Policy Brief: Impacts of Unconditional Cash Transfers*

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Direct transfers are a common approach to poverty reduction. Historically, such programs have often made in-kind transfers, as in the case of food aid, asset transfers (e.g. livestock), or the provision of medical care.[1] In recent years, cash transfers have garnered attention as a potential alternative poverty alleviation strategy, and cash transfer programs in developing countries now reach up to a billion people.[2] However, the majority of these transfers are conditional, or targeted at specific groups of individuals.[3] Truly unconditional cash transfers to general populations are relatively less common and less well understood, both in terms of their impacts, and in terms of how their design affects these impacts.

We conducted a randomized controlled trial (RCT) of the unconditional cash transfer program implemented by the NGO *GiveDirectly* in Western Kenya between 2011 and 2012, in which poor rural households received unconditional cash transfers through the mobile money system *M-Pesa*. Importantly, *GiveDirectly* delivered cash transfers to a general population sample, and without stipulations; households were not chosen for membership in any particular group, such as business owners or lending groups, but simply for meeting a basic eligibility criterion, i.e., living in a thatched-roof house. In addition, recipients were explicitly told that they were free to spend the transfer

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[1] For instance, the 2014 US budget allocates $1.8 billion for international food aid (http://www.usaid.gov/foodaidreform), and food transfers are frequently employed as a form of public welfare assistance in developing countries. India, for example, recently passed legislation which would increase the cost of food subsidy expenditures to approximately $20bn (Wall Street Journal, 2013).

[2] DfID, 2011. http://r4d.dfid.gov.uk/PDF/Articles/cash-transfers-literature-review.pdf

[3] For instance, they may make make transfers only to women (e.g. Ecuador's *Bono de desarollo*) or require group applications and the development of business plans (e.g. Uganda's *Youth Opportunities Program*).

as they wished, without suggesting, explicitly or implicitly, that transfers should be spent in a particular fashion. We randomized three design features of unconditional cash transfers: whether the transfer recipient is the husband or the wife within the household, whether the transfer was made in a single a lump sum, or in nine monthly installments, and the size of the transfer (either $300 or $1,100). We measure a broad array of outcomes and behaviors, including assets, consumption, income, enterprise activity, food security, health, education, female empowerment, psychological well-being, and the stress hormone cortisol. Data analysis followed a pre-analysis plan registered at www.socialscienceregistry.org. These and further results will be the subject of a forthcoming paper by the authors.

Results summary

- **Transfers allow poor households to build assets.** Recipients increased asset holdings by PPP USD 279, representing a 58% increase over the control group mean, and 39% of the average amount transferred. These increases occurred primarily through home improvements and increased livestock holdings: households receiving transfers are 23 percentage points more likely to have an iron roof as opposed to a grass-thatch roof, and livestock holdings increase by 51% (PPP USD 85).

- **Transfers increase consumption.** Recipients spend cash transfers on a very broad variety of goods and services, including food, healthcare, education, and social or family events such as weddings and funerals. We observe particularly strong increases in spending on food, medical, and social expenses.

- **Transfers reduce hunger.** With an increase in food consumption by 20%, we observe significant reductions in hunger and food insecurity, e.g. a 30% reduction in the likelihood of the respondent having gone to bed hungry in the preceding week, and a 42% reduction in the number of days children go without food.

- **Transfers do not increase spending on alcohol and tobacco.** We find no evidence of increased expenditure on temptation goods such as alcohol, tobacco and gambling.

- **Transfers increase investment in and revenue from livestock and small businesses.** Revenue from animal husbandry increases by 48% (PPP USD 2 per month), and total revenue from self-employment increases by PPP USD 11 per month (38%) as a result of the transfers. Existing evidence on the effect of cash transfers on income comes from programs that were specifically targeted at existing or new non-agricultural businesses, often with the explicit or implicit expectation that these transfers should be invested in the enterprise (De Mel, McKenzie, and Woodruff 2008; Fafchamps et al. 2011; Blattman, Fiala, and Martinez 2013); the results from this study suggest that these results may extend to a broader population.

- **Transfers increase psychological well-being of recipients and their families.** Unconditional cash transfers lead to a 0.18 SD increase in happiness, a 0.15 SD increase in life satisfaction, and a 0.14 SD reduction in stress, all measured by psychological questionnaires. Large transfers lead to a reduction in levels of the stress hormone cortisol.

- **Transfers affect many, but not all, indicators of poverty.** We find little to no impact on health or education over the time horizon considered in the data. We find suggestive evidence that cash transfers reduce domestic violence and increase female empowerment in both recipient households and other households in the same village.

- **Specific design features of cash transfer programs differentially affect impacts and imply policy trade-offs.** Monthly transfers have stronger effects on food security than lump-sum transfers, while lump-sum transfers show larger effects than monthly transfers on particular types of assets such as metal roofs. Large transfers produce larger treatment effects than small transfers on most outcomes, but with decreasing marginal returns. We do not observe significant differences in outcomes when making transfers to the female vs. the male in the household. Together, these results suggest that when policy-makers consider different design choices for cash transfers, they may come to different conclusions depending on how they weight different potential outcomes relative to one another.

Attachment: Study Design and Detailed Results

1 The *GiveDirectly* Unconditional Cash Transfer Program

GiveDirectly, Inc. (GD; www.givedirectly.org) is an international NGO founded in 2010, whose mission is to make unconditional cash transfers to poor households in developing countries. We note that Jeremy Shapiro, an author of this study, is a co-founder and former Director of *GiveDirectly* (2009–2012). It began operations in Kenya in 2011 (Goldstein 2013). It selects poor households by first identifying poor regions of Kenya according to census data. In the case of the present study, the region chosen was Rarieda, a peninsula in Lake Victoria west of Kisumu in Western Kenya. Following the choice of a region in which to operate, *GD* identifies target villages. In the case of Rarieda, this was achieved through a rough estimation of the population of villages and the proportion of households lacking a solid roof; villages with a high proportion of households living in thatched roof homes (rather than iron), which is *GD's* targeting criterion, were prioritized. The criterion was established by *GD* in prior work as an objective and highly predictive indicator of poverty. In each chosen village, *GD* conducted a census, usually with the help of the village elder, which identified all households in the village that met this targeting criterion. Among the eligible households, treatment households were chosen randomly (details are described in Section 2). Households were aware that recipients would be chosen by lottery, but the actual selection was done privately by means of random number generation.

Each selected household was then visited by a representative of *GD*. The *GD* representative asked to speak to the member of the household that had been chosen as the transfer recipient *ex ante* (for the purposes of the present study, the recipient was randomly chosen to be either the husband or the wife, with equal probability; details in Section 2). A conversation in private was then requested from this household member, in which they were asked a few questions about demographics, and informed that they had been chosen to receive a cash transfer of KES 24,000 (USD 274, PPP 384). The recipient was informed that this transfer came without strings attached, that they were free to spend it however they chose, that any future transfers from GD were not contingent on any particular use to which they might put the money, and that GD would not engage in any monitoring of how they spent then transfer.

Recipients were also informed about the timing of this transfer; for the purposes of the present study, 50% of recipients were told that they would receive the transfer as one lump-sum payment, and the remaining 50% were told that they would receive the transfer as a stream of nine monthly installments. The timing of the transfer delivery was also announced. In the case of monthly transfers, the first installment was transferred on the first of the month following the initial visit, and continued for eight months thereafter. In the case of lump-sum transfers, a month was randomly chosen among the nine months following the date of the initial visit. The purpose of this randomization in timing was to ensure that the net present value of the announced transfers was identical across the lump-sum and monthly conditions.

For receipt of the transfer, recipients were provided with a SIM card by Kenya's largest mobile service provider, *Safaricom*, and asked to activate it and register for *Safaricom's* mobile money service *M-Pesa* (Jack and Suri 2010). *M-Pesa* is, in essence, a bank account on the SIM card, protected by a four-digit PIN code, and enables the holder to send and receive money to and from other *M-Pesa* clients. Prior to receiving any transfer, recipients were required to register for *M-Pesa*. For lump sum recipients, a small initial transfer of KES 1,200 was sent on the first of the month following the initial *GD* visit as an incentive for prompt registration. Registration had to occur in the name of the designated transfer recipient, rather than any other person. The *M-Pesa* system allows *GD* to observe the name in which the account is registered in advance of the transfer, and transfers were not sent unless the registered name had been confirmed to match the intended recipient within the household. In our sample, all treatment households complied with these instructions[4] Transfers commenced on the first of the month following registration. Each transfer was announced with a text message to the recipient's SIM delivered through the *M-Pesa* system. However, receipt of these text messages was not necessary to ensure the receipt of transfers; recipients who did not own cell phones could rely on the information about 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 any incoming transfers. To facilitate easier communication with recipients and reliable 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).

Withdrawals and deposits can be made at any *M-Pesa* agent, of which *Safaricom* operates about 11,000 throughout Kenya. Typically an *M-Pesa* agent is a shopkeeper in the recipients' village or the nearest town (other types of businesses that operate as *M-Pesa* agents are petrol stations, supermarkets, courier companies, "cyber" cafes, retail outlets, and banks). *GD* estimates the average travel time and cost from recipient households to the nearest *M-Pesa* agent at 42 minutes and $0.64. Withdrawals incur costs between 27% for KES 100 withdrawals and 0.06% for KES 50,000 withdrawals, with a gradual decrease of the percentage for intermediate amounts.[5] *GD* reports that recipients typically withdraw the entire balance of the transfer upon receipt.

The sender also incurs costs for *M-Pesa* transfers; according to *GD*'s estimates, the costs of transferring money to recipients in this fashion amount to 1.5% of the transfer amount for foreign exchange fees, and 1.6% for *M-Pesa* fees. Together with 4.8% of transfers spent on recipient identification and staff costs, GD estimates that 92.1% of the donations it receives are transferred to recipients' *M-Pesa* accounts.

[4]In a minority of cases, delays in registration occurred due to delays in obtaining an official identification card, which is a prerequisite for registering with *M-Pesa*.

[5]As a result of the Kenyan Finance Act of 2012, which introduced a 10% excise duty tax on transaction fees for all money transfer services provided by cellular phone providers, banks, money transfer agencies and other financial service providers, Safaricom revised the cost structure for sending and receiving money through *M-Pesa*. The costs for transfers over KES 100 increased by 10%, while fees remained the same for smaller transfers (http://www.safaricom.co.ke/personal/m-pesa/m-pesa-services-tariffs/tariffs/tariff-faqs). However, these changes did not take effect until February 8, 2013, by which time the endline survey for this study had already been concluded. Our results are therefore unlikely to be affected by this new cost structure.

2 Evaluation Design

Sample selection This study employs a two-level cluster-randomized controlled trial. An overview of the design is shown in Figure 1. In collaboration with *GD*, we identified 126 villages from a list of villages in Rarieda district of Western Kenya. In the first stage of randomization, 63 of these villages were randomly chosen to be treatment villages. Within all villages, we conducted a census with the support of the village elder, which identified all eligible households within the village. As described above, eligibility was based on living in a house with a thatch roof. Control villages were only surveyed at endline; in these villages, we sampled 432 households from among eligible households, to which we refer as "pure control" households in the following.

In treatment villages, we performed a second stage of randomization, in which we randomly assigned 50% of the eligible households in each treatment village to the treatment condition, and 50% to the control condition. This process resulted in 503 treatment households at baseline, and 505 control households in treatment villages, to which we refer as "spillover" households in the following. Due primarily to registration issues with *M-Pesa* 18 treatment households had not received transfers at the time of the endline, thus only 485 of the treatment households had in fact received transfers. In the analysis below we use an intent-to-treat approach, and consider all households assigned to receive a transfer as the treatment group, regardless of whether they had received a transfer at the time of the endline survey. Our design also allows us to identify spillover effects.

