PRE-ANALYSIS PLAN

for

PUBLIC WORKS PROGRAMS, WELL-BEING, SOCIAL COHESION AND WOMEN'S EMPOWERMENT: EVIDENCE FROM A RANDOMIZED CONTROL TRIAL IN EGYPT

December 2019

Abstract

Public work programs (PWPs) constitute a popular form of providing social protection for households in low- in middle-income countries, and middle-income countries. While the growing number of empirical studies has focused on assessing the impact of PWPs on material outcomes (e.g., labor market; economic welfare; etc.), rigorous evidence about their impact on non-material outcomes is scarce.

This paper analyses a large-scale randomized control trial (RCT) of a PWP conducted in Egypt between 2015 and 2017 to provide rigorous evidence on three sets of issues: (ii) non-material aspects of the community's life such as social capital, trust collective action and violence; (ii) subjective well-being; and (iii) women's empowerment and intimate partner violence. One defining characteristic of this PWP is that the jobs created were supposed to be "good jobs", providing decent working conditions and rewards, and that these jobs were predominantly targeted towards women and people of low social status. The program was randomized across villages and within villages, allowing us to measure both its individual and spillover effects.

We exploit a rich survey on non-material outcomes to assess the impact of the ELIIP PWP at the individual level as well as at the community level. At the individual level, we evaluate the impact of ELIIP on the perceptions of economic conditions, income security, subjective well-being, psychological health, and gender norms. At the community level, we examine whether the program has been deemed as "fair" by the community at large, and whether it has led to an improvement in the perceptions of the economic conditions, social capital, trust, conflict in the community, and confidence in government.

Key words: Public works programs; subjective well-being; social Cohesion; women empowerment.

JEL code(s): D13; I38; J12; O12.

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 contexts geographical contexts are still rare.

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/ THEORY OF CHANGE AND RESEARCH QUESTIONS

3.1. Theory of change

The ELIIP PWP might affect welfare at the level of the individual and at the level of the community. We examine each of these levels in turn. Moreover, there is an important gender angle to this issue, which needs to be taken explicitly into account.

Individual level

At the individual level, revealed preference reasoning suggests that the participation in the PWP is necessarily welfare-increasing: if it weren't, the individual would have stayed home. The most basic channel for this increase in welfare is increased (individual) consumption thanks to higher earnings.

However, whether or not participation in a PWP leads to an individual increase in welfare is not the most policy-relevant question. Ideally, we would like to be able to compare the *magnitude* of the welfare increase to the total cost of the program. This is especially relevant when trying to compare the relative merits of a PWP and of a cash transfer program (Ravallion, 2019). Compared to the latter, PWPs are more expensive to run, as they require important overhead as well as inputs of qualified work in order to function. Moreover, they entail important costs to the participant, who has to perform chores, sometimes in difficult conditions, in exchange for the cash. Additionally, there might be psychological costs related to the stigma of earning one's livelihood to solidarity. These individual costs need to be taken into account in the assessment of PWP programs; otherwise the individual benefits of the program risk being over-estimated.

On the other hand, an important literature on work and subjective well-being in developed countries context shows that work is an important source of self-esteem, and, conversely, that unemployment is detrimental to happiness, above and beyond the loss of income (Winkelmann & Winkelmann, 1998). What's more, the impact of unemployment on subjective well-being is both *durable* and *scarring*: out of a series of major life events (such as marriage, divorce, widowhood, birth of a child, or layoff), unemployment it is the only one to which individuals don't seem to adapt over time (Clark et al., 2008). Thus, providing vulnerable people with employment opportunities might be preferable to keeping them inactive – even with an equivalent monetary transfer – if it helps them maintain or rebuild a sense of self-worth. However, little evidence on this point exists in developing country context.

Finally, by providing a sense of income security in a context of high susceptibility to shocks, PWP might also decrease stress at the individual and at the household level, which, in turn, has been linked to reduced cognitive capacities (Mani et al., 2013). All these reasons justify a detailed exercise in unpacking the various channels through which participation in a PWP affects the various dimensions of subjective well-being.

Community level

Similar to the individual level, armchair theorizing might lead us to believe that the community-level impacts of a PWP are necessarily positive: an injection of cash in a community, by increasing the well-being of its elements, will increase the well-being of the sum. If there are positive externalities from this cash injection (say, if the PWP draws some people away from criminal activity), the gain to the community may well be higher than the sum of the individual gains.

Here again, some countervailing mechanisms may exist. First, the rationing of the PWP, due to the logic of the evaluation which calls for the random exclusion of individuals that had previously been deemed eligible to the program, might create jealousy among the excluded people, especially if the selection process is judged as unfair or opaque (Ellis, 2012). In extreme cases, such grievances and feelings of unfairness have been linked to in criminality (burglaries and property crimes, cf. Cameron & Shah, 2013).

Moreover, a large-scale PWP may have distributional effects. As noted by Muralidharan et al. (2017), PWP act in large part through an increase in the wage of non-program workers, due to the "soaking up" of unskilled labor by the program. This means that the operation of a PWP creates winners and losers: small business owners or farmers that rely on hired labor may be worse-off as the result of the program. Depending on the composition of the community, this might make the total effect of PWP on community well-being negative.

