The Income Elasticity for Nutrition: Evidence from Unconditional Cash Transfers in Kenya Pre-Analysis Plan*

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Abstract

How calorie consumption and the food share of expenditures respond to income changes has long been of interest to economists and policymakers. We use a randomized controlled trial to study the effect of large income changes through unconditional cash transfers on the consumption of calories and the food share of expenditures among poor households in rural Kenya. This document describes the analysis plan for the project.

1 Introduction

The response of households in developing countries to income changes in terms of calorie consumption and food share is of significant interest to both policymakers and economists. It is a crucial element in modeling the consumption and savings choices of households, and a central ingredient in designing tax and transfers policy, labor market policy, and insurance markets (Deaton 1992; Hall and Mishkin 1982; Jappelli and Pistaferri 2010). In developing countries, it can inform the design of consumption support policies and redistribution programs (Fenn et al. 2015; Luseno et al. 2014; Robertson et al. 2013; Fernald and Hidrobo 2011; Schady and Paxson 2007; Aguero, Carter, and Woolard 2006; Cunha 2014; Blattman, Fiala, and Martinez 2013; Aker 2015; Schofield 2014).

A main reason for the importance of such responses is that they provide information about the source of possible poverty traps: if households show a strong response to income changes in terms of calorie consumption, a nutrition-based poverty trap is plausible. The potential for nutrition-based poverty traps has received significant attention in the literature (Dasgupta and Ray 1986), and there

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is debate as to whether they exist (Deaton and Drèze 2009). However, estimating the income elasticity of calorie consumption from observational data alone presents significant challenges. Previous approaches have used either cross-sectional estimates (Deaton and Subramanian 1996; Jappelli and Pistaferri 2010; Skoufias 2003) or time-series data (Dynarski et al. 1997; Krueger and Perri 2010; Krueger and Perri 2006; Browning and Crossley 2001; Hall and Mishkin 1982); another set of studies has used natural or policy shocks to study household responses (Johnson, Parker, and Souleles 2006; Souleles 2002; Shapiro and Slemrod 1995; Agarwal, Liu, and Souleles 2007). However, in the cross-section, households that have different resources may have different tastes, different opportunities, and face different prices, which complicates the interpretation of cross-sectional estimates of elasticities. Cross-sectional estimates may also be biased by reverse causality, e.g. if calorie intake affects productivity, or simultaneous causality, e.g. if health affects both calorie intake and income. In the time series, changes in income are typically accompanied with changes in the economic environment faced by the household (e.g. changes in wages or the productivity of labor). Finally, because policymakers in developing countries have often been wary of unconditional income transfers, most income redistribution to the poor is either in kind or attached to conditionalities, and therefore few natural experiments exist. Indeed, Heckman (1992) praised the early social experiments (such as the Negative Income Tax Experiments in the US) for distinguishing income and substitution effects from higher wages, precisely because this is one of the few cases where it is difficult to think of a substitute for an experiment.

More recently, studies have begun to use experimental variation in total expenditure to estimate changes in food share and calorie consumption. For instance, Angelucci and Attanasio (2013) show that the conditional cash transfer (CCT) program Oportunidades in Mexico increases households' budget shares for food, and this effect is larger than what would be predicted based on baseline Engel curves. Attanasio, Battistin, and Mesnard (2012) find similar results in Colombia, and Schady and Rosero (2008) for unconditional cash transfers in Ecuador. In all cases, the fact that the transfers were made to women is a possible reason for the observed increases. Hoddinott and Skoufias (2004) study the impact of the CCT program PROGRESA (later Oportunidades) on calorie consumption, and Gangopadhyay, Lensink, and Yadav (2012) find substitution towards non-staple food categories following a UCT program in India.