Due to the fact that the pure control households were selected into the sample just before the endline, the thatched roof criterion was applied to them 12 months later than to households in treatment villages. This fact potentially introduces bias into the comparison of households in treatment and control villages; in the absence of transfers, a proportion of households in treatment villages that had a thatched roof at baseline might have purchased an iron roof independently of the transfers and thus are not comparable to homes with thatched roofs in pure control villages at endline. We therefore focus on the within-village treatment effect when reporting results; in the presence of positive spillovers, this is a conservative estimate of the treatment effect.[6]

To obtain a lower-bound estimate for spillover effects, we compare households which still have thatched roofs at endline to pure control households which still have thatched roofs at endline. The logic behind this choice is the following. First, note that in the absence of spillover effects on roof purchases, this comparison provides an unbiased estimate of the spillover effects for this group of households. Second, relax the assumption of no spillovers and assume instead (as is likely) that spillover effects predominantly induce the better-off control households in treatment villages to upgrade to an iron roof. If this is the case, restricting the sample to households which still have a thatched roof at endline selects for poorer households in treatment villages, but not pure control

[6] Note that this strategy would overestimate the treatment effect in the presence of negative spillovers. However, we find little evidence for negative spillovers, as discussed below; this includes psychological well-being, i.e. untreated households in treatment villages did not experience a decrease in psychological well-being. We thus believe that the within-village treatment effects are a conservative estimate.

villages, and thus provides a lower bound estimate of the spillover effect. To be conservative, in what follows we report this lower-bound estimate.

Treatment arms A goal of this study was to assess the relative welfare impacts of three design features of unconditional cash transfers: the gender of the transfer recipient; the temporal structure of the transfers (monthly vs. lump-sum transfers); and the magnitude of the transfer. The intervention was therefore structured as follows:

1. **Transfers to the woman vs. the man in the household.** Among households with both a primary female and primary male member, we stratified on recipient gender and randomly assigned the woman or the man to be the transfer recipient in an equal number of households. A further 110 households had a single household head and were therefore 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, 244 of the 503 treatment households were assigned to the monthly condition, and 256 to the lump-sum condition. The total amount of each type of transfer was KES 25,200 (USD 287, PPP 404[7]). This amount includes an initial transfer of KES 1,200 (USD 14, PPP 19) to incentivize *M-Pesa* registration, followed by either a lump-sum payment of KES 24,000 (USD 274, PPP 384) in the lump-sum condition, or a sequence of nine monthly transfers of KES 2,800 (USD 32, PPP 45) in the monthly condition. 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 an initial transfer of KSH 1,200 on the first of the month following *M-Pesa* registration to incentivize 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. This procedure ensured that the monthly and lump-sum transfers had the same net present value.

3. **Large vs. small transfers.** Finally, a third treatment arm 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 798, PPP 1,112), paid in seven monthly installments of KES 10,000 (USD 114, PPP 160) each, beginning in February 2012. Thus, the transfers previously assigned to these households, whether monthly or lump-sum, were augmented by

[7] The KES to USD conversion rate used is the rate as of the half-way date between the first and last transfers. PPP conversion rate used is the 2012 World Bank estimate for private consumption in Kenya.

KES 10,000 from February 2012 to August 2012[8], and therefore the total transfer amount received by these households was KES 95,200 (USD 1,085, PPP 1,525). The remaining 348 treatment households constitute the "small" transfer group, and received transfers totaling KES 25,200 (USD 287, PPP 404) 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. Section 3.1 outlines how this issue is dealt with in the analysis.

Timeline The timeline of the study is outlined in Figure 2. Baseline surveys took place between May and November 2011. Endline surveys took place with September and December 31, 2012. Transfers were made between June 2011 and January 2013. Monthly transfers were made in nine monthly installments of KES 2,800 (USD 32, PPP 45), and lump sum transfers were made all at once, in one of nine randomly selected monthly bins. Thus, the transfers were timed so that the total lump sum transfers in a given month is the same as the amount of monthly transfers given that month, and thus the net present value of transfers is identical for the monthly and lump-sum groups. For the large transfer group, an additional transfer of KES 70,000 (USD 798, PPP 1,121) was issued in seven monthly installments.

Data collection methods and outcome measures In treatment villages, we surveyed treatment and control households both at baseline and endline; in control villages, we surveyed "pure control" households at endline only. In each surveyed household, we collected two distinct modules: a household module, which collected information about assets, consumption, income, food security, health, and education, administered to either the primary male or female member of the household; and an individual module, which collected information about psychological well-being, intrahousehold bargaining and domestic violence, and preferences. From a randomly selected subset of respondents in each village, we also obtained village-level information about prices, wages, and crime, to assess general equilibrium effects of the intervention (Angelucci and De Giorgi 2009). The entire questionnaire is available from the authors upon request. In addition, in the individual survey, we collected two saliva samples from each respondent, one at the beginning of the survey, and one at the end; these were later assayed for cortisol. Finally, we measured the height, weight, and upper-arm circumference of the children under 5 years who lived in the household.

The two surveys were administered on different (usually subsequent) days. The household survey was administered to any household member who could give information about the outcomes in

[8]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; details follow in Section 3.

question for the entire household; this was usually one of the primary members. The individual survey was administered to both primary members of the household, i.e. husband and wife, for double-headed households; and to the single household head otherwise. During individual surveys, particular care was taken to ensure privacy; respondents were interviewed by themselves without the interference of other household members, in particular the spouse. All interviews were conducted by trained interviewers, and informed consent was obtained by signature or thumbprint from all respondents. Surveys were administered on *Netbooks* using the *Blaise* survey software. We performed backchecks consisting of 10% of the survey, with a focus on non-changing information, on 10% of all interviews. This procedure was known to field officers *ex ante*.

Pre-analysis plan To constrain our analysis, we wrote a pre-analysis plan (PAP) for this study, which is published at www.socialscienceregistry.org. Pre-analysis plans have recently gained prominence in economics as tools to prevent data-mining and cherry-picking of results ((Casey, Glennerster, and Miguel 2012); see also (Rosenthal 1979; Simes 1986; Horton and Smith 1999)). In the PAP, we specify the variables to be analyzed, the construction of indices, our approach to dealing with multiple inference, the econometric specifications to be used, and the handling of attrition. The analyses and results reported in this paper correspond to those outlined in the PAP, with the exception of the restriction of the sample to thatched-roof households at endline when identifying spillover effects to account for the time delay in applying the thatched roof criterion to the pure control group. However, this restriction is conservative.

Accounting for multiple comparisons Due to the large number of outcome variables in the present study, false positives are a potential concern when conventional approaches to statistical inference are used. We employ two strategies to avoid this problem, following broadly the approaches of Kling et al. (2007), Anderson (2008), and Casey et al. (2012).

First, we compute standardized indices for several main groups of outcomes, and choose focal variables of interest for others. In particular, we use the total value of household assets, total household consumption in the past month, and total household agricultural and business income in the past month, as focal variables for the asset, consumption, and income outcome groups, respectively. For psychological well-being, food security, female empowerment, health, and education, we compute indices, which are standardized weighted averages of several key outcomes of interest within each of these groups of outcomes. The particular outcomes composing each index and the focal variables were pre-specified in our pre-analysis plan.

Second, even after collapsing variables into indices and choosing focal variables of interest for each group of outcomes, we are still left with multiple indices, creating the need to further control the probability of Type I errors. To this end, we use the Family-Wise Error Rate (FWER; Westfall, Young, and Wright 1993; Efron and Tibshirani 1993; Anderson 2008; Casey, Glennerster, and Miguel 2012), which controls the probability of Type I errors across a group of coefficients. In our case, we control the FWER across the treatment coefficients on the indices for our main outcome groups, i.e. assets, consumption, income, psychological well-being, education, food security, health, and intrahousehold bargaining and domestic violence. As specified in our pre-analysis plan, we apply this correction to the index variables only; when discussing the results within particular outcome groups, we use conventional significance levels.

3 Results

3.1 Index variables

Treatment effects In the following we discuss the basic treatment and spillover effects on the indices summarizing the variables in our eight outcome groups. Variables reflecting monetary outcomes were top-coded at 99% to eliminate outliers.

1. Total assets: the value, in USD PPP, of all moveable assets the household owns, including savings, plus the value of the roof of the home.

2. Total consumption: the amount of money, in USD PPP, spent on goods and services in the past month, plus the value, in USD PPP, of consumption from own production in the past month.

3. Food security index: weighted standardized average of dummy variables asking whether households skipped meals, went whole days without food, went to bed hungry, etc.

4. Agricultural and business income: the value, in USD PPP, of agricultural goods produced in the past month, plus the revenue, in USD PPP, of non-agricultural enterprises in the past month.

5. Health index: weighted standardized average of frequency of illness, dummies for visiting the doctor when ill, being able to afford treatment, vaccinations and checkups for children, under-5 mortality, and an anthropometrics index composed of body-mass index, height-for-age, weight-for-age, and upper-arm circumference of children under 5.

6. Education index: weighted standardized average of proportion of school-aged children in school and monthly education expenditure per child.

7. Psychological well-being index: weighted standardized average of scores on the CESD depression questionnaire (Radloff 1977), a custom worries questionnaire, Cohen's Perceived Stress Scale (Cohen, Kamarck, and Mermelstein 1983), the happiness and life satisfaction variables from the World Values Survey (Stevenson and Wolfers 2008), and levels of the stress hormone cortisol. This index, and all variables contained in it, are measured at the individual level.

8. Female empowerment index: weighted standardized average of violence and attitude indices. The violence index is a weighted standardized average of dummy variables indicating the occurrence of different types of physical, sexual, and emotional violence by the husband against the wife in the preceding six months, as reported by the wife. The attitude index is a weighted standardized average of dummy variables indicating the justifiability of violence within the marriage, and of dummy variables indicating agreement with statements arguing that decision-making power within the household should rest with the male, again as reported by the wife.

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

$$y_{\{i\}hvE} = \alpha_v + \beta_0 + \beta_1 T_{hv} + \delta_1 y_{\{i\}hB} + \delta_2 \mathbb{1}(y_{\{i\}hB} = \text{missing}) + \varepsilon_{\{i\}hvE} \tag{1}$$

where $y_{\{i\}hvE}$ is the outcome of interest for household h in village v, measured at endline ($t = E$); 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 and control households in treatment villages, as explained above. Village-level fixed effects are captured by α_v. T_{hv} is a treatment indicator that takes value 1 for treatment households, and 0 otherwise. $\varepsilon_{\{i\}hvt}$ is an idiosyncratic error term. The omitted category is control households in treatment villages; thus, β_1 identifies the treatment effect for treated households relative to control households in treatment villages. Following (McKenzie 2012), we condition on the baseline level of the outcome variable, $y_{\{i\}hB}$, to improve statistical power. To include observations where the baseline outcome is missing, we code missing values as zero and include a dummy indicator that the variable is missing. Standard errors are clustered at the level of the unit of randomization, i.e. the household. In addition to this standard inference, we compute FWER-corrected p-values across the set of index variables. Finally, we estimate the system of equations jointly using seemingly unrelated regression (SUR), which allows us to perform Wald tests of joint significance of the treatment coefficient.

Spillover effects are identified as follows (Duflo and Saez 2003):

$$y_{\{i\}hvE} = \beta_0 + \beta_1 S_{hv} + \varepsilon_{\{i\}hvE} \tag{2}$$

Here, S_{hv} is a dummy variable that takes value 1 for control households in treatment villages ("spillover households") and 0 otherwise. In this specification, we restrict to households with a thatched roof at endline to account for the delay in which the selection criteria was applied to the pure control village (discussed above). Thus, β_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 level and within-village (household) level (Cameron, Gelbach, and Miller 2011; Pepper 2002). Note that the inclusion of baseline covariates is not feasible here because no baseline data exists for the pure control group. Similarly, village-level fixed effects are not feasible because they would be collinear with S_{hv}.

Table 1 displays the mean and standard deviation of each index variable in the within-village control group in column (1), and treatment and spillover effects for these variables in columns (2) and (3), estimated using equations 1 and 2. Standard errors are shown in parentheses, and the bootstrapped FWER p-value in brackets. The last row of the table reports the joint significance of all coefficients in the corresponding column, using seemingly unrelated regression (SUR).

We find statistically and economically meaningful impacts of cash transfers across the majority of outcomes measured by our indices, including assets, consumption, food security, revenue from self-employment and psychological well-being. Overall, the joint significance of the treatment effects across outcomes has a p-value of less than 0.001. Specifically, we observe an increase in the value of assets by PPP USD 279 on average; this represents 58% of the control group mean PPP USD 478, and 39% of the average transfer.[9] The effect is statistically different from zero at the 1% confidence level according to both standard and FWER-corrected p-values. Similarly, the transfers increase the treatment group's average monthly consumption by PPP USD 36 at endline, which corresponds to 23% of average consumption among control households. The effect is again statistically significant at the 1% confidence level according to both standard and FWER p-values. For monthly agricultural and business income, the point estimate on the treatment effect shows a PPP USD 15 increase. This is an increase of 33% over the control group mean, and on an annual basis, it represents 26% of the average transfer amount. The effect is statistically significant using both standard and FWER-corrected p-values. In addition, we find substantial improvements in food security, with a statistically significant 0.25 SD increase in the food security index among treatment households. However, we do not observe changes in other measures which may be related to increased nutrition and overall consumption, namely health and education.

