# THE LONG-TERM IMPACT OF UNCONDITIONAL CASH TRANSFERS: EXPERIMENTAL EVIDENCE FROM KENYA*

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**Abstract**

This paper describes the impacts of unconditional cash transfers distributed on economic and psychological outcomes three years after the beginning of the program. Using a randomized controlled trial, we find that transfer recipients have higher levels of asset holdings, consumption, food security and psychological well-being relative to non-recipients in the same village. The effects are similar in magnitude to those observed in a previous study nine months after the beginning of the program. Comparing recipient households to non-recipients in distant villages, we find that transfer recipients have 40% more assets (USD 422 PPP) than control households three years after the transfer, equivalent to 60% of the initial transfer (USD 709 PPP). In contrast, other outcomes do not show significant treatment effects in the across-village analysis, possibly owing to lower power and within-village spillovers. We do find some spillover effects. Households impacted by spillovers have lower consumption and food security than pure control households, perhaps due to the sale of productive assets. Estimates of spillover effects on other outcomes are inconclusive due to differential attrition between spillover and pure control households. We also find little evidence of differential treatment effects depending on the transfer design (whether transfers are made men or women, in monthly payments or a single lump-sum, or a large or small transfer). Thus, cash transfers result in sustained increases in assets. Long-term impacts on other dimensions, and potential spillover effects, remain to be substantiated by future work.

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# 1 Introduction

A substantial body of research documents positive impacts of unconditional cash transfers (UCTs) to low-income households on economic outcomes (Arnold, Greenslade, and Conway 2011; Baird, De Hoop, and Özler 2013; Blattman, Fiala, and Martinez 2014). Our prior work (Haushofer and Shapiro 2016) demonstrated that unconditional cash transfers have large effects in the short run. In particular, we studied the economic and psychological impacts of cash transfers provided by the NGO *GiveDirectly* (GD) in Kenya. Between 2011 and 2013, *GD* sent unconditional cash transfers averaging USD 709 PPP, which corresponds to almost two years of per-capita expenditure, to randomly chosen households in western Kenya using *M-Pesa*, a cell-phone–based mobile money service.[1]

The present study analyzes additional data collected from the sample of our prior study, using the same randomized controlled trial (RCT) design. We carried out a two-stage randomization, one at the village level, resulting in treatment and control villages, and another at the household level, resulting in "treatment" and "spillover" households in treatment villages, and "pure control" households in control villages. Within the treatment group, we randomized the transfer recipient within the household (wife vs. husband), the transfer timing (monthly installments over nine months vs. one-time lump-sum transfer), and transfer magnitude (USD 404 PPP vs. USD 1,525 PPP). This setup allows us to assess the impact of unconditional cash transfers, and address a number of additional questions related to transfer design.

In our earlier study, when comparing cash transfer recipients to non-recipients in the same village nine months after the start of the program, we observed an increase in monthly non-durable expenditure of USD 36 PPP relative to a control group mean of USD 157 PPP. We found a significant increase of USD 302 PPP in asset holdings, relative to a control group mean of USD 495 PPP. We also observed an increase in monthly revenue from agriculture, animal husbandry, and enterprises of USD 16 PPP relative to a control group mean of USD 49 PPP. However, this revenue increase was largely offset by an increase in flow expenses (USD 13 PPP relative to a control group mean of USD 24 PPP). We found no effects on health and educational outcomes. Transfers led to a 0.18 SD increase in happiness, a 0.13 SD increase in life satisfaction, a 0.23 SD reduction in stress, and a significant reduction in depression (all measured by psychological questionnaires). We also found large reductions in intimate partner violence (IPV) for treatment and spillover households, but no other spillover effects. These findings on the short-term impacts of unconditional cash transfers

[1] All USD values are calculated at purchasing power parity, using the 2012 World Bank PPP estimate for private consumption in Kenya: 0.016 USD/KES.

compliment other work in this area, summarized in Haushofer and Shapiro (2016).

To study the long-term impacts of these transfers, we conducted a long-term follow-up survey among the sample in our initial study. The results reported here capture the impacts of unconditional cash transfers approximately 3 years after the transfers were sent. When comparing recipients of any cash transfer (all cash treatment arms combined) to non-recipients in the same village, we find large and sustained positive impacts of cash transfers. Specifically, we observe a 40 percent increase in asset holdings (USD 416 PPP) among recipient households, a 25 percent increase in consumption (USD 47 PPP), and a concomitant reduction in hunger. We also observe increases in education expenditure and psychological well-being. When comparing the short and long-run impacts, we find the impacts do not significantly decrease over time, suggesting that cash transfers may have sustained effects that persist for at least three years. Comparing recipients households to non-recipients in distant villages, we find that recipients of cash transfers have 40% more assets than control households three years post transfer. This amount (USD 422 PPP) is equivalent to 60% of the initial transfer (USD 709 PPP). However, we do not find statistically significant across-village treatment effects on other outcomes. This difference could stem from lower power in the across-village analysis due to the absence of village-level fixed effects, lack of baseline data, and village level clustering; and from potential spillover effects at the village level. Indeed, non-recipient households in treatment villages show differences to pure control households on several dimensions. The point estimates suggest spillover households spend USD 30 PPP less than pure control households, or about 16% based on a pure control mean of USD 188 PPP, and score ˜0.25 SD less on an index of food security than pure control households. Spillover households also score ˜0.18 SD less on an index of psychological well-being than pure control households. On the positive side, they score 0.16 SD higher on an index of female empowerment. When applying Lee Bounds to assess whether attrition may drive these spillover results, the consumption and food security spillovers are robust, but we do not see conclusive evidence of spillovers on other dimensions. We do not have conclusive evidence of the mechanism behind spillovers, but speculate it could be due to the sale of productive assets by spillover households to treatment households, which in turn reduces consumption among the spillover group. Though not always statistically different from zero, we do see suggestive evidence of negative spillover effects on the value of productive assets such as livestock, bicycles, motorbikes and appliances.

Thus, we can confidently conclude that cash transfers result in sustained increases in assets, while differences on other dimensions are not statistically different when comparing transfer recipients to non-recipients in distant villages. To study across-village treatment effects with greater power, we anticipate combining this data with data collected in an ongoing

study of the GiveDirectly program in 654 villages, in collaboration with Miguel, Niehaus, and Walker. In addition, to further explore spillover effects, the authors are presently replicating this study.

# 2 Intervention

The main treatment was the provision of cash transfers, to 50% of the sample. In addition to measuring the impacts of these broadly-targeted unconditional cash transfers, a goal of this study is to assess the relative impacts of three design features of unconditional cash transfers on economic and other outcomes: the gender of the transfer recipient, the temporal structure of the transfers (monthly vs. lump-sum transfers), and the magnitude of the transfer. Within the group receiving transfers, the treatment arms were structured as follows:

1. **Transfers to the woman vs. the man in the household.** Among households with both a primary female and a primary male member, we stratified on recipient gender and randomly assigned the woman or the man to be the transfer recipient with equal probability. 110 households had a single household head and were not considered in the randomization of recipient gender.

2. **Lump-sum transfers vs. monthly installments.** Across all treatment households, we randomly assigned the transfer to be delivered either as a lump-sum amount or as a series of nine monthly installments. Specifically, 258 of the 503 treatment households were assigned to the monthly condition, and 245 to the lump-sum condition. In the analysis we only consider the 173 monthly recipient and 193 lump-sum recipient households that did not receive large transfers, because large transfers were not unambiguously monthly or lump-sum (see below). The total amount of each type of transfer was KES 25,200 (USD 404 PPP). In the lump-sum condition, this amount includes an initial transfer of KES 1,200 (USD 19 PPP) to incentivize *M-Pesa* registration, followed by a lump-sum payment of KES 24,000 (USD 384 PPP). In the monthly condition, the total amount consists of a sequence of nine monthly transfers of KES 2,800 (USD 45 PPP) each. The timing of transfers was structured as follows: In the monthly condition, recipients received the first transfer of KES 2,800 on the first of the month following *M-Pesa* registration, and the remaining eight transfers of KES 2,800 on the first of the eight following months. In the lump-sum condition, recipients received the initial transfer of KES 1,200 on the first of the month following *M-Pesa* registration, and the lump-sum transfer of KES 24,000 on the first of a month that was chosen randomly among the nine months following the time at which they were

enrolled in the *GD* program.

3. **Large vs. small transfers.** Finally, a third pair of treatment arms was created to study the relative impact of large compared to small transfers. To this end, 137 households in the treatment group were randomly chosen and informed in January 2012 that they would receive an additional transfer of KES 70,000 (USD 1,121 PPP), paid in seven monthly installments of KES 10,000 (USD 160 PPP) each, beginning in February 2012. Thus, the transfers previously assigned to these households, whether monthly or lump-sum, were augmented by KES 10,000 from February 2012 to August 2012, and therefore the total transfer amount received by these households was KES 95,200 (USD 1,525 PPP, USD 1,000 nominal).[2] The remaining 366 treatment households constitute the "small" transfer group, and received transfers totaling KES 25,200 (USD 404 PPP, USD 300 nominal) per household.

These three treatment arms were fully cross-randomized, except that, as noted above, the "large" transfers were made to existing recipients of KES 25,200 transfers in the form of a KES 70,000 top-up that was delivered as a stream of payments after respondents had already been told that they would receive KES 25,200 transfers.

# 3 Evaluation design, attrition, and baseline balance

## 3.1 Sampling and identification strategy

This study is a two-level cluster-randomized controlled trial. The selection and surveying of recipient households proceeded as follows:

1. *GD* first identified Rarieda, Kenya, as a study district, based on data from the national census. The research team then identified the 120 villages with the highest proportion of thatched roofs within Rarieda. Sixty villages were randomly chosen to be treatment villages (first stage of randomization). Villages had an average of 100 households. An average of 19 percent of households per village were surveyed, and an average of 9 percent received transfers. The transfers sent to villages amounted to an average of 10 percent of aggregate baseline village wealth.

[2]Note that for the households originally assigned to the "lump-sum" condition, this new transfer schedule implied that these households could no longer be unambiguously considered to be lump-sum households; we therefore restrict the comparison of lump-sum to monthly households to those households which received small transfers, as described above.

2. The research team then identified all eligible households within treatment villages through a census administered with the assistance of the village elder. Census exercises were conducted before the baseline survey (March–November 2011) in treatment villages, and before the endline survey (April–June 2012) in control villages. The census was conducted in the same fashion in treatment and control villages. A household was considered eligible if it had a thatched roof. The purpose of the census and baseline was described to village elders and respondents as providing information to researchers about living conditions in the area; no mention was made of *GD* or transfers.

3. Following the census, all eligible households completed the baseline survey. Baseline was not conducted for control villages and thus baseline surveys were administered between April and November 2011. The order of census and surveys was randomized at the village level (after the first four villages, which were chosen for proximity to the field office). No transfers or transfer announcements were made before or during census or baseline in each village. The surveys were described to respondents in the same fashion as the census, that is, without reference to *GD* or transfers.

4. *GD* then repeated the census to confirm that all households deemed eligible by the research team were in fact eligible. The final eligible sample was the overlap between the households that completed baseline and *GD's* census exercise. We excluded 89 households who completed baseline but were not identified as eligible in the *GD* census. After baseline, the research team randomly chose half of the eligible households to be transfer recipients (second stage of randomization). This process resulted in 503 treatment households and 505 control households in treatment villages at baseline. We refer to the control households in treatment villages as "spillover" households.

5. Within a few weeks after all households in a village had completed baseline and the *GD* census, recipient households were visited by a representative of *GD*, who announced the transfer, including the amount and timing (although large transfers were announced later as a top-up to existing small transfers). We have no data on how transfers were perceived by the households; anecdotally, because *GD* worked with village elders, had objectively verifiable targeting criteria, and was otherwise highly transparent, we have reason to believe that recipients had accurate beliefs about the nature of the transfers as fully unconditional and one-time. Control households were not visited, but those who asked were told that they had not won the lottery for transfers. The control group did not receive SIM cards and were not asked to register for *M-Pesa*; thus, our treatment effects reflect the joint impact of cash transfers and incentives to register for *M-Pesa* (Jack and Suri 2014).

6. The transfer schedule commenced on the first day of the month following the initial visit. For monthly transfers, the first installment was transferred on that day, and continued for eight months thereafter; for lump-sum transfers, a month was randomly chosen among the nine months following the date of the initial visit. Each transfer was announced with a text message; recipients who did not own cell phones could rely on the transfer schedule given to them by *GD* to know when they would receive transfers, or insert the SIM card into any mobile handset periodically to check for incoming transfers. To facilitate transfer delivery, *GD* offered to sell cell phones to recipient households which did not own one (by reducing the future transfer by the cost of the phone).

7. The first endline survey was administered by the research team between August and December 2012. The order in which villages were surveyed followed the same order as the baseline. In a small number of households, the endline survey was administered before the final transfer was received. These households are nevertheless included in the analysis – i.e., we report intent-to-treat analysis. Control villages were surveyed only at endline; in these villages, we sampled 432 households from among eligible households. We refer to these households as "pure control" households. The census exercise to select these households was identical to that in treatment villages, except that no *GD* census was administered. Because these pure control households were selected into the sample just before the endline, the thatched-roof criterion was applied to them about one year later than to households in treatment villages. This fact potentially introduces bias into the comparison of households in treatment and control villages; we describe below how it was dealt with in the second endline. Results from the first endline survey are reported in in Haushofer and Shapiro (2016)

8. We administered a second endline between February and September 2015. In this survey we made two changes to the sampling design. First, we surveyed an additional 349 households in pure control villages who had never been surveyed, these households were drawn from names in a village census collected at baseline. The comparison of these households to previously surveyed households in pure control villages identifies survey effects (i.e., the possibility that having being surveyed previously affects responses in a later survey). The analysis to identify these demand effects is covered in a separate pre-analysis plan. Second, we surveyed 71 additional households in the pure control villages to correct for the fact that the eligibility criterion (living in a thatched roof house) was applied one year later in pure control villages than in treatment villages. The identification of these households is discussed below and in the analysis of the first

endline. The present paper will primarily focus on the results of this second endline.

## 3.2 Risk and treatment of attrition

As detailed below, our basic analytical approach to capture the impact of cash transfers is to compare recipient households to non-recipient households, either within villages (comparing cash transfer recipients to non-recipients in the same village), or across villages (comparing cash transfer recipients to non-recipients in villages where no one received a transfer). To obtain unbiased estimates of the impact of cash transfers, attrition across survey rounds should not be correlated with treatment assignment. To test for this, we firstly compare attrition rates across treatment, spillover and pure control households. Secondly, we compare baseline characteristics for treatment and spillover households in the estimation (endline 2) sample to assess whether attrition has introduced differences in baseline characteristics across groups. Note that this analysis is only feasible for treatment and spillover households as baseline data was not collected for pure control households.

As additional robustness checks (included in the Online Appendix), we compare the characteristics of treatment and control households in the attrition group, and of attrition and non-attrition households. We also replicate the results from endline 1 data using only those households that appear in the endline 2 sample. Assuming endline 1 results are valid, consistency of endline 1 results using only those household appearing at endline 2 with the full sample endline 1 results indicates that the subset of the full sample found at endline 2 is generally representative of the entire sample: the results are generally in accordance.

### 3.2.1 Attrition rate comparison

In treatment villages, attrition between baseline and endline 1 was 6.4 percent among treatment households and 7.1 percent among spillover households. Between the baseline and endline 2, attrition was 9.7 percent for treatment households and 10.1 percent for spillover households. In control villages, attrition between the first and second endline was 14 percent. We only report attrition between the first endline and follow-up, as there was no baseline survey.

We next assess the severity of attrition using three approaches. The following equations estimate whether the magnitude of attrition is different for treatment, spillover, and pure control households across the relevant survey rounds:

$$attrit_{vhBE_1} = \alpha_v + \beta_0 + \beta_1 T_{vh} + \varepsilon_{vhE_1} \quad (1)$$

$$attrit_{vhE_1E_2} = \alpha_v + \beta_0 + \beta_1 T_{vh} + \beta_2 S_{vh} + \varepsilon_{vhE_2} \tag{2}$$

$$attrit_{vhBE_2} = \alpha_v + \beta_0 + \beta_1 T_{vh} + \varepsilon_{vhE2} \tag{3}$$

$\alpha_v$ captures village-level fixed effects. $T_{vh}$ is an indicator for households which received a cash transfer. $S_{vh}$ is an indicator for households in treatment villages which did not receive a cash transfer. $\varepsilon_{vh}$ is the idiosyncratic error term. Where the outcomes are at the individual level, standard errors are clustered at the household level. The results, shown in Table 1, do not show differential attrition between baseline and either endline for the treatment group relative to the spillover group. However, there is a statistically significant difference in attrition levels for households in control villages relative to households in treatment villages from endline 1 to endline 2: 6 percentage points more pure control households were not found at endline 2 relative to either group of households in treatment villages. In the analysis of across-village treatment effects and spillover effects we use Lee bounds to deal with this differential attrition; details are given below.

### 3.2.2 Comparison of baseline characteristics in the estimation sample

In Table 2 we assess whether the baseline characteristics of treatment and spillover households who appear at endline 2, and are therefore included in the analysis that follows, are different. We find no significant differences between the baseline characteristics of those treatment and spillover group households which are included in the endline 2 estimation sample. This result suggests that attrition did not change the composition of households in the treatment and spillover groups. Note, however, that we cannot conduct this analysis for pure control households because of the lack of baseline data for these households.

