

PRE-ANALYSIS PLAN

UNCONDITIONAL CASH TRANSFERS IN KIRYANDONGO REFUGEE SETTLEMENT, UGANDA

August 2020

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1. BACKGROUND INFORMATION

INTRODUCTION

This document outlines the pre-analysis plan (PAP) for an impact evaluation of GiveDirectly's unconditional cash transfer program in Kiryandongo, Uganda. The PAP was finalized after baseline data collection.

ORGANIZATION OVERVIEW

GiveDirectly is an international nonprofit that implements unconditional cash transfer programs directed at communities living in extreme poverty. GiveDirectly implements a range of transfer designs across its projects, including one-off amounts that roughly equal the annual income of poor households. To ensure the integrity of the process, GiveDirectly employs a set of independent checks to verify that recipients meet the eligibility criteria and were not asked to pay bribes during the selection process.

PROJECT OVERVIEW

GiveDirectly aims to understand the role of unconditional cash transfers as one possible route to improving the self-reliance of refugees in protected displacement contexts. The program intends to provide around USD\$1000 in unconditional cash transfers to all of the ~10,000 refugee households in Kiryandongo Refugee Settlement and about 5,000 host community households in Kiryandongo district, Uganda.

UNHCR estimates indicate that there are around 1.4 million refugees in Uganda.¹ Many of the forcibly displaced peoples are expected to remain in Uganda over the long-term given the prolonged character of the conflicts in their countries of origin. The expected long-term stay and progressive refugee policies in Uganda designate the country as an appropriate setting to analyze the lasting impacts of cash transfers.

GiveDirectly recently conducted a pilot in Kyaka II Settlement in Uganda. Cash transfers were provided to 4,371 refugee and host community households. An internal evaluation of the pilot concluded that the transfer had positive indicative results and was operationally feasible. The project in Kiryandongo will build on the pilot by conducting a larger and more rigorous independent study. The impact evaluation of the transfer will be conducted by IDinsight and consists of a randomized control trial (RCT) with refugee households, a cross-sectional study with host recipients, and a longitudinal qualitative study with both refugee and host community households. The mixed methods approach will examine the impact of the cash transfers by carefully identifying what changed as well as why and how the change happened. Results of the program will inform future decision-making regarding cash transfers by donors and implementing organizations.

The timing of this study overlaps with the global COVID-19 pandemic of 2019-2020. The first transfers to our treatment group took place in late February 2020. On March 30, Uganda's president ordered a nationwide lockdown, which restricted almost all movement and commerce throughout

¹https://reporting.unhcr.org/sites/default/files/UNHCR%20Uganda%20Fact%20Sheet%20%20-%20January%202020_0.pdf

the country. This had an immediate effect on livelihoods in Kiryandongo, as many members in the settlement make money by traveling for casual labor or through running small enterprises. Therefore, the study will provide evidence as to the efficacy of cash transfers in the context of the COVID crisis.

2. RESEARCH QUESTIONS

IDinsight will answer the following research questions in this evaluation:

1. What is the impact of large, unconditional cash transfers on refugee households for outcomes such as income, assets, consumption, enterprise, food security and psychological well-being?
2. What do any changes caused by the cash transfers mean for the lives of refugee households and how did those changes come about?
3. What is the effect of cash transfers on host communities, and the relationship between host and refugee communities?

3. EVALUATION METHODOLOGY

STUDY DESIGN

The study has three major parts:

1. A randomized control trial will be used to measure the causal impact of the cash transfers for refugees
2. A longitudinal qualitative study to examine how and why change does (or doesn't) take place in refugee and host community households
3. An observational study will examine cash transfer recipients from the host community.

This pre-analysis plan primarily relates to the randomized control trial. Section 6 briefly outlines the analysis approach for the longitudinal qualitative study.

The randomized control trial will use a phased-in design. Under GiveDirectly's program, eligible households in the refugee settlement have been divided into 24 cohorts through a public lottery (see 'Assignment method' below). Cash transfers will be distributed by cohort, with cohort 1 receiving its cash transfers in the first, cohort 2 receiving its cash transfer second etc. Cohorts are expected to be phased-in on a monthly basis, though in practice operational factors will cause some deviations from the planned schedule. For the randomized control trial's phased-in design, the treatment group will consist of households in cohorts 1, 2, and perhaps 3. The control group is households in cohorts 17-20.

