Welfare Effects of Unconditonal Cash Transfers: Pre-Analysis Plan*

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Abstract

This document describes the analysis plan for the randomized controlled trial (RCT) evaluating the Unconditional Cash Transfer (UCT) of GiveDirectly, Inc. Between June 2011 and January 2013, GiveDirectly distributed unconditional cash transfers to 500 poor rural households in Western Kenya. The transfers were sent to recipients' mobile phones using the M-Pesa technology. The present RCT includes three treatments: first, the magnitude of the total transfer to each treatment household was randomly chosen to be either \$300 or \$1,100. Second, the transfers were randomly chosen to be sent to either the primary female or the primary male member of the household. Third, the transfers were randomly assigned to be sent as either a large lump-sum payment, or a series of nine monthly installments of the same total amount. The present document outlines the outcome variables and econometric methods we will use to assess the effect of the program on expenditure, food security, assets, income and enterprise activity, intrahousehold bargaining, domestic violence, education, health, and preferences, as well as psychological well-being and neurobiological measures of stress.

JEL Codes: C93, D13, I15, I25, O12

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1 Introduction

Cash transfers are a simple intervention to improve the lives of the poor, and have attracted increased attention both from researchers and policy-makers (Angelucci and De Giorgi 2009; Cunha 2010; Bertrand et al. 2003; Ardington et al. 2009; Posel et al. 2006; Mel et al. 2008; Maluccio and Flores 2005; Attanasio and Mesnard 2006; Gertler et al. 2012; Duflo 2003; Rubalcava et al. 2004; Sadoulet et al. 2001; Blattman et al. 2013). However, existing research is still lacking on several practical and theoretical questions that are crucial for understanding the impact of cash transfers. First, should money be given all at once or in many smaller installments? Theory suggests that this depends on recipients' ability to exercise self-control and on their investment options. Second, should money be given to the husband or wife? This may not only affect how the money is spent, but also the intra-household distribution of power. Third, should policy-makers give small transfers to many recipients or larger transfers to a few? The Unconditional Cash Transfers (UCT) Project in Rarieda, Western Kenya, aimed to narrow these gaps by providing evidence on the effects of three key design parameters – transfer frequency, recipient gender, and transfer size – on the impacts of a UCT program in rural Kenya.

As CTs can affect a broad range of outcomes, we measure impacts on a wide and innovative range of dimensions. First, we document consumption, food security, school enrollment, the physical devleopment of children, entrepreneurial activities, assets and loans, transfers between households, fertility and contraception use, and health. Second, we measure stress and mental health through psychological questionnaires, and employ a relatively novel neurobiological measure of stress (cortisol), measured through saliva. Third, we collect detailed information on spousal controlling behavior, intra-household bargaining, and domestic violence. And finally, we consider the impact of UCTs not only on the recipients, but also on the community. We examine spillovers and general equilibrium effects by measuring effects on non-treated households both in treatment and pure control villages, and the effect on prices, wages, locally available varieties, transportation, and local industrial organization. Understanding such effects is crucial for assessing the feasibility of UCTs as a social protection strategy, yet existing evidence is scarce (Angelucci and De Giorgi 2009). We use a dual-level stratified randomized design that allows us to rigorously identify spillover effects both within and across villages.

By filling critical gaps in our knowledge of UCTs, we hope that this study will help to provide both academics and policy-makers with a better understanding of the effect of unconditional cash transfers on welfare, and the channels through which these effects operate.

2 Intervention

The UCT Program implemented by GiveDirectly Inc. (GD) targets impoverished households in Kenya. The study evaluates GD's intervention in the Rarieda District, in Western Kenya. GD's intended beneficiaries are especially disadvantaged households, with per capita incomes below $1 per day. Households are identified as eligible using objective and transparent criteria that are highly correlated with poverty: dwellings lacking solid walls, floors, or roofs.

GD's goal is to provide flexible financial assistance to those in greatest need, while maximizing cost efficiency by transferring cash electronically using M-Pesa mobile money technology, a key innovative feature of the program. M-Pesa is a mobile money system offered by Safaricom, the largest Kenyan mobile phone operator. GD transfers the money from GD's M-Pesa account to that of the recipient. If the recipient does not have a cell phone, GD distributes a SIM card and asks the recipient to sign up for M-Pesa; then, money is transferred to the SIM card, and the recipient can withdraw the balance at an M-Pesa agent by putting the SIM card into the agent's cell phone.

This delivery method drastically cuts the costs of reaching the recipient: GD transfers 90% of the program's total budget directly to a poor household, with the remainder covering recipient identification, including staff costs (7%), and mobile transfer fees for both GD and recipients (3%). Not only is the intervention cheap; the insights from the study can also be applied broadly, as the program can be implemented in any area with access to mobile money technology. As such technology spreads throughout the developing world, the program will become increasingly scalable.