In addition to meaningful increases in common measures of economic welfare, we find an impact on psychological well-being resulting from the receipt of a cash transfer; individuals in the treatment group score 0.20 SD higher on our index of psychological well-being than individuals in the control group. This effect is statistically significant above the 5% level according to both standard and FWER-adjusted p-values.

Column (3) in Table 1 reports the coefficients on the spillover dummies. These are generally small and not significant, with one notable exception: we observe an increase of 0.23 SD in the female empowerment index among the control group in treatment villages, significant at the 5% level using conventional p-values and at the 10% level using FWER-corrected p-values. Together with a non-significant direct treatment effect of SD -0.01 on this measure, this spillover effect implies that the treatment group show a treatment effect relative to the pure control group.

Effects of treatment arms We now discuss each of the three sub-treatments in turn: transfers to the primary female vs. the primary male in the household; monthly vs. lump-sum transfers; and

[9] 28% of the treatment group received a transfer of KES 95,200 (USD 1,085, PPP 1,525), while the remaining 72% received KES 25,200 (USD 287, PPP 404); the average transfer was thus KES 45,016 (USD 513, PPP 721).

large vs. small transfers. First, the effect of making the transfer to the female vs. the male in the household is captured by the following model:

$$y_{\{i\}hvE} = \alpha_v + \beta_0 + \beta_1 T^{\mathrm{F}}_{hv} + \beta_2 T^{\mathrm{W}}_{hv} + \delta_1 y_{\{i\}hB} + \delta_2 \mathbb{1}(y_{\{i\}hB} = \text{missing}) + \varepsilon_{\{i\}hvE} \tag{3}$$

In this specification, the sample is restricted to households in treatment villages. The variables T^{x}_{hv} are indicator functions that specify the branch of the different treatment arms. Specifically, they indicate whether the transfer recipient is female (T^{F}_{hv}), male (T^{M}_{hv}; omitted category), or that the gender of the recipient could not be randomized because the household only had one head (T^{W}_{hv}; most commonly in the case of widows/widowers). Since the omitted category is male transfer recipients, the coefficient on T^{F}_{hv} identifies the difference in treatment effects between female and male recipient households. Village fixed effects are captured by α_v, and standard errors are clustered at the household level. Note that spillover effects cannot be identified separately for the sub-treatments since these were randomized within villages and there was little variation in the intensity of each arm of a given sub-treatment across villages.

Column (4) in Table 1 reports the coefficients and standard errors comparing female to male recipient households on the index variables. With the exception of psychological well-being, which is significant at the 10% level, none of the differences between the treatment effects for transfers to the female vs. the male are statistically significant at conventional significance levels. Thus we find little evidence that providing cash transfers to women vs. men differentially affect outcomes. However, we note a trend in the point estimates suggesting that transferring cash to the primary male in the household leads to a larger impact on standard measures of economic welfare, namely assets and consumption, while transferring cash to the primary female in the household improves outcomes most likely to benefit children, i.e. food security, health, and education, as well as psychological well-being and female empowerment.

To asses the effect of monthly vs. lump-sum transfers, recall first that a subset of households originally assigned to receive KES 25,200 (USD 278, PPP 404) in either lump-sum or monthly transfers was randomly assigned to receive additional monthly transfers beginning in February 2012 to achieve a total transfer of KES 95,200 (USD 1085, PPP 1,525). Households in this category which had previously been assigned the lump-sum condition can therefore not be unambiguously assigned to the "lump sum" or "monthly" conditions. To control for this ambiguity, the regression comparing lump-sum and monthly transfers is estimated only for the groups which did not receive the large transfers, as follows:

$$y_{\{i\}hvE} = \alpha_v + \beta_0 + \beta_1 T^{\mathrm{MTH}}_{hv} \times T^{\mathrm{S}}_{hv} + \beta_2 T^{\mathrm{L}}_{hv} + \delta_1 y_{\{i\}hB} + \delta_2 \mathbb{1}(y_{\{i\}hB} = \text{missing}) + \varepsilon_{\{i\}hvE} \tag{4}$$

In this specification, T^{MTH}_{hv} is an indicator variable for having *originally* been assigned to receiving monthly transfers, and T^{S}_{hv} and T^{L}_{hv} are indicators for *later* being randomly assigned to receive the smaller vs. the larger of the two transfer amounts, respectively. The sample is again restricted to households in treatment villages. The omitted category is and $T_{hv}^{\mathrm{LS}} \times T_{hv}^{S}$, i.e an indicator variable for having *originally* been assigned to receiving small lump-sum transfers; β_1 thus identifies the difference in the treatment effect between monthly and lump-sum transfers for recipients of small transfers.

Results are shown in column (5) of Table 1. The joint significance across outcomes is at $p < 0.05$, suggesting that monthly and lump-sum transfers have significantly different effects on our outcomes. In the individual variables, we find that monthly payments increase food security by 0.26 SD relative to lump-sum payments, which is statistically different from zero, and some indication that lump sum transfers lead to higher asset values.

Finally, to assess the effect of receiving large compared to small transfers, we use the following specification:

$$y_{\{i\}hvE} = \alpha_v + \beta_0 + \beta_1 T_{hv}^{\mathrm{L}} + \delta_1 y_{\{i\}hB} + \delta_2 \mathbb{1}(y_{\{i\}hB} = \text{missing}) + \varepsilon_{\{i\}hvE} \tag{5}$$

Again the sample is restricted to households in treatment villages. The omitted category is T_{hv}^{S}, i.e. receiving small transfers; β_1 thus identifies the difference in outcomes for large compared to small transfers. Results are shown in column (6) of Table 1. We find large and highly significant differences between large and small transfers, all in the direction of "better" outcomes for large transfers. The joint significance across outcome has a p-value of less than 0.001. Regarding individual outcomes, most prominently, the increase in asset holdings resulting from the large transfer is approximately twice as large as that for the small transfer. The differences between the subgroups on these outcomes are statistically significant in terms of both conventional and FWER-adjusted p-values. In addition, we find that larger transfers improve the psychological well-being of household members to a greater extent than small transfers; this difference is also significant in terms of both standard and FWER-adjusted p-values. Finally, we observe an additional increase in female empowerment for large transfers, significant at the 5% level using conventional p-values, but not FWER-corrected inference.

Distributional effects In addition to average effects of treatments, we are concerned with the distributional impact of cash transfers. In particular, we consider whether the average impacts described above are the result of shifting particular portions of the distribution of that outcome, and, where no average impact was observed, whether the lack of an average impact may mask shifts in specific portions of the distribution for outcomes. To this end, we run quantile regressions for the outcomes of interest. In particular, we estimate the parameter β_q that minimizes the following expression:

$$\sum_{i:y_{\{i\}hv} \geqq T_{hv}\beta_q} q|y_{\{i\}hv} - T_{hv}\beta_q| + \sum_{i:y_{\{i\}hv} \leqq T_{hv}\beta_q} (1-q)|y_{\{i\}hv} - T_{hv}\beta_q| \tag{6}$$

In estimating β_q we again restrict the sample to treatment and control households within treatment villages. The parameter β_q thus estimates the within-village treatment effect on quantile q of the distribution. In the results below, we present results for each decile in the outcome distribution.

These results are shown in Figure 3, where we plot the parameter estimates for all deciles and their 95% confidence intervals. We note three patterns. First, the plots for assets, consumption, and cash flows from self-employment are strongly upward-sloping, suggesting that the treatment effects on these outcomes are strongest for wealthier households. Second, the plots for food security and psychological well-being show a treatment effect throughout the distribution, suggesting that cash transfers impact households at all levels of those particular measures of welfare. Finally, the plots for health, education, and female empowerment show no treatment effects anywhere in the distribution.

Temporal dynamics Our data includes a moderate degree of temporal variation in the end date of the transfers, enabling us to ask whether the treatment effects outlasted the period during which households received transfers. We stress, however, that the current study was not designed to investigate long-term effects; further endline surveys will be required to obtain a more complete understanding of long term impacts.

We begin by creating separate indicators for the transfer having been completed a specific number of month before the endline survey. In doing so, we allow the temporal dynamics to vary based on whether the household received a lump or monthly transfer, since we might expect that the impacts of the large lump-sum transfers to unfold differently over time than that of the smaller monthly transfers. Further, since there is limited variation in the time since the end of the transfer for households receiving large transfers, we restrict the sample to households receiving a small transfer.

We then create indicator variables for time elapsed since the end of transfers. Specifically, we first define a dummy for households that receive transfers contemporaneously, i.e. within the last month. Second, we perform a median split on the delay since the last transfer for the remaining households, which results in one group of households which received their last transfer 1 to 4 months ago, and another group which received their last transfer more than 4 months ago. We then estimate the following model:

$$\begin{aligned} y_{\{i\}hvE} =& \beta_0 + \beta_2 T_{hv}^{\text{LS:<1}} + \beta_3 T_{hv}^{LS\text{:1-4}} + \beta_4 T_{hv}^{\text{LS: 4+}} + \beta_5 T_{hv}^{\text{MTH:<1}} + \beta_6 T_{hv}^{\text{MTH: 1-4}} \\ &+ \beta_7 T_{hv}^{\text{MTH: 4+}} + \delta_1 y_{\{i\}hB} + \delta_2 \mathbb{1}(y_{\{i\}hB} = \text{missing}) + \varepsilon_{\{i\}hvE} \end{aligned} \tag{7}$$

The sample is again restricted to treatment and control households in treatment villages. In this specification, T^{x-y} takes value 1 if the transfer was completed between x and y months prior to the survey.

In Figure 4, we show the resulting treatment effect estimates separately for households receiving their last transfer less than 1 month ago, households receiving their last transfer 1 to 4 months ago, and households receiving their last transfer 4 or more months ago, separately for monthly and lump sum households. We also list the p-values from Wald tests for joint significance at each time horizon across the monthly and lump-sum groups. The figure indicates that the observed average impact on overall asset values in treatment households persists over time: both for households receiving lump-sum and monthly transfers, levels of asset holdings are significantly higher than in the control group at all time horizons. We observe no decrease over time in either group; the 95% confidence intervals of the coefficients overlap across all time horizons.

Similarly, consumption is elevated relative to control at all time horizons. The point estimates suggest declining impacts on total non-durable consumption over time for the group receiving monthly transfers, though not for the lump sum group; however, the confidence intervals of the treatment effects at shorter vs. longer time horizons overlap, i.e. these differences are not statistically significant.

For agricultural and business revenue, we find no strong indication of changing impacts over time; however, note that the treatment effects are small overall and not distinguishable from zero in this restricted and highly disaggregated sample.

Cash transfers also had persistent impacts on food security. However, this effect is driven by the monthly transfer group; in the lump-sum group we find little evidence of treatment effects at any time horizon, consistent with the larger overall treatment effect on food security in the monthly group. We also observe that the impact on food security is largest among the group receiving contemporaneous transfers; the treatment effect on food security falls by more than 50% over time in the group receiving monthly transfers, although remains positive and statistically different from zero.

The temporal dynamics of cash transfers for additional outcomes (psychological well being, health, education, female empowerment) reveal no differential impacts at different delays. In the case of psychological well-being, this reflects the fact that the restricted sample used here is underpowered to detect the overall treatment effect we observe for this outcome measure.

3.2 Asset holdings

Impacts of cash transfers on various types of assets are presented in Table 2. The overall effect on assets amounts to PPP USD 278, and is mainly driven by investment in livestock, furniture and iron roofs. Livestock holdings increase by PPP UDS 85, a 51% increase relative to the control group mean. Holdings of all types of livestock increase, with the largest increase by value occurring in cattle holdings. The value of durable goods owned by treatment households increases by PPP USD 53, primarily due to purchases of furniture (beds, chairs, tables, etc.). Cash transfers increase the likelihood of having an iron roof by 23 percentage points relative to a control group mean of 16%. The purchase of an iron roof represents an expenditure of approximately KES 35,220 (USD 402, PPP 564), or 75% of the average transfer value. In addition to a store of value (roofs can be resold), an iron roof potentially provides an investment return to households by obviating the need to periodically replace their thatched roofs, which must be done ever 1 to 2 years, costing approximately KES 4,800 (USD 55, PPP 77) per replacement, implying a simple return on the investment in the roof of between 7 and 14%. Reported savings balances double as a result of cash transfers, but from low initial levels (PPP USD 10).