In this study, we will investigate the nutritional consumption response of households to income changes using data from a randomized controlled trial (RCT) of a large, one-time, unanticipated unconditional cash transfer (UCT). Between 2011 and 2013, the NGO *GiveDirectly* sent unconditional cash transfers of at least USD 404, corresponding to at least twice monthly average household consumption, to randomly chosen poor households in Kenya through the mobile money system M-Pesa. The transfers were explicitly described to households as fully unconditional, and as short-term windfalls (in one lump sump or monthly installment over 9 months), rather than as a promise of recurring payments for the long term. We surveyed randomly selected treatment and control households both before the program and between 1 and 15 months after it ended. We will use the exogenous change in income and total expenditure generated by the transfer program, together with detailed household-level data on consumption and village level data on prices, to estimate their consumption response to income changes using the (Quadratic) Almost Ideal Demand System (Banks, Blundell, and Lewbel 1997; Deaton and Muellbauer 1980). We will separately estimate the income elasticity for the food share and calories. This analysis will provide a causal estimate of the effect of unanticiapted income changes on food shares and calorie consumption.

2 Sample and data

The UCT program implemented by *GiveDirectly Inc.* (GD) targets impoverished households in Kenya by sending them unconditional cash transfers through the mobile money system M-Pesa. After determining eligibility, GD transfers the money from GD's M-Pesa account to that of the recipient. To facilitate the transfers, 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, or using their own 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%). The technology also contributes to high coverage: the program can be implemented in any area with access to mobile money technology.

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. To establish a causal relationship between the program and changes in outcomes, this study uses an RCT design. The program identified 120 villages with the highest shares of eligible households in Rarieda, Kenya. In these villages, the program identified 1,500 eligible households, with eligibility determined by residing in a home with a thatched roof. This criterion was not pre-announced to avoid "gaming" of the eligibility rules. The randomization was done on two levels — across villages, and within villages. Specifically, 60 villages were randomly assigned to be treatment villages, while the other 60 were pure control villages. In each of the latter, we surveyed 8 households that did not receive a cash transfer. Within treatment villages, we conducted a within-village randomization: 50% of eligible households were randomly assigned a cash transfer; the other 50% received no transfer.

The intervention consistent of three treatment arms.

1. Transfers to the woman vs. the man in the household. Half of the transfers were made to the woman, while the other half were made to the man.

2. Lump-sum transfers vs. monthly installments. Half of the transfers were lump sum, and the

other half was paid in 9 monthly installments. We randomized the month in which the lump sum transfer was made among the nine months following the initial announcement.

3. Large vs. small transfers. A proportion (28%) of the transfers were \$1,525 in magnitude, while the remainder were \$404.

We surveyed both treatment and control households in treatment villages at baseline, i.e. before the transfers, and at endline, i.e. on average nine months after the beginning of transfers. In pure control villages, we only conducted an endline survey, but no baseline survey.

Estimating the income elasticities for the food share and calories

The random variation of the cash transfer across households allows us to identify causal effects of income changes on food share and calories using an instrumental variable approach: we regress food share and calorie consumption on total expenditure, while using the randomly assigned cash transfers as instruments for total expenditure. We can then compare the results of this estimation to cross-sectional estimates, obtained both from this study and others.

We will use three approaches to estimate these effects: a "naïve" approach that does not take into account treatment effects on prices; an intermediate approach where we take account of prices and use a linearized version of the (quadratic) almost ideal demand structure; and fully fledged linear and quadratic almost ideal demand systems (AIDS and QUAIDS).

In addition, where we have both baseline and endline data available, we compare the results when estimating the responses using first differences vs. only endline data. At endline, we have village price data as well as detailed expenditure and nutrition data for both control and treatment villages (Cunha, Giorgi, and Jayachandran 2011). At baseline we have detailed expenditure and nutrition data for households in the treated villages. Below we give an overview of the approaches before we turn to a description of the (quadratic) almost ideal demand system.