3 Evaluation questions

Our overall question is: how should UCTs be structured to maximize impact? In particular, how do outcomes vary with three key, yet understudied, design parameters: recipient gender, transfer frequency, and transfer size? The intervention therefore contains the following treatment arms:

1. **Transfers to the woman vs. the man in the household.** First, half of the transfers were made to the woman, while the other half were made to the man. This feature allows us to identify the differential welfare effects of gender-specific cash transfers.

2. **Lump-sum transfers vs. monthly installments.** Second, half of the transfers were lump sum, and the other half was paid in 9 monthly installments. By timing the announcements and delivery schedule of the transfers such that the total amount of

money transferred to was on average the same in a given month across the lump-sum and monthly groups, we kept the discounted present value of the lump-sum and installment transfers similar across groups (if individual utility functions are homogeneous of degree 1, they are identical).

3. **Large vs. small transfers.** Finally, a proportion (28%) of the transfers were $1,100 in magnitude, while the remainder were $300. This manipulation allows us to estimate the effect of transfer magnitude on welfare outcomes.

These three treatment arms are fully crossed with each other, except that the $1,100 transfers were made to existing recipients of $300 transfers in the form of a $800 top-up that was delivered as a stream of payments after respondents had already been told that they would receive $300 transfers. Section 5 outlines how this issue is dealt with in the analysis.

4 Evaluation Design

4.1 Sampling and Identification strategy

To establish a causal relationship between the program and changes in outcomes, this study uses a Randomized Control Trial (RCT). We first identified Rarieda as an intervention area because it has (i) high poverty rates according to census data, and (ii) sufficient M-Pesa access to make transfers feasible. We then randomly identified 100 villages there. In these villages, we identified 1,500 eligible households, with eligibility determined by residing in a home made of mud, grass, and other non-solid materials. These criteria are simple, objective, and transparent, maximizing accountability. The criteria were not pre-announced to avoid "gaming" of the eligibility rules. We designed a separate targeting module to compare our eligibility criteria to traditional approaches, such as community and elder opinion. We then randomized on two levels – across villages, and within villages. Specifically, 50 villages were randomly assigned to be treatment villages, while the other 50 were pure control villages. In each of the latter, we surveyed 10 households that did not receive a cash transfer. Within treatment villages, we conducted a within-village randomization: 10 households were randomly assigned a cash transfer; a further 10 received no transfer (GD will seek to make transfers to this group after the study). This strategy allows us to identify spillover effects (detailed below).

4.2 Spillover effects

We use three approaches to quantify spillover effects. First, we used pure control villages to quantify within-village spillovers. We randomly selected 50 treatment villages; in each of these, we randomly selected 10 treatment and 10 control households. In addition, we selected 50 pure control villages, with 10 control households each. Comparing control households in treatment villages to those in pure control villages identifies within-village spillover effects. Second, we identify spillover effects across villages. Note that these effects could potentially be even more pronounced than within villages, if, for instance, entire villages are affected by weather shocks (Rosenzweig and Stark 1989). Using GPS data on village location, we can identify cross-village spillovers, under the assumption that these spillovers are geographically correlated. Third, a separate village-level survey elicited general equilibrium effects of the intervention at the level of the local economy; we surveyed local shops to assess effects on prices, and the village elder and other residents of the village to estimate effects on labor supply, wages, crime, investment, community relations (e.g. perceived fairness of targeting criteria) and power dynamics.

4.3 Data collection methods and instruments

Data was collected at baseline and one year after the intervention. A midline with a subset of questions was collected 6 months after the intervention. Trained interviewers visited the households; both the primary male and the primary female of the household were interviewed (separately). Surveys were administered on Netbooks using the Blaise survey software. Following standard IPA procedure, 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*. Saliva samples were collected using the Salivette (Sarstedt, Germany), which has been used extensively in psychological and medical research (Kirschbaum and Hellhammer 1989), and more recently in randomized trials in developing countries similar to this one (Fernald and Gunnar 2009). It requires the respondent to chew on a sterile cellulose swab, which is then centrifuged and analyzed for salivary cortisol.

4.4 Power calculation

The sample size of 500 individuals in each of the treatment, control, and pure control conditions was chosen based on a power calculation, computed using data compiled by GiveDirectly, which showed that a sample of 1,000 individuals is sufficient to detect effect sizes of 0.2 SD for all treatment vs. pure control households with 80% power. The selected sample size leaves room for attrition up to 20% in each group. Different treatment arms within the treatment groups (male vs. female recipient, lump-sum vs. monthly, large vs. small transfers) can be compared with 60% power.

4.5 Risk and treatment of attrition

Attrition was not a significant concern in this study because it became evident early on in GD's work in Kenya that respondents were highly interested in maintaining relations with GiveDirectly in the hope of receiving future transfers (although these are never promised). Nevertheless, we used five approaches to control attrition. First, the survey contained a detailed tracking module developed by Innovations for Poverty Action (IPA), the NGO implementing the fieldwork. IPA and GD collaborated closely throughout the study to facilitate tracking. Second, we incentivized survey completion through a small appreciation gift (a jar of cooking fat); in addition, respondents earned money from the economic games in the survey. Third, in our power calculations, we allowed for an attrition rate of 20%. This is a conservative estimate, as we only observed 3% attrition between the two visits of the baseline. Finally, we control for attrition econometrically in the analysis, as detailed below.

