A Systematic Review and Meta-Analysis of the Educational Effects of Unconditional Cash Transfers

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Table of Contents

Table of Contents 1

List of Tables and Figures 2

Abstract 3

1. Introduction 4

2. Literature Review

2.1 Theoretical Framework 7

2.2 Empirical Evaluations of Cash Transfer Programmes 7

2.3 Systematic Reviews and Meta-Analyses of Cash Transfer Programmes 8

3. Literature Search

3.1 Creating a List of UCTs 10

3.2 Identifying Evaluation Papers for Each Programme 10

3.3 Selecting Eligible Papers 11

3.4 Coding Effect Sizes and Standard Errors 12

3.5 Coding Programme and Country Characteristics 13

3.6 Coding Risk of Bias 13

4. Meta-Analysis Method

4.1 Obtaining the Summary UCT Effect Size 16

4.2 Assessing Heterogeneity 17

4.3 Exploring Heterogeneity 17

5. Results

5.1 Meta-Analysis Results 18

5.2 Meta-Regression Results 21

6. Conclusion 26

References 29

List of Tables

Table 1: Number of Papers in Analysis Sample by Publication Year 12
Table 2: Summary Statistics of Analysis Sample 15
Table 3: Meta-Analysis Results 18
Table 4: Meta-Regression Results 21

List of Figures

Figure 1: Forest Plot of Overall UCT Impact on Enrolment 19
Figure 2: Forest Plot of Overall UCT Impact on Attendance 20
Figure 3: Forest Plot of Overall UCT Impact on Enrolment (High Risk of Bias Studies Excluded) 20
Figure 4: Forest Plot of Overall UCT Impact on Attendance (High Risk of Bias Studies Excluded) 21
Figure 5: Subgroup Analysis of UCT Impact on Enrolment (Pilot vs. Non-Pilot) 23
Figure 6: Subgroup Analysis of UCT Impact on Attendance (Pilot vs. Non-Pilot) 24
Figure 7: Meta-Regression Bubble Plots 25

Abstract

In this review of the educational impact of unconditional cash transfer programmes (UCTs), I systematically search the impact evaluation literature and find 38 papers that evaluate 22 programmes in 18 countries. I quantitatively synthesise the reported effect sizes from these papers using a random-effects meta-analysis model and find a statistically significant positive impact on both enrolment and attendance, suggesting that UCTs are an effective social intervention for policymakers aiming to improve educational attainment. In line with previous research syntheses of cash transfer programmes, I also find significant heterogeneity in effect sizes across studies, which I attempt to explain in a meta-regression with two programme design features and two country-specific characteristics. The results suggest that transfer size and whether the programme is a pilot are irrelevant to UCT impact. I also find no moderating effects of income per capita and the proportion of people living in poverty in the economy, lending no support to a recently developed theoretical framework for UCTs. The findings and limitations of the systematic review and meta-analysis yield three recommendations for future UCT evaluations.

Keywords: Cash transfer programmes, unconditional cash transfers, education, impact evaluation, systematic review, meta-analysis

1. Introduction

"By 2030, ensure that all girls and boys complete free, equitable and quality primary and secondary education leading to relevant and effective learning outcomes."

– Target 4.1, Sustainable Development Goals.

In 2015, the United Nations set the ambitious goal of universal completion of at least secondary education, recognising that education is a key driver for poverty alleviation, reduction of inequality, and improvement of other welfare measures (Glewwe and Kremer, 2006). However, the task remains arduous as educational attainment in many developing countries remains low. For example, out-of-school rates stand at 33.6 percent in low-income countries in 2020, only one percentage point lower than a decade ago (UNESCO Institute for Statistics, 2021).

Achieving universal completion of at least secondary education requires not only the supply of schools and teachers, but also household demand for education (Baird et al., 2014). Conditional cash transfer programmes (CCTs) stimulate this demand by providing cash to poor households on the condition that they send their children to school. The *PROGRESA* (later rebranded as *Oportunidades*) in Mexico is one of the first nationwide implementations of a CCT, and Skoufias and Parker (2001) are among the earliest to evaluate its impact, reporting a 10-percent increase in educational attainment. As the evidence for its effectiveness grew, many countries started implementing their own programmes. The World Bank (2018) reports that CCTs are now present in 61 countries.

