PRE-ANALYSIS PLAN FOR

The Social and Psychological Effects of Public Works Programs: Evidence from a Randomized Control Trial with the Urban Poor in Eastern DRC.

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Abstract

This document describes the pre-analysis plan for a randomized impact evaluation of a World Bank funded, labor-intensive public works (LIPW) program in Eastern DRC. The project, implemented by the Social Fund of DRC, sought to increase resilience and livelihoods in five major cities in Eastern DRC by offering short-term employment to the urban poor. In order to make a lasting impact, the project also offered a savings incentive and a training program to develop professional skills. Between November 2016 and December 2018, 2,775 beneficiaries were randomly assigned into one of four groups. A first group of beneficiaries was offered short-term employment. A second group was offered a savings incentive in addition to the temporary works component. A third group was offered the employment component in addition to a training program while a fourth group was offered all three program activities. The impact evaluation seeks to produce rigorous evidence on non-material impacts of these four LIPW schemes. Because these schemes entered a broader program seeking stabilization and recovery of Eastern DRC, this study will investigate their impact on areas of social and political engagement, exposure to and engagement into violence, views and behaviors regarding gender equality and psychological well-being. This document provides an overview of the interventions and hypothesized effects, describes the key outcomes of interest and outlines econometric methods that will be employed to ascertain the effects of the interventions.

Keywords: Workfare; Savings; Training; Poverty; Conflict; Field experiment; DRC.

JEL: I3, D14, J24, C93

1 INTRODUCTION

Over the past decades, the eastern provinces of the Democratic Republic of Congo (DRC) have been host to an explosive mix of weak governance, widespread poverty, natural resource mismanagement, land disputes and the exploitation of ethnic divisions for political and economic gain by foreign and Congolese armed groups. This instability has frequently spilled over into outright violent conflict. The cumulative impact has been catastrophic: since 1998 over 5.4 million people are estimated to have been killed, while millions of others have been plunged into a state of acute vulnerability. With poverty and social unrest being both a result and a predictor of violent conflict, this region could easily be caught in a violence-poverty trap (e.g. Blattman and Miguel, 2010).

Against this backdrop, the international community has been actively involved in efforts to end conflict and to support economic recovery in Eastern DRC, as part of broader efforts to re-establish peace and security in the region. The World Bank supports these efforts in part through the International Development Association (IDA)-funded Productive Opportunities for Stabilization and Recovery in the DRC (STEP, in its French acronym)—an 80USD millions project, being implemented by the Social Fund of the DRC (FSRDC). The project aims to improve resilience and livelihoods in conflict-affected communities in North Kivu, South Kivu, and Oriental Province.

The project has a number of key components, including a community-driven development (CDD) program, which aims to strengthen community resilience, and a livelihood and employment generation component, which aims to support employment creation and sustainable livelihoods. The latter component includes a labor-intensive public works (LIPW) program (or 'cash-for-work'), which provides temporary employment opportunities to vulnerable households and individuals in both rural and urban areas. In order to make a lasting impact, this component also offered two additional activities: a savings incentive where beneficiaries were incentivized to save 1$ a day against a 1$ extra-pay, and a training program to build professional skills or business management capacity.

A randomized impact evaluation was designed to investigate the impact of the LIPW program on the economic welfare of beneficiary households. Given that the evidence in this field is still relatively scarce – especially in conflict-affected regions, this study will provide unique perspectives on how a temporary public works program may improve economic and social outcomes. Furthermore, as the project offers additional activities to ensure a long-term impact, this study offers the possibility to investigate which strategies work best to improve living conditions in a durable way. If ways are found to increase social cohesion, protect the poor from adverse economic shocks and stimulate sustainable income-generating activities, this might help to break the poverty-conflict trap in Eastern DRC.

This document, written before the commencement of data analysis, details the econometric approach to investigate those questions. In the following sections, we provide an overview of the main interventions and hypothesized effects, describe the key outcomes of interest and how they are linked to the end-line survey that was conducted and outline econometric methods that will be employed.

