Evaluating Impacts of Rural Road Maintenance Employment among Women in Laos

Pre-analysis plan

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1. Background

We aim to strengthen the evidence-base on the effectiveness of public works programs (PWPs) by carrying out a Randomized Control Trial (RCT) of one such program – Road Maintenance Groups (RMGs) in Laos. PWPs are an important part of the social protection toolkit in low and middle-income countries around the world. While several non-experimental studies and one evaluation using a randomized phase-in design find that PWPs can lead to development objectives such as reducing poverty and generating employment, two RCTs find weak support for the development impacts and cost-effectiveness of these programs. The Laos RMG program offers jobs to carry out routine maintenance tasks on feeder roads to women in remote, rural Laos. By carrying out an RCT of this intervention, we aim to contribute to experimental evidence on PWPs, for an intervention that appears to be better targeted than PWPs previously evaluated experimentally. Second, we evaluate the effect of this PWP on women’s empowerment, including an extreme measure of disempowerment – exposure to Gender Based Violence (GBV). This area of inquiry is underexplored in the current literature on PWPs.

This pre-analysis plan (PAP) describes the methodology and data that will be used to assess the impacts of the program. The next section provides an overview, including a description of the interventions, sampling, and data. Subsequent sections describe the hypotheses, outcome variable definitions, and methodology and estimation methods.

2. Overview

2.1 Interventions and program context

The RMG intervention is a part of a broader program, the Laos Poverty Reduction Fund (PRF), which aims to reduce poverty through infrastructure improvements in sectors such as education, drinking water, irrigation, health and transport. These improvements are identified using a community driven development (CDD) approach. A process evaluation of PRF’s projects carried out in 2015 highlighted that they had been successful in improving infrastructure but that the post-completion sustainability of road projects was not very good: road quality deteriorated quickly due to a lack of maintenance.

In response, PRF introduced the Road Maintenance Groups program. Viewing the labor-intensive task of carrying out road maintenance as an opportunity to create jobs for the vulnerable, PRF is targeting

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2 Rosas and Sabarwal, 2016.
3 Beegle et al. (2017), Bertrand et al. (2017).
4 It is important to mention that there are several other on-going RCTs including at the World Bank that aim to fill this gap: in Tunisia, Egypt, Comoros (Development Impact Evaluation unit) and in Central African Republic (Africa Gender Innovation Lab).
5 Data from the baseline shows that households selected for participation in RCT are worse off on a number of development outcomes (income, assets, etc.) than a random sample of households in the village.
6 Unless otherwise specified, the information in this section is drawn from the PRF III Operations manual document available here: http://prflaos.org/sites/default/files/Library%20Items/files/1080/eng//prf_operationsmanual_Eng.pdf
women from poor households living in these villages, where wage-earning opportunities are limited. These women are organized into RMGs and tasked with carrying out routine road maintenance, tasks such as clearing roads of vegetation, clearing the drainage system and making small repairs to the road surface. They are provided with simple training and basic hand tools and paid a fixed-daily rate set slightly below the prevailing market wage in each village. Wage payments are made monthly or quarterly, depending on the payment preference of each RMG, corresponding to the number of days worked. Typically, this is a few days each month, with monthly variation based on maintenance needs. There may be deductions to wage payments in the case of poor performance. To enforce this, PRF carries out road quality audits each month.

The road maintenance activities under this cycle started in June 2018, soon after the road improvements under PRF III were completed. The maintenance contracts run for 18 months, from October 2018 up to the January 2020, covering 2 full rainy seasons and providing on average 75 days of employment for each RMG member. This equates to just over 4 days of work per month. The RMG members were paid wages of around LAK 60,000/day (US$7.3/day), resulting in a total average income of around $550 per RMG member over the implementation period.

2.2 Sampling
PRF identified 71 road segments covering 344 km of rural roads in 7 provinces and 24 districts for the RMG intervention in this cycle. Usually, each road segment is maintained by one RMG consisting of 3-5 members from one village. RMG members were selected from each village prior to the intervention. The Village Head played an important role in the selection process by informing and then identifying eligible candidates. There were two eligibility criteria:

(i) Must belong to a poor household, and up to one woman from each such household could participate in the selection.

(ii) Must be between 18 and 50 years old, though the upper age limit was not strictly enforced.

The number of eligible and interested women exceeded the number of available RMG jobs in every village. Therefore, a lottery was carried out to select RMG group members. The lottery was carried out manually, with women drawing numbered balls from an urn. Women who drew numbers 1 through n, where n was the number of spots in the RMG group, became RMG members. Women who pulled numbers starting from n + 1 joined the waitlist. For the waitlist, the number also determined their position on the waitlist. Our final sample includes 339 RMG members and 843 waitlist women across 85 villages in 7 provinces. The women in the RMG groups form our treatment group and the women on the waitlist form our control group.

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7 Generally, the RMG members work more days in the rainy season months (e.g. 6-7) and fewer days in the dry season (2-3).
8 In the pilot, the RMGs were paid for 50 person-days of work per km per year, divided into equal monthly payments. These were paid in full each month, unless PRF inspections found the performance of the RMG to be poor, in which case a deduction was applied.
9 The size of the groups was set based on the length of the road segment, with appx one RMG member per km.
10 Although most RMGs were drawn from only one village, some were drawn from two. This is why we ended up with a higher number of villages than road-segments. Further, we had targeted 87 villages when planning data.
One point worth noting is that the members of the waitlist could be used to replace RMG members who dropped out or did not take up the job, leading to imperfect compliance with our treatment assignment. As of the writing of this PAP, approximately one month before program completion, 6% of women have either not taken up the program or have dropped out, based on PRF records.

2.3 Data

The primary instruments we use to measure impacts on household welfare and women’s empowerment are a household and women’s survey administered to the treatment and control groups at baseline and endline. Baseline data were collected between September and October 2018, before any salary payments were made. We were able to interview 339 treatment households and women, and 843 control households and women. The baseline data allowed us to verify that treatment and control groups were balanced on observable characteristics prior to the intervention, supporting our understanding that the lotteries were conducted fairly and without bias

Endline data took place in December 2019 and January 2020, 18 months after the program started.

We also carried out a market price survey at baseline and endline, concurrently with the household and women’s surveys, to determine if there were any inflationary impacts of wage payments. Prices were collected for a 5-page list of items from RMG villages and proximal non-RMG villages. They were collected from at least one market in each village, with markets being selected based on representativeness: i.e. where most people in the community made common purchases. Baseline and endline price data allow us to capture pre-existing and post-program differences in prices respectively.

3. Hypotheses and variable definitions

Groups of hypotheses:

Group A: Impacts on primary outcomes
Group B: Impacts on secondary outcomes
Group C: Mechanisms
Group D: Heterogeneity of impacts

We test all hypotheses by estimating treatment effect on the outcomes specified below for each group. Econometric specifications for all hypotheses are described in Section 4.

