

Pre-Analysis Plan: The Effects of Cash Transfers on Social Preferences

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ABSTRACT: We describe the analysis plan for a lab-in-the-field study that will examine the effects of unconditional cash transfers on children's social preferences. The study will be conducted with children living in Nakuru County, Kenya whose parents participated in a previously implemented RCT that provided cash transfers to poor households. Participating households either received a cash transfer, did not receive a transfer but lived in the same village as those who did, or did not receive a transfer and lived in a village in which nobody received a transfer. Participants will be surveyed and will complete several standard economic games measuring their social preferences. For these tasks, they will be paired with individuals of varying relative wealth to assess their social preferences and how social preferences vary with the identity of the other player. We outline the study design, the outcomes of interest, and the econometric strategy for the analysis.

Keywords: cash, social preferences

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1. Introduction

Children have a well-documented penchant toward for fairness (Fehr, Bernhard and Rockenbach, 2008). In particular, while concerns for equality develop early, they are steadily supplanted by equity-based concerns as children age (Huppert, Cowell, Cheng, Contreras-Ibáñez, Gomez-Sicard, Gonzalez-Gadea, Huepe, Ibanez, Lee, Mahasneh et al., 2019). Children become more discriminating when it comes to deservingness based on, for example, achievement and effort (Almås, Cappelen, Sørensen and Tungodden, 2010) or in-group identity (Fehr et al., 2008, Fehr, Glätzle-Rützler and Sutter, 2013). Thus, socialization likely plays a key role in children and young adults' display of prosocial behavior (e.g., Huppert et al., 2019).

Formative experiences during development can be a critical driver of the degree to which individual children act prosocially. For example, attending preschool (Cappelen, List, Samek and Tungodden, 2020) or participating in a mentorship program in elementary school (Kosse, Deckers, Pinger, Schildberg-Hörisch and Falk, 2020) have both been shown to increase altruism. Moreover, having poor classmates can make rich students more prosocial, generous, and egalitarian, and less likely to discriminate against poor students (Rao, 2019).

The importance of particular socialization experiences may help shed light on why past research on the relationship between prosociality and socioeconomic status (SES) among children have produced mixed results. In some cases, lower-SES children were found to be more altruistic than higher-SES children (Chen, Zhu and Chen, 2013). In other cases, the reverse was found: higher-SES children are more altruistic (Benenson, Pascoe and Radmore, 2007). Still other research has found that objective SES has no relationship to children's prosocial behavior, but parents' perceptions regarding relative wealth and local levels of inequality do (Chernyak, Harvey, Tarullo, Rockers and Blake, 2018). Thus, there may have been something unique about the nature of wealth in the particular study contexts which drives the results.

One factor that could potentially affect prosocial behavior is exogenous changes in wealth, such as the receipt of an unconditional cash transfer (UCT). UCTs are increasingly seen as a viable tool for reducing poverty (Baird, De Hoop and Özler, 2013, Blattman, Green, Jamison, Lehmann and Annan, 2016, Blattman, Fiala and Martinez, 2020). They have been shown to improve numerous economic and psychological outcomes; recipients report increased expenditures, asset holdings, and revenue, as well as greater happiness and life satisfaction (Haushofer and

Shapiro, 2016). Moreover, these improvements are still reported years after the cash transfers have ended (Haushofer and Shapiro, 2018). However, few studies have looked at how the children of cash-transfer recipients are affected, particularly with regard to their psychosocial outcomes (but see Baird et al., 2013). Even less is known about how UCTs affect prosocial behavior.

On the one hand, members of UCT-recipient households may behave in ways that seek to maintain their new status, for example, by being less generous toward those with less wealth (Côté, House and Willer, 2015). On the other hand, people have strong preferences for equity (Chernyak et al., 2018, Dawes, Fowler, Johnson, McElreath and Smirnov, 2007, Fehr et al., 2008, Huppert et al., 2019) so cash-transfer recipients may feel compelled to redistribute wealth. Those who do not receive cash transfers may also be affected by the UCT program, to the extent that it drives their expectations for redistribution from recipients and their envy of recipients. Finally, with regard to children and adolescents, the effect of UCT-receipt on behavior is further complicated in that it is likely moderated by the age at which the cash transfer was received, that is, how long the child has experienced increased household wealth and whether it happens during particularly formative years.

