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The Effectiveness of Cash and Cash Plus Interventions on Livelihoods Outcomes

Evidence from a Systematic Review and Meta-analysis

Jessica Leight

Kalle Hirvonen

Sarim Zafar

Poverty, Gender, and Inclusion Unit

INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE

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AUTHORS

Jessica Leight (j.leight@cgiar.org) is a Senior Research Fellow in the Poverty, Gender, and Inclusion (PGI) Unit of the International Food Policy Research Institute (IFPRI), Washington, DC.

Kalle Hirvonen (k.hirvonen@cgiar.org) is a Senior Research Fellow in IFPRI's PGI Unit, Helsinki, Finland.

Sarim Zafar (s.zafar@cgiar.org) is a Research Analyst in IFPRI's PGI Unit, Washington, DC.

Notices

Abstract

Over the last 20 years, a burgeoning scholarly literature has analyzed the effects of cash transfer and cash plus interventions in a wide range of contexts and using a range of empirical designs. We conduct a systematic review and meta-analysis to estimate the pooled effect of any cash or cash plus intervention on livelihoods-related outcomes (consumption, income and labor supply), ultimately compiling 305 different treatment estimates from 155 treatment arms in 104 studies (and in 43 countries). Using random effects and multilevel models, our findings suggest that cash transfer programming is associated with an increase of between $1 and $2 in monthly household consumption and income per $100 in cumulative transfers, an effect that persists for a period of roughly three years (inclusive of the period of program implementation); this effect is meaningfully larger (as much as $4 larger) for cash transfer programs that also include a cash-plus livelihoods intervention. There are no significant effects observed on labor force participation. We also present a range of estimates capturing the longer-term (cumulative) effects of cash transfers on consumption under alternate assumptions.

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1 Introduction

In recent decades, cash transfer programs such as conditional cash transfers (CCTs) and unconditional cash transfers (UCTs) have become increasingly popular, particularly in low- and middle-income countries (Fiszbein, Kanbur and Yemtsov, 2014; Gentilini, 2022). The primary goal of these programs, as stated by national governments, international development agencies, and humanitarian organizations, is to assist poor households in meeting basic needs and escaping extreme poverty (Fiszbein and Schady, 2009; USAID, 2022; WFP, 2023). Cash transfers allow flexibility by permitting recipients to finance immediate consumption needs or to increase future income streams through investments and savings. They are also, in general, an intervention that is feasible to implement and scale across a range of contexts (Gentilini, 2016, 2022).

A wide range of available evidence — explored extensively in this systematic review and meta-analysis — shows that cash transfers are highly effective in reducing poverty in the short run, but the limited literature analyzing medium or long-run effects of cash transfers suggests more mixed evidence around the sustainability of these effects following the cessation of cash payments (Blattman, Fiala and Martinez, 2020; Haushofer and Shapiro, 2018). "Cash plus" interventions then couple transfers with complementary interventions aiming to help households enhance their existing livelihood activities or to diversify into new activities to break the cycle of chronic poverty and build resilience; the use of cash plus is often motivated in part by the goal of making the effects of cash more sustainable (FAO, 2018). Examples of common cash plus interventions include training on financial literacy or business management, or provision of assets like livestock and/or large lump sum cash to launch income generating activities. Importantly, these marginal benefits will most plausibly be observed in a context where viable income-generating activities exist, and may be accordingly attenuated in fragile or conflict-affected contexts where local economic activity is disrupted and complementary public services are largely absent (Lind, Sabates-Wheeler and Szyp, 2023).

Over the last 20 years, a growing body of research has evaluated the impact of both cash and cash-plus interventions in a wide range of settings, using both experimental and non-experimental methods. Our goal here is to systematically review and meta-analyze the literature analyzing the effects of all forms of cash and cash plus interventions on livelihoods-related outcomes (defined to be consumption, income, and labor force participation). The eligibility criteria for inclusion in the review thus require an experimental or quasi-experimental causal analysis of a cash or cash plus intervention (compared to a control or status quo without cash) conducted in a low-or middle-income country analyzing one of the outcomes of interest. We also seek to examine how these effects vary in different contexts and when analyzed using different experimental designs; and assess any potential bias in this literature introduced by selective reporting (the "file–drawer problem").

Following a comprehensive and structured search of both published and unpublished literature, our review includes 104 studies encompassing 155 different treatment arms, of which 26 arms coupled cash with livelihood related activities. Using this sample, we then characterize all cash transfer programs in terms of the cumulative transfer received by sample households, and meta-analyze all treatment effects as the linear effect of $100 of cumulative cash (with or without supplemental cash plus interventions) on the specified outcome of interest using standard random effects and multilevel meta-analysis models. Consumption and income are specified as monthly household aggregates, and all monetary estimates are converted to 2010 U.S. dollars in purchasing power parity terms.[1]

The sample of studies and programs represented encompasses a huge range of cash transfer programs implemented around the world (Mexico is the most commonly represented country, but sub-Saharan Africa the most commonly represented region) and by a range of actors. It includes long-running and widely known government transfer programs such as PROGRESA and much smaller, targeted programs implemented by non-governmental organizations. More specifically, the sample of programs analyzed is characterized by a median cumulative transfer of $850 in 2010 PPP; for a plurality of transfers (64%), this cumulative amount was disbursed monthly or bi-monthly, with lump-sum transfer structures also common (21%). The median duration of the intervention in our sample (inclusive of cash disbursements and any cash plus programming) is 19 months, though there is wide heterogeneity. In terms of the duration of follow-up, 53% of the evaluations are reporting on cash transfer programs that are ongoing at endline, and within the subsample of those reporting on cash transfers that are not ongoing, the median period observed following the final transfer is 13 months, suggesting the majority of these estimates are capturing short-term effects.[2]

Our primary findings suggest that cash transfers lead to an increase of around $1.50 to $2 in monthly household consumption and income per $100 transferred, at a median follow-up

[1]Much of our analytical strategy here follows Kondylis and Loeser (2021) and Crosta et al. (2024), and we describe how our meta-analysis builds on these papers and contributes to the broader literature in further detail below.

[2]For papers that reported multiple rounds of data, we extracted effect estimates from the final round, implying we already are capturing the upper bound of the follow-up period.

point of 24 months following the first transfer (or five months following the last transfer, with evaluations implemented for ongoing programs coded as zero months following the last transfer). We find no economically meaningful effect (positive or negative) of cash transfers on labor force participation for a pooled sample or samples of men and women. Moreover, we find no meaningful difference in the results between programs that were ongoing at the point of follow-up measurement and those that had already concluded. This suggests no pronounced decay in cash effects within the relatively short follow-up periods generally observed in this set of estimated effects. [3]

When we examine variation across transfer types, we find that cash plus livelihoods interventions are, on average, more effective, yielding an additional marginal effect of $4 - $5 per $100 transferred relative to the estimated cash-only effect. We find no significant difference in the effects between unconditional and conditional cash transfers. Lump sum transfers generally have a smaller effect on consumption, as do programs implemented by governments, though these patterns are not fully consistent. We also analyze variation in effects by context. In general, the effects of cash transfers are somewhat smaller in sub-Saharan Africa, the most common site of studies included in this review. There is no evidence of any differential effects in contexts that are fragile and/or conflict-affected (using the World Bank definition); however, the body of evidence is extremely limited, as only 14 studies including 42 intervention arms (and only four cash plus arms) were evaluated in a fragile setting. This highlights a clear evidence gap.

For evaluation methods, we cannot reject the null that randomized controlled trials (RCTs) yield the same effect sizes as other evaluation methods. However, studies that use regression discontinuity (RD) designs consistently report smaller effect sizes, presumably reflecting the fact that RD methods quantify a local average treatment effect around the program eligibility threshold that may not be applicable to all eligible households, many of which are considerably poorer.

To contextualize our estimates in terms of the magnitude of the estimated effects on consumption, we note that the structure of our analysis estimates the effect of a cumulative stock of cash transfers (total amount transferred) on a flow measure of consumption (per household per month); accordingly, the key question is how long these consumption effects persist. While the structure of this analysis does not allow us to directly estimate this time horizon, we can draw on general stylized facts: the average program duration is 19

[3]We cannot provide any systematic evidence around the longer-term persistence of cash effects, though we discuss the implications of some different assumptions around persistence in the Discussion section.

months, and the average measured post-program follow-up period (conditional on an evaluation following program completion) is 13 months, with little evidence that effects fluctuate or attenuate during this period. Accordingly, we estimate that the program effects persist for roughly 2.75 years, implying a total consumption effect in the range of $50 per $100 transferred.

We also explore a range of other scenarios with alternate assumptions around the longer-term persistence of effects, discounting of these longer-term effects, and variation in the main treatment effect estimates. In general, this analysis suggests cumulative effects between $40 and $70 per $100 transferred for modalities that are cash only (UCTs, CCTs) or primarily cash (cash plus a non-livelihoods intervention).[4] We discuss a range of reasons why the effect of transfers on consumption appears to be other than one-to-one: implementation errors leading to true transfer receipt being lower than estimated transfer receipt; household use of transfers for investments with low returns or returns observed only in the medium-term; and transfers directed to expenditure categories not typically captured in consumption aggregates. Re-estimating the same analysis for a cumulative effect of $100 cash transfers delivered as part of cash plus livelihoods programming suggests a meaningfully higher cumulative effect — from a range of $80 to $100 up to $500 — but these more optimistic impact estimates must also be weighed against considerably higher costs for cash plus programming. While these costs are often not reported, the limited estimates available suggest that the costs of some more intensive cash plus programming may be comparable to, or higher than, the estimated cumulative benefits.

Finally, we use methods from both health and economics (Egger et al., 1997; Brodeur, Cook and Heyes, 2020) to assess non-reporting (publication) bias in the sample of studies analyzed. We find substantial evidence of publication bias of equal magnitude in both the RCT and non-RCT literature, suggesting that there may be some meaningful risk of overestimating the effects of cash on consumption and income if noisily estimated null and negative effects are missing from the sample.

Our research builds on and contributes to a previous meta-analytic literature aggregating the impact of cash transfers on livelihood outcomes (as summarized in Table A1).[5] Our

[4]These aggregated estimates are somewhat lower than those reported in the context of PROGRESA in Mexico. Gertler, Martinez and Rubio-Codina (2012) estimate that the average PROGRESA beneficiary household consumes 74 percent of the transfer and invests the remainder.

[5]Another growing body of research evaluates the impact of cash transfers on non-economic outcomes. Meta-analyses of these effects have aggregated the impact of cash transfers on health outcomes, such as children's nutritional outcomes (Manley et al., 2020; Manley, Alderman and Gentilini, 2022; Little et al., 2021), mental health (McGuire, Kaiser and Bach-Mortensen, 2022; Wollburg et al., 2023), and neglected

study is closest to Kondylis and Loeser, 2021 who conduct a meta-analysis of randomized controlled trials of temporary UCTs and contrast their impacts vis-a-vis "Targeting the Ultra Poor" (TUP) programs, multifaceted graduation programs combining large asset transfers with intensive livelihood training.[6] A second comprehensive recent meta-analysis of RCTs evaluating UCT programs only is Crosta et al., 2024, a paper that reports on a wide-range of outcomes, including consumption, income, assets, labor force participation, child health, schooling and mental health, but does not consider cash plus programs.

By contrast, the scope of our meta-analysis is broader and the focus is somewhat different, offering a number of contributions to the existing meta-analytic literature. First, our definition of cash-plus is not restricted to the most intensive and successful graduation programs. Second, we consider both UCT and CCT programs and do not limit the evaluation methodology to RCTs only. In our sample, over half of the UCT interventions occurred in sub-Saharan Africa, whereas only 16 percent of the CCT programs were implemented there. Consequently, a focus on UCTs implies a sample concentrated in sub-Saharan Africa, without drawing on evidence from many long-running large-scale CCT programs in Latin America (Fiszbein and Schady, 2009). Moreover, the focus on RCTs implies a sample that overrepresents NGO-implemented programs, as many government programs are not randomized and thus the evaluations necessarily employ other designs.

In particular, the decision to limit the sample to RCTs presents an important trade-off. Evaluations of government-implemented programs generally report smaller effects compared to those of NGO-implemented programs (Vivalt, 2020) – possibly because they typically operate at scale, making them more challenging to implement effectively (Muralidharan and Niehaus, 2017). However, our risk-of-bias assessment reveals that, as expected, non-RCTs typically have a higher risk of bias, on average, than RCTs. To address this trade-off, we will explore sensitivity of our main findings to the inclusion or exclusion of non-RCT designs. Finally, we assess the degree of publication bias in this literature, which constitutes another unique contribution relative to the earlier meta-analyses in this space.

tropical diseases (Ahmed et al., 2022). Similarly, other studies have conducted meta-analyses of the impact of cash transfers on educational attainment (Baird et al., 2013; García and Saavedra, 2017), temptation goods, such as alcohol or tobacco (Evans and Popova, 2017), and intimate partner violence (Baranov et al., 2021).

[6]Correa et al., 2023 also contrast cash only interventions against cash plus interventions in their meta-analysis, but focus on programs implemented in rural Africa and consider a different set of outcomes such as asset accumulation and labor allocation. Their meta-analysis is also considerably smaller in terms of scope, covering 16 cash only interventions and 12 cash plus (including graduation programs) interventions.

2 Methods

2.1 Search strategy and selection criteria

The inclusion criteria for this review included the following: we aimed to aggregate experimental or quasi-experimental quantitative analyses probing the effects of cash or cash plus programming on livelihoods outcomes in low- and middle-income countries relative to a comparison group that does not receive cash.[7] An experimental or quasi-experimental design is one including a plausible counterfactual design: in practice, this encompasses randomized controlled trials, regression discontinuity analyses, and difference-in-differences and matching designs. Following our initial overview of the literature, we defined livelihoods outcomes to include consumption (total consumption, food consumption, or non-food consumption); income; and labor force participation. Consumption and income will ultimately be defined and analyzed at the household level; labor force participation is defined at the individual level, and will be analyzed for pooled samples of adults as well as for men and women separately.

Interventions are eligible only if they include at least some disbursement of cash (in any installment or form). Thus interventions that provide only asset, food, or other in-kind transfers are not eligible. Public works interventions programs in which cash is provided in exchange for work (e.g., MGNREGA in India or the Productive Safety Net Program in Ethiopia, or other similar interventions) are likewise ineligible. Any multifaceted intervention including additional programming that supplements cash (including but not limited to training, behavioral change communication, and asset or food transfers) is eligible. Some trials may include multiple arms in which some meet the eligibility criteria for this review and some do not (e.g., a comparison of cash and food transfers vis-a-vis a control arm); in that case, we retain and analyze data only from the eligible arms.[8]

With respect to the experimental design, the review criteria entails the exclusion of papers that are purely qualitative; papers that report on small-scale pilots (e.g., with fewer than 200 observations); analyses that do not compare outcomes using a plausible control or comparison group; and papers that do not include a control or comparison group with minimal programming (and no access to cash). Thus trials that include multiple arms in

[7] We follow the World Bank classification of countries as low- or middle-income, and use the classification from the year in which a particular study was launched. Thus studies from Chile and Uruguay, both now categorized as high-income countries, remain eligible since they were conducted prior to the year (2013) in which both countries were re-designated as high-income.

[8] The reason for this exclusion is that we do not have a systematic sample of papers analyzing, for example, food transfers, and thus any aggregation of evidence from the limited selection of arms analyzing food transfers observed in trials that also analyze cash transfers will inevitably be of limited value.

which all arms receive cash, with some variation in modality or supplemental programming, are ineligible. Trials in which there is a delayed phase-in of cash and the control arm corresponds to recipients who will receive cash following the endline assessment are eligible. In addition, the criteria exclude papers that do not report at least one of the outcomes of interest for a general sample; thus papers that report the effects of cash transfers only on educational, nutritional, or human capital outcomes, or on livelihoods outcomes other than those identified here, are excluded.[9]

We identified papers eligible for the review by conducting a structured search in multiple databases: Web of Science, ScienceDirect, EconLit, and the 3ie database of evaluations, and Google scholar. Our search strategy entailed various combinations of terms capturing the interventions of interest (cash transfer, cash grant, UCT, etc.); the experimental designs of interest (RCT, regression discontinuity, etc.); and, when feasible, the outcomes and contexts of interest. The detailed search strategy is provided in Table A2 in the Appendix. The references generated by all database searches (10,726 total records) were compiled in Rayyan, where duplicate-detection, keyword-based screening and title and abstract screening were conducted by SZ (see appendix for details). We then added papers from our Google Scholar search and also added some papers manually by cross-checking the papers cited in three other recent systematic reviews (Correa et al., 2023; Crosta et al., 2024; Kondylis and Loeser, 2021).[10] Full-text screening was conducted by KH and SZ, with JL double-screening a random subsample. Any discrepancies were resolved by discussion.

2.2 Data extraction and cleaning

We conducted the meta-analysis based on study reports and did not draw on individual participant data. We extracted data using a customized form, including the study design; descriptive data on the intervention, including the types of any cash plus programming implemented; the timeline (duration of the intervention's implementation and its timing vis-a-vis any survey assessments): the setting (country, region, urban/rural, and whether the context is fragile or conflict-affected); the sample and type of data collection; and the estimated effects for all relevant outcomes reported, as well as standard errors or confidence intervals.

Given our interest in identifying fragile and conflict-affected settings in this analysis, we

[9] By general sample, we refer to a broad sample of eligible households or adults; there is also a substantial literature that analyzes the effects of cash transfers on the labor supply of children or adolescents, and those papers are likewise excluded here.

[10] We reviewed the first approximately 150 records identified in Google Scholar to assess their inclusion.

draw on the World Bank classification at the country level, reported by year back to the year 2006. We code a particular study as fragile or conflict-affected if the country site was identified as such in the baseline year of the study.[11] We also supplement this by manually identifying studies that are conducted with refugee samples, given the inherent fragility experienced by these samples; or that motivate their study around themes of conflict, as identified by the use of the word conflict in the title or abstract.

We assessed studies' risk of bias at the study level; for randomized controlled trials, we used the Cochrane risk-of-bias tool (Higgins et al., 2019), and for quasi-experimental studies, we used the JBI [12] risk-of-bias tool (Barker et al., 2024). For RCTs, the five domains assessed are the randomization process, deviations from intended interventions, missing outcome data, measurement of the outcome, and selection of the reported result. For non-RCTs, the eight domains assessed are clarity about cause and effect, similar pre-intervention, environment similar over time, sufficient pre- and post- measurements, complete follow-up, consistent measurement, appropriately powered, and tests of no violations of assumptions. Using information about risk of bias, we also generate a variable corresponding to high risk of bias if at least three domains are characterized by some or high risk of bias (out of five domains, for an experimental study, or out of eight domains, for a non-experimental study). Note the higher (proportional) threshold for risk of bias for experimental studies is also defined in light of the fact that it is extremely common for experimental studies to be characterized by high risk of bias in the fifth domain, corresponding to selective reporting, given that a large number of these studies were launched prior to the widespread adoption of preregistration or pre-analysis plans for trials in economics.[13]

If results were reported for multiple follow-up periods, the estimated treatment effects for the longest follow-up period were extracted. If results for multiple specifications were reported, we extracted the specification that was identified by the authors as of primary interest or the most robust specification; or, if this was not clearly designated in the text, we used our own judgment to identify a preferred or more robust specification. If results for specifications with and without additional covariate controls were reported, we preferentially

[11]A number of studies in the sample had a baseline year prior to 2006; these studies are all in contexts that were never, at any point, identified as fragile or conflict-affected (e.g., Mexico). Accordingly, we feel we can confidently code these settings as non-fragile.

[12]Formerly known as the Joanna Briggs Institute, JBI is an international research organisation based at the University of Adelaide in Australia.

[13]For some quasi-experimental designs, particularly regression discontinuity, three domains pertaining to variation and trends over time are not applicable, and thus the relevant criteria is at least three domains out of six are characterized by some or high risk of bias.

extract the results estimated without additional controls; however, in practice the majority of effect estimates correspond to effects estimated conditional on same baseline controls. When relevant data were not available in published study reports, JL contacted study authors up to two times to request missing information.

We also focus on extracting intent-to-treat effects. While some recent meta-analyses of cash transfers (Kondylis and Loeser, 2021) and educational interventions (Angrist and Meager, 2023) have emphasized the value of meta-analyzing treatment on the treated effects, cash transfers (and closely related interventions such as cash for public works employment) are an intervention for which there is robust evidence of local spillovers, in some cases positive (Egger et al., 2022; Angelucci, Attanasio and Di Maro, 2012; Franklin et al., 2021; Muralidharan, Niehaus and Sukhtankar, 2023) and in some cases negative (Beegle, Galasso and Goldberg, 2017; Haushofer and Shapiro, 2016; Filmer et al., 2021). Accordingly, estimating a treatment on the treated effect for cash transfers is not possible without substantial additional assumptions around the structure of spillover effects; and meta-analyzing treatment-on-the-treated effects is infeasible.

Our primary effect measure for this analysis is the estimated coefficient β from a linear regression capturing the effects of cash on outcomes of interest. First, we normalized all coefficients for continuous variables (consumption and income) to correspond to a linear estimate of the effect of a transfer on a monthly variable at the household level. Estimates reported as normalized coefficients (in standard deviations) or in logs were converted to linear coefficients using data provided about the mean and standard deviation of the outcome of interest in the control arm. Second, we converted estimates reported per capita or per adult equivalent to estimates at the household level using the reported household size, and estimates reported for other time periods (daily, weekly or yearly consumption or income) were converted to monthly. Third, all coefficient estimates were converted to 2010 purchasing power parity-adjusted U.S. dollars. For some papers, additional assumptions were required to execute these calculations; and in some cases, essential information was simply not available and thus the paper could not be included in the meta-analysis (though it is still documented in the systematic review). More information about data cleaning and harmonization is provided in Section A2 in the Appendix.

For the binary variables corresponding to labor force participation, the only required cleaning was to convert standardized effect estimates or effect estimates generated using probit or other non-linear models into coefficients corresponding to linear models.

A parallel process was implemented to generate estimates of transfer size, with the objective of estimating the full value of the transfer received by households in the analytical sample during the analytical period of interest (generally, between two rounds of assessment).[14] In cases where transfers varied in amount based on different household criteria — typically, demographic criteria such as the number of children or other socioeconomic characteristics — we preferentially used estimates provided in the paper text of the average transfer received by sample households. If no such estimate was available, we attempted to construct one using other available data (i.e., average household size or typical number of children); and for conditional cash transfers, these estimates were constructed assuming that the payment conditions were satisfied. The estimates of transfer size were again converted to 2010 U.S. dollar purchasing power parity estimates.

The final step entailed the construction of normalized coefficients capturing the effect per dollar of transfer. For the purposes of exposition, we will present these effects multiplied by 100, or the effect of a (cumulative) $100 transfer. We calculate cumulative transfer values given the wide and complex variation in transfer structure, and will analyze effects on monthly household consumption and income given that these are standard economic measures of interest in the literature; this does imply that we are effectively analyzing the effect of an accumulated stock of cash transfers on a flow measure of monthly household consumption. This is consistent with other recent work in the cash meta-analysis literature (Crosta et al., 2024; Kondylis and Loeser, 2021), but we also discuss in some detail in Section 4.1 below how to re-interpret our findings as a causal effect on a cumulative measure of consumption.

2.3 Meta-analysis estimation

First, we estimate a random-effects meta-analysis model using the restricted maximum likelihood method; we estimate this model separately for each outcome of interest, employing all available coefficients (from all relevant arms) from each trial, and present the results in forest plots.[15] We assess between-study heterogeneity using the I^2 statistics. Given that there are some effect estimates that are extremely large, we also present meta-analysis estimates that exclude outliers, defined as effect estimates above the 97th percentile.

[14] Thus for transfer programs that may have been ongoing, the aim was to estimate how much the average household would have received by endline, since baseline or since the inception of their eligibility. However, information about the timeline of cash receipt and whether or not a transfer program was ongoing was often imprecisely specified.

[15] The assumptions for a fixed-level meta-analysis (that all studies shared a common effect size, and factors influencing this size are consistent across studies) does not plausibly hold in this context.