A note on variables and transformations,

- When more than one outcome variable is mentioned, we will run the estimation for each outcome variable.

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This evidence is not presented here but is available upon request.

At the time of submission of the PAP, the data collection firm is cleaning the data to share it with the team.

For example, if the majority of people bought food items from small shops in the village, but ingredients and seasoning in a larger market in a nearby town, the prices of food items would be collected from small shops within the village while the prices for ingredients and seasoning would be taken from the town market.
• z-score indices are calculated as the sum of z-scores of each component of the index. z-scores for each component of the index are computed by subtracting control group mean and dividing by the control group SD (as in Kling, Katz and Liebman, 2007).
• Winsorization wherever mentioned is carried out on raw variables (i.e. pre-transformation) unless otherwise specified.
• Questions for which 90% of observations have the same value within the relevant sample will be omitted from the analysis, including any indicators or hypothesis tests.
• In general, we treat “Do not know”, “Do not remember”, “Not Applicable” and other responses as missing values in the analysis, unless otherwise specified.

3.1 Group A: Impacts of the treatment on primary outcomes

The objective of the RMG program is threefold: (i) improve the lifespan of public goods by maintaining rural feeder roads; (ii) increase women’s employment opportunities; (iii) serve as a social protection mechanism for poor households by providing families with additional income through RMG work. Our IE design does not allow for rigorously testing of how well the program achieves the first objective. We focus Group A hypotheses on understanding whether the program has accomplished its second and third objectives.

3.1.1 Hypothesis A1: the treatment increases women’s productive work

1. RMG-eligible women’s labor force participation [binary indicator, constructed based on LF5, LF7, LF8, LF9, LF10, LF11]
2. RMG-eligible women’s employment in paid work [binary indicator, constructed based on LF5 and LF7]
3. RMG-eligible women’s monthly earnings from the first job [sum of LF16.1 and LF16.2 adjusted for payment frequency LF17]
4. RMG-eligible women’s monthly earnings from the first job, transformed to inverse hyperbolic sine values;
5. RMG-eligible women’s monthly earnings from the first job, winsorized at 99th percentile;
6. RMG-eligible women’s monthly earnings from the second job [sum of LF26.1 and LF26.2 adjusted for payment frequency LF27]
7. RMG-eligible women’s monthly earnings from the second job, transformed to inverse hyperbolic sine values;
8. RMG-eligible women’s monthly earnings from the second job, winsorized at 99th percentile;
9. RMG-eligible women’s monthly earnings from the first and second jobs combined [see 3 and 6 above]
10. RMG-eligible women’s monthly earnings from the first and second jobs combined, transformed to inverse hyperbolic sine values;
11. RMG-eligible women’s monthly earnings from the first and second jobs combined, winsorized at 99th percentile;
12. Number of hours per day (averaged over the last two days) spent on paid work by RMG-eligible women [constructed from TU1, total of hours coded 4 “Paid work”]
13. Number of hours per day (averaged over the last two days) spent on paid work by RMG-eligible women, winsorized at 99th percentile;

Of the groups of outcomes 3-5, 6-8 and 9-11 we treat indicators 5, 8 and 11, respectively, as primary; other outcomes serve as robustness checks. Of outcomes 12 and 13, we treat 13 as primary.
In constructing labor force participation indicators, we use the following definitions from the Laos Labor Force Survey,

Currently employed: There are two situations in which a person can be defined as being currently employed. Either the person is actually working in the reference week, or he or she has an attachment to a job or business but did not work during the reference week. This second group (those with a job attachment) is identified by asking a separate question. Those with a job attachment are defined as those who have a job, business, farming or other economic activity to definitely return to.

Currently unemployed: The strict international standard definition of unemployment is based on three criteria which must be satisfied simultaneously. These criteria are: ‘without work’, ‘currently available for work’, and ‘seeking work’. However, the ‘seeking work’ criterion is usually considered too restrictive and is often relaxed for countries in which the labor market is not well developed. Accordingly, in the Lao Labor Force Survey the currently unemployed are made up of those persons who did not have a job or business or were not employed (as defined above), and who did one of the following:

- Either they looked for work in the last 7 days before the interview;
- or they did not look for work in the last 7 days but were available to work and did not look for work because they thought no work was available, or they were awaiting the results of previous enquiries, or waiting to start work, or considered that it was the off-season for fishing or agriculture.

In operational terms, unemployed is defined as the sum of two groups: all those who looked for work in the last 7 days, together with those who did not as long as they did not give ‘not available’ in response to whether they were available for work.

We interpreted these definitions in the context of our survey as follows,

1. Employed:
   - Either LF5 is 1, or
   - LF5= 2 but LF7 = 1 LF8 is either 1 or 2, or
   - LF5 = 2 but LF7 == 1 & LF8 is 3, 4, 5, 6, 7 or 9 (Using the less strict definition of employment as described in the Labor Force guidance above).

   Note that in effect employment is defined based on LF5 and LF7 alone (as per the less strict definition).

2. Unemployed
   - If LF5 = 2 and LF7 = 2 and LF9 = 1
   - If LF5 =2 and LF7 = 2 and LF9 = 2 and LF11 = 1

3. In labor force: Classified as either employed or unemployed.

4. Not in labor force: Any working-age adult (aged over 15) not in the labor force.

3.1.2 Hypothesis A2: the treatment increases household income

1. Average monthly household income over past 12 months [HN22 (or sum of HN2, HN4, HN6, HN8, HN10, HN12, HN14, HN16, HN18, HN20 and HN21) divided by 12];
2. Average monthly household income over past 12 months, transformed to inverse hyperbolic sine values;
3. Average monthly household income over past 12 months, winsorized at 99th percentile;
4. Farm and forestry household income over past 12 months, including agriculture, aquaculture and forestry [sum of HN2, HN4, HN6, HN8 and HN10, divided by 12];
5. Farm and forestry household income over past 12 months, transformed to inverse hyperbolic sine values;
6. Farm and forestry household income over past 12 months, winsorized at 99th percentile;
7. Non-farm business household income over past 12 months [sum of HN12 and HN14 divided by 12]
8. Non-farm business household income over past 12 months, transformed to inverse hyperbolic sine values;
9. Non-farm business household income over past 12 months, winsorized at 99th percentile;
10. Household income from wages over past 12 months [sum of HN16 and HN18 divided by 12]
11. Household income from wages over past 12 months, transformed to inverse hyperbolic sine values;
12. Household income from wages over past 12 months, winsorized at 99th percentile;

Our primary income outcome is income winsorized at 99th percentile. We use other as robustness checks.