2 INTERVENTION OVERVIEW

The urban LIPW component created short-term employment opportunities in five major cities in Eastern DRC: Beni, Butembo, Goma, Bukavu and Bunia. Local NGOs – under the supervision of the FSRDC – offered temporary employment to implement activities such as road rehabilitation, street cleaning or garbage collection. In each of the cities, a sensitization campaign was launched to announce the program. The LIPW programs – implemented on a rolling basis – were designed to target the urban poor through two mechanisms: first, self-targeting based on the minimum wage and, second, geographic targeting of the most deprived neighborhoods in each of the five cities.[1] Any resident from a targeted neighborhood was eligible to receive project benefits as long as he/she was willing to work for the set wage – the country's minimum wage of 3USD a day – and apt for physical labor. As designed, the project offered all selected beneficiaries with a full-time work for around 4 months, paid at minimum wage (3USD/day) as well as soft-skills training (health at work, cooperation, etc.) aiming to prepare efficient teamwork.

In order to make the impact last, the project offered two additional activities: 1) an incentivized-savings scheme, and 2) a training program. The incentivized savings arm of the program contained the opening (if needed) of a savings account, at no cost to the beneficiary. Then, beneficiaries where incentivized to save US$ 1 a day (out of the 3$ pay) against a 1$ extra-pay, directly placed on the savings account and available only by the end of the LIPW. In other words, beneficiaries of the savings scheme chose between being paid 3$/day, every day, or 2$/day everyday plus 2$ per worked day (on an account) at the end of the contract. Beneficiaries of the training program arm, on the other hand, were trained by professional NGOs on specific skills right after the end of each LIPW project. The skills were chosen based on a market study of local economic conditions. Trainings mainly provided beneficiaries with new professional skills or business management techniques. Training days were paid as any workday (3USD) and so beneficiaries of this treatment arm theoretically also benefited from an extra-income.

Because a large number of applications were expected, public lotteries were used to select the beneficiaries of the program, employing a randomization design.[2] That is, intervention subjects – selected to be 50 percent male and 50 percent female – were randomly assigned to receive access to temporary employment. At the same time beneficiaries were selected, they were assigned to one of the four treatment arms: job offer only (group "A"), job offer and savings incentives (group "B"), job offer and training (group "C") or job offer and savings incentives and training (group "D").[3] Around 2,775

[1] Pre-program data, however, indicate that the formal minimum wage – US$ 3 per day – exceeds the reservation wage of non-poor individuals (see Mvukiyehe *et al.*, 2016), suggesting that targeting, if any, must have occurred through the place-based aspect.

[2] Lotteries have the advantage of providing a fair and transparent rule for selecting applicants, but they also allow to evaluate program impacts using a randomized controlled trial.

[3] Technically, beneficiaries were assigned to a particular treatment arm based on the draw order: first draw assigned beneficiary to group A, second to group B, third to group C, fourth to group D, and so on until all groups were filled. Remaining applicants were assigned to the pool of potential control.

individuals benefited from LIPW activities, earning US$ 3 a day during at least 4 months. Of those 2,775 LIPW beneficiaries, 695 were additionally offered the savings incentive (group "B"), 693 were additionally offered the training program (group "C") and 687 were additionally offered both the savings incentive and the training program (group "D").[4] An extra pool of 1,678 individuals entered the project as potential replacement workers; since these individuals weren't part of the first lottery draw we do not consider them in this analysis.

In total 26 LIPW projects were started between November 2016 and October 2017. Table 1 below summarizes the number of interventions and beneficiaries and shows their distribution across the 5 cities. There is substantial variation in the end date of the projects evaluated, while the first ended on April 11th, 2017, the latest ended on December 30th, 2018.

	Number of projects (2016-2017)	Number of beneficiaries
Beni	4	483
Bukavu	4	451
Bunia	10	893
Butembo	4	429
Goma	4	519
Total	26	2,775

Table 1: Distribution of LIPW projects and number of beneficiaries across cities.

LIPW projects completed by December 2018.

3 Theory of Change, Evaluation Questions and Main Outcomes

This section details the theory of change and the main hypothesis we intend to test with the impact evaluation.

3.1 Theory of Change

Our goal in this study is to measure the impact of the workfare program on a set of outcomes that are directly related with the peace-building objective of the STEP initiative. We focus directly on non-material aspects of beneficiaries' lives and behaviors: their social integration and participation, their exposure and participation to violence (either domestic or local) and their psychological well-being.

[4] For equity reasons, beneficiaries of all four groups are to receive all components after the completion of the end line survey. As such, potential future survey would not permit to identify the particular effect of additional components per se, but of their timing only.

