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

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

March 28, 2024

Abstract

We use Bayesian meta-analysis methods to estimate the impact of unconditional cash transfers (UCTs) on twelve primary outcomes from 114 studies of 73 UCT programs in middle and low income countries. Cash transfers generate strong and positive average treatment effects on nine of twelve outcomes: total consumption, food consumption, food security, income, assets, labor supply, children height-for-age, schooling, and psychological well-being. We draw six conclusions: First, households consume more of streams and invest more of lump sums, however once stream programs end the impacts mirror those of lump sum, indicating some propensity to save a portion of stream transfers. Second, we find long-run treatment effects remain strong, but the effects of lump sum transfers measured more than 18 months after the transfer are substantially smaller. Third, as returns are linear with respect to grant amount, we do not find evidence of either threshold-based poverty traps or diminishing marginal returns (within the observed range of transfers). Fourth, effects on consumption and income are greater for UCTs targeted to women. Fifth, including light-touch framing related to child welfare or food security generates weakly stronger impacts. Sixth, positive impacts on labor supply and income suggest no evidence of "dependency" theories that cash transfers demotivate income-generating activity on average.

---

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

1 Introduction

Unconditional cash transfers (UCTs) have become a common policy tool, and have been subject to close attention. At least 73 UCT programs have been evaluated using randomized assignment, ranging widely in scale and purpose, from large government programs to small nongovernmental efforts, from humanitarian to development. The breadth of this empirical evidence now allows 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 to income, the differential impact from targeting women within households and from adding framing a la "nudges" to the transfers.

Our meta-analysis identified 114 papers ("studies") reporting results from randomized evaluations of 73 UCTs ("programs") in 34 low- and middle- income countries.[1] We examine impacts on 12 primary as well as several secondary outcomes (typically components of a primary outcome). We also explore heterogeneity with respect to the following sources of variation: transfer size (both linear, the primary specification, as well as curvature), 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 frames that suggest a child-focus to households.

We use a Bayesian hierarchical model to jointly estimate average treatment effects of UCT programs. We find strong, positive impacts on nine of twelve primary outcomes: Total household consumption, household food consumption, food security z-scores, total assets, income and profits, labor force participation (binary), school enrollment (binary), height-for-age z-scores, and psychological well-being z-score. Results for hours worked are positive but weaker. Results for weight-for-age z-scores and wasting (binary) also suggest

[1]Table A.1 describes the key design features of the 73 programs in our sample.

UCTs improve child health but findings are not statistically significant at 95% credibility.

Marginal impacts are constant across a wide variety of transfer sizes. That is, there is little evidence that asset-based poverty traps, i.e. indivisible investment thresholds necessary for further wealth accumulation, are at the root of poverty.[2]

We examine six 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, we also find that 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 examine long-run versus short-run results. The simplest test breaks the data at 18 months post transfers, comparing before to after, and finds strong positive treatment effects in the long run although at about 40% of the magnitude of the treatment effect in the first 18 months. Few papers however report long-run past 36 months. For ongoing stream transfers, we find increasing returns after 18 months, consistent with households both consuming and investing some of the grants.

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 thresholds 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. However, we are not able to reject linearity for impacts with respect to grant size.

Fourth, we examine how results differ for programs that target women and find that targeted transfers lead to higher observed consumption and higher income (versus

[2] Though it does not entirely rule out asset-based poverty traps as thresholds may be heterogeneous across sites, households, or beyond the range of transfer sizes tested.

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 in line with previous empirical work on the topic (Banerjee, Hanna, et al. 2017) and with poverty-trap models of labor supply in which poor households decrease labor supply simply because they are poor to begin with. It 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.

Table 1 situates 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 five dimensions.

First, we explicitly account for transfer size in estimating treatment effects instead of merely coding transfer receipt as a binary. This is consistent with Kondylis and Loeser 2021, the closest meta-analysis to ours in method and questions.

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 and fifth, we examine heterogeneity in impacts with respect to targeting females (versus males and versus untargeted) and with respect to child-focused framed (or "nudge") cash transfers, i.e., that are accompanied with either labels or some communication aspect promoting the cash transfers as intended for children's wellbeing.

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 publically 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 114 meet the inclusion criteria of our meta-analysis.[4]

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 behavioral compliance with certain conditions to continue receiving the cash transfer (most commonly school attendance).[5]
   (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).

[3]A complete description of our systematic search and a hyper-linked list of all the papers in our sample are provided in the supplementary materials.

[4]Table E.1 provides citations to the 114 papers in our sample.

[5]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).

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, or female empowerment.

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. 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 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.[6] 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 73 programs in our sample, 30 target women, 11 target men, 26 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

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

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.[7]

Total transfer amount and monthly tranche amount: 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 tranche variable by taking the average amount of money transferred per month during the UCT program. For stream transfer programs, this is often 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 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 allowing us to use older publications from before norms of data publication were more widespread and working papers for which data is 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

[7]See Appendix Table A.2 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.

arm and that contain fewer control variables.[8] Outcomes denominated in currency 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.[9] Once converted to appropriate units, we divide all treatment effects by the total transfer amount and monthly tranche amount to construct the two outcome variables used in our analysis. This allows our 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. See Figures 3 and 4 for a visualization of the extracted treatment effects per total transfer amount and per monthly tranche amount.

We prefer to use treatment effect per monthly tranche amount for stream transfers, because if much of the stream is consumed then using total transferred rather than monthly tranche amount would lead tenure of the program to bias results focused on consumption. Specifically, if streams are mostly immediately consumed rather than invested, examining impact per total amount transferred would lead a 24 month program to appear to be half as effective as a 12 month program even if their true impact is identical. By contrast, for lump sum the reverse is true: if recipients mostly invest lumpsum transfers (rather than immediately consume), impact per total transfer is the optimal outcome variable rather than impact per total transfer divided by months since the transfer. If on the other hand, the transfer is saved and then consumed down over time, the latter is optimal. For these reasons, we prefer to use treatment effect per monthly tranche amount for stream transfers and treatment effect per total amount transferred for lump sum UCT programs.

Months since program onset: Short-term and long-term effects: We extract the

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

[9]See supplementary materials for a complete description of how each outcome variable is converted to common units. Appendix Tables D.1 and D.2 also present the treatment effects on food security and psychological well-being outcomes before and after standardization.

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. This is in line with McGuire et al. 2022. 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 consider 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 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 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 is how to best aggregate information from multiple RCTs to estimate a measure of the general effect of its 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, lack of skilled implementers, 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, as well as regarding 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 $\hat{TE}$ of average treatment effects (ATEs) from comparable RCTs with corresponding standard errors $\hat{SE}$ and a set of RCT-level covariates X (e.g. whether the transfer schedule is a stream or a lump sum). 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 of the interventions correlate with higher or lower effects. Assume that the data generating model follows a linear hierarchical structure of the following nature:

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

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

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

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

The interpretation of the model is that treatment effect estimates are drawn from distinct and conditionally independent distributions centered around a parameter θ 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. $\mathcal{N}\left(X\beta,\ \sigma_\theta^2 I_N\right)$. The model is a generalization of the classical Rubin (1981) model, a simple random effects model, in line with a growing literature that uses more complex formulations to uncover dyanmic 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 outline the different specifications we use for the distribution of $\theta \mid \beta, \sigma_\theta$ in subsection 3.3.

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 1981). 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 1981 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 allows 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; 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 much weaker assumption than independence that many Frequentist approaches require). This allows modeling data as 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. 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, Noam Angrist 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. $\hat{se}$) 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 (A. B. Gelman et al. 1995, Noam Angrist 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 (A. B. Gelman et al. 1995). Moreover, priors can help improve the stability of estimates by providing what is known in the Frequentist framework as regularization (A. Gelman 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 (A. Gelman 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.

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 $\theta \sim \mathcal{N}\left(X\beta,\ \sigma_\theta^2 I_N\right)$ by expanding the set of covariates in X.

We start from the original Rubin (1981) random effects model:

$$\theta \mid \beta, \sigma_\theta \sim \mathcal{N}\left(\beta_1 \mathbf{1},\ \sigma_\theta^2 I_N\right) \tag{1}$$

Building on Equation (1), 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, that is 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:

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

In the subsequent version of our model, we build further on Equation (2) 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 so can collect interest, additional revenues, and can make further investments in assets:

$$\theta \mid \beta, \sigma_\theta \sim \mathcal{N}\big(\beta_1 L + \beta_2 CS + \beta_3 OS + \beta_4 M \odot C + \beta_5 M^2 \odot C \\ + \beta_6 M \odot OS + \beta_7 M^2 \odot OS,\ \sigma_\theta^2 I_N\big) \tag{3}$$

One drawback of Equation (3) 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 discretise 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:

$$\theta \mid \beta, \sigma_\theta \sim \mathcal{N}(\beta_1 ST \odot L + \beta_2 LT \odot L + \beta_3 ST \odot C + \beta_4 LT \odot C \\ + \beta_5 ST \odot OS + \beta_6 LT \odot OS,\ \sigma_\theta^2 I_N) \tag{4}$$

The disadvantage of this model is that it loses some information in discretising 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 decreasing marginal returns for lump sum transfers. Starting from Equation (3), we augment the model with the total amount transferred in PPP \$ as an additional covariate interacted with an indicator for lump sum transfers ($TA \odot L$). The model then becomes:

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

The last dimension of heterogeneity we choose to investigate is whether targeting the transfers to women or labelling it as for children 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:

$$
\begin{aligned}
\theta \mid \beta, \sigma_\theta &\sim \mathcal{N} \left(\beta_1 T + \beta_2 (1 - T),\ \sigma_\theta^2 I_N\right) \\
\theta \mid \beta, \sigma_\theta &\sim \mathcal{N} \left(\beta_1 F + \beta_2 (1 - F),\ \sigma_\theta^2 I_N\right)
\end{aligned} \tag{6}
$$

4 Results

Table 3 presents average treatment effects in the full sample, estimated using Equation (1). 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.

Tables 4 examines heterogeneity by disbursement schedule, i.e., by ongoing streams, completed streams, and lump sums, estimated using Equation (2). In Table 5, we show dynamic treatment effects on monthly household consumption estimated using Equations (3) and (4). In Panel B, we also estimate the curvature of effects with respect to transfer size, i.e. whether there are decreasing, increasing, or constant marginal returns to cash using Equation (5). Tables 6 and 7 analyze the impact of targeting by gender and framing by food security and child development goals, based on Equation (6). Finally, Table 8 presents benefit-cost ratios under three sets of assumptions regarding the temporal evolution of the treatment effect on consumption and program costs.

4.1 Do Cash Transfers Shift Labor Supply and Income?

Our first key finding is that UCTs generate positive impacts on income, with credibility intervals considerably removed from zero, thus clearly rejecting "dependency" theories that predict negative impacts on income. Specifically, Table 3 Column 1 shows positive impact on total income for both total transfer ($1.4/month per $100, 95% CI: 1.0, 1.8) and the monthly tranche amount ($21.3/month per $100, 95% CI: 14.1, 29.0).[10] Results are qualitatively similar in Table 4, in which we disaggregate estimates by disbursement schedule into ongoing streams, completed streams, and lump sum transfers.

Results on income are further supported by positive effects on labor force participation (LFP). Table 3 shows that UCTs increase LFP by 3.5 percentage points (95% CI: 1.7, 5.3) predicted at the median total transfer amount, and by 5.9 percentage points

[10]To construct the sample of treatment effects on total income, we use measures of total individual or household income when reported or the largest sub-category of income (e.g., wage earnings, household enterprise profits, etc.) available when total income is not reported. Table C.1 reports treatment effects on alternative measures of income, including a sample that just uses estimates on total individual or household income; predicted treatment effect sizes based on this sample are slightly larger than the effects we report in Table 3. Also, 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, which reflects a limitation of relying on estimates extracted directly from papers rather than using the studies' underlying data. We discuss this issue further in Appendix Section 6.2.

(95% CI: 2.7, 9.3) predicted at the median monthly tranche amount.[11] Table 4 further breaks down the analysis by disbursement schedule and shows consistently positive point estimates. With fewer studies per estimate, however, several of the credibility intervals include zero.

Total hours worked, in contrast, is one of three primary outcomes whose 95% credibility interval includes zero: Table 3 reports an increase of 0.4 hours per week (95% CI: -0.3 to 1.0) for the median total transfer amount and 0.2 hours per week (95% CI: -0.1 to 0.4) for the median monthly tranche amount. Table 4, which further disaggregates by disbursement schedule, finds even wider intervals. However estimates are from as few as two studies, and at most seven, so we draw little to no inference from the analysis on differential impact by disbursement schedule on hours worked.

Taken together, cash transfers consistently generate positive impacts, and at worst no effect, on labor supply. These results are consistent with the analysis in Banerjee, Hanna, et al. 2017, which examined seven studies (six conditional cash transfers and one UCT) and documents predominantly positive and at worst null results.

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 visa versa for investments or durable goods.

[11] 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 (less than half the sample median) 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.

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 \$9.3 (95% CI: 7.4, 11.4) increase in monthly total household consumption for the median total transfer amount and a \$14.5 (95% CI: 11.3, 18.0) increase for the median monthly tranche amount. The majority of the consumption increase comes from food: \$7.4 (95% CI: 5.2, 9.8) increase in monthly household food consumption for the median total transfer amount and \$14.3 (95% CI: 9.8, 19.3) for the median monthly tranche amount. Assets increase by 19.6% (95% CI: 12.2, 27.3) 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, completed stream programs produce results similar to lump sum transfers but different from ongoing stream programs. Table 4 Panel A reports similar point estimates regarding the treatment effect per total transfer amount for household consumption across all three disbursement schedules, with credibility intervals of each including the other two. However, when analyzed per monthly tranche amount (Panel B), the treatment effects on consumption are considerably stronger for ongoing streams. This is likely the consequence of recipients treating ongoing transfers similar to income, resulting in a higher marginal propensity to consume. 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. Specifically, ongoing streams of a \$100 monthly tranche boost consumption by \$48.5 (95% CI: 35.4, 62.5) compared to \$24.1 (95% CI: 8.3, 40.4) for completed stream programs and \$27.3 (95% CI: 17.2, 37.8) for lump sum transfers. Treatment effects per \$100 monthly tranche on monthly household food consumption are as large as \$50.9 (95% CI: 37.8, 65.4) for ongoing stream programs but only \$13.6 (95% CI: 1.9, 26.5) for lump sum transfers and close to zero and not statistically significant for completed stream programs.[12]

[12]Note, however, that data limitations are severe for completed stream programs: Only three such

Examining food security, differences between disbursement schedules persist but look less stark. [13] Table 4, Panel B shows that a \$100 monthly tranche yields a 0.8 standard deviation improvement (95% CI: 0.5, 1.1) in food security for ongoing streams, compared to 0.7 for completed streams (95% CI: 0.4, 1.0) 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.

Assets show 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 \$33.4 (95% CI: 16.4, 50.5) and \$21.7 (95% CI: 11.8, 32.2), respectively, while ongoing streams yield no statistically significant increase ($\beta = 1.5$; 95% CI: -16.9, 19.9). Estimates based on the amount of the monthly tranche yield qualitatively similar results across disbursement schedules.[14]

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 0.15 standard deviation increase at the median total transfer amount (95% CI: 0.09, 0.21).[15] The positive average treatment effect on psychological well-being is primarily driven by ongoing stream UCT programs (Table 4), i.e., even though economic impacts persist, the psychological well-being impacts dissipate more rapidly. Ongoing

programs report food consumption.

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

[14]Appendix Table C.2 reports treatment effects on various types of assets: durable assets, productive assets, and financial savings. However, we do not have sufficient data to conduct meaningful comparisons of impact by disbursement schedule on these disaggregated outcomes.

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

stream UCTs improve subjective measures of well-being by 1.1 standard deviations per $ 100 monthly tranche (95% CI: 0.7, 1.5; multiply by 44% to scale to the median monthly tranche amount). These large estimates are partially driven by three positive outliers from the Zambia Child Grant Program (CGP). [16] 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 and Curvature with respect to transfer amount

Next we examine dynamics. Considering the timing of impact assessment relative to program onset and completion offers further insight into patterns of consumption behavior by program type. In Table 5, we explore the dynamic impacts on total monthly household consumption over time. We choose to focus on this outcome for substantive and practical reasons: total household consumption is an aggregate measure of economic well-being, and with 83 estimates we have more observations than nearly any other outcome, thus more ability to estimate dynamic effects by disbursement schedule. It also contains a relatively balanced sample of ongoing stream, completed stream, and lump sum programs.

[16] 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: 0.2, 0.6) in the full sample or from 1.1 (95% CI: 0.7, 1.5) to 0.6 (95% CI: 0.4, 0.9) in the ongoing streams sample, as reported in Table C.3. 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 use an equivalent variable to construct our standardized outcome for any other program. Table D.2 reports treatment effects on psychological well-being before and after standardization.

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 substantially smaller effects for lump sum transfers in the long run. Figures 5 and 6 plot treatment effects for each primary outcome on the number of months since program onset to visualize the evidence base of our results.

As seen in Table 5, Panel A, we find evidence that the effects of ongoing stream transfers on household consumption are greater in the long run (18 months after transfer onset). As seen in Column 4, the long-term treatment effect per \$100 monthly tranche is \$70.9 (95% CI: 51.4, 91.6) while the short-term treatment effect per \$100 monthly tranche is \$31.6 (95% CI: 15.7, 48.3).[17] For completed stream programs and lump sum transfers, we do not observe statistically significant differences between short-term and long-term effects.

Panel B of Table 5 presents results from a polynomial model which interacts a continuous months variable and its squared term with ongoing and completed UCT programs.[18] Consistent with our findings in Panel A, we observe greater benefits over time for ongoing stream programs but virtually no dynamic effects for completed stream programs and lump sum transfers. As seen in Column 4, we estimate that an ongoing stream program raises monthly household consumption by \$28.9 (95% CI: 15.0, 43.6) per \$100 monthly

[17]Note this finding is not robust to our alternative outcome variable definition, as seen in Column 1 of Table 5. While we still estimate a larger long-term treatment effect, the credibility intervals of our estimates overlap. We observe the same pattern for our predicted treatment effects from the polynomial model in Panel B.

[18]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 A, we pool completed stream programs and lump sum transfers to estimate the coefficients on the months and months squared terms.

tranche at month 12 and by \$65.3 (95% CI: 47.9, 83.8) per \$100 monthly tranche at month 24. The coefficients on the months and months squared covariates, however, are not statistically significant. We also do not estimate statistically significant dynamic effects for completed stream programs and lump sum transfers, and predicted treatment effects do not change much between months 12 and 24. Yet, there is evidence from our binary model that lump sums have greater effects in the short run.[19]

Last, we examine whether lump sum transfers exhibit evidence of diminishing returns to capital by adding a covariate for transfer size to our model. Since our outcome variable is the treatment effect per dollar transferred, the interpretation of the coefficient on this covariate is equivalent to the second derivative of the treatment effect with respect to transfer amount. Results from this model are presented in Column 3 of Table 5, Panel B. We estimate a fairly precise null effect, meaning that there are constant returns to capital for lump sum UCTs.[20]

4.4 Targeting and Framing Effects

In Table 6, we report on the differential impact of programs targeted to women (versus to men or non-targeted). Programs targeted to women produce greater consumption effects than programs without any gender targeting: Female-targeted UCTs lead to a \$3.1 increase per \$100 total transfer amount in monthly total household consumption (95% CI: 2.4, 3.9) compared to a \$1.7 increase per \$100 total transfer amount (95% CI: 1.1, 2.2) for non-targeted programs. This difference appears to driven primary by greater food consumption. Female-targeted transfers on average also generate considerably larger

[19]Figure 7.2 plots the posterior average treatment effects on total consumption sorted by months since first transfer to visualize the relationship between effect size and months since first transfer.

[20]Figure 7.3 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 (curvature with respect to transfer size).

treatment effects on income: \$1.8 per \$100 of total transfer (95% CI: 1.1, 2.4) versus a 95% credibility interval of 0.4 to 1.4 for non-targeted UCTs. Other results do not differ, with credibility interval 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 lack the ability to credibly distinguish differences between male-targeted programs and female-targeted or non-targeted programs for any outcomes.

In Table 7, 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 larger and outside the 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.

4.5 Benefit-Cost Analysis

We construct three simple models of future cash flows to estimate the returns to UCTs and compare the relative benefits of various program designs. Similar to Blattman et al. 2016, we define benefits as the predicted treatment effects on consumption and costs as the total transfer amount, discounting all values to the first month of the program using a 5% discount rate. Our approach, however, adds a layer of sophistication by leveraging our dynamic effects results from Table 5.

We present the results of our benefit-cost analysis in Table 8. In Panel A, we use our estimates from Panel A of Table 5, assuming that short-term effects are constant until month 18 and long-term effects are constant after month 18. In Panels B and C, we use our estimates from Panel B of Table 5. In Panel B, we use moderate assumptions that our dynamic effects persist as predicted by our model until the month where the dynamic dissipation effect reaches its minimum. We then assume constant effects from that month on. In Panel C, our assumptions are consistent with Panel B until the month where the dissipation effect reaches its local minimum. At that point, we use the pessimistic assumption that benefits dissipate linearly to zero using the average rate of change over the months since the last transfer. See Figure 8 for a visualization of how our three models predict benefits over time.

Under moderate assumptions, the benefits of UCTs exceed their costs, regardless of transfer frequency or program duration. This remains true even when we assume administrative costs comprising 24% of the transfer amount except under our most pessimistic assumptions.[21] For lump sum UCTs, we estimate the benefit-cost ratio (BCR) is 2.91 according to our binary model, as seen in Panel A. Using our dynamic effects polynomial model, we see the BCR of lump sum transfers is 1.6 under moderate assumptions (Panel B) or 0.9 under pessimistic assumptions (Panel C). Applying 24% administrative costs, we instead find BCRs of 2.36, 1.27, and 0.70 for Panels A, B, and C, respectively.

We detect diminishing marginal returns to longer stream programs using our binary model and our polynomial model under moderate assumptions. While ongoing stream transfers have greater impacts on consumption than completed programs, especially in the long-term, as shown in Table 5, these additional benefits do not outweigh ongoing program costs. This finding does not hold under pessimistic assumptions where effects dissipate to zero after program completion. Using our binary model (Panel A), we calculate BCRs ranging from 5.32 for a stream UCT program that lasts 12 months to 1.72 for a stream

[21] As reported in Appendix Table A.3, 24% is the median and mean administrative costs as a percent of transfer among the 10 of 73 programs in our sample that report costs.

UCT program that lasts 48 months. Using our dynamic effects polynomial model, we estimate BCRs ranging from 4.07 to 1.60 under moderate assumptions (Panel B) or 1.01 to 0.72 under pessimistic assumptions (Panel C). Across all models, 12-month stream programs outperform lump sums and longer duration stream programs.

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 transfers are an opportune type of 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 114 studies from 73 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, food security, income, assets, labor force participation, child height-for-age, school enrollment, and psychological well-being. Total monthly household consumption increases by $49 per $100 monthly transfer in response to ongoing stream programs and by $1.8 per $100 transferred (i.e., a 22% annualized social return on investment) in response to lump sums. Monthly income improves by $30 per $100 monthly tranche for ongoing stream transfers and by $1.5 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.[22] Furthermore, considering transfer frequency and timing relative 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

[22] 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 $100 total transfer amount, respectively (**<empty citation>**)citebaird$_c$onditional$_2$014.Thisislargerthanourestimateof1.0percentagepoints(95%CI : 0.5, 1.5)onoverallenrollment.BairdEtal.2014alsodirectlycomparesCCTstoUCTs, estimatinglargerbutnotstatisticallysi al.2020; Manley, Alderman, Etal.2022).

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. Negligible dissipation rates simplify lead to particularly favorable benefit-cost analyses ranging from 1.3 to 3.3, but we point out that while measurement up to 48 months is common, even longer run measurement remains quite rare.

Second, we find constant marginal returns with respect to transfer size. The coefficients on the squared term for transfer size is precisely estimated and close to zero, 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; i.e., our failure to find evidence to support is not the same as evidence against.

We close with two methodological considerations. With respect to many of the most interesting questions, our analysis is severely constrained by the lack of more fine-grained data. 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).

More broadly, there is a clear need for more, and more long-term, follow-up data (Bouguen et al. 2019). Further follow-ups would help trace out potential dissipation effects, as most data on lump sum transfers are collected 12 to 48 months after treatment. Furthermore, while pressure comes from both research and policy for long-run measurement, we suggest that more *immediate* data would be beneficial, particularly 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).

Despite these limitations, we believe aggregating reported point estimates at the study-level sheds important light on several theoretical and policy questions. That said, many questions cannot be addressed without access to the primary data, and the questions posed could be answered even better with the more granular household-level data. By just using reported point estimates at the study-wave-level, 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. In addition, important program, study, and context variables we do have variation on could not be included in our preferred specifications due to power considerations. Clearly, more is left to be learned from the more arduous but worthy task of merging data across primary studies (see, e.g., Meager 2019, 2022). Yet with 114 studies and viable methods for aggregation, we can learn much more as a whole than considering each paper in isolation.

References

[1] L. Brown Alexander et al. *Meta-Analysis of Empirical Estimates of Loss-Aversion.* eng. CESifo Working Paper 8848. Munich, 2021.

[2] Joshua Alley. “Using Hierarchical Models to Estimate Heterogeneous Effects”. In: *Working Paper* (Sept. 2022).

[3] Ciro Avitabile, Jesse M. Cunha, and Ricardo Meilman Cohn. *The Medium Term Impacts of Cash and In-Kind Food Transfers on Learning.* Dec. 2019.

[4] Sarah Baird et al. “Conditional, Unconditional and Everything in Between: a Systematic Review of the Effects of Cash Transfer Programmes on Schooling Outcomes”. In: *Journal of Development Effectiveness* 6.1 (Jan. 2014). Number: 1, pp. 1–43.

[5] Oriana Bandiera et al. “Do Women Respond Less to Performance Pay? Building Evidence from Multiple Experiments”. In: *American Economic Review: Insights* 3.4 (Dec. 2021), pp. 435–54.

[6] Abhijit Banerjee, Esther Duflo, et al. “A Multifaceted Program Causes Lasting Progress for the Very Poor: Evidence from Six Countries”. In: *Science* 348.6236 (2015), p. 1260799.

[7] Abhijit Banerjee, Rema Hanna, et al. “Debunking the Stereotype of the Lazy Welfare Recipient: Evidence from Cash Transfer Programs”. In: *World Bank Research Observer* 32.2 (Aug. 2017), pp. 155–184.

[8] Francesca Bastagli, Jessica Hagen-Zanker, and Georgina Sturge. *Cash Transfers: What Does the Evidence Say?* Overseas Development Institute, July 2016.

[9] Michael Betancourt. *Hierarchical Modeling.* Nov. 2020.

[10] Christopher Blattman et al. “The Returns to Microenterprise Support among the Ultrapoor: A Field Experiment in Postwar Uganda”. In: *American Economic Journal: Applied Economics* 8.2 (Apr. 2016), pp. 35–64.

[11] Michael Borenstein et al. "A basic introduction to fixed-effect and random-effects models for meta-analysis". In: *Research Synthesis Methods* 1.2 (2010), pp. 97–111.

[12] Adrien Bouguen et al. "Using Randomized Controlled Trials to Estimate Long-Run Impacts in Development Economics". In: *Annual Review of Economics* 11.1 (2019), pp. 523–561.

[13] Marshall Burke, Solomon M. Hsiang, and Edward Miguel. "Climate and Conflict". In: *Annual Review of Economics* 7.1 (2015), pp. 577–617.

[14] *Cash Evidence Explorer*. GiveDirectly. Apr. 2023. URL: `https://www.givedirectly.org/cash-evidence-explorer/`.

[15] Haitao Chu, Sining Chen, and Thomas A. Louis. "Random Effects Models in a Meta-Analysis of the Accuracy of Two Diagnostic Tests Without a Gold Standard". In: *Journal of the American Statistical Association* 104.486 (2009), pp. 512–523.

[16] Kevin Croke et al. *Meta-Analysis and Public Policy: Reconciling the Evidence on Deworming*. July 2016.

[17] Bruno De Finetti. *Probability, Induction and Statistics. The Art of Guessing.* In collab. with Bruno De Finetti. John Wiley & Sons, Jan. 1972.

[18] Andrew Gelman, Daniel Simpson, and Michael Betancourt. "The Prior Can Often Only Be Understood in the Context of the Likelihood". In: *Entropy* 19.10 (2017).

[19] Andrew B. Gelman et al. *Bayesian Data Analysis.* Boca Ratan, Florida: Chapman and Hall/CRC, 1995.

[20] Trevor Hastie, Robert Tibshirani, and Jerome Friedman. *The Elements of Statistical Learning*. Springer Series in Statistics. New York, NY, USA: Springer New York Inc., 2001.

[21] Bart Heeg et al. "Bayesian hierarchical model-based network meta-analysis to overcome survival extrapolation challenges caused by data immaturity". In: *Journal of Comparative Effectiveness Research* 12 (Jan. 2023).

[22] Melissa Hidrobo and Lia Fernald. “Cash Transfers and Domestic Violence”. In: *Journal of Health Economics* 32.1 (Jan. 2013). Number: 1, pp. 304–319.