Table 1: Attrition – Differential attrition across treatment groups

| | Baseline to endline 1 (treatment and spillover only) | | Baseline to endline 2 (treatment and spillover only) | | Endline 1 to endline 2 (full sample) | | | |
|---|---|---|---|---|---|---|---|---|
| | Spillover mean (SD) | Treatment | Spillover mean (SD) | Treatment | Pure Control mean (SD) | Treatment within village | Treament across village | Spillover |
| Attrition | 0.07 | −0.01 | 0.10 | −0.00 | 0.14 | 0.01 | −0.06** | −0.06*** |
| | (0.26) | (0.02) | (0.30) | (0.02) | (0.35) | (0.02) | (0.02) | (0.02) |

*Notes:* Difference in attrition probability in treatment vs. control groups, estimated with an OLS regression of the attrition dummy on the treatment dummy. We report the coefficient on the treatment dummy and its standard error in parentheses, clustered at the household level for within village comparisons and at the village level for across village comparisons. The latter regression includes 121 village level clusters. Columns (1) and (2) report attrition between baseline and endline 1, taking spillover households as the control. Columns (3) and (4) report attrition between baseline and endline 2, taking spillover households as the control. Columns (5) through (7) report attrition between endline 1 and endline 2, taking pure control households as the control. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table 2: Attrition – Baseline difference in index variables between treated and non-treated in estimation sample

| | Baseline to Endline 1 | | | Baseline to Endline 2 | | |
|---|---|---|---|---|---|---|
| | Control mean (SD) | Treatment | N | Control mean (SD) | Treatment | N |
| Value of non-land assets (USD) | 389.37 | −6.98 | 940 | 389.12 | 5.24 | 908 |
| | (377.70) | (25.52) | | (376.17) | (26.09) | |
| Non-durable expenditure (USD) | 183.58 | −9.23 | 940 | 179.63 | 1.03 | 908 |
| | (122.86) | (8.11) | | (120.74) | (8.63) | |
| Total revenue, monthly (USD) | 88.88 | −35.95* | 940 | 87.73 | −32.92 | 908 |
| | (417.47) | (20.12) | | (423.61) | (20.76) | |
| Food security index | −0.00 | 0.00 | 940 | −0.01 | 0.04 | 908 |
| | (1.01) | (0.06) | | (1.00) | (0.06) | |
| Health index | −0.00 | 0.06 | 940 | 0.02 | 0.04 | 908 |
| | (1.00) | (0.07) | | (1.03) | (0.07) | |
| Education index | −0.00 | −0.06 | 802 | 0.02 | −0.06 | 783 |
| | (1.00) | (0.06) | | (1.02) | (0.07) | |
| Psychological well-being index | −0.01 | 0.03 | 1482 | −0.01 | 0.01 | 1427 |
| | (1.01) | (0.05) | | (1.00) | (0.06) | |
| Female empowerment index | −0.00 | −0.06 | 709 | 0.01 | −0.09 | 685 |
| | (1.00) | (0.08) | | (1.02) | (0.08) | |
| Joint test ($p$-value) | | 0.57 | | | 0.67 | |

*Notes:* This table reports differences in baseline index variables between treated and non-treated, estimated with an OLS regression of baseline index variables on the treatment dummy for those in the estimation sample only. All regressions are restricted to households in treatment villages. Outcome variables are listed on the left. Assets, consumption, revenue are all Winsorized at the 99th percentile. Columns (1) through (3) analyze attrition between baseline and endline 1. Columns (4) through (6) analyze attrition between baseline and endline 2. Columns (1) and (4) report the mean of the spillover group for a given outcome variable at baseline. Columns (2) and (5) reports the baseline difference between treatment and spillover groups within villages. Columns (3) and (6) report the number of observations in each regression sample. The unit of observation is the household for all outcome variables, except the psychological variables index, where it is the individual. Standard errors are reported in parentheses and clustered at the household level. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

### 3.3 Baseline balance

Baseline balance was assess in our previous analysis, and originally published in Haushofer and Shapiro (2016). The analytic approach and results are reproduced in the Online Appendix. Briefly, we find that baseline characteristics for the treatment group as a whole do not appear different from that of other groups. When considering the smaller treatment arm samples, however, we are able to reject the hypothesis that the baseline characteristics of the female recipient group and the monthly transfer group are the same as the male recipient group and lump-sum recipient group, respectively. In the analysis below, we control for the baseline outcome wherever possible in order to mitigate any bias created by baseline differences.

## 4 Results

### 4.1 Reduced form specifications

Our basic treatment effects specification to capture the impact of cash transfers is

$$y_{vhiE} = \alpha_v + \beta_0 + \beta_1 T_{vh} + \delta_1 y_{vhiB} + \delta_2 M_{vhiB} + \varepsilon_{vhiE} \tag{4}$$

Here, $y_{vhiE}$ is the outcome of interest for household $h$ in village $v$, measured at endline, of individual $i$ (subscript $i$ is included for outcomes measured at the level of the individual respondent, and omitted for outcomes measured at the household level). Village-level fixed effects are captured by $\alpha_v$. $T_{vh}$ is a treatment indicator that takes value 1 for treatment households, and 0 otherwise. $\varepsilon_{vhiE}$ is an idiosyncratic error term. We restrict the sample to treatment and control households in treatment villages; we discuss the spillover effect in Section 4.4. Following McKenzie (2012), we condition on the baseline level of the outcome variable when available, $y_{vhiB}$, to improve statistical power. To include observations where the baseline outcome is missing, we code missing values as 0 and include a dummy indicator that the variable is missing ($M_{vhiB}$).

To distinguish between the effects of different treatment arms, we use the following specifications. First, to calculate differences between treatment households in which transfers were made to the female vs. the male in the household, we estimate:

$$y_{vhiB} = \alpha_v + \beta_0 + \beta_1 T^{\mathrm{F}}_{vh} + \beta_2 T^{\mathrm{W}}_{vh} + \beta_3 S_{vh} + \varepsilon_{vhiB} \tag{5}$$

Here, the variables $T^{\mathrm{x}}_{vh}$ are indicator functions that specify whether the transfer recipient is female ($T^{\mathrm{F}}_{vh}$) or that the gender of the recipient could not be randomized because the

household had only one head (most commonly in the case of widows/widowers) ($T_{vh}^{\mathrm{W}}$). $S_{vh}$ is an indicator variable for the spillover group. The omitted category is two-headed households in which the primary male received a transfer. $\beta_1$ is the difference in baseline outcomes between female and male recipient households.

To assess baseline differences between monthly vs. lump-sum transfers, we analyzed the following:

$$y_{vhiB} = \alpha_v + \beta_0 + \beta_1 T_{vh}^{\mathrm{MTH}} \times T_{vh}^{\mathrm{S}} + \beta_2 T_{vh}^{\mathrm{L}} + \beta_3 S_{vh} + \varepsilon_{vhiB} \tag{6}$$

Here, $T_{vh}^{\mathrm{MTH}}$ is an indicator variable for having been assigned to monthly transfers, and $T_{vh}^{\mathrm{S}}$ and $T_{vh}^{\mathrm{L}}$ for being assigned to the small and large transfer conditions, respectively. Note that households assigned to the large transfer condition cannot unambiguously be considered monthly or lump-sum, and therefore this regression compares households which did not receive large transfers. The omitted category is thus households that received a (small) lump-sum transfer. $\beta_1$ is the difference in baseline outcomes between monthly and lump-sum recipient households.

Finally, to assess baseline differences between households receiving large compared to small transfers, we used the following specification:

$$y_{vhiB} = \alpha_v + \beta_0 + \beta_1 T_{vh}^{\mathrm{L}} + \beta_2 S_{vh} + \varepsilon_{vhiB} \tag{7}$$

Here, $T_{vh}^{\mathrm{L}}$ is an indicator variable for having been assigned to receiving large transfers. Thus, $\beta_1$ is the difference in baseline outcome measures between households receiving large transfers and households receiving small transfers.

As cash transfers are likely to impact a large number of economic behaviors and dimensions of welfare, and given that our survey instrument often included several questions related to a single behavior or dimension, we account for multiple hypotheses by using outcome variable indices and family-wise $p$-value adjustment. We pre-specified eight outcome groups, each summarized by an index, that comprise our primary outcomes of interest. For each of outcome group, we construct either an index variable following the procedure proposed by Anderson (2008), or choose a focal variable. For these indices and focal variables, we report both unadjusted $p$-values, and $p$-values adjusted for multiple comparisons using Anderson's (2008) variant of Efron & Tibshirani's (1993) non-parametric permutation test.

## 4.2 Within-village treatment effects

The within-village treatment effects estimated by the equations above are shown in column 1 of Table 3. The estimated coefficients, indicating the mean difference in each outcome

between the cash transfer and spillover groups, suggest that cash transfers have sustained benefits for recipients. Recipients have US PPP 416 (SE 43.21) more in assets than the spillover group, spend US PPP 47 (SE 9.78) more per month on non-durable consumption, and report greater food security (0.20 SD, SE 0.06), better educational outcomes for their children (0.15 SD, SE 0.07), and elevated psychological well-being (0.16 SD, SE 0.05). All of these differences are statistically different from zero using both conventional and FWER adjusted $p$-values.

We do not observe significantly different impacts based on treatment arms. The naïve $p$-values suggest that transferring to women as opposed to men leads to better health, greater female empowerment, and lower increases in business revenues but these differences are not statistically different from zero when adjusting for multiple hypothesis testing. We do not observe significant differences between households receiving monthly vs. lump-sum transfers or those receiving small vs. large transfers, although we can not rule out meaningful differences due to limited power. Detailed estimates by treatment arm are available in the online appendix.

In the Appendix, we decompose the indices into components, allowing us to assess the main drivers of the results. Similar to our earlier analysis, we find that the increases in total assets are driven by large increases in iron roofs and livestock holdings by cash transfer recipients. However, asset values increase for nearly all categories, including durable goods other than metal roofs and financial savings. Similarly, non-durable expenditure increases for nearly all categories, except alcohol, tobacco, and health. Notably, we observe a US PPP 32 increase in food consumption, which represents a 25 percent increase relative to the mean in the spillover group. This increase in food consumption is also reflected in increased food security, including (among other indicators detailed in the Appendix) fewer meals skipped by adults and children and higher consumption of protein. We observe increases in total revenue from farming, animal husbandry and non-agricultural business activities, but these are offset by increased expenditures relating to these activities, resulting in an insignificant increase in measured profits. The observed increase in our education index appears entirely driven by increased spending on school fees, uniforms, books and supplies per child (although note that this increase in not reflected in the educational spending measured in the consumption table which is measured at the aggregate household level). Finally, the change in psychological well-being is driven by decreases in stress and depression and increases in self-reported life satisfaction and happiness.

To further explore the drivers of the overall results, we next considered heterogeneous impacts by age, assets, consumption, food security, land holdings, and psychological well-being, but did not find strong evidence of heterogeneous treatment effects along these dimensions

(see pre-analysis plan and Online Appendix).

Finally, to test whether attrition might impact these results, show Lee bounds in 4. With the exception of education spending, the within-village treatment effects are positive and significant even for the lower bound. In addition, to assess robustness, we report the minimum detectable effect size we can measure in the Online Appendix.

One important caveat to the interpretation of the within-village treatment effects is that they may be biased by within-village spillover effects. This concern is less salient if the across-village treatment effects are of similar magnitude. We therefore turn next to the across-village treatment effects to ask if they show similar results. Note, however, that the across-village analysis has lower power due to the omission of village-level fixed effects, baseline outcomes, and clustering standard errors at the village level.

Table 3: Within-village treatment effects

| | (1) Control mean (SD) | (2) Treatment effect | (3) Female recipient | (4) Monthly transfer | (5) Large transfer | (6) N |
|---|---|---|---|---|---|---|
| Value of non-land assets (USD) | 992.46 | 416.27*** | −105.96 | 18.87 | 23.83 | 912 |
| | (682.39) | (43.21) | (72.10) | (67.87) | (78.58) | |
| | | [0.00]*** | [0.60] | [1.00] | [1.00] | |
| Non-durable expenditure (USD) | 187.55 | 47.04*** | −2.08 | 17.35 | −5.37 | 912 |
| | (134.79) | (9.78) | (16.34) | (16.54) | (17.09) | |
| | | [0.00]*** | [0.90] | [0.60] | [1.00] | |
| Total revenue, monthly (USD) | 73.01 | 20.70* | −33.40* | −2.54 | 14.63 | 912 |
| | (158.53) | (10.60) | (19.23) | (16.22) | (22.76) | |
| | | [0.10] | [0.40] | [1.00] | [1.00] | |
| Food security index | 0.00 | 0.20*** | 0.04 | −0.01 | 0.01 | 912 |
| | (1.00) | (0.06) | (0.10) | (0.10) | (0.11) | |
| | | [0.00]*** | [0.90] | [1.00] | [1.00] | |
| Health index | −0.00 | −0.07 | 0.22** | 0.11 | −0.10 | 912 |
| | (1.00) | (0.06) | (0.10) | (0.10) | (0.10) | |
| | | [0.40] | [0.40] | [0.60] | [1.00] | |
| Education index | 0.00 | 0.15** | 0.07 | 0.19 | 0.17 | 817 |
| | (1.00) | (0.07) | (0.12) | (0.13) | (0.12) | |
| | | [0.00]*** | [0.90] | [0.60] | [0.80] | |
| Psychological well-being index | 0.00 | 0.16*** | 0.08 | −0.03 | 0.12 | 1491 |
| | (1.00) | (0.05) | (0.07) | (0.08) | (0.08) | |
| | | [0.00]*** | [0.70] | [1.00] | [0.70] | |
| Female empowerment index | −0.00 | 0.07 | 0.10 | 0.10 | 0.03 | 1256 |
| | (1.00) | (0.05) | (0.07) | (0.09) | (0.07) | |
| | | [0.40] | [0.70] | [0.60] | [1.00] | |
| Joint test ($p$-value) | | 0.00*** | 0.09* | 0.61 | 0.60 | |

*Notes:* This table summarizes OLS estimates of treatment effects. Outcome variables are listed on the left. Higher values correspond to "positive" outcomes. Outcome variables are listed on the left. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. FWER-corrected $p$-values are shown in brackets. Column (1) reports the mean and standard deviation of the spillover group and Column (2) reports the basic treatment effect, i.e. comparing treatment households to control households within villages. Column (3) reports the relative treatment effect of transferring to the female compared to the male; Column (4) reports the relative effect of monthly compared to lump-sum transfers; and Column (5) reports the relative effect of large compared to small transfers. Assets, consumption, revenue are all Winsorized at the 99th percentile. The unit of observation is the household for all outcome variables except for the psychological variables index, where it is the individual. The sample is restricted to co-habitating couples for the female empowerment index, and households with school-age children for the education index. The comparison of monthly to lump-sum transfers excludes large transfer recipient households, and that for male vs. female recipients excludes single-headed households. All columns include village-level fixed effects, control for baseline outcomes, and cluster standard errors at the household level. The former regression includes 121 village level clusters. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table 4: Lee bounds on within-village treatment effects

| | Treatment effect | | Female recipient | | Monthly transfer | | Large transfer | |
|---|---|---|---|---|---|---|---|---|
| | (1) Lower bound | (2) Upper bound | (3) Lower bound | (4) Upper bound | (5) Lower bound | (6) Upper bound | (7) Lower bound | (8) Upper bound |
| Value of non-land assets (USD) | 416.63*** | 429.70*** | −112.01* | −60.22 | 19.60 | 52.05 | −4.48 | 93.88 |
| | (33.84) | (39.95) | (64.27) | (59.20) | (57.22) | (59.17) | (59.66) | (63.34) |
| | [354.98] | [502.49] | [−221.65] | [40.76] | [−80.93] | [155.99] | [−103.03] | [198.52] |
| Non-durable expenditure (USD) | 47.91*** | 51.73*** | −17.27 | −5.56 | 7.81 | 16.94 | −13.62 | 8.13 |
| | (6.95) | (8.86) | (13.97) | (12.01) | (12.30) | (13.74) | (12.19) | (13.44) |
| | [35.47] | [67.58] | [−41.03] | [14.86] | [−13.49] | [40.72] | [−33.74] | [30.30] |
| Total revenue, monthly (USD) | 20.36*** | 25.24* | −44.21*** | −34.28*** | −3.67 | 9.11 | 10.87 | 31.70* |
| | (7.73) | (13.20) | (14.29) | (12.75) | (11.16) | (16.41) | (15.79) | (16.51) |
| | [6.38] | [49.09] | [−68.87] | [−12.29] | [−22.76] | [37.18] | [−15.37] | [59.14] |
| Food security index | 0.20*** | 0.22*** | 0.03 | 0.10 | 0.01 | 0.04 | −0.08 | 0.06 |
| | (0.06) | (0.05) | (0.08) | (0.09) | (0.09) | (0.08) | (0.08) | (0.08) |
| | [0.10] | [0.31] | [−0.11] | [0.25] | [−0.15] | [0.18] | [−0.22] | [0.20] |
| Health index | −0.12** | −0.07 | 0.21*** | 0.27*** | 0.08 | 0.14* | −0.17** | −0.03 |
| | (0.05) | (0.05) | (0.07) | (0.08) | (0.08) | (0.08) | (0.08) | (0.08) |
| | [−0.20] | [0.01] | [0.08] | [0.40] | [−0.06] | [0.27] | [−0.31] | [0.10] |
| Education index | 0.08 | 0.16** | −0.07 | 0.10 | 0.17 | 0.28*** | 0.11 | 0.20* |
| | (0.07) | (0.07) | (0.09) | (0.11) | (0.11) | (0.11) | (0.10) | (0.10) |
| | [−0.04] | [0.27] | [−0.22] | [0.28] | [−0.02] | [0.46] | [−0.07] | [0.38] |
| Psychological well-being index | 0.16* | 0.19** | 0.03 | 0.16* | 0.01 | 0.03 | 0.04 | 0.21** |
| | (0.09) | (0.08) | (0.09) | (0.09) | (0.13) | (0.12) | (0.10) | (0.10) |
| | [0.00] | [0.34] | [−0.11] | [0.31] | [−0.24] | [0.26] | [−0.12] | [0.37] |
| Female empowerment index | −0.02 | 0.00 | 0.14 | 0.16 | 0.09 | 0.13 | −0.03 | 0.29** |
| | (0.10) | (0.18) | (0.24) | (0.13) | (0.17) | (0.33) | (0.12) | (0.13) |
| | [−0.20] | [0.35] | [−0.32] | [0.41] | [−0.23] | [0.76] | [−0.23] | [0.50] |

*Notes:* This table reports upper and lower within-village treatment effect bounds using Lee (2009) treatment effect bounds. Outcome variables are listed on the left. Higher values correspond to "positive" outcomes. For each outcome variable, we report both the lower and upper bound for the spillover effct, bootstrapped standard errors in parenthesis, and Imbens-Manski CIs in brackets. Columns (1), (3), (5), and (7) report the lower bound and its standard error for each treatment arm. Columns (2), (4), (6), and (8) report the upper bounds and standard error. Assets, consumption, revenue are all Winsorized at the 99th percentile. The unit of observation is the household for all outcome variables except for the psychological variables index, where it is the individual. The sample is restricted to co-habitating couples for the female empowerment index, and households with school-age children for the education index. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

## 4.3 Across-village treatment effects

In this section, we estimate the treatment effect of cash transfers by comparing treatment households to pure control households. We omit spillover households. The specification is

$$y_{vhiE} = \beta_0 + \beta_1 T_{vh} + \varepsilon_{vhiE} \tag{8}$$

where variables are defined as above. We omit village-level fixed effects, as they would be collinear with treatment status. Additionally, since pure control households were not surveyed at baseline, we do not include baseline values of outcome variables on the right-hand side. Since this analysis leverages village-level randomization, we cluster standard errors at the village level. Thus $\beta_1$ identifies the treatment effect relative to control households in control villages. Any difference between the treatment effect measured in this specification and the within-village specification will be due to spillover effects.

