Pre-Analysis Plan

for

“A Randomized Controlled Trial Varying Unconditional Cash Transfer Amounts in the United States”

July 17, 2020

1 Index Content

The four tables below describe the variables we will use in our indices as part of our confirmatory hypotheses. Please see the accompanying “Study Materials” document for a complete description of the variables. All outcomes listed below will be measured in the baseline survey and at each time point thereafter, except for the Raven’s matrices, which will only be measured after the intervention (i.e., not in the baseline survey). Control variables, manipulation check variables, variables corresponding to exploratory analyses, and other variables not part of these confirmatory hypotheses are not listed here.

1.1 Financial

Variable Name	**Description**
EMPLOYMENT	Binary indicator for whether the participant checks off any of the 3 "employed" options.
WORKPERFORMANCE	
WORKSATISFACTION	
SAVINGS	Primary specification: non-transformed values. Secondary specification: binary indicator for whether the participant has any savings at all.
EARNEDINCOME	Primary specification: non-transformed values.
FINWELLBEING1, FINWELLBEING2, FINWELLBEING3, MEETNEEDS, MEETWANTS	Composite measure of all questions. FINWELLBEING2 and FINWELLBEING3 are reverse coded. Rescaled so all are on the same scale.
CREDITCONSTRAINTPOSSIBLE	

1.2 Psychological well-being

Variable Name	**Description**
AGENCY1, AGENCY2	Composite measure of all questions. AGENCY2 is reverse coded.
BESTLIFE, POSMENTALHEALTH1, POSMENTALHEALTH2, POSMENTALHEALTH3, HAPPY	Composite measure of all questions. Rescaled so all are on the same scale.
ANXIOUS, LONELY, DEPRESSION1, DEPRESSION2, DEPRESSION3, DEPRESSION4, DEPRESSION5, DEPRESSION6, DEPRESSION7, DEPRESSION8	Composite measure of all questions. All reverse coded. Rescaled so all are on the same scale.

1.3 Cognitive capacity

Variable Name	Description
RAVENS	Score for the entire set of nine questions. Collected at every timepoint except t1.
MONEYMIND[t]A, MONEYMIND[t]B	The extent to which the participant would think about the cost-related statements, reverse coded and averaged across the two scenarios presented at each timepoint t. The cost-related statements for each scenario are marked in the study materials.
MEMORY1, MEMORY2, MEMORY3, MEMORY4	Composite measure of all questions; reverse coded.

1.4 Physical health

Variable Name	Description
HEALTH	
SLEEP	
FOODSECURITY	
DIET	
EXERCISE	

2 Index Construction

We will construct composite indices using a technique described in detail in M. L. Anderson (2008), which we summarize here. First, all outcomes will be oriented such that higher values are "better." Next, we will demean the outcomes and divide each one by the control group standard deviation at each time point. These "transformed" outcomes can then be compared on a common scale. After standardizing the effect sizes, we will create a weighted average of the transformed outcomes for each individual for each measure in each domain. The weight of each input is equal to the sum of the row entries in the inverted covariance matrix in each domain. This weighted average will be applied to the measures we use in our analyses. We plan to construct four indices: financial well-being, psychological well-being, cognitive capacity, and physical health.

As noted in M. L. Anderson (2008), with this procedure, the final outcome measure ignores missing values. As described below, we will also conduct robustness analyses where missing values are imputed, which in turn means the indices will include the imputed values at that point. Moreover, outcomes within a given domain that are highly correlated with one another receive relatively little weight within the index, while outcomes that are not highly correlated (and thus carry additional information) receive comparatively more weight.

3 Non-compliance

We do not expect serious issues of non-compliance, as participants in both the Small Cash and Large Cash treatments will receive their unconditional cash transfer payments through direct deposit into their bank accounts. This means that participants in these groups should always be compliant with the receipt of the cash. The only exceptions to this are if there are clerical or technological issues that prevent the cash transfer, or if a participant refuses to receive payment. While our primary analysis is an Intent-to-Treat, and so even participants in the Small Cash and Large Cash groups who do not receive their UCTs will be treated in our analyses as if they did. However, we will observe any clerical or technological issues that prevent the cash transfer in our data. Thus, we will be able to examine these participants separately, as well.

