Unconditional Cash Transfers: A Bayesian Meta-Analysis of Randomized Evaluations in Low and Middle Income Countries

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Abstract

We use Bayesian meta-analysis methods to estimate the impact of unconditional cash transfers (UCTs). Aggregating evidence from 115 studies of 72 UCT programs in middle and low income countries, we find strong and positive average treatment effects on ten of thirteen outcomes: monthly household total and food consumption, monthly income, labor supply, school enrollment, food security, psychological well-being, total assets, financial assets, and children height-for-age. We examine seven specific theoretical and policy hypotheses, such as presence of savings frictions, dynamic effects, curvature of marginal returns, targeting effects, "nudge" effects, labor supply elasticity and related "dependency" theories, and contextual heterogeneity.

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1 Introduction

Unconditional cash transfers (UCTs) have become a common and heavily studied policy tool. At least 72 UCT programs have been evaluated using a randomized controlled trial ("RCT"), ranging widely in scale and purpose, from large government programs to small non-governmental efforts, from humanitarian aid to economic development. The breadth of this empirical evidence now permits us to establish a basic understanding of the average expected treatment effects from cash transfers across a variety of important outcomes, potentially serving as a benchmark for development policy. The plethora of studies and design variations facilitate investigations of several commonly posed theoretical and policy questions of critical importance, such as the presence of threshold-based poverty traps, the elasticity of labor supply, the differential impact from targeting women within households and from adding framing (i.e. "nudges") to the transfers.

Our meta-analysis includes 115 studies reporting results from 72 randomized evaluations ("programs") of UCTs in 34 low and middle income countries over both short and long time horizons (mostly between 12 and 48 months).[1] We examine impacts on 13 primary outcomes and several secondary outcomes (typically components of a primary outcome). We also explore heterogeneity with respect to the following sources of variation: transfer size (to test for increasing or decreasing marginal returns to grant size), frequency of transfer (lump-sum transfers versus ongoing streams versus completed streams), measurement timing (i.e., amplification or dissipation of effects over time), target population (female-targeted versus male-targeted versus non-targeted), and framings that suggest a child or food security focus to households.

We use a Bayesian hierarchical model to jointly estimate average treatment effects of UCT programs. We find strong, positive impacts on ten of thirteen primary outcomes: Monthly household consumption, monthly household food consumption, monthly income, labor force participation (binary), school enrollment (binary), z-scores for food security

[1] Appendix Tables A.1a-b describe the key design features of the 72 programs in our sample. We consider a single paper that reports on two RCTs from two countries as two studies; in total the 115 studies derive from 112 papers.

and for psychological well-being, the stock of total assets, the stock of financial assets, and height-for-age z-scores. Results for hours worked, weight-for-age z-scores, and stunting (binary) are positive but not statistically significant at 95% credibility.

We examine seven main hypotheses. First, we find support for an oft-hypothesized pattern that people consume more of streams and invest more of lump-sums. Perhaps surprising, however, completed stream programs generate results much closer to lump sum transfers than to ongoing streams, suggesting that households are able, and choose to, save or borrow sufficiently to roughly equilibrate the two types of transfer (once the stream transfers are no longer incoming).

Second, we compare longer-run to shorter-run results. Lump sum and completed streams produce impacts that after two years modestly dissipate for consumption but remain constant for assets; ongoing stream, on the other hand, generates increasing treatment effects over time for consumption, consistent with households consuming some and investing some of the monthly stream transfers. Few papers however report long-run outcomes past 48 months.

Third, we examine whether impacts are linear (versus concave or convex) with respect to transfer size. Asset threshold-based poverty traps are a central idea of development economics and an important motivation for the use of unconditional (and large enough) cash transfers to deliver development aid. Fixed costs or increasing returns may imply an asset threshold below which investments are not worthwhile and, in the presence of binding barriers to saving and borrowing, poverty may beget poverty. In theory, a large enough temporary cash transfer could break such a cycle, but our estimates are fairly close to linear with respect to grant size. Absence of evidence, however, is not evidence of absence. This test does not rule out asset-based poverty traps as thresholds as they may be heterogeneous across sites, households, or beyond the range of transfer sizes tested. Our analysis is at the study (rather than household) level and thus particularly vulnerable to obscuring the existence of heterogeneous poverty trap thresholds, rendering this a weak test of such theories.

Fourth, we examine how results differ for programs that target women: targeted transfers lead to higher observed consumption and higher income (versus untargeted programs), but no difference in assets. On child-related outcomes, we find inconsistent results, with results stronger for weight-for-age of children but worse on height-for-age.

Fifth, we find that programs that include some form of a "nudge" (Thaler and Sunstein 2009) with respect to the transfer being intended to benefit children do lead to stronger impacts on total consumption, food consumption, food security, and psychological well-being but no difference for the more obvious outcomes of child anthropometrics and school enrollment.

Sixth, on labor supply, a key outcome of policy interest, unconditional cash transfers generate a strong positive effect on the extensive margin and a noisier but positive point estimate on the intensive margin (i.e., hours worked). Considering the strong positive effects on income, this implies that unconditional cash transfers do not "demotivate" recipients. This result is consistent with previous meta-analysis (Banerjee, Hanna, et al. 2017) and with poverty-trap models of labor supply in which poor households supply less labor because they need resources to find and maintain labor or to make investments for self-employment. The positive impact on labor supply is also consistent with imperfect labor markets and an increased demand for labor in the household due to downstream investments facilitated by the transfers received.[2]

Seventh, we examine two types of contextual heterogeneity: rural versus non-rural and income level of the country (specifically, the poverty rate and per capita Gross Domestic Product, "GDP"). We find strikingly similar treatment effects regardless of rural status and underlying economic prosperity of the country.

Table 1a and 1b situate our study in the context of the extant meta-analytical literature on the impacts of cash transfer programs on particular outcome classes. We add to this meta-analysis literature along four dimensions.

[2]Increased spending on temptation goods is another oft-hypothesized deleterious effect of cash transfers. We do not analyze these anew, as a recent meta-analysis reports of 42 studies finds mostly nulls or even negative point estimates, indicating that similar to labor supply the fears of increased spending on temptation goods are unsupported by the evidence (Evans and Popova 2017).

First, we explicitly account for transfer size in estimating treatment effects instead of coding transfer receipt as a binary. This is consistent with Kondylis and Loeser (2021), the closest meta-analysis to ours in method and questions. Aggregating treatment effects from "any cash transfer" as a binary rather than per dollar of the transfer renders the aggregate point estimate uninterpretable on its own. One would need to multiply the point estimate for "any cash transfer" by the average grant amount across studies to be interpretable (and also would need to assume that the marginal treatment effects are constant with respect to grant size).

Second, we analyze a wide range of social and economic outcomes, while most existing meta-analyses focus on a particular outcome class (e.g., education, mental health, child health etc). These other studies are accompanied by more nuanced and theoretically deep discussions of the link between cash transfers and a particular set of outcomes, while ours is a more comparative perspective. On this dimension, the closest study to ours is Kabeer and Waddington (2015) which spans consumption, investment, and labor.

Third, we investigate the temporal evolution of impacts using a binary model that compares short-term and long-term impacts as well as a polynomial model that adds a covariate for months since the intervention and its squared term. This analysis complements three other analyses, Wollburg et al. (2023), McGuire et al. (2022), and Kondylis and Loeser (2021), that quantify effect dissipation in different ways. Closest to this paper's binary dynamic effects model, Wollburg et al. (2023) compares short-run to more long-run estimates of mostly UCT RCTs on mental health outcomes to show that small but statistically significant short-run effects on depression dissipate substantially in the longer run. McGuire et al. (2022), using a more diverse sample including both RCTs and non-randomized designs as well as CCTs and UCTs, finds little dissipation of the small effects they estimate on depression. Employing a model that uses a continuous time variable similar to our dynamic effects polynomial model, Kondylis and Loeser (2021) studies treatment effect persistence specifically with respect to transfer size and finds that the impact of larger transfers dissipates at higher rates. Our study does not detect evidence of dissipation of effects on household consumption and instead finds some evidence that effects compound over time for ongoing transfer streams.

Fourth, we extend the extant work with additional tests of heterogeneity, specifically targeting by gender, adding "nudge"-like components to promote using proceeds for children, rural versus non-rural, and overall economic conditions in the country.

2 Data

2.1 Study inclusion

Our meta-analysis focuses on RCTs of UCT programs in low and middle income countries. Following the approach by Croke et al. (2016) and Kondylis and Loeser (2021), we identify studies using two approaches. First, we gather studies from secondary sources: the GiveDirectly Cash Evidence Explorer, the Overseas Development Institute's 2016 report "Cash transfers: what does the evidence say?" (*Cash Evidence Explorer* 2023; Bastagli et al. 2016), and existing meta-analyses on cash transfers with publicly available data. Second, we conduct a search of databases and registers of scholarly research using key words.[3] As displayed in Figure 1, our combined search yields a universe of 6,949 studies, of which 115 meet the inclusion criteria of our meta-analysis.

We employ the following inclusion criteria:

1. The study is an RCT in which the control group received no or minimal cash.

2. At least one of the study's treatment arms is an UCT.

    (a) This may include UCT programs with some minimal behavioral change components to the treatment, such as an onsite information session or labelled cash transfers. It excludes conditional cash transfers (CCTs), which require ongoing

[3] See Figure 1 for a complete description of our systematic search and Appendix Table A.2 for a hyper-linked list of the 115 included studies from the 72 programs.

behavioral compliance with certain conditions to continue receiving the cash transfer (most commonly school attendance).[4]

(b) This includes non-contributory pension programs.

(c) This excludes RCTs with cash transfers that are delivered in conjunction with other costly and non-trivial interventions, such as training, savings group formation, coaching, etc.

3. The study's experiment takes place in a low or middle income country (as defined by World Bank classification).

4. The study reports results on any outcomes related to consumption, food security, income, savings and investment, business performance, labor supply, child health and development, education, psychological well-being.

2.2 Data extraction

We collect the following information each included study:

Transfer frequency: Lump sum and stream transfers: As an important example of program design, we distinguish between stream and lump sum transfer programs. In general terms, a lump sum transfer delivers a one-off payment, while a stream transfer delivers repeated cash payments at regular intervals over an extended period of time. We define an intervention as a lump sum program if the cash is delivered in no more than three installments over no more than two months (28 out of 34 included lump sum transfers with exactly one transfer). All other transfer schedules, ranging from five weekly transfers to six quarterly transfers, are considered stream transfer programs.

Gender targeting: We construct a categorical variable that identifies whether programs

[4] Two programs in our sample, Bono de Desarrollo Humano (BDH) in Ecuador and Programa de Apoyo Alimentario (PAL) in Mexico, were nominally conditional cash transfers. In practice, PAL's conditions were not enforced, and participants mostly did not adhere to them (Avitabile et al. 2019). The BDH's conditions were never implemented due to administrative constraints (Hidrobo and Fernald 2013).

target UCTs to men, women, or neither. For programs that give cash to households, we only consider a program to target females (males) if it ensures the cash transfer is delivered to a woman (man) in the household.[5] We do not define a program as targeting females (males) if it allows households to choose who receives the transfer, even if recipients are largely women (men). For programs that give cash to individuals, we say a program targets females (males) if greater than 80% of the individuals in the sample are women (men). Of the 72 programs in our sample, 32 target women, 6 target men, 28 have no targeting, and 6 randomize targeting to men or women.

Child and food security framing: By definition, UCT programs neither place conditions on how recipients spend the transfer nor require certain behavior as a condition for receiving the transfer. Nonetheless, certain programs in our sample use framing devices to encourage the cash transfer to be directed towards particular ends. These devices vary from a simple labeling of the UCT (e.g., "Child Grant Program," "Hunger Safety Net Program," etc.) to free (voluntary) information sessions on related topics such as education or child nutrition. We construct a binary indicator variable that identifies programs using framing related to food security or child development, including maternal health, child nutrition, and education.[6]

Total transfer amount and monthly tranche amount: We employ two measures for the size of the transfer, the total amount transferred and the monthly tranche amount. The definition of the total transfer amount is straightforward: the sum of the value of all transfers made to program beneficiaries by the time of the endline survey, as in Kondylis and Loeser (2021) (if individuals varied, we report the average each recipient received in total).

The second measure, the monthly tranche amount, is equal to the total transfer amount divided by the number of months since the first transfer. For ongoing stream transfers,

[5]There are no programs in the sample that target males in this manner.

[6]See Appendix Table A.3 for a complete description of targeting and framing across all programs in the sample, including framing related to goals other than improving child welfare or food security.

this measure is equivalent to the monthly transfer amount (if ongoing stream transfers are not monthly, we convert the amount to the average monthly transfer amount). For completed stream transfers and lump sum transfers, we take the sum of all transfers made and divide by the number of months since the first transfer; this thus facilitates comparing to ongoing stream by using a monthly tranche amount that corresponds to what would have been transferred had the same total been spread over the full time period from first transfer to measurement (i.e., just like the ongoing stream programs). All transfer amounts are then converted to 2010 USD PPP.

We do not include estimates for stock outcomes (e.g., assets, anthropometrics) when using the monthly tranche amount, because this would be confounding the tenure of the program with the monthly transfer amount, rendering results difficult to interpret. Similarly, for lump sum transfers, while we do estimate the impact using the monthly tranche amounts, we consider the total transfer amount to generate the more interpretable estimate.

Treatment effects: We extract treatment effects directly from the papers' results tables rather than using the studies' underlying data. This approach means that we cannot ensure that our estimates come from identical regression specifications. It has the advantage, however, of being faster to produce and allows inclusion of both older publications from before norms of data publication were more widespread and newer papers (e.g., working papers) for which data are not yet available.

While we cannot guarantee regressions specifications are perfectly consistent across studies, we prefer estimates from regressions that disaggregate by survey round and treatment arm and that contain fewer control variables.[7] Outcomes are converted to 2010 USD PPP. Flow variables, such as consumption and income, are converted to common periods of time (i.e. per month or per week). Psychological well-being and food security outcomes are standardized, if necessary, by dividing by the control group standard deviation.[8] Once

[7]See *Regression specification* in the Appendix for a complete description of our preferred specifications.

[8]See *Data Harmonization* in the Appendix for a complete description of how each outcome variable is converted to common units. Appendix Tables B.1 and B.2 also present the treatment effects on food

converted to appropriate units, we divide all treatment effects by the total transfer amount or monthly tranche amount to construct the outcome variables standardized relative to the transfer amount, thus allowing results to be interpreted as the treatment effect per dollar transferred. We typically scale treatment effects by $100 or the median transfer amount of the programs in our sample.

Months since program onset: Short-term and long-term effects: We extract the average number of months between the first transfer (not the baseline survey) and the endline survey. Figure 2 visualizes the temporal distribution of our data for each of the outcomes[9]. If a study does not report time since first transfer, we infer timing from the program's scheduled timeline. We consider a treatment effect measured at an endline up to 18 months after program onset to be a short-term effect. All treatment effects measured more than 18 months after program onset are considered long-term effects. Note a program may administer one follow-up survey one year after program onset and another follow-up two years after program onset. Results from the first follow-up are considered short-term and the second are long-term.

Months since program completion: Ongoing and completed programs: We also extract the average number of months since last transfer, as for months since first transfer. We consider a UCT program ongoing if the number of months since last transfer is equal to zero or if transfers are still being administered to participants at the time of survey. If the number of months since last transfer is greater than zero and the final transfer of the program has been delivered, we consider a program completed. Note, all lump sum programs are completed programs. Several of the UCT programs in our sample are large government-run social protection programs that administer stream transfers indefinitely. While participants may flow in and out of the program over time due to changing eligibility status, we generally do not have information on the proportion of RCT participants still

security and psychological well-being outcomes before and after standardization.

[9] Appendix Table D presents the distribution of months since first and last transfer, broken down by disbursement schedule type

receiving transfers at endline. We thus consider these programs ongoing. Combining completion status (ongoing vs. completed) with transfer frequency (stream vs. lump sum), our subsequent analysis considers three disbursement schedules: ongoing stream programs, completed stream programs, and lump sum transfer programs.

3 Methodology

A crucial methodological challenge in any meta-analysis based on RCTs is how to best aggregate information from multiple studies to estimate a measure of the general effect of the treatment with credible external validity. An individual RCT can provide a consistent estimate of the average treatment effect of cash transfers on a given outcome in a particular population during a specific time period and context. But how much of the estimate is due to idiosyncratic elements of the context (e.g., political instabilities, natural catastrophes, implementation fidelity, etc.) and how much due to statistical regularities with generalizable external validity (e.g., consumption increases from cash transfers are stronger in lower income samples)? In the following, we lay out key characteristics of our model and estimation method, and discuss the assumptions we make with respect to the generative process of the data and our statistical framework.

3.1 Hierarchical Linear Models for Meta-Analysis

Assume a researcher has gathered N estimates $\widehat{TE}$ of average treatment effects (ATEs) from comparable RCTs with corresponding standard errors $\widehat{se}$ and a set of RCT-level covariates X (e.g. whether the transfer schedule is a stream or a lump sum). For example, if $T_{i,n}$ is an indicator of individual i's treantment status in RCT n and $y_{i,n}$ is the outcome of interest for that individual, $\widehat{TE}_n$ is the estimate of θ_n from the regression $y_{i,n} = \alpha_n + T_{i,n}\theta_n + u_{i,n}$, and $\widehat{TE}_n$ has an estimated standard error of $\widehat{se}_n$. We are not assuming that the researcher has access to the underlying micro data from which these estimates of ATEs are created. Access to such data would permit a richer analysis of heterogeneity in effects across subgroups.

Given the design of randomized interventions, studies in our surveyed literature commonly assume that estimate $\widehat{TE}_n$ is a consistent estimator of θ_n, and that it is asymptotically normally distributed with variance equal to its empirical estimate $\widehat{se}_n^2$. We further assume that the treatment effect estimates are drawn from distinct and conditionally independent distributions, i.e.:

$$\widehat{TE} \mid \theta, \widehat{se} \sim \mathcal{MN}\left(\begin{bmatrix}\theta_1 \\ \vdots \\ \theta_N\end{bmatrix}, \begin{bmatrix}\widehat{se}_1^2 & \cdots & 0 \\ \vdots & \ddots & \vdots \\ 0 & \cdots & \widehat{se}_N^2\end{bmatrix}\right)$$

The researcher is not only interested in understanding the common evidence of a statistically significant effect across RCTs, but also in identifying if certain features (X) of the interventions or their settings systematically correlate with higher or lower effects. The treatment effect estimate drawn from study n, $\widehat{TE}_n$ is a consistent estimate of θ_n. In order to aggregate information across studies, we assume that these parameters (θ) are drawn from a joint distribution that depends on K RCT-specific covariates X (so X is $N \times K$). For each intervention n we have

$$\theta_n = X_n\beta + \epsilon_n \quad \forall n \in \{1, ..., N\}$$

The threats from selection bias in meta-analyses are apparent: publication bias and related selection may imply that estimates $\widehat{TE}$ are not consistent estimates of θ. And unobserved characteristics of an intervention or its context incorporated into ϵ, may be correlated with the observed X. These observations are generic to meta-analysis, mitigated here by our work to gather as complete a set of evaluations of UCTs as possible (both published and working papers, e.g.). We begin by assuming that the distribution of ϵ is iid normal with mean zero and variance σ_θ^2, but examine alternatives in subsection 3.3. Therefore,

$$\theta \mid \beta, \sigma_\theta \sim \mathcal{MN}\left(X\beta,\ \sigma_\theta^2 I_N\right)$$

The model is a generalization of the classical, simple random effects Rubin (1981a) model, in line with a growing literature that uses more complex formulations to uncover dynamic effects of treatment or subgroup heterogeneity (e.g. Kondylis and Loeser (2021), Alley (2022), Bandiera et al. (2021)). Here, θ is not centered around a common mean but instead around an expectation depending on an RCT-specific set of covariates with constant additive and linear effects. This allows us to aggregate information across studies, while also estimating parameters that characterize the underlying heterogeneity across RCTs.

We choose a random effects model specifically to avoid the much stronger assumption of no true heterogeneity inherent in fixed effects models. Fixed effects models assume that each estimate is an independent draw from a common distribution such that variation in estimates results exclusively by sampling variation (Rubin 1981a). Study-level effects are modeled as measurements of a common effect plus some sampling error, either using the underlying data or an estimator of the treatment effect of choice (Borenstein et al. 2010). Examples of fixed effects models include taking the average of the estimates weighted by the inverse of their estimated variance (e.g. Kondylis and Loeser (2021)) or running a pooled regression using all the underlying RCT-level data and controlling for study fixed effects (e.g. Banerjee, Duflo, et al. (2015)).

On the other hand, random effects models in the tradition of Rubin (1981a) allow for non-sampling based heterogeneity in treatment effects across RCTs by introducing a hierarchical structure. Single estimates are assumed to be sampled realizations from distinct distributions (i.e. the first hierarchical layer) whose central parameters come from a common distribution (i.e. the second hierarchical layer). This permits us to both control for the sampling variability of the estimates and identify their idiosyncratic heterogeneity. In line with previous work (e.g. Raudenbush and Bryk (1985), Vivalt (2020)), we assume a hierarchical additive model, allowing the heterogeneity across RCT-estimates to vary across a set of study-level covariates and thus making less stringent assumptions, while potentially uncovering what features of the interventions correlate with higher average treatment effects (Meager (2019) and Meager (2022)).

3.2 Bayesian Estimation

The next challenge is estimating our data generating model, by choosing a suitable statistical approach. The Bayesian approach naturally fits such a data structure and can be flexibly implemented by relying on the assumption of exchangeability (a strictly weaker assumption than independence). Under this assumption, the data are independent conditional on a set of parameters (De Finetti 1972). In our model we assume conditional exchangeability, as we characterize the second layer distribution to depend on a set of covariates (X) and parameters (β). This assumption means that, conditional on the RCT features that we consider, observations can be permuted across contexts, without affecting their joint probability distribution.

As previously outlined, Bayesian additive hierarchical models have been widely adopted in the meta-analytical literature in economics (Burke et al. 2015, Meager 2019, Vivalt 2020, Bandiera et al. 2021, Alexander et al. 2021, Meager 2022, Angrist and Meager 2023) and in other disciplines (e.g., Chu et al. 2009, Heeg et al. 2023, Liu et al. 2017).

As Raudenbush and Bryk (1985) notice, this approach is formally of an Empirical Bayes nature since we use the data (i.e. $\widehat{se}$) to inform the likelihood distribution. This combines advantages from both the Frequentist and the Bayesian frameworks. On one hand, Frequentist asymptotic distributional results guarantee that each estimate of an average treatment effect is asymptotically Gaussian. This renders the choice of the likelihood less restrictive (Gelman, Carlin, et al. 1995, Angrist and Meager 2023) since it hinges on the same assumptions that render legitimate the Frequentist inference of the original papers.

Frequentist estimation techniques such as maximum likelihood (MLE), on the other hand, condition on the modal point estimate of the higher layers' parameters and thus do not take into account their posterior uncertainty, on the other hand Bayesian techniques sample the parameters from their own estimated posterior distribution, thus taking into consideration a wider range of possible values. (Gelman, Carlin, et al. 1995, Chapter 5). Moreover, priors can help improve the stability of estimates by providing what is known in the Frequentist framework as regularization (Gelman, Simpson, et al. 2017, Hastie et al. 2001). Regularization, a Frequentist technique, can help reduce the variance of estimates and focus the estimation on regions of the parameter space that are relevant (e.g. away from treatment effects of exaggerated magnitude), at the cost of introducing some bias. This can render estimates more precise than with MLE or inappropriately flat priors (Gelman, Simpson, et al. 2017). Indeed, Stegmueller (2013) finds that, in simulation studies of additive hierarchical models, MLE tends to have both more severe finite sample bias and/or lower confidence interval coverage, the latter being exacerbated when the number of hierarchical groups (that is, in the meta-analytical context, the sample size itself) is smaller.

We complete the model, therefore, by specifying a prior distribution for β centered on zero, unless the data suggest otherwise, but fairly dispersed. This procedure is similar to Frequentist penalization methods such as Ridge, LASSO or Elastic Net (Hastie et al.

2001).

$$\beta_k \sim \mathcal{N}(0, 16) \quad \forall k \in \{1, ..., K\} \tag{1}$$

In line with the recommendation from Gelman (2006), we choose to use a Half-Cauchy prior for θ's scaling parameter:

$$\sigma_\theta \sim \mathcal{H}alf - \mathcal{C}auchy(0, 16). \tag{2}$$

The numerical estimation of the model is conducted using Stan (Stan 2022), a software for Bayesian simulations, that uses a Hamiltonian Monte Carlo procedure (Betancourt 2020) to explore posterior density distributions using gradients. This approach allows for flexible definitions of priors and to estimate even relatively complex models.

3.3 Model Specifications

Throughout our analysis, we estimate increasingly richer and more general versions of $\theta \sim \mathcal{N}(X\beta,\ \sigma_\theta^2 I_N)$ by expanding the set of covariates in X.

We start from the original Rubin (1981a) random effects model[10]:

$$\theta \mid \beta, \sigma_\theta \sim \mathcal{MN}\left(\beta\mathbf{1},\ \sigma_\theta^2 I_N\right) \tag{3}$$

Building on Equation (3), our second model allows for heterogeneity with respect to the type of the transfer and the time of measurement of the effect. The type is defined by the disbursement schedule of the RCT, i.e. whether the transfer was delivered as a lump sum (L) or a stream (S); the timing of measurement, which is relevant only for stream transfers, is whether the programs were completed (CS for "completed stream") or ongoing (OS for "ongoing stream") at the time of measurement:

$$\theta \mid \beta, \sigma_\theta \sim \mathcal{MN}\left(\beta_1 L + \beta_2 CS + \beta_3 OS,\ \sigma_\theta^2 I_N\right) \tag{4}$$

In the subsequent version of our model, we build further on Equation (4) adding covariates for the number of months since first or last cash transfer (M) and the squared value of this term to estimate the temporal dynamics of treatment effects. We allow for heterogeneity in dynamic effects between ongoing streams and completed programs (i.e., both completed streams and lump sum transfers). Note that the interpretation of the two trends differs: for completed interventions (C), we estimate a dissipation effect after payments end ($M \odot C + M^2 \odot C$). For ongoing streams, we estimate a multiplicative effect ($M \odot OS + M^2 \odot OS$), such as when an individual saves or invests part of the tranche and

[10] Some outcomes have different categories, e.g. assets are sometimes measured as total assets, financial assets, productive assets and durable assets. In model (3), we run the specification on a sample of observations of **only** the specific category considered (e.g. only total assets), thus possibly not pooling useful information across categories. In an alternative specification, we **jointly** estimate coefficients for the different categories, by running an augmented model on a sample of observations from **all** categories (e.g. a sample of total, financial, productive and durable assets). In general, assuming an outcome has K categories, we will run the following specification:

$$\theta \mid \beta, \sigma_\theta \sim \mathcal{MN}\left(\sum_{k=1}^{K} \beta_k \mathbb{I}_k,\ \sigma_\theta^2 I_N\right) \tag{3b}$$

so can collect interest, additional revenues, and can make further investments in assets:

$$\theta \mid \beta, \sigma_\theta \sim \mathcal{MN}(\beta_1 L + \beta_2 CS + \beta_3 OS + \beta_4 M \odot C + \beta_5 M^2 \odot C + \beta_6 M \odot OS + \beta_7 M^2 \odot OS, \sigma_\theta^2 I_N) \tag{5}$$

One drawback of Equation (5) is that it takes a considerable amount of observations to estimate a dynamic trend with precision and, even though our sample for total consumption is sizable for the standards of meta analyses, it might still lead to imprecise measurements. Therefore, as a further complementary estimation we specify a model where we discretize the dynamic dimension of our observations into two categories: short run measurements from up to 18 months from the first transfer and long run measurements after 18 months. The resulting specification of the model is the following, denoting short run by ST and long run by LT:

$$\theta \mid \beta, \sigma_\theta \sim \mathcal{MN}(\beta_1 ST \odot L + \beta_2 LT \odot L + \beta_3 ST \odot C + \beta_4 LT \odot C + \beta_5 ST \odot OS + \beta_6 LT \odot OS,\ \sigma_\theta^2 I_N) \tag{6}$$

The disadvantage of this model is that it loses some information in discretizing the dynamic dimension of our dataset, however it is able to detect average differences between short term and long term measurements of average treatment effects more robustly, since it does not rely on a specification of such underlying decaying or accumulation effects, which might have small sample noisy estimates.

We also want to test for non-constant marginal returns for transfer amount, taking into consideration the disbursement type. For ended interventions, we are interested in estimating the marginal effect of a higher total amount transferred, hence, starting from Equation (4), we augment the model with the total amount transferred in PPP \$ interacted with an indicator for the program being either a lump sum transfer or and ended stream ($TT \odot C$). On the other hand, for ongoing stream transfers, we are interested in estimating the effect of a marginal increase in the monthly tranche and so we run a different model by adding monthly tranche interacted with an indicator for ongoing stream transfer ($MT \odot OS$). The two specifications are the following:

$$\theta \mid \beta, \sigma_\theta \sim \mathcal{MN}\left(\beta_1 L + \beta_2 CS + \beta_3 OS + TT \odot C,\ \sigma_\theta^2 I_N\right) \tag{7}$$

$$\theta \mid \beta, \sigma_\theta \sim \mathcal{MN}\left(\beta_1 L + \beta_2 CS + \beta_3 OS + MT \odot OS,\ \sigma_\theta^2 I_N\right) \tag{8}$$

The last dimension of heterogeneity we choose to investigate is whether targeting the transfers by gender or labelling it as for children or food lead to differential effects. In order to do this, we go back to a simpler model: let T denote whether the transfer was targeted to women and F if it was framed for children, then the previous model becomes:

$$\begin{aligned} \theta \mid \beta, \sigma_\theta &\sim \mathcal{MN}\left(\beta_1 T + \beta_2 (1-T),\ \sigma_\theta^2 I_N\right) \\ \theta \mid \beta, \sigma_\theta &\sim \mathcal{MN}\left(\beta_1 F + \beta_2 (1-F),\ \sigma_\theta^2 I_N\right) \end{aligned} \tag{9}$$

Lastly, in the Benefit-Cost analysis section, we want to predict expected treatment effects for a specific transfer amount and duration, hence we use a model that includes both a dynamic trend and marginal effects of transfer size, i.e.:

$$\theta \mid \beta, \sigma_\theta \sim \mathcal{MN}(\beta_1 L + \beta_2 CS + \beta_3 OS + \beta_4 M \odot C + \beta_5 M^2 \odot C + \beta_6 M \odot OS + \beta_7 M^2 \odot OS + \beta_8 TT \odot L + \beta_9 TT \odot CS + \beta_{10} TT \odot OS, \sigma_\theta^2 I_N) \tag{10}$$

3.4 Pooling Factor and Explained Variance

In the context of our meta analysis, we also want to measure how much treatment effect estimates from different RCTs show a degree of pooling towards a common distribution, indicating that our results can be more safely generalised to other contexts. Moreover, we also want to check that our model explains a satisfactory amount of the variation in the treatment effect estimates, once cleaned of their sampling variability, i.e. how much θ is explained by the linear predictor $X\beta$. In order to do so, we employ two useful measures developed by Gelman and Pardoe (2004) in the context of hierarchical models: the pooling factor and the layer-specific R^2.

An important feature of hierarchical models is that estimates are partially pooled towards a common upper layer distribution, thus compromising between maintaining important idiosyncratic variability of individual treatment effect estimates and aggregating evidence across RCTs. The pooling factor is

$$\lambda := 1 - \frac{\mathbb{V}_{n=1}^N(\mathbb{E}(\varepsilon_n | X, \beta, \sigma_\theta))}{\mathbb{E}(\mathbb{V}_{n=1}^N(\varepsilon_n | X, \beta, \sigma_\theta))}$$

The numerator in the expression denotes the variation across the elements of θ of the average error term, while the denominator captures the average unexplained variation of the model with respect to the θ parameters.[11] Overall, the ratio will be greater whenever

[11] $\mathbb{V}_{n=1}^N(\varepsilon_n | X, \beta, \sigma_\theta))$ denotes the posterior variance across elements of ε, i.e. $\frac{1}{N-1}\sum_{n=1}^N \left(\varepsilon_n - \frac{1}{N}\sum_{n=1}^N \varepsilon_n\right)^2$

the model predicts certain elements of θ, on average, much better than others, taking into account the degree of average variability in the error terms themselves.

By construction, λ can take value between zero and one. Gelman and Pardoe (2004) suggests a value less than 0.5 suggests that within RCT information dominates population-level one, thus dictating for less pooling of the posterior of θ towards the common linear predictor $X\beta$. On the other hand, if λ is greater than 0.5, it suggests that there is evidence that the model detects more population-level information, thus suggesting a higher external validity of the posterior values of β for RCTs in different contexts (Meager 2019).

On the other hand, it is also important to check that the model is correctly predicting the variable of interest. In the context of our model, we want to be able to explain variability in θ, i.e. the measures of the RCT-level treatment effect cleaned from the sampling variability (i.e. $\widehat{TE}$). Hence, we define the R^2 for the θ hierarchical layer as:

$$R^2_\theta := 1 - \frac{\mathbb{E}(\mathbb{V}_{n=1}^N(\varepsilon_n|X,\beta,\sigma_\theta))}{\mathbb{E}(\mathbb{V}_{n=1}^N(\theta_n|X,\beta,\sigma_\theta))}$$

This measure closely resembles the standard R^2 employed in regression frameworks, where the ratio is higher if the average unexplained variation of θ (the numerator) is small compared to its average total variation.[12]

[12] As Gelman and Pardoe (2004) point out, the measure can technically take negative values, if the fit of the model is particularly bad and generates error terms with higher average variability than the explained variation.

3.5 Model Extensions

A crucial assumption of the model concerns the distribution of the aggregate RCT-level treatment effects θ. Throughout the literature, it is routinely assumed that such parameters are normally (and independently) distributed. Such assumption crucially assumes a symmetry in their realizations, with negative treatment effects being as likely as positive ones. This assumption, which might plausibly hold in certain contexts, is generally strong whenever the researcher can instead expect or inspect from the data a more skewed distribution.

In the context of the effects of UCTs, our data suggest that results are usually skewed, with a big mass concentrated around zero or generally small positive numbers and a second mass, of smaller size, concentrated around positive numbers of higher value. While it is true that there are also negative estimates, they are usually of small magnitude, rarer, and noisier.

Motivated by this evidence, we test two further model extensions:

- Skewed model:

$$\theta_n \mid X_n, \beta, \sigma_\theta \sim \mathcal{Gumbell}\big(X_n'\beta,\ \sigma_\theta^2\big) \tag{11}$$

- Normal Mixture model:

$$\theta_n \mid \lambda, X_n, \beta, \delta, \sigma_\theta \sim \lambda\mathcal{N}\big(X_n'\beta,\ \sigma_{1,\theta}^2\big)\ +\ (1-\lambda)\,\mathcal{N}\big(X_n'(\beta+\delta),\ \sigma_{2,\theta}^2\big) \tag{12}$$

We provide graphical posterior checks to illustrate how both point estimates and uncertainty quantification vary with the model assumptions.[13] In order to formally establish model performance by outcome, we use the approach developed by Vehtari, Gelman,

[13]See Appendix Figures 13.1 to 13.3

and Gabry (2016), which uses a Leave One Out-Cross Validation Criterion ("LOO-CV") to compute the point expected log-likelihood for each observation under different model assumptions to compare model performance.[14]

In order to quantify whether the best performing model (i.e. the "Reference Model") has statistically better predictions than the other two comparison models, we calculate the probability that the Reference Model has a smaller absolute mean prediction error (i.e. $\mathcal{L}_1$-loss) than the two other models. In order to do so we: (i) sample posterior predictions from each model, (ii) predict the Leave-One-Out posterior mean prediction, i.e. the weighted expectation using the importance weights obtained from the PSIS smoothing procedure (Vehtari, Simpson, et al. 2024) obtained in (i), (iii) calculate the mean absolute prediction error of the model.[15] Once the mean absolute prediction errors for all models are obtained, we compute the following:

$$\mathbb{P}\left(\{\mathcal{L}_R(\widehat{TE}, \widetilde{TE}_R, w_{p,R}) - \mathcal{L}_k(\widehat{TE}, \widetilde{TE}_k, w_{p,k}) < 0\}\right), \quad \forall k \neq R,$$

i.e. the probability that the "Reference" model has lower $\mathcal{L}_1$-loss than the other alternative models.

We compute this probability using both a Normal approximation of the difference in mean absolute prediction error, and a Bayesian bootstrapping procedure (Rubin 1981b). However, as Sivula et al. (2023) shows, in contexts with low sample sizes (i.e. $N < 100$), such as our case, the normal approximation of the differences in expected point-wise likelihood leads to misleading conclusions. Hence, as Vehtari, Gelman, and Gabry (2016)

[14]We use (3) as the benchmark specification for comparing the three models.

[15]Formally, let $\widetilde{TE}$, be a random draw from the posterior distribution $p(\widetilde{TE}|\theta, \hat{se})$, where θ is sampled from its posterior $p(\theta|\widehat{TE}, \hat{se}, \sigma_\theta)$ and w_p the importance weights obtained from the LOO procedure. The absolute mean prediction error is defined as $\mathcal{L}_1(\widehat{TE}, \widetilde{TE}, w_p) := \frac{1}{N}\sum_{n=1}^{N}\left|\widehat{TE}_n - \mathbb{E}(w_p\widetilde{TE})_n\right|$.

recommends, we also report critical thresholds, i.e.:

- if $|\Delta\mathbb{E}(log\mathcal{L})| < \sqrt{\mathbb{V}(\Delta\mathbb{E}(log\mathcal{L}))}$, then there is evidence that the two models have equivalent predictive performance.
- If $|\Delta\mathbb{E}(log\mathcal{L})| > 2\sqrt{\mathbb{V}(\Delta\mathbb{E}(log\mathcal{L}))}$, then the reference model performs better in a significant way.

4 Results

Table 3 presents average treatment effects in the full sample, estimated using Equation (3). Panel A displays the predicted treatment effect of a $100 total transfer amount, our preferred outcome variable for estimating impact of lump sum transfers, while Panel B displays the predicted treatment effect of a $100 monthly tranche amount, our preferred outcome variable for stream transfers. Table 4 further explores the impact of cash on shifts to and from wage versus non-wage income, and within non-wage income to and from non-farming versus farming.