We do not observe any spillover effects on asset holdings, shown in column (2) of Table 2. Columns (4)-(6) of Table 2 show the differences in asset variables between groups in each treatment arm. Though most individual coefficients are not statistically different from zero when comparing female vs. male recipient households, we observe a somewhat greater propensity of male recipient households to invest in an iron roof (12 percentage points, statistically different from zero at the 5% level). Similarly, households receiving the transfer in a single payment are 12 percentage points more likely to have an iron roof than households receiving the transfer in monthly installments. This difference, which is statistically significant at the 5% level, and is sufficient to account for the observed overall difference in the value of the households' assets between the treatment arms. Recipients of large transfers increase asset holdings significantly more than recipients of small transfers, by PPP USD 253. This difference is also primarily driven by investment in livestock, furniture and iron roofs. Recipients of large transfers are 23 percentage points more likely to have an iron roof than recipients of small transfers. These differences are statistically significant at conventional levels.

3.3 Consumption

Table 3 shows detailed results for consumption variables. With the exception of temptation goods (defined as spending on alcohol, tobacco and gambling), cash transfers increase all categories of consumption, including food, medical and education expenses, durables, home improvement, and social events. On an absolute basis, the largest increases in consumption are food (USD PPP 20 per month, a 19% increase). Spending on medical care, education and social expenditures (e.g., weddings, funerals, recreation) increase significantly on a percentage basis, but from relatively lower levels. Spending on other items, including airtime, household and personal goods increases by PPP USD 10. These impacts amount to a total increase of PPP USD 36 in non-durable expenditures among the treatment group.

Turning to spillover effects and differential impacts of treatment arms, the point estimates suggest a slight negative spillover on total nondurable consumption, which is individually significant at the 10% level, though the p-value for joint significance is insignificant. Differences in impacts on consumption between male and female recipients are not apparent. There is some indication that monthly transfers increase food consumption more than lump sum transfers, with recipients of monthly transfers consuming more food produced at home (worth PPP USD 4) than other transfer recipients, significant at the 5% level. Recipients of large transfers increase consumption more than recipients of small transfers primarily in the non-food "other" category, individually significant at the 1% level and with a joint significance test at $p < 0.05$.

3.4 Food Security

In addition to food consumption, our survey module included various measures of food security, including those related to hunger, nutrition and the availability of food. Food security is low in this population. Though instance of skipped meals are not extreme, 20% of the control group reports that not all household members usually eat until they are content, 23% of respondents report sleeping hungry in the last week, and only 36% report having enough food in the house for the next day. Table 4 shows the treatment effects on food security. To increase sensitivity, where possible we use as outcome variables the *number* of instances in the past month where households suffered from different types of food insecurity, such as having to skip meals, going for whole days without food, having to rely on others for help with food, etc. Four variables are only available as dummies (going to bed hungry, regularly eating two meals, eating until content every day, and having enough food for the next day).

With one exception (where the coefficient is small and non-significant), all of the signs of the coefficients are in the direction of increased food security, resulting in a statistically significant 0.25 SD increase in our food security index. This effect is driven by a broad range of individual variables, many of which are individually highly significant. For instance, cash transfers reduce the likelihood of the respondent having gone to bed hungry in the preceding week from 23% to 16% (a 30% decrease), increase the likelihood of having enough food in the house for the next day from 36% to 43% (a 20% increase), and reduce by 42% the number of days children go without food.

We find no spillover effects on food security and no evidence for differential effects for male vs. female recipients. In contrast, we find some evidence that food security is affected differentially for monthly vs. lump sum transfers and large compared to small transfers. The point estimates generally indicate increased food security for recipients of monthly transfers compared to those receiving lump sums, and the index variable is 0.26 SD higher for monthly transfer recipients (significant at the 5% level). Based on these results, and our findings in Section 3.2, we speculate that monthly transfers tend to disproportionately increase flow consumption such as food, while lump sum transfers are more likely to be spent on high cost assets such as iron roofs. Similarly, point estimated generally suggest further food security resulting from large transfers. The point estimate for the food security index suggests that recipients of large transfers score 0.16 SD higher than other transfer recipients on the index, though it is marginally significant. The test of joint significance, however, rejects equality among large and small transfer recipients in terms of food security.

3.5 Agricultural and Business Activities

Table 5 presents impacts of cash transfers on income generating activities, and agricultural and business activities in particular. There is little evidence that transfers change the primary source of income for recipient households; they are no more or less likely to report farming, wage labor or non-agricultural businesses as a primary source of income than control households. Cash transfers increase investment in and revenue from income generating activities, primarily non-agricultural businesses and also livestock. Flow expenditures in nonagricultural enterprises (e.g., inputs and inventory) increase by PPP USD 10 per month, with a corresponding increase of PPP USD 11 per month in revenues from these activities. Similarly spending on food and care for livestock increases by PPP USD 1 per month, while revenue from the sale of animal products (e.g., milk and eggs) increases by USD PPP 2 per month, 46% higher than the control group average. Recipient households also report PPP USD 2 higher income from the sale of livestock and meat than control households. These effects translate into an overall increase in monthly revenues for recipient households, PPP USD 15 higher (including revenues from the sale of animals and meat), but we do not observe a significant increase in estimated profits from self-employment.

Though most individual coefficients are not statistically different from zero, the joint test suggests potential spillover effects; the joint significance of the individual variables from the SUR estimation is $p = 0.08$. The point estimates are generally negative, but the overall magnitude is small (e.g., PPP USD 5 difference in total revenues). Whether the transfer is given to a men or women, or made in a lump sum or monthly installment, has little effect on the impacts on business activities. The joint significant test for large vs. small transfers rejects equality at the 1% level, largely driven by a difference in livestock expenditures, but the differences are generally small in magnitude.

3.6 Health and Education

As discussed above, we fail to detect an impact of cash transfers on our index of health outcomes. When considering the individual health outcomes, in Table 6, we see some individually significant coefficients on the treatment indicator, but are hesitant to put much stock in these results due to fact that relatively few individual coefficients are statistically significant, the point estimates of the coefficients do not universally indicate improved health outcomes, and we fail to find any difference in our index of health outcomes. It is therefore difficult to draw any general conclusions about the impact of transfers on health outcomes in this sample. We note that many of these outcomes change slowly, and further measurement may reveal a more coherent picture.

Consistent with the lack of an average impact on our educational index, in Table 7 we do not detect significant impacts on educational outcomes. The joint test of the coefficients comparing monthly vs. lump-sum transfers is weakly significant, but the corresponding index variable does not reach significance. Together, we find little evidence of treatment effects for educational outcomes. Recall, however, that in Table 3, we find a significant increase in spending on education.

3.7 Psychological well-being and neurobiological measures of stress

Treatment effects on psychological and neurobiological outcomes are shown in Table 8. Overall, we find a 0.20 SD increase in the index of psychological well-being in the treatment compared to the spillover group. The increase stems mainly from a 0.18 SD increase in happiness scores, a 0.15

SD increase in life satisfaction, a 0.14 SD reduction in stress, and a 0.99 point reduction in scores of the CESD depression questionnaire[10]. We observe no effect on cortisol levels, although several pre-defined subgroups show significant effects; these are discussed below.

One potential concern about delivering cash transfers to some households in a village but not others is that the untreated households may experience a reduction in psychological well-being simply by virtue of not having received transfers. However, our spillover analysis shows that on the contrary, spillover effects on psychological well-being were broadly positive: most coefficients on the spillover dummies go in the direction of "positive" outcomes. The spillover effect for optimism is 0.14 SD and significant at the 10% level. Note, however, that the spillover coefficients are not jointly significant in the SUR estimation ($p = 0.31$).

Comparing psychological and neurobiological outcomes when the female vs. the male in the households receives the transfer, we observe that cortisol levels in both men and women are reduced to a greater extent when women receive the transfer compared to when men receive it, and the difference between these subgroups is significant at the 1% level. The magnitude of this difference is 0.22 log units, which corresponds to 2.57 nmol/l; this effect is larger than the average difference between depressed and healthy individuals (0.58 nmol/l; Knorr et al. 2010). In addition, when the woman receives the transfer we also observe a greater reduction in worries and a greater increase in self-esteem compared to when the man receives the transfer. Overall the index of psychological well-being if 0.16 SD higher when women receive the transfer (significant at the 10% level).

Cortisol levels are also lower when the transfers are lump-sum compared to when they are monthly, although there is no overall difference in psychological outcomes between these treatment arms. In contrast, the size of the transfer had a differential effect on psychological well-being. The overall index is 0.35 SD higher for households receiving a large transfer compared to those receiving a small transfer, which is driven by reductions in depression, stress, and cortisol levels, and increases in life satisfaction. All of these differences are significant at the 5% level or above. Together, these results suggest that large transfers had significantly more pronounced effects on psychological well-being and neurobiological measures of stress than small transfers.

3.8 Female empowerment[11]

We included measures of female empowerment, specifically intrahousehold bargaining and domestic violence, in our survey module. These outcomes are of interest because domestic violence is a significant concern in this context and previous research suggests economic factors may be related to this violence. In our data, 29% of women report having been physically abused by their husbands in the preceding six months, 9% report being sexually abused and 89% report having been emotionally abused (humiliated, insulted, etc.).

[10] The control group mean of this questionnaire was 26.6. The established cutoff for the presence of depression is a score of 16.

[11] We thank Petra Persson for suggesting domestic violence outcome variables, and adapting relevant questions from the Demographic & Health Survey for this study.

In Table 9 we show the effects on domestic violence and intrahousehold bargaining outcomes as reported by the primary women in the household. The rationale for this restriction is that women may report the husband's transgressions against them more accurately than the husbands themselves. Results are shown in Table 9. As discussed above, we observe a statistically significant spillover effect on the female empowerment index: the index is 0.23 SD higher for control households in treatment villages than for pure control households. Since the treatment effect is measured in within-village comparison and there is no difference between the treatment and control group *within* villages, this finding also implies a positive treatment effect *across* villages. This result suggests that *all* households in treatment villages experienced an increase in female empowerment on average. Additional variables in table 9 show the frequency of any episode of physical, sexual or emotional violence in the last six months, and the percentage of respondents who believe that domestic violence is justified in some instances. The point estimates for these variables suggest a reduction in domestic violence, although none are individually different from zero at conventional significance levels.

As changes in domestic violence were hypothesized to arise through mechanisms directly associated with cash transfers (such as a change in women's bargaining power, or a reduction in domestic tension over economic hardships), these spillover effects are somewhat surprising. One possible explanation is that the results are simply an artifact of reporting bias, where the spillover sample believed that a different answer was desired from them than the control group. However, given that we do not find spillover effects in other measures that target unobservable outcomes, we find this explanation implausible. Another possibility is that the presence of the cash transfer program in the village motivated the husbands in untreated households to change their behavior in the hope of receiving transfers in the future. For instance, knowing that the primary female in the household was equally likely to receive the transfer as the primary male, men may have shifted their behavior to establish better relationships with their spouse. Alternatively, the spillover effect may operate through changes in attitudes among either or both husbands and wives in non-treated households. Our data does not distinguish between these possibilities; we find these unexpected results intriguing and believe they warrant further investigation.

3.9 Village level effects

To investigate whether individual cash transfers caused general equilibrium shifts at the village level, we collected village level outcomes from multiple individuals in both treatment and control villages. Specifically, a random subset of on average 3 respondents per village were surveyed about prices for a standard basket of foods and other goods, wages, and crime rates. Related variables were combined into summary indices. We regress average village-level outcomes ($\bar{y}$) on an indicator variable for whether village v is a treatment village:

$$\bar{y}_{vE} = \beta_0 + \beta_1 T_v + \varepsilon_{vE} \tag{8}$$

The results are shown in Table 10. There are no significant village-level effects on any variable group, suggesting that cash transfers to a group of particularly disadvantaged households within these villages did not impact the general village-level economy.