This research project is interested in better understanding how cash transfers shape social preferences in the East African context. Specifically, we are interested in how cash transfers impact the children of the recipients with regard to their social preferences. We will do this by conducting lab-in-the-field experiments among participants of a previously implemented RCT that provided unconditional cash transfers to poor households in Kenya. In 2017–2018, the Busara Center for Behavioral Economics delivered UCTs to a randomly chosen group of households ($n = 540$) in a randomly chosen group of 60 villages. The households neighboring those which received the cash transfers comprised the spillover group ($n = 1,077$), while 60 nearby villages in which nobody received cash transfers comprised the pure control group ($n = 1,545$).

Participants' children in each of the three groups (treatment, spillover, and pure control) will be surveyed and will complete several economic games. The surveys will measure self-reported social behavior, with modules measuring specifically trust, envy, depression, social norms, social ties, perceptions about sharing and redistribution, locus of control, and IQ. The games will be standard economic games that have been modified to be appropriate for children. In the games, the children will get limited information on the other player, namely their age and gender. In some rounds of the games, children will also know the other player's family household wealth

relative to their own family's wealth. By comparing the behaviors of the children across the three groups, we are able to assess how cash transfers to parents affect children's prosocial behavior. And by comparing the behaviors within-child across different kinds of partner identities, we are able to assess how prosocial behavior depends on the current distribution of wealth.

In addition, we will resurvey adults, asking them questions about asset ownership, household consumption, and individual well-being. These questions will allow us to assess the persistence of the cash transfers impact on these outcomes for each of the children's households.

2. Survey Design

2.1. Sampling Strategy

We will work with a sample of individuals who participated in a previously implemented RCT evaluating UCTs, conducted between May 2017 and January 2018 in Nakuru County, Kenya by the Busara Center for Behavioral Economics ([Haushofer, Mudida and Shapiro, 2020](#)). Households were sampled as follows: Sixty villages in Nakuru County were randomly selected to receive the cash treatment. Potential sample households within each village were identified by houses built without brick, stone, or metal walls, as a proxy for low-income. Once identified, ten of these houses in each village were randomly selected. The two households nearest to each of the initially selected ten were also included in the sample. Within each cluster of three households, one was randomly selected to receive the UCT, while the other two acted as a "spillover" condition. One adult in each of the selected households was chosen as the recipient (in the case of the treatment households) or the survey participant (in the case of the spillover households). Another 60 villages acted as a "control" condition, for which participants were simply surveyed without any intervention. Eligible control households were identified in the same manner as in the treatment condition, with every household participating in the survey.

In all, the full sample consisted of 540 treatment households, 1,077 spillover households, and 1,545 control households. A year after the trials had ended, 521 treatment, 1,045 spillover, and 1,473 control households participated in an endline survey. Of those, 362 treatment, 733 spillover, and 1,019 control households had at least one child between the ages of 6 and 17 years old. Our sample of children was selected from those who were present at both the baseline and endline rosters and who are in the targeted age range. Due to budget constraints we could not target this

sample in full. We selected all treatment households, and randomly dropped the same number of households from spillover and pure control households (155 each). Dropping full households from the sample leaves us with all children in the targeted age range within selected households. We aim to visit 4,424 children within our target age range in 1,804 households. This will correspond to 906 children in the treatment households, 1,446 children in the spillover households, and 2,072 children in the pure control households. If it emerges during data collection that the budget allows for it, we will visit a larger number of households, up to the full sample.

2.2. Treatment

In the original trial, those in the cash treatment group (i.e., 540 households across 60 villages) received a UCT of KES 50,000 (equivalent of USD 1,076 PPP at the time of study). Cash transfers were delivered either as lump-sum transfers or in five weekly installments of KES 10,000, randomly assigned at the village level (the differences between these two conditions are not of interest to the current study).

Before sending the participants a cash transfer, they were contacted over the phone or in person by a Busara field officer, and informed that they had been entered into a lottery and their name had been selected to receive KES 50,000. The field officer emphasized that the cash transfer was entirely unconditional (“The money is yours to do whatever you like with – we have no preferences about what you do with the money. You should use it however you think best.”)

2.3. Data Collection

We will conduct lab-in-the-field sessions with each participant individually. Each session will consist of a survey and participation in standard economic games. The survey and the games will both be administered on tablet computers, using the SurveyCTO software for the surveys and oTree software for the games. Field officers will read instructions to the participants in Swahili or English, depending on the participant’s preference, and will provide examples of how to play the games using physical props to maximize comprehension.

For all games, participants will be playing with a randomly selected partner (or partners, depending on the game). Participants are never aware of their partner’s actual identity nor is their partner ever present. Rather, they will be matched to other participants who have taken

part in the study, whose demographics (specifically their age, gender, and relative wealth) and decisions will be implemented.

Compensation for participants will consist of a participation fee and task-based payout. Tokens will be used instead of money. The tokens can then be converted into prizes which the field officers will show the children before the session. Prizes will vary in the number of tokens required to “purchase” them to mimic a real store.