Table 5 examines heterogeneity by disbursement schedule, i.e., by ongoing streams, completed streams, and lump sums, estimated using Equation (4). In Table 6, we show dynamic treatment effects on monthly household consumption estimated using Equations (5) and (6). In Table 7, we estimate the curvature of effects with respect to transfer size, i.e. whether there are decreasing, increasing, or constant marginal returns to cash using Equations (7) and (8). In Table 8, we compare our model's predicted concavity (or convexity) with that observed in studies using random grant assignments. Table 9 and Table 10 analyze the impact of targeting by gender and framing by food security and child development goals, based on Equation (9). Next, Table 11 and Table 12 examine contextual heterogeneity, specifically rural versus non-rural and country-level economic conditions. Finally, Table 13 presents benefit-cost ratios under different assumptions (regarding duration of stream transfers and program costs) and specifications (estimating dynamic effects as binary estimates for under or over 18 months versus a quadratic specification).

4.1 Do Cash Transfers Shift Labor Supply and Income?

We start by discussing whether UCTs shift labor supply and income because this result then sets the stage for understanding investment and consumption changes. We find positive impacts on income, with credibility intervals considerably removed from zero, thus clearly rejecting "dependency" theories that predict negative impacts on income. Specifically, Column 1 of Table 3 shows positive impact on monthly income for both total transfer ($1.4/month per $100, 95% CI: 1.0, 1.9) and the monthly tranche amount ($22.7/month per $100, 95% CI: 15.4, 30.7).[16] Note that papers vary in their reporting of treatment effects on income at the individual or household level. We do not adjust for this inconsistency, because that would require assuming an elasticity of household income with respect to individual income.

Results on income are further supported by positive effects on labor force participation (LFP). Table 3 shows that UCTs increase LFP by 3.4 percentage points (95% CI: 1.7, 5.2) predicted at the median total transfer amount, and by 4.6 percentage points (95% CI: 1.8, 7.5) predicted at the median monthly tranche amount.[17] Table 5 further breaks down the analysis by disbursement schedule and shows consistently positive point

[16]To construct the sample of treatment effects on monthly income, we use measures of total individual or household income when reported. If no such aggregate measure is reported, we impute it by using the sub-category of income (e.g., wage earnings, household enterprise profits, etc.) reported that has the largest control group mean. See Appendix: *Outcome Selection*.

[17]These large effects are in part driven by two positive outliers (in a sample of only 17 estimates) from the Child Development Grant Programme in Nigeria which finds a $20 monthly stream transfer (about two-thirds the sample median of $29) to increase paid work among wives in treatment households by 6.0 percentage points after 24 months and 10.7 percentage points after 48 months. The same program raised female labor force participation by 30 and 53 percentage points per $100 monthly tranche at months 24 and 48, respectively.

estimates. With fewer studies per estimate, however, several of the credibility intervals include zero.

We also see positive, but less robust, results on total hours worked. The point estimates are positive for both methods (total transfer and monthly tranche), however the 95% credibility interval includes zero for total transfer but is strictly above zero for monthly tranche. Specifically, Table 3 reports an increase of 0.3 hours per week (95% CI: -0.3 to 0.9) for the median total transfer amount and 0.02 hours per week (95% CI: -0.2 to 0.2) for the median monthly tranche amount. Table 5, which further disaggregates by disbursement schedule, finds even wider intervals. However estimates are from as few as two programs, and at most seven, so we draw little to no inference from the analysis on differential impact by disbursement schedule on hours worked.

These results are consistent with the analysis in Banerjee, Hanna, et al. (2017), which examines seven studies (six conditional cash transfers and one UCT) and documents predominantly positive and at worst null results.

In Table 4, we examine effects on categories of income, specifically wage versus non-wage and farming non-wage versus non-farming non-wage. UCTs systematically shift labor supply, measured as a binary LFP, from wage employment (-11.6 percentage points; 95% CI: -21.2, -2.1) towards non-wage employment (14.7 percentage points; 95% CI: 8.4, 21.2). The similar analysis for monthly income (Column 2) finds an increase in non-wage employment income but the result for wage employment is noisy as it is based on only five estimates (and is a positive point estimate, despite the negative extensive margin result on LFP). Notably, the aggregate effects for LFP and Monthly Income remain robust to a "Pooled Specification", which jointly estimates aggregate and category-specific effects.[18] A further disaggregation of the non-wage employment into farming and

[18] In order to ensure that we are not simply picking up variation **across** studies, we further restrict our sample to only studies that report a decomposition of treatment effects for both wage and non-wage employment (see Appendix Table E.2). Unfortunately, for income, we do not have any study in our sample that reports both the wage and non-wage income category. On LFP, our results remain robust to

non-farming suggests that the positive effect on non-wage employment is mostly driven by non-farming LFP (21.2 percentage points; 95% CI: 13.5, 29.1), which is once again matched by the effects on non-farming non-wage income ($19.1; 95% CI: 7.7, 31.3).[19]

4.2 Investment and Consumption Patterns

Next, we examine the impact of UCTs on investment and consumption, and patterns observed across disbursement schedule and over time. We find support for the oft-hypothesized result that stream transfers generate more change in consumption relative to lump sums, and vice versa for investments or durable goods.

Transfer recipients trade off spending on consumption goods (durable or non-durable) and investing in productive assets. We find positive effects across the board on both consumption and investment. Table 3 reports a $13.3 (95% CI: 10.1, 16.8) increase in monthly total household consumption for the median total transfer amount and a $17.4 (95% CI: 13.1, 21.9) increase for the median monthly tranche amount. The majority of the consumption increase comes from food: $9.1 (95% CI: 6.5, 11.8) increase in monthly household food consumption for the median total transfer amount and $12.5 (95% CI: 9.2, 16.1) for the median monthly tranche amount. The stock of total assets increases by $19.6 (95% CI: 12.6, 26.8) for each $100 of the total transfer amount.

Transfer frequency and timing of the endline measurement relative to program completion drive heterogeneity in consumption and investment behavior. Specifically, com-

this "overlapping sample" analysis, confirming that the estimates are indeed correctly capturing **within** study variation.

[19]Additional analyses breaks down the LFP category analysis by disbursement schedule (Appendix Table E.3) and gender targeting (Appendix Table E.4). Lump sum UCTs have a high positive effect (24.4 percentage points, 95% CI: 14.9, 34.3) on non-wage employment, while both ongoing and ended streams do not have any statistically significant effect. Both lump sum and stream interventions do not have a statistically significant effect on wage employment. UCTs targeted at women have a much higher effect (42.2 percentage points; 95% CI: 31.8, 53.1) on non-wage LFP compared to non-targeted interventions (2.6 percentage points; 95% CI: -4.2, 9.3); at the same time, negative effects on wage employment are statistically significant only for non-targeted interventions (-10.5 percentage points; 95% CI: -20.2, -0.9).

pleted stream programs produce results similar to lump sum transfers but different from ongoing stream programs. Table 5 Panel A reports similar point estimates regarding the treatment effect per total transfer amount for household consumption across all three disbursement schedules, with ongoing streams having a marginally higher effect than the other two. However, when analyzed per monthly tranche amount (Panel B), the treatment effects on consumption are notably stronger for ongoing streams. Specifically, ongoing streams of a \$100 monthly tranche boost consumption by \$83.7 (95% CI: 65.8, 102.7) compared to \$56.6 (95% CI: 31.6, 82.4) for completed stream programs and \$42.0 (95% CI: 27.4, 57.3) for lump sum transfers. This is likely the consequence of recipients treating ongoing transfers similar to income, resulting in a higher marginal propensity to consume. In fact, it is not possible to reject the simple joint hypothesis that recipients expect ongoing transfers to continue indefinitely and the Permanent Income Hypothesis that consumption should rise by the full amount of the ongoing transfer. Completed streams and lump sum transfers do not generate the same expectation of future cash and so their impact is driven entirely by savings and potential increases in income from prior additional investments. The \$2.5 (\$3.1) increase in monthly consumption after receipt of a \$100 lump sum (completed stream) would be consistent with an annual return on capital of approximately 30 (37) percent under the PIH. This pattern of consumption impacts is incompatible with a simple hand-to-mouth model of consumption; households have at least some ability to save or borrow. However, results to follow on assets, income, and the evolution of impacts over time all show evidence contrary to the extreme PIH.

Treatment effects per \$100 monthly tranche on monthly household food consumption are as large as \$71.1 (95% CI: 57.1, 86.0) for ongoing stream programs but only \$17.8 (95% CI: 5.9, 30.7) for lump sum transfers and not statistically significant for completed stream programs.[20]

[20]Note, however, that data limitations are severe for completed stream programs: Only four such programs report food consumption.

Examining food security, differences between disbursement schedules look less stark.[21] Table 5, Panel B shows that a \$100 monthly tranche yields a 0.8 standard deviation improvement (95% CI: 0.5, 1.2) in food security for ongoing streams, compared to 0.9 for completed streams (95% CI: 0.6, 1.3) and 0.4 for lump sum transfers (95% CI: 0.1, 0.6). We conjecture this inconsistency between impacts on food consumption and food security arises since very small increases in food consumption can have substantial impacts on measures of food security (e.g., of skipping meals, experiencing hunger, etc.) for households near the threshold.

The stock of total assets shows similar differences across disbursement schedules to consumption, with completed streams yielding results more similar to lump sum transfers than to ongoing streams. Specifically, for each \$100 total transfer, completed streams and lump sum transfers generate increases in total assets of \$26.0 (95% CI: 12.2, 39.9) and \$21.7 (95% CI: 12.8, 30.9), respectively, while ongoing streams yield no statistically significant increase (\$1.6; 95% CI: -15.5, 18.7). The later finding is consistent with the simple PIH, but the observed increases in assets after receipt of \$100 in a lump sum or completed stream are too small to be consistent with full consumption smoothing. The increase in the stock of financial assets is \$1.4 (0.02, 2.75) for completed streams, whereas ongoing streams increase financial assets by \$2.4 (95% CI: 1.0, 3.9) for each \$100 of the total transfer amount, and for lump sum transfers increases by \$1.8 (95% CI: 1.1, 2.7) [22]. Estimates based on the amount of the monthly tranche yield qualitatively similar results across disbursement schedules.[23] Beyond sizable effects on direct economic measures, such as consumption, income, and assets, UCTs also meaningfully improve psychological well-being. Table 3, Column 2 reports a 0.1 standard deviation increase at the median

[21] Since we use z-scores, we show in Appendix Table B.1 a complete list of treatment effects on food security measures before and after standardization.

[22] In our sample, Ambler et al. (2018) classify livestock as household savings, arguing that these assets often function as a buffer stock, following the rationale advanced by Udry and Kazianga (2006). Four studies in our sample incorporate in-kind savings into their measurements.

[23] Appendix Table E.5 reports treatment effects on various types of assets: durable assets, productive assets, and financial assets. However, we do not have sufficient data to conduct meaningful comparisons of impact by disbursement schedule on these disaggregated outcomes.

total transfer amount (95% CI: 0.1, 0.2).[24] The positive average treatment effect on psychological well-being is primarily driven by ongoing stream UCT programs (Table 5), i.e., even though economic impacts persist, the psychological well-being impacts dissipate more rapidly. Ongoing stream UCTs improve subjective measures of well-being by 1.1 standard deviations per $ 100 monthly tranche (95% CI: 0.7, 1.5). These large estimates are partially driven by three positive outliers from the Zambia Child Grant Program (CGP). [25] In contrast, lump sum transfers and completed stream programs produce effects close to zero that are not statistically significant. This is generally in line with the literature on cash transfers and mental health that finds more modest ameliorating effects on subjective well-being in combined samples of CCTs and UCTs (McGuire et al. 2022) and depression (McGuire et al. 2022; Wollburg et al. 2023).

4.3 Dynamic Effects

Next we examine temporal dynamics. Considering the timing of impact assessment relative to program onset and completion offers further insight into patterns of consumption and investment behavior by program type. In Table 6, we explore the dynamic impacts on total monthly household consumption. We choose to focus on this outcome for substantive and practical reasons. Total household consumption is an aggregate measure of economic well-being. With 82 estimates, we have more observations than nearly any other outcome and thus more ability to estimate dynamic effects by disbursement schedule. Also, our sample of reported treatment effects on household consumption is relatively

[24]See Appendix Table B.2 for a complete list of treatment effects in our sample on outcomes related to psychological well-being before and after standardization.

[25]When we exclude three outliers that originate from the Zambia Child Grant Program (CGP), the treatment effect per $100 monthly tranche is still strongly positive, but reduced from 0.5 standard deviations (95% CI: 0.3, 0.7) to 0.4 (95% CI: 0.3, 0.5) in the full sample or from 1.1 (95% CI: 0.7, 1.5) to 0.8 (95% CI: 0.5, 1.1) in the ongoing streams sample, as reported in Appendix Table E.6. The estimates from the Zambia CGP are not only positive outliers, they are also constructed from a binary indicator variable for whether the respondent was feeling happy or happier than 12 months prior. We do not extract an equivalent outcome variable to construct our standardized outcome for any other program. Appendix Table B.2 reports all treatment effects on psychological well-being before and after standardization.

balanced between ongoing stream, completed stream, and lump sum programs. In addition to consumption, we examine dynamic effects on the stock of total assets, in order to shed light on savings and investment behavior not fully captured by consumption. With a smaller sample, however, we are less able to draw robust conclusions.

Our analysis reveals little evidence that treatment effects dissipate over time. In fact, the benefits of ongoing stream UCTs appear to grow. This suggests that while transfers continue some funds get consumed and others invested, leading to increasing income over time that feeds back into consumption. We do, however, note suggestive evidence of smaller consumption effects for lump sum transfers in the long run. Figure 3.1 plots the posterior average treatment effects on total consumption sorted by months since first transfer to visualize the relationship between effect size and measurement timing.

As seen in Table 6, Panel B1, we find evidence that the effects of ongoing stream transfers on household consumption are greater in the long run (18 months after transfer onset). The long-term treatment effect per \$100 monthly tranche is \$100.7 (95% CI: 78.8, 123.0) while the short-term treatment effect per \$100 monthly tranche is \$59.9 (95% CI: 36.2, 85.2).[26] For completed stream programs and lump sum transfers, we find positive impacts in both short and long run, with a smaller point estimate in the long run but with overlapping prediction intervals.

Panels A2 and B2 of Table 6 present results from a polynomial model which interacts a continuous months variable and its squared term with ongoing and completed program indicators.[27] Consistent with our findings in Panels A1 and B1, we observe greater consumption effects over time for ongoing stream programs but virtually constant effects for

[26]Note this finding is not robust to our alternative outcome variable definition, as seen in Panel A1 of Table 6. While we still estimate a larger long-term treatment effect, the credibility intervals of our estimates largely overlap.

[27]Due to the limited number of estimates for completed stream programs and the fact that the dynamic effects of completed stream programs appear more similar to lump sum transfers than to ongoing stream programs as shown in Panel A1, we pool completed stream programs and lump sum transfers to estimate the coefficients on the months and months squared terms.

completed stream programs and lump sum transfers. The predicted treatment effect of a \$100 UCT stream at month 12 is \$64.1 (95% CI: 42.7, 86.9) and at month 24 is \$99.4 (95% CI: 77.0, 122.3), whereas the treatment effects for completed stream and lump sum are \$35.6 (95% CI: 21.5, 50.3) and \$47.3 (95% CI: 26.0, 69.7).

4.4 Curvature with respect to transfer amount

Whether UCTs exhibit increasing marginal returns is not only a key question for economic theory but also a critical policy question. If there are increasing marginal returns beyond a certain threshold, then this may justify giving larger sums of cash to a small number of recipients to push them out of a poverty trap. Whereas if there are diminishing returns, then policymakers should give smaller transfers to many more recipients. The line of thinking, however, ignores other moral considerations, such as equity, and practical concerns, such as the interaction between transfer size and administrative costs

Figure 3.2 plots the posterior average treatment effects on total consumption sorted by monthly tranche amount to visualize the relationship between the treatment effect per dollar and transfer size. The forest plot indicates no clear pattern of increasing or decreasing marginal returns. In Table 7, we test explicitly for increasing or decreasing marginal returns to UCTs by incorporating covariates for transfer size interacted with disbursement type into our model. Since our outcome variable is the treatment effect per dollar transferred, the interpretation of the coefficient on these covariates is equivalent to the second derivative of the treatment effect (i.e. curvature) with respect to transfer amount. For all disbursement types, we find negative (i.e., concave) but not statistically significant curvature effects on monthly household consumption for any disbursement type.

Thus we do not find evidence for "threshold" poverty trap models, at least for thresholds within the range of transfer amounts where our evidence is robust. But absence of evidence is not evidence of absence, particularly in this case, as this is a fairly weak test for the poverty trap theory given this is examining patterns at the study-level across markets and countries, rather than a household-level micro examination that attempts to incorporate household level heterogeneity which inevitably affects any such threshold.

We find mixed evidence of curvature when examining total assets. Columns 4-6 report these results. Note that only lump sum has a large sample of studies (41 estimates from 23 programs) and finds a slightly positive (but neither large economically nor significant statistically) estimate for the squared-term (20th to 80th percentile shifts from 19.1 to 22.8). However ended streams (which has only 12 estimates from 4 programs) does yield statistically significant and economically meaningful decline in marginal returns to increases in the magnitude of stream transfers that have ended (20th to 80th percentile shifts from 44.9 to 25.2).

To further examine this question of convexity or concavity with respect to grant size, Table 8 presents the curvature estimates results from each of the studies which randomly assigned individuals to different grant amounts. Column 3 reports the transfer sizes tested within each study. Column 4 reports the ratio of the treatment effects on consumption for the different grant amounts within the study. And thus Column 5 is then the ratio of the ratios, such that > 1 indicates increasing returns to grant size (convexity) and < 1 indicates decreasing returns to grant size (concavity). Column 6 then reports the analogous estimate from our model (using the model specified in Table 7). The estimates for study-specific ratios range from 0.23 (quite concave) to 5.29 (quite convex), but the half of the estimates (9 of 18 rows) are between 0.70 and 1.05. Column 6 then shows the model estimates as predicted by our Bayesian analysis, which as expected from the Table 7 estimates are typically near 1. Columns 7, 8, and 9 then show the same, but for stock of total assets. Here Column 8 shows that there is higher variance across studies with respect to whether there is concavity or convexity, whereas the estimates from the model are almost exactly linear for lump sums, and slightly concave for completed streams.

4.5 Targeting and Framing Effects

In Table 9, we report on the differential impact of programs targeted to women (versus to men or non-targeted). We consider a program targeted to women (men) if the cash is intentionally given to women (men) exclusively or if greater than 80% of the intended recipients are female (male). Programs targeted to women produce greater consumption effects than programs without any gender targeting: Female-targeted UCTs lead to a \$4.6 increase per \$100 total transfer amount in monthly total household consumption (95% CI: 3.5, 5.7) compared to a \$2.5 increase per \$100 total transfer amount (95% CI: 1.6, 3.4) for non-targeted programs. This difference appears to driven primary by greater food consumption. Female-targeted transfers on average also generate considerably larger treatment effects on income than non-targeted programs: \$1.9 per \$100 of total transfer (95% CI: 1.2, 2.5) versus a 95% credibility interval of 0.4 to 1.5 for non-targeted UCTs.

Other results do not differ between targeting categories, with credibility intervals overlapping substantially for treatment effects on child welfare outcomes, such as height-for-age (HAZ), weight-for-age z-scores (WAZ), and school enrollment, which may be a consequence of the imprecision of our estimates. As there are very few male-targeted programs, we generally lack the ability to credibly distinguish differences between male-targeted programs and female-targeted or non-targeted programs for any outcomes. The exception is income, where we have relatively more data on male-targeted programs. Here we observe larger effects for male-targeted programs than either non-targeted or female-targeted programs.

In Table 10, we compare impacts from programs that employ framing to encourage spending on children or food and programs without such framing. In Panel A, we find point estimates for framed transfers are larger and outside the 95% credibility interval for non-framed for five outcomes: total consumption, food consumption, food security, income, and psychological well-being. Findings from our monthly tranche specification in Panel B are similar, with even more stark differences for food consumption and food security z-scores. These results suggest that framing improves food-security related outcomes, but we do not find credible evidence that it has any positive effect on child-related outcomes, such as HAZ, WAZ, and school enrollment.

Lastly, we also provide in the Appendix two further explorations: differential effects for government vs. non-governmental institutions and differential effects for physical cash delivery vs. mobile or bank transfers.[28]

4.6 Contextual Heterogeneity

We examine contextual heterogeneity, specifically whether the core results vary for rural versus non-rural and depending on country-level economic conditions. Table 11 presents the average treatment effect results broken by rural or non-rural. We then also provide the breakdown for each category by disbursement type and explanation for non-rural (a combination of urban, mixed, and non-specified). As the sample counts show in Columns 3-10, there is typically not enough data to estimate the breakdown of treatment effects by a more granular categorization, but we show the composition for each in order to understand the overlap.

While a few variables differ, our broad conclusion is that there is not much difference between rural and non-rural areas. For example, the headline result for monthly consumption is 2.9 versus 2.3. We do observe a noticeable difference for income: 0.9 (rural) versus 4.8 (non-rural), and this also manifests itself for labor supply (0.04 rural, and 0.3 non-rural). This may be a by-product of urban programs more likely to specifically target household enterprises. The stock of total assets however does not follow this pattern, with rural outperforming non-rural (22.2 versus 10.5) although both estimates are within the

[28]See Appendix Table C.1 for effects by delivery modality and Appendix Table C.2 for effects by implementer type.

prediction interval of the other.

For country-wide economic conditions, we examine two metrics in Table 12: poverty rate and GDP per capita. Here, for each outcome we report three point estimates derived from the estimation: the 20th percentile, median, and 80th percentile, in order to examine whether either metric leads to important differences across the observed distribution. We find strikingly flat results, i.e., little to no change across the distribution.

4.7 Benefit-Cost Analysis

We leverage our estimated model to predict the returns of UCTs and compare the relative benefits of various program designs. Similarly to Blattman et al. (2016). we define benefits as the average expected treatment effects on consumption[29] and costs as the total transfer amount, discounting all values to the first month of the program, as well as its cost if it is a stream program, using a 5% discount rate. Our approach, however, adds a layer of sophistication by leveraging the estimated model, thus allowing to also take into account uncertainty in the benefit-cost estimates.

We present the results of our benefit-cost analysis in Table 13. We consider five optional alternatives for a program with a $100PPP\$$ total budget. We consider a lump sum disbursement or a stream program with equal tranches over a period of 12, 24, 36 or 48 months. We then use the model to calculate the NPV of the different programs after 48 months from the start. Crucially, we also take into account that, although the total transferred money is equal across programs, the actual cost needs to take into account the different disbursement timing, so we discount the tranches by the same annual interest rate as the benefits.

[29]Formally, for a fixed vector of covariates x_{n+1} (which contains information on the disbursement schedule, the month since first or last transfer and the total transfer amount), the average expected treatment effect is $\mathbb{E}(\theta_{n+1}|\beta, X = x_{n+1})$.

As reported in Table 13, the Lump Sum program has the lowest expected NPV out of all the programs of \$101.5 (95% CI: 20.2, 185.6). The expected NPV for the stream programs is fairly flat with respect to program duration, ranging from \$180.8 (95% CI: 90.9, 274.0) at 24 months to \$177.4 (95% CI: 97.3, 260.4) at 36 months and \$144.3 (95% CI: 40.1, 252.1) at 48 months.

The benefit-cost ratios of the program follow the same order, hence the 24 and 36 months stream programs end up achieving the highest benefit-cost ratio at \$1.9 (95% CI: 1.0, 2.8). It is important to notice that all these estimates have a high degree of uncertainty, reflecting the fact that many idiosyncratic or unobserved aggregate factors might play a role in the cost-effectiveness of the program.

4.8 External Validity and Model Extensions

We present results about the pooling factor and the Bayesian-R^2 in all tables. Overall, there is a great deal of heterogeneity in pooling depending on the outcome (0.1-0.8), i.e., aggregating results from the different RCTs sometimes reduces the overall variance of the error term but for some outcomes it does not. The pooling factor λ provides a measure of how much our results can be safely generalized across different contexts, with higher values associated with greater evidence of external validity and lower context-specific idiosyncratic variability. We compare our pooling factors to Meager (2019), which studies a sample of seven microcredit randomized evaluations (and finds pooling factors between 0.6 and 1.0). We find the highest pooling factor for anthropometrics (0.7, 0.8 and 0.7, for height-for-age, weight-for-age and stunting, respectively). For both income and consumption, we find 0.5 (compared to 0.7 and 0.9 for microcredit as reported by Meager). Psychological well-being generates our lowest pooling factor of 0.1.

On the other hand, the Bayesian-R^2 provides a measure of how well the model specification of the distribution of θ explains the variability of the data at the corresponding hierarchical layer of interest. Interestingly, there is heterogeneity in model performance both across outcomes, but also within outcomes, depending on the specification of the model. Overall, the results tend to indicate that there is a high degree of variability not explained by any specification of our model. This indicates two non-mutually exclusive paths to pursue. First, there may be important contextual or intervention-specific factors which we do not specify (and maybe do not observe). Second, there may be important heterogeneous treatment effects combined with sample composition differences across studies that require the micro-data to estimate.

In Table 14, we present results from the LOO-CV model comparison criterion. As the table shows, the Symmetric (i.e. Normal) model outperforms the two alternative ones, i.e. the skewed and mixture models, for two out of thirteen outcomes, namely Hours Worked per Week and Stunting. For nine of the remaining outcomes, the skewed model performed better, while for the remaining two, the mixture model did. However, these differences are rarely meaningful. In columns (1)-(3) we report the absolute value of the estimated difference in expected point-wise likelihood. Below each value, we provide the two critical thresholds as discussed in subsection Model Extensions. Reassuringly, for most outcomes, the difference in expected point-wise likelihood is below the lower bound of the interval, suggesting that the difference in model performance is negligible (this is true for Total Consumption, Food Consumption and School Enrollment). Of all outcomes, only Height-for-Age exceeds the upper bound, suggesting that the difference in model performance is not negligible. All other outcomes have values in between the bounds.

It is important to stress that due to the small sample sizes of each sample this procedure might be affected by a significant amount of noise (Sivula et al. 2023). Hence, we focus in columns 4-12 on comparing mean absolute prediction errors of the different models. Columns 4, 7 and 10 report the difference in means and standard errors between the symmetric, skewed and mixture models and the reference model for that outcome. Columns 5 and 6 report the probability that the reference model has lower absolute mean prediction error than the symmetric model. As detailed in Model Extensions, we compute such probability using both a Normal approximation and a Bayesian bootstrap procedure. Columns 8 and 9, and 11 and 12 repeat this exercise for the skewed and mixture distributions. In general, the probability that the symmetric model used in the main analyses has higher absolute mean prediction error than the reference model is less than 50% across all outcomes, except for Total Consumption, Financial Assets and Height-for-Age. For all of these outcomes, column (4) reports very small differences in absolute mean prediction error, hence likely negligible.

However, these results still suggest the necessity, in future work, to adjust model specification to better fit the shape of the dataset in consideration, the particular outcome and context, abandoning a "One-model-fits-all" approach. Table 15 shows how the estimates vary under the assumption of a symmetric distribution (the main results presented) and under an alternative skewed distribution. The mean results are quite similar under both models, but the right-skew leads to medians lower than the mean by about 20% typically. The distinction points to the difference between two objectives here: a model-based causal inference analysis to estimate structural parameters of the data-generating process versus using the aggregate data set to make precise predictive forecasts for policy makers and stakeholders for future interventions.

5 Conclusion

The large-scale expansion of randomized evaluations over the past several decades provides an opportunity for aggregating information across evaluations to make important contributions both to policy and to the adjudication of whether or not the empirical lessons from evaluations are robust. Cash transfers are an especially well-suited type of intervention for such an exercise, because the degrees of intervention variation are more limited and the implementation fidelity is easier to define and less likely to vary and drive results. We therefore conduct a meta-analysis based on 115 studies from 72 randomized evaluations.

We present two layers of main results. First, for the average effects, we find positive and strong average treatment effects on a wide range of outcomes, and irrespective of whether transfer frequency is lump-sum or stream: consumption, income, labor force participation, school enrollment, food security, psychological well-being, assets, and child height-for-age. Monthly household consumption increases by $83.7 per $100 monthly transfer in response to ongoing stream programs and by $2.5 per $100 transferred (i.e., a 30% annualized social return on investment) in response to lump sums. Monthly income improves by $26.2 per $100 monthly tranche for ongoing stream transfers and by $1.2 per $100 total transfer for lump sums. Furthermore, we find similarly strong impacts in the long run (18-48 months) as well as short run (0-18 months), although the impacts dissipate partially if transfers stop and amplify if transfers continue (i.e., ongoing stream transfers are partially consumed and partially invested, leading to larger long-run than short-run impacts). Lastly, we demonstrate that UCTs encourage or at worst do not lower labor supply, contradicting "dependency" theories that cash transfers discourage work.

Second, key elements of program design generate substantial impact variation. UCTs targeted to women have larger impacts on consumption and income than non-targeted programs (although transfers targeted to men generate even higher impact on income yet smaller impacts on consumption, but also are derived from only four programs as compared to 16 and 19 programs for female-targeted and untargeted, respectively). There is also evidence that accompanying UCTs with child-focused framing may improve outcomes related to food security.[30] Furthermore, considering transfer frequency and timing rela-

[30]While we do not include conditional cash transfers (CCTs), other meta-analyses have, and find for example that CCTs increase primary and secondary school enrollment by 1.6 percentage points (95% CI: 0.9, 2.4) and 3.5 percentage points (95% CI: 2.4, 4.6) per $100 total transfer amount, respectively (Baird et al. 2014). This is larger than our estimate of 0.9 percentage points (95% CI: 0.4, 1.4) on overall enrollment. Baird et al. 2014 also directly compares CCTs to UCTs, estimating larger but not statistically significant marginal impacts of conditionality. Studies investigating anthropometric outcomes find conditionality limits improvements in child weight but has no effect on height (Manley, Balarajan,

tive to program completion proves critical to understanding households' consumption and investment response to cash transfers. Ongoing stream transfers produce larger consumption effects while completed stream programs and lump sum transfers facilitate greater asset accumulation. Impacts on income are similar regardless of disbursement schedule.

The fact that lump sum cash transfers spur gains in consumption and income comparable to streams that have ended contradicts the common intuition that lump sums should have a "comparative advantage" in facilitating productive investment. One possibility is that, when assured of a continuing stream of cash transfers, poor households are adept at transferring resources across time to take advantage of investment opportunities. This suggests further analysis that explores heterogeneity in outcomes with respect to access to quality savings opportunities may be a fruitful avenue. This could motivate the design of cash transfers that combine access to savings with stream cash flows, an increasingly easy and low-cost add-on, given the expansion of mobile money. A second possibility is that lump sum transfers create in a sense too much slack, and the marginal dollars are not spent efficiently. This could be due to other market frictions leading to rapidly diminishing marginal returns or due to psychological mechanisms such as cognitive scarcity (see, Mullainathan and Shafir 2013).

We further highlight two important cross-cutting lessons from the data. First, treatment effects appear to be constant over time, which given our data is best understood as up to 48 months after the onset of transfer. This is broadly in line with McGuire et al. (2022) which finds that effects on subjective well-being and depression dissipate at modest rates. There is a clear need for more long-term, follow-up data (Bouguen et al. 2019). Further follow-ups would help trace out potential dissipation or augmentation effects, as most data on lump sum transfers are collected 12 to 48 months after treatment.

Second, we find fairly constant marginal returns with respect to transfer size. The coefficient on the squared term for transfer size is precisely estimated and close to zero,

et al. 2020; Manley, Alderman, et al. 2022).

and we do not have the power to estimate functional form more precisely. This null effect is not consistent with "threshold" poverty trap models with large indivisible goods that assume expanding returns. However, with such thresholds inevitably differing across people and markets (or perhaps being above the transfer sizes tested), we cannot rule out asset-based threshold models of poverty.

We close with three methodological considerations that limit how much one can learn from a meta-analysis of this style. First, with respect to many of the most interesting questions, our analysis is severely constrained by not incorporating household-level data. We lack sufficient variation on many important dimensions that require estimating within-study heterogeneity or more detailed re-formulation of outcome variables from raw data in order to sync data across studies. For example, we are largely unable to speak to consumption patterns beyond distinguishing total from food consumption. We are also unable to identify the type of assets recipients tend to purchase as this information is not commonly being collected, in particular not for stream programs. Among other things, this impedes a further investigation into the question as to whether the discrepancy between the positive but more modest effects of lump sum transfers on consumption despite their pronounced effect on total assets is due to investments in unproductive, but potentially welfare-enhancing, types of assets (e.g., furniture, house improvements).

Second, while as discussed above there is a constant push for longer term follow-ups (true not just for cash transfers, but for most development interventions), we suggest that we also need more *immediate* data, data that helps illuminate how transfers get spent. This is particularly true for lump sum transfers, to have clearer understanding of households' immediate consumption and investment decisions upon receipt of funds. This question in general is understudied, and cannot be answered well by merely asking people what they did with the funds (Karlan et al. 2016). Instead, we need more studies that do the first follow-up at about one month, in order to establish the initial changes in outflows that occur because of the receipt of the cash transfer. Then, and particularly if this turned out to be predictable from baseline questions (either broadly generic questions, or intent-questions about what they would want to spend any funds received in the next month), analysis could sort households into likely short-run patterns, to then examine how that then led to longer-run changes for households. Furthermore, an exercise could lead to development of "surrogate" measures, i.e. "predictive" outcomes that can be tracked in the short-run and are good predictors of long-run impact. Validation of such measures would then create opportunities for more rapid-fire learning about how to transfer cash, what messages to include, timing, amounts, etc.

Third, we have a herding cats measurement methods problem. While some standards exist with respect to survey and question design, much variation persists, and is both inevitable and healthy. We do not suggest our community knows the best ways to measure; we want innovation in measurement methods. And some variation in survey methods are a natural and important by-product of contextualizing a survey to a given country, culture, economy, etc. These challenges are exacerbated by inconsistent reporting standards at journals (although this has improved considerably, see Nosek et al. (2015)). But while improved norms and compliance in sharing data and survey instruments help considerably, that does not address the challenge created by the variation in what is actually collected in surveys.

Despite these limitations, we believe aggregating reported point estimates at the study-level sheds important light on several theoretical and policy questions. But, important program, study, and context variables– variables either in hand or easily accessible– could not be included in our preferred specifications due to power considerations. For example, we did not have sufficient variation on modality (mobile money versus cash), or timing within the year (particularly important for farmers). Yet despite the limitations, aggregating results from 115 studies yields important theoretical and policy insights, and also points to specific questions that can and should be tackled with synced micro-level data. Lastly, and perhaps most critically, these estimates can serve as a "cash benchmark": if designing a program to try to improve a specific outcome, this analysis provides an estimate for what a simple cash transfer can deliver.

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Table 1a
Comparison of Cash Transfer Meta-Analyses Studies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Number of Observations			*Count of Studies*					
				Identification		*Conditionality*		*Disbursement*	
Meta-Analysis	Studies	Programs	Estimates	RCT	Quasi-experimental	UCT	CCT	Lump sum	Stream
This study	**115**	**72**	**638**	**115**		**115**		**44**	**77**
Baird et al. (2014)	75	35	64	12	23	9	30		
Banarov et al. (2021)	14	11		9	5	6	8	2	14
Evans and Popova (2017)	13	11	19	5	8	5	8	1	12
Garcia and Saavedra (2017)	59	47	94	Yes	Yes		94	7	40
Guimarães et al. (2021)	16	14		16		2	14	1	15
Kabeer and Waddington (2015)	46	11		Yes	Yes		46		46
Kondylis and Loeser (2021)	7	7	18	7		7		4	4
Little et al. (2021)	17	17		14	3	7	10		17
Manley et al. (2022)	112	64	129	Yes	Yes	62	50	1	111
McGuire et al. (2022)	45		110	27	18	31	14	13	32
Wollburg et al. (2023)	18	13		18		16	3	3	15

For Baird et al. (2014) and Garcia and Saavedra (2017), the counts represent the number of programs rather than studies because study-level information was not reported. For this study, the sum of the count of lump sum and stream studies in columns 8 and 9 exceed the total number of studies in column 1 because seven studies report results on both stream and lump sum transfers.

Table 1b
Comparison of Cash Transfer Meta-Analyses

Meta-Analysis	(1) Average Total Transfer Amount	(2) Average Follow-up Timing	(3) Effect Interpretation	(4) Outcomes
This study	**$799**	**19 months since first transfer**	**Treatment Effect (TE) per dollar transferred**	**Consumption, food security, assets, income, labor supply (adults), psychological well-being, school enrollment, and child development**
Baird et al. (2014)	$351 (per year)		Binary TE of receiving UCT	School enrollment, attendance, and test scores
Banarov et al. (2021)			Binary TE of receiving UCT	Intimate Partner Violence
Evans and Popova (2017)			Binary TE of receiving UCT	Temptation goods expenditure
Garcia and Saavedra (2017)			Binary TE of receiving UCT and TE per dollar transferred	School enrollment and attendance
Guimarães et al. (2021)	$143	13 months since baseline	Binary TE of receiving UCT	HIV testing, treatment, and incidence
Kabeer and Waddington (2015)			Binary TE of receiving UCT	Labor supply (child and adult), consumption
Kondylis and Loeser (2021)	$963	18 months since first transfer	TE per dollar transferred	Consumption
Little et al. (2021)	$8-$75 (per month)		Binary TE of receiving UCT	Child development and child nutrition
Manley et al. (2022)	$83	29 months since baseline	Binary TE of receiving UCT	Child development, child nutrition, and incidence of child illness
McGuire et al. (2022)	$855	23 months since first transfer	Binary TE of receiving transfer with covariate for transfer amount	Psychological well-being
Wollburg et al. (2023)	$773	13 months since last transfer	Binary TE of receiving UCT	Psychological well-being

Transfer amounts reported in 2010 *$PPP*. For this study, we report means accross programs in the primary outcomes analysis sample.

Table 2
Count of Programs (Monthly Household Consumption Estimates), by Program Design Features

	(1) All	(2) Lump Sum	(3) Stream	(4) Stream Ended	(5) Stream Ongoing
Total	72 (82)	39 (41)	37 (41)	16 (14)	30 (27)
Framing for Child Development or Food Security	20 (18)	2 (0)	18 (18)	6 (3)	17 (15)
No Framing for Child Development or Food Security	53 (64)	37 (41)	24 (23)	10 (11)	14 (12)
Transfer Targeted to Women	32 (31)	11 (10)	21 (21)	7 (4)	19 (17)
Transfer Targeted to Men	9 (3)	7 (2)	2 (1)		2 (1)
Transfer not Targeted or Randomized to Men or Women	35 (48)	24 (29)	15 (19)	9 (10)	10 (9)
Transfer Paid Physical Cash	33 (30)	12 (8)	21 (22)	9 (5)	18 (17)
Transfer Paid Mobile Money or Bank Transfer	38 (50)	25 (31)	17 (19)	7 (9)	13 (10)
Implemented by Government	22 (22)	5 (4)	17 (18)	5 (1)	16 (17)
Implemented by NGO	51 (60)	35 (37)	21 (23)	13 (13)	14 (10)

The sum of lump sum and stream programs in Columns 2 and 3 of Panel A does not always equal the total number of programs in Column 1 because some programs implement both stream and lump sum transfers. Similarly, the sum of estimates in Columns 2 and 3 of Panels B and C does not always equal the total number of estimates in Column 1 because Column 1 includes some additional estimates from regressions that pool across lump sum and stream treatment arms. Also, the sum of stream-ended and stream-ongoing programs in Columns 4 and 5 of Panel A does not always equal the total number of stream programs in Column 3 because some stream programs administer follow-up surveys both as the program is ongoing and after it has ended.

