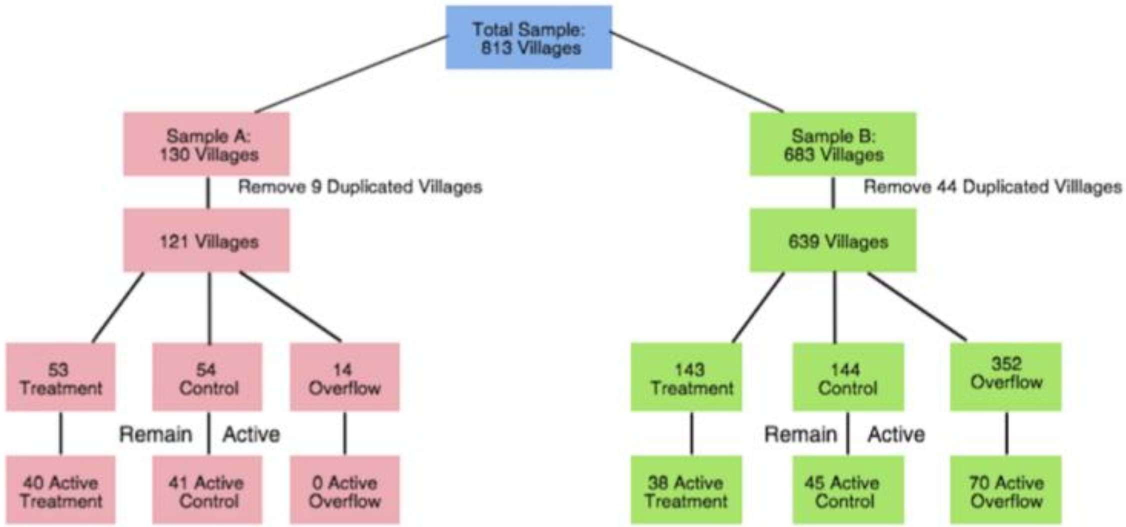
Our impact evaluation is designed to shed light on these evaluation questions through the use of randomization at two levels: the village and the individual level.

4.1.1 Village level randomization

We randomly allocate villages into treatment and control groups using two methods. The first method involved a list of 121 unique villages. At the time of the IE design, some NGO's had already proposed social services projects to SFD in specific locations. We asked these NGOs to extend their proposal to include an equal number of alternative locations in which they could also operate compared to the original locations proposed. Using these two lists for each NGO, we then randomly allocated villages to either treatment or control. This means that both villages that were originally proposed and other locations that were later added to the proposal could receive treatment. Out of the total of 53 treatment and 54 control pairs, contracts were signed and projects were implemented stretching across 40 treatment locations and 41 control locations in this branch as illustrated in Figure 1.

The second method started with a list of 639 unique locations as illustrated in the right branch of Figure 1. For this method B, the research team has created a set of 150 potential matched pairs of locations that are observationally similar. For each matched pair, a single location was randomly selected to be in the candidate list of locations for which NGO's could propose a project, in addition to a set of overflow villages for which there was no control. NGOs could submit proposals including villages from the matched pairs that were assigned treatment and the overflow list. For the villages in the overflow list, naturally no matched control pair exists. We included these locations nevertheless in the survey as we can still draw upon the within village comparison between workers receiving treatment and those not receiving treatment.

FIGURE 1: VILLAGE-LEVEL RANDOMIZATION



4.1.2 Workers Randomization

In each treated village, NGOs were asked to provide worker lists of twice the number of workers needed in order to enable randomization at the worker level. Worker lists provide detail on the name, gender, age, national ID, type of work, telephone number, residence and official residence. Workers are only excluded from the list if the village registered on their National ID is a control village. We have made the distinction on the workers' list registration form between where someone lives and the residence information listed on their National ID, since for many people there is a discrepancy between this information. Through the training sessions, SFD HQ, with support from the WB team, explained to all implementing actors, that if a person is only working in a village, but his/her family reside in another village that s/he travels to regularly and sends money to, then s/he is not considered as residing in the village where s/he works. Workers' mobility is not anticipated to be a significant problem in Community component projects as NGOs target hiring workers from the village in which projects are being implemented.