5 Econometric specifications

5.1 Basic specifications

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

$$y_{\{i\}hvE} = \beta_0 + \beta_1 T_{hv} + \beta_2 S_{hv} + \varepsilon_{\{i\}hvt} \tag{1}$$

where $y_{\{i\}hv}$ is the outcome of interest for household h in village v, measured at endline ($t = E$); index i is included for outcomes measured at the level of the individual respondent, and omitted for outcomes measured at the household level. T_{hv} is a treatment indicator that takes value 1 for households which received a cash transfer ("treatment housholds") and 0 otherwise. S_{hv} is a dummy variable that takes value 1 for control households in treatment villages ("spillover households") and 0 otherwise. $\varepsilon_{\{i\}hvt}$ is an idiosyncratic error term. The omitted category is control households in pure control villages ("control households"). Thus, β_1 identifies the treatment effect for treated households relative to control households in control villages, and β_2 identifies within-village spillover effects by comparing control households in treatment villages to control households in pure control villages. To account for possible correlation in outcomes within 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 et al. 2011; Pepper 2000).

Note that, while the above specification allows us to estimate the treatment effect relative to control households in treatment villages, this comparison is better undertaken with a modified regression on a restricted sample which includes only treatment and spillover households and adds village-level fixed effects. The spillover treatment is the omitted group in this regression:

$$y_{\{i\}hvE} = \alpha_v + \beta_0 + \beta_1 T_{hv} + \varepsilon_{\{i\}ht} \tag{2}$$

Here, α_v is the village fixed effect, and β_1 identifies the treatment effect relative to spillover households. This specification provides more statistical power due to the inclusion of village fixed effects, but provides a lower bound on the treatment effect in the presence of spillovers in the same direction as the treatment effect. In this specification we cluster at the household level, the unit at which randomization ocurred for all households in this sample.

Following McKenzie (2012), where possible, we will condition on the baseline level of the household outcome, $y_{\{i\}hB}$, to improve statistical power. Note that this is only possible in equation 2 and not in equation 1 because baseline data was only collected from the treatment and spillover samples. The modified specification takes the following form:

$$y_{\{i\}hvE} = \alpha_v + \beta_0 + \beta_1 T_{hv} + \delta y_{\{i\}hB} + \varepsilon_{\{i\}ht} \tag{3}$$

5.2 Comparison of treatment arms

In the following, we present modifications on these basic specifications. We list these modifications with reference to equation 1, which identifies the treatment effect with respect to pure control villages, and the spillover effect. However, note that when comparing treatment to spillover households, we will apply these modifications to equation 2 and 3 instead.

To assess the heterogeneous impact of each cross-randomized treatment arm (recipient gender, transfer frequency, and magnitude of the total amount transferred), we will proceed as follows. First, the effect of making the transfer to the female vs. the male in the household is captured by the following model:

$$y_{\{i\}hvE} = \beta_0 + \beta_1 T^{\mathrm{F}}_{hv} + \beta_2 T^{\mathrm{M}}_{hv} + \beta_3 T^{\mathrm{W}}_{hv} + \beta_4 S_{hv} + \varepsilon_{\{i\}hvt} \tag{4}$$

Here, the variables T^{x} are indicator functions that specify the branch of the different treatment arms. Specifically, they indicate whether the transfer recipient is female (T^{F}), male (T^{M}), or that the gender of the recipient could not be randomized because the household only had one head (most commonly in the case of widows/widowers) (T^{W}).

To asses the effect of monthly vs. lump-sum transfers, note first that a subset of households originally assigned to receive \$300 in either lump-sum or monthly transfers was additionally randomly assigned to receive monthly transfers beginning in February 2012 to achieve a total transfer of \$1,100. 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 will compare only the groups which did not receive the \$1,100 transfers, as follows:

$$y_{\{i\}hvE} = \beta_0 + \beta_1 T_{hv}^{\text{MTH}} \times T_{hv}^{\text{S}} + \beta_2 T_{hv}^{\text{LS}} \times T_{hv}^{\text{S}} + \beta_3 T_{hv}^{\text{L}} + \beta_4 S_{hv} + \varepsilon_{\{i\}hvt} \tag{5}$$

In this specification, T^{MTH} and T^{LS} are indicator variables for having *originally* been assigned to receiving monthly or lump-sum transfers, respectively, and T^{S} and T^{L} are indicators for *later* being randomly assigned to receive the smaller vs. the larger of the two transfer amounts, respectively. Note that the group that was originally assigned to the lum-sum condition and was then additionally assigned to the "large" treatment began to receive a stream of transfers after February 2012, and was thus no longer unambiguouly lump-sum after this time.