These two mechanisms have to be distinguished analytically. But these two mechanisms (jealousy and grievances due to the rationing of the program on the one hand; and adverse effects on employers on the other hand) have different implications for the generalization of the program. The perceived unfairness of the allocation of program benefits in the pilot/evaluation phase might vanish if the program is generalized to the entire population. The allocation mechanism might also come to be socially accepted, if its fairness is established with sufficient credibility[1]. On the other hand, the distributional effects of the PWP will not vanish with its generalization; on the contrary, they are likely to become more important as the size of the program increases.

The gender angle

The phenomena outlined above may apply to any type of PWP, irrespective of the nature of the works performed. In the case of the ELIIP Community Social Services, there is a distinct gender component: the NGOs that were conducting the projects had to hire a minimum percentage of women; and the tasks performed by these temporary workers were for the most part designed so as to be deemed as socially acceptable, even rewarding, for women. This creates a separate set of issues that need to be recognized and analyzed explicitly.

In a context where prevailing social norms are generally biased against women working outside the home, an intervention such at the ELIIP community social services might challenge those social norms by stimulating a "taste for freedom" that comes with earning a living[2].

On the other hand, the empowerment of women through public work programs may create a "male backlash", especially if men do not benefit from the same employment opportunities. This backlash, which typically manifest itself through increased intimate partner violence (IPV), has been shown to appear in other contexts, but has not as yet been linked to the operation of public work programs (Vyas & Watts, 2009)

3.2. Research questions

This research paper tackles three main questions, which can be decomposed is sub-questions as follows:

(1) What is the impact of the participation in a PWP on subjective well-being?
   a. Does PWP lead to an increased level of subjective well-being?
   b. Does PWP lead to a higher level of confidence in the future?
   c. Does PWP lead to a feeling of economic security?

[1] Bertrand et al., 2019 mention how, in the context of Côte d'Ivoire, the fairness of the random allocation of spots in the public work programs was established

[2] Such a mechanism is at play in the context of Senegal and chronic Illness of the household head, cf. Comblon & Marazyan (2017)

(2) What is the impact of a PWP on social capital, trust and violence at the community level?
    a. Does the randomized allocation of the ELIIP program within the community lead to grievances?
    b. Is there a positive effect on PWP on civic participation and engagement?
    c. Does PWP lead to a higher level of violence or social tensions?
(3) What is the impact of a PWP on female empowerment?
    a. Does the participation in a PWP modify attitudes towards work for women?
    b. Does the participation in a PWP lead to greater decision-making power for women?
    c. Is there any backlash in the form of increased intimate partner violence for female participants in PWP?

4/ EVALUATION DESIGN & DATA

4.1. Randomization

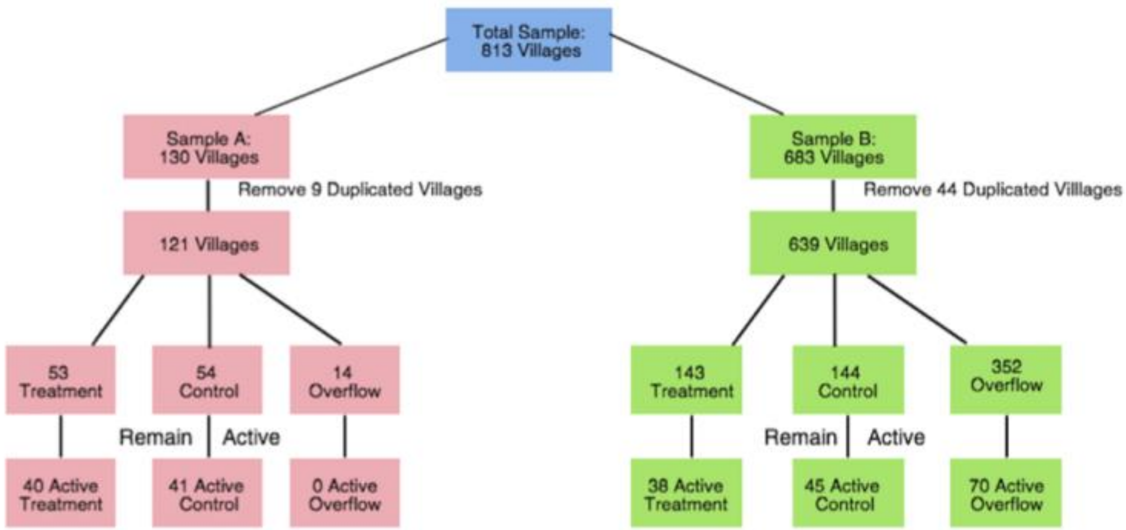
Our impact evaluation is designed to shed light on these evaluation questions through the use of randomization at two levels: the village and the individual level.

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.

Our overall sample of locations included in this study is summarized in Table 1, while Figure 2 plots the locations on a map.