[23] 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 2015). Number: 3, pp. 290–303.

[24] Dean Karlan, Adam Osman, and Jonathan Zinman. “Follow the Money Not the Cash: Comparing Methods for Identifying Consumption and Investment Responses to a Liquidity Shock”. In: *Journal of Development Economics* 121 (July 1, 2016), pp. 11–23. ISSN: 0304-3878.

[25] Florence Kondylis and John Loeser. *Intervention Size and Persistence*. Publisher: World Bank, Washington, DC. Sept. 2021.

[26] Yulun Liu, Stacia DeSantis, and Yong Chen. “Bayesian Mixed Treatment Comparisons Meta-Analysis for Correlated Outcomes Subject to Reporting Bias”. In: *Journal of the Royal Statistical Society: Series C (Applied Statistics)* 67 (Mar. 2017).

[27] James Manley, Harold Alderman, and Ugo Gentilini. “More Evidence on Cash Transfers and Child Nutritional Outcomes: a Systematic Review and Meta-Analysis”. In: *BMJ Global Health* 7.4 (Apr. 2022). Number: 4, e008233.

[28] James Manley, Yarlini Balarajan, et al. “Cash Transfers and Child Nutritional Outcomes: a Systematic Review and Meta-Analysis”. In: *BMJ global health* 5.12 (Dec. 2020), e003621.

[29] 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, pp. 359–370.

[30] Rachael Meager. “Aggregating Distributional Treatment Effects: A Bayesian Hierarchical Analysis of the Microcredit Literature”. In: *American Economic Review* 112.6 (June 2022), pp. 1818–47.

[31] Rachael Meager. “Understanding the Average Impact of Microcredit Expansions: A Bayesian Hierarchical Analysis of Seven Randomized Experiments”. In: *American Economic Journal: Applied Economics* 11.1 (Jan. 2019). Number: 1, pp. 57–91.

[32] Sendhil Mullainathan and Eldar Shafir. *Scarcity: Why Having Too Little Means So Much*. Times Books, Sept. 2013. 302 pp.

[33] Rachael Meager Noam Angrist. “Implementation Matters: Generalizing Treatment Effects in Education”. In: 802 (June 2023).

[34] Stephen W. Raudenbush and Anthony S. Bryk. “Empirical Bayes Meta-Analysis”. In: *Journal of Educational Statistics* 10.2 (1985). Number: 2, pp. 75–98.

[35] Donald B. Rubin. “Estimation in Parallel Randomized Experiments”. In: *Journal of Educational Statistics* 6.4 (1981). Number: 4, pp. 377–401.

[36] Stan. *Stan User’s Guide*. 2022. URL: `https://mc-stan.org/docs/stan-users-guide/index.html`.

[37] Daniel Stegmueller. “How Many Countries for Multilevel Modeling? A Comparison of Frequentist and Bayesian Approaches”. In: *American Journal of Political Science* 57.3 (2013), pp. 748–761.

[38] Richard H. Thaler and Cass R. Sunstein. *Nudge: Improving Decisions About Health, Wealth, and Happiness*. Penguin, Feb. 24, 2009. 322 pp. ISBN: 978-0-14-311526-7.

[39] Eva Vivalt. “How Much Can We Generalize From Impact Evaluations?” In: *Journal of the European Economic Association* 18.6 (Sept. 2020), pp. 3045–3089.

[40] Clara Wollburg et al. “Do Cash Transfers Alleviate Common Mental Disorders in Low- and Middle-Income Countries? A Systematic Review and Meta-Analysis”. In: *PloS One* 18.2 (Feb. 2023). Number: 2, e0281283.

Table 1a
Comparison of Cash Transfer Meta-Analyses Papers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Number of observations			*Identification (count of studies)*		*Conditionality (count of studies)*		*Timing (count of studies)*	
Meta-analysis	**Studies**	**Programs**	**Estimates**	**RCT**	**Quasi-experimental**	**UCT**	**CCT**	**Lump sum**	**Stream**
This study	114	73	699	114	0	114	0	44	77
Baird et al. (2014)	75	35	64	12	23	9	30		
Baranov 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	0	94	7	40
Guimarães et al. (2023)	16	14		16	0	2	14	1	15
Kabeer and Waddington (2015)	46	11		Yes	Yes	0	46	0	46
Kondylis and Loeser (2021)	7	7	18	7	0	7	0	4	4
Little et al. (2021)	17	17		14	3	7	10	0	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	0	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 exceeds 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	854	19 months since first transfer	Treatment effect (TE) per dollar transferred	Consumption, food security, assets, income, labor supply (adult), 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
Baranov 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. (2023)	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 USD PPP. For this study, we report means across programs in the primary outcomes analysis sample.

Table 2
Count of Programs and Estimates by Program Design Features

	(1) All	(2) Lump Sum	(3) Stream-Ended	(4) Stream-Ongoing
Panel A: Count of Programs for Primary Outcomes				
# of Programs	73	39	17	30
# of Programs, Transfer paid physical cash	33	12	9	18
# of Programs, Transfer paid via mobile money or bank transfer	38	25	8	12
# of Programs, Implemented by government	22	5	6	15
# of Programs, Implemented by NGO	37	25	10	11
# of Programs, Implemented by researchers	15	10	1	4
# of Programs, Framing for child development or food security	20	3	6	16
# of Programs, No framing for child development or food security	53	36	11	14
# of Programs, Transfer targeted to women	33	11	8	19
# of Programs, Transfer not targeted or randomized to men or women	35	24	9	10
# of Programs, Transfer targeted to men	5	4	0	1
Panel B: Count of Estimates for Primary Outcomes				
# of Estimates	494	242	84	147
# of Estimates, Transfer paid physical cash	186	55	30	101
# of Estimates, Transfer paid via mobile money or bank	291	170	54	46
# of Estimates, Implemented by government	136	27	11	98
# of Estimates, Implemented by NGO	303	175	70	41
# of Estimates, Implemented by researchers	55	40	3	8
# of Estimates, Framing for child development or food security	123	12	22	89
# of Estimates, No framing for child development or food security	371	230	62	58
# of Estimates, Transfer targeted to women	208	72	41	93
# of Estimates, Transfer not targeted or randomized to men or women	265	155	41	48
# of Estimates, Transfer targeted to men	21	15	0	6

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) Predicted Treatment Effect of $100 Transfer	(2) Predicted Treatment Effect of Median Transfer	(3) Estimates (Programs)
Panel A. Treatment Effect per Total Transfer Amount			
Monthly Household Consumption	2.2 (1.7, 2.7)	9.3 (7.4, 11.4)	82 (45)
Monthly Household Food Consumption	1.7 (1.2, 2.3)	7.4 (5.2, 9.8)	49 (31)
Food Security z-Score	0.03 (0.02, 0.04)	0.14 (0.1, 0.17)	47 (25)
Total Monthly Income	1.4 (1, 1.8)	5.8 (4, 7.7)	88 (38)
Stock of Total Assets	19.6 (12.2, 27.3)	82.5 (51.4, 115.1)	57 (28)
Total Hours Worked per Week	0.1 (-0.1, 0.2)	0.4 (-0.3, 1)	25 (13)
Labor Force Participation (percentage points)	0.8 (0.4, 1.3)	3.5 (1.7, 5.3)	17 (11)
Height-for-Age z-Score	0.0 (0.002, 0.01)	0.03 (0.01, 0.06)	32 (18)
Weight-for-Age z-Score	0.0 (-0.0001, 0.01)	0.03 (-0.0005, 0.05)	15 (10)
Stunting (percentage points)	-0.2 (-0.6, 0.2)	-0.9 (-2.5, 0.7)	12 (8)
School Enrollment (percentage points)	1.0 (0.5, 1.5)	4.1 (1.9, 6.4)	26 (16)
Psychological Well-being z-Score	0.04 (0.02, 0.05)	0.15 (0.09, 0.21)	56 (30)
Panel B. Treatment Effect per Monthly Tranche Amount			
Monthly Household Consumption	33.2 (25.9, 41.2)	14.5 (11.3, 18.0)	82 (45)
Monthly Household Food Consumption	32.7 (22.4, 44.1)	14.3 (9.8, 19.3)	49 (31)
Food Security z-Score	0.6 (0.4, 0.7)	0.2 (0.2, 0.3)	47 (25)
Total Monthly Income	21.3 (14.1, 29)	9.3 (6.2, 12.7)	88 (38)
Stock of Total Assets	245.5 (146.8, 352.9)	107.3 (64.2, 154.2)	57 (28)
Total Hours Worked per Week	0.5 (-0.1, 1)	0.2 (-0.1, 0.4)	25 (13)
Labor Force Participation (percentage points)	13.6 (6.2, 21.2)	5.9 (2.7, 9.3)	17 (11)
Height-for-Age z-Score	0.13 (0.05, 0.2)	0.06 (0.02, 0.09)	32 (18)
Weight-for-Age z-Score	0.08 (0.01, 0.2)	0.04 (0.003, 0.07)	15 (10)
Stunting (percentage points)	-4.5 (-12.5, 3.7)	-1.9 (-5.5, 1.6)	12 (8)
School Enrollment (percentage points)	14.5 (6.4, 23.2)	6.3 (2.8, 10.1)	26 (16)
Psychological Well-being z-Score	0.5 (0.3, 0.7)	0.2 (0.1, 0.3)	56 (30)

95% credibility intervals in parentheses. All currency values are reported in 2010 USD PPP. Treatment effect per total transfer amount (Panel A) is our preferred outcome variable for lump sum transfers.Treatment effect per monthly tranche amount (Panel B) is our preferred outcome variable for stream transfers. For lump sum UCTs, the monthly tranche amount is calculated by dividing the total transfer amount by the number of months since the first transfer. The median total transfer amount is $422, which is calculated by taking the median of the average total transfer amounts of the 39 lump sum programs in our sample. The median monthly tranche amount is $44, which is calculated by taking the median of the average monthly tranche amounts of the 38 stream programs in our sample. Our dataset for Total 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 Appendix Table C.1. for a comparison to analysis that only uses reported estimates on total household or individual income.

Table 4
Heterogenous Treatment Effects by Disbursement Schedule

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Predicted Treatment Effect of $100			*Predicted Treatment Effect of Median Transfer*				*Estimates (Programs)*	
	Ongoing Stream	Completed Stream	Lump Sum	Ongoing Stream	Completed Stream	Lump Sum	Ongoing Stream	Completed Stream	Lump Sum
Panel A. Treatment Effect per Total Transfer Amount									
Monthly Household Consumption	2.7	2.4	1.8	11.4	9.9	7.7	27	14	41
	(1.9, 3.5)	(1.2, 3.5)	(1.2, 2.5)	(8.1, 14.9)	(5.2, 14.7)	(4.9, 10.6)	(20)	(7)	(25)
Monthly Household Food Consumption	2.5	0.4	0.9	10.5	1.6	3.7	22	5	21
	(1.8, 3.1)	(-0.8, 1.6)	(0.2, 1.6)	(7.7, 13.6)	(-3.4, 6.7)	(0.9, 6.8)	(15)	(3)	(15)
Food Security z-Score	0.04	0.04	0.02	0.15	0.18	0.10	14	12	19
	(0.02, 0.05)	(0.03, 0.06)	(0.01, 0.04)	(0.09, 0.22)	(0.12, 0.25)	(0.04, 0.16)	(9)	(6)	(13)
Total Monthly Income	1.7	1.1	1.5	7.2	4.5	6.3	11	12	63
	(0.6, 2.9)	(0.04, 2.1)	(0.9, 2.1)	(2.5, 12.1)	(0.2, 9)	(3.9, 8.8)	(7)	(4)	(28)
Stock of Total Assets	1.5	33.4	21.7	6.4	140.8	91.6	7	9	38
	(-16.9, 19.9)	(16.4, 50.5)	(11.8, 32.2)	(-71.0, 84.0)	(69.3, 213.1)	(49.6, 135.6)	(5)	(3)	(22)
Total Hours Worked per Week	0.3	0.0	0.2	1.4	-0.2	0.9	3	5	13
	(-0.1, 0.7)	(-0.4, 0.3)	(-0.001, 0.4)	(-0.3, 3)	(-1.5, 1.1)	(-0.003, 1.8)	(2)	(2)	(7)
Labor Force Participation (percentage points)	0.6	0.8	1.1	2.7	3.4	4.6	6	5	6
	(-0.1, 1.4)	(-0.01, 1.6)	(0.3, 1.9)	(-0.6, 5.9)	(-0.03, 6.8)	(1.3, 8)	(5)	(2)	(4)
Height-for-Age z-Score	0.01	0.02	0.01	0.02	0.09	0.04	20	6	4
	(-0.001, 0.01)	(0.01, 0.04)	(-0.01, 0.03)	(-0.005, 0.1)	(0.03, 0.2)	(0, 0.1)	(13)	(5)	(2)
Weight-for-Age z-Score	0.02	0.01	0.00	0.07	0.03	-0.01	7	2	4
	(0.003, 0.03)	(-0.01, 0.02)	(-0.01, 0.01)	(0.01, 0.1)	(-0.05, 0.1)	(-0.1, 0.04)	(6)	(2)	(2)
School Enrollment (percentage points)	1.2	0.6	0.3	4.8	2.5	1.1	15	2	6
	(0.4, 2)	(-1.3, 2.5)	(-0.8, 1.3)	(1.7, 8.3)	(-5.6, 10.4)	(-3.2, 5.5)	(10)	(2)	(4)
Psychological Well-being z-Score	0.07	0.01	0.02	0.29	0.06	0.08	15	12	26
	(0.04, 0.1)	(-0.02, 0.04)	(0, 0.04)	(0.19, 0.4)	(-0.07, 0.18)	(-0.01, 0.16)	(9)	(7)	(16)
Panel B. Treatment Effect per Monthly Tranche Amount									
Monthly Household Consumption	48.5	24.1	27.3	21.2	10.5	11.9	27	14	41
	(35.4, 62.5)	(8.3, 40.4)	(17.2, 37.8)	(15.5, 27.3)	(3.6, 17.6)	(7.5, 16.5)	(20)	(7)	(25)
Monthly Household Food Consumption	50.9	6.4	13.6	22.2	2.8	6.0	22	5	21
	(37.8, 65.4)	(-14.9, 28.4)	(1.9, 26.5)	(16.5, 28.6)	(-6.5, 12.4)	(0.9, 11.6)	(15)	(3)	(15)
Food Security z-Score	0.8	0.7	0.4	0.3	0.3	0.2	14	12	19
	(0.5, 1.1)	(0.4, 1)	(0.1, 0.6)	(0.2, 0.5)	(0.2, 0.4)	(0.1, 0.3)	(9)	(6)	(13)
Total Monthly Income	29.9	15.4	22.5	13.1	6.7	9.8	11	12	63
	(12.1, 48.5)	(-0.7, 32.5)	(13.3, 32.4)	(5.3, 21.2)	(-0.3, 14.2)	(5.8, 14.1)	(7)	(4)	(28)
Stock of Total Assets	33.4	241.0	344.2	14.6	105.3	150.4	7	9	38
	(-232.8, 300.3)	(5.6, 477.7)	(193.8, 509.9)	(-101.7, 131.2)	(2.4, 208.7)	(84.7, 222.8)	(5)	(3)	(22)
Total Hours Worked per Week	1.7	-0.1	0.6	0.7	0.0	0.2	3	5	13
	(0.4, 2.9)	(-1.1, 0.8)	(-0.2, 1.4)	(0.2, 1.3)	(-0.5, 0.4)	(-0.1, 0.6)	(2)	(2)	(7)
Labor Force Participation (percentage points)	9.6	15.1	16.2	4.2	6.6	7.1	6	5	6
	(-5, 24)	(1.2, 29.4)	(2.4, 30.1)	(-2.2, 10.5)	(0.5, 12.9)	(1, 13.2)	(5)	(2)	(4)
Height-for-Age z-Score	0.1	0.3	0.2	0.04	0.12	0.09	20	6	4
	(-0.004, 0.2)	(0.1, 0.5)	(-0.1, 0.5)	(-0.002, 0.09)	(0.03, 0.21)	(-0.05, 0.24)	(13)	(5)	(2)
Weight-for-Age z-Score	0.1	0.1	0.0	0.06	0.06	-0.01	7	2	4
	(-0.015, 0.3)	(-0.2, 0.5)	(-0.2, 0.2)	(-0.01, 0.1)	(-0.1, 0.2)	(-0.1, 0.1)	(6)	(2)	(2)
School Enrollment (percentage points)	17.5	11.2	-2.2	7.7	4.9	-1.0	15	2	6
	(8.7, 27.8)	(-11.2, 32.3)	(-13.2, 8.7)	(3.8, 12.2)	(-4.9, 14.1)	(-5.8, 3.8)	(10)	(2)	(4)
Psychological Well-being z-Score	1.1	0.1	0.2	0.5	0.0	0.1	15	12	26
	(0.7, 1.5)	(-0.4, 0.5)	(-0.1, 0.5)	(0.3, 0.6)	(-0.2, 0.2)	(-0.04, 0.2)	(9)	(7)	(16)

95% credibility intervals in parentheses. All currency values are reported in 2010 USD PPP. Treatment effect per total transfer amount (Panel A) is our preferred outcome variable for lump sum transfers. Treatment effect per monthly amount (Panel B) is our preferred outcome variable for stream transfers. To compute Column 2, we use the median lump sum transfer amount for Panel A ($422) and the median stream monthly transfer amount for Panel B ($44). Our dataset for **Total 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 Appendix Table C.1. for a comparison to analysis that only uses reported estimates on total household or individual income. We do not report results on stunting due to data limitations.

Table 5
Dynamic Effects on Monthly Household Consumption by Disbursement Schedule and Curvature with respect to Transfer Amount for Lump Sums

	(1)	(2)	(3)	(4)	(5)	(6)
	Predicted Treatment Effect per $100 Total Transfer Amount			*Predicted Treatment Effect per $100 Monthly Tranche Amount*		
	Sum of Stream Transfers, Ongoing Program	Sum of Stream Transfers, Completed Program	Lump Sum	Ongoing Stream Program	Completed Stream Program	Lump Sum / Months Since Transfer Onset
Panel A: Dynamic Effects Binary Model: Short-run versus Long-run						
Predicted Treatment Effects						
Estimated on Short-Term Estimates (measurement up to 18 months after first transfer)	2.2	4.4	2.7	31.6	21.4	33.0
	(1.1, 3.3)	(2.2, 6.7)	(1.7, 3.6)	(15.7, 48.3)	(-4.0, 46.8)	(19.7, 46.3)
Estimated on Long-Term Estimates (measurement more than 18 months after first transfer)	3.1	1.7	1.1	70.9	25.1	19.8
	(2.1, 4.2)	(0.5, 2.9)	(0.2, 1.9)	(51.4, 91.6)	(5.7, 45.1)	(5.1, 35.1)
Count of Estimates						
Short-Term	15	4	23	15	4	23
Long-Term	12	10	18	12	10	18
Panel B. Dynamic Effects Polynomial Model (months and months-squared)						
Base and Dynamic Effects						
Base Effect	-1.1	2.9	2.8	3.3	27.1	32.4
	(-5.8, 3.5)	(1.4, 4.4)	(1.0, 4.6)	(-61.1, 67.2)	(7.0, 47.3)	(9.0, 55.5)
Months	0.3	-0.1		1.7	-0.6	
	(-0.1, 0.8)	(-0.2, 0.1)		(-2.3, 9.0)	(-2.3, 1.2)	
Months-Squared	-0.01	0.00		0.04	0.01	
	(-0.02, 0.01)	(-0.001, 0.001)		(-0.14, 0.21)	(-0.01, 0.03)	
Curvature Effect (squared term of transfer amount) of $100 Increase in Transfer Amount			0.00			
			(-0.001, 0.001)			
Predicted Treatment Effects						
Estimated at Month 12	2.1	2.2	2.1	29.2	21.7	26.9
	(1.0, 3.2)	(1.0, 3.4)	(1.3, 2.9)	(15.1, 44.0)	(6.5, 37.6)	(17.0, 37.1)
Estimated at Month 24	3.5	1.6	1.5	66.0	19.4	24.7
	(2.3, 4.7)	(-0.1, 3.4)	(0.6, 2.5)	(47.8, 85.3)	(-3.8, 43.7)	(10.5, 39.4)
Estimated at 20th Percentile of Transfer Amount at Month 12			2.0			
			(0.9, 3.2)			
Estimated at 80th Percentile of Transfer Amount at Month 12			2.1			
			(1.3, 3.0)			
Estimated at 20th Percentile of Transfer Amount at Month 24			1.4			
			(0.2, 2.7)			
Estimated at 80th Percentile of Transfer Amount at Month 24			1.6			
			(0.6, 2.5)			
Count of Estimates						
0 to 12 months since first (last) transfer	14	8	18	14	8	18
13 to 24 months since first (last) transfer	10	4	19	10	4	19
25 to 36 months since first (last) transfer	3	2	2	3	2	2
37 to 48 months since first (last) transfer	0	0	1	0	0	1
108 months since first (last) transfer	0	0	1	0	0	1

95% credibility intervals in parentheses. All currency values are reported in 2010 USD PPP. In Panel B, for dynamic effects for ongoing stream programs we define "months" as months since first transfer, whereas for completed stream programs and lump sums, we define "months" as months since the last transfer. Accordingly, estimate counts are based on months since first transfer for ongoing stream programs and months since last transfer for completed stream programs and lump sum transfers. Due to data limitations and similarity of average results, we estimate dynamic effects jointly on completed stream programs and lump sum programs in our polynomial model, which is why the estimates in Columns 2 and 3 (and in 5 and 6) of Panel B are reported as one for the two columns. We only estimate curvature effects for lump sum UCTs. **In Panel B**, The curvature effect of a $100 increase in transfer amount and the predicted treatment effects of 20th and 80th percentile transfer amounts using a different model that includes a covariate for total transfer amount interacted with the indicator for lump sum programs. The 20th and 80th percentile total transfer amounts across lump sum programs are $242 and $1248, respectively.

Table 6

Heterogenous Treatment Effects on Primary Outcomes by Gender Targeting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Predicted Treatment Effect of $100 Transfer			*Predicted Treatment Effect of Median Transfer*				*Estimates (Programs)*	
	Not Targeted	Targeted to Women	Targeted to Men	Not Targeted	Targeted to Women	Targeted to Men	Not Targeted	Targeted to Women	Targeted to Men
Panel A. Treatment Effect per Total Transfer Amount									
Monthly Household Consumption	1.7	3.1	1.2	7.0	13.2	5.2	45	33	5
	(1.1, 2.2)	(2.4, 3.9)	(-1.6, 4.1)	(4.6, 9.4)	(10.1, 16.5)	(-6.5, 17.1)	(20)	(22)	(5)
Monthly Household Food Consumption	0.8	2.6		3.4	10.9		23	27	
	(0.2, 1.4)	(2.4, 3.9)		(0.9, 6.1)	(10.1, 16.5)		(13)	(18)	
Food Security z-Score	0.03	0.03		0.14	0.13		26	21	
	(0.02, 0.04)	(0.02, 0.05)		(0.09, 0.19)	(0.08, 0.19)		(12)	(14)	
Total Monthly Income	0.9	1.8	3.8	3.7	7.4	16.0	41	40	7
	(0.4, 1.4)	(1.1, 2.4)	(1.8, 5.8)	(1.6, 6)	(4.8, 10.1)	(7.6, 24.6)	(19)	(16)	(4)
Stock of Total Assets	17.1	19.7	44.3	71.9	83.1	186.6	39	14	4
	(7.5, 26.8)	(5.7, 34.1)	(15.3, 74.2)	(31.5, 112.8)	(24.2, 143.5)	(64.7, 312.8)	(16)	(10)	(4)
Labor Force Participation (percentage points)	0.9	0.8		3.6	3.5		7	10	
	(0.2, 1.5)	(0.2, 1.4)		(0.3, 6.5)	(1.0, 6.0)		(5)	(6)	
Height-for-Age z-Score	0.02	0.00		0.07	0.01		11	21	
	(0.01, 0.03)	(-0.002, 0.01)		(0.03, 0.12)	(-0.01, 0.04)		(4)	(14)	
Weight-for-Age z-Score	0.00	0.01		0.01	0.06		7	8	
	(-0.01, 0.01)	(0.005, 0.02)		(-0.02, 0.04)	(0.02, 0.09)		(3)	(7)	
School Enrollment (percentage points)	0.8	1.3		3.4	5.4		16	10	
	(0.2, 1.5)	(0.4, 2.2)		(0.7, 6.3)	(1.7, 9.3)		(10)	(6)	
Psychological Well-being z-Score	0.03	0.05	0.02	0.1	0.2	0.1	26	25	6
	(0.01, 0.05)	(0.03, 0.08)	(-0.03, 0.07)	(0.03, 0.2)	(0.1, 0.3)	(-0.1, 0.3)	(12)	(16)	(5)
Panel B. Treatment Effect per Monthly Tranche Amount									
Monthly Household Consumption	23.0	57.0	10.8	10.1	24.9	4.7	45	33	5
	(14.6, 31.6)	(44, 70.9)	(-39.9, 61.8)	(6.4, 13.8)	(19.2, 31)	(-17.4, 27)	(20)	(22)	(5)
Monthly Household Food Consumption	13.0	50.6		5.67	22.11		23	27	
	(1.81, 24.6)	(37.44, 65)		(0.789, 10.74)	(16.36, 28.41)		(13)	(18)	
Food Security z-Score	0.5	0.6		0.2	0.3		26	21	
	(0.3, 0.7)	(0.4, 0.9)		(0.1, 0.3)	(0.2, 0.4)		(12)	(14)	
Total Monthly Income	13.5	29.0	61.4	5.9	12.7	26.8	41	40	7
	(5.3, 22.4)	(18.3, 40.5)	(24.1, 99)	(2.3, 9.8)	(8, 17.7)	(10.5, 43.3)	(19)	(16)	(4)
Stock of Total Assets	203.9	370.9	827.0	89.1	162.1	361.4	39	14	4
	(46.6, 365.1)	(134.8, 630.8)	(288.7, 1424.2)	(20.4, 159.5)	(58.9, 275.6)	(126.1, 622.3)	(16)	(10)	(4)
Labor Force Participation (percentage points)	12.2	14.6		5.4	6.4		7	10	
	(-0.2, 24.6)	(4.6, 25.0)		(-0.1, 10.8)	(2.0, 10.9)		(5)	(6)	
Height-for-Age z-Score	0.2	0.0		0.09	0.01		11	21	
	(0.11, 0.3)	(-0.04, 0.1)		(0.049, 0.14)	(-0.02, 0.06)		(4)	(14)	
Weight-for-Age z-Score	0.1	0.2		0.05	0.09		16	10	
	(0.01, 0.2)	(0.08, 0.4)		(0.01, 0.09)	(0.03, 0.15)		(10)	(6)	
School Enrollment (percentage points)	10.8	21.3		4.7	9.3		16	10	
	(1.2, 21.3)	(7.6, 35.4)		(0.5, 9.3)	(3.3, 15.5)		(10)	(6)	
Psychological Well-being z-Score	0.3	0.8	0.1	0.1	0.3	0.0	26	25	6
	(0.03, 0.6)	(0.4, 1.1)	(-0.6, 0.8)	(0.01, 0.3)	(0.2, 0.5)	(-0.3, 0.3)	(12)	(16)	(5)

95% credibility intervals in parentheses. 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 compised of women (men). Treatment effect per total transfer amount (Panel A) is our preferred outcome variable for lump sum transfers. When there are at least for 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. Treatment effect per monthly tranche amount (Panel B) is our preferred outcome variable for stream transfers (and for this outcome, lump sum transfers are divided by number of months since the lump sum transfer in order to generate an effective monthly transfer amount). To compute Column 2, we use the median lump sum transfer amount for Panel A ($422) and the median stream monthly transfer amount for Panel B ($44). We do not present results on total hours worked or stunting due to data limitations. Our dataset for **Total 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 Appendix Table C.1. for a comparison to analysis that only uses reported estimates on total household or individual income.