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

$$y_{\{i\}hvE} = \beta_0 + \beta_1 T_{hv}^{\text{L}} + \beta_2 T_{hv}^{\text{S}} + \beta_3 S_{hv} + \varepsilon_{\{i\}hvt} \tag{6}$$

We will also estimate analogs of the models above which incorporate the relevant interaction terms of the treatment arms, and assess the differential effects of the treatment arms using the sample and control variables specified in 2 and 3.

5.3 Comparison of recipient vs. spouse within households

To assess whether cash transfers have differential impacts on the recipient compared to their spouse within the household, we will allow the impact of the transfer to vary between the individual in the household who receives the transfer and the spouse. The relevant specification takes the following form:

$$\begin{aligned} y_{ihvE} = \beta_0 &+ \beta_1 T_{hv}^{\text{SELF}} + \beta_2 T_{hv}^{\text{SPOUSE}} + \beta_3 T_{hv}^{\text{SINGLE}} + \beta_4 S_{hv}^{\text{SINGLE}} + \beta_5 S_{hv}^{\text{COUPLE}} \\ &+ \beta_6 PC_{hv}^{\text{SINGLE}} + \varepsilon_{ihvt} \end{aligned} \tag{7}$$

In this specification, T_{hv}^{SELF} is an indicator variable which takes value 1 for respondents who live with a spouse and received the transfer themselves, and 0 otherwise; T_{hv}^{SPOUSE} indicates the respondent's spouse received a transfer; and T_{hv}^{SINGLE} is an indicator for an individual who received a transfer and does not live with a spouse. For the spillover, S, and pure control, PC, the dummy variables S^{SINGLE} and PC^{COUPLE} indicate whether the respondent resides without or with a spouse, respectively. The omitted category is PC_{hv}^{COUPLE}. Thus, in this model, β_1 and β_2 identify the treatment effect for respondents who live with their spouse when they receive the transfer and when the spouse receives the transfer, respectively, and β_3 identifies the treatment effect for unmarried respondents who receive a transfer. As detailed in section 5.4, we will also estimate the mean outcome separately for single and joint control households to account for a potential main effect of household structure on outcomess. Finally, as before, we will additionally estimate the equation above using the sample and control variables specified in 2 and 3.

5.4 Heterogeneous effects

We will test whether the impact of the cash transfers varies with pre-determined household and individual characteristics, measured at baseline and denoted by X, which may be either a household or individual level characteristic. Note that the pure control group was not included in the baseline; we will therefore estimate, for household-level outcomes:

$$y_{\{i\}hvE} = \alpha_v + \beta_0 + \beta_1 T_{hv} + \beta_2 X_{\{i\}hv} + \beta_3 T_{hv} \times X_{\{i\}hv} + \varepsilon_{\{i\}hvt} \tag{8}$$

As before, standard errors in this type of specification will be clustered at the household level.

The following model incorporates heterogeneous effects in the equation distinguishing between recipient and non-recipient effects:

$$\begin{aligned} y_{\{i\}hvE} = \alpha_v + \beta_0 &+ \beta_1 T_{hv}^{\text{SELF}} + \beta_2 T_{hv}^{\text{SPOUSE}} + \beta_3 T_{hv}^{\text{SINGLE}} + \beta_4 S_{hv}^{\text{SINGLE}} \\ &+ \beta_5 T_{hv}^{\text{SELF}} \times X_{\{i\}hv} + \beta_6 T_{hv}^{\text{SPOUSE}} \times X_{\{i\}hv} \\ &+ \beta_7 T_{hv}^{\text{SINGLE}} \times X_{\{i\}hv} + \varepsilon_{\{i\}hvt} \end{aligned} \tag{9}$$

We will also estimate analogs of these specifications which incorporate the main effects and all interaction terms of additional treatment arms (gender or recipient, frequency of transfer, and aggregate size of the transfer).

This analysis will take the form:

$$
\begin{aligned}
y_{\{i\}hvE} = {} & \alpha_v + \beta_0 + \beta_1 T^{\mathrm{F}}_{hv} + \beta_2 T^{\mathrm{M}}_{hv} + \beta_3 T^{\mathrm{W}}_{hv} + \beta_4 T^{\mathrm{F}}_{hv} \times X_{\{i\}hv} + \beta_5 T^{\mathrm{M}}_{hv} \times X_{\{i\}hv} \\
& + \beta_6 T^{\mathrm{W}}_{hv} \times X_{\{i\}hv} + \beta_8 X_{\{i\}hv} + \beta_9 S_{hv} \\
& + \beta_{10} S_{hv} \times X_{\{i\}hv} + \varepsilon_{\{i\}hvt}
\end{aligned} \tag{10}
$$

$$
\begin{aligned}
y_{\{i\}hvE} = {} & \alpha_v + \beta_0 + \beta_1 T^{\mathrm{MTH}}_{hv} + \beta_2 T^{\mathrm{LS}}_{hv} + \beta_3 T^{\mathrm{MTH}}_{hv} \times X_{\{i\}hv} + \beta_4 T^{\mathrm{LS}}_{hv} \times X_{\{i\}hv} \\
& + \beta_5 X_{\{i\}hv} + \varepsilon_{\{i\}hvt}
\end{aligned} \tag{11}
$$

$$
\begin{aligned}
y_{\{i\}hvE} = {} & \alpha_v + \beta_0 + \beta_1 T^{\mathrm{L}}_{hv} + \beta_2 T^{\mathrm{S}}_{hv} + \beta_3 T^{\mathrm{L}}_{hv} \times X_{\{i\}hv} + \beta_4 T^{\mathrm{S}}_{hv} \times X_{\{i\}hv} \\
& + \beta_5 X_{\{i\}hv} + \varepsilon_{\{i\}hvt}
\end{aligned} \tag{12}
$$