Table 1: characteristics of sample of villages

	A	B
	NGO Proposed Matched Pairs	**Constructed Matched Pairs**
Proposed Sample Size	121 villages in 65 matched pairs suggested by NGOs in which they could operationalize a project *within a district*. Special focus on illiteracy/ kindergarten projects for operational constraints.	287 villages. Matched pairs were created based on poverty map characteristics to ensure that treatment and control villages are comparable based on baseline characteristics.
NGOs selection	The villages that would receive treatment from the proposal list is randomly selected.	NGOs propose projects operationalized in villages from the set of 143 treatment villages. Villages are allocated to NGOs on a first-come first-serve basis. For larger projects that stretch across multiple villages, they may select additional villages from an overflow list of 352 villages.
Worker Selection	Randomization of participants based on NGO enumerated lists.	Randomization of participants based on NGO enumerated lists.
Expected Sample Size	40 active matched pairs	38 active matched pairs with an additional 70 overflow villages that are unmatched to a control village.

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 2: 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.

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):

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

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).

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

We next turn to presenting evidence on the compliance with the randomization protocol. Remember, there were two stages of randomization: villages were selected at random to create matched pairs in which one randomly selected village received treatment, while the other village served as control. We refer to any comparisons based on this to be the between village analysis. 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. Implementation issues

A further concern that emerged is due to the heterogeneity of different spelling variations and translations of Arabic to English and vice versa. This has resulted in overlaps between villages that were assigned to treatment and control group villages for a separate impact evaluation, that explores an infrastructure construction PWP in Egypt.

One implication for this is that the control villages may not be appropriate counterfactuals. We need to manually compare information from the control villages to the treatment villages in the ELIIP community infrastructure and possibly compute new survey weights in order to reach a suitable "pure control" group (see Bertrand et al., 2017, that encountered similar issues).

5.2.5. Multiple testing

As noted in the in the discussion of the theory of change underlying ELIIP cash for work activities, we expect interventions under evaluation to impact economic welfare as well as social and psychological outcomes that are likely to be multi-dimensional. Moreover, as described in our data collection subsection, our various survey instruments that include several questions that measure a single dimension of a particular key outcome of interest. In other words, we measure key outcomes using multiple measures. Thus our empirical framework necessarily entails testing multiple hypotheses. We will follow standard practice for this type of analyses (see, for example, Haushofer and Shapiro 2013) and account for multiple hypotheses by using outcome variable indices and family-wise p-value adjustments.

6/ OUTCOME VARIABLES

This research has three levels of analysis: individual, community, and gender; it seeks to test 9 hypothesis in total. In this section, we detail the variables used for this purpose.

6.1. Individual level

The analysis will be conducted on the following indices.

- Perception of economic conditions. Composed of questions :
  - B.6.12 (Economic conditions of household compared to previous year)
  - B.6.13 ("poverty ladder" question)
- income security. Composed of
  - C.5.4 (How long are you able to pay your expenses with your savings)
  - C.5.5 (Comfortable paying for unexpected or emergency expenses)
- Subjective well-being
  - D.1.1 §A-E (Self-esteem)
  - D.1.4 (Acceptance by family)
- Psychological health
  - Anxiety/Depression scale (Q366-Q393)
- Victimization / violence : (Q409-Q414)

6.2. Community level

We construct summative indices of the following dimensions:

- Fairness of allocation process:
  - Conflict about program about how workers were chosen (B.6.9)
  - Satisfied with way beneficiaries identified and served (B.6.10)
  - Efficiency of NGO implementation of SFD program (C.2.4)
  - Poverty of participants in SFD program (C.2.6)
  - How hardworking are beneficiary households (C.2.7)
  - Presence of inclusion errors (C.2.8)
  - Presence of exclusion errors (C.2.10)

- Fairness of selection process (C.2.12)

- Perception of economic conditions in village
    - Economic condition compared to other villages in governorate (B.6.11)
    - Economic conditions now and past year (Q150-Q151)
- Social capital
    - Acceptance by other households (D.1.5)
    - Membership in village committees (F.1.6)
    - Social divisions in village (F.1.13 – F.1.15)
    - Collective action in village (F.2.1)
    - Involvement in community initiatives (F.2.6)
    - Trust (F.2.8 & F.2.9)
- Conflict in the community: Q521-530
- Citizenship and confidence in government: Q536-Q522

6.3. Gender issues

With regards to the effect of participation on attitudes, we examine the effect of participation in the program on the following aspects, constituted of summative indices

- Attitude towards marriage/partnership, composed of :
    - Ideal partner characteristics (B.2.1)
    - Have you tried to marry? (B.2.2)
    - After how many years do you think you will be married? (B.2.3)
- Attitude towards fertility :
    - Desired # of children (B.2.4 & B.2.5)
    - Self-worth through motherhood (Q74-Q75)
- Women's decision making (D.4.1 – D.4.10)
- Violence against women (D.5.1)
- Control over household resources (Q416-Q422)

Intimate partner Violence (Q427-Q441)

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