Food share

To estimate the household response of food share to the transfers, we first define variables as follows: ω is budget share for food; d_v is a village dummy; z is total nominal expenditure; p is price, where superscripts f and n refer to food and non-food prices, respectively; subscript 0 indicates baseline, subscript 1 indicates endline; and $\ln z^* = \ln \frac{z_i}{a^*(\mathbf{p})}$, where $\ln a^*(\mathbf{p})$ is the Stone price index: $\ln a^*(\mathbf{p}) = \overline{w^f} lnp^f + \overline{w^n} lnp^n$. $\overline{w^x}$ is the average budget share for good $x \in \{f, n\}$ in the sample. For the cross-sectional variants of the AIDS and QUAIDS we will use the consumer price index for food and non-food, respectively, to retrieve prices at baseline from the price survey data at endline. To deal with possible zeroes in the expenditure data, we use the inverse hyperbolic sine transform wherever we mention logs (Burbidge, Magee, and Robb 1988; MacKinnon and Magee 1990; Pence 2006).

We then estimate the models listed below. Each model will be run with two different sample restrictions: first, we will restrict the sample to treatment and within-village control households (i.e. households in treatment villages); in the "non-experimental" version of this specification, the sample will simply be within-village control households. The choice of comparing treatment households to within-village control households (as opposed to pure control households) is driven by two factors: first, we found in previous work (Haushofer and Shapiro 2013) that the spillover effects in this study were small, making the within-village control households a valid comparison group. Second, using the household-level randomization allows us to include village level fixed effects and obtain more power. Thus, we will include village level fixed effects in this first type of specification when appropriate. Standard errors will be un-clustered because the randomization occurs at the household level.

Second, we will use the whole sample and pool the two control groups, i.e. within-village control and pure control groups. The "non-experimental" version of this regression uses within-village control and pure control groups only. Whenever we use variables in first differences, we need to deal with the fact that the pure control group was only surveyed at endline; this will be achieved by setting the values of these households to zero and including an indicator variable on the right-hand side to denote these observations.[1]

Our main specifications will include a basic set of demographic controls (baseline number of children, number of adults). As robustness checks, we will estimate versions of these regressions where we a) omit demographic controls on the RHS, b) include a fuller set of controls, all measured at baseline (separate variables for number of girls, boys, women, men; age of primary respondent; marital status/household type (single vs. married); highest level of education attained by primary respondent; consumption, asset levels, and land holdings; ownership of non-agricultural enterprise; ownership of agricultural enterprise; and participation in wage labor).

Our main specifications will be run at the household level. As a robustness check, we will also estimate versions of these regressions where we use equivalence scales to convert the outcomes into per capita and equivalence adjusted terms.

In addition to analyzing the food share and total calorie consumption, we will also report estimation results for subcategories of consumption. These subcategories are of particular interest in the context of analyzing differences across treatment arms.

Below we list the QUAIDS models we will estimate. However, in addition, each model will also be estimated in a linear version that omits the quadratic term. If the quadratic terms in the QUAIDS are insignificant, the linear AIDS will become the preferred model.

[1] This approach has been shown to yield biased estimates in some cases (Jones 1996); we will therefore focus on the previously described specification.

1. Naïve approach (without prices)

   (a) First differences

      i. Experimental:

$$\Delta\omega_i = \beta_0 + \beta_1 \Delta \ln z_i + \beta_2 \Delta(\ln z_i)^2 + \varepsilon_i$$

Here, $\Delta \ln z_i$ and its square are instrumented with the log of the transfer amount to each household and its square. In addition, we will test whether the first stage is stronger when we add an indicator for monthly vs. lump-sum transfers as instruments; or when the sample is restricted to lump-sum recipient households and transfer timing is included as an instrument. These specifications may be chosen as primary if they achieve better identification of the full demand system.

      ii. Non-experimental:

$$\Delta\omega_i = \beta_0 + \beta_1 \Delta \ln z_i + \beta_2 \Delta(\ln z_i)^2 + \varepsilon_i$$

No instruments are used.

