# Design for "The causal effects of sustained unconditional cash transfers: Experimental evidence from two U.S. states" – Supplemental Analysis Plan for Post-Transfer Outcomes

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**Abstract**

The regular provision of unconditional cash transfers to individuals is a tactic to fight poverty that has attracted significant interest from researchers and policymakers. Despite this interest, many fundamental questions about the effects of receiving sustained unconditional cash transfers remain. OpenResearch Lab, a nonprofit research organization, conducted the U.S.'s first modern, large-scale randomized trial of a guaranteed income to help answer these questions. In the experiment, 1,000 participants received $1,000 per month for 3 years. A control group of 2,000 individuals received $50 per month and served as the comparison group. The study offers an opportunity to inform both the debate over unconditional cash assistance and other questions about the effects of income that typically elude causal identification. This document supplements the original pre-analysis plans and describes how analysis will be conducted for outcomes measured in the medium-run after the end of the transfers.

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# 1 Introduction

In recent years, there has been renewed public interest in cash transfers as an approach for expanding or replacing the current social safety net, provide more flexible assistance to households, cushioning people from labor market changes, and allow families to invest in their children's future. Discussions of cash transfers relate to broader debates about income effects on employment, consumption, health, and other topics that are central to many areas of economics.

To help guide academic, policy, and political debates, we conducted an experiment to provide evidence about the effects of sustained unconditional cash transfers in the U.S. We collaborated with two non-profit organizations that implemented a cash assistance program. Our partners recruited 3,000 individuals across two U.S. states and randomly assigned 1,000 in total to receive $1,000 per month for 3 years. We conducted extensive quantitative measurement of outcomes related to individuals' economic, social, and physiological self-sufficiency and well-being, as well as gathered data on how individuals use their time and money and how their receipt of monthly cash transfers impacted their children and those in their households. We partnered with state and local government agencies and private entities to measure many outcomes with administrative data.

In the original pre-analysis plan, pre-registered as AEARCTR-0006750, we focused on the analysis of results pooled across the period in which individuals were receiving transfers. This

plan explicitly noted that we hoped to later analyze follow-up data from after the end of the transfer period (”post-transfer data”), but did not specify the full details how we would do so. In part, this reflected uncertainty over what survey data would be available, given funding constraints, and what response rates would be obtainable. Before this pre-analysis plan was posted, we reiterated our intent to analyze post-transfer data in several successful grant applications which funded the post-transfer quarterly surveys, from J-PAL, NIH, NSF and SSHRC. This plan details how we will do so.

The post-transfer effects of the transfer are important both for understanding the economic interpretation and policy implications of the study. Whether the effects of the transfer rise, fade, or remain the same after the transfer ends will provide insight into the mechanisms driving the effect of the transfers and on whether temporary transfers have persistent effects on outcomes.

In this document, we present a supplemental pre-analysis plan that specifies our approach for conducting statistical inference on post-transfer outcomes.

# 2 Estimation and Inference

We will exactly follow the estimation procedure described in our original pre-analysis plan. Specifically, to estimate treatment effects, we will compare outcomes for individuals who were assigned to the treatment group to individuals who were assigned to the “program control” group. When combining items into component indices or component indices into family indices, we will jointly estimate models for different outcomes using seemingly unrelated regression and then test the joint hypothesis that the weighted sum of the treatment effects are zero using robust standard errors. The weights will be determined as described below in the “combining data from multiple sources” section.

Below, we summarize this approach in more detail. Much of this material is repeated for clarity from the original analysis plan. We provide detail when necessary about how the original approach

will be implemented in the post transfer time period. When not specified, it should be assumed that we follow the approach in the original pre-analysis plan.

### 2.0.1 Regression Adjustment to Increase Precision

We will compute regression-adjusted treatment effects using the procedures outlined in Bloniarz et al. (2016), using the LASSO to select baseline covariates to use for regression adjustment, then including the selected covariates in an OLS regression with the treatment indicator present. These OLS regressions with clustered standard errors will represent our main estimates.

## 2.1 Combining Data from Multiple Sources During Post-Transfer Period

Unless otherwise specified, we will construct pooled measures of post transfer outcomes as follows. First, we will construct measures of the average outcomes in each of the the first roughly two years after the end of the transfer (November 2023 through December 2024 and January 2025 through March 2026). This will be the simple average of all available measures within each of these periods. We will then aggregate these measures to create an overall post transfer outcome by taking the simple average of these two post transfer periods. We will do this for both survey and administrative data measures.

We will also present disaggregated results by individual administrative data periods, survey waves, and year to characterize time dynamics, but these analyses will have a lower hypothesis-testing tier as specified in Table 1.