Table 7
Heterogenous Treatment Effects by Framing related to Child Development or Food Security

	(1)	(2)	(3)	(4)	(5)	(6)
	Predicted TE of $100 Transfer		*Predicted TE of Median Transfer*		*Estimates (Programs)*	
	No Framing	With Framing	No Framing	With Framing	No Framing	With Framing
Panel A. Treatment Effect per Total Transfer Amount						
Monthly Household Consumption	2.0	3.2	8.4	13.6	64	18
	(1.5, 2.5)	(2.1, 4.4)	(6.2, 10.7)	(9, 18.4)	(34)	(11)
Monthly Household Food Consumption	1.4	2.5	5.9	10.6	33	16
	(0.8, 2)	(1.6, 3.5)	(3.4, 8.6)	(6.7, 14.8)	(22)	(9)
Food Security z-Score	0.03	0.04	0.12	0.17	34	13
	(0.02, 0.04)	(0.03, 0.1)	(0.08, 0.17)	(0.11, 0.24)	(18)	(7)
Total Monthly Income	1.2	2.8	5.0	12.0	76	12
	(0.8, 1.6)	(1.6, 4.1)	(3.2, 6.9)	(6.6, 17.5)	(33)	(5)
Stock of Total Assets	20.2	7.9	85.3	33.5	51	6
	(12.6, 28.2)	(-25.2, 41.6)	(52.9, 119.1)	(-106.2, 175.2)	(25)	(3)
Total Hours Worked per Week	0.1	-0.7	0.5	-3.0	24	1
	(-0.03, 0.3)	(-1.4, 0.01)	(-0.1, 1.1)	(-5.94, 0.03)	(12)	(1)
Labor Force Participation (percentage points)	1.0	0.7	4.3	2.8	9	8
	(0.4, 1.6)	(0.1, 1.3)	(1.67, 7)	(0.2, 5.4)	(6)	(5)
Height-for-Age z-Score	0.01	0.01	0.04	0.03	16	16
	(0.001, 0.02)	(-0.002, 0.02)	(0.004, 0.1)	(-0.01, 0.1)	(8)	(10)
Weight-for-Age z-Score	0.01	0.01	0.02	0.04	8	7
	(-0.003, 0.01)	(-0.003, 0.02)	(-0.01, 0.1)	(-0.01, 0.1)	(4)	(6)
School Enrollment (percentage points)	0.8	1.1	3.4	4.7	12	14
	(0.04, 1.6)	(0.4, 1.9)	(0.2, 6.9)	(1.7, 7.8)	(6)	(10)
Psychological Well-being z-Score	0.03	0.07	0.11	0.31	44	12
	(0.01, 0.04)	(0.04, 0.1)	(0.04, 0.17)	(0.19, 0.45)	(23)	(7)
Panel B. Treatment Effect per Monthly Tranche Amount						
Monthly Household Consumption	28.4	57.3	12.4	25.1	64	18
	(20.8, 36.6)	(39.8, 76)	(9.1, 16)	(17.4, 33.2)	(34)	(11)
Monthly Household Food Consumption	22.0	52.8	9.6	23.1	33	16
	(11.6, 33.7)	(36.2, 70.8)	(5.1, 14.7)	(15.8, 30.9)	(22)	(9)
Food Security z-Score	0.4	1.0	0.2	0.4	34	13
	(0.3, 0.6)	(0.7, 1.3)	(0.1, 0.3)	(0.3, 0.6)	(18)	(7)
Total Monthly Income	15.7	70.3	6.9	30.7	76	12
	(9.6, 22.6)	(47.2, 93.7)	(4.2, 9.9)	(20.6, 40.9)	(33)	(5)
Stock of Total Assets	254.5	128.1	111.2	56.0	51	6
	(151.8, 367.8)	(-317.8, 577.3)	(66.3, 160.7)	(-138.8, 252.2)	(25)	(3)
Total Hours Worked per Week	0.6	-3.4	0.2	-1.5	24	1
	(0.01, 1.1)	(-6.5, -0.2)	(0.003, 0.5)	(-2.8, -0.1)	(12)	(1)
Labor Force Participation (percentage points)	12.2	15.5	5.3	6.8	9	8
	(1.9, 22.7)	(3.8, 27.4)	(0.8, 9.9)	(1.7, 12)	(6)	(5)
Height-for-Age z-Score	0.1	0.1	0.1	0.1	16	16
	(0.02, 0.2)	(-0.01, 0.3)	(0.01, 0.1)	(-0.005, 0.1)	(8)	(10)
Weight-for-Age z-Score	0.1	0.1	0.0	0.1	8	7
	(-0.03, 0.1)	(-0.04, 0.3)	(-0.01, 0.1)	(-0.02, 0.1)	(4)	(6)
School Enrollment (percentage points)	13.9	15.2	6.1	6.6	12	14
	(1.6, 27.3)	(3.9, 26.9)	(0.7, 11.9)	(1.7, 11.8)	(6)	(10)
Psychological Well-being z-Score	0.3	1.3	0.1	0.6	44	12
	(0.1, 0.5)	(0.8, 1.8)	(0.02, 0.2)	(0.4, 0.8)	(23)	(7)

95% credibility intervals in parentheses. All currency values are reported in 2010 USD PPP. Treatment effect per total transfer amount (Panel A) is our preferred outcome variable for lump sum transfers. Treatment effect per monthly tranche amount (Panel B) is our preferred outcome variable for stream transfers (and for this outcome, lump sum transfers are divided by number of months since the lump sum transfer in order to generate an effective monthly transfer amount). To compute Column 2, we use the median lump sum transfer amount for Panel A ($422) and the median stream monthly transfer amount for Panel B ($44). Our dataset for **Total 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 Appendix Table C.1. 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.

Table 8
Benefit-Cost Ratios of UCT Programs

	(1)	(2)	(3)	(4)
			Benefit-Cost Ratio (BCR)	
	Total Benefit	Total Transfer Amount	No Admin. Costs	Median Admin. Costs (24%)
Panel A. Dynamic Effects Binary Model				
Lump Sum Program	2.9	1.0	2.92	2.36
12-Month Stream Program	62.5	11.7	5.32	4.30
24-Month Stream Program	65.6	22.9	2.86	2.31
36-Month Stream Program	70.4	33.6	2.10	1.70
48-Month Stream Program	75.1	43.7	1.72	1.39
Panel B. Dynamic Effects Polynomial Model - Moderate Assumptions				
Lump Sum Program	1.6	1.0	1.57	1.27
12-Month Stream Program	47.7	11.7	4.07	3.29
24-Month Stream Program	50.6	22.9	2.21	1.78
36-Month Stream Program	57.7	33.6	1.72	1.39
48-Month Stream Program	69.9	43.7	1.60	1.28
Panel C. Dynamic Effects Polynomial Model - Pessimistic Assumptions				
Lump Sum Program	0.9	1.0	0.86	0.70
12-Month Stream Program	11.9	11.7	1.01	0.82
24-Month Stream Program	16.4	22.9	0.72	0.58
36-Month Stream Program	25.2	33.6	0.75	0.61
48-Month Stream Program	38.9	43.7	0.89	0.72

Costs and benefits are presented as a proportion of the transfer amount (monthly tranche for stream and total amount for lump sum). Total cost and benefit are discounted to the month of program onset using a 5% discount rate. We use our estimated treatment effects on monthly household consumption from Table 6 to calculate the total benefit. In Panel A, we use our estimates from Panel A of Table 6, assuming that short-term effects are constant until month 18 and long-term effects are constant after month 18. In Panels B and C, we use our estimates from Panel B of Table 6. In Panel B, we use the moderate assumption that dynamic effects persist as predicted by our model until the month where the dynamic accumulation (dissipation) effect reaches its maximum (minimum). We then assume constant effects from that month on. In Panel C, we use the pessimistic assumption that the dynamic effects persist as predicted by our model until the month when overall treatment effect equals zero. We then assume effects remain at zero from that month on. 24% is the median administrative costs as a proportion of the transfer of the 10 of 73 programs that report costs. 24% is also the average administrative cost for all programs with a minumum of 6% and maximum of 60%.

Figure 1: PRISMA Diagram

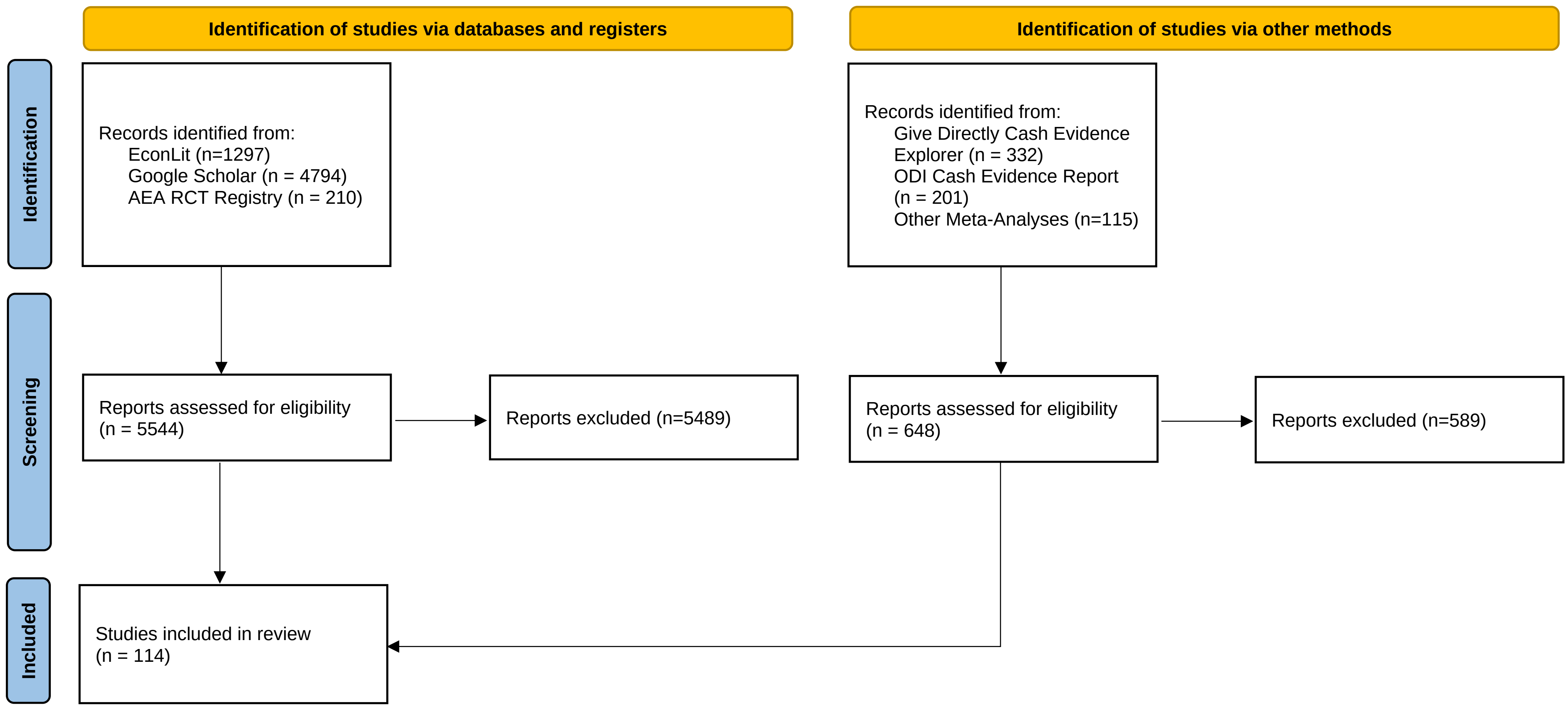


Figure 2: Histograms of Months Since First UCT by Outcome for Lump Sums and Streams

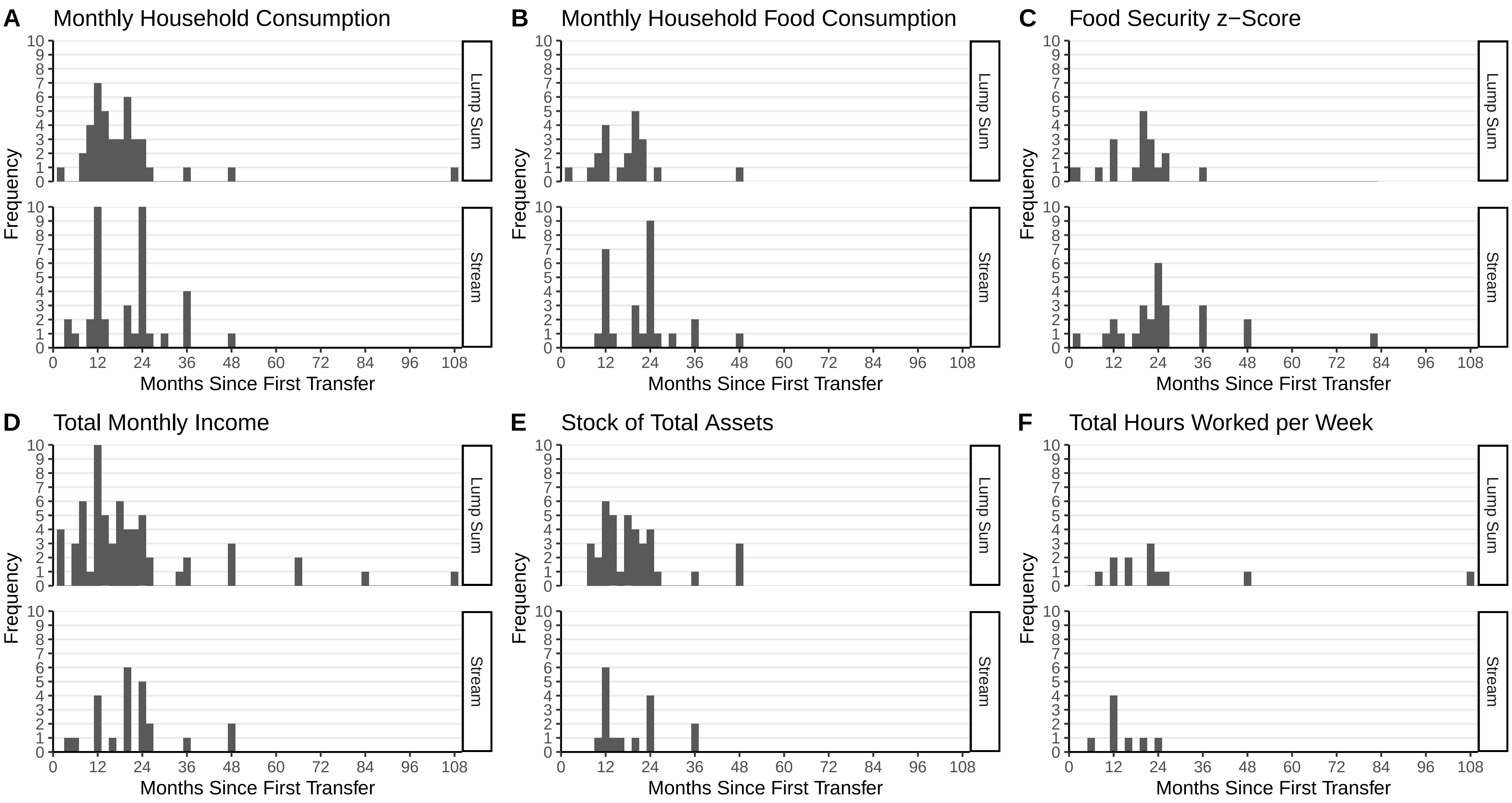


Figure 2: Histograms of Months Since First UCT by Outcome for Lump Sums and Streams

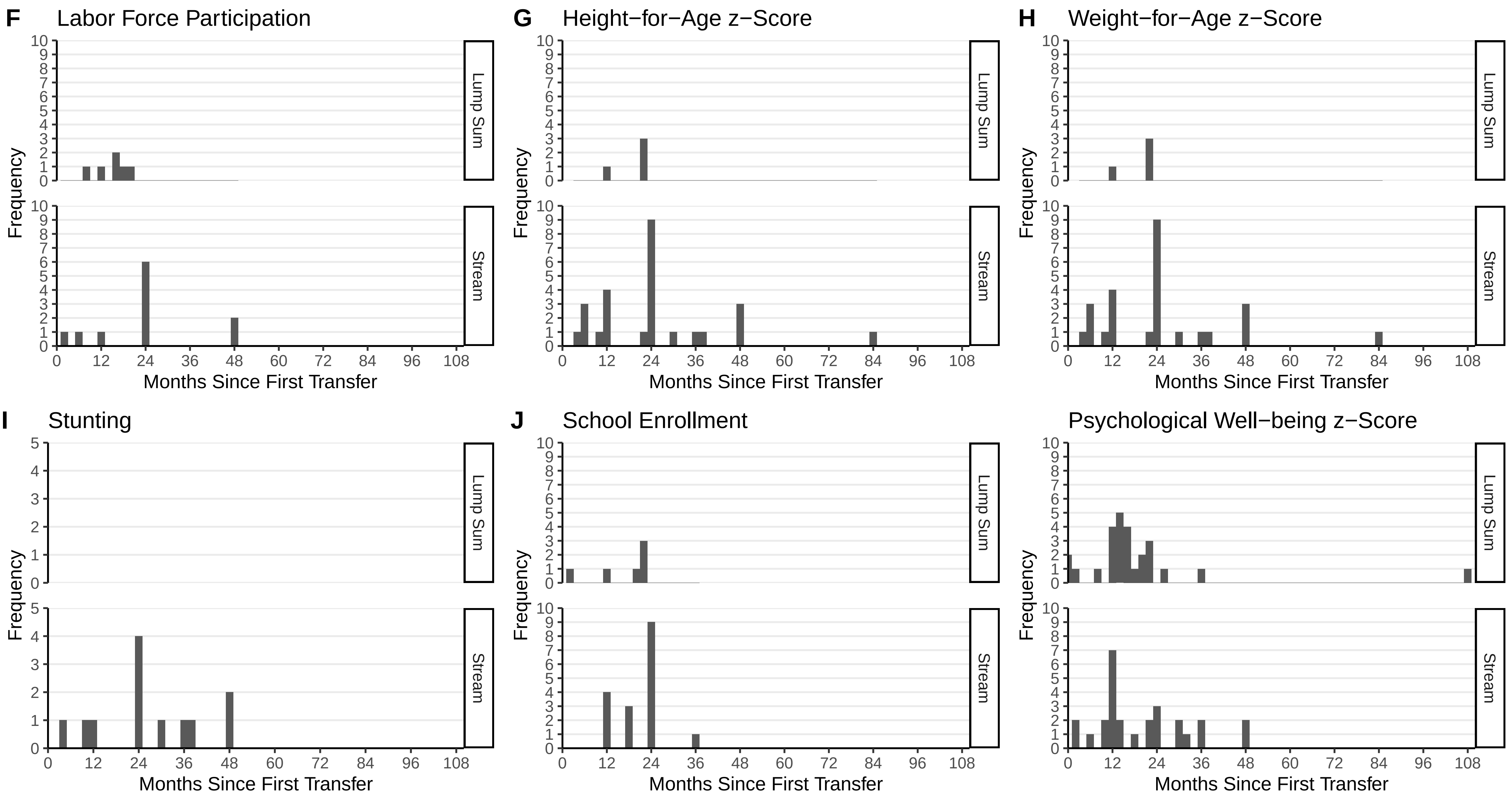


Figure 3: Histograms of Treatment Effects per Total Transfer Amount for Lump Sums and Streams

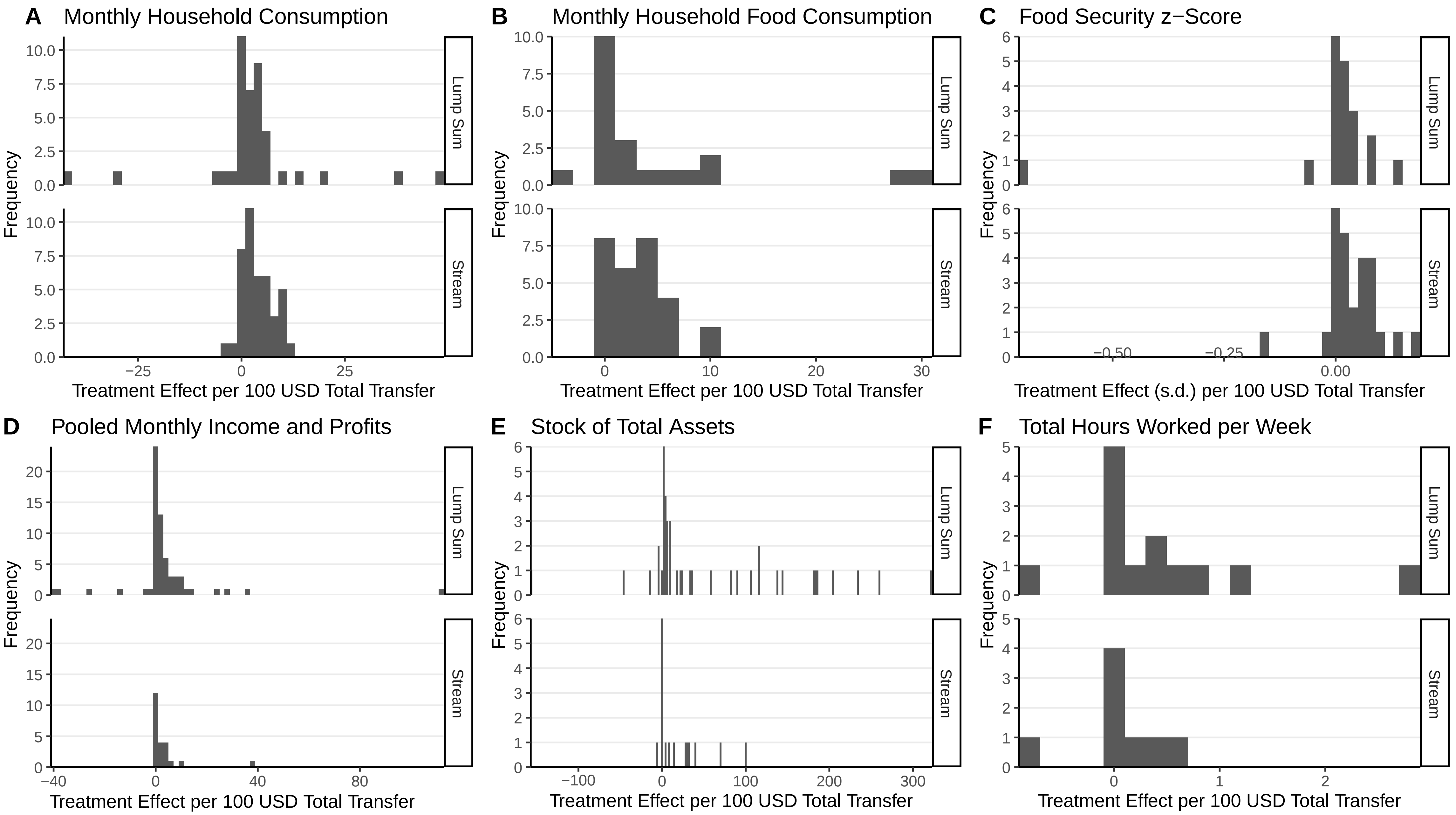


Figure 3 cont.: Histograms of Treatment Effects per Total Transfer Amount for Lump Sums and Streams

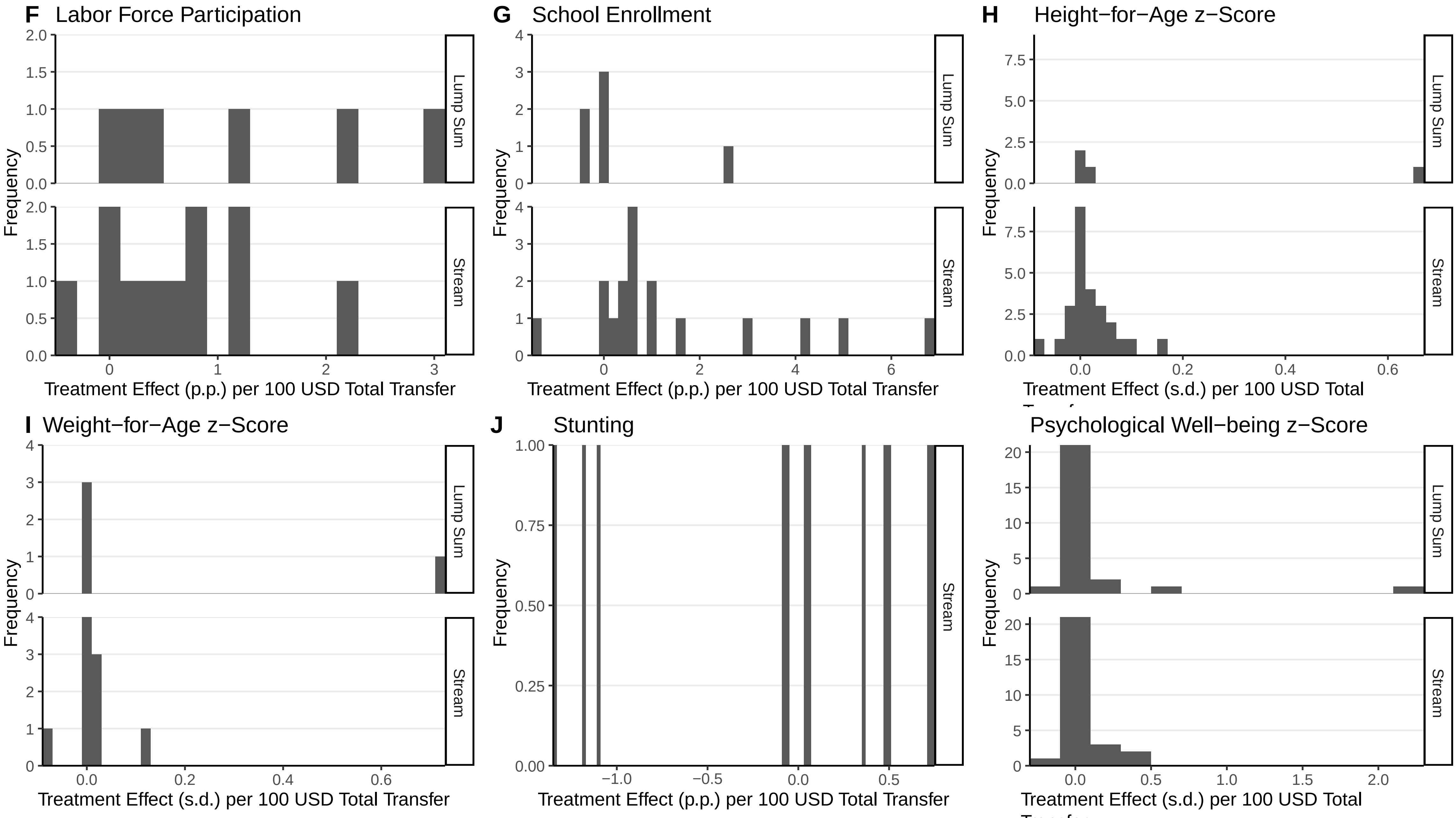


Figure 4: Histograms of Treatment Effects per Monthly Tranche Amount for Lump Sums and Streams

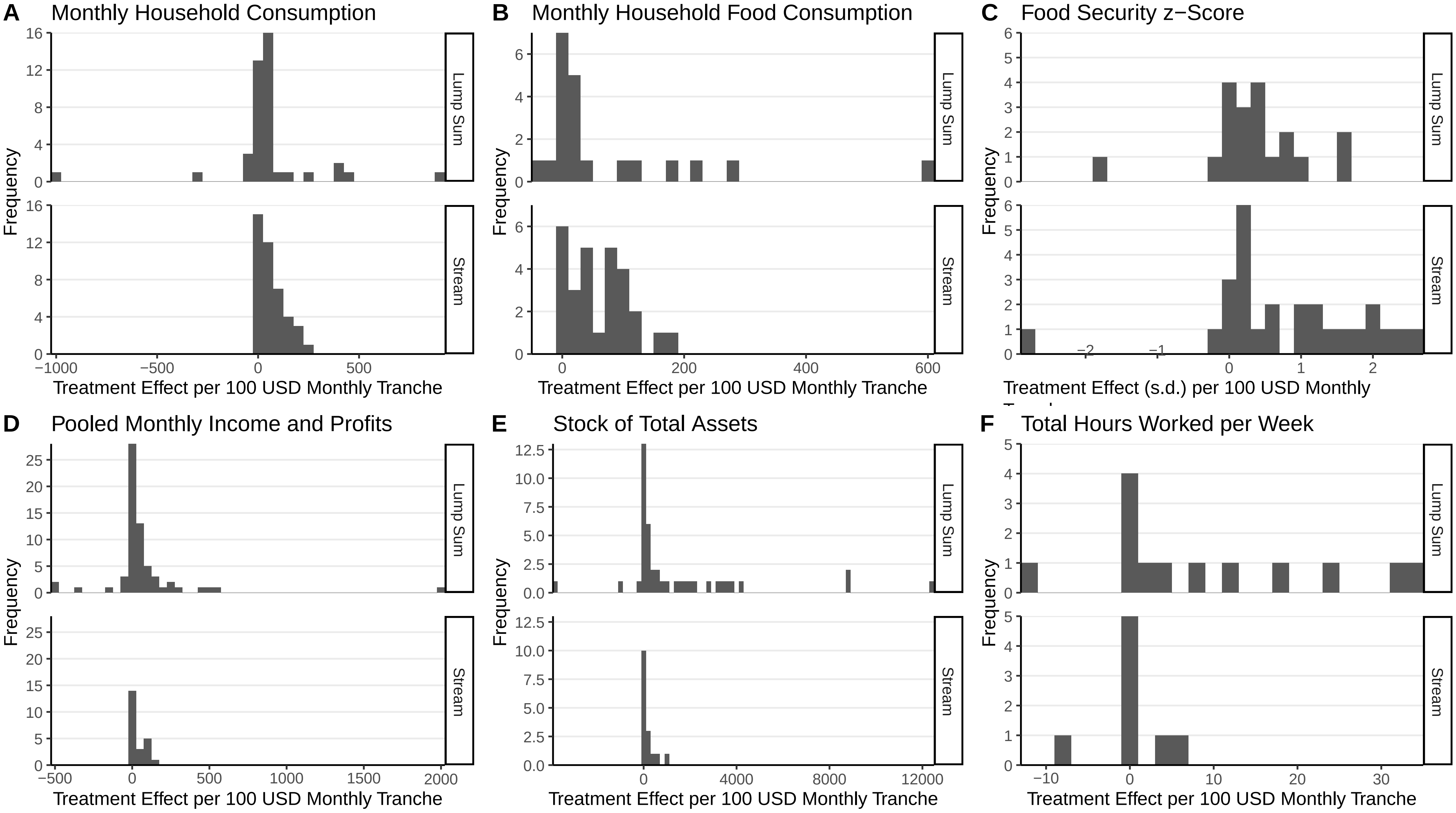


Figure 4 cont.: Histograms of Treatment Effects per Monthly Tranche Amount for Lump Sums and Streams

