# Unconditional Cash Transfers: A Bayesian Meta-Analysis of 50 Randomized Evaluations in 26 Low and Medium Income Countries

Tommaso Crosta, Dean Karlan, Finley Ong,
Julius Rüschenpöhler, and Christopher Udry*

**Abstract**

Using Bayesian meta-analysis methods, we estimate the impact of unconditional cash transfers on nine primary outcomes in a sample of 50 randomized evaluations from middle and low income countries. We find strong and positive average treatment effects per dollar transferred on consumption, food security, and income. Critically, given theoretical and policy debates on the topic, we do not find evidence that cash discourages work. We document important heterogeneity along four key dimensions. First, there are important differences by disbursement schedule. Though both stream and lump sum transfers generate increases in consumption and income, the impact of streams flows through food consumption and labor supply while lump sum transfers tend to facilitate asset accumulation. Second, treatment effects across the board show no sign of dissipation over time. Third, effects generally tend to be larger for samples that are less poor to begin with. Fourth, and in line with earlier work, we find slightly diminishing marginal returns to transfer size. Benefit-cost analyses that define benefits as the net present value of the predicted treatment effect on consumption yield ratios between 83% and 414%.

*We thank Lily Ge, Samir Khatri, Anjali Patel, and Donny Tou for excellent research assistance.
Contacts: Tommaso Crosta (tommaso.crosta@phd.unibocconi.it, Bocconi University); Dean Karlan (karlan@northwestern.edu, Northwestern University); Finley Ong (finley.ong@kellogg.northwestern.edu); Julius Rüschenpöhler (julius.ruschenpohler@kellogg.northwestern.edu, Northwestern University, Global Poverty Research Lab); Chris Udry (christopher.udry@northwestern.edu, Northwestern University).

# 1 Introduction

Cash transfers have emerged in recent decades as a primary instrument for administering development aid. There are practical, theoretical, and empirical reasons for this trend. On a practical level, cash transfers are operationally simpler, and thus require lower administrative costs, can be rolled out quickly (e.g., for humanitarian and emergency purposes), and are scaled more easily than interventions that require more extensive logistical efforts. On a theoretical level, cash transfers appeal to principles of autonomy by empowering recipients to make their own choices with transferred resources. On an empirical level, there have been many evaluations, typically randomized controlled trials (Schady and Araujo 2006; Covarrubias et al. 2012; Mel et al. 2012; Blattman et al. 2014; Haushofer and Shapiro 2016), often finding positive results. We analyze this corpus using a meta-analysis that draws on 83 academic papers reporting on 50 unconditional cash transfer (UCT) programs from 26 low- and middle-income countries. This leaves us with 329 treatment effect estimates across 9 primary outcomes from 64 distinct endline surveys.

The modal program in our sample is aimed at poor households and motivated by long-term "development" goals, as opposed to humanitarian interventions that are more narrowly addressed at recent or ongoing emergencies. Almost all programs in our sample broadly target poor households, with secondary target populations being children (17 programs), micro-enterprise owners (five programs), and smallholder farmers (four programs). Fourty-four of the 50 randomized interventions are motivated by long-term concerns about post-conflict development, social protection, child health and development, business and livelihood promotion, and other development aims, broadly construed. Of the six humanitarian programs we analyze, three are COVID-19 related and two target victims of natural disasters.

A key dimension by which the programs in our sample differ is their disbursement schedule. Thirty of the 50 programs include a transfer disbursed as a stream of typically monthly payments of a median amount of 42 USD PPP (ranging from 10 to 195 USD PPP per month). Twenty-five of the 50 programs feature a lump sum transfer of a median amount of 403 USD PPP (ranging from 15 to 2,571 USD PPP total).[1] Transfers were more likely to be disbursed in cash (31 programs) than electronically, with mobile money transfers (16 programs) vastly more prevalent than bank transfers (three studies).

We use Bayesian hierarchical models to jointly estimate the average effect of UCT programs across nine primary outcomes: total household consumption, household food consumption, food security, assets, income, labor supply, psychological well-being, education, and physical child health. We focus primarily on stream versus lump sum transfer schedules and on the dissipation or amplification of treatment impact over time. That being said, we also examine how impacts vary based on the average consumption level of the sample and present second-order effects to estimate whether the marginal benefit decreases with the size of the transfer.

We are not the first to conduct a meta-analysis of cash transfer studies. Table 1 compares our meta-analysis to the existing meta-analytical literature on the impacts of cash transfer programs. We add to the literature along three key dimensions. First, we analyze a wide range of social and economic outcomes. Most existing meta-analyses focus on a narrow set of outcomes, and are thus accompanied by more nuanced and theoretical discussions of the link from cash transfers to, e.g., child health. On this dimension, the closest study to ours is Kabeer and Waddington 2015 which spans consumption, investment, and labor.

Second, we include dynamic estimates, i.e., whether treatment effects dissipate or compound over time. This analysis complements three other analyses, Wollburg et al. 2023, McGuire et al. 2022, and Kondylis and Loeser 2021, that address dissipation in different ways. Wollburg et al. 2023 compares short-run and long-run estimates of mostly UCT RCTs on mental health outcomes to show that the small but statistically significant short-run effects they find on depression dissipate in the long-run. McGuire et al. 2022, in a more diverse sample that includes both RCTs and non-randomized designs as well

[1] Programs with stream and lump sum transfer elements add up to more than the total sample size of 50 programs because five programs involve both a stream and a lump sum transfer.

as both CCTs and UCTs, finds little dissipation of the small effects they estimate on depression. 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.