The double randomization at the village and individual level will allow for the identification of direct effects on program beneficiaries (including consumption, assets, labor market outcomes, and human capital accumulation amongst others) as well as general equilibrium changes in local economic activity. The project activities taking place in overflow villages as selected by NGOs will still allow for randomization at the worker level. Figure 2 plots the locations included in this study on a map.

4.2. Data Collection

We carried out a single round of data collection for individual level outcomes through a survey instrument upon completion of the project. In addition to data collection for individuals, we also carried out a community level survey interviewing local community leaders. For the community level survey component we interviewed two local community leaders (the official/ traditional leader and a secondary leader) in all 234 villages. For the household-level survey, data collection involved surveying households in both treatment and control communities as well program participants and non-participants.

There are three distinct samples: the first, a sample of program participating individuals and (randomly selected) non-participating individuals in “treated” communities (about 15 individuals per village in all treated villages). The second is a synthetic control sample of individuals in control communities who have the similar characteristics of the program participating individuals in “treated” communities (about 5 household per village in all control villages). The third will be a random representative sample of non-participants across treatment and control villages (about 5 households per village in all 234 villages in the study).

Table 2 presents a tabulation of the sample that was realized across the different experimental conditions. The left column provides the indication treatment status of the respective village as either treatment, control or overflow. The columns present the different individual samples. The first three columns indicate the worker samples, which can be either treatment and control workers from treated villages; alternatively, in control villages, we collected a synthetic set of control group workers that would satisfy the eligibility criteria for ELIIP social services employment. Columns 4 and 5 are the respective samples of random households in treatment communities.

Community sub-projects, which last between 12-18 months, ended by the end of April 2017. Data collection was carried out in May 2017. Data entry was carried out by the same survey firm responsible for data collection. The surveys were implemented using electronic tablets enabling the field teams to collect and transmit data from the field to a cloud-based server.

FIGURE 2 : GEOGRAPHIC DISPOSITION OF SAMPLE VILLAGES

TABLE 1: TABULATION OF SAMPLE

	Worker Sample			Household sample		
	Treatment	Control	Synthetic	Treatment	Control	Total
Treatment	690	331	0	390	0	1,411
Overflow	618	280	0	350	0	1,248
Control	0	0	430	0	430	860
Total	1,308	611	430	740	430	3,519

Notes: Table presents distribution of survey respondents across treatment, control and overflow conditions.

2 5/ EMPIRICAL FRAMEWORK

Due to randomization at the individual as well as at the village level, the starting point for the analysis is the simple comparison of means between treated and non-treated individuals (overall), as well as the comparison between treated individual and non-treated individuals in non-treated village. A greater difference in the latter than in the former would be indicative of spillover effects between treated and non-treated units within the treatment village. In order to increase the precision of the estimates, we include controls at the individual and the village level. We detail the econometric specifications used below.

5.1 Econometric specification

5.1.1. Baseline (direct effects)

The baseline specification rests on a comparison of the outcome in treated individuals with outcome in non-treated individuals. The specification is written as follows:

$$y_{ic} = \alpha + \beta T_i + \xi X_{ic} + \epsilon_{ic} \qquad (1)$$

Where y_{ic} is the outcome value for individual *i* in community *c.* T_i is a dummy variable equal to 1 if the individual was offered the participation in the PWP program. X_{ic} is a vector of pre-determined individual, household and community-level characteristics, and ϵ_{ic} is the idiosyncratic error. Standard errors are clustered at the community level to account for intra-community correlation in outcomes.

5.1.2. Spillover effects

When estimating equation (1), the empirical counterpart of parameter β is similar to the weighted average of two different quantities: the difference between treated and untreated individuals within treatment villages, and the difference between treated units in and untreated units in control villages. The first of these two quantities could be biased due to spillovers. This bias would then contaminate the overall estimate of β. Note that the direction of the bias due to spillover effects depends on the outcome under consideration; it is likely to be negative for wages, while likely to be positive for employment.