Individual outcomes may be tested for heterogeneous impacts both in terms of variables which vary at the household level, X_{hv}, as well as those that vary at the individual level, X_{ihv} . In cases where the outcome variable is at the household level and heterogeneity is at the individual level, the value of the individual-level variable for the transfer recipient (rather than their spouse) as the source of heterogeneity.

Dimensions of heterogeneous effects

Heterogeneous effects will be considered along the following dimensions:

1. Respondent gender
2. Household type (single vs. joint control)
3. Baseline expenditure, asset levels, and land holdings
4. Baseline food security
5. Education level of primary household members
6. Ownership of non-agricultural enterprise at baseline
7. Risk and time preference and decreasing impatience
8. Psychological welfare
9. Domestic violence and bargaining power

5.5 Temporal dynamics of the treatment effect

Both in the lump-sum and monthly conditions, households received transfers at different points in time; the transfers were spread out over the period June 2011 – January 2013. This temporal heterogeneity creates an opportunity to assess the temporal dynamics of the treatment effect. We achieve this in two ways.

First, we perform a median split on the treatment group according to either the half-way date between each recipient's first and last transfer from GD, or the last transfer received from GD, and define indicator variables $T^{\text{EARLY}}_{\{i\}hv}$ and $T^{\text{LATE}}_{\{i\}hv}$ for households which received transfers early vs. late in the study, respectively. We then estimate the following model:

$$y_{\{i\}hvE} = \beta_0 + \beta_2 T^{\text{EARLY}}_{hv} + \beta_3 T^{\text{LATE}}_{hv} + \beta_4 S_{hv} + \varepsilon_{\{i\}hvt} \tag{13}$$

The difference between coefficients β_2 and β_3 identifies how effect of transfers unfolds over time. To assess whether the temporal dynamics of the treatment effect are affected by individual treatment arms, we estimate the following models:

$$\begin{aligned} y_{\{i\}hvE} =& \beta_0 + \beta_1 T^{\text{F}}_{hv} \times T^{\text{EARLY}}_{hv} + \beta_2 T^{\text{M}}_{hv} \times T^{\text{EARLY}}_{hv} + \beta_3 T^{\text{W}}_{hv} \times T^{\text{EARLY}}_{hv} \\ &+ \beta_4 T^{\text{F}}_{hv} \times T^{\text{LATE}}_{hv} + \beta_5 T^{\text{M}}_{hv} \times T^{\text{LATE}}_{hv} + \beta_6 T^{\text{W}}_{hv} \times T^{\text{LATE}}_{hv} \\ &+ \beta_4 S_{hv} + \varepsilon_{\{i\}hvt} \end{aligned} \tag{14}$$

$$\begin{aligned} y_{\{i\}hvE} =& \beta_0 + \beta_1 T^{\text{MTH}}_{hv} \times T^{\text{S}}_{hv} \times T^{\text{EARLY}}_{hv} + \beta_2 T^{\text{LS}}_{hv} \times T^{\text{S}}_{hv} \times T^{\text{EARLY}}_{hv} + \beta_3 T^{\text{L}}_{hv} \times T^{\text{EARLY}}_{hv} \\ &+ \beta_4 T^{\text{MTH}}_{hv} \times T^{\text{S}}_{hv} \times T^{\text{LATE}}_{hv} + \beta_5 T^{\text{LS}}_{hv} \times T^{\text{S}}_{hv} \times T^{\text{LATE}}_{hv} + \beta_6 T^{\text{L}}_{hv} \times T^{\text{LATE}}_{hv} \\ &+ \beta_7 S_{hv} + \varepsilon_{\{i\}hvt} \end{aligned} \tag{15}$$

$$\begin{aligned} y_{\{i\}hvE} =& \beta_0 + \beta_1 T^{\text{L}}_{hv} \times T^{\text{EARLY}}_{hv} + \beta_2 T^{\text{S}}_{hv} \times T^{\text{EARLY}}_{hv} \\ &+ \beta_3 T^{\text{L}}_{hv} \times T^{\text{LATE}}_{hv} + \beta_4 T^{\text{S}}_{hv} \times T^{\text{LATE}}_{hv} \\ &+ \beta_5 S_{hv} + \varepsilon_{\{i\}hvt} \end{aligned} \tag{16}$$