Table 3
Average Treatment Effects on Primary Outcomes

	(1)	(2)	(3)	(4)
	Predicted Treatment Effect of \$100 Transfer	Predicted Treatment Effect of Median Transfer Amount (Panel A = *PPP\$*409 Panel B = *PPP\$*29)	Estimates (Programs)	Pooling Factor
Panel A. Treatment Effect per Total Transfer Amount				
Flow outcomes				
Monthly Household Consumption (with controls)	3.3 (2.5, 4.1)	13.3 (10.1, 16.8)	82 (45)	0.54
Monthly Household Food Consumption	2.2 (1.6, 2.9)	9.1 (6.5, 11.8)	49 (31)	0.34
Monthly Income	1.4 (1.0, 1.9)	5.9 (4.1, 7.8)	88 (38)	0.51
Hours Worked per Week	0.1 (-0.1, 0.2)	0.3 (-0.3, 0.9)	25 (13)	0.26
Labor Force Participation (percentage points)	0.8 (0.4, 1.3)	3.4 (1.7, 5.2)	17 (11)	0.32
School Enrollment (percentage points)	0.9 (0.4, 1.4)	3.8 (1.8, 5.9)	26 (16)	0.25
Food Security z-Score	0.03 (0.02, 0.04)	0.1 (0.1, 0.2)	47 (25)	0.33
Psychological Well-being z-Score	0.04 (0.02, 0.05)	0.1 (0.1, 0.2)	56 (30)	0.16
Stock outcomes				
Stock of Total Assets	19.6 (12.6, 26.8)	79.9 (51.5, 109.4)	60 (28)	0.31
Stock of Financial Assets	1.8 (1.2, 2.5)	7.5 (5.1, 10.1)	49 (24)	0.56
Height-for-Age z-Score	0.01 (0.002, 0.014)	0.03 (0.01, 0.06)	32 (18)	0.70
Weight-for-Age z-Score	0.01 (-0.0001, 0.0126)	0.03 (-0.0004, 0.0517)	15 (10)	0.84
Stunting (percentage points)	-0.2 (-0.6, 0.2)	-0.8 (-2.4, 0.7)	12 (8)	0.71
Panel B. Treatment Effect per Monthly Tranche Amount				
Flow outcomes				
Monthly Household Consumption (with controls)	59.2 (44.8, 74.6)	17.4 (13.1, 21.9)	82 (45)	0.55
Monthly Household Food Consumption	42.7 (31.4, 55.0)	12.5 (9.2, 16.1)	49 (31)	0.31
Monthly Income	22.7 (15.4, 30.7)	6.7 (4.5, 9.0)	88 (38)	0.53
Hours Worked per Week	0.1 (-0.6, 0.7)	0.02 (-0.2, 0.2)	25 (13)	0.30
Labor Force Participation (percentage points)	15.6 (6.1, 25.6)	4.6 (1.8, 7.5)	17 (11)	0.19
School Enrollment (percentage points)	14.0 (6.5, 22.2)	4.1 (1.9, 6.5)	26 (16)	0.30
Food Security z-Score	0.6 (0.4, 0.8)	0.2 (0.1, 0.2)	47 (25)	0.28
Psychological Well-being z-Score	0.5 (0.3, 0.7)	0.1 (0.1, 0.2)	56 (30)	0.12
Panel C. Treatment Effect on Monthly Household Consumption without Controls				
Treatment Effect per Total Transfer Amount	2.8 (2.2, 3.4)	11.3 (9.0, 13.8)	82 (45)	0.49
Treatment Effect per Monthly Tranche Amount	48.0 (38.3, 58.4)	14.1 (11.2, 17.1)	82 (45)	0.50

All currency values are reported in 2010 USD PPP. For lump sum transfers, the monthly tranche amount for Panel B is calculated by dividing the total transfer amount (used in Panel A) by the number of months since the first transfer. Our dataset for Monthly Household Consumption uses treatment effects on total consumption when reported; we use treatment effects on non-durable consumption or food consumption when total consumption is unavailable. Our analysis controls for whether food and durable goods are included in total consumption. Panel C shows results on Total Household Consumption from a model that does not include these controls. Our dataset for Monthly Income uses reported treatment effects on total household or individual income when reported; if treatment effects are only reported by sub-category of income, e.g., wage earnings, non-farm enterprise profits, etc., then the sub-category with the highest control group mean is used instead. See Table 4 for a comparison to analysis that only uses reported estimates on total household or individual income. 95% credibility intervals in parentheses.

Table 4
Treatment Effects on Components of Labor Force Participation (LFP) and Monthly Income

	(1)	(2)	(3)	(4)
	Predicted Treatment Effect of $100 Transfer		Estimates (Programs)	
	LFP - Binary Outcome *(percentage points)*	Monthly Income *(PPP $)*	LFP	Monthly Income
Panel A. Repeats from Table 3, Panel B				
Aggregate	15.6 (6.1, 25.6)	22.7 (15.4, 30.7)	17 (11)	88 (38)
Panel B. Pooled Specification (Simultaneously estimate aggregate measure and components)				
Aggregate	16.5 (5.0, 28.0)	29.0 (17.8, 41.3)	17 (11)	34 (14)
Wage Employment	-11.6 (-21.2, -2.1)	18.6 (-9.0, 46.7)	25 (12)	5 (4)
Non-Wage Employment	14.7 (8.4, 21.2)	18.6 (8.4, 29.5)	62 (22)	49 (20)
Panel C. Pooled Specification (Simultaneously estimate aggregate measure and components)				
Aggregate	16.5 (5.0, 28.0)	28.4 (17.5, 40.5)	17 (11)	34 (14)
Wage Employment	-11.6 (-21.2, -2.1)	18.5 (-8.5, 46.0)	25 (12)	5 (4)
Non-Wage Farming Employment	2.7 (-7.7, 13.1)	46.7 (11.4, 82.2)	21 (10)	5 (3)
Non-Wage Non-Farming Employment	21.2 (13.5, 29.1)	19.1 (7.7, 31.3)	41 (21)	39 (12)

Effects with seven or fewer estimates in gray. 95% credibility intervals in parentheses. "Aggregate" refers to having any reported income generating activity for LFP in all 3 panels. For Monthly Income in Panel A, we use total income for "Aggregate" when reported, and otherwise impute it with the results from the income category with the largest control group mean. For Panels B and C, however, because we include estimates for wage and non-wage components, we no longer impute Total Income when it is missing. The construction of these samples is described in Appendix: *Outcome Selection*. **Panel A** reports the coefficients from Table 3 using model (3), i.e. our "Original Specification". **Panel B** reports the coefficients from (3b), our "Pooled Specification", that jointly estimates category-specific effects as well as the aggregate result. **Panel C** reports estimates similar to Panel B, except breaks non-wage employment into two categories, farming and non-farming.

Table 5
Heterogeneous Treatment Effects by Disbursement Schedule

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Predicted Treatment Effect of $100 Transfer			*Estimates (Programs)*			*Model Fit*	
	Ongoing Stream	Completed Stream	Lump Sum	Ongoing Stream	Completed Stream	Lump Sum	R^2	Pooling Factor
Panel A. Treatment Effect per Total Transfer Amount								
Flow outcomes								
Monthly Household Consumption (with controls)	4.2 (3.2, 5.3)	3.1 (1.6, 4.6)	2.5 (1.5, 3.4)	27 (20)	14 (7)	41 (25)	0.24	0.57
Monthly Household Food Consumption	3.4 (2.7, 4.3)	1.0 (-0.2, 2.3)	1.0 (0.3, 1.8)	22 (15)	6 (4)	21 (15)	0.35	0.44
Monthly Income	1.4 (0.6, 2.4)	0.8 (0.1, 1.6)	1.2 (0.8, 1.7)	11 (7)	13 (5)	53 (25)	0.01	0.57
Hours Worked per Week	0.3 (-0.1, 0.7)	-0.2 (-0.4, 0.1)	0.2 (0.005, 0.413)	3 (2)	9 (4)	13 (7)	0.19	0.35
Labor Force Participation (percentage points)	0.6 (-0.1, 1.4)	0.9 (0.0004, 1.7251)	1.0 (0.3, 1.8)	6 (5)	4 (1)	7 (5)	-0.083	0.41
School Enrollment (percentage points)	1.0 (0.4, 1.8)	1.6 (0.4, 2.7)	0.3 (-0.7, 1.2)	15 (10)	5 (3)	6 (4)	0.05	0.33
Food Security z-Score	0.04 (0.02, 0.05)	0.04 (0.03, 0.06)	0.02 (0.01, 0.04)	14 (9)	13 (7)	20 (14)	0.06	0.37
Psychological Well-being z-Score	0.1 (0.05, 0.10)	0.03 (0.01, 0.05)	0.02 (-0.002, 0.036)	16 (10)	14 (7)	26 (16)	0.14	0.20
Stock outcomes								
Stock of Total Assets	1.6 (-15.5, 18.7)	26.0 (12.2, 39.9)	21.7 (12.8, 30.9)	7 (5)	12 (4)	41 (23)	0.04	0.35
Stock of Financial Assets	2.4 (1.0, 3.9)	1.4 (0.02, 2.75)	1.8 (1.1, 2.7)	6 (4)	10 (4)	33 (17)	0.02	0.59
Height-for-Age z-Score	0.005 (-0.001, 0.013)	0.01 (0.002, 0.026)	0.01 (-0.01, 0.03)	21 (14)	8 (6)	3 (1)	0.12	0.75
Weight-for-Age z-Score	0.02 (0.01, 0.03)	0.01 (-0.002, 0.014)	-0.002 (-0.01, 0.01)	8 (7)	4 (3)	3 (1)	0.69	0.97
Panel B. Treatment Effect per Monthly Tranche Amount								
Flow outcomes								
Monthly Household Consumption (with controls)	83.7 (65.8, 102.7)	56.6 (31.6, 82.4)	42.0 (27.4, 57.3)	27 (20)	14 (7)	41 (25)	0.36	0.59
Monthly Household Food Consumption	71.1 (57.1, 86.0)	13.7 (-6.5, 34.4)	17.8 (5.9, 30.7)	22 (15)	6 (4)	21 (15)	0.51	0.47
Monthly Income	26.2 (12.0, 41.3)	11.5 (-1.0, 25.1)	18.0 (10.7, 25.8)	11 (7)	13 (5)	53 (25)	0.03	0.58
Hours Worked per Week	0.3 (-1.5, 2.0)	-0.2 (-1.3, 0.8)	0.3 (-0.6, 1.1)	3 (2)	9 (4)	13 (7)	0.02	0.32
Labor Force Participation (percentage points)	9.2 (-8.9, 27.1)	28.3 (7.7, 50.4)	13.7 (-2.2, 29.8)	6 (5)	4 (1)	7 (5)	-0.004	0.29
School Enrollment (percentage points)	16.5 (8.3, 25.7)	29.9 (15.4, 44.9)	-2.2 (-12.7, 8.2)	15 (10)	5 (3)	6 (4)	0.34	0.48
Food Security z-Score	0.8 (0.5, 1.2)	0.9 (0.6, 1.3)	0.4 (0.1, 0.6)	14 (9)	13 (7)	20 (14)	0.14	0.33
Psychological Well-being z-Score	1.1 (0.7, 1.5)	0.4 (0.04, 0.83)	0.2 (-0.1, 0.5)	16 (10)	14 (7)	26 (16)	0.16	0.16

All currency values are reported in 2010 USD PPP. Treatment effect per total transfer amount (Panel A) is our preferred outcome variable for completed streams and lump sum transfers. Treatment effect per monthly tranche amount (Panel B) is our preferred outcome variable for ongoing stream transfers. Median monthly tranche amounts are $23, $47, and $45 for ongoing streams, completed streams, and lump sum programs, respectively. Median total transfer amounts are $464, $842 , and $440 for ongoing streams, completed streams, and lump sum programs, respectively. Our dataset for Monthly Household Consumption uses treatment effects on total consumption when reported; we use treatment effects on non-durable consumption or food consumption when total consumption is unavailable. Our analysis controls for whether food and durable goods are included in total consumption. Our dataset for Monthly Income uses reported treatment effects on total household or individual income when reported. If treatment effects are only reported by sub-category of income, e.g., wage earnings, non-farm enterprise profits, etc., then the sub-category with the highest control group mean is used instead. We do not report results on stunting due to data limitations. Effects with four or fewer estimates in gray. 95% credibility intervals in parentheses.

Table 6
Dynamic Effects by Disbursement Schedule

	(1)	(2)	(3)	(4)	(5)	(6)
	Monthly Household Consumption			*Stock of Total Assets*		
	Ongoing Stream	Completed Stream	Lump Sum	Ongoing Stream	Completed Stream	Lump Sum
Panel A. Treatment Effect per Total Transfer Amount						
A1: Dynamic Effects Binary Model: Short-run versus Long-run						
Predicted Treatment Effects per $100						
Estimated on Short-Term Estimates (measurement up to 18 months after first transfer)	4.4 (2.7, 6.2)	4.5 (1.8, 7.1)	3.1 (2.0, 4.3)	-11.9 (-73.7, 49.9)	21.7 (-39.6, 82.2)	11.3 (-46.1, 68.7)
Estimated on Long-Term Estimates (measurement more than 18 months after first transfer)	4.0 (2.8, 5.2)	2.4 (0.7, 4.2)	1.7 (0.4, 3.1)	-10.6 (-76.6, 55.9)	12.7 (-46.1, 71.0)	12.1 (-47.9, 72.2)
R^2		0.29			0.04	
Pooling Factor		0.60			0.43	
A2. Dynamic Effects Polynomial Model (months and months-squared)						
Predicted Treatment Effects per $100						
Estimated at Month 12	4.3 (2.7, 5.9)	3.0 (1.2, 4.7)	2.5 (1.5, 3.6)		26.9 (12.3, 41.6)	17.3 (7.3, 27.7)
Estimated at Month 24	4.7 (3.3, 6.1)	2.9 (0.5, 5.2)	2.4 (1.1, 3.8)		37.1 (15.7, 58.8)	27.5 (15.4, 40.1)
R^2		0.24			0.13	
Pooling Factor		0.59			0.38	
Panel B. Treatment Effect per Monthly Tranche Amount						
B1: Dynamic Effects Binary Model: Short-run versus Long-run						
Predicted Treatment Effects per $100						
Estimated on Short-Term Estimates (measurement up to 18 months after first transfer)	59.9 (36.2, 85.2)	53.6 (18.8, 88.8)	41.0 (24.1, 58.2)			
Estimated on Long-Term Estimates (measurement more than 18 months after first transfer)	100.7 (78.8, 123.0)	44.5 (15.4, 74.9)	31.5 (10.7, 53.4)			
R^2		0.45				
Pooling Factor		0.64				
B2. Dynamic Effects Polynomial Model (months and months-squared)						
Predicted Treatment Effects per $100						
Estimated at Month 12	64.1 (42.7, 86.9)	55.6 (28.8, 83.1)	35.6 (21.5, 50.3)			
Estimated at Month 24	99.4 (77.0, 122.3)	67.2 (29.6, 106.2)	47.3 (26.0, 69.7)			
R^2		0.52				
Pooling Factor		0.64				
Count of Estimates						
0 to 18 months since first transfer	15	4	23	3	6	20
19 to 36 months since first transfer	12	9	16	4	6	18
37 to 54 months since first transfer	0	1	1	0	0	3
55 to 108 months since first transfer	0	0	1	0	0	0
146 months since first transfer	0	0	0	0	0	0

All currency values are reported in 2010 USD PPP. Panel A1 and B1 consider the months since first transfer for every disbursement schedule, whereas in Panels A2 and B2 we present estimates at months 12 and 24 since the first (last) transfer for ongoing stream (lump sum and ended stream) programs. The distinction between disbursement schedules in the polynomial model captures the dissipation effects of ongoing programs relative to the first transfer, whereas for ended streams and lump sum programs (i.e., ended programs) dissipation effects are presented relative to the months since the last transfer. Due to data limitations and the similarity of average results, we estimate dynamic effects jointly on ended programs in the polynomial model. Due to data limitations of the Stock of Total Assets, the parameters for months and months-squared interacted with ongoing streams (n = 7) performed poorly; we therefore present results from a model that only estimates dynamic effects for ended programs. Our dataset for Monthly Household Consumption uses treatment effects on total consumption when reported; we use treatment effects on food consumption when total consumption is unavailable. Our analysis controls for whether food and durable goods are included in total consumption. Treatment effects per total transfer amount (Panel A) is our preferred outcome variable for ended programs. Treatment effect per monthly tranche amount (Panel B) is our preferred outcome variable for ongoing stream transfers. Effects with seven or fewer estimates in gray. 95% credibility intervals in parentheses.

Table 7
Curvature With Respect to Transfer Amount by Disbursement Schedule

	(1)	(2)	(3)	(4)	(5)	(6)
	Monthly Household Consumption			*Stock of Total Assets*		
	Ongoing Stream	Completed Stream	Lump Sum	Ongoing Stream	Completed Stream	Lump Sum
Panel A. Treatment Effect per Total Transfer Amount						
Base and Curvature Effects per $100						
Base Effect		4.5 (1.6, 7.5)	2.4 (0.8, 4.0)		48.6 (18.8, 78.2)	18.4 (1.7, 35.6)
Change in Effect with Respect to a $100 Increase in Transfer Amount		-0.2 (-0.5, 0.1)	0.01 (-0.1, 0.1)		-1.8 (-3.9, 0.3)	0.3 (-1.1, 1.8)
Predicted Treatment Effect per $100						
Estimated at 20th Percentile of Transfer Amount ($204)		4.2 (1.8, 6.6)	2.4 (1.0, 3.8)		44.9 (18.9, 70.9)	19.1 (4.8, 33.8)
Estimated at 50th Percentile of Transfer Amount ($408)		3.8 (1.8, 5.9)	2.4 (1.1, 3.7)		41.3 (18.8, 63.8)	19.7 (7.6, 32.3)
Estimated at 80th Percentile of Transfer Amount ($1,313)		2.3 (0.2, 4.3)	2.4 (1.5, 3.4)		25.2 (11.5, 39.0)	22.8 (12.7, 33.2)
R^2		0.25			0.07	
Pooling Factor		0.58			0.38	
Panel B. Treatment Effect per Monthly Tranche Amount						
Base and Curvature Effects per $100						
Base Effect	96.9 (71.3, 123.6)					
Change in Effect with Respect to a $100 Increase in Transfer Amount	-25.6 (-62.4, 10.1)					
Predicted Treatment Effect per $100						
Estimated at 20th Percentile of Transfer Amount ($17)	92.6 (71.0, 115.2)					
Estimated at 50th Percentile of Transfer Amount ($29)	89.4 (70.0, 109.7)					
Estimated at 80th Percentile of Transfer Amount ($58)	82.0 (64.0, 100.8)					
R^2		0.60				
Pooling Factor		0.29				
Count of Estimates (Programs)	27 (20)	14 (7)	41 (25)		12 (4)	41 (23)

All currency values are reported in 2010 USD PPP. Since the outcome variable of our model is divided by the transfer amount, the transfer amount covariate is equivalent to the squared term of the transfer amount (i.e. the curvature effect) in a model where the outcome variable is not divided by the transfer amount. Results in Panel A are estimated using a model that includes interaction terms between total transfer amount and indicator variables for completed streams and lump sums as well as indicators for all three disbursement schedules. Results in Panel B are estimated using a model includes an interaction term between monthly tranche amount and an indicator for ongoing streams as well as indicator variables for all three disbursement schedules. Our dataset for Monthly Household Consumption uses treatment effects on total consumption when reported; we use treatment effects on non-durable consumption or food consumption when total consumption is unavailable. Our analysis controls for whether food and durable goods are included in total consumption. 95% credibility intervals in parentheses.

Table 8
Ratios of Treatment Effects to Transfer Amounts

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			Monthly Household Consumption			*Stock of Total Assets*		
Program ID (Disbursement Schedule)	Months Since Last Transfer	Transfer Amount Comparison	Treatment Effect (TE) Ratio *(TE large transfer/ TE small transfer)*	TE Ratio/Transfer Ratio: Reported	TE Ratio/Transfer Ratio: Model-Predicted	Treatment Effect (TE) Ratio *(TE large transfer/ TE small transfer)*	TE Ratio/Transfer Ratio: Reported	TE Ratio/Transfer Ratio: Model-Predicted
56 (Lump Sum)	12	$1,265 vs. $1,035	0.85	0.70	1.01 (0.90, 1.13)	1.11	0.91	1.11 (0.97, 1.24)
56 (Lump Sum)	12	$1,035 vs. $801	1.35	1.05	1.02 (0.91, 1.15)	7.50	5.81	1.13 (0.97, 1.32)
37 (Lump Sum)	22	$1,672 vs. $1,115	2.02	1.35	1.02 (0.76, 1.29)	1.93	1.29	1.24 (0.93, 1.53)
34 (Lump Sum)	19	$1,267 vs. $845	0.74	0.49	1.03 (0.83, 1.26)	0.51	0.34	1.23 (0.95, 1.54)
34 (Completed Stream)	5	$845 vs. $422	1.54	1.03	0.72 (0.19, 1.17)	5.73	3.82	0.78 (0.57, 1.00)
56 (Lump Sum)	12	$1,265 vs. $801	1.15	0.73	1.03 (0.82, 1.29)	8.31	5.27	1.26 (0.94, 1.63)
56 (Lump Sum)	12	$1,890 vs. $1,035	1.33	0.73	1.04 (0.64, 1.47)	0.93	0.51	1.40 (0.89, 1.88)
34 (Completed Stream)	5	$845 vs. $422	1.41	0.71	0.85 (0.55, 1.20)	0.27	0.13	0.83 (0.70, 1.00)
37 (Lump Sum)	23	$1,115 vs. $557	5.89	2.94	1.05 (0.80, 1.41)	5.37	2.69	1.42 (0.93, 2.12)
34 (Lump Sum)	20	$845 vs. $422	10.58	5.29	1.01* (0.86, 1.34)	1.92	0.96	1.43 (0.95, 2.15)
56 (Lump Sum)	12	$1,890 vs. $801	1.80	0.76	1.07 (0.58, 1.68)	6.94	2.94	1.61 (0.86, 2.47)
37 (Lump Sum)	22	$1,672 vs. $557	11.89	3.96	1.11 (0.61, 1.82)	10.37	3.46	1.85 (0.86, 3.24)
34 (Completed Stream)	5	$1,267 vs. $422	2.18	0.73	0.70 (0.10, 1.41)	1.53	0.51	0.66 (0.39, 1.01)
34 (Lump Sum)	19	$1,267 vs. $422	7.85	2.62	1.03* (0.71, 1.69)	0.98	0.33	1.86 (0.89, 3.30)
25 (Completed Stream)	24	$1,449 vs. $384	0.85	0.23	0.61 (-0.09, 1.52)	1.10	0.29	0.58 (0.26, 1.01)
25 (Completed Stream)	3	$1,449 vs. $384	2.32	0.61	0.61 (-0.09, 1.52)	2.16	0.57	0.58 (0.26, 1.01)
55 (Ongoing Stream)	0	$112 vs. $17	6.51	0.99	0.75 (0.42, 1.12)	17.06	2.60	-1.23 (-14.02, 13.93)
55 (Lump Sum)	12	$1,341 vs. $204	4.35	0.66	1.18 (0.64, 2.10)	-3.22	-0.49	2.78 (0.78, 6.49)

Currency values reported in 2010 USD PPP. We use monthly tranche amount for ongoing streams and total transfer amount for lump sums and completed streams. Column (2) reflects the number of months elapsed since the last transfer and the measurement of the outcome. For most studies this was identical for large and small transfers, but for some they differed by a month or two; we report the median here. If the TE Ratio / Transfer Ratio in Columns (4), (6), (8) and (9) is less (greater) than 1, then there are decreasing (increasing) marginal returns with respect to transfer amount. 95% credibility intervals in parentheses (Columns (6) and (9)). Estimates with * in column (6) present the median (rather than the mean) whenever the estimate's posterior mean falls below the 5th percentile of its posterior distribution.

Table 9
Heterogeneous Treatment Effects on Primary Outcomes by Gender Targeting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Predicted Treatment Effect of $100 Transfer			*Estimates (Programs)*			*Model Fit*	
	Not Targeted	Targeted to Women	Targeted to Men	Not Targeted	Targeted to Women	Targeted to Men	R^2	Pooling Factor
Panel A. Treatment Effect per Total Transfer Amount								
Flow outcomes								
Monthly Household Consumption	2.5 (1.6, 3.4)	4.6 (3.5, 5.7)	1.2 (-4.7, 7.2)	48 (21)	31 (21)	3 (3)	0.34	0.56
Monthly Household Food Consumption	0.7 (0.2, 1.2)	3.8 (3.1, 4.6)		23 (13)	26 (18)		0.57	0.45
Monthly Income	0.9 (0.4, 1.5)	1.9 (1.2, 2.5)	3.8 (1.8, 5.8)	41 (19)	40 (16)	7 (4)	0.23	0.56
Labor Force Participation (percentage points)	0.9 (0.2, 1.5)	0.8 (0.2, 1.4)		7 (5)	10 (6)		-0.06	0.36
School Enrollment (percentage points)	0.8 (0.2, 1.5)	1.2 (0.4, 2.0)		16 (10)	10 (6)		-0.01	0.28
Food Security z-Score	0.03 (0.02, 0.04)	0.03 (0.02, 0.05)		26 (12)	21 (14)		-0.01	0.35
Psychological Well-being z-Score	0.03 (0.01, 0.05)	0.1 (0.03, 0.07)	0.02 (-0.03, 0.07)	26 (12)	25 (16)	6 (5)	0.03	0.20
Stock outcomes								
Stock of Total Assets	16.1 (9.4, 23.0)	13.2 (1.9, 24.8)	16.8 (-9.3, 43.7)	45 (16)	13 (9)	3 (3)	-0.02	0.35
Stock of Financial Assets	2.0 (1.2, 2.7)	1.9 (0.6, 3.4)	0.2 (-2.5, 2.9)	36 (15)	10 (6)	3 (3)	0.06	0.59
Height-for-Age z-Score	0.02 (0.01, 0.03)	0.002 (-0.002, 0.009)		11 (4)	21 (14)		0.40	0.79
Weight-for-Age z-Score	0.002 (-0.01, 0.01)	0.01 (0.004, 0.023)		7 (3)	8 (7)		0.52	0.93
Panel B. Treatment Effect per Monthly Tranche Amount								
Flow outcomes								
Monthly Household Consumption	41.9 (28.2, 56.0)	92.6 (73.9, 112.1)	10.3 (-75.0, 96.5)	48 (21)	31 (21)	3 (3)	0.51	0.59
Monthly Household Food Consumption	10.1 (2.7, 17.7)	76.0 (64.2, 88.2)		23 (13)	26 (18)		0.76	0.53
Monthly Income	13.2 (5.3, 21.9)	32.4 (21.7, 43.9)	60.6 (23.9, 97.6)	41 (19)	40 (16)	7 (4)	0.27	0.57
Labor Force Participation (percentage points)	12.0 (-4.0, 27.8)	18.3 (5.3, 32.0)		7 (5)	10 (6)		-0.04	0.24
School Enrollment (percentage points)	10.7 (1.3, 20.8)	19.7 (7.3, 32.5)		16 (10)	10 (6)		0.02	0.33
Food Security z-Score	0.6 (0.3, 0.8)	0.7 (0.4, 1.0)		26 (12)	21 (14)		-0.01	0.29
Psychological Well-being z-Score	0.4 (0.1, 0.7)	0.7 (0.4, 1.0)	0.1 (-0.6, 0.7)	26 (12)	25 (16)	6 (5)	0.04	0.15

All currency values are reported in 2010 USD PPP. A transfer is considered targeted to women (men) if the UCT is explicitely delivered to women (men) or if greater than 80% of the sample is composed of women (men). When there are at least four estimates from programs targeted to men, we conduct our analysis on all three sub-sets: Not Targeted, Targeted to Women, and Targeted to Men. When there are fewer than four estimates from programs targeted to men, we instead conduct our analysis on two sub-sets: Not Targeted to Women and Targeted to Women. We do not present results on total hours worked or stunting due to data limitations. Our dataset for Monthly Household Consumption uses treatment effects on total consumption when reported; we use treatment effects on non-durable consumption or food consumption when total consumption is unavailable. Our analysis controls for whether food and durable goods are included in total consumption. Our dataset for Monthly Income uses reported treatment effects on total household or individual income when reported; if treatment effects are only reported by sub-category of income, e.g., wage earnings, non-farm enterprise profits, etc., then the sub-category with the highest control group mean is used instead. See Table 4 for a comparison to analysis that only uses reported estimates on total household or individual income. Effects with seven or fewer estimates estimates in gray. 95% credibility intervals in parentheses.

Table 10
Heterogeneous Treatment Effects by Framing Related to Child Development or Food Security

	(1)	(2)	(3)	(4)	(5)	(6)
	Predicted Treatment Effect of $100 Transfer		*Estimates (Programs)*		*Model Fit*	
	No Framing	With Framing	No Framing	With Framing	R^2	Pooling Factor
Panel A. Treatment Effect per Total Transfer Amount						
Flow outcomes						
Monthly Household Consumption	2.6 (1.7, 3.4)	5.0 (3.7, 6.3)	64 (34)	18 (11)	0.30	0.56
Monthly Household Food Consumption	1.5 (0.8, 2.1)	3.8 (2.7, 4.9)	33 (22)	16 (9)	0.27	0.38
Monthly Income	1.3 (0.8, 1.7)	2.8 (1.6, 4.2)	76 (33)	12 (5)	0.12	0.54
Hours Worked per Week	0.1 (-0.03, 0.25)	-0.8 (-1.5, -0.1)	24 (12)	1 (1)	0.20	0.32
Labor Force Participation (percentage points)	1.0 (0.4, 1.7)	0.7 (0.1, 1.3)	9 (6)	8 (5)	-0.01	0.37
School Enrollment (percentage points)	0.8 (0.1, 1.6)	1.1 (0.4, 1.7)	12 (6)	14 (10)	-0.02	0.28
Food Security z-Score	0.03 (0.02, 0.04)	0.04 (0.03, 0.06)	34 (18)	13 (7)	0.03	0.35
Psychological Well-being z-Score	0.03 (0.01, 0.04)	0.08 (0.05, 0.11)	44 (23)	12 (7)	0.13	0.18
Stock outcomes						
Stock of Total Assets	20.2 (13.0, 27.6)	7.6 (-24.1, 39.9)	54 (25)	6 (3)	0.03	0.33
Stock of Financial Assets	1.8 (1.2, 2.5)	2.1 (0.01, 4.34)	41 (20)	8 (4)	0.04	0.57
Height-for-Age z-Score	0.01 (0.001, 0.018)	0.01 (-0.002, 0.015)	16 (8)	16 (10)	0.02	0.68
Weight-for-Age z-Score	0.01 (-0.003, 0.013)	0.01 (-0.003, 0.021)	8 (4)	7 (6)	0.14	0.85
Panel B. Treatment Effect per Monthly Tranche Amount						
Flow outcomes						
Monthly Household Consumption	43.7 (30.6, 57.5)	102.5 (80.7, 124.7)	64 (34)	18 (11)	0.49	0.60
Monthly Household Food Consumption	22.7 (13.3, 33.4)	82.0 (64.4, 100.2)	33 (22)	16 (9)	0.52	0.43
Monthly Income	17.8 (11.3, 25.0)	77.3 (51.0, 104.2)	76 (33)	12 (5)	0.44	0.57
Hours Worked per Week	0.1 (-0.5, 0.8)	-0.7 (-3.9, 2.4)	24 (12)	1 (1)	0.01	0.31
Labor Force Participation (percentage points)	12.4 (-1.0, 25.9)	19.9 (4.8, 35.4)	9 (6)	8 (5)	-0.03	0.24
School Enrollment (percentage points)	12.8 (1.2, 25.4)	15.2 (4.9, 25.9)	12 (6)	14 (10)	-0.03	0.32
Food Security z-Score	0.5 (0.3, 0.7)	1.2 (0.8, 1.6)	34 (18)	13 (7)	0.21	0.31
Psychological Well-being z-Score	0.30 (0.1, 0.5)	1.26 (0.8, 1.7)	44 (23)	12 (7)	0.19	0.15

All currency values are reported in 2010 USD PPP. Our dataset for Monthly Household Consumption uses treatment effects on total consumption when reported; we use treatment effects on non-durable consumption or food consumption when total consumption is unavailable. Our analysis controls for whether food and durable goods are included in total consumption. Our dataset for Monthly Income uses reported treatment effects on total household or individual income when reported; if treatment effects are only reported by sub-category of income, e.g., wage earnings, non-farm enterprise profits, etc., then the sub-category with the highest control group mean is used instead. See Table 4 for a comparison to analysis that only uses reported estimates on total household or individual income. We do not present results on Stunting due to data limitations. Effects with seven or fewer estimates in gray. 95% credibility intervals in parentheses.

Table 11
Heterogeneous Treatment Effects on Primary Outcomes by Rural Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Predicted Treatment Effect of $100 Transfer		Estimates *(Programs)*							
			By Disbursement Schedule						By Non-Rural Sub-Classification	
	Rural	Non-Rural *(Urban, Mixed and Unspecified)*	Rural			Non-Rural *(Urban, Mixed and Unspecified)*				
			Stream Ongoing	Stream Ended	Lump Sum	Stream Ongoing	Stream Ended	Lump Sum	Urban	Mixed and Unspecified
Panel A. Treatment Effect per Total Transfer Amount										
Flow outcomes										
Monthly Household Consumption	2.7 (2.1, 3.4)	3.0 (1.5, 4.5)	23 (16)	12 (5)	31 (19)	4 (4)	2 (2)	10 (6)	8 (6)	8 (4)
Monthly Household Food Consumption	2.2 (1.5, 2.9)	2.5 (0.9, 4.3)	19 (12)	5 (3)	16 (11)	3 (3)	1 (1)	5 (4)	7 (5)	2 (2)
Monthly Income	1.2 (0.7, 1.6)	2.8 (1.8, 3.8)	10 (6)	11 (5)	30 (16)	1 (1)	2 (1)	34 (13)	31 (10)	6 (5)
Hours Worked per Week	0.01 (0.003, 0.009)	0.01 (-0.001, 0.018)	2 (1)	7 (4)	10 (4)	1 (1)	2 (1)	3 (3)	4 (3)	2 (2)
Labor Force Participation (percentage points)	7.7 (-9.4, 24.2)	9.7 (-26.5, 47.4)	6 (5)	4 (1)	3 (2)			4 (3)	2 (1)	2 (2)
School Enrollment (percentage points)	1.1 (0.5, 1.8)	0.7 (-0.2, 1.5)	9 (6)	4 (2)	4 (2)	6 (4)	1 (1)	2 (2)	1 (1)	8 (5)
Food Security z-Score	0.03 (0.02, 0.04)	0.1 (0.03, 0.08)	13 (8)	12 (6)	17 (11)	1 (1)	1 (1)	3 (3)	3 (2)	2 (2)
Psychological Well-being z-Score	0.04 (0.02, 0.05)	0.03 (-0.0003, 0.0617)	11 (6)	13 (6)	21 (12)	5 (4)	1 (1)	5 (4)	3 (2)	8 (6)
Stock outcomes										
Stock of Total Assets	20.7 (13.4, 28.2)	8.1 (-15.9, 32.0)	7 (5)	10 (4)	31 (19)		2 (1)	10 (4)	7 (3)	5 (2)
Stock of Financial Assets	1.9 (1.3, 2.6)	0.4 (-2.1, 2.9)	6 (4)	8 (4)	20 (11)		2 (1)	13 (6)	11 (6)	4 (1)
Height-for-Age z-Score	0.01 (0.003, 0.015)	-0.001 (-0.02, 0.02)	19 (12)	7 (5)	3 (1)	2 (2)	1 (1)		1 (1)	2 (2)
Weight-for-Age z-Score	0.004 (-0.002, 0.010)	0.02 (0.01, 0.04)	7 (6)	4 (3)	3 (1)	1 (1)			1 (1)	
Panel B. Treatment Effect per Monthly Tranche Amount										
Flow outcomes										
Monthly Household Consumption	51.5 (40.5, 63.5)	33.7 (10.9, 56.5)	23 (16)	12 (5)	31 (19)	4 (4)	2 (2)	10 (6)	8 (6)	8 (4)
Monthly Household Food Consumption	45.8 (33.0, 59.6)	30.3 (3.5, 57.8)	19 (12)	5 (3)	16 (11)	3 (3)	1 (1)	5 (4)	7 (5)	2 (2)
Monthly Income	21.1 (13.0, 30.1)	28.2 (13.9, 43.2)	10 (6)	11 (5)	30 (16)	1 (1)	2 (1)	34 (13)	31 (10)	6 (5)
Hours Worked per Week	0.2 (0.1, 0.3)	0.1 (-0.2, 0.3)	2 (1)	7 (4)	10 (4)	1 (1)	2 (1)	3 (3)	4 (3)	2 (2)
Labor Force Participation (percentage points)	7.2 (-65.4, 79.3)	8.5 (-120.0, 136.7)	6 (5)	4 (1)	3 (2)			4 (3)	2 (1)	2 (2)
School Enrollment (percentage points)	18.6 (8.9, 29.2)	7.0 (-5.6, 19.6)	9 (6)	4 (2)	4 (2)	6 (4)	1 (1)	2 (2)	1 (1)	8 (5)
Food Security z-Score	0.6 (0.4, 0.8)	0.9 (0.3, 1.4)	13 (8)	12 (6)	17 (11)	1 (1)	1 (1)	3 (3)	3 (2)	2 (2)
Psychological Well-being z-Score	0.53 (0.3, 0.8)	0.29 (-0.2, 0.8)	11 (6)	13 (6)	21 (12)	5 (4)	1 (1)	5 (4)	3 (2)	8 (6)

All currency values are reported in 2010 USD PPP. Effects with 7 or fewer estimates in gray. 95% credibility intervals in parentheses.