Second, we estimate a multi-level (three-level) random-effects model that accounts for the dependence across multiple treatment effects estimated for the same program (Moeyaert et al., 2017; Van den Noortgate et al., 2015). We also estimate this model using two different strategies. In our first and preferred strategy, we seek to link treatment effect estimates generated by evaluation of the same program in the same country. Accordingly, if a paper reports estimates of multiple different programs in different countries in a single analysis, we now code the analysis implemented in each country as a separate trial; and conversely, we code all estimates of the impacts of (for example) PROGRESA as part of an evaluation of a single program, to capture the presumed dependence across effects. In the second analysis conducted as a robustness check, we simply use published papers to define trials: thus a published paper including multiple estimates from different countries is coded as one trial, and conversely, two separate papers reporting analyses of the same intervention (i.e., Progresa or another frequently-analyzed cash transfer program) are coded as two separate trials.[16]

We then present meta-regressions that analyze how characteristics of the context, transfer, and study moderate the observed effect sizes. We will estimate meta-regressions only for the outcomes of total consumption and food consumption, as these are the outcomes for which the sample size is largest. The characteristics of interest include two binary variables capturing the context (a binary variable equal to one for sub-Saharan Africa, and a binary variable equal to one for a fragile or conflict-affected context); four variables capturing characteristics of the transfer (a binary variable equal to one if the transfer is implemented by government, a binary variable equal to one for a conditional cash transfer, a binary variable equal to one for a lump-sum or near-lump-sum transfer, and a binary variable equal to one for a cash plus livelihoods program); and two variables capturing the study design (a binary variable equal to one for a regression discontinuity design, and a binary variable equal to one for a randomized controlled trial).[17] We again estimate meta-regressions using both the

[16]To cite concrete examples, Banerjee et al. (2015) reports the results of a graduation model intervention evaluated in six countries; in our first analysis, this is treated as a series of separate trials, and in the second analysis, these estimates are linked as part of the study. Conversely, there are three papers in our sample that report effect estimates of the Child Support Grant in South Africa (Mostert and Vall Castello, 2020; Scarlato and d'Agostino, 2019; d'Agostino, Scarlato and Napolitano, 2018); in our first analysis, these estimates are treated as part of the same trial, and in the second, they are treated as independent estimates.

[17]A lump sum transfer is defined as a transfer with three or fewer installments in a short period of time, roughly six to nine months. This is broadly similar to the definition employed by Crosta et al. (2024), who define a lump sum transfer as no more than three installments within no more than two months; however, we slightly lengthen the timeline to encompass the structure of GiveDirectly transfer programs. These transfers are generally defined as lump-sum, but are also inclusive of a first "token" transfer that is separated from the final transfer by up to eight months (Egger et al., 2022); there are also some lump sum cash transfer

simple random effects and multilevel model.

3 Results

3.1 Sample and descriptive statistics

The screening process included an initial review of 10,726 records; Figure 1 summarizes the screening steps. Full-text screening was conducted on 381 separate records, and this ultimately yielded a sample of 104 studies for inclusion in the meta-analysis, including 155 different treatment arms and 310 treatment effects for the variables of interest. The largest samples for specific indicators are for total consumption and food consumption, reported in 68 and 67 treatment arms, respectively. Details are summarized in Table 1. There are seven studies for which insufficient information about the econometric analysis or the transfer was provided in the paper (and no supplementary information was provided by the authors), and these studies are excluded from the meta-analysis; more information is provided in Table A3 in the Appendix. Table A4 summarizes all of the studies included in the systematic review.

Figure 2 summarizes the geographic distribution of the evidence included in the systematic review,[18] drawn from 43 unique countries. Mexico is the most commonly represented site, included in 12 studies, followed by Kenya (eight), Uganda (eight), and Malawi (seven). Figure 3 summarizes how the geographic composition of evidence has shifted over time, with each study coded with respect to its baseline year. The earlier body of evidence (studies characterized by a baseline year prior to 2010) was overwhelmingly dominated by Latin America, but this has rapidly shifted to a literature increasingly dominated by evidence from sub-Saharan Africa.

Evidence in fragile or conflict-affected settings, however, is quite limited. There are nine studies identified as such based on the core World Bank coding, and an additional four identified using a sample of refugees (two) or a motivation around conflict (three).[19] The

programs in our sample where the final transfer was somewhat delayed by the pandemic.

[18]Note that there are some multicountry studies in the sample, and for those studies, each site is mapped separately; this leads to a total of 122 country sites represented.

[19]The nine studies include one conducted in Afghanistan (Kashefi and Naito, 2023), one in Cote d'Ivoire (Marguerie and Premand, 2023), three in Lebanon surveying Syrian refugees (Salti et al., 2022; Altındağ and O'Connell, 2023; de Hoop et al., 2018), one in Myanmar (Maffioli et al., 2023), one in Sierra Leone (Rosas, Acevedo and Zaldivar, 2022), one in Somalia (Ali et al., 2022), and one in Yemen (Kurdi, 2021). The studies including refugee populations were conducted in Uganda (Gupta et al., 2024) and Colombia (Hidrobo et al., 2014) and three papers analyze a cash transfer implemented in a conflict-affected region of Uganda and explicitly motivated with reference to the salience of local conflict (Blattman, Fiala and Martinez, 2014; Calderone, 2017; Blattman, Fiala and Martinez, 2020).

full sample of 14 studies in sites identified as fragile or conflict-affected includes seven in sub-Saharan Africa, two in Asia, four in the Middle East, and one in Latin America.

Figure 4 then summarizes the 155 separate interventions analyzed, showing how the composition of interventions analyzed has shifted over time (again, when each study is indexed by its baseline year). Our primary classification identifies transfers as a conditional cash transfer or unconditional cash transfer, implemented either in isolation or in conjunction with a supplemental intervention that is focused on enhancing livelihoods outcomes or a supplemental intervention that has a different, non-livelihoods-related objective.[20] The full sample includes 79 UCTs, representing a slight majority of the interventions evaluated; 38 CCTs; 13 UCTs implemented in conjunction with livelihoods-focused interventions; 13 CCTs with livelihoods-focused interventions, nine UCTs with other interventions; and two CCTs with other interventions; as well as one intervention not easily categorized.[21] The early body of evidence is dominated by conditional cash transfers (as noted above, largely implemented in Latin America), with the evidence around unconditional cash transfers growing sharply after 2005 and becoming the most common form of evidence beginning in 2010.

Figure 5 then summarizes the incidence of various intervention types by region: conditional cash transfers clearly dominate in Latin America, while unconditional cash transfers dominate in sub-Saharan Africa. Within the sample of studies conducted in fragile and conflict-affected contexts, there are 14 interventions implemented and they are again dominated by unconditional cash transfers (12). Approximately two-thirds of the interventions were primarily implemented by governments and one-third by another implementer, typically an NGO (as summarized in Figure 6). All but one CCT intervention were implemented by governments, while about half of the UCTs were implemented by governments and the other half by NGOs. Cash plus programs were more likely to have been implemented by an NGO.

Figures 7 and 8 summarize the primary characteristics of transfers in terms of their periodicity and duration. A majority of transfers (63%) are structured to be monthly or bimonthly (the distinction between these two is not always clearly delineated), though lump-sum transfers are also common (23%). The median number of payment tranches received by

[20] A typical example of a livelihoods-focused supplemental intervention would be training or the formation of savings groups; a typical example of a non-livelihoods focused supplemental intervention would be information or behavioral change communication related to nutrition or health.

[21] Azeem, Mugera and Schilizzi (2019) conduct a pooled analysis of the effects of social protection in Pakistan, including households / individuals participating in a range of different programs with different characteristics and different criteria. It is not possible to categorize the transfer type here; nor is it possible to estimate the amount of the typical transfer. Accordingly, this paper is excluded from the meta-analysis.

recipients is 10, and the median duration of the intervention (during which these tranches are disbursed and any supplementary interventions conducted) is 19 months, with wide heterogeneity (the 25th percentile is six months and the 75th percentile 24 months, with minimum and maximum of one and 174 months, respectively). The median time elapsed between the first payment and the period in which outcome data is collected or measured is 24 months (summarized in Figure A1, implying then a relatively short median gap (five months) between the final payment and outcome measurement: in other words, this sample is generally capturing short-term effects, and in fact 53% of the evaluations are reporting on cash transfer programs that are ongoing at endline.[22]

Over the intervention period, the median total transfer amount is $850 in 2010 PPP terms (25th percentile of $424 and 75th percentile of $1495); the distribution is also characterized by substantial extreme values, and thus we present histograms with and without transfer amounts above the 95th percentile of the distribution in Figure 9. The median amount per tranche is around $90 (25th percentile $35 and 75th percentile $258), and again we present figures with and without extreme total transfer values. Figure A2a and A2b in the Appendix further summarize the average magnitude of transfers by type. Here, it is evident that interventions including a cash plus intervention focused on enhancing livelihoods are generally characterized by total transfer amounts that are meaningfully smaller in comparison to transfers that do not have this characteristic: the mean transfer amount is less than half the mean transfer amount for simple cash transfers, and the gap widens further if lump-sum transfers are excluded. This difference primarily reflects the inclusion in the "cash only" category of a number of long-term social protection programs such as PROGRESA in Mexico or the child grant in South Africa, where households can receive ongoing support for many years and thus the estimated total transfer is substantial.

Moving on to the empirical designs employed in this literature, Figure 10 summarizes the study types in the sample. Within the 104 studies included, there are 45 cluster-randomized and 11 individually randomized trials, 16 studies implemented using a regression discontinuity, eight implemented using propensity score matching, and 20 using difference-in-differences designs. (Four have other designs or report on multiple interventions using different designs.) Figure 10a summarizes the composition of study designs over time, using data at the level of the intervention; while cluster-randomized trials were virtually always the most common

[22]This estimate should, however, be generously caveated: it is surprisingly common for papers to fail to note explicitly whether the households included in the sample are in fact continuing to receive the transfer of interest at endline (a point that is related to, but distinct from, the question of whether the program is ongoing).

form of evaluation, their incidence also begins to sharply increase around 2010.[23] Figure 10b summarizes the evaluation design according to implementer type: just over half (51%) of the government implemented interventions were evaluated using an RCT design, while the corresponding share for NGO implemented interventions was 76%.

Figure 11 then summarizes our overall assessment of study risk of bias. Within the non-RCT sample, we assess 27 of 48 studies (a majority) to be characterized by high risk of bias, a judgment driven by the studies using difference-in-differences and propensity score matching designs. Within the RCT sample, by contrast, we assess 46 of 66 studies to be characterized by low risk of bias. Figures A3a and A3b summarize the number of studies characterized by bias in each domain for both quasi-experimental and experimental studies. For quasi-experimental studies, the domains characterized by highest risk of bias were tests of no violations of assumptions (11 studies identified as characterized by high risk of bias) and similar pre-intervention (six studies). For experimental studies, as previously noted domain five was characterized by an unusually high number of studies (30) identified as characterized by high risk of bias in this domain particularly (not across all domains) due to the absence of a preregistration or pre-analysis plan; many of these studies were launched prior to the widespread adoption of these tools in economics. Outside of this domain, there was no other in which there were a significant number of randomized controlled trials identified as characterized by high risk of bias.

Within the sample of 104 studies, 86 or 83% are published journal articles; the remaining 17% are working papers or technical reports.[24]

3.2 Meta-analysis findings

The main findings from the meta-analysis are presented in Table 2 and the corresponding forestplots are presented in Table A4 through A14. In general, we label each study using the author-date citation and a notation as to the type of intervention (UCT, CCT, UCT+ Livelihoods, CCT+ Livelihoods, UCT+ Other, and CCT+ Other). For papers that include multiple treatment arms with the same intervention type, we add additional information to distinguish the arms that pertains either to the location in a multicountry or multisite

[23] Summarizing this data at the intervention level allows us to capture the cases in which one study includes multiple study designs, and to also capture the relative volume of evidence — in terms of effect estimates — generated in studies of different types.

[24] In some cases, the automated search strategy generated a working paper instead of or in addition to a peer-reviewed publication reporting the same results; in those cases, we always privilege the peer-reviewed publication and exclude the working paper.

study; the relative size of the transfer (small, medium, large, etc.); or, if there are multiple arms differentiated only by the specific type of cash plus intervention, for example, they are labeled with numbers.

For total consumption (Figures A4 and A5), the pooled coefficient estimate suggests that a \$100 transfer leads to an increase of \$1.99 in monthly household total consumption, or \$1.62 when outliers are excluded. For food consumption (Figures A6 and A7), the comparable estimates are \$2.13 and \$1.76, respectively; and for non-food consumption (Figures A8 and A9), the pooled effect estimates are \$13.55 and \$0.61. Note that the samples for all three indicators are somewhat different, accounting for the larger effect observed on food consumption; and as the sample of effect estimates is considerably smaller for non-food consumption, a small number of outliers are considerably more influential. For income (Figures A11 and A11) the impact estimates are \$2.13 with outliers and \$1.50 without outliers. Overall, this evidence is fairly consistent and suggests a reasonable inference of an effect of around \$1.50 on consumption or income per one hundred dollars of transfer, with the majority of this effect driven by increased food consumption. We interpret the magnitudes of these effects in the next section.

For labor force participation (Figures A12 and A13), the pooled coefficient both with and without outliers suggests a positive, but tiny effect on labor force participation: an \$100 transfer is associated with a .00001 increase in the probability of working; despite its tiny magnitude, this effect is significantly different from zero given the precision generated by a large number of estimates included. Despite its statistical significance, however, this effect is clearly far too small to be economically meaningful. This is contrast to both an earlier hypothesis that cash transfers might serve to reduce labor supply or economic activity, a hypothesis largely disproven in earlier literature (Handa et al., 2018*b*; Banerjee et al., 2017), and to the evidence provided in Crosta et al. (2024) of a positive effect of transfer on labor supply, a finding that we do not observe in this sample.[25] Figure A14 shows the estimated effects for labor force participation estimated for samples of men and women separately, and here the effects are weakly negative and statistically insignificant. Note that we cannot rule out a response on the intensive margin (number of hours or days worked), as we do not analyze this outcome.

Note that for all meta-analysis estimates, the I^2 is high (100%); I^2 describes the percentage of the variability in effect estimates that is due to heterogeneity rather than random

[25] We also do not observe this effect in the subset of randomized trials of unconditional cash transfers in our sample.

variation in the sample (Cumpston et al., 2019). This is a pattern of heterogeneity broadly consistent with (though generally even higher than) the findings in other recent meta-analyses of cash transfers (Correa et al., 2023; Hidrobo et al., 2014; Manley, Alderman and Gentilini, 2022; Baird et al., 2013).

To probe the robustness of these results, we then estimate a series of multilevel meta-analyses that allow us to fully account for the hierarchical structure of the data, including multiple coefficients from evaluations of the same program in the same country. (We also uniformly exclude outliers from this and all subsequent analysis.) Here, we find largely consistent effects, though somewhat attenuated for income measures: \$2.21 for total consumption, \$2.39 for food consumption, and \$1.01 for income.[26] We then analyze findings from the meta-regression, where as previously noted, we present estimates only for the total consumption and food consumption outcomes, but use both the simple model and both alternate multilevel models. The results are presented in Table 3.

In terms of the effects of intervention context, in general the pattern of evidence is not consistent across specifications, particularly across estimates based on total consumption versus food consumption. There is some weak evidence that the effects of cash transfers may be larger in fragile and conflict-affected areas but this pattern is variable and is statistically significant only at the ten percent level. There is more consistent evidence that the effects are smaller in sub-Saharan Africa, where the coefficients of interest are uniformly negative but vary in magnitude and precision and are statistically significant only for total consumption. The magnitude of the expected decrease in effectiveness is also large and statistically significant, implying that, on average, the expected total consumption effect size is \$1.87 lower in interventions conducted in African countries compared to interventions conducted elsewhere. Considering the adjusted expected effect size of \$6.06 for the hypothetical base category (a non-CCT, non-lump sum, and non-cash plus livelihoods intervention conducted in a non-fragile non-African country, implemented by a non-government entity, and evaluated using non-RCT or RD designs), this translates into a 31 % reduction in expected effect size for interventions taking place in Africa.

If we evaluate the characteristics of the transfer: there is again some mixed evidence of reduced effectiveness of transfers implemented by governments, particularly for total consumption. Again considering the base category, the coefficients imply a decline of 38% and 45%, but do not hold when we look at food consumption. There is no evidence of any differ-

[26] If we estimate the multilevel model accounting only for dependence within multiple arms analyzed within the same paper, again the estimates are relatively stable: \$2.17 for total consumption, \$2.51 for food consumption, and \$0.89 for total income.

ential effects of conditional cash transfers. The same specifications also show a reduced effect of a lump-sum transfer on consumption, consistent with existing evidence in the literature that transfers disbursed as lump sums have reduced effects on consumption, and again the effect is large, around 71–74%. The most consistent pattern is the increased positive effect of cash plus programs, consistently positive and significant and suggesting an increase in the effects of cash transfers between \$2 and \$6, or a 46%–66% increase in the expected effect size.

The final two variables capture the study design. Here, there is no evidence of any differential effect size in studies conducted as randomized controlled trials in comparison to the omitted category (dif-in-dif and matching designs). There, however, is a consistent pattern of reduced effect sizes in regression discontinuity studies, consistent with the general intuition that RD designs capture a local treatment effect around the discontinuity exploited in the RD, and the effects of transfers are thus considerably smaller for that less-poor sample close to the discontinuity. Compared to the mean effect size in the base category, the expected impacts trend toward zero in evaluations based on an RD design. This also suggests that, in general, meta-analyses drawing only on randomized controlled trials are likely to generate more optimistic estimates around the effects of cash.

To sum up, our analysis suggests that cash transfers lead to precisely estimated positive effects on consumption and income, with the estimated magnitude ranging between \$1.50 and \$2 in monthly income and consumption for each cumulative \$100 transferred. The effect on labor force participation is positive but extremely small and thus not meaningful in economic terms. For the median transfer in the sample (\$850), this implies a total effect on monthly household consumption of around \$13.60; for transfers at the 25th and 75th percentiles (\$424 and \$1495), this implies monthly treatment effects of \$6.70 and \$24.22, respectively. We will discuss the duration of these effects further below (unpacking how long an increase in consumption plausibly persists), but as a simple descriptive point the average duration of a cash transfer observed in our sample is 19 months.

Meta-regression analysis allows us to assess what characteristics are predictive of increases in consumption of varying sizes, though we should note that this variation is subject to different interpretations. A lower effect on consumption could be interpreted as higher leakage (a lower transfer actually received, conditional on the same program target); or reduced returns on any part of the transfer that is saved or invested (such that over time, the additional consumption stemming from that investment is smaller in magnitude). It would also be possible to interpret a reduced effect on consumption as corresponding to a larger increase in savings that has yet to generate a positive return, but will in future, though it is not clear how plausible a hypothesis this is for households living close to subsistence.

With these caveats, the findings suggest that while there is some variation across different contexts, in general the effects of cash and related interventions on consumption and food consumption of cash transfers are meaningfully smaller in sub-Saharan Africa; and meaningfully larger for cash plus programs. The former pattern has not been documented to date in the cash meta-analytic literature, but the latter is broadly consistent with the evidence in Kondylis and Loeser (2021) of larger effects of graduation programs, the primary difference being that our definition of cash plus is broader than only graduation programs.[27]

There is also some weak evidence that the effects on consumption may be smaller for lump-sum transfers, a finding consistent with (Haushofer and Shapiro, 2016), or for transfers implemented by governments, in line with the meta-analysis findings reported by Vivalt (2020). The reduced treatment effects observed for government programs is also consistent with a number of analyses comparing government versus NGO performance in more specific sectors Bold et al. (2018); Cameron, Olivia and Shah (2019); Fitch-Fleischmann and Kresch (2021); Henderson and Lee (2015); Mo et al. (2020); however, it is notable that this effect extends even to the disbursement of cash, an intervention that is in principle relatively easy to implement and scale (Gentilini, 2016, 2022). The estimated effects of cash transfers are also consistently smaller when assessed using a regression discontinuity design.

3.3 Non-reporting bias and missing results

The meta-analytic literature has widely acknowledged the potential for bias when relevant results are not reported, particularly if there is selective non-reporting based on the sign, magnitude, or statistical significance of the estimated effects (Page, Higgins and Sterne, 2019). Our search strategy was designed to be inclusive of findings that are not published in peer-reviewed journals, and working papers or technical reports constitute 17% of the sample; however, some evaluations may have findings that were never disseminated publicly, and those disseminated publicly but without a peer-reviewed publication may be less likely to be identified in a structured search (i.e., if a working paper or presentation is provided only on an author or university website and is not indexed in any broader database.)[28]

[27]Comparing effects across regions is also more feasible given our broader inclusion criteria, since some regions, particularly Latin America, are primarily represented in the non-UCT sample.

[28]We preferentially use the term non-reporting bias rather than publication bias following the existing literature (Page, Higgins and Sterne, 2019); and also given that our sample does include non-published reports.

We follow existing recommendations to assess non-reporting bias using and the associated Egger's test. While the literature has highlighted a number of potential limitations of these tests, including limited power, these challenges are most acute in small meta-analyses (those including fewer than 10 studies) and in meta-analyses using the log odds-ratio as a summary measure (Peters et al., 2006; Furuya-Kanamori et al., 2020; Lin et al., 2018); neither condition applies here. For the analysis of publication bias, we again exclude the studies characterized by outliers.

Figure 12 presents the for the four outcomes characterized by the largest sample: total consumption, food consumption, income, and labor force participation.[29] We observe substantial asymmetry in the in the first three plots consistent of substantial non-reporting bias: effect estimates that are noisily estimated (large standard errors) are entirely concentrated on the right side of the graph where there are large positive treatment effects, while hypothetical noisily estimated and negative treatment effects are entirely absent. Unsurprisingly, in all three cases the Egger's test strongly rejects the null hypothesis of no non-reporting bias ($p < 0.000$ for all three outcomes). Labor force participation (Figure 12d) is somewhat more symmetric, though in this case there is some weak evidence of non-reporting bias of positive coefficients ($p = 0.062$).

For consumption, we can also further assess publication bias in various subsamples: we assess the subsamples of RCTs and non-RCTs, studies characterized by low and high risk of bias, and evaluations of both cash only and cash plus interventions. The are provided in Figure A16 in the Appendix, and suggest there is no evidence that non-reporting bias is restricted to any of these subsamples; on the contrary, there is robust evidence of bias ($p < 0.000$) for all six subsamples.

It is important to note that the observed pattern in which smaller trials (generating more imprecise estimates) report treatment effects that are larger in magnitude could also reflect underlying patterns of heterogeneity (Egger et al., 1997; Terrin et al., 2003). In the medical literature, this can reflect a phenomenon where smaller trials are conducted earlier or conducted with higher-risk subsamples, both associated with larger treatment effects; however, it is not obvious that either hypothesis is plausible here, as the treatment effect of cash has not necessarily evolved over time, and the trials reporting more imprecise treatment effects are not concentrated in particularly poor or fragile contexts. Another

[29]Note we also omit from the graph a wider set of outliers, corresponding to the estimated effect sizes above the 90th percentile; this is implemented in order to render the graph more readable, rather than generating an extremely dense cluster of points around the mean effect size. These studies are still included in the estimation of p-value corresponding to Egger's test.

hypothesis is that the pattern reflects underlying design considerations around statistical power, in which researchers have chosen to conduct smaller trials precisely when large effects are expected: however, the asymmetry is so pronounced that it is challenging to believe that this fully explains the observed pattern. Rather, an alternate hypothesis would be that a larger set of underpowered studies were conducted, generating a more symmetric distribution of imprecisely estimated positive and negative coefficients, but only the former were reported.

We also draw on more recent methods employed by Brodeur, Cook and Heyes (2020) to assess both publication bias and p-hacking in the published empirical economics literature; here, we present simple graphs of the t-statistics for the primary effect estimates extracted in this meta-analysis. (We focus here on the effect estimates for consumption and income and exclude labor force participation, given that there was no meaningful evidence of publication bias in the funnel plot). These graphs are reported in Figure A16a and show evidence of a clear trough to the left of the line indicating significance at the ten percent level (t-statistic of 1.64), suggestive of effect estimates that are either missing or have been p-hacked. Interestingly, the pattern appears almost identical for the subsample of randomized controlled trials as presented in Figure A16b – a finding that is notably different from Brodeur, Cook and Heyes (2020), who find little evidence of bias in a much broader sample of published RCTs. Given that this analysis draws on roughly 200 coefficient estimates (compared to thousands in the preceding analysis conducted by Brodeur, Cook and Heyes (2020)), we generally do not have sufficient precision to conduct the more detailed tests of p-hacking reported by those authors, but the broad pattern is suggestive, and potentially troubling. Insofar as this literature is characterized by biased patterns of non-reporting and p-hacking, the estimates of the effects of cash may be meaningfully overestimated, though the magnitude of this overestimation requires further exploration.

