Evaluating the Effect of Previous Exposure to Survey Instruments: Pre-Analysis Plan*

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Abstract

This document outlines an approach for analyzing the effect on key outcomes of differences in the number of surveys administered to households in control village and households in treatment villages collected as part of our previous randomized controlled trial (RCT) evaluation of the Unconditional Cash Transfer (UCT) of GiveDirectly, Inc. (2013) Between June 2011 and January 2013, GiveDirectly distributed unconditional cash transfers to 503 randomly selected poor rural households in Western Kenya. Households in treatment villages were surveyed both at baseline and endline, but households in control villages were surveyed only at endline. To determine whether this difference in the number of surveys had an impact on survey responses, we surveyed an additional group of households who had not previously been surveyed two years after the original endline. These "demand effects" households were chosen from the pool of households in control villages that met the eligibility criterion for the original evaluation, but were not selected to participate at that time. Comparing outcomes in the second endline survey between this group and the original set of pure control households will allow us to identify the effect of having previously been surveyed on outcomes of interest.

JEL codes: O12, C93, D12, D13, D14

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1 Introduction

In our previous impact evaluation of the GiveDirectly Inc. (*GD*) unconditional cash transfer program (Haushofer and Shapiro 2013), the number of surveys administered to households in treatment and control villages differed: households in treatment villages were surveyed both at baseline and endline, while households in control villages were only surveyed at endline. In this document, we outline a follow-up study designed to test the hypothesis that previous exposure to a survey affects an individual's response.

When we revisited the study households from the original *GD* evaluation for a second endline in 2015, we surveyed an additional set of households in control villages who had been eligible for participation in the original endline, but who had randomly been chosen to not be surveyed at that time. Thus, the 2015 administration of the survey is the first time these households have been asked this set of questions, while it is the second exposure for control village households that were included in the original study. Since the choice of which subset of eligible control village households to survey in the original study was random, we are able to identify the effects of having previously been given a survey on outcomes of interest.

2 Design

Details on the original evaluation are available in Haushofer and Shapiro (2013) and in the pre-analysis plan at https://www.socialscienceregistry.org/trials/19. We briefly summarize the original and augmented design below.

2.1 Sampling and identification strategy

In the original study, we 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 identified 120 villages based on the overall prevalence of eligible households in the village. In these villages, we identified roughly 1,500 eligible households, with eligibility determined by residing in a house with a thatched roof. The criterion were not pre-announced to avoid "gaming" of the eligibility rules. We then randomized on two levels – across villages, and within villages. Specifically, 60 villages were randomly assigned to be treatment villages, while the other 60 were control villages. In treatment villages, half of eligible households were assigned to the treatment condition, and half to the spillover condition. The short-term impact of transfers to these households is described in (Haushofer and Shapiro 2013).

In 2012, we randomly selected 8 households in each control village (roughly 50% of eligible households, for a total of 464 households) to participate in the first endline survey, of which 432 participated in that survey. In villages in which 8 or fewer households were eligible, we surveyed all eligible households. In 2015, we returned to administer a second endline survey. We sought to resurvey all households that participated in the original endline survey both in treatment and control villages. In addition, we used our original 2012 census of pure control villages to identify households that had been eligible to participate in the 2012 survey, but that had not previously been surveyed. There were 428 such households. We administered the same survey to this set of households in 2015 as to households involved in the original endline.

Thus, the focus of this study are two groups of households in pure control villages: those which were surveyed in both 2012 and 2015, and those which were only surveyed in 2015. Neither of these two groups of households received an intervention; the only difference between them is the number of surveys they completed. In the following, we label the households that were surveyed twice "demand effect" households, because their second endline is potentially affected by a demand effect from having completed the first survey.[1] Note that these households were referred to as "pure control" households in previous pre-analysis plans and papers. The households which were only surveyed in 2015 are referred to as "demand control" households.

