Timing Psychological Engagement with Cash Transfer Recipients:

Analysis Plan for Endline Outcomes

8 March 2019

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Transparency and Reproducibility: The proposed project (referred to as the "timing trial" throughout this document) will be registered here: socialscienceregistry.org/trials/3737. Additional study materials are available online at osf.io/uwmjn. The timing trial builds on a larger trial by Orkin et al. (forthcoming), which is referred to as "trial #996" throughout this document and has been registered here: socialscienceregistry.org/trials/996.

Acknowledgments: I thank BRAC Uganda, Munshi Sulaiman, and Ricardo Morel for data access. The following contributions to trial #996 are gratefully acknowledged: Video development: Johannes Haushofer, Kate Orkin, Catherine Thomas, Michala Iben Riis-Vestergaard. Experimental design: Stefan Dercon, Rob Garlick, Johannes Haushofer, Kate Orkin. Census questionnaire development and coordination: Rob Garlick, Kate Orkin, Gabor Nyeki, Emma Riley, Victoria Schreitter. Sampling and randomization: Rob Garlick, Mahreen Mahmud, Kate Orkin, Natalie Quinn, Emma Riley. Psychological scale validation: Catherine Thomas, Hannah Uckat. Baseline questionnaire development and coordination: Gabor Nyeki, Kate Orkin, Natalie Quinn, Victoria Schreitter. Coordination with GiveDirectly for cash transfer intervention: Stefan Dercon, Kate Orkin, Emma Riley. Intervention questionnaire design and coordination: Rob Garlick, Mahreen Mahmud, Geetika Nagpal, Kate Orkin, Emma Riley. Endline questionnaire design and coordination: Stefan Dercon, Lizzie Dipple, Rob Garlick, Mahreen Mahmud, Geetika Nagpal, Kate Orkin, Natalie Quinn, Emma Riley, Gabor Nyeki. Endline analysis: Marta Grabowska, Axel Eizmendi Larrinaga, Geetika Nagpal. Overall fieldwork management: Sam Fishman, Mungai Mwangi, Charles Amuku. Staff support: Suleiman Asman, Rose Page, Stephanie Palla, Allison Stone, staff at the Blavatnik School of Government. GiveDirectly partnership: Michael Cooke, Will Le, Stephanie Palla, Caroline Teti, Carolina Toth. Support from the General Equilibrium study: Ted Miguel, Paul Niehaus and Michael Walker. Comments: Doug Gollin, Alan Stein, Duncan Thomas, Chris Udry.

An "Analysis Plan for Cognitive Outcomes" (registered[1] on 13 March 2018) discussed the analysis of outcomes that were collected just before the psychological intervention. An "Analysis Plan for Recall and Agency Variables in the Manipulation Check Data" (registered[2] 4 November 2018) discussed the analysis of outcomes collected immediately after the psychological intervention. This comprehensive analysis plan subsumes both and adds behavioral and economic outcomes that were collected at the time of the endline.

Motivation

This study is motivated in part by an empirical finding. Throughout the year 2013, the microfinance organization BRAC evaluated the case for engaging loan takers in a psychological intervention involving goal-setting and plan-making. In five rural Ugandan branches, it invited a convenience sample of microcredit customers to participate in the intervention on the day they picked up their loans. Individuals were assigned at random to participate in one of three conditions: no psychological intervention; the psychological exercise administered just before the loan is disbursed; or the psychological intervention administered in the reverse order (so just after the cash disbursements). The intervention exercises were inspired by literature on implementation intentions (Gollwitzer & Sheeran, 2006) and in line with a BRAC tradition to engage its customers in so-called "promises" to make development commitments to BRAC. They were conducted by the branch managers, who spent an estimated 10 minutes with participants following an intervention template.[3]

[1] See socialscienceregistry.org/docs/analysisplan/1473/document, originally registered under trial #996: socialscienceregistry.org/trials/996.
[2] See socialscienceregistry.org/docs/analysisplan/1876/document, originally registered under trial #996: socialscienceregistry.org/trials/996.
[3] See online study materials.