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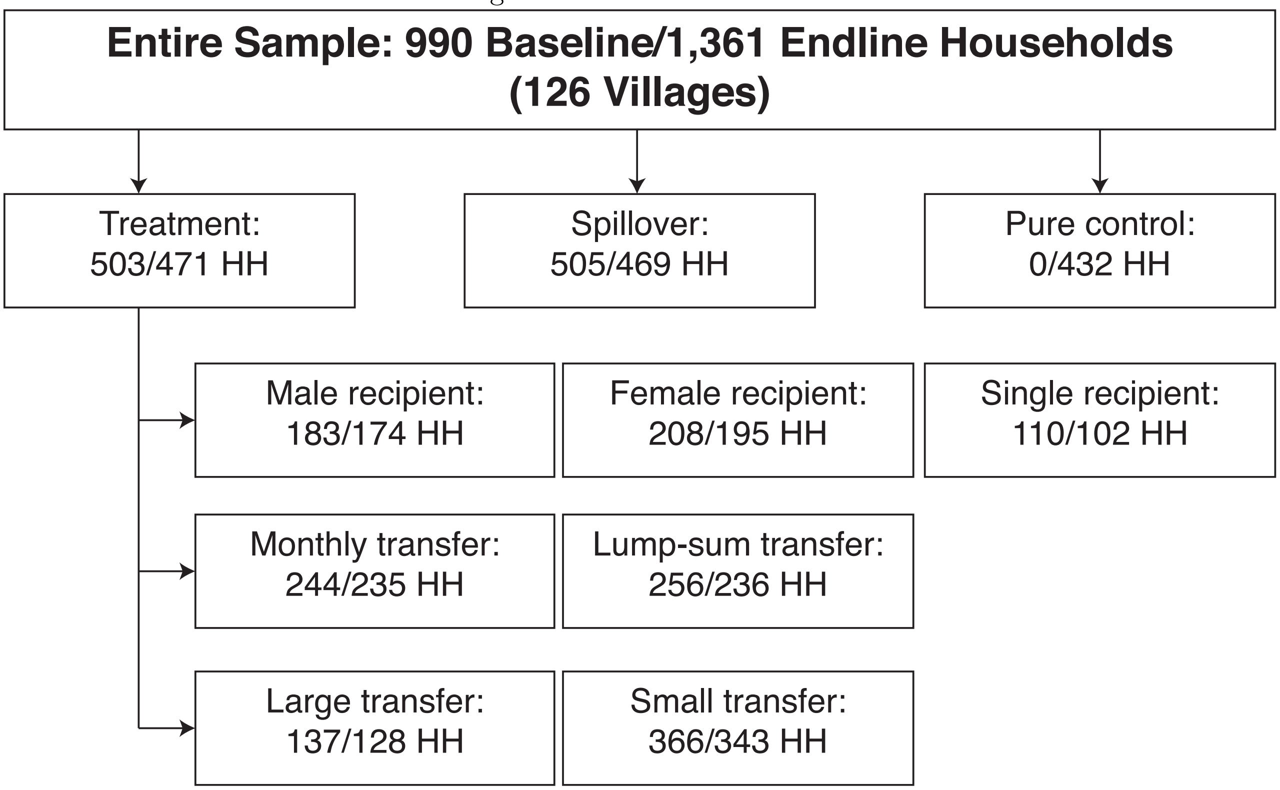
Figure 1: Treatment arms



Notes: Diagram of treatment arms. Numbers designate baseline/endline number of households in each treatment arm.

Figure 2: Timeline of study

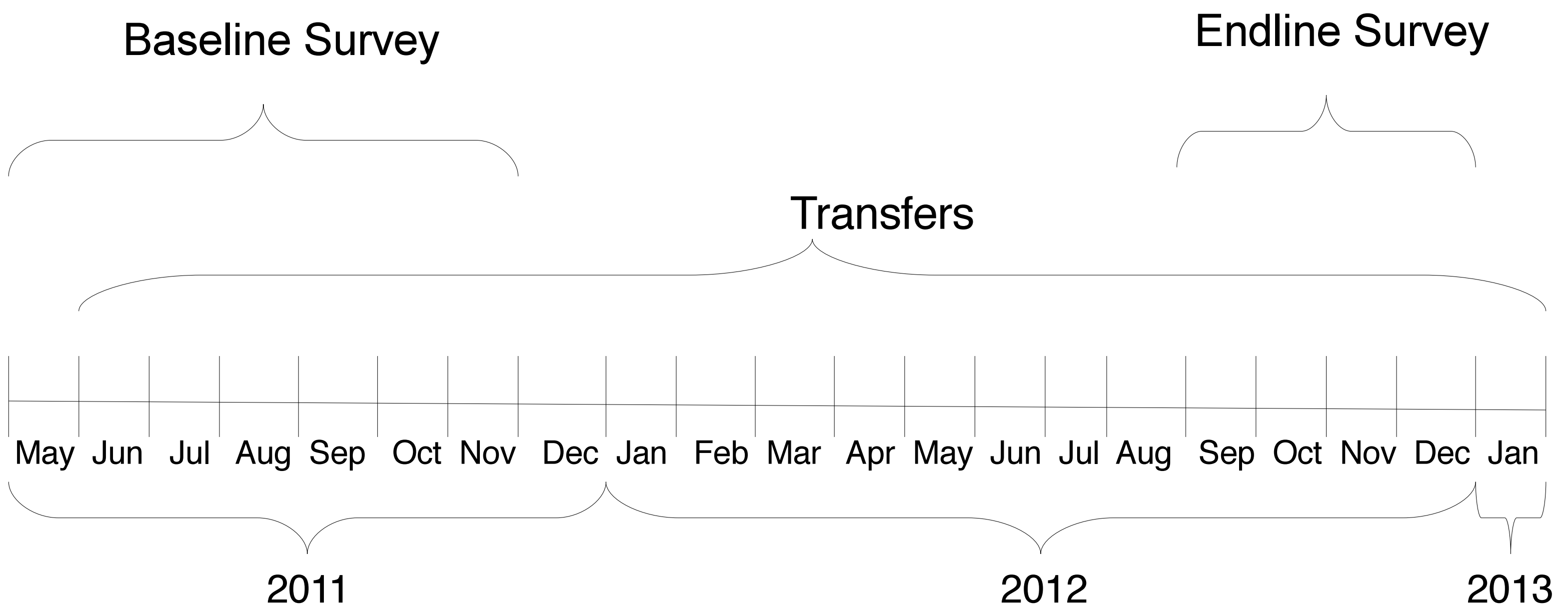
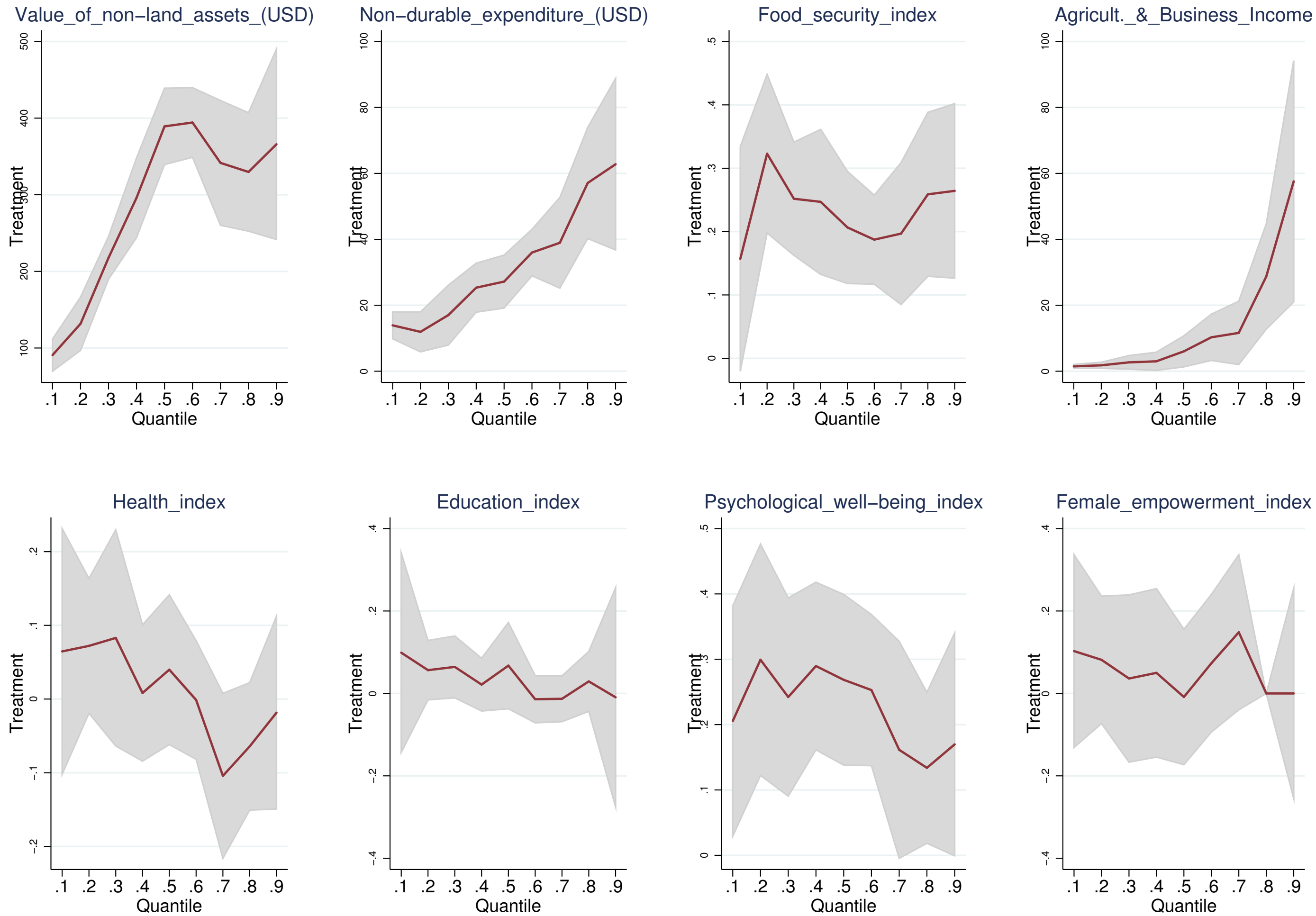
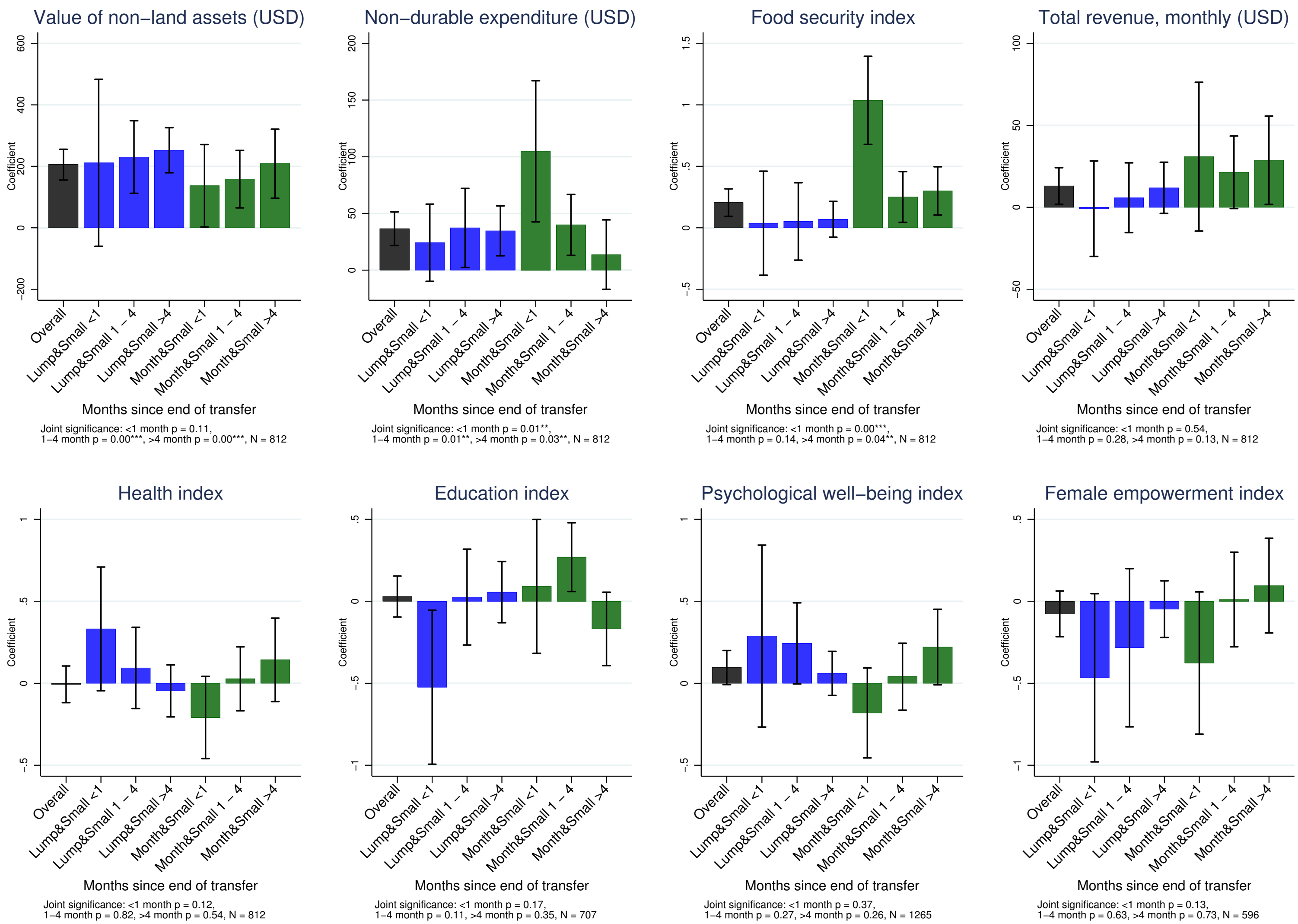