In addition, we will distinguish the early vs. late effects of transfers as a function of whether the respondent or the spouse is the recipient:

$$\begin{aligned} y_{ihvE} =& \beta_0 + \beta_1 T^{\text{SELF}}_{hv} \times T^{\text{EARLY}}_{hv} + \beta_2 T^{\text{SPOUSE}}_{hv} \times T^{\text{EARLY}}_{hv} + \beta_3 T^{\text{SINGLE}}_{hv} \times T^{\text{EARLY}}_{hv} \\ &+ \beta_4 T^{\text{SELF}}_{hv} \times T^{\text{LATE}}_{hv} + \beta_5 T^{\text{SPOUSE}}_{hv} \times T^{\text{LATE}}_{hv} + \beta_6 T^{\text{SINGLE}}_{hv} \times T^{\text{LATE}}_{hv} \\ &+ \beta_7 S^{\text{SINGLE}}_{hv} + \beta_8 S^{\text{COUPLE}}_{hv} + \beta_9 PC^{\text{SINGLE}}_{hv} + \varepsilon_{ihvt} \end{aligned} \tag{17}$$

In addition to the endline survey, we conducted a midline survey between November 2011 – May 2012, in which a subset of respondents answered an abridged version of the questionnaire. For outcomes where both endline and midline data is available, we estimate the following model, where $\mathbb{1}(t = M)$ is an indicator variable specifying data from the midline:

$$\begin{aligned} y_{\{i\}hvt} =& \alpha_v + \beta_0 + \beta_2 T_{hv} \mathbb{1}(t = M) + \beta_3 T_{hv} \mathbb{1}(t = E) \\ &+ \delta \mathbb{1}(t = E) + \varepsilon_{\{i\}hvt} \end{aligned} \tag{18}$$

5.6 General equilibrium effects at the village level

As cash transfers may cause general equilibrium effects at the village level, we collected village level outcomes from multiple individuals in villages where some individuals received transfers and villages where no individuals received transfers. We will regress village level outcomes (y) as reported by individuals (denoted by i) on an indicator variable for whether that individual resides in a village that received transfers or not. Standard errors will be clustered at the village level in these specifications.

$$y_{ivE} = \beta_0 + \beta_1 T_v + \varepsilon_{ivt} \tag{19}$$

5.7 Attrition

To assess whether attrition potentially confounds our results, we proceed as follows. First, we define:

$$attrit_{hv} = \mathbb{1}\left(\sum_{j=1}^{J}\sum_{k=1}^{K}\left(\mathbb{1}\left(y_{hvB}^{jk} \neq \text{missing}\right)\right) > 0 \quad \cap \quad \sum_{j=1}^{J}\sum_{k=1}^{K}\left(\mathbb{1}\left(y_{hvB}^{jk} \neq \text{missing}\right)\right) = 0\right) \tag{20}$$

Here, y^{jk} is the $\{j, k\}$ element in the $j \times k$ matrix of outcomes $\mathbf{Y}$ and represents outcome k in outcome group j. Thus, $attrit_{hv}$ indicates whether household h was surveyed at baseline but not at endline. We then calculate overall attrition by treatment group. Note that this calculation applies only to the treatment and spillover sample, since the pure control sample was not surveyed at baseline.

We then assess the severity of attrition using three approaches. First, equation 21 estimates whether the magnitude of attrition is different for treatment and control households:

$$attrit_{hv} = \alpha_v + \beta_0 + \beta_1 T_{hv} + \varepsilon_{hvt} \tag{21}$$

Second, equation 22 assesses whether attrition households are different in terms of a comprehensive range of baseline characteristics:

$$attrit_{hv} = \alpha_v + \beta_0 + \beta_1 y_{hvB} + \varepsilon_{hvt} \tag{22}$$

And third, equation 23 measures whether the baseline characteristics of attrition households in the treatment group are significantly different from those in the control group. The sample for regression will be restricted to attrition households:

$$y_{hvB} = \beta_0 + \beta_1 T_{hv} + \varepsilon_{hvt} \tag{23}$$

We will also estimate analogs of equations 21, 22, and 23 at the individual level, clustering standard errors at the village level. If worrying levels of attrition are found, we will adjust for the potential effect of such attrition using Lee bounds.

5.8 Accounting for multiple inference

As cash transfers are likely to impact a large number of economic behaviors and dimensions of welfare, and given that our survey instrument often included several questions related to a single behavior or dimension, we will account for multiple hypotheses by using outcome variable indices and family-wise p-value adjustment.

We have catalogued below the primary groups of outcomes that we intend to consider in the analysis outlined above. For each of these outcome groups, we will construct indices (where possible) and for each of the components of these indices, and will report both unadjusted p-values as well as p-values corrected for multiple comparisons using the Family-Wise Error Rate.

5.8.1 Construction of indices

To keep the number of outcome variables low and thus allow for greater statistical power even after adjusting p-values for multiple inference, we will construct indices for several of our groups of outcome variables. To this end, we will follow the procedure proposed by Anderson (2008), which is reproduced below:

First, for each outcome variable y_{jk}, where j indexes the outcome group and k indexes variables within outcome groups, we re-code the variable such that high values correspond to positive outcomes.