Third, our work covers a larger set of papers, and three key covariates: the disbursement schedule (stream versus lump sum), time since transfer onset, the poverty level of the sample, and the second derivative of the transfer amount (the first derivative is part of the core estimates throughout). These covariates for which we examine heterogeneity are critical for mapping the impacts of UCT programs to theories of change, for example in examining the hypothesis that borrowing and saving constraints would lead stream transfers to be spent more on consumption.

# 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 two secondary sources: the GiveDirectly Cash Evidence Explorer and the Overseas Development Institute's 2016 report "Cash transfers: what does the evidence say?". Second, we conduct a search of databases and registers of scholarly research using key words. Our combined search yields a universe of 2,577 studies, of which 83 meet the inclusion criteria of our meta-analysis.[2]

We restrict our analysis to studies that meet the following inclusion criteria:

1. The study analyzes an RCT. We consider a study to be an RCT if a) treatment assignment is randomized and b) the control group receives no or minimal cash (or a placebo).

[2] A complete description of our systematic search is provided in our supplementary materials.

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

   (a) This includes UCT programs with non-coercive behavioral components to the treatment, such as an onsite information session or labelled cash transfers. It excludes conditional cash transfers (CCTs), which require ongoing behavioral compliance with certain conditions to continue receiving the cash transfer (most commonly school attendance).[3]

   (b) This includes non-contributory pension programs.

   (c) This excludes cash transfers that are merely a component of a multi-faceted program without a pure UCT treatment arm.

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, assets, savings, business performance, labor supply, child health and development, education, or psychological well-being.

## 2.2 Data extraction

We collect the following information from the studies in our sample:

**Stream and lump sum transfers**: To determine the effect of program design on the impact of UCTs, we distinguish between stream and lump sum transfer programs. In general terms, a stream transfer program delivers small amounts of cash at regular intervals over an extended period of time while a lump sum transfer program delivers a large amount of cash all at once. We define an intervention as a lump sum program if the

[3] 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).

cash is delivered in no more than three installments over no more than two months. All other transfer schedules, ranging from five weekly transfers to six quarterly transfers, are considered stream transfer programs.

**Months since first transfer**: We use the number of months between the first transfer of the UCT program and the endline survey to capture the dynamic impacts of UCTs, as in McGuire et al. 2022.[4] If a study does not report time since last transfer, we infer it based on the program's scheduled timeline.

**Control group mean monthly consumption**: We collect extract the control group mean from the results tables of all papers that report consumption as an outcome. If control group mean is not reported at endline, we extract the baseline control group consumption level. We convert consumption as reported to monthly household consumption by scaling the reported amount for time period and household size if necessary (e.g., weekly per capita consumption is multiplied by 4.3 and the average household size to convert to monthly household consumption). For programs that don't report total consumption as an outcome, we predict household consumption from related outcomes (food consumption, food security, total assets, income, and labor supply) using a linear regression when possible.

**Electronic transfers**: We identify which programs in our sample deliver UCTs electronically, which includes both bank transfers and mobile money transfers (such as M-PESA).

**Total amount transferred and monthly amount transferred**: We calculate the total transfer amount by taking the average sum of the value of all transfers made to program beneficiaries by the time of the endline survey, as in Kondylis and Loeser 2021. Additionally, we construct a monthly amount transferred variable by taking the average amount of money transferred per month during the UCT program. For stream trans-

[4]McGuire et al. 2022 uses months since first transfer. Kondylis and Loeser 2021 uses month since last transfer to be conservative with respect to finding large persistence of impact of cash transfers.

fer programs, this is usually simply the transfer amount as most stream programs pay monthly, but in some instances they pay more or less frequently and thus we calculate the average total monthly transferred. For lump sum transfer programs, we divide the lump sum amount by the number of months since the first transfer. This thus converts the lump sum to a figure more comparable to the stream design, as it is the amount that would have been transferred monthly had the total transfer amount remained the same but been paid in a stream rather than all at once. Both transfer amounts are then converted to 2010 USD PPP.

**Treatment effect per dollar transferred**: We extract treatment effects directly from the papers' results tables, prioritizing estimates that disaggregate by survey round and treatment arm and that contain fewer control variables.[5] Outcomes denominated in currency are converted to 2010 USD PPP. Psychological well-being and food security outcomes are standardized, if necessary, by dividing by the control group standard deviation.[6] See supplementary materials for a complete description of how each outcome variable is converted to common units. Once converted to appropriate units, we divide all treatment effects by the total amount transferred and monthly amount transferred to construct the two outcome variables used in our analysis. This allows our results to be interpreted as the treatment effect per (monthly) dollar transferred.

We prefer to use treatment effect per monthly amount transferred for stream transfers, because theory suggests that much of the impact of stream disbursements comes from their immediate consumption rather than their investment and long-term returns. Therefore, it seems inappropriate to consider a stream UCT program half as effective at month 24 as at month 12 if its treatment effects are consistent over that time period, which would be what treatment effect per total amount transferred captures. By contrast, theory suggests that recipients will save and invest a significant proportion of a lump sum transfer. We

[5]See supplementary materials for a complete description of our preferred specifications.

[6]See supplementary materials for the studies' unstandardized treatment effects on food security and psychological well-being outcomes.

therefore prefer to use treatment effect per total amount transferred for lump sum UCT programs. Furthermore, using treatment effect per (average) monthly amount for lump sum transfers would cause us to inflate the returns of estimates from longer term endline surveys.