Note: although our preferred income aggregate is from the income module, we will use the data on wages from LFP module as a robustness check. Specifically, for household wage income (#10 above), we will use a second construct by summing income from LF16.1, LF16.2, LF26.1 and LF26.2 and adjusting for payment frequency using LF17 and LF27 in addition to data from the household income module (sum of HN16 and HN18)

Note: if the household does not have income in a specific category, we record it as zero income.

3.2 Group B: Impacts of the treatment on secondary outcomes

We explore three broad areas of secondary outcomes: (i) household welfare outcomes; (ii) women's empowerment outcomes; (iii) outcomes of other adult household members; (iv) outcomes of children; (v) community level outcomes.

Household welfare outcomes

The households may use additional income due to participation in the RMG program in several ways, some of which may lead to sustainable improvement in household welfare beyond the expiration of the program. We will explore such potential uses of income focusing on: consumption, captured by improved household conditions, purchase durable assets and better nutrition (B1 – B3) and investment (B4).

The intervention may also enable households to increase their savings, and thus create demand for greater financial inclusion. We will explore impacts on these outcomes, too (B5).

3.2.1 Hypothesis B1: the treatment improves housing conditions

First principal component of housing characteristics coded as explained below:

1. House has a brick wall [binary indicator equal to 1 if HC6=1 or HC6=2]
2. House has a solid floor [binary indicator equal to 1 if HC10 =1,2 or 3]
3. Number of rooms in house [HC13]
4. House has piped water [binary indicator equal to 1 if HC15 and HC16=1 or HC15 and HC16=2]
5. House has toilet [binary indicator equal to 1 if HC21 =11]
6. House has outside roofed kitchen [binary indicator equal to 1 if HC24=2]
7. House uses electricity [binary indicator equal to 1 if HC28=1 or 2 or 3]

Note: we carry out this analysis on the primary residence of the household (i.e. first listed residence in the data).

As a secondary check, we estimate treatment effects on a housing index, a z-score index of elements 1-8 above.

3.2.2 Hypothesis B2: the treatment improves household ownership of durable assets
1. First principal component of durable goods coded as explained below:
   - Sqm of residential land [PD1.4, winsorized at the upper 90% percentile]
   - Owns a vehicle [PD1.6]
   - Owns a motorcycle [PD1.7]
   - Owns a refrigerator or freezer [PD1.10]
   - Owns a steam rice cooker [PD1.14]
   - Owns an electric rice cooker [PD1.15]
   - Owns a tractor [PD1.17, PD1.18]
   - Owns a rice mill [PD1.23]
   - Owns a television [PD1.24]
   - Number of cell phones owned by all household members [PD1.27]
2. Total amount spent on large household purchases over the last year [sum of all items in PD4]
3. Total amount spent on large household purchases over the last year [sum of all items in PD4], transformed to inverse hyperbolic sine values
4. Total amount spent on large household purchases over the last year [sum of all items in PD4], winsorized at 99th percentile

Of outcomes 2-4, we treat 4 as the primary outcome. For outcome 1, we also construct a z-score index of durable assets using the number of assets reported for each of the assets listed in 1 as a robustness check.

3.2.3 Hypothesis B3: the treatment improves household nutrition
1. Per-person household consumption of meat, chicken, duck and fish per week [NT4.1 + NT4.2, divided by the number of household members (count of HR2 if HR13=1)]
2. Per-person household consumption of fruit and vegetables per week [NT5.1 + NT5.2, divided by the number of household members (count of HR2 if HR13=1)]

Note: to capture age-specific differences in consumption, we will control for the number of grown-ups and children in these regressions.

3.2.4 Hypothesis B4: the treatment increases household investments

Agricultural investments
1. Household made investments in its farm (binary indicator, constructed based on HE9; sample limited to households who reported having a farm at baseline, 0 for households who got rid of the farm at endline);
2. Total investments on household farm in preceding 12 months [Sum of items in HE10A, 0 for households who did not make any investments]
3. Total investments on household farm in preceding 12 months [Sum of items in HE10A, 0 for households who did not make any investments], transformed to inverse hyperbolic sine values
4. Total investments on household farm in preceding 12 months [Sum of items in HE10A, 0 for households who did not make any investments], winsorized at 99th percentile
5. Household produces cash crops (binary indicator, constructed based on HE3)
6. Size of agricultural land (PD1.2)
7. First principal component of livestock: specifically, the number of cows [PD1.38], buffaloes [PD1.40], goats [PD1.41], pigs [PD1.43], chicken [PD1.44] and ducks [PD1.45].

Of outcomes 2-4, we treat 4 as primary.

**Investments in non-agricultural enterprise**

8. Total number of non-agricultural businesses (HE11.2)
9. Household opened a new business (binary indicator, equal to 1 if HE11.1=1)
10. Household made investments in the household enterprises in preceding 12 months (binary indicator of whether HE27=1)
11. Total investments in the household enterprises in preceding 12 months [Sum of items in HE29]
12. Total investments in the household enterprises in preceding 12 months [Sum of items in HE29], transformed to inverse hyperbolic sine value
13. Total investments in the household enterprises in preceding 12 months [Sum of items in HE29], winsorized at 99th percentile
14. Number of business buildings [PD1.5]

Of outcomes 11-13, we treat 13 as primary.

**Investments in human capital**

15. Total spending on education [Average per child of ED12.10]
16. Total spending on education [Average per child of ED12.10], transformed to inverse hyperbolic sine value
17. Total spending on education [Average per child of ED12.10], winsorized at 99th percentile
18. Total spending on education for children under the aged under 12 [Average per child aged under 12 of ED12.10]
19. Total spending on education for children under the aged under 12 [Average per child aged under 12 of ED12.10], transformed to inverse hyperbolic sine value
20. Total spending on education for children under the aged under 12 [Average per child aged under 12 of ED12.10], winsorized at 99th percentile

Of groups of outcomes 15 – 17 and 18 – 20, we treat 17 and 20, respectively, as primary.

