



PRE-ANALYSIS PLAN FOR

From Workfare to Work and Economic Well-being:

A Randomized Evaluation of Labor-Intensive Public Works on 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 the economic impacts of these four LIPW schemes. 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; Dem. Republic of Congo.

JEL: I3, D14, J24, C93

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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 schem, 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 individuals benefited from LIPW activities, earning US\$ 3 a day during at least 4 months. Of those 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.

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

Given the substantial unemployment in the region and lack of opportunities, it is expected that the LIPW activities will raise the short-term income of the beneficiaries participating in the program. Depending on prevailing income elasticities, beneficiaries may spend part of the windfall income on ‘productive’ goods (e.g., health and education spending) while another part of the income for wage labor may be spend on ‘unproductive’ – or temptation – goods (e.g., cigarettes, alcohol). Part of the extra income not consumed may be saved or invested in human and/or physical capital, which in turn could lead to consumption smoothing, increased coping strategies and long-run improvements in income-generating capacity (Alderman and Yemtsov, 2012; Beegle, Galasso, and Goldberg, 2014; Schady and Smitz, 2009).

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.

However, research has shown that the world’s poor may have difficulty in generating savings (Demirguc-Kunt and Klapper, 2012; Karlan et al, 2014). Furthermore, some scholars cast doubt on the ability of LIPWs to have positive investment effects (McCord and Farrington, 2008). The savings incentive may therefore help in sustaining savings, while the training program could to alleviate important human capital constraints. All the activities thus have the potential to smooth consumption, improve coping strategies and allow individuals to increase their future earning capacity. A priori, however, it is unclear what constraints matter most and which activities are best fit to relax those constraints (Blattman and Annan, 2014). The core of the proposed impact evaluation aims to investigate which combination of program components is most cost-effective to increase economic outcomes in the medium-term, that is, 1.5 years (on average) after LIPW activities have been completed.

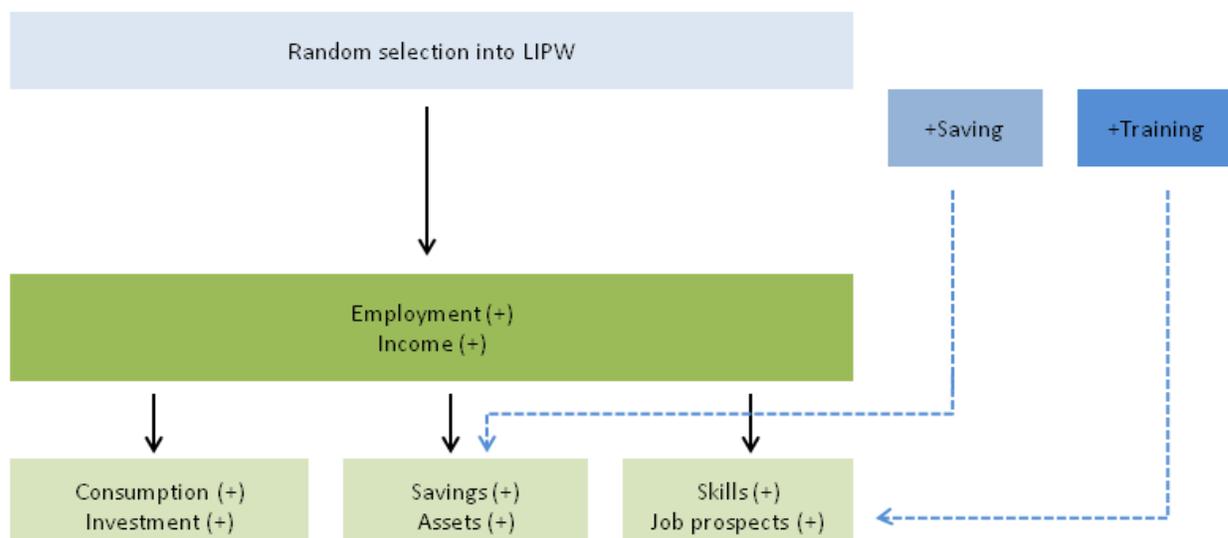


Figure 1: Theory of Change

3.2 HYPOTHESES

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

- Q1: What is the impact of the LIPW program on economic outcomes (income, consumption, employment, investment, savings)?

H1: The labor-intensive public works program leads to increased economic outcomes in the medium term, potentially including improved coping strategies, productive investments and increased savings.

- Q2: What is the additional impact of the savings incentive?

H2: Assuming savings and/or behavioral constraints, we hypothesize that the savings incentive leads to increased economic outcomes in the medium term.

- Q3: What is the additional impact of the training program?

H3: Assuming imperfect factor markets and incomplete provision of public services, we hypothesize that the training program leads to increased economic outcomes in the medium term.

- Q4: What is the impact of all three activities on economic outcomes?

H4: Assuming both savings constraints and imperfect factor markets, we hypothesize that the combination of LIPW, the training program and the savings incentive leads to increased economic outcomes 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.

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.

The following table summarizes the experimental design.

Table 2: Evaluation Design

	Control	Program participants			
		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%

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

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

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.