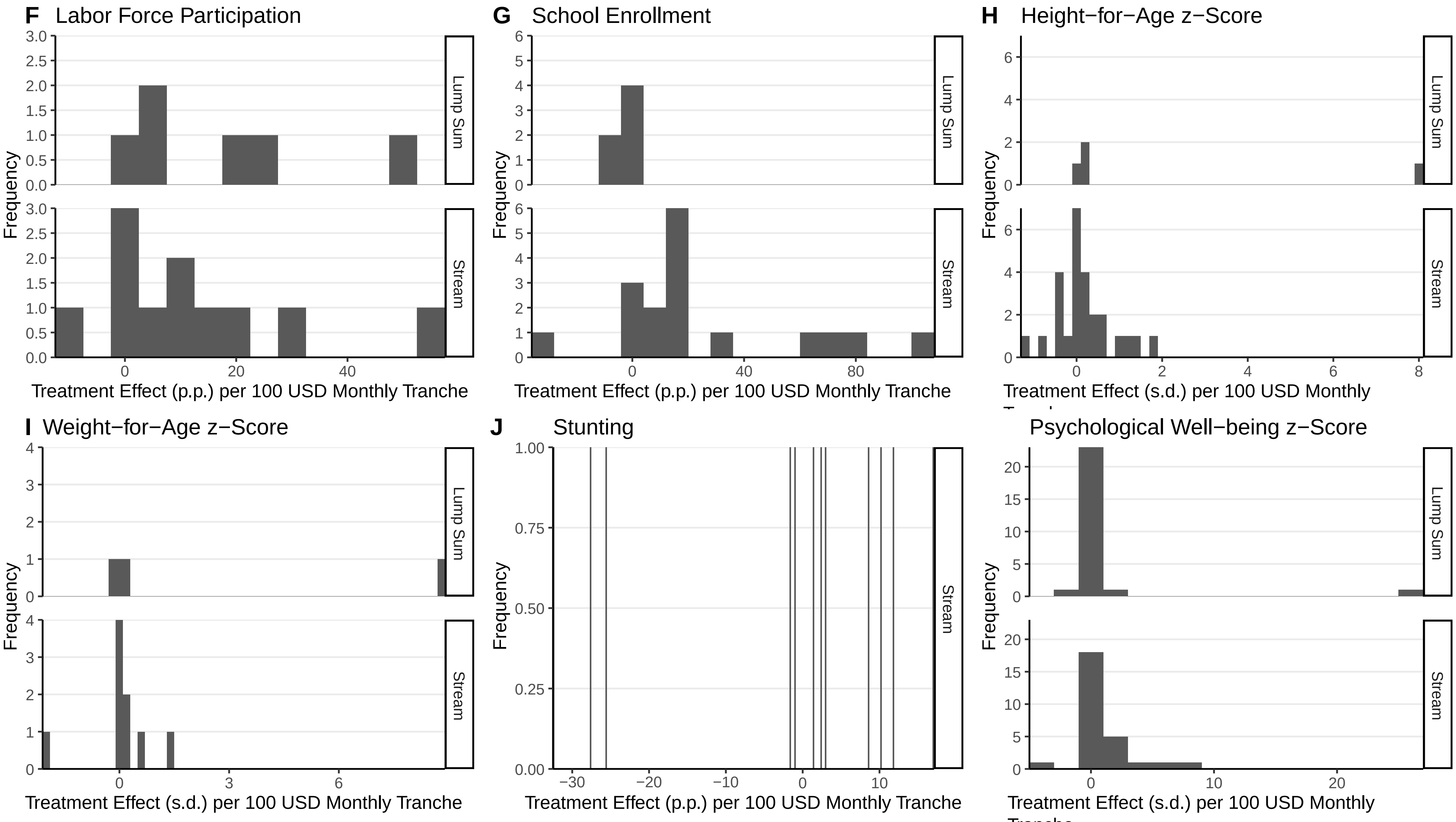
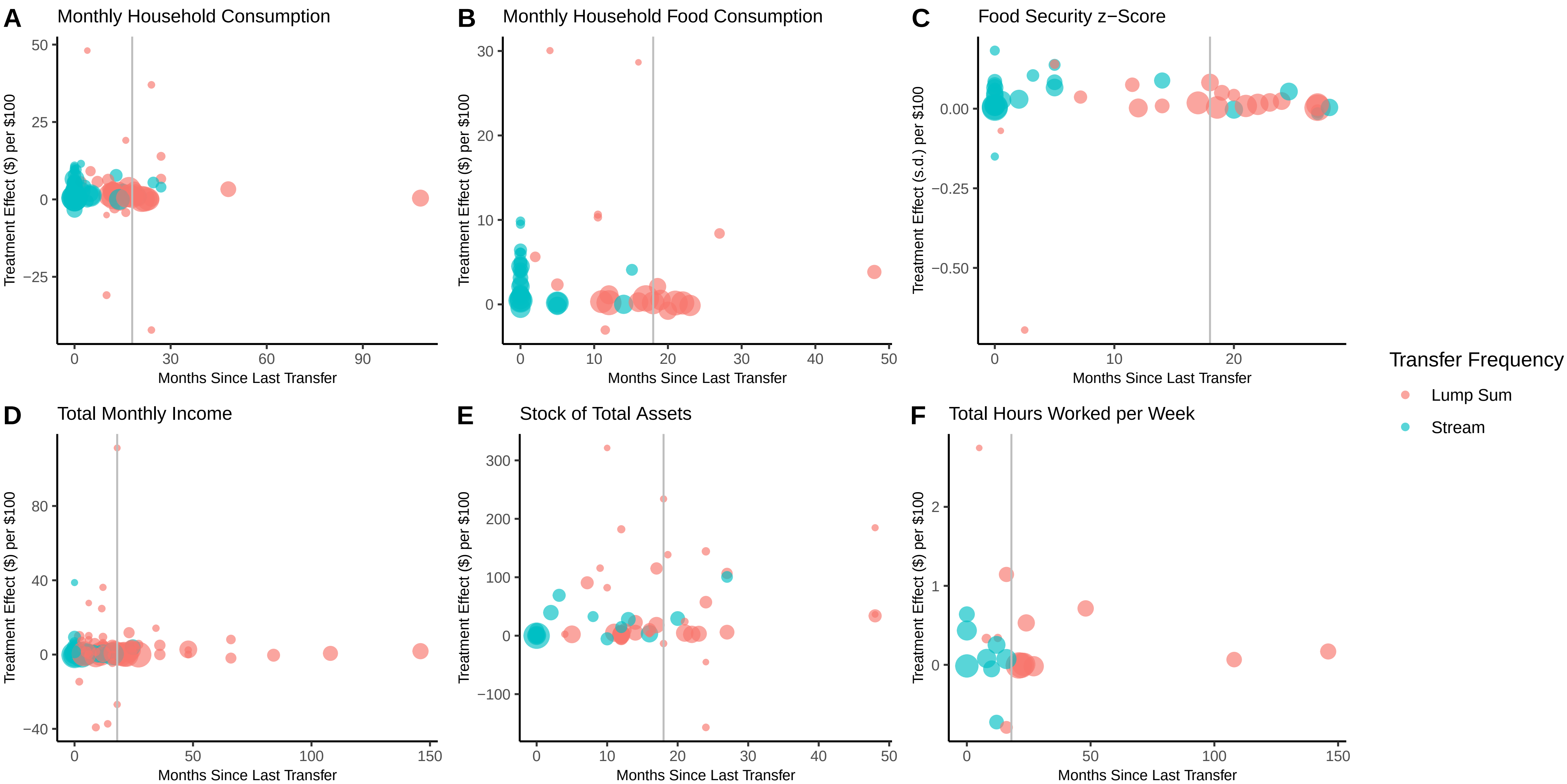


Figure 5: Treatment Effect per Total Transfer Amount vs. Months Since First Transfer



Larger circle size indicates greater meta−analytic weight.

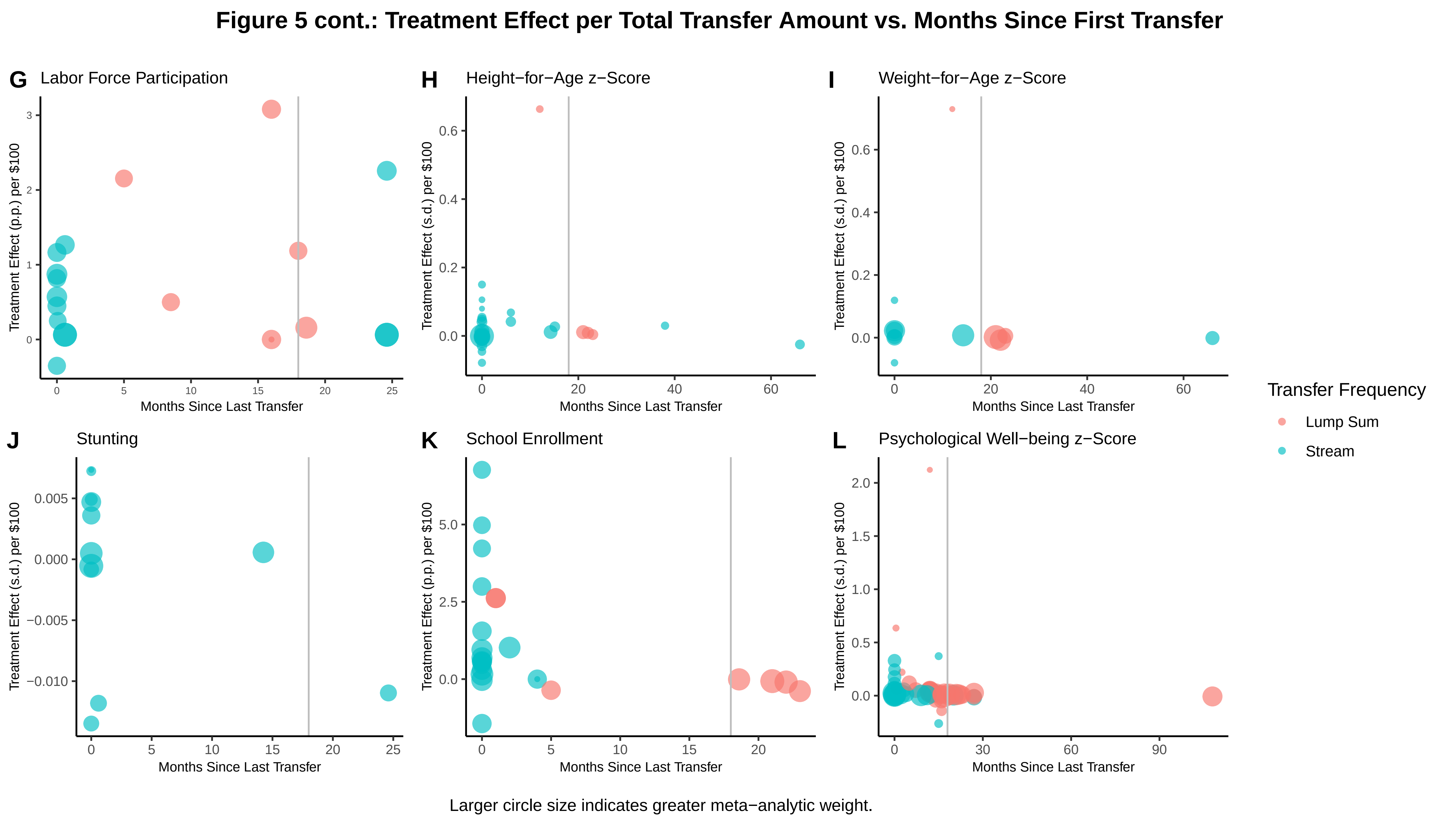
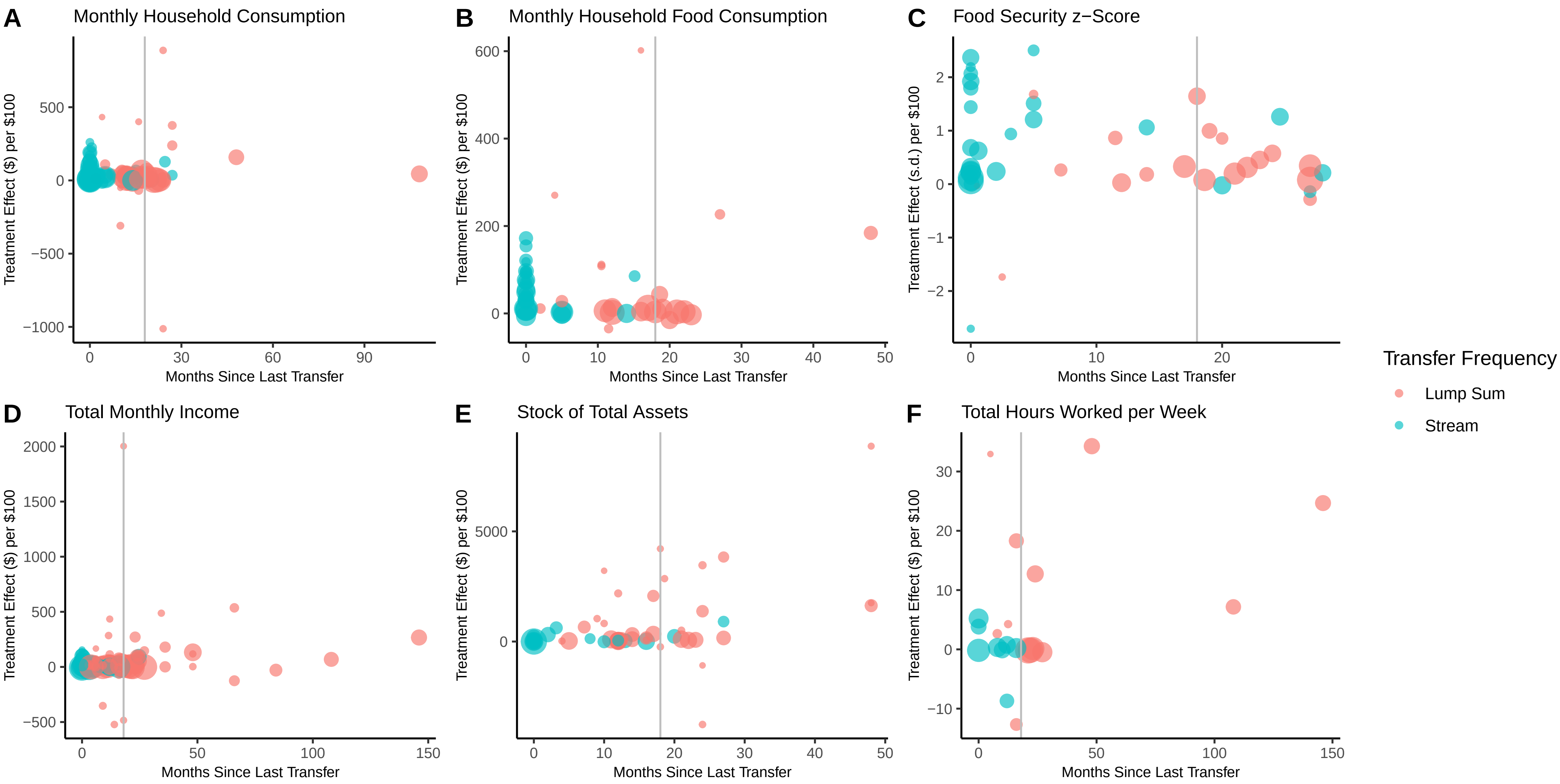


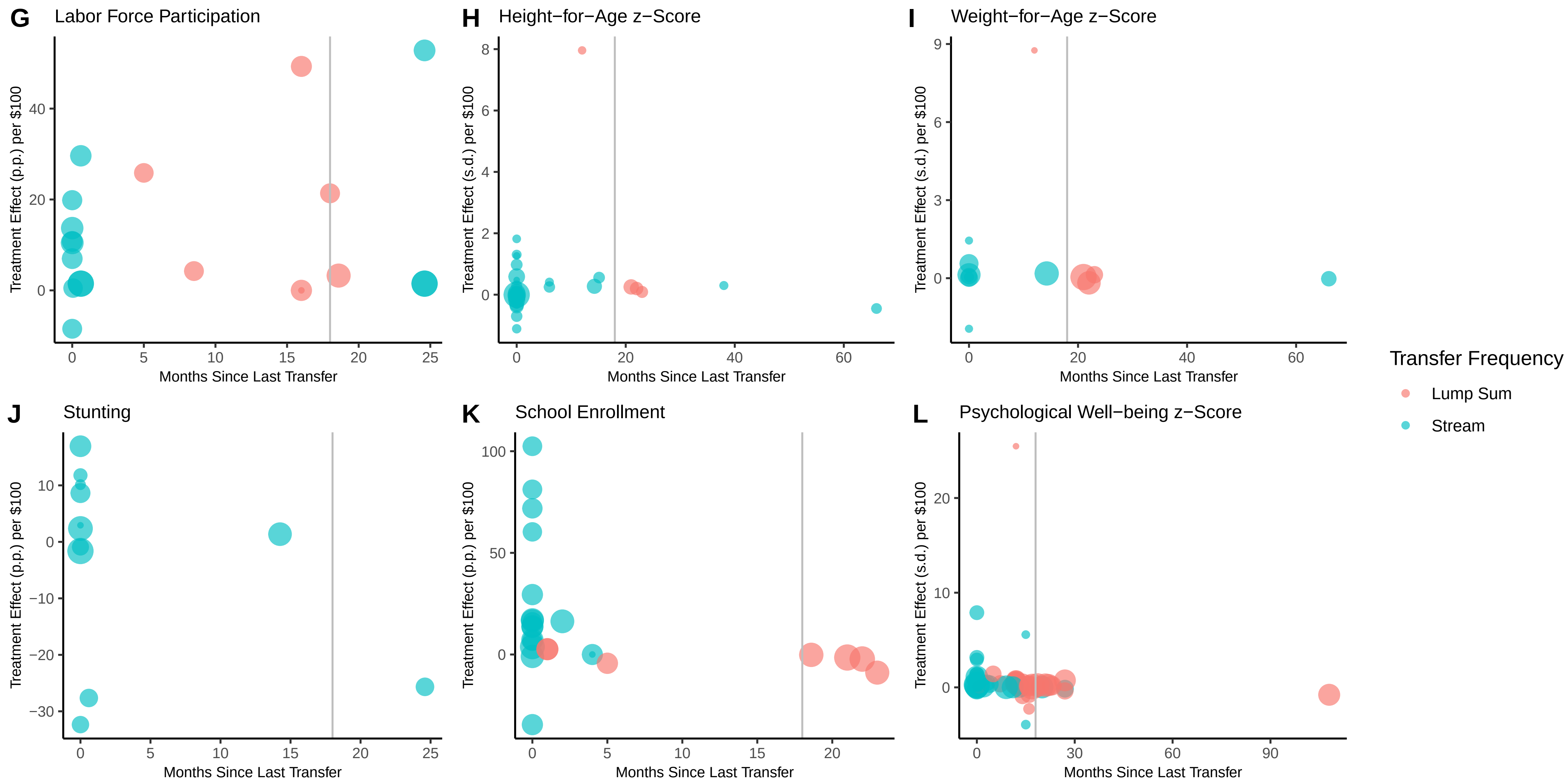
Figure 5 cont.: Treatment Effect per Total Transfer Amount vs. Months Since First Transfer
G Labor Force Participation
Treatment Effect (p.p.) per $100
0
1
2
3
0 5 10 15 20 25
Months Since Last Transfer
H Height−for−Age z−Score
Treatment Effect (s.d.) per $100
0.0
0.2
0.4
0.6
0 20 40 60
Months Since Last Transfer
I Weight−for−Age z−Score
Treatment Effect (s.d.) per $100
0.0
0.2
0.4
0.6
0 20 40 60
Months Since Last Transfer
Transfer Frequency
Lump Sum
Stream
J Stunting
Treatment Effect (s.d.) per $100
0.005
0.000
−0.005
−0.010
0 5 10 15 20 25
Months Since Last Transfer
K School Enrollment
Treatment Effect (p.p.) per $100
0.0
2.5
5.0
0 5 10 15 20
Months Since Last Transfer
L Psychological Well−being z−Score
Treatment Effect (s.d.) per $100
0.0
0.5
1.0
1.5
2.0
0 30 60 90
Months Since Last Transfer
Larger circle size indicates greater meta−analytic weight.

Figure 6: Treatment Effect per Monthly Tranche Amount vs. Months Since First Transfer



Larger circle size indicates greater meta−analytic weight.

Figure 6 cont.: Treatment Effect per Monthly Tranche Amount vs. Months Since First Transfer



Larger circle size indicates greater meta−analytic weight.

Figure 7.1a: Posterior Average Treatment Effects on Total Consumption sorted by Effect Size - Ongoing Streams

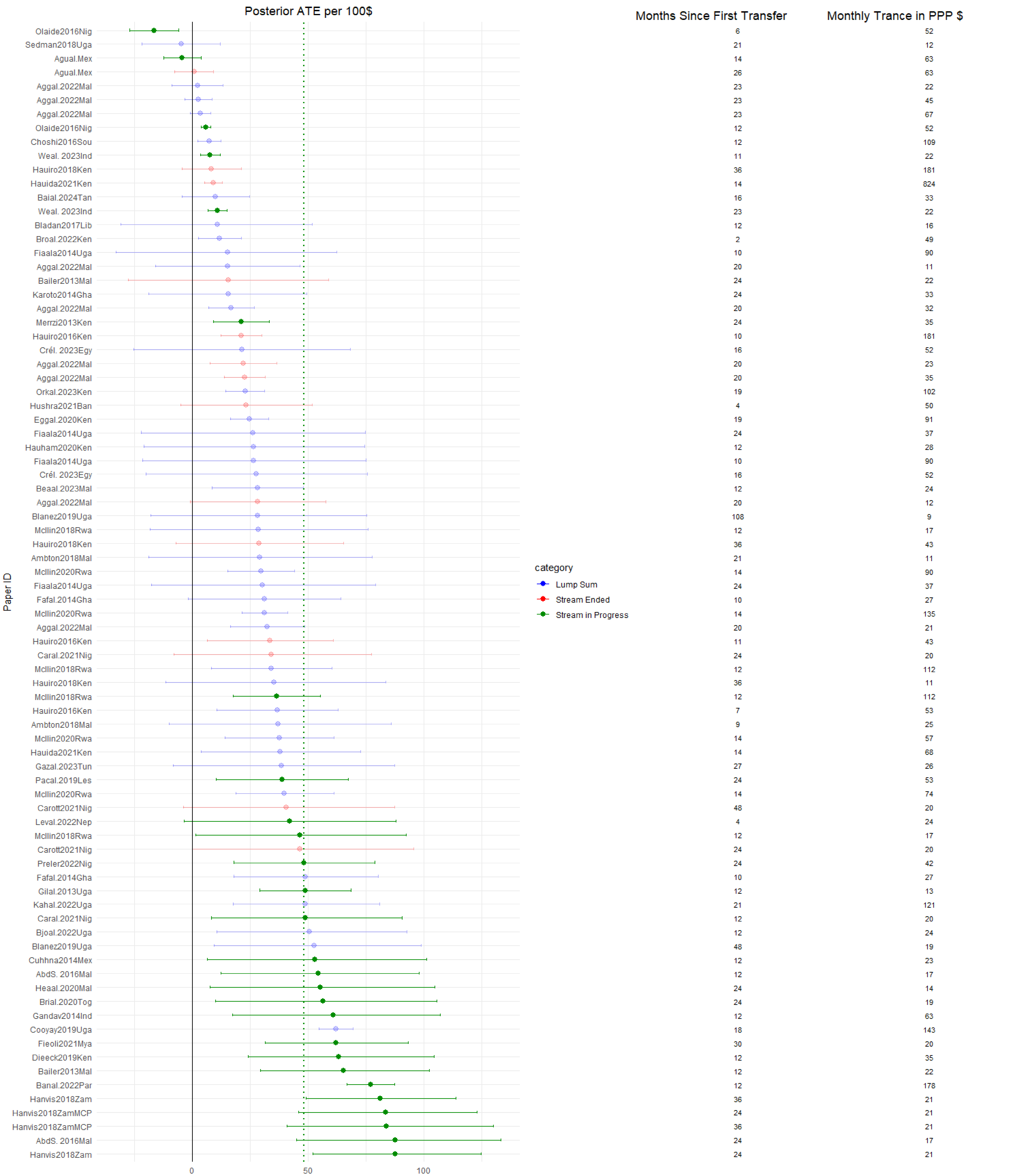
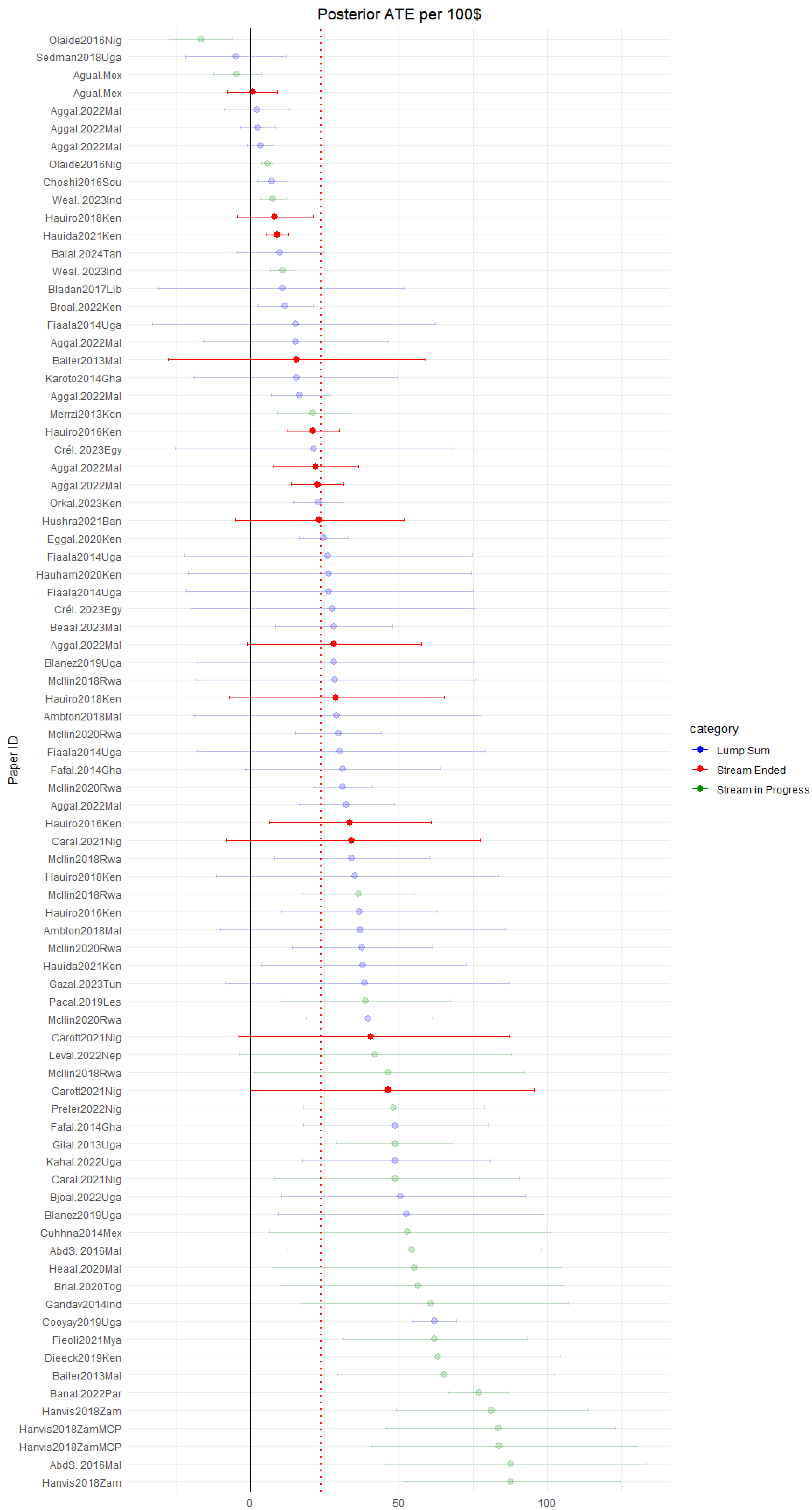


Figure 7.1b: Posterior Average Treatment Effects on Total Consumption sorted by Effect Size - Ongoing Streams