# 3 The Model

A crucial methodological challenge is how to best aggregate information from multiple RCTs to estimate a measure of the general effect of unconditional cash transfers with credible external validity. An individual RCT can provide a consistent estimates of the average treatment effect of cash transfers on an outcome of interest in a particular population during a specific time period and context. But how much of such estimate is due to idiosyncratic elements of the intervention (e.g. political instabilities in some treatment regions dampened the effect of the intervention) and how much of it is due to statistical regularities with generalizable external validity (e.g. cash transfers have higher effects on consumption in lower income samples)? To carry out such task one needs assumptions regarding the generative process of the data and a model to estimate it. However, there is a tension between the strength of the maintained assumptions of the model and their credibility (Manski, 2004[7]) .

A first naive approach to aggregate evidence from multiple RCTs is to take an average of the estimates, weighted by the inverse of their estimated variance. However, the approach relies on the very strong assumption that each estimate is an independent draw from a common distribution (Rubin 1981). A consequence of this very restrictive assumption, is that there cannot be any true heterogeneity in the effects across RCTs: indeed variation in estimates is purely caused by variation in sampling. Rubin 1981 offers a different approach, which both relies on less stringent assumptions and allows for

[7]"The credibility of inference decreases with the strength of the assumptions maintained"

heterogeneity in the effects across RCTs. By introducing a hierarchical structure, the single estimates are assumed to be sampled realizations from **distinct** distributions (i.e. the first hierarchical layer), but whose parameters, in turn, come from a **common** distribution (i.e. the second hierarchical layer). In this way, we are able to both control for the sampling variability of the estimates to look for a common effect, but at the same time identify their true underlying heterogeneity, which can be due to certain contextual features of the intervention.

Hierarchical Bayesian models naturally fit such a data structure and can be flexibly implemented by relying on the assumption of exchangeability (a much weaker assumption than independence). Building on the "Representation Theorem" by De Finetti 1972, this allows modeling data as independent, conditional on a set of parameters. Interpreting exchangeability is, however, much less straightforward than interpreting independence: Micheal Betancourt 2020 writes that "[o]ften we are left without any underlying theory for why a data generating process behaves differently in different contexts. In this case we know that the behavior should vary across contexts but we don't have any canonical way of ordering or arranging those contexts by the unknown behavior. ... [E]xchangeability is not a fundamental property of a system being observed but rather a consequence of how we choose to observe that system!". Our model is a further generalization of the original Rubin (1981) model since we assume conditional exchangeability, as we characterize the second layer distribution (or population distribution) to depend on a set of covariates. This assumption means that, conditional on the RCT features that we consider, namely type of transfer (lump sum or stream) and time of measurement (months since first transfer), observations can be permuted across contexts, without affecting their joint probability distribution.

Hinging on these two assumptions, we then assume a distributional and parametric structure for our hierarchical model, where we denote by $\hat{TE}$, the empirical estimates of the Average Treatment Effects and by $\hat{se}$ their respective standard errors[8]:

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

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

$$\forall k \in \{1, ..., K\} \quad \beta_k \sim \mathcal{N}(0, 25)$$

$$\sigma_\theta \sim \mathcal{H}alf - \mathcal{N}ormal(0, 25).$$

As previously outlined, the model is a generalization of Rubin (1981), similar to the hierarchical linear models of Hedges and Olkin (1983) and Raudenbush and Bryk (1985), though the estimation is conducted in a Bayesian fashion.[9] The treatment effect estimates for a given outcome from all the studies (the $Nx1$ vector $\hat{TE}$) are drawn from distinct and conditionally independent distributions centered around a parameter $\theta$ with variances corresponding to their empirical estimates $\hat{SE}^2$, which are supposed to be consistent estimators of the former. Crucially, these parameters come from a common distribution with a common mean and standard deviation, i.e. $\mathcal{N}\left(X\beta,\ \sigma_\theta^2 I_N\right)$. However, contrary to

[8]Note that we choose a value of 25 for the variance of the coefficients of the expectation of $\theta$'s distribution. Considering that these coefficients are very small numbers (i.e., usually smaller than one in absolute terms), these distributions are very dispersed and thus not very informative and are centered around zero, since our agnostic prior hypothesis is of no effect for any of the variables.

[9]As Raudenbush and Bryk (1985) notices, this approach is formally of an Empirical Bayes nature, since we use the data, i.e. $\hat{se}$, to inform the prior distributions. An advantage of this approach is that it hinges on Frequentist asymptotic distributional results for which ATE estimates are asymptotically Gaussian. In light of such considerations, the choice of the prior for $\theta$ is more general than it might seem at first.