In order to test for the presence of those spillover effects, as well as in order to assess the magnitude of those spillover effects, we take the following specification to the data, after Angelucci & Di Giorgi (2016):

$$y_{ic} = \alpha + \beta_1 T_i \times TREATVILL_c + \ \beta_2 TREATVILL_c + \xi X_{ic} + \epsilon_{ic} \qquad (2)$$

Where the $TREATVILL_c$ is a dummy equal to 1 if the individual is in the treated village. The β_1 parameter will recover the difference between treated and untreated unit in treatment villages; the β_2 parameter represents the difference between untreated units in the treatment villages and untreated units in control villages (the spillover effects).

The total effect of the program *TITT* will be a weighted average of the (direct) effect and the spillover effects:

$$TITT = w_1\beta_1 + w_2\beta_2$$

Where w_1 and $w_2$represent respectively the sample proportions of the eligible (those who have been offered the program) and ineligible groups.

5.1.3. ITT vs. LATE

Assuming that the experimental setting is valid (see below), estimation of specifications (1) and (2) using the ELIIP program data will give a valid estimate of the Intent-to-treat (ITT). ITT is a policy-relevant parameter, as it represent the effect the program would have, if extended to the whole population. In some cases, we are interested in the effect of the programs on the "compliers", those people who have been induced to change their behavior due to the program, known as LATE (Local Average Treatment Effect). The distinction between ITT and LATE is especially relevant in case of low or insignificant effects, in order to distinguish between two possible mechanisms: "low take up, high individual effects" and "high take-up, but low individual effects". In order to estimate LATE, the treatment (the random attribution of eligibility to the program) is used as an instrument for the take-up of the program in a two stages least squares (2SLS) specification.

In order to be valid, LATE needs to satisfy the usual exclusion restriction that there is no direct effect on the outcome merely from being offered to participate in the program. This might be the case if the participation in the public work program serves an insurance function. It is easy to think of situations where this would be the case (Glennerster & Takavarasha, 2013). For instance, an individual may be willing to take on riskier activities, safe in the knowledge that should the project fail, she will be able to make ends meet by participating in the PWP.

The LATE estimator also assumes that the entire difference in outcomes between control and treatment group can be attributed to the people who take-up the program: this is known as the stable unit treatment value assumption (SUTVA). However, this assumption is not valid in the case of spillover effects, which are a violation of the SUTVA assumption (Angelucci & Di Maro, 2016).

The implications of this discussion is that, in our setting, the ITT is more appropriate than the LATE, which risks being biased. Therefore, we mainly rely on the ITT for our analysis. However, on a case-by-case basis, we do not preclude the use of LATE in order to investigate the possible mechanisms behind an eventual non-significant effect. In this case, we would use LATE only in the specification (2) (which allows for spillovers).

5.1.4. Heterogeneity analysis

As with many interventions of this kind, it is likely that the program may work differently for different socioeconomic groups. For instance, the program may have relatively larger effects on the most poor or geographically isolated communities that have limited access to markers, as compared to participants who might be less poor or live in communities that are more connected to markets. More specifically, we will investigate potential heterogeneity of the effects of both the community infrastructure and the cash for work components of ELIIP based on a host of pre-treatment (or time-invariant) characteristics related to program activities, the context in which these activities are carried out and targeted participants. We model heterogeneous treatment effects by the following equation:

$$y_{iv} = \beta_0 + \beta_1 ELIIP_{iv} + \beta_2 X_{iv} + \beta_3 ELIIP \times Z_{iv} + Z_{iv} + \epsilon_{iv} \qquad (5)$$

Where y_{iv} is the outcome for household/individual i in village v; $ELIIP_{iv}$ is a dummy indicating whether or not individual i was employed in a temporary employment project/ whether or not village v had received an infrastructure project; X_{iv} is a vector of cluster- and individual-level and/or village-level imbalanced covariates at baseline; $ELIIP \times Z_{iv}$ represents a set of interaction terms between the treatment dummies (i.e., participation in employment or not at the individual level; assignment to infrastructure project or not at the village level) and important program-related or contextual factors at the village or individual levels represented by Z_{iv}; and ϵ_v is the disturbance term for the regression assumed clustered at the village-level.