Table 1: First-Month Effects among Loan Takers

Impact of Psychological Intervention (Counterfactual: No Psychological Intervention)

	Internal Control Index		Monthly Cash Outflow		Weekly Cash Inflow	
Specification	(1)	(2)	(1)	(2)	(1)	(2)
Coefficient	0.036 *	0.049 ***	-162.1	-41.3	23.33 **	7.77
Standard Error	(0.019)	(0.016)	(106.0)	(86.8)	(10.62)	(9.47)
Observations	271	326	263	329	268	329

Impact of Psychological Intervention *After* Loan Disbursement (Conterfactual: *Before*)

	Internal Control Index		Monthly Cash Outflow		Weekly Cash Inflow	
Specification	(1)	(2)	(1)	(2)	(1)	(2)
Coefficient	0.007	0.031 **	24.0	80.4	-2.92	18.07 *
Standard Error	(0.019)	(0.015)	(74.7)	(56.7)	(12.41)	(9.33)
Observations	213	248	210	249	208	249

Notes:

- Regression specification (1) is $y_{ij} = \alpha_j + \beta T_{ij} + \delta X_{ij} + \varepsilon_{ij}$, where y_{ij} is the outcome of interest for individual i from village j; α_j is a village fixed effect; T_{ij} is the randomized assignment, coded to 1 among participants who were assigned to the treatment condition; β is the coefficient estimating the impact of treatment assignment; X_{ij} is a vector of three socioeconomic covariates selected using least angle regression (Efron, Hastie, Johnstone, & Tibshirani, 2004); and ε_{ij} is an idiosyncratic error term. Specification (2) excludes village fixed effects and socioeconomic covariates: $y_{ij} = \alpha + \beta T_{ij} + \varepsilon_{ij}$. This specification allows for the inclusion of more observations (i.e., it includes ones for which covariates are unavailable).
- * indicates a p value < 0.1; ** p < 0.05; *** p < 0.01.
- The internal control index is derived from Duttweiler (1984); measures are standardized.
- Monthly cash outflows are the USD cash expenses associated with the respondent household's agricultural, livestock & poultry, and micro-enterprise operations, measured using a 30-day recall period.
- Weekly cash inflows are the USD cash revenues associated with the respondent household's agricultural, livestock & poultry, and micro-enterprise operations, as well as any income from salaried or day labor, measured using a 7-day recall period.

Follow-up of participants was conducted one and two months after the intervention, using a basic financial diary survey.[4] 329 participant responses were collected in the first month. A subset of participants had previously participated in routine socioeconomic data collection, allowing for the creation of covariates for most observations. As illustrated in the top row of Table 1, the psychological intervention did in fact appear to alter outcomes: treated participants reported a higher sense of control over their lives, and there are indications that their cash revenues increased.

[4] See online study materials.

But of central interest to the work presented here is the second row of Table 1: conditional on participating in the psychological intervention, both impacts on internal control and cash revenues appeared more pronounced when the intervention was administered immediately after, as opposed to immediately before, the loan disbursement. This appears useful from the perspective of policy action: conditional on implementing a goal setting exercise at all, it must be implemented either before or after the disbursement, and the costs of both options appear equivalent. However, a number of questions remain open.

First, are the findings a mere fluke? The trial should be regarded as a pilot at best. The significance of economic effects is statistically borderline and very sensitive to specification choices; impacts subsided on economic measures by the second month; none emerged on additional available outcomes (i.e., working hours and consumption); and none are robust to multiple inference adjustments. Also, sampling and tracking procedures were subject to tight budget constraints. It would be useful to subject the test to a stricter scientific standard, using a larger sample.

Second, if this effect is true, why (and therefore, in what range of contexts) might it hold? Post hoc, different scientific disciplines jointly provide a testable theoretical framework, as sketched in the "Analysis Plan for Cognitive Outcomes". This framework relies on two propositions that can be gleaned from the literature. One is that goal-setting and plan-making activities demand cognitive resources; the other is that cash inflows generate temporary cognitive boosts among the poor. The cash transfer may therefore "cognitively prepare" cash transfer recipients to pay attention to, internalize, act upon, and ultimately benefit from the psychological intervention.