4 Discussion

In this section, we provide some further guidance to assess the magnitude of the primary estimated effects and what they imply for the total effects of cash and the persistence of these effects. We then return to the original conceptual framework to discuss some of the reasons why the direct consumption effects of cash may be lower than the original transfer disbursed; and describe the implications of this analysis for policymakers seeking to design cash transfer programs to close the poverty gap in low- and middle-income countries.

4.1 Benchmarking the total consumption effect

In our main analysis, we meta-analyze a treatment effect that captures the effect of the total transfer disbursed (normalized to $100) on monthly household consumption. As noted above, we are analyzing the effect of a stock on a flow measure of consumption. This implies two obvious follow-up questions: how does the effect of the transfer on consumption evolve over time, and what is the accumulated effect on consumption over the full horizon in which the transfer plausibly shifts consumption. While we can provide only very partial answers to both questions, exploring this evidence may prove very useful.

As previously noted, around half of the evaluations in this sample are analyzing ongoing cash transfer programs. To assess whether estimated treatment effects vary for programs that are ongoing versus those that are completed, we pool both total consumption and food consumption (to increase power) and estimate the treatment effect for ongoing and completed cash transfer programs. Again using the multilevel model that pools multiple treatment effects for the same program, our estimate suggests an effect of $2.54 during the program period and $3.40 during the post-program period.[30] Interpreting this evidence cautiously, we would suggest that the weight of evidence suggests a consistent (i.e., non-decreasing) positive effect of cash transfers during the transfer implementation period and the post period observed in a typical evaluation. Again, the median trial reporting on a non-ongoing program evaluates data collected only 13 months following the last transfer payment, and thus should be interpreted as a relatively short-term follow-up.[31]

As previously noted, if we assume that the treatment effect on household consumption is constant over the program period (median 19 months) and the post-program period (median 13 months), that implies the roughly $1.50-$2 monthly increase in household consumption persists for around 32 months for a total, accumulated effect of consumption in the range of $50-$60. We can then further develop this estimate, varying along four dimensions. First, we vary the main estimated treatment effect; given the previously presented evidence that the main program modality that generated meaningfully different effects was cash plus livelihoods programming, we estimate the separate meta-analytic treatment effect on consumption for all cash modalities excluding cash plus livelihoods ($1.33), and for cash plus livelihoods

[30] If we estimate the corresponding treatment effects separately for total and food consumption, the patterns are distinct: the effect on total consumption appears higher in the post-program period, while the effect on food consumption disappears in the post-program period.

[31] Note that this pattern is in contrast to the evidence presented in (Crosta et al., 2024), who find that the effects of transfers attenuate by around 40% in the post-program period: we can replicate this pattern in the subsample of studies that are reporting on RCTs of UCTs only, but find a somewhat different pattern in our broader sample.

programming ($8.37).[32] We also use as an indicative estimate the pooled effect of cash plus livelihoods excluding treatment effect estimates above the 90th percentile in the full sample (i.e., using a more stringent criteria to identify outliers). Bearing in mind the scope for overestimation driven by publication bias, this third estimate ($1.88) may also be a useful proxy for a lower estimate of the effects of cash plus livelihoods programming that corrects for this potential bias.

Second, we vary the implied program duration during which the cash effects are observed, using the 25th, 50th, and 75th sample percentiles of six, 19, and 24 months respectively (and in all cases assuming, in addition, a 13-month post-program period in which treatment effects are fully consistent, matching the median follow-up period of the evaluations in our sample).[33] Third, we vary the longer-term persistence of these effects: though we cannot directly substantiate in this analysis persistence of cash effects beyond the typical follow-up period, further persistence is possible, and we consider three alternate scenarios: no persistent effects beyond 13 months post-program, effects that decay at an (annual) rate of 75% out to a five year horizon, and effects that decay at an (annual) rate of 50% out to a five year horizon.[34] Fourth, we explore alternate assumptions around discounting: no discounting of future program effects vis-a-vis a discount rate of 5% following the first program year.

The estimates for the total accumulated treatment effect on consumption under these various scenarios are reported in Table A6. Panel A reports the effects for cash only or cash primarily interventions (excluding cash plus livelihoods interventions). There is considerable variation in this panel, but the majority of the estimates are between $40 and $70, and thus the weight of evidence seems to suggest that the accumulated positive effect of a $100 transfer on household consumption is somewhat lower than $100; the only of assumptions that yield an effect similar to the original transfer amount are those postulating high persistence of treatment effects over a long period and no discounting of future treatment effects.

The estimates for the total accumulated treatment effect on consumption for cash plus livelihoods interventions are reported in Panels B and C for the meta-analytic treatment

[32] Both estimates exclude outliers.

[33] This set of assumptions will generate a mechanical pattern in which programs assumed to have a longer duration — conditional on the same total transfer — have larger treatment effects. We do not claim that we have strong evidence for this hypothesis and are simply presenting a range of plausible scenarios consistent with the stylized facts around transfer timing in our sample. In addition, we would not suggest extrapolating further to conclude that the same transfer disbursed in small installments over a period of, e.g., five years, could have the same positive effect on consumption over this much longer period; that would be a very optimistic interpretation, but there is no evidence for that interpretation in our findings.

[34] These alternate scenarios around dissipation of program effects are consistent with the scenarios analyzed in, for example, Bossuroy et al. (2022).

estimate with and without outliers. Here, we see cumulative effects that are meaningfully larger: between \$200 and \$600 in Panel A, and between \$60 and \$100 in Panel B. Accordingly, it seems more plausible in this case that the cumulative effect of a \$100 cash transfer — in conjunction with the livelihoods-oriented supplementary programming — has an effect equal to or greater than \$100.

However, from a cost-effectiveness standpoint it is of course important to note that the interpretation of these findings for cash plus programs is very different. The total programmatic cost of delivering \$100 of cash will be somewhat larger than \$100 given costs of program administration, but in many contexts not dramatically larger. The costs of delivering a cash plus program inclusive of \$100 cash can be considerably higher, though the cost structure of cash plus programs, needless to say, varies greatly, and cost information is rarely available. To cite a limited number of examples, the graduation models in the original Banerjee et al. (2015) paper report costs per beneficiary of between \$1000 and \$2000, including a total transfer value of \$500 to \$1000; a more recent, psychosocial-oriented graduation model implemented in Niger and analyzed in Bossuroy et al. (2022) reports costs per beneficiary household of between \$530 and \$650, all in PPP terms, including \$320 of transfer value. Sedlmayr, Shah and Sulaiman (2020) present detailed cost information for a relatively lighter cash plus intervention encompassing savings groups and training (similar to a graduation model) that has an estimated cost of \$215 per beneficiary household in Uganda (of which around \$80 is cash transfers). In just these three examples, the total cost of the programming inclusive of cash plus is around double the value of the cash transfer itself, implying that for the program to be cost-effective, the effects on consumption of the \$100 transfer plus supplementary intervention should be at least \$200. Generalizing very broadly, the estimates of cumulative consumption benefits derived from this meta-analysis are suggestive of this level of benefits using the higher treatment effect (inclusive of treatment outliers, as reported in Panel B) but not when using the lower treatment effect.

4.2 Understanding the total effects on consumption

In assessing the reasons why the total treatment effect of cash transfers on consumption may plausibly be lower than the original amount transferred, we can consider three potential hypotheses: investments or savings vehicles characterized by low returns; imperfect implementation of transfers; and imperfect measurement of consumption. We discuss each channel in turn, highlighting that each channel is somewhat speculative.

First, as noted in our original conceptual framework, the effects of transfers on consumption encompasses both direct effects (money transferred is consumed immediately) and indirect effects (money transferred is saved or invested, generating some future income stream that is directed toward consumption). Certainly, the pattern of consistent transfer effects both during and after the transfer period is consistent with the presence of the latter channel. However, the fact that the ultimate cumulative effect on consumption is (plausibly, though not certainly) smaller than the original transfer amount would be suggestive of some investments made that have low returns or even negative returns. Examples of this phenomenon could be assets purchased that fully depreciate in value due to adverse shocks (animals dying or equipment destroyed); cash savings physically lost or stolen; or business investments that do not yield meaningful streams of income. It is also possible that there are positive returns that are not yet captured in our time horizon and that manifest in the long-term, though these calculations already allow for some at least partial persistence of positive income effects up to five years.[35]

A second hypothesis is that the amount of the cash transfer actually received by households is lower than what they are postulated to receive: in other words, the $100 intended transfer is attenuated to a lower amount in terms of the realized transfer.[36] In estimating the amount of the transfer in the sample, we do preferentially use amounts reported received by sample households, if available. These estimates would incorporate any incomplete implementation (if eligible households receive no transfer or a lower transfer than designed), but these estimates are rarely available, and simpler reports on coverage (providing an estimate of the number of eligible households who receive transfers) are also not available in all papers.[37]

In general, the analysis assumes that eligible beneficiaries receive the full amount of the transfer they are entitled to given the statutory program structure, and for some cash

[35]For example, Gertler, Martinez and Rubio-Codina (2012) document meaningful positive effects of PROGRESA on household investments also yielding positive effects on income over a time horizon up to nine years (though in this case, households can also continue to receive transfers for this entire period). Clearly, investments generating positive returns over such a long period would imply a very different profile of cumulative benefits; but there is no robust evidence suggesting that such a long period of persistent effects is a typical trajectory.

[36]As noted above, we meta-analyze intent-to-treat effects given that the probability of high spillovers, either positive or negative, for cash transfers renders treatment-on-the treated estimates arguably somewhat implausible.

[37]The appropriate definition of this concept also varies depending on the empirical design: many papers engage in some comparison of households eligible for transfers versus households ineligible for transfers, but some—particularly non-experimental or matching designs—may be focused on a treatment sample that is defined by realized payment status, rather than eligibility, in which case there is little scope for attenuation due to implementation errors.

transfer programs (particularly those implemented by non-governmental organizations and those implemented at a smaller scale), this may be a plausible assumption. For example, Haushofer and Shapiro (2016) reports that around 85% of eligible households receive transfers in programming rolled out by GiveDirectly; Premand and Barry (2022), reports that 98% of target households receive cash transfers in a government cash transfer program in Niger. However, some transfer programs are characterized by much lower coverage (much higher rates of non-compliance): for example, Briaux et al. (2020) reports that in an unconditional cash transfer program implemented in Togo, 58% of target beneficiaries never received any cash. Some long-running government transfer programs such as the child support grant in South Africa have been initially characterized by relatively low coverage rates that have shifted over time (Case, Hosegood and Lund, 2005; Heinrich and Brill, 2015). Quantitatively summarizing this evidence is extremely challenging, but it is clear that at least in some contexts, low coverage rates for eligible households imply that our constructed estimate of transfer magnitude may be an overestimate.

A third hypothesis is that the effects on consumption, broadly defined, are in fact larger than those captured in typical evaluations of cash transfer effects and that some key categories of cash transfer expenditure are missing. One potential category is expenditure on housing: this is an extremely popular form of investment, particularly for larger lump-sum transfers (see for example Haushofer and Shapiro (2016)). However, its inclusion in consumption aggregates is problematic, requiring analysts to consider the flow of utility the household receives from occupying its dwelling within a given reference period instead of the total purchase value in the same period (Deaton and Zaidi, 2002). In practice, this requires using actual or imputed rents, but there is no consistent approach to imputing rents for non-rental households and, as a result, housing-related expenditures are often not considered at all in standard consumption aggregates (Mancini and Vecchi, 2022).

A second potential category is transfers to other households, particularly if cash transfers are incorporated into systems of informal insurance. Again, this type of expenditure would not typically be included in standard consumption aggregates for fear of double-counting if the same transfers would show up in the consumption of other households (Deaton and Zaidi, 2002). Available evidence about positive spillover effects of cash transfers has documented the salience of interhousehold transfers or gifts in generating positive spillovers in some contexts (Angelucci and De Giorgi, 2009) though not in others (Egger et al., 2022). It is also possible that transfers flowing into the household (remittances or other forms of informal insurance) decrease in response to receipt of a cash transfer, reducing the positive effect on consumption.

4.3 Evaluating transfers required for exit from poverty

Using our estimates of transfer effects on consumption, we also aim to evaluate the size of the (cumulative) transfer required to move households in poverty above the poverty line in a range of lower- and middle-income country households. One of the main goals of cash transfer programs is poverty alleviation; accordingly, the analysis in this section seeks to understand what these estimates imply for policymakers and donors regarding the magnitude of the transfer required to achieve this goal.

This analysis involves three steps: first, we use estimates of the poverty gap for a majority of countries in the sample from the World Bank's Poverty and Inequality Platform or PIP(World Bank, 2022)[38] to identify the magnitude of the increase in individual consumption required to exit poverty. Second, we use the poverty gap in conjunction with data on household size (from the studies included in our sample) to estimate the household-level monthly poverty gap in absolute terms: i.e., the amount of additional consumption a household would have to report each month in order to exit poverty.[39] Third, we use our meta-analytic estimates of the average effect of cash transfers on consumption (excluding outliers, \$1.62 per \$100 transferred, as reported in Table 2) to calculate the size of the cumulative transfer required to generate an exit from poverty. For poverty gap estimates, we use the most recent data year available for each country, with the objective of estimating the size of transfers that would be required currently; however, we express this data on poverty gaps using the 2011 PPP dollars and 2011 poverty line (\$1.90), as this year is closest to the PPP year employed in the remainder of our analysis (2010).[40] As discussed in detail previously, while it is challenging to be precise about the duration of the positive effect on consumption generated by these transfers, the weight of evidence generally suggests that within our sample, the effect persists for around 2.75 years (inclusive of the period of program implementation).

The estimated value for the transfer required for each country is reported in Column (5) of Table A7. There is considerable variation in these estimates due to large differences in

[38]We used the user-written Stata command *pip* (Castaneda, 2023) to compute the poverty gap estimates. For more details about the PIP, see: `https://pip.worldbank.org/about`.

[39]If we have multiple studies in the sample reporting household size for a particular country, we calculate the average.

[40]However, for some countries, the latest available data preceded 2000, and these countries were excluded from this analysis. Afghanistan and Somalia have no PIP data available. We also exclude sample countries that are characterized by a poverty gap of zero or near-zero, and those with no studies in our sample where household sizes are reported.

poverty gap estimates and reported household size across the countries in our sample; the cumulative transfer required for Zambia, for example, is $5950, while for Bangladesh where the average household is closer to the poverty line and has fewer members, the required transfer estimate is only $149. Again, this is a cumulative transfer amount; the typical intervention in our sample has a duration of 19 months with monthly or bimonthly transfers. Column (6) shows the average monthly transfer required in 2011 PPP terms for two years; this is around $300 monthly for Zambia and only around $8 for Bangladesh. For comparison, Table A7 also reports the 2010 PPP-adjusted cumulative dollar value of the average transfer within that country observed in our sample (in Column (3). It is important to note that in many cases, these transfer programs were implemented at least 10 years ago (and in Latin America, the time gap is usually at least 20 years). Given that extreme poverty has been generally declining over time and in some cases quite rapidly, comparing the past transfer to the current required transfer is only an indicative exercise.

With that caveat, however, we observe that in general for countries in Latin America and Asia, the transfers implemented and evaluated in our sample are in fact significantly larger than the estimated transfer required to lift a typical poor household out of extreme poverty given the most recent available data (the only exception is Honduras).[41] This may partly reflect the fact that the extreme poverty line is no longer a particularly relevant benchmark for these less poor or middle income countries, and in fact the national poverty line generally increases with country income level.[42]

However, in sub-Saharan Africa, exactly the opposite pattern is observed: the cumulative transfer required to generate a household exit from poverty is meaningfully larger than the transfers observed in this sample, and this is true for every country site excluding only South Africa and Zimbabwe.[43] In Ghana, Malawi, and Togo, the required transfer is around 7–7.5 times larger than the average transfer observed; in Kenya, Liberia, Lesotho, and Rwanda, around 2.5 times larger; and in Niger, Nigeria, and Uganda around 3.6–4 times larger. [44] The highest estimated required trnasfers are in Malawi and Zambia, where the monthly transfer

[41] In the case of Latin America, this is partly because the cash transfer programs evaluated are generally long-running CCTs in which households have a lengthy period of eligibility, and thus the cumulative transfer estimate is large.

[42] See for example Ferriera et al., “ A richer array of international poverty lines”: https://blogs.worldbank.org/en/developmenttalk/richer-array-international-poverty-lines

[43] In South Africa, the high estimated cumulative transfer value again reflects the long-running characteristics of the child grant program.

[44] Note that this is assuming a consistent effect of cash transfers in all contexts; it is also the case that there was some evidence that cash transfer had reduced effects in sub-Saharan Africa, suggesting an even larger transfer would be required.

amount required to lift a household out of poverty reaches \$270–300. We argue this exercise is useful in understanding the magnitude of cash transfers required to generate household exit from poverty, how it compares to current transfer amounts, and regional variation, highlighting that in sub-Saharan Africa in particular, the magnitude of current cash transfer programs is significantly lower than the transfer size required to shift households out of poverty.

5 Conclusion

We have presented a systematic review and meta-analysis around the effects of cash transfer and cash plus programs on livelihoods outcomes across a range of contexts. Our findings suggest that cash transfers generate robust positive effects on household consumption and income but no significant shifts in labor force participation. These positive effects are significantly larger for cash plus programs. We also present indicative evidence around the cumulative effects on consumption of cash transfer programs under a range of scenarios, and compare the transfers observed to the transfers required to generate an exit from poverty in a number of sample countries.

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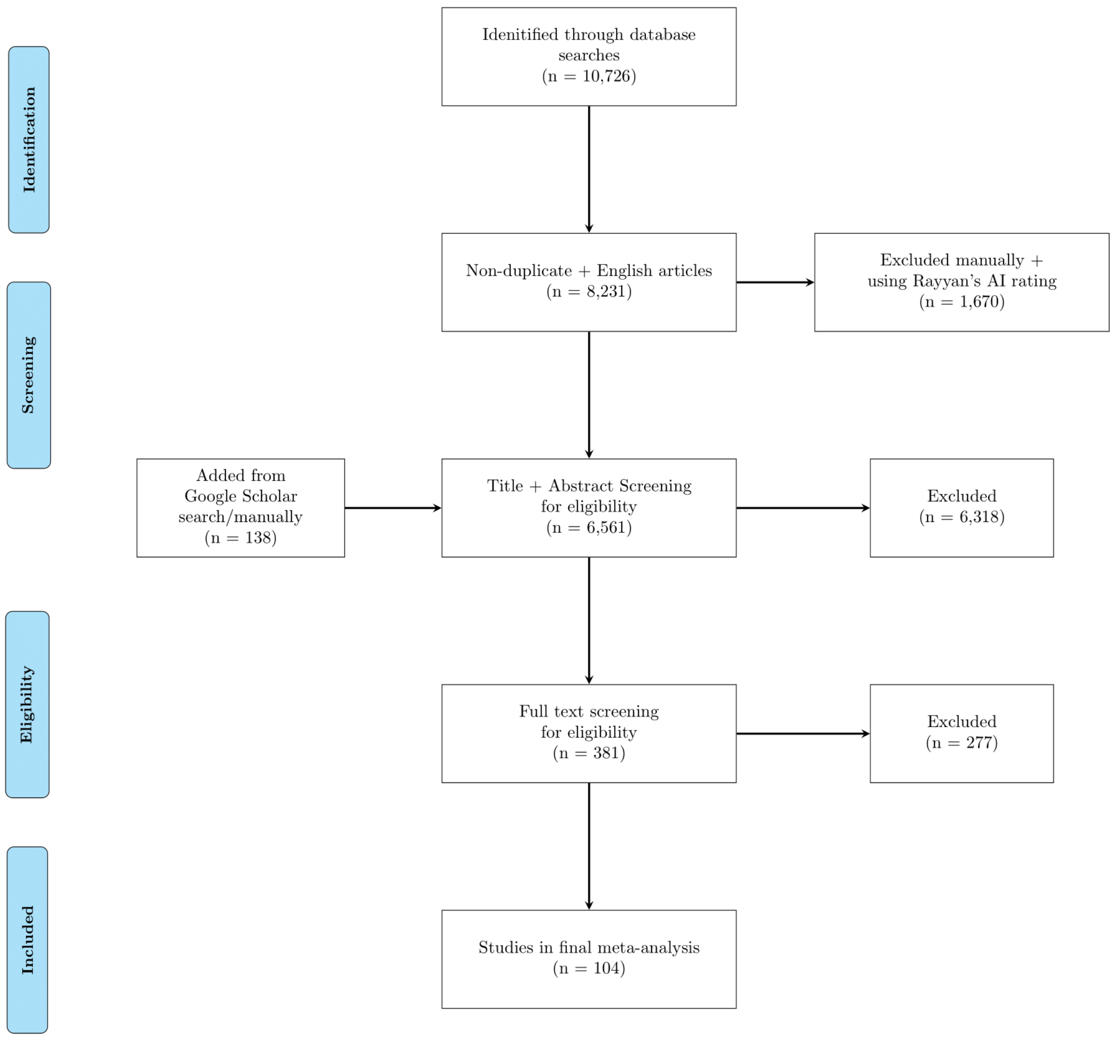
Figures and Tables

Figure 1: Flow chart



Notes: This flowchart summarizes the sample of studies screened and ultimately included in the study.

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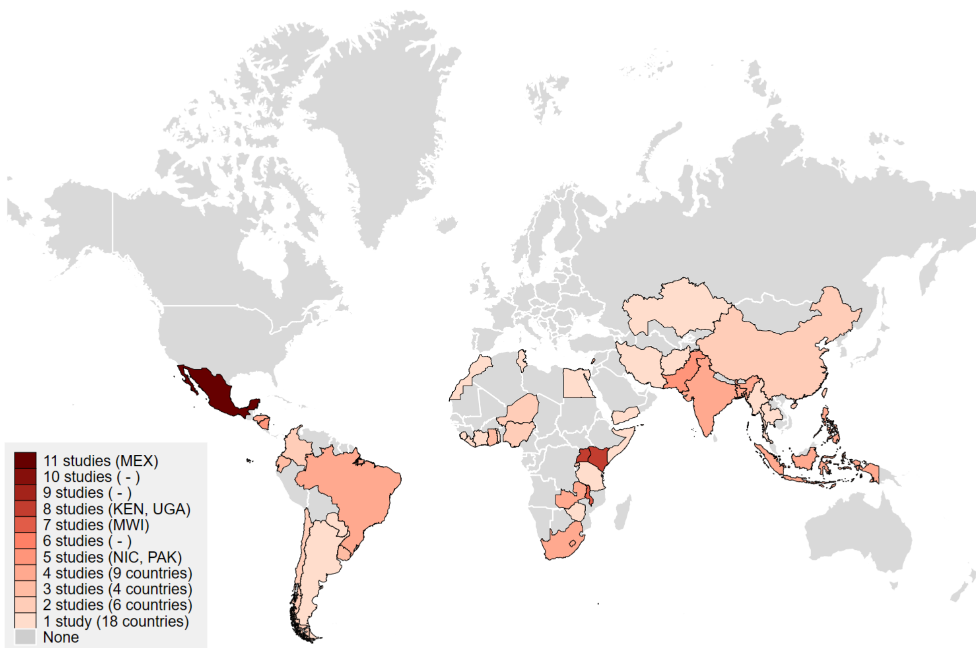
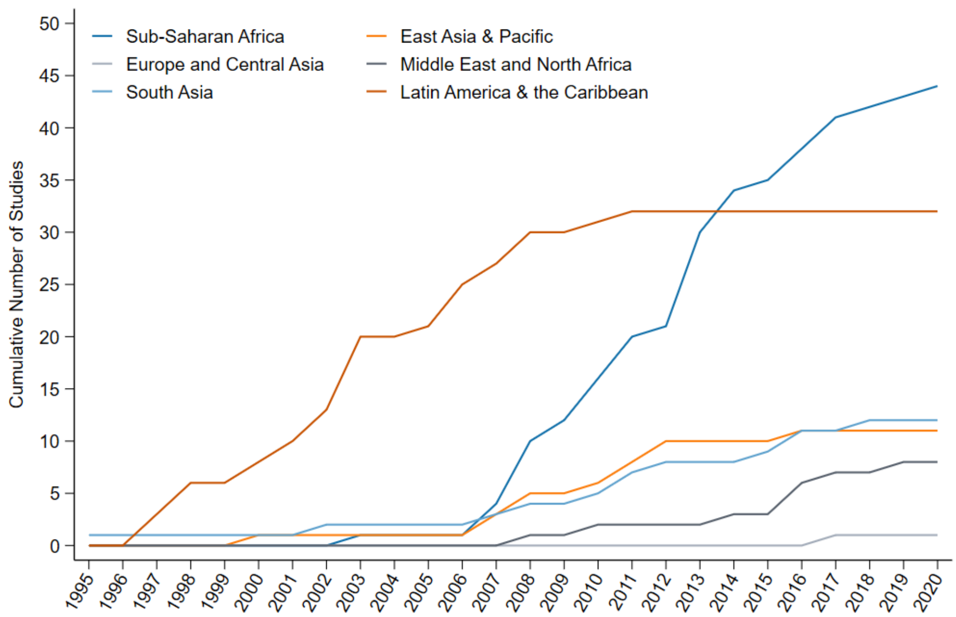
Figure 2: Geographic distribution of sample studies



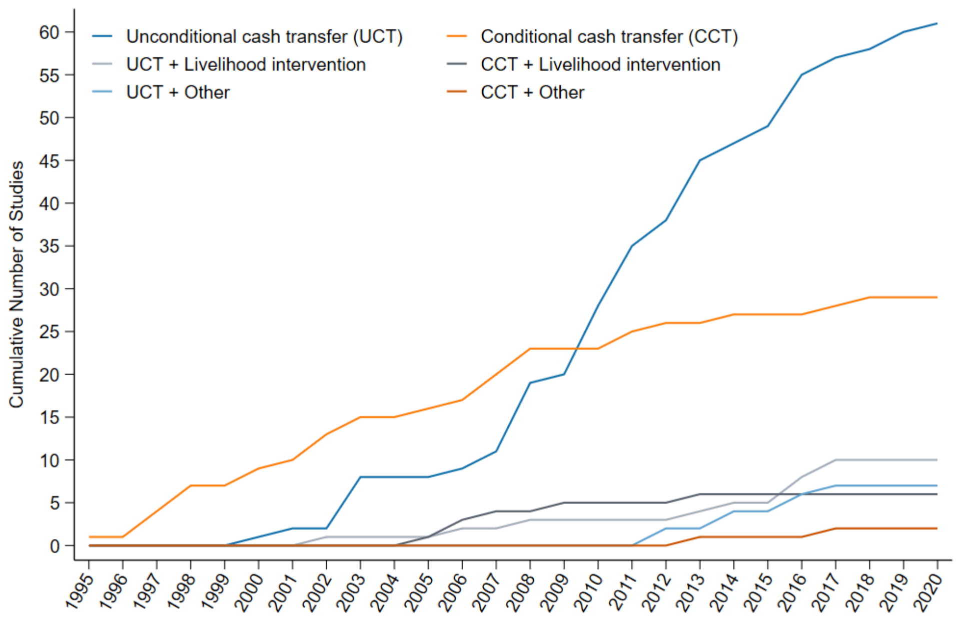
Notes: The map summarizes the distribution of studies in our sample across countries. See Table A4 for full list of included studies.