**Other**
21. Whether household saves for investment or business [coded =1 if AV8=4, 7 or 8 and coded 0 otherwise]

3.2.5 Hypothesis B5: the treatment increases household savings and financial inclusion
1. At least one member of the household is saving [coded =1 if AV6=1, coded 0 otherwise]
2. At least one member of the household has formal savings [coded=1 if AV7 =1,2,3,5 or 6 and coded= 0 otherwise]
3. At least one member of the household has a bank account [based on AV1]
4. Number of bank accounts owned by all household members [count of AV3.1, minus duplicates if bank account shared with household member (AV5.1) and both members listed in AV3.1]

Women’s empowerment and well-being outcomes

While we expect the RMG intervention to improve women’s short-run labor market outcomes, the effects on empowerment and well-being outcomes are ambiguous. The interventions provide women with their own income, in the setting where wage earning opportunities, especially for women, are scant. Theories of Nash-bargaining (Manser and Brown, 1980) suggest that by improving women’s outside options, the program is likely to improve women’s outcomes within marriage, including exposure to GBV. However, psychological theories of back-lash, or use of violence for extractive purposes (Block and Rao, 2006, Bobonis et al., 2013) suggest a possibility of increase of GBV. Certain theories of violence as an instrument for extraction allow for either possibility. Instrumental violence theory posits that increasing a woman’s economic opportunities may increase or decrease the incidence of violence she experiences depending on her initial bargaining level (Eswaran and Malhotra 2011, Heath 2014). At lower levels of empowerment, working can lead to an increased incidence of GBV and at higher levels, it can decrease it. We will explore if the experimental evidence is consistent with instrumental violence models, Nash bargaining models or psychological back-lash models by evaluating differences in the ATE of the RMG intervention and heterogenous effects by baseline empowerment levels.

Physical exhaustion from work, especially if the work comes on top of usual women’s responsibilities, may decrease wellbeing. On the other hand, if the jobs are associated with a sense of purpose and pride, it may increase.

We explore several types of such outcomes, focusing on women’s internal empowerment (i.e. how empowered they feel), women’s empowerment within household and women’s empowerment in the community. To capture impacts on women’s internal empowerment we use locus of control scale. To capture empowerment within household, we measure impacts on (i) women’s decision-making within household; (ii) women’s nutrition and (iii) exposure to GBV. We capture empowerment within community through a series of questions about women’s participation in community events and decision-making.

We also explore impacts on women’s wellbeing through a series of questions on (i) time-use, focusing on potential reduction in leisure time; (ii) subjective well-being; (iii) non-domestic violence.

3.2.6 Hypothesis B6: the treatment increases women’s internal empowerment
1. Measure of internal locus (i.e. indicating how much control one has over the events in one’s life) [a z-score index of LC7, LC4, LC16, LC20, LC31, LC40, LC47]
2. Measure of external locus (i.e. indicating how much control external events have over one’s life) [a z-score index of LC10, LC13, LC18, LC23, LC24, LC36, LC39, LC43, LC45 with items reverse coded]

3. Aggregate locus measure [a z-score of items LC7-LC47, with external locus items reverse coded]

4. Count of decisions women consider themselves capable of making

As a robustness check, we will run multinomial logits for 1 and 2.

3.2.7 Hypothesis B7: the treatment increases women’s decision-making in the household

1. Woman is among final decision makers on at least one decision [binary indicator, equal to 1 if at least one of DM1c, DM6c, DM7c or DM8c =1]
2. Fraction of decisions where woman is among final decision makers [sum of DM1c, DM6c, DM7c or DM8c =1, divided by 4]
3. Woman is among final decision makers on at least one decision she cares about [1 if at least one of DM1c, DM6c, DM7c or DM8c =1 and corresponding value question DM1b1, DM6b1, DM7b1 or DM8b1 = 1]
4. Fraction of decisions where the woman is among final decision makers, among decisions she cares about [sum of DM1c, DM6c, DM7c or DM8c =1 when corresponding value question DM1b1, DM6b1, DM7b1 or DM8b1 = 1, divided by count of decisions in DM1b1, DM6b1, DM7b1 or DM8b1 = 1]
5. Woman believes she should be decision maker on at least one decision [1 if at least one of DM1e, DM6e, DM7e or DM8e =1]
6. Fraction of decisions where woman believes she should be decision maker [sum of DM1e, DM6e, DM7e or DM8e =1, divided by 4]
7. Count of decisions women care about making [sum of DM1b1=1, DM6b1=1, DM7b1=1 and DM8b1=1]

Note: DM6c is only administered to women who earn their own income. In the primary specification (as for example, in 2 above), we include women who don’t earn an income in the sample (treating them as non-decision makers). As a robustness check, we will restrict the sample to those who earn and income, and re-run 1 and 2.

3.2.8 Hypothesis B8: the treatment increases women’s nutrition

1. Women’s consumption of glutinous rice in last two days [NT2 for women]
2. Women’s consumption of ordinary rice in last two days [NT3 for women]
3. Number of days women consumed meat, fish, chicken or duck last week [NT3.1 for women]
4. Number of days women consumed eggs last week [NT3.2 for women]
5. Number of days women consumed milk, yogurt or dairy last week [NT3.3 for women]

Our primary specification will use an index based on z-scores of 3-5.

3.2.9 Hypothesis B9: the treatment reduces women’s exposure to GBV

1. Exposure to controlling behavior in past 12-months [dummy=1 if yes to any component of DV03]
2. Exposure to emotional violence in past 12-months [dummy=1 if yes to any component of DV04]
3. Exposure to physical violence in past 12 months [dummy=1 if yes to any component of DV05a-DV05g]
4. Exposure to sexual violence in past 12 months [dummy=1 if yes to any component of DV05h-DV05j]
5. Exposure to violence perpetrated by other household members (not intimate partner) [dummy=1 if yes to any component of DV16.1, DV16.2, DV24.1, DV24.2 and DV17a=1]

3.2.10 Hypothesis B10: the treatment increases women’s participation in community life
1. Visits to/by non-household members [binary variable, equal to 1 if SN1 = 1]
2. Number of people who visited in the past two weeks [SN2]
3. Number of people who visited to talk about business in the past two weeks [SN3]
4. Number of people who visited to talk about personal matter in the past two weeks [SN3]
5. Social contact index [z-score index composed of SN2, SN3, SN4]
6. Comfortable raising hand in public [binary indicator equal to one if LM1 is equal to 2 or 3]
7. Comfortable of speaking up to ensure payment of wages for PWP [binary indicator equal to one if LM2 is equal to 2 or 3]
8. Comfortable criticizing authorities [binary indicator equal to one if LM3 is equal to 2 or 3]
9. Time spent on community activities [average per day, over the past two days, TU1 code 18]

For indicators 6, 7, 8 as a robustness check we will also use multinomial logit on raw variables (categorical).

3.2.11 Hypothesis B11: the treatment reduces leisure time
1. Time spent in past two days on leisure or entertainment activities [TU1 code 17, averaged over past two days]
2. Time spent in past two days on leisure, entertainment or other personal activities such as sleeping and eating [TU1 code 1,2,17, average over past two days]

We treat 2 as a primary outcome.