We also analyze the various treatment arms using across-village comparisons. For the across-village treatment effect for households in which the primary male vs. the primary female received the transfer, we now include pure control households in the analysis and estimate:

$$y_{vhiE} = \beta_0 + \beta_1 T_{vh}^{\mathrm{F}} + \beta_2 T_{vh}^{\mathrm{M}} + \beta_3 T_{vh}^{\mathrm{W}} + \beta_4 S_{vh} + \beta_5 PC_{vh}^{\mathrm{SINGLE}} + \varepsilon_{vhiE} \tag{9}$$

Here, $PC_{vh}^{\mathrm{SINGLE}}$ is an indicator for pure control households with a single head. Thus, the omitted category is cohabiting pure control households. $\beta_1$ identifies the treatment effect when the primary female in the household receives the transfer. $\beta_2$ identifies the treatment effect when the primary male in the household receives the transfer. Again we omit baseline outcomes, as they were not measured for households in the pure control group, and cluster standard errors at the village level.

For across-village treatment effect for monthly and lump-sum transfers, we now include pure control households and estimate:

$$y_{vhiE} = \beta_0 + \beta_1 T_{vh}^{\mathrm{MTH}} \times T_{vh}^{\mathrm{S}} + \beta_2 T_{vh}^{\mathrm{LS}} \times T_{vh}^{\mathrm{S}} + \beta_3 T_{vh}^{\mathrm{L}} + \beta_4 S_{vh} + \varepsilon_{vhiE} \tag{10}$$

Thus, the omitted category is pure control households. $\beta_1$ identifies the effect of a monthly transfer using an across-village comparison. $\beta_2$ identifies the effect of a lump-sum transfer using an across-village comparison. Again we omit baseline outcomes, as they were not measured for households in the pure control group, and cluster standard errors at the village level. As above, note that we focus on "small" transfer recipients because large transfers were not unambiguously monthly or lump-sum.

For across-village treatment effect for large and small transfers, we include pure control households and estimate:

$$y_{vhiE} = \beta_0 + \beta_1 T_{vh}^{\mathrm{L}} + \beta_2 T_{vh}^{\mathrm{S}} + \beta_3 S_v h + \varepsilon_{vhiE} \tag{11}$$

Thus, the omitted category is pure control households. $\beta_1$ identifies the effect of a large transfer using an across-village comparison. $\beta_2$ identifies the effect of a small transfer using an across-village comparison. Again we omit baseline outcomes and cluster standard errors at the village level.

A potential weakness in this analysis is that the thatched-roof selection criterion for participation in the study was applied to households in control villages one year after it was applied to households in treatment villages. As a result, there is endogenous selection into the pure control condition, as some proportion of households in pure control villages are likely to have upgraded to a metal roof over this time period. These households are excluded from endline in the pure control villages, potentially introducing bias into the across-village analysis.

To deal with this potential bias, as part of the follow-up survey, we visited households in pure control villages that purchased metal roofs between the dates of the baseline and first endline surveys. Since these are the households that were excluded due to the late application of the thatched-roof selection criterion, including them allows us to calculate unbiased across-village treatment effect estimates as of the follow-up. Our approach was as follows:

1. We first assessed the reliability with which individuals could recall the date when they upgraded their roof. To do this, we asked households who upgraded in treatment villages when they did so. Since we know that this upgrade must fall between baseline and endline 1, we can assess the proportion who accurately place the date in that period. We surveyed 108 households we know upgraded to a metal roof between baseline and endline surveys (from our objective data). Of these, 78 respondents (72.2 percent) reported upgrading within the baseline and endline 1 window, 17 respondents (15.7 percent) reported upgrading outside the baseline and endline 1 window, 13 respondent (12.0 percent) could not recall at all.

2. Having established reasonable reliability of the date of recall, we returned to all households with iron roofs in pure control villages to inquire when they upgraded their roof. If they informed us that they upgraded at a date between baseline and endline 1, we classify them as eligible to be surveyed as part of the pure control group (though they were excluded in endline 1). We refer to these households as "new criterion pure con-

trol" (NCPC), in contrast to original criterion pure control households (OCPC). We determined there were 170 NCPC households in this exercise.

3. We then used the same algorithm originally used to select pure control households to calculate the probability that each of these households would have been included in the study had they been identified as eligible at the time. The original sampling method required us to select 8 households from the pool of eligible households in each village (those with thatched roofs). When there were 8 or fewer eligible households in a given village, we selected all households. When there more than 8 eligible households, we randomly selected 8, with equal probability for each. We were thus able to calculate the exact probability that a given household would be selected in each village. In villages with 8 or fewer eligible households, the probability of selection was 1. In villages with more than 8 eligible households, the probability was 8 divided by the total number of eligible households.

4. Based on these probabilities, we then selected a subsample of 71 NCPC households to survey at the follow-up that we include in this analysis. We will include these additional 71 households in all analyses involving pure control households.

We estimate the across-village equations described above for three samples: including OCPC and NCPC pure control households and all treatment households; including only pure control households surveyed at endline 1 and all treatment households; and including only pure control households surveyed at endline 1 and treatment households that did not upgrade their roof from baseline to endline 1.

Results are reported in Table 5. Aside from an economically and statistically significant increase in assets, we are unable to reject the null hypothesis that the difference between households receiving cash transfers and comparable households in control villages is zero on other outcome variables. We also present Lee bounds of these estimates in Table 6, which show a similar pattern of results: the Lee bounds confidence intervals for asset holdings exclude zero for all sample definitions, while those for other outcome variables include zero for all sample definitions (even though some of the individual upper and lower bounds are statistically different from zero).

Thus, the across-village treatment effects are smaller and less robust than the within-village treatment effects. Several possible reasons suggest themselves for this difference. First, the across-village analysis has lower power because it does not use village-level fixed effects. Second, in the across-village analysis standard errors are clustered at the village level, which in the presence of positive within-cluster correlation increases them. Third, we cannot make our treatment effect estimates more precise by including control variables

because pure control households were not surveyed at baseline. Finally, it may be that the within-village treatment effect estimates are biased upward by within-village spillovers. To test for this latter possibility, we next explore the presence of within-village spillover effects.

Table 5: Across-village treatment effects

| | (1) Control mean (SD) | (2) Treatment (within villages) | (3) Treatment (across villages) | (4) Female recipient | (5) Male recipient | (6) Monthly transfer | (7) Lump sum transfer | (8) Large transfer | (9) Small transfer | (10) N |
|---|---|---|---|---|---|---|---|---|---|---|
| Value of non-land assets (USD) | 992.46 | 416.27*** | 421.91*** | 395.37*** | 471.85*** | 430.55*** | 406.11*** | 447.11*** | 417.39*** | 1286 |
| | (682.39) | (43.21) | (57.12) | (69.82) | (80.09) | (74.61) | (64.68) | (72.39) | (58.70) | |
| | | [0.00]*** | [0.00]*** | [0.00]*** | [0.00]*** | [0.00]*** | [0.00]*** | [0.00]*** | [0.00]*** | |
| Non-durable expenditure (USD) | 187.55 | 47.04*** | 17.41 | 15.26 | 24.36 | 26.79* | 16.01 | 13.67 | 20.98 | 1286 |
| | (134.79) | (9.78) | (12.09) | (14.20) | (15.89) | (16.10) | (14.46) | (15.52) | (13.01) | |
| | | [0.00]*** | [0.60] | [0.90] | [0.50] | [0.50] | [0.70] | [0.90] | [0.20] | |
| Total revenue, monthly (USD) | 73.01 | 20.70* | 2.67 | −18.50 | 16.51 | −2.40 | 0.65 | 12.41 | −0.76 | 1286 |
| | (158.53) | (10.60) | (12.30) | (15.39) | (19.18) | (14.47) | (14.69) | (21.29) | (12.16) | |
| | | [0.30] | [1.00] | [0.90] | [0.80] | [1.00] | [0.90] | [1.00] | [1.00] | |
| Food security index | 0.00 | 0.20*** | −0.05 | −0.04 | −0.11 | −0.03 | −0.06 | −0.04 | −0.05 | 1286 |
| | (1.00) | (0.06) | (0.10) | (0.11) | (0.12) | (0.10) | (0.12) | (0.14) | (0.09) | |
| | | [0.00]*** | [1.00] | [1.00] | [0.80] | [1.00] | [0.90] | [1.00] | [1.00] | |
| Health index | −0.00 | −0.07 | −0.06 | 0.03 | −0.21** | 0.02 | −0.09 | −0.14 | −0.04 | 1286 |
| | (1.00) | (0.06) | (0.06) | (0.08) | (0.09) | (0.09) | (0.08) | (0.10) | (0.07) | |
| | | [0.50] | [0.70] | [1.00] | [0.30] | [1.00] | [0.50] | [0.40] | [0.90] | |
| Education index | 0.00 | 0.15** | 0.09 | 0.13 | 0.06 | 0.17 | −0.05 | 0.18 | 0.05 | 1129 |
| | (1.00) | (0.07) | (0.09) | (0.13) | (0.10) | (0.15) | (0.09) | (0.12) | (0.10) | |
| | | [0.10] | [0.80] | [1.00] | [0.80] | [0.90] | [0.90] | [0.40] | [0.90] | |
| Psychological well-being index | 0.00 | 0.16*** | −0.02 | 0.03 | −0.07 | −0.04 | −0.06 | 0.06 | −0.05 | 2097 |
| | (1.00) | (0.05) | (0.06) | (0.07) | (0.07) | (0.08) | (0.07) | (0.07) | (0.07) | |
| | | [0.00]*** | [1.00] | [1.00] | [0.70] | [1.00] | [0.80] | [0.90] | [0.90] | |
| Female empowerment index | 0.00 | 0.01 | 0.15* | 0.24** | 0.08 | 0.17 | 0.08 | 0.23** | 0.12 | 943 |
| | (1.00) | (0.07) | (0.08) | (0.09) | (0.10) | (0.12) | (0.10) | (0.10) | (0.09) | |
| | | [1.00] | [0.40] | [0.00]*** | [0.80] | [0.80] | [0.90] | [0.10] | [0.50] | |
| Joint test ($p$-value) | | 0.00*** | 0.00*** | 0.00*** | 0.00*** | 0.00*** | 0.00*** | 0.00*** | 0.00*** | |

*Notes:* This table summarizes OLS estimates of within and across village treatment effects. Outcome variables are listed on the left. The unit of observation is the household for all variables expect psychological well-being, where it is the individual. The sample includes all households and individuals, except for the intrahousehold index, where it is restricted to co-habitating couples, and for the education index, where it is restricted to households with school-age children. Assets, consumption, revenue are all Winsorized at the 99th percentile. Column (1) reports the mean of a given outcome variable among control households in treatment villages. Column (2) reports the treatment effect within villages, i.e. comparing treatment households to spillover households. Column (3) reports the treatment effect across villages, i.e. comparing treatment households to pure control households. Column (4) reports the effect of transfers to the primary female in the household compared to pure control; Column (5) reports the effect of transfers to the primary male in the household compared to pure control; Column (6) reports the effect of monthly transfers to pure control; Column (7) reports the effect of lump sum transfers to pure control; Column (8) reports the effect of large transfers to pure control; Column (9) reports the effect of small transfers to pure control. For each outcome variable, we report the coefficient of interest and its standard error in parentheses, and FWER-corrected p-value in brackets. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. Standard errors are clustered at the village level in Columns (3) through (9), and at the household level in column (2). This regression includes 121 village level clusters. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table 6: Lee bounds on across-village treatment effects

| | Includes Recall HHs | | No Metal Roofs | | Original Sample | |
|---|---|---|---|---|---|---|
| | (1) Lower bound | (2) Upper bound | (3) Lower bound | (4) Upper bound | (5) Lower bound | (6) Upper bound |
| Value of non-land assets (USD) | 200.73*** | 328.38*** | 278.59*** | 387.24*** | 324.40*** | 481.51*** |
| | (62.89) | (58.60) | (77.23) | (67.83) | (61.59) | (56.84) |
| | [97.21] | [424.83] | [150.71] | [499.56] | [223.10] | [575.00] |
| Non-durable expenditure (USD) | −10.93 | 16.64 | 3.85 | 26.17** | −6.64 | 26.45** |
| | (12.28) | (11.20) | (16.39) | (13.18) | (12.83) | (11.59) |
| | [−31.13] | [35.07] | [−23.31] | [48.00] | [−27.74] | [45.51] |
| Total revenue, monthly (USD) | −32.39** | −2.46 | −17.29 | 11.88 | −27.11** | 7.28 |
| | (14.04) | (13.33) | (15.92) | (15.56) | (13.44) | (13.25) |
| | [−55.49] | [19.48] | [−43.51] | [37.50] | [−49.23] | [29.07] |
| Food security index | −0.21** | −0.01 | −0.11 | 0.09 | −0.16* | 0.08 |
| | (0.08) | (0.08) | (0.10) | (0.10) | (0.09) | (0.09) |
| | [−0.34] | [0.13] | [−0.27] | [0.25] | [−0.31] | [0.22] |
| Health index | −0.19*** | 0.03 | −0.11 | 0.09 | −0.16** | 0.10 |
| | (0.07) | (0.07) | (0.08) | (0.08) | (0.08) | (0.07) |
| | [−0.31] | [0.15] | [−0.25] | [0.23] | [−0.28] | [0.21] |
| Education index | −0.11 | 0.13 | −0.06 | 0.16 | −0.09 | 0.18** |
| | (0.08) | (0.09) | (0.10) | (0.11) | (0.08) | (0.09) |
| | [−0.24] | [0.28] | [−0.22] | [0.35] | [−0.21] | [0.33] |
| Psychological well-being index | −0.17*** | 0.13* | −0.13* | 0.02 | −0.11 | 0.11 |
| | (0.07) | (0.07) | (0.08) | (0.08) | (0.07) | (0.07) |
| | [−0.28] | [0.23] | [−0.26] | [0.15] | [−0.22] | [0.22] |
| Female empowerment index | −0.05 | 0.41*** | 0.05 | 0.26** | 0.01 | 0.34*** |
| | (0.10) | (0.10) | (0.12) | (0.13) | (0.11) | (0.11) |
| | [−0.22] | [0.57] | [−0.15] | [0.48] | [−0.16] | [0.51] |

*Notes:* This table repports upper and lower across-village treatment effect bounds using Lee (2009) treatment effect bounds. OLS estimates of treatment effects across time. Outcome variables are listed on the left. Higher values correspond to "positive" outcomes. Assets, consumption, revenue are all Winsorized at the 99th percentile. The unit of observation is the household for all outcome variables except for the psychological variables index, where it is the individual. The sample is restricted to co-habitating couples for the female empowerment index, and households with school-age children for the education index. Columns (1) and (2) include all treated and pure control households from the original study with the addition of a random sample of 71 households in pure control villages who purchased metal roofs between baseline and endline 1. Columns (3) and (4) exclude households that purchased a metal roof between baseline and endline 1, whether they were part of the old sample or the new. Columns (5) and (6) includes only treated and pure control households from the original work. Within each group, we report both the lower and upper bound for the spillover effect, bootstrapped standard errors in parenthesis, and Imbens-Manski CIs in brackets. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

## 4.4 Spillover effects

For the within-village specifications to provide an unbiased estimate of the treatment effect, within-village spillovers of treatment on non-recipient households must be small. This includes both spillover effects that operate through economic channels, and those that have psychological roots, such as John Henry effects. To address this question, we estimate the magnitude of within-village spillovers by comparing spillover to pure control households:

$$y_{vhiE} = \beta_0 + \beta_1 S_{vh} + \varepsilon_{vhiE} \tag{12}$$

Here, the sample includes only non-treatment households (in treatment and control villages). Thus, $\beta_1$ identifies within-village spillover effects by comparing control households in treatment villages to control households in pure control villages. The error term is clustered at the village level, reflecting the dual-level randomization at the village and within-village (household) levels (Cameron, Gelbach, and Miller 2011; Pepper 2002). Note that the inclusion of baseline covariates is not feasible here because no baseline data exist for the pure control group. Similarly, village-level fixed effects are not feasible because they would be collinear with $S_{vh}$.

As for the across-village analysis, we estimate equation 12 for three samples: including OCPC and NCPC pure control households and all spillover households; including only pure control households surveyed at endline 1 and all spillover households; and including only pure control households surveyed at endline 1 and spillover households that did not upgrade their roof from baseline to endline 1. The results are shown in Table 7.

When including OCPC and NCPC households, the results suggest large and significant spillover effects. In particular, spillovers appear negative for economic variables (assets, expenditure and food security) and for psychological well-being, and positive for female empowerment. The estimates for assets are sensitive to the particular sample choice, while the spillover effects for consumption, food security, psychological well-being, and female empowerment are significant across samples.

These results appear to differ from those found in the initial endline, where we found positive spillover effects on female empowerment, but no spillover effects on other dimensions. However, the present estimates are potentially affected by differential attrition from endline 1 to endline 2: as described above, the pure control group showed significantly greater attrition than both treatment and spillover households between these endlines. To assess the potential impact of attrition, we bound the spillover effects using Lee bounds (Table 8). This analysis suggests that differential attrition may account for several of these spillover effects. Specifically, for health, education, psychological well-being, and female empowerment, the

Lee bounds confidence intervals include zero for all sample definitions. For asset holdings, revenue, and food security, they include zero in two of the three sample definitions. Only for expenditure do the Lee bounds confidence intervals exclude zero across all sample definitions. Thus, we find some evidence for spillover effects when using Lee bounds, although most of them are not significantly different from zero after bounding for differential attrition across treatment groups.

We do not have conclusive evidence of what might cause observed spillovers, but speculate it could be due to the sale of productive assets by spillover households to treatment households, which in turn reduces consumption among the spillover group. Though not always statistically different from zero, we do see suggestive evidence of negative spillover effects on the value of productive assets such as livestock, bicycles, motorbikes and appliances. These results are shown in Table B.9 in the appendix.