Figure 3: Timeline of study regions



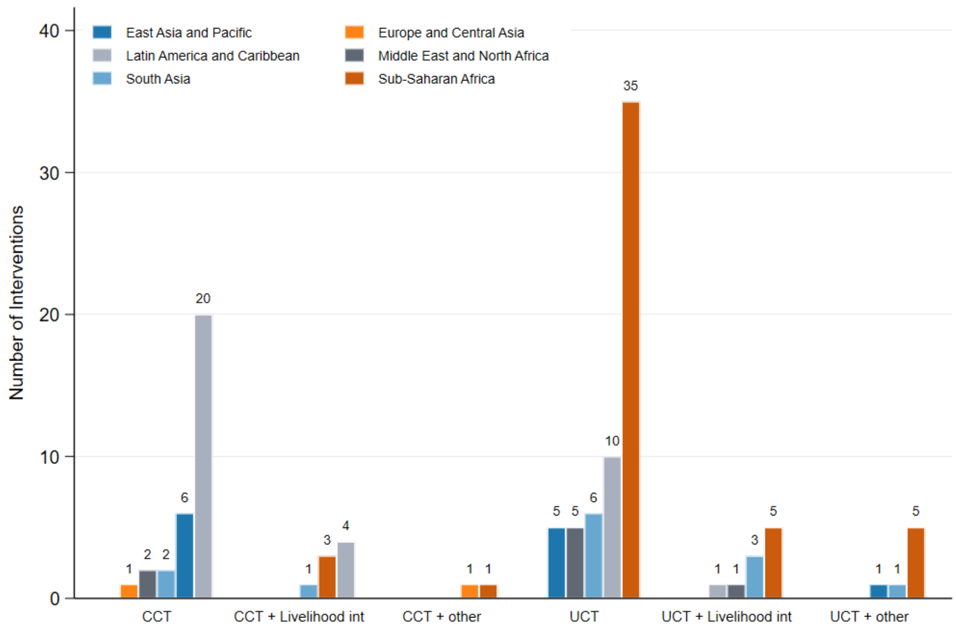
Notes: The graph is plotted at the study-region level and the x-axis shows the reported baseline year for the study. Studies including multiple regions appear more than once.

Figure 4: Timeline of interventions



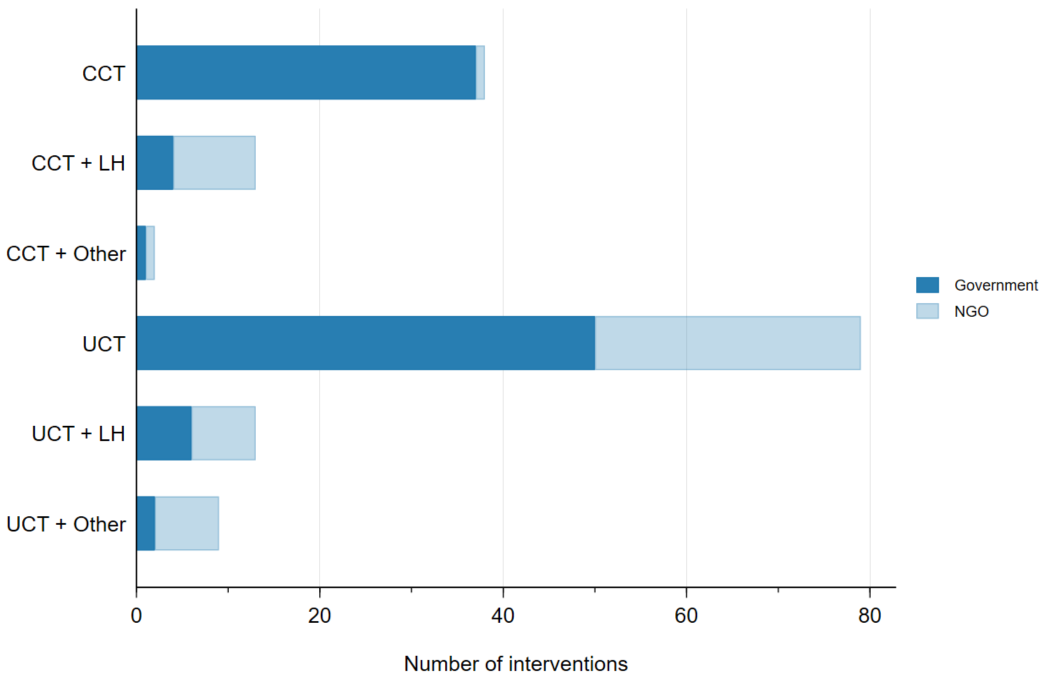
Notes: The graph is plotted at the study-intervention level and the x-axis shows the reported baseline year for the study. Studies covering multiple interventions appear more than once.

Figure 5: Distribution of study regions by type of intervention



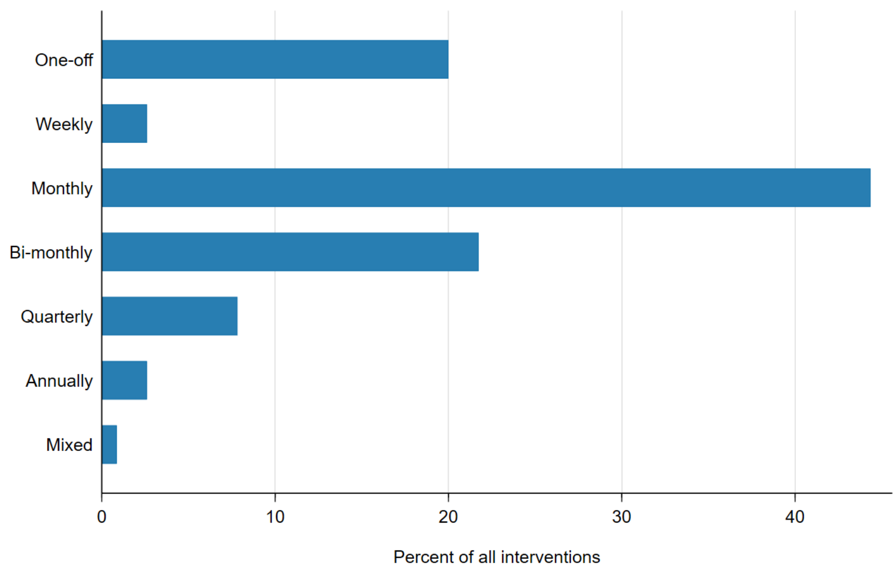
Notes: The graph is plotted at the study-region-intervention level and the x-axis shows the reported baseline year for the study. Studies covering multiple regions or multiple interventions appear more than once. CCT = Conditional cash transfer, UCT = Unconditional cash transfer

Figure 6: Primary implementing organization and intervention type



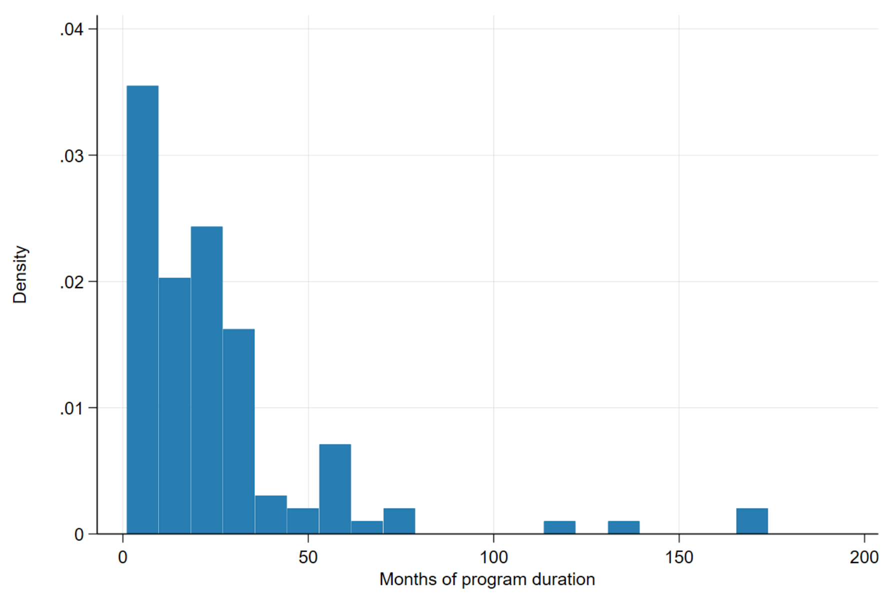
Notes: The NGO category also includes a total of five interventions implemented by UN-agencies and researchers themselves. UCT = Unconditional cash transfer, CCT = Conditional cash transfer program.

Figure 7: Transfer frequency



Notes: This figure summarizes the transfer frequency or periodicity within the sample of transfer programs evaluated.

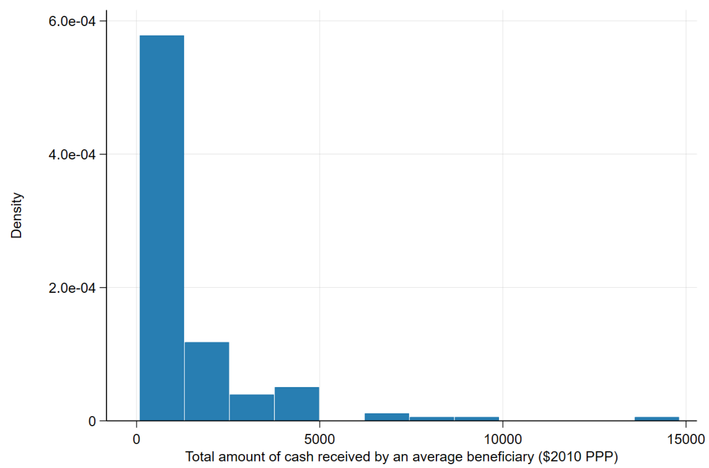
Figure 8: Transfer duration



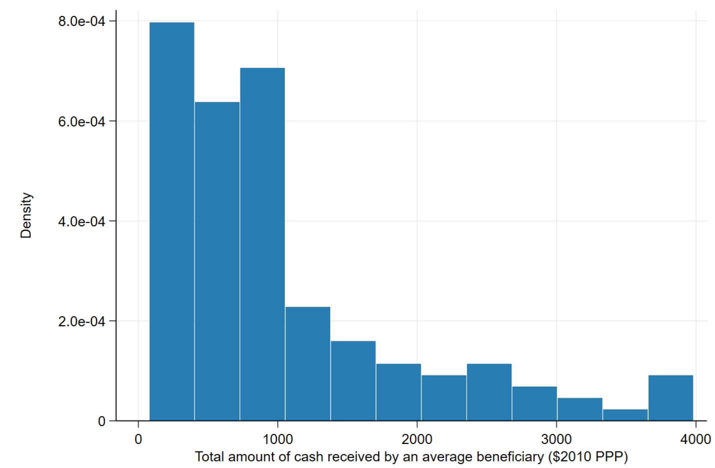
Notes: This summarizes the average program duration (inclusive of the period of cash disbursement and the period of any cash plus implementation).

Figure 9: Transfer magnitude

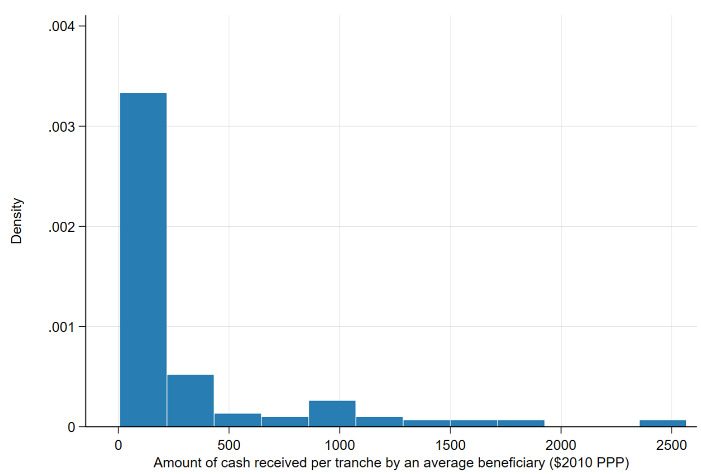
(a)



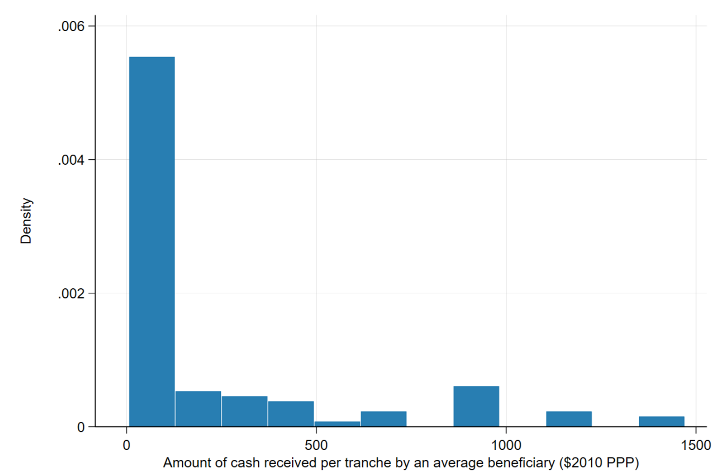
(b)



(c)



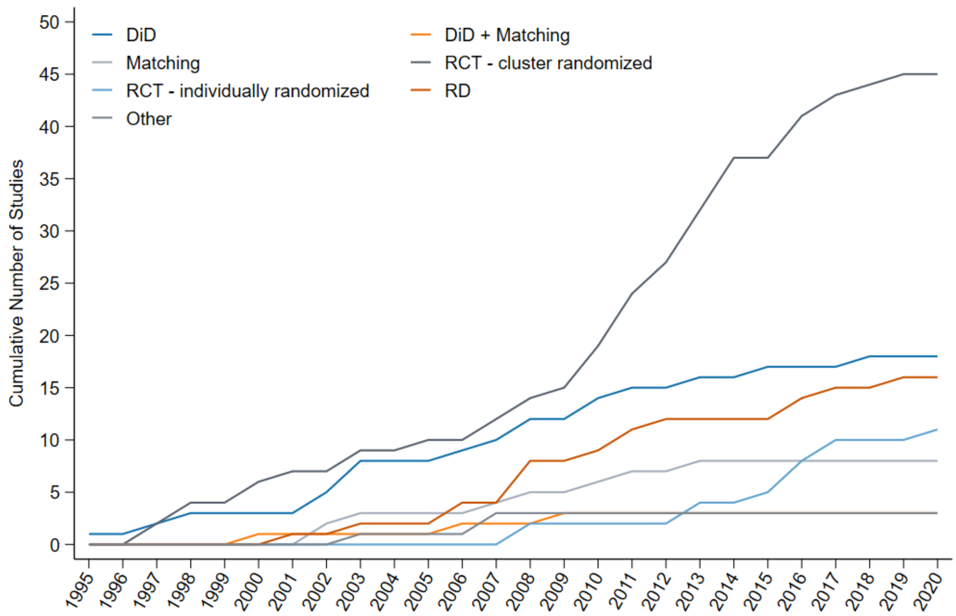
(d)



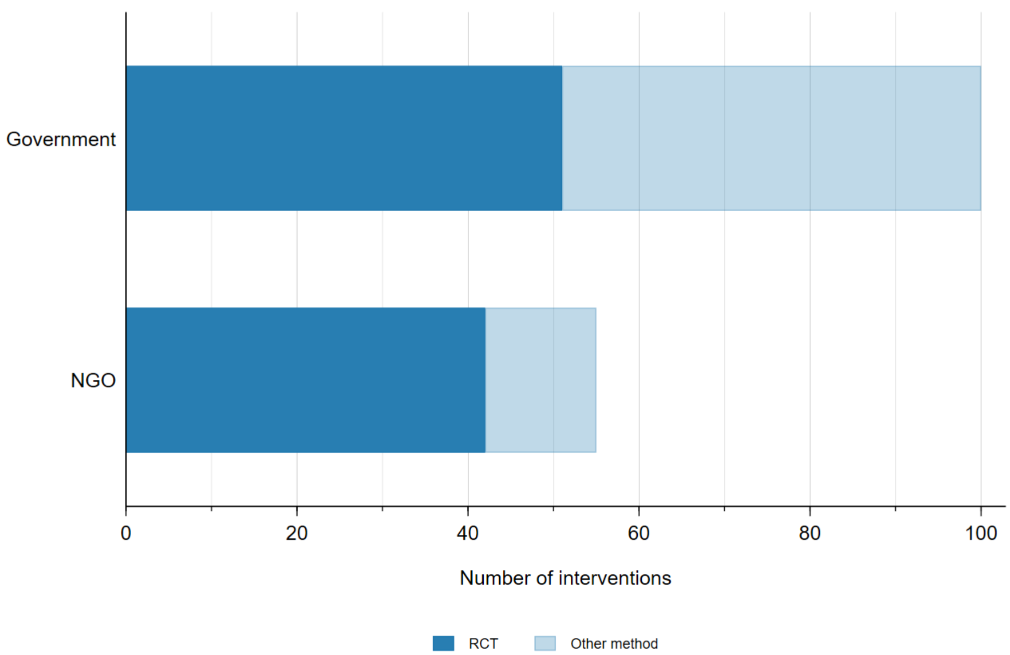
Notes: These graphs summarize the transfer amount in total (Figure 9a) and monthly (Figure 9c), and the same estimates excluding outliers, defined as transfer amounts above the 95th percentile across the sample in Figures 9b and 9d.

Figure 10: Sample of empirical designs

(a)

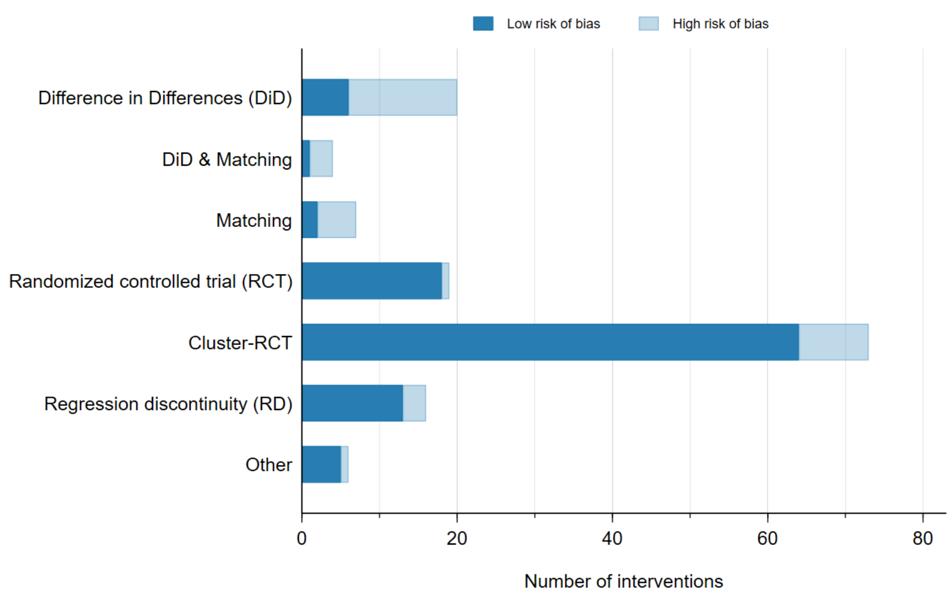


(b)



Notes: Figure (a) shows the sample of empirical designs over time, with each study indexed to its baseline year. Figure (b) shows the number of interventions implemented by different types of implementers and their empirical strategy; the NGO category includes a total of 5 interventions implemented by UN-agencies and researchers themselves.

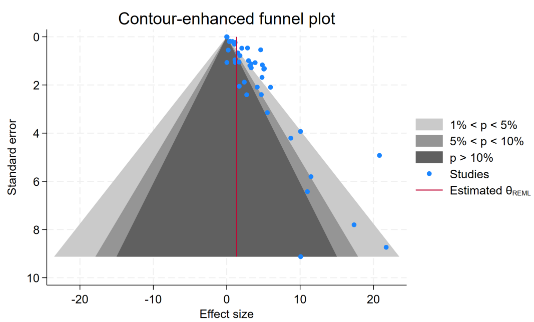
Figure 11: Risk of bias by study design



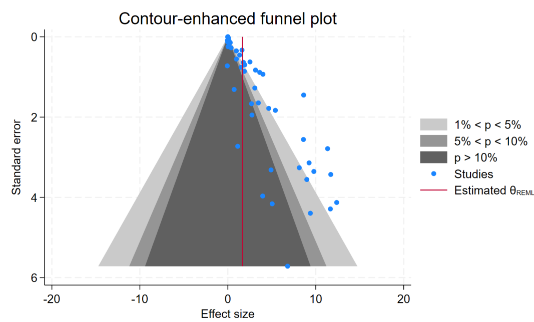
Notes: This figure summarizes the risk of bias by study design.

Figure 12

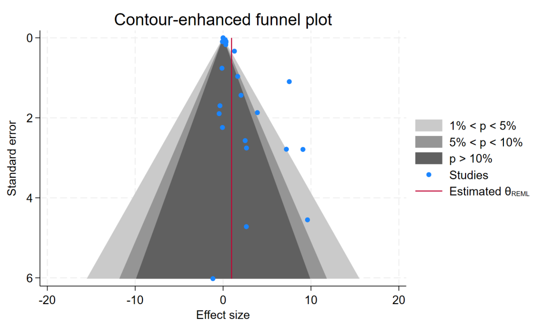
(a) Consumption



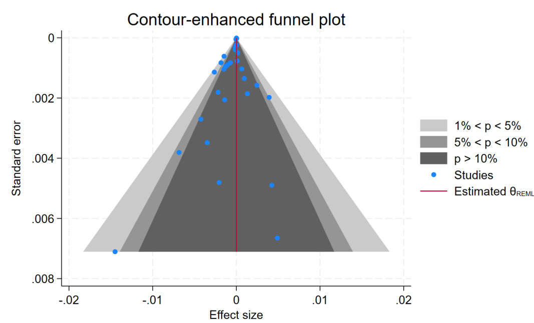
(b) Food consumption



(c) Income



(d) Labor force participation



Notes: This figure presents summarizing potential publication bias for each of the four primary outcome variables of interest.