3.2.12 Hypothesis B12: the treatment increases women’s subjective well-being
1. Life-satisfaction [UW1, reverse coded]
2. Current position on 6-step ladder [UW2]
3. Expected position on 6-step ladder [UW4]
4. Expected improvement in wellbeing [UW4 – UW2]

3.2.13 Hypothesis B13: the treatment changes women’s exposure to non-domestic violence
1. Exposure to physical violence by non-household member [binary indicator equal to one if either DV16.1 or DV16.2 =1]
2. Exposure to sexual violence by non-household member [binary indicator equal to one if either DV24.1 or DV24.2 = 1]

Outcomes of other adult household members
The RMG program may have impacts on other members of the households, apart from the RMG members. If the RMG participants need to free time from other activities in order to participate in the RMG work, other household members may adjust their time allocation to pick up household work. They may do so by cutting on non-household work or on leisure activities.

3.2.14 Hypothesis B14: the treatment changes time allocation of other household members between non-household work and household work

1. Employment in the past seven days in primary or secondary non-household job [coded 1 if LF5=1 or LF21=1, 0 otherwise]
2. Hours of employment in past seven days in primary or secondary non-household job [sum of LF19, LF29; zero if missing]
3. Employment in past seven days on household farm [1 if listed in HE5]
4. Hours of employment in past seven days on household farm [HE7, zero if missing]
5. Employment in past seven days on household enterprise [1 if listed in HE14]
6. Hours of employment in past seven days on household enterprise [HE17, zero if missing]
7. Employment in past seven days on household farm or enterprise [1 if listed in HE5 or HE14]
8. Hours of employment in past seven days on household farm or enterprise [sum of HE7 and HE17, zero if missing]

Note: analysis in this section is carried out for all household members except RMG and waitlist women.

Outcomes of children

We expect the intervention to affect children through increase in women’s earnings directly (as women are more likely to spend on children) and indirectly through increase in women’s bargaining power. We will explore impacts on several indicators which capture children’s well-being: nutrition, school attendance and educational expenses.

3.2.15 Hypothesis B15: the intervention improves children’s outcomes in nutrition

1. Children’s consumption of glutinous rice in last two days [per-child average of NT2]
2. Children’s consumption of ordinary rice in last two days [per-child average of NT3]
3. Number of days children consumed meat, fish, chicken or duck last week [per-child average of NT3.1]
4. Number of days children consumed eggs last week [per-child average of NT3.2]
5. Number of days children consumed milk, yogurt or dairy last week [per-child average of NT3.3]

Our primary specification will use a z-score of a-e.

Note: the children in these sample are children of RMG/WL women aged 2-5.

3.2.16 Hypothesis B16: the intervention improves children’s outcomes in education

1. Time spent at school in the past two days [average of variables TU2_1_3 and TU2_2_3]
2. Time spent at school for children aged 12 and above [average of variables TU2_1_3 and TU2_2_3]
3. Number of children enrolled in school [Count of ED12]
4. Number of children aged 12 and above, enrolled in school [Count of ED12 for children aged over 12]

Note: outcomes 1 and 2 are measured at the child level; outcomes 3 and 4 – at the household level.

3.2.17 Hypothesis B17: the intervention decreases child labor
1. Time spent on paid labor in the past two days [average of variables TU2_1_1 and TU2_2_1]
2. Time spent on paid labor in the past two days by children aged 12 and over [average of variables TU2_1_1 and TU2_2_1]
3. Time spent on unpaid labor in the past two days [Total of TU2, averaged over past 2 days]
4. Time spent on unpaid labor in the past two days by children aged 12 and over [Total of TU2, averaged over past two days]

Community level outcomes

3.2.18 Hypothesis B18: the treatment increased prices
We will non-experimentally explore the possibility that the RMG intervention increased prices through the influx of cash payments to the RMG members.

3.3 Group C: Mechanisms of impacts
We explore selected mechanisms behind impacts on household welfare: participation in the RMG program may (i) enable households to diversify their income and (ii) smooth the impact of shocks.

3.3.1 Hypothesis C1: the treatment triggered income diversification
1. Intervention increases the number of sources of household income [Count of individual items of HN2, HN4, HN10, HN12, HN14, HN16, HN18]
2. Intervention increases household non-farm income [HN22 minus (HN2+HN4)]
3. Intervention increases the share of non-farm income in household’s total income [HN22 minus (HN2+HN4) divided by HN22]

3.3.2 Hypothesis C2: the treatment smooths the impact of shocks on household income
For this hypothesis, we will explore the same outcomes as in hypothesis A2. However, we will focus on the interaction of the treatment impact with an indicator for shock. We present econometric specification in Section 4, and list the shock variables below:
1. Death of a household member [binary indicator, equal to 1 if EH1A = 1]
2. Illness or serious accident of any household member that required hospitalization [binary indicator, equal to 1 if EH1B=1]
3. Unemployment or business failure of any household member [binary indicator, equal to 1 if EH1C=1]
4. House or business loss due to a drought, flood, or other natural disaster [binary indicator, equal to 1 if EH1D=1]
5. Total loss of crop [binary indicator, equal to 1 if EH1E=1]
6. Loss, robbery, or death of production animals [binary indicator, equal to 1 if EH1F=1]
Our primary variable of interest is defined as exposure to at least one shock [binary indicator equal to 1 if any of EH1A-EH1F=1]; however, we will also explore influence of individual shocks. Note that the questionnaire asks about shocks in the last 2 years.

3.4 Group D: heterogeneity of impacts

We hypothesize that the intervention will vary for several groups of beneficiaries, defined by ethnicity, relative poverty status, age, level of education, level of education relative to partner’s education and the baseline level of empowerment. For instance, instrumental violence theory suggests that the baseline level of empowerment is important in determining how improved labor market opportunities will affect the incidence of violence a woman experiences.

We present below the definitions of these groups:

3.4.1 Ethnicity

Laos has a large number of ethnic groups. Some groups have small numbers in our sample. To balance the distinctions between ethnic groups and statistical power consideration, we combine several smaller ethnicities into larger groups:

5. Lao-Tai: Lao, Tai, Leu, Phou thai
6. Mon-Khmer: Khmu, Katang, Makong, Tree, Phong, Bid, Paheko, Than, La med, Xouay
7. Chinese-Tibet: A kha, Phou noy
8. Hmoung-Emien: Hmongverus

As a robustness check we will also create ethnic minority dummy, equal to 1 if individual belongs to groups 3 and 4, and 0 otherwise.

3.4.2 Relative poverty

Although the intervention was targeted at poor households, they may vary in the level of poverty. We will capture this heterogeneity using two indicators, based on:

1. Baseline household income: binary indicator equal to 1 if total household Income over past 12 months is below 50th percentile [HN22]
2. PRF poverty ranking, updated by the village chiefs, at baseline: binary indicator equal to 1 if variable “Q5_2” from “Database Applicant Registration (rev3)” is either 1 or 2.

We treat outcome 1 as primary outcome of interest.