Table 7: Spillover effects

| | Includes Recall HHs | | No Metal Roofs | | Original Sample | |
|---|---|---|---|---|---|---|
| | (1) Spillover effect | (2) N | (3) Spillover effect | (4) N | (5) Spillover effect | (6) N |
| Value of non-land assets (USD) | −139.60*** | 901 | −69.78 | 754 | 1.34 | 830 |
| | (51.11) | | (51.69) | | (50.13) | |
| | [0.04]** | | [0.53] | | [1.00] | |
| Non-durable expenditure (USD) | −37.66*** | 901 | −30.00*** | 754 | −29.73*** | 830 |
| | (9.90) | | (11.30) | | (10.98) | |
| | [0.01]*** | | [0.07]* | | [0.06]* | |
| Total revenue, monthly (USD) | −26.84** | 901 | −19.20 | 754 | −17.92 | 830 |
| | (13.02) | | (13.51) | | (13.05) | |
| | [0.18] | | [0.53] | | [0.60] | |
| Food security index | −0.32*** | 901 | −0.25*** | 754 | −0.26*** | 830 |
| | (0.08) | | (0.09) | | (0.09) | |
| | [0.00]*** | | [0.06]* | | [0.02]** | |
| Health index | −0.04 | 901 | 0.01 | 754 | 0.01 | 830 |
| | (0.07) | | (0.07) | | (0.07) | |
| | [0.55] | | [0.98] | | [1.00] | |
| Education index | −0.09 | 775 | −0.08 | 644 | −0.05 | 712 |
| | (0.08) | | (0.09) | | (0.08) | |
| | [0.45] | | [0.73] | | [0.95] | |
| Psychological well-being index | −0.22*** | 1456 | −0.18*** | 1227 | −0.20*** | 1352 |
| | (0.06) | | (0.07) | | (0.06) | |
| | [0.01]*** | | [0.07]* | | [0.01]*** | |
| Female empowerment index | 0.17* | 647 | 0.16* | 546 | 0.17* | 604 |
| | (0.09) | | (0.09) | | (0.09) | |
| | [0.18] | | [0.33] | | [0.30] | |
| Joint test ($p$-value) | 0.00*** | | 0.01** | | 0.00*** | |

*Notes:* This table report OLS estimates of spillover effects. Outcome variables are listed on the left. Higher values correspond to "positive" outcomes. Assets, consumption, revenue are all Winsorized at the 99th percentile. The unit of observation is the household for all outcome variables except for the psychological variables index, where it is the individual. The sample is restricted to co-habitating couples for the female empowerment index, and households with school-age children for the education index. For each outcome variable, we report the coefficients of interest and their bootstrapped standard errors in parentheses. Columns (1) and (2) include all spillover and pure control households from the original study with the addition of a random sample of 71 households in pure control villages who purchased metal roofs between baseline and endline 1. Columns (3) and (4) exclude households that purchased a metal roof between baseline and endline 1, whether they were part of the old sample or the new. Columns (5) and (6) includes only spillover and pure control households from the original work. Within each of these groupings, the first column reports the OLS estimates of spillover effect from a comparison of spillover households to pure control households at endline 2. The second column reports the number of observations included in the comparison. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table 8: Lee bounds on spillover effects

| | Includes Recall HHs | | No Metal Roofs | | Original Sample | |
|---|---|---|---|---|---|---|
| | (1) Lower bound | (2) Upper bound | (3) Lower bound | (4) Upper bound | (5) Lower bound | (6) Upper bound |
| Value of non-land assets (USD) | −212.79*** | −105.94* | −134.91** | −46.47 | −89.18 | 45.51 |
| | (63.87) | (55.84) | (67.15) | (55.13) | (59.37) | (54.13) |
| | [−318.10] | [−13.88] | [−246.30] | [44.98] | [−186.83] | [134.55] |
| Non-durable expenditure (USD) | −54.14*** | −32.41*** | −44.50*** | −25.20** | −49.05*** | −22.97** |
| | (10.83) | (10.35) | (12.78) | (11.19) | (11.04) | (10.74) |
| | [−71.97] | [−15.37] | [−65.63] | [−6.71] | [−67.21] | [−5.30] |
| Total revenue, monthly (USD) | −51.35*** | −24.20* | −41.17*** | −16.51 | −45.59*** | −14.60 |
| | (13.32) | (12.93) | (14.97) | (13.19) | (12.56) | (12.83) |
| | [−73.26] | [−2.92] | [−65.86] | [5.25] | [−66.25] | [6.51] |
| Food security index | −0.42*** | −0.19** | −0.35*** | −0.13 | −0.38*** | −0.12 |
| | (0.08) | (0.08) | (0.09) | (0.09) | (0.09) | (0.09) |
| | [−0.56] | [−0.06] | [−0.50] | [0.02] | [−0.53] | [0.03] |
| Health index | −0.19** | 0.08 | −0.14* | 0.12 | −0.16** | 0.15* |
| | (0.08) | (0.07) | (0.08) | (0.08) | (0.08) | (0.08) |
| | [−0.32] | [0.20] | [−0.28] | [0.25] | [−0.29] | [0.27] |
| Education index | −0.25*** | −0.00 | −0.24*** | −0.04 | −0.22*** | 0.04 |
| | (0.08) | (0.08) | (0.08) | (0.08) | (0.08) | (0.08) |
| | [−0.38] | [0.13] | [−0.38] | [0.10] | [−0.35] | [0.18] |
| Psychological well-being index | −0.37*** | −0.06 | −0.27*** | −0.06 | −0.31*** | −0.07 |
| | (0.07) | (0.07) | (0.07) | (0.08) | (0.07) | (0.07) |
| | [−0.48] | [0.06] | [−0.39] | [0.06] | [−0.42] | [0.05] |
| Female empowerment index | −0.00 | 0.52*** | 0.02 | 0.43*** | 0.04 | 0.45*** |
| | (0.11) | (0.10) | (0.12) | (0.12) | (0.11) | (0.11) |
| | [−0.18] | [0.68] | [−0.17] | [0.62] | [−0.14] | [0.63] |

*Notes:* This table reports upper and lower spillover effect bounds using Lee (2009) treatment effect bounds. Outcome variables are listed on the left. Higher values correspond to "positive" outcomes. Assets, consumption, revenue are all Winsorized at the 99th percentile. The unit of observation is the household for all outcome variables except for the psychological variables index, where it is the individual. The sample is restricted to co-habitating couples for the female empowerment index, and households with school-age children for the education index. Columns (1) and (2) include all spillover and pure control households from the original study with the addition of a random sample of 71 households in pure control villages who purchased metal roofs between baseline and endline 1. Columns (3) and (4) exclude households that purchased a metal roof between baseline and endline 1, whether they were part of the old sample or the new. Columns (5) and (6) includes only spillover and pure control households from the original work. Within each group, we report both the lower and upper bound for the spillover effct, bootstrapped standard errors in parenthesis, and Imbens-Manski CIs in brackets. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

## 4.5 Comparison of short and long-run impacts

Since we have outcome data measured in the short run (˜9 months after the beginning of the transfers) and in the long-run (˜3 years after the beginning of transfers), we test equality between short and long-run effects. Specifically, we combine endline one and endline two data in long form, and estimate:

$$y_{vhiE} = \alpha_v + \beta_0 + \beta_1 T_{vh} + \beta_2 T_{vh} \times E_2 + \beta_3 E_2 + \delta_1 y_{vhiB} + \delta_2 M_{vhiB} + \varepsilon_{vhiE} \qquad (13)$$

Here, $y_{vhiE}$ is the outcome of interest for household $h$ in village $v$, measured at endline one or two, of individual $i$ (subscript $i$ is included for outcomes measured at the level of the individual respondent, and omitted for outcomes measured at the household level). The sample is restricted to treatment villages. $T_{vh}$ is a treatment indicator that takes value 1 for households which received a cash transfer ("treatment households") and 0 otherwise. $E_2$ takes value one if the observation is from endline 2. $\beta_2$ identifies the change in treatment effect over time. Analogues of this equation are run for the across-village estimates, treatment arms and spillover effects, interacting the relevant treatment variables with $E_2$.

Results are reported in Table 9. Focusing on the within-village treatment effects, we find no evidence for differential effects at endline 2 compared to endline 1, with the exception of assets, which show a significantly larger treatment effect at endline 2 than endline 1. However, this effect is largely driven by spillovers; for across-village treatment effects, we cannot reject equality of the endline 1 and endline 2 outcomes. This is true for all variables in the across-village treatment effects except for food security and psychological well-being, which show a smaller treatment effect at endline 2 compared to endline 1. Thus, we find some evidence for decreasing treatment effects over time, but for most outcome variables, the endline 1 and 2 outcomes are similar.

Table 9: Comparing short- and long-run effects

| | (1) Treatment (within village) | (2) Spillover | (3) Treatment (across village) |
|---|---|---|---|
| Value of non-land assets (USD) | 139.27*** | −110.30** | 28.97 |
| | (43.50) | (44.35) | (48.45) |
| Non-durable expenditure (USD) | 13.75 | −21.90** | −8.14 |
| | (10.00) | (11.05) | (11.29) |
| Total revenue, monthly (USD) | 8.54 | −17.01 | −8.47 |
| | (12.70) | (12.58) | (12.21) |
| Food security index | −0.08 | −0.29** | −0.37*** |
| | (0.09) | (0.12) | (0.12) |
| Health index | −0.06 | 0.06 | −0.00 |
| | (0.09) | (0.11) | (0.10) |
| Education index | 0.12 | −0.06 | 0.10 |
| | (0.09) | (0.11) | (0.10) |
| Psychological well-being index | −0.07 | −0.24*** | −0.31*** |
| | (0.07) | (0.09) | (0.10) |
| Female empowerment index | 0.03 | −0.05 | −0.02 |
| | (0.07) | (0.08) | (0.08) |
| Joint test ($p$-value) | 0.03** | 0.02** | 0.02** |

*Notes:* This table reports OLS estimates of treatment effects across time. Outcome variables are listed on the left. Higher values correspond to "positive" outcomes. Assets, consumption, revenue are all Winsorized at the 99th percentile. The unit of observation is the household for all outcome variables except for the psychological variables index, where it is the individual. The sample is restricted to co-habitating couples for the female empowerment index, and households with school-age children for the education index. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. Column (1) shows treatment effects across time calculated within village, Column (2) shows spillover effects, and Column (3) shows treatment effects across time calculated across villages. These show the difference in the treatment (spillover) effect between endline 2 and endline 1, and thus illustrates whether effects are decreasing, increasing or constant across time. The unit of observation is the household for all outcome variables except for the psychological variables index, where it is the individual. The sample is restricted to co-habitating couples for the female empowerment index, and households with school-age children for the education index. Column (1) includes village-level fixed effects, control for baseline outcomes, and cluster standard errors at the household level. Column (1) does not include village-level fixed effects or baseline outcomes and cluster standard errors at the village level. This regression includes 121 village level clusters. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

# 5 Conclusion

In our previous work, we found large and significant effects of cash transfers on asset holdings, consumption, food security, agricultural and business revenue, and psychological well-being nine months after the beginning of transfers. In the present study, comparing transfer recipients to non-recipients in the same village, we find that most of these effects persist until at least three years after transfers, suggesting that cash transfers have sustained benefits and households do not simply "consume away" the transfers. One possible mechanism behind these findings is investment in productive assets: in our first endline, we found that households invested in livestock (especially cows) and metal roofs, and we corroborate these findings here. As a result, we observed an increase in revenues in the first endline, and we observe in this follow-up study that this increase persists and may have increased further, suggesting that households maintain their increased levels of consumption through an increase in productive assets and the resulting increases in income.

However, it is also possible that these estimates are biased by spillovers onto control households in treatment villages. To account for this bias, we compare treatment to pure control households in distant villages. We find a large and sustained increase in asset holdings resulting from cash transfers, but do not find statistically significant impacts on other dimensions. In line with this finding, the signs on the spillover effects are mostly negative, although when controlling for differential attrition using Lee bounds most of them do not reach statistical significance, with the exception of monthly consumption. It is nevertheless possible that the within-village treatment effects described above are biased by spillover effects. The fact that the treatment effect on assets is significant even in the across-village analysis suggests that cash transfers do indeed entail a sustained increase on this dimension for recipients. The lack of significant findings on other dimensions in the across-village analysis may result from lower power in that analysis due to the lack of baseline data, the absence of village-level fixed effects in the analysis, and clustering of standard errors at the village level. We therefore conclude that a larger sample is needed to robustly assess treatment effects across villages and to validate spillover effects.

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# Appendix

# A Outcome variables

## A.1 Components of indices and focal variables

The following indices and focal variable will be the primary subject of our analysis and will be included in multiple comparison adjustments.

**Total assets:** Total value in 2012 PPP adjusted dollars of all household assets:

1. Movable assets
    (a) Livestock
        i. Cows
        ii. Small livestock
        iii. Birds
    (b) Furniture
    (c) Agricultural tools
    (d) Radio or TV
    (e) Other assets
2. Savings
3. Value of roof (inclusion not pre-specified for endline 1 analysis)
4. Omitted: Value of land (omission not pre-specified for endline 1 analysis)

**Total consumption:** Total spending per month in 2012 PPP adjusted dollars:

1. Food
    (a) Food own production
    (b) Food bought
    (c) Meat & fish
    (d) Fruit & vegetables
    (e) Other food
2. Temptation good expenditure
3. Medical expenditure
    (a) Medical expenditure (respondent)

(b) Medical expenditure (spouse)

(c) Medical expenditure (children)

4. Education expenditure

5. Social expenditure

6. Omitted: Durables expenditure, house expenditure (omission not pre-specified for endline 1 analysis)

7. Other expenditure

**Agricultural and business income:** Total household enterprise revenue per month in 2012 PPP adjusted dollars:

1. Agricultural income

   (a) Agricultural income (own consumption, total)

      i. Agricultural income (own consumption, harvest)

      ii. Agricultural income (own consumption, animals)

   (b) Agricultural income (sales, total)

      i. Agricultural income (sales, harvest)

      ii. Agricultural income (sales, animal products)

      iii. Agricultural income (sales, animals)

2. Non-farm enterprise revenue

**Psychological well-being index:** Standardized weighted average of psychological and neurobiological measures:

1. Depression (CESD) - negatively coded

2. Worries - negatively coded

3. Stress (Cohen) - negatively coded

4. Happiness (WVS)

5. Life satisfaction (WVS)

6. Cortisol (in log nm/l adjusted for confounds) - negatively coded

**Food security index (household):** Weighted average of measures of food security and hunger:

1. Meals skipped in the last month (adults) - negatively coded
2. Whole days without food in the last month (adults) - negatively coded
3. Meals skipped in the last month (children) - negatively coded
4. Whole days without food in the last month (children) - negatively coded
5. Household ate less preferred/cheaper foods in the last month (# of times) - negatively coded
6. Household relied on help from others for food in the last month (# of times) - negatively coded
7. Household purchased food on credit in the last month (# of times) - negatively coded
8. Household had to hunt, gather wild food, harvest prematurely in the last month (#of times) - negatively coded
9. Household begged because not enough food in the house in the last month (# of times) - negatively coded
10. All members usually eat two meals (dummy)
11. All members usually eat until content (dummy)
12. Number of times ate meat or fish (last week)
13. Enough food in the house for tomorrow (dummy)
14. Respondent slept hungry in the last week (dummy) - negatively coded
15. Respondent ate protein in the last 24 hours (dummy)
16. Proportion of HH who ate protein in the last 24 hours
17. Proportion of children who ate protein in the last 24 hours

**Health index:** Standardized weighted average:

1. Proportion of household sick/injured - negatively coded
2. Proportion of children sick/injured - negatively coded
3. Proportion of sick/injured who could afford treatment
4. Proportion of illnesses where doctor was consulted
5. Proportion of newborns vaccinated
6. Proportion of children $< 14$ getting checkup in the last 6 months
7. Proportion of children $< 5$ who died in the past 12 months - negatively coded

**Education index:** Standardized weighted average:

1. Education expenditure per child
2. Proportion of school-aged children in school

**Female empowerment index:** Standardized weighted average of attitude index and violence index:

1. Violence index (standardized weighted average):
   (a) Female report of number of instances of physical violence - negatively coded
   (b) Female report of number of instances of sexual violence - negatively coded
   (c) Female report of number of instances of emotional violence - negatively coded
2. Attitudes index (standardized weighted average):
   (a) Justifiability of violence score - negatively coded
   (b) Male-focused attitudes score - negatively coded

## A.2 All variables collected

In addition to the indices which are our primary focus, we will also explore impacts on other variables collected. Analysis of the variables not included in the above indexes is intended to be exploratory. We will look at these variables to better understand mechanisms and to generate hypotheses for future work.

**1. Assets**

**(a) Movable assets**

**i. Livestock:** Sum of all livestock assets owned by respondents in KES (later converted to USD PPP), including cows, small livestock, and birds.

**ii. Furniture:** Value of cupboards, sofas, chairs, tables, clocks, stoves, and beds as self reported in KES (later converted to USD PPP).

**iii. Agricultural tools:** Value of farming tools, wheelbarrows, and hand carts, in KES (later converted to USD PPP).

**iv. Radio or TV:** Value of radio and television assets in KES (later converted to USD PPP)

**v. Other assets:** Value of bicycles, motorbikes, solar panels, cellphones, and any other assets that respondents reported when asked if they owned any additional assets apart from those listed, in KES (later converted to USD PPP).

**(b) Savings:** Value of savings, in KES (later converted to USD PPP), in all savings accounts for the household (including mobile money accounts).

**(c) Land owned:** Land owned in acres.

**(d) House has non-thatch roof:** Dummy variable indicating that responding has a non-thatch roof (i.e. iron sheets, wood, etc.)

**(e) House has non-mud floor:** Dummy variable indicating that respondent has floor consisting of materials other than mud (i.e. tiles, wood, stones, concrete, etc.)

**(f) House has non-mud walls:** Dummy variable indicating that respondent has wall constructed from materials other than mud (i.e. wood, bricks/stones, plaster/cement).

**(g) House has electricity:** Dummy variable indicating that respondent has electricity

**(h) House has toilet or pit latrine:** Dummy variable indicating that the respondent has a pit latrine or mobile / portable toilet.

**2. Consumption**

**(a) Food**

**i. Food own production:** Value of milk consumed, other animal products consumed (cattle, small livestock, birds), meat consumed (cattle, small livestock, birds), eggs consumed, as well as the value of the crops consumed both for the long rains and short rains seasons, on average per week in KES (later converted to USD PPP).

**ii. Food bought:** Value of cereals, vegetables, fruit, meat, fish, dairy, fats, sugars, drinks, spices, and prep food purchased in the past week in KES (later converted to USD PPP).

**ii.a. Meat & fish:** Value of meat and fish purchased in the past week in KES (later converted to USD PPP).

**ii.b. Fruit & vegetables:** Value of fruits and vegetables purchased in the past week in KES (later converted to USD PPP).

**ii.c. Other food:** Value of cereals, dairy products, fats, prep foods, drinks, and spices purchased in the past week in KES (later converted to USD PPP).

**(b) Temptation good expenditure:** Value of expenditure on alcohol, tobacco, and lottery tickets in the past week in KES (later converted to USD PPP).

**(c) Medical expenditure:** Value of medical expenditure (consultation fees, medicines, hospitalizations) for the respondent, spouse, and children of the respondent in the past 1 month, in KES (later converted to USD PPP).

**i. Medical expenditure (respondent):** Value of medical expenditures (consultation fees, medicines, hospitalizations) in the past 1 month in KES (later converted to USD PPP) for the respondent.

**ii. Medical expenditure (spouse):** Value of medical expenditures (consultation fees, medicines, hospitalizations) in the past 1 month in KES (later converted to USD PPP) for the spouse of the respondent.