2.4. Player Behavior for Matching

For payout calculation, participants are matched to other participants who have taken part in the study. In order to facilitate this, we will collect data from at least one participant of the possible player types (i.e., age, gender, and household wealth) and observe their game play in the different games against other player types. To determine payoffs, participants in the main study are randomly matched to this pool of player behavior which continuously grows as the study progresses. Payouts are then determined by randomly selecting one round from each game and implementing the outcome of that round.

2.5. Schedule of Tasks and Treatments

The sessions will follow the schedule of tasks that are outlined below.

- Participant identification
- Welcome
- Consent from the parent or guardian
- Assent from the child
- Demographics
- Child Survey
- Tasks with no information on partner’s wealth (specific game order randomized at the participant level): instructions, examples, and comprehension questions followed by game play.
 - Dictator game

- Ultimatum game (as player 1 and player 2)
 - Third-party Dictator game
 - Joy of destruction game
 - Public goods game
- Tasks including opponent's family wealth variation (randomized at the participant level, 2 rounds each game: once with a child from a poorer family and once a child from a richer family, also in random order): for each game we will ask them whether they remember how to play, if they remember we proceed with the game, otherwise we explain the rules of the game again before playing.
 - Dictator game
 - Ultimatum game (as player 1 and player 2)
 - Third-party Dictator game
 - Joy of destruction game
 - Debrief and selection of prizes
 - Adult short survey

2.6. *Experimental Design*

2.6.1. *Lab experiments*

Each participant completes five different experimental activities. These activities are the dictator game (DG), ultimatum game as a player 1 (UG₁) and as a player 2 (UG₂), third-party dictator game (3PDG), joy of destruction game (JOD), and public goods game (PGG). These experimental activities are described in detail below. Game order is randomized at the participant level. Participants are paid based on their and their partners' decisions from one randomly selected round from each game. All activities are completed on tablet computers using oTree software.

For each of the experimental tasks described below, participants are randomly paired with one or several other players, depending on the game. Participants are told that the other players that they are paired with are from a village like theirs. They are also informed that they will not be told the exact identity of the other player that they are paired with, but that they will

be given information about their age and gender, and in some rounds of the games they will also know their family's wealth relative to their partner. Participants are told the other player will have the same information about them. The participants are paired with each gender with equal probability. Given that we are interested in behavior with peers, we over-weight the probability of being paired with someone close in age. More precisely, the random assignment of each opponent's age come from a probability distribution where 50 percent is within the age interval given by the player's age ± 2 years, and the remaining 50 percent come from a uniform distribution of the other ages.

Participants will play all games three times, except for the PGG. For each iteration of these tasks, the participant is paired with one individual who is described as coming from a family that is either wealthier or poorer than the participant's family or no information on relative wealth is given. For the PGG, participants are paired with three individuals, but no information about relative wealth is provided. Participants first play each game once in the no-wealth-information condition, and then play each game again with the wealth information provided (for a total of 3 iterations per game). The order of the games when wealth-information is provided will be randomized at the participant level, and the presentation of the wealthier or poorer partner will be randomized within each game.

To ensure that the game instructions are well understood, the enumerators review several examples of each task with the participants using physical props. Then, the participants complete a series of test questions that they must answer correctly prior to being able to participate in each task. The number of incorrect responses to test questions will be recorded.

1. Dictator Game

In the Dictator Game, the participant is assigned to be player 1. Player 1 chooses how much of a 10-token endowment to send to player 2. The participant is told that they will not know exactly who the other player is, but that they will have several pieces of information on the other player. These pieces of information are age and gender (and relative wealth in in the second and third rounds of the game). Likewise, the participant is told that the other player will have the same information on the participant. The participant is then asked to decide how much they want to allocate to the other player by dragging each of the 10 tokens presented on screen into one of two baskets, representing the participant's and the

other player's payout. The participant completes this task three times as a player 1, each time paired with a new player 2.

The outcome of interest is the amount the participant sends to player 2. A version of this game has been previously used in a number of studies, including by [Habyarimana, Humphreys, Posner and Weinstein \(2007\)](#) and [Berge, Bjorvatn, Galle, Miguel, Posner, Tungodden and Zhang \(2015\)](#), as well as in studies using children as young as 3 ([Benenson et al., 2007](#), [Blake and Rand, 2010](#), [Smith, Blake and Harris, 2013](#)). This game is widely considered to be a measure of altruism.