SAMPLING

The treatment and control groups have been constructed by a random allocation of refugee households through the public lottery. Randomization will happen at the household level. For the purposes of this study, a household is defined as a group of individuals who have registered as a single household under the UNHCR/OPM system.

SAMPLING FRAME AND SAMPLE SIZE

The sampling frame will be built on data provided by UNHCR which encompasses information on all (~10,000) registered refugee households living in Kiryandongo. UNHCR and GiveDirectly have identified around 1005 households as PSN (persons with specific needs), who will receive the cash transfer first but will not be a part of the study. The remaining 8886 refugee households were placed in a public lottery for random assignment.

ASSIGNMENT METHOD (LOTTERY)

GiveDirectly and IDinsight ran a public lottery to assign each non-PSN household into one of 24 cohorts. The lottery was conducted during the World Food Programme's (WFP) monthly food and cash distributions in August 2019. The lottery was run publicly to ensure transparency and buy-in of the community. This approach was recommended by community leaders.

During the lottery, each household was given the opportunity to draw a cohort number. A household representative drew a single number from a bucket containing 24 ping-pong balls labeled 1 to 24. The number that each refugee picked corresponds to their assigned cohort number. After each draw, the ping-pong ball was returned into the bucket for the next participant. This lottery with replacement approach was chosen to ensure that each respondent had the same chance of picking a number between 1 and 24.

574 households that did not attend food and cash distribution were randomized in a separate public event, during which community leaders picked the cohort number on behalf of those remaining households.

One of the limitations of this large public lottery randomization process came from the practical challenge of ensuring no tampering from interested groups. The early cohorts 1 and 2 together comprise 9.56% of the non-PSN settlement population, compared to the expected value of 8.33% (2/24) and are thus over-represented. The later cohorts of 23 and 24, on the other hand, together represent 7.5% of the lotterized households and are thus under-represented by 0.83% compared to the expected value of 8.33%. These differences from the expected value are statistically significant at $p < 0.01$, indicating some tampering with the lottery process. Our control cohorts (17 to 20) together represent 16% of the non-PSN settlement population, compared to the expected value of 16.67%. The difference is not statistically significant ($p = 0.0886$). Despite these disappointing findings from the lottery, we believe we can still achieve credible results given the limited magnitude of tampering and the ability to control for some of the differences.

SAMPLE SIZE

The treatment group consists of refugee households in cohorts 1 and 2 who will have received their cash transfer before the endline survey (note that we may add some households from cohort 3 to the treatment group if non-compliance or attrition cause statistical power to dip to unacceptable levels). The control group consists of a random sample of households from cohorts 17-20 who will have not yet received their cash transfer at the endline survey. There are no additional criteria beyond being a GiveDirectly cash transfer recipient, the assignment to treatment timing through GiveDirectly's public randomization, and the ability and willingness to provide informed consent to participate in the study.

Prior to the lottery process, we planned to sample approximately 1500 households (750 treatment, 750 control). This would have resulted in a minimal detectable effect size (MDES) of 0.145 SDs with 80% power. This effect size is less than the 0.18 SD effect of business revenue observed in a previous GiveDirectly evaluation (Haushofer and Shapiro 2016). IDinsight has powered the study on business outcomes, as the smallest effect size is expected for business outcomes compared to other outcomes of interest.

During baseline data collection, IDinsight learned that some physical households will receive more than one cash transfer. This is because cash transfers will be made based on a list of households as registered with UNHCR. However, the group of people that live together and the “household” registered with UNHCR are not always the same. This would be problematic for the study as some of our control households live with family members who will receive a cash transfer before our endline survey, essentially meaning these control households will be treated. Such “spillover” households in the control group, as well as equivalent households from the treatment group, were therefore dropped from the study.² As a result of both the issue of multiple transfers per household and challenges locating some of the sampled households, our sample size dropped to 1264 at baseline.³ This reduced sample size will still yield an MDES of 0.16 SD.

DATA COLLECTION

The data will be collected in two rounds, the baseline and the endline. The baseline took place from September to November 2019, immediately after the lottery process, but before the distribution of the cash transfer to study households. The baseline survey instrument has been shortened compared to the endline instrument, containing a streamlined assets module and no section on household savings. The average survey time was about 120 minutes.

The endline data collection is expected to take place around 12 months later, from October to November 2020.

IDinsight will collect the data via enumerator-administered in-person surveys using tablets and the software SurveyCTO.

STUDY TIMELINE

The study will run from July 2019 to early 2021. Note that exact timing of the endline survey is still to be determined, with final timing to be decided based on the feasibility of fieldwork given COVID and the date of the Ugandan Presidential election in early 2021 and possible security risks associated with it.