Table 12
Heterogeneity by Poverty/GDP per Capita

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Heterogeneity by Poverty Rate *Decreasing Poverty Rate →*			Heterogeneity by GDP per capita *Increasing GDP per capita →*			Estimates *(Programs)*
	80th percentile (61%)	Median (42%)	20th percentile (28%)	20th percentile (*PPP $* 1,458)	Median (*PPP $* 2,188)	80th percentile (*PPP $* 4,052)	
Panel A: Predicted Treatment Effect per $100 of Total Transfer Amount at Specific Percentiles in Poverty Rate (Col 1-3) or GDP per capita (Col 4-6)							
Flow outcomes							
Monthly Household Consumption	2.6 (1.9, 3.3)	2.8 (2.2, 3.4)	2.9 (2.3, 3.7)	2.8 (2.2, 3.4)	2.7 (2.1, 3.4)	2.7 (2.1, 3.4)	82 (45)
Monthly Household Food Consumption	1.9 (1.1, 2.8)	2.3 (1.6, 2.9)	2.4 (1.7, 3.2)	2.3 (1.7, 3.0)	2.2 (1.5, 2.9)	2.1 (1.4, 2.8)	49 (31)
Monthly Income	1.6 (1.1, 2.1)	1.5 (1.0, 1.9)	1.2 (0.6, 1.7)	1.7 (1.2, 2.2)	1.4 (0.9, 1.8)	1.2 (0.7, 1.7)	88 (38)
Hours Worked per Week	0.01 (0.003, 0.012)	0.01 (0.004, 0.013)	0.01 (0.005, 0.016)	0.1 (-0.1, 0.3)	0.1 (-0.1, 0.2)	0.1 (-0.1, 0.2)	25 (13)
Labor Force Participation (percentage points)	0.7 (0.3, 1.2)	0.8 (0.4, 1.3)	1.0 (0.5, 1.6)	0.8 (0.4, 1.3)	0.8 (0.4, 1.3)	0.6 (0.1, 1.2)	17 (11)
School Enrollment (percentage points)	0.9 (0.1, 1.7)	0.9 (0.4, 1.5)	1.0 (0.2, 1.8)	0.9 (0.4, 1.5)	0.9 (0.4, 1.5)	0.9 (0.4, 1.6)	26 (16)
Food Security z-Score	0.03 (0.02, 0.05)	0.03 (0.02, 0.04)	0.03 (0.02, 0.04)	0.03 (0.02, 0.04)	0.03 (0.02, 0.04)	0.03 (0.02, 0.04)	47 (25)
Psychological Well-being z-Score	0.05 (0.03, 0.07)	0.03 (0.02, 0.05)	0.02 (0.004, 0.043)	0.03 (0.01, 0.05)	0.04 (0.03, 0.05)	0.04 (0.03, 0.06)	56 (30)
Stock outcomes							
Stock of Total Assets	14.7 (7.3, 22.4)	20.6 (13.9, 27.5)	26.9 (18.2, 35.9)	21.2 (13.6, 29.1)	18.7 (11.6, 26.2)	17.7 (9.9, 25.8)	60 (28)
Stock of Financial Assets	1.1 (0.4, 1.8)	2.1 (1.6, 2.7)	2.6 (1.9, 3.4)	2.4 (1.9, 2.9)	1.6 (1.1, 2.0)	1.0 (0.4, 1.5)	49 (24)
Height-for-Age z-Score	0.02 (0.01, 0.03)	0.01 (0.003, 0.016)	0.003 (-0.004, 0.010)	0.01 (0.003, 0.016)	0.01 (0.002, 0.013)	0.004 (-0.002, 0.013)	32 (18)
Weight-for-Age z-Score	0.01 (-0.002, 0.016)	0.01 (-0.001, 0.013)	0.01 (-0.01, 0.02)	0.01 (-0.003, 0.014)	0.01 (-0.001, 0.014)	0.01 (-0.001, 0.014)	15 (10)
Panel B: Predicted Treatment Effect per $100 of Monthly Tranche Amount at Specific Percentiles in Poverty Rate (Col 1-3) or GDP per capita (Col 4-6)							
Flow outcomes							
Monthly Household Consumption	47.9 (35.8, 60.9)	48.3 (38.5, 59.0)	48.6 (37.5, 60.4)	47.9 (37.8, 58.8)	48.9 (38.5, 60.1)	49.2 (38.1, 61.1)	82 (45)
Monthly Household Food Consumption	43.4 (27.0, 61.0)	43.1 (31.5, 55.7)	43.0 (30.0, 56.9)	43.6 (31.7, 56.6)	42.6 (30.7, 55.3)	41.9 (29.0, 55.8)	49 (31)
Monthly Income	27.6 (19.5, 36.4)	23.9 (16.9, 31.7)	15.6 (7.4, 24.7)	26.5 (18.1, 35.5)	21.7 (14.5, 29.6)	19.0 (10.9, 27.8)	88 (38)
Hours Worked per Week	0.1 (0.03, 0.27)	0.2 (0.1, 0.3)	0.2 (0.05, 0.30)	0.1 (-0.7, 1.0)	0.1 (-0.8, 0.9)	0.1 (-0.8, 1.0)	25 (13)
Labor Force Participation (percentage points)	14.7 (3.3, 26.5)	15.5 (5.6, 25.8)	16.9 (4.5, 29.8)	15.7 (5.6, 26.1)	15.7 (5.5, 26.1)	15.3 (1.8, 29.0)	17 (11)
School Enrollment (percentage points)	12.5 (1.0, 25.1)	14.2 (6.6, 22.5)	15.8 (3.9, 27.9)	14.2 (6.5, 22.5)	14.6 (6.0, 23.9)	14.6 (5.6, 24.5)	26 (16)
Food Security z-Score	0.8 (0.5, 1.1)	0.6 (0.4, 0.8)	0.6 (0.4, 0.8)	0.6 (0.4, 0.8)	0.6 (0.4, 0.8)	0.6 (0.4, 0.9)	47 (25)
Psychological Well-being z-Score	0.70 (0.4, 1.0)	0.44 (0.2, 0.7)	0.28 (-0.03, 0.58)	0.39 (0.1, 0.6)	0.54 (0.3, 0.8)	0.60 (0.3, 0.9)	56 (30)

95% credibility intervals in parentheses. All currency values are reported in 2010 USD PPP. The poverty rate is defined as the percentage of the population living on less than *PPP$*2.15 per day. Poverty and GDP per capita data were obtained from the World Bank and matched to the closest available year. Percentiles are calculated at the study level (i.e., one observation from each of the 72 programs).

Table 13
Benefit-Cost Ratios of UCT Programs

	(1)	(2)	(3)
		Benefit-Cost Ratio	
	NPV Predicted Effect of *PPP$*100 Transfer	No Administrative Costs	Median Administrative Costs (24%)
Treatment Effect per Total Transfer Amount			
Lump Sum	101.5 (20.2, 185.6)	1.0 (0.2, 1.9)	0.8 (0.2, 1.5)
12-Month Stream Program	174.2 (62.1, 288.7)	1.8 (0.6, 3.0)	1.4 (0.5, 2.4)
24-Month Stream Program	180.8 (90.9, 274.0)	1.9 (1.0, 2.9)	1.5 (0.8, 2.3)
36-Month Stream Program	177.4 (97.3, 260.4)	1.9 (1.0, 2.8)	1.5 (0.8, 2.3)
48-Month Stream Program	144.3 (40.1, 252.1)	1.6 (0.4, 2.8)	1.3 (0.4, 2.2)

Benefits are calculated as expected treatment effects in consumption for a given value of total transfer amount and months since first transfer for ongoing stream programs, and months since last transfer for completed stream and lump sum programs, using the model in (5). 95% credibility intervals in parentheses.

Table 14
Model Predictive Performance by Outcome

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	$\lvert\Delta\mathbb{E}(\log\mathcal{L})\rvert$			$\mathbb{P}\left(\{\mathcal{L}_R(\widehat{TE},\widehat{TE}_R,w_{p,R})-\mathcal{L}_k(\widehat{TE},\widehat{TE}_k,w_{p,k})<0\}\right)$								
	Symmetric	Skewed	Mixture	Symmetric			Skewed			Mixture		
				$\Delta\mathcal{L}_1$	Normal Probability	Bayesian Bootstrapping	$\Delta\mathcal{L}_1$	Normal Probability	Bayesian Bootstrapping	$\Delta\mathcal{L}_1$	Normal Probability	Bayesian Bootstrapping
Flow outcomes												
Monthly Household Consumption	1.1 [1.3, 2.6]	*Reference Model*	0.9 [0.6, 1.3]	-0.01 [0.003]	1.0	1.0		*Reference Model*		-0.002 [0.002]	0.9	0.9
Monthly Household Food Consumption	1.5 [3.9, 7.8]	2.1 [3.5, 7.0]	*Reference Model*	0.01 [0.01]	0.1	0.1	0.01 [0.01]	0.2	0.2		*Reference Model*	
Monthly Income	9.5 [5.0, 10.0]	*Reference Model*	–	0.003 [0.004]	0.3	0.3		*Reference Model*			–	
Hours Worked per Week	*Reference Model*	2.5 [2.4, 4.9]	–		*Reference Model*		-0.9 [3.2]	0.6	0.6		–	
Labor Force Participation	5.2 [4.6, 9.3]	1.7 [2.4, 4.8]	*Reference Model*	0.2 [0.2]	0.1	0.1	0.1 [0.1]	0.2	0.2		*Reference Model*	
School Enrollment	1.8 [2.1, 4.2]	*Reference Model*	1.6 [1.2, 2.3]	0.001 [0.05]	0.5	0.5		*Reference Model*		-0.1 [0.1]	0.8	0.8
Food Security z-Score	1.5 [1.2, 2.4]	*Reference Model*	5.4 [3.2, 6.5]	0.1 [0.1]	0.1	0.0		*Reference Model*		-0.5 [0.4]	0.9	0.9
Psychological Well-being z-Score	13.7 [14.7, 29.4]	*Reference Model*	1.6 [5.4, 10.8]	0.4 [0.3]	0.1	0.1		*Reference Model*		-0.2 [0.3]	0.8	0.8
Stock outcomes												
Stock of Total Assets	3.6 [2.3, 4.6]	*Reference Model*	–	0.01 [0.004]	0.1	0.1		*Reference Model*			–	
Stock of Financial Assets	1.8 [1.2, 2.5]	*Reference Model*	–	-0.0001 [0.0004]	0.6	0.6		*Reference Model*			–	
Height-for-Age z-Score	1.7 [0.7, 1.3]	*Reference Model*	–	-0.003 [0.002]	0.9	0.9		*Reference Model*			–	
Weight-for-Age z-Score	0.2 [0.1, 0.2]	*Reference Model*	–	-0.0003 [0.002]	0.5	0.5		*Reference Model*			–	

Columns (1)-(3) report the absolute value of the difference in expected point-wise log-likelihood between the model in the column and the reference model, i.e. the best performing model. Underneath each value, we report in square brackets the two critical thresholds, as detailed in Model Extensions. A value smaller than the lower bound is evidence of no difference in predictive performance, while a value greater than the upper bound is evidence of non-negligible difference. A value in-between the two thresholds does not provide strong evidence against any of the two previous scenarios. Columns (4), (7) and (10) report the difference in mean absolute prediction error (i.e. $\mathcal{L}_1$-loss) between the reference model and the model in the column with their corresponding standard error in square brackets. A negative value means that the reference model has lower $\mathcal{L}_1$-loss, while a positive value means that the reference model has higher $\mathcal{L}_1$-loss. Columns (5), (6), (8), (9), (11) and (12) report the probability that the reference model has a lower $\mathcal{L}_1$-loss than the model in the column. We compute these probabilities using both a normal approximation of the difference in expected point-wise log-likelihood and a Bayesian bootstrap procedure, as detailed in Model Extensions. For certain outcomes, there is evidence that the mixture model was not performing properly (i.e. low effective sample size and $\widehat{R}$ (Vehtari, Gelman, Simpson, et al. 2021)), hence we excluded it from the LOO-CV procedure.

Table 15
Model Comparison: Average Treatment Effects on Primary Outcomes

	(1)	(2)	(3)	(4)	(5)
	Predicted Treatment Effect of $100 Transfer				
	Symmetric Model (Repeats of Table 3)	Skewed Model			Estimates (Programs)
		Mean	Median	Mode	
Panel A. Treatment Effects per Monthly Tranche Amount					
Flow outcomes					
Monthly Household Consumption (with controls)	59.2 (44.8, 74.6)	51.2 (40.4, 63.3)	44.0 (34.1, 54.8)	31.5 (22.8, 40.6)	82 (45)
Monthly Household Food Consumption	42.7 (31.4, 55.0)	43.8 (32.0, 57.2)	36.8 (26.1, 48.8)	24.6 (15.2, 34.5)	49 (31)
Monthly Income	22.7 (15.4, 30.7)	23.4 (16.6, 31.1)	18.9 (12.7, 25.8)	11.0 (5.5, 16.8)	88 (38)
Labor Force Participation (percentage points)	15.6 (6.1, 25.6)	15.8 (8.5, 24.6)	12.9 (5.9, 20.7)	7.7 (0.9, 14.4)	17 (11)
School Enrollment (percentage points)	14.0 (6.5, 22.2)	14.6 (7.7, 22.9)	11.4 (5.0, 18.8)	5.9 (-0.1, 12.1)	26 (16)
Food Security z-Score	0.6 (0.4, 0.8)	0.7 (0.5, 0.8)	0.6 (0.4, 0.7)	0.4 (0.2, 0.5)	47 (25)
Psychological Well-being z-Score	0.5 (0.3, 0.7)	0.5 (0.4, 0.7)	0.4 (0.3, 0.6)	0.2 (0.1, 0.4)	56 (30)
Panel B. Treatment Effects per Total Transfer Amount					
Stock outcomes					
Stock of Total Assets	19.6 (12.6, 26.7)	18.0 (12.9, 23.8)	14.6 (9.8, 19.8)	8.6 (4.2, 13.0)	60 (28)
Stock of Financial Assets	1.8 (1.2, 2.5)	1.9 (1.3, 2.6)	1.6 (1.1, 2.2)	1.1 (0.6, 1.6)	49 (24)
Height-for-Age z-Score	0.01 (0.002, 0.014)	0.01 (0.003, 0.016)	0.01 (0.001, 0.013)	0.003 (-0.002, 0.008)	32 (18)
Weight-for-Age z-Score	0.01 (-0.0001, 0.0126)	0.01 (0.001, 0.014)	0.01 (-0.0004, 0.0120)	0.003 (-0.003, 0.009)	15 (10)

Column (1) reports estimates from Table 3, while columns (2)-(4) report estimates from the skewed model in (11). In particular, column (2) reports the posterior estimate of the mean of the Gumbel distribution defined as $\mathbb{E}(\theta_n) := \beta + \sigma_\theta \gamma$, where γ is the Euler-Mascheroni constant, column (3) reports the posterior estimate of the median, defined as $\mathbb{Me}(\theta_n) := \beta - \sigma_\theta ln(ln(2))$ and column (4) reports the estimate of the mode, which is β. All currency values are reported in 2010 USD PPP. 95% credibility intervals in parentheses.

FIGURE 1: PRISMA DIAGRAM

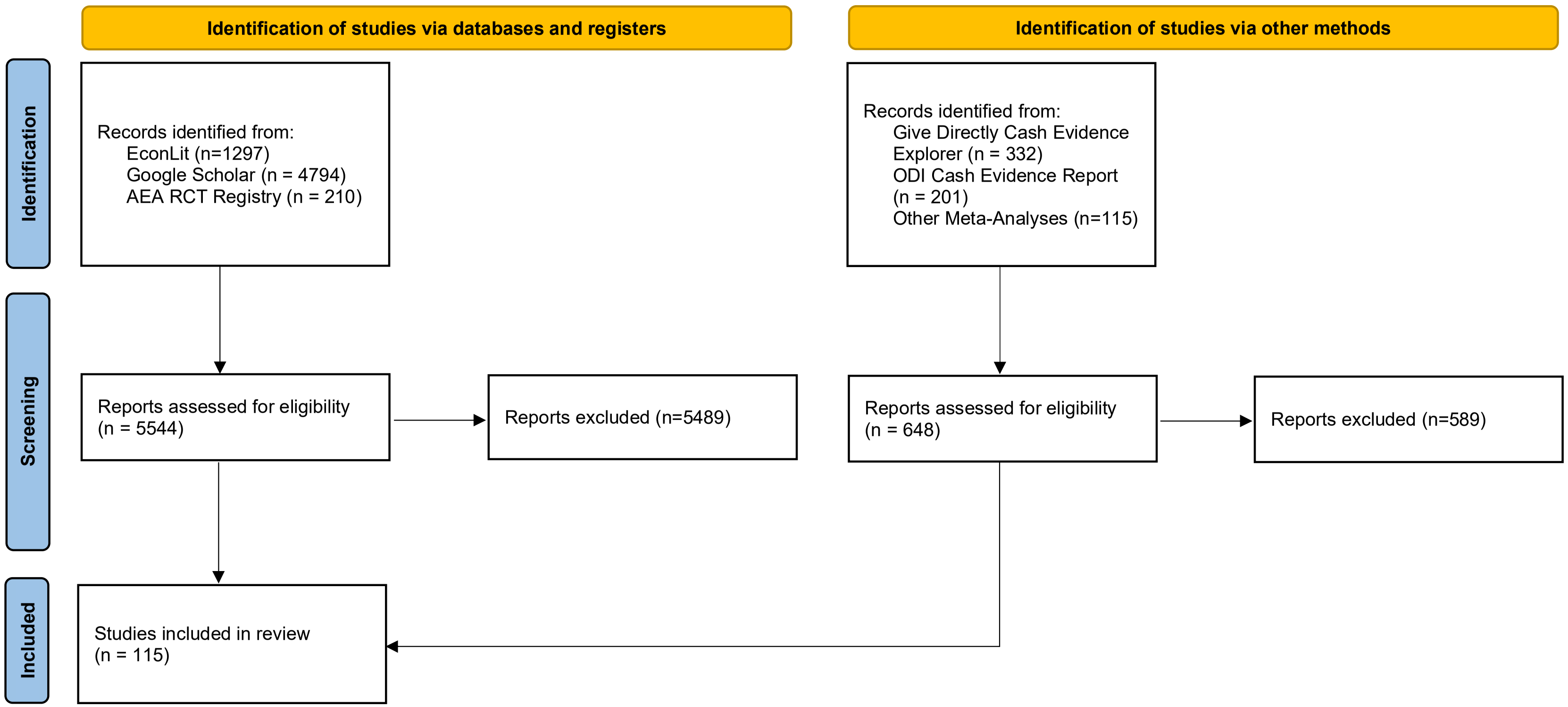


Figure 2: Histograms of Months Since First Transfer by Outcome for Lump Sums and Streams

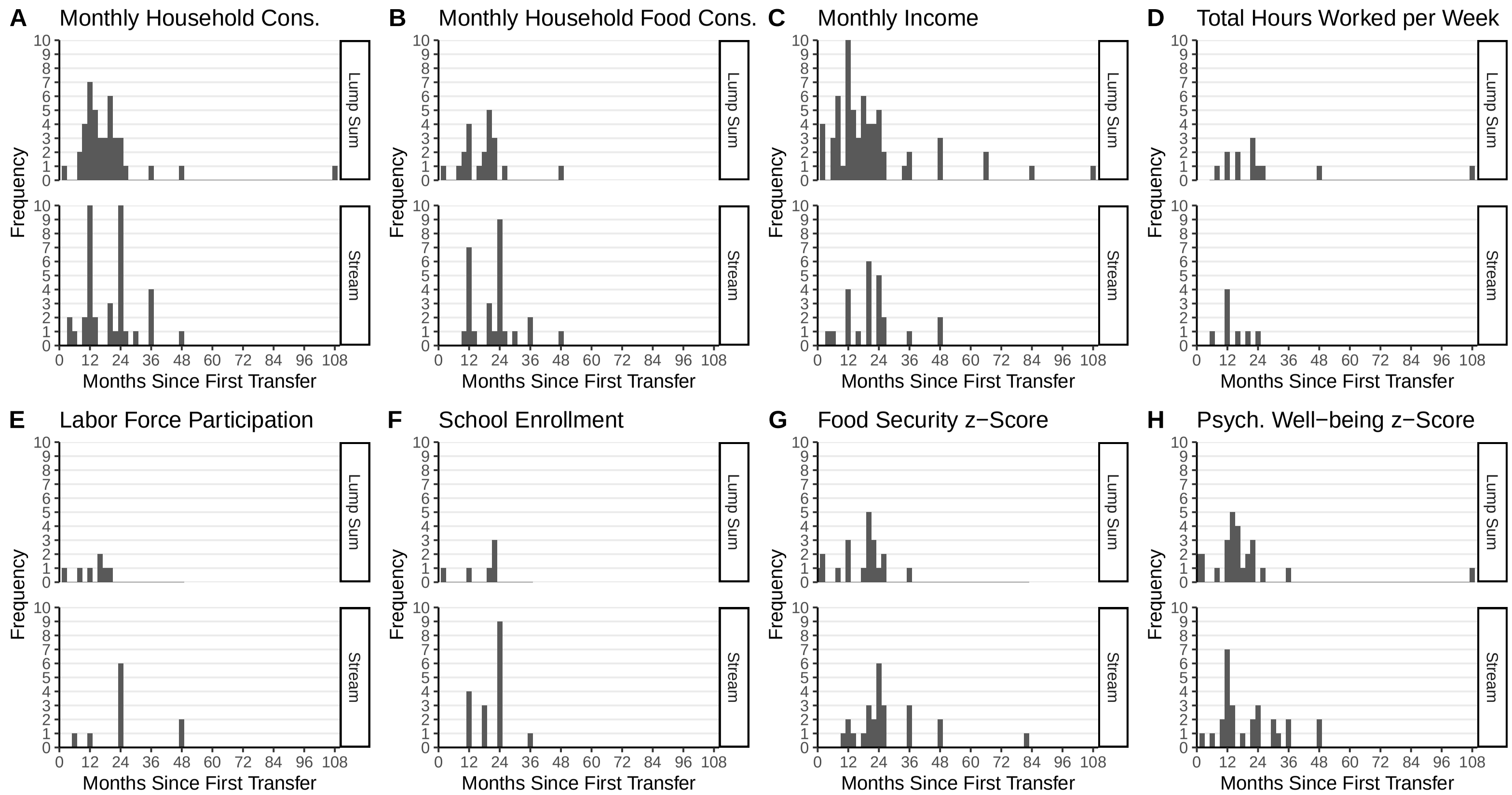


Figure 2 (cont.): Histograms of Months Since First UCT by Outcome for Lump Sums and Streams

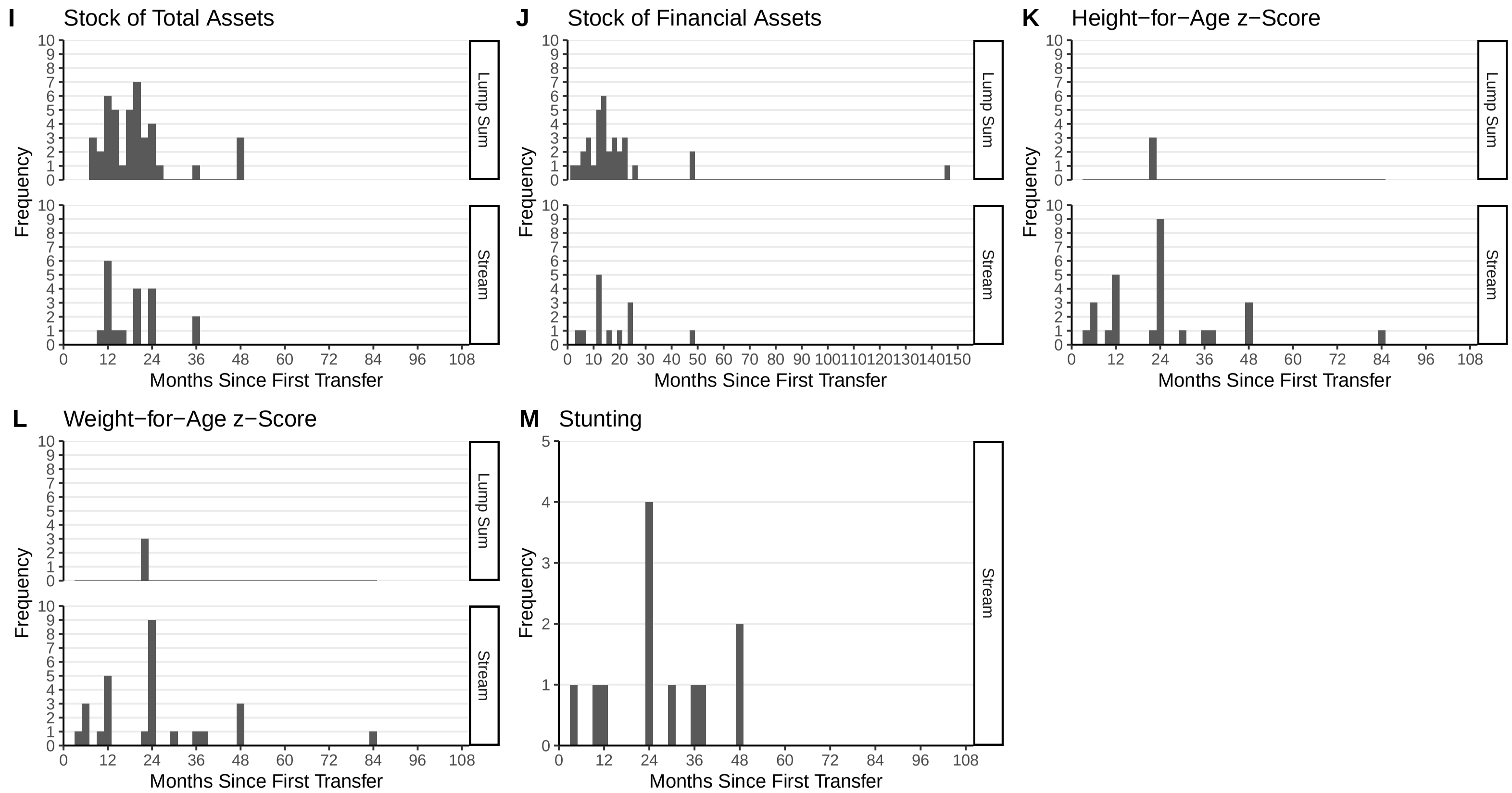


Figure 3.1: Posterior Average Treatment Effects on Total Consumption Sorted by Months Since First Transfer

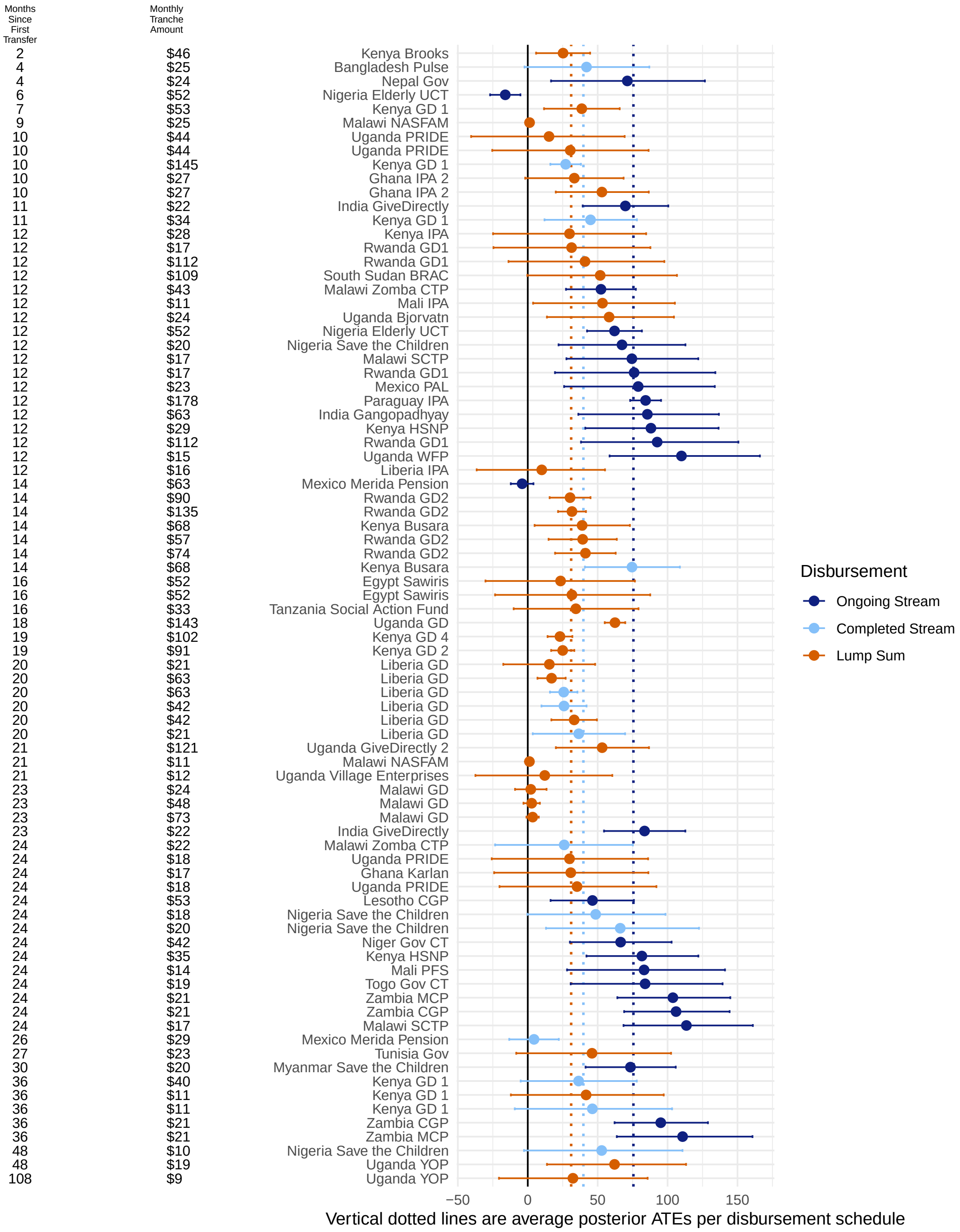


Figure 3.2: Posterior Average Treatment Effects on Total Consumption Sorted by Monthly Tranche Amount

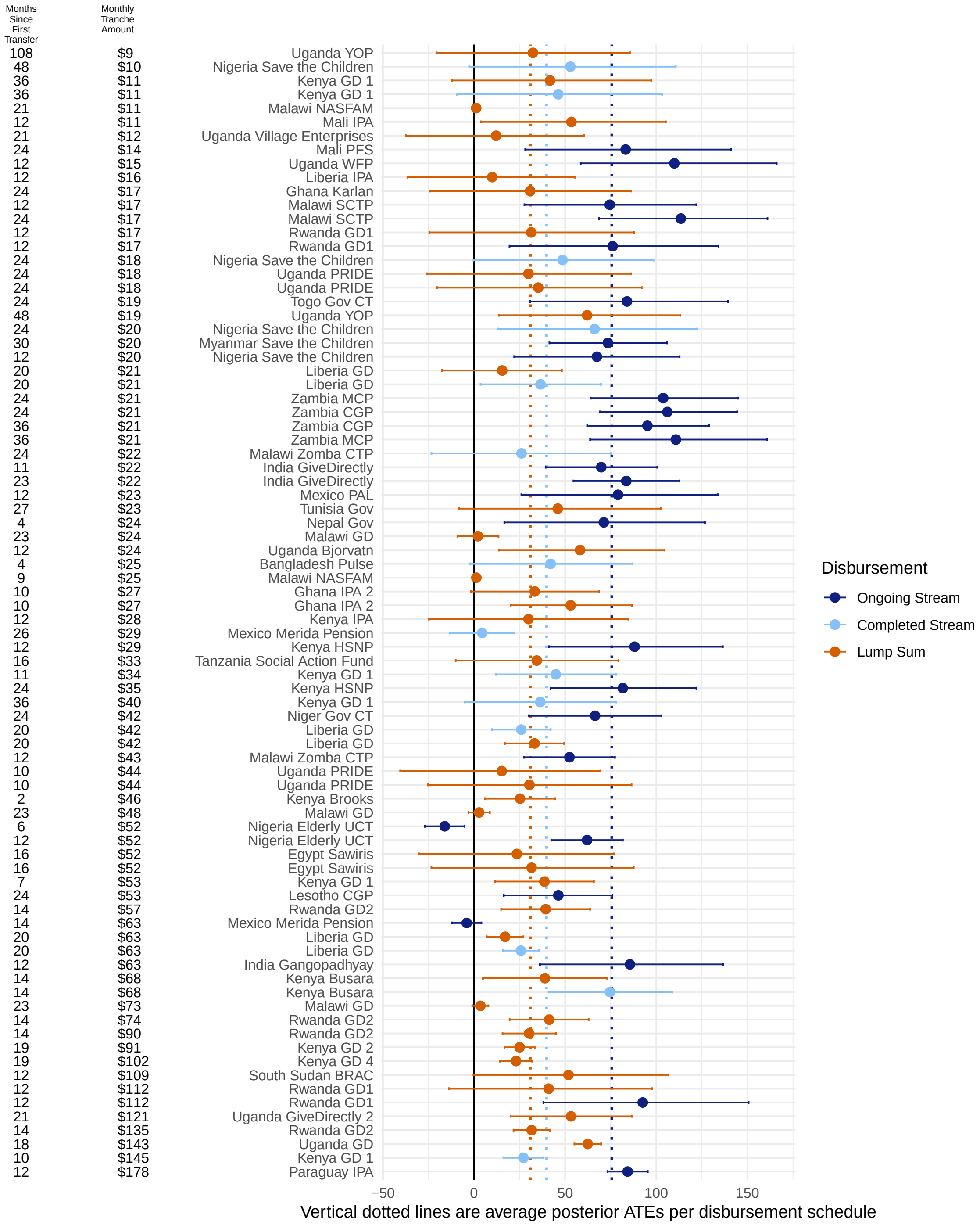
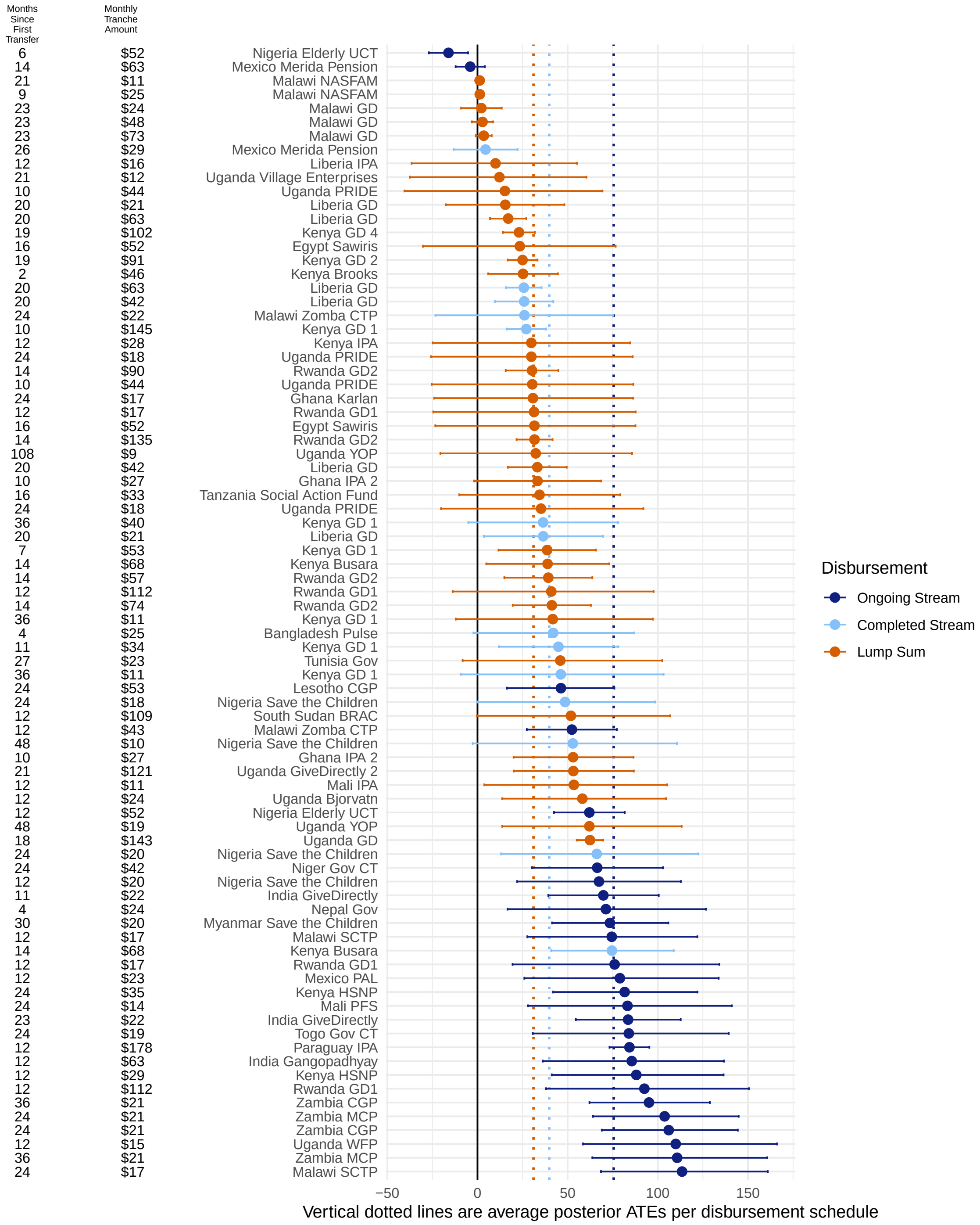


Figure 3.3: Posterior Average Treatment Effects on Total Consumption Sorted by Effect Size



Appendix

Study search

We develop a initial sample by collecting studies from two secondary sources: the GiveDirectly Cash Evidence Explorer and the Overseas Development Institute's 2016 report "Cash transfers: what does the evidence say?" (*Cash Evidence Explorer* 2023; Bastagli et al. 2016). We also use the publicly available data from three existing meta-analyses on cash transfers: Kondylis and Loeser 2021; Manley, Alderman, et al. 2022, and McGuire et al. 2022. From these sources, we identify 47 studies.

After building this initial sample, we conduct searches on Google Scholar, EconLit, and the AEA RCT Registry with the following search terms:

Database	Search terms	Search settings	Number of results
Google Scholar	(randomized, OR evaluation, OR experiment) AND unconditional AND ("cash transfer", OR "cash grant"), ("randomized control trial" OR "randomized controlled trial" OR "randomized experiment") AND unconditional AND ("cash transfer" OR "cash grant" OR "non-contributory pensions")	n/a	4,797
EconLit	(unconditional AND cash) OR "cash grant" OR "capital grant" OR "cash transfer"	Apply related words, also search with the full text of the articles, apply equivalent subjects	1,297
AEA RCT Registry	"cash grant" OR "cash transfer"	Search within abstract	210

Data selection and harmonization

This section outlines how we extract estimates from the papers in our sample and then convert them to as comparable units as possible before running our Bayesian meta-analysis.