Figure 3: Quantile regression plots for index variables



Notes: Quantile regression plots of primary index variables. The red lines represent point estimates for each quartile, and the grey bands are the corresponding 95

Figure 4: Treatment effects on index variables over time



Notes: Treatment effects on index variables over time. Shown are coefficient estimates and error bars representing 95

Table 1: Treatment and spillover effects: Index variables

	Control mean (SD)	Treatment effect	Spillover effect	Female recipient	Monthly transfer	Large transfer	N
Value of non-land assets (USD)	477.66	278.52***	−18.73	−66.19	−74.97*	252.84***	1372
	(389.23)	(25.44)	(21.07)	(47.25)	(42.92)	(45.94)	
		[0.00]***	[0.90]	[0.78]	[0.52]	[0.00]***	
Non-durable expenditure (USD)	157.40	36.18***	−7.53	−2.74	−4.40	20.37*	1372
	(82.18)	(5.91)	(7.24)	(10.35)	(10.82)	(10.55)	
		[0.00]***	[0.90]	[0.97]	[1.00]	[0.23]	
Total revenue, monthly (USD)	48.98	16.64***	−5.23	5.30	16.20	−1.64	1372
	(90.52)	(5.93)	(5.67)	(10.61)	(11.11)	(8.96)	
		[0.00]***	[0.90]	[0.97]	[0.65]	[0.91]	
Food security index	−0.00	0.25***	0.04	0.05	0.26**	0.16	1372
	(1.00)	(0.06)	(0.10)	(0.09)	(0.11)	(0.10)	
		[0.00]***	[0.93]	[0.97]	[0.10]	[0.29]	
Health index	−0.00	−0.04	−0.08	0.09	0.01	−0.07	1372
	(1.00)	(0.06)	(0.08)	(0.09)	(0.10)	(0.09)	
		[0.74]	[0.90]	[0.88]	[1.00]	[0.81]	
Education index	0.00	0.08	−0.00	0.05	−0.05	0.04	1174
	(1.00)	(0.06)	(0.08)	(0.09)	(0.10)	(0.09)	
		[0.33]	[0.98]	[0.97]	[1.00]	[0.84]	
Psychological well-being index	−0.00	0.20***	0.08	0.16*	−0.10	0.35***	2140
	(1.00)	(0.06)	(0.07)	(0.09)	(0.10)	(0.10)	
		[0.00]***	[0.20]	[0.97]	[1.00]	[0.00]***	
Female empowerment index	−0.00	−0.01	0.23**	0.16	0.04	0.21**	1010
	(1.00)	(0.07)	(0.09)	(0.10)	(0.12)	(0.11)	
		[0.93]	[0.12]	[0.71]	[1.00]	[0.23]	
Joint test (p-value)		0.00***	0.13	0.41	0.01**	0.00***	

Notes: OLS estimates of treatment and spillover effects. Outcome variables are listed on the left. Assets, consumption, and income are coded in KES; the other variables are indices in z-score units, with higher values corresponding to "positive" outcomes. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. FWER-corrected standard errors are shown in brackets. Column (1) reports the mean and standard deviation of the control group for a given outcome variable. Column (2) reports the basic treatment effect, i.e. comparing treatment households to control households within villages. Column (3) reports the spillover effect, i.e. the treatment effect on spillover households compared to pure control households. Column (4) reports the relative treatment effect of transferring to the female compared to the male; column (5) the relative effect of monthly compared to lump-sum transfers; and column (6) that of large compared to small transfers. 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. All columns except the spillover regressions include village-level fixed effects, control for baseline outcomes, and cluster standard errors at the household level (in the spillover column, 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 2: Treatment and spillover effects: Assets

	Control mean (SD)	Treatment effect	Spillover effect	Female recipient	Monthly transfer	Large transfer	N
Value of non-land assets (USD)	477.66	278.52***	−18.73	−66.19	−74.97*	252.84***	1372
	(389.23)	(25.44)	(21.07)	(47.25)	(42.92)	(45.94)	
Value of livestock (USD)	166.82	84.52***	−11.61	7.86	2.07	63.19**	1372
	(240.59)	(15.24)	(16.88)	(29.35)	(27.46)	(28.30)	
Value of cows (USD)	101.78	56.78***	−0.53	20.07	−13.46	43.79*	1372
	(211.82)	(13.88)	(14.89)	(26.89)	(25.40)	(25.88)	
Value of small livestock (USD)	25.30	15.15***	−4.75	−10.30	6.84	20.09***	1372
	(49.67)	(3.30)	(3.88)	(6.29)	(5.88)	(5.88)	
Value of birds (USD)	39.74	11.98***	−6.33*	−3.33	8.63*	−0.70	1372
	(40.80)	(2.77)	(3.40)	(4.92)	(5.17)	(4.50)	
Value of durable goods (USD)	207.30	53.27***	−8.74	−1.01	−8.42	63.97***	1372
	(130.60)	(8.68)	(10.88)	(14.53)	(14.34)	(15.70)	
Value of furniture (USD)	138.11	34.67***	0.33	1.48	0.65	46.08***	1372
	(89.29)	(6.06)	(7.59)	(10.26)	(10.18)	(11.40)	
Value of agricultural tools (USD)	10.77	1.61	−0.97	−2.22	−1.00	4.12**	1372
	(14.08)	(1.00)	(1.04)	(1.85)	(1.58)	(2.06)	
Value of radio/TV (USD)	9.73	2.84**	−2.12*	−0.90	2.17	0.64	1372
	(17.09)	(1.11)	(1.11)	(2.02)	(2.06)	(1.81)	
Value of bike/motorbike (USD)	21.06	2.92	−2.03	−0.48	−1.27	2.53	1372
	(35.01)	(2.27)	(2.34)	(4.21)	(3.82)	(3.82)	
Value of appliances (USD)	3.78	0.70*	−0.04	−0.04	0.22	0.52	1372
	(5.22)	(0.36)	(0.37)	(0.58)	(0.58)	(0.67)	
Value of cell phone (USD)	23.86	12.71***	−3.89*	−0.24	−2.92	7.37***	1372
	(24.85)	(1.53)	(2.00)	(2.41)	(2.54)	(2.49)	
Value of savings (USD)	10.93	10.22***	1.62	−3.43	1.81	10.22**	1372
	(29.09)	(2.48)	(2.13)	(5.10)	(4.63)	(5.07)	
Land owned (acres)	1.31	0.03	−0.08	−0.12	0.01	0.35	1372
	(1.88)	(0.14)	(0.16)	(0.18)	(0.18)	(0.31)	
Has non-thatched roof (dummy)	0.16	0.23***	–	−0.12**	−0.12**	0.23***	1372
	(0.37)	(0.03)		(0.05)	(0.05)	(0.05)	
Joint test (p-value)		0.00***	0.32	0.42	0.31	0.00***	

Notes: OLS estimates of treatment and spillover effects. Outcome variables are listed on the left. Value of asset variables are reported in PPP adjusted USD. Has non-thatched roof is an indicator variable. Value of livestock includes value of cows, small livestock (e.g., goats) and birds. Value of durable goods is the sum of furniture, agricultural tools, radio/TV, bike/motorbike, appliances and cell phone. Furniture includes cupboards, sofas, chairs, tables and beds. Appliances include clocks, stoves and solar panels. Value of non-land assets includes the value of livestock, durable goods, savings and the value of iron sheets used in roofing the home. Variables in PPP adjusted USD are topcoded for the highest 1 percent of observations. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. Column (1) reports the mean and standard deviation of the control group for a given outcome variable. Column (2) reports the basic treatment effect, i.e. comparing treatment households to control households within villages. Column (3) reports the spillover effect, i.e. the treatment effect on spillover households compared to pure control households. Column (4) reports the relative treatment effect of transferring to the female compared to the male; column (5) the relative effect of monthly compared to lump-sum transfers; and column (6) that of large compared to small transfers. The unit of observation is the household. All columns except the spillover regressions include village-level fixed effects, control for baseline outcomes, and cluster standard errors at the household level (in the spillover column, 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 3: Treatment and spillover effects: Consumption

	Control mean (SD)	Treatment effect	Spillover effect	Female recipient	Monthly transfer	Large transfer	N
Food total (USD)	104.46	19.60***	−3.48	−2.26	1.76	7.71	1372
	(58.50)	(4.22)	(4.66)	(7.43)	(7.51)	(7.62)	
Food own production (USD)	13.64	2.45**	−2.09*	0.16	3.94**	−0.23	1372
	(14.79)	(0.96)	(1.18)	(1.72)	(1.77)	(1.48)	
Food bought (USD)	90.82	16.98***	−1.39	−3.11	−3.03	7.49	1372
	(52.77)	(3.81)	(4.31)	(6.61)	(6.73)	(6.84)	
Cereals (USD)	22.55	2.24**	0.30	0.24	−1.24	2.45	1372
	(17.18)	(1.14)	(1.58)	(1.87)	(1.87)	(2.08)	
Meat & fish (USD)	12.97	5.10***	−0.35	0.76	−3.12	2.41	1372
	(13.75)	(1.02)	(1.22)	(1.83)	(1.95)	(1.64)	
Fruit & vegetables (USD)	23.50	3.46***	0.20	−0.95	0.13	2.29	1372
	(17.06)	(1.15)	(1.39)	(1.96)	(2.05)	(1.99)	
Dairy (USD)	7.26	1.71***	−0.16	−0.73	0.82	0.49	1372
	(9.43)	(0.64)	(0.74)	(1.10)	(1.09)	(1.09)	
Fats (USD)	6.84	0.80**	0.01	−0.28	−0.27	0.91	1372
	(5.51)	(0.37)	(0.46)	(0.62)	(0.64)	(0.58)	
Sugars (USD)	11.25	1.05**	−0.52	−0.53	0.10	0.41	1372
	(7.18)	(0.48)	(0.56)	(0.81)	(0.84)	(0.78)	
Other food (USD)	42.42	5.98***	−0.36	−1.55	−0.86	3.31	1372
	(28.28)	(1.94)	(2.40)	(3.24)	(3.23)	(3.43)	
Alcohol (USD)	6.38	−0.93	−0.41	1.50	1.00	−1.55	1372
	(16.56)	(1.00)	(1.26)	(1.64)	(1.65)	(1.35)	
Tobacco (USD)	1.52	−0.16	−0.00	0.11	0.43	−0.31	1372
	(4.13)	(0.22)	(0.29)	(0.34)	(0.34)	(0.30)	
Medical expenditure past month (USD)	6.56	2.83***	1.52	2.06	−1.49	−0.35	1372
	(13.17)	(0.98)	(0.93)	(1.86)	(1.87)	(1.73)	
Medical expenditure, children (USD)	3.52	0.66	1.03*	0.63	−0.37	−0.10	1203
	(8.52)	(0.60)	(0.60)	(1.06)	(1.09)	(0.97)	
Education expenditure (USD)	4.71	1.08**	0.32	0.44	−0.10	1.10	1372
	(8.68)	(0.51)	(0.61)	(0.89)	(0.88)	(0.92)	
Social expenditure (USD)	4.36	2.46***	−1.42***	−2.06**	−0.46	0.67	1372
	(5.38)	(0.49)	(0.46)	(0.98)	(1.01)	(0.90)	
Other expenditure (USD)	34.36	10.06***	−3.72	−2.05	−3.56	11.76***	1372
	(24.62)	(1.74)	(2.27)	(3.05)	(3.17)	(3.01)	
Non-durable expenditure (USD)	157.40	36.18***	−7.53	−2.74	−4.40	20.37*	1372
	(82.18)	(5.91)	(7.24)	(10.35)	(10.82)	(10.55)	
Joint test (p-value)		0.00***	0.15	0.82	0.38	0.04**	