the original Rubin (1981) model, $\theta$ is not centered around a common mean, but instead around an expectation which depends on an RCT-specific set of covariates, with constant additive and linear effects. This is what allows us to aggregate information across studies, while also estimating the parameters that characterize the underlying heterogeneity across RCTs. In particular, we estimate the aggregated treatment effects separately for lump sum and stream interventions, using two separate indicator variables $L$ and $S$, i.e. $\theta \sim \mathcal{N}\left(\beta_1 L + \beta_2 S,\ \sigma_\theta^2 I_N\right)$. In a second version of the model, we add a covariate for the number of months since the first transfer $M$ interacted with an indicator for lump sum transfers, i.e. $\theta \sim \mathcal{N}\left(\beta_1 L + \beta_2 S + \beta_3 M * L + \beta_4 M * S,\ \sigma_\theta^2 I_N\right)$. In this way, we are able to estimate the dissipation or accumulation of treatment impact over time for lump sum and stream interventions. In a third version of the model, we also add covariates for transfer size (either total transfer amount or monthly tranche amount, depending on the outcome specification) and average total consumption per month in the control group, i.e. $\theta \sim \mathcal{N}\left(\beta_1 L + \beta_2 S + \beta_3 M * L + \beta_4 M * S + \beta_5 T * L + \beta_6 T * S + \beta_7 C * L + \beta_8 C * S,\ \sigma_\theta^2 I_N\right)$.[10] This specification allows us to perform two estimations of crucial importance to policy-makers: whether treatment effects are higher or lower in poorer samples and whether the marginal effects of transfers display decreasing or increasing returns to scale.

The numerical estimation of the model is conducted using Stan (Stan Development Team 2019), a state of the art software for Bayesian simulations, that uses an Hamiltonian Monte Carlo procedure (M. Betancourt 2017) to explore posterior density distributions using gradients. This approach allows for flexible definitions of priors and to estimate even relatively complex models.

[10] To address missing data issues, we impute values using a simple regression with control group mean as the only predictor, depending on the outcome of the observation.

# 4 Results

We present average and heterogeneous treatment effects of UCT programs on a wide range of economic, social, and health outcomes. The section is organized by outcome class, split into household consumption and food security; assets, income, and labor supply; child health, development, and education; and psychological well-being. We then discuss whether the impact of UCTs accumulates or dissipates over time. We also examine the marginal ("second derivative") impact of the transfer amount on treatment effects per dollar and the influence of the poverty level of the study sample on the impact of UCTs.

Tables 2 to 4 present our main results. Table 2 Panel A (top right quadrant) displays average treatment effects per monthly amount transferred, our preferred treatment effect definition for stream transfers. To enable comparisons, we show the equivalent estimate for lump-sum transfers (top left quadrant), which treats the lump sum as if it had been disbursed as a monthly stream. Panel B presents treatment effects per total amount transferred. This is displayed for lump sum transfers only since the total transfer amount of stream transfers is driven mostly by the tenure of the program.

Tables 3 and 4 display heterogeneous treatment effects. Table 3 presents the dynamic treatment effects when the covariate months since first transfer is added to the model. Results are arranged by program design (stream versus lump sum) and outcome variable definition (per monthly amount versus per total amount) as in Table 2 with added columns to illustrate the direction and magnitude of dynamic effects. Table 4 presents results from our full model that also includes covariates for transfer amount (effectively the squared term) and control group mean consumption level (i.e., poverty level of the sample for each study). Table 5 uses the estimates from Tables 2 & 3 to inform a simple accounting model of the overall costs and benefits of three hypothetical UCT programs.

## 4.1 Household consumption and food security

We find that UCTs have a substantial impact on household welfare. Table 2, Panel B shows that a lump sum UCT of 100 USD increases monthly household consumption by 1.5 USD (95% CI: 0.9, 2.2). Table 1, Panel A shows that a stream UCT program with a monthly tranche of 100 USD increases monthly household consumption by 43.4 USD (95% CI: 31.7, 56.3), while the effect of a cost-equivalent lump sum UCT program is about half as large ($\theta$ = 23.7; 95% CI: 12.1, 35.6). This difference becomes plausible when accounting for the substantially larger average transfer amounts for lump sum UCT programs. Treatment effects at the *median monthly transfer amount* on consumption are about 10 USD for both stream and lump sum transfers, which represents an increase in monthly consumption of about 8% for the average household in the sample. This specific result connects to Kondylis and Loeser 2021 which finds diminishing returns to scale for UCT programs, though the distinction between lump-sum and stream is not estimated in their analysis. The broader finding that cash transfers boost household consumption is in line with the meta-analytical literature (see, Kondylis and Loeser 2021; Kabeer and Waddington 2015).

We further find that the composition of consumption effects differs between stream and lump sum transfers. While we estimate a large and statistically significant effect of stream transfers on monthly household food consumption ($\theta$ = 50.3; 95% CI: 35.4, 66.9), the effect of lump sum programs is not significant and the point estimate is small ($\theta$ = 6.3; 95% CI: -9.0, 22.2).[11] In line with these results, we estimate a large 0.8 standard deviation increase in food security per monthly 100 USD transferred for stream transfers (95% CI: 0.6, 1.1). This compares to an increase for lump sum transfers that is half as large ($\theta$ = 0.4; 95% CI: 0.1, 0.6). The treatment effect per total amount transferred for lump sum is 0.02 standard deviations for a 100 USD transfer (95% CI: 0.01, 0.04) and

[11] The majority of studies unfortunately lack data on a more comprehensive set of consumption expenditures beyond food consumption.

0.1 standard deviations for the median lump sum transfer amount (95% CI: 0.05, 0.2). This evidence accords with economic theory that predicts stream transfers to translate into more non-durable consumption and lump sum transfers into more productive investments.