Table 1: Overview of sample

	N
Sample of studies	
Studies in systematic review	104
Treatment arms in systematic review	155
Studies in meta-analysis	97
Treatment arms in meta-analysis	145
Sample of coefficient estimates	
Total consumption	68
Food consumption	67
Non-food consumption	26
Total income	32
Labor force participation - men	36
Labor force participation - pooled	36
Labor force participation - women	40
Total for all effects	305

Notes: This table summarizes the number of studies, treatment arms, and estimates in the systematic review and meta-analysis.

Table 2: Primary meta-analysis estimates

Outcome variable	Sample restriction	Effect size	Confidence Interval
Panel A: Consumption and income			
Total consumption		1.99	[1.33, 2.68]
Total consumption	No outliers	1.62	[1.09, 2.15]
Food consumption		2.13	[1.43, 2.83]
Food consumption	No outliers	1.76	[1.17, 2.34]
Non-food consumption		13.55	[0.52, 26.58]
Non-food consumption	No outliers	0.61	[0.21, 1.01]
Total income		2.13	[0.92, 3.35]
Total income	No outliers	1.50	[0.62, 2.38]
Panel B: Labor force participation			
Labor force participation		0.00001	[0.00000, 0.00003]
Labor force participation	No outliers	0.00001	[0.00000, 0.00003]
Labor force participation	Men	-0.00018	[-0.00070, 0.00035]
Labor force participation	Women	-0.00007	[-0.00052, 0.00038]

Notes: This table presents the meta-analysis estimates of the effects per $100 cumulative transfer on a range of outcome variables. The forest plots can be found in Figure A4 through A14.

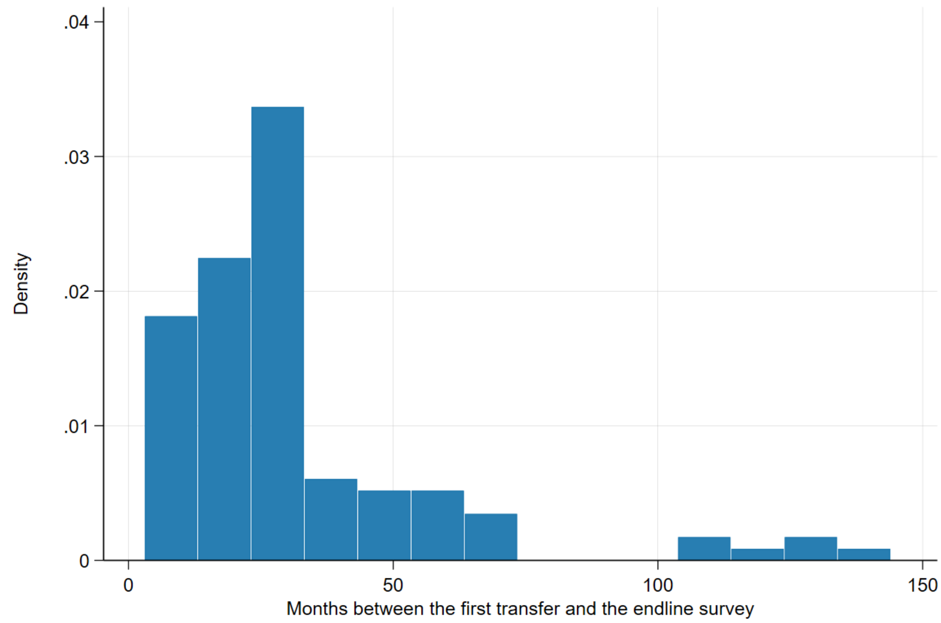
Table 3: Meta-regression findings

	(1)	(2)	(3)	(4)
	Simple model		Multilevel model	
	Total Consumption	Food Consumption	Total Consumption	Food Consumption
Fragile / conflict-affected	1.554	-.461	3.282	-1.071
	(.954)	(1.028)	(1.655)**	(1.496)
Sub-Saharan Africa	-1.137	-1.968	-1.875	-3.376
	(.781)	(.944)**	(1.341)	(1.358)**
Government	-1.740	.676	-2.739	.553
	(.561)***	(.758)	(1.093)**	(1.009)
CCT	-1.086	-.601	-1.108	-1.119
	(.736)	(1.070)	(1.101)	(1.554)
Cash + livelihoods	2.164	6.365	4.017	4.905
	(.863)**	(1.768)***	(1.245)***	(1.981)**
Lump sum	-3.291	-.540	-4.500	-.091
	(.896)***	(.945)	(1.417)***	(1.264)
Randomized trial	.063	.838	.002	-.563
	(.707)	(.781)	(.004)	(.480)
RD	-4.920	-1.759	-6.537	-1.826
	(1.274)***	(1.197)	(2.119)***	(.700)***
Constant	4.619	2.027	6.061	4.634
	(1.165)***	(1.434)	(1.395)***	(1.438)***
Obs.	62	61	62	61

Notes: This table reports the results from a series of meta-regressions analyzing heterogeneity in estimated treatment effects for total consumption and food consumption. Columns (1) and (2) report simple random effects meta-analysis models; Columns (3) and (4) report multilevel models. The independent variables include a binary variable equal to one for sub-Saharan Africa; a binary variable equal to one for a fragile or conflict-affected context); a binary variable equal to one if the transfer is implemented by government; a binary variable equal to one for a conditional cash transfer; a binary variable equal to one for a lump-sum or near-lump-sum transfer; a binary variable equal to one for a cash plus livelihoods program; and a binary variable equal to one for a regression discontinuity design; and a binary variable equal to one for a randomized controlled trial.

Appendix

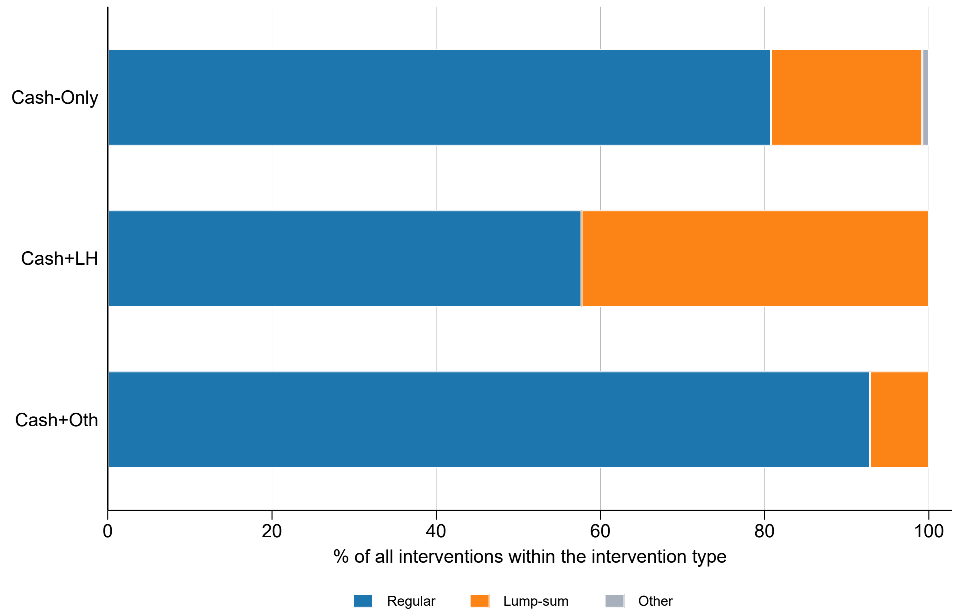
Figure A1: Time between first transfer and endline survey



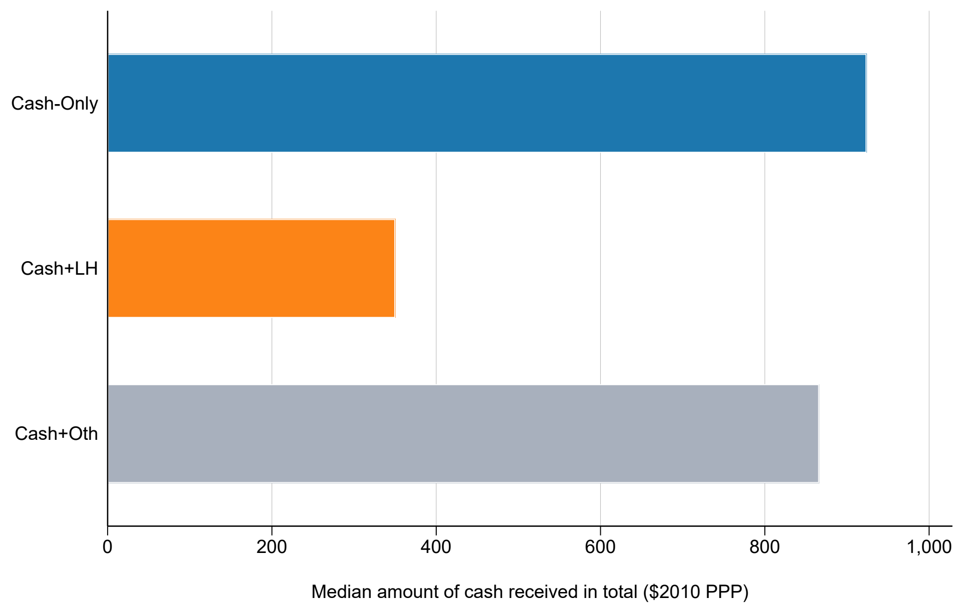
Notes: This histogram summarizes the number of months observed between the first month and the timing of the endline survey or primary assessment that is used to assess cash transfer effects.

Figure A2: Transfers by intervention type

(a)



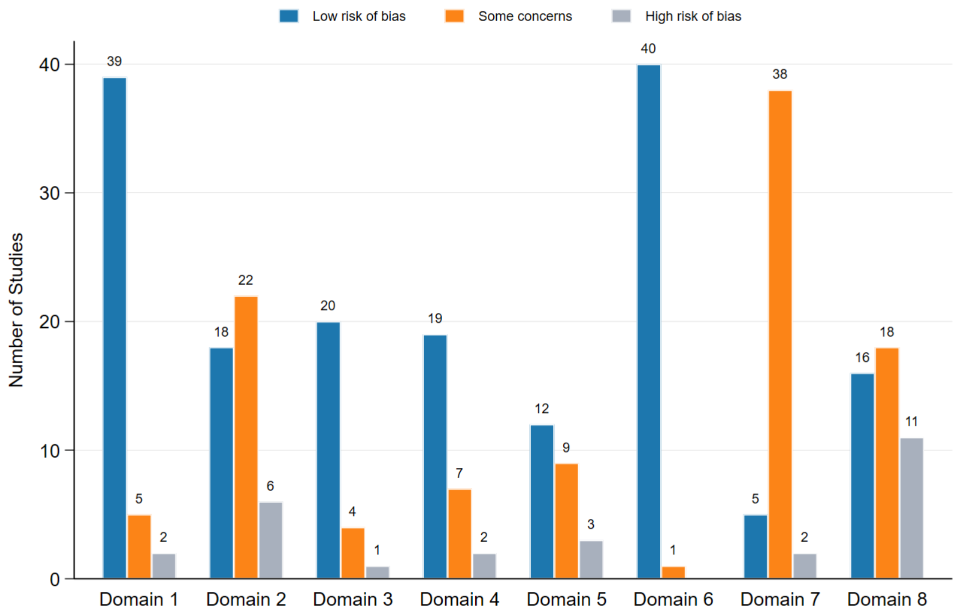
(b)



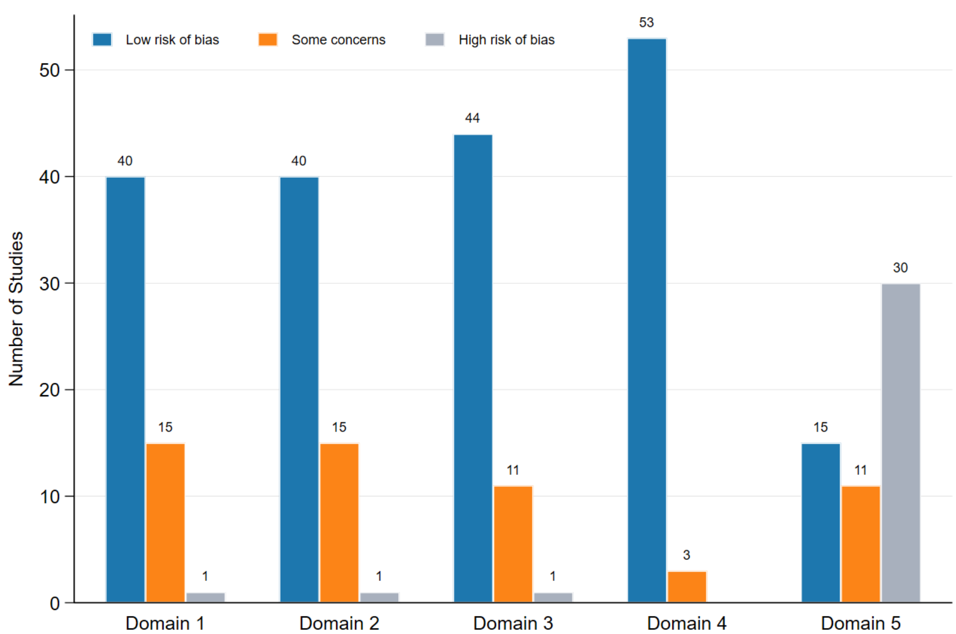
Notes: These figures summarize the amount of transfer by intervention type.

Figure A3: Risk of bias by domain

(a)



(b)



Notes: These figures summarize the risk of bias by domain, for quasi-experimental studies in Figure A3a and for experimental studies in Figure A3b.