More recently, unconditional cash transfer programmes (UCTs) have become popular and are found to be equally effective (Baird, McIntosh, and Özler, 2011; Haushofer and Shapiro, 2016). In contrast to CCTs, these programmes do not place any specific conditions for cash given to eligible households, which are often selected using means-tested or proxy-means tested identification methods. In this sense, a UCT is a type of targeted cash transfer programme. While the most common forms of UCTs are social pensions and child support grants, they are also found in the form of aid to vulnerable groups in society, such as the Cash Transfer to Orphans and Vulnerable Children (CT-OVC) in Kenya, or cash assistance to refugees such as the Multipurpose Cash Assistance administered by the United Nations High Commissioner of Refugees (UNHCR) to Syrian refugees in Lebanon. The objectives of UCTs are to alleviate poverty, increase educational attainment, and improve health outcomes and food consumption, among others.

There are at least three reasons for preferring UCTs over CCTs. The first reason stems from theoretical results of household decision-making models for investment in human capital showing that conditions can distort household behaviour from the optimal if households are fully rational (Hanlon, Barrientos, and Hulme, 2010). It is argued that an increase in income alone through a cash transfer is sufficient to induce the socially optimal level of schooling for children in financially constrained households. Another reason for UCTs is the consideration that monitoring and enforcement of conditions are expensive. Özler (2020) suggests that removing conditionality can be more cost-effective if UCTs yield similar results. Finally, Freeland (2007) posits a moral argument against CCTs, insisting that attaching conditions to social assistance can deprive the neediest groups in society of the help that they deserve, especially when households that most desperately need the assistance are the ones who cannot meet all the conditions.

Previous reviews of the cash transfer impact evaluation literature have found that the number of UCT evaluations is scarce compared to that of CCTs, and there are calls from both researchers and practitioners to increase the evidence base for UCTs (Baird et al., 2014; Bastagli et al., 2016). How has the impact evaluation literature evolved in response to these calls, and do UCT evaluations tend to find positive results on the educational attainment of beneficiary households? To answer this first research question, I systematically search the impact evaluation literature and find 38 papers that evaluate the educational impact of 22 programmes in 18 countries. Using a random-effects meta-analysis model, I quantitatively synthesise the evidence and find a statistically significant positive impact on both enrolment and attendance that is robust to the exclusion of papers with a high risk of bias.

In line with previous systematic reviews and meta-analyses of cash transfer programmes, I also find significant heterogeneity in effect sizes across studies. This implies that there is considerable variation in UCT educational impact across different programmes and raises the question of what factors determine the effectiveness of UCTs. I attempt to answer this second research question using a multivariate meta-regression with four explanatory variables encompassing programme design features and country-specific characteristics. The results show that there is no association between these factors and UCT impact on school enrolment or attendance.

This review contributes to the empirical literature on cash transfer programmes in four ways. Firstly, it fills a gap in the impact evaluation literature as the first systematic review and meta-analysis of the educational impact of UCTs. Previous meta-analyses of UCTs, such as Pega et al. (2017) and Siddiqi et al. (2018), focus on health and nutritional outcomes. Only two reviews have meta-analysed the educational impact of cash transfer programmes: Baird et al. (2014) compared the effectiveness of CCTs and UCTs using a sample of 26 CCTs and five UCTs, while Garcia and Saavedra (2017) synthesised the evidence from 94 empirical evaluations of 47 CCTs. This review is the first synthesis of the rapidly growing literature on UCT educational impact.

Secondly, this review enhances our understanding of how heterogeneity in UCT programme design features affects its impact. Previous meta-analyses of CCTs find that the transfer size and whether the programme is a pilot have no statistically significant effect on programme impact (Baird et al., 2014; Garcia and Saavedra, 2016). The meta-regression results reveal the same pattern for UCTs, suggesting that (i) changing the intensity of the income effect by varying the transfer size may be irrelevant to UCT effectiveness and (ii) there may be negligible differences between the effect of national programmes that raise permanent income and pilots that do not.