   (b) Levels

      i. Experimental:

$$\omega_{i1} = \beta_0 + \sum_v \delta_v d_v + \beta_1 \ln z_{i1} + \beta_2 (\ln z_{i1})^2 + \alpha_v + \delta' \mathbf{X_i} + \varepsilon_i$$

Here, $\ln z_{i1}$ and its square are instrumented with the log of the transfer amount to each household and its square. Village level fixed effects are only added in the specification that restricts the sample to treatment villages.

      ii. Non-experimental:

$$\omega_{i0} = \beta_0 + \sum_v \delta_v d_v + \beta_1 \ln z_{i0} + \beta_2 (\ln z_{i0})^2 + \alpha_v + \delta' \mathbf{X_i} + \varepsilon_i$$

Village level fixed effects are only added in the specification that restricts the sample to treatment villages. No instruments are used.

2. Linearized QUAIDS

   (a) First differences

      i. Experimental:

$$\Delta\omega_i = \alpha + \gamma \Delta(\ln p^f - \ln p^n) + \beta \Delta \ln z_i^* + \lambda \Delta(\ln z_i^*)^2 + \varepsilon_i$$

Here, $\Delta \ln z_i^*$ and its square are instrumented with the log of the transfer amount to each household and its square.

ii. Non-experimental:

$$\Delta\omega_i = \alpha + \gamma\Delta(\ln p^f - \ln p^n) + \beta\Delta \ln z_i^* + \lambda\Delta(\ln z_i^*)^2 + \varepsilon_i$$

No instruments are used.

(b) Levels

i. Experimental:

$$\omega_{i1} = \alpha + \sum_v \delta_v d_v + \gamma(\ln p_1^f - \ln p_1^n) + \beta \ln z_{i1}^* + \lambda(\ln z_{i1}^*)^2 + \alpha_v + \delta'\mathbf{X_i} + \varepsilon_i$$

Here, $\ln z_{i1}^*$ and its square are instrumented with the log of the transfer amount to each household and its square. Village level fixed effects are only added in the specification that restricts the sample to treatment villages.

ii. Non-experimental:

$$\omega_{i0} = \alpha + \sum_v \delta_v d_v + \gamma(\ln p_0^f - \ln p_0^n) + \beta \ln z_{i0}^* + \lambda(\ln z_{i0}^*)^2 + \alpha_v + \delta'\mathbf{X_i} + \varepsilon_i$$

Village level fixed effects are only added in the specification that restricts the sample to treatment villages. No instruments are used.

3. Full QUAIDS

(a) Levels

i. Experimental:

$$\omega_{i1} = \alpha + \sum_v \delta_v d_v + \gamma(\ln p_1^f - \ln p_1^n) + \beta \ln(\frac{z_{i1}}{a(\mathbf{p_1})}) + \frac{\lambda}{b(\mathbf{p_1})}\left(\ln(\frac{z_{i1}}{a(\mathbf{p_1})})\right)^2 + \alpha_v + \delta'\mathbf{X_i} + \varepsilon_i.$$

Here, $\ln(z_{i1})$ and its square are instrumented with the log of the transfer amount to each household and its square. Village level fixed effects are only added in the specification that restricts the sample to treatment villages.

ii. Non-experimental:

$$\omega_{i0} = \alpha + \sum_v \delta_v d_v + \gamma(\ln p_0^f - \ln p_0^n) + \beta \ln(\frac{z_{i0}}{a(\mathbf{p_0})}) + \frac{\lambda}{b(\mathbf{p_0})}\left(\ln(\frac{z_{i0}}{a(\mathbf{p_0})})\right)^2 + \alpha_v + \delta'\mathbf{X_i} + \varepsilon_i$$

Village level fixed effects are only added in the specification that restricts the sample to treatment villages. No instruments are used.

Calories

To estimate the response of calorie consumption to the transfers, we will use the following specifications. In addition to calories, we will also use other nutrients (carbohydrate, fat, fiber, protein) as outcome variables. Thus, c_i indicates calories or one of the other nutrients. As above, these models will be estimated both with and without a quadratic term; if the quadratic terms are significant, those specifications will be considered the primary models of interest.