Figure A4: Total consumption - full sample

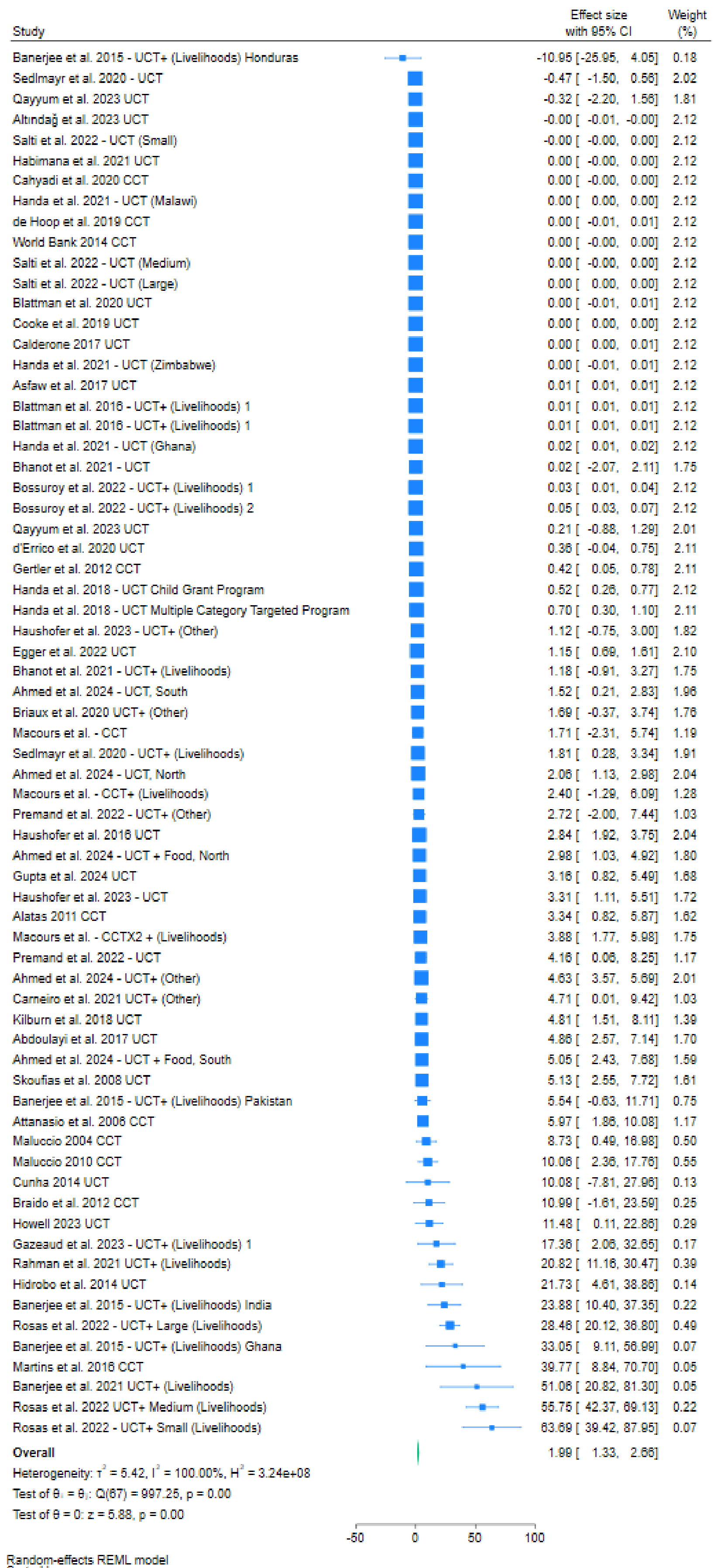


Figure A5: Total consumption - without outliers

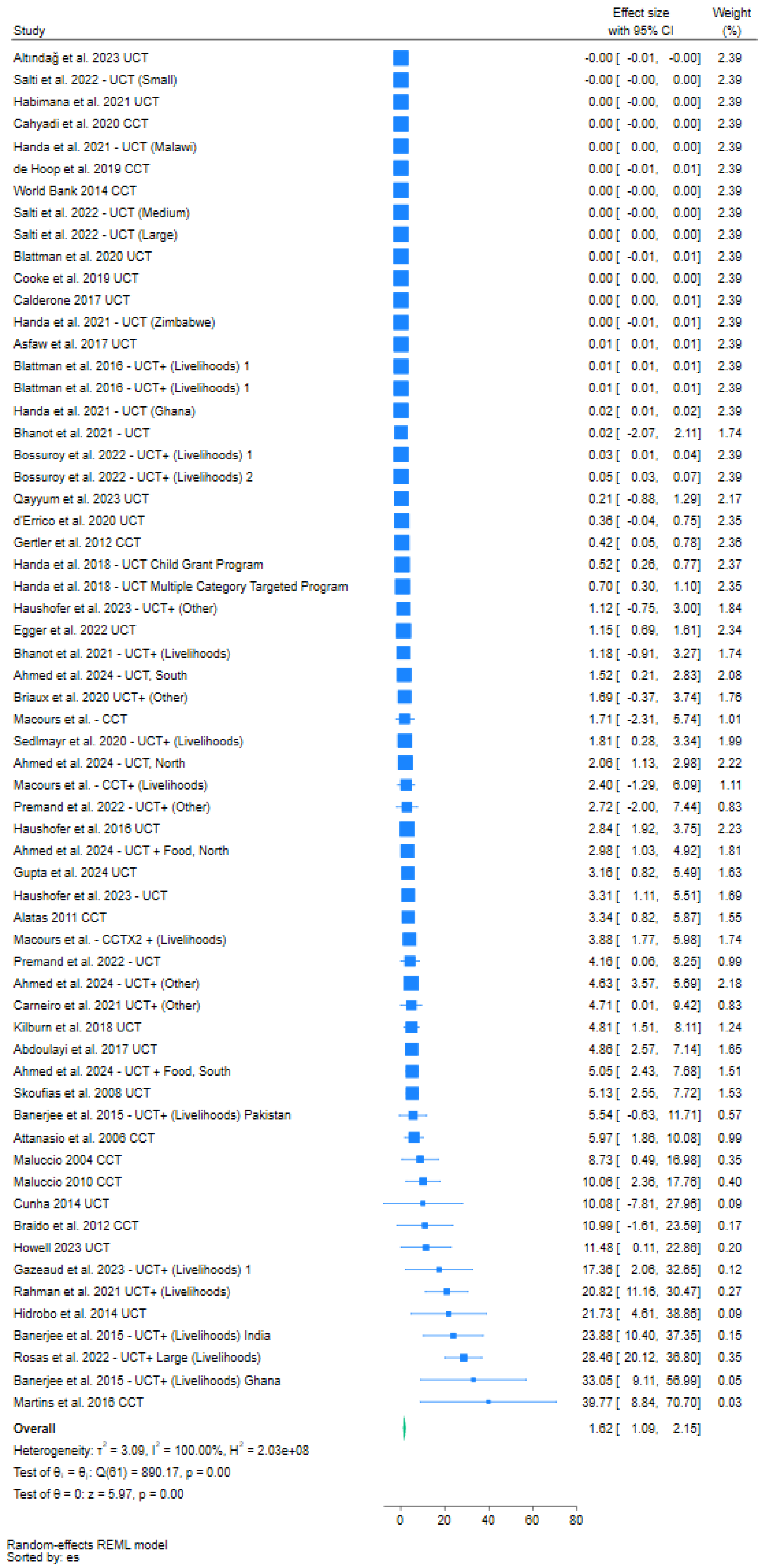


Figure A6: Food consumption - full sample

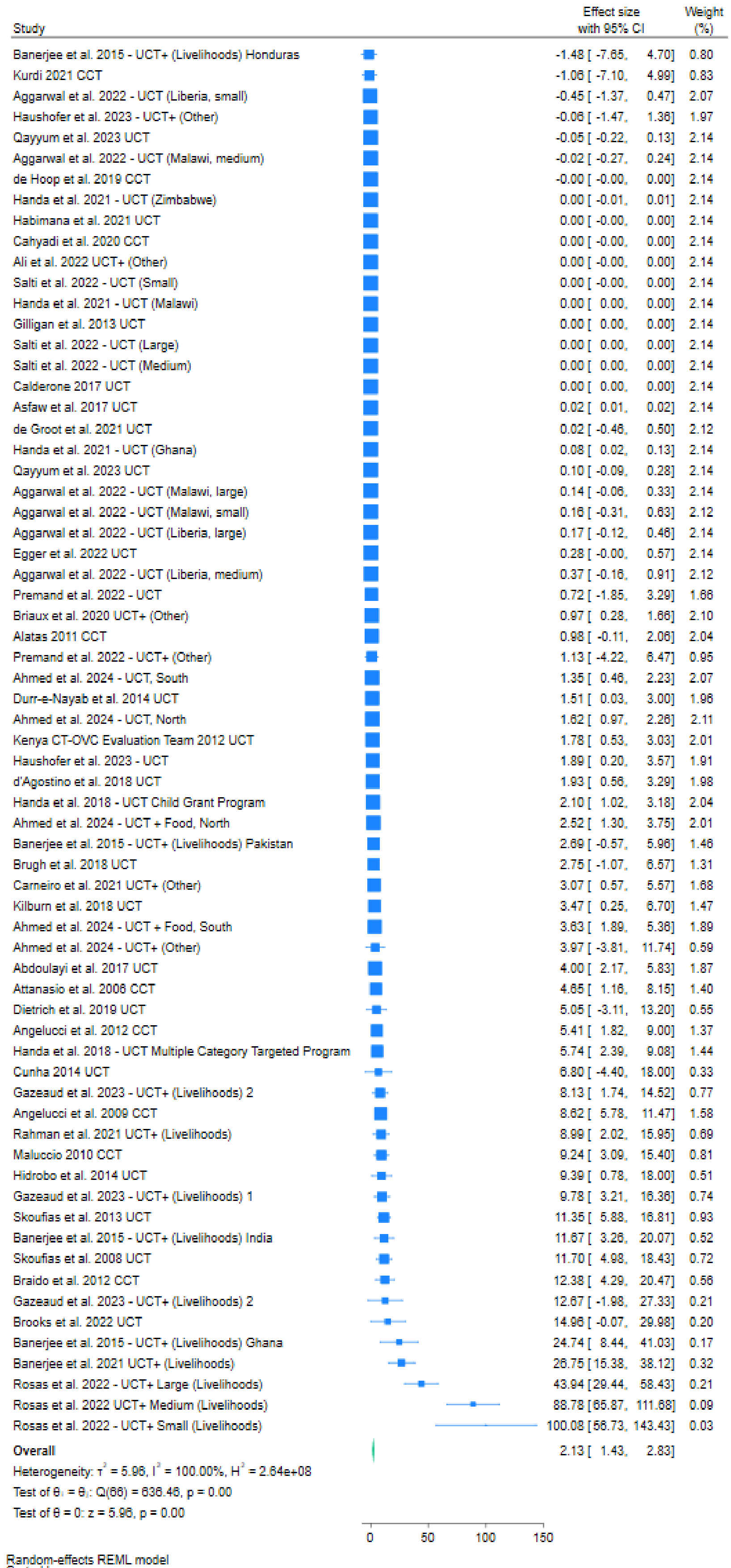


Figure A7: Food consumption - without outliers

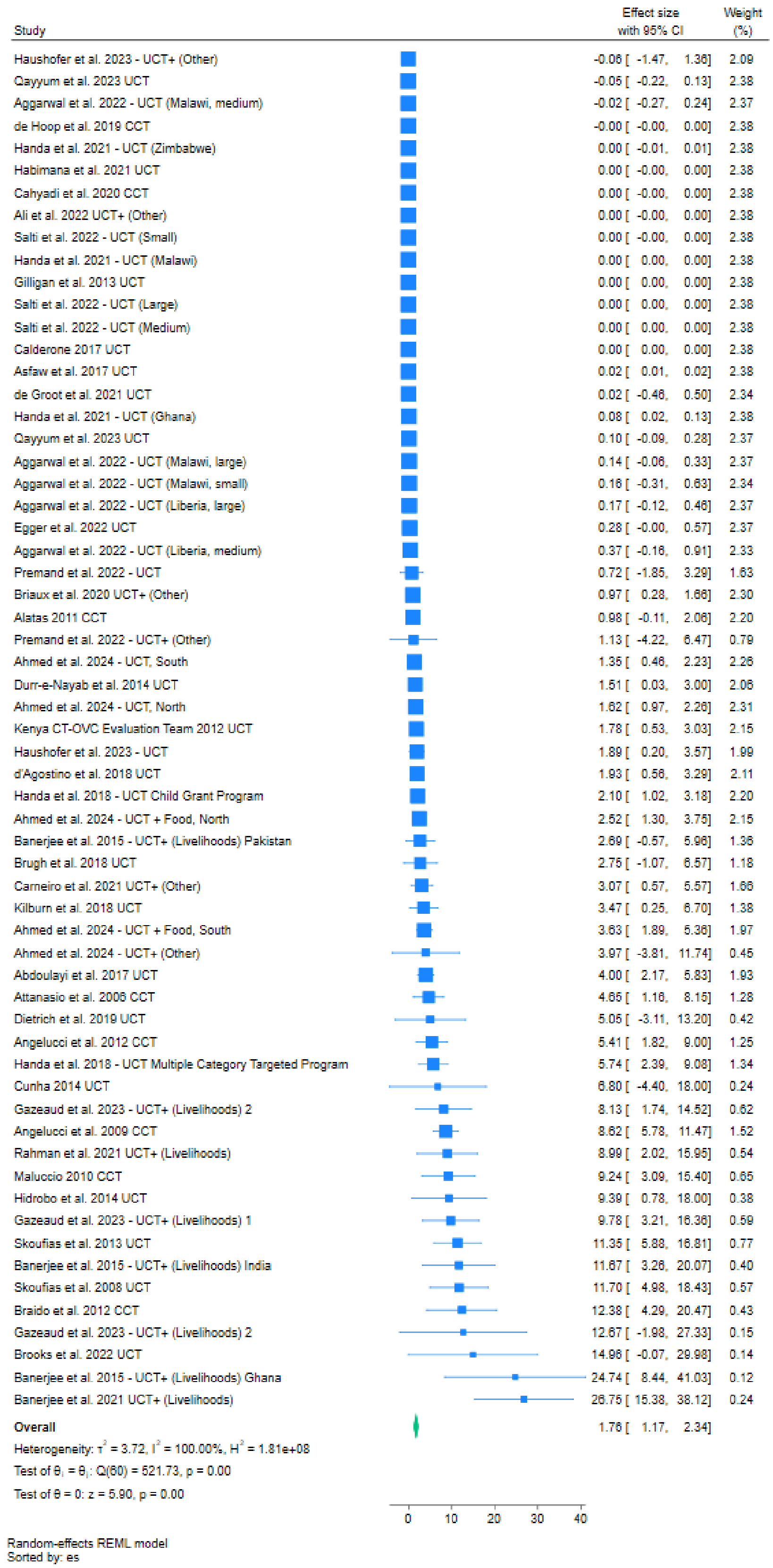


Figure A8: Non-food consumption - full sample

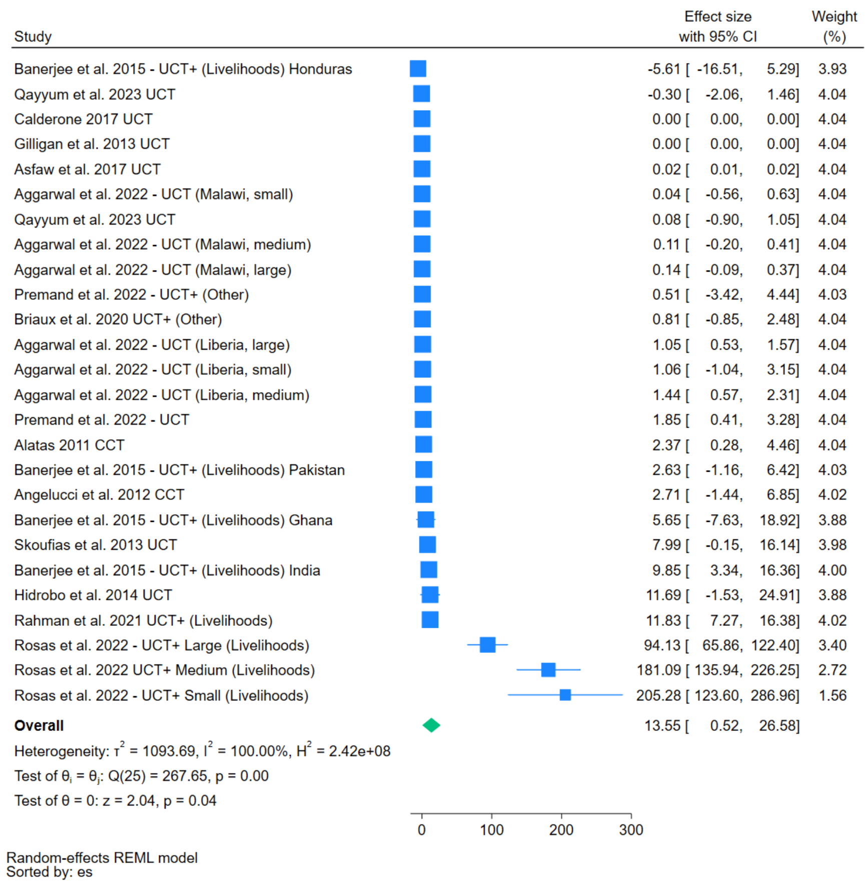


Figure A9: Non-food consumption - without outliers

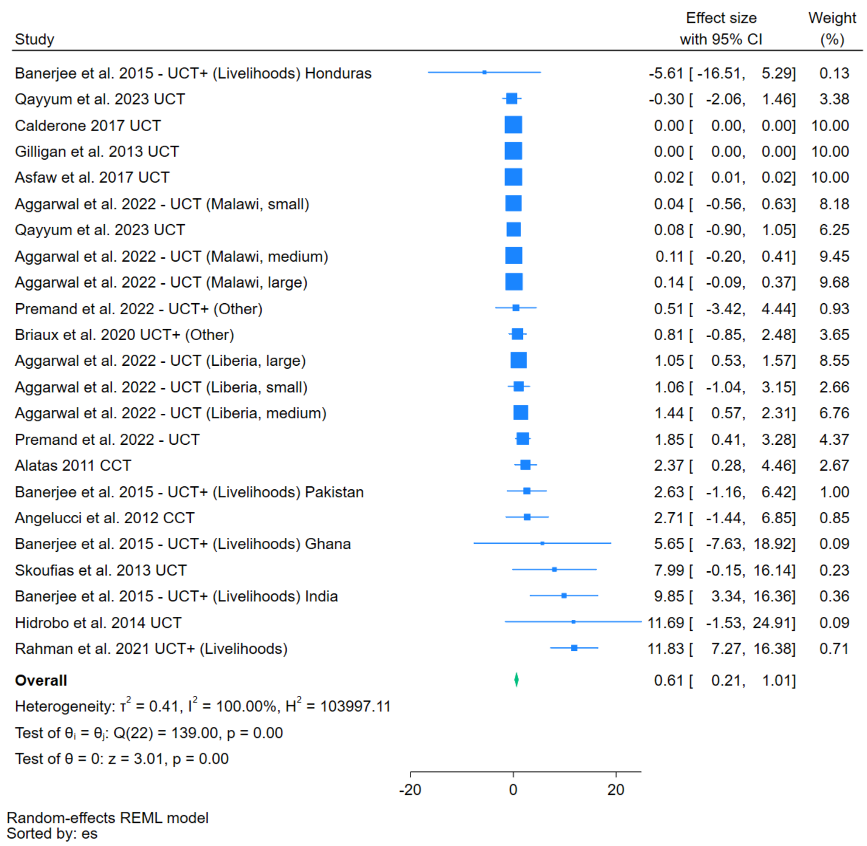


Figure A10: Total income - full sample

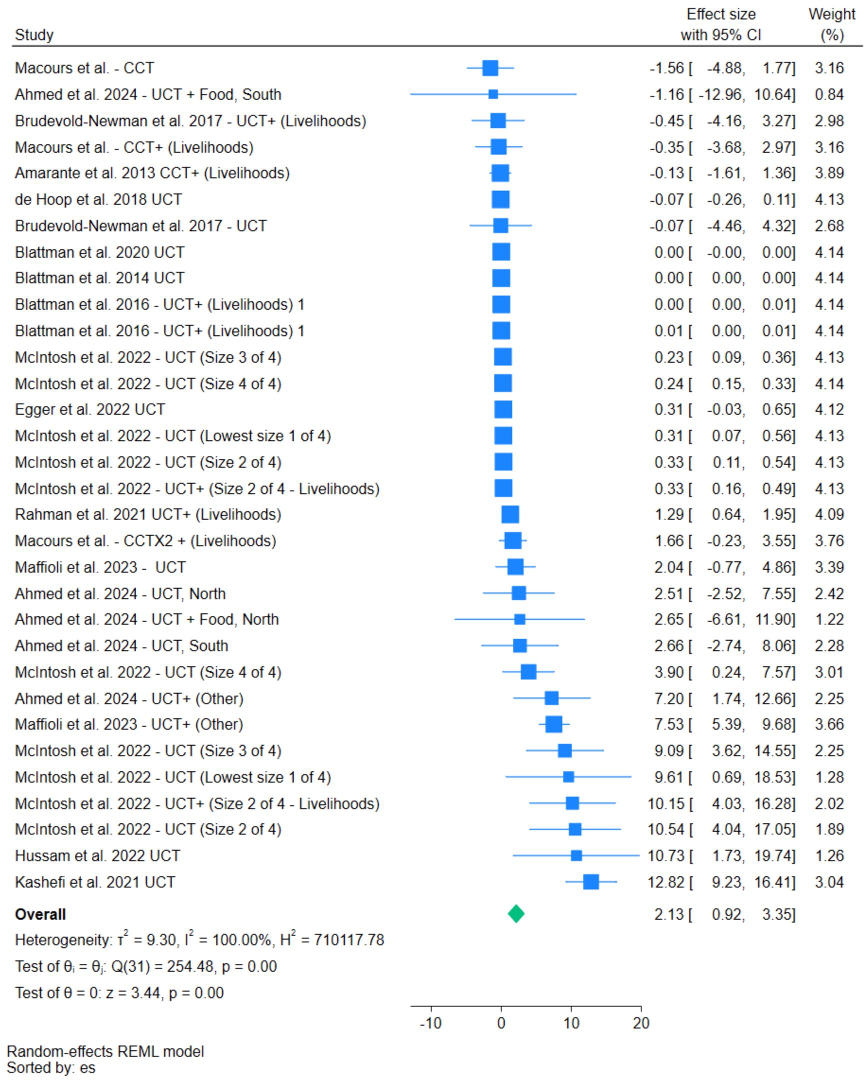


Figure A11: Total income - without outliers

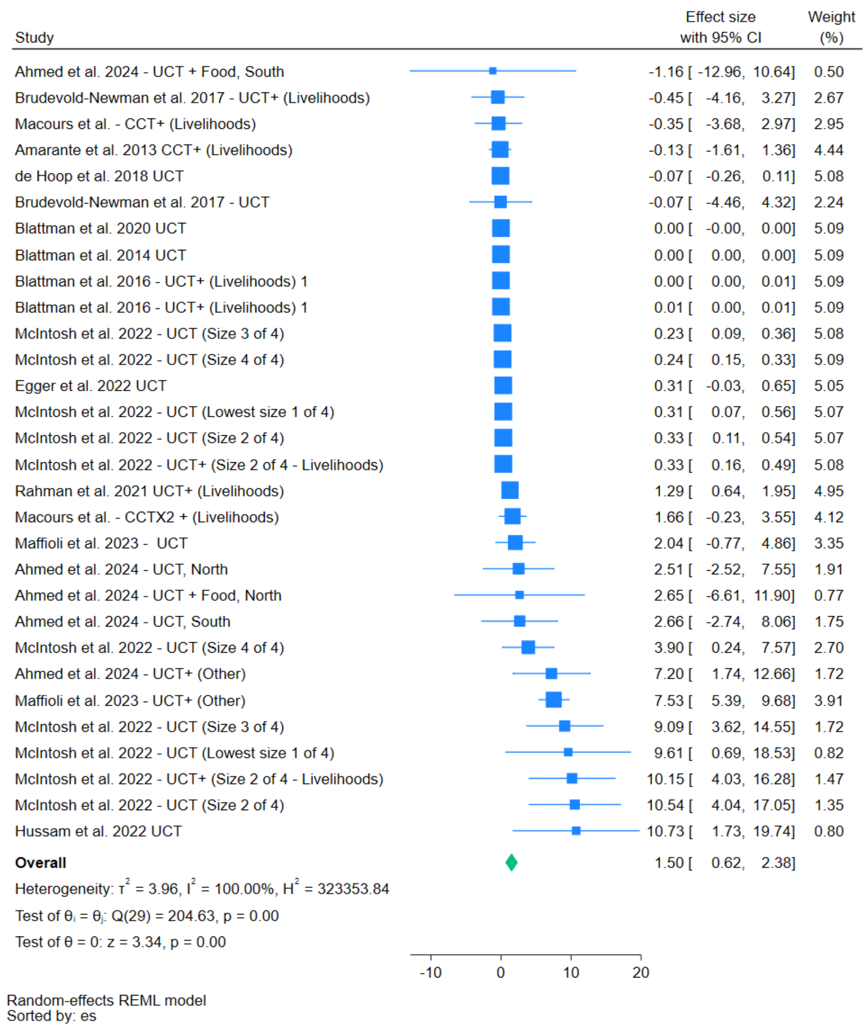


Figure A12: Labor force participation - full sample

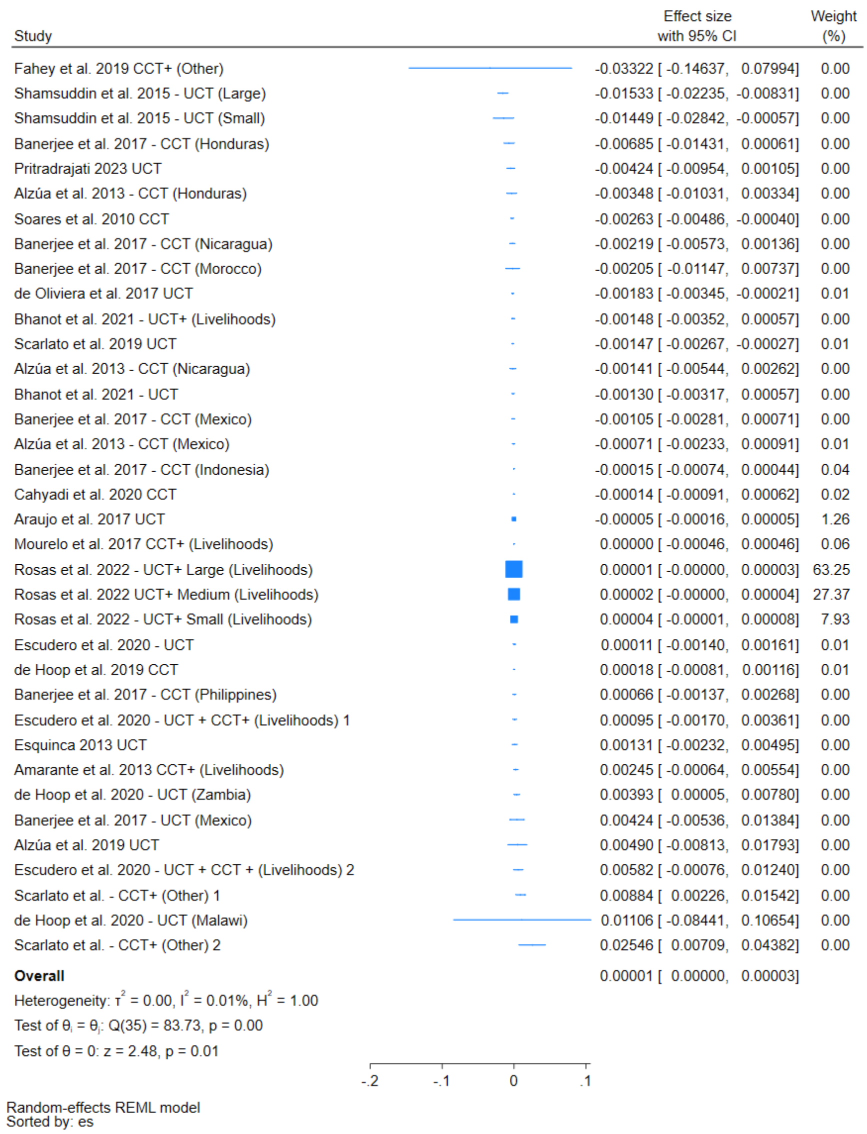


Figure A13: Labor force participation - without outliers

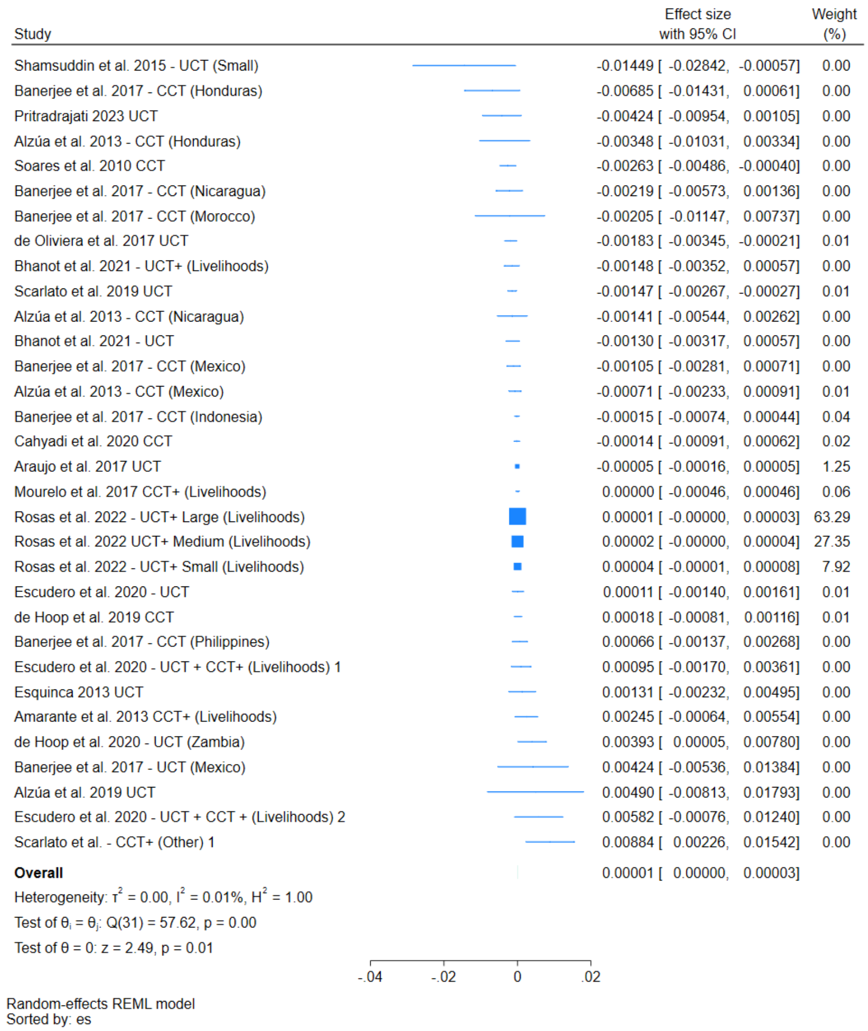
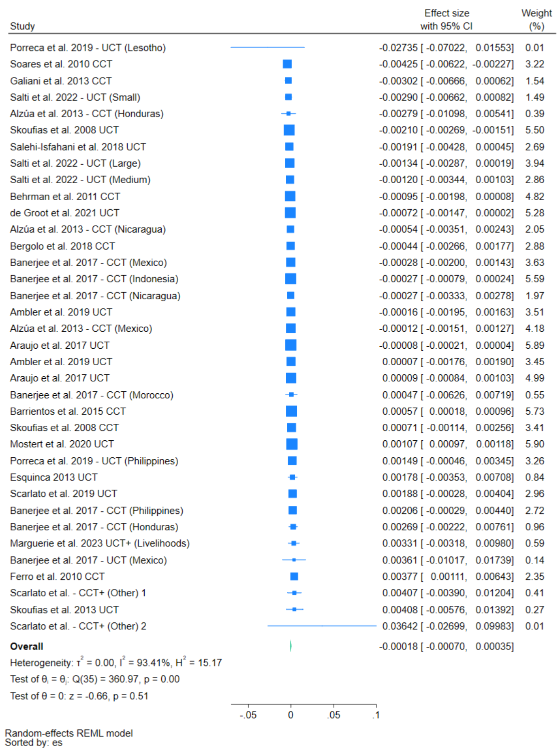
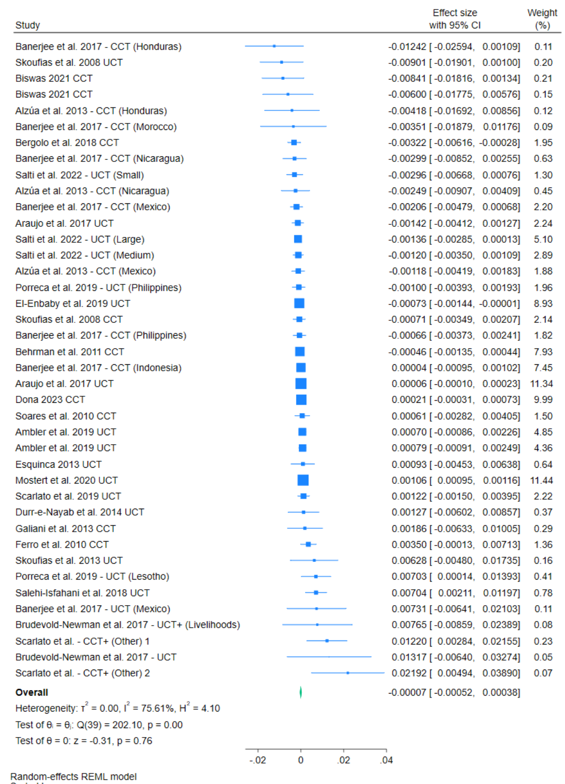