The set of factors we employ for heterogeneous effects (at the community or individual levels) analysis will include: gender; literacy levels; pre-existing unemployment levels; whether the respondent is household head or not; the presence of a shock in the past year (to detect insurance effect).

Additionally, we will also examine whether the precise nature of the project implemented makes a difference. While the basic principle of the ELIIP PWP is the same, the projects differ with regards of their precise goals: environmental cleanliness, maternal and child health, kindergarten projects, and campaigns for literacy promotion. While the pay is approximately similar across projects, the precise projects may differ in some dimensions, that are typically unobserved. PWP of the same domain are likely to be more homogeneous.

5.2 Validity of the experimental setup

5.2.1 Balancing test

If randomization was successful, we would not expect there to be any systematic differences a) between treated as well as control group villages as well as b) between treated as well as control group individuals. Naturally, the focus for the balance checks is on variables elicited through the survey instrument that are unlikely to be altered by the treatment itself and thus, should be considered as outcome variables. Balancing tests are performed at the village as well as at the household and individual level.

5.2.2. Contamination and geographical spillovers

The geographic proximity of different treatment locations is quite evident in Figure 3. The median distance between treatment and control villages is just 2890.5 meters. This is making it very likely that estimates are downward biased due to geographical spillovers. These geographical spillovers may be due to "leakage" of program benefits to non-eligible individuals (see *infra*). But it can also materialize because the control group (ineligible workers in neighboring villages) may benefit from the increase in wages caused by the program, due to its interaction with labor demand. The first mechanism will translate to lower individual program effects; the second mechanism will mean that the outcome of the comparison group will be upwardly biased relative to a pure counterfactual.

In order to deal with this threats to internal validity linked to leakage and geographical spillovers of the program, we follow two strategies. First, we split the comparison group in two based on the distance from treatment villages. Several thresholds can be tested (20, 30, 50 km). We then use only the "distant" group as a comparison group, with the justification that in such a group, contamination is less likely. However, such a reduction of the comparison group will lower the statistical power of our estimates.

Another approach to test for the presence of geographical spillovers is to draw a circle for each treated village (of radius 20 or 30 km) and to compute the number of treated and untreated villages and/or households in this radius. It is then possible to estimate the following equation, after Merfeld (2019):

$$y_{ic} = \alpha + \beta T_{ic} + \gamma_1 N_{ic} + \gamma_2 N_{ic}^T + \epsilon_{ic} \qquad (3)$$

Where N_{ic} is the total number of treated household (or villages) in the radius and N_{ic}^T is the number of treated households (or villages) in the radius. Geographical spillovers to near untreated villages will be captured by the γ_1 coefficient.

5.2.3. Compliance with randomization

There are two stages of randomization: villages are selected at random to create matched pairs in which one randomly selected village receives treatment, while the other village served as control. The second stage of randomization is happening within treated villages, whereby only a randomly selected subset of eligible workers actually receive the treatment.

In addition to these two stages of randomization, we obtained information on three distinct groups of individuals. In treatment villages, we surveyed a) workers who where participating and non-participating, b) randomly selected households. In control group villages, we surveyed a) workers who might be eligible for SFD employment to serve as additional control group as well as b) randomly selected households. This gives us five types of workers.

We first show whether individuals who were assigned treatment- and control conditions received the treatment (or did not receive it) in accordance with the protocol before turning to studying the balance with regard to characteristics.

5.2.4. Multiple testing

Our study looks at a variety of variables (see 6/). In order to avoid a multiple testing problem and an increased risk of type I error, we specify, for each research question (see above, 3.1.) a primary outcome variable. We interpret the remaining outcomes only to the extent that the primary outcome variable is significant. Thus, we hold ourselves to the "one theory, one test" rule. The only research questions where there are multiple outcome variables for one question are (c.3) on the structure of consumption, and (a.3), on the activity of other household members. For these questions, there are respectively 3 and 2 tests that are conducted. We thus adjust the upwards significance level for rejection of the the null hypothesis accordingly.