**iii. Medical expenditure (children):** Value of medical expenditures (consultation fees, medicines, hospitalizations) in the past 1 month in KES (later converted to USD PPP) for the children of the respondent.

**(d) Education expenditure:** Value of educations costs consumed (school fees, uniforms, etc.) in the past 12 months in KES (later converted to USD PPP).

**(e) Durables expenditure:** Value of household durables (cutlery, pots/pans, light bulbs, curtains, carpets, etc.) in the past 12 months in KES (later converted to USD PPP).

**(f) House expenditure:** Value of expenditure on house/land rent and repair in the past 12 months in KES (later converted to USD PPP).

**(g) Social expenditure:** Value of expenditure on ceremonies, weddings, funerals, dowry, village elders, and any other recreation (cinema tickets, music/CDs, books/magazines, etc.). in the past 12 months in KES (later converted to USD PPP).

**(h) Other expenditure:** Value of expenditure on airtime, traveling (petrol, bus fare, hotel stays), clothing, personal items (haircut, hair oil, cosmetics, etc.), household items (soap, toilet paper, candles, etc.), firewood, electricity bill, and water bills in the past 1 month in KES (later converted to USD PPP).

**3. Food security**

**(a) Meals skipped (adults):** Frequency of adults having to cut the size of meals or skip them entirely in the past 1 month.

**(b) Whole days without food (adults):** Frequency that adults have gone without any meals by in the past month.

**(c) Meals skipped (children):** Frequency of children (<14 years of age) having to cut the size of meals or skip them entirely in the past 1 month.

**(d) Whole days without food (children):** Frequency that children (<14 years of age) have gone without any meals by in the past month.

**(e) Eat less preferred / cheaper foods:** Frequency that household members have had to eat less preferred or less expensive foods in the past month.

**(f) Rely on help from others for food:** Frequency that household members have had to borrow food or rely on help from a friend or relative in the past month.

**(g) Purchase food on credit:** Frequency that household members have had to purchase food on credit.

**(h) Hunt, gather wild food, harvest prematurely:** Frequency that household members have had to gather wild food, hunt, or harvest immature crops in the past month.

**(i) Beg because not enough food in the house:** Frequency of household members having to beg because there was not enough food in the household in the past month.

**(j) All members eat two meals:** Dummy variable indicating whether all members of the household regularly eat at least 2 meals a day.

**(k) All members eat until content:** Dummy variable indicating whether all members usually eat until they are content each day.

**(l) Number of times ate meat or fish:** Frequency of respondent eating meat, eggs, or fish in the last week.

**(m) Enough food in the house for tomorrow?:** Dummy variable indicating whether the respondent believes that the household has enough food for tomorrow.

**(n) Respondent slept hungry:** Dummy variable indicating whether the respondent has gone to sleep hungry in the past week.

**(o) Respondent ate protein:** Dummy variable indicating whether the respondent ate protein in the past week.

**(p) Proportion of household who ate protein:** Number of people listed by respondent as having eaten protein in the past week divided by the total number of members in the household.

**(q) Proportion of children who ate protein:** Number of children listed by respondent (including own children and stepchildren) who ate protein divided by the total number of children in the household.

**4. Psychological and neurobiological outcomes**

**(a) Depression (CES-D)**

**(b) Worries**

**(c) Stress (Cohen)**

**(d) Happiness (WVS)**

**(e) Life satisfaction (WVS)**

**(f) Cortisol**

**(g) Trust (WVS)**

**(h) Locus of control (Rotter and WVS)**

**(i) Optimism (Scheier)**

**(j) Self-esteem (Beck)**

**(k) Self-Efficacy (Schwarzer & Jerusalem)**

**(l) Sense of power (Anderson, John, & Keltner)**

**(i) Aspirations** Standardized weighted average of the following:

1. Income aspired to and likely to achieve
2. Assets aspired to and likely to achieve
3. Social status aspired to and likely to achieve
4. Education (oldest child) aspired to and likely to achieve

**5. Female empowerment**

**(a) Physical violence dummy:** Dummy indicating if any physical violence occurred in the last six months, including if the spouse pushed, twisted the arm of, punched, kicked, chokes, or pulled a knife on the respondent.

**(b) Sexual violence dummy:** Dummy indicating if any sexual violence occurred in the last six months, including if the spouse raped or performed non-consensual sexual acts on the respondent.

**(c) Emotional violence dummy:** Dummy indicating if any emotional violence occurred in the last six months, including if the spouse was jealous or angry if you talked to other men/women, accused you of being unfaithful, did not permit you to meet your friends of the same gender, tried to limit your contact with your family, or did not trust you with any money.

**(d) Justifiability of violence score:** Total number of situations in which the respondent feels that the husband is justified in beating his wife: can beat if he/she goes out without telling her, if he/she neglects the children, he/she argues with her, he/she refused to have sex with him/her, he/she burns the food. Additional scenarios included in the follow-up.

**(e) Male-focused attitudes score:** Sum of all dummy variables indicating whether the respondent agree with the following male oriented statements: men should make the important decisions in the family, the wife has the right to express her opinion even when she disagrees with her husband (reverse coded), wife should tolerate getting beaten to keep family together, husband has the right to beat his wife, it is more important to send a son to school than to send a daughter. Additional scenarios included in the follow-up.

**(f) Male makes decisions:** Sum of dummy situations in which the respondent believes the male should have the final say: contraception use, children's schooling, buying clothes or shoes, what to do if a child falls ill, disciplining children, whether to have children, how much to spend on food, extra spending, saving.

**(g) Proportion choosing money for spouse vs. self:** Number of respondents choosing to give their spouse 130 KES vs. keeping 100 KES (later converted to USD PPP) for themselves divided by total number of married respondents.

**(h) Physical violence frequency:** Number of incidents of physical violence in the last six months, including if the spouse pushed, twisted the arm of, punched, kicked, chokes, or pulled a knife on the respondent in the past six months.

**(i) Sexual violence frequency:** Number of incidents of sexual violence in the last six months, including if the spouse raped or performed non-consensual sexual acts on the respondent in the past six months.

**(j) Emotional violence frequency:** Number of incidents of emotional violence in the last six months, including if the spouse was jealous or angry if you talked to other men/women, accused you of being unfaithful, did not permit you to meet your friends of the same gender, tried to limit your contact with your family, or did not trust you with any money.

**(k) Economic control frequency:** Number of occurrences of the following: including whether husband expected you to ask permission to purchase large or small items, took your earnings against your will, tells you he does not have enough money to give you for household expenses, tells you he does not have enough money to give you to spend on yourself, refuses to give you money for household expenses, even when he had money for other things, require that you give up or refuse a job for money outside the home because he did not want you to work, make important financial decisions without talking to you about them, demand to know how you spent money, hide money from you, spent money set aside for household benefits on himself, threaten not to give you money or take it away from you, given you little money or reduced your spacing when he is angry.

**(l) Perceived village IPV dummy** Dummy indicating an affirmative response to the question "Do men in your village beat, slap, or act physically violent towards their wives?"

**(j) Perceived village IPV frequency** Numerical response to the question "How many times per month do you think a man in your village beats, slaps or acts violently towards his wife?"

**(k) Physical injury dummy** A dummy variable indicating whether individual answers yes to whether the respondent experienced any of the following as a result of an act by her partner: had cuts or bruises; had eye injuries, sprains, dislocations, or burns; had deep wounds, broken bones, broken teeth or any other serious injury.

**(l) Perceived community justifiability of violence score** Total number of situations in which the respondent feels that most individuals in her community believe the husband is justified in beating his wife: can beat if he/she goes out without telling her, if he/she neglects the children, he/she argues with her, he/she refused to have sex with him/her, he/she burns the food. Additional scenarios included in the follow-up.

**(m) Perceived community male-focused attitudes score** Sum of all dummy variables indicating whether the respondent believes most individuals in her community agrees with the following male oriented statements: men should make the important decisions in the family, the wife has the right to express her opinion even when she disagrees with her husband (reverse coded), wife should tolerate getting beaten to keep family together, husband has the right to beat his wife, it is more important to send a son to school than to send a daughter. Additional statements included in the follow-up.

**(n) Marital control** Frequency with which the respondent has experience one of the following in the last six months: was jealous if you talked to other men, accused your of being unfaithful, did not permit you to meet female friends, tried to limit your contact with your family, did not trust you with money, insisted on knowing where you were at all times, expected you to ask permission before leaving the house.

**(o) Who in household owns livestock assets** Dummy variable for whether livestock are owned by the wife or jointly by the husband and wife.

**(p) Who manages money from livestock income** Dummy variable for whether livestock income is managed by the wife or jointly by the husband and wife.

**(q) Who controls savings accounts** Dummy variable for whether savings accounts are managed by the wife or jointly by the husband and wife.

**(r) Who borrows** Dummy variable for whether borrowing decisions are made by the wife or jointly by the husband and wife.

**6. Health**

**(a) Medical expenses per episode (entire household):** Sum of all treatment costs (direct and indirect) in KES (later converted to USD PPP) for any episodes in the past month among all household members divided by the total number of incidents in the household.

**(b) Medical expenses per episode (spouse):** Sum of all treatment costs (direct and indirect) in KES (later converted to USD PPP) for any episodes in the past month among spouses in the household divided by the total number of incidents among spouses in the household.

**(c) Medical expenses per episode (children):** Sum of all treatment costs (direct and indirect) in KES (later converted to USD PPP) for any episodes in the past month among spouses in the household divided by the total number of incidents among children in the household.

**(d) Proportion of household sick / injured:** Total number of household members who were sick or injured in the past month divided by the total number of household members.

**(e) Proportion of children sick / injured:** Total number of children in the household who were sick or injured in the past month divided by the total number of children in the household.

**(f) Proportion of sick / injured who could afford treatment:** Total number of household members who were sick / injured who reported being able to pay for treatments divided by the total number of people who reported being sick/injured in the past month.

**(g) Average number of sick days per household member:** Total number of sick days among household members divided by the number of household members in the past month.

**(h) Proportion of illnesses where doctor was consulted:** Total number of illness/injury episodes where a doctor was consulted divided by the total number of illnesses and injuries in the household in the past month.

**(i) Proportion of newborns vaccinated:** Total number of children under one years of age who have been vaccinated divided by the total number of children under one years of age in the household.

**(j) Proportion of children <14 getting checkup:** Total number of children under the age of 14 reporting having a regular checkup in the past six months divided by the total number of children under the age of 14.

**(k) Proportion of children <5 who died:** Total number of children in the household who have died in the past twelve months divided by the total number of children under 5 (living and passed) in the household.

**7. Education**

**(a) Total education expenditure:** Value spend on educations goods (school fees, uniforms, books, or other supplies, in KES (later converted to USD PPP) for the household in the past 12 months.

**(b) Education expenditure per child:** Value spent on education goods (school fees, uniforms, books, or other supplies, in KES (later converted to USD PPP) for the household in the past 12 months divided by the number of school age children (aged 3-18) in the household.

**(c) Proportion of school-aged children in school:** Number of school age children (aged 3-18) currently attending school divided by the total number of school age children in the household.

**(d) School days missed for economic reasons, per child:** Sum of total number of days per child reported as missed for economic reasons (No breakfast / food, can't pay fees, needs to work for money, needed for household, child or elder care) divided by the total number of school aged children in the past month.

**(e) Income generating activities per school-aged child >6:** Sum of total number of income generating activities per child 6-18 years of age in the household divided by the number of children 6-18 in the household engaged in the past twelve months.

**8. Enterprise**

**(a) Agricultural income (total)**

**i. Agricultural income (own consumption, total):** Sum of consumed harvest income and consumed animal income in KES (later converted to USD PPP) per month.

**ii. Agricultural income (sales, total):** Sum of harvest sales, animal product sales, and livestock sales to create a monthly agricultural income average.

**(b) Enterprise profits (6 months):** Value in KES (later converted to USD PPP) of profits (or losses if negative) of all non-agricultural, non-livestock income generating enterprises owned and operated (partially or fully) by the respondent in the past six months.

**(c) Enterprise revenue (1 month):** Value in KES (later converted to USD PPP) of all money received from all non-agricultural, non-livestock income generating enterprises owned and operated (partially or fully) by the respondent in the past one month.

**(d) Enterprise revenue (typical month):** Value in KES (later converted to USD PPP) of the sales of all non-agricultural, non-livestock income generating enterprises owned and operated (partially or fully) by the respondent in an average month.

**(e) New non-agricultural business owner (dummy):** Dummy variable indicating whether a respondent did not have a non-agricultural business at baseline but now does at endline.

**(f) Non-agricultural business owner (dummy):** Dummy variable indicating whether a respondent owns and operates a non-agricultural business.

**(g) Number of employees:** Number of non-household member employees in all entrepreneurial activities owned and operated by the respondent (partially or fully owned).

**(h) Value of investment in non-agricultural income (total):** Costs of electricity, wages, water, transport ,inputs, and any other expenses for all enterprises owned and operated (partially or fully) by the respondent for the past three months in KES (later converted to USD PPP).

**9. Financial variables**

**(a) Value of outstanding loans:** Amount in KES (later converted to USD PPP) outstanding from any loan taken by a member of the household, including debts to local shops and kiosks.

**(b) Unable to pay loans (12 months):** Dummy variable indicating that household was unable to make payments on at least one loan in the past 12 months

**(c) Value of remittance sent:** Value of all cash and goods sent as remittances to non-household members or members outside of their compound in the past month in KES (later converted to USD PPP).

**(d) Value of remittances received:** Value of all cash and goods received as remittances from non-household members or members outside of their compound in the past month in KES (later converted to USD PPP).

**(e) Net remittances:** Value of remittances sent less value of remittances received in KES (later converted to USD PPP).

**10. List method**

**(a) Temptation goods:** Estimated number of alcohol and tobacco users in treatment and control groups.

**(b) Intimate partner violence:** Estimated rate of violence in treatment and control groups.

**11. Political Variables**

**(a) Will vote in the next election:** Indicator for answering yes to "will you be voting in the upcoming national elections that will be held next year?"

**(b) Political knowledge:** Indicator for knowing the names of the candidates running for Prime Minister and President in the next election.

**(c) Attitudes towards voting:** Indicator for responding that it is very Kenyan citizen's responsibility to vote when asked about responsibility to vote.

**(d) Trust in government institutions:** Indicator for answering "let the Kenyan government decide how to spend it" when asked the how foreign aid should be spent to reduce poverty.

**12. Labor and Time Use Variables:**

**(a) Salaried jobs:**

**i. Salaried labor is the household's primary source of income:** Indicator for answering that a salaried job is household's primary source of income.

**ii. Proportion of household members working in a salaried job:** Proportion of adults in the household for who were reported as having worked in a salaried job at any point in the last 12 months.

**iii. Time (in days) spent working in a salaried job** Days in the last month spent working a salaried job by household adults.

**iv. Income working in a salaried job** Typical income the last month from working a salaried job by household adults.

**(b) Casual Labor for other households**

Time spent by household adults performing casual labor, performing housework for pay, farming land, or tending animals for another household.

**i. Casual labor is the household's primary source of income:** Indicator for answering that casual labor for other households is household's primary source of income.

**ii. Proportion of household members performing casual labor:** Proportion of adults in the household for who were reported as having performed casual labor at any point in the last 12 months.

**iii. Time (in days) spent performing casual labor** Days in the last month spent performing casual labor by household adults.

**iv. Income from performing casual labor** Typical income the last month from performing casual labor by household adults.

**(c) Household enterprises:**

Time spent by household adults working in a business owned by a household member, farming on land owned by the household, tending animals owned by the household, performing housework in this household, or fishing.

**i. Household or agricultural enterprise is the household's primary source of income:** Indicator for answering that a household / agricultural enterprise is household's primary source of income.

**ii. Proportion of household members working for household or agricultural enterprise:** Proportion of adults in the household for who were reported as having worked in household / agricultural enterprise at any point in the last 12 months.

**iii. Time (in days) spent working for household or agricultural enterprise** Days in the last month spent working in household / agricultural enterprise by household adults.

**iv. Income from household / agricultural enterprise** Typical income the last month from working in household / agricultural enterprise.

**(d) Time use**

**i. Time working for HH in days** Farming fishing, tending animals, housework, working for an enterprise owned by the household

**ii. Housework in days** Performing housework for pay for another household

**iii. Time working outside the HH in days** Farming fishing, tending animals, housework, daily labor, salaried job

**iv. Income from work outside the HH in days** Farming fishing, tending animals, housework, daily labor, salaried job

**13. Social Capital**

**(a) Group membership** Total number of types of groups in which he individual has been active in the last 12 months: work related / trade union, community associations, women's groups or Chama, political groups, religious groups, credit /funeral groups, sports groups, other.

**(b) Support from groups** Total number of types of groups from which the individual has received emotional help, economic help, or assistance in learning to do things: work related / trade union, community associations, women's groups or Chama, political groups, religious groups, credit /funeral groups, sports groups, other.

**(c) Support from individuals** Total number of individuals from which the individual has received emotional help, economic help, or assistance in learning to do things: family, neighbors, friends, community leaders, religious leaders, politicians, government officials / civil servants, charities / NGOs.

**(d) Collective action** Indicator for whether the individual had joined together with other community members to address a problem or common issue.