While previous meta-analyses explain the heterogeneity in impact estimates using programme design characteristics, a novel contribution of this review is assessing the moderating effects of two country-specific factors, income per capita and the poverty headcount ratio, on UCT impact. The inclusion of these contextual characteristics is motivated by a recently developed theoretical framework for UCTs by Churchill et al. (2021). Their model predicts that the level of income and the proportion of people living in poverty in the economy affect the magnitude of the change in household investment in education due to government transfers. By including these two contextual characteristics as explanatory variables in the meta-regression, I test these hypotheses empirically and find no statistically significant moderating effect of income per capita or the poverty headcount ratio on UCT impact.

Finally, this review reveals gaps in the impact evaluation literature and offers three recommendations for future UCT evaluations. After reviewing the existing evidence, I highlight the need to improve both the quantity and content of UCT evaluations. The first recommendation is to incorporate evaluation and monitoring of educational outcomes into national social safety net programmes as a means of expanding the evidence base for UCT educational impact. Second, I present best practices for the reporting of impact estimates, evaluation methodology, and study context to allow meta-analyses to be a useful tool for inferring generalised causal associations between UCTs and their intended outcomes across different settings. Lastly, I point to the evaluation of the longer-term effects of UCTs, such as those on learning outcomes and future earnings, as a worthwhile pursuit for future research.

The rest of this paper is organised as follows: Section 2 contains a literature review of the theoretical framework for household investment in human capital and highlights research gaps in the empirical evaluation of UCTs as well as existing systematic reviews and meta-analyses. Section 3 reports the process and results of literature search and presents some summary statistics. Section 4 describes the meta-analysis methodology. Section 5 presents the results. Section 6 discusses the findings and limitations and concludes with implications for policy and future research.

2. Literature Review

2.1 Theoretical Framework

The household decision-making models for investment in human capital first introduced by Becker (1962) and Ben-Porath (1967) underpin arguments for the educational effects of cash transfer programmes. These models suggest that households send their children to attend an additional year of school when the expected marginal benefits of investment in human capital exceed the present discounted costs. Mincer's (1974) human capital earnings function specifies foregone earnings as the opportunity cost of schooling; thus, households face a trade-off between earnings from labour and education. In particular, when child labour is a substitute for adult labour, Basu and Van (1998) show that multiple equilibria exist in the labour market: one where children work, and another where adult income is high and children do not work. This result implies that an increase in household income can induce higher levels of investment in education for children. Baland and Robinson (2000) support this hypothesis by analysing the trade-off between child labour and investment in child human capital, concluding that child labour arises in equilibrium due to household financial constraints.

Building upon the child labour models of Basu and Van (1998) and Baland and Robinson (2000), Ferreira, Filmer, and Schady (2017) develop the earliest theoretical framework for the educational effects of cash transfer programmes. Their model demonstrates that a CCT has an unambiguous positive impact on school enrolment of eligible children. More recently, Churchill et al. (2021) develop a theory on how cash transfers without conditions affect household decisions on investment in education. They highlight the importance of the magnitude of the income effect of the transfers. The model predicts that cash transfers are more effective in increasing schooling when the economy has higher average levels of income and a smaller fraction of poor households, such that there is a stronger income effect for each individual household receiving transfers. I test these predictions by including income per capita and the poverty headcount ratio as explanatory variables in a meta-regression.

2.2 Empirical Evaluations of Cash Transfer Programmes

Empirical evaluations of cash transfer programmes aim to demonstrate causal effect. Researchers utilise experimental designs to establish that improvements in an outcome of interest can be directly attributed to a programme and not to other factors. Randomised controlled trials are the most popular experimental design in impact evaluation. Participants are randomly assigned to receive a transfer, and researchers observe outcome differences between the treatment and control groups to infer the causal effect of the programme (see for example Barrera-Osorio et al., 2008; Robertson et al., 2013; Benhassine et al., 2015; and Akresh et al., 2016). Even though experimental designs are the preferred method of demonstrating causal effect, they are often costly and difficult to implement. An alternative way of establishing causality is by analysing administrative data using quasi-experimental methods such as difference-in-differences (see de Carvalho Filho, 2012, and Ponczek, 2011), regression discontinuity design (see Skoufias and McClafferty, 2001, Attanasio et al., 2010, and Bergolo and Galván, 2018), and propensity score matching (see Ferra et al., 2010, Veras Soares, 2010, and Coetzee, 2013). Regardless of the evaluation method used, reviews show that studies often find statistically significant positive effects of cash transfers across a range of outcome domains, including health, nutrition, economic, and educational outcomes (Fiszbein et al., 2009; Snilstveit et al., 2016; Bastagli et al., 2018).