We then compute the covariance matrix $\hat{\mathbf{\Sigma}}_{\mathbf{j}}$ for outcomes in outome group j, which consists of elements:

$$\hat{\Sigma}_{jmn} = \sum_{i=1}^{N_{jmn}} \frac{y_{ijm} - \bar{y}_{jm}}{\sigma^y_{jm}} \frac{y_{ijn} - \bar{y}_{jn}}{\sigma^y_{jn}} \tag{24}$$

Here, N_{jmn} is the number of non-missing observations for outcomes m and n in outcome group j, $\bar{y}_{jm}$ and $\bar{y}_{jn}$ are the means for outcomes m and n, respectively, in outcome group j, and σ^y_{jm} and σ^y_{jn} are the standard deviations in the pure control group for the same outcomes.

Next, we invert the covariance matrix, and define weight w_{jk} for each outcome k in outcome group j by summing the entries in the row of the inverted covariance matrix corresponding to that outcome:

$$\mathbf{\hat{\Sigma}_j^{-1}} = \begin{bmatrix} c_{j11} & c_{j12} & \cdots & c_{j1K} \\ c_{j21} & c_{j22} & \cdots & \cdots \\ \vdots & \vdots & \ddots & \ddots \\ c_{jK1} & \vdots & \ddots & c_{jKK} \end{bmatrix} \tag{25}$$

$$w_{jk} = \sum_{l=1}^{K_j} c_{jkl} \tag{26}$$

Here, K_j is the total number of outcome variables in outcome group j. Finally, we transform each outcome variable by subtracting its mean and dividing by the control group standard deviation, and then weighting it with the weights obtained as described above. We denote the result $\hat{y}_{ij}$ because this transformation yields a generalized least squares estimator (Anderson 2008).

$$\hat{y}_{ij} = \left(\sum_{k \in \mathbb{K}_{ij}} w_{jk} \right)^{-1} \sum_{k \in \mathbb{K}_{ij}} w_{jk} \frac{y_{ijk} - \bar{y}_{jk}}{\sigma^y_{jk}} \tag{27}$$

Here, $\mathbb{K}_{ij}$ denotes the set of non-missing outcomes for observation i in outcome group j. The specifications described in Section 5 will use these transformed outcome variables wherever this is specified in Section 6.

5.8.2 Family-wise Error Rate

Because combining individual outcome variables in indices as described above still leaves us with multiple outcome variables (viz. separate index variables for health, education, etc.), we additionally adjust the p-values of our coefficients of interest for multiple statistical inference. These coefficients are those on the treatment dummies in the basic specifications, or those on the dummies for individual treatment arms. To this end, we proceed as follows, reproduced again from Anderson (2008).

First, we compute naïve p-values for all index variables $\hat{y}_j$ of our j main outcome groups (see Section 6), and sort these p-values in ascending order, i.e. such that $p_1 < p_2 < \cdots < p_J$.

Second, we follow Anderson's (2008) variant of Efron & Tibshirani's (1993) non-parametric permutation test: for each index variable $\hat{y}_j$ of our j main outcome groups (see Section 6), we randomly permute the treatment assignments across the entire sample, and estimate the model of interest to obtain the p-value for the coefficient of interest. We enforce monotonicity in the resulting vector of p-values $[p_1^*, p_2^*, \cdots p_J^*]'$ by computing $p_r^{**} = \min\{p_r^*, p_{r+1}^*, \cdots p_J^*\}$, where r is the position of the outcome in the vector of naïve p-values.

We then repeat this procedure 10,000 times. The non-parametric p-value, p_r^{fwer*}, for each outcome is the fraction of iterations on which the simulated p-value is smaller than the observed p-value. Finally we enforce monotonicity again: $p_r^{fwer} = \min\{p_r^{fwer*}, p_{r+1}^{fwer*}, \cdots p_J^{fwer*}\}$. This yields the final vector of family-wise error-rate corrected p-values. We will report both these p-values and the naïve p-values. Within outcome groups, we report naïve p-values for individual outcome variables other than the indices.

6 Outcome variables

In the following, we list the outcome variables, by outcome group, which we will consider. Variables and outcome groups marked by an asterisk (*) will be excluded when adjusting for multiple testing, either because we have weak *a priori* hypotheses about them, because they partly overlap with other variables which are included when adjusting for multiple testing, because they are of lesser interest than other variables, or because the variables are only of interest for particular treatment arms (e.g. domestic violence variables when females receive the transfer).

6.1 Household & Individual

1. ***Assets***
   (a) Moveable assets
      i. Livestock
         A. Cows
         B. Small livestock
         C. Birds
      ii. Furniture
      iii. Agricultural tools

    iv. Radio or TV
    v. Other assets
    vi. Savings

  (b) Land owned

  (c) House has non-thatch roof

  (d) House has non-mud floor

  (e) House has non-mud walls

  (f) House has electricity

  (g) House has toilet or pit latrine

  Index: Total value of movable assets

2. ***Expenditure***

  (a) Food

    i. Food own production
    ii. Food bought
    iii. Meat & fish
    iv. Fruit & vegetables
    v. Other food

  (b) Temptation good expenditure

  (c) Medical expenditure

    i. Medical expenditure (respondent)
    ii. Medical expenditure (spouse)
    iii. Medical expenditure (children)

  (d) Education expenditure

  (e) Durables expenditure

  (f) House expenditure

  (g) Social expenditure

  (h) Other expenditure

  Index: Total expenditure (variables a.-h.)