² This means that all households who live with someone who picked number 1 to 12 or a PSN household were dropped.

³ 642 households in the treatment group and 622 households in the control group.

4. ANALYTICAL MODEL

ECONOMETRIC SPECIFICATION, ESTIMATION STRATEGY

The primary question of this study looks at changes in household welfare captured in terms of household consumption, asset ownership, business revenue⁴. The unit of analysis will be at the household level and individual level, whereas the unit of treatment is at the household level.

The specification used for the main outcomes is as follows:

$$y_{ih;t=1} = \beta_0 + \beta_1 * T_h + \beta_2 * X_{ih} + \delta_1 y_{ih;t=0} + \delta_2 M_{ih} + \varepsilon_{ih}$$

Here, h indexes the household. As we collect data on individual household members, including the survey respondent, i indexes individual members. y_{ih} is the outcome of interest for household h or respondent i ; T_i is an indicator that takes value 1 if household h has been assigned to treatment, and 0 otherwise; β_0 is a constant; X_{ih} is a vector of baseline covariates. Following McKenzie (2012), we also condition on the baseline values of the outcome variable $y_{ih;t=0}$ to improve statistical power; When $y_{ih;t=0}$ is missing for an observation, we include an indicator term for missingness M_{ih} and replace $y_{ih;t=0}$ with its mean ε_{ih} is an idiosyncratic error term. Therefore, β_1 estimates the treatment effect in this specification.

The above equation will be estimated using OLS, with Huber-White robust standard errors. Analysis will be done using the statistical software Stata.

OUTCOME VARIABLES

A cash transfer program can impact the welfare of refugees through multiple channels. In terms of our study outcomes, IDinsight will narrow down the list of indicators in order to conduct effective inference. The primary outcome is the *economic* well-being of our households. This will be measured using three primary indicators:

- **Household consumption:** This indicator measures the total monthly value of household consumption and spending including food consumption⁵, consumption of temptation goods, and spending on non-food items. Specifically, the indicator will be constructed as follows:
 - Sum of food consumed by the household at home and away from home in the last 7 days, multiplied by 4.3 to obtain a monthly value
 - Sum of temptation goods (alcohol, tobacco, lottery and gambling) consumed by the household at home and away from home in the last 7 days, multiplied by 4.3 to obtain a monthly value
 - Sum of frequent non-food spending in the last 7 days (e.g., fuel and transport, etc.), multiplied by 4.3 to obtain a monthly value

⁴ As noted below, we also investigate supplementary outcomes to explore other dimensions of household welfare.

⁵ Please note that for food consumption, the survey probed for purchased, home-produced, and in-kind food.

- Sum of frequent non-food spending in the last 4 weeks (e.g., water, rent, etc.), divided by 4 and multiplied by 4.3 to obtain a monthly value
- Sum of infrequent non-food spending in the last 12 months (e.g., clothing, education expenses and taxes, etc.), divided by 12 to obtain a monthly value
- All total values are added, divided by the World Bank's 2018 Ugandan Shillings (UGX) and United States Dollar (USD) Purchasing Power Parity (PPP) conversation rate, and winsorized at the top 1%
- **Household assets (including savings and loans):** The assets indicator estimates the total *current market value* of common moveable and unmoveable household assets (including livestock), savings and the net balance of currently outstanding loans given and taken. All total values are added, divided by the World Bank's 2018 Ugandan Shillings (UGX) and United States Dollar (USD) Purchasing Power Parity (PPP) conversation rate, and winsorized at the top 1%
- **Agricultural and non-agricultural business revenue:** This indicator measures the total monthly revenue from all formal and informal enterprises fully or partially owned by any household member including agricultural (based on crops sold) and non-agricultural business revenue. Monthly agricultural revenue is calculated by collecting total agricultural revenue from the six months prior to the survey and dividing by six. The total revenue from all agricultural and non-agricultural businesses are divided by the World Bank's 2018 Ugandan Shillings (UGX) and United States Dollar (USD) Purchasing Power Parity (PPP) conversation rate, and winsorized at the top 1%.