Regression specification:

We apply the following set of rules to decide which treatment effects to extract from papers:

1. Sometimes papers pool results across different UCT treatment arms (that vary either by disbursement schedule or transfer amount). When multiple regression specifications are reported, we prefer estimates with more disaggregation by treatment arm.
2. When impacts are measured across multiple rounds of data collection, we prefer estimates from regressions with more disaggregated effects by survey round.
3. Except for the two rules above, we prefer estimates from the simplest regression specification (i.e., the regression specification that is closest to a simple mean comparison). In practice, this means:
   (a) We prefer estimates from regressions with fewer controls (except for treatment arm indicators, survey round indicators, and stratification indicators).
   (b) We prefer estimates from regressions on untransformed outcome variables over log, inverse hyperbolix sine, or other transformations.
4. When both intent-to-treat (ITT) and treatment-on-the-treated (TOT) impacts are reported, we prefer ITT estimates.[31]
5. We exclude treatment effects reported as odds ratios.

[31] No TOT effects are included in our analysis.

Outcome selection

Consumption: We extract treatment effect estimates on total consumption. If total household consumption is not reported, we extract the reported category of consumption with the largest control group mean, typically non-durable or food consumption; this results in the use of food consumption as the substitute for total consumption in 7 estimates. Estimates on food consumption are also extracted as a primary outcome.

Food security: If a paper reports multiple outcomes on food security, we select only one outcome for inclusion in our analysis. We prioritize outcome selection in the following order: international food security scores and indexes (e.g., HFIAS, HHS, etc.), paper-specific food security indexes, hunger indicators, and finally meal frequency indicators.

Stock of total assets: When total assets are not reported, we extract the reported category of assets with the largest control group mean; this results in the use of productive assets as the substitute for total assets in 21 estimates. Productive assets and durable assets are also extracted as secondary outcomes.

Stock of financial assets: Refers to the stock of financial savings of the household. The vast majority of studies restrict this outcome to liquid savings, measuring savings in both formal and informal institutions.

Monthly Income: When total income is not reported but some sub-category of total income (e.g., wage earnings, business profits, etc.) is reported, we use the sub-category with the largest control group mean as the preferred treatment effect for total income. Wage earnings, non-farm enterprise profits, agricultural enterprise profits, and all household enterprise profits are also extracted as secondary outcomes.

Hours worked per week: We extract estimates on the the number of hours worked in *any* income generating activity per a unit of time, typically a week.

Labor force participation: We extract treatment effects on binary variables of whether the respondent participated in *any* economic activity over a given period of time, typically a month. In other words, we present estimates on whether participants engaged in *any* income-generating activity, whether self-employment or working for wage, salary, or commission. For Panel B, in Table 4, we relax this criterion and present estimates of labor force participation in non-wage employment (i.e., non-farm self-employment and farm self-employment) and wage employment.

School enrollment: We extract treatment effects on binary variables on whether the survey respondent (or their child) is enrolled in school. If such a variable is unavailable, we instead use estimates on the proportion of children in the household enrolled in school.

Anthropometrics: We extract treatment effects on height-for-age and weight-for-age z-scores as well as stunting. Stunting is not reported enough for much of our analysis, but we do report the main results for average treatment effects (i.e., not disaggregated by distribution type or other design features).

Psychological well-being: If a paper reports multiple outcomes on psychological well-being, we select only one outcome for inclusion in our analysis. We prioritize outcome selection in the following order: standard psychological well-being scores or indexes (e.g., GHQ-12, WVS Life Satisfaction Scale, WHO Quality of Life Scale, etc.), standard mental health/depression scores or indexes (e.g., CES-D, PSS, GDS, etc.), paper-specific psychological well-being score or index, psychological well-being indicators, and mental health/depression indicators.

Data harmonization

Monetary units conversion: We convert all monetary units to 2010 USD PPP using the following rules:

1. If an amount is reported in USD PPP, we simply convert it to 2010 price levels

using USD inflation.

2. If an amount is reported in local currency units (LCU), we convert it to USD PPP using the contemporary World Bank PPP Conversion Factor (PPP CF) and then to 2010 price levels using USD inflation.

3. If an amount is reported in nominal USD, we convert it to LCU using the contemporary nominal USD exchange rate, then to USD PPP using the contemporary PPP CF, and finally to 2010 price levels using USD inflation.[32]

Unit transformations: Recall that we prioritize extracting estimated treatment effects from regressions on untransformed outcome variables. When estimates are only reported on transformed outcome variables, we use the following calculations to account for the transformation.

1. Percent change: We multiplied the estimate by the counterfactual mean (typically the control group mean at endline).

2. Inverse hyperbolic sine: Same as percent change.

3. Log: For an estimate β, we multiplied $(e^{\beta} - 1)$ by the control group mean.

Monthly household consumption conversions: Treatment effects on consumption vary widely in their reporting across papers. We convert all reported treatment effects to monthly household consumption using the following calculations.

1. If consumption is reported over 1 week or 2 weeks, we multiply the treatment effect by 4.3 or 2.15 respectively. If consumption is reported annually, we divide the treatment effect by 12.

[32] We do not follow this approach for the two programs in our sample that take place in Liberia, because the World Bank PPP Conversion Factor applies USD, which is legal tender in Liberia. We thus convert nominal USD directly to USD PPP before adjusting for USD inflation.

2. If consumption is reported on a per capita basis, we multiply the treatment effect by the average household size as reported in the balance table. If household size is not reported, we assume it is equal to 5.6 for the calculation, the mean household size in the sample.

3. If consumption is reported on a per adult equivalent basis, we multiply the treatment effect by the average number of adult equivalents per household. If this number is not reported, we use the household size as reported in the balance table to estimate the number of adult equivalents in the household. To make this calculation, we count the first member of the household as 1 adult equivalent, the second member of the household as 0.7 adult equivalents, and all subsequent household members as 0.5 adult equivalents. For example, we estimate a household of 5 to contain 3.2 adult equivalents. If household size is not reported, we assume there are 3.5 adult equivalents per household (i.e. we assume the household size is 5.6).

Food security standardization: We standardize all food security treatment effects by dividing by the control mean standard deviation if necessary. See Appendix Table B.1 for the unstandardized treatment effects.

Assets conversions: Total assets is stock, rather than flow variable, so no further conversion is necessary after converting to common monetary units. We do the same for secondary assets outcomes: productive assets, consumption assets, and financial savings.

Monthly income conversion: We convert all reported treatment effects on income to monthly income using the same methods as points 1 and 2 under Consumption Conversion. Note that unlike for consumption, we do not convert to the household level. Papers vary in their reporting of treatment effects on income at the individual or household level. Rather than trying to adjust for this discrepancy across papers, we assume researchers only measured income at the individual level if they had good reason to expect the impact of the treatment would be almost entirely at the individual, not household, level. We follow the same approach for sub-categories of income.

Hours worked per week conversion: If total hours worked is reported per month, we divide the treatment effect by 4.3.

Labor force participation conversion: We convert proportions to percentage points by multiply by 100, if necessary.

School enrollment conversion: We extract two types of education outcomes: a binary indicator of whether a given student is enrolled in school or continuous 0-1 variable of the proportion of children enrolled in school in a given household. We treat these different measures as equivalent. When necessary we convert proportions to percentage points by multiplying by 100.

Anthropometrics conversion: We extract treatment effects on height-for-age (HAZ) and weight-for-age z-scores (WAZ), which have equivalent units by construction. No conversion is necessary. Similarly, papers that report stunting use a standard definition. We merely scale from proportions to percentage point units when necessary.

Psychological well-being standardization: We standardize all psychological well-being treatment effects by dividing by the control group mean standard deviation if necessary. See Appendix Table B.2 for the unstandardized treatment effects.

Appendix Table A.1a
Program Characteristics

(1) Program ID	(2) Papers	(3) Country	(4) Program Purpose	(5) Implementer Type	(6) Program/Implementer Name	(7) Delivery Method	(8) Framing/Labeling	(9) Disbursement Schedule
1	Kashefi and Naito (2023)	Afganistan	Development	Government		Bank Transfer	Business development	Lump Sum
2	Ahmed et al. (2019)	Bangladesh	Development	NGO		Physical Cash		Stream - Ongoing
3	Hossain et al. (2022)	Bangladesh	Development	Government		Mobile money	Health, Child development	Lump Sum
4	Hussam et al. (2021)	Bangladesh	Humanitarian (refugees)	NGO	Pulse	Physical cash		Stream - Completed
5	Undurraga et al. (2016)	Bolivia	Development	NGO (researchers)		Physical cash (in-kind)		Lump Sum
6	Grimm et al. (2021)	Burkina Faso	Development	NGO	Innovations for Poverty Action (IPA)	Bank Transfer	Micro-enterprise growth	Lump Sum
7	Houngbe et al. (2017), Houngbe et al. (2018)	Burkina Faso	Development	NGO (researchers)	Mam'Out	Mobile money	Child development	Stream - Ongoing
8	Akresh et al. (2019)	Burkina Faso	Development	Government	Nahouri CTTP	Physical cash		Stream - Ongoing
9	Londoño-Vélez and Querubin (2022)	Colombia	Humanitarian (COVID)	Government	Compensación del IVA	Mobile money	COVID-19 emergency aid	Stream - Completed
10	Javier et al. (2022)	Congo, Dem. Rep.	Development	NGO	Give Directly	Mobile money		Stream - Completed
11	Grellety et al.	Congo, Dem. Rep.	Development	NGO (researchers)		Physical cash		Stream - Ongoing
12	4 papers, see notes	Ecuador	Development	Government	Bono de Desarrollo Humano (BDH)	Bank transfer	Education, Child dev.	Stream - Ongoing
13	Crépon et al. (2023)	Egypt	Development	NGO	Sawiris Foundation	Bank Transfer	Micro-enterprise growth	Lump Sum
14	Karlan et al. (2015), Fafchamps et al. (2014)	Ghana	Development	NGO	IPA	Physical cash	Micro-enterprise growth	Lump Sum
15	Fafchamps et al. (2014)	Ghana	Development	NGO	IPA	Bank Transfer		Lump Sum
16	Karlan et al. (2014)	Ghana	Development	NGO	IPA	Physical cash	Farm investment	Lump Sum
17	Gangopadhyay et al (2014)	India	Development	NGO (researchers)		Bank transfer		Stream - Ongoing
18	Weaver et al. (2023)	India	Development	NGO	Give Directly	Bank transfer	Child development	Stream - Ongoing/Completed
19	Hussam et al (2022)	India	Development	NGO (researchers)		Bank transfer	Micro-enterprise growth	Lump Sum
20	McKelway et al. (2023)	India	Development	NGO (researchers)		Physical cash		Lump Sum
21	Acampora et al. (2022)	Kenya	Development	NGO (researchers)		Mobile money		Stream (Annual)
22	Brooks et al. (2022)	Kenya	Humanitarian (COVID)	NGO (researchers)		Mobile money		Lump Sum
23	Haushofer et al. (2021)	Kenya	Development	NGO (researchers)		Mobile money		Lump Sum, Stream
24	4 papers, see notes	Kenya	Development	Government	Kenya CT-OVC	Bank transfer	Child support	Stream - Ongoing
25	Haushofer and Shapiro (2016, 2018), Bhargava (2019)	Kenya	Development	NGO	Give Directly	Mobile money		Lump Sum, Stream
26	Egger et al. (2020)	Kenya	Development	NGO	Give Directly	Mobile money		Lump Sum
27	Banerjee et al. (2020)	Kenya	Humanitarian (COVID)	NGO	Give Directly	Mobile money		Lump Sum, Stream
28	Orkin et al. (2023)	Kenya	Development	NGO	Give Directly	Mobile money		Lump Sum
29	Merttens et al. (2013), Dietrict and Schmerzeck (2019)	Kenya	Development	Government	Kenya HSNP	Bank transfer	Food security	Stream - Ongoing
30	Haushofer et al. (2020)	Kenya	Development	NGO	IPA	Mobile money		Lump Sum
31	Brudevold-Newman et al. (2017)	Kenya	Development	NGO	International Rescue Committee (IRC)	Phys. cash, mobile money		Lump Sum
32	Maluccio et al. (2023)	Kenya	Development	NGO (researchers)		Bank Transfer	Education	Lump Sum
33	3 papers, see notes	Lesotho	Development	Government	Lesotho Child Grant Program (CGP)	Physical cash	Child support	Stream - Ongoing/Completed
34	Aggarwal et al. (2022)	Liberia	Development	NGO	Give Directly	Mobile money		Lump Sum, Stream
35	Blattman et al. (2017)	Liberia	Development	NGO	Global Communities	Physical cash		Lump Sum
36	Datta et al. (2021)	Madagascar	Humanitarian (COVID)	NGO	World Bank + UNICEF	Physical Cash	Child development	Stream - Ongoing
37	Aggarwal et al. (2022)	Malawi	Development	NGO	Give Directly	Mobile money		Lump Sum
38	Ambler et al. (2018, 2020), Ambler et al. (2018b)	Malawi	Development	NGO	NASFAM	Physical Cash	Agriculture	Lump Sum
39	5 papers, see notes	Malawi	Development	Government	Malawi SCTP	Physical cash	Education, Food security	Stream - Ongoing
40	5 papers, see notes	Malawi	Development	NGO	Zomba CTP	Physical cash		Stream - Ongoing/Completed
41	Beaman et al. (2023)	Mali	Development	NGO	IPA	Bank Transfer		Lump Sum
42	Sessou and Henning (2019), Heath et al. (2020)	Mali	Development	Government	Programme de Filets Sociaux	Physical cash	Livelihoods, Edu., Child dev.	Stream - Ongoing
43	Aguila et al. (preliminary)	Mexico	Development	Government		Bank Transfer		Stream - Ongoing/Completed
44	Cuhna (2014), Avitabile et al. (2019)	Mexico	Development	Government	Programa de Apoyo Alimentario (PAL)	Physical cash	Health, Child Development	Stream - Ongoing/Completed
45	Benhassine et al. (2015)	Morocco	Development	Government		Physical cash	Education	Stream - Completed
46	Berkel et al. (2021)	Mozambique	Humanitarian (cyclone)	NGO (researchers)		Mobile money	Micro-enterprise growth	Lump Sum
47	Field and Maffioli (2021)	Myanmar	Humanitarian (drought)	NGO	Save the Children	Bank transfer		Stream - Ongoing
48	Levere et al. (2022)	Nepal	Development	Government		Physical Cash	Child development	Stream - Ongoing
49	Premand and Stoeffler (2020), Premand and Stoeffler (2022)	Niger	Development	Government		Physical cash		Stream - Ongoing
50	Cullen et al. (2020)	Nigeria	Development	NGO	Catholic Relief Services (CRS)	Physical Cash		Stream - Completed
51	Olajide (2016), Alzua et al. (2020)	Nigeria	Development	Government		Physical cash		Stream - Ongoing
52	3 papers, see notes	Nigeria	Development	NGO	Child Development Grant Programme	Physical cash	Child development	Stream - Ongoing/Completed
53	Fenn et al. (2017)	Pakistan	Development	NGO	Action Against Hunger	Physical cash		Stream - Ongoing/Completed
54	Bando et al. (2022)	Paraguy	Development	NGO	IPA	Bank Transfer		Stream - Ongoing
55	McIntosh and Zeitlin (2020)	Rwanda	Development	NGO	Give Directly	Mobile money		Lump Sum, Stream
56	McIntosh and Zeitlin (2022)	Rwanda	Development	NGO	Give Directly	Mobile money		Lump Sum
57	Ambler et al. (2018b)	Senegal	Development	NGO	FONGS		Agriculture	Lump Sum
58	Chowdhury et al. (2017)	South Sudan	Development	NGO	BRAC	Physical cash		Lump Sum
59	de Mel et al. (2010)	Sri Lanka	Development	NGO (researchers)		Bank check		Lump Sum
60	Baird et al. (2024)	Tanzania	Development	NGO (researchers)		Physical Cash		Lump Sum
61	Briaux et al. (2020)	Togo	Development	Government		Physical cash	Child development	Stream - Ongoing
62	Gazeaud et al. (2023)	Tunisia	Development	Government		Bank Transfer	Female financial development	Lump Sum
63	Bjorvatn et al. (2022)	Uganda	Development	NGO (researchers)		Mobile money	Business development	Lump Sum
64	Cooke and Mukhopadhyay (2019)	Uganda	Development	NGO	Give Directly	Mobile money		Lump Sum
65	Genehmigt and Tafese (2019)	Uganda	Development	NGO (researchers)		Mobile money	Business development	Lump Sum
66	Kahura et al. (2022)	Uganda	Development	NGO	GiveDirectly	Mobile money		Lump Sum
67	Fiala (2014), Fiala (2017), Fiala et al. (2022)	Uganda	Humanitarian (Refugees)	NGO	PRIDE Microfinance	Bank Transfer	Business development	Lump Sum
68	Sedlmayr et al. (2018)	Uganda	Development	NGO	Village Enterprises	Physical cash		Lump Sum
69	Gilligan et al. (2013)	Uganda	Development	NGO	World Food Programme (WFP)	Physical cash	Child development	Stream - Ongoing
70	3 papers, see notes	Uganda	Development	Government	Youth Opportunities Program (YOP)	Bank transfer	Micro-enterprise growth	Lump Sum
71	8 papers, see notes	Zambia	Development	Government	Zambia CGP	Physical cash	Child support	Stream - Ongoing/Completed
72	Handa et al. (2018), Handa et al. (2020)	Zambia	Development	Government	Zambia Multiple Category Program (MCP)	Physical cash		Stream - Ongoing

Program ID 13 reported in 4 papers: Schady and Araujo (2006), Schady and Paxson (2010), Fernald and Hidrobo (2011), and Edmonds and Schady (2012). Program ID 25 reported in 4 papers: Palermo et al. (2012), Handa et al. (2014), Handa et al. (2014), and Kilburn et al. (2016). Program ID 34 reported in 3 papers: Pace et al. (2019), Sebastian et al. (2019), and Prifti et al. (2019). Program ID 40 reported in 5 papers: Covarrubias et al. (2012), Abdoulayi et al. (2016), Kilburn et al. (2018), de Hoop et al. (2019), and Molotsky and Handa (2021). Program ID 41 reported in 5 papers: Baird et al. (2011, 2012, 2013, 2016), and Sessou et al. (2022). Program ID 53 reported in 3 papers: Carneiro et al. (2021), Carneiro et al. (2021b), and Mason (2019). Program ID 71 reported in 3 papers: Blattman et al. (2013), Calderone (2017), and Blattman et al. (2019). Program ID 72 reported in 8 papers: AIR (2014), Handa et al. (2015), Handa et al. (2016), Handa et al. (2018), Natali et al. (2018), Handa et al. (2019), de Hoop et al. (2019), and Chakrabarti et al. (2019).

Appendix Table A.1b
Program Characteristics cont.

(1) Program ID	(2) Papers	(3) Disbursement Schedule	(4) Baseline Year	(5) Baseline Sample	(6) Months Since First Transfer	(7) Months Since Last Transfer	(8) Total Transfer Amount	(9) Monthly Transfer Amount	(10) Nominal/PPP Ratio
1	Kashefi and Naito (2023)	Lump Sum	2016	3,490	23	23	1717 - 1744	75	3.9
2	Ahmed et al. (2019), Ahmed et al. (2021), Tauseef (2021)	Stream - Ongoing	2012	5,000	23	0	1392	61	1.8
3	Hossain et al. (2022)	Stream - Ongoing	2017	594	14	0	227	16	1.3
4	Hussam et al. (2021)	Stream - Completed	2019	745	3 - 4	1 - 2	100	50	2.1
5	Undurraga et al. (2016)	Lump Sum	2008	494	16	16	29 - 87	4	1.0
6	Grimm et al. (2021)	Lump Sum	2018	1,300	9	9	8484	943	3.1
7	Houngbe et al. (2017), Houngbe et al. (2018)	Stream - Ongoing	2013	1,185	24	0	420	42	1.4
8	Akresh et al. (2019)	Stream - Ongoing	2008	2,775	12 - 24	0	127 - 253	10	1.4
9	Londoño-Vélez and Querubin (2022)	Lump Sum	2020	3,462	2	0	160	80	2.1
10	Javier et al. (2022)	Stream - Completed	2019	2,358	12 - 21	8 - 16	1371 - 2742	685	2.6
11	Grellety et al.	Stream - Ongoing	2015	1,481	6	0	406	68	1.0
12	4 papers, see notes	Stream - Ongoing	2003	1,883	15 - 23	0	617 - 812	36	1.2
13	Crépon et al. (2023)	Lump Sum	2016	3,293	16	16	682 - 825	43 - 52	3.8
14	Karlan et al. (2015), Fafchamps et al. (2014)	Lump Sum	2009	160	2 - 14	2 - 14	300	21 - 150	1.9
15	Fafchamps et al. (2014)	Lump Sum	2008	793	3 - 34	3 - 34	284	8 - 95	1.9
16	Karlan et al. (2014)	Lump Sum	2008	502	24	24	795	33	2.0
17	Gangopadhyay et al (2014)	Stream - Ongoing	2010	450	12	0	761	63	3.1
18	Weaver et al. (2023)	Stream - Ongoing/Completed	2018	2,400	11 - 38	0 - 14	242 - 527	22	3.5
19	Hussam et al (2022)	Lump Sum	2015	1,345	12	12	300	25	3.5
20	McKelway et al. (2023)	Lump Sum	2021	1,120	1 - 3	1 - 3	35	14 - 69	2.2
21	Acampora et al. (2022)	Stream (Annual)	2019	521	24	12	45	2	2.3
22	Brooks et al. (2022)	Lump Sum	2020	753	2	2	92 - 98	48	2.3
23	Haushofer et al. (2021)	Lump Sum, Stream	2017	5,756	14	13 - 14	958 - 1197	68 - 824	2.1
24	4 papers, see notes	Stream - Ongoing	2007	2,294	24 - 48	0	1269 - 2322	49	1.7
25	Haushofer and Shapiro (2016, 2018), Bhargava (2019)	Lump Sum, Stream	2011	1,008	7 - 36	2 - 27	384 - 1449	11 - 181	2.2
26	Egger et al. (2020)	Lump Sum	2014	7,845	19	11	1723 - 2090	91 - 110	2.3
27	Banerjee et al. (2020)	Lump Sum, Stream	2017	8,753	20 - 27	0 - 27	3937 - 5269	161 - 217	2.3
28	Orkin et al. (2023)	Lump Sum	2017	8,339	19	17	1942	102	2.4
29	Merttens et al. (2013), Dietrict and Schmerzeck (2019)	Stream - Ongoing	2009	5,108	12 - 24	0	351 - 835	35	2.2
30	Haushofer et al. (2020)	Lump Sum	2011	789	12	12	321	28	2.2
31	Brudevold-Newman et al. (2017)	Lump Sum	2013	905	9 - 18	9 - 18	480 - 516	27 - 61	2.3
32	Maluccio et al. (2023)	Lump Sum	2020	1,912	1	1	294	294	2.3
33	3 papers, see notes	Stream - Ongoing/Completed	2011	3,054	24	0 - 12	386 - 1420	32 - 59	1.9 - 2.0
34	Aggarwal et al. (2022)	Lump Sum, Stream	2018	1,220	20	5 - 20	211 - 1267	11 - 70	1.9
35	Blattman et al. (2017)	Lump Sum	2009	999	1 - 13	1 - 13	200	16 - 246	1.8
36	Datta et al. (2021)	Stream - Ongoing	2017	4,373	18	0	998	55	3.2
37	Aggarwal et al. (2022)	Lump Sum	2019	1,378	23	21 - 23	211 - 1672	9 - 73	2.4
38	Ambler et al. (2018, 2020), Ambler et al. (2018b)	Lump Sum	2014	1,187	9 - 26	4 - 21	204 - 225	9 - 25	2.9
39	5 papers, see notes	Stream - Ongoing	2012	3,531	12 - 24	0	177 - 614	11 - 33	2.5
40	5 papers, see notes	Stream - Ongoing/Completed	2008	3,796	12 - 48	0 - 38	218 - 521	22	1.3
41	Beaman et al. (2023)	Lump Sum	2010	6,201	12 - 84	12 - 84	173 - 285	3 - 24	2.1
42	Sessou and Henning (2019), Heath et al. (2020)	Stream - Ongoing	2014	3,080	24	0	342 - 1026	14 - 42	1.9
43	Aguila et al. (preliminary)	Stream - Ongoing/Completed	2009	2,593	14 - 26	0 - 14	756 - 883	63	1.4
44	Cuhna (2014), Avitabile et al. (2019)	Stream - Ongoing/Completed	2003	5,414	12 - 84	0 - 66	278 - 436	24	1.1
45	Benhassine et al. (2015)	Stream - Completed	2008	2,010	18	2	726	45	1.0
46	Berkel et al. (2021)	Lump Sum	2019	475	5	5	227	45	2.8
47	Field and Maffioli (2021)	Stream - Ongoing	2016	2,338	30	0	596 - 742	23	2.4
48	Levere et al. (2022)	Stream - Ongoing	2013	4,228	4	0	95	24	3.4
49	Premand and Stoeffler (2020), Premand and Stoeffler (2022)	Stream - Ongoing	2012	4,330	24	0	1006	42	2.3
50	Cullen et al. (2020)	Stream - Completed	2015	2,539	30	15	552	37	2.0
51	Olajide (2016), Alzua et al. (2020)	Stream - Ongoing	2013	6,720	6 - 12	0	309 - 619	52	1.7
52	3 papers, see notes	Stream - Ongoing/Completed	2014	3,688	12 - 48	0 - 25	243 - 912	20	1.7
53	Fenn et al. (2017)	Stream - Ongoing/Completed	2015	3,584	6 - 12	0 - 6	264 - 528	44 - 88	1.0
54	Bando et al. (2022)	Stream - Ongoing	2016	3,000	12	0	2131	178	2.2
55	McIntosh and Zeitlin (2020)	Lump Sum, Stream	2016	2,017	12	0 - 12	194 - 1341	16 - 112	2.1
56	McIntosh and Zeitlin (2022)	Lump Sum	2017	1,848	14	12	761 - 1890	54 - 135	2.6
57	Ambler et al. (2018b)	Lump Sum	2014	600	9 - 21	9 - 21	379	18 - 42	2.1
58	Chowdhury et al. (2017)	Lump Sum	2013	649	12	12	1313	109	1.3
59	de Mel et al. (2010)	Lump Sum	2010	387	12 - 66	12 - 66	263	4 - 22	2.8
60	Baird et al. (2024)	Lump Sum	2008	293	16	16	529	33	2.6
61	Briaux et al. (2020)	Stream - Ongoing	2014	2,658	24	0	460	19	1.7
62	Gazeaud et al. (2023)	Lump Sum	2016	2,000	27	27	667 - 708	26	3.4
63	Bjorvatn et al. (2022)	Lump Sum	2018	1,496	12	5	279 - 293	24	3.0
64	Cooke and Mukhopadhyay (2019)	Lump Sum	2016	2,018	18	17	2571	143	2.9
65	Genehmigt and Tafese (2019)	Lump Sum	2012	174	18 - 48	18 - 48	308	6 - 17	2.7
66	Kahura et al. (2022)	Lump Sum	2020	1,264	21	19	2406 - 2485	118	2.9
67	Fiala (2014), Fiala (2017), Fiala et al. (2022)	Lump Sum	2012	1,551	6 - 24	6 - 24	899	37 - 150	1.8 - 2.9
68	Sedlmayr et al. (2018)	Lump Sum	2014	5,774	15 - 27	8 - 20	242	9 - 16	2.7
69	Gilligan et al. (2013)	Stream - Ongoing	2011	2,959	12	0	180	13	2.7
70	3 papers, see notes	Lump Sum	2008	2,677	24 - 146	24 - 146	773 - 925	6 - 39	2.1 - 2.8
71	8 papers, see notes	Stream - Ongoing/Completed	2010	3,078	24 - 82	0 - 28	490 - 1102	22	1.9 - 3.1
72	Handa et al. (2018), Handa et al. (2020)	Stream - Ongoing	2010	3,078	24 - 36	0	507 - 761	21	1.9

All currency values are reported in 2010 USD PPP. Whenever a column displays two numbers, it represents the range of values within a program. Column 4 refers to the largest baseline sample size among the studies within the program. Program ID 12 reported in 4 studies: Schady and Araujo (2006), Schady and Paxson (2010), Fernald and Hidrobo (2011), and Edmonds and Schady (2012). Program ID 24 reported in 3 studies: Palermo et al. (2012), Handa et al. (2014), and Kilburn et al. (2016). Paper ID 33 reported in 3 studies: Daidone et al. (2014), Pace et al. (2019) and Sebastian et al. (2019). Paper ID 39 reported in 5 studies: Covarrubias et al. (2012), Abdoulayi et al. (2016), Kilburn et al. (2018), de Hoop et al. (2019), and Molotsky and Handa (2021). Program ID 40 reported in 4 studies: Baird et al. (2011), Baird et al. (2012), Baird et al. (2013), Baird et al. (2016). Program ID 52 reported in 3 studies: Carneiro et al. (2021), Carneiro et al. (2012), and Mason (2019). Program ID 70 reported in 3 studies: Blattman et al. (2013), Fiala et al. (2022) and Calderone (2017). Program ID 71 reported in 7 papers: AIR (2014), Handa et al. (2015), Handa et al. (2016), Handa et al. (2018), Daidone et al. (2014), Natali et al. (2018), de Hoop et al. (2019), and Chakrabarti et al. (2019).

Appendix Table A.2: Citations of Full Sample

Program ID	Citation(s)
1	— Kashefi, Fatema, and Hisahiro Naito. "Does Receiving a Cash Grant Improve Individual Earnings in a War-Torn Country? Evidence from a Randomized Experiment in Afghanistan [version 2; peer review: 2 approved]." *F1000 Research*, April 2023.
2	— Ahmed, Akhter, John F. Hoddinott, and Shalini Roy. *Food Transfers, Cash Transfers, Behavior Change Communication and Child Nutrition: Evidence from Bangladesh.* IFPRI Discussion Paper 01868, September 2019. — Ahmed, Akhter U., Jena Hamadani, Md Zahidul Hassan, Melissa Hidrobo, John Hoddinott, Bastien Koch, Kalyani Raghunathan, and Shalini Roy. *Post-Program Impacts of Transfer Programs on Child Development: Experimental Evidence from Bangladesh.* IFPRI Discussion Paper 2090, December 2021. — Tauseef, Salauddin. "The Importance of Nutrition Education in Achieving Food Security and Adequate Nutrition of the Poor: Experimental Evidence from Bangladesh." *Oxford Bulletin of Economics and Statistics* 84, no.1 (February 2022): 241–71.
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4	— Hussam, Reshmaan, Erin Kelley, Gregory Lane, and Fatima Zahra. "The Psychological Value of Employment." *NBER Working Paper Series* 28924, June 2021. — Hussam, Reshmaan, Erin M. Kelley, Gregory Lane, and Fatima Zahra. "The Psychosocial Value of Employment: Evidence from a Refugee Camp." *American Economic Review* 112, no. 11 (November 2022): 3694–3724.
5	— Undurraga, Eduardo A., Jere R. Behrman, William R. Leonard, and Ricardo A. Godoy. "The Effects of Community Income Inequality on Health: Evidence from a Randomized Control Trial in the Bolivian Amazon." *Social Science & Medicine* 149 (January 2016): 66–75.

Appendix Table A.2 (Cont.)

Program ID	**Citation(s)**
6	— Grimm, Michael, Sidiki Soubeiga, and Michael Weber. "Short-Term Impacts of Targeted Cash Grants and Business Development Services: Experimental Evidence from Entrepreneurs in Burkina Faso." *Policy Research Working Papers*, December 2021.
7	— Houngbe, Freddy, Audrey Tonguet-Papucci, Chiara Altare, Myriam Ait-Aissa, Jean-François Huneau, Lieven Huybregts, and Patrick Kolsteren. "Unconditional Cash Transfers Do Not Prevent Children's Undernutrition in the Moderate Acute Malnutrition Out (Mam'out) Cluster-Randomized Controlled Trial in Rural Burkina Faso." *The Journal of Nutrition* 147, no. 7 (July 2017): 1410–17. — Puett, Chloe, Cécile Salpéteur, Freddy Houngbe, Karen Martínez, Dieynaba S. N'Diaye, and Audrey Tonguet-Papucci. "Costs and Cost-Efficiency of a Mobile Cash Transfer to Prevent Child Undernutrition During the Lean Season in Burkina Faso: A Mixed Methods Analysis from the Mam'out Randomized Controlled Trial." *Cost Effectiveness and Resource Allocation* 16, no. 1 (April 2018): 13.
8	—Akresh, Richard, Damien de Walque, and Harounan Kazianga. *Evidence from a Randomized Evaluation of the Household Welfare Impacts of Conditional and Unconditional Cash Transfers Given to Mothers or Fathers.* World Bank Policy Research Working Paper 7730, June 2016.
9	— Londoño-Vélez, Juliana, and Pablo Querubin. "The Impact of Emergency Cash Assistance in a Pandemic: Experimental Evidence from Colombia." Working Paper, November 2020. — Londoño-Velez, Juliana, and Pablo Querubin. "The Impact of Emergency Cash Assistance in a Pandemic: Experimental Evidence from Colombia." *The Review of Economics and Statistics* 104, no. 1 (March 2022): 157–65.
10	— Javier Kaleb, Jeremy Magruder, Nicolas Polasek, and Eleanor Wiseman. *DRC Benchmarking Report.* Washington, DC: USAID, September 2022.
11	— Grellety, Emmanuel, Pélagie Babakazo, Amina Bangana, Gustave Mwamba, Ines Lezama, Noël Marie Zagre, and Eric-Alain Ategbo. "Effects of Unconditional Cash Transfers on the Outcome of Treatment for Severe Acute Malnutrition: A Cluster-Randomised Trial in the Democratic Republic of the Congo." *BMC Medicine* 215, no. 1 (April 2017): 87.

Appendix Table A.2 (Cont.)

Program ID	**Citation(s)**
12	— Schady, Norbert, and Maria Caridad Araujo. *Cash Transfers, Conditions, School Enrollment, and Child Work: Evidence from a Randomized Experiment in Ecuador.* World Bank Policy Research Working Paper 3930, June 2006. — Paxson, Christina, and Norbert Schady. "Does Money Matter? The Effects of Cash Transfers on Child Development in Rural Ecuador." *Economic Development and Cultural Change* 59, no. 1 (October 2010): 187–229. — Fernald, Lia C. H., and Melissa Hidrobo. "Effect of Ecuador's Cash Transfer Program (Bono De Desarrollo Humano) on Child Development in Infants and Toddlers: A Randomized Effectiveness Trial." *Social Science & Medicine (1982)* 72, no. 9 (May 2011): 1437–46. — Edmonds, Eric V, and Norbert Schady. "Poverty Alleviation and Child Labor." *American Economic Journal: Economic Policy* 4, no. 4 (November 2012): 100–124.
13	— Crépon, Bruno, Mohamed El Komi, and Adam Osman. "Is It Who You Are or What You Get? Comparing the Impacts of Loans and Grants for Microenterprise Development." *American Economic Journal: Applied Economics* 16, no. 1 (February 2023): 286–313.
14	— Karlan, Dean, Ryan Knight, and Christopher Udry. "Consulting and Capital Experiments with Microenterprise Tailors in Ghana." *Journal of Economic Behavior & Organization, Economic Experiments in Developing Countries* 118 (October 2015): 281–302.
15	— Fafchamps, Marcel, David McKenzie, Simon Quinn, and Christopher Woodruff. "Microenterprise Growth and the Flypaper Effect: Evidence from a Randomized Experiment in Ghana." *Journal of Development Economics* 106 (January 2014).
16	— Karlan, Dean, Robert Osei, Isaac Osei-Akoto, and Christopher Udry. "Agricultural Decisions After Relaxing Credit and Risk Constraints *." *The Quarterly Journal of Economics* 129, no. 2 (May 2014): 597–652.
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18	— Weaver, Jeffrey, Sandip Sukhtankar, and Karthik Muralidharan. "Cash Transfers for Child Development: Experimental Evidence from India," July 2023.

Appendix Table A.2 (Cont.)

Program ID	**Citation(s)**
19	— Hussam, Reshmaan, Natalia Rigol, and Benjamin N. Roth. "Targeting High Ability Entrepreneurs Using Community Information: Mechanism Design in the Field." *American Economic Review* 112, no. 3 (March 2022): 861–98.
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21	— Acampora, Michelle, Lorenzo Casaburi, and Jack Willis. "Land Rental Markets: Experimental Evidence from Kenya." *NBER Working Paper Series*, September 2022.
22	— Brooks, Wyatt, Kevin Donovan, Terence R. Johnson, and Jackline Oluoch-Aridi. "Cash Transfers as a Response to Covid-19: Experimental Evidence from Kenya." *Journal of Development Economics* 158 (September 2022): 102929.
23	— Haushofer, Johannes, Robert Mudida, and Jeremy P. Shapiro. "The Comparative Impact of Cash Transfers and a Psychotherapy Program on Psychological and Economic Well-Being," *NBER Working Paper Series*, November 2020.
24	— The Kenya CT-OVC Evaluation Team. "The Impact of Kenya's Cash Transfer for Orphans and Vulnerable Children on Human Capital." *Journal of Development Effectiveness* 4, no. 1 (April 2012): 38–49. — Handa, Sudhanshu, Bruno Martorano, Carolyn Halpern, Audrey Pettifor, and Harsha Thirumurthy. *The Impact of the Kenya CT – OVC on Parents' Wellbeing and Their Children.* June 2014. — Handa, Sudhanshu, Carolyn Tucker Halpern, Audrey Pettifor, and Harsha Thirumurthy. "The Government of Kenya's Cash Transfer Program Reduces the Risk of Sexual Debut Among Young People Age 15-24." *PLoS ONE* 9, no. 1 (January 2014): e85473. — Kilburn, Kelly, Harsha Thirumurthy, Carolyn Tucker Halpern, Audrey Pettifor, and Sudhanshu Handa. "Effects of a Large-Scale Unconditional Cash Transfer Program on Mental Health Outcomes of Young People in Kenya." *Journal of Adolescent Health* 58, no. 2 (February 2016): 223–29.
25	— Haushofer, Johannes, and Jeremy Shapiro. "Policy Brief: Impacts of Unconditional Cash Transfers." Policy Brief, October 2013.

Appendix Table A.2 (Cont.)