## 4.2 Assets, income, and labor supply

Results on asset accumulation are more mixed but provide suggestive evidence in favor of the economic intuition that lump sum transfers are more conducive to investment. Table 1 presents evidence that a 100 USD lump sum transfer facilitates asset accumulation on the order of 23.8 USD (95% CI: 10.3, 37.7). This corresponds to a 223.8 USD increase in the stock of assets for the median lump sum transfer (95% CI: 96.8, 354.7). While the point estimate for the impact of a 100 USD monthly stream transfer is positive ($\theta = 208.3$; 95%CI: -60.3, 476.0), it is not statistically significant and is half the magnitude of the estimated impact of a cost-equivalent lump sum transfer ($\theta = 290.7$; 95% CI: 183.7, 611.9). This finding differs from Kabeer and Waddington 2015 who report modest effects of stream transfers on investment, however their sample is comprised entirely of CCTs.[12]

We detect sizeable effects for both stream and lump sum UCTs on income, as Table 2 shows. While a 100 USD monthly stream transfer results in a 27.8 USD increase in monthly income (95% CI: 14.7, 42.0), a cost-equivalent lump sum transfer increases income by 17.0 USD (95% CI: 8.0, 26.5). Higher incomes may be driven by a combination of productive investment and greater labor supply. We estimate a 6.9 increase in the likelihood of participating in the labor force per monthly 100 USD transferred for stream transfers (95% CI: 3.3, 9.4) but we do not find that lump sum transfers cause a statistically significant increase in labor supply ($\theta = 0.1$; 95% CI: -0.5, 0.6). We do detect, however, that lump sum transfers lead to an increase in productive assets, as shown in Table B.1,

[12]We are not aware of any previous meta-analysis that studies the effect of UCT programs on asset accumulation.

Panel B ($\theta = 9.7$; 95% CI: 2.9, 17.9). This evidence suggests stream and lump sum transfers be may raising income by different mechanisms: stream transfers lead to greater labor supply while lump sum transfers lead to more productive investment. [13]

## 4.3 Child development and education

In line with the literature that finds little consistent impact of cash transfers on child development outcomes (see, Manley et al. 2022), results on height-for-age z-scores (HAZ) and weight-for-age z-scores (WAZ) are inconclusive. In Table 2 we show that stream transfers have a small positive impact on a pooled sample of HAZ and WAZ ($\theta = 0.1$; 95% CI: 0.01, 0.2). However, treatment effects for stream transfers are not statistically significant for either the HAZ or WAZ sub-samples, as shown in Table B.3. We do not detect any effect of lump sum transfers on HAZ and WAZ ($\theta = 0.0$; 95% CI: -0.01, 0.01), but data limitations are severe.[14]

In contrast, Table 2 shows that a stream UCT of 100 USD per month increases school enrollment by 16.8 percentage points (95% CI: 5.1, 28.6). Considering the median monthly transfer in our sample is 22.8 USD, this translates into an increase in school enrollment of the median stream UCT by 3.8 percentage points (95% CI: 1.2, 6.5). Broadly in line with our results, the most recent meta-analyses on education find ameliorating effects of cash on enrollment, attendance, completion, and dropout (Garcia and Saavedra, 2017; Baird et al. 2014), though these studies focus exclusively (Garcia and Saavedra, 2017) or mostly (Baird et al., 2014) on CCT programs. We do not detect any effect of lump sum UCTs on school enrollment, but once again we have extremely limited data. [15]

[13] Unfortunately, we lack sufficient data to comment on the impact of stream transfers on productive assets. See Table B.1 for the impacts of stream and lump sum UCTs on different categories of assets and Table B.2 for impacts on different components of income.

[14] Only two studies in our dataset report the impacts of lump sum transfers on either HAZ or WAZ (Aggarwal et al. n.d.; Hossain et al. 2022).

[15] Only one study in our dataset reports the impact of a lump sum transfer on school enrollment

## 4.4 Psychological well-being

As Table 2 shows, stream transfers lead to substantial increases in psychological well-being. In particular, a transfer of 100 USD per month increases self-reported well-being by 1.1 standard deviations (95% CI: 0.5, 1.6). Since the typical stream UCT transfers less than 50 USD per month, we estimate the median transfer to boost well-being by 0.4 standard deviations (95% CI: 0.2, 0.6). Furthermore, when we exclude three outlier estimates from the Zambia Child Grant Program (CGP), the treatment effect per monthly 100 USD is reduced to 0.4 standard deviations (95% CI: 0.2, 0.7).[16] In contrast, lump sum transfers have no effect on well-being ($\theta = 0.1$; 95% CI: -0.5, 0.6). This is generally in line with the literature on cash transfers and mental health that finds more modest ameliorating effects on subjective well-being (McGuire et al., 2022) and depression (McGuire et al. 2022; Wollburg et al. 2023). However, these studies incorporate both UCTs and CCTs and so it is not clear if the results are driven by either type.

## 4.5 Dynamic treatment effects

In Table 3, we present the dynamic effects of UCT programs by adding a covariate for months since first transfer to our model, describing the coefficient on this covariate as a monthly accumulation (dissipation) effect. As shown in Figure 1, the vast majority of endline surveys across outcomes for both streams and lump sums take place between 12 and 48 months of the onset of cash transfers. We therefore urge caution in applying our accumulation (dissipation) estimates outside of this range. There is reason to believe outside of this time-horizon, the benefits of UCTs fade. In a 9-year follow-up of a lump

(Aggarwal et al. n.d.).

[16]The estimates from the Zambia CGP are not only extreme positive outliers, they are also uniquely constructed from a binary indicator variable for whether the respondent was feeling happy/happier than 12 months ago. See supplementary materials for the complete list of all the different reported treatment effects on psychological well-being that get standardized for analysis.

sum UCT program, Blattman et al. (2019) find that the initial gains, which persist up until at least 48 months, largely dissipate as the recipients' investment levels off and the control group catches up.