Goal-Setting and Plan-Making are Cognitively Demanding

The concept of bounded rationality concedes that it is tall proposition for economic agents to navigate a complex and dynamic world with perfection, and that they therefore apply a set of cognitive crutches (Simon, 1955). Goals may be among these crutches: Selten's (1998) so-called *aspiration adaptation theory* proposes that the cognitive challenge of constantly re-optimizing one's objective function can be mitigated

by setting and (at least temporarily) sticking to goals. This is easily reconciled with psychological science: the well-established *mental contrasting and implementation intentions* approach holds that a process of goal-setting and plan-making (involving the imagining one's future desired future, reflecting on the obstacles to attaining it, and creating behavioral strategies to overcome these obstacles) helps make ongoing goal striving cognitively easy and quasi-automatic. However, the goal-setting and plan-making process itself is a cognitive investment, and people are not thought to perform it effectively while cognitive resources are taxed by other activities (Oettingen & Gollwitzer, 2010).

Goals and plans can be regarded as a subset of a broader constellation of mental constructs that humans use to navigate their complex and dynamic surroundings. Different strands of literature emphasize the role of other schematic cognitions (such as narratives and beliefs) in human behavior and socio-economic outcomes – ranging from economic theory (Bénabou & Tirole, 2011; Collier, 2016; Gabaix, 2014; Hanna, Mullainathan, & Schwartzstein, 2014; Hoff & Stiglitz, 2015), to applied social psychology (Dweck, 1999; Hall, Zhao, & Shafir, 2014; Paunesku et al., 2015; Walton, 2014; D. S. Yeager & Walton, 2011; David S Yeager, Paunesku, Walton, & Dweck, 2013), to clinical psychology (Padesky, 1994). So-called *cognitive load theory* holds that cognitive load more generally impedes schema adoption (Sweller, Ayres, & Kalyuga, 2011; Swezller & Sweller, 1994). However, there is no consensus that this holds universally: Gilbert (1991) suggests that the acceptance of new beliefs is cognitively easier than their subsequent rejection, implying that they are malleable (i.e., that people are more persuadable) when cognitive load is high.

Cash Inflows Generate Cognitive Boosts Among the Poor

Poverty may impede cognitive performance via a number of physical stressors such as malnutrition, heat, and noise pollution, and associated discomfort, disease, and sleep deprivation – but also via a more direct mechanism that does not invoke physical stressors at all: it may draw the cognitive resources of the poor towards immediate poverty-related concerns and thereby away from other potential objects of attention (Boswell Dean, Schilbach, & Schofield, 2017). Working with Indian sugarcane farmers, Shah,

Mullainathan, Shafir, & Zhao (2013) demonstrate that cognitive performance (as measured by fluid intelligence and cognitive control) increased substantially in the immediate aftermath of harvest.

Of course, wealth in the broad sense of "economic prospects" or "net worth" was not technically affected by the harvest in any systematic manner; it would be less precise to say that the harvest made farmers richer than that it made them more liquid (by transforming an illiquid asset into a liquid one). But in the terminology of Mullainathan & Shafir (2013), the farmers experienced a change in *scarcity*: the psychological burden of having too little. For the poor, who juggle pressing expenses and manage unsteady income, the object of this scarcity is thought to be cash. The cognitive impacts could therefore hold for our Ugandan loan takers in much the same way as they did for the Indian sugarcane farmers, as the cash inflow from a loan disbursement may be used for cash flow smoothing (Banerjee, Duflo, Glennerster, & Kinnan, 2015).

The evidence on the cognitive impacts of cash inflows remains somewhat ambiguous: Carvalho, Meier, & Wang (2016) detect no differences in the cognitive performance of low-income in the Unites States before and after payday. One noteworthy difference between the sample of Carvalho et al and that of Shah, Mullainathan, Shafir, & Zhao (2013) is that cash inflows were much more frequent, and arguably more predictable.