Note that in a first and separate analysis (PAP: Adjognon et al. 2019), we seek to unveil material outcomes change due to the program. That previous analysis focuses on changes in labor, earnings, savings, consumption and investments. We thus exclude these outcomes from our main analysis here, although (i) we consider they are key in the mechanisms at play and (ii) we will leverage them in our interpretation of the results.

Various theories from social sciences predict that employment may benefit social cohesion. Although stemming from an old tradition in sociology, most recent works have been empirical and there exists only a few attempts to categorize the pathways from employment to social cohesion (Wietzke, 2015). Synthetizing five case studies of public work programs, Andrews and Kryeziu (2013) identifies three main pathways: (i) promoting voice an participation (providing opportunities for voice of excluded groups and interaction with local officials), (ii) improving social inclusion and equality (empowering certain excluded groups), and (iii) building trust (by inferring a sense of dignity and social identity). In an older work, Pickering (2006) drew from social network theories to enumerate "the necessary characteristics that institutions must have in order to enable people to forge ties that bridge ethnic division" (namely, (i) "be culturally diverse", (ii) "promote ties that are acquaintance", (iii) "possess a norm that at least allows for interethnic cooperation", (iv) "promote repeated, mutually dependent interaction among individuals from different groups"). Because the STEP urban LIPW used random selection and provided work for 4-months long duration we hypothesize that they could potentially increase social cohesion. Causal empirical evidence remain scarce and, although some studies have shown that contacts and interactions at the workplace can generate trust and bridge social cleavages (Mutz and Mondak, 2006).

Regarding criminality, another theoretical mechanism could also operate beyond social cohesion: opportunity cost. Becker's (1968) theory states that, as peaceful sources of utility become more easily available, individuals' interest to engage in criminal activities decreases. Because LIPW has provided work experience, earnings as well as (potentially) training and incentivized savings, we hypothesize the program allows beneficiaries to prefer peaceful activities by the time of the survey. There is a growing empirical literature on this aspect showing, for instance: a negative association between illegal activities and employment (e.g. Draca and Machin, 2015) or earning (e.g., Machin and Meghir, 2004). Regarding workfare program specifically, T. Fetzer (2020) finds it can break the link between negative income shocks and conflicts (in the context of India's NREGA) while Amaral et al. (2015) find in the same context that increased female labor participation increased gender-based violence. However, evidence on workfare program remains mixed and especially scarce in the context of fragile states (Blattman and Ralston (2015), Ferguson et al. (2019)).

Third, we build on a large literature that relates improved economic conditions to positive impacts on psychological outcomes. This literature has mainly been empirical and started from the positive correlation between income and life satisfaction (Easterlin (1974) and Clark (2018) for a literature review). It then expanded to other cognitive aspects, down to the idea of feedback loops (cf. e.g. Mani et al. (2013) and Haushofer (2019)). Recently, randomized evaluations have started to build evidence that cash transfer may have positive psychological impact (cf. notably Baird, De Hoop, and Özler (2013)). This is also the case in the large (across-country) study of Banerjee et al. (2015) were productive asset transfers were combined with other interventions, including skills training and savings account. Based

on this accumulation of evidence, we also hypothesize that the urban LIPW can improve psychological conditions of beneficiaries.

The random allocation of benefits was designed in order to measure complementarities of the cash transfer (treatment arm A) with incentivized savings (treatment arm B), hard skills training (treatment arm C) or both (treatment arm D). We expect these complementary interventions to foster the program's impacts we hypothesized above. Incentivized savings may foster all the economic impacts we presented here, from decreasing the opportunity to engage in violence or criminal activity (e.g. Blattman and Annan (2016) find that capital transfer reduce young men's incentives to enroll in illicit and mercenary occupations) to increase psychological wellbeing. We thus expect savings to impact outcomes mostly throughout the economic outcomes. Trainings on the other hand do influence both channels of transmission: by increasing the number of interaction with team-workers and authorities, but also by increasing human capital (and so economic outcomes). In their review of employment programs in fragile states, Blattman and Ralston (2015) note that some specific soft-skills trainings (targeting "character" skills) have proven "successful at deterring crime, violence and other anti-social behaviors". The urban LIPW component of STEP did not include such specific program but a broader life-skill and teamwork training, which could also have benefited recipients. For these reasons, we will devote an important part of our analysis to the comparing the effects of each specific treatment arm.