1. Naïve approach (without prices)

   (a) First differences

      i. Experimental:

      $$\Delta \ln c_i = \beta_0 + \beta_1 \Delta \ln z_i + \beta_2 \Delta (\ln z_i)^2 + \varepsilon_i$$

      Here, $\Delta \ln z_i$ and its square are instrumented with the log of the transfer amount to each household and its square.

      ii. Non-experimental:

      $$\Delta \ln c_i = \beta_0 + \beta_1 \Delta \ln z_i + \beta_2 \Delta (\ln z_i)^2 + \varepsilon_i$$

      No instruments are used.

   (b) Levels

      i. Experimental:

      $$\ln c_{i1} = \beta_0 + \sum_v \delta_v d_v + \beta_1 \ln z_{i1} + \beta_2 (\ln z_{i1})^2 + \alpha_v + \delta' \mathbf{X_i} + \varepsilon_i$$

      Here, $\ln z_{i1}$ and its square are instrumented with the log of the transfer amount to each household and its square. Village level fixed effects are only added in the specification that restricts the sample to treatment villages.

      ii. Non-experimental:

      $$\ln c_{i0} = \beta_0 + \sum_v \delta_v d_v + \beta_1 \ln z_{i0} + \beta_2 (\ln z_{i0})^2 + \alpha_v + \delta' \mathbf{X_i} + \varepsilon_i$$

      Village level fixed effects are only added in the specification that restricts the sample to treatment villages. No instruments are used.

2. With price controls

   (a) Levels

i. Experimental:

$$\ln c_{i1} = \alpha + \sum_v \delta_v d_v + \beta \ln z_{i1} + \lambda \left(\ln z_{i1}\right)^2 + \lambda_f \ln p_1^f + \lambda_n \ln p_1^n + \alpha_v + \delta' \mathbf{X_i} + \varepsilon_i$$

Here, $\ln z_{i1}$ and its square are instrumented with the log of the transfer amount to each household and its square. Village level fixed effects are only added in the specification that restricts the sample to treatment villages.

ii. Non-experimental:

$$\ln c_{i0} = \alpha + \sum_v \delta_v d_v + \beta \ln z_{i0} + \lambda \left(\ln z_{i0}\right)^2 + \lambda_f \ln p_0^f + \lambda_n \ln p_0^n + \alpha_v + \delta' \mathbf{X_i} + \varepsilon_i$$

Village level fixed effects are only added in the specification that restricts the sample to treatment villages. No instruments are used.

Treatment arms

We will also estimate the difference in the response for the gender treatment. As an example, for nutrients, we will estimate the following model:

$$\Delta \ln c_i = \beta_0 + \beta_1 \ln \Delta z_i + \beta_2 \Delta(\ln z_i F_i) + \beta_3 \Delta(\ln z_i F_i)^2 + \beta_4 \Delta \ln z_i + \beta_5 \Delta(\ln z_i)^2 + \varepsilon_i,$$

where the dummy variable F_i indicates that the female is the recipient. Note that there is no main effect for the gender dummy because the outcomes are in first differences.

Correspondingly, we will investigate whether there is any difference in food share, and also between the other treatment arms. As above, these specifications will be run both with and without instrumenting z_i with the treatment.

Heterogeneous treatment effects

For both nutrients and food share, we will estimate specifications where we add heterogeneity in baseline income. As an example, we will estimate the following model:

$$\Delta \ln c_i = \beta_0 + \beta_1 \Delta(\ln z_i z_{i0}) + \beta_2 \Delta(\ln z_i z_{i0})^2 + \beta_3 \Delta \ln z_i + \beta_4 (\ln z_{i0}) + \beta_5 \Delta(\ln z_i)^2 + \beta_6 (\ln z_{i0})^2 + \varepsilon_i.$$

Again $\Delta \ln z_i$ is instrumented as described above.

Other extensions

In additional analyses, we will allow the consumption response to price changes to transfers to vary by household income level.