6/ OUTCOME VARIABLES

This section lists the outcome variables used in the analysis, by research question. The main outcomes (those who are commented in priority) are underlined. The other outcomes are commented only to the extent that they complement the findings of the primary outcomes.

I. Labor market outcomes

a. Employment and earnings of main respondent

i. <u>Has a job/type of job (e.g., waged/self/casual; fulltime/part-time, etc.)</u>
ii. Has primary wage employment or income generating activity (IGA)
iii. Has primary self-employment or IGA
iv. How long (in months) have you been in this job
v. Total days you spend in wage-employment in a typical month
vi. Total days you spend in self-employment in a typical month
vii. <u>Total monthly earnings (aggregated over parallel activities and types of employment)</u>
viii. Total earnings from wage employment in the last 30 days?
ix. Total earnings from self-employment in last 30 days
x. Having a secondary job (e.g., waged/self/casual; fulltime/part-time, etc.)

b. Economic activity and earnings at the Household (HH) level

i. <u>Number of jobs or IGA of other HH members</u>
ii. Head of the HH has a job or IGA
iii. Type of job or IGA of the head of HH
iv. Number of other HH members of have a job or IGA
v. Other HH members have additional IGAs
vi. <u>Total earnings from other members jobs / IGA</u>
vii. How much money/earnings did the HH head bring in with this job or IGA?

viii. How much money/earning did the other HH members bring in with this job or IGA?
ix. How much money/earning did the other HH members bring in with any such additional IGAs

c. Farm employment, productivity and earnings at the HH level

i. % of land owned by the HH cultivated during the last two seasons
ii. Hired labor to work in the farms
iii. Took loans to buy equipment, fertilizers, insecticides, etc. to improve productivity of the field
iv. Used chemicals fertilizers
v. Received advice from an agricultural monitor

d. Migration

i. How many members are no longer living in this household (post-treatment) for work
ii. How many members are now living in this household (post-treatment) for work
iii. Have you travelled or move to another city/province or country for work (for some period) of time in the last 24 months
iv. Number of days in total did you or other HH members spent outside of the city for work
v. How much money did you earn while working outside of your home town

II. Economic welfare at the HH level

a. Consumption expenditure

i. <u>Total expenditures : aggregation of the following categories</u>
ii. Total value of food own consumption (note: see quantity of farm products produced in the farm employment section)
iii. Types and source of food consumed by the HH
iv. Food/drinks at home expenditures
v. Transportation-related expenditures
vi. Electricity, gas, water expenditures in the last month
vii. Landline, mobile calls and internet expenditures in the last month
viii. Soap, detergent, cosmetics
ix. Rent expenditures in the last month
x. Other services (hairdresser, veterinarian, repairman) in the last month
xi. Medical expenditures in the last 6 months (and amount)
xii. School/education expenditures in the last 6 months (and amount)
xiii. House renovations/repairs

b. Assets accumulation and investments (purchased post-treatment)

i. Dwelling type (roof)

ii. Dwelling type (walls)
iii. Land purchased
iv. Real estate purchased

c. Savings, debts and re remittances

i. Has saved money over past 3 months (0/1)
ii. Total money saved (post-treatment)
iii. Total money borrowed (post treatment)
iv. Having a bank account (0/1)
v. How long able to cover current expenses through savings

d. Coping strategies

i. Household suffer an event that led to a loss of income / cash in the last 12 months (and number)
ii. Coping strategies (dummies)
iii. Received money, food or other social assistance THAT IS NOT CASH FOR WORK from the government or NGOs in the last 12 months
iv. Received money, food or other assistance from the family or social network in the last 12 months (0/1)
v. Net amount of money received from friends or family in last 12 month

e. Children welfare and schooling

i. Number school-age children (5-14 years) of this household have never been to school
ii. Number of school-age children (5-14 years) in this household have interrupted their schooling
iii. Number of school-age children (5-14 years) in this household who have engaged in any type of work in the last month

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