Beyond these main outcomes, IDinsight will present results on the components of these indices to provide suggestive evidence about mechanisms. We will investigate the following supplementary outcomes to understand other aspects of welfare:

- **Psychological well-being:** This individual-level outcome variable is based on a composite index following the procedure proposed by Anderson (2008) and adapted from Haushofer and Shapiro (2016). It comprises the standardized weighted average of the following components:
 - The total score of ten elements of the Center for Epidemiologic Studies Depression Scale (CES-D scale), which surveys the frequency of different types of feelings among respondents
 - The total score of the four elements of the Cohen's stress scale
 - The numerical responses to the World Value Survey's sections that focus on happiness and life satisfaction; and
 - The total score from a thirteen-element-custom-worries questionnaire
 - All components are coded in a way that higher values represent higher levels of well-being
- **Food security:** The food security index is a standardized weighted index which measures the number of days in the last seven days that adults and children: skipped or cut meals; went entire days without food; and went to bed hungry. It also includes the number of meals eaten yesterday that contained protein. Responses are appropriately signed so that

higher values represent better food security. The index is taken from Egger et al. (2019) to allow comparability across GiveDirectly projects.⁶

- **Female empowerment:** This individual-level composite index comprises the standardized weighted average of a time-use measure that captures the proportion of hours spent on household and care work by female respondents on the day before the survey (negatively coded), the proportion of school-aged girls enrolled in school, and the average number of days school-aged girls attended school in the last five days when school was open. School-aged girls are between 6 and 18 years old.⁷ The index is created using the inverse-covariance weighting technique of Anderson (2008).
- **Business ownership:** This indicator is based on a binary variable that takes the value 1 if the respondent's household has one or more businesses as of endline date.
- **Employment status of survey respondent:** This individual-level outcome measures whether the survey respondent is in formal or informal employment or self-employment. This is measured for households with a working-age (15-64) respondent.⁸
- **Migration:** These outcomes measure the actual migration pattern of the household. We measure the number of household members who have migrated out of the settlement since baseline, and number of household members who have migrated into the settlement since baseline.
- **Household composition:** This outcome measures the size of households and the dependency ratio as scattered sub-outcomes:
 - Household size: Total number of household members
 - Dependency ratio: ratio of dependents – people younger than 15 or older than 64 – to the working aged population (ages 15-64).

We will also explore two community level outcomes as part of the study, as outlined below:

- **Social cohesion:** Community level data is collected about relationships within the refugee settlement and also among refugees and host communities. This composite index is based on a custom social cohesion questionnaire covering a subjective rating of the relationships for different groups of interest (refugee-host relations, inter-refugee relations, and intra-family relations) and the self-reported frequency of disputes between refugee and host communities. It is coded in a way that higher values represent higher levels of social cohesion.
- **Safety and security:** This composite index is based on a custom safety and security questionnaire covering self-reported incidences of crimes experienced by the respondent's household, a worries scale about crime and safety in the community, and perception of

⁶ We originally planned to use the World Food Programme's (WFP) Food Consumption Score (FCS) as our food security index. The FCS measures both dietary diversity and consumption frequency. After collecting FCS data at baseline we found that over 99% of households had 'acceptable consumption' per WFP's definition. We therefore decided to change our approach for endline data collection, noting we do not have baseline data for the updated index.

⁷ According to Uganda Demographic and Health Survey (Uganda Bureau of Statistics (UBOS) and ICF, 2018).

⁸ According to ILO (2012). By measuring this indicator for all working-age households, we are deviating from the ILO's definition of the employment rate by and using ILO's employment to population ratio instead. This is because the size of the labor force (as technically defined in employment rate) is very small, shrinking sample size and power to be able to detect any effects.

crime and conflict levels in the community. It is coded in a way that positive values represent higher levels of safety and security.

Since questions for these outcomes are phrased to gather community level data, analysis for these indices will focus on descriptive outputs and possibly a pre-post examination, rather than estimating a treatment effect between treatment and control households.

COVARIATES

In addition to baseline value of specified outcomes, we plan to include the following covariates (collected at baseline) in the regression:

- Gender of Household Head (per enrollment in the GiveDirectly intervention)
- Self-reported household size at baseline for household level outcomes
- Ethnicity
- Time in settlement.

5. LIMITATIONS AND CORRECTIONS TO DATA

SPIILOVERS

There are a few theoretical mechanisms by which spillovers could bias our results. For instance, if cash transfers caused prices to increase, this may decrease the purchasing power of the control group, and therefore a biased treatment effect. (The direction of bias could go in either way, depending on the indicator.) Additionally, cash transfers could increase the consumption of the control group, for instance by increasing demand for goods or labor sold by the control group, or increasing the amount of gifts and loans they receive. This may cause our treatment effect to be biased downwards. Overall, although there are risks to biased treatment effects due to spillovers, we believe they will be low enough such that the estimated treatment effects still provide valuable information.