3.4.3 Age

We will analyze heterogeneity of impacts depending on age, by interacting treatment variable with two age variables (in different specifications) defined as:

1. Biological age at the start of the program [HR6]
2. Binary indicators for three groups of women: 15 to 24, 25 to 34, 35 and older.

3.4.4 Education

Categorical variable capturing: (i) no education, (ii) primary school only, (iii) until lower secondary, and (iv) upper secondary and above.
3.4.5 Education relative to partner’s education
Binary indicator equal to 1 if a woman’s level of education (in years\textsuperscript{14}) exceeds the level of education of her partner

3.4.6 Baseline level of empowerment
1. Binary indicator equal to 1 if above the 50\textsuperscript{th} percentile of the z-score index, constructed based on the following measures of internal empowerment at baseline:
   a. Measure of internal locus (i.e. indicating how much control one has over the events in one’s life) [a z-score index of LC7, LC4, LC16, LC20, LC31, LC40, LC47]
   b. Measure of external locus (i.e. indicating how much control external events have over one’s life) [a z-score index of LC10, LC13, LC18, LC23, LC24, LC36, LC39, LC43, LC45 with items reverse coded]
   c. Aggregate locus measure [a z-score of items LC7-LC47, with external locus items reverse coded]
   d. Count of decisions women consider themselves capable of making
2. Binary indicator equal to 1 if above the 50th percentile of the z-score index, constructed based on the following measures a woman’s decision-making in the household at baseline:
   a. Fraction of decisions where woman is among final decision makers [sum of DM1c, DM6c, DM7c or DM8c =1, divided by 4]
   b. Fraction of decisions where the woman is among final decision makers, among decisions she cares about [sum of DM1c, DM6c, DM7c or DM8c =1 when corresponding value question DM1b1, DM6b1, DM7b1 or DM8b1 = 1, divided by count of decisions in DM1b1, DM6b1, DM7b1 or DM8b1 = 1]
   c. Fraction of decisions where woman believes she should be decision maker [sum of DM1e, DM6e, DM7e or DM8e =1, divided by 4]
   d. Count of decisions women care about making [sum of DM1b1=1, DM6b1=1, DM7b1=1 and DM8b1=1]
3. Binary indicator equal to 1 if above the 50th percentile of the z-score index, constructed based on the following measures a women’s participation in community life at baseline:
   a. Number of people who visited in the past two weeks [SN2]
   b. Number of people who visited to talk business in the past two weeks [SN3]
   c. Number of people who visited to talk about personal matter in the past two weeks [SN3]
   d. Social contact index [z-score index composed of SN2, SN3, SN4]
   e. Time spent on community activities [average per day, over the past two days, TU1 code 18]

As a robustness check, we will analyze heterogeneity based on being above/below median of each individual measure.

\textsuperscript{14} To construct years of education, we use this as a guide: https://en.wikipedia.org/wiki/Education_in_Laos_(post-1990). Specifically, the stata code,
\begin{verbatim}
gen edu_yrs=.; replace edu_yrs = 0 if inlist(edu_lev, 97,0,1); replace edu_yrs = edu_grade if edu_lev == 1;
replace edu_yrs = 5 + edu_grade if inlist(edu_lev, 2,3); replace edu_yrs = 9 + edu_grade if edu_lev == 4;
replace edu_yrs = 12 + edu_grade if edu_lev == 5; replace edu_yrs = 18 if edu_lev == 6
\end{verbatim}
4. Methodology

4.1 Randomization

The identification strategy in this impact evaluation relies on exogenous variation generated by the randomization process. We describe the randomization process below:

- From a pool of eligible and interested women in each village, women were selected at random to receive jobs. Women who did not receive a job were placed on a waitlist to replace any RMG dropouts.
- The randomization was conducted via a “lucky draw” carried out by Indochina Research Laos (IRL). In each village, a target size for the waitlist was established beforehand based on the size of the RMG in that village (1.5 times the size of the RMG, rounded up to the nearest whole number). In some cases, multiple villages formed a single RMG. In these cases, we had target numbers of RMG and waitlist members for each village, and IRL conducted a separate lucky draw in each village.
- Information about the RMG jobs was disseminated in each village by PRF local staff and the village chief. Interested women were invited to gather on a pre-assigned day.
- On the day, IRL invited interested women to register by recording their names and ages.
- Poverty-rankings were used to give preference to women from poorer households. IRL had PRF’s household poverty rankings from 2016. These rankings divided households into poorest, poor, middle-income, and better-off. If there were enough interested women from “Poorest” ranked households to fill the village’s RMG and waitlist spots, the lucky draw was restricted to women from these households only. If there were not enough women from “Poorest” households, the lucky draw was opened up to households with the next poverty rank – “Poor” – households, and so on.
- The village heads were present during registration and reviewed and updated the poverty rankings to account for any shocks or changes in the households since 2016.
- Once the women were registered, a lottery was carried out which determined women’s status (RMG or waitlist) and position on the waitlist (see section 2 for details).

4.2 Econometric specification

In a randomized controlled trial, estimating an intent to treat (ITT) effect using the assignment to treatment during the selection process is a straightforward difference between the group means, or

\[ y_i = \alpha + \beta \text{Treat}_i + \epsilon_i \]

\[ y_{it} = \alpha + \beta Treatment_{it} + \epsilon_{it} \] (1)

---

15 For instance, in Village A if there was a 4-member RMG, we assigned a target waitlist size of 6, whereas if in Village B where there were only 2 RMG spots, we assigned a target waitlist size of 3. In some cases, multiple villages formed a single RMG. In these cases, we had target numbers of RMG and waitlist members for each village, and IRL conducted a separate lucky draw in each village.

16 The program administration chose to rely on local knowledge of village chiefs to update poverty ranking, initially carried out through a proxy means combined with community validation. To assess the possibility of capture, we collected data on a random sample of households in each village at baseline. We find that village chiefs selected as eligible households that are significantly worse off than a random sample of households on several indicators. Results available upon request.
where $y_i$ is the outcome of interest and the $Treat_i$ is a dummy that takes the value of one if a woman was treated (i.e. selected for a RMG job) and zero otherwise. In our case, we will have two rounds of data - baseline and one-follow up – and can obtain more precise estimates and improve statistical power by controlling for baseline characteristics and the baseline value of the outcome variable in an ANCOVA specification (McKenzie, 2014).

$$y_{i1} = \alpha + \beta Treat_{i0} + \gamma X_{i0} + \theta y_{i0} + \epsilon_i$$  \hspace{1cm} (2)

where $y_{i1}$ is the value of the outcome of interest in the post-intervention period (i.e. in the endline data), $Treatment_{i0}$ is a dummy that takes the value of one for the treatment group in the pre-intervention period (original assignment), $X_{i0}$ contains a set of baseline covariates (or pre-intervention characteristics) and $y_{i0}$ is the baseline value of the outcome variable $y$.