One possibility is that some participants may receive the cash but not be aware of it. Awareness of the cash payments will be assessed through the RECEIVEDSTUDY survey question in t2. A participant who received a UCT but who does not check off the box indicating that they have received money from the non-profit organization, and/or indicates that they have received no more than $40 in the last month (where $40 is the sum of the payments that they would receive for completing the first two surveys), will be marked as unaware of their treatment status. Similarly, a participant who does not receive a UCT but reports that they did will also be marked as unaware.

Because the primary analysis will use an Intent-to-Treat approach, even those participants who are unaware of their treatment status will be analyzed based on the treatment group to which they were assigned. However, in additional analyses, we will also examine the outcomes of only those participants who correctly reported their treatment status, using a Treatment-on-the-Treated approach.

4 Data Exclusions

We do not plan on excluding any observations in our primary analyses. However, as described above, we intend on separately analyzing those participants who seem to hold incorrect beliefs about their treatment status.

5 Attrition and Missing Data

Conditional on being randomized (i.e., completing both the profile survey and the t1 surveys), participants will be invited to complete all subsequent surveys, regardless of how many previous ones they have skipped. That is, we will not remove any participants from the experiment, unless they request to be removed.

In our main analyses, we will not alter or impute missing values. Index components that have partially missing data will be rescaled accordingly. For instance, a participant answering only one of the two agency questions will receive an agency score reflecting only data for the one answered question.

To assess the extent to which missing data may be non-random (either due to attrition or skipping survey questions), we will test whether treatment group or any of our observed measures (including demographics and financial status) can predict the missingness. We will calculate Lee bounds (Lee 2009) to establish conservative estimates of our treatment effects. In addition, we will apply a multiple imputation method for missing values in time series data (Honaker and King 2010). In robustness checks, we will examine whether our results hold when missing values are imputed.

6 Outliers

To account for outliers, we will employ a 95% winsorization for upper values on variables without upper bounds, such as SAVINGS and EARNEDINCOME. In addition, in case we find that the skew and kurtosis of our unbounded data exceeds recommended thresholds, we will employ commonly used data transformations, such as the natural log (e.g., see He and Côté 2019, for a similar analysis strategy).

7 Confirmatory Analyses

Our identification strategy is based on random assignment to a treatment group. Our primary analyses will involve collapsing across the participants who do and do not have access to the non-profit organization's online platform, simply examining the main effects of UCT levels. We will use an Intent-to-Treat approach.

We will use analysis of covariance (ANCOVA) to estimate the treatment effects, conditioning on the baseline measure of the composite index of interest to improve statistical power (McKenzie 2012). Let $t = 1$ be the baseline survey before intervention, and t=2, 3, and 4 be the surveys after the intervention. Our primary specification is:

$$y_{i,t>1} = \beta_0 + \beta_1 SC_i + \beta_2 LC_i + \beta_3 OP_i + \delta y_{i,t=1} + \epsilon_i \tag{1}$$

Where y is one of four composite indices (financial, psychological, cognitive capacity, and physical health) for individual i at time t. SC is an indicator variable that equals 1 if the participant is in the Small Cash condition and LC is an indicator variable that equals 1 if the participant is in the Large Cash condition. The omitted category is the Control group. OP is an indicator variable that equals 1 if the participant has access to the non-profit organization's online platform. Finally, $y_{i,t=1}$ is the baseline measure of the composite index. All standard errors will be robust.

In robustness checks, we will include participant demographics and survey time variables (when the survey was taken) as covariates. We will also separately analyze participants who did and did not correctly report whether they received money, analyze the subcomponents of the indices, and analyze alternative methods of constructing the indices (e.g., placing the FOODSECURITY question into the financial index rather than the physical health index).

In exploratory analyses, we will examine the main effects of having access to the online platform versus not having access, and test for any possible interactions with the UCT amounts. We will also conduct a wide range of subgroup analyses (e.g., based on household size, baseline poverty level, geography, and demographics) to identify any heterogeneity in the effectiveness of either treatment or interactions between the treatments.

8 Multiple Hypothesis Testing Corrections

To address potential multiple hypothesis testing concerns, we will employ a Benjamini-Hochberg approach (Benjamini and Hochberg 1995). This approach uses a step-down False Discovery Rate (FDR) method of controlling Type I error rates. It ranks the naïve p-values of related comparisons (i.e., the outcomes in our setting) and divides the rank of each p-value by the number of tests (i.e., four outcomes in our setting). We will use a standard significance threshold of 5% and an FDR threshold of 10%, and report the adjusted p-values in our regressions (for recent empirical examples, see Heller et al. 2017; Seira, Elizondo, and Laguna-Müggenburg 2017).

References

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