2. Ultimatum Game

In the Ultimatum Game player 1 receives an endowment of 10 tokens and proposes a split of that endowment to player 2. Player 2 can then decide to accept or reject player 1's offer. If player 2 accepts, both players receive the allocation proposed by player 1. If player 2 rejects, both players receive 0 tokens. Participants will play this game once as player 1 (UG₁) and once as player 2 (UG₂). As with the DG, participants will play this game (both the UG₁ and UG₂) three times, once in the no-wealth-information condition, once with a wealthier partner, and once with a poorer partner.

Versions of this game have previously been used in a number of studies using children (e.g., [Wittig, Jensen and Tomasello, 2013](#), [Sutter, 2007](#), [Takagishi, Kameshima, Schug, Koizumi and Yamagishi, 2010](#)). In addition to measuring altruism (UG₁), this task also measures expectations regarding fairness (UG₂).

3. Third-Party Dictator Game

The Third-Party Dictator Game is similar to the DG except that, rather than dividing the 10-token endowment between themselves and another player, the participant divides the endowment between two anonymous players. In this task, the participant has no opportunity to keep any of the endowment for themselves. Again, this game is played three times. The first time, participants play with no information about the other players' wealth. In the second and third round, one of the players is described as wealthier than the other player, who in turn is described as poorer than the first player. In one round, Player A is the wealthier child, and in the other round Player B is the wealthier child; the order of this is randomized.

Variations of this game have previously been used in children (e.g., [Cappelen et al., 2020](#)). The outcome of interest is whether participants from different treatment arms make unequal distributions and how this relates to the other players' characteristics, particularly their relative wealth.

4. Joy of Destruction Game

The Joy of Destruction Task, first introduced by [Zizzo and Oswald \(2001\)](#), allows participants to pay from their own endowment to reduce the payout of another player. The participant and their partner will both receive an endowment of 10 tokens. The participant can then decide to reduce their partner's endowment by giving up some of their own endowment using an exchange rate of 2:1. In other words, for each token the participant spends, their partner loses two tokens. Again, this game is played three times (no-wealth information, wealthier partner, poorer partner).

This task measures an individual's willingness to engage in costly destructive behavior, which is interpreted as a sign of envy. We intend to examine whether individuals are more likely to destroy a part of the other player's endowment based on the participant's treatment status and the other player's relative wealth. Versions of this game have been previously used in rural African settings to measure inequality aversion and retaliation ([Kebede and Zizzo, 2015](#)), though to the best of our knowledge it has never been employed in children.

5. Public Goods Game

In the Public Goods Game, participants play in groups of four. All four players are given an endowment of 10 tokens, any proportion of which they can "invest" in the public good. The total amount invested in the public good by all four players gets doubled, and this doubled amount is then evenly divided between the four players.

To maximize personal payoffs, it is in participants' interest to keep their entire endowment for themselves and "free-ride" on the other three players' investment. However, to maximize communal payoffs, all participants should invest their entire endowment. Thus, the key measure here is how much participants contribute to the public good, whether they act in line with free-riding or communal maximization. This game has typically been used in adults, though it has recently been adapted for children (e.g., [Silva, Boccardi, Dutra, Hattori, Yamamoto and Alencar, 2016](#), [Vogelsang, Jensen, Kirschner, Tennie and Tomasello, 2014](#)).

2.6.2. Comprehension

We will record the number of attempts a participant needs to get each task comprehension question right.

2.7. Surveys

2.7.1. Child Survey

To measure children's explicitly self-reported social preferences, we will administer a survey. Namely, we are interested in participants' mental health, dispositional envy, expectations regarding redistribution and cooperation, and feelings of trust toward various other parties. This survey includes some standard modules: CESD-10 module for kids ([Weissman, Orvaschel and Padian, 1980](#)), World Value Survey's happiness and life satisfaction questions, Children's Hope Scale ([Snyder, Hoza, Pelham, Rapoff, Ware, Danovsky, Highberger, Ribinstein and Stahl, 1997](#)), MacArthur ladder ([Adler, Epel, Castellazzo and Ickovics, 2000](#)), the Benign and Malicious Envy Scale (BeMaS; [Lange and Crusius, 2015](#)), and Raven's Matrices. Other modules were put together to measure envy, social norms prescriptively and descriptively, trust, social ties and closeness, locus of control, and zero-sum thinking.

2.7.2. Adult Survey

To assess the persistence of the cash transfers impact on household consumption, asset ownership, and individual well-being we will survey adults in the household. We will use virtually the same consumption and assets modules used in [Haushofer et al. \(2020\)](#) for comparison with the previous endline. The individual well-being module is a short questionnaire including World Value Survey's happiness and life satisfaction questions, MacArthur ladder, and PHQ-2.