Notes: OLS estimates of treatment and spillover effects. Outcome variables are listed on the left. All variables are reported in PPP adjusted USD. Food bought includes all sub-categories except alcohol and tobacco. Education expenditures include tuition and other costs (e.g., uniform, supplies). Social expenditures include charitable donations, dowry, fees paid to village elder or chiefs, religious ceremonies, weddings, funerals and recreation (e.g., books, music). Other expenditures include airtime, travel and transportation, clothing, personal items (e.g., toiletries), household items (e.g., soap, candles), firewood, electricity and water. Non-durable expenditures are the sum of expenditures on food (own production and purchased), alcohol and tobacco, medical, education, social activities and other goods. Variables in PPP adjusted USD are topcoded for the highest 1 percent of observations. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. Column (1) reports the mean and standard deviation of the control group for a given outcome variable. Column (2) reports the basic treatment effect, i.e. comparing treatment households to control households within villages. Column (3) reports the spillover effect, i.e. the treatment effect on spillover households compared to pure control households. Column (4) reports the relative treatment effect of transferring to the female compared to the male; column (5) the relative effect of monthly compared to lump-sum transfers; and column (6) that of large compared to small transfers. The unit of observation is the household. All columns except the spillover regressions include village-level fixed effects, control for baseline outcomes, and cluster standard errors at the household level (in the spillover column, 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 4: Treatment and spillover effects: Food security

	Control mean (SD)	Treatment effect	Spillover effect	Female recipient	Monthly transfer	Large transfer	N
Meals skipped (adults, # last month)	4.38	−0.99***	0.25	−0.14	−0.50	−0.30	1372
	(5.75)	(0.35)	(0.49)	(0.48)	(0.56)	(0.52)	
Whole days without food (adults, # last month)	0.87	−0.27*	0.07	0.01	−0.20	0.26	1372
	(2.73)	(0.15)	(0.17)	(0.19)	(0.15)	(0.28)	
Meals skipped (children, # last month)	2.03	−0.59**	0.61	0.23	−0.37	−0.69**	1203
	(4.48)	(0.27)	(0.42)	(0.34)	(0.48)	(0.33)	
Whole days without food (children, # last month)	0.33	−0.14*	0.13	−0.04	−0.18	−0.08	1203
	(1.37)	(0.08)	(0.10)	(0.08)	(0.11)	(0.10)	
Eat less preferred/cheaper foods (# last month)	8.17	−0.99**	0.87	0.37	−0.50	0.43	1372
	(7.69)	(0.46)	(0.73)	(0.72)	(0.78)	(0.73)	
Rely on help from others for food (# last month)	1.87	−0.08	−0.28	0.50	−0.20	0.63	1372
	(3.86)	(0.25)	(0.36)	(0.34)	(0.41)	(0.45)	
Purchase food on credit (# last month)	3.12	−0.44*	−0.43	0.08	−0.43	−0.67*	1372
	(4.57)	(0.26)	(0.44)	(0.38)	(0.42)	(0.38)	
Hunt, gather wild food, harvest prematurely (# last month)	4.10	0.04	−0.15	−0.74	−0.67	0.73	1372
	(6.78)	(0.41)	(0.70)	(0.68)	(0.67)	(0.71)	
Beg because not enough food in the house (# last month)	0.31	−0.05	−0.08	0.01	−0.05	−0.03	1372
	(0.80)	(0.05)	(0.08)	(0.07)	(0.08)	(0.07)	
All members usually eat two meals (dummy)	0.90	0.03*	0.03	0.01	0.02	0.02	1372
	(0.29)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	
All members usually eat until content (dummy)	0.79	0.04*	−0.04	−0.02	0.06	0.06	1372
	(0.41)	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	
Number of times ate meat or fish (last week)	2.41	0.49***	−0.04	0.49*	0.60**	0.29	1372
	(2.07)	(0.14)	(0.22)	(0.25)	(0.27)	(0.23)	
Enough food in the house for tomorrow? (dummy)	0.36	0.07**	0.02	−0.02	0.11**	0.13**	1372
	(0.48)	(0.03)	(0.04)	(0.05)	(0.05)	(0.05)	
Respondent slept hungry (last week, dummy)	0.23	−0.07***	0.03	−0.02	−0.07*	−0.08***	1372
	(0.42)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	
Respondent ate protein (last 24h, dummy)	0.29	0.07**	−0.03	0.04	0.08	0.07	1372
	(0.46)	(0.03)	(0.05)	(0.05)	(0.05)	(0.05)	
Proportion of HH who ate protein (last 24h)	0.27	0.07**	−0.03	0.05	0.06	0.04	1372
	(0.42)	(0.03)	(0.04)	(0.04)	(0.05)	(0.04)	
Proportion of children who ate protein (last 24h)	0.26	0.07**	−0.03	0.05	0.04	0.03	1203
	(0.42)	(0.03)	(0.04)	(0.05)	(0.05)	(0.05)	
Food security index (children)	0.00	0.21***	−0.16*	0.05	0.16	0.14	1203
	(1.00)	(0.06)	(0.09)	(0.09)	(0.11)	(0.09)	
Food security index	−0.00	0.25***	0.04	0.05	0.26**	0.16	1372
	(1.00)	(0.06)	(0.10)	(0.09)	(0.11)	(0.10)	
Joint test (p-value)		0.00***	0.16	0.79	0.60	0.02**	

Notes: OLS estimates of treatment and spillover effects. Outcome variables are listed on the left. Variables indicating frequency of events in the last month are based on midpoint of ranges measured (2-4 = 3, 5-10 = 7.5, >10 = 20). Number of time ate meat or fish in last week is measured directly. The remaining outcomes are indicator variables or proportions, as indicated. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. Column (1) reports the mean and standard deviation of the control group for a given outcome variable. Column (2) reports the basic treatment effect, i.e. comparing treatment households to control households within villages. Column (3) reports the spillover effect, i.e. the treatment effect on spillover households compared to pure control households. Column (4) reports the relative treatment effect of transferring to the female compared to the male; column (5) the relative effect of monthly compared to lump-sum transfers; and column (6) that of large compared to small transfers. The unit of observation is the household. All columns except the spillover regressions include village-level fixed effects, control for baseline outcomes, and cluster standard errors at the household level (in the spillover column, 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 5: Treatment and spillover effects: Agricultural and Business Activities

	Control mean (SD)	Treatment effect	Spillover effect	Female recipient	Monthly transfer	Large transfer	N
Wage labor primary income (dummy)	0.16	−0.00	−0.05	0.02	0.02	0.00	1372
	(0.37)	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	
Own farm primary income (dummy)	0.56	−0.01	0.05	−0.00	0.00	0.02	1372
	(0.50)	(0.03)	(0.05)	(0.05)	(0.05)	(0.05)	
Non-ag business primary income (dummy)	0.12	0.02	0.01	−0.02	0.01	0.01	1372
	(0.32)	(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	
Non-agricultural business owner (dummy)	0.32	0.02	0.01	−0.03	0.07	0.02	1372
	(0.47)	(0.03)	(0.04)	(0.05)	(0.05)	(0.05)	
Non-ag business revenue, monthly (USD)	28.62	11.15^{*}	−2.19	5.14	12.86	−0.24	1372
	(86.25)	(5.71)	(5.51)	(10.28)	(10.76)	(8.61)	
Non-ag business flow expenses, monthly (USD)	16.61	10.18^{**}	−0.73	6.52	9.92	−3.87	1372
	(60.12)	(4.16)	(3.77)	(7.26)	(7.75)	(6.11)	
Non-ag business profit imputed, monthly (USD)	12.01	−0.58	−1.46	−0.25	3.34	3.43	1372
	(44.10)	(3.65)	(3.21)	(6.63)	(7.90)	(5.39)	
Non-ag business profit self-reported, monthly (USD)	8.26	1.86	1.70	0.34	1.30	0.10	1372
	(24.73)	(1.73)	(1.77)	(3.21)	(3.09)	(2.83)	
Non-ag business investment in durables, monthly (USD)	0.17	0.24^{***}	−0.10	−0.15	0.01	−0.15	1372
	(0.74)	(0.08)	(0.07)	(0.17)	(0.17)	(0.13)	
Farm revenue, monthly (USD)	9.66	0.23	−0.21	−0.10	−0.03	0.02	1372
	(8.89)	(0.54)	(0.76)	(0.90)	(0.91)	(0.82)	
Farm flow expenses, monthly (USD)	5.01	1.47^{***}	−0.61	−0.67	−0.18	1.18^{*}	1372
	(5.84)	(0.36)	(0.60)	(0.63)	(0.60)	(0.66)	
Farm profit, monthly (USD)	4.65	-1.21^{***}	0.41	0.48	0.18	−1.22	1372
	(7.47)	(0.47)	(0.67)	(0.80)	(0.79)	(0.77)	
Livestock flow revenue, monthly (USD)	6.44	3.02^{***}	-2.09^{**}	1.24	3.70^{*}	−1.08	1372
	(14.04)	(0.98)	(1.05)	(1.86)	(1.91)	(1.52)	
Livestock flow expenses, monthly (USD)	2.33	1.31^{***}	−0.51	−0.49	−0.81	2.41^{***}	1372
	(4.64)	(0.33)	(0.36)	(0.63)	(0.52)	(0.67)	
Livestock flow profit, monthly (USD)	4.11	1.68^{*}	−1.57	1.52	4.32^{**}	-3.60^{***}	1372
	(13.21)	(0.94)	(1.03)	(1.77)	(1.84)	(1.37)	
Livestock sales and meat revenue, monthly (USD)	4.25	2.21^{***}	−0.75	−0.42	−0.18	−0.54	1372
	(8.40)	(0.61)	(0.57)	(1.21)	(1.16)	(1.07)	
Total revenue, monthly (USD)	48.98	16.64^{***}	−5.23	5.30	16.20	−1.64	1372
	(90.52)	(5.93)	(5.67)	(10.61)	(11.11)	(8.96)	
Total expenses, monthly (USD)	23.95	12.90^{***}	−1.85	5.38	8.98	−0.18	1372
	(61.71)	(4.23)	(3.91)	(7.38)	(7.84)	(6.29)	
Total profit, monthly (USD)	20.78	−0.16	−2.63	1.37	7.60	−1.54	1372
	(46.22)	(3.74)	(3.40)	(6.72)	(8.00)	(5.39)	
Joint test (p-value)		0.00^{***}	0.11	0.87	0.57	0.02^{**}	

Notes: OLS estimates of treatment and spillover effects. Outcome variables are listed on the left. Variables indicating primary source of income and operation of non-agricultural enterprise are dummy variables. All other outcome variables are reported as PPP adjusted USD, converted to a monthly time horizon. Enterprise flow expenses includes the total cost of electricity, wages, water, transport, inputs, inventory and any other expenses for all enterprises owned and operated (partially or fully) by the respondent. Farm revenue includes the value of crops sold or consumed in the short and long rain seasons, expressed on a monthly basis. Farm flow expenses include seeds, pertilizers, herbicides, rental of machines, water, labor and other expenses related to agricultural production. Livestock flow revenue includes the value of animal products sold or consumed, including milk, eggs and other animal products. Livestock flow expenses include all expenses in caring for animals, such as fodder or veterenary care. Total revenue, expenses and profits are the sum across farm, animal and non-agricultural enterprises. Variables in PPP adjusted USD are topcoded for the highest 1 percent of observations. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. Column (1) reports the mean and standard deviation of the control group for a given outcome variable. Column (2) reports the basic treatment effect, i.e. comparing treatment households to control households within villages. Column (3) reports the spillover effect, i.e. the treatment effect on spillover households compared to pure control households. Column (4) reports the relative treatment effect of transferring to the female compared to the male; column (5) the relative effect of monthly compared to lump-sum transfers; and column (6) that of large compared to small transfers. The unit of observation is the household. All columns except the spillover regressions include village-level fixed effects, control for baseline outcomes, and cluster standard errors at the household level (in the spillover column, 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 6: Treatment and spillover effects: Health

