The Effects of Cash-Plus-Job-Training Programs on Young Adults: Evidence from a Randomized Controlled Trial
Pre-Analysis Plan

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1 Design of the Experiment

In the proposed project, we are conducting a randomized controlled trial (RCT) which will be evaluated using surveys. In August 2022, the Department of Social Services of Los Angeles (LA DPSS) began implementing a two-part program called the "TAYportunity plus Guaranteed Income" Program in which work-ready young adults aged 18 to 24 receive an unconditional guaranteed income in addition to job-training and apprenticeship opportunities for 36 months. Among eligible individuals (see eligibility criteria below), we randomly selected 300 individuals to participate in the TAYportunity program, and another 650 individuals were randomly chosen for the control group. Throughout the 36-month program we will conduct several short, 15-minute surveys of all 950 study participants that will be completed online or by phone. The surveys will be the same as across survey rounds, so that we obtain repeated measures of the same outcomes over time. All respondents will be compensated $20 for completing each of the surveys.

2 The TAYportunity Plus Guaranteed Income Program

LA DPSS announced the TAYportunity program and collected applications in June 2022. Applicants were given full information about the "lottery" nature of the program – they were told that they were applying to be in a research study and had a chance to be randomly selected into the "TAYportunity plus Guaranteed Income" program. LA DPSS accepted applications from members of their General Relief Opportunities for Work (GROW) program, which has approximately 6,000 participants. After randomly selecting the 300 individuals in the treatment group and the 650 in the control group, participants were informed of whether they are in the TAYportunity plus Guaranteed Income program. The TAYportunity plus Guaranteed Income program will continue for a period of 36 months. The treatment group is receiving an unconditional cash transfer of $1,000 per month and is being offered the opportunity to participate in the TAYportunity program. The TAYportunity program consists of the following paths that interested participants can choose between:

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1. Transitional subsidized employment for six months, 40 hours per week, at a rate of $15/hr

2. Apprenticeships in fields of construction, aero-space and bioscience

3. Entrepreneurship support for participants interested in self-employment

Transfers are being made through participants' electronic benefit transfer cards. Participants have been given full information regarding the magnitude of the transfers they will be receiving, when they are receiving those transfers, and the date on which transfers will end.

3 Sample Selection, Stratification, and Enrolment

3.1 Eligibility and Stratification

In order to participate in the study, individuals must be between the ages of 18 to 24, a resident of Los Angeles County, and a member of the LA GROW program. Additionally, individuals must be work-ready, meaning that they have a valid government identification, a social security card, have obtained their high school diploma or GED (or select a vocational/employment path that does not require this), and have no criminal felonies.

3.2 Recruitment and Enrollment

Over 3,211 individuals applied to the TAYportunity plus Guaranteed Income Program. Of those 3,211, LA DPSS randomly selected (with help from the research team) 300 individuals into its program. Additionally, the research team randomly selected 650 individuals from the applicant pool who were not selected for the program to serve as the control group.

The 300 treatment individuals were informed by LA DPSS that they have been selected for the program and have been invited to participate in a survey study. The 650 control individuals were contacted by LA DPSS and were informed that while they were not selected for the program, they have been invited to participate in a research study about their experience in the GROW program and in the labor market as a young adult in Los Angeles County. Individuals from both groups who accept the invitation to participate will be consented into the study. All individuals will be told clearly that they can stop their participation at any time, and for the treatment group, we will ensure participants are aware that their participation in TAYportunity plus Guaranteed Income program is not conditional on participating in the survey and interview study.

4 Econometric Specifications

4.1 Enrolment Balance

Once assignment to the treatment and control groups has been completed, in order to assess enrollment balance, we will regress the outcome variables from the recruitment application on the treatment indicators. We will run the following regression for the following variables measured at recruitment: income; educational attain ment; an indicator for the respondent being Hispanic; an indicator for the respondent being Black or African American; age; and sex:

$$y_i = \beta_0 + \beta_1 T_i + \varepsilon_i$$

where i is the individual and T_i is a treatment dummy. Finally, we test for joint significance across all outcomes of interest using seemingly unrelated regression (SUR).