In general, we do not find a statistically significant accumulation or dissipation effects for most outcomes. For stream transfers, we estimate a monthly accumulation effect on total consumption of 1.8 USD per monthly 100 USD transferred (95% CI: 0.6, 2.9). We model that by month 18, the treatment effect of a monthly 100 USD transfer on monthly household consumption will have grown from 9.1 USD (95% CI: -14.4, 34.1) to 40.7 USD (95% CI: 29.5, 53.1). We also detect statistically significant dissipation of stream transfers on HAZ/WAZ but the magnitude is trivial ($\theta = -0.007$; 95% CI: -0.012, -0.001). We detect no statistically significant accumulation or dissipation effects for lump sum transfers. This in and of itself is a meaningful finding. There is no evidence to suggest that the effects of lump sum transfers dissipate within the 12 to 48 month range where we have support.

It is important to note that the timing of endlines relative to the end of the UCT program differs substantially for stream and lump sum UCT programs. Twenty-five of the 30 stream programs in our main sample administer endline surveys while transfers are ongoing or within three months of the last transfer. By contrast, all but four of the 25 lump sum programs administer follow-up surveys more than eight months after the last transfer is disbursed. Our estimates of accumulation effects for stream transfers should not be used to draw conclusions about how treatment effects accumulate or dissipate in the medium- or long-term after transfers stop.

## 4.6 Heterogeneous treatment effects

In Table 4, we examine treatment effects from a model that adds covariates for transfer amount and average consumption level of the control group to the dynamic effects model.

Since our outcome variable is divided by the transfer amount, the coefficient on the transfer amount covariate can effectively be interpreted as a squared term. It shows whether the treatment effect per dollar is getting larger or smaller as the transfer amount increases (our base and dynamic effects models assume treatment effects increase linearly with transfer amount). We find either no effect or a weakly negative effect across all outcomes for both lump sum and stream transfers. As we show in column 2, the treatment effect of transfer amount per monthly 100 USD for stream transfers is negative and statistically significant ($\theta = -0.03$; 95% CI: -0.06, -0.01), but the magnitude of this negative effect is smaller than past findings. Kondylis and Loeser 2021 estimate that a doubling of transfer amount causes a 30% decrease in the treatment effect per total amount transferred.

Column 6 of Table 4 shows the treatment effect of the consumption level of the sample population being 100 USD higher per 100 USD transferred. Note that a 100 USD increase amounts to nearly a doubling of household consumption for the average study population in our sample. Columns 7 and 8 offer a comparison of the treatment effect per 100 USD at month 18 for populations in the 20$^{\text{th}}$ percentile and 80$^{\text{th}}$ for monthly household consumption. For stream transfers, we find that a 100 USD higher consumption population experiences a 34.0 USD greater treatment effect on monthly food consumption per monthly 100 USD (95% CI: 14.1, 52.6). We also estimate for stream transfers that the treatment effect of a 100 USD increase in sample consumption level per monthly 100 USD on food security to be a -0.6 standard deviation decrease (95% CI: -0.9, -0.3). This seeming contradiction may be explained by the fact that food insecurity is largely experienced by the ultra-poor, so there's less of a margin on which higher-consumption populations can improve their food security even if they're spending more on food compared to lower-consumption populations.

We don't detect any statistically significant effects of sample consumption level on food consumption or food security for lump sum transfers. We do estimate, however, that the treatment effect per 100 USD transferred of a 100 USD increase in sample consumption level for lump sum transfers is positive for monthly total household consumption ($\theta = 1.6$; 95% CI: 0.5, 2.6) and the stock of total assets ($\theta = 22.7$; 95% CI: 5.2, 40.1). For both stream and lump sum transfers, we estimate that richer samples experience a much greater treatment effect per 100 USD transferred on monthly income as illustrated in columns 7 and 8 of Table 4. We cannot provide a clear explanation of why there seem to be higher returns to capital in richer study samples. When we attempt to incorporate GDP per capita as a covariate to disentangle the effect of country-level differences in treatment effects versus within country differences in sample poverty level, we find strong evidence of model misspecification.

## 4.7 Benefit-Cost Analysis

Table 5 estimates the Benefit-Cost ratios of three supposed UCT programs under various assumptions using estimates from Tables 1 and 2. Scenario 1 considers a stream transfer program that once it begins continues indefinitely, like a government universal basic income program. The second scenario considers a stream tranfer program that continues for 18 months and then stops. Both hypothetical stream programs deliever 55.6 USD per month (or 1,000 USD over 18 months). The final scenario considers a simple 1,000 USD lump sum transfer program. It is important to note that the cost of the program is assumed merely to be the total transfer amount; we don't account for administrative and implementation costs in our model.

Table 5 displays the benefit-cost ratios for all three scenarios of all the consumption derived from the UCT program up until month 18 and the total consumption derived from the program factoring the net present value of all future consumption it will cause. Panel A uses estimates from our basic model (see Table 1) to inform its assumptions for the consumption impacts of the UCT programs whereas panel B uses estimates from the dynamic effects model (see Table 2).