For outcomes where we don’t have a baseline value, we will run the same regression, excluding the baseline outcome values, where the notation is the same as in equation (2):

$$y_{i1} = \alpha + \beta Treat_{i1} + \gamma X_{i0} + \epsilon_i$$  \hspace{1cm} (3)

In the case of perfect compliance with treatment assignment, we can interpret the ITT as a Treatment-on-Treated (ToT) effect and this would be equal to the ATE. However, in our case, perfect compliance is unlikely. We know from the pilot of the RMG program that women assigned to RMG jobs sometimes drop out for personal reasons and need to be replaced by other women. We have information on the women who drop out, which allows us to track actual treatment. This allows us to instrument for take-up using the random assignment in a 2-stage regression to estimate a ToT. Since this is only relevant for women who take up the intervention, it should be interpreted as a Local Average Treatment Effect (LATE). In the first stage, we estimate the likelihood of taking up the treatment based on (exogenous) assignment to treatment through the randomization process,

$$T_i = \alpha + \pi Z_i + u_i$$  \hspace{1cm} (4)

And in the second stage, we regress the outcome of interest on the predicted take-up $\hat{T}_i$,

$$y_{i1} = \alpha + \beta \hat{T}_{i1} + \gamma X_{i0} + \theta y_{i0} + \epsilon_i$$  \hspace{1cm} (5)
where $Z_i$ is an indicator that takes the value of 1 for women assigned to the RMG during the lucky draw and 0 for women assigned to the waitlist. $T_i$ is equal to 1 if the woman worked in an RMG, and is 0 otherwise\footnote{If a woman dropped out after 9 months, we consider her to be an RMG recipient.}. $T_{i1}$ is the predicted value of $T_i$ from the first stage-regression in (4).

We use robust standard errors. We do not cluster errors since treatment was randomized within village at the individual level.

**Mechanisms of impact**

To test whether the intervention affected household’s ability to cope with shocks, we run the following regression:

$$y_{i1} = \alpha + \beta \text{Treatment}_{i1} + \eta M_i + \lambda M_i T \text{reatment}_{i1} + \gamma X_{i0} + \theta y_{i0} + \epsilon_i$$

(6)

where $M_{i1}$ captures a household’s exposure to shock. The primary coefficients of interest are $\eta$ and $\lambda$. While $\eta$ captures the impact of a shock, $\lambda$ captures the ability of the program to lower the impact of a shock. Note that shocks are captured for the period of 2 years before the endline data collection.

**Heterogeneous effects**

Based on our understanding of the study context and the literature, we do not expect the effects of the program to be uniform on all the beneficiaries.

The approach to exploring heterogeneous impacts, sample size permitting\footnote{Our sample size calculations and sampling strategy took into account the fact that we were planning to estimate heterogeneous impacts on different ethnic groups. We did not stratify by headship or poverty status when selecting baseline data.}, in each case is to estimate a modification of equation 2 where we would include an indicator capturing heterogeneity among beneficiaries, and interaction term between the treatment variable and this indicator.

$$y_{i1} = \alpha + \beta_1 \text{Treatment}_{i0} + \beta_2 H_i \text{Treatment}_{i0} + \kappa H_i + \gamma X_{i0} + \theta y_{i0} + \epsilon_i$$

(7)

where $H_i$ is a continuous or binary variable capturing specific heterogeneity dimension (see Section X for the full list of such variables). The set up is similar to equation (6); however, in interpretation of coefficients we focus on the differences in treatment impacts across different groups.

**Multiple hypothesis testing**

Lastly, given the broad range of outcomes we test and the plans for sub-group specific analysis, we plan to adjust for multiple hypothesis testing in two ways. First, on some measures, where it makes sense to create indices that capture aggregate changes across components of a single outcome, we will follow Kling, Katz and Liebman (2007), and estimate impact on the indices.
4.3 Statistical power as a mediator

We are highly conscious of the statistical power of this study, which affects our ability to detect effects. Therefore, we carried out power calculations at two points. First, before the start of the study, we carried out power calculations using data from the Laos Expenditure and Consumption Survey, collected in 2008-2009 (LECS). This were the most recent publicly available nationally representative household level data in Laos. We found that we were adequately powered (brief description below). Then we carried out power calculations a second time after we finished baseline data collection, and again observed that we were adequately powered. However, power is of important enough concern that we plan to carry out power calculations a third time, after endline data collection\(^{19}\) and discuss power alongside any actual reported effects.

Description of initial power calculations using LECS

For our initial power calculations, we assumed that the experimental sample was fixed and estimated whether the minimum detectable effects (MDEs) were within the range of effects implied by the existing literature. Using data from the LECS to parameterize sample means and standard deviations, we found that our minimum detectable effects sizes were well within the range estimated from the literature for two outcomes – expenditure on food consumption and on total consumption. However, our MDEs were larger than the effects in the literature for other outcomes - agricultural income, use of agricultural inputs and entrepreneurship.

Revised power calculations using baseline data

Since those initial calculations, we carried out the randomization to select the treatment and control groups and collected baseline data from these groups. Both these changes had implications for power. First, the size of our control group is now substantially larger: while in the original power calculations we assumed a control group as large as our treatment group, in practice, we recruited a larger number of women into the control group during randomization, leaving us with a control group that is over two times the size of our treatment group. Second, we have data from our study sample on the key outcomes of interest to us in this study. To reflect these changes, we decided to revisit our power calculations and update them using the newly available data.

For these power calculations, we make the following assumptions:

- The power level is set at 80 percent and we show MDEs for two alpha levels: 5% and 10%.
- Since randomization was carried out at individual level, we do not cluster.
- The treatment group has 339 women and the control group has 843 women.
- 10% of selected candidates (treatment) may not accept job offers or quit after they start and 10% of the waitlist (control) will be used to fill their vacancies\(^{20}\).
- We have two rounds of data collection and assume a high correlation between rounds of 0.8 for women’s labor force participation and time use, where we don’t expect much change in the

\(^{19}\) We will do power calculations prior to carrying out any of the other analysis.

\(^{20}\) After 8 months of program implementation, we see a figure of closer to 6%. We use 10% as a conservative estimate to account for additional non-compliance in the final months of the program.
In the absence of the intervention. In the case of household income, other studies indicate noise, and so we expect correlation between rounds to be lower, at 0.5.

- We calculate power using Stata’s sampsi command for an ANCOVA (our preferred) estimation.