Figure A14: Labor force participation: gender-specific estimates



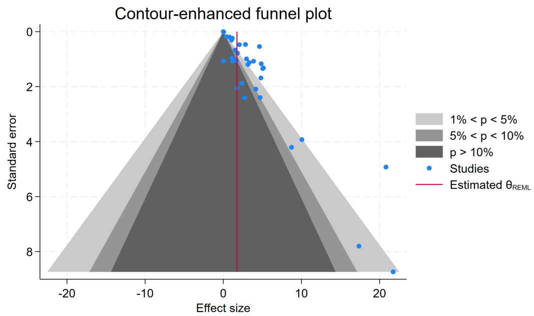
(a) Men



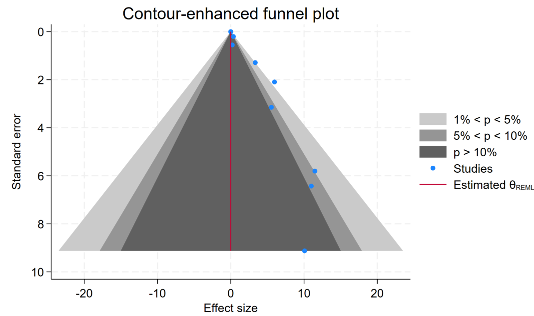
(b) Women

Figure A15: for total consumption: by subsample

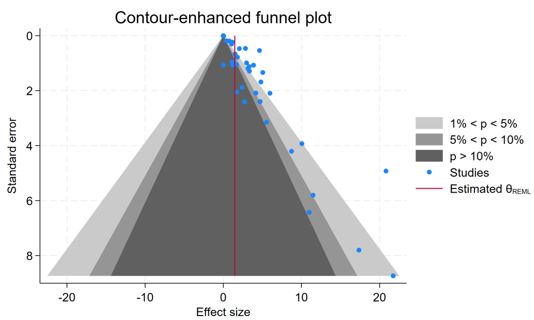
(a) RCTs



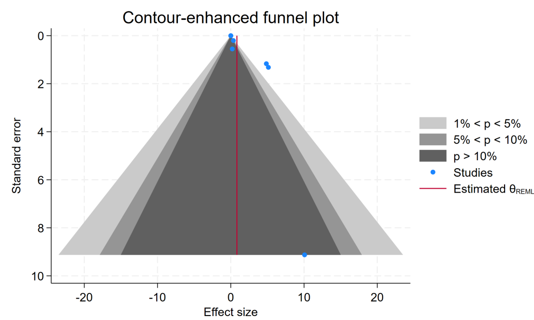
(b) Non-RCTs



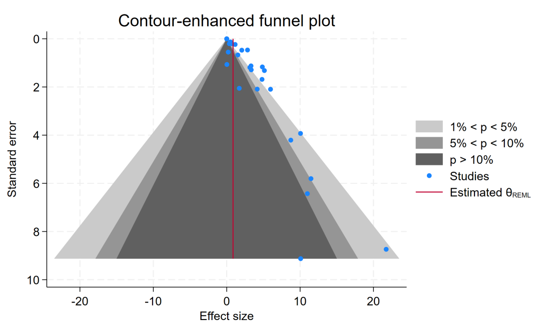
(c) Low risk-of-bias



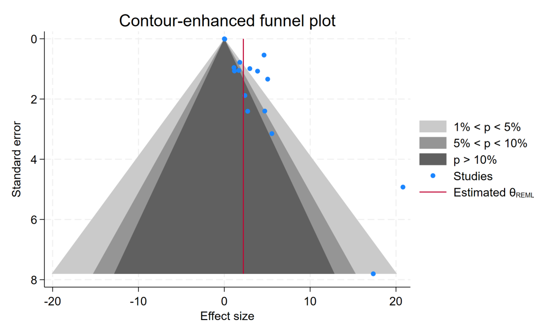
(d) High risk-of-bias



(e) Cash only interventions



(f) Cash plus interventions

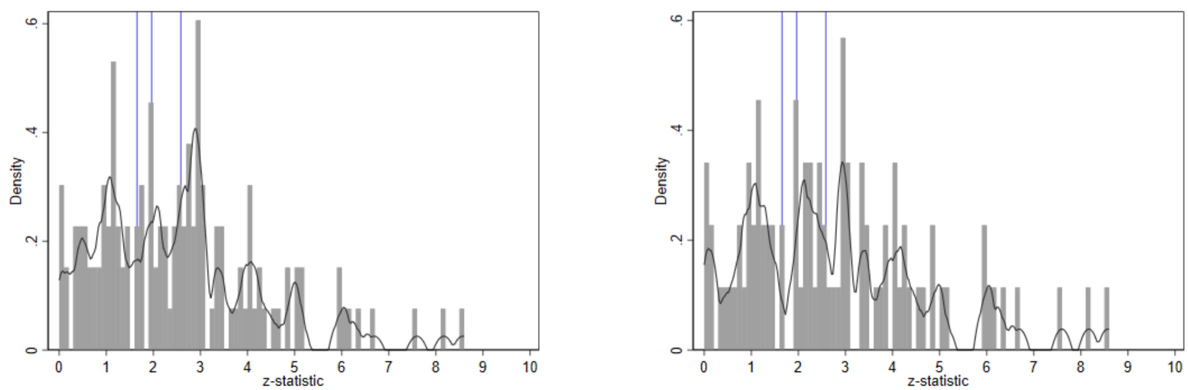


Notes: These figures summarize risk-of-bias for subsamples of studies: RCTs (Figure A15a), non-RCTs (Figure A15b), studies characterized by low risk-of-bias (Figure A15c), studies characterized by high risk-of-bias (Figure A15d), cash only interventions (Figure A15e), and cash plus interventions (Figure A15f).

Figure A16: Histograms of t-statistics

(a) All studies

(b) RCTs only



Notes: These figures capture a histogram of t-statistics within the sample of estimated effects, restricting to the coefficients estimates for consumption and income-related variables. Figure A16a shows the t-statistics for all studies; Figure A16b shows the t-statistics for RCTs only. The vertical lines show the thresholds for significance at the ten, five, and one percent level.

Table A1: Other cash transfer meta-analyses focusing on livelihood outcomes)

Paper	Intervention types	Evaluation methods	Outcomes	Scope
Kabeer and Waddington (2015)	CCTs	No restriction	Child and adult labor, consumption, investment, savings, risk sharing, community level outcomes	46 studies; 11 programs; 9 countries Total consumption only: 3 estimates, 4 programs
Hidrobo et al. (2018)	UCTs, CCTs, food transfers	No restriction	Food security, assets	58 studies; 46 programs; 25 countries Food consumption only: 68 estimates, 37 programs
Sulaiman (2018)	Cash, livelihood, graduation	No restriction	Consumption	48 studies; 26 countries 48 consumption estimates
Kondylis & Loeser 2021	UCTs, graduation	RCTs	Consumption	17 studies; 14 countries 38 consumption estimates;
Correa et al. (2023)	Cash, cash+, graduation	No restriction	Assets, agriculture, labor allocation	40 studies; 13 countries
Crosta et al. 2024	UCTs	RCTs	Consumption, food security, assets, income, labor force participation (+ education) (+ health)	All: 83 studies; 50 programs; 26 countries Consumption only: 58 estimates, 31 programs
This paper	UCTs, CCTs Cash+	No restriction	Consumption, income, labor force participation	All: 104 studies; 78 programs 42 countries Consumption only: 68 estimates, 40 programs

Notes: This table summarizes the characteristics of multiple meta-analyses in the cash transfer literature, including the current study.

Table A2: Search strategy

Database	**Boolean Search**
"Web of Science	"cash" OR "transfer" OR "CCT" OR "UCT" OR "grant" (Abstract) AND "controlled" OR "randomized" OR "randomisation" OR "randomization" OR "longitudinal" OR "control" OR "randomised" OR "treatment" OR "RCT" OR "RCTs" OR "difference-in-difference" OR "difference in difference" OR "DiD" OR "diff-in-diff" OR "discontinuity" OR "RDD" OR "RD" OR "triple difference" OR "DDD" OR "DD" OR "PSM" OR "propensity score" OR "experiment" OR "within-study" OR "quasi-experiment" OR "quasi-experimental" OR "replication" OR "experimental" OR "Mixed methods" (Abstract) AND "poverty" OR "socio-economic" OR "livelihood" OR "welfare" OR "income" OR "consumption" OR "expenditure" OR "assets" OR "food security" OR "food insecurity" OR "family economic outcomes" OR "labor" OR "labour" OR "work" OR "agricultural" OR "productivity" OR "wage" OR "earning" OR "graduation" OR "salary" OR "employment" OR "Unemployment" OR "Debt" OR "financial" OR "inequality" OR "payment" OR "living condition" OR "livestock" OR "job" (Abstract) AND "LMIC" OR "LMICS" OR "LAMI" OR "LAMIC" OR "developing" OR "low income" OR "middle income" OR "low-income" OR "middle-income" OR "Transitional" OR "deprived" OR "poorer" OR " poor" OR " poorest" OR " under developed" OR "under-developed" OR "third world" OR "under served" OR "under-served" OR "less developed" OR "underserved" OR "fragile" OR "conflict-affected" OR "conflict" OR "war" OR "Latin America" OR "Latin American" OR "Central American" OR "Asia" OR "Afghanistan" OR "Albania" OR "Algerian" OR "Angola" OR "Barbuda" OR "Barbudans" OR "Argentine" OR "Armenian" OR "Azerbaijani" OR "Bahraini" OR "Bangladeshi" OR "Barbadian" OR "Belarusian" OR "Belizean" OR "Bhutan" OR "Bolivia" OR "Bosnian" OR "Herzegovinian" OR "Botswana" OR "Brazil" OR "Bulgarian" OR "Burkinabe" OR "Burmese" OR "Burundian" OR "Cape Verdian" OR "Cambodian" OR "Cameroonian"

OR “Central African” OR “Chadian” OR “China” OR “Colombia” OR “Comoros” OR “Congo” OR “Costa Rica” OR “Cote d’Ivo*” OR “Cuba” OR “Djibouti” OR “Ecuador” OR “Egypt” OR “Salvador” OR “Equatorial Guinea” OR “Equatoguinean” OR “Eritrean” OR “eSwatini” OR “Ethiopia” OR “Fijian” OR “Gabonese” OR “Gambian” OR “Georgian” OR “Ghanaian” OR “Grenadian” OR “Guatemalan” OR “Guinean” OR “Guyanese” OR “Honduras” OR “Hungary” OR “Indian” OR “Indonesian” OR “Islamic Republic” OR “Iraqi” OR “Jordan” OR “Kazakhstan” OR “Kenya” OR “I-Kiribati” OR “Korean” OR “Kyrgyz Republic” OR “Kyrgyzstani” OR “Laotian” OR “Latvian” OR “Lebanese” OR “Mosotho” OR “Libya” OR “Lithuania” OR “Madagascar” OR “Malawi” OR “Malaysia” OR “Maldives” OR “Malian” OR “Maltese” OR “Marshallese” OR “Mauritanian” OR “Mauritian” OR “Mahorais” OR “Mexican” OR “Micronesian” OR “Moldovan” OR “Mongolian” OR “Montenegrin” OR “Mozambican” OR “Burmese” OR “Namibian” OR “Nauruan” OR “Nicaragua” OR “Niger” OR “Nigerian” OR “Macedonian” OR “Oman” OR “Pakistani” OR “Palauan” OR “Panamanian” OR “New Guinean” OR “Guinean” OR “Paraguayan” OR “Peruvian” OR “Filipino” OR “Puerto Rico” OR “Romania” OR “Russia” OR “Rwandan” OR “Samoan” OR “Tomean” OR “Saudi” OR “Senegalese” OR “Serbian” OR “Montenegrin” OR “Seychellois” OR “Sierra Leonean” OR “Solomon Islands” OR “Somalia” OR “South Africa” OR “Sudan” OR “Lanka” OR “Nevis” OR “St. Lucia” OR “the Grenadines” OR “Sudan” OR “Suriname” OR “Republic” OR “Tajikistan” OR “Tanzanian” OR “Thai” OR “Timorese” OR “Tonga” OR “Trinidad” OR “Trinidadian” OR “Tunisia” OR “Turk” OR “Turkmen” OR “Tuvaluan” OR “Ugandan” OR “Ukrainian” OR “Uruguayan” OR “Uzbekistani” OR “Venezuelan” OR “Vietnamese” OR “Gaza” OR “Yemenite” OR “Yugoslav” OR “Zambian” OR “Zimbabwean” OR “South America” OR “Latin Americas” OR “Central America” OR “Africa” OR “Asian” OR “Afghan” OR “Albanian” OR “American Samoa” OR “Angolan” OR “Antiguans” OR “Argentina” OR “Armenia” OR “Azerbaijan” OR “Bahrain” OR “Bangladesh” OR “Barbados” OR “Belarus” OR “Belize” OR “Benin” OR “Bhutanese” OR “Bolivian“

	OR “Herzegovina” OR “Bosnia and Herzegovina” OR “Batswana” OR “Brazilian” OR “Burkina” OR “Burma” OR “Burundi” OR “Cape Verde” OR “Cambodia” OR “Cameroon” OR “Central African Republic” OR “Chad” OR “Chile” OR “Chinese” OR “Colombian” OR “Comoran” OR “Congolese” OR “Costa Rican” OR “Fiji” OR “Cuban” OR “Dominican” OR “Ecuadorean” OR “Egyptian” OR “Salvadorian” OR “Equatorial Guinean” OR “Eritrea” OR “Estonia” OR “Elswat*” OR “Ethiopian” OR “Gabon” OR “Gambia” OR “Georgia” OR “Ghana” OR “Grenada” OR “Guatemala” OR “Guinea” OR “Guinea-Bissau” OR “Haiti” OR “Honduran” OR “Hungarian” OR “Indonesia” OR “Iran” OR “Iraq” OR “Jamaica” OR “Jordanian” OR “Kazakhstani” OR “Kenyan” OR “Koran” OR “Kosovo” OR “Kyrgyz” OR “Laos” OR “Latvia” OR “Lebanon” OR “Lesotho” OR “Liberia” OR “Libyan” OR “Lithuanian” OR “Malagasy” OR “Malawian” OR “Malaysian” OR “Maldivian” OR “Malta” OR “Marshall Islands” OR “Mauritania” OR “Mauritius” OR “Mayotte” OR “Mexico” OR “Micronesia” OR “Moldova” OR “Mongolia” OR “Montenegro” OR “Morocco” OR “Myanmar” OR “Namibia” OR “Nauru” OR “Nepal” OR “Nicaraguan” OR “Nigerien” OR “Macedonia” OR “Northern Mariana Islands” OR “Omani” OR “Palau” OR “Panama” OR “Papua New Guinea” OR “New Guinea” OR “Paraguay” OR “Peru” OR “Philippines” OR “Poland” OR “Puerto Rican” OR “Romanian” OR “Russian” OR “Samoa” OR “São Tomé and Principe” OR “Saudi Arabia” OR “Senegal” OR “Serbia” OR “Serbia and Montenegro” OR “Seychelles” OR “Sierra Leone” OR “Slovakia” OR “Solomon Islander” OR “Somalian” OR “South African” OR “South Sudanese” OR “Sri Lankan” OR “Kittitian” OR “Saint Lucia” OR “Vincentian” OR “Sudanese” OR “Surinamer” OR “Syrian” OR “Tajik” OR “Thailand” OR “Timor-Leste” OR “Togo” OR “Tongan” OR “Tobago” OR “Tobagonian” OR “Tunisian” OR “Turkmenistan” OR “Tuvalu” OR “Uganda” OR “Ukraine” OR “Uruguay” OR “USSR” OR “Vanuatu” OR “Vietnam” OR “West Bank” OR “Yemen” OR “Yugoslavia” OR “Zambia” OR “Zimbabwe” OR “Algeria” OR “Bosnia” OR “Antigua and Barbuda” OR “Beninese” OR “Chilean” OR “Côte d’iv*” OR “Ivorian” OR “Czech” OR “Guyana” OR “Haitian” OR “Guinea-Bissauan” OR “India” OR “Iranian” OR “Jamaican” OR “Kiribati” OR “Kosovar” OR “Lao” OR “Liberian” OR “Mali” OR “Moroccan” OR “Mozambique” OR “Nepalese” OR “Nigeria” OR “Pakistan” OR “Polish” OR “Rwanda” OR “Slovak” OR “Kitts and” OR “Nevisian” OR “Syrian Arab” OR “Syria” OR “Tanzania” OR “Togolese” OR “Turkey” OR “Uzbekistan” OR “Uzbek” OR “Venezuela” OR “Yemeni”

Science Direct (RCT: 2018-2024)	TI OR AB OR KW: ("cash transfer" OR "cash plus" OR "UCT" OR "CCT" OR "cash payment" OR "cash grant") AND ("RCT" OR "randomized controlled trial")
Science Direct (RCT: 1950-2018)	TI OR AB OR KW: ("cash transfer" OR "cash plus" OR "UCT" OR "CCT" OR "cash payment" OR "cash grant") AND ("RCT" OR "randomized controlled trial")
Science Direct (DiD)	TI OR AB OR KW: ("cash transfer" OR "cash plus" OR "UCT" OR "CCT" OR "cash payment" OR "cash grant") AND ("difference in difference" OR "DiD" OR "diff-in-diff")
Science Direct (RD)	TI OR AB OR KW: ("cash transfer" OR "cash plus" OR "UCT" OR "CCT" OR "cash payment" OR "cash grant") AND ("RD" OR "RDD" OR "discontinuity")
Science Direct (Abstracts)	abstract:(("cash transfer" OR "cash plus" OR "UCT" OR "CCT" OR "cash payment" OR "cash grant")) AND abstract:("RCT" OR "RCTs" OR "randomized controlled trial" OR "difference in difference" OR "DiD" OR "diff-in-diff" OR "dif-in-dif" OR "triple difference" OR "DDD" OR "DD" OR "regression discontinuity" OR "RDD" OR "RD")
3ie	TI: ("cash transfer" OR "cash plus" OR "UCT" OR "CCT" OR "cash payment" OR "cash grant") AND TI: ("RCT" OR "randomized controlled trial" OR "difference in difference" OR "DiD" OR "diff-in-diff" OR "regression discontinuity" OR "RDD" OR "RD")) OR (AB: ("cash transfer" OR "cash plus" OR "UCT" OR "CCT" OR "cash payment" OR "cash grant") AND AB: ("RCT" OR "randomized controlled trial" OR "difference in difference" OR "DiD" OR "diff-in-diff" OR "regression discontinuity" OR "RDD" OR "RD"))

Google Scholar	(“cash” OR “cash transfer” OR “CCT” OR “UCT” OR “cash+” OR “transfer“) AND (“RCT” OR “randomized controlled trial” OR “difference in difference” OR “DiD” OR “diff-in-diff” OR “regression discontinuity” OR “RDD” OR “RD” OR “Matching“) AND (“poverty” OR “socio-economic” OR “livelihood” OR “welfare” OR “income” OR “consumption” OR “expenditure” OR “assets” OR “food security” OR “food insecurity” OR “family economic outcomes” OR “labor” OR “labour” OR “work” OR “agricultural” OR “productivity” OR “wage” OR “earning” OR “graduation” OR “salary” OR “employment” OR “Unemployment” OR “Debt” OR “financial” OR “inequality” OR “payment” OR “living condition” OR “livestock” OR “job“)

Table A3: Papers included in SR but excluded from meta-analysis (insufficient information)

Herrmann, T., Leckcivilize, A., & Zenker, J. (2021). The impact of cash transfers on child outcomes in rural Thailand: Evidence from a social pension reform. The Journal of the Economics of Ageing, 19, 100311.	Mean of control arm is not reported; treatment effect is a log specification.
Yang L. The urban Dibao programme in China: Targeting and its effect. Indian Journal of Labour Economics. 2013 Oct 1;56(4).	Mean or standard deviation of control arm is not reported; treatment effect is reported as a standardized coefficient.
von Fintel, D., & Pienaar, L. (2016). Small-Scale Farming and Food Security: The Enabling Role of Cash Transfers in South Africa's Former Homelands.	Standard errors are not reported.
Asadullah, M. N., & Ara, J. (2016). Evaluating the long-run impact of an innovative anti-poverty programme: evidence using household panel data. Applied Economics, 48(2), 107-120.	Standard errors are not reported.
Miller, Candace M., Maxton Tsoka, and Kathryn Reichert. "The impact of the Social Cash Transfer Scheme on food security in Malawi." Food Policy 36.2 (2011): 230-238.	Standard errors are not reported.
Tiwari, Smriti, Silvio Daidone, Maria Angelita Ruvalcaba, Ervin Prifti, Sudhanshu Handa, Benjamin Davis, Ousmane Niang, Luca Pellerano, Paul Quarles Van Ufford, and David Seidenfeld. "Impact of cash transfer programs on food security and nutrition in sub-Saharan Africa: A cross-country analysis." Global Food Security 11 (2016): 72-83.	Standard errors are not reported.
Azeem, Muhammad Masood, Amin W. Mugera, and Steven Schilizzi. "Do social protection transfers reduce poverty and vulnerability to poverty in Pakistan? Household level evidence from Punjab." The Journal of Development Studies 55, no. 8 (2019): 1757-1783.	Information about the transfer magnitude is not reported and cannot be imputed since this is a pooled analysis of multiple different transfer programs.

This table summarizes the papers that were included as eligible in the systematic review but excluded from the meta-analysis due to insufficient information, and notes the reason for the latter decision.

Table A4: All included studies

Short citation	Country	Intervention type	Program name	Empirical design
de Groot et al. 2021	Lesotho	UCT	Lesotho Child Grant Programme	RCT - cluster randomized
Abdoulayi et al. 2017	Malawi	UCT	Malawi's Social Cash Transfer Program	RCT - cluster randomized
Maffioli et al. 2023	Myanmar	UCT	Myanmar's Maternal and Child Cash Transfer	RCT - cluster randomized
Haushofer et al. 2016	Kenya	UCT	GiveDirectly	RCT - cluster randomized
d'Errico et al. 2020	Lesotho	UCT	Lesotho's Child Grants Programme	Dif-in-dif
Skoufias et al. 2008	Mexico	CCT	PROGRESA	RCT - cluster randomized
Skoufias et al. 2008	Mexico	UCT	Programa de Apoyo Alimentario	RCT - cluster randomized
Porreca et al. 2019	Lesotho	UCT	Lesotho's Child Grants Programme	RCT - cluster randomized
Salti et al. 2022	Lebanon	UCT	Multipurpose cash assistance to refugees	Regression discontinuity
Shamsuddin 2015	Bangladesh	CCT	Bangladesh's female secondary education stipend programme	Dif-in-dif
Salehi-Isfahani et al. 2018	Iran	UCT	Iran's national cash transfer program	Dif-in-dif

Short citation	Country	Intervention	Program name	Empirical design
Kilburn et al. 2018	Malawi	UCT	Malawi's Social Cash Transfer Program	RCT - cluster randomized
Scarlato et al. 2016	Chile	CCT	Chile Solidario	Dif-in-dif
Behrman et al. 2011	Mexico	CCT	PROGRESA/ Oportunidades	Dif-in-dif
El-Enbaby et al. 2019	Egypt	UCT	Egypt's national cash transfer program, Takaful	Regression discontinuity
Sedlmayr et al. 2020	Uganda	UCT+ (Liveli-hoods)	Village Enterprise program	RCT - cluster randomized
Kashefi et al. 2021	Afghanistan	UCT	Promoting Entrepreneurship among the Youth in Afghanistan	RCT - individually randomized
de Hoop et al. 2018	Lebanon	UCT	"Min Ila" Cash Transfer Programme for Displaced Syrian Children in Lebanon (UNICEF and WFP)	Regression discontinuity
Soares et al. 2010	Paraguay	CCT	Paraguayan pilot CCT programme, Tekopora	Propensity score matching
Habimana et al. 2021	Rwanda	UCT	Rwanda's unconditional cash transfer program (VUP-Direct Support)	Propensity score matching

Short citation	Country	Intervention	Program name	Empirical design
Braido et al. 2012	Brazil	CCT	Bolsa Alimentação (BA)	Propensity score matching
Gazeaud et al. 2023	Tunisia	UCT+ (Livelihoods)	Small scale pilot	RCT - individually randomized
de Oliviera et al. 2017	Brazil	UCT	Benefício de Prestação Continuada, pension program	Regression discontinuity
Barrientos et al. 2015	Colombia	CCT	Columbia's Familias en Acción	Regression discontinuity
Ambler et al. 2019	Pakistan	UCT	Pakistan's Benazir Income Support Program (BISP)	Regression discontinuity
Durr-e-Nayab et al. 2014	Pakistan	UCT	Pakistan's Benazir Income Support Program (BISP)	Propensity score matching
Brugh et al. 2018	Malawi	UCT	Malawi's Social Cash Transfer Program	RCT - cluster randomized
Esquinca 2013	Mexico	UCT	Pensión para Adultos Mayores (PAM)	Dif-in-dif
Pritradrajati 2023	Indonesia	UCT	Indonesia's Unconditional Cash Transfer program	Dif-in-dif combined with matching