**(e) Trust** Indicator for whether the individual believes he majority of people in the community can be trusted.

**14. Impact of transfers on relationships**

**(a) Intimate partner relationships** Relationship satisfaction, respect, jealousy, marital status, quality of relationship, conflict; decision-making on use of transfer; use of transfers

**(b) Family relationships** Respect from in-laws

**(c) Community relationships** Tension between household and community; respect from other households; change in how household was treated; change in relationship due to the transfer

**(d) Marital satisfaction** Total score across martial satisfaction questions

**(e) Social comparisons**

# B Detailed Results Tables

## B.1 Outcome group variables

Table B.1: Treatment effects – Assets

| | (1) Control mean (SD) | (2) Treatment effect | (3) Female recipient | (4) Monthly transfer | (5) Large transfer | (6) N |
|---|---|---|---|---|---|---|
| Value of non-land assets excluding roof (USD) | 592.24 | 263.80*** | −97.14 | 46.06 | 48.24 | 912 |
| | (554.51) | (37.59) | (66.46) | (62.45) | (73.23) | |
| Value of livestock (USD) | 286.19 | 148.40*** | −94.40* | 39.16 | 89.97 | 912 |
| | (456.14) | (29.54) | (51.10) | (47.54) | (59.79) | |
| Value of cows (USD) | 202.09 | 127.15*** | −87.85* | 25.81 | 98.47* | 911 |
| | (399.23) | (26.73) | (47.04) | (44.70) | (53.63) | |
| Value of small livestock (USD) | 53.17 | 14.24** | −6.35 | 12.43 | −7.38 | 911 |
| | (91.93) | (5.83) | (9.87) | (9.64) | (9.63) | |
| Value of birds (USD) | 31.56 | 5.21** | −1.07 | 2.65 | −1.07 | 911 |
| | (35.52) | (2.45) | (4.28) | (4.30) | (4.28) | |
| Value of durable goods (USD) | 283.40 | 107.52*** | 16.51 | 5.16 | −29.61 | 912 |
| | (237.12) | (17.80) | (30.72) | (30.41) | (31.55) | |
| Value of furniture (USD) | 175.98 | 65.29*** | −0.75 | −2.27 | 6.99 | 912 |
| | (121.44) | (8.91) | (15.42) | (15.20) | (17.71) | |
| Value of agricultural tools (USD) | 13.97 | 4.15*** | −1.72 | 0.28 | −3.98** | 912 |
| | (17.95) | (1.32) | (2.36) | (2.46) | (1.99) | |
| Value of radio/TV (USD) | 12.75 | 3.77** | −0.59 | −2.85 | −0.52 | 912 |
| | (27.29) | (1.87) | (3.45) | (3.15) | (3.09) | |
| Value of bike/motorbike (USD) | 33.79 | 28.66** | 14.08 | 14.05 | −25.97 | 912 |
| | (135.91) | (11.64) | (21.75) | (22.69) | (19.29) | |
| Value of appliances (USD) | 14.08 | 4.23* | 4.37 | 2.17 | −3.58 | 912 |
| | (34.39) | (2.40) | (4.10) | (4.30) | (3.49) | |
| Value of cell phone (USD) | 32.82 | 4.69*** | 0.79 | 2.47 | −3.48 | 912 |
| | (25.67) | (1.80) | (3.11) | (3.36) | (3.10) | |
| Value of savings (USD) | 19.14 | 10.07*** | −14.07* | 5.09 | 2.02 | 912 |
| | (49.50) | (3.83) | (7.81) | (7.36) | (6.92) | |
| Total land owned (acres) | 4.81 | 5.90 | 3.45 | −7.94 | −12.62 | 844 |
| | (49.24) | (6.16) | (14.48) | (15.43) | (8.26) | |
| Has non-thatched roof (dummy) | 0.60 | 0.23*** | −0.01 | −0.04 | −0.03 | 912 |
| | (0.49) | (0.03) | (0.04) | (0.04) | (0.04) | |
| Joint test ($p$-value) | | 0.00*** | 0.83 | 0.81 | 0.05** | |

*Notes:* OLS estimates of treatment effects. Outcome variables are listed on the left. The unit of observation is the household for all variables. All variables are Winsorized at the the 99th percentile. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the mean taken among control households in treatment villages (spillover) for a given outcome variable. Column (2) reports the treatment effect within villages, i.e. comparing treatment households to spillover households. Column (3) reports the difference in effect for households in which the primary female received the transfer in comparison to households in which the priamry male received the transfer. Column (4) reports the difference in effect for households that received monthly transfers in comparison to housedhods that received lump sum transfers. Column (5) reports the the difference in effect or households that received large transfers in comparison to households that received small transfers. Column (6) reports the sample size. Standard errors are clustered at the household level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table B.2: Treatment effects – Consumption

| | (1) Control mean (SD) | (2) Treatment effect | (3) Female recipient | (4) Monthly transfer | (5) Large transfer | (6) N |
|---|---|---|---|---|---|---|
| Food total (USD) | 127.74 | 31.64*** | −6.21 | 14.68 | −9.41 | 912 |
| | (101.00) | (7.39) | (11.57) | (11.96) | (13.11) | |
| Food own production (USD) | 21.92 | 5.49 | 1.71 | 7.79 | −0.80 | 912 |
| | (53.19) | (3.54) | (3.98) | (4.89) | (4.53) | |
| Food bought (USD) | 105.82 | 26.09*** | −8.58 | 6.62 | −8.90 | 912 |
| | (73.12) | (5.65) | (9.80) | (10.19) | (11.00) | |
| Cereals (USD) | 30.05 | 7.48*** | 0.28 | 3.59 | −0.08 | 911 |
| | (26.40) | (1.99) | (3.58) | (3.73) | (3.54) | |
| Meat & fish (USD) | 18.37 | 5.56*** | −3.29 | 1.81 | −3.85 | 912 |
| | (21.00) | (1.55) | (2.69) | (2.71) | (2.70) | |
| Fruit & vegetables (USD) | 26.07 | 7.24*** | −3.33 | −0.09 | −2.86 | 911 |
| | (25.56) | (1.99) | (3.59) | (3.76) | (3.56) | |
| Dairy (USD) | 5.63 | 2.50*** | −0.42 | 1.14 | −1.95 | 911 |
| | (9.26) | (0.69) | (1.20) | (1.24) | (1.24) | |
| Fats (USD) | 6.94 | 1.63*** | −0.45 | 0.80 | 0.42 | 912 |
| | (7.06) | (0.56) | (0.88) | (0.94) | (1.27) | |
| Sugars (USD) | 10.79 | 1.30** | 0.26 | 0.18 | −0.83 | 911 |
| | (7.96) | (0.53) | (0.84) | (0.90) | (0.80) | |
| Other food (USD) | 50.16 | 12.24*** | −2.06 | 4.69 | −1.42 | 912 |
| | (36.74) | (2.83) | (4.84) | (5.08) | (5.67) | |
| Alcohol (USD) | 3.01 | −0.31 | −1.16 | 0.67 | −1.33 | 902 |
| | (11.78) | (0.78) | (1.24) | (1.18) | (1.02) | |
| Tobacco (USD) | 0.74 | 0.19 | 0.40 | 0.23 | 0.16 | 908 |
| | (3.76) | (0.26) | (0.44) | (0.45) | (0.44) | |
| Medical expenditure past month (USD) | 13.14 | 3.46 | −0.56 | 0.65 | 1.82 | 911 |
| | (38.60) | (2.37) | (3.89) | (3.69) | (3.41) | |
| Medical expenditure, children (USD) | 6.03 | −1.15 | −3.21 | −1.15 | −1.61 | 824 |
| | (34.59) | (1.98) | (2.22) | (1.98) | (1.88) | |
| Education expenditure (USD) | 7.99 | 2.63** | −0.50 | −1.17 | 2.39 | 910 |
| | (16.87) | (1.24) | (2.23) | (2.10) | (2.16) | |
| Social expenditure (USD) | 5.04 | 1.27* | 2.00* | −0.13 | −0.62 | 912 |
| | (9.12) | (0.67) | (1.14) | (1.26) | (1.03) | |
| Other expenditure (USD) | 29.95 | 8.47*** | 1.52 | 2.09 | 1.66 | 912 |
| | (27.65) | (2.03) | (3.56) | (3.69) | (3.54) | |
| Non-durable expenditure (USD) | 187.55 | 47.04*** | −2.08 | 17.35 | −5.37 | 912 |
| | (134.79) | (9.78) | (16.34) | (16.54) | (17.09) | |
| Joint test ($p$-value) | | 0.00*** | 0.17 | 0.89 | 0.33 | |

*Notes:* OLS estimates of treatment effects. Outcome variables are listed on the left. The unit of observation is the household for all variables. All variables are Winsorized at the 99th percentile. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the mean taken among control households in treatment villages (spillover) for a given outcome variable. Column (2) reports the treatment effect within villages, i.e. comparing treatment households to spillover households. Column (3) reports the difference in effect for households in which the primary female received the transfer in comparison to households in which the priamry male received the transfer. Column (4) reports the difference in effect for households that received monthly transfers in comparison to housedhods that received lump sum transfers. Column (5) reports the the difference in effect or households that received large transfers in comparison to households that received small transfers. Column (6) reports the sample size. Standard errors are clustered at the household level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table B.3: Treatment effects – Agricultural and Business Activities

| | (1) Control mean (SD) | (2) Treatment effect | (3) Female recipient | (4) Monthly transfer | (5) Large transfer | (6) N |
|---|---|---|---|---|---|---|
| Wage labor primary income (dummy) | 0.25 | −0.06** | 0.01 | 0.01 | 0.08* | 912 |
| | (0.44) | (0.03) | (0.04) | (0.04) | (0.04) | |
| Own farm primary income (dummy) | 0.40 | −0.00 | −0.02 | −0.08 | −0.07 | 912 |
| | (0.49) | (0.03) | (0.05) | (0.05) | (0.05) | |
| Non-ag business primary income (dummy) | 0.16 | 0.05** | −0.03 | 0.07* | 0.01 | 912 |
| | (0.36) | (0.02) | (0.04) | (0.04) | (0.04) | |
| Non-agricultural business owner (dummy) | 1.55 | −0.03 | 0.04 | −0.10* | −0.04 | 912 |
| | (0.50) | (0.03) | (0.05) | (0.05) | (0.05) | |
| Non-ag business revenue, monthly (USD) | 44.92 | 11.61 | −31.92* | −8.45 | 17.31 | 902 |
| | (145.36) | (9.90) | (18.93) | (15.35) | (21.88) | |
| Non-ag business flow expenses, monthly (USD) | 27.45 | 11.54* | 9.02 | 17.62 | 5.98 | 910 |
| | (82.45) | (6.73) | (12.49) | (13.84) | (11.67) | |
| Non-ag business profit imputed, monthly (USD) | 18.87 | −0.42 | −36.39** | −19.51 | 9.41 | 901 |
| | (109.50) | (8.35) | (16.85) | (13.83) | (19.64) | |
| Non-ag business profit self-reported, monthly (USD) | 11.26 | −0.07 | −4.62 | 0.44 | 6.00* | 898 |
| | (32.95) | (2.06) | (3.28) | (3.00) | (3.58) | |
| Farm revenue, monthly (USD) | 12.82 | 3.02*** | 1.76 | −2.49 | −3.03** | 912 |
| | (13.42) | (1.00) | (1.64) | (1.98) | (1.35) | |
| Farm flow expenses, monthly (USD) | 4.53 | 1.53*** | 0.89 | −0.69 | −0.53 | 912 |
| | (5.47) | (0.40) | (0.69) | (0.73) | (0.64) | |
| Farm profit, monthly (USD) | 8.29 | 1.48* | 0.80 | −1.77 | −2.52** | 912 |
| | (11.43) | (0.88) | (1.44) | (1.80) | (1.20) | |
| Livestock flow revenue, monthly (USD) | 6.16 | 3.57*** | 0.02 | 1.96 | 2.78 | 912 |
| | (15.79) | (1.37) | (2.66) | (2.48) | (2.94) | |
| Livestock flow expenses, monthly (USD) | 9.10 | 5.34*** | −1.47 | 0.52 | 0.57 | 912 |
| | (17.57) | (1.34) | (2.52) | (2.40) | (2.43) | |
| Livestock flow profit, monthly (USD) | −2.95 | −1.82 | 1.51 | 0.97 | 1.20 | 912 |
| | (19.82) | (1.64) | (3.17) | (3.02) | (3.16) | |
| Livestock sales and meat revenue, monthly (USD) | 9.49 | 2.75 | −3.47 | 6.86 | −2.63 | 912 |
| | (50.31) | (3.33) | (2.93) | (4.32) | (3.45) | |
| Total revenue, monthly (USD) | 73.01 | 20.70* | −33.40* | −2.54 | 14.63 | 912 |
| | (158.53) | (10.60) | (19.23) | (16.22) | (22.76) | |
| Total expenses, monthly (USD) | 40.96 | 18.53*** | 8.56 | 17.92 | 7.01 | 912 |
| | (85.10) | (6.87) | (12.76) | (14.22) | (11.78) | |
| Total profit, monthly (USD) | 24.01 | −1.41 | −33.60** | −20.02 | 7.86 | 912 |
| | (111.66) | (8.39) | (16.78) | (13.65) | (19.60) | |
| Joint test ($p$-value) | | 0.00*** | 0.69 | 0.26 | 0.20 | |

*Notes:* OLS estimates of treatment effects. Outcome variables are listed on the left. The unit of observation is the household for all variables. All variables for which the unit of measurement is a monetary value are Winsorized at the 99th percentile For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the mean taken among control households in treatment villages (spillover) for a given outcome variable. Column (2) reports the treatment effect within villages, i.e. comparing treatment households to spillover households. Column (3) reports the difference in effect for households in which the primary female received the transfer in comparison to households in which the priamry male received the transfer. Column (4) reports the difference in effect for households that received monthly transfers in comparison to housedhods that received lump sum transfers. Column (5) reports the the difference in effect or households that received large transfers in comparison to households that received small transfers. Column (6) reports the sample size. Standard errors are clustered at the household level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table B.4: Treatment effects – Food Security

| | (1) Control mean (SD) | (2) Treatment effect | (3) Female recipient | (4) Monthly transfer | (5) Large transfer | (6) N |
|---|---|---|---|---|---|---|
| Meals skipped (adults, # last month) | 4.02 | −0.73** | −0.01 | 0.06 | −0.18 | 912 |
| | (5.09) | (0.31) | (0.46) | (0.48) | (0.44) | |
| Whole days without food (adults, # last month) | 1.52 | −0.20 | −0.09 | −0.14 | 0.01 | 912 |
| | (2.58) | (0.16) | (0.25) | (0.26) | (0.21) | |
| Meals skipped (children, # last month) | 3.18 | −1.01*** | 0.05 | 0.24 | 0.10 | 802 |
| | (4.55) | (0.27) | (0.35) | (0.34) | (0.36) | |
| Whole days without food (children, # last month) | 1.00 | −0.24* | −0.15 | −0.33 | 0.06 | 802 |
| | (2.31) | (0.14) | (0.20) | (0.21) | (0.19) | |
| Eat less preferred/cheaper foods (# last month) | 6.24 | −1.07** | −0.17 | 0.30 | 0.61 | 912 |
| | (6.81) | (0.42) | (0.63) | (0.64) | (0.71) | |
| Rely on help from others for food (# last month) | 2.46 | −0.47* | −0.01 | −0.03 | 0.24 | 912 |
| | (4.14) | (0.25) | (0.37) | (0.36) | (0.43) | |
| Purchase food on credit (# last month) | 2.84 | −0.33 | −0.06 | −0.24 | −0.20 | 912 |
| | (3.75) | (0.23) | (0.35) | (0.36) | (0.35) | |
| Hunt, gather wild food, harvest prematurely (# last month) | 3.05 | −0.32 | −0.37 | 0.19 | 0.90 | 912 |
| | (5.70) | (0.36) | (0.54) | (0.55) | (0.59) | |
| Beg because not enough food in the house (# last month) | 0.82 | −0.09 | −0.09 | −0.63*** | −0.08 | 912 |
| | (1.79) | (0.13) | (0.24) | (0.24) | (0.17) | |
| All members usually eat two meals (dummy) | 0.72 | 0.07** | 0.07 | 0.02 | −0.00 | 912 |
| | (0.45) | (0.03) | (0.04) | (0.04) | (0.04) | |
| All members usually eat until content (dummy) | 0.51 | −0.00 | −0.03 | 0.00 | 0.04 | 912 |
| | (0.50) | (0.03) | (0.05) | (0.06) | (0.05) | |
| Number of times ate meat or fish (last week) | 1.90 | 0.31** | −0.06 | −0.15 | 0.31 | 912 |
| | (1.72) | (0.13) | (0.24) | (0.19) | (0.30) | |
| Enough food in the house for tomorrow? (dummy) | 0.20 | −0.00 | 0.03 | 0.03 | −0.01 | 912 |
| | (0.40) | (0.03) | (0.04) | (0.04) | (0.04) | |
| Respondent slept hungry (last week, dummy) | 0.45 | −0.00 | 0.05 | −0.03 | 0.03 | 912 |
| | (0.50) | (0.03) | (0.05) | (0.05) | (0.05) | |
| Proportion of HH who ate protein (last 24h) | 0.19 | 0.04* | −0.01 | −0.05 | 0.03 | 909 |
| | (0.37) | (0.02) | (0.04) | (0.04) | (0.04) | |
| Food security index (children) | 0.00 | 0.19*** | 0.03 | 0.05 | −0.03 | 802 |
| | (1.00) | (0.06) | (0.08) | (0.08) | (0.08) | |
| Food security index | 0.00 | 0.20*** | 0.04 | −0.01 | 0.01 | 912 |
| | (1.00) | (0.06) | (0.10) | (0.10) | (0.11) | |
| Joint test ($p$-value) | | 0.01** | 0.97 | 0.42 | 0.92 | |

*Notes:* OLS estimates of treatment effects. Outcome variables are listed on the left. The unit of observation is the household for all variables. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the mean taken among control households in treatment villages (spillover) for a given outcome variable. Column (2) reports the treatment effect within villages, i.e. comparing treatment households to spillover households. Column (3) reports the difference in effect for households in which the primary female received the transfer in comparison to households in which the priamry male received the transfer. Column (4) reports the difference in effect for households that received monthly transfers in comparison to househods that received lump sum transfers. Column (5) reports the the difference in effect or households that received large transfers in comparison to households that received small transfers. Column (6) reports the sample size. Standard errors are clustered at the household level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The index is a standardized weighted average of the (negatively coded) number of times household adults and children skipped meals, went whole days without food, had to eat cheaper or less preferred food, had to rely on others for food, had to purchase food on credit, had to hunt for or gather food, had to beg for food, or went to sleep hungry in the preceding week; a (negatively coded) indicator for whether the respondent went to sleep hungry in the preceding week; the (positively coded) number of times household members ate meat or fish in the preceding week; (positively coded) indicators for whether household members ate at least two meals per day, ate until content, had enough food for the next day, and whether the respondent ate protein in the last 24 hours; and the (positively coded) proportion of household members who ate protein in the last 24 hours, and proportion of children who ate protein in the last 24 hours.