Analyses previously run

In an early version of the main paper resulting from this study (Haushofer and Shapiro 2013), we already ran a subset of the analyses described here. The present analysis plan builds on that section and turns it into a separate paper at the suggestion of the journal editor in charge of the main paper. We estimated the following cross-sectional model (Deaton and Subramanian 1996):

$$\ln(x_{ij}) = \alpha_v + \beta_0 + \beta_1 \ln(y_i) + \varepsilon_i \tag{1}$$

Here, x_{ij} is expenditure on budget head j in household i at endline, and y_i is total endline expenditure. The sample is restricted to the within-village control group. For the experimental analysis, we estimated a version of Equation 1 in which we instrumented total expenditure with a dummy for being in any of the treatment groups. In this specification, the sample included both the treatment and within-village control group. We then present and compare both the cross-sectional and IV estimates.

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Appendix

The Quadratic Almost Ideal Demand System

The quadratic version of the Almost Ideal Demand System (Deaton and Muellbauer, 1980) is due to Banks, Blundell and Lewbel (1997). This system allows for flexible Engel curves in addition to flexible substitution patterns. For household h, at time t, the share spent on food can be written as (disregarding equivalence scale adjustments for now):

$$\omega_{h,t}^{i} = \alpha_i + \sum_{j}^{n} \gamma_{i,j} \ln p_j + \beta \ln y_{h,t} + \frac{\lambda}{b(\mathbf{p_t})}(\ln y_{h,t})^2, \tag{2}$$

where $y_{h,t} = z_{h,t}/a(\mathbf{p_t})$. $z_{h,t}$ is the total expenditure of the household, and $a(\mathbf{p_t})$ and $b(\mathbf{p_t})$ are price indices. p_t^f and p_t^n are prices for food and non-food, respectively.

The price index $a(\mathbf{p_t})$ is translog and given by:

$$\ln a(\mathbf{p_t}) = \alpha_0 + \sum_{k=f,n} \alpha_k \ln p_t^k + \sum_{k=f,n} \sum_{l=f,n} \gamma_{k,l} \ln p_t^k \ln p_t^l. \tag{3}$$

$b(\mathbf{p_t})$ is Cobb-Douglas and given by:

$$b(\mathbf{p_t}) = \prod_{k=f,n} (p_t^k)^{\beta_k}, \tag{4}$$

where $\sum_{k=f,n} \alpha_k = 1$ and $\sum_{k=f,n} \gamma_{k,l} = \sum_{l=f,n} \gamma_{k,l} = \sum_{k=f,n} \beta_k = 0$.

In order to find the expression for the income elasticity, we differentiate the budget share equation (1):

$$\mu_{h,t} := \frac{\partial \omega_{h,t}}{\partial \ln z_{h,t}} = \beta + \frac{2\lambda}{b(\mathbf{p_t})} \left\{ ln \left[\frac{z_{h,t}}{\ln a(\mathbf{p_t})} \right] \right\}, \tag{5}$$

and find the income elasticity to be:

$$e_{h,t} = \frac{\mu_{h,t}}{\omega_{h,t}} + 1 \tag{6}$$

We can see that the income elasticity depends on the level of income as well as the budget share. We will look at the average response, but we will also look at the income specific elasticities

As the main aim of the paper is to investigate the income elasticity for nutrition, we will focus on the estimation of the income elasticity for food and hence, the demand system reduces to one equation only:

$$\omega_{h,t} = \alpha + \gamma(\ln p_t^f - \ln p_t^n) + \beta_1 \ln y_{h,t} + \beta_2 (\ln y_{h,t})^2, \tag{7}$$

where $\omega_{h,t}$ is the budget share for food for household h at time t. And the income elasticity can be expressed as follows:

$$\mu_{h,t} := \frac{\partial \omega_{h,t}}{\partial \ln z_{h,t}} = \beta + 2\beta_2 \left\{ ln \left[\frac{z_{h,t}}{\ln a(\mathbf{p_t})} \right] \right\}, \tag{8}$$

$$e_{h,t} = \frac{\mu_{h,t}}{\omega_{h,t}} + 1, \tag{9}$$