The power calculations focus on the key outcomes in our study. These are,

1. Household food consumption
2. Household income
3. Women’s labor force participation
4. Women’s time-use

Table 1 shows the minimum detectable effect (MDE) sizes for our study using baseline data. The effect sizes on food consumption hover around 10%-13% for most categories of food consumption except for the consumption of ordinary rice and fruit where the effects sizes are close to 30% and 15% respectively. The 10-13% range is similar to the 7-11% range of effects detected in the literature (Deininger and Liu, 2013; Ravi and Engler, 2015), though on the upper end of that range.

The size of the MDE for total income in our study is between 22 and 25% and the MDE for effects on income from employment are approximately double these figures. The effects sizes appear reasonable because they are much smaller than the size of the direct wage transfer resulting from working as a member of the RMG. The implied increase is only about 1 million Kip in the case of earnings from wage employment (or 1.6 million kip for total income), which is only a third (half) of the approximate 3 million kip each RMG member will earn in a year—this is within the range of effects in the literature (24% in Chacaltana, 2003, 33% in Ravallion and Galasso 2004, 39% in Departamento Nacional de Planeación, 2004, and 93% in Backiny-Yetna, Wodon, and Zampaglione, 2011).

For time-use, only some women do any paid work at baseline, so there is a lot of variance in this measure. The implied MDE is an increase of 32-35% in the number of hours women spend on paid work. In absolute terms however, this corresponds to only an increase of about 24 minutes of work over two days, or 12 minutes per day. The RMG is expected to engage women in work for a few hours of work each week, so this is less than the amount of time we expect RMG women to be spending on carrying out RMG maintenance work. We expect the increase in time spent on RMG jobs to eat into the time women have available for unpaid work and household non-economic work, though the decrease may not be one-to-one because women can substitute time from other activities too. However, the MDEs for these outcomes correspond to decreases of 36-42 minutes over two days which again, appear reasonable given the amount of work we expect these women to be doing on the RMGs.

Table 1: MDEs for different outcomes of interest

<table>
<thead>
<tr>
<th>OUTCOME</th>
<th>BL SAMPLE</th>
<th>correlation b/w rounds</th>
<th>MDE in absolute terms</th>
<th>MDE in % terms</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
</tbody>
</table>

21 Though actual wage payments vary, we show in Chapter 2 that average wage payments to RMG members is about 4.5 million kip over 18 months, which translates to 3 million kip in 12 months.
<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>SD</th>
<th>N</th>
<th>$\alpha = 0.05$</th>
<th>$\alpha = 0.1$</th>
<th>$\alpha = 0.05$</th>
<th>$\alpha = 0.1$</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Consumption</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No. balls of glutinous</td>
<td>2.97</td>
<td>2.89</td>
<td>1146</td>
<td>0.8</td>
<td>0.33</td>
<td>0.29</td>
<td>11%</td>
</tr>
<tr>
<td>rice</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>10%</td>
</tr>
<tr>
<td>No. balls of ordinary</td>
<td>0.68</td>
<td>1.98</td>
<td>1146</td>
<td>0.8</td>
<td>0.22</td>
<td>0.20</td>
<td>32%</td>
</tr>
<tr>
<td>rice</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>29%</td>
</tr>
<tr>
<td>No. kilos meat in last</td>
<td>1.22</td>
<td>1.25</td>
<td>1146</td>
<td>0.8</td>
<td>0.14</td>
<td>0.12</td>
<td>11%</td>
</tr>
<tr>
<td>week (HH)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>10%</td>
</tr>
<tr>
<td>No. kilos fish in last</td>
<td>1.05</td>
<td>1.26</td>
<td>1146</td>
<td>0.8</td>
<td>0.14</td>
<td>0.12</td>
<td>13%</td>
</tr>
<tr>
<td>week (HH)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>11%</td>
</tr>
<tr>
<td>No. kilos vegetables</td>
<td>4.65</td>
<td>4.50</td>
<td>1146</td>
<td>0.8</td>
<td>0.51</td>
<td>0.45</td>
<td>11%</td>
</tr>
<tr>
<td>last week (HH)</td>
<td></td>
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<td></td>
<td></td>
<td></td>
<td>10%</td>
</tr>
<tr>
<td>No. kilos fruit in last</td>
<td>2.04</td>
<td>3.00</td>
<td>1146</td>
<td>0.8</td>
<td>0.34</td>
<td>0.30</td>
<td>17%</td>
</tr>
<tr>
<td>week (HH)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>15%</td>
</tr>
<tr>
<td><strong>Income</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Farm income</td>
<td>3,114,203</td>
<td>6,119,246</td>
<td>1146</td>
<td>0.5</td>
<td>995,797</td>
<td>885,797</td>
<td>32%</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>28%</td>
</tr>
<tr>
<td>Employment income</td>
<td>1,966,497</td>
<td>6,288,970</td>
<td>1146</td>
<td>0.5</td>
<td>1,013,503</td>
<td>903,503</td>
<td>52%</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>46%</td>
</tr>
<tr>
<td>Total income</td>
<td>6,642,101</td>
<td>10,123,037</td>
<td>1146</td>
<td>0.5</td>
<td>1,627,899</td>
<td>1,447,899</td>
<td>25%</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>22%</td>
</tr>
<tr>
<td><strong>Time use</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Paid work</td>
<td>1.20</td>
<td>3.80</td>
<td>1146</td>
<td>0.8</td>
<td>0.42</td>
<td>0.38</td>
<td>35%</td>
</tr>
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<td>32%</td>
</tr>
<tr>
<td>Unpaid work (decrease)</td>
<td>9.50</td>
<td>6.59</td>
<td>1146</td>
<td>0.8</td>
<td>0.74</td>
<td>0.65</td>
<td>8%</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>7%</td>
</tr>
<tr>
<td>HH non-economic work</td>
<td>9.05</td>
<td>5.69</td>
<td>1146</td>
<td>0.8</td>
<td>0.64</td>
<td>0.53</td>
<td>7%</td>
</tr>
<tr>
<td>(decrease)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td>6%</td>
</tr>
<tr>
<td><strong>Labor force participation</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Work on household</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>farm/enterprise, or</td>
<td>0.79</td>
<td>0.41</td>
<td>1146</td>
<td>0.8</td>
<td>0.046</td>
<td>0.041</td>
<td>6%</td>
</tr>
<tr>
<td>outside household</td>
<td></td>
<td></td>
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<td>5%</td>
</tr>
<tr>
<td>Wage work outside</td>
<td>0.29</td>
<td>0.46</td>
<td>1146</td>
<td>0.8</td>
<td>0.051</td>
<td>0.046</td>
<td>17%</td>
</tr>
<tr>
<td>household</td>
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<td></td>
<td></td>
<td></td>
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<td>16%</td>
</tr>
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5. References


Beegle, K., E. Galasso and J. Goldberg. 2017. Direct and indirect effects of Malawi’s public works program on food security. JDE, 128.