## 2.2 Adjusting for Multiple Comparisons

We will adjust for multiple comparisons following our originally pre-registered approach. Specifically, we will organize our outcomes at four levels:

1. **Topic**. E.g., financial health, employment and labor supply related outcomes, health, time

use, geographic mobility and housing outcomes, child outcomes, material hardship, intra-household, and psychosocial outcomes.

2. **Family**. This is the level at which we will conduct the multiple comparison adjustment. Therefore, within each topicwe will make false discovery rate (FDR) adjustments within each family of outcomes in the topic. E.g., within the financial health topic, financial behavior, credit access, and net worth are all families of outcomes (among others) within the topic.

3. **Outcome**. Each family will have multiple outcomes. E.g., within the financial behavior family, there are measures of credit utilization, use of alternative credit products, delinquencies, bankruptcies, and financial habits.

4. **Outcome Measures**. An outcome may be composed of multiple measurements. E.g., an financial habits index might be composed of three different survey items about different financial behaviors.

We categorize all outcomes into outcomes, families, and topics *ex ante* following the classification in the original pre-analysis plan.

We will use Benjamini and Hochberg (1995)'s False Discovery Rate (FDR) adjustment to compute $q$ values that control the rate of false positives within the family of tests to be no more than the nominal level. We use a tiered approach as in Guess et al. (2023) where we group tests into tiers of hypotheses and the FDR is controlled within tests of the same or lower tiers. We will report per comparison $p$-values in addition.

We will treat ordinal outcomes as continuous by default. We will further categorize outcomes (*i.e.*, items) into primary, secondary, and tertiary outcomes. Only primary outcomes will contribute to the component and family level index estimates.

The original pre-analysis plan did not specify how inference would be done for post-transfer outcomes. We will apply the same multiple-testing approach for post-transfer outcomes that we

used for analyzing outcomes during the transfer, treating the post-transfer outcomes for each topic as a separate set of analyses. We separate the multiple-testing adjustments for the during-transfer and post-transfer outcomes for two reasons. First, measuring the effect of the transfers on post-transfer outcomes answers a distinct substantive question from their effect on outcomes while transfers were being received. Specifically, it answers whether treatment effects fade, persist, increase, or newly emerge after the end of the transfer. Second, given that we did not pre-register the analysis of post-transfer data before the start of the experiment, we do not want the inclusion of post-transfer outcomes to alter our inferential approach regarding treatment effects during the transfer.

For each topic, we will separately implement FDR corrections for the estimated effects on post-transfer outcomes pooled between the end of the transfer and the first quarter of 2026. As we discuss more below, this reflects the time period over which we conducted post-transfer surveys. We will follow the original structure of the tiers of hypothesis testing, with family level estimates being the highest tier, primary outcomes being the second tier, by specific time period estimates being a tertiary tier, etc. The FDR tiers, which match those used in the original pre-analysis plan, are summarized in the table below.

# 3 Outcomes

We will follow the original pre-analysis plan when constructing outcomes during the post-treatment period when possible. However, the post transfer surveys were only conducted online. Furthermore, neither treatment or control were receiving monthly transfers anymore in the post-transfer period. Consequently, to maintain high response rates a more limited set of survey questions was included in the post-transfer period. This means that some items, components, or families of outcomes do not have measures in the post-transfer period. In cases where some outcomes are missing, we will use the non-missing measures within a family or component to construction

**Table 1:** FDR Tiers

| | Pooled Across Post-Transfer Period | Estimates At Each Time Period |
|---|---|---|
| Family | K0 | K3 |
| Primary Components | K1 | K3 |
| Primary Items | K2 | K3 |
| Secondary Items | K3 | K3 |
| Tertiary Items | K3 | K3 |
| Heterogeneous treatment effects | K3 | Not calculated |
| Any post-PAP tests | K4 | K4 |

family or component level outcomes.

For instance, in the example we gave above of the financial behavior family, credit utilization, delinquencies and bankruptcies are measured in the credit records data, and usage of alternative credit products is measured using a survey question that is asked both during the period when the transfer is being received and in the post-transfer period. However, financial habits are only asked about during the post-transfer period. Consequently, we will construct the index based only on the available items in the post-transfer period. In cases such as this, for comparison we will also compute a version of the treatment effect estimate during the transfer using just the subset of measures that we observe inthe post-transfer period.

We will also perform a set of regressions limiting attention to administrative data outcomes, from which individuals cannot attrit. These estimates will be preferred, where they exist, over the survey-based measures. However, only a restricted subset of items, components, and families will have outcomes in administrative data.

# 4 Analysis as of the Posting of this Supplementary-Analysis Plan

This supplementary analysis plan detailing our approach to analyzing the post transfer data is being posted after the end of the transfers and after some analysis on post-transfer analysis has been done, but before analysis of all post-transfer outcomes has been conducted and before some administrative data are available. Furthermore, we are as much as possible following the measured outcomes and analysis approach that we pre-registered for the period during the transfer, which means most of the analytical decisions remain pre-determined.

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