	Control mean (SD)	Treatment effect	Spillover effect	Female recipient	Monthly transfer	Large transfer	N
Medical expenses per episode, entire HH (USD)	5.81	0.85	0.81	1.44	−0.30	1.47	1184
	(13.57)	(0.87)	(1.13)	(1.24)	(1.37)	(1.31)	
Medical expenses per episode, children (USD)	3.39	1.19**	0.15	0.47	0.27	0.83	866
	(4.27)	(0.54)	(0.77)	(1.05)	(1.17)	(0.94)	
Proportion of household sick/injured (1 month)	0.49	0.02	0.05*	−0.05	−0.05*	0.04	1372
	(0.31)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	
Proportion of children sick/injured (1 month)	0.45	0.01	0.06**	−0.06	−0.05	0.01	1203
	(0.36)	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	
Proportion of sick/injured who could afford treatment	0.82	0.01	0.02	0.05*	−0.03	0.01	1184
	(0.32)	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	
Average number of sick days per HH member	1.81	0.07	−0.01	−0.04	−0.67**	0.44	1372
	(3.00)	(0.18)	(0.26)	(0.23)	(0.27)	(0.28)	
Propotion of illnesses where doctor was consulted	0.73	0.05**	−0.03	0.00	−0.04	0.03	1184
	(0.36)	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	
Proportion of newborns vaccinated	0.59	−0.09	0.12	0.05	−0.16	0.06	357
	(0.49)	(0.07)	(0.07)	(0.12)	(0.13)	(0.11)	
Proportion of children <14 getting checkup (6 months)	0.25	0.04*	−0.03	−0.06	−0.05	−0.01	1201
	(0.37)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	
Proportion of children <5 who died (1 year)	0.03	0.01	−0.01	0.03*	−0.00	−0.02	959
	(0.13)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	
BMI to age z-score	−0.00	0.08	−0.02	−0.03	0.22	−0.09	303
	(1.00)	(0.16)	(0.11)	(0.21)	(0.31)	(0.16)	
Height to age z-score	0.00	0.05	0.09	−0.26	−0.65**	−0.40*	319
	(1.00)	(0.14)	(0.14)	(0.23)	(0.25)	(0.21)	
Weight to age z-score	−0.00	0.27*	0.13	0.22	0.25	−0.30	304
	(1.00)	(0.16)	(0.15)	(0.26)	(0.30)	(0.24)	
Arm circumference to age z-score	−0.00	0.07	−0.08	−0.15	0.06	0.20	320
	(1.00)	(0.15)	(0.15)	(0.25)	(0.27)	(0.26)	
Health index (children)	−0.00	−0.01	−0.04	0.04	0.03	0.01	1239
	(1.00)	(0.07)	(0.09)	(0.10)	(0.12)	(0.11)	
Health index	−0.00	−0.04	−0.08	0.09	0.01	−0.07	1372
	(1.00)	(0.06)	(0.08)	(0.09)	(0.10)	(0.09)	
Joint test (p-value)		0.06*	0.06*	0.12	0.04**	0.13	

Notes: OLS estimates of treatment and spillover effects. Outcome variables are listed on the left. The scale on which each variable is measured is indicated. For variables referring to spouses and children, the sample is restricted to households which have such members. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. Column (1) reports the mean and standard deviation of the control group for a given outcome variable. Column (2) reports the basic treatment effect, i.e. comparing treatment households to control households within villages. Column (3) reports the spillover effect, i.e. the treatment effect on spillover households compared to pure control households. Column (4) reports the relative treatment effect of transferring to the female compared to the male; column (5) the relative effect of monthly compared to lump-sum transfers; and column (6) that of large compared to small transfers. The unit of observation is the household. All columns except the spillover regressions include village-level fixed effects, control for baseline outcomes, and cluster standard errors at the household level (in the spillover column, 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 7: Treatment and spillover effects: Education

	Control mean (SD)	Treatment effect	Spillover effect	Female recipient	Monthly transfer	Large transfer	N
Education expenditure past month (USD)	66.28	18.56	−3.44	−9.80	−21.83	−3.15	1174
	(120.95)	(11.66)	(10.03)	(21.33)	(21.27)	(16.60)	
Education expenditure per child past month (USD)	22.97	3.61	0.86	1.93	1.21	0.45	1174
	(36.91)	(2.26)	(2.63)	(3.45)	(4.06)	(3.31)	
Proportion of school-aged children in school	0.69	0.01	−0.01	0.01	−0.03	0.02	1174
	(0.34)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	
School days missed past month (per child)	1.07	−0.12	0.15	−0.28	−0.26	0.05	1173
	(1.84)	(0.12)	(0.17)	(0.22)	(0.20)	(0.22)	
Income-generating activities per school-age child >6	0.83	−0.03	0.06	−0.01	-0.16^{*}	−0.01	1022
	(0.85)	(0.05)	(0.07)	(0.09)	(0.08)	(0.08)	
Education index	0.00	0.08	−0.00	0.05	−0.05	0.04	1174
	(1.00)	(0.06)	(0.08)	(0.09)	(0.10)	(0.09)	
Joint test (p-value)		0.48	0.62	0.55	0.03^{**}	1.00	

Notes: OLS estimates of treatment and spillover effects. Outcome variables are listed on the left. The reference period for school missed and income-generating activities is 1 month. The sample is restricted to households with children of the age range specified in the variable description. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. Column (1) reports the mean and standard deviation of the control group for a given outcome variable. Column (2) reports the basic treatment effect, i.e. comparing treatment households to control households within villages. Column (3) reports the spillover effect, i.e. the treatment effect on spillover households compared to pure control households. Column (4) reports the relative treatment effect of transferring to the female compared to the male; column (5) the relative effect of monthly compared to lump-sum transfers; and column (6) that of large compared to small transfers. The unit of observation is the household. All columns except the spillover regressions include village-level fixed effects, control for baseline outcomes, and cluster standard errors at the household level (in the spillover column, 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 8: Treatment and spillover effects: Psychological well-being

	Control mean (SD)	Treatment effect	Spillover effect	Female recipient	Monthly transfer	Large transfer	N
Log cortisol (no controls)	2.46	0.04	−0.09	−0.22**	0.26**	−0.12	2102
	(0.89)	(0.06)	(0.06)	(0.09)	(0.10)	(0.09)	
Log cortisol (with controls)	−0.03	0.05	−0.08	−0.21**	0.27***	−0.16*	2102
	(0.88)	(0.06)	(0.06)	(0.09)	(0.10)	(0.09)	
Depression (CESD)	26.48	−0.99*	−0.73	−0.98	−1.36	−1.76**	2140
	(9.31)	(0.55)	(0.78)	(0.82)	(0.89)	(0.82)	
Worries	0.00	−0.09	0.03	−0.15*	−0.13	−0.11	2140
	(1.00)	(0.06)	(0.08)	(0.08)	(0.10)	(0.09)	
Stress (Cohen)	0.00	−0.14**	0.05	0.04	0.02	−0.30***	2140
	(1.00)	(0.06)	(0.07)	(0.09)	(0.10)	(0.09)	
Happiness (WVS)	−0.00	0.18***	0.13	−0.01	0.01	0.07	2140
	(1.00)	(0.06)	(0.08)	(0.09)	(0.10)	(0.09)	
Life satisfaction (WVS)	−0.00	0.15***	0.04	−0.01	0.00	0.17**	2140
	(1.00)	(0.05)	(0.08)	(0.08)	(0.09)	(0.08)	
Trust (WVS)	−0.00	0.06	−0.09	0.08	0.06	−0.15	2140
	(1.00)	(0.06)	(0.07)	(0.09)	(0.10)	(0.10)	
Locus of control	0.00	0.03	−0.06	0.04	0.03	0.02	2140
	(1.00)	(0.06)	(0.08)	(0.10)	(0.10)	(0.10)	
Optimism (Scheier)	−0.00	0.10	0.14*	0.12	0.05	0.13	2140
	(1.00)	(0.06)	(0.07)	(0.09)	(0.10)	(0.10)	
Self-esteem (Rosenberg)	0.00	0.01	−0.11	0.23**	0.09	−0.10	2140
	(1.00)	(0.06)	(0.08)	(0.10)	(0.11)	(0.12)	
Psychological well-being index	−0.00	0.20***	0.08	0.16*	−0.10	0.35***	2140
	(1.00)	(0.06)	(0.07)	(0.09)	(0.10)	(0.10)	
Joint test (p-value)		0.00***	0.31	0.15	0.19	0.01***	

Notes: OLS estimates of treatment and spillover effects. Outcome variables are listed on the left. All variables are coded in z-score units, except raw cortisol, which is coded in nmol/l. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. Column (1) reports the mean and standard deviation of the control group for a given outcome variable. Column (2) reports the basic treatment effect, i.e. comparing treatment households to control households within villages. Column (3) reports the spillover effect, i.e. the treatment effect on spillover households compared to pure control households. Column (4) reports the relative treatment effect of transferring to the female compared to the male; column (5) the relative effect of monthly compared to lump-sum transfers; and column (6) that of large compared to small transfers. The unit of observation is the individual. All columns except the spillover regressions include village-level fixed effects, control for baseline outcomes, and cluster standard errors at the household level (in the spillover column, 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 9: Treatment and spillover effects: Female empowerment

	Control mean (SD)	Treatment effect	Spillover effect	Female recipient	Monthly transfer	Large transfer	N
Physical violence (dummy)	0.29	0.00	−0.05	−0.05	−0.08	−0.03	1010
	(0.45)	(0.03)	(0.04)	(0.05)	(0.06)	(0.05)	
Sexual violence (dummy)	0.09	−0.02	−0.04	−0.03	−0.01	−0.04	1010
	(0.29)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	
Emotional violence (dummy)	0.89	0.04^{*}	−0.02	−0.00	0.01	0.01	1010
	(0.32)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	
Justifiability of violence (dummy)	0.64	0.00	−0.03	−0.04	0.04	−0.02	1010
	(0.48)	(0.04)	(0.04)	(0.05)	(0.06)	(0.06)	
Female empowerment index	−0.00	−0.01	0.23^{**}	0.16	0.04	0.21^{**}	1010
	(1.00)	(0.07)	(0.09)	(0.10)	(0.12)	(0.11)	
Joint test (p-value)		0.34	0.19	0.69	0.71	0.36	

Notes: OLS estimates of treatment and spillover effects. Outcome variables are listed on the left. All variables are coded in z-score units. The sample is restricted to co-habitating couples. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. Column (1) reports the mean and standard deviation of the control group for a given outcome variable. Column (2) reports the basic treatment effect, i.e. comparing treatment households to control households within villages. Column (3) reports the spillover effect, i.e. the treatment effect on spillover households compared to pure control households. Column (4) reports the relative treatment effect of transferring to the female compared to the male; column (5) the relative effect of monthly compared to lump-sum transfers; and column (6) that of large compared to small transfers. The unit of observation is the female in the household, who is asked about behavior of the male towards her and her own attitudes. All columns except the spillover regressions include village-level fixed effects, control for baseline outcomes, and cluster standard errors at the household level (in the spillover column, 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 10: Village level effects

	Control mean (SD)	Treatment	N
Food price index	−0.029	0.060	117
	(0.223)	(0.067)	
Non-food price index	0.035	−0.076	117
	(0.474)	(0.096)	
Wages Index	0.091	−0.131	117
	(0.644)	(0.110)	
Crime Frequency Index	0.010	−0.020	117
	(0.444)	(0.099)	

Notes: OLS estimates of village level effects. For each outcome variable we report the coefficient of interest and its standard error. Standard errors are clustered at the village level. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.