4.2 Attrition

To limit attrition, participants will receive a \$20 payment for completing each survey. Additionally, on advice from our survey partner and LA DPSS, we will interact with study participants frequently to ensure they remain engaged. We will do this by reaching out on holidays, birthdays, and at random times with small gifts (pens, magnets, etc.) thanking them for their participation in the study. Additionally, we are administering surveys frequently (every six weeks) and making sure the surveys take no more than 10 minutes to complete. This will serve as another interaction with our study participants that will help them remain engaged. Our calculations assume that we will achieve a tracking rate of 80%. In order to determine how serious the attrition is in our study, we will use three approaches:

1. We will test whether attrition is correlated with treatment by regressing an indicator variable for whether a participant attrited on the treatment indicators.

2. We will test whether attriters differ from non-attriters by testing if attrition status can be predicted from baseline outcomes and stratification variables.

3. We will test whether baseline characteristics of attriters in the treatment group are different from those of attriters in the control group by restricting the sample to attriters and regressing baseline outcomes on treatment assignment.

If we find worrying levels of attrition, we will conduct robustness checks in which we use matching and bounding techniques to obtain corrected or bounded estimates.

4.3 Power Calculations

The required sample size was computed as follows. Desired power was 80%, at a significance level of 5%. We assume a 20% attrition rate and a correlation between multiple midlines of 0.5 (a higher correlation is conservative). Using these parameters, a control group of 650 and a treatment group of 300 households yield a minimum detectable effect size (MDE) for the treatment group relative to control of 0.17 standard deviations (SD). When we allow for multiple hypothesis testing, the MDE remains relatively low. Using Haushofer and Shaprio (2016) as reference, we obtain a FWER-adjusted target significance level of 3%. Repeating these power calculations with all other assumptions unchanged, we find an MDE of 0.19 for the treatment group relative to control.

4.4 Estimating Equation

The study is a straightforward randomized controlled trial with a single treatment. The main equation to assess treatment effects of transfers is:

$$y_{iSt} = \beta_0 + \beta_1 T_i + \gamma' X_i + \delta y_{iB} + \theta S_t + \varepsilon_{it}$$

Here, y_i is an outcome measured at baseline (B)[1] or at the survey round (S); T indicates a dummy for being

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[1] We do not have baseline measures for all outcomes, but this specification will apply to those outcomes for which we have a baseline measure.

in the treatment group; t takes on values 1:n depending on the survey round; S_t is a dummy variable which takes on a value of 1 for survey round t; and X is a vector of control variables, which includes baseline demographics.

4.5 Imperfect Compliance

The approach described above yields intent-to-treat estimates of the treatment effect. We do not expect imperfect compliance, given that the treatment seems as though it would be desirable to participants. If, unexpectedly, we do have imperfect compliance, we will modify our main equations in the following way: we will replace the indicators for treatment with variables measuring the presence and intensity of treatment, and these will be instrumented with treatment assignment. The equation will be estimated using two-stage least squares (2SLS).

4.6 Heterogenous Treatment Effects

We test for heterogeneous treatment effects in the following categories:

1. Race/ethnicity (Black, Hispanic, White)

2. Sex (indicator for female respondent/recipient)

3. Educational attainment (High school diploma/GED, some college, college degree)

The econometric specification for heterogeneous treatment effects is as follows:

$$y_{iSt} = \beta_0 + \beta_1 T_i + \beta_2 T_i \times H_i + \beta_3 H_i + \gamma' X_i + \delta y_{iB} + \theta S_t + \varepsilon_{it}$$

where H_i is an indicator variable for the dimension of heterogeneity. Again, y_i is an outcome measured at baseline (B)[2] or at the survey round (S); T indicates a dummy for being in the treatment group; t takes on values 1:n depending on the survey round; S_t is a dummy variable which takes on a value of 1 for survey round t; and X is a vector of control variables, which includes baseline demographics.