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:

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

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 is selected as a beneficiary and X_i 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:

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

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 β_1 , β_2 , β_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:

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

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 13 outcomes of interest distributed into 5 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 FAMILY 1: LABOR MARKET OUTCOMES

1. Active labor market participation

is a binary variable taking the value one if the individual declares either having a job (in wage or self-employment, or agriculture) or having looked for one over the last month. We will subsequently decompose this measure into the employed vs. non-employed measure and then, among the unemployed, between actively searching for a job or not.

- Has worked in a job (C1.1 & C1.36)
- Has looked for a job in the last 30 days (C1.3a)

2. Monthly hours worked

sums the hours spent in wage employment or self-employment over the week and uses the total number of days worked in a typical month to infer monthly hours worked.

- Hours worked in wage employment (C1.9 & C1.20) /week
- Hours worked in self-employment (C1.9 & C1.20) /week
- Total days you spend in wage-employment in a typical month (C1.10 & C1.21)
- Total days you spend in self-employment in a typical month (C1.10 & C1.21)

6.2 FAMILY 2: EARNINGS AND SAVINGS

3. Total monthly earnings

sums all the earnings declared by an individual, whether they come from wage employment, self-employment or agricultural activities (regardless of the individual's classification of primary vs. secondary activity). We will decompose this measure into source of income (wage, self-employment and agricultural activities).

- Total income from wage employment (C1.13, C1.24)
- Total profits from self-employment (C1.12, C1.23)
- Total sell from agricultural activities (C1.5.12)

4. Total net financial savings

computed from declared savings and debts.

- Plus total savings (C2.4)

- Minus total debt (C2.2.1 and interest rates C2.2.2b)

5. Use of formal financial institutions

aims at measuring the impact of the project on household behavior regarding the formal financing sector.

- Has opened a bank account (C2.4) following LIPW (C2.4.1)
- Used a formal saving system after LIPW (C2.3.1)
- Used a formal lending system after LIPW (C2.2.2)

6.3 FAMILY 3: HUMAN CAPITAL

6. Hard skills

We build a summary index (à la Anderson (2008)) for skills based on four different measures relating to hard skills. We will then decompose this index.

- Received training in a trade since LIPW started (C3.1)
- Possesses unused skills (C3.3)
- Labor market difficulties when exist (C3.4) are not due to lack of skills (C3.4.1=(1|3))
- Business difficulties when exist (C3.5) are not due to lack of skills (C3.5.1=1)

6.4 FAMILY 4: HOUSEHOLD ECONOMIC ACTIVITY AND EARNINGS

7. Employment from other members of the household

We will summarize labor market outcomes of other household members by creating a summary index à la Anderson (2008).

- Other household members or head is working (C1.28, C1.31 & C1.32)
- Type of works from other household members or head (C1.29, C1.32)

8. Earnings by other members of the household

We sum the total labor income from the other household members.

- Earnings from employment by the head of the household (if different than respondent) (C1.30)
- Earnings from employment by other household members (C1.33, C1.35)

6.5 FAMILY 5: HOUSEHOLD EXPENDITURE AND INVESTMENTS

9. Total expenditures aggregates

we sum total expenditures from individuals and their household into one measure expressed in monetary value.

- Food expenditure:
 - Total spent on food (B3.1.1.a)
- Non-food expenditure:
 - Total spent on medical (B3.1.1.b)
 - Total spent on leisure (B3.1.1.c)
 - Total spent on clothing (B3.1.1.d)
 - Total spent on transportation (B3.1.1.e)
 - Total spent on energy and water (B3.1.1.f)
 - Total spent on phone and internet (B3.1.1.g)
 - Total spent on cosmetics (B3.1.1.h)
 - Total spent on other services (B3.1.1.j)

10. Assets Accumulation

We will follow Filmer and Pritchett (2001) and use principal component analysis to build an asset index.

- Dwelling (B2.4-B2.6);
- Furniture & electronic equipment (B2.1);
- Livestock (B2.1);
- Real estate (B2.2);
- Agricultural land (C1.37, C1.39, C1.40 if C1.36=1)

11. Children schooling and welfare

This is an index made of 4 binary variables, each taking the value one when the outcome positively affects child's welfare (we will adjust survey instrument accordingly).

- Schooling (B1.2, B1.6);
- Worked over last month (C5.4);
- Domestic work over the last month (C5.8);
- Suffered from a common disease (at least one "yes" from E2.7 to E2.17)

12. Coping strategies

Coping strategies is only measured on families that suffered a negative income shock. Although many of these shocks are potentially exogenous we intend to measure whether LIPW allowed individuals to protect themselves ex-ante and whether it helped them cope with the shock ex-post. We thus construct two binary variables: the first one takes the value 1 if the household has suffered a negative shock or more; the second one takes the value 1 if affected household coped without reducing basic expenditures, investments or depleting assets. We also check whether household relied on official assistance program (other than the STEP-LIPW).

- Suffered a negative income shock or more (B4.1)

- Were able to use savings or went through with no need for asset/investment depletion (B4.1.1)
- Has received assistance from an NGO or the government (B4.2)

13. Remittances

LIPW money may also have been used to help others, or reduce the need for assistance from them. We summarize this outcome by subtracting all monetary assistance given by relatives from the one offered by the treated household. If LIPW helped household on this aspect on average, their net position on this matter should have increased.

- Household net position regarding financial assistance with relatives (Money given, B3.8, minus money received B3.5, B3.6, B3.7) is null or positive

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