Program ID	**Citation(s)**
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27	— Banerjee, Abhijit, Michael Faye, Alan Krueger, Paul Niehaus, and Tavneet Suri. *Effects of a Universal Basic Income During the Pandemic.* Working Paper, December 2020.
28	— Orkin, Kate, Robert Garlick, Mahreen Mahmud, Richard Sedlmayr, Johannes Haushofer, and Stefan Dercon. *Aspiring to a Better Future: Can a Simple Psychological Intervention Reduce Poverty?* Working Paper, January 2023.
29	— Merttens, Fred, Alex Hurrell, Marta Marzi, Ramla Attah, Maham Farhat, Andrew Kardan, and Ian MacAuslan. *Kenya Hunger Safety Net Programme Monitoring and Evaluation Component.* Impact Evaluation Report. Oxford: Oxford Policy Management, June 2013. — Dietrich, Stephan, and Georg Schmerzeck. "Cash Transfers and Nutrition: The Role of Market Isolation After Weather Shocks." *Food Policy* 87 (August 2019): 101739.
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Appendix Table A.2 (Cont.)

Program ID	Citation(s)
31	— Brudevold-Newman, Andrew, Maddalena Honorati, Pamela Jakiela, and Owen Ozier. *"A Firm of One's Own: Experimental Evidence on Credit Constraints and Occupational Choice.* World Bank Policy Research Working Paper 7977, February 2017.
32	— Maluccio, John A., Erica Soler-Hampejsek, Beth Kangwana, Eva Muluve, Faith Mbushi, and Karen Austrian. "Effects of a Single Cash Transfer on School Re-Enrollment During Covid-19 Among Vulnerable Adolescent Girls in Kenya: Randomized Controlled Trial." *Economics of Education Review* 95 (August 2023): 102429.
33	— Covarrubias, Katia, Benjamin Davis, and Paul Winters. "From Protection to Production: Productive Impacts of the Malawi Social Cash Transfer Scheme." *Journal of Development Effectiveness* 4, no. 1 (March 2012): 50–77. — Pace, Noemi, Silvio Daidone, Benjamin Davis, and Luca Pellerano. "Shaping Cash Transfer Impacts Through 'Soft-Conditions': Evidence from Lesotho." *Journal of African Economies*, June 2018. — Sebastian, Ashwini, Ana Paula de la O Campos, Silvio Daidone, Noemi Pace, Benjamin Davis, Ousmane Niang, and Luca Pellerano. "Cash Transfers and Gender Differentials in Child Schooling and Labor: Evidence from the Lesotho Child Grants Programme." *Population and Development Review* 45 (December 2019): 181–208. — Prifti, Ervin, Silvio Daidone, and Benjamin Davis. "Causal Pathways of the Productive Impacts of Cash Transfers: Experimental Evidence from Lesotho." *World Development* 115 (March 2019): 258–68.
34	— Aggarwal, Shilpa, Jenny C. Aker, Dahyeon Jeong, Naresh Kumar, David Sungho Park, Jonathan Robinson, and Alan Spearot. *Final Report for Cash Benchmarking Study in Liberia and Malawi.* June 9, 2022.
35	— Blattman, Christopher, Julian C. Jamison, and Margaret Sheridan. "Reducing Crime and Violence: Experimental Evidence from Cognitive Behavioral Therapy in Liberia." *American Economic Review* 107, no. 4 (April 2017): 1165–1206.
36	— Datta, Saugato, Joshua Martin, Catherine MacLeod, Laura B Rawlings, and Andrea Vermehren. "Do Behavioral Interventions Enhance the Effects of Cash on Early Childhood Development and Its Determinants? Evidence from a Cluster-Randomized Trial in Madagascar," World Bank Policy Research Working Paper 9747, August 2021.

Appendix Table A.2 (Cont.)

Program ID	**Citation(s)**
37	— Aggarwal, Shilpa, Jenny C. Aker, Dahyeon Jeong, Naresh Kumar, David Sungho Park, Jonathan Robinson, and Alan Spearot. *Final Report for Cash Benchmarking Study in Liberia and Malawi.* June 9, 2022.
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39	— Covarrubias, Katia, Benjamin Davis, and Paul Winters. "From Protection to Production: Productive Impacts of the Malawi Social Cash Transfer Scheme." *Journal of Development Effectiveness* 4, no. 1 (March 2012): 50–77. — Sara Abdoulayi, Gustavo Angeles, Clare Barrington, Kristen Brugh, Jacobus de Hoop, Sudhanshu Handa, Kelly Kilburn, Adria Molotsky, Frank Otchere, Tia Palermo, Amber Peterman, Peter Mvula, Maxton Tsoka, and Susannah Zietz. *Malawi Social Cash Transfer Programme Impact Evaluation.* Impact Evaluation Report. Carolina Population Center, University of North Carolina at Chapel Hill, December 31, 2016. — Kilburn, Kelly, Sudhanshu Handa, Gustavo Angeles, Peter Mvula, and Maxton Tsoka. "Short-Term Impacts of an Unconditional Cash Transfer Program on Child Schooling: Experimental Evidence from Malawi." *Economics of Education Review* 59 (2017): 63–80. — Brugh, Kristen, Gustavo Angeles, Peter Mvula, Maxton Tsoka, and Sudhanshu Handa. "Impacts of the Malawi Social Cash Transfer Program on Household Food and Nutrition Security." *Food Policy* 76 (2018): 19–32. — Kilburn, Kelly, Sudhanshu Handa, Gustavo Angeles, Maxton Tsoka, and Peter Mvula. "Paying for Happiness: Experimental Results from a Large Cash Transfer Program in Malawi." *Journal of Policy Analysis and Management* 37, no. 2 (February 2018): 331–56.

Appendix Table A.2 (Cont.)

Program ID	Citation(s)
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Appendix Table A.3
Targeting and Framing by Program

(1) Program ID	(2) Transfer Type	(3) Target Population	(4) Female Targeting	(5) Child/Food Framing	(6) Goal of Framing	(7) Description of Framing
1	Lump Sum	Micro-entrepreneurs aged 18-35 and illiterate	No		Business development	Participants had to submit business proposals
2	Stream	Rural households with young children	Yes			
3	Stream	Poor households with young children	Yes	Yes	Health, Child development	Voluntary basic health education orientation program
4	Stream	Refugees	Randomized			
5	Lump Sum	Farmers, rural	Randomized			
6	Lump Sum	Agriculturual entrepreneurs	No		Entrepreneurship/enterprise development	Given to businesses along with a business training
7	Stream	Poor households with young children	Yes	Yes	Child development	Told the UCT was to support their child's development and to prevent undernutrition
8	Stream	Rural households with school-age children	Randomized			
9	Lump Sum	Poor households	Yes		COVID-19 emergency aid	Expedited UCT delivery after COVID-19 outbreak to assist the extreme poor
10	Stream	Urban Youth	80% women			
11	Stream	Households with young children with severe malnutrition	Yes			
12	Stream	Households with young children		Yes	Education, Child dev.	Promoted as a way to support the human capital of poor children
13	Lump Sum	Rural entrepreneurs aged 21-35	No		Entrepreneurship/enterprise development	Transfers given to business loan applicants
14	Lump Sum	Urban micro-entrepreneurs			Micro-enterprise growth	Asked to spend money on their businesses
15	Lump Sum	Urban Microentropreneurs	80% women		Business Development	Transfers given to micro-entrepreneurs
16	Lump Sum	Farmers, rural		Yes	Farm investment	Individualized deliverty based on farmers' preferences and uses for grant
17	Stream	Poor households	Yes			
18	Stream	Mothers	Yes	Yes	Health, child development	Transfers given to pregnant mothers along with messaging in the form of flyers and automated calls encouraging beneficiaries to spend transfers on nutritious food for the mother and child
19	Lump Sum	Micro-entrepreneurs			Micro-enterprise growth	Encouraged to invest money in their business
20	Lump Sum	Elderly, living alone	Yes			
21	Lump Sum	Farmers, rural				
22	Lump Sum	Female micro-entrepreneurs	Yes			
23	Lump Sum, Stream	Poor households, rural				
24	Stream	Households with vulnerable children		Yes	Child support	Told the money is to be used for the care of vulnerable children
25	Lump Sum, Stream	Poor households, rural	Randomized			
26	Lump Sum	Poor households, rural				
27	Lump Sum, Stream	Poor households, rural				
28	Lump Sum	Poor or widowed, rural households	Yes			
29	Stream	Poor households		Yes	Food security	Labelled: "Hunger Safety Net Programme"
30	Lump Sum	Informal workers, urban				
31	Lump Sum	Young, poor women, urban	Yes			
32	Lump Sum	Households with daughters	No	Yes	Education	Messaging around the transfer states that the transfer is meant to support the cost of daughters re-enrollment in school
33	Stream	Poor households with vulnerable children		Yes	Child support	Instructed to spend the money on children
34	Lump Sum, Stream	Poor households, rural	77% women			
35	Lump Sum	High-risk men (Criminally Engaged)				
36	Stream	Households with young children	Yes	Yes	Child Development	Mother Leaders groups give "nudges" on intervention days regarding child development
37	Lump Sum	Poor households, rural	77% women			
38	Lump Sum	Poor Farmers	No		Agriculture	Given to farmer clubs
39	Stream	Ultra-poor, labour-constrained households	Yes	Yes	Education, Food security	Encouraged to invest the UCT in the human capital of children and household necessities
40	Stream	Adolescent girls, parents, poor region	Yes			
41	Lump Sum	Rural Households	Yes		Agriculture	Given to farmers during planting time
42	Stream	Poor households, men		Yes	Livelihoods, Edu., Child dev.	Voluntary activities related to livelihoods, education, child health and nutrition, etc.
43	Stream	Elderly	No			
44	Stream	Poor households, rural	Yes	Yes	Health, Child Development	Health, nutrition, and hygiene classes
45	Stream	Poor households with school-age children, rural	Randomized	Yes	Education	Promoted as for supporting child education
46	Lump Sum	Micro-entrepreneurs			Micro-enterprise growth	Instructed to spend the money on their business
47	Stream	Households with young children	Yes			
48	Stream	Households with pregnant mothers or children under 2 years old	Yes	Yes	Child Development	Transfers given to mothers of young children alongside messaging about child health
49	Stream	Poor households, rural	Yes			
50	Stream	Extremely Vulnerable households	Yes			
51	Stream	Poor elderly				
52	Stream	Households with young children and in extreme poverty	Yes	Yes	Child development	Information provided on pre-natal health and infant feeding
53	Stream	Poor households with young children				
54	Stream	Elderly	No			
55	Lump Sum, Stream	Young, poor, underemployed adults				
56	Lump Sum	Young, poor, underemployed adults				
57	Lump Sum	Farmers	No		Agriculture	Transfers given alongside farm management plans and agricultural advisory visits
58	Lump Sum	Poor women, post-conflict				
59	Lump Sum	Micro-entrepreneurs	Randomized			
60	Lump Sum	Vulnerable groups, (widowed, disabled, elderly)	No			
61	Stream	Households with young children, rural	Yes	Yes	Child development	Case management of child illness and malnutrition (also provided to control group)
62	Lump Sum	Poor rural women	Yes		Female Financial Development	Transfers given alongside gender sensitive financial trainings
63	Lump Sum	Households with exactly one child aged 3-5	Yes		Business development	Transfers labeled as a business grant
64	Lump Sum	Poor farmers, rural				
65	Lump Sum	Businesses	No		Business development	Given to businesses
66	Lump Sum	Refugee Communities	75% women			
67	Lump Sum	Micro Enterprises	No		Business Development	Given to businesses
68	Lump Sum	Poor households				
69	Stream	Households with young children	Yes	Yes	Child development	UCTs provided at UNICEF-supported early childhood development centers.
70	Lump Sum	Young adults, post-conflict			Micro-enterprise growth	Required to submit business grant proposal before receiving transfer
71	Stream	Households with young children, rural	Yes	Yes	Child support	Labelled: "Child Grant Program"
72	Stream	Households with vulnerable adults and children, poor region	Yes			

Specific citations associated with each Program ID reported in Table A.1.

Appendix Table B.1

Standardization of Reported Food Security Outcomes

(1) Program ID	(2) Disbursement Schedule	(3) Total Transfer Amount	(4) Monthly Tranche Amount	(5) Months Since First Transfer	(6) Reported Outcome	(7) Reported Units	(8) Unstandardized Treatment Effect (TE)	(9) Control Group Mean	(10) Standardized TE
2	Stream	1,392	61	23	Household Hunger Scale	Binary	0.04 (0.02)	0.92 (0.27)	0.15 (0.07)
8	Stream	420	18	24	Household Food Insecurity Access Scale	Score	0.2 (0.35)	3.5 (3.85)	0.05 (0.09)
10	Lump Sum	160	80	2	Food security index	Standard deviations	0 (0.03)	0 (1)	0 (0.03)
17	Lump Sum	795	33	24	Household reports missing a meal in last 12 months	Days	0.08 (0.04)	0.77 (0.42)	0.19 (0.09)
21	Lump Sum	35	69	1	Food security (skipped meal)	Binary	-0.01 (0.06)	0.22 (0.42)	-0.02 (0.14)
21	Lump Sum	35	14	3	Food security (skipped meal)	Binary	-0.1 (0.05)	0.22 (0.42)	-0.24 (0.13)
22	Pooled (Lump Sum & Stream)	45	2	24	Experienced Hunger	Binary	-0.02 (0.02)	0.84 (0.37)	-0.05 (2.51)
24	Pooled (Lump Sum & Stream)	958	68	14	Food security index	Standard deviations	0.14 (0.06)	0 (1)	0.14 (0.06)
26	Stream	384	34	11	Food security index	Standard deviations	0.4 (0.12)	0 (1)	0.4 (0.12)
26	Lump Sum	384	11	36	Food security index	Standard deviations	-0.03 (0.1)	0 (1)	-0.03 (0.1)
26	Stream	1,449	40	36	Food security index	Standard deviations	-0.04 (0.14)	0 (1)	-0.04 (0.14)
26	Stream	384	11	36	Food security index	Standard deviations	-0.06 (0.12)	0 (1)	-0.06 (0.12)
26	Stream	1,449	145	10	Food security index	Standard deviations	0.43 (0.12)	0 (1)	0.43 (0.12)
26	Lump Sum	384	53	7	Food security index	Standard deviations	0.14 (0.11)	0 (1)	0.14 (0.11)
28	Stream	3,940	146	27	Experienced Hunger	Binary	0.05 (0.02)	0.32 (0.47)	0.11 (0.04)
28	Lump Sum	4,356	161	27	Experienced Hunger	Binary	0.06 (0.02)	0.32 (0.47)	0.13 (0.04)
28	Stream	3,937	146	27	Experienced Hunger	Binary	0.11 (0.02)	0.32 (0.47)	0.24 (0.04)
31	Lump Sum	321	28	12	Times went hungry in past month	Days	0.14 (0.04)	0.19 (0.58)	0.24 (0.07)
35	Lump Sum	211	11	20	Food Security Index	Standard deviations	0.09 (0.07)	0 (1)	0.09 (0.07)
35	Lump Sum	632	32	20	Food Security Index	Standard deviations	0.52 (0.07)	0 (1)	0.52 (0.07)
35	Stream	632	32	20	Food Security Index	Standard deviations	0.42 (0.07)	0 (1)	0.42 (0.07)
35	Lump Sum	422	21	20	Food Security Index	Standard deviations	0.21 (0.07)	0 (1)	0.21 (0.07)
35	Stream	211	11	20	Food Security Index	Standard deviations	0.29 (0.07)	0 (1)	0.29 (0.07)
35	Stream	422	21	20	Food Security Index	Standard deviations	0.35 (0.07)	0 (1)	0.35 (0.07)
37	Stream	998	55	18	Food Insecurity Score (mean number of days experienced seven types of food insecurity)	Score	-0.21 (0.24)	6.06 (0.14)	-1.5 (1.71)
38	Lump Sum	1,549	67	23	Household Hunger Score (past month)	Score	0.17 (0.07)	0.95 (1.28)	0.13 (0.05)
38	Lump Sum	1,032	45	23	Household Hunger Score (past month)	Score	0.18 (0.06)	0.95 (1.28)	0.14 (0.05)
38	Lump Sum	516	22	23	Household Hunger Score (past month)	Score	0.13 (0.06)	0.95 (1.28)	0.1 (0.05)
40	Stream	407	17	24	Eats more than 1 meal per day	Binary	0.14 (0.03)	0.82 (0.39)	0.35 (0.08)
40	Stream	177	15	12	More than 1 meal/day	Binary	0.11 (0.03)	0.88 (0.34)	0.32 (0.09)
44	Stream	756	29	26	Food availability index	Standard deviations	0.67 (0.11)	0 (1)	0.67 (0.11)
44	Stream	883	63	14	Food availability index	Standard deviations	0.43 (0.11)	0 (1)	0.43 (0.11)
50	Stream	1,006	42	24	Moderate or severe food Insecurity	Binary	0.07 (0.04)	0.59 (0.49)	0.13 (0.09)
53	Stream	474	20	24	Whether child did not have enough food	Binary	0.05 (0.02)	0.83 (0.37)	0.13 (0.04)
53	Stream	474	10	48	Whether child did not have enough food	Binary	0.1 (0.02)	0.83 (0.37)	0.26 (0.05)
59	Lump Sum	1,313	109	12	Food security composite z-score (going a day without eating, going to sleep hungry, being without any food in the house, eating fewer meals than normal at mealtimes, limiting portions)	Standard deviations	0.03 (0.11)	-0.01 (1)	0.03 (0.11)
62	Stream	460	19	24	Severely food insecure	Binary	0.11 (0.04)	0.99 (0)	0.28 (0.11)
63	Lump Sum	667	25	27	Extreme coping strategy (dummy equal to one if the household reduced the number of meals, took children out of school or fostered children to friends to face a shock)	Binary	0.03 (0.01)	0.88 (0.33)	0.09 (0.04)
64	Lump Sum	279	23	12	Household food-insecurity (past 7 days)	Binary	0.19 (0.1)	0.61 (0.49)	0.39 (0.21)
65	Lump Sum	2,571	143	18	Food Security index	Standard deviations	0.47 (0.08)	0 (1)	0.47 (0.08)
67	Lump Sum	2,406	117	21	Food Security Index	Standard deviations	0.09 (0.08)	0 (1)	0.09 (0.08)
69	Lump Sum	242	12	21	Nutrition index (Household Dietary Diversity Score and the inverse of the Household Food Insecurity Access Score)	Standard deviations	0.02 (0.05)	0 (1)	0.02 (0.05)
72	Stream	547	23	24	Food security scale	Standard deviations	0.41 (0.1)	0 (1)	0.41 (0.1)
72	Stream	1,094	23	48	Meal frequency (3 or more indicator)	Binary	0.18 (0.05)	0.23 (0.42)	0.44 (0.12)
72	Stream	821	23	36	HFIAS	Standard deviations	0.54 (0.1)	0 (1)	0.54 (0.1)
72	Stream	1,102	13	82	HFIAS	Standard deviations	0.04 (0.13)	0 (1)	0.04 (0.13)

Standard errors reported in parentheses. All currency values are reported in 2010 USD PPP. Specific citations associated with each Program ID reported in Table A.1. Standardized treatment effects in Column 10 are calculated by dividing the unstandardized treatment effect in Column 8 by the control group mean standard error in Column 9. All values have been transformed if necessary so that higher values represent greater food security and lower values represent less food security.

Appendix Table B.2

Standardization of Reported Psychological Well-being Outcomes

(1) Program ID	(2) Disbursement Schedule	(3) Total Transfer Amount	(4) Monthly Tranche Amount	(5) Months Since First Transfer	(6) Reported Outcome	(7) Reported Units	(8) Unstandardized Treatment Effect (TE)	(9) Control Group Mean	(10) Standardized TE
3	Stream	227	16	14	Maternal self-esteem (Rosenberg 30 point scale)	Standard Deviations	0.32 (0.1)	0 (0)	0.32 (0.1)
4	Stream	100	33	3	Psychosocial Well-being Index	Standard Deviations	0.06 (0.05)	0 (1)	0.06 (0.05)
5	Lump Sum	29	2	16	Stress score (Episodes of negative emotions during the seven days before the survey).	Score	-0.28 (0.14)	6.91 (6.77)	-0.04 (0.02)
5	Lump Sum	87	5	16	Stress score (Episodes of negative emotions during the seven days before the survey).	Score	-0.27 (0.12)	6.91 (6.77)	-0.04 (0.02)
9	Lump Sum	160	80	2	Household mental health index	Standard Deviations	0.03 (0.03)	0 (1)	0.03 (0.03)
10	Stream	1,371	114	12	Depression, Well-Being, Trust Index	Standard Deviations	0.06 (0.08)	0 (1)	0.06 (0.08)
10	Stream	2,742	228	12	Depression, Well-Being, Trust Index	Standard Deviations	0.07 (0.1)	0 (1)	0.07 (0.1)
12	Stream	812	35	23	Mother's depressive symptoms score	Score	-0.71 (0.79)	18.9 (10.6)	-0.07 (0.07)
12	Stream	617	41	15	Depressive Symptoms Index	Standard Deviations	0.09 (0.13)	0 (1)	0.09 (0.13)
13	Lump Sum	682	43	16	Mental Health Index	Standard Deviations	0.11 (0.08)	0 (1)	0.11 (0.08)
13	Lump Sum	682	43	16	Mental Health Index	Standard Deviations	0.05 (0.07)	0 (1)	0.05 (0.07)
18	Stream	242	22	11	Depression Index	Standard Deviations	0.08 (0.07)	3.19 (0)	0.08 (0.07)
18	Stream	505	22	23	Depression Index	Standard Deviations	0.24 (0.16)	3.19 (0)	0.24 (0.16)
20	Lump Sum	35	69	1	Geriatric Depression Scale	Score	1.01 (0.54)	6.4 (4.59)	0.22 (0.12)
20	Lump Sum	35	14	3	Geriatric Depression Scale	Score	0.35 (0.53)	6.4 (4.59)	0.08 (0.11)
23	Lump Sum	958	68	14	Psychological Wellbeing Index	Standard Deviations	0.25 (0.08)	0 (1)	0.25 (0.08)
23	Stream	958	68	14	Psychological Wellbeing Index	Standard Deviations	0.22 (0.07)	0 (1)	0.22 (0.07)
24	Stream	2,322	48	48	CES-D depression scale greater than 10 (depressed)	Binary	0.05 (0.02)	0.63 (0.48)	0.1 (0.04)
25	Lump Sum	384	53	7	Psychological well-being index	Standard Deviations	0.2 (0.08)	0 (1)	0.2 (0.08)
25	Stream	1,449	40	36	Psychological well-being index	Standard Deviations	0.06 (0.07)	0 (1)	0.06 (0.07)
25	Stream	1,449	145	10	Psychological well-being index	Standard Deviations	0.47 (0.11)	0 (1)	0.47 (0.11)
25	Stream	384	34	11	Psychological well-being index	Standard Deviations	0.21 (0.1)	0 (1)	0.21 (0.1)
25	Stream	384	11	36	Psychological well-being index	Standard Deviations	-0.06 (0.07)	0 (1)	-0.06 (0.07)
25	Lump Sum	384	11	36	Psychological well-being index	Standard Deviations	-0.04 (0.08)	0 (1)	-0.04 (0.08)
28	Lump Sum	1,942	102	19	Mental Health z-score	Standard Deviations	0.09 (0.03)	0 (1)	0.09 (0.03)
30	Lump Sum	321	28	12	Subjective Well-being Index	Standard Deviations	0.03 (0.09)	0 (0.92)	0.03 (0.09)
34	Pooled (Lump Sum & Stream)	211	11	20	Psychological Well-being (past 2 weeks)	Standard Deviations	0.28 (0.06)	0 (1)	0.28 (0.06)
34	Pooled (Lump Sum & Stream)	632	32	20	Psychological Well-being (past 2 weeks)	Standard Deviations	0.37 (0.05)	0 (1)	0.37 (0.05)
34	Pooled (Lump Sum & Stream)	422	21	20	Psychological Well-being (past 2 weeks)	Standard Deviations	0.36 (0.06)	0 (1)	0.36 (0.06)
35	Lump Sum	200	16	13	Positive self regard/mental health index	Standard Deviations	-0.03 (0.09)	0 (1)	-0.03 (0.09)
35	Lump Sum	200	246	1	Positive self regard/mental health index	Standard Deviations	0.14 (0.09)	0 (1)	0.14 (0.09)
37	Lump Sum	516	22	23	Psychological Well-being (past 2 weeks)	Standard Deviations	0.04 (0.06)	0 (1)	0.04 (0.06)
37	Lump Sum	1,032	45	23	Psychological Well-being (past 2 weeks)	Standard Deviations	0.11 (0.06)	0 (1)	0.11 (0.06)
37	Lump Sum	1,549	67	23	Psychological Well-being (past 2 weeks)	Standard Deviations	0.16 (0.06)	0 (1)	0.16 (0.06)
39	Stream	266	15	18	Overall psychological state index	Standard Deviations	0.47 (0.09)	0 (1)	0.47 (0.09)
39	Stream	177	15	12	Quality of Life Scale	Score	2.95 (0.48)	18.1 (6.8)	0.43 (0.07)
40	Stream	521	22	24	GHQ-12 Binary Measure of Psychological Distress	Binary	0.04 (0.05)	0.69 (0.46)	0.08 (0.1)
40	Stream	260	22	12	GHQ-12 Binary Measure of Psychologic	Binary	0.14 (0.04)	0.63 (0.48)	0.29 (0.09)
42	Stream	342	14	24	Standardized stress index	Standard Deviations	0.19 (0.12)	0.02 (0.07)	0.19 (0.12)
50	Stream	552	18	30	Self Esteem based on Rosenberg scale	Score	0.07 (0.03)	3.3 (1.17)	0.06 (0.03)
50	Stream	552	18	30	Self Esteem based on Rosenberg scale	Score	-0.04 (0.02)	3.34 (1.08)	-0.04 (0.02)
51	Stream	309	52	6	Life Satisfaction Index	Score	0.49 (0.19)	6.66 (2.3)	0.21 (0.08)
51	Stream	619	52	12	Life Satisfaction Index	Score	1.02 (0.29)	6 (3.22)	0.32 (0.09)
54	Stream	2,131	178	12	Subjective Well-being Index	Standard Deviations	0.48 (0.03)	0 (1)	0.48 (0.03)
56	Lump Sum	761	54	14	Subjective well-being index	Standard Deviations	0.4 (0.09)	0 (1)	0.4 (0.09)
56	Lump Sum	983	70	14	Subjective well-being index	Standard Deviations	0.53 (0.1)	0 (1)	0.53 (0.1)
56	Lump Sum	1,202	86	14	Subjective well-being index	Standard Deviations	0.48 (0.09)	0 (1)	0.48 (0.09)
56	Lump Sum	1,795	128	14	Subjective well-being index	Standard Deviations	0.55 (0.09)	0 (1)	0.55 (0.09)
62	Lump Sum	667	25	27	Current life satisfaction	Score	0.27 (0.06)	2.36 (1.47)	0.18 (0.04)
63	Lump Sum	279	23	12	Happiness with life score	Score	0.81 (0.16)	4.98 (2.45)	0.33 (0.07)
66	Lump Sum	2,406	117	21	Psychological Well-being index	Standard Deviations	0.28 (0.08)	0 (1)	0.28 (0.08)
68	Lump Sum	242	12	21	Psychological Outlook Index	Standard Deviations	-0.11 (0.07)	0 (1)	-0.11 (0.07)
70	Lump Sum	773	7	108	Mental health index	Standard Deviations	-0.06 (0.05)	0 (1)	-0.06 (0.05)
71	Stream	547	23	24	Feeling happy indicator	Binary	0.46 (0.04)	0.07 (0.26)	1.8 (0.17)
71	Stream	1,094	23	48	Considers self better off than 12 months ago	Binary	0.1 (0.02)	0.78 (0.41)	0.25 (0.05)
71	Stream	630	20	32	Quality of life index	Standard Deviations	0.01 (0.02)	0 (1)	0.01 (0.02)

Standard errors reported in parentheses. All currency values are reported in 2010 USD PPP. Specific citations associated with each Program ID reported in Table A.1. Reported outcomes have been transformed when necessary so that higher values indicate greater food security. Standardized treatment effects in Column 10 are calculated by dividing the unstandardized treatment effect in Column 8 by the control group mean standard error in Column 9. All values have been transformed if necessary so that higher values represent better psychological well-being and lower values represent worse psychological well-being.

Appendix Table C.1
Average Treatment Effects on Primary Outcomes by Delivery Modality

	(1)	(2)	(3)	(4)
	Predicted Treatment Effect of $100		*Estimates (Programs)*	
	Physical Cash	Bank Transfer or Mobile Money	Physical Cash	Bank Transfer or Mobile Money
Panel A. Treatment Effect per Total Transfer Amount				
Flow outcomes				
Monthly Household Consumption	3.5 (2.5, 4.6)	3.0 (2.0, 4.1)	30 (21)	50 (23)
Monthly Household Food Consumption	2.9 (2.0, 3.9)	1.6 (0.9, 2.4)	20 (13)	27 (17)
Monthly Income	2.4 (1.7, 3.2)	0.9 (0.5, 1.3)	29 (14)	48 (22)
Hours Worked per Week	0.2 (-0.3, 0.6)	0.1 (-0.1, 0.2)	5 (4)	20 (9)
Labor Force Participation (percentage points)	0.7 (0.2, 1.3)	1.1 (0.2, 2.0)	10 (6)	5 (4)
School Enrollment (percentage points)	1.3 (0.5, 2.0)	0.7 (0.01, 1.37)	13 (8)	13 (8)
Food Security z-Score	0.03 (0.02, 0.05)	0.03 (0.02, 0.04)	18 (11)	29 (14)
Psychological Well-being z-Score	0.05 (0.03, 0.08)	0.03 (0.01, 0.04)	20 (11)	36 (19)
Stock outcomes				
Stock of Total Assets	9.1 (-5.5, 24.2)	22.3 (14.6, 30.2)	15 (12)	45 (16)
Stock of Financial Assets	3.5 (2.6, 4.5)	1.1 (0.5, 1.7)	15 (10)	34 (14)
Height-for-Age z-Score	0.01 (0.01, 0.02)	0.002 (-0.005, 0.010)	19 (10)	13 (8)
Weight-for-Age z-Score	0.02 (0.01, 0.03)	0.003 (-0.004, 0.009)	7 (6)	8 (4)
Stunting (percentage points)	-0.3 (-0.8, 0.2)	-0.02 (-0.8, 0.7)	9 (6)	3 (2)
Panel B. Treatment Effect per Monthly Tranche Amount				
Flow outcomes				
Monthly Household Consumption	69.5 (50.8, 89.2)	50.9 (33.1, 69.5)	30 (21)	50 (23)
Monthly Household Food Consumption	63.1 (45.5, 81.9)	27.1 (14.0, 40.9)	20 (13)	27 (17)
Monthly Income	42.1 (28.8, 56.2)	14.3 (6.8, 22.7)	29 (14)	48 (22)
Hours Worked per Week	0.8 (-6.6, 8.1)	1.6 (-2.0, 5.3)	5 (4)	20 (9)
Labor Force Participation (percentage points)	14.0 (2.9, 25.3)	14.5 (-0.6, 29.9)	10 (6)	5 (4)
School Enrollment (percentage points)	21.8 (10.4, 34.0)	8.4 (-1.9, 19.0)	13 (8)	13 (8)
Food Security z-Score	0.8 (0.5, 1.2)	0.6 (0.4, 0.9)	18 (11)	29 (14)
Psychological Well-being z-Score	0.68 (0.3, 1.0)	0.37 (0.1, 0.6)	20 (11)	36 (19)

Effects with seven or fewer estimates in gray. All currency values are reported in 2010 USD PPP. 95% credibility intervals in parentheses.

Appendix Table C.2
Average Treatment Effects on Primary Outcomes by Implementer Type

	(1)	(2)	(3)	(4)
	Predicted Treatment Effect of $100		*Estimates (Programs)*	
	NGO	Government	NGO	Government
Panel A. Treatment Effect per Total Transfer Amount				
Flow outcomes				
Monthly Household Consumption	3.3 (2.3, 4.3)	3.3 (2.2, 4.4)	60 (30)	22 (15)
Monthly Household Food Consumption	1.9 (1.2, 2.7)	2.6 (1.6, 3.6)	31 (19)	18 (12)
Monthly Income	1.0 (0.6, 1.5)	3.1 (2.1, 4.0)	75 (30)	13 (8)
Hours Worked per Week	0.02 (-1.8, 1.8)	2.3 (-0.2, 4.7)	17 (9)	8 (4)
Labor Force Participation (percentage points)	0.1 (0.04, 0.16)	0.1 (-0.01, 0.13)	10 (5)	7 (6)
School Enrollment (percentage points)	0.1 (0.0003, 0.1505)	0.1 (0.04, 0.18)	11 (6)	15 (10)
Food Security z-Score	0.3 (0.2, 0.4)	0.5 (0.3, 0.6)	35 (18)	12 (7)
Psychological Well-being z-Score	0.3 (0.1, 0.4)	0.6 (0.4, 0.9)	41 (20)	15 (10)
Stock outcomes				
Stock of Total Assets	21.2 (13.5, 29.2)	12.4 (-4.2, 29.2)	52 (22)	8 (6)
Stock of Financial Assets	1.5 (0.9, 2.1)	3.9 (2.3, 5.6)	44 (20)	5 (4)
Height-for-Age z-Score	0.1 (0.04, 0.18)	-0.04 (-0.2, 0.1)	21 (10)	11 (8)
Weight-for-Age z-Score	0.1 (-0.02, 0.13)	0.1 (-0.1, 0.3)	8 (4)	7 (6)
Stunting (percentage points)	-3.8 (-9.6, 1.7)	-0.1 (-6.1, 5.7)	6 (4)	6 (4)
Panel B. Treatment Effect per Monthly Tranche Amount				
Flow outcomes				
Monthly Household Consumption	52.0 (36.2, 68.3)	74.6 (54.2, 96.1)	60 (30)	22 (15)
Monthly Household Food Consumption	30.8 (17.9, 44.5)	63.7 (44.5, 84.0)	31 (19)	18 (12)
Monthly Income	11.1 (6.3, 16.5)	72.4 (57.3, 87.9)	75 (30)	13 (8)
Hours Worked per Week	-0.1 (-7.7, 7.4)	2.9 (-8.2, 14.0)	17 (9)	8 (4)
Labor Force Participation (percentage points)	2.1 (0.8, 3.4)	0.8 (-0.8, 2.3)	10 (5)	7 (6)
School Enrollment (percentage points)	0.8 (-0.2, 2.0)	1.9 (0.9, 3.0)	11 (6)	15 (10)
Food Security z-Score	4.9 (2.8, 6.9)	11.0 (7.3, 14.7)	35 (18)	12 (7)
Psychological Well-being z-Score	3.1 (0.7, 5.5)	9.9 (5.8, 14.0)	41 (20)	15 (10)

Effects with seven or fewer estimates in gray. All currency values are reported in 2010 USD PPP. 95% credibility intervals in parentheses.

Appendix Table D
Distribution of Months Since First and Last Transfer per Disbursement Schedule

	(1) Ongoing Stream	(2) Completed Stream	(3) Lump Sum
Number of Programs	30	16	39
Number of Estimates	190	99	318
Months Since First Transfer			
Mean	20	25	21
Min	4	3	1
20th percentile	12	12	12
Median	24	21	18
80th percentile	24	36	23
Max	48	84	146
Months Since Last Transfer			
Mean		12	
Min		1	
20th percentile		3	
Median		10	
80th percentile		20	
Max		66	

Seven lump sum programs were distributed in two, or three installments within a month or two of each other. We ignore this distinction and treat the entire lump sum as transferred at the time of the first transfer.

Appendix Table E.1
Treatment Effects on Monthly Household Consumption: Alternative Consumption Measures

	(1)	(2)	(3)
	Predicted Treatment Effect of $100 Transfer		
	Original Specification	Pooled Specification	Estimates (Programs)
Panel A. Repeat of Table 3, Column 1, Panel C			
Monthly Household Consumption	46.3 (36.7, 56.7)		82 (45)
Panel B. Treatment Effects per Monthly Tranche Amount			
Monthly Household Total Consumption	48.1 (37.8, 59.3)	49.2 (38.8, 60.5)	75 (41)
Monthly Household Food Consumption	43.7 (9.1, 82.6)	41.7 (15.2, 68.9)	7 (5)

Panel A shows the coefficients from Table 3 using model (3), i.e. our "Original Specification", for the main analysis sample of Monthly Household Consumption; the construction of this sample is described in Appendix: *Outcome Selection*. Panel B disaggregates the main analysis sample of Monthly Household Consumption into Monthly Household Total Consumption and Monthly Household Food Consumption categories. For each category, column (1) shows coefficients from (3), i.e. the "Original Specification", estimated using only estimates from the restricted sample of each category (e.g., using only observations of Monthly Household Food Consumption), while column (2) shows coefficients from (3b), our "Pooled Specification", that jointly estimates category-specific effects, estimated using the combined sample of consumption categories. Effects with seven or fewer estimates in gray. 95% credibility intervals in parentheses.

Appendix Table E.2
Treatment Effects on Components of Labor Force Participation (LFP). Table 4 Modified to the "Overlapping Sample": Only Estimated with Studies that Report Each of the Components

	(1) LFP - Binary Outcome *(percentage points)*	(2) Estimates *(Programs)*
Wage Employment	-12.4 (-23.7, 1.3)	25 (12)
Non-Wage Employment	15.7 (7.1, 24.6)	40 (12)

We show coefficients from model (3b), our "Pooled Specification". Results are reported for an "Overlapping Sample" (i.e. studies reporting both Wage and Non-Wage LFP). 95% credibility intervals in parentheses.

Appendix Table E.3
Treatment Effects on Labor Force Participation: Heterogeneity by Disbursement Schedule

	(1)	(2)	(3)	(4)	(5)	(6)
	Predicted Treatment Effect of $100				*Estimates (Programs)*	
	Ongoing Stream	Completed Stream	Lump Sum	Ongoing Stream	Completed Stream	Lump Sum
Panel A. Repeat of Table 4, Panel B						
Labor Force Participation	9.2 (-8.9, 27.1)	28.3 (7.7, 50.4)	13.7 (-2.2, 29.8)	6 (5)	4 (1)	7 (5)
Panel B. Treatment Effect per Monthly Tranche Amount						
Wage Employment	-16.7 (-37.4, 3.3)	-35.8 (-61.0, -11.1)	-3.9 (-16.2, 8.3)	6 (4)	4 (2)	15 (8)
Non-Wage Employment	7.9 (-3.5, 19.2)	2.0 (-14.4, 18.5)	24.4 (14.9, 34.3)	22 (8)	12 (4)	25 (13)
Farming	9.0 (-8.8, 26.8)	-5.8 (-31.4, 19.8)	4.1 (-14.9, 23.2)	8 (6)	5 (3)	6 (3)
Non-Farming	7.1 (-7.6, 22.0)	7.5 (-14.1, 29.3)	31.3 (20.2, 42.9)	14 (8)	7 (4)	19 (12)

Panel A shows the coefficients from Table 5 using model (4) for the main analysis sample of "Any" LFP; the construction of this sample is described in Appendix: *Outcome Selection*. Panel B uses an augmented version of model (4) where we interact disbursement schedule with Wage and Non-Wage categories, estimated on the full sample of Wage and Non-Wage LFP estimates. Within Non-Wage LFP, we further split into Farming and Non-Farming Employment. Effects with four or fewer estimates in gray. 95% credibility intervals in parentheses.