New Study Context

Endline data collection is currently ongoing for a large trial in Kenya that crosses a cash transfer intervention and a psychologically active intervention in the form of a factorial design, with treatment clustered at the village level (Orkin et al., forthcoming).[5] In those villages that were assigned to both the cash and psychologically active intervention, the opportunity presented itself to randomize the timing of the psychologically active intervention relative to the cash intervention (more specifically, to assign it at

[5] See socialscienceregistry.org/trials/996.

random to take place either before or after the first lump sum). This allows for a test of the hypothesis that timing the psychologically active intervention after a major cash inflow is in fact more impactful than timing it in the opposite order.

Sampling, Balance, and Attrition

Participants whose villages had for the purposes of the trial by Orkin et al. (forthcoming) been assigned into that arm which involves both cash transfers and the psychologically active intervention were further randomized at the individual level to either be candidates for receiving the psychologically active intervention in the weeks before or in the weeks after the transfer in question. (Note: The cash transfer provider GiveDirectly makes three transfers labeled Token (10,000 Kenyan shillings), Lump Sum A (50,000 Kenyan shillings), and Lump Sum B (50,000 Kenyan shillings, minus the price of a mobile phone where applicable) in approximately two-month intervals. The transfer in question is currently defined as Lump Sum A). The individual-level randomization was stratified on the respondent's per capita housing space relative to the village median; widow status; and secondary educational attainment. By design, the sample will therefore be limited to respondents who were randomized into the individual (before vs after) condition. In addition, the initial analysis will be limited to respondents with the following attributes:

A. Successfully baselined; and

B. Received the transfer in question in that month in which GiveDirectly first made the transfer in question in the same village; and

C. Received the psychologically active intervention within 72 hours of the first participant who received the psychologically active intervention in the same village and sub-arm.

To assess if this restriction introduces bias, we will test if the odds of exclusion differ by individual level assignment. We will also test if the odds of attrition in the manipulation check data and the endline data differ by individual level assignment. In addition, we will test for covariate balance by testing if the eleven census covariates listed below predict individual level assignment, using an F test of joint orthogonality.

Depending on our analysis, one or more amended samples may be formed that exhibit a higher degree of attrition and/or noncompliance, but are less susceptible to bias and/or better balanced. If so, results for the restricted sample will continue to be shown.

Outcomes Collected Immediately Before the Psychological Intervention

1. Cognitive Performance:*[6] An index[7] comprised of the following three variables:
    1.1. Working memory (digit span):* Where no response is correct, scored as zero. Otherwise scored as the length of the longest sequence that respondents can correctly recall, minus two points.
    1.2. Fluid intelligence (Raven's matrices):* Scored as the number of correct responses provided within the time limit.
    1.3. Cognitive control (numerical Stroop):* Scored as the number of correct responses provided within the time limit.

Outcomes Collected Immediately After the Psychological Intervention

2. Recall:* An index comprised of three variables:
    2.1. Name Recall 1:* A dummy variable coded to one if the respondent correctly recalls that a character name was mentioned in the movie.
    2.2. Name Recall 2:* A dummy variable coded to one if the respondent correctly recalls that the name of the lead character in the movie is "Judy".
    2.3. Fact Recall:* A dummy variable coded to one if the respondent answers that young Kenyan men who do secondary school are more likely to end up working in a paid job than those who do not.[8]
3. Self-Beliefs: An index[9] comprised of two variables:

[6] Symbol * is added to all outcomes for which the baseline covariate is not used or not available.
[7] All indices in this paper will follow the index constructed methodology used in Kling, Liebman, & Katz (2007).
[8] This statement was made during the video intervention.
[9] These were labeled "agency" variables in the "Analysis Plan for Recall and Agency Variables in the Manipulation Check Data". At the time these outcomes were registered for the timing study, the study team associated with the main study by Orkin et al. (forthcoming) had already analyzed the impact of cash and/or psychological interventions on these variables. However, the impact of timing as described in this analysis plan had (and still has) not been analyzed.

3.1. Self-efficacy: A scale from 0 to 28, constructed from the sum of 7 items, each scored from 0 to 4 points.[10]

3.2. Growth mindset:* A scale from 6 to 36, constructed from the sum of 6 items, each scored from 1 to 6 points.

Endline Outcomes

The cleaning and operationalization of all outcomes is equivalent to the analysis plan[11] for trial #996 by Orkin et al. (forthcoming), with one exception: in the timing trial, index construction will follow the methodology used in Kling, Liebman, & Katz (2007), so as to avoid deviation from previous versions of the analysis plan for this study.