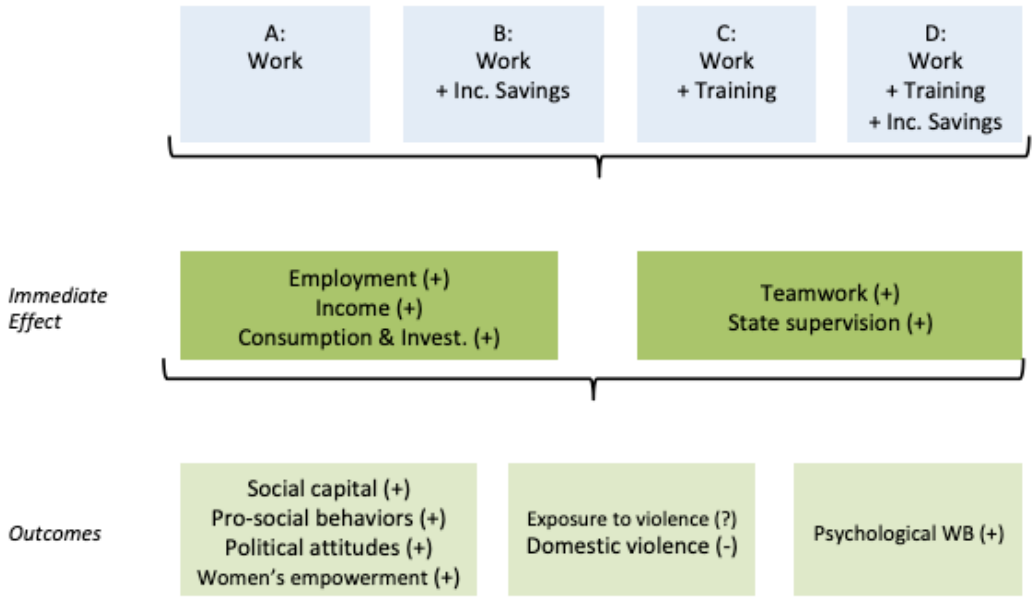

Figure 1: Theory of Change

3.2 HYPOTHESES

Building on the above theory of change, the impact evaluation will seek to answer the following research questions:

- Q1: Can workfare programs help (re)build social capital and promote civic engagement on the part of participants?

H1: The labor-intensive public works program leads to an improvement of social capital and civic engagement of beneficiaries in the medium term, potentially including a reduction in criminality at the local level.

- Q2: Can workfare programs improve psychological well-being of the participants?

H2: The labor-intensive public works program leads to an improvement of psychological outcomes of beneficiaries in the medium term.

4 Evaluation Design and Data

As mentioned above, the impact evaluation is a two-stage, randomized controlled trial. In this section we detail how beneficiaries were selected and elaborate on data collection process for the empirical analysis.

4.1 Sampling and identification Strategy

One week before the start of each LIPW program, a sensitization campaign was launched, including announcements on the radio and through a public speaker system. After the sensitization, a registration list was opened to record basic information and to verify eligibility of interested individuals. Registered individuals received a coupon that they had to bring to the lottery. The selection of beneficiaries usually took place two days after the closing of the registration list. At the lottery, individuals – stratified by gender – were randomly selected into one of the four abovementioned groups. Also a list of replacement candidates was randomly selected. Generally, the remaining unsuccessful candidates largely outnumbered the total number of beneficiaries (and replacements). Therefore, stratified random sampling from this group was employed to select a pure control group.[5] Gender, neighborhood, literacy, age and displaced status were used as stratification variables.

Between November 2016 and October 2017, 26 LIPW projects were started across the five cities with, in total, 2,775 randomly selected beneficiaries (see table 1). There is substantial variation in the end date of the projects evaluated, while the first ended on April 11th, 2017, the latest ended on December 30th, 2018.

The following table summarizes the experimental design.

[5] To limit attrition, an extra pool of potential control-group individuals were randomly selected. This group could be interviewed when the response rate of the initial control group turned out too small.

	Control	Beneficiaries			
		LIPW Only	+ Additional components		
			Saving	Training	Both
Group name:	Control	A	B	C	D
Group size	3,205	700	695	693	687
Share of men	51.20%	52.29%	50.65%	52.53%	50.80%

Table 2: Evaluation Design.