Appendix Table E.4
Heterogeneous Treatment Effects on Labor Force Participation by Gender Targeting

	(1)	(2)	(4)	(5)
	Predicted Treatment Effect of $ 100		*Estimates (Programs)*	
	Not Targeted	Targeted to Women	Not Targeted	Targeted to Women
Panel A. Repeat of Table 8, Column 1, Panel B				
Labor Force Participation	12.0 (-4.0, 27.8)	18.3 (5.3, 32.0)	7 (5)	10 (6)
Panel B. Treatment Effect per Monthly Tranche Amount				
Wage Employment	-10.5 (-20.2, -0.9)	-11.4 (-27.4, 4.2)	8 (5)	17 (7)
Non-Wage Employment	2.6 (-4.2, 9.3)	42.2 (31.8, 53.1)	23 (10)	36 (12)

Panel A shows the coefficients from Table 9 using model (9) for the main analysis sample of "Any" LFP; the construction of this sample is described in Appendix: *Outcome Selection*. Panel B uses an augmented version of model (9) where we interact disbursement schedule with Wage and Non-Wage Employment categories, estimated on the full sample of Wage and Non-Wage LFP estimates. Within Non-Wage LFP, we further split into Farming and Non-Farming Employment. Effects with seven or fewer estimates in gray. 95% credibility intervals in parentheses.

Appendix Table E.5
Treatment Effects on Stock of Total Assets: Alternative Asset Measures

	(1)	(2)	(3)
	Predicted Treatment Effect of $100 Transfer		
	Original Specification	Pooled Specification	Estimates (Programs)
Panel A. Repeat of Table 3, Column 1 (Panel A)			
Stock of Total Assets	19.6 (12.6, 26.8)		60 (28)
Panel B. Treatment Effects per Total Transfer Amount			
Stock of Total Assets *(only using estimates on total assets)*	20.4 (11.6, 29.4)	19.2 (12.6, 26.0)	39 (17)
Stock of Financial Assets	1.8 (1.2, 2.5)	2.7 (-3.3, 8.8)	49 (24)
Stock of Productive Assets	29.5 (14.8, 44.9)	23.1 (15.3, 31.2)	37 (19)
Stock of Durable Assets	6.2 (3.5, 9.1)	6.6 (-5.1, 18.4)	12 (7)

Panel A shows the coefficients from Table 3 using model (3), i.e. our "Original Specification", for the main analysis sample of Stock of Total Assets; the construction of this sample is described in Appendix: *Outcome Selection*. Panel B disaggregates the main analysis sample of Stock of Total Assets into the Stocks of Total, Financial, Productive, and Durable Assets categories. For each category, column (1) shows coefficients from (3), i.e. the "Original Specification", estimated using only estimates from the restricted sample of each category (e.g., using only observations of Durable Assets), while column (2) shows coefficients from (3b), our "Pooled Specification", that jointly estimates category-specific effects, estimated using the combined sample of asset categories. Effects with seven or fewer estimates in gray. 95% credibility intervals in parentheses.

Appendix Table E.6
Treatment Effects on Psychological Well-being z-Scores: Robustness to Inclusion of Zambia CGP Outlier

	(1) Predicted Treatment Effect of $100 Transfer	(2) Estimates (Programs)
Panel A. Repeats of Main Tables. Treatment Effects per Monthly Tranche Amount		
Psychological Well-being z-Score	0.5	56
(Full Sample, i.e. with Zambia CGP; repeat of Table 3, Col. 1)	(0.3, 0.7)	(30)
Psychological Well-being z-Score	1.1	16
(Ongoing Streams, Full Sample, i.e. with Zambia CGP; repeat of Table 4, Col. 1)	(0.7, 1.5)	(10)
Panel B. Treatment Effects per Monthly Tranche Amount		
Psychological Well-being z-Score	0.4	53
(Full Sample without Zambia CGP)	(0.3, 0.5)	(29)
Psychological Well-being z-Score	0.8	13
(Ongoing Stream Programs without Zambia CGP)	(0.5, 1.1)	(9)

Panel B excludes three positive outlier estimates from Zambia's Child Grant Program. These outliers were derived from a binary indicator measuring whether respondents felt happy or happier compared to 12 months prior. We do not extract an equivalent outcome variable to construct our standardized outcome for any other program. 95% credibility intervals in parentheses.

Appendix Table F.1
Program Design Features by Outcome

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		Percentages by Targeting			Percentages by Child or Food Framing		Percentages by Transfer Modality		Percentages by Implementer	
	Count of Estimates (Programs)	No Targeting	Female Targeting	Male Targeting	No Framing	With Framing	Mobile Money or Bank Transfer	Physical Cash	Government	NGO
All Primary Outcomes	638 (72)	56% (74%)	43% (44%)	4% (7%)	77% (74%)	26% (28%)	61% (53%)	39% (46%)	27% (31%)	65% (51%)
Flow Outcomes										
Monthly Household Consumption	82	57%	38%	5%	78%	22%	61%	37%	27%	73%
Monthly Household Food Consumption	49	45%	53%	2%	67%	33%	55%	41%	37%	63%
Monthly Income	88	56%	11%	3%	86%	14%	55%	33%	15%	85%
Hours Worked per Week	25	24%	40%	4%	96%	4%	80%	20%	32%	68%
Labor Force Participation	17	35%	59%	6%	53%	47%	29%	59%	41%	59%
Wage Labor Force Participation	25	68%	32%	0%	88%	12%	76%	24%	24%	76%
Non-Wage Labor Force Participation	62	58%	40%	2%	73%	27%	58%	42%	32%	68%
School Enrollment	26	54%	38%	8%	46%	54%	50%	50%	58%	42%
Food Security z-Score	47	49%	43%	6%	70%	28%	60%	38%	23%	74%
Psychological Well-being z-Score	56	46%	43%	11%	79%	21%	63%	38%	25%	75%
Stock Outcomes										
Stock of Total Assets	60	73%	22%	5%	90%	10%	75%	25%	13%	73%
Stock of Financial Assets	49	73%	20%	6%	84%	16%	69%	31%	10%	80%
Height-for-Age z-Score	32	34%	66%	0%	50%	50%	41%	59%	34%	53%
Weight-for-Age z-Score	15	47%	53%	0%	53%	47%	53%	47%	47%	47%
Stunting (percentage points)	12	0%	100%	0%	8%	92%	25%	75%	50%	50%

The sum of percentages by targeting, framing, modality, or implementer may exceed 100% for programs (in parentheses) because some programs randomize these design features across different treatment arms or let recipients select design features endogenously.

Appendix Table F.2
Administrative Costs

(1) Program ID	(2) Country	(3) Implementer-Treatment Arm	(4) Disbursement Schedule	(5) Administrative Cost	(6) Transfer Amount	(7) Ratio of Admin. Cost/Transf. Amount
27	Kenya	Give Directly (GD)- small	Lump sum, stream	153	664	23%
27	Kenya	GD- large	Lump sum, stream	250	2,214	11%
31	Kenya	International Rescue Committee (IRC)	Lump sum	177	493	36%
35	Liberia	Innovations for Poverty Action (IPA)	Lump sum	16	200	8%
41	Mali	IPA	Lump sum	130	140	93%
45	Morocco	Government	Stream	19	167	11%
55	Rwanda	GD- small	Lump sum, stream	62	104	60%
55	Rwanda	GD- lower-middle	Lump sum, stream	69	211	33%
55	Rwanda	GD- upper-middle	Lump sum, stream	72	295	24%
55	Rwanda	GD- large	Lump sum, stream	87	1,341	6%
56	Rwanda	GD- small	Lump sum	195	799	24%
56	Rwanda	GD- lower-middle	Lump sum	210	1,035	20%
56	Rwanda	GD- upper-middle	Lump sum	220	1,267	17%
56	Rwanda	GD- large	Lump sum	243	1,891	13%
64	Uganda	GD	Lump sum	683	2,651	26%
68	Uganda	Village Enterprises	Lump sum	83	242	35%
69	Uganda	World Food Programme (WFP)	Stream	65	186	35%

Costs are reported in 2010 USD PPP per recipient household. Specific citations associated with each Program ID reported in Table A.1.

Appendix Table F.3a

Reported Treatment Effects per $100 Monthly Tranche- Stream UCT Programs

(1) Program ID	(2) Monthly Tranche Amount	(3) Months Since First Transfer	(4) Completion Status	(5) TE Reported by Sub-group Only	(6) Monthly Household Total Consumption	(7) Monthly Household Food Consumption	(8) Monthly Income	(9) Hours Worked per Week	(10) Labor Force Participation (percentage points)	(11) School Enrollment (percentage points)	(12) Food Security z-Score	(13) Psychological Well-being z-Score
2	61	23	Ongoing	North							0.2 (0.1)	
2	61	23	Ongoing	South							0.2 (0.1)	
3	16	14	Ongoing									2.0 (0.6)
4	25	4	Ongoing		46.0 (42.3)							
4	33	3	Ongoing									0.2 (0.2)
7	18	24	Ongoing								1.1 (2.0)	
8	10	12	Ongoing							6.0 (3.1)		
8	10	24	Ongoing							10.1 (4.4)		
10	98	21	Ongoing				6.2 (5.4)	1.5 (1.4)				
10	114	12	Ongoing				-7.6 (20.7)	-0.6 (1.8)				
10	114	12	Ongoing									0.05 (0.07)
10	121	17	Ongoing				9.2 (10.7)	4.3 (2.0)				
10	228	12	Ongoing				17.8 (18.9)	1.0 (1.0)				
10	228	12	Ongoing									0.03 (0.04)
12	35	23	Ongoing									-0.2 (0.2)
12	36	18	Ongoing							1.7 (0.6)		
12	36	19	Ongoing							2.9 (1.4)		
12	41	15	Ongoing									0.2 (0.3)
17	63	12	Ongoing		122.8 (62.8)	71.8 (22.1)						
18	22	11	Ongoing			67.4 (22.5)						0.4 (0.3)
18	22	23	Ongoing			87.1 (20.7)						1.1 (0.7)
23	68	14	Ongoing		95.2 (24.9)							0.3 (0.1)
24	48	48	Ongoing									0.21 (0.09)
24	53	24	Ongoing							0.4 (0.3)		
25	11	36	Ongoing		142.7 (128.9)						-0.6 (1.1)	-0.6 (0.7)
25	34	11	Ongoing		48.0 (24.5)						1.2 (0.3)	0.6 (0.3)
25	40	36	Ongoing		32.3 (36.6)						-0.10 (0.35)	0.1 (0.2)
25	145	10	Ongoing		26.5 (6.8)						0.30 (0.08)	0.33 (0.08)
27	146	27	Ongoing								0.16 (0.03)	
27	146	27	Ongoing								0.07 (0.03)	
27	169	27	Completed				-3.1 (3.2)					
27	195	27	Completed				-6.0 (2.7)					
27	197	20	Completed				-8.8 (4.7)					
27	197	20	Completed				10.6 (7.6)					
29	29	12	Ongoing			119.4 (51.6)						
29	35	24	Ongoing		88.8 (34.5)	100.7 (50.3)				-3.4 (1.8)		
33	53	24	Ongoing		33.7 (21.5)	28.5 (17.2)				1.7 (0.8)		
33	59	24	Completed						-0.8 (2.2)			
34	11	20	Ongoing								2.8 (0.7)	
34	11	20	Ongoing				18.0 (23.4)					
34	21	20	Ongoing								1.7 (0.3)	
34	21	20	Ongoing		34.7 (24.5)	-3.5 (10.7)						
34	22	20	Ongoing				3.7 (7.2)					
34	32	20	Ongoing								1.3 (0.2)	
34	33	20	Ongoing				1.6 (5.8)					
34	42	20	Ongoing		24.6 (10.2)	4.8 (6.3)						
34	63	20	Ongoing		25.2 (6.1)	3.6 (3.4)						
36	55	18	Ongoing								-2.7 (3.1)	
39	11	24	Ongoing				98.7 (27.9)					
39	15	12	Ongoing							8.1 (1.4)	2.2 (0.6)	2.9 (0.5)
39	15	18	Ongoing									3.2 (0.6)
39	17	12	Ongoing		72.4 (50.6)	42.9 (41.3)						
39	17	24	Ongoing		179.6 (43.2)	147.9 (34.5)				7.2 (2.8)	2.1 (0.5)	
39	20	24	Completed						1.1 (1.3)			
40	18	24	Ongoing							0 (0.2)		
40	22	12	Ongoing									1.4 (0.4)
40	22	24	Ongoing		-14.7 (56.9)							0.3 (0.5)
40	43	12	Ongoing		46.1 (17.0)					0.7 (0.6)		
42	14	24	Ongoing		259.9 (159.0)				2.0 (1.9)	1.3 (1.4)		1.4 (0.8)
42	43	24	Ongoing							-0.09 (0.65)		
43	29	26	Ongoing			0.1 (11.3)					2.3 (0.4)	
43	63	14	Ongoing			-5.9 (4.9)					0.7 (0.2)	
44	23	12	Ongoing		110.4 (100.0)	74.5 (62.6)				0.7 (1.8)		
45	40	18	Ongoing							1.8 (0.4)		
47	20	30	Ongoing			72.6 (24.1)						
48	24	4	Ongoing		-15.5 (149.3)		155.1 (88.0)					
49	42	24	Completed		59.5 (29.3)	39.4 (21.9)	-18.9 (27.0)				0.3 (0.2)	
50	18	30	Ongoing	Female								-0.21 (0.10)
50	18	30	Ongoing	Male								0.3 (0.1)
51	52	6	Ongoing		-20.0 (6.6)		40.0 (23.7)	3.8 (1.1)	0.7 (0.3)			0.4 (0.2)
51	52	12	Ongoing		60.0 (12.8)		112.0 (17.4)	5.2 (0.8)	1.0 (0.3)			0.6 (0.2)
52	10	48	Ongoing	Female			191.1 (51.8)		10.8 (1.6)			
52	10	48	Ongoing	Male			155.7 (97.0)		0.3 (0.2)			
52	10	48	Ongoing		262.0 (133.3)						2.6 (0.5)	
52	18	24	Completed		75.2 (56.9)	98.2 (52.2)						
52	20	24	Ongoing	Female			89.5 (31.9)		3.0 (1.0)			
52	20	24	Ongoing	Male			48.0 (83.0)		0.2 (0.1)			
52	20	24	Ongoing		230.3 (82.5)						0.6 (0.2)	
52	20	24	Ongoing			93.8 (41.3)						
52	20	12	Completed		51.4 (46.8)	118.2 (41.9)						
54	178	12	Ongoing		84.7 (6.8)	58.3 (6.3)	18.1 (24.2)	-0.2 (0.3)				0.27 (0.02)
55	17	12	Ongoing		370.5 (817.0)							
55	112	12	Ongoing		367.2 (133.6)							
61	19	24	Ongoing	Female		83.9 (42.8)						
61	19	24	Ongoing		163.9 (102.0)						144.1 (59.8)	
69	15	12	Ongoing		265.6 (70.3)	237.2 (73.8)						
71	13	82	Ongoing								0.3 (1.0)	
71	20	32	Ongoing									0.05 (0.10)
71	20	24	Completed						1.4 (0.6)			
71	21	24	Ongoing		131.3 (29.5)	96.8 (21.7)	58.2 (24.0)					
71	21	36	Ongoing		106.7 (24.7)	76.3 (18.7)	22.0 (20.9)					
71	21	48	Ongoing			48.5 (19.8)						
71	23	36	Ongoing							1.6 (1.2)	2.4 (0.4)	
71	23	24	Ongoing							0.7 (1.1)	1.8 (0.4)	7.9 (0.7)
71	23	48	Ongoing								1.9 (0.5)	1.1 (0.2)
72	21	24	Ongoing		134.5 (33.8)	121.4 (34.0)						
72	21	36	Ongoing		190.6 (49.4)	172.0 (44.7)						

All currency values reported in 2010 USD PPP. Standard errors reported in parentheses. Specific citations associated with each Program ID reported in Table A.1.

Appendix Table F.3b

Reported Treatment Effects per $100 Monthly Tranche- Stream UCT Programs

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Program ID	Monthly Tranche Amount	Months Since First Transfer	Completion Status	TE Reported by Sub-group Only	Stock of Total Assets	Stock of Financial Assets	Height-for-Age z-Score	Weight-for-Age z-Score	Stunting (basis points)
2	61	23	Ongoing	North					
2	61	23	Ongoing	South					
3	16	14	Ongoing						
4	25	4	Ongoing			13.1 (6.3)			
4	33	3	Ongoing						
7	18	24	Ongoing				-0.003 (0.010)		
8	10	12	Ongoing		1.4 (57.9)		1.8 (1.8)	1.4 (1.1)	
8	10	24	Ongoing		13.2 (62.0)		-1.1 (1.6)	-1.9 (1.5)	
10	98	21	Ongoing		68.8 (23.9)	-5.7 (3.3)			
10	114	12	Ongoing		-64.3 (115.9)	50.4 (599.1)			
10	114	12	Ongoing						
10	121	17	Ongoing		250.6 (261.0)	12.7 (7.6)			
10	228	12	Ongoing		392.7 (258.0)	2633.1 (4939.7			
10	228	12	Ongoing						
12	35	23	Ongoing				0.03 (0.27)		
12	36	18	Ongoing						
12	36	19	Ongoing						
12	41	15	Ongoing						
17	63	12	Ongoing						
18	22	11	Ongoing				0.02 (0.23)	0.009 (0.182)	-0.009 (0.091)
18	22	23	Ongoing						
23	68	14	Ongoing		392.5 (67.9)				
24	48	48	Ongoing						
24	53	24	Ongoing						
25	11	36	Ongoing		3618.8 (576.4				
25	34	11	Ongoing		770.4 (108.5)				
25	40	36	Ongoing		1055.5 (170.9				
25	145	10	Ongoing		395.5 (33.4)				
27	146	27	Ongoing						
27	146	27	Ongoing						
27	169	27	Completed						
27	195	27	Completed						
27	197	20	Completed						
27	197	20	Completed						
29	29	12	Ongoing						
29	35	24	Ongoing						
33	53	24	Ongoing			-11.9 (12.8)			
33	59	24	Completed						
34	11	20	Ongoing						
34	11	20	Ongoing						
34	21	20	Ongoing						
34	21	20	Ongoing		174.1 (306.3)				
34	22	20	Ongoing						
34	32	20	Ongoing						
34	33	20	Ongoing						
34	42	20	Ongoing		23.3 (169.9)				
34	63	20	Ongoing		89.0 (102.2)				
36	55	18	Ongoing						
39	11	24	Ongoing						
39	15	12	Ongoing						
39	15	18	Ongoing						
39	17	12	Ongoing						
39	17	24	Ongoing				-0.7 (0.5)	0.08 (0.49)	0.1 (0.3)
39	20	24	Completed						
40	18	24	Ongoing						
40	22	12	Ongoing						
40	22	24	Ongoing						
40	43	12	Ongoing						
42	14	24	Ongoing		212.2 (103.7)				
42	43	24	Ongoing						
43	29	26	Ongoing						
43	63	14	Ongoing						
44	23	12	Ongoing						
45	40	18	Ongoing						
47	20	30	Ongoing						
48	24	4	Ongoing				-0.3 (0.4)	0.04 (0.29)	0.03 (0.11)
49	42	24	Completed		14.3 (35.9)				
50	18	30	Ongoing	Female					
50	18	30	Ongoing	Male					
51	52	6	Ongoing			52.0 (9.7)			
51	52	12	Ongoing			66.0 (11.3)			
52	10	48	Ongoing	Female					
52	10	48	Ongoing	Male					
52	10	48	Ongoing			530.5 (200.5)			-0.5 (0.3)
52	18	24	Completed				0.6 (0.5)		
52	20	24	Ongoing	Female					
52	20	24	Ongoing	Male					
52	20	24	Ongoing			-255.5 (215.9)			-0.3 (0.1)
52	20	24	Ongoing						
52	20	12	Completed				1.3 (0.5)		
54	178	12	Ongoing						
55	17	12	Ongoing		2.4 (50.7)	-50.8 (32.7)			
55	112	12	Ongoing		6.2 (11.4)	0.3 (5.5)			
61	19	24	Ongoing	Female					
61	19	24	Ongoing				1.3 (0.7)		-0.3 (0.1)
69	15	12	Ongoing						0.09 (0.23)
71	13	82	Ongoing						
71	20	32	Ongoing						
71	20	24	Completed						
71	21	24	Ongoing		9.0 (8.9)	90.7 (15.7)			
71	21	36	Ongoing						
71	21	48	Ongoing						
71	23	36	Ongoing				-0.4 (0.4)		0.2 (0.1)
71	23	24	Ongoing				0.04 (0.31)	0.6 (0.3)	0.09 (0.13)
71	23	48	Ongoing				-0.3 (0.5)		0.02 (0.16)
72	21	24	Ongoing						

All currency values reported in 2010 USD PPP. Standard errors reported in parentheses. Specific citations associated with each Program ID

Appendix Table F.4a

Reported Treatment Effects per *PPP$* 100 Total Transfer — Lump Sum UCT Programs

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Program ID	Total Transfer Amount	Months Since First Transfer	TE Reported by Sub-group Only	Monthly Household Consumption	Monthly Food Consumption	Monthly Income	Food Security z-Score	Hours Worked per Week	Labor Force Participation (percentage points)	School Enrollment (percentage points)	Psychological Well-being z-Score
1	1,717	23				11.8 (1.7)					
1	1,744	23									
5	29	16									-0.14 (0.07)
5	87	16									-0.05 (0.02)
6	8,484	9				-0.6 (0.2)					
9	160	2					0.002 (0.017)		0.2 (1.2)		0.02 (0.02)
13	682	16	Female						3.1 (0.4)		0.007 (0.010)
13	682	16	Male						0 (0.004)		0.02 (0.01)
13	682	16									
13	825	16	Female	-4.3 (7.2)		4.3 (1.6)					
13	825	16	Male	3.5 (13.3)		-0.5 (4.7)					
14	300	2				-14.6 (14.2)					
14	300	8									
14	300	14				-37.3 (20.2)					
15	284	3	Female			7.2 (5.8)					
15	284	3	Male			3.2 (9.5)					
15	284	6	Female			-0.1 (6.5)					
15	284	6	Male			10.1 (10.8)					
15	284	9	Male			7.9 (12.7)					
15	284	9	Female			1.5 (7.8)					
15	284	11	Female	6.3 (2.4)	10.3 (6.6)						
15	284	11	Male	3.4 (2.7)	10.6 (8.4)						
15	284	12	Female			6.3 (10.2)					
15	284	12	Male			36.2 (13.1)					
15	284	34				14.2 (16.6)					
16	407	24		0.9 (8.1)		1.3 (1.8)					
16	795	24					0.02 (0.01)				
19	300	12				9.4 (6.8)					
20	35	1					-0.07 (0.41)				0.6 (0.3)
20	35	3					-0.7 (0.4)				0.2 (0.3)
22	92	2			12.2 (6.3)						
22	98	2				9.8 (2.5)					
23	958	14		3.2 (1.9)							0.026 (0.008)
25	384	7		5.7 (2.6)			0.04 (0.03)				0.05 (0.02)
25	384	9				0 (0.9)					
25	384	36	Male	6.6 (4)			-0.01 (0.03)				-0.01 (0.02)
26	1,723	19		1.3 (0.3)	0.3 (0.2)	0.35 (0.2)					
27	4,336	20				0.3 (0.2)		0.0002 (0.0001)			
27	4,356	27				0 (0.1)	0.003 (0.001)	0.0005 (0.0007)			
28	1,942	19		1.2 (0.3)		0.8 (0.3)					0.004 (0.002)
30	321	12		0.3 (14.7)	-3 (4.9)	24.75 (22.49)	0.08 (0.02)				0.009 (0.028)
31	480	9							0.5 (1.0)		
31	480	18							1.2 (0.9)		
31	505	9				5.7 (2.1)					
31	505	18				-0.1 (2.2)					
32	294	1								2.6 (0.5)	
34	211	20					0.04 (0.03)				
34	217	20				1.2 (1.2)					
34	422	20					0.05 (0.02)				
34	422	20		0.3 (1.2)	-0.8 (0.5)						
34	434	20				0.6 (0.4)					
34	632	20					0.08 (0.01)				
34	651	20				-0.1 (0.3)					
34	845	20		1.7 (0.5)	0.5 (0.3)						
34	1,267	20		0.8 (0.3)	0.2 (0.2)						
35	200	1									0.07 (0.05)
35	200	13		-2.8 (3.9)		2.9 (3.6)					-0.02 (0.05)
35	352	13									
37	516	23					0.03 (0.01)	0 (0.01)		-0.4 (0.2)	0.008 (0.012)
37	557	23		0 (0.3)	-0.1 (0.3)	1 (0.5)					
37	1,032	23					0.02 (0.01)	0.003 (0.003)		-0.001 (0.001)	0.011 (0.006)
37	1,115	23		0.1 (0.2)	0.2 (0.1)	-0.1 (0.2)					
37	1,549	23					0.011 (0.005)	0.003 (0.002)		-0.06 (0.06)	0.010 (0.004)
37	1,672	23		0.1 (0.1)	0.2 (0.1)	0.1 (0.2)					
38	204	9				1 (0.3)					
38	225	9		0.1 (0.1)	0.1 (0.1)			-0.02 (0.01)			
38	225	21		0.1 (0.1)	0.1 (0)						
41	136	12		11.7 (5.4)	5.6 (2.3)	2.5 (1)					
41	136	24				3.7 (1.1)					
41	136	84				-0.3 (2)					
41	173	12									
41	285	12									
46	227	5									
55	204	12		50.5 (112.3)							
55	204	12									
55	1,341	12		33.5 (16.8)							
55	1,341	12									
56	761	14									0.05 (0.01)
56	801	14		3 (1.2)		1.9 (0.9)					
56	983	14									0.05 (0.01)
56	1,035	14		3.1 (1)		2.1 (0.7)					
56	1,202	14									0.040 (0.007)
56	1,265	14		2.2 (0.7)		1.8 (0.6)					
56	1,795	14									0.031 (0.005)
56	1,890	14		2.3 (0.4)		0.8 (0.4)					
57	379	9									
57	379	21									
58	1,313	12		17.8 (7.7)	5.9 (2.6)	0 (1.6)	0.002 (0.008)				
59	263	12	Female			0.6 (1.8)					
59	263	12	Male			4.3 (1.9)					
59	263	24	Female			1.4 (3)					
59	263	24	Male			4.2 (2.7)					
59	263	36	Female			0 (2.9)					
59	263	36	Male			5 (2.7)					
59	263	66	Female			-1.9 (3.07)					
59	263	66	Male			8.1 (4.1)					
60	529	16		2.6 (2.9)	1.3 (1.9)	-8.8 (16.2)					
62	628	27		13.9 (5.8)	8.4 (2.5)	5.4 (4.7)					
62	667	27					0.01 (0.01)	0.004 (0.002)			0.027 (0.006)
63	279	12					0.1 (0.1)		2.2 (1.1)	-0.4 (0.7)	0.12 (0.02)
63	293	12		9.1 (3.7)	2.3 (1.9)	1.4 (3)					
64	2,571	18	Female		0.7 (0.1)						
64	2,571	18		3.5 (0.3)		1 (0.2)	0.018 (0.003)				
65	308	18	Bank Transfer			111.3 (141.9)					
65	308	18	Physical Cash			-26.9 (181.7)					
65	308	48	Bank Transfer			2.5 (137.3)					
65	308	48	Physical Cash			0.1 (144.4)					
66	2,406	21					0.004 (0.003)		0.2 (0.1)	-0.00008 (0.0017)	0.012 (0.003)
66	2,485	21		3.2 (1.2)	2.1 (0.7)						
67	440	6	Female								
67	440	6	Male								
67	440	6				27.8 (17.9)					
67	440	9	Female								
67	440	9	Male								
67	440	9				-39.2 (16.4)					
67	440	10	Female	-30.9 (15.1)							
67	440	10	Male	-5.1 (34.3)							
67	440	24	Female	37 (19.9)							
67	440	24	Male	-42.2 (40.9)							
68	242	15									
68	242	21		-2.6 (2.9)			0.01 (0.02)				-0.04 (0.03)
68	242	27									
70	773	24									
70	773	48									
70	773	108									-0.007 (0.006)
70	924	48			2.2 (0.8)						
70	925	24				2.2 (0.6)					
70	925	48		3.3 (1.2)		2.8 (0.7)					
70	925	108		0.4 (1)		0.6 (1.3)					
70	925	146				1.8 (1)					

All currency values reported in 2010 USD PPP. Standard errors reported in parentheses. Specific citations associated with each Program ID reported in Table A.1.

Appendix Table F.4b

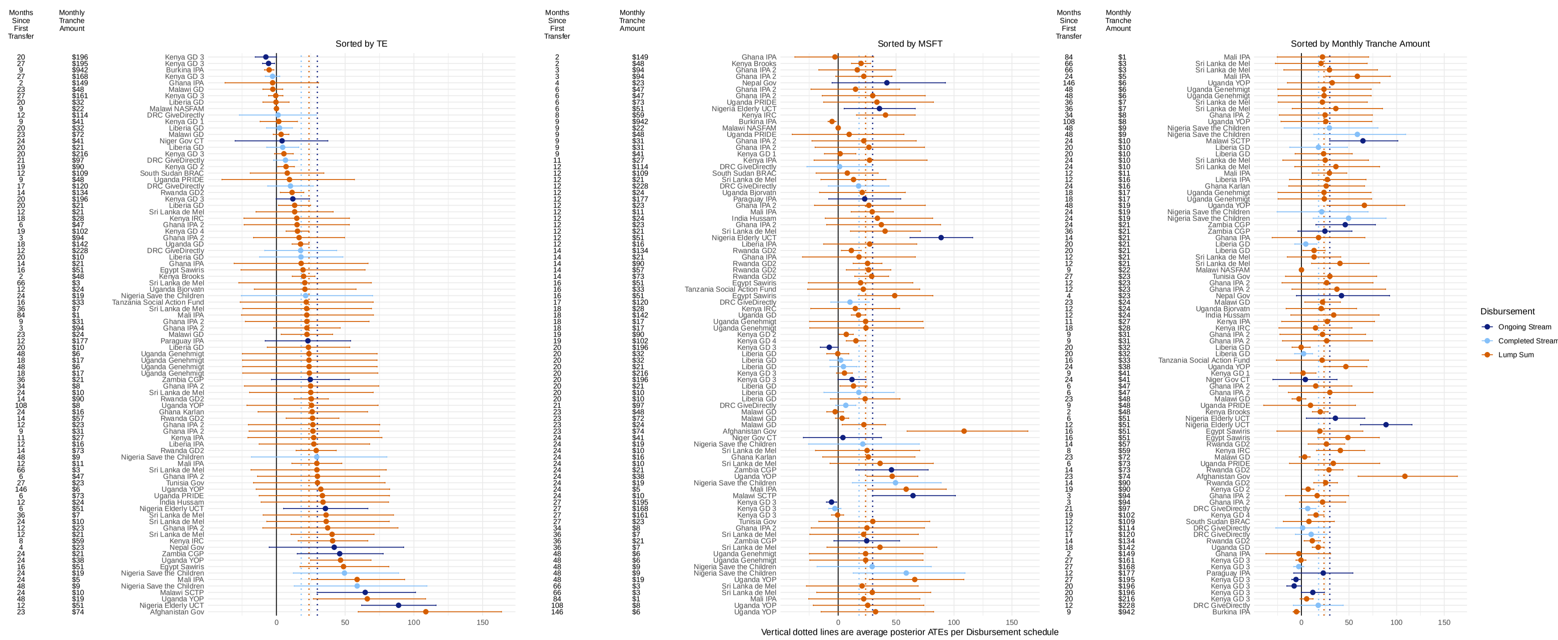
Reported Treatment Effects per 100 USD Total Transfer- Lump Sum UCT Programs

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Program ID	Total Transfer Amount	Months Since First Transfer	TE Reported by Sub-group Only	Stock of Total Assets	Stock of Financial Assets	Height-for-Age z-Score	Weight-for-Age z-Score	Stunting (basis points)
1	1,717	23						
1	1,744	23						
5	29	16						
5	87	16						
6	8,484	9						
9	160	2						
13	682	16	Female					
13	682	16	Male					
13	682	16						
13	825	16	Female		14.3 (16.1)			
13	825	16	Male		6.3 (2.7)			
14	300	2			5.8 (15.5)			
14	300	8						
14	300	14			3.3 (21.1)			
15	284	3	Female					
15	284	3	Male					
15	284	6	Female					
15	284	6	Male					
15	284	9	Male					
15	284	9	Female					
15	284	11	Female					
15	284	11	Male					
15	284	12	Female					
15	284	12	Male					
15	284	34						
16	407	24		144.3 (63.5)				
16	795	24						
19	300	12						
20	35	1						
20	35	3						
22	92	2						
22	98	2						
23	958	14		22.8 (4.5)				
25	384	7		90.5 (9.8)				
25	384	9			2.5 (0.6)			
25	384	36	Male	106.6 (18.5)				
26	1,723	19		9.6 (0.7)				
27	4,336	20						
27	4,356	27						
28	1,942	19		18.1 (2.1)	1.3 (0.5)			
30	321	12			84.3 (100.9)			
31	480	9						
31	480	18						
31	505	9						
31	505	18						
32	294	1						
34	211	20						
34	217	20						
34	422	20						
34	422	20		29.3 (15.5)				
34	434	20						
34	632	20						
34	651	20						
34	845	20		28.1 (8.6)				
34	1,267	20		9.6 (5.2)				
35	200	1						
35	200	13		9.7 (7.6)				
35	352	13			0.6 (2.9)			
37	516	23				0.004 (0.021)	0.006 (0.017)	
37	557	23		2.2 (3.8)	0.8 (0.4)			
37	1,032	23				0.009 (0.015)	-0.008 (0.007)	
37	1,115	23		6.0 (1.9)	0.1 (0.2)			
37	1,549	23				0.011 (0.009)	0.002 (0.006)	
37	1,672	23		7.7 (1.7)	0.9 (0.5)			
38	204	9						
38	225	9		2.5 (142.0)	11.3 (71.1)			
38	225	21		3.3 (148.5)	4.3 (77.2)			
41	136	12						
41	136	24						
41	136	84						
41	173	12						
41	285	12		182.1 (66.9)				
46	227	5			0.027 (0.007)			
55	204	12						
55	204	12		-4.2 (9.1)	2.2 (4.1)			
55	1,341	12						
55	1,341	12		2.1 (1.4)	0.04 (0.92)			
56	761	14			2.1 (0.7)			
56	801	14		0.6 (2.1)				
56	983	14			2.0 (0.7)			
56	1,035	14		3.3 (1.2)				
56	1,202	14			2.0 (0.5)			
56	1,265	14		3.0 (0.9)				
56	1,795	14			1.2 (0.4)			
56	1,890	14		1.7 (0.6)				
57	379	9		115.6 (126.8)				
57	379	21		24.1 (96.0)				
58	1,313	12		-4.1 (6.3)	3.0 (1.3)			
59	263	12	Female					
59	263	12	Male					
59	263	24	Female					
59	263	24	Male					
59	263	36	Female					
59	263	36	Male					
59	263	66	Female					
59	263	66	Male					
60	529	16		10.1 (8.6)				
62	628	27		6.0 (4.7)				
62	667	27						
63	279	12						
63	293	12		2.3 (0.9)				
64	2,571	18	Female					
64	2,571	18		115.1 (12.6)				
65	308	18	Bank Transfer	234.0 (203.7)	203.4 (170.3)			
65	308	18	Physical Cash	-13.4 (133.4)	9.1 (192.3)			
65	308	48	Bank Transfer	184.8 (238.3)	260.2 (156.5)			
65	308	48	Physical Cash	36.5 (247.2)	185.1 (327.0)			
66	2,406	21						
66	2,485	21		138.6 (138.6)	2.4 (0.8)			
67	440	6	Female		10.2 (7.0)			
67	440	6	Male		-6.8 (24.0)			
67	440	6						
67	440	9	Female		-8.2 (8.5)			
67	440	9	Male		-9.4 (31.7)			
67	440	9						
67	440	10	Female	82.1 (123.8)				
67	440	10	Male	321.3 (414.7)				
67	440	24	Female	-156.9 (113.3)				
67	440	24	Male	-45.1 (260.2)				
68	242	15			2.8 (0.3)			
68	242	21		5.1 (2.7)				
68	242	27			4.5 (0.5)			
70	773	24						
70	773	48						
70	773	108						
70	924	48						
70	925	24		57.4 (11.9)				
70	925	48		34.0 (9.5)				
70	925	108						
70	925	146			20.1 (9.8)			

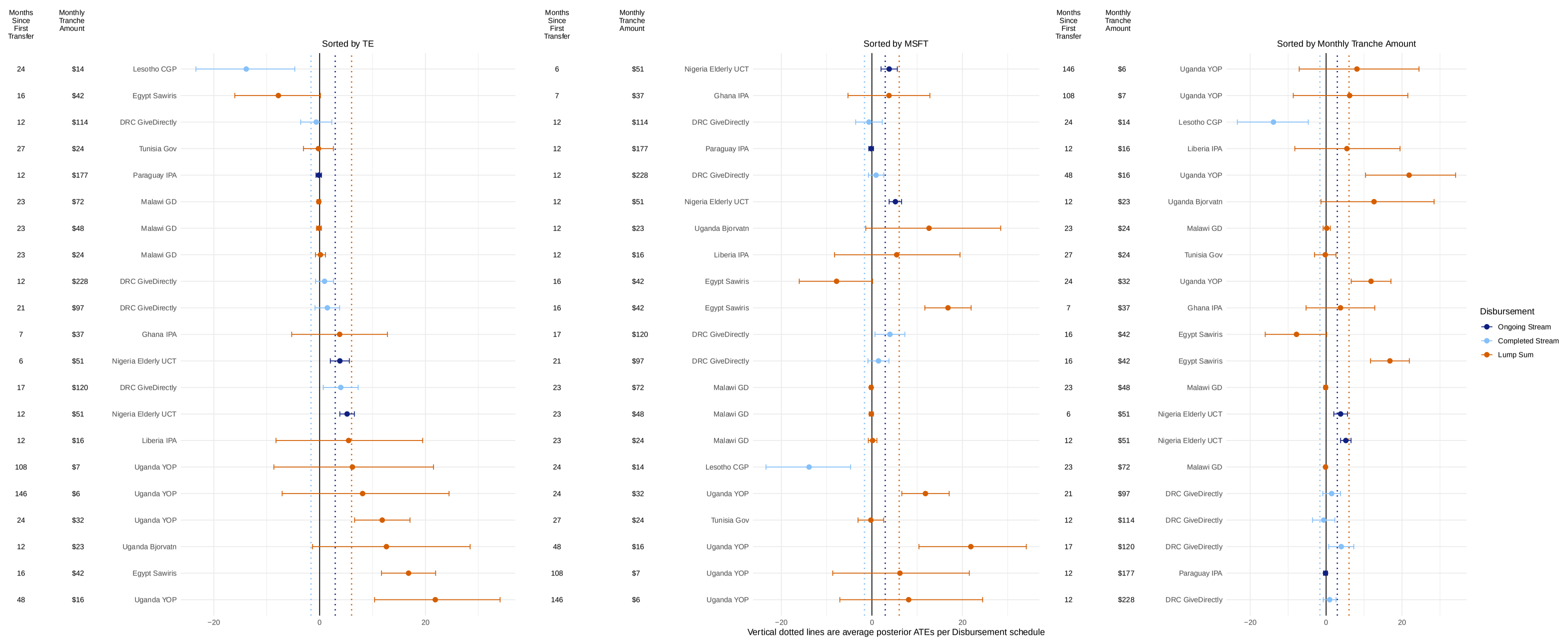
All currency values reported in 2010 USD PPP. Standard errors reported in parentheses. Specific citations associated with each Program ID reported in Table A.1. No lump sum programs in our sample report treatment effects on stunting. Column 10 reports basis points (100 basis points = 1 percentage point).