While impact evaluation techniques can determine the causal effect of a programme, it is more difficult to study how programme design features and the economic context affect programme effectiveness. An experiment must be specifically designed to study such research questions. For example, to study how payment amount affects increases in school enrolment due to a cash transfer, Filmer and Schady (2011) estimate the impact of a CCT in Cambodia that offers different magnitudes of payments to comparable households. In addition, a programme evaluation captures the effect size in its experimental setting; to study how a programme will perform in another economic context (e.g., in a developing country or in a rural area) requires implementation of an identical programme elsewhere to make a controlled comparison. As much as researchers may want to implement such programmes to generate insights, these are often expensive and may not be aligned with the social purpose of these interventions and the objectives of policymakers who determine the final programme design.

A meta-regression is an alternative method for exploring how UCTs with different designs perform in different economic contexts without the need to set up a new experiment. By performing a multiple linear regression with the effect sizes of several UCTs as the dependent variable and programme features and country-specific factors as explanatory variables, I simultaneously control for programme and contextual characteristics to explore their moderating effect on UCT impact.

2.3 Systematic Reviews and Meta-Analyses of Cash Transfer Programmes

Systematic reviews and meta-analyses are becoming increasingly popular in the social sciences as a means of synthesising the most relevant research evidence to answer specific research questions and testing hypotheses (Petticrew and Roberts, 2006). A systematic review goes beyond the scope of a traditional literature review; instead, it identifies, organises, and appraises all relevant studies on a given topic. Many systematic reviews include a meta-analysis, which involves statistical techniques for quantitatively synthesising the results of several studies into a single summary estimate. The increasing popularity of such reviews is in part due to the evidence-based policy movement, as they address uncertainty and combine potentially conflicting studies to enable policy decisions based on all available scientifically-sound research. Systematic reviews also inform researchers and practitioners on the current state of knowledge in an area, any inconsistencies within it, and potential research directions to clarify what remains to be known.

Among the many systematic reviews of cash transfer programmes, few focus exclusively on educational outcomes: Gaarder et al. (2010), Glassman et al. (2013), Owusu-Addo et al. (2018), Burch and Ciapponi (2020), and Onwuchekwa et al. (2021) review effects on health, Garoma et al. (2017) and Manley et al. (2020) review effects on nutrition, while Hagen-Zanker et al. (2011) and Yoong et al. (2012) review effects on poverty. Fiszbein et al. (2009) are the first to conduct a systematic review for the educational impact of cash transfer programmes, and they report positive effects for 12 of the 13 programmes included. Other reviews focus on the educational impact of CCTs (see Parker, Rubalcava, and Teruel, 2008; Adato and Bassett, 2009; and Kabeer and Waddington., 2012). To the best of my knowledge, there is no systematic review on the educational effects of UCTs yet.

Only three systematic reviews of the educational effects of cash transfer programmes include a meta-analysis, and none of them focus on UCTs. Baird et al. (2014) use a meta-analysis to compare the effects of UCTs and CCTs and find that the combined effect size of CCTs is larger. The authors also find that, on average, programmes with more stringent conditions and enforcement have larger effect sizes. Snilstveit et al. (2016) review and meta-analyse for the impact of 38 unique cash transfer programmes, only three of which were UCTs. Finally, Garcia and Saavedra (2017) meta-analyse for the impact and cost-effectiveness of 94 CCTs and find similar results to Baird et al. (2014). This systematic review is the first to include a meta-analysis of the evidence for UCT educational impact.

3. Literature Search

3.1 Creating a list of UCTs

It is important for a systematic review to have full coverage of existing studies so that the subsequent meta-analysis is representative of the literature (Nijkamp and Poot, 2004). To study the question of whether UCTs are effective in raising school enrolment and attendance, this systematic review aims to cover the entire population of UCTs. Thus, the first step of the review is to create a list of all past and current UCTs.

I construct this list using information from the two latest publications of the World Bank's (2015, 2018) Social Safety Net Inventory, which are found in the *State of Social Safety Nets* reports and contain information on social safety net programmes from 142 countries, including both conditional and unconditional cash transfers, in-kind transfers, social pensions, child grants, public works, and school feeding programs. I extract the names of all programmes under the "Unconditional Cash Transfer" label and save them onto a spreadsheet together with the country name. The resulting list contains a total of 157 unique programmes in 131 countries.