4.2 DATA

4.2.1 DATA COLLECTION

Endline data collection took place from June to August 2019, which is 1.8 years after the median LIPW project ended.[6] Data collection was carried out by two professional survey firms under the close supervision of the research team. Again, 2,775 beneficiaries were targeted to be interviewed and 3,205 subjects from the control group. Several measures were put in place to ensure a high survey quality. First, survey manuals were provided for the enumerators. Second, full-time field coordinators closely followed the data collection on a daily basis and organized back-check interviews. In order to minimize measurement error, data were collected using computer-assisted personal interviewing.

Attrition is likely to be a problem in the context of Eastern DRC cities. We hope to address this issue relying on the extensive identification data gathered at registration of candidates (this notably includes: names and nicknames, address, phone number, relative's phone number and place of worship) as well as public information campaigns (using local radios and presence in public places such as markets or places of worship). Since beneficiaries worked in teams, we also intend to rely on their social networks to sensitize their ex-coworkers. Finally, it was decided that respondents would be compensated for their time with the interviewer.[7]

4.2.2 QUANTITATIVE INSTRUMENTS

[6] There is quite some variation in time after treatment, i.e., the time between the end of an LIPW project and the endline survey. While 66 percent of the LIPW's ended between 1 and 2 years before the endline survey, the minimum time after treatment is 167 days while the maximum time after treatment is 795 days. This variation can be used to explore the evolution of impacts over time.

[7] Due to various reasons, the compensation was slightly higher in Goma and Bukavu than in the other three cities.

This evaluation relies mainly on data collected at endline using a household questionnaire. The household questionnaire consists of several modules: the individual (treatment or control) and household roster; a section regarding knowledge about and exposure to the STEP program (and, in particular, its urban-LIPW component); a socio-economic section including household assets, consumption and economic shocks; a section on employment savings and agricultural activities; a section on mental health and trauma; a section on access to public infrastructures and services; and finally a section on social cohesion, community participation and collective action.

Furthermore, the data from the household questionnaire will be complemented by four other important sources of information. First, along with the household survey, two additional questionnaires were submitted by enumerators to neighborhood chefs and the local NGOs that were in charge of LIPW activities. Second, during 2014-2015, a representative household survey for each of the 5 cities was conducted which provides detailed socio-economic information on the pre-program situation (Mvukiyehe *et al.*, 2016). Third, next to time-invariant variables from the endline questionnaire, the information gathered at registration can be used for balancing tests (see table 3 for an overview). And fourth, monitoring data from the project will be used to inform our understanding of the factors that shaped the performance of the project and ultimately its influence on program participants.

1. From Registration	1.1. Gender 1.2. Age at the time of the lottery 1.3. Address (neighborhood and block - *'avenue'*) 1.4. Profession[8] 1.5. Displaced (because of violence) 1.6. Returned from displacement 1.7. Ex-fighter 1.8. Survivor of gender-based violence 1.9. Literacy
2. From endline survey	2.1. Age at the time of the survey 2.2. Number of children between 5 and 14, i.e. born before LIPW (B1.2+C5.1-B1.1) 2.3. Mother tongue (B1.19) 2.4. Father's highest educational attainment (B1.23) 2.5. Mother's highest educational attainment (B1.24)

Table 3: Pre-treatment covariates.

5 EMPIRICAL FRAMEWORK

We will estimate the average treatment effect (ATE) for all treatment arms, but given we also expect some non-compliance we will also estimate the intention-to-treat (ITT) effect, which may be used as an instrument to derive the local average treatment effect (LATE).

[8] This variable is uncoded so we will categorize it into 'unemployed' vs. 'employed'.

5.1 Balance at baseline

We will test balance on observables between control and treatment groups using characteristics observed at registration, or unlikely to be affected by LIPW (cf. table 3 for the extensive list). We will consider these tests to inform our selection of controls in the estimations. We will report results both with and without the inclusion of potential controls.

5.2 Main Specifications

5.2.1 Direct Effect: Pooled Treatment

First, next to a simple comparison in mean outcomes between treatment and control, we will estimate the following model:

$$y_{ign} = \alpha + \beta . T_i + \delta . X_{.} + \epsilon_{igt} \quad (1)$$

Where y_{ign} is the outcome of interest for individual *i* of gender *g* and living in neighborhood *n* (notation for strata *g* and *n* is dropped hereafter), T_i an indicator variable taking the value 1 if individual *i* was selected as a beneficiary and $X_{.}$ is a vector of control variables including a gender dummy, neighborhood fixed-effects, and potentially pre-program characteristics that are expected to affect the outcome. Furthermore, since we are estimating the average treatment effect for the whole group of beneficiaries at a fixed point in time, we will control for potential treatment heterogeneity by including a variable that measures for each individual *i* the time after treatment. Finally, ε_i is a well-behaved error term. The model will be estimated using OLS with appropriate weighting and cluster-robust standard errors.