Table B.5: Treatment effects – Health

| | (1) Control mean (SD) | (2) Treatment effect | (3) Female recipient | (4) Monthly transfer | (5) Large transfer | (6) N |
|---|---|---|---|---|---|---|
| Medical expenses per episode, entire HH (USD) | 5.21 | 14.65 | 2.61 | −35.47 | −19.21 | 834 |
| | (12.72) | (13.10) | (5.87) | (36.59) | (20.25) | |
| Medical expenses per episode, children (USD) | 0.66 | 0.10 | −0.37 | 0.01 | 0.10 | 825 |
| | (2.70) | (0.21) | (0.36) | (0.30) | (0.48) | |
| Proportion of household sick/injured (1 month) | 0.52 | 0.03* | −0.04 | −0.00 | −0.00 | 912 |
| | (0.32) | (0.02) | (0.03) | (0.03) | (0.03) | |
| Proportion of children sick/injured (1 month) | 0.47 | 0.02 | −0.07* | −0.00 | 0.03 | 825 |
| | (0.36) | (0.02) | (0.04) | (0.04) | (0.04) | |
| Proportion of sick/injured who could afford treatment | 0.58 | 0.03 | 0.07* | 0.05 | −0.03 | 834 |
| | (0.42) | (0.03) | (0.04) | (0.05) | (0.04) | |
| Average number of sick days per HH member | 2.19 | 0.41** | −0.11 | 0.18 | −0.40 | 912 |
| | (2.74) | (0.20) | (0.31) | (0.38) | (0.29) | |
| Propotion of illnesses where doctor was consulted | 0.73 | 0.04 | 0.03 | 0.03 | 0.05 | 834 |
| | (0.36) | (0.02) | (0.04) | (0.04) | (0.04) | |
| Children ¡14 had checkups (6 months) | 0.26 | −0.02 | 0.00 | 0.00 | −0.00 | 776 |
| | (0.44) | (0.03) | (0.04) | (0.05) | (0.05) | |
| Proportion of children ¡5 who died (1 year) | 0.03 | 0.02 | −0.03 | −0.01 | 0.02 | 588 |
| | (0.13) | (0.01) | (0.02) | (0.02) | (0.03) | |
| Health index (children) | 0.00 | −0.07 | 0.19 | 0.01 | −0.22* | 825 |
| | (1.00) | (0.07) | (0.12) | (0.13) | (0.13) | |
| Health index | −0.00 | −0.07 | 0.22** | 0.11 | −0.10 | 912 |
| | (1.00) | (0.06) | (0.10) | (0.10) | (0.10) | |
| Joint test ($p$-value) | | 0.20 | 0.67 | 0.97 | 0.08* | |

*Notes:* OLS estimates of treatment effects. Outcome variables are listed on the left. The unit of observation is the household for all variables. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the mean taken among control households in treatment villages (spillover) for a given outcome variable. Column (2) reports the treatment effect within villages, i.e. comparing treatment households to spillover households. Column (3) reports the difference in effect for households in which the primary female received the transfer in comparison to households in which the priamry male received the transfer. Column (4) reports the difference in effect for households that received monthly transfers in comparison to househods that received lump sum transfers. Column (5) reports the the difference in effect or households that received large transfers in comparison to households that received small transfers. Column (6) reports the sample size. Standard errors are clustered at the household level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The index is standardized weighted average of the (negatively coded) proportion of household adults who were sick or injured in the last month, the (negatively coded) proportion of household children who were sick or injured in the last month, the (positively coded) proportion of sick or injured family members for whom the household could afford treatment, the (positively coded) proportion of illnesses for which a doctor was consulted, the (positively coded) proportion of newborns who were vaccinated, the (positively coded) proportion of children below age 14 who received a health checkup in the preceding six months, the (negatively coded) proportion of children under 5 who died in the preceding year, and a children's anthropometrics index consisting of BMI, height-for-age, weight-for-age, and upper arm circumference relative to WHO development benchmarks.

Table B.6: Treatment effects – Education

| | (1) Control mean (SD) | (2) Treatment effect | (3) Female recipient | (4) Monthly transfer | (5) Large transfer | (6) N |
|---|---|---|---|---|---|---|
| Education expenditure past month (USD) | 104.25 | 31.68* | −2.69 | −5.58 | 13.62 | 815 |
| | (212.79) | (16.31) | (27.34) | (26.39) | (27.46) | |
| Education expenditure per child past month (USD) | 37.31 | 11.10* | 8.15 | 13.25 | 12.49 | 815 |
| | (81.28) | (6.51) | (12.35) | (13.69) | (11.81) | |
| Proportion of school-aged children in school | 0.87 | 0.02 | 0.01 | 0.04 | 0.03 | 817 |
| | (0.23) | (0.02) | (0.02) | (0.02) | (0.02) | |
| School days missed past month (per child) | 2.26 | 0.18 | 0.50 | 0.94 | −0.47 | 817 |
| | (2.87) | (0.33) | (0.36) | (0.71) | (0.34) | |
| Income-generating activities per school-age child ¿6 | 1.04 | 0.12** | 0.16* | 0.07 | −0.04 | 752 |
| | (0.80) | (0.06) | (0.09) | (0.10) | (0.09) | |
| Education index | 0.00 | 0.15** | 0.07 | 0.19 | 0.17 | 817 |
| | (1.00) | (0.07) | (0.12) | (0.13) | (0.12) | |
| Joint test ($p$-value) | | 0.08* | 0.16 | 0.15 | 0.37 | |

*Notes:* OLS estimates of treatment effects. Outcome variables are listed on the left. The unit of observation is the household for all variables, restricting to households with schoolage children. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the mean taken among control households in treatment villages (spillover) for a given outcome variable. Column (2) reports the treatment effect within villages, i.e. comparing treatment households to spillover households. Column (3) reports the difference in effect for households in which the primary female received the transfer in comparison to households in which the priamry male received the transfer. Column (4) reports the difference in effect for households that received monthly transfers in comparison to households that received lump sum transfers. Column (5) reports the the difference in effect or households that received large transfers in comparison to households that received small transfers. Column (6) reports the sample size. Standard errors are clustered at the household level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The index is standardized weighted average of the proportion of household children enrolled in school and the amount spent by the household on educational expenses per child.

Table B.7: Treatment effects – Psychological Wellbeing

| | (1) Control mean (SD) | (2) Treatment effect | (3) Female recipient | (4) Monthly transfer | (5) Large transfer | (6) N |
|---|---|---|---|---|---|---|
| Depression (CESD) | 27.61 | −1.14** | −0.76 | −0.64 | 0.48 | 1491 |
| | (9.71) | (0.48) | (0.68) | (0.73) | (0.78) | |
| Stress (Cohen) | 0.00 | −0.06 | −0.12 | −0.01 | −0.12 | 1491 |
| | (1.00) | (0.05) | (0.08) | (0.08) | (0.08) | |
| Happiness (WVS) | −0.00 | 0.09* | −0.07 | 0.02 | 0.14* | 1487 |
| | (1.00) | (0.05) | (0.08) | (0.09) | (0.08) | |
| Life satisfaction (WVS) | 0.00 | 0.08* | 0.04 | −0.14* | 0.04 | 1489 |
| | (1.00) | (0.05) | (0.07) | (0.08) | (0.08) | |
| Trust (WVS) | 0.00 | −0.01 | −0.01 | −0.05 | −0.00 | 1491 |
| | (1.00) | (0.05) | (0.08) | (0.09) | (0.08) | |
| Locus of control | −0.00 | −0.12** | 0.08 | −0.03 | 0.03 | 1491 |
| | (1.00) | (0.05) | (0.07) | (0.08) | (0.08) | |
| Optimism (Scheier) | 0.00 | 0.02 | 0.08 | −0.04 | −0.06 | 1491 |
| | (1.00) | (0.05) | (0.07) | (0.08) | (0.08) | |
| Self-esteem (Rosenberg) | −0.00 | −0.02 | 0.19** | −0.04 | 0.04 | 1491 |
| | (1.00) | (0.05) | (0.08) | (0.08) | (0.08) | |
| Psychological well-being index | 0.00 | 0.16*** | 0.08 | −0.03 | 0.12 | 1491 |
| | (1.00) | (0.05) | (0.07) | (0.08) | (0.08) | |
| Joint test ($p$-value) | | 0.02** | 0.17 | 0.76 | 0.41 | |

*Notes:* OLS estimates of treatment effects. Outcome variables are listed on the left. The unit of observation is the individual for all variables. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the mean taken among control households in treatment villages (spillover) for a given outcome variable. Column (2) reports the treatment effect within villages, i.e. comparing treatment households to spillover households. Column (3) reports the difference in effect for households in which the primary female received the transfer in comparison to households in which the priamry male received the transfer. Column (4) reports the difference in effect for households that received monthly transfers in comparison to househods that received lump sum transfers. Column (5) reports the the difference in effect or households that received large transfers in comparison to households that received small transfers. Column (6) reports the sample size. Standard errors are clustered at the household level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The index is standardized weighted average of their (negatively coded) scores on the CES-D scale , a custom worries questionnaire (negatively coded), Cohen's stress scale (negatively coded), their response to the the World Values Survey happiness, and life satisfaction questions.

Table B.8: Treatment effects – Female Empowerment

| | (1) Control mean (SD) | (2) Treatment effect | (3) Female recipient | (4) Monthly transfer | (5) Large transfer | (6) N |
|---|---|---|---|---|---|---|
| Female empowerment index | −0.00 | 0.07 | 0.10 | 0.10 | 0.03 | 1256 |
| | (1.00) | (0.05) | (0.07) | (0.09) | (0.07) | |
| Violence index | −0.00 | 0.06 | 0.13* | 0.08 | −0.01 | 1256 |
| | (1.00) | (0.05) | (0.08) | (0.09) | (0.08) | |
| Physical violence | 0.51 | −0.03 | −0.01 | −0.06 | 0.11 | 1256 |
| | (1.16) | (0.06) | (0.09) | (0.11) | (0.10) | |
| Sexual violence | 0.14 | −0.02 | −0.08** | 0.01 | −0.04 | 1255 |
| | (0.47) | (0.02) | (0.03) | (0.04) | (0.03) | |
| Emotional violence | 0.74 | −0.03 | −0.19* | −0.14 | −0.02 | 1256 |
| | (1.19) | (0.07) | (0.10) | (0.12) | (0.11) | |
| Attitude index | −0.00 | 0.06 | 0.02 | 0.08 | 0.07 | 1256 |
| | (1.00) | (0.05) | (0.07) | (0.09) | (0.08) | |
| Male-focused attitudes index | −0.00 | 0.08 | 0.04 | −0.04 | 0.06 | 1256 |
| | (1.00) | (0.05) | (0.08) | (0.09) | (0.09) | |
| Justifiability of violence index | −0.00 | 0.06 | 0.07 | 0.00 | 0.05 | 1256 |
| | (1.00) | (0.05) | (0.08) | (0.10) | (0.08) | |
| Joint test ($p$-value) | | 0.73 | 0.20 | 0.42 | 0.44 | |

*Notes:* OLS estimates of treatment effects. Outcome variables are listed on the left. The unit of observation is the household, but we consider only the reports of the primary female. The sample is restricted to cohabiting households. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the mean taken among control households in treatment villages (spillover) for a given outcome variable. Column (2) reports the treatment effect within villages, i.e. comparing treatment households to spillover households. Column (3) reports the difference in effect for households in which the primary female received the transfer in comparison to households in which the priamry male received the transfer. Column (4) reports the difference in effect for households that received monthly transfers in comparison to househods that received lump sum transfers. Column (5) reports the the difference in effect or households that received large transfers in comparison to households that received small transfers. Column (6) reports the sample size. Standard errors are clustered at the household level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The index is a standardized weighted average of a measure of two other indices, a violence and an attitude index. The violence index is a weighted standardized average of the frequency with which the respondent reports having been physically, sexually, or emotionally abused by her husband in the preceding six months; the attitude index is a weighted standardized average of a measure of the justifiability of violence against women, and a scale of male-focused attitudes.

Table B.9: Spillover effects – Assets

| | Includes Recall HHs | | No Metal Roofs | | Original Sample | |
|---|---|---|---|---|---|---|
| | (1) Spillover effect | (2) N | (3) Spillover effect | (4) N | (5) Spillover effect | (6) N |
| Value of non-land assets excluding roof (USD) | −149.24*** | 901 | −89.58** | 755 | −55.69 | 830 |
| | (45.07) | | (42.89) | | (42.95) | |
| Value of livestock (USD) | −75.10** | 901 | −51.11 | 755 | −27.41 | 830 |
| | (29.88) | | (32.54) | | (31.12) | |
| Value of cows (USD) | −52.80** | 900 | −33.29 | 754 | −15.05 | 829 |
| | (26.14) | | (28.34) | | (27.11) | |
| Value of small livestock (USD) | −8.79 | 900 | −8.76 | 754 | −4.50 | 829 |
| | (6.60) | | (6.60) | | (6.71) | |
| Value of birds (USD) | −12.87*** | 900 | −8.46** | 755 | −7.23** | 829 |
| | (3.32) | | (3.53) | | (3.46) | |
| Value of durable goods (USD) | −67.53*** | 901 | −34.85* | 755 | −24.66 | 830 |
| | (22.71) | | (19.34) | | (18.83) | |
| Value of furniture (USD) | −24.04** | 901 | −11.23 | 755 | −8.42 | 830 |
| | (11.04) | | (10.67) | | (10.33) | |
| Value of agricultural tools (USD) | −0.73 | 901 | −0.75 | 755 | 0.01 | 830 |
| | (1.33) | | (1.33) | | (1.35) | |
| Value of radio/TV (USD) | −1.53 | 901 | 0.28 | 755 | 0.12 | 830 |
| | (1.82) | | (1.89) | | (1.72) | |
| Value of bike/motorbike (USD) | −30.59** | 901 | −17.07 | 755 | −12.90 | 830 |
| | (13.05) | | (10.30) | | (10.39) | |
| Value of appliances (USD) | −6.89** | 901 | −4.37 | 755 | −2.78 | 830 |
| | (3.14) | | (2.92) | | (2.99) | |
| Value of cell phone (USD) | −3.77* | 901 | −1.71 | 755 | −0.69 | 830 |
| | (1.99) | | (2.08) | | (2.04) | |
| Value of savings (USD) | −6.25* | 901 | −3.33 | 755 | −3.22 | 830 |
| | (3.77) | | (4.26) | | (3.95) | |
| Total land owned (acres) | 2.60 | 824 | 3.00 | 685 | 2.66 | 755 |
| | (2.44) | | (2.90) | | (2.44) | |
| Has non-thatched roof (dummy) | 0.01 | 901 | 0.03 | 755 | 0.09** | 830 |
| | (0.03) | | (0.03) | | (0.03) | |
| Joint test ($p$-value) | 0.01** | | 0.50 | | 0.34 | |

*Notes:* OLS estimates of spillover effects. Outcome variables are listed on the left. The unit of observation is the household for all variables. All variables are Winsorized at the the 99th percentile. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the estimated spillover including Recall HHs, who upgraded from a thatch to metal roof since baseline. Column (3) reports the estimated spillover effect excluding all households (in spillover and pure control) that upgraded to a metal roof since baseline. Column (5) reports the spillover effect from the endline 1 sample, in which the spillover sample included those that upgraded roofs since baseline but the pure control sample does not. Columns (2), (4) and (6) report respective sample sizes. Standard errors are clustered at the village level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table B.10: Spillover effects – Consumption

| | Includes Recall HHs | | No Metal Roofs | | Original Sample | |
|---|---|---|---|---|---|---|
| | (1) Spillover effect | (2) N | (3) Spillover effect | (4) N | (5) Spillover effect | (6) N |
| Food total (USD) | −25.28*** | 901 | −19.54** | 755 | −20.78** | 830 |
| | (7.18) | | (8.53) | | (7.98) | |
| Food own production (USD) | −5.05 | 901 | −3.24 | 755 | −3.48 | 830 |
| | (3.32) | | (3.83) | | (3.45) | |
| Food bought (USD) | −20.24*** | 901 | −16.30** | 755 | −17.30*** | 830 |
| | (5.69) | | (6.69) | | (6.38) | |
| Cereals (USD) | −4.10* | 901 | −3.56 | 755 | −4.20* | 830 |
| | (2.15) | | (2.42) | | (2.37) | |
| Meat & fish (USD) | −5.17*** | 901 | −4.33** | 755 | −4.29*** | 830 |
| | (1.50) | | (1.74) | | (1.60) | |
| Fruit & vegetables (USD) | −5.41** | 901 | −5.17** | 755 | −4.87** | 830 |
| | (2.09) | | (2.37) | | (2.34) | |
| Dairy (USD) | −2.61*** | 901 | −1.71** | 755 | −1.80** | 830 |
| | (0.75) | | (0.80) | | (0.75) | |
| Fats (USD) | −1.21** | 901 | −1.01* | 755 | −1.06** | 830 |
| | (0.50) | | (0.54) | | (0.53) | |
| Sugars (USD) | −0.65 | 901 | 0.13 | 755 | −0.21 | 830 |
| | (0.47) | | (0.54) | | (0.50) | |
| Other food (USD) | −8.92*** | 901 | −6.84** | 755 | −7.85** | 830 |
| | (2.70) | | (3.20) | | (3.03) | |
| Alcohol (USD) | −0.45 | 890 | −0.81 | 748 | −0.90 | 821 |
| | (0.76) | | (0.85) | | (0.81) | |
| Tobacco (USD) | 0.12 | 894 | 0.15 | 750 | 0.07 | 824 |
| | (0.21) | | (0.24) | | (0.22) | |
| Medical expenditure past month (USD) | −0.45 | 900 | −1.10 | 754 | 0.62 | 829 |
| | (2.63) | | (1.82) | | (2.41) | |
| Medical expenditure, children (USD) | 1.53 | 793 | −0.12 | 661 | 1.26 | 729 |
| | (1.85) | | (1.01) | | (1.88) | |
| Education expenditure (USD) | −1.50 | 899 | −0.37 | 754 | −0.09 | 828 |
| | (1.21) | | (1.23) | | (1.21) | |
| Social expenditure (USD) | −0.89 | 901 | −0.54 | 755 | −0.85 | 830 |
| | (0.68) | | (0.77) | | (0.73) | |
| Other expenditure (USD) | −9.21*** | 901 | −7.53*** | 755 | −7.78*** | 830 |
| | (2.25) | | (2.55) | | (2.48) | |
| Non-durable expenditure (USD) | −37.66*** | 901 | −29.76*** | 755 | −29.73*** | 830 |
| | (9.90) | | (11.28) | | (10.98) | |
| Joint test ($p$-value) | 0.01*** | | 0.16 | | 0.04** | |