The recent literature on cash transfers offers mixed results. Haushofer and Shapiro (2016) study Kenyan households receiving unconditional cash transfers under a GiveDirectly program and find limited evidence of spillovers 9 months after the intervention. However, three years after the cash transfer, authors find significantly higher levels of spillovers that might be reducing consumption and food security among spillover households (those who live in villages where others receive treatment) versus pure control households (those who live in villages where no-one receives treatment).⁹ A new study of a GiveDirectly program that looks at general equilibrium effects of their cash transfer scheme finds “large positive spillovers on non-recipient households and firms, and minimal price inflation”.¹⁰ Therefore both theory and literature do not provide clear indications, in general, as to whether spillover effects will exist or in what direction they would bias our estimate.

Due to logistical and ethical constraints, it was not possible to conduct the randomization at a higher level (such as geographic cluster), which would have reduced spillover concerns to some extent. It was also not possible to design a study with the ability to measure spillover effects well. However,

⁹ Haushofer & Shapiro (2018)

¹⁰ Egger et al. (2019)

we decided that despite these worries the study would still provide value due to it being the first RCT of a large cash transfer in a refugee settlement.

Therefore, our results will rest on the assumption that spillovers will not significantly bias our estimated treatment effects. We will explore this assumption through multiple approaches. We will explicitly analyze potential mechanisms for spillovers during the longitudinal qualitative section of the study by investigating if control households are taking loans from treatment households and asking if prices of common goods appeared to have changed due to the transfers. Moreover, we will measure consumption and business revenue during both baseline and endline of the study which allow us to construct time trends for both control and treatment groups across multiple indicators that might serve as potential channels for spillovers.

As discussed in section 3, we discovered during baseline some individuals are registered as separate households with UNHCR when they actually live together and usually eat out of the same pot. This can lead to spillovers, and we therefore decided to drop all households in treatment and control that live with someone that has their own UNHCR registration and picked a cohort number between 1 and 12 or is a PSN household.

ATTRITION AND NON-RESPONSE

We will take specific measures to address attrition and non-response throughout our study. At baseline, we collected contact information of the respondents and relatives, GPS and other data to help us find households again at endline. The messaging that preceded both the lottery and the baseline survey emphasized the importance of participation in the study. At the beginning of both the base- and endline survey, the consent process will highlight the importance of the study and participation therein. Moreover, we will attempt to trace the movement of refugees who participate in the baseline but migrate afterwards. If attrition is significant (>10%) and correlated with treatment, we will bound the treatment effects on our main variable of interest using Lee bounds¹¹. Respondents will likely be given small gifts of thanks during the qualitative study and at endline.

MULTIPLE INFERENCES

Our study aims to look at the welfare impacts of a large one-time unconditional cash transfer in the refugee context. To measure changes in welfare, we will look at primary and secondary outcomes that are in turn constructed from multiple sub-indicators. In order to not artificially inflate the chance of finding significant effects, we will employ multiple hypothesis testing corrections on these three primary outcomes. Using index variables for our outcomes already reduces the number of tests. In addition, we will also correct for the False Detection Rate (Benjamani and Hochberg 1995) on the three primary index variables of consumption, household assets and enterprise income. We will use unadjusted p-values for the secondary outcomes, as the goal is to test specific hypotheses in different areas rather than reaching a final common conclusion in this section.

OUTLIERS AND MISSING VALUES

Standard techniques will be used to deal with outliers, including winsorizing continuous variables at the 99th percentile. We intend to minimize the number of missing variables through enumerator

¹¹ Lee (2009)

training directed at providing further explanation when respondents initially hesitate to answer questions.

6. LONGITUDINAL QUALITATIVE STUDY APPROACH

FOCUS AREAS

As noted in section 3, the study design includes a longitudinal qualitative study. The primary objective is to understand what changes do (or do not) occur within and between households in the context of the COVID-19 pandemic, along with how and why this happens. Secondary objectives include collecting data that will help explain the results obtained in the RCT and collecting data on high priority topics that cannot be easily measured quantitatively.

The main data that will be collected during the qualitative study includes:

- Households' ideas for and actual transfer utilization
- Changes in the market environment, including prices, supply and demand of goods and services, and the (formal or informal) job market
- Service accessibility and quality, particularly for health and education
- Hope, aspirations, subjective socioeconomic standing and long-term plans
- Social cohesion, and safety and security, within the refugee settlement and between refugees and the Ugandan host community
- Psychological well-being, family relations and dynamics, and idleness
- Loans, savings, remittances and assets
- Unintended consequences, particularly unintended negative effects.