The validity of the experimental setup will be examined in detail. In the case of evaluation design involving more complex design features, the analysis will be adjusted accordingly, and detailed explanation will be discussed.

5.2.2 Direct Effect: Across Treatment Arms

To estimate the ITT across the different groups of treatment we estimate the following model:

$$y_i = \alpha_1 + \beta_1 . TA_i + \beta_2 . TB_i + \beta_3 . TC_i + \beta_4 . TD_i + \delta_1 X_i + \epsilon_i \quad (2)$$

Where TA_i, TB_i, TC_i and TD_i are indicator variables taking the value 1 if individual *i* was assigned to group A (LIPW only) B (LIPW + incentivized savings), C (LIPW + training) or D (LIPW + incentivize savings + training), respectively. As in model (1), X_i is a vector of pre-program variables and strata fixed-effects.

Coefficient $\beta_1, \beta_2, \beta_3$ and β_4 capture the effect of the respective treatment arms. Furthermore, equation (2) allows us to perform a wide variety of statistical tests. The estimation of the model will follow the same lines as for the pooled treatment effect (cf. previous subsection).

5.3 Heterogeneity Analysis

We analyze heterogeneity in treatment effects for subgroups determined by a set of pre-treatment characteristics *Z*. In this case, we use a third family of equations of the following form:

$$y_i = \alpha_2 + \gamma_0 . G_i + \gamma_1 . T_i + \gamma_2 . (T_i . G_i) + \delta_2 . X_i + \epsilon_i \quad (3)$$

Where $G_i$is an indicator for belonging to the subgroup determined by Z. Coefficient γ_1 measures the effect of the program on beneficiaries that do not belong to group G, while $\gamma_1 + \gamma_2$ measures the effect on beneficiaries from group G. We will test the significance of γ_2 (i.e. whether there is a different treatment effect for the subgroup belonging to G) but also the null hypothesis that $(\gamma_1 + \gamma_2)$ is equal to zero (i.e. whether the program has a significant effect on beneficiaries of group G), and report the p-value. This type of model is naturally extended to the inclusion of treatment arms indicators.

The analysis of heterogeneous effects will focus on 'means effects' indices, rather than the full set of outcomes investigated. A first category we will test for is gender as the randomization was built so as to estimate the effects conditional on gender. Depending on actual variance in the sample, we also want to study the change in treatment effect across cities, literacy, ex-ante employment and displaced status (all these dimensions were measured at baseline, cf. Table 3).

5.4 Validity of the Experimental Set-up

5.4.1 Multi-Hypothesis Testing

We will deal with multiple hypotheses testing in two ways, in the spirit of Anderson (2008). First, we will reduce the number of required tests by grouping outcome variables that measure related concepts into "families". To do so, in a first step we will express responses in terms of standard deviations from the control group mean. Before, in a second step, summing all standardized primary outcomes related to a family into an index (switching signs if necessary to ensure that the positive direction always indicates a "better" outcome). We will also report a robustness test weighting the index items by their inverse covariance matrix. We will also present estimates of individual indicators within each family to better gauge how various indicators contribute to overall effects within families.

Second, for each family we will report both the unadjusted p-value (of the coefficient of the treatment effect) and the adjusted p-value to control for the false discovery rate (cf. Benjamini, Krieger and Yekutieli, 2006). When examining individual indicators within a family we will control the FDR across indicators within the family.

5.4.2 Measurement Error

As noted before, attrition is likely to be a problem in our context. To limit its impact we have first ensured data collection includes an extensive search for selected respondents and compensations (cf. details in the dedicated subsection above). In our analysis we will report actual attrition rates (including across treatment groups) and we will construct Horowitz-Manski (2000) and Lee (2009) bounds for the treatment effect.

In order to limit noise caused by variables with minimal variation, variables listed in the remainder of this plan and for which 95% or more of observations have the same value within the relevant sample will be omitted from the analysis and will not be included in any indicators nor hypothesis testing. In the event that omission decisions result in the exclusion of all constituent variables for an indicator, the indicator will be excluded. In order to avoid outliers, outcomes adding one or more continuous variable will be winsorized (e.g. earnings) at 99%.