Appendix Figure 1: Food Consumption Forest Plots

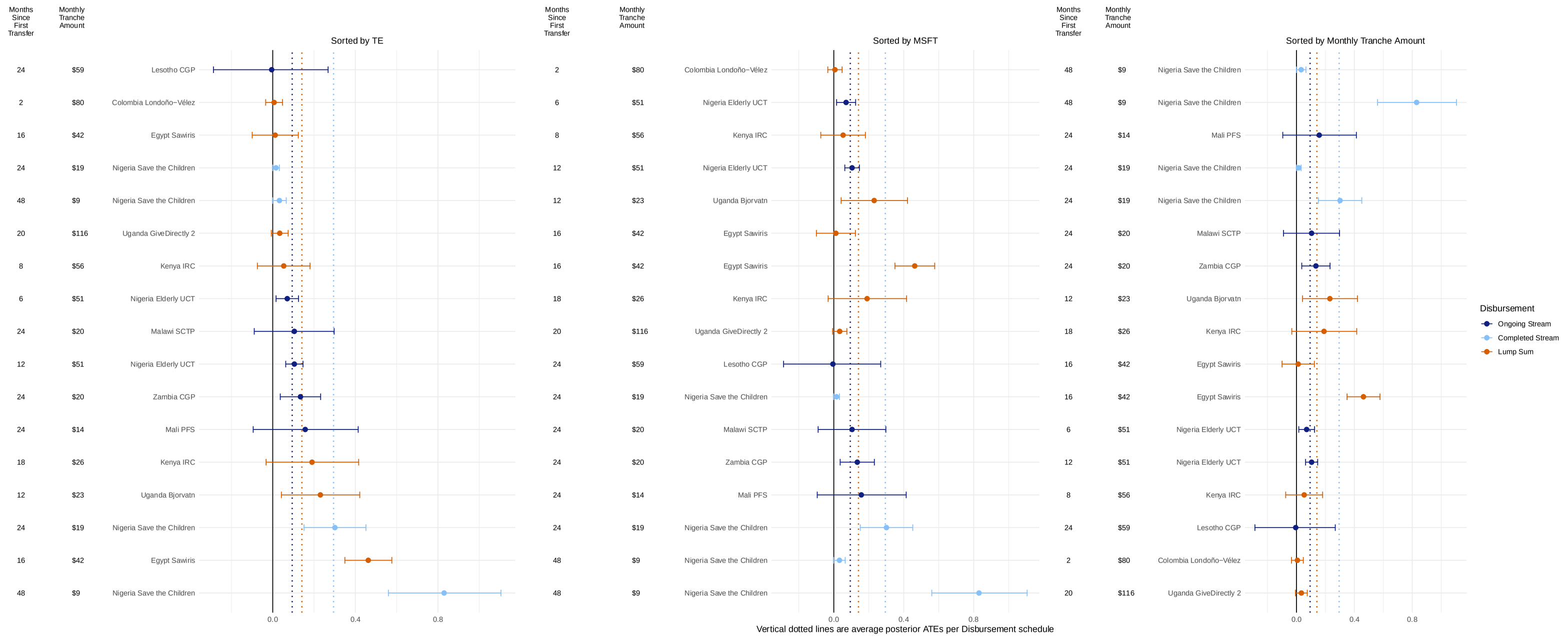
Appendix Figure 2: Monthly Income Forest Plots



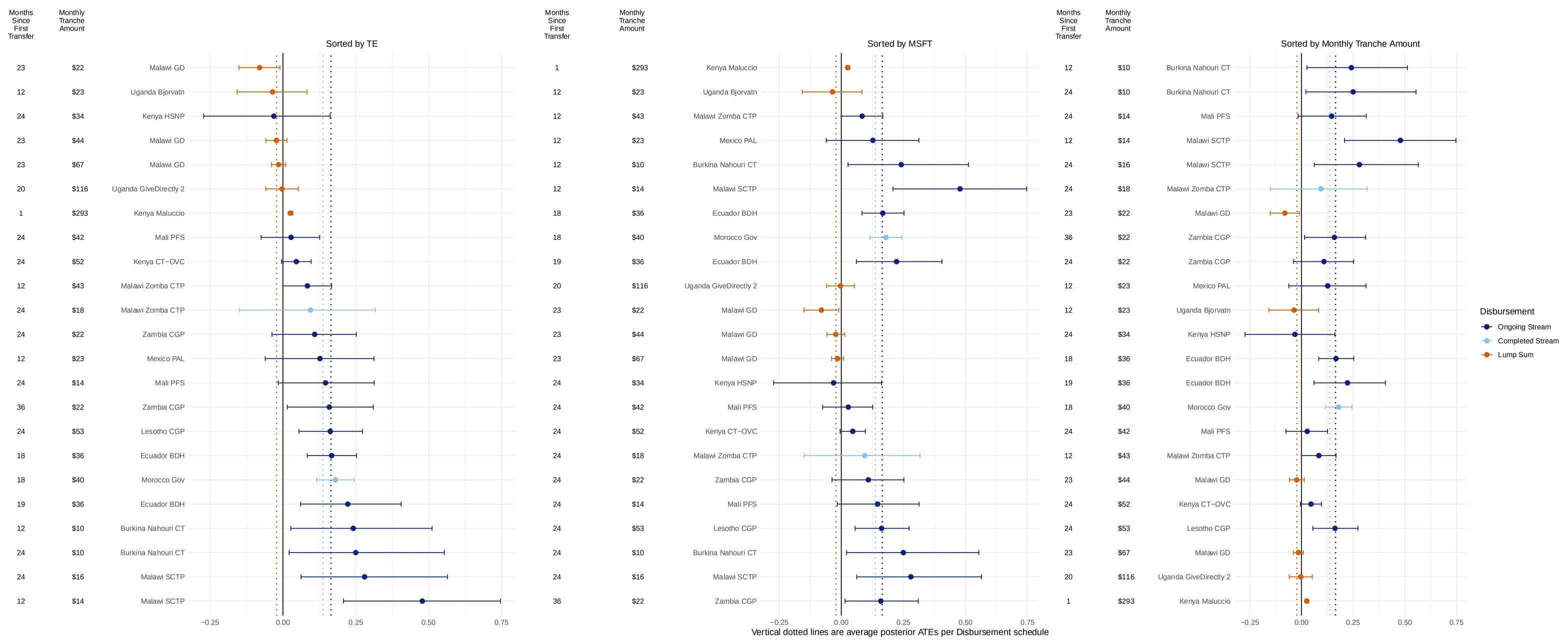
Appendix Figure 3: Hours Worked per Week Forest Plots



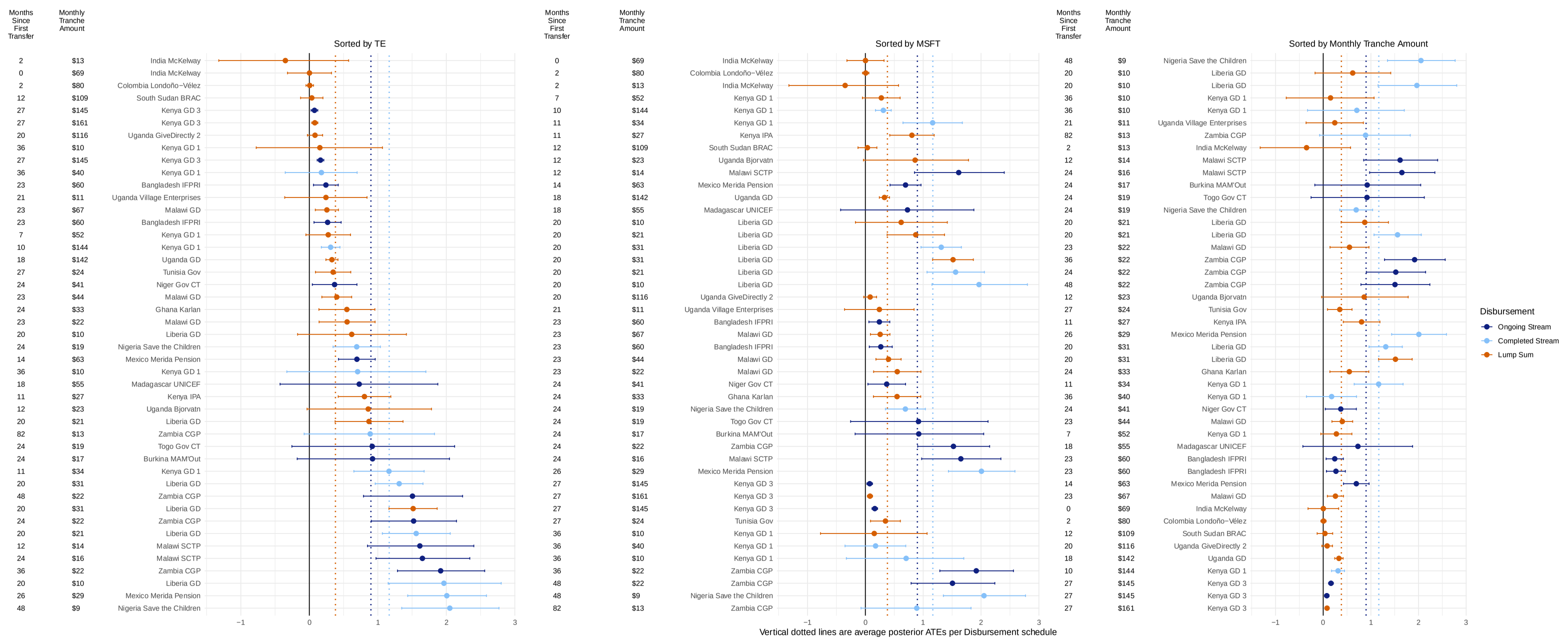
Appendix Figure 4: Labor Force Participation Forest Plots



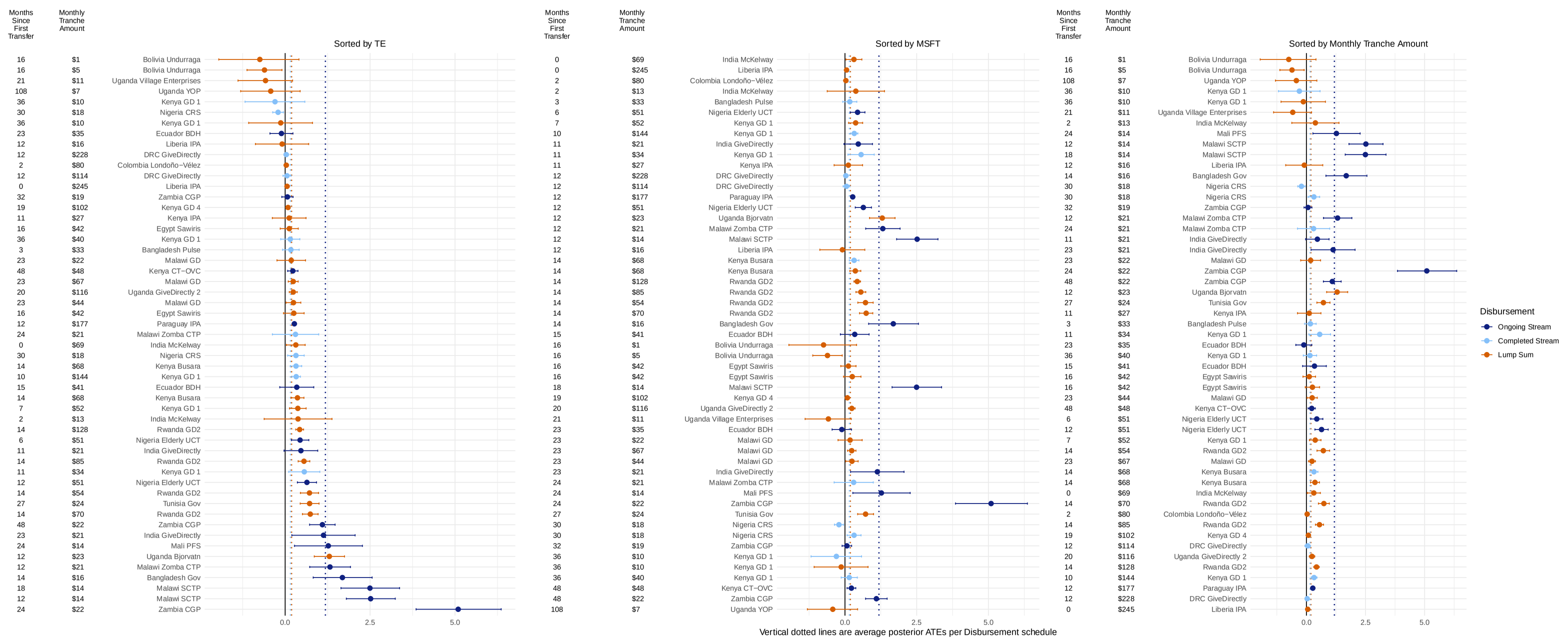
Appendix Figure 5: School Enrollment Forest Plots



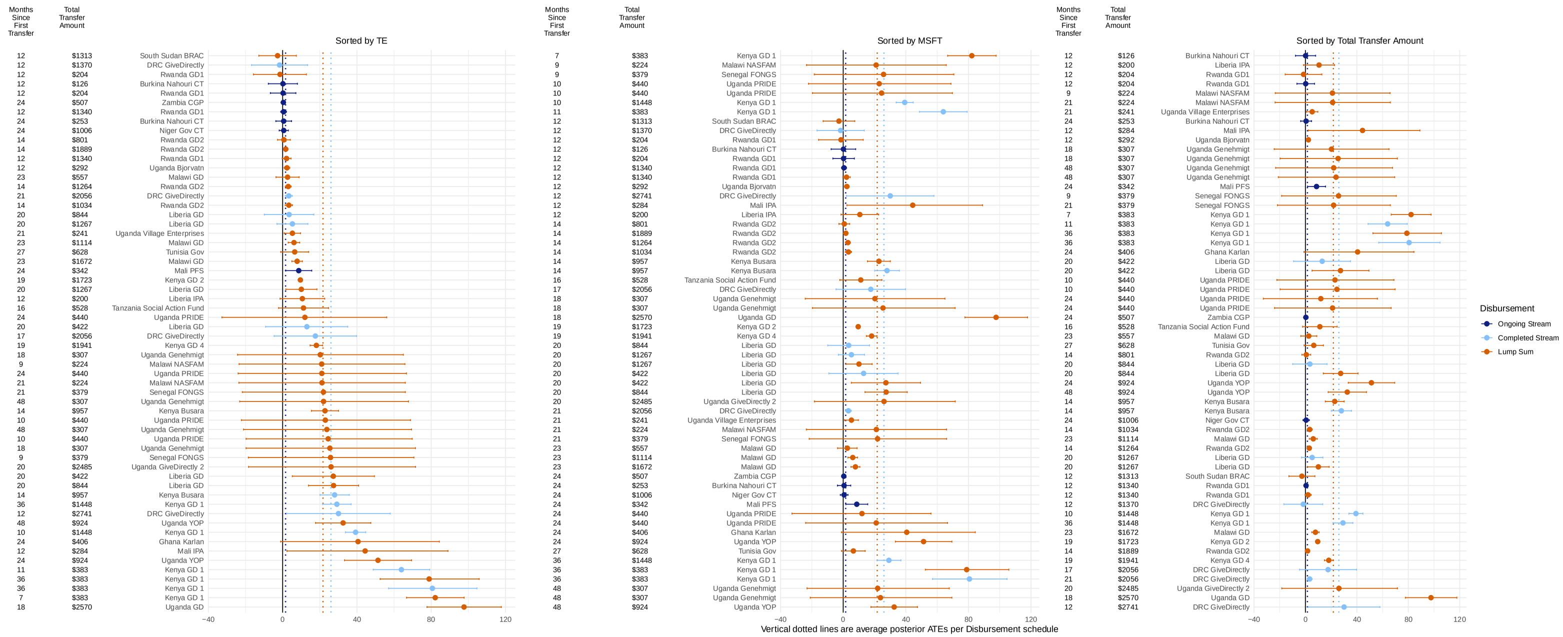
Appendix Figure 6: Food Security Forest Plots



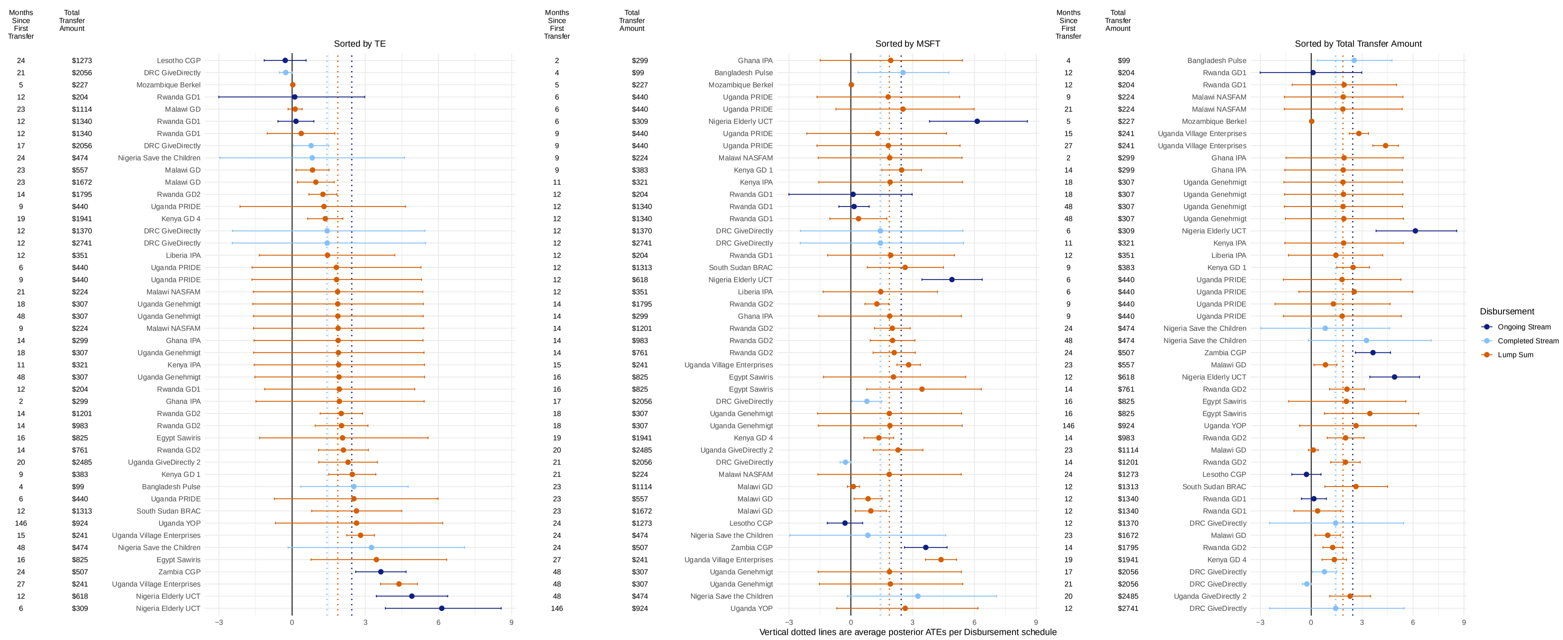
Appendix Figure 7: Pyschological Well-Being Forest Plots



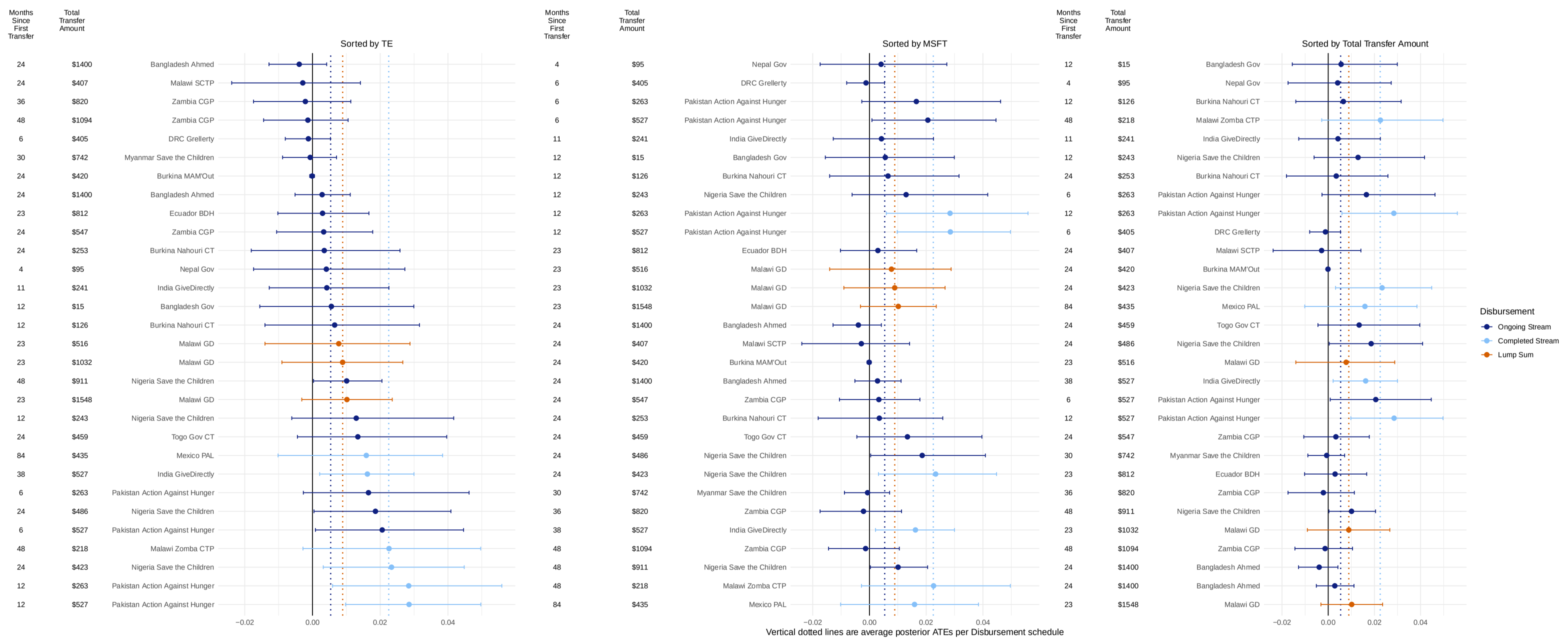
Appendix Figure 8: Stock of Total Assets Forest Plots



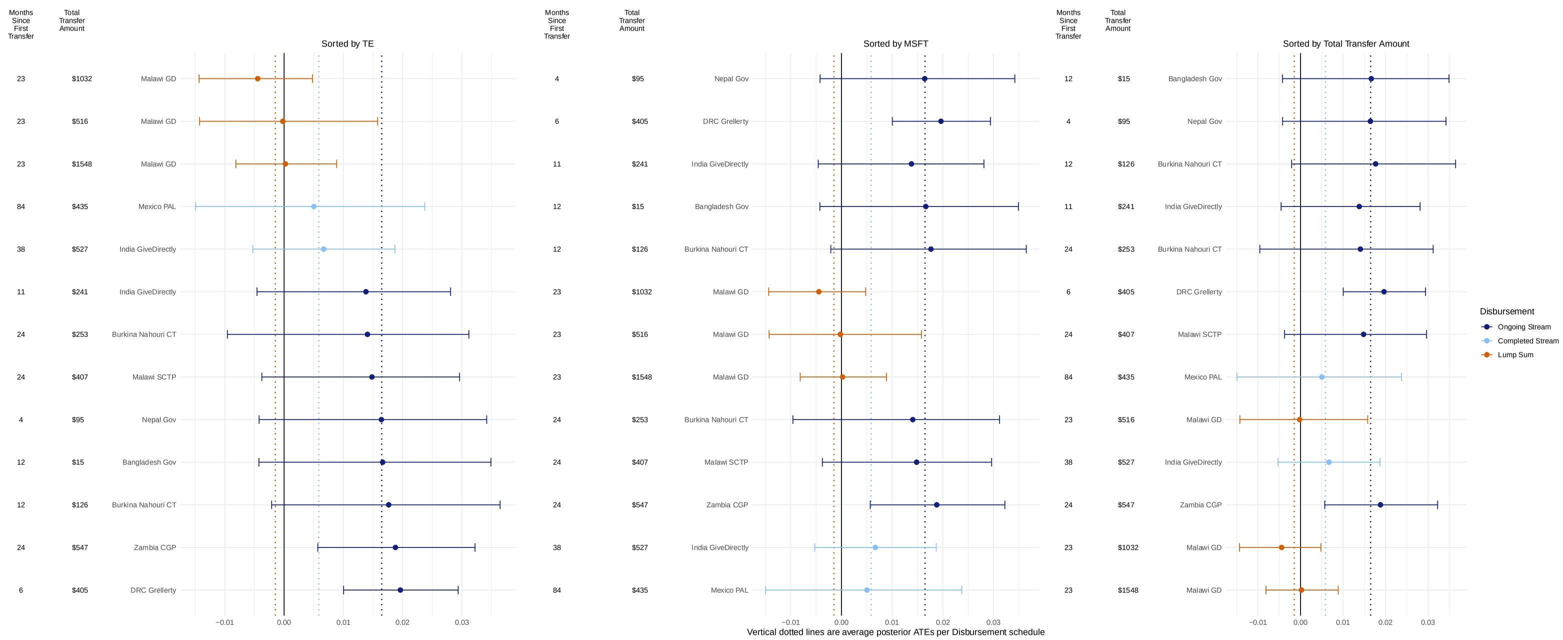
Appendix Figure 9: Stock of Financial Assets Forest Plots



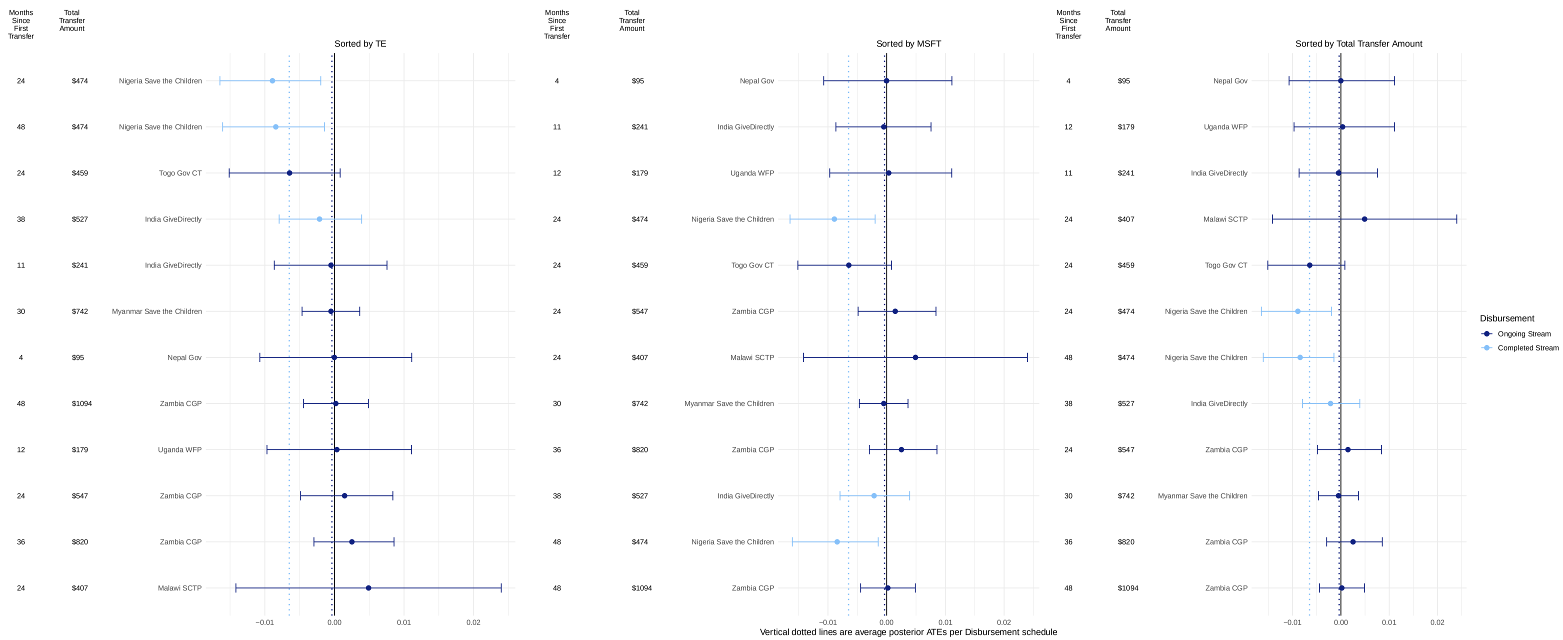
Appendix Figure 10: Height-for-Age z-Score Forest Plots



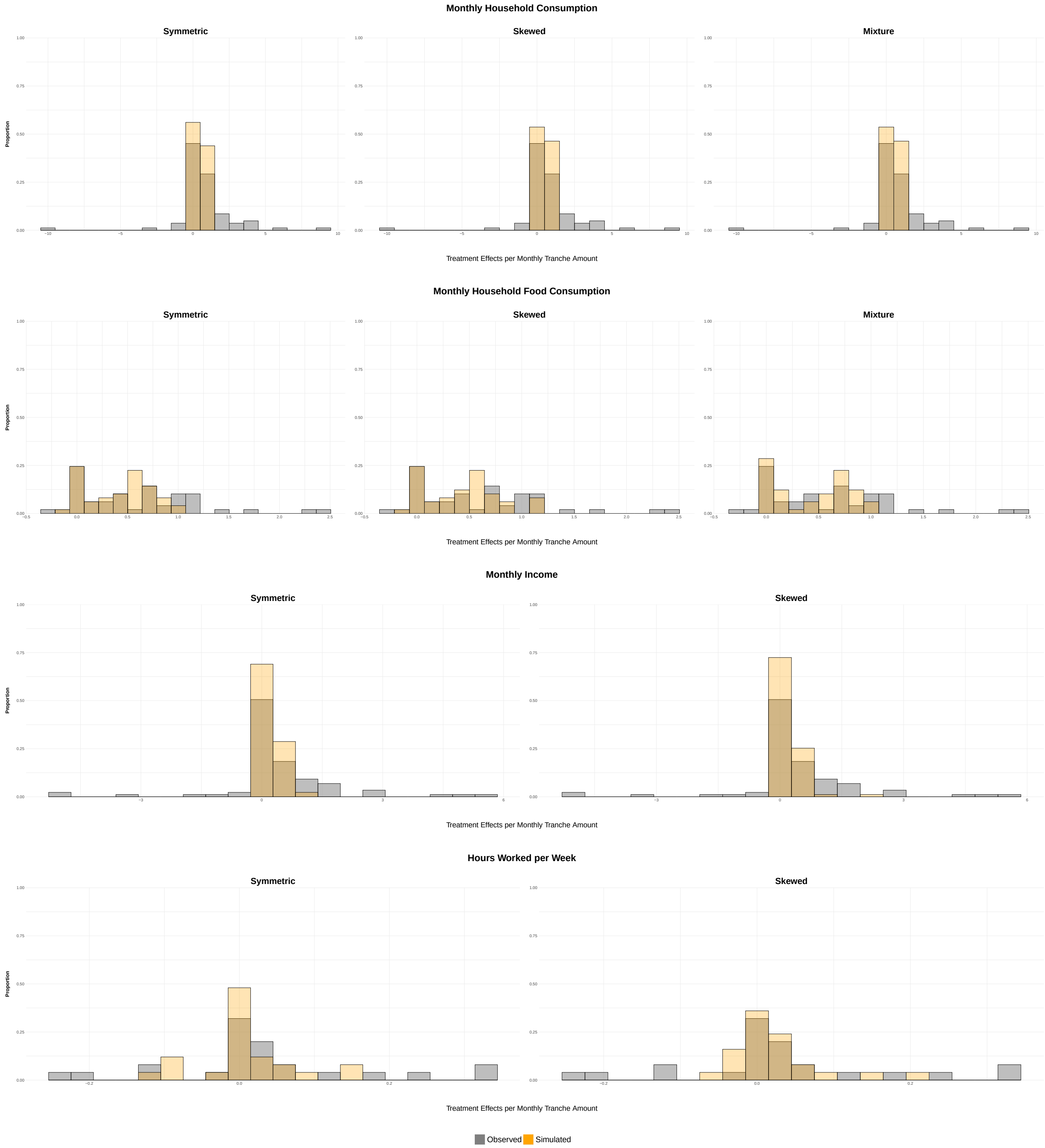
Appendix Figure 11: Weight-for-Age z-Score Forest Plots



Appendix Figure 12: Stunting Forest Plots

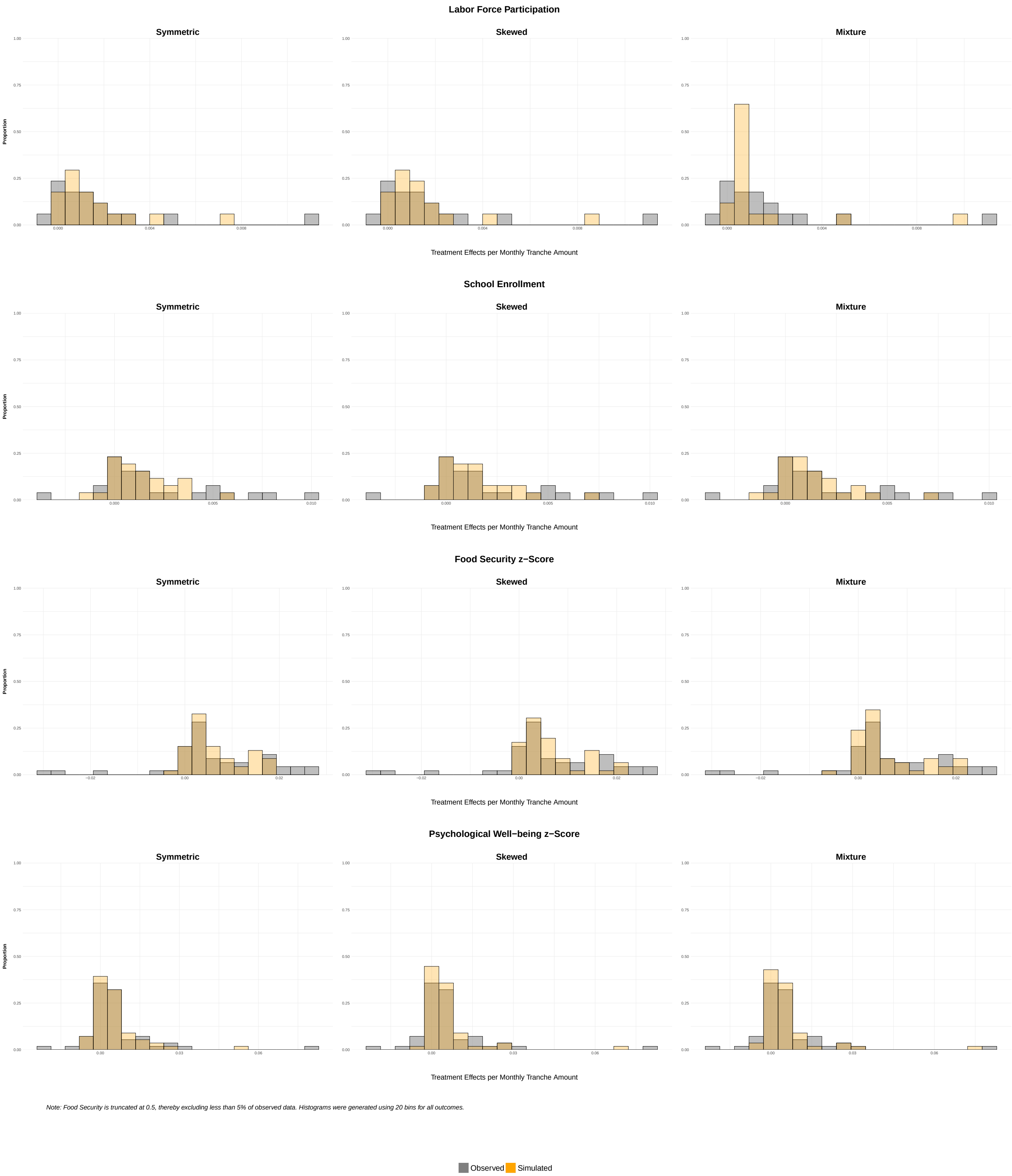


Appendix Figure 13.1: Posterior Checks



Note: Monthly Income is truncated at 10, thereby excluding less than 3% of observed data. Histograms were generated using 20 bins for all outcomes.

Appendix Figure 13.2: Posterior Checks



Note: Food Security is truncated at 0.5, thereby excluding less than 5% of observed data. Histograms were generated using 20 bins for all outcomes.

Appendix Figure 13.3: Posterior Checks