SAMPLING

Prior to the onset of the COVID-19 pandemic, we planned that our longitudinal qualitative study sample would include:

- 20 refugee households randomly selected¹² from cohort 3. These respondents will be interviewed once prior to receiving their cash transfer and seven times following their cash transfer.
- 12 refugee households randomly selected from cohort 12. These respondents will be interviewed eight times prior to receiving their cash transfer.
- 12 Ugandan host households randomly selected from the nearby villages. Nine households will be selected from villages where the GiveDirectly intervention has already commenced, meaning they will be interviewed eight times following their cash transfer. A further three households will be randomly selected from villages where the intervention is yet to commence, allowing one pre-transfer interview and up to seven post-transfer interviews.¹³

¹² Stratified random sampling will be used, with households stratified on gender of household head. We will also ensure the selected sample provides variance on other relevant aspects such as household size, ethnicity and age.

¹³ The final number of interviews for these households will depend on when they receive the intervention (TBD), as there may not be sufficient time to interview these households eight times.

We also planned that Interviews would be conducted in eight 'rounds', with most households being interviewed once per round.¹⁴ All interviews were to be conducted between February – November 2020.

In March 2020, we have completed one and a half interview rounds with the above sample. COVID-19 restrictions then meant we had to stop in-person interviews. We are now conducting remote (phone) interviews using the following approach:

- The sample size will reduce to around 20-25 households for the phone interviews. This is because some households do not have phones or reliable phone reception. In addition, our qualitative enumerators do not cover the languages spoken in some households and using a translator for phone interviews is not viable.
- Each 'round' of interviews will involve three 30 minute interviews, rather than one 1.5 hour interview. This is to reduce fatigue associated with phone interviews
- We will provide respondents with around USD1 per interview to compensate them for airtime and phone charging fees.

We will return to in-person interviews when the COVID-19 restrictions are lifted. While we continue to aim for eight rounds of interviews, time and budget restrictions created by COVID-19 mean the final number of interview rounds may be reduced.

DATA COLLECTION AND ANALYSIS

IDinsight will hire and train senior enumerators and interpreters to conduct the interviews. The key data collection and analysis steps include:

- With consent, each interview will be recorded, and enumerators will take notes
- After each in-person interview, enumerators will expand on their notes and share them with IDinsight. During remote interviewing, enumerators are fully transcribing all interviews, given that these interviews are either in English or a language spoken by the enumerators themselves and no interpreters are involved.
- The IDinsight team will conduct rapid analysis of notes and transcripts as they are submitted to identify major trends or gaps, and to discuss questions and adaptations with the enumerators.
- After each round of interviews, IDinsight will have an extended debrief with the enumerators to identify unexpected topics and themes that occurred and may warrant future exploration.
- IDinsight will then conduct an in-depth analysis of interview notes and transcripts using a thematic analysis approach.
- Based on the debrief and the analysis, IDinsight will update the interview protocol and guide as needed, prior to subsequent interview rounds.

¹⁴ The exception may be host community households discussed under footnote nine above.

7. ETHICAL CONSIDERATIONS

RISKS, DATA MANAGEMENT AND CONFIDENTIALITY

The study poses limited risks to beneficiaries. Personal Identifiable information (PII) will not generally be shared¹⁵ and data will be safely collected and stored using encryption technology. Any information collected on pen and paper will be safely stored and archived. We therefore evaluate risks of data leaks as very minimal. We will also ensure privacy throughout the interview to avoid anyone obtaining information that the respondent may not want to share with them. All results will be anonymized and controlled for any information that might be tracked back to a specific individual or small group of individuals before publication.

INFORMED CONSENT

All recipients will be given an option to read or be read detailed information regarding the consent process. Consent will be explicitly asked during the lottery registration process, baseline, endline and each interview of the qualitative survey. The consent process will be recorded on audio to ensure backchecks can be implemented to assess the quality of the procedure.

ETHICAL APPROVALS

We have received ethical approval from the Institutional Review Board of Mildmay (REC REF 1010-2019), and from the Uganda National Council for Science and Technology (UNCST, #SS281ES).

¹⁵ Note that we may share some PII, such as telephone numbers, with GiveDirectly to assist them to identify and enrol households in their intervention. This will only be done with respondents' permission.

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