*Notes:* OLS estimates of spillover effects. Outcome variables are listed on the left. The unit of observation is the household for all variables. All variables are Winsorized at the 99th percentile. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the estimated spillover including Recall HHs, who upgraded from a thatch to metal roof since baseline. Column (3) reports the estimated spillover effect excluding all households (in spillover and pure control) that upgraded to a metal roof since baseline. Column (5) reports the spillover effect from the endline 1 sample, in which the spillover sample included those that upgraded roofs since baseline but the pure control sample does not. Columns (2), (4) and (6) report respective sample sizes. Standard errors are clustered at the village level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table B.11: Spillover effects – Agricultural and Business Activities

| | Includes Recall HHs | | No Metal Roofs | | Original Sample | |
|---|---|---|---|---|---|---|
| | (1) Spillover effect | (2) N | (3) Spillover effect | (4) N | (5) Spillover effect | (6) N |
| Wage labor primary income (dummy) | 0.02 | 901 | 0.02 | 755 | 0.02 | 830 |
| | (0.03) | | (0.03) | | (0.03) | |
| Own farm primary income (dummy) | 0.03 | 901 | 0.05 | 755 | 0.04 | 830 |
| | (0.03) | | (0.04) | | (0.04) | |
| Non-ag business primary income (dummy) | −0.01 | 901 | −0.02 | 755 | −0.01 | 830 |
| | (0.03) | | (0.03) | | (0.03) | |
| Non-agricultural business owner (dummy) | 0.00 | 901 | 0.01 | 755 | −0.00 | 830 |
| | (0.04) | | (0.04) | | (0.04) | |
| Non-ag business revenue, monthly (USD) | −20.39 | 893 | −15.27 | 747 | −13.88 | 822 |
| | (12.52) | | (12.50) | | (12.22) | |
| Non-ag business flow expenses, monthly (USD) | −13.88** | 899 | −11.79* | 753 | −10.74 | 828 |
| | (6.43) | | (6.64) | | (6.51) | |
| Non-ag business profit imputed, monthly (USD) | −5.65 | 892 | −2.47 | 746 | −2.42 | 821 |
| | (8.68) | | (8.26) | | (8.24) | |
| Non-ag business profit self-reported, monthly (USD) | −1.97 | 890 | −1.07 | 745 | −1.57 | 819 |
| | (2.35) | | (2.51) | | (2.32) | |
| Farm revenue, monthly (USD) | −0.87 | 901 | −0.21 | 755 | −0.17 | 830 |
| | (1.22) | | (1.34) | | (1.28) | |
| Farm flow expenses, monthly (USD) | −1.12*** | 901 | −0.49 | 755 | −0.58 | 830 |
| | (0.38) | | (0.41) | | (0.39) | |
| Farm profit, monthly (USD) | 0.25 | 901 | 0.28 | 755 | 0.40 | 830 |
| | (1.09) | | (1.18) | | (1.13) | |
| Livestock flow revenue, monthly (USD) | −0.86 | 901 | −1.11 | 755 | −0.60 | 830 |
| | (1.12) | | (1.31) | | (1.25) | |
| Livestock flow expenses, monthly (USD) | −4.02*** | 901 | −2.74* | 755 | −3.19** | 830 |
| | (1.22) | | (1.39) | | (1.28) | |
| Livestock flow profit, monthly (USD) | 3.17** | 901 | 1.64 | 755 | 2.59* | 830 |
| | (1.45) | | (1.50) | | (1.47) | |
| Livestock sales and meat revenue, monthly (USD) | −4.91 | 901 | −2.62 | 755 | −3.51 | 830 |
| | (3.03) | | (3.45) | | (3.08) | |
| Total revenue, monthly (USD) | −26.84** | 901 | −19.02 | 755 | −17.92 | 830 |
| | (13.02) | | (13.45) | | (13.05) | |
| Total expenses, monthly (USD) | −19.14*** | 901 | −15.16** | 755 | −14.62** | 830 |
| | (6.65) | | (6.98) | | (6.77) | |
| Total profit, monthly (USD) | −2.22 | 901 | −0.57 | 755 | 0.60 | 830 |
| | (8.71) | | (8.49) | | (8.39) | |
| Joint test ($p$-value) | 0.00*** | | 0.22 | | 0.13 | |

*Notes:* OLS estimates of spillover effects. Outcome variables are listed on the left. The unit of observation is the household for all variables. All variables for which the unit of measurement is a monetary value are Winsorized at the 99th percentile For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the estimated spillover including Recall HHs, who upgraded from a thatch to metal roof since baseline. Column (3) reports the estimated spillover effect excluding all households (in spillover and pure control) that upgraded to a metal roof since baseline. Column (5) reports the spillover effect from the endline 1 sample, in which the spillover sample included those that upgraded roofs since baseline but the pure control sample does not. Columns (2), (4) and (6) report respective sample sizes. Standard errors are clustered at the village level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table B.12: Spillover effects – Food Security

| | Includes Recall HHs | | No Metal Roofs | | Original Sample | |
|---|---|---|---|---|---|---|
| | (1) Spillover effect | (2) N | (3) Spillover effect | (4) N | (5) Spillover effect | (6) N |
| Meals skipped (adults, # last month) | 0.61* | 901 | 0.35 | 755 | 0.45 | 830 |
| | (0.31) | | (0.36) | | (0.34) | |
| Whole days without food (adults, # last month) | 0.18 | 901 | −0.06 | 755 | 0.08 | 830 |
| | (0.18) | | (0.19) | | (0.19) | |
| Meals skipped (children, # last month) | 0.86*** | 770 | 0.78** | 644 | 0.72** | 710 |
| | (0.33) | | (0.37) | | (0.35) | |
| Whole days without food (children, # last month) | 0.25 | 770 | 0.14 | 644 | 0.19 | 710 |
| | (0.15) | | (0.16) | | (0.16) | |
| Eat less preferred/cheaper foods (# last month) | 0.63 | 901 | 0.50 | 755 | 0.55 | 830 |
| | (0.53) | | (0.56) | | (0.54) | |
| Rely on help from others for food (# last month) | 0.52** | 901 | 0.30 | 755 | 0.41 | 830 |
| | (0.25) | | (0.28) | | (0.26) | |
| Purchase food on credit (# last month) | 0.59** | 901 | 0.41 | 755 | 0.45 | 830 |
| | (0.26) | | (0.29) | | (0.27) | |
| Hunt, gather wild food, harvest prematurely (# last month) | 0.12 | 901 | −0.39 | 755 | −0.23 | 830 |
| | (0.39) | | (0.42) | | (0.43) | |
| Beg because not enough food in the house (# last month) | −0.08 | 901 | −0.13 | 755 | −0.17 | 830 |
| | (0.17) | | (0.19) | | (0.18) | |
| All members usually eat two meals (dummy) | −0.09*** | 901 | −0.05 | 755 | −0.06* | 830 |
| | (0.03) | | (0.04) | | (0.04) | |
| All members usually eat until content (dummy) | −0.13*** | 901 | −0.10** | 755 | −0.11*** | 830 |
| | (0.04) | | (0.04) | | (0.04) | |
| Number of times ate meat or fish (last week) | −0.74*** | 901 | −0.81*** | 755 | −0.79*** | 830 |
| | (0.24) | | (0.27) | | (0.27) | |
| Enough food in the house for tomorrow? (dummy) | −0.07** | 901 | −0.07** | 755 | −0.06** | 830 |
| | (0.03) | | (0.03) | | (0.03) | |
| Respondent slept hungry (last week, dummy) | 0.11*** | 901 | 0.07** | 755 | 0.08** | 830 |
| | (0.03) | | (0.04) | | (0.04) | |
| Proportion of HH who ate protein (last 24h) | −0.03 | 897 | −0.03 | 752 | −0.02 | 826 |
| | (0.03) | | (0.03) | | (0.03) | |
| Food security index (children) | −0.18** | 770 | −0.14* | 644 | −0.14* | 710 |
| | (0.07) | | (0.08) | | (0.07) | |
| Food security index | −0.32*** | 901 | −0.25*** | 755 | −0.26*** | 830 |
| | (0.08) | | (0.09) | | (0.09) | |
| Joint test ($p$-value) | 0.00*** | | 0.00*** | | 0.00*** | |

*Notes:* OLS estimates of spillover effects. Outcome variables are listed on the left. The unit of observation is the household for all variables. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the estimated spillover including Recall HHs, who upgraded from a thatch to metal roof since baseline. Column (3) reports the estimated spillover effect excluding all households (in spillover and pure control) that upgraded to a metal roof since baseline. Column (5) reports the spillover effect from the endline 1 sample, in which the spillover sample included those that upgraded roofs since baseline but the pure control sample does not. Columns (2), (4) and (6) report respective sample sizes. Standard errors are clustered at the village level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The index is a standardized weighted average of the (negatively coded) number of times household adults and children skipped meals, went whole days without food, had to eat cheaper or less preferred food, had to rely on others for food, had to purchase food on credit, had to hunt for or gather food, had to beg for food, or went to sleep hungry in the preceding week; a (negatively coded) indicator for whether the respondent went to sleep hungry in the preceding week; the (positively coded) number of times household members ate meat or fish in the preceding week; (positively coded) indicators for whether household members ate at least two meals per day, ate until content, had enough food for the next day, and whether the respondent ate protein in the last 24 hours; and the (positively coded) proportion of household members who ate protein in the last 24 hours, and proportion of children who ate protein in the last 24 hours.

Table B.13: Spillover effects – Health

| | Includes Recall HHs | | No Metal Roofs | | Original Sample | |
|---|---|---|---|---|---|---|
| | (1) Spillover effect | (2) N | (3) Spillover effect | (4) N | (5) Spillover effect | (6) N |
| Medical expenses per episode, entire HH (USD) | −0.57 | 799 | −0.56 | 679 | −0.50 | 745 |
| | (0.88) | | (1.02) | | (0.93) | |
| Medical expenses per episode, children (USD) | −0.18 | 795 | −0.15 | 663 | −0.08 | 731 |
| | (0.29) | | (0.25) | | (0.25) | |
| Proportion of household sick/injured (1 month) | 0.02 | 901 | −0.01 | 755 | −0.01 | 830 |
| | (0.02) | | (0.03) | | (0.02) | |
| Proportion of children sick/injured (1 month) | 0.01 | 795 | −0.03 | 663 | −0.02 | 731 |
| | (0.03) | | (0.03) | | (0.03) | |
| Proportion of sick/injured who could afford treatment | −0.01 | 799 | 0.01 | 679 | 0.00 | 745 |
| | (0.04) | | (0.04) | | (0.04) | |
| Average number of sick days per HH member | 0.14 | 901 | 0.05 | 755 | 0.01 | 830 |
| | (0.21) | | (0.24) | | (0.23) | |
| Propotion of illnesses where doctor was consulted | −0.03 | 799 | −0.03 | 679 | −0.03 | 745 |
| | (0.02) | | (0.02) | | (0.02) | |
| Children ¡14 had checkups (6 months) | 0.02 | 748 | −0.01 | 626 | −0.00 | 691 |
| | (0.04) | | (0.04) | | (0.04) | |
| Proportion of children ¡5 who died (1 year) | −0.01 | 569 | −0.01 | 488 | −0.01 | 536 |
| | (0.01) | | (0.02) | | (0.01) | |
| Health index (children) | 0.04 | 796 | 0.07 | 664 | 0.06 | 732 |
| | (0.08) | | (0.08) | | (0.08) | |
| Health index | −0.04 | 901 | 0.00 | 755 | 0.01 | 830 |
| | (0.07) | | (0.07) | | (0.07) | |
| Joint test ($p$-value) | 0.89 | | 0.92 | | 0.96 | |

*Notes:* OLS estimates of spillover effects. Outcome variables are listed on the left. The unit of observation is the household for all variables. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. CColumn (1) reports the estimated spillover including Recall HHs, who upgraded from a thatch to metal roof since baseline. Column (3) reports the estimated spillover effect excluding all households (in spillover and pure control) that upgraded to a metal roof since baseline. Column (5) reports the spillover effect from the endline 1 sample, in which the spillover sample included those that upgraded roofs since baseline but the pure control sample does not. Columns (2), (4) and (6) report respective sample sizes. Standard errors are clustered at the village level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The index is standardized weighted average of the (negatively coded) proportion of household adults who were sick or injured in the last month, the (negatively coded) proportion of household children who were sick or injured in the last month, the (positively coded) proportion of sick or injured family members for whom the household could afford treatment, the (positively coded) proportion of illnesses for which a doctor was consulted, the (positively coded) proportion of newborns who were vaccinated, the (positively coded) proportion of children below age 14 who received a health checkup in the preceding six months, the (negatively coded) proportion of children under 5 who died in the preceding year, and a children's anthropometrics index consisting of BMI, height-for-age, weight-for-age, and upper arm circumference relative to WHO development benchmarks.

Table B.14: Spillover effects – Education

| | Includes Recall HHs | | No Metal Roofs | | Original Sample | |
|---|---|---|---|---|---|---|
| | (1) Spillover effect | (2) N | (3) Spillover effect | (4) N | (5) Spillover effect | (6) N |
| Education expenditure past month (USD) | −21.72 | 773 | −5.53 | 643 | −1.86 | 710 |
| | (16.05) | | (16.21) | | (15.83) | |
| Education expenditure per child past month (USD) | −8.26 | 773 | −4.71 | 643 | −2.89 | 710 |
| | (5.75) | | (5.91) | | (5.81) | |
| Proportion of school-aged children in school | −0.01 | 775 | −0.01 | 644 | −0.01 | 712 |
| | (0.02) | | (0.02) | | (0.02) | |
| School days missed past month (per child) | 0.23 | 775 | 0.23 | 644 | 0.24 | 712 |
| | (0.21) | | (0.22) | | (0.21) | |
| Income-generating activities per school-age child ¿6 | −0.20*** | 704 | −0.19*** | 584 | −0.17** | 646 |
| | (0.06) | | (0.07) | | (0.07) | |
| Education index | −0.09 | 775 | −0.08 | 644 | −0.05 | 712 |
| | (0.08) | | (0.09) | | (0.08) | |
| Joint test ($p$-value) | 0.05* | | 0.09* | | 0.19 | |

*Notes:* OLS estimates of treatment effects. Outcome variables are listed on the left. The unit of observation is the household for all variables, restricting to households with schoolage children. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the estimated spillover including Recall HHs, who upgraded from a thatch to metal roof since baseline. Column (3) reports the estimated spillover effect excluding all households (in spillover and pure control) that upgraded to a metal roof since baseline. Column (5) reports the spillover effect from the endline 1 sample, in which the spillover sample included those that upgraded roofs since baseline but the pure control sample does not. Columns (2), (4) and (6) report respective sample sizes. Standard errors are clustered at the village level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The index is standardized weighted average of the proportion of household children enrolled in school and the amount spent by the household on educational expenses per child.

Table B.15: Spillover effects – Psychological Wellbeing

| | Includes Recall HHs | | No Metal Roofs | | Original Sample | |
|---|---|---|---|---|---|---|
| | (1) Spillover effect | (2) N | (3) Spillover effect | (4) N | (5) Spillover effect | (6) N |
| Depression (CESD) | 1.71*** | 1456 | 1.17** | 1227 | 1.26** | 1352 |
| | (0.56) | | (0.58) | | (0.55) | |
| Stress (Cohen) | 0.17*** | 1456 | 0.17*** | 1227 | 0.18*** | 1352 |
| | (0.06) | | (0.06) | | (0.06) | |
| Happiness (WVS) | −0.09 | 1453 | −0.08 | 1225 | −0.12* | 1349 |
| | (0.06) | | (0.07) | | (0.07) | |
| Life satisfaction (WVS) | −0.06 | 1453 | −0.03 | 1224 | −0.03 | 1349 |
| | (0.05) | | (0.06) | | (0.05) | |
| Trust (WVS) | 0.01 | 1456 | −0.04 | 1227 | −0.03 | 1352 |
| | (0.06) | | (0.06) | | (0.06) | |
| Locus of control | 0.09* | 1456 | 0.08 | 1227 | 0.06 | 1352 |
| | (0.05) | | (0.06) | | (0.06) | |
| Optimism (Scheier) | −0.10** | 1456 | −0.09 | 1227 | −0.09* | 1352 |
| | (0.05) | | (0.06) | | (0.06) | |
| Self-esteem (Rosenberg) | 0.00 | 1456 | 0.03 | 1227 | 0.04 | 1352 |
| | (0.06) | | (0.06) | | (0.06) | |
| Psychological well-being index | −0.22*** | 1456 | −0.18*** | 1227 | −0.20*** | 1352 |
| | (0.06) | | (0.07) | | (0.06) | |
| Joint test ($p$-value) | 0.00*** | | 0.02** | | 0.00*** | |

*Notes:* OLS estimates of treatment effects. Outcome variables are listed on the left. The unit of observation is the individual for all variables. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the estimated spillover including Recall HHs, who upgraded from a thatch to metal roof since baseline. Column (3) reports the estimated spillover effect excluding all households (in spillover and pure control) that upgraded to a metal roof since baseline. Column (5) reports the spillover effect from the endline 1 sample, in which the spillover sample included those that upgraded roofs since baseline but the pure control sample does not. Columns (2), (4) and (6) report respective sample sizes. Standard errors are clustered at the village level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The index is standardized weighted average of their (negatively coded) scores on the CES-D scale , a custom worries questionnaire (negatively coded), Cohen's stress scale (negatively coded), their response to the the World Values Survey happiness, and life satisfaction questions.

Table B.16: Spillover effects – Female Empowerment

| | Includes Recall HHs | | No Metal Roofs | | Original Sample | |
|---|---|---|---|---|---|---|
| | (1) Spillover effect | (2) N | (3) Spillover effect | (4) N | (5) Spillover effect | (6) N |
| Female empowerment index | 0.17* | 647 | 0.16* | 546 | 0.17* | 604 |
| | (0.09) | | (0.09) | | (0.09) | |
| Violence index | 0.15* | 647 | 0.12 | 546 | 0.15 | 604 |
| | (0.09) | | (0.10) | | (0.09) | |
| Physical violence | −0.15 | 647 | −0.15 | 546 | −0.19 | 604 |
| | (0.13) | | (0.14) | | (0.13) | |
| Sexual violence | −0.01 | 647 | 0.00 | 546 | −0.00 | 604 |
| | (0.05) | | (0.05) | | (0.05) | |
| Emotional violence | −0.28** | 647 | −0.21* | 546 | −0.25** | 604 |
| | (0.11) | | (0.12) | | (0.12) | |
| Attitude index | 0.10 | 647 | 0.12 | 546 | 0.11 | 604 |
| | (0.09) | | (0.10) | | (0.09) | |
| Male-focused attitudes index | 0.07 | 647 | 0.11 | 546 | 0.08 | 604 |
| | (0.09) | | (0.10) | | (0.10) | |
| Justifiability of violence index | 0.10 | 647 | 0.08 | 546 | 0.09 | 604 |
| | (0.09) | | (0.10) | | (0.09) | |
| Joint test ($p$-value) | 0.22 | | 0.45 | | 0.23 | |

*Notes:* OLS estimates of treatment effects. Outcome variables are listed on the left. The unit of observation is the household, but we consider only the reports of the primary female. The sample is restricted to cohabiting households. For each outcome variable, we report the coefficient of interest and its standard error in parentheses. Column (1) reports the estimated spillover including Recall HHs, who upgraded from a thatch to metal roof since baseline. Column (3) reports the estimated spillover effect excluding all households (in spillover and pure control) that upgraded to a metal roof since baseline. Column (5) reports the spillover effect from the endline 1 sample, in which the spillover sample included those that upgraded roofs since baseline but the pure control sample does not. Columns (2), (4) and (6) report respective sample sizes. Standard errors are clustered at the village level. The last row shows joint significance of the coefficients in the corresponding column from SUR estimation. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. The index is a standardized weighted average of a measure of two other indices, a violence and an attitude index. The violence index is a weighted standardized average of the frequency with which the respondent reports having been physically, sexually, or emotionally abused by her husband in the preceding six months; the attitude index is a weighted standardized average of a measure of the justifiability of violence against women, and a scale of male-focused attitudes.