Behavioral / Activity Outcomes

4. Technology Adoption and Information Seeking:* An index comprised of the following:

    4.1. Technology adoption in crop agriculture*

    4.2. Technology adoption in livestock related activities*

    4.3. Enterprise expansion activities*

    4.4. Information seeking activities*

Economic Outcomes

5. Economic Investment: An aggregate comprised of the following:

    5.1. Agricultural input expenditures

    5.2. Livestock input expenditures

    5.3. Non-agricultural expenditures

6. Revenue: An aggregate comprised of the following:

    6.1. Revenue from agriculture

[10] The "Analysis Plan for Recall and Agency Variables in the Manipulation Check Data" did not account for the fact that baseline data are available for self-efficacy. The impact of the relative timing of the cash and psychologically active interventions on this outcome has not been initiated.
[11] socialscienceregistry.org/docs/analysisplan/2134/document

6.2. Revenue from livestock rearing and produce

6.3. Revenue from non-agricultural activities

6.4. Total household labor earnings

7. Consumption: An aggregate comprised of the following:

7.1. Food consumption

7.2. Non-food non-durable consumption

7.3. Expenditure on durable goods

7.4. Social expenditure

7.5. Education expenditure

8. Assets: An aggregate comprised of the following:

8.1. Durable assets

8.2. Livestock

8.3. Savings

8.4. Net financial liabilities

8.5. Stocks of dried maize*

Empirical Strategy

The following model will be estimated:

$$y_{ij} = \alpha_j + \beta A_{ij} + \gamma b_{ij} + \delta X_{ij} + \varepsilon_{ij}$$

Here y_{ij} is the outcome of interest for individual i in village j; α_j is a village fixed effect; A_{ij} is the randomized assignment, coded to 0 (to 1) among participants who are assigned to receive the cognitive test and the psychological intervention before (after) the first lump sum; coefficient β estimates the impact of intended assignment to the 'after' condition; X_{ij} is a vector of socioeconomic covariates comprised of the following census variables:

- Respondent's age;

- Household's per capita housing space;
- Respondent's secondary educational attainment (a binary variable; seventh grade or below versus eighth grade or above);

ε_{ij} is an idiosyncratic error term. The term b_{ij} is the baseline value of y_{ij}; it is only available for a subset of aforementioned outcome measures. The analysis of outcomes that are marked with * will not include a baseline covariate. Composites (i.e., indices and aggregates) are defined as having a baseline available as long as baseline values are available for at least one of the sub-composites.

Robustness checks

In addition to the above, we will estimate a specification without socioeconomic covariates, and one substituting X_{ij} for a vector of three socioeconomic covariates selected using the least angle regression algorithm by Efron et al. (2004). In addition to the aforementioned covariates, this algorithm will select among the following census variables:

- Household marital status (binary variable; married versus not)
- Household size (continuous variable)
- Roof material (binary variable; metal versus not)
- Toilet facility (binary variable; covered latrine, flush, or vip toilet versus other)
- Wall materials (binary variable; mud versus other)
- Floor material (binary variable; mud versus other)
- Asset index (continuous variable; number of assets reported)
- Enumerator's quintile ranking (continuous variable; five classes)

We will also aim to estimate a specification that replaces village fixed effects with stratum fixed effects. (This may result in a better powered analysis; however, the data from which the strata must be derived is

not within the control of the author). We will test the sharp null hypothesis using randomization inference (random number seed 98765; 10,000 simulations).

Multiple Inference Adjustments

Separate tables will show treatment effects on the sub-components of each of the eight outcome families, which are:

1. Cognitive Performance
2. Recall
3. Self-Beliefs
4. Technology Adoption and Information Seeking
5. Economic Investment
6. Revenue
7. Consumption
8. Assets

Each table will individually control for the false discovery rate, following Anderson (2008) and Benjamini & Hochberg (1995). In addition, a summary table showing the impacts on the eight outcome families described above will control for the false discovery rate among the aggregates.

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