In order to address concerns about potential missing observation and response bias (especially regarding sensitive questions) we will introduce a response rate threshold of 20%. Above this threshold, missing values will be accounted for through the use of multiple imputations. In addition, we shall consider any variable with 20% or more of “I don’t want to answer” and/or “I don’t know”, to be potentially subject to response bias and will then be excluded from our outcome indices.

6 KEY OUTCOMES VARIABLES

The following list contains 27 outcomes of interest distributed into 8 families of outcomes. For each outcome all required survey items are listed along with the methodology to construct the outcome on which treatment effect will be measured.

6.1 SOCIAL CAPITAL AND PARTICIPATION INDEX

1. Social capital and trust

This outcome is a standardized index built by summing the following 4 standardized measures:

- HH member belongs to an association (G1.2);
- One association at least accepts people from other community (G1.3)
- Interpersonal trust: toward local community (G1.16)
- Interpersonal trust: from local community (G1.17)

2. Intra-community collective action

This outcome is a standardized index built à la Anderson as a weighted-average of the 6 following items:

- HH member participates in local committees (G1.5);
- HH member contributed to local project (G1.11);
- Engagement (time or money) for charities (G1.23, G1.24)
- Participation in community meeting (G3.1.1)
- Speaking at community meetings (G3.1.2)
- Amount of money given for local project (I2.2)

3. Inter-community collective action

This outcome is a standardized index built à la Anderson as a weighted-average of the 4 following items:

- Committee accept members from other ethic groups or communities (G1.6)
- Project involved members from other communities (G1.12)
- Preference for local community over city (G1.22)
- Amount of money given for another community (I2.3)

6.2 PRO-SOCIAL BEHAVIORS INDEX

4. Respondent's pro-social behaviors

This outcome is an indicator of the following measure:

- Respondent engaged in one of the listed pro-social behavior over the last 12 months (outcomes O to X in question H1.2)

5. Others' pro-social behaviors

This outcome is an indicator of the following measure:

- Respondent knows individuals that engaged in one of the listed pro-social behavior over the last 6 months (outcomes O to X in question H1.1)

6. Respondent's anti-social behaviors

This outcome is an indicator of the following measure:

- Respondent engaged in one of the listed anti-social behavior over the last 12 months (outcomes A to C and G to N in question H1.2)

7. Others' anti-social behaviors

This outcome is an indicator of the following measure:

- Respondent knows individuals that engaged in one of the listed anti-social behavior over the last 12 months (outcomes A to C and G to N in question H1.2)

6.3 State Authority and Service Provision

8. Use of State-provided services.

This outcome is a standardized index built by summing the following 5 indicators:

- Relying on the state to help with SAFETY issue (G3.2A)
- Relying on the state to help with CONFLICT issue (G3.2B)
- Relying on the state to help with HEALTH issue (G3.2C)
- Relying on the state to help with EDUCATION issue (G3.2D)
- Relying on the state to help with DEVELOPMENT issue (G3.2E)

9. In favor of accountable state authority

This outcome is a standardized index built by summing the following 3 indicators:

- Agrees with "we must verify and question the actions of this neighborhood chief" (G4.1)
- Agrees with "we have the duty to verify and question the actions of our national political leaders" (G4.2)
- Agrees with "government has the right to collect taxes only if it fulfills its duty (…)" (G4.3)

10. Trust in local public institution

This outcome is an indicator of preference for local institution over other options, built from the following question:

- "If the village received FC10,000 for its development, who should be given the responsibility to manage that amount to ensure that the money is used for the welfare of the village?", where individual are given 7 options, 2 of which refer to local community (chief and local committee) (G1.21)

11. Contribution game: amount paid in taxes

This outcome is a monetary measure (continuous variable expressed in USD and winsorized) build from a contribution game where individuals were offered about 5USD and could pay back in taxes:

- "(…) according to the law, the tax rate for such revenue is 10% (…) You can decide to pay this tax now, at any rate you prefer, or not at all. If you do not wish to pay, government will not be informed of your decision nor about this money you received. Do you whish to pay taxes? If yes, how much?" (I1, I2)

6.4 Political Attitudes and Participation Index

12. Political efficacy and interest

This outcome is a standardized index built by summing the following 5 standardized measures:

- Often discuss community and country issues with friend (G3.7.1)
- Feels influential on these matters among his friends (G3.7.2)
- Feels capable to influence authorities (G3.7.3)
- Feels citizens are capable to influence authorities (G3.7.4)
- Often talks about politics (G3.4.1)

13. Contacting political leaders

This outcome is a standardized index built by summing the following 5 indicators:

- Met mayor/community leader (G3.1.3)

- Met local committee member (G3.1.4)
- Contacted the Police (G3.1.5)
- Contacted a State agency (G3.1.6)
- Met MP or representative (G3.1.7)

14. Contacting opinion leaders

This outcome is a standardized index built by summing the following 5 indicators:

- Met with NGO representative (G3.1.8)
- Met with local experts, medic, committee (G3.1.9)
- Contacted the media (G3.1.12)
- Met with influential people (G3.1.13)
- Met with religious leader (G3.1.14)

15. Electoral participation

This outcome is a standardized index built by summing the following 4 indicators:

- Voted in last election (G3.3.1)
- Participated actively to a campaign: any (G3.3.2)
- Participated actively to a campaign: last presidential or legislative (G3.4.2)
- Was candidate (G3.3.3)

6.5 Exposure to crime, conflict and violence index

16. Presence of criminal or armed organizations

This outcome is a standardized index built by summing the following 4 indicators:

- Was the following group active locally during the last 6 months: armed group or militia (H5.1)
- Was the following group active locally during the last 6 months: criminal group or gang (H5.2)
- Was the following group active locally during the last 6 months: paramilitary/ government militia (H5.3)
- Was the following group active locally during the last 6 months: community defense forces (H5.4)

17. Perceived and actual victimization

This outcome is a standardized index built by summing the following 3 indicators:

- Fears robbery, theft or physical aggression (H3.4)
- Was victim of robbery, theft or physical aggression (H3.1-H3.3)
- Witnessed robbery, theft or physical aggression in neighborhood (H3.5-H3.7)

18. Conflicts and violence within the community

This outcome is a standardized index built by summing the following 2 standardized measure:

- Number of different types of conflict in the community that remained non-violent (H4.1)
- Number of (…) that ended in violence (H4.2)

6.6 Women's empowerment and agency index

19. Women's economic empowerment and autonomy in financial decision-making

This outcome is a standardized index built by summing the following 4 indicators:

- Women involved in decision related to money earned (H2.4 & H2.4.1)
- (Inverse of) Was not allowed to work or earn money (H2.1F) by a household member (H2.3)
- (Inverse of) Money was taken (H2.1G) by a household member (H2.3)

- (Inverse of) Was evicted from home (H2.1H) by a household member (H2.3)

20. Views About Women's Access to Power

This outcome is a standardized index built by summing the following 3 indicators:

- In favor of women should have the same rights as men (G4.8)
- In favor of women should have equal access to powerful position (G4.9)
- In favor of women should have equal access to management of local committee (G4.10)

21. Perception of gender-based violence

This outcome is a standardized version of the following measure:

- Score of legitimation (from 0 to 8) for domestic violence against women.

6.7 Intimate partner violence

22. Physical violence

This outcome is the sum of the following 2 indicators:

- Was beaten (H2.1C) by a household member (H2.3)
- Was threatened (H2.1D) by a household member (H2.3)

23. Sexual violence

This outcome is the following indicator:

- Suffered sexual coercion (H2.1E) by a household member (H2.3)

24. Emotional violence

This outcome is the sum of the following 2 indicators:

- Was insulted (H2.1A) by a household member (H2.3)
- Was frightened (H2.1B) by a household member (H2.3)

6.8 Psychological well-being index

25. Cantril ladders

This outcome is the standardized sum of the following 2 measures:

- Present days Cantril 10-steps ladder (D1.1)
- Future days Cantril 10-steps ladder (D1.2)

26. Mental health index

This outcome is the MHI-5 index built as the standardized sum of 3 positives and 2 negatives items:

- Frequency (on a 3-1 scale) of happiness, calm, anxiety (D1.3)
- Frequency (on a 1-3 scale) of sadness and depression (D1.3)

27. Perception of social acceptance (by friends, family and in community)

This outcome is the standardized sum of the following 4 standardized measure:

- Feels often understood by neighbors (G1.13)
- Has few issues with neighbors (G1.14)
- Feels important in the local community (G1.15)
- Feels integrated within his household/family and with friends (D3.7)

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