Paper ID	Months Since First Transfer	Monthly Trance in PPP $
Olaide2016Nig	6	52
Sedman2018Uga	21	12
Agual.Mex	14	63
Agual.Mex	26	63
Aggal.2022Mal	23	22
Aggal.2022Mal	23	45
Aggal.2022Mal	23	67
Olaide2016Nig	12	52
Choshi2016Sou	12	109
Weal. 2023Ind	11	22
Hauiro2018Ken	36	181
Hauida2021Ken	14	824
Baial.2024Tan	16	33
Weal. 2023Ind	23	22
Bladan2017Lib	12	16
Broal.2022Ken	2	49
Fiaala2014Uga	10	90
Aggal.2022Mal	20	11
Bailer2013Mal	24	22
Karoto2014Gha	24	33
Aggal.2022Mal	20	32
Merzri2013Ken	24	35
Hauiro2016Ken	10	181
Crél. 2023Egy	16	52
Aggal.2022Mal	20	23
Aggal.2022Mal	20	35
Orkal.2023Ken	19	102
Hushra2021Ban	4	50
Eggal.2020Ken	19	91
Fiaala2014Uga	24	37
Hauham2020Ken	12	28
Fiaala2014Uga	10	90
Crél. 2023Egy	16	52
Beaal.2023Mal	12	24
Aggal.2022Mal	20	12
Blanez2019Uga	108	9
Mcllin2018Rwa	12	17
Hauiro2018Ken	36	43
Ambton2018Mal	21	11
Mcllin2020Rwa	14	90
Fiaala2014Uga	24	37
Fafal.2014Gha	10	27
Mcllin2020Rwa	14	135
Aggal.2022Mal	20	21
Hauiro2016Ken	11	43
Caral.2021Nig	24	20
Mcllin2018Rwa	12	112
Hauiro2018Ken	36	11
Mcllin2018Rwa	12	112
Hauiro2016Ken	7	53
Ambton2018Mal	9	25
Mcllin2020Rwa	14	57
Hauida2021Ken	14	68
Gazal.2023Tun	27	26
Pacal.2019Les	24	53
Mcllin2020Rwa	14	74
Carott2021Nig	48	20
Leval.2022Nep	4	24
Mcllin2018Rwa	12	17
Carott2021Nig	24	20
Preler2022Nig	24	42
Fafal.2014Gha	10	27
Gilal.2013Uga	12	13
Kahal.2022Uga	21	121
Caral.2021Nig	12	20
Bjoal.2022Uga	12	24
Blanez2019Uga	48	19
Cuhhna2014Mex	12	23
AbdS. 2016Mal	12	17
Heaal.2020Mal	24	14
Brial.2020Tog	24	19
Gandav2014Ind	12	63
Cooyay2019Uga	18	143
Fieoli2021Mya	30	20
Dieeck2019Ken	12	35
Bailer2013Mal	12	22
Banal.2022Par	12	178
Hanvis2018Zam	36	21
Hanvis2018ZamMCP	24	21
Hanvis2018ZamMCP	36	21
AbdS. 2016Mal	24	17
Hanvis2018Zam	24	21

Figure 7.1c: Posterior Average Treatment Effects on Total Consumption sorted by Effect Size - Lump Sums

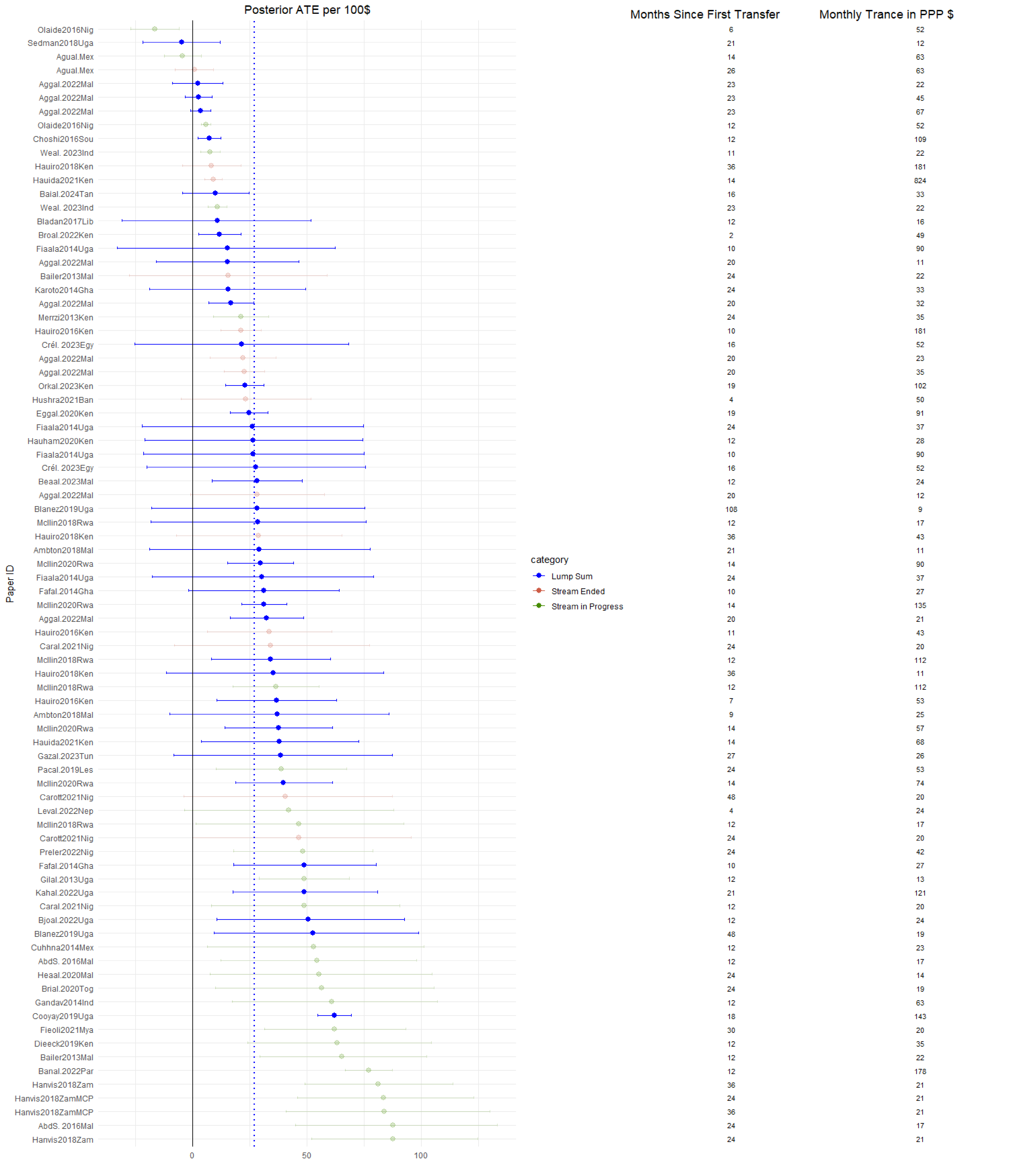
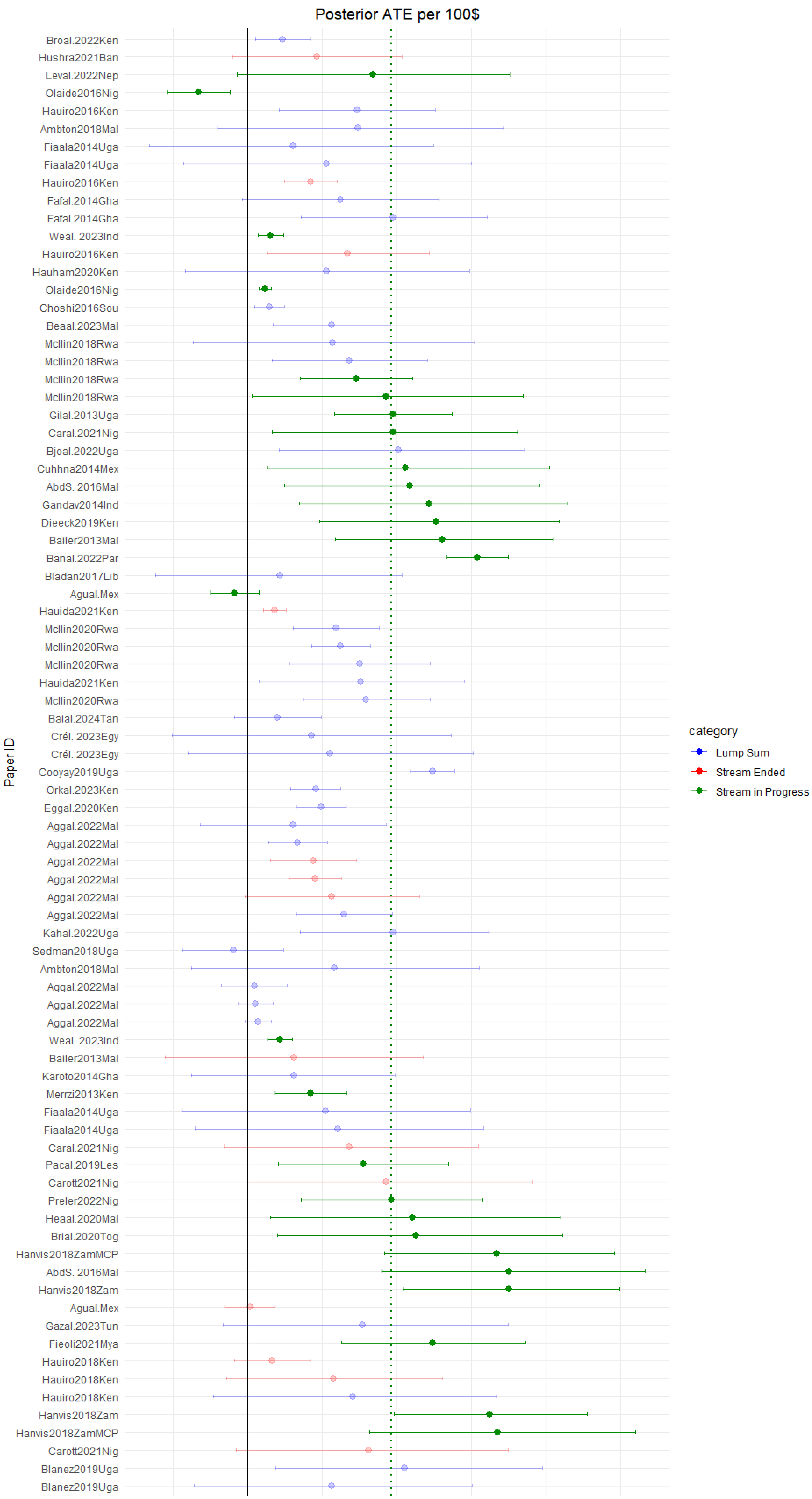


Figure 7.2a: Posterior Average Treatment Effects on Total Consumption sorted by Months Since First Transfer - Ongoing Streams



Paper ID	Months Since First Transfer	Monthly Trance in PPP $
Broal.2022Ken	2	49
Hushra2021Ban	4	50
Leval.2022Nep	4	24
Olaide2016Nig	6	52
Hauiro2016Ken	7	53
Ambton2018Mal	9	25
Fiaala2014Uga	10	90
Fiaala2014Uga	10	90
Hauiro2016Ken	10	181
Fafal.2014Gha	10	27
Fafal.2014Gha	10	27
Weal. 2023Ind	11	22
Hauiro2016Ken	11	43
Hauham2020Ken	12	28
Olaide2016Nig	12	52
Choshi2016Sou	12	109
Beaal.2023Mal	12	24
Mcllin2018Rwa	12	17
Mcllin2018Rwa	12	112
Mcllin2018Rwa	12	112
Mcllin2018Rwa	12	17
Gilal.2013Uga	12	13
Caral.2021Nig	12	20
Bjoal.2022Uga	12	24
Cuhhna2014Mex	12	23
AbdS. 2016Mal	12	17
Gandav2014Ind	12	63
Dieeck2019Ken	12	35
Bailer2013Mal	12	22
Banal.2022Par	12	178
Bladan2017Lib	12	16
Agual.Mex	14	63
Hauida2021Ken	14	824
Mcllin2020Rwa	14	90
Mcllin2020Rwa	14	135
Mcllin2020Rwa	14	57
Hauida2021Ken	14	68
Mcllin2020Rwa	14	74
Baial.2024Tan	16	33
Crél. 2023Egy	16	52
Crél. 2023Egy	16	52
Cooyay2019Uga	18	143
Orkal.2023Ken	19	102
Eggal.2020Ken	19	91
Aggal.2022Mal	20	11
Aggal.2022Mal	20	32
Aggal.2022Mal	20	23
Aggal.2022Mal	20	35
Aggal.2022Mal	20	12
Aggal.2022Mal	20	21
Kahal.2022Uga	21	121
Sedman2018Uga	21	12
Ambton2018Mal	21	11
Aggal.2022Mal	23	22
Aggal.2022Mal	23	45
Aggal.2022Mal	23	67
Weal. 2023Ind	23	22
Bailer2013Mal	24	22
Karoto2014Gha	24	33
Merrzi2013Ken	24	35
Fiaala2014Uga	24	37
Fiaala2014Uga	24	37
Caral.2021Nig	24	20
Pacal.2019Les	24	53
Carott2021Nig	24	20
Preler2022Nig	24	42
Heaal.2020Mal	24	14
Brial.2020Tog	24	19
Hanvis2018ZamMCP	24	21
AbdS. 2016Mal	24	17
Hanvis2018Zam	24	21
Agual.Mex	26	63
Gazal.2023Tun	27	26
Fieoli2021Mya	30	20
Hauiro2018Ken	36	181
Hauiro2018Ken	36	43
Hauiro2018Ken	36	11
Hanvis2018Zam	36	21
Hanvis2018ZamMCP	36	21
Carott2021Nig	48	20
Blanez2019Uga	48	19
Blanez2019Uga	108	9

Figure 7.2b: Posterior Average Treatment Effects on Total Consumption sorted by Months Since First Transfer- Completed Streams

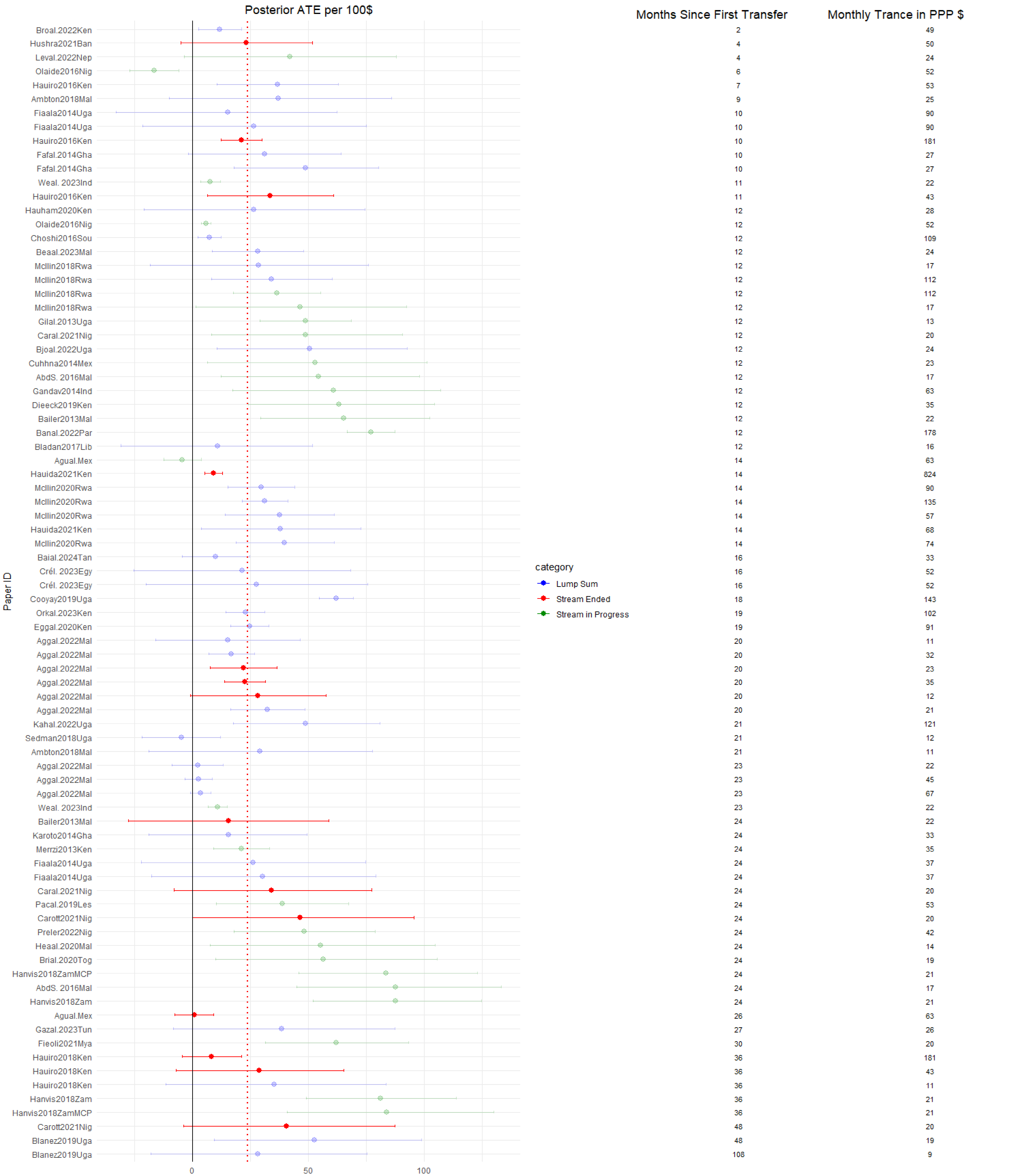
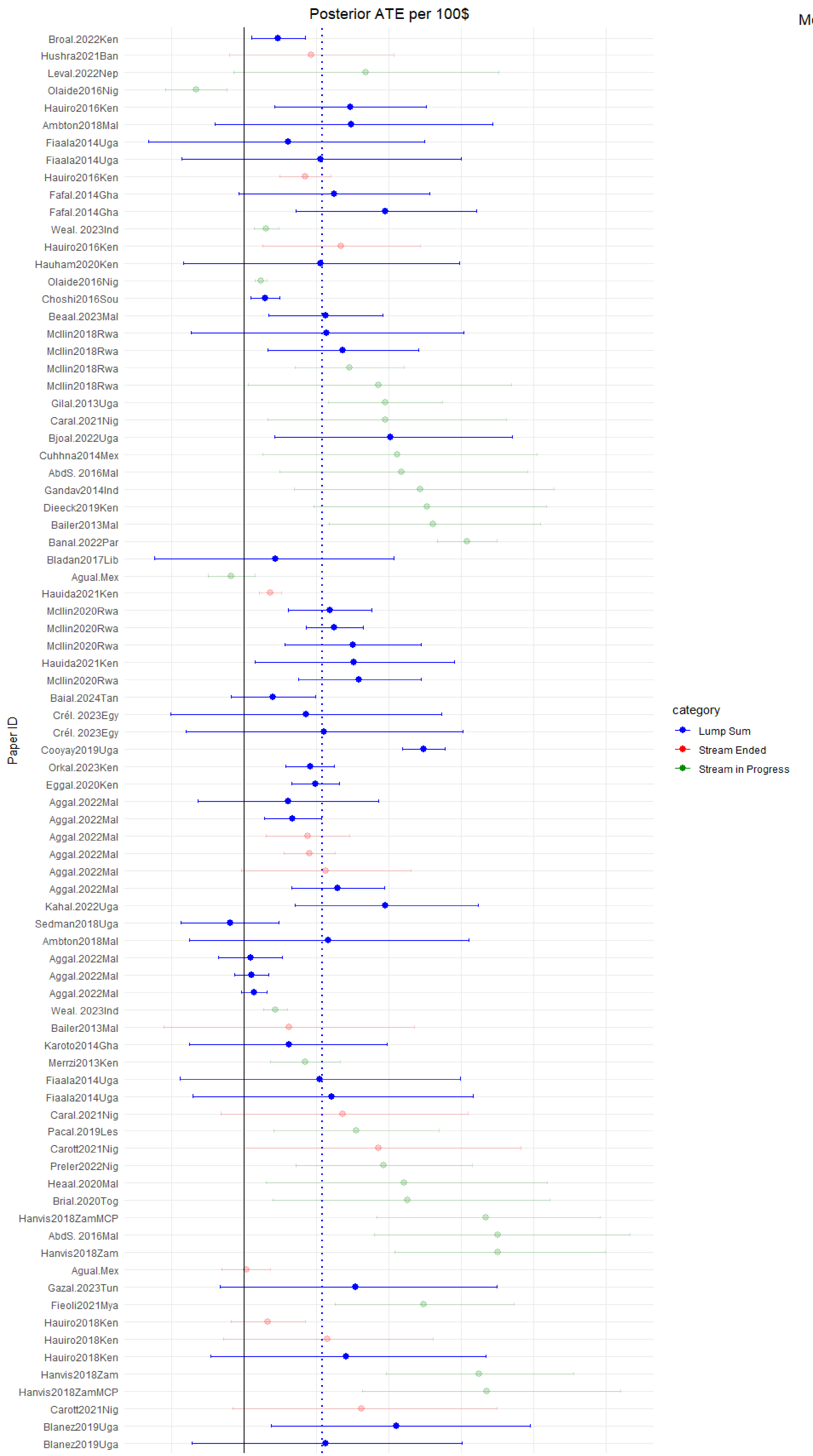
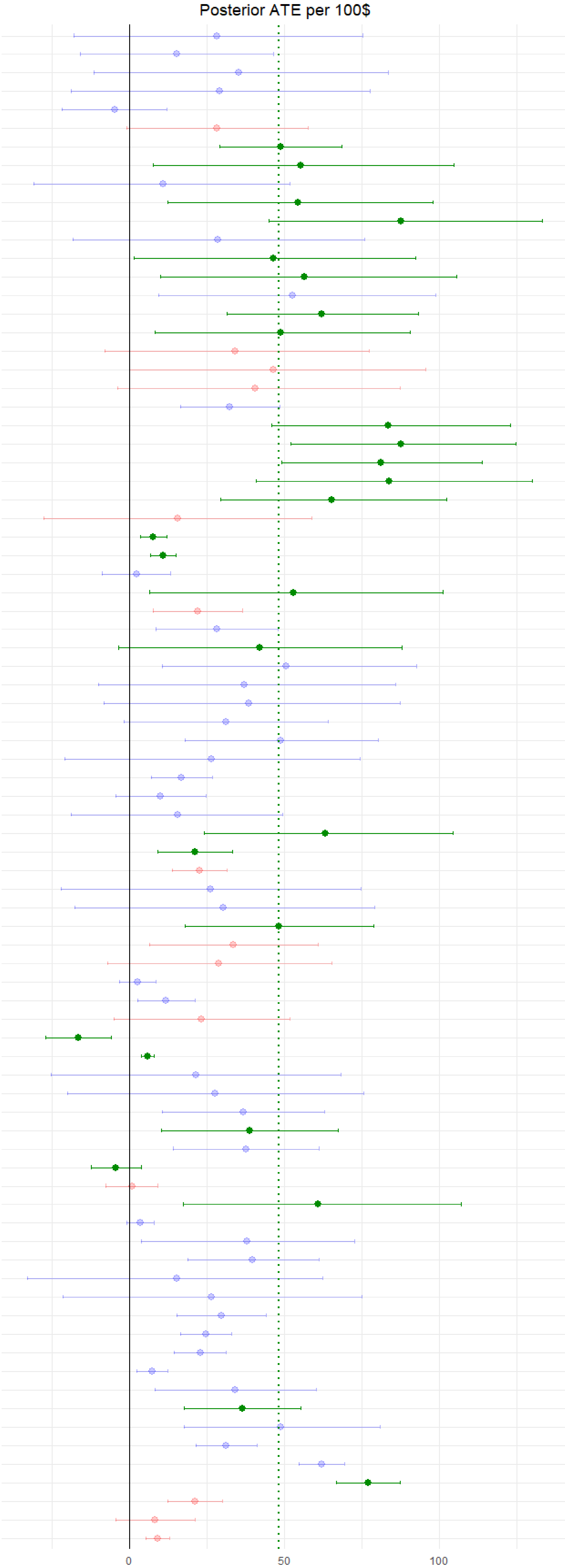


Figure 7.2c: Posterior Average Treatment Effects on Total Consumption sorted by Months Since First Transfer- Lump Sums



Paper ID	Months Since First Transfer	Monthly Trance in PPP $
Broal.2022Ken	2	49
Hushra2021Ban	4	50
Leval.2022Nep	4	24
Olaide2016Nig	6	52
Hauiro2016Ken	7	53
Ambton2018Mal	9	25
Fiaala2014Uga	10	90
Fiaala2014Uga	10	90
Hauiro2016Ken	10	181
Fafal.2014Gha	10	27
Fafal.2014Gha	10	27
Weal. 2023Ind	11	22
Hauiro2016Ken	11	43
Hauham2020Ken	12	28
Olaide2016Nig	12	52
Choshi2016Sou	12	109
Beaal.2023Mal	12	24
Mcllin2018Rwa	12	17
Mcllin2018Rwa	12	112
Mcllin2018Rwa	12	112
Mcllin2018Rwa	12	17
Gilal.2013Uga	12	13
Caral.2021Nig	12	20
Bjoal.2022Uga	12	24
Cuhhna2014Mex	12	23
AbdS. 2016Mal	12	17
Gandav2014Ind	12	63
Dieeck2019Ken	12	35
Bailer2013Mal	12	22
Banal.2022Par	12	178
Bladan2017Lib	12	16
Agual.Mex	14	63
Hauida2021Ken	14	824
Mcllin2020Rwa	14	90
Mcllin2020Rwa	14	135
Mcllin2020Rwa	14	57
Hauida2021Ken	14	68
Mcllin2020Rwa	14	74
Baial.2024Tan	16	33
Crél. 2023Egy	16	52
Crél. 2023Egy	16	52
Cooyay2019Uga	18	143
Orkal.2023Ken	19	102
Eggal.2020Ken	19	91
Aggal.2022Mal	20	11
Aggal.2022Mal	20	32
Aggal.2022Mal	20	23
Aggal.2022Mal	20	35
Aggal.2022Mal	20	12
Aggal.2022Mal	20	21
Kahal.2022Uga	21	121
Sedman2018Uga	21	12
Ambton2018Mal	21	11
Aggal.2022Mal	23	22
Aggal.2022Mal	23	45
Aggal.2022Mal	23	67
Weal. 2023Ind	23	22
Bailer2013Mal	24	22
Karoto2014Gha	24	33
Merrzi2013Ken	24	35
Fiaala2014Uga	24	37
Fiaala2014Uga	24	37
Caral.2021Nig	24	20
Pacal.2019Les	24	53
Carott2021Nig	24	20
Preler2022Nig	24	42
Heaal.2020Mal	24	14
Brial.2020Tog	24	19
Hanvis2018ZamMCP	24	21
AbdS. 2016Mal	24	17
Hanvis2018Zam	24	21
Agual.Mex	26	63
Gazal.2023Tun	27	26
Fieoli2021Mya	30	20
Hauiro2018Ken	36	181
Hauiro2018Ken	36	43
Hauiro2018Ken	36	11
Hanvis2018Zam	36	21
Hanvis2018ZamMCP	36	21
Carott2021Nig	48	20
Blanez2019Uga	48	19
Blanez2019Uga	108	9

Figure 7.3a: Posterior Average Treatment Effects on Total Consumption sorted by Monthly Tranche Amount - Ongoing Streams



Paper ID	Months Since First Transfer	Monthly Trance in PPP $
Blanez2019Uga	108	9
Aggal.2022Mal	20	11
Hauiro2018Ken	36	11
Ambton2018Mal	21	11
Sedman2018Uga	21	12
Aggal.2022Mal	20	12
Gilal.2013Uga	12	13
Heaal.2020Mal	24	14
Bladan2017Lib	12	16
AbdS. 2016Mal	12	17
AbdS. 2016Mal	24	17
Mcllin2018Rwa	12	17
Mcllin2018Rwa	12	17
Brial.2020Tog	24	19
Blanez2019Uga	48	19
Fieoli2021Mya	30	20
Caral.2021Nig	12	20
Caral.2021Nig	24	20
Carott2021Nig	24	20
Carott2021Nig	48	20
Aggal.2022Mal	20	21
Hanvis2018ZamMCP	24	21
Hanvis2018Zam	24	21
Hanvis2018Zam	36	21
Hanvis2018ZamMCP	36	21
Bailer2013Mal	12	22
Bailer2013Mal	24	22
Weal. 2023Ind	11	22
Weal. 2023Ind	23	22
Aggal.2022Mal	23	22
Cuhhna2014Mex	12	23
Aggal.2022Mal	20	23
Beaal.2023Mal	12	24
Leval.2022Nep	4	24
Bjoal.2022Uga	12	24
Ambton2018Mal	9	25
Gazal.2023Tun	27	26
Fafal.2014Gha	10	27
Fafal.2014Gha	10	27
Hauham2020Ken	12	28
Aggal.2022Mal	20	32
Baial.2024Tan	16	33
Karoto2014Gha	24	33
Dieeck2019Ken	12	35
Merrzi2013Ken	24	35
Aggal.2022Mal	20	35
Fiaala2014Uga	24	37
Fiaala2014Uga	24	37
Preler2022Nig	24	42
Hauiro2016Ken	11	43
Hauiro2018Ken	36	43
Aggal.2022Mal	23	45
Broal.2022Ken	2	49
Hushra2021Ban	4	50
Olaide2016Nig	6	52
Olaide2016Nig	12	52
Crél. 2023Egy	16	52
Crél. 2023Egy	16	52
Hauiro2016Ken	7	53
Pacal.2019Les	24	53
Mcllin2020Rwa	14	57
Agual.Mex	14	63
Agual.Mex	26	63
Gandav2014Ind	12	63
Aggal.2022Mal	23	67
Hauida2021Ken	14	68
Mcllin2020Rwa	14	74
Fiaala2014Uga	10	90
Fiaala2014Uga	10	90
Mcllin2020Rwa	14	90
Eggal.2020Ken	19	91
Orkal.2023Ken	19	102
Choshi2016Sou	12	109
Mcllin2018Rwa	12	112
Mcllin2018Rwa	12	112
Kahal.2022Uga	21	121
Mcllin2020Rwa	14	135
Cooyay2019Uga	18	143
Banal.2022Par	12	178
Hauiro2016Ken	10	181
Hauiro2018Ken	36	181
Hauida2021Ken	14	824