Short citation	Country	Intervention	Program name	Empirical design
Marguerie et al. 2023	Cote d'Ivoire	UCT+ (Livelihoods)	A program in post-conflict setting implemented by International Rescue Committee (IRC)	RCT - cluster randomized
Handa et al. 2018	Zambia	UCT	The Child Grant Program (CGP)	RCT - cluster randomized
de Hoop et al. 2020	Malawi	UCT	Malawi Social Cash Transfer Program	RCT - cluster randomized
Biswas 2021	India	CCT	Apni Beti Apna Dhan (ABAD)	Dif-in-dif
Mostert et al. 2020	South Africa	UCT	South Africa Cash Transfer program	Other - specify in notes
Blattman et al. 2016	Uganda	CCT+ (Livelihoods)	Women's INncome Generating Support (WINGS)	RCT - cluster randomized
Skoufias et al. 2013	Mexico	UCT	Programa de Apoyo Alimentario	Dif-in-dif
Calderone 2017	Uganda	UCT	Youth Opportunities Programme	RCT - individually randomized
McIntosh et al. 2022	Rwanda	UCT	Give directly	RCT - individually randomized
Blattman et al. 2020	Uganda	UCT	Youth Opportunities Programme	Dif-in-dif
Alzúa et al. 2013	Mexico	CCT	Progresa	RCT - cluster randomized

Short citation	Country	Intervention	Program name	Empirical design
Rosas et al. 2022	Sierra Leone	CCT+ (Liveli-hoods)	Cash plus training program	RCT - individually randomized
Howell 2023	China	UCT	China's Minimum Income Living Allowance	Regression discontinuity
Asfaw et al. 2017	Zambia	UCT	Child Grant Programme	RCT - cluster randomized
Maluccio 2010	Nicaragua	CCT	Red de Protección Social	RCT - cluster randomized
Kenya CT-OVC Evaluation Team 2012	Kenya	UCT	Cash Transfer Program for Orphans and Vulnerable Children (CT-OVC)	Dif-in-dif
de Hoop et al. 2019	Philippines	CCT	Pantawid Pamilyang Pilipino Program (PPPP)	RCT - cluster randomized
Blattman et al. 2014	Uganda	UCT	Youth Opportunities Programme	RCT - cluster randomized
Kurdi 2021	Yemen	CCT	Cash for Nutrition program within Yemen ECR program	RCT - cluster randomized
Mourelo et al. 2017	Argentina	CCT+ (Liveli-hoods)	Seguro de Capacitacion y Empleo, SCE	Dif-in-dif
Bhanot et al. 2021	Kazakhstan	CCT	Kazakhstan Youth Corps	RCT - cluster randomized

Short citation	Country	Intervention	Program name	Empirical design
Angelucci et al. 2009	Mexico	CCT	Progresa	RCT - cluster randomized
Cunha 2014	Mexico	UCT	Programa de Apoyo Alimentario	Dif-in-dif
Haushofer et al. 2023	Kenya	UCT	program conducted with a large international NGO	RCT - cluster randomized
Galiani et al. 2013	Honduras	CCT	Programa de Asignación Familiar (PRAF), Honduras	RCT - cluster randomized
Bergolo et al. 2018	Uruguay	CCT	Uruguay – Asignaciones Familiares-Plan de Equidad	Regression discontinuity
Qayyum et al. 2023	Pakistan	UCT	Pakistan's Benazir Income Support Program (BISP)	Regression discontinuity
Macours et al. 2022	Nicaragua	CCT	Atencion a Crisis (pilot)	RCT - cluster randomized
Egger et al. 2022	Kenya	UCT	GiveDirectly	RCT - cluster randomized
Scarlato et al. 2019	South Africa	UCT	Child Support Grant (CSG), South Africa	Regression discontinuity
Araujo et al. 2017	Ecuador	UCT	Bono Solidario / Bono de Desarrollo Humano	Regression discontinuity

Short citation	Country	Intervention	Program name	Empirical design
Bossuroy et al. 2022	Niger	UCT+ (Liveli-hoods)	Additional programming within Niger national safety net program	RCT - cluster randomized
Rahman et al. 2021	Bangladesh	UCT+ (Liveli-hoods)	BRAC's Ultra-Poor Graduation, Bangladesh	RCT - cluster randomized
Amarante et al. 2013	Uruguay	CCT+ (Liveli-hoods)	PANES, Uruguay	Regression discontinuity
Escudero et al. 2020	Uruguay	UCT	Plan de Asistencia Nacional a la Emergencia Social	Dif-in-dif combined with matching
Cahyadi et al. 2020	Indonesia	CCT	Program Keluarga Harapan	RCT - cluster randomized
Banerjee et al. 2021	India	UCT+ (Liveli-hoods)	TUP program in West Bengal, India	RCT - individually randomized
Handa et al. 2021	Ghana	UCT	Livelihood Empowerment Against Poverty (LEAP)	Dif-in-dif
Ahmed et al. 2024	Bangladesh	UCT	Transfer Modality Research Initiative in Bangladesh (TMRI)	RCT - cluster randomized
Attanasio et al. 2006	Colombia	CCT	Familias en Accion Programme	Dif-in-dif

Short citation	Country	Intervention	Program name	Empirical design
Fahey et al. 2019	Tanzania	CCT+ (Other)	Small intervention set up by the researchers	Dif-in-dif
Maluccio 2004	Nicaragua	CCT	Red de Protección Social (RPS), Nicaragua	RCT - cluster randomized
Altındağ et al. 2023	Lebanon	UCT	Cash transfers to Syrian refugees in Lebanon	Regression discontinuity
Premand et al. 2022	Niger	UCT	Additional programming within Niger national safety net program	RCT - cluster randomized
Gupta et al. 2024	Uganda	UCT	GiveDirectly, refugee setting	RCT - cluster randomized
Ali et al. 2022	Somalia	UCT+ (Other)	Save the children intervention in an IDP setting	RCT - individually randomized
d'Agostino et al. 2018	South Africa	UCT	Child Support Grant (CSG), South Africa	Regression discontinuity
Briaux et al. 2020	Togo	UCT+ (Other)	Pilot CT program by Togo government	RCT - cluster randomized
Angelucci et al. 2012	Mexico	CCT	Oportunidades (Urban)	Propensity score matching
Ferro et al. 2010	Brazil	CCT	Brazilian Bolsa Escola CCT	Propensity score matching
Gertler et al. 2012	Mexico	CCT	PROGRESA	RCT - cluster randomized

Short citation	Country	Intervention	Program name	Empirical design
Dona 2023	Chile	CCT	Unique Family Subsidy (SUF, after its name in Spanish).	Dif-in-dif
Banerjee et al. 2015	Ghana	CCT+ (Livelihoods)	Graduation model programs implemented by a local NGO	Other - specify in notes
Banerjee et al. 2017	Honduras	CCT	Programa de Asignación Familiar - Phase II (PRAF II)	RCT - cluster randomized
Martins et al. 2016	Brazil	CCT	Bolsa Família	Propensity score matching
Alatas 2011	Indonesia	CCT	Keluarga Harapan	Other - specify in notes
World Bank 2014	Philippines	CCT	Pantawid Pamilyang Pilipino Program (PPPP)	RCT - cluster randomized
Hussam et al. 2022	India	UCT	Small scale intervention set up by researchers	RCT - individually randomized
Brooks et al. 2022	Kenya	UCT	Small scale CT program during COVID set up by the researchers	RCT - individually randomized
Dietrich et al. 2019	Kenya	UCT	Kenya's Hunger Safety Net Programme (pilot)	RCT - cluster randomized

Short citation	Country	Intervention	Program name	Empirical design
Brudevold-Newman et al. 2017	Kenya	UCT	A microfranchising intervention + UCT	RCT - individually randomized
Aggarwal et al. 2022	Malawi	UCT	GiveDirectly	RCT - cluster randomized
Alzúa et al. 2019	Nigeria	UCT	Government pilot program in one Nigerian state	RCT - cluster randomized
Cooke et al. 2019	Uganda	UCT	GiveDirectly	RCT - individually randomized
Hidrobo et al. 2014	Ecuador	UCT	WFP assistance to Colombian refugees	RCT - cluster randomized
Carneiro et al. 2021	Nigeria	UCT+ (Other)	The Child Development Grant Programme	RCT - cluster randomized
Gilligan et al. 2013	Uganda	UCT	WFP Food and CT Intervention Linked to ECD Center Participation	RCT - cluster randomized
Araujo et al. 2017	Ecuador	UCT	Bono Solidario (Solidarity Bond).	RCT - cluster randomized

Table A5: Screening: Excluded terms

Excluded Terms	**Excluded Journals:**	**Not excluded if:**
in-vitro	Fertility and sterility	cash
fish	Reproductive BioMedicine Online	UCT
mouse/mice	Chemical Engineering Journal	CCT
rats	Chinese Journal of Chemical Engineering	
rabbit/rabbits	International Journal of Heat and Mass Transfer	
rodent	Applied Thermal Engineering	
cadavers/cadaver/cadaveric	Blood	
piglet/piglets	ACS Applied Materials and Interfaces	
porcine	Journal of Material Chemistry A	
murine	Journal of Energy Chemistry	
broiler	Journal of Alloys and Compounds	
catalytic	International Journal of Electrochemical Sciences	
molecular	Chinese Chemical Letters	
cells	Archives of Physical Medicine and Rehabilitation	
surgery/surgical	Energy Conversion and Management	
chemical + engineering	Journal of Evidence Based Dental Practice	
hydrogen	Journal of Hazardous Materials	

electrochemical	Electrochimica Acta	
nitrogen	Applied Catalysts B-Environmental	
membrane	Mitochondrial DNA Part B-Resources	
bacteria	Chinese Journal of Catalysis	
thermal	International Journal of Thermal Sciences	
reactor	Accounts of Chemical Research	
temperature	European Journal of Obstetrics & Gynecology and Reproductive Biology	
pollution	Nuclear Engineering and Design	
chemical	Journal of Physical Chemistry C	
carbon	Chemosphere	
engineering	Aquaculture	
Obstetrics	eClinicalMedicine	
chemistry	Nuclear Engineering and Technology	
sea	Journal of Lipid Research	
cardiac	BMC Pregnancy and Childbirth	
neural	ACS Sustainable Chemistry & Engineering	
river	Atherosclerosis	
psychotherapy	Journal of the American Chemical Society	
social media	Animals	
hiv	ACS Nano	

disease	Human Reproduction	
pesticide	Science of the Total Environment	
healthy control(s)	Applied Energy	
elections		
cancer		
laws		
embryo		
soil		
irrigation		
epa-		
placebo		
antibiotic		
europe		
lipids		
pollination		
shareholder		
species		
stem		
ocean		
cultivation		
marine		
corporation		

corporate		
transplant		
expropriation		
agroforestry		
acid		
climate change		
Heat transfer		
IVF		
Nanoparticles		
stock market		
stock exchange		
bankruptcy		
electoral		
holistic		
posture/postural		
lipid		
blastocyst		
sleep		
bilingual		
addiction		
engine		

Table A6: Estimated total treatment effects on consumption

	No discounting			5% discounting		
	No effects	Dissipation rate:		No effects	Dissipation rate:	
	> 13 months	75%	50%	> 13 months	75%	50%
Panel A: Main treatment effect estimate, non cash+ livelihoods ($1.33)						
Program duration: 6 months	25.31	61.88	40.79	30.21	54.67	38.14
19 months	42.62	79.20	58.11	50.22	71.83	54.01
24 months	49.28	85.86	64.77	57.92	75.80	59.42
Panel B: Main treatment effect estimate, cash+ livelihoods ($8.37)						
Program duration: 6 months	159.03	388.85	256.33	156.10	343.53	239.68
19 months	267.84	497.66	365.14	259.47	451.39	339.38
24 months	309.69	539.51	406.99	299.23	476.31	373.38
Panel C: Treatment effects for livelihoods cash+ excluding outliers ($1.881)						
Program duration: 6 months	35.74	87.39	57.61	35.08	77.20	53.86
19 months	60.19	111.84	82.06	58.31	101.44	76.27
24 months	69.60	121.24	91.46	67.25	107.04	83.91

Notes: This table presents the estimated cumulative effect of cash transfers on consumption under a range of scenarios. Panel A employs the estimated consumption effect for cash interventions excluding cash + livelihoods; Panel B employs the estimated effect for cash + livelihoods interventions; Panel C employs the estimated effect for cash + livelihoods interventions, while dropping estimates above the 90th percentile of the full distribution. The rows summarize different assumptions about the program duration in which the consumption effects are observed; the columns different assumptions about the rate of dissipation of effects post-program and the discounting of longer-term effects. The discount rate employed in these scenarios is 5%, and all consumption benefits are discounted vis-a-vis the first program year identified as the baseline year. We then multiple by this gap by 100 / 1.63 (our main pooled treatment effect estimate for the effects of cash transfers on consumption) to calculate the total transfer amount required.

Table A7: Poverty Gap Analysis by Country

Country	Household size	Total transfer observed	Poverty gap index	Cumulative transfer required	Monthly transfer required	Baseline year	PIP data year
Bangladesh	4.74	794.34	0.009	148.63	7.82	2013.6	2022
Brazil	5.23	1682.88	0.011	194.07	10.21	2003.7	2022
Colombia	6.02	2084.43	0.024	501.51	26.40	2002	2022
Ecuador	4.12	4246.28	0.010	149.46	7.87	2011	2022
Ghana	7.63	191.48	0.048	1282.87	67.52	2014.4	2016
Honduras	5.81	368.52	0.050	1025.54	53.98	2014	2019
Indonesia	5.19	1995.09	0.001	22.87	1.20	2007	2023
India	3.79	433.28	0.027	358.02	18.84	2015.5	2021
Kenya	4.72	945.59	0.136	2266.03	119.26	2014.8	2021
Liberia	4.76	980.39	0.145	2428.75	127.83	2018	2016
Lesotho	6.14	795.73	0.094	2031.14	106.90	2012	2017
Mexico	5.02	882.62	0.003	54.09	2.85	2002	2022
Malawi	4.57	682.31	0.321	5159.86	271.57	2016.1	2019
Niger	9.60	818.70	0.100	3363.23	177.01	2013.3	2021
Nigeria	7.56	745.91	0.125	3314.98	174.47	2014	2018
Nicaragua	5.61	902.17	0.008	149.29	7.86	2003.3	2014
Pakistan	6.95	4677.83	0.004	105.10	5.53	2011.8	2018
Philippines	5.93	2423.11	0.012	256.92	13.52	2011	2021

Continued on next page

Table A7 – *Continued from previous page*

Country	Household size	Total transfer observed	Poverty gap index	Cumulative transfer required	Monthly transfer required	Baseline year	PIP data year
Rwanda	5.08	1321.24	0.209	3733.46	196.50	2016.3	2016
Togo	8.50	234.92	0.055	1649.20	86.80	2014	2021
Tunisia	4.61	671.35	0.001	13.91	0.73	2016	2021
Uganda	6.31	798.85	0.130	2873.98	151.26	2010.4	2019
Yemen	6.36	918.02	0.042	946.49	49.82	2016	2014
South Africa	4.11	6572.49	0.061	884.57	46.56	2008	2014
Zambia	5.48	680.67	0.308	5949.62	313.14	2010	2022
Zimbabwe	1.69	948.73	0.134	794.32	41.81	2013	2019

Notes: The table summarizes data for all countries in the sample that have data available in the PIP as well as data on household size reported in the sample. National-level poverty and consumption data is drawn from World Bank PIP database and uses the most recent data available on the poverty gap; all poverty estimates are expressed in 2011 PPP. The total transfer observed reports the mean transfer observed in this sample (in 2010 PPP) for each country. The total transfer amount required to close the poverty gap is estimated as follows: the individual-level poverty gap (expressed as a percentage) is multiplied by the poverty line, the household size, and 30 to generate an estimated absolute household monthly poverty gap (the amount of additional consumption that is required to bring a typical household to the poverty line).

A1 Details on search and screening

Following the initial search outlined in Table A2 and described in Section 2.1, we identify 10,726 records. These were added to Rayyan, an online tool used for compiling and screening academic literature (particularly for meta-analyses). Using the "detect duplicates" feature on the platform we were able to identify and then exclude any duplicates; a threshold of 95%+ similarity score was used for this purpose.[45]

Studies identified as being in a language other than English were then excluded. We initially screened records by hand, and following the screening of around 200 records, we then utilized Rayyan's AI rating; this uses previous decisions to assign a rating system of zero to five, with zero corresponding to a very high likelihood of inclusion (To limit the risk of false positives for exlusion, we conservatively excluded only studies with a rating < 1. All other records, numbering 6561, were passed to title and abstract screening.

At this point, we also included results from our Google Scholar search and a small number of we found to be relevant but were missed in the initial searches. The title and abstract screening involved, first, excluding records based on keywords and journals of publication; the full list of exclusion terms and journals deemed to be corresponding to domains outside of the area of interest is provided in Table A5. Then, studies were judged for eligibility based on title and abstract reading. Following this screening, 381 records were moved to the full-text download and screening stage. These papers were read in full and eligibility was judged on relevance of intervention, outcome and methodology, as well as quality and availability of results, as described in Section 2.1.

[45]Details on how the score is calculated can be found here: https://help.rayyan.ai/hc/en-us/articles/22697630697617-Understanding-Rayyan-A-Comprehensive-Overview.

A2 Details on data cleaning

The objective of this section is to provide more details about the data cleaning process used to construct estimates of the intervention effects and transfer sizes.

First, there are four studies that report exact p-values rather than standard errors; in these cases, we construct standard errors using the Stata function "invttail". Since the degrees of freedom is not generally specified in the text, we assume a number of degrees of freedom that is one lower than the number of clusters. For de Hoop et al. (2018), we assume 73 degrees of freedom; for Habimana et al. (2021), we assume 42 degrees of freedom; for Premand and Barry (2022), we assume 151 degrees of freedom; for De Hoop, Groppo and Handa (2020), we assume 28 degrees of freedom for the results in Malawi and 91 for the results in Zambia.

Second, there are three studies that report standardized coefficient estimates; we seek to convert these to linear estimates using the mean and standard deviation reported in the control arm, and are able to implement this conversion for Bossuroy et al. (2022). For two other relevant papers, Handa et al. (2018*a*) and Martins and Monteiro (2016), the analysis reports means of the relevant variables (in both cases, consumption) at baseline but no standard deviations; accordingly, we calculate the standard deviation as a fraction of the mean using the ratio of the mean to the standard deviation reported in Bossuroy et al. (2022).

Third, there are six studies that report log coefficients; we convert these coefficients to linear estimates by multiplying by the mean in the control arm or, if not reported, the mean at baseline. Kilburn et al. (2018) reports baseline summary statistics for total consumption, and we construct an estimated mean for food consumption using the food share in total consumption from another related paper (Handa et al., 2018*a*), given that the mean for food consumption was not directly reported. Braido, Olinto and Perrone (2012), Macours, Premand and Vakis (2022), and Salti et al. (2022) directly report the relevant means in the control arm. Brooks et al. (2022) reports a mean that seems to already be an inverse hyperbolic sine transformation of the relevant variable; thus we replace it with the linear mean extracted from the replication data.

Fourth, there are a large number of studies that report estimates for consumption or income per capita (or per adult equivalent) and we convert these estimates to household-level estimates by multiplying by household size (again, as reported in the control arm or at baseline). For a majority of papers, we can draw on an estimate of household size reported in the paper text (Abdoulayi, 2017; Alatas, 2012; Banerjee et al., 2015; Banerjee, Duflo and Sharma, 2021; Briaux et al., 2020; Brugh et al., 2018; de Hoop et al., 2018; Durr-E-Nayab and Farooq, 2014; Gazeaud et al., 2023; Gertler, Martinez and Rubio-Codina, 2012; Gilligan et al., 2013; Habimana et al., 2021; Handa et al., 2018*a*; McIntosh and Zeitlin, 2022; Macours, Premand and Vakis, 2022; Maluccio, 2010; Porreca and Rosati, 2019; Premand and Barry, 2022; Rahman, Bhattacharjee and Das, 2021; Skoufias, Unar and Gonzalez-Cossio, 2008; Skoufias, Unar and Gonzalez De Cossio, 2013; World Bank, 2013). For a number of other papers, household size is not reported and thus we preferentially utilize an estimate of household size from another study in this analysis conducted in the same country.[46] There are three papers missing estimates of household size that are unique country sites, and thus we use estimates from other sources.[47]

For papers reporting estimates per adult equivalent, the information specified varies. Four papers directly report the mean number of adult equivalents in the control arm (Angelucci, Attanasio and Di Maro, 2012; Azeem, Mugera and Schilizzi, 2019; Dietrich and Schmerzeck, 2019); and for one paper (The Kenya CT-OVC Evaluation Team, 2012), the information was extracted from a related report for the same trial.[48] For other papers, we use a similar strategy of extracting data from other studies in the sample. For Qayyum and Nigar (2023) conducted in Pakistan, we use an estimate of household size from the Pakistan sample in (Banerjee et al., 2015), but the paper then specifies the definition of adult equivalent. For d'Agostino, Scarlato and Napolitano (2018) conducted in South Africa, we use an estimate of household size from Von Fintel and Pienaar (2016), and attempt to calculate adult equiva-

[46]For d'Errico et al. (2020) conducted in Lesotho, we use an estimate of household size from the Lesotho sample in Porreca and Rosati (2019). For Shamsuddin (2015) conducted in Pakistan, we use an estimate of household size from the Pakistan sample in Banerjee et al. (2015). For De Hoop et al. (2019) conducted in the Philippines, we use an estimate from the Philippins sample in Porreca and Rosati (2019). For Cunha (2014) conducted in Mexico, we use an estimate of the household size from Skoufias, Unar and Gonzalez De Cossio (2013). For Haushofer, Mudida and Shapiro (2020) conducted in Kenya, we use an estimate of the household size from Egger et al. (2022). For Macours, Premand and Vakis (2022) and Maluccio (2010) conducted in Nicaragua, we use the household size from the Nicaraguan sample analyzed in Alzúa, Cruces and Ripani (2013). For Cahyadi et al. (2020) conducted in Indonesia, we use an estimate of household size from Alatas (2012). For Martins and Monteiro (2016) conducted in Brazil, we use an estimate from Braido, Olinto and Perrone (2012).

[47]For Rosas, Acevedo and Zaldivar (2022) conducted in urban Sierra Leone, we use an estimate of household size from the most recent Demographic and Health Survey. For Amarante and Burn (2018) conducted in Uruguay, we use an estimate from this source, retrieved on April 5, 2024: https://www.arcgis.com/home/item.html?id=ba279db8d2454eb3a9fc21415db24fed. For Howell (2023) conducted in China, we use an estimate from this source, retrieved on April 5, 2024: https://www.caixinglobal.com/2021-05-13/charts-of-the-day-chinas-shrinking-household-size-101712537.html.

[48]This report can be found at the following link, and was retrieved on April 4, 2024: https://www.opml.co.uk/files/Publications/6020-cash-transfer-OVC-kenya/opm-ct-ovc-evaluation-report-july2010-final-kenya-2010-019.pdf

lence using guidance from Woolard and Leibbrandt (1999), though an exact equivalent scale is not provided. For Bossuroy et al. (2022) conducted in Niger, we use an estimate of household size from Premand and Barry (2022), and estimate the number of adult equivalents using a formula provided directly in the paper.

The final step for both the intervention effects and the transfer size estimates is to convert all quantities to 2010 purchasing power parity dollars. We proceed using the following steps: first, we download the CPI series for each country from the World Bank and merge these using the appropriate base year. We perform the same merge for the USD exchange rate series and the PPP exchange rate series, again drawing on World Bank data. We then convert any quantities expressed in USD to local currency units (LCUs) using the USD exchange rates in the appropriate year; and then convert to 2010 prices using CPI data, and to USD PPP using the 2010 PPP estimated rates.[49]

[49] There are several countries for which modifications to this procedure were required. For Argentina, no CPI data is available from the World Bank or IMF, and the PPP conversion factor for private consumption is only available starting from 2011. Accordingly, we use the CPI series compiled by the Federal Reserve Bank of St. Louis and use the PPP conversion factor for gross domestic product. For Liberia and Yemen, no CPI data is available from the World Bank, and thus we use price series provided by the IMF. For Somalia, the relevant estimates are provided in 2017 USD; no CPI data is available from the World Bank and the IMF series begins only in 2014; we use the 2017 USD-shilling exchange rate and the 2017 PPP rate for GDP. For Sierra Leone, the leone had a redenomination of the old leone (SLL) at a rate of SLL 1000 to SLE 1. The World Bank's PPP exchange rate has been adjusted for this (even before the reform), and thus the LCUs are adjusted accordingly for this redenomination before converting to PPPs.

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IFPRI HEADQUARTERS
1201 Eye Street, NW
Washington, DC 20005 USA
Tel.: +1-202-862-5600
Fax: +1-202-862-5606
Email: ifpri@cgiar.org