The primary comparison of interest in this study will be between these two types of households. Since both types of households were selected randomly from the pool of eligible households in control villages, comparison of the two groups will allow us to identify the effect on outcomes of interest of having previously been surveyed.[2]

2.2 Data collection methods and instruments

We collected data through a baseline survey, an first endline survey administered on average 4.4 months after the last transfer received by a household, and a second endline survey roughly two years after the first endline. A midline with a subset of questions was administered to a sample of respondents for a number of months after the original intervention. However, only the first and second endline surveys are relevant to the present analysis.

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 (for the baseline and first endline) or SurveyCTO (for the second endline) 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*. Saliva samples were collected using the Salivette (Sarstedt, Germany), which requires the respondent to chew on a sterile cellulose swab, which is then centrifuged and analyzed for salivary cortisol.

[1] Having participated in the first endline could affect responses on the second endline for reasons other than a demand effect; we use this term for simplicity and because it is one prominent possibility.

[2] For this comparison to identify the effect of interest, having been surveyed previously must not affect the propensity of being surveyed a second time. We address this issue in Section 2.3.

2.3 Risk and treatment of attrition

We targeted 464 "demand effect" households for the first endline survey, of which 432 participated in the first endline survey, and 376 participated in the second endline survey. We treat any demand effect household that participated in the census or first endline, but not the second endline, as attriters. Thus, the attrition rate was 19% from census to second endline in this group, and 13% from first to second endline. Using the original census, we assigned 428 households to the "demand control" condition. Of these, 351 participated in the second endline survey. The remaining 77 households (18%) either refused to participate or could not be located. For the purposes of our analysis, we will also consider these households as attriters.

We use the following specification to estimate whether the magnitude of attrition is different for demand effect and demand control households:

$$attrit_{vh} = \alpha_v + \beta_0 + \beta_1 D_{vh} + \varepsilon_{vh} \tag{1}$$

where $attrit_{hv}$ is an indicator variable taking the value of 1 if household h in village v attrited and 0 otherwise. D_{hv} is an indicator variable taking the value of 1 if household h in village v was assigned to the demand effects condition. The omitted category is demand control households. α_v is a village-specific fixed effect. ε_{hvt} is an idiosyncratic error term. Thus β_1 identifies the difference in attrition between the demand effect and demand control groups.

To bound the best and worst case scenarios for the effects of differential attrition on the analysis outlined in Section 3.1, we will use the approach described in Lee (2009).

3 Econometric specification

3.1 Evaluating Demand Effects

Our basic specification to capture the effect of having been previously surveyed is:

$$y_{vh\{i\}t} = \alpha_v + \beta_0 + \beta_1 D_{vh} + \varepsilon_{vh\{i\}t} \tag{2}$$

where $y_{\{i\}hv}$ is the outcome of interest for household h in village v, measured in the second endline ($t = F$); index i is included for outcomes measured at the level of the individual respondent, and omitted for outcomes measured at the household level. D_{vh} is a dummy variable that takes value 1 for control village households surveyed in the original endline ("demand effect households"), and value 0 for control village households that were not surveyed in the original endline ("demand control households"). α_v is a village fixed effect. $\varepsilon_{vh\{i\}t}$ is an idiosyncratic error term. The omitted category is demand control households. The analysis excludes all households in treatment villages. Thus,

β_1 identifies the effect of having been previously surveyed. To account for possible correlation in outcomes, the error term is clustered at the household level.

3.2 Accounting for multiple inference

Given that our survey instrument often include 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. Across the indices, we will report both unadjusted p-values as well as p-values corrected for multiple comparisons using the Family-Wise Error Rate. The indices will be the same as in the original study. Within each outcome group, we will report unadjusted p-values.

References

Haushofer, Johannes, and Jeremy Shapiro. 2013, November. "Household Response to Income Changes: Evidence from an Unconditional Cash Transfer Program in Kenya."

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