Figure 7.3b: Posterior Average Treatment Effects on Total Consumption sorted by Monthly Tranche Amount - Completed Streams

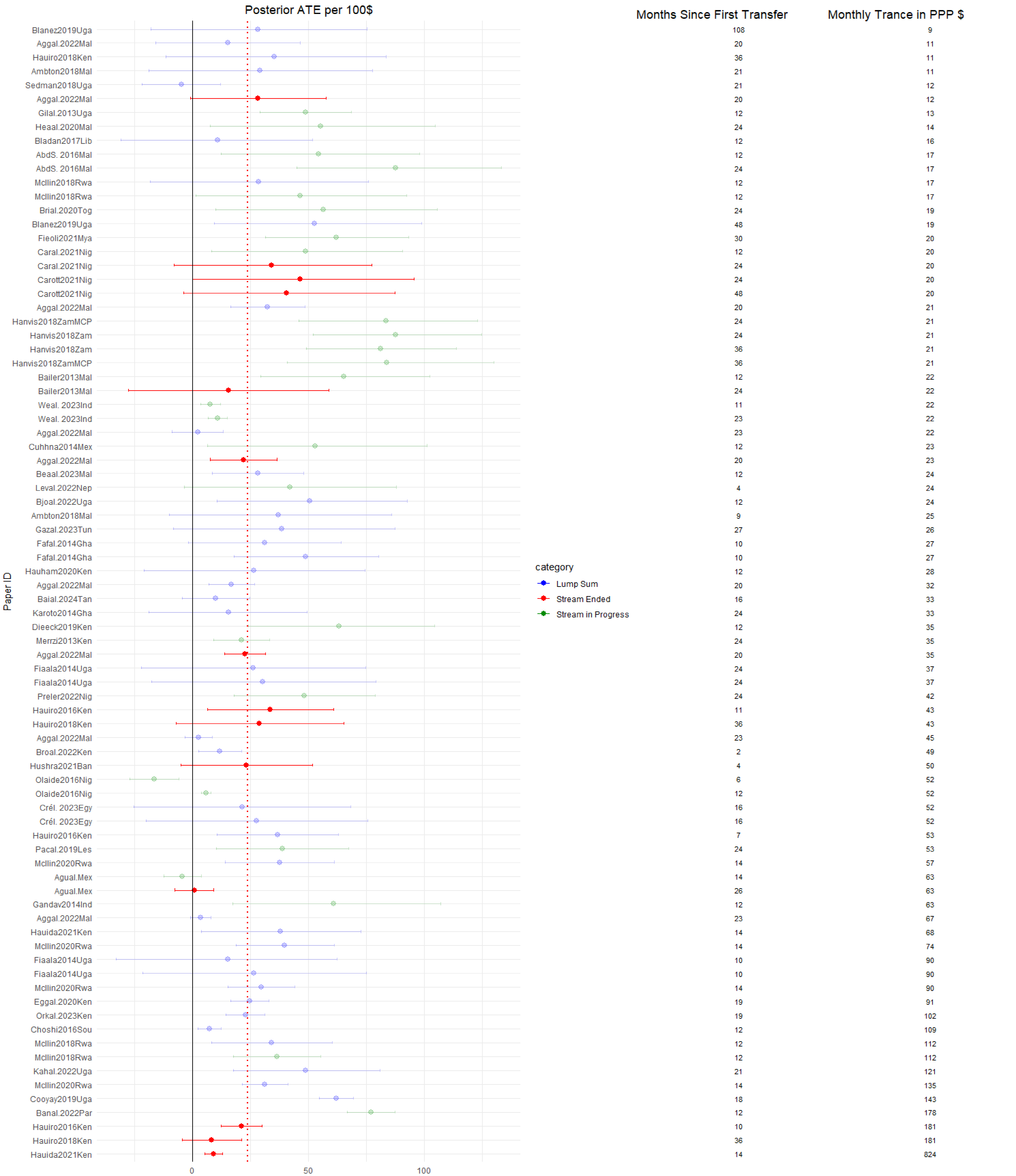


Figure 7.3c: Posterior Average Treatment Effects on Total Consumption sorted by Monthly Tranche Amount - Lump Sums

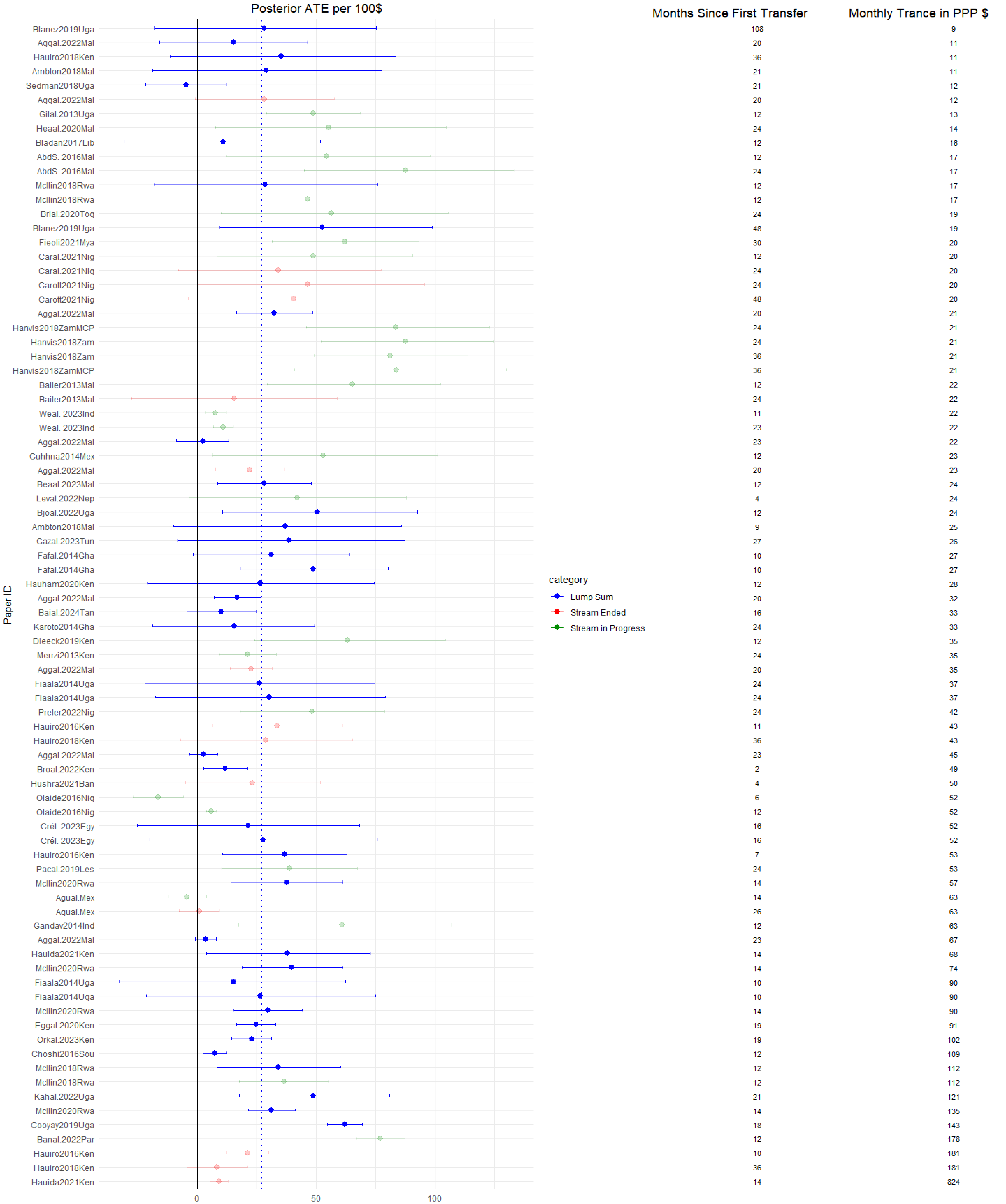


Figure 8.1: Monthly Benefit of Stream UCTs as a Percentage of Monthly Tranche

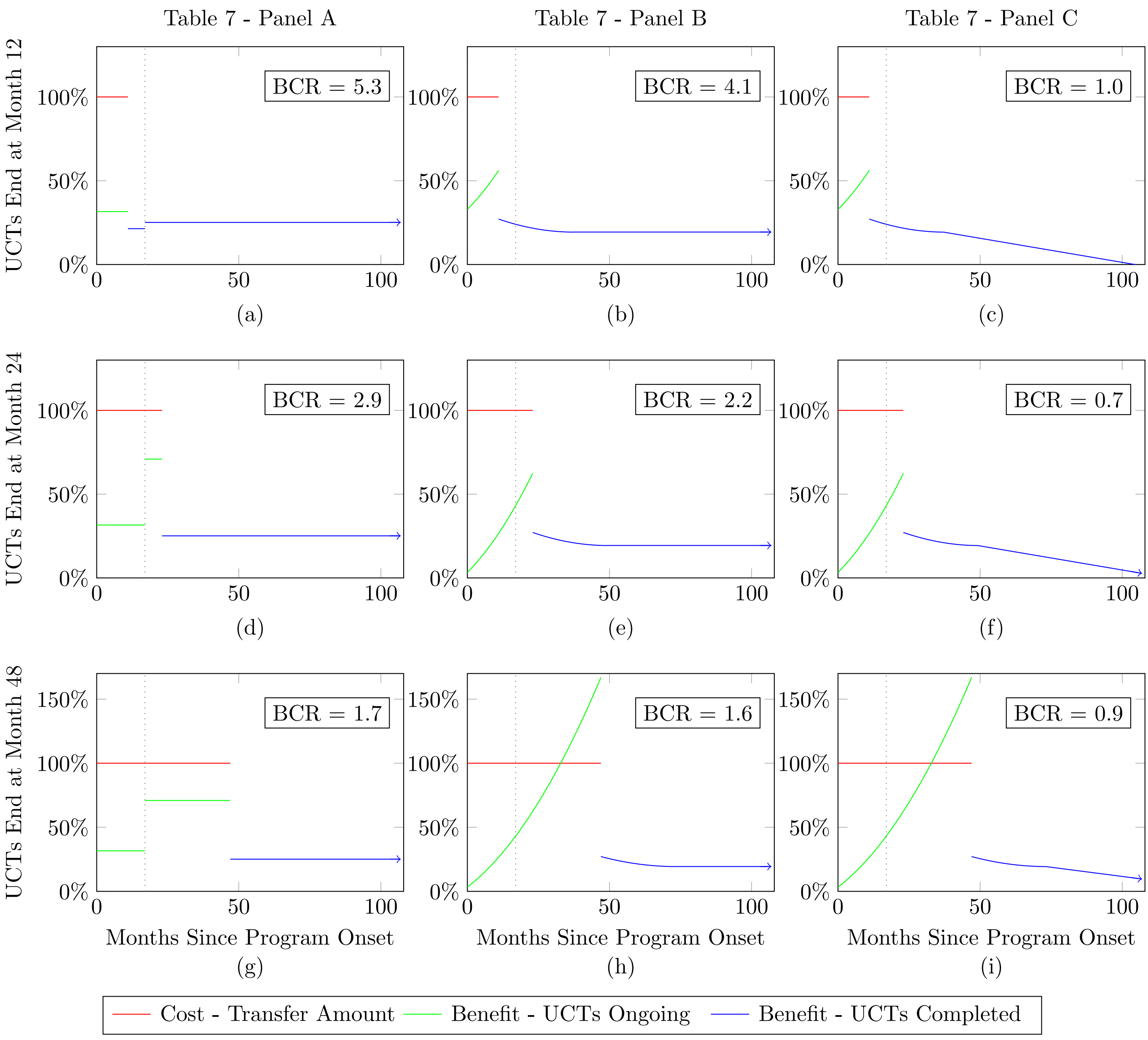
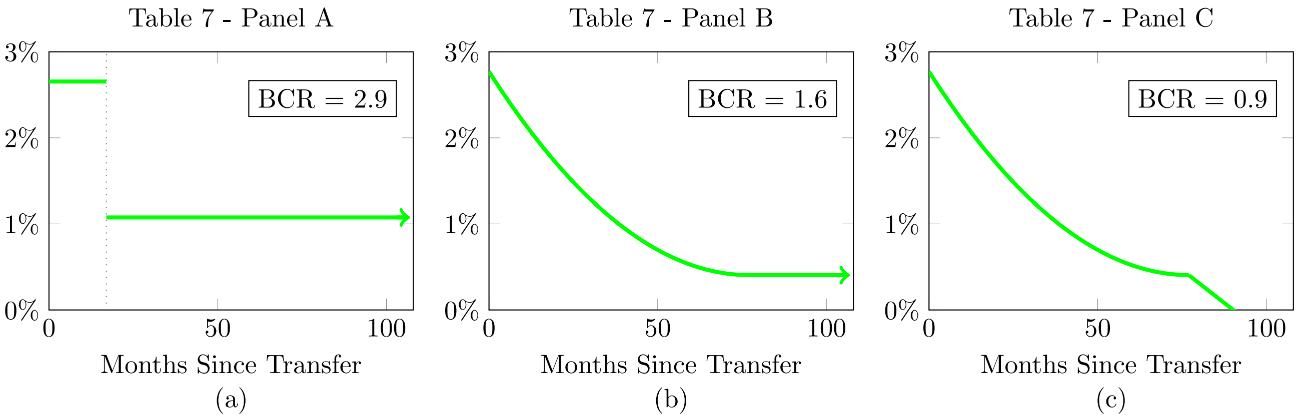


Figure 8.2: Monthly Benefit of Lump Sum UCT as a Percentage of Total Transfer



6 Appendix

6.1 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 publically 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

6.2 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.[23]
5. We exclude treatment effects reported as odds ratios.

[23] 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 non-durable consumption instead. If non-durable consumption is also unavailable, we use food-consumption. 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.

Total assets: When total Assets is not reported, we use either productive/business assets or consumption/household/durable assets instead. If both productive assets and consumption assets are reported, we use whichever has the bigger control group mean as the substitute for total assets. Productive assets, consumption assets, and financial savings are also extracted as secondary outcomes.

Total 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, all household enterprise profits, and enterprise revenues are also extracted as secondary outcomes.

Total hours worked: We extract estimates on the the number of hours worked 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're looking for estimates on whether participants engaged in any income-generating activity, whether self-employment or working for wage, salary, or commission. As secondary outcomes, we also extract binary variables on whether the participant engaged in any non-farm self-employment, farm self-employment, or (non-self) 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 and wasting indicators. Due to data limitations, we did not conduct analysis on wasting.

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.[24]

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.
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.

[24] 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.

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 D.1 for the unstandardized treatment effects.

Total 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.

Total 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, stunting and wasting have standard definitions. 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 D.2 for the unstandardized treatment effects.

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

All currency values are reported in 2010 USD PPP. If a program has multiple endlines, the total transfer amount at the last endline is reported. If a program has both stream and lump sum treatment arms, total transfer amount is reported for lump sum treatments and monthly transfer amount is reported for stream treatments. Program ID 14 reported in 4 papers: Schady and Araujo (2006), Schady and Paxson (2010), Fernald and Hidrobo (2011), and Edmonds and Schady (2012). Program ID 26 reported in 4 papers: Palermo et al. (2012), Handa et al. (2014), Handa et al. (2014), and Kilburn et al. (2016). Program ID 35 reported in 3 papers: Pace et al. (2019), Sebastian et al. (2019), and Prifti et al. (2019). Program ID 41 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 42 reported in 5 papers: Baird et al. (2011, 2012, 2013, 2016), and Sessou et al. (2022). Program ID 54 reported in 3 papers: Carneiro et al. (2021), Carneiro et al. (2021), and Mason (2019). Program ID 72 reported in 3 papers: Blattman et al. (2013), Calderone (2017), and Blattman et al. (2019). Program ID 73 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).

Table A.1b
Program Characteristics

(1) Program ID	(2) Transfer Type	(3) Baseline Year	(4) Baseline Sample	(5) # Survey Rounds	(6) Months Since First Transfer	(7) Months Since Last Transfer	(8) # UCT Treatments	(9) Total Transfer Amount	(10) Monthly Transfer Amount
1	Lump Sum	2016	2,017	1	12.0	12.0, 0.0	4	204.2, 1,340.5	17.0, 111.7
2	Lump Sum	2016	3490	1	23.0	23.0	1	1717.4	74.7
3	Lump Sum	2016	2597	2	0.5, 8.0	0.5, 8.0	1	269.8	33.7
4	Lump Sum	2014	600	4	21.0	21.0	1	379.3	18.1
5	Stream	2012	4,992	1	24.0	0.0	1	1,400.7	58.4
6	Lump Sum	2017	594	1	12.0	12.0	1	15.1	1.3
7	Stream	2015	3,584	2	6.0, 12.0	0.0, 6.0	2	264.0, 527.9	44.0, 88.0
8	Stream	2012	5000	1	23.0	0.0	2	1391.9	60.5
9	Stream	2019	745	1	4.0	2.0	1	99.9	49.5
10	Lump Sum	2008	494	1	16.0	16.0	2	29.2, 87.5	1.8, 5.5
11	Stream	2017	3000	1	12.0	0.0	1	2133.9	177.8
12	Lump Sum	2018	1300	1	9.0	9.0	1	8483.6	942.6
13	Stream	2013	1,185	1	24.0	0.0	1	420.4	42.0
14	Stream	2015	2539	1	30.0	15.0	1	552.0	18.4
15	Stream	2008	2,775	2	12.0, 24.0	0.0, 0.0	1	126.6	10.4
16	Lump Sum	2013	649	1	12.0	12.0	1	1,313.4	109.4
17	Stream	2020	3,462	1	2.0	0.1	1	160.1	80.1
18	Stream	2019	2358	1	12.0-21.0	9.0-16.0	2	1370.9-2741.9	65.3-130.6
19	Stream	2012	4,330	1	24.0	0.0	1	1,006.1	41.9
20	Stream	2015	1,481	1	6.0	0.0	1	405.7	67.6
21	Stream	2003	697	2	15.0, 23.0	0.0, 0.0	1	812.5	35.3
22	Lump Sum	2016	3293	1	16.0	16.0	1	681.8	42.6
23	Lump Sum	2009	160	1	2.0	2.0	1	299.6	149.8
24	Lump Sum	2008	793	6	3.0-34.4	3.0-34.4	1	284.2	8.3
25	Lump Sum	2008	502	1	24.0	24.0	1	794.5	33.1
26	Stream	2010	300	1	12.0	0.0	1	760.9	63.4
27	Stream	2018	2400	2	11.0-38.3	0.0-14.3	1	241.7-527.3	22.0-13.8
28	Lump Sum	2015	1,345	1	12.0	12.0	1	299.6	25.0
29	Stream	2014	3,688	2	24.0, 48.0	0.6, 24.6	1	474.1	20.3
30	Lump Sum	2021	1,120	1	0.5	0.5	1	34.6	69.2
31	Lump Sum	2019	521	1	24.0	12.0	1	44.9	1.9
32	Lump Sum	2020	753	1	2.0	2.0	1	98.0	49.0
33	Lump Sum	2010	387	1	12.0	12.0	1	263.3	21.9
34	Lump Sum, Stream	2017	5,756	1	14.0	14.0, 13.0	2	957.6	823.6
35	Stream	2007	2,294	1	24.0	0.0	1	1,268.8	52.9
36	Lump Sum, Stream	2011	1,008	2	11.2, 36.0	11.2/3.2, 36.0	3	383.8	52.9, 181.1
37	Lump Sum	2008	1017	2	36.0	16.0	3	528.8-18111.5	14.7-503.1
38	Lump Sum	2014	7,845	1	19.0	11.0	1	1,723.2	90.7
39	Stream	2014	2,658	1	24.0	0.0	1	460.0	19.2
40	Lump Sum, Stream	2017	8,753	2	20.0, 27.0	20.0/0.0, 27.0/3.0	3	4,356.4	168.7, 195.2
41	Lump Sum	2017	8339	1	19.0	17.0	1	1941.7	102.2
42	Stream	2013	6,720	2	6.0, 12.0	0.0	1	309.3	51.5
43	Stream	2009	2,865	2	12.0, 24.0	0.0	1	835.1	34.8
44	Lump Sum	2000	1824	1	27.0	27.0	2	768.0	28.4
45	Lump Sum	2011	789	1	11.5	11.5	1	321.1	27.9
46	Lump Sum	2013	905	1	8.5	8.5	1	516.3	60.7
47	Lump Sum	2020	1912	1	1.0	1.0	1	294.0	294.0
48	Stream	2011	1,486	1	24.0	0.0	1	1,273.6	53.1
49	Lump Sum	2018	1496	2	12.0	5.0	1	114.6	9.6
50	Lump Sum, Stream	2019	1,220	1	20.0	20.0, 5.0	6	210.8-632.3	11.7-35.1
51	Stream	2010	3,078	2	24.0, 36.0	0.0	1	761.2	21.1
52	Lump Sum	2009	999	2	12.5	12.5	1	200.0	16.0
53	Stream	2017	4373	1	18.0	0.0	1	998.2	55.5
54	Lump Sum	2019	1,378	1	23.0	21.0-23.0	3	516.2-1,548.5	22.4-67.3
55	Lump Sum	2016	2,018	1	18.0	17.0	1	2,570.7	142.8
56	Lump Sum	2014	1187	3	26.0	21.0	1	204.2	7.9
57	Stream	2013	3,531	2	12.0, 24.0	0.0	1	407.3	17.0
58	Lump Sum	2012	174	6	48.0	48.0	2	307.9	6.4
59	Stream	2007	3,796	3	12.0-48.0	0.0, 4.0, 28.0	1	434.0	21.7
60	Lump Sum	2010	6201	3	12.0-84.0	12.0-84.0	2	172.8	2.1
61	Lump Sum	2020	1264	1	20.6	18.6	1	2189.7	106.3
62	Stream	2014	2,560	1	24.0	0.0	1	342.0	14.3
63	Lump Sum	2008	2598	4	10.0-146.0	10.0-146.0	1	899.1	6.2
64	Lump Sum	2014	5,774	1	21.0	14.0	1	241.9	11.5
65	Stream	2009	2593	4	26.0	14.0	2	756.4-1639.0	29.1-63.0
66	Stream	2011	2,959	1	12.0	0.0	1	179.6	12.8
67	Lump Sum	2008	2,017	3	24.0-108.0	24.0, 48.0, 108.0	1	924.5	38.5
68	Stream	2010	2,519	3	24.0-48.0	0.0, 0.0, 0.0	1	1,094.4	22.8
69	Stream	2003	3,773	1	84.0	66.0	1	435.7	24.2
70	Stream	2008	2,010	1	18.0	2.0	1	725.6	45.3
71	Lump Sum	2019	475	1	5.0	5.0	1	227.3	45.5
72	Stream	2016	2,338	1	30.0	0.0	1	742.1	24.7
73	Stream	2013	4228	2	7.0	2.0	1	95.3	13.6

Table A.2
Targeting and Framing by Program

(1) Program ID	(2) Transfer Type	(3) Target Population	(3) Targeted Females?	(4) Child/Food Framing?	(5) Goal of Framing	(6) 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	Lump Sum	Poor households with young children	Yes	Yes	Health, Child development	Voluntary basic health education orientation program
4	Stream	Mothers with children aged 0-2	Yes	Yes	Child Development	Given to mothers with children under 2 years old along with messaging about nutrition
5	Stream	Refugees	Randomized			
6	Lump Sum	Farmers, rural	Randomized			
7	Lump Sum	Agricultural entrepreneurs	No		Entrepreneurship/enterprise development	Given to businesses along with a business training
8	Stream	Poor households with young children	Yes	Yes	Child development	Told the UCT was to support their child's development and to prevent undernutrition
9	Stream	Rural households with school-age children	Randomized			
10	Stream	Poor households	Yes		COVID-19 emergency aid	Expedited UCT delivery after COVID-19 outbreak to assist the extreme poor
11	Stream	Urban Youth	80% women			
12	Stream	Households with young children with severe malnutrition	Yes			
13	Stream	Households with young children		Yes	Education, Child dev.	Promoted as a way to support the human capital of poor children
14	Lump Sum	Rural entrepreneurs aged 21-35	No		Entrepreneurship/enterprise development	Transfers given to buseness loan applicants
15	Lump Sum	Urban micro-entrepreneurs			Micro-enterprise growth	Asked to spend money on their businesses
16	Lump Sum	Urban Microentroprenuers	80% women		Business Development	Transfers given to micro-entrepreneurs
17	Lump Sum	Farmers, rural		Yes	Farm investment	Individualized deliverty based on farmers' preferences and uses for grant
18	Stream	Poor households	Yes			
19	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
20	Lump Sum	Micro-entrepreneurs			Micro-enterprise growth	Encouraged to invest money in their business
21	Lump Sum	Elderly, living alone	Yes			
22	Lump Sum	Farmers, rural				
23	Lump Sum	Female micro-entrepreneurs	Yes			
24	Lump Sum, Stream	Poor households, rural				
25	Stream	Households with vulnerable children		Yes	Child support	Told the money is to be used for the care of vulnerable children
26	Lump Sum, Stream	Poor households, rural	Randomized			
27	Lump Sum	Poor households, rural				
28	Lump Sum, Stream	Poor households, rural				
29	Lump Sum	Poor or widowed, rural households	Yes			
30	Stream	Poor households		Yes	Food security	Labelled: "Hunger Safety Net Programme"
31	Lump Sum	Informal workers, urban				
32	Lump Sum	Young, poor women, urban	Yes			
33	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
34	Stream	Poor households with vulnerable children		Yes	Child support	Instructed to spend the money on children
35	Lump Sum, Stream	Poor households, rural	77% women			
36	Lump Sum	High-risk men (Criminally Engaged)				
37	Stream	Households with young children	Yes	Yes	Child Development	Mother Leaders groups give "nudges" on intervention days regarding child development
38	Lump Sum	Poor households, rural	77% women			
39	Lump Sum	Poor Farmers	No		Agriculture	Given to farmer clubs
40	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
41	Stream	Adolescent girls, parents, poor region	Yes			
42	Lump Sum	Rural Households	Yes		Agriculture	Given to farmers during planting time
43	Stream	Poor households, men		Yes	Livelihoods, Edu., Child dev.	Voluntary ctivities related to livelihoods, education, child health and nutrition, etc.
44	Stream	Elderly	No			
45	Stream	Poor households, rural	Yes	Yes	Health, Child Development	Health, nutrition, and hygiene classes
46	Stream	Poor households with school-age children, rural	Randomized	Yes	Education	Promoted as for supporting child education
47	Lump Sum	Micro-entrepreneurs			Micro-enterprise growth	Instructed to spend the money on their business
48	Stream	Households with young children	Yes			
49	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
50	Stream	Poor households, rural	Yes			
51	Stream	Extremely Vulnerable households	Yes			
52	Stream	Poor elderly				
53	Stream	Households with young children and in extreme poverty	Yes	Yes	Child development	Information provided on pre-natal health and infant feeding
54	Stream	Poor households with young children				
55	Stream	Elderly	No			
56	Lump Sum, Stream	Young, poor, underemployed adults				
57	Lump Sum	Young, poor, underemployed adults				
58	Lump Sum	Farmers	No		Agriculture	Transfers given alongside farm management plans and agricultural advisory visits
59	Lump Sum	Poor women, post-conflict				
60	Lump Sum	Micro-entrepreneurs	Randomized			
61	Lump Sum	vulnerable groups, (widowed, disabled, elderly)	No			
62	Stream	Households with young children, rural	Yes	Yes	Child development	Case management of child illness and malnutrition (also provided to control group)
63	Lump Sum	Poor rural women	Yes		Female Financial Development	Transfers given alongside gender sensitive financial trainings
64	Lump Sum	Households with exactly one child aged 3-5	Yes		Business development	Transfers labeled as a business grant
65	Lump Sum	Poor farmers, rural				
66	Lump Sum	Businesses	No		Business development	Given to businesses
67	Lump Sum	Refugee Communities	75% women			
68	Lump Sum	Micro Enterprises	No		Business Development	Given to businesses
69	Lump Sum	Poor households				
70	Stream	Households with young children	Yes	Yes	Child development	UCTs provided at UNICEF-supported early childhood development centers.
71	Lump Sum	Young adults, post-conflict			Micro-enterprise growth	Required to submit business grant proposal before receiving transfer
72	Stream	Households with young children, rural	Yes	Yes	Child support	Labelled: "Child Grant Program"
73	Stream	Households with vulnerable adults and children, poor region	Yes			

Table A.3
Administrative Costs

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

Costs are reported in 2010 USD PPP per recipient household.