4.7 Multiple Comparisons

To adjust for multiple comparisons during analysis, we will follow the technique used in Haushofer and Shapiro (2016). We will define one index for each separate outcome. Then, we will use a false discovery rate across the different unrelated indexes as discussed in Anderson (2008).

5 Institutional Board Approval (IRB)

This study has been approved by the BRANY Institutional Review Board. The protocol number for this study is #22-107-796.

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[2]We do not have baseline measures for all outcomes, but this specification will apply to those outcomes for which we have a baseline measure.

6 Outcome Variables

Primary outcomes are marked with an asterisk (*) and secondary outcomes are marked with an obelus (†). The remainder of the outcomes listed are exploratory analysis.

6.1 Income and Employment

1. Participation in the labor market (measured using a dummy variable in which 1 denotes "participating in labor market" and 0 otherwise)[3]

2. Labor hours supplied in the past month*

3. Income earned in the labor market in the past month*

4. Satisfaction with employment situation (measured using a five-point Likert scale)

5. Whether or not respondent has been looking for work in the past month (measured using a dummy variable in which 1 denotes "looking for work" and 0 otherwise)

6. Whether or not respondent would like to be working more than the current situation (measured using a dummy variable in which 1 denotes "yes" and 0 denotes "no").

6.2 Expenditure and Consumption

1. Total expenditure (measured by calculating the sum of expenditure on items below)*

   (a) Rent
   (b) Food
   (c) Utilities
   (d) Other Bills and Debt Payments
   (e) Shopping Expenses
   (f) Leisure Activities
   (g) Education Expenses
   (h) Transportation

6.3 Education

1. Educational Attainment (measured by calculating the sum of degrees/certificates below)*

   (a) High school or GED
   (b) Some college (at least one semester)
   (c) Associates Degree
   (d) Certificate Program (Number of certificates in total acheived)
   (e) Bachelors Degree
   (f) Masters Degree

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[3]Participation is defined as formal employment, informal employment, self-employment, freelance work, and gig work.

6.4 Food Security

1. **Index:** Standardized index of (a) and (b)†

   (a) Whether or not anyone in household had to eat less than they felt they should in past 30 days (dummy variable in which 1 denotes "yes" and 0 denotes "no").

   (b) Whether or not household had to eat a lower quality diet because of cost in past 30 days (dummy variable in which 1 denotes "yes" and 0 denotes "no").

6.5 Time Use

1. Time spent asleep

2. Time spent studying

3. Time spent on unpaid labor

4. Time spent on leisure

5. Time spent on transportation

6.6 Housing

1. **Index:** Standardized index of (a) and (b)†

   (a) Likelihood of eviction/foreclosure (Four-point Likert scale)

   (b) Homeless status (dummy variable in which 1 denotes "yes" and 0 denotes "no")

2. Number of times individual has moved in the past 90 days

6.7 Financial Security

1. **Index:** Standardized index of (a) - (d)*

   (a) Whether or not the household can cope with a $400 emergency bill (dummy variable where 0 = "household must go into debt to handle the expense or cannot handle the expense" and 1 = "the household can pay for the expense with current resources")

   (b) Whether or not household could pay all bills in past 30 days (dummy variable)

   (c) Whether or not household could put money aside for future in past 30 days (dummy variable)

   (d) Whether or not household could pay down debt in past 30 days (dummy variable)

2. Total value of savings†

3. Total value of debt (measured by calculating the sum of components below)†

   (a) Mortgage debt

   (b) Vehicle loans

   (c) Medical debt

   (d) Legal bills

(e) Credit card debt

(f) Loans from friends and family

(g) Student debt

(h) Other debt

6.8 Psychological well-being

1. Pyschological well-being measured using Kessler-6 score*

References

[1] Anderson, Michael L. 2008. "Multiple Inference and sex Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." Journal of the American Statistical Association 103 (484): 1481–95. https://doi.org/10.1198/016214508000000841

[2] Haushofer, Johannes, and Jeremy Shapiro. 2016. "The Short-Term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya." The Quarterly Journal of Economics 131 (4): 1973–2042. https://doi.org/10.1093/qje/qjw025.