In Panel A, we calculate that the benefit-cost ratio of the indefinite stream program is 0.43. This is equal to the treatment effect on monthly consumption per monthly dollar transferred. When we incorporate the positive accumulation effect from Table 2 into our assumptions, we find that the benefit-cost ratio of this program is 4.55. For the 18-month stream program, our findings our similar across Panels A and B. This is because we don't use assume there are accumulation/dissipation effects after the transfers stop since we lacked the data to confidently estimate this sort of effect.[17] We find the 18-month stream program has a benefit-cost ratio of 4.14 or 3.94 in Panels A and B respectively. Last, we calculate in Panel A that the lump sum program has a 4.06 benefit-cost ratio. However, when we incorporate a negative dissipation effect into our assumptions in Panel B, we calculate the program has a benefit-cost ratio of only 0.83.

# 5 Conclusion

The large-scale expansion of randomized evaluations over the past several decades provides an opportunity for pooling 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 is an opportune intervention for such an exercise, not least because the degrees of variation are more limited, and the implementation fidelity is easier to define and less likely to vary and drive results. We conduct a meta-analysis based on 50 randomized evaluations from 26 low and middle income countries.

We present two layers of results. First, the broad average results are strong and transcend across first-stage and second-stage outcomes. For example, the average impact on consumption is $43 per $100 transferred per month for a stream cash transfer program and $18 per $100 transferred under a lump sum program. For stream, this may appear to

[17] As previously mentioned, 25 of the 30 stream programs in the sample administer endline surveys as transfers were ongoing or shortly after the last transfer.

be underwater, but is likely not once two additional points are considered: Consumption is almost invariably incompletely measured, and dynamic effects indicate the impact on consumption increases over time. The impact on food security is likewise strong: At 18 months, a stream (lump sum) program at the median transfer amount is estimated to improve food security by 0.8 (0.3) standard deviations. This suggests that even if food security were the only impact to consider in a cost-effectiveness analysis, cash transfers would likely surpass even more targeted interventions. Downstream, i.e. second-stage, impacts are also strong and important: Psychological wellbeing, for example, improves by 0.4 standard deviations at the median transfer amount (although lump sum programs do not generate such an effect).

A key design feature that distinguishes different UCT programs is the disbursement schedule, whether as a stream of payments over time or as a lump sum transfer. Often this design decision is made based on purpose: Is the goal to support consumption (in which case transfer monthly) or to trigger investment (in which case transfer lump-sum)? Our meta-analysis only finds weak evidence supporting the intuition that lump sum transfers will have a "comparative advantage" in facilitating investment while stream transfers spur consumption increases. While we do estimate that after 18 months a lump sum transfer will have generated asset accumulation twice as large as streams, the credibility intervals of these estimates overlap. Indeed, on income, we find positive and sizeable treatment effects for both stream and lump sum transfers, which calls into question whether their ability to generate long-run investment is different to begin with. One possibility is that, when assured of a continuing stream of cash transfers, these poor households are adept at transferring resources across time in order 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, and 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 'scarcity" (similar to the "large suitcase" versus "small suitcase" analogy put forth by Mullainathan and Shafir, 2013)

As a second potentially important result, we find slightly diminishing marginal returns with respect to transfer size. This supports the second set of results just discussed (to explain the stream versus lump-sum differences) due to whichever of the two proposed mechanisms. We note the coefficients on the squared term on transfer size, albeit negative, are small relative to the main effect, and we do not have the power to estimate functional form more precisely. However, the direction (diminishing rather than expanding marginal returns) is not consistent with 'threshold" poverty trap models with large indivisible goods. However, with such thresholds inevitably differing across people and markets, the diminishing marginal returns that we find are not in any way dispositive of such models.

Third, and in line with Banerjee et al. (2017), we can clearly reject the hypothesis that cash transfers, and their implicit income effect, lead to reductions in labor supply. If anything, we find evidence of positive income effects, which is also consistent with recent evidence from multi-faceted "graduation" style social protection programs (Banerjee et al., 2022).

Lastly, while we believe the preponderance of evidence and ability to aggregate reported point estimates at the study-level (often with multiple estimates per study) has shed important evidence, many questions could not be addressed with this methodological approach. By just using reported study-wave-level point estimates, we lack sufficient variation on many important dimensions, and some which we do have variation on could not be included because these methods can only handle so many covariates (e.g., local level macroeconomic conditions). Clearly more can be learned from the more arduous but worthy task of syncing the underlying data across these studies, similar to Meager (2019, 2022) with respect to microcredit.

# References


[1] Shilpa Aggarwal, Jenny C Aker, Dahyeon Jeong, Naresh Kumar, David Sungho Park, Jonathan Robinson, and Alan Spearot. "The Dynamic Effects of Cash Transfers: Evidence from Rural Liberia and Malawi". In: ().

[2] Ciro Avitabile, Jesse M. Cunha, and Ricardo Meilman Cohn. *The Medium Term Impacts of Cash and In-Kind Food Transfers on Learning*. World Bank, Washington, DC, Dec. 2019. DOI: `10.1596/1813-9450-9086`. URL: `https://openknowledge.worldbank.org/handle/10986/33055` (visited on 10/02/2023).

[3] M. Betancourt. "A Conceptual Introduction to Hamiltonian Monte Carlo". In: *arXiv: Methodology* (2017). URL: `https://arxiv.org/pdf/1701.02434.pdf`.

[4] Micheal Betancourt. *Hierarchical Modeling*. 2020. URL: `https://betanalpha.github.io/assets/case_studies/hierarchical_modeling.html#1_Modeling_Heterogeneity`.