Table B.1
Program Design Features by Outcome

	Count of Estimates (Programs)	*Percentage by Targeting*			*Percentage by Child/Food Framing*		*Percentage by Transfer Modality*		*Percentage by Implementer*		
		No Targeting	Female Targeting	Male Targeting	No Framing	With Framing	Mobile Money or Bank Transfer	Physical Cash	Government	NGO	Researcher
All Primary Outcomes	494 (73)	53.6% (47.9%)	42.1% (45.2%)	4.3% (6.8%)	75.1% (72.6%)	24.9% (27.4%)	58.9% (52.1%)	37.7% (45.2%)	27.5% (30.1%)	61.3% (50.7%)	11.1% (20.5%)
Monthly Household Consumption	82	54.2%	39.8%	6.0%	78.0%	22.0%	61.0%	36.6%	26.8%	67.1%	6.1%
Monthly Household Food Consumption	49	44.0%	54.0%	2.0%	67.3%	32.7%	55.1%	40.8%	36.7%	57.1%	6.1%
Food Security z-Score	47	48.9%	44.7%	6.4%	72.3%	27.7%	61.7%	38.3%	25.5%	61.7%	12.8%
Total Monthly Income	88	47.1%	46.0%	6.9%	86.4%	13.6%	54.5%	33.0%	14.8%	65.9%	19.3%
Stock of Total Assets	57	70.5%	23.0%	6.6%	89.5%	10.5%	73.7%	26.3%	14.0%	71.9%	14.0%
Total Hours Worked per Week	25	56.0%	40.0%	4.0%	96.0%	4.0%	80.0%	20.0%	32.0%	60.0%	8.0%
Labor Force Participation (percentage points)	17	35.3%	58.8%	5.9%	52.9%	47.1%	29.4%	58.8%	41.2%	52.9%	5.9%
Height-for-Age z-Score	32	34.4%	65.6%	0.0%	50.0%	50.0%	40.6%	59.4%	34.4%	53.1%	12.5%
Weight-for-Age z-Score	15	46.7%	53.3%	0.0%	53.3%	46.7%	53.3%	46.7%	46.7%	46.7%	6.7%
Stunting (percentage points)	12	0.0%	100.0%	0.0%	8.3%	91.7%	25.0%	75.0%	50.0%	50.0%	0.0%
School Enrollment (percentage points)	26	53.8%	38.5%	7.7%	46.2%	53.8%	50.0%	50.0%	57.7%	38.5%	3.8%
Psychological Well-being z-Score	56	45.6%	43.9%	10.5%	78.6%	21.4%	64.3%	35.7%	26.8%	60.7%	12.5%

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.

Table C.1
Treatment Effects on Total Monthly Income: Alternative Income Measures

	(1) $100 Transfer	(2) Median Transfer	(3) Estimates (Programs)
Panel A. Treatment Effect per Total Transfer Amount			
Total Monthly Income (as reported in Table 3)	1.4	5.8	88
	(1, 1.8)	(4, 7.7)	(38)
Total Monthly Income (only using estimates on total household or individual income)	1.9	7.8	34
	(1.1, 2.7)	(4.5, 11.4)	(14)
Panel B. Treatment Effect per Monthly Tranche Amount			
Total Monthly Income (as reported in Table 3)	21.3	9.3	88
	(14.3, 29)	(6.2, 12.7)	(38)
Total Monthly Income (only using estimates on total household or individual income)	34.4	15.0	34
	(18.1, 51.9)	(7.9, 22.7)	(14)

95% credibility intervals in parentheses. All currency values are reported in 2010 USD PPP. Treatment effect per total transfer amount (Panel A) is our preferred outcome variable for lump sum transfers. Treatment effect per monthly tranche amount (Panel B) is our preferred outcome variable for stream transfers. The median total transfer amount is $422, which is calculated by taking the median of the average total transfer amounts of the 39 lump sum programs in our sample. The median monthly tranche amount is $44, which is calculated by taking the median of the average monthly tranche amounts of the 47 stream programs in our sample. Our dataset for **Total Monthly Income** as reported in Table 3 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 compare this to analysis instead using a dataset only including estimates on total household or individual income.

Table C.2
Treatment Effects on Stock of Total Assets: Alternative Asset Measures

	(1) $100 Transfer	(2) Median Transfer	(3) Estimates (Programs)
Panel A. Treatment Effect per Total Transfer Amount			
Stock of Total Assets (as reported in Table 3)	19.6	82.5	57
	(12.2, 27.3)	(51.4, 115.1)	(28)
Stock of Durable Assets	4.4	18.4	16
	(1.9, 6.9)	(8.1, 28.9)	(8)
Stock of Productive Assets	4.1	17.4	37
	(2.2, 6.8)	(9.1, 28.5)	(19)
Stock of Savings	1.7	7.1	49
	(1.1, 2.3)	(4.6, 9.7)	(24)
Panel B. Treatment Effect per Monthly Tranche Amount			
Stock of Total Assets (as reported in Table 3)	245.5	107.3	57
	(146.8, 352.9)	(64.2, 154.2)	(28)
Stock of Durable Assets	77.1	33.7	16
	(37.6, 117.8)	(16.4, 51.5)	(8)
Stock of Productive Assets	42.5	18.6	37
	(23.5, 64.1)	(10.3, 28)	(19)
Stock of Savings	22.6	9.9	49
	(15.1, 30.4)	(6.6, 13.3)	(24)

95% credibility intervals in parentheses. All currency values are reported in 2010 USD PPP. Treatment effect per total transfer amount (Panel A) is our preferred outcome variable for lump sum transfers. Treatment effect per monthly tranche amount (Panel B) is our preferred outcome variable for stream transfers. The median total transfer amount is $422, which is calculated by taking the median of the average total transfer amounts of the 39 lump sum programs in our sample. The median monthly tranche amount is $44, which is calculated by taking the median of the average monthly tranche amounts of the 47 stream programs in our sample.

Table D.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
4	Stream	1392	61	23	Household Hunger Scale	Binary	0.04 (0.02)	0.92 (0.27)	0.15 (0.07)
4	Stream	1392	61	23	Household Hunger Scale	Binary	0.04 (0.02)	0.9 (0.3)	0.13 (0.07)
8	Stream	420	42	24	Household Food Insecurity Acces Scale	Score	0.2 (0.35)	3.5 (3.85)	0.05 (0.09)
10	Stream	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	43	11	Food security index	Standard deviations	0.4 (0.12)	0 (1)	0.4 (0.12)
26	Stream	384	43	36	Food security index	Standard deviations	-0.06 (0.12)	0 (1)	-0.06 (0.12)
26	Stream	1449	181	36	Food security index	Standard deviations	-0.04 (0.14)	0 (1)	-0.04 (0.14)
26	Lump Sum	384	53	7	Food security index	Standard deviations	0.14 (0.11)	0 (1)	0.14 (0.11)
26	Stream	1449	181	10	Food security index	Standard deviations	0.43 (0.12)	0 (1)	0.43 (0.12)
26	Lump Sum	384	11	36	Food security index	Standard deviations	-0.03 (0.1)	0 (1)	-0.03 (0.1)
28	Stream	3940	197	27	Experienced Hunger	Binary	0.05 (0.02)	0.32 (0.47)	0.11 (0.04)
28	Stream	3937	197	27	Experienced Hunger	Binary	0.11 (0.02)	0.32 (0.47)	0.24 (0.04)
28	Lump Sum	4356	161	27	Experienced Hunger	Binary	0.06 (0.02)	0.32 (0.47)	0.13 (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	Stream	211	12	20	Food Security Index	Standard deviations	0.29 (0.07)	0 (1)	0.29 (0.07)
35	Lump Sum	422	21	20	Food Security Index	Standard deviations	0.21 (0.07)	0 (1)	0.21 (0.07)
35	Lump Sum	632	32	20	Food Security Index	Standard deviations	0.52 (0.07)	0 (1)	0.52 (0.07)
35	Lump Sum	211	11	20	Food Security Index	Standard deviations	0.09 (0.07)	0 (1)	0.09 (0.07)
35	Stream	632	35	20	Food Security Index	Standard deviations	0.42 (0.07)	0 (1)	0.42 (0.07)
35	Stream	422	23	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	516	22	23	Household Hunger Score (past month)	Score	0.13 (0.06)	0.95 (1.28)	0.1 (0.05)
38	Lump Sum	1032	45	23	Household Hunger Score (past month)	Score	0.18 (0.06)	0.95 (1.28)	0.14 (0.05)
38	Lump Sum	1549	67	23	Household Hunger Score (past month)	Score	0.17 (0.07)	0.95 (1.28)	0.13 (0.05)
40	Stream	177	15	12	Eats more than 1 meal per day	Binary	0.11 (0.03)	0.88 (0.34)	0.32 (0.09)
40	Stream	407	17	24	More than 1 meal/day	Binary	0.14 (0.03)	0.82 (0.39)	0.35 (0.08)
44	Stream	756	63	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	1006	42	24	Moderate or severe food Insecurity	Binary	0.07 (0.04)	0.59 (0.49)	0.13 (0.09)
53	Stream	474	20	48	Whether child did not have enough food	Binary	0.1 (0.02)	0.83 (0.37)	0.26 (0.05)
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)
59	Lump Sum	1313	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 (0.02)
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	2571	143	18	Food Security index	Standard deviations	0.47 (0.08)	0 (1)	0.47 (0.08)
67	Lump Sum	2406	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	821	23	36	Food security scale	Standard deviations	0.54 (0.1)	0 (1)	0.54 (0.1)
72	Stream	1094	23	48	Meal frequency (3 or more indicator)	Binary	0.18 (0.05)	0.23 (0.42)	0.44 (0.12)
72	Stream	1102	20	82	HFIAS	Standard deviations	0.04 (0.13)	0 (1)	0.04 (0.13)
72	Stream	547	23	24	HFIAS	Standard deviations	0.41 (0.1)	0 (1)	0.41 (0.1)

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.

Table D.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	Lump Sum	15	1	12	Maternal self-esteem (Rosenberg 30 point scale)	Standard Deviations	0.32 (0.1)	0 (0)	0.32 (0.1)
5	Stream	100	50	3	Psychosocial Well-being Index	Standard Deviations	0.06 (0.05)	0 (1)	0.06 (0.05)
6	Lump Sum	87	5	16	Stress score (Episodes of the following negative emotions during the seven days before the survey: nervousness, anger, worry, sadness, inability to sleep, shame, frazzled at not having enough time to do all the subsistence and household chores needed, and envy (adults))	Score	-0.27 (0.12)	6.91 (6.77)	-0.04 (0.02)
6	Lump Sum	29	2	16	Stress score (Episodes of the following negative emotions during the seven days before the survey: nervousness, anger, worry, sadness, inability to sleep, shame, frazzled at not having enough time to do all the subsistence and household chores needed, and envy (adults))	Score	-0.28 (0.14)	6.91 (6.77)	-0.04 (0.02)
10	Stream	160	80	2	Household mental health index	Standard Deviations	0.03 (0.03)	0 (1)	0.03 (0.03)
11	Stream	2742	685	12	Depression, Well-Being, Trust Index	Standard Deviations	0.07 (0.1)	0 (1)	0.07 (0.1)
11	Stream	1371	685	12	Depression, Well-Being, Trust Index	Standard Deviations	0.06 (0.08)	0 (1)	0.06 (0.08)
13	Stream	812	35	23	Mother's depressive symptoms score	Score	-0.71 (0.79)	18.9 (10.6)	-0.07 (0.07)
13	Stream	617	36	15	Depressive Symptoms Index	Standard Deviations	0.09 (0.13)	0 (1)	0.09 (0.13)
14	Lump Sum	682	43	16	Mental Health Index	Standard Deviations	0.05 (0.07)	0 (1)	0.05 (0.07)
14	Lump Sum	682	43	16	Mental Health Index	Standard Deviations	0.11 (0.08)	0 (1)	0.11 (0.08)
19	Stream	242	22	11	Depression Index	Standard Deviations	0.08 (0.07)	3.19 (0)	0.08 (0.07)
19	Stream	505	22	23	Depression Index	Standard Deviations	0.24 (0.16)	3.19 (0)	0.24 (0.16)
21	Lump Sum	35	14	3	Geriatric Depression Scale	Score	0.35 (0.53)	6.4 (4.59)	0.08 (0.11)
21	Lump Sum	35	69	1	Geriatric Depression Scale	Score	1.01 (0.54)	6.4 (4.59)	0.22 (0.12)
24	Lump Sum	958	68	14	Psychological Wellbing Index	Standard Deviations	0.25 (0.08)	0 (1)	0.25 (0.08)
24	Stream	958	824	14	Psychological Wellbing Index	Standard Deviations	0.22 (0.07)	0 (1)	0.22 (0.07)
25	Stream	2322	48	48	CES-D depression scale greater than 10 (depressed)	Binary	0.05 (0.02)	0.63 (0.48)	0.1 (0.04)
26	Stream	1449	181	36	Psychological well-being index	Standard Deviations	0.06 (0.07)	0 (1)	0.06 (0.07)
26	Stream	384	43	36	Psychological well-being index	Standard Deviations	-0.06 (0.07)	0 (1)	-0.06 (0.07)
26	Lump Sum	384	53	7	psychological well-being index	Standard Deviations	0.2 (0.08)	0 (1)	0.2 (0.08)
26	Lump Sum	384	11	36	Psychological well-being index	Standard Deviations	-0.04 (0.08)	0 (1)	-0.04 (0.08)
26	Stream	384	43	11	psychological well-being index	Standard Deviations	0.21 (0.1)	0 (1)	0.21 (0.1)
26	Stream	1449	181	10	psychological well-being index	Standard Deviations	0.2 (0.08)	0 (1)	0.2 (0.08)
29	Lump Sum	1942	102	19	Mental Health z-score	Standard Deviations	0.09 (0.03)	0 (1)	0.09 (0.03)
31	Lump Sum	321	28	12	Subjective Well-being Index	Standard Deviations	0.03 (0.09)	0 (0.92)	0.03 (0.09)
35	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)
35	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	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)
36	Lump Sum	200	16	13	Positive self regard/mental health index	Standard Deviations	-0.03 (0.09)	0 (1)	-0.03 (0.09)
36	Lump Sum	200	246	1	Positive self regard/mental health index	Standard Deviations	0.14 (0.09)	0 (1)	0.14 (0.09)
38	Lump Sum	1549	67	23	Psychological Well-being (past 2 weeks)	Standard Deviations	0.16 (0.06)	0 (1)	0.16 (0.06)
38	Lump Sum	516	22	23	Psychological Well-being (past 2 weeks)	Standard Deviations	0.04 (0.06)	0 (1)	0.04 (0.06)
38	Lump Sum	1032	45	23	Psychological Well-being (past 2 weeks)	Standard Deviations	0.11 (0.06)	0 (1)	0.11 (0.06)
40	Stream	266	15	18	Overall psychological state index	Standard Deviations	0.47 (0.09)	0 (1)	0.47 (0.09)
40	Stream	177	15	12	Quality of Life Scale	Score	2.95 (0.48)	18.1 (6.8)	0.43 (0.07)
41	Stream	260	22	12	GHQ-12 Binary Measure of Psychological Distress	Binary	0.14 (0.04)	0.63 (0.48)	0.29 (0.09)
41	Stream	521	22	24	GHQ-12 Binary Measure of Psychological Distress	Binary	0.04 (0.05)	0.69 (0.46)	0.08 (0.1)
43	Stream	342	14	24	Standardized stress index	Standard Deviations	0.19 (0.12)	0.02 (0.07)	0.19 (0.12)
51	Stream	552	37	30	Self Esteem based on Rosenberg scale	Score	0.07 (0.03)	3.3 (0.03)	2.05 (0.95)
51	Stream	552	37	30	Self Esteem based on Rosenberg scale	Score	-0.04 (0.02)	3.34 (0.03)	-1.45 (0.65)
52	Stream	309	52	6	Life Satisfaction Index	Score	0.49 (0.19)	6.66 (2.3)	0.21 (0.08)
52	Stream	619	52	12	Life Satisfaction Index	Score	1.02 (0.29)	6 (3.22)	0.32 (0.09)
55	Stream	2131	178	12	Subjective Well-being Index	Standard Deviations	0.48 (0.03)	0 (1)	0.48 (0.03)
57	Lump Sum	761	54	14	Subjective well-being index	Standard Deviations	0.4 (0.09)	0 (1)	0.4 (0.09)
57	Lump Sum	1795	128	14	Subjective well-being index	Standard Deviations	0.55 (0.09)	0 (1)	0.55 (0.09)
57	Lump Sum	1202	86	14	Subjective well-being index	Standard Deviations	0.48 (0.09)	0 (1)	0.48 (0.09)
57	Lump Sum	983	70	14	Subjective well-being index	Standard Deviations	0.53 (0.1)	0 (1)	0.53 (0.1)
63	Lump Sum	667	25	27	Current life satisfaction	Score	0.27 (0.06)	2.36 (1.47)	0.18 (0.04)
64	Lump Sum	279	23	12	Happiness with life score	Score	0.81 (0.16)	4.98 (2.45)	0.33 (0.07)
67	Lump Sum	2406	117	21	Psychological Well-being index	Standard Deviations	0.28 (0.08)	0 (1)	0.28 (0.08)
69	Lump Sum	242	12	21	Psychological Outlook Index (Aggregate of subjective well-being, aspirations, self-control, sense of control, sense of status, sense of prid)	Standard Deviations	-0.11 (0.07)	0 (1)	-0.11 (0.07)
71	Lump Sum	773	7	108	Mental health index	Standard Deviations	-0.06 (0.05)	0 (1)	-0.06 (0.05)
72	Stream	1094	23	48	Feeling happy indicator	Binary	0.1 (0.02)	0.78 (0.41)	0.25 (0.05)
72	Stream	547	23	24	Considers self better off than 12 months ago	Binary	0.46 (0.04)	0.07 (0.26)	1.8 (0.17)
72	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.

Table D.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 Total Consumption	(7) Monthly Food Consumption	(8) Food Security z-Score	(9) Monthly Total Income	(10) Stock of Total Assets	(11) Total Hours Worked per Week
2	58.4	24	Ongoing	North						
2	58.4	24	Ongoing	South						
4	60.5	23	Ongoing	North			0.2 (0.11)			
4	60.5	23	Ongoing	South			0.2 (0.12)			
5	49.5	3	Completed							
5	49.5	4	Completed		23.2 (21.3)					
8	42.0	24	Ongoing				0.1 (0.21)			
9	10.4	12	Ongoing						1.4 (57.9)	
9	10.4	24	Ongoing						13.2 (62)	
10	80.1	2	Completed				0 (0.03)			
11	685.5	12	Completed					5.9 (6.3)	130.9 (86)	0.3 (0.3)
11	685.5	12	Completed							
11	685.5	12	Completed					-1.3 (3.5)	-10.7 (19.3)	-0.1 (0.3)
11	685.5	12	Completed							
11	685.5	17	Completed					1.6 (1.9)	44.2 (46.1)	0.8 (0.4)
11	685.5	21	Completed					0.9 (0.8)	9.8 (3.4)	0.2 (0.2)
12	67.6	6	Ongoing							
13	35.3	23	Ongoing							
13	36.3	15	Ongoing							
13	36.3	18	Ongoing							
13	36.3	19	Ongoing							
18	63.4	12	Ongoing		122.8 (62.8)	71.8 (22.1)				
19	22.0	11	Ongoing		7.6 (2.5)	7.6 (2.5)				
19	22.0	23	Ongoing		10.6 (2.5)	10.6 (2.5)				
19	22.0	38	Completed							
24	823.6	14	Completed		9 (2.4)				32.6 (5.6)	
25	48.4	48	Ongoing							
25	52.9	24	Ongoing							
26	42.6	11	Completed		38.8 (19.8)		0.9 (0.28)		621.8 (87.6)	
26	42.6	36	Completed		35.7 (32.2)		-0.1 (0.28)		904.7 (144.1)	
26	181.1	10	Completed		21.2 (5.4)		0.2 (0.07)		315.7 (26.7)	
26	181.1	36	Completed		7.2 (8.1)		0 (0.08)		234.5 (38)	
28	168.7	27	Completed					-3.1 (3.2)		
28	195.2	27	Ongoing					-6 (2.7)		
28	196.9	20	Ongoing					-8.8 (4.7)		
28	196.9	27	Ongoing				0.1 (0.02)			
28	197.0	20	Ongoing					10.6 (7.6)		
28	197.0	27	Ongoing				0.1 (0.02)			
30	34.8	12	Ongoing		95.7 (41.4)	95.7 (41.4)				
30	34.8	24	Ongoing		19.3 (7.5)	96 (48)				
34	32.2	24	Completed							-8.7 (2.7)
34	53.1	24	Ongoing		33.7 (21.5)	28.5 (17.2)				
34	59.2	24	Ongoing							
35	11.6	20	Completed				2.5 (0.6)			
35	11.7	20	Completed		31.2 (22)	-3.2 (9.6)		16.2 (21)		
35	23.2	20	Completed				1.5 (0.3)			
35	23.4	20	Completed		22.1 (9.2)	4.3 (5.7)		3.3 (6.5)		
35	34.8	20	Completed				1.2 (0.2)			
35	35.1	20	Completed		22.7 (5.5)	3.2 (3)		1.4 (5.2)		
37	55.5	18	Ongoing				-2.7 (3.09)			
40	10.7	24	Ongoing					98.7 (27.9)		
40	14.8	12	Ongoing				2.2 (0.6)			
40	14.8	18	Ongoing							
40	17.0	12	Ongoing		75.6 (52.9)	44.8 (43.1)				
40	17.0	24	Ongoing		187.6 (45.1)	154.5 (36)	2.1 (0.49)			
40	20.4	24	Ongoing							
41	21.7	12	Ongoing		87.9 (32.4)					
41	21.7	24	Completed		-14 (54.2)					
41	21.8	48	Completed							
43	14.1	24	Ongoing							
43	14.3	24	Ongoing		259.9 (159)				212.2 (103.7)	
43	42.3	24	Ongoing							
44	63.0	14	Ongoing		-5.9 (4.9)	-5.9 (4.9)	0.7 (0.17)			
44	63.0	26	Completed		0.1 (5.2)	0.1 (5.2)	1.1 (0.18)			
45	23.2	12	Ongoing		110.4 (100)	74.5 (62.6)				
45	24.2	84	Completed							
46	45.3	18	Completed							
48	19.9	30	Ongoing		72.6 (24.1)	72.6 (24.1)				
48	24.7	30	Ongoing							
49	23.8	4	Ongoing		-8.4 (80.5)			155.1 (88)		
50	41.9	24	Ongoing		48.8 (24)	32.3 (17.9)	0.3 (0.21)	-18.9 (27)	0 (0.01)	
51	36.8	30	Completed	Female						
51	36.8	30	Completed	Male						
52	51.5	6	Ongoing		-20 (6.6)			40 (23.7)		3.8 (1.1)
52	51.5	12	Ongoing		6 (1.3)			112 (17.4)		5.2 (0.8)
53	19.9	24	Ongoing			93.8 (41.3)				
53	20.3	12	Ongoing		51.4 (46.8)	118.2 (41.9)				
53	20.3	24	Ongoing							
53	20.3	24	Completed	Female				87.3 (31.1)		
53	20.3	24	Completed	Male				46.8 (80.9)		

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.

Table D.3b

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) Labor Force Participation (percentage points)	(7) Height-for-Age z-Score	(8) Weight-for-Age z-Score	(9) Stunting (percentage points)	(10) School Enrollment (percentage points)	(11) Psychological Well-being z-Score
2	58.4	24	Ongoing	North		0.06 (0.14)				
2	58.4	24	Ongoing	South		-0.17 (0.14)				
4	60.5	23	Ongoing	North						
4	60.5	23	Ongoing	South						
5	49.5	3	Completed							0.1 (0.1)
5	49.5	4	Completed							
8	42.0	24	Ongoing			0 (0)				
9	10.4	12	Ongoing			1.82 (1.83)	144.58 (114.6)		60.3 (60.3)	
9	10.4	24	Ongoing			-1.11 (1.66)	-194.37 (148.6)		102.5 (102.5)	
10	80.1	2	Completed		0.5 (2.5)					0 (0.04)
11	685.5	12	Completed							
11	685.5	12	Completed							0 (0.01)
11	685.5	12	Completed							
11	685.5	12	Completed							0 (0.01)
11	685.5	17	Completed							
11	685.5	21	Completed							
12	67.6	6	Ongoing			-0.01 (0.03)	13.31 (4)			
13	35.3	23	Ongoing			0.03 (0.27)				-0.2 (0.21)
13	36.3	15	Ongoing							0.3 (0.36)
13	36.3	18	Ongoing						17.1 (17.1)	
13	36.3	19	Ongoing						29.5 (29.5)	
18	63.4	12	Ongoing							
19	22.0	11	Ongoing			0.02 (0.23)	0.91 (18.2)	-0.9 (9.1)		0.4 (0.32)
19	22.0	23	Ongoing							1.1 (0.73)
19	22.0	38	Completed			0.27 (0.23)	18.21 (18.2)	1.4 (9.1)		
24	823.6	14	Completed							0 (0.01)
25	48.4	48	Ongoing							0.2 (0.09)
25	52.9	24	Ongoing						3.8 (3.8)	
26	42.6	11	Completed							0.5 (0.23)
26	42.6	36	Completed							-0.1 (0.16)
26	181.1	10	Completed							0.1 (0.04)
26	181.1	36	Completed							0 (0.04)
28	168.7	27	Completed							
28	195.2	27	Ongoing							
28	196.9	20	Ongoing							
28	196.9	27	Ongoing							
28	197.0	20	Ongoing							
28	197.0	27	Ongoing							
30	34.8	12	Ongoing							
30	34.8	24	Ongoing						-34.5 (-34.5)	
34	32.2	24	Completed							
34	53.1	24	Ongoing						16.6 (16.6)	
34	59.2	24	Ongoing		-8.45 (21.5)					
35	11.6	20	Completed							
35	11.7	20	Completed							
35	23.2	20	Completed							
35	23.4	20	Completed							
35	34.8	20	Completed							
35	35.1	20	Completed							
37	55.5	18	Ongoing							
40	10.7	24	Ongoing							
40	14.8	12	Ongoing						81.2 (81.2)	2.9 (0.48)
40	14.8	18	Ongoing							3.2 (0.61)
40	17.0	12	Ongoing							
40	17.0	24	Ongoing			-0.7 (0.53)	8.25 (48.5)	11.8 (28.1)	71.9 (71.9)	
40	20.4	24	Ongoing		10.77 (13.09)					
41	21.7	12	Ongoing						13.8 (13.8)	1.4 (0.4)
41	21.7	24	Completed						0 (0)	0.3 (0.47)
41	21.8	48	Completed			0.3 (0.81)				
43	14.1	24	Ongoing		19.85 (19.14)				13.5 (13.5)	1.4 (0.84)
43	14.3	24	Ongoing							
43	42.3	24	Ongoing						-0.9 (-0.9)	
44	63.0	14	Ongoing							
44	63.0	26	Completed							
45	23.2	12	Ongoing						6.9 (6.9)	
45	24.2	84	Completed			-0.45 (0.56)	-2.07 (40.9)			
46	45.3	18	Completed						16.3 (16.3)	
48	19.9	30	Ongoing							
48	24.7	30	Ongoing			-0.07 (0.17)		-1.6 (8.5)		
49	23.8	4	Ongoing			-0.31 (0.42)	4.2 (29.4)	2.9 (11.3)		
50	41.9	24	Ongoing							
51	36.8	30	Completed	Female						-3.9 (1.76)
51	36.8	30	Completed	Male						5.6 (2.58)
52	51.5	6	Ongoing		6.98 (3.32)					0.4 (0.16)
52	51.5	12	Ongoing		10.48 (2.55)					0.6 (0.17)
53	19.9	24	Ongoing							
53	20.3	12	Ongoing			1.27 (0.53)				
53	20.3	24	Ongoing			0.98 (0.34)				
53	20.3	24	Completed	Female	29.61 (9.38)					
53	20.3	24	Completed	Male	1.48 (0.99)					

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.