3. ***Food security***

   (a) Meals skipped (adults)
   (b) Whole days without food (adults)
   (c) Meals skipped (children)
   (d) Whole days without food (children)
   (e) Eat less preferred/cheaper foods
   (f) Rely on help from others for food
   (g) Purchase food on credit
   (h) Hunt, gather wild food, harvest prematurely
   (i) Beg because not enough food in the house
   (j) All members eat two meals
   (k) All members eat until content
   (l) Number of times ate meat or fish
   (m) Enough food in the house for tomorrow?
   (n) Respondent slept hungry
   (o) Respendent ate protein
   (p) Propotion of HH who ate protein
   (q) Proportion of children who ate protein

   Food security index (children)*: weighted standardized average of variables c., d., q.

   Food security index (household): weighted standardized average of variables a.-q.

4. ***Psychological and neurobiological outcomes***

   (a) Depression (CESD)
   (b) Worries
   (c) Stress (Cohen)
   (d) Happiness (WVS)
   (e) Life satisfaction (WVS)
   (f) Cortisol

(g) Trust (WVS)

(h) Locus of control (Rotter + WVS)

(i) Optimism (Scheier)

(j) Self-esteem (Rosenberg)

Index: weighted standardized average of variables a.-f.

5. ***Intrahousehold bargaining and domestic violence***

   (a) Physical violence dummy

   (b) Sexual violence dummy

   (c) Emotional violence dummy

   (d) Justifiability of violence score

   (e) Male-focused attitudes score

   (f) Male makes decisions dummy

   (g) Proportion choosing money for spouse vs. self

   Violence index*: weighted standardized average of variables a.-c.

   Attitude index*: weighted standardized average of variables d.-e.

   Overall intrahousehold index: weighted standardized average of violence and attitude indices

6. ***Health***

   (a) Medical expenses per episode (entire HH)

   (b) Medical expenses per episode (spouses)

   (c) Medical expenses per episode (children)

   (d) Proportion of household sick/injured

   (e) Proportion of children sick/injured

   (f) Proportion of sick/injured who could afford treatment

   (g) Average number of sick days per HH member

   (h) Proportion of illnesses where doctor was consulted

   (i) Proportion of newborns vaccinated

(j) Proportion of children < 14 getting checkup

(k) Proportion of children < 5 who died

(l) Children's anthropometrics index

i. BMI

ii. Height for age

iii. Weight for age

iv. Upper-arm circumference

Children's health index*: weighted standardized average of variables c., e., i.-l.

Household health index: weighted standardized average of variables a., c.-f., h.-l.

7. ***Education***

(a) Total education expenditure

(b) Education expenditure per child

(c) Proportion of school-aged children in school

(d) School days missed for economic reasons, per child

(e) Income-generating activities per school-aged child > 6

Index variable: weighted standardized average of variables b.-c.

8. ***Enterprise variables***

(a) Agricultural income (total)

i. Agricultural income (own consumption, total)

A. Agricultural income (own consumption, harvest)

B. Agricultural income (own consumption, animals)

ii. Agricultural income (sales, total)

A. Agricultural income (sales, harvest)

B. Agricultural income (sales, animal products)

C. Agricultural income (sales, animals)

(b) Enterprise profit (6 months)

(c) Enterprise revenue (1 month)

(d) Enterprise revenue (typical month)

(e) New non-agricultural business owner (dummy)
(f) Non-agricultural business owner (dummy)
(g) Number of employees
(h) Value of investment in non-agricultural business

Index variable: Agricultural and enterprise income (total)

9. ***Financial variables****

(a) Value of outstanding loans
(b) Unable to pay loans (12 months)
(c) Value of remittances sent
(d) Value of remittances received
(e) Net remittances

10. ***Preferences****

(a) Impatience
(b) Decreasing impatience
(c) Risk aversion
(d) Other-regarding preferences
(e) Favors cash transfers from NGOs or government
(f) Random allocation is fair
(g) I am likely to receive the benefit if random allocation is used

11. ***Temptation goods****: List method

12. ***Targeting****

6.2 Village-level*

1. ***Prices***

(a) Prices of individual standard items

Index: sum of prices for above list

2. ***Wages***

    (a) Likelihood of working for another villager in the same village (spillover vs. pure control group only)

    (b) Average daily wage for working for another villager in the same village (spillover vs. pure control group only)

3. ***Conflict***

    (a) Number of conflict episodes in the village in the past year

    (b) Multinomial dummy for having less, the same, or more conflict in the village compared to a year ago

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