[5] Christopher Blattman, Nathan Fiala, and Sebastian Martinez. "Generating Skilled Self-Employment in Developing Countries: Experimental Evidence from Uganda*". In: *The Quarterly Journal of Economics* 129.2 (May 1, 2014), pp. 697–752. ISSN: 0033-5533, 1531-4650. DOI: `10.1093/qje/qjt057`. URL: `https://academic.oup.com/qje/article/129/2/697/1866610` (visited on 10/02/2023).

[6] Katia Covarrubias, Benjamin Davis, and Paul Winters. "From protection to production: productive impacts of the Malawi Social Cash Transfer scheme". In: *Journal of Development Effectiveness* 4.1 (Mar. 1, 2012). Publisher: Routledge _eprint: https://doi.org/10.1080/19439342.2011.641995, pp. 50–77. ISSN: 1943-9342. DOI: `10.`

1080/19439342.2011.641995. URL: https://doi.org/10.1080/19439342.2011.641995 (visited on 10/02/2023).

[7] Bruno De Finetti. *Probability, induction and statistics. The art of guessing.* John Wiley  Sons, 1972.

[8] Johannes Haushofer and Jeremy Shapiro. "The Short-term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya*". In: *The Quarterly Journal of Economics* 131.4 (Nov. 1, 2016), pp. 1973–2042. ISSN: 0033-5533. DOI: 10.1093/qje/qjw025. URL: https://doi.org/10.1093/qje/qjw025 (visited on 10/02/2023).

[9] Larry V. Hedges and Ingram Olkin. "Regression Models in Research Synthesis". In: *The American Statistician* 37.2 (1983), pp. 137–140. ISSN: 00031305. URL: http://www.jstor.org/stable/2685874 (visited on 09/16/2023).

[10] Sheikh Jamal Hossain, Bharaty Rani Roy, Hasan Mahmud Sujon, Thach Tran, Jane Fisher, Fahmida Tofail, Shams El Arifeen, and Jena Derakhshani Hamadani. "Effects of integrated psychosocial stimulation (PS) and Unconditional Cash Transfer (UCT) on Children's development in rural Bangladesh: A cluster randomized controlled trial". In: *Social Science & Medicine* 293 (Jan. 1, 2022), p. 114657. ISSN: 0277-9536. DOI: 10.1016/j.socscimed.2021.114657. URL: https://www.sciencedirect.com/science/article/pii/S0277953621009898 (visited on 10/02/2023).

[11] Naila Kabeer and Hugh Waddington. "Economic impacts of conditional cash transfer programmes: a systematic review and meta-analysis". In: *Journal of Development Effectiveness* 7.3 (July 3, 2015). Publisher: Routledge _eprint: https://doi.org/10.1080/19439342.2( pp. 290–303. ISSN: 1943-9342. DOI: 10.1080/19439342.2015.1068833. URL: https://doi.org/10.1080/19439342.2015.1068833 (visited on 10/02/2023).

[12] Florence Kondylis and John Loeser. "Intervention Size and Persistence". In: (Sept. 2021). Publisher: World Bank, Washington, DC. DOI: 10.1596/1813-9450-9767. URL: http://hdl.handle.net/10986/36242 (visited on 10/02/2023).

[13] Joel McGuire, Caspar Kaiser, and Anders M. Bach-Mortensen. "A systematic review and meta-analysis of the impact of cash transfers on subjective well-being and mental health in low- and middle-income countries". In: *Nature Human Behaviour* 6.3 (Mar. 2022). Number: 3 Publisher: Nature Publishing Group, pp. 359–370. ISSN: 2397-3374. DOI: 10.1038/s41562-021-01252-z. URL: https://www.nature.com/articles/s41562-021-01252-z (visited on 10/02/2023).

[14] Suresh de Mel, David Mckenzie, and Christopher Woodruff. "One-Time Transfers of Cash or Capital Have Long-Lasting Effects on Microenterprises in Sri Lanka". In: *Science (New York, N.Y.)* 335 (Feb. 24, 2012), pp. 962–6. DOI: 10.1126/science.1212973.

[15] Stephen W. Raudenbush and Anthony S. Bryk. "Empirical Bayes Meta-Analysis". In: *Journal of Educational Statistics* 10.2 (1985), pp. 75–98. ISSN: 03629791. URL: http://www.jstor.org/stable/1164836 (visited on 09/16/2023).

[16] Donald B. Rubin. "Estimation in Parallel Randomized Experiments". In: *Journal of Educational Statistics* 6.4 (1981), pp. 377–401. ISSN: 03629791. URL: http://www.jstor.org/stable/1164617 (visited on 09/16/2023).

[17] Norbert Schady and Maria Caridad Araujo. "Cash Transfers, Conditions, School Enrollment, and Child Work : Evidence from a Randomized Experiment in Ecuador". In: (June 2006). Publisher: World Bank, Washington, DC. DOI: 10.1596/1813-9450-3930. URL: http://hdl.handle.net/10986/8452 (visited on 10/02/2023).

[18] Stan Development Team. *Stan User's Guide*. 2019. URL: https://mc-stan.org/docs/2_27/stan-users-guide/index.html.

[19] Clara Wollburg, Janina Isabel Steinert, Aaron Reeves, and Elizabeth Nye. "Do cash transfers alleviate common mental disorders in low- and middle-income countries? A systematic review and meta-analysis". In: *PloS One* 18.2 (2023), e0281283. ISSN: 1932-6203. DOI: 10.1371/journal.pone.0281283.