Table D.4a
Reported Treatment Effects per 100 USD Total Transfer- Lump Sum UCT Programs

(1) Program ID	(2) Total Transfer Amount	(3) Months Since First Transfer	(4) TE Reported by Sub-group Only	(5) Monthly Total Consumption	(6) Monthly Food Consumption	(7) Food Security z-Score	(8) Monthly Total Income	(9) Stock of Total Assets	(10) Total Hours Worked per Week
1	1,717	23					11.8 (1.7)		
3	15	12							
6	29	16							
6	87	16							
7	8,484	9					-0.6 (0.2)		
14	682	16	Female						1.1 (0.2)
14	682	16	Fale						-0.8 (0.3)
14	825	16	Female	-4.3 (7.3)			4.3 (1.6)		
14	825	16	Male	3.5 (13.3)			-0.5 (4.7)		
15	300	2					-14.6 (14.2)		
15	300	8							0.3 (0.8)
15	300	14					-37.3 (20.2)		
16	284	3	Female				7.2 (5.8)		
16	284	3	Male				3.2 (9.5)		
16	284	6	Female				-0.1 (6.5)		
16	284	6	Male				10.1 (10.8)		
16	284	6	Male				7.9 (12.7)		
16	284	9	Female				1.5 (7.8)		
16	284	11	Female	6.3 (2.4)	10.3 (6.6)				
16	284	11	Male	3.4 (2.7)	10.6 (8.4)				
16	284	12	Female				6.3 (10.2)		
16	284	12	Male				36.2 (13.1)		
16	284	34					14.2 (16.6)		
17	795	24		0.1 (1.2)		0.02 (0.01)	1.3 (1.8)	144.3 (63.5)	
20	300	12					9.4 (6.8)		
21	35	1				-0.07 (0.41)			
21	35	3				-0.7 (0.37)			
23	98	2		5.6 (2.9)	5.6 (2.9)		9.8 (2.5)		
24	958	14		3.6 (2.1)				22.8 (4.5)	
26	384	7		5.7 (2.6)		0.04 (0.03)		90.5 (9.8)	
26	384	9					0 (0.9)		
26	384	27		6.6 (4)		-0.01 (0.03)		106.6 (18.5)	
27	1,723	11		1.3 (0.3)	0.3 (0.2)		0.4 (0.2)	5.1 (0.7)	
28	4,336	20					0.3 (0.2)		
28	4,356	27				0.003 (0.001)	0 (0.1)		
29	1,942	17		1.2 (0.3)			0.8 (0.3)	18.1 (2.1)	
31	321	12		0.3 (14.7)	-3 (4.9)	0.08 (0.02)	24.8 (22.5)		
32	480	9							
32	480	18							
32	516	9					5.7 (2.1)		
32	516	18					-0.1 (2.2)		
33	294	1							
35	211	20		0.3 (1.2)	-0.8 (0.5)	0.04 (0.03)	1.2 (1.2)		
35	422	19		1.7 (0.5)	0.5 (0.3)	0.05 (0.02)	0.6 (0.4)		
35	632	18		0.8 (0.3)	0.2 (0.2)	0.08 (0.01)	-0.1 (0.3)		
36	200	1							
36	200	13		-2.8 (3.9)			2.9 (3.6)	9.7 (7.6)	0.3 (1.3)
38	516	23		0 (0.3)	-0.1 (0.3)	0.02 (0.01)	1 (0.5)	3.3 (2.5)	0.01 (0.03)
38	1,032	22		0.1 (0.2)	0.2 (0.2)	0.01 (0)	-0.1 (0.2)	2.3 (1.1)	-0.01 (0.01)
38	1,549	21		0.1 (0.1)	0.2 (0.1)	0.01 (0)	0.1 (0.2)	4.6 (1.1)	-0.01 (0.01)
39	204	4					0.5 (0.1)		
39	225	4		48.1 (20)	30 (18.2)			2.5 (142)	
39	225	16		19.1 (18.8)	28.7 (16.9)			3.3 (148.5)	
42	285	12		2.4 (1.1)	1.1 (0.5)		0.3 (1)	182.1 (66.9)	
42	285	24					3.7 (1.1)		
42	285	84					-0.3 (2)		
56	204	12		4.7 (10.5)				-4.2 (9.1)	
56	1,341	12		3.1 (1.6)				2.1 (1.4)	
57	761	12							
57	801	12		3 (1.2)			1.9 (0.9)	0.6 (2.1)	
57	983	12							
57	1,035	12		3.1 (1)			2.1 (0.7)	3.3 (1.2)	
57	1,202	12							
57	1,265	12		2.2 (0.7)			1.8 (0.6)	3 (0.9)	
57	1,795	12							
57	1,890	12		2.3 (0.4)			0.8 (0.4)	1.7 (0.6)	
58	379	9						115.6 (126.8)	
58	379	21						24.1 (96)	
59	1,313	12		0.6 (0.3)	0.2 (0.1)	0 (0.01)	0 (1.6)	-4.1 (6.3)	
60	263	12	Female				0.6 (1.8)		
60	263	12	Male				4.3 (1.9)		
60	263	24	Female				1.4 (3)		
60	263	24	Male				4.2 (2.7)		
60	263	36	Female				0 (2.9)		
60	263	36	Male				5 (2.7)		
60	263	66	Female				-1.9 (3.1)		
60	263	66	Male				8.1 (4.1)		
61	529	16		0.5 (0.6)	0.3 (0.4)		-4.4 (8.1)	10.2 (8.6)	
63	667	27				0.01 (0.003)			0 (0.1)
63	708	27		13.9 (5.8)	8.4 (2.5)		5.4 (4.7)	6 (4.7)	
64	279	5				0.14 (0.07)			2.7 (1.4)
64	293	5		9.1 (3.7)	2.3 (1.9)		1.4 (3)	2.3 (0.9)	
65	2,571	17		3.5 (0.3)	0.7 (0.1)	0.02 (0.003)	1 (0.2)	115.1 (12.6)	
66	308	18	Bank transfer				111.3 (141.9)	234 (203.7)	
66	308	18	Physical cash				-26.9 (181.7)	-13.4 (133.4)	
66	308	48	Bank transfer				2.5 (137.3)	184.8 (238.3)	
66	308	48	Physical cash				0.1 (144.4)	36.5 (247.2)	
67	2,406	19				0.004 (0.003)			
67	2,485	19		3.2 (1.2)	2.1 (0.7)			138.6 (138.6)	
68	899	6					27.8 (17.9)		
68	899	9					-39.2 (16.4)		
68	899	10	Female	-30.9 (15.1)				82.1 (123.8)	
68	899	10	Male	-5.1 (34.3)				321.3 (414.7)	
68	899	24	Female	37 (19.9)				-156.9 (113.3)	
68	899	24	Male	-42.2 (40.9)				-45.1 (260.2)	
69	242	14		-0.5 (0.5)		0.01 (0.02)		5.1 (2.7)	
71	773	24							0.5 (0.1)
71	773	48							0.7 (0.2)
71	773	108							0.1 (0.2)
71	924	48			3.8 (1.3)				
71	925	24					2.2 (0.6)	57.4 (11.9)	
71	925	48		3.3 (1.2)			2.8 (0.7)	34 (9.5)	
71	925	108		0.4 (1)			0.6 (1.3)		
71	925	146					1.8 (1)		0.2 (0.2)

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.

Table D.4b
Reported Treatment Effects per 100 USD Total Transfer- Lump Sum UCT Programs

(1) Program ID	(2) Total Transfer Amount	(3) Months Since First Transfer	(4) TE Reported by Sub-group Only	(5) Labor Force Participation (percentage points)	(6) Height-for-Age z-Score	(7) Weight-for-Age z-Score	(8) School Enrollment (percentage points)	(9) Psychological Well-being z-Score
1	1,717	23						
3	15	12			0.66 (0.69)	0.73 (0.68)		2.12 (0.69)
6	29	16						-0.14 (0.07)
6	87	16						-0.05 (0.02)
7	8,484	9						
14	682	16	Female	3.1 (0.4)				0.01 (0.01)
14	682	16	Male	0 (0.4)				0.02 (0.01)
14	825	16	Female					
14	825	16	Male					
15	300	2						
15	300	8						
15	300	14						
16	284	3	Female					
16	284	3	Male					
16	284	6	Female					
16	284	6	Male					
16	284	6	Male					
16	284	9	Female					
16	284	11	Female					
16	284	11	Male					
16	284	12	Female					
16	284	12	Male					
16	284	34						
17	795	24						
20	300	12						
21	35	1						0.64 (0.34)
21	35	3						0.22 (0.33)
23	98	2						
24	958	14						0.03 (0.01)
26	384	7						0.05 (0.02)
26	384	9						
26	384	27						-0.01 (0.02)
27	1,723	11						
28	4,336	20						
28	4,356	27						
29	1,942	17						0 (0)
31	321	12						0.01 (0.03)
32	480	9		0.5 (1)				
32	480	18		1.2 (0.9)				
32	516	9						
32	516	18						
33	294	1					2.6 (0.5)	
35	211	20						
35	422	19						
35	632	18						
36	200	1						0.07 (0.05)
36	200	13						-0.02 (0.05)
38	516	23			0 (0.02)	0.01 (0.02)	-0.4 (0.2)	0.01 (0.01)
38	1,032	22			0.01 (0.01)	-0.01 (0.01)	-0.1 (0.1)	0.01 (0.01)
38	1,549	21			0.01 (0.01)	0 (0.01)	-0.1 (0.1)	0.01 (0.004)
39	204	4						
39	225	4						
39	225	16						
42	285	12						
42	285	24						
42	285	84						
56	204	12						
56	1,341	12						
57	761	12						0.05 (0.01)
57	801	12						
57	983	12						0.05 (0.01)
57	1,035	12						
57	1,202	12						0.04 (0.01)
57	1,265	12						
57	1,795	12						0.03 (0.01)
57	1,890	12						
58	379	9						
58	379	21						
59	1,313	12						
60	263	12	Female					
60	263	12	Male					
60	263	24	Female					
60	263	24	Male					
60	263	36	Female					
60	263	36	Male					
60	263	66	Female					
60	263	66	Male					
61	529	16						
63	667	27						0.03 (0.01)
63	708	27						
64	279	5		2.2 (1.1)			-0.4 (0.7)	0.12 (0.02)
64	293	5						
65	2,571	17						
66	308	18	Bank transfer					
66	308	18	Physical cash					
66	308	48	Bank transfer					
66	308	48	Physical cash					
67	2,406	19		0.2 (0.1)			0 (0.2)	0.01 (0.003)
67	2,485	19						
68	899	6						
68	899	9						
68	899	10	Female					
68	899	10	Male					
68	899	24	Female					
68	899	24	Male					
69	242	14						-0.04 (0.03)
71	773	24						
71	773	48						
71	773	108						-0.01 (0.01)
71	924	48						
71	925	24						
71	925	48						
71	925	108						
71	925	146						

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.

Table E.1: 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, September 2019.
3	Hossain, Sheikh Jamal, Bharaty Rani Roy, Hasan Mahmud Sujon, Thach Tran, Jane Fisher, Fahmida Tofail, Shams El Arifeen, and Jena Derakhshani Hamadani. “Effects of Integrated Psychosocial Stimulation and Unconditional Cash Transfer on Children’s Development in Rural Bangladesh: A Cluster Randomized Controlled Trial.” *Social Science & Medicine* 293 (January 2022): 114657.
4	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 Econommics and Statistics* 84, no.1 (February 2022) 241-71.
5	Hussam, Reshmaan, Erin Kelley, Gregory Lane, and Fatima Zahra. “The Psychological Value of Employment,” NBER Working Paper Series 28924, June 2021.
6	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.
7	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.
8	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.
9	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 Papers, June 2016.
10	Londono-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.
11	Javier, Kaleb, Jeremy Magruder, Nicolas Polasek, and Eleanor Wiseman. “DRC Benchmarking Report.” USAID: Washington, DC, USA, September 2022.

12 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.

13 Edmonds, Eric V, and Norbert Schady. “Poverty Alleviation and Child Labor.” *American Economic Journal: Economic Policy* 4, no. 4 (November 2012): 100–124.

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.

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.

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 Papers, June 2006.

14 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.

15 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.

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 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).

17 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.

18 Gangopadhyay, Shubhashis, Robert Lensink, and Bhupesh Yadav. “Cash or In-Kind Transfers? Evidence from a Randomised Controlled Trial in Delhi, India.” *Journal of Development Studies* 51, no. 6 (June 2015): 660–73.

19 Weaver, Jeffrey, Sandip Sukhtankar, and Karthik Muralidharan. “Cash Transfers for Child Development: Experimental Evidence from India,” July 2023.

20 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.

21 McKelway, Madeline, Abhijit Banerjee, Erin Grela, Frank Schilbach, Miriam Sequeira, Garima Sharma, Girija Vaidyanathan, and Esther Duflo. “Effects of Cognitive Behavioral Therapy and Cash Transfers on Older Persons Living Alone in India: A Randomized Trial.” *Annals of Internal Medicine* 176, no. 5 (May 2023): 632–41.

22 Acampora, Michelle, Lorenzo Casaburi, and Jack Willis. “Land Rental Markets: Experimental Evidence from Kenya,” NBER Working Paper Series, September 2022.

23 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.

24 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.

25 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 15-25.” 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.

26 Haushofer, Johannes, and Jeremy Shapiro. “The Short-Term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya.” *The Quarterly Journal of Economics* 131, no. 4 (November 2016): 1973–2042.

Haushofer, Johannes, and Jeremy Shapiro. “The Long-Term Impact of Unconditional Cash Transfers: Experimental Evidence from Kenya.” Working Paper, January 2018.

Bhargava, Iti. “Unconditional Cash Transfers and Their Impact on Well-Being in Kenya,” Independent, May 2019.

27 Egger, Dennis, Johannes Haushofer, Edward Miguel, Paul Niehaus, and Michael Walker. “General Equilibrium Effects of Cash Transfers: Experimental Evidence from Kenya.” *Econometrica* 90, no. 6 (November 2022): 2603–43.

28 Banerjee, Abhijit, Michael Faye, Alan Krueger, Paul Niehaus, and Tavneet Suri. “Effects of a Universal Basic Income During the Pandemic.” Working Paper, December 2020.

29 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.

30 Dietrich, Stephan, and Georg Schmerzeck. “Cash Transfers and Nutrition: The Role of Market Isolation After Weather Shocks.” *Food Policy* 87 (August 2019): 101739.

Merttens, Fred, Alex Hurrell, Marta Marzi, Ramla Attah, Maham Farhat, Andrew Kardan, and Ian MacAuslan. “Kenya Hunger Safety Net Programme Monitoring and Evaluation Component,” Oxford Policy Management Impact Evaluation Report, June 2013.

31 Haushofer, Johannes, Matthieu Chemin, Chaning Jang, and Justin Abraham. "Economic and Psychological Effects of Health Insurance and Cash Transfers: Evidence from a Randomized Experiment in Kenya." *Journal of Development Economics* 144 (May 2020): 102416.

32 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 Papers 7977, February 2017.

33 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.

34 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.

35 Aggarwal, Shilpa, 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." Working Paper, October 2022.

36 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.

37 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

38 Aggarwal, Shilpa, 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." Working Paper, October 2022.

39 Ambler, Kate, Alan de Brauw, and Susan Godlonton. "Agriculture Support Services in Malawi: Direct Effects, Complementarities, and Time Dynamics," IFPRI Discussion Paper 1725, May 2018.

Ambler, Kate, Alan de Brauw, and Susan Godlonton. "Rural Labor Market Responses to Large Lumpy Cash Transfers: Evidence from Malawi," Working Paper, December 2018.

Ambler, Kate, Alan de Brauw, and Susan Godlonton. "Cash, Inputs, and Information: Direct Effects and Complementarities in Malawi," Working Paper, April 2020.

40 Abdoulayi, Sara, Sudhanshu Handa, Gustavo Angeles, Clare Barrington, Peter Mvula, and Maxton Tsoka. "Malawi Social Cash Transfer Programme Impact Evaluation." Impact Evaluation Report, December 2016.

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.

De Hoop, Jacobus, Valeria Groppo, and Sudhanshu Handa. “Cash Transfers, Microentrepreneurial Activity, and Child Work: Evidence from Malawi and Zambia.” *The World Bank Economic Review* 34, no. 3 (October 2020): 670–97.

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.

Molotsky, Adria, and Sudhanshu Handa. “The Psychology of Poverty: Evidence from the Field.” *Journal of African Economies* 330, no. 3 (June 2021): 207–24.

41 Baird, Sarah, Craig McIntosh, and Berk Özler. “Cash or Condition? Evidence from a Cash Transfer Experiment.” *The Quarterly Journal of Economics* 126, no. 4 (November 2011): 1709–53.

Baird, Sarah J, Richard S Garfein, Craig T McIntosh, and Berk Özler. “Effect of a Cash Transfer Programme for Schooling on Prevalence of Hiv and Herpes Simplex Type 2 in Malawi: A Cluster Randomised Trial.” *The Lancet* 379, no. 9823 (April 2012): 1320–29.

Baird, Sarah, Ephraim Chirwa, Jacobus De Hoop, and Berk Özler. “Girl Power: Cash Transfers and Adolescent Welfare. Evidence from a Cluster-Randomized Experiment in Malawi,” NBER Working Paper Series, March 2013.

Baird, Sarah, Craig McIntosh, and Berk Özler. “Whent the Money Runs Out: Do Cash Transfers Have Sustained Effects on Human Capital Accumulation?,” Policy Research Working Papers, December 2016.

Sessou, Eric, Melissa Hidrobo, Shalini Roy, and Lieven Huybregts. “Schooling Impacts of an Unconditional Cash Transfer Program in Mali,” IFPRI Discussion Paper, October 2022.

42 Beaman, Lori, Dean Karlan, Bram Thuysbaert, and Christopher Udry. “Selection Into Credit Markets: Evidence From Agriculture in Mali.” Econometrica 91, no. 5 (September 2023): 1595–1627.

43 Heath, Rachel, Melissa Hidrobo, and Shalini Roy. “Cash Transfers, Polygamy, and Intimate Partner Violence: Experimental Evidence from Mali.” *Journal of Development Economics* 143 (March 2020): 102410.

Sessou, Eric, and Christian H C A Henning. “Cash Transfers and School Enrolment,” Working Papers of Agricultural Policy, February 2019.

44 Aguila, Emma, Arie Kapteyn, and Erik Meijer. “Effects of Permanent Income Increases on Neighbors: Evidence from an Rct,” Working Paper, (preliminary).

45 Cunha, Jesse M. “Testing Paternalism: Cash Versus in-Kind Transfers.” *American Economic Journal: Applied Economics* 6, no. 2 (April 2014): 195–230.

Avitabile, Ciro, Jesse M Cunha, and Ricardo Meilman Cohn. “The Medium Term Impacts of Cash and In-Kind Food Transfers on Learning,” Working Paper, July 2020.

46 Benhassine, Najy, Florencia Devoto, Esther Duflo, Pascaline Dupas, and Victor Pouliquen. “Turning a Shove into a Nudge? A ‘Labeled Cash Transfer’ for Education.” *American Economic Journal: Economic Policy* 7, no. 3 (August 2015): 86–125.

47 Berkel, Hanna, Peter Fisker, and Finn Tarp. “Cash Grants to Manufacturers After Cyclone Idai: RCT Evidence from Mozambique,” WIDER Working Paper 2021/87, May 2021.

48 Field, Erica M., and Elisa M. Maffioli. “Are Behavioral Change Interventions Needed to Make Cash Transfer Programs Work for Children? Experimental Evidence from Myanmar,” NBER Working Paper Series, February 2021.

49 Levere, Michael, Gayatri Acharya, and Prashant Bharadwaj. “The Role of Information and Cash Transfers in Early Childhood Development: Short and Long Run Evidence from Nepal.” Economic Development and Cultural Change, November 2022.

50 Premand, Patrick, and Quentin Stoeffler. “Do Cash Transfers Foster Resilience? Evidence from Rural Niger,” World Bank Policy Research Working Papers, November 2020.

Premand, Patrick, and Oumar Barry. “Behavioral Change Promotion, Cash Transfers and Early Childhood Development: Experimental Evidence from a Government Program in a Low-Income Setting.” Journal of Development Economics 158 (September 2022): 102921.

51 Cullen, Claire, and Paula Gonzalez Martinez. “Empowering Women Without Backlash?,” Working Paper, January 2020.

52 Alzua, Maria Laura, Natalia Cantet, Ana C Dammert, and Damilola Olajide. “Mental Health Effects of an Old Age Pension: Experimental Evidence for Ekiti State in Nigeria,” Agricultural & Applied Economics Association Working Paper, July 2020.

Olajide, Damilola, Adaku Ezeibe, Olusegun Sotola, Kafilah Gold, Olufunke Olufemi, and Florence Adebayo. “Randomised Evaluation of Unconditional Cash Transfer Scheme for the Elderly in Ekiti State Nigeria,” Partnership for Economic Policy Working Paper, April 2016.

53 Carneiro, Pedro, Lucy Kraftman, Giacomo Mason, Lucie Moore, Imran Rasul, and Molly Scott. “The Impacts of a Multifaceted Prenatal Intervention on Human Capital Accumulation in Early Life.” *American Economic Review* 111, no. 8 (August 2021): 2506–49.

Carneiro, Pedro, Lucy Kraftman, Imran Rasul, and Molly Scott. “Do Cash Transfers Promoting Early Childhood Development Have Unintended Consequences on Fertility?,” Working Paper, September 2021.

Mason, Giacomo. “Essays in the Economics of Child Health and Skill Formation,” University College London Dissertation, June 2019

54 Fenn, Bridget, Tim Colbourn, Carmel Dolan, Silke Pietzsch, Murtaza Sangrasi, and Jeremy Shoham. “Impact Evaluation of Different Cash-Based Intervention Modalities on Child and Maternal Nutritional Status in Sindh Province, Pakistan, at 6 Months and at 1 Year: A Cluster Randomised Controlled Trial.” *PLOS Medicine* 14, no. 5 (May 2017): e1002305.

55 Bando, Rosangela, Sebastian Galiani, and Paul Gertler. “Another Brick on the Wall: On the Effects of Non-Contributory Pensions on Material and Subjective Well Being.” Journal of Economic Behavior & Organization 195 (March 2022): 16–26.

56 McIntosh, Craig, and Andrew Zeitlin. “Benchmarking a Child Nutrition Program Against Cash: Evidence from Rwanda,” Working Paper, December 2020.

57 McIntosh, Craig, and Andrew Zeitlin. “Using Household Grants to Benchmark the Cost Effectiveness of a USAID Workforce Readiness Program.” *Journal of Development Economics*, June 2022.

58 Ambler, Kate, Alan de Brauw, and Susan Godlonton. “Cash Transfers and Management Advice for Agriculture: Evidence from Senegal.” The World Bank Economic Review 34, no. 3 (October 2020): 597–617.

59 Chowdhury, Reajul, Elliott Collins, Ethan Ligon, and Kaivan Munshi. “Valuing Assets Provided to Low-Income Households in South Sudan,” Working Paper, July 2017.

60 Mel, Suresh de, and David Mckenzie. “One-Time Transfers of Cash or Capital Have Long-Lasting Effects on Microenterprises in Sri Lanka.” *Science* 335 (February 2012): 962–66.

61 Baird, Sarah, Craig McIntosh, Berk Özler, and Utz Pape. “Asset Transfers and Anti-Poverty Programs: Experimental Evidence from Tanzania.” Journal of Development Economics 166 (January 2024): 103182.

62 Briaux, Justine, Yves Martin-Prevel, Sophie Carles, Sonia Fortin, Yves Kameli, Laura Adubra, Andréa Renk, et al. “Evaluation of an Unconditional Cash Transfer Program Targeting Children’s First-1,000-Days Linear Growth in Rural Togo: A Cluster-Randomized Controlled Trial.” *PLoS Medicine* 17, no. 11 (November 2020): e1003388.

63 Gazeaud, Jules, Nausheen Khan, Eric Mvukiyehe, and Olivier Sterck. “With or Without Him? Experimental Evidence on Cash Grants and Gender-Sensitive Trainings in Tunisia.” Journal of Development Economics 165 (October 2023): 103169.

64 Bjorvatn, Kjetil, Denise Ferris, Selim Gulesci, Arne Nasgowitz, Vincent Somville, and Lore Vandewalle. “Childcare, Labor Supply, and Business Development: Experimental Evidence from Uganda,” GLMLIC Working Paper, June 2022.

65 Cooke, Michael, and Piali Mukhopadhyay. “Cash Crop: Evaluating Large Cash Transfers to Coffee Farming Communities in Uganda,” Impact Evaluation Report, May 2019.

66 Klühs, Theres, and Tevin Tafese. “Rethinking the Effectiveness of Cash Transfers - Evidence from a Field Experiment in Uganda.” Leibniz University Hannover Dissertation n. d., July 2019.

67 Kahura, Christine, Dan Stein, Emma Kimani, Emmanuel Nshakira Rukundo, Gabrielle Posner, Heather Lanthorn, K J Zhao, et al. “GiveDirectly Uganda Endline Report.” ID Insight, August 2022.

68 Fiala, Nathan. “Stimulating Microenterprise Growth: Results from a Loans, Grants and Training Experiment in Uganda,” Working Paper, April 2014.

Fiala, Nathan. “Business Is Tough, but Family Is Worse: Household Bargaining and Investment in Microenterprises in Uganda,” Working Paper, April 2017.

Fiala, Nathan, Julian Rose, Jörg Ankel-Peters, and Filder Aryemo. “The (Very) Long-Run Impacts of Cash Grants During a Crisis,” EconStor Working Paper, August 2022.

69 Sedlmayr, Richard, Anuj Shah, and Munshi Sulaiman. “Cash-Plus: Poverty Impacts of Alternative Transfer-Based Approaches.” *Journal of Development Economics* 144 (May 2020): 102418.

70 Gilligan, Daniel O, Amy Margolies, Esteban Quiñones, and Shalini Roy. “Impact Evaluation of Cash and Food Transfers at Early Childhood Development Centers in Karamoja, Uganda,” Impact Evaluation Report, May 2013.

71 Blattman, Christopher, Nathan Fiala, and Sebastian Martinez. “Generating Skilled Self-Employment in Developing Countries: Experimental Evidence from Uganda.” *The Quarterly Journal of Economics* 129, no. 2 (May 2014): 697–752.

Blattman, Christopher, Nathan Fiala, and Sebastian Martinez. “The Long Term Impacts of Grants on Poverty: 9-Year Evidence from Uganda’s Youth Opportunities Program,” Working Paper, April 2019.

Calderone, Margherita. “Are There Different Spillover Effects from Cash Transfers to Men and Women? Impacts on Investments in Education in Post-War Uganda,” WIDER Working Paper 2017/93, April 2017.

72 Chakrabarti, Averi, Sudhanshu Handa, Luisa Natali, David Seidenfeld, and Gelson Tembo. “Cash Transfers and Child Nutrition in Zambia,” UNICEF Office of Research - Innocenti Working Paper, August 2019.

De Hoop, Jacobus, Valeria Groppo, and Sudhanshu Handa. “Cash Transfers, Microentrepreneurial Activity, and Child Work: Evidence from Malawi and Zambia.” *The World Bank Economic Review* 34, no. 3 (October 2020): 670–97.

Handa, Sudhanshu, David Seidenfeld, Benjamin Davis, Gelson Tembo, and Zambia Cash Transfer Evaluation Team. “The Social and Productive Impacts of Zambia’s Child Grant.” *Journal of Policy Analysis and Management* 35, no. 2 (April 2016): 357–87..

	Handa, Sudhanshu, Luisa Natali, David Seidenfeld, Gelson Tembo, and Benjamin Davis. "Can Unconditional Cash Transfers Raise Long-Term Living Standards? Evidence from Zambia." *Journal of Development Economics* 133 (July 2018): 42–65. Handa, Sudhanshu, Luisa Natali, David Seidenfeld, and Gelson Tembo. "The Impact of Zambia's Unconditional Child Grant on Schooling and Work: Results from a Large-Scale Social Experiment." *Journal of Development Effectiveness* 8, no. 3 (June 2016): 346–67. Natali, Luisa, Sudhanshu Handa, Amber Peterman, David Seidenfeld, and Gelson Tembo. "Does Money Buy Happiness? Evidence from an Unconditional Cash Transfer in Zambia." *SSM - Population Health* 4 (April 2018): 225–35. Seidenfeld, David. "Zambia's Child Grant Program: 36-Month Impact Report." *Ministry of Community Development, Mother and Child Health*, December 2014. Handa, Sudhanshu, Gelson Tembo, Palm Associates and University of Zambia, Luisa Natali, UNICEF Office of Research-Innocenti, Gustavo Angeles, University of North Carolina at Chapel Hill, Gean Spektor, and University of North Carolina at Chapel Hill. "In Search of the Holy Grail: Can Unconditional Cash Transfers Graduate Households Out of Poverty in Zambia?," International Initiative for Impact Evaluation, September 2019.
73	Handa, Sudhanshu, Luisa Natali, David Seidenfeld, Gelson Tembo, and Benjamin Davis. "Can Unconditional Cash Transfers Raise Long-Term Living Standards? Evidence from Zambia." *Journal of Development Economics* 133 (July 2018): 42–65. Handa, Sudhanshu, David Seidenfeld, and Gelson Tembo. "The Impact of a Large-Scale Poverty-Targeted Cash Transfer Program on Intertemporal Choice," Economic Development and Cultural Change, September 2020.