3. Econometric Approach

We are interested in how the behavior of participants varies based on treatment status.

3.1. Baseline estimating equation:

Our baseline estimating equation for the games will take the following basic form:

$$y_{ijpv} = \beta_1 \text{CashTransfer}_{ipv} + \beta_2 \text{Spillover}_{ipv} + \varepsilon_{ijpv} \quad (1)$$

In equation 1, y_{ijpv} is the outcome of interest, where i indexes the child of parent p in village v who is paired with another child j . The variable $\text{CashTransfer}_{ipv}$ is an indicator variable equal to 1 if the participant's parents received the cash transfer. The variable Spillover_{ipv} is an indicator variable equal to 1 if the participant's parents were in the spillover condition. Standard errors will be clustered at the village level.

We are interested in several coefficients. First, β_1 represents the effect of a participant's parent receiving the cash transfer on allocation choices in the experiment; β_2 represents the effect of a participant's parents being in the spillover group.

In a variation on our baseline equation (see equation 2), we will include fixed effects for the child's age and gender, denoted by $\alpha_{a(i)}$ and $\alpha_{g(i)}$ and for the other player's age and gender, likewise denoted by $\alpha_{a(j)}$ and $\alpha_{g(j)}$. For the games with relative wealth of player j , we will also have a specification where we include fixed effects for relative wealth (ie. poorer or richer than player i).

$$y_{ijpv} = \alpha_{a(i)} + \alpha_{a(j)} + \alpha_{g(i)} + \alpha_{g(j)} + \beta_1 \text{CashTransfer}_{ipv} + \beta_2 \text{Spillover}_{ipv} + \varepsilon_{ipjv} \quad (2)$$

We will also estimate a version of equation 2 where we include a set of parent level control variables, such as baseline measures of wealth, consumption, and household structure. In addition, we will examine heterogeneity based on a child's social proximity to the other player, e.g. based on whether they are of similar age or of the same gender.

For the survey question analysis, we will estimate equations analogous to equations 1 and 2, but without out any information on the other player since the survey questions refer only to the respondent.

3.2. *Wealth information games*

Our estimating equation for the wealth information games will take the following form:

$$y_{ijpv} = \beta_1 \text{PoorerOpponent}_{ipj} + \beta_2 \text{CashTransfer}_{ip} + \beta_3 \text{Spillover}_{ip} + \beta_4 \text{PoorerOpponent} \times \text{CashTransfer}_{ipj} + \beta_5 \text{PoorerOpponent} \times \text{Spillover}_{ipj} + \varepsilon_{ijpv} \quad (3)$$

In equation 3, y_{ijpv} is the outcome of interest exclusively for the wealth information games, where i indexes the child of parent p in village v who is paired with another child with characteristics j . The variable $\text{PoorerOpponent}_{ipj}$ is an indicator variable equal to 1 if child j comes from a poorer family than child i 's family. The base category here is a richer opponent. Variables $\text{PoorerOpponent} \times \text{CashTransfer}_{ipj}$ and $\text{PoorerOpponent} \times \text{Spillover}_{ipj}$ are interaction terms between the relative wealth of the opponent and the treatment status.

3.3. *Child and Adult Surveys*

Our econometric approach will follow [Haushofer et al. \(2020\)](#) for analyzing the survey outcomes.

3.4. *Outcomes*

In the Dictator Game, the dependent variable is the amount given by player i to player j . In the Third-Party Dictator Game the dependent variable is whether the participant makes unequal offers. In the Ultimatum Game the dependent variable is the amount offered by player i to player j . In the Joy of Destruction Game, the outcome of interest is whether the participant chooses to decrease the other player's endowment. In the Public Goods Game we will examine how much the participant allocates to the public good.

We will also include outcomes coming from the child survey, around their mental health, dispositional envy, expectations regarding redistribution and cooperation, and feelings of trust toward various other parties. Main outcomes of interest for the adult survey are indexes of asset ownership, total expenditure, and a well-being index.

3.5. *Robustness*

Given the potential non-independence of observations across the choices of an individual, in addition to robust standard errors, we will also calculate standard errors clustered at the player i level. We will also calculate standard errors using randomization inference.

3.6. Robustness and sensitivity checks

We will undertake a number of additional sensitivity checks to test the robustness of our findings.

These include:

1. Controlling for round 1 vs. round 2 fixed effects in the wealth information games (i.e., which iteration of the game they played against the poorer opponent vs. the richer opponent).
2. Controlling for day-of-week fixed effects.
3. Controlling for enumerator fixed effects.
4. Checking for and omitting influential observations using standard methods e.g., Cooks distance, DFITS, etc.

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