3.2 Identifying Evaluation Papers for Each Programme

Next, I search two literature databases, Google Scholar and EconLit, to identify impact evaluation papers for each programme. The search is restricted to evaluation papers in English. I use the following key search terms to retrieve the relevant papers:

- *[COUNTRY]* cash transfer evaluation
- *[PROGRAMME]* evaluation
- *[COUNTRY] [PROGRAMME]* evaluation

After searching for all the countries and programmes, I obtain a total of 152 papers for 90 programmes in 64 countries. To ensure that I have not left out any papers, I cross-validate my list with the reference lists of three recent systematic reviews, namely Baird et al. (2014), Garcia and Saavedra (2017), and Bastagli et al. (2018). After checking, I add 12 papers that were previously left out, bringing the total number of UCT evaluation papers to 164.

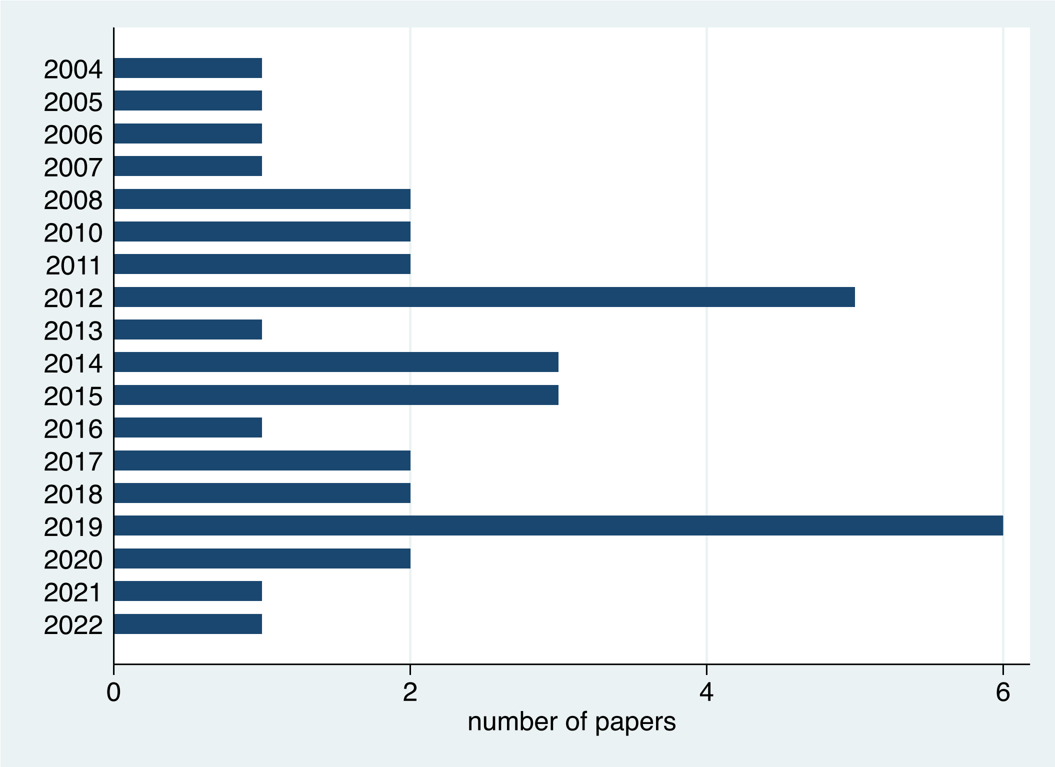
3.3 Selecting Eligible Papers

I then investigate each of the 164 UCT impact evaluation papers and select papers to include in the meta-analysis based on three criteria: (i) the paper must report effect size estimates for either enrolment or attendance, (ii) the paper must report an associated error statistic such as the standard error, t-statistic, or p-value, and (iii) the paper conducts an *ex post* evaluation utilising a treatment-comparison research design (both experimental or quasi-experimental are accepted) rather than an *ex ante* evaluation using structural models or simulations. The first two criteria are necessary for conducting the meta-analysis, as it summarises study effect sizes weighted by their standard errors. The third criterion is included to ensure that the analysis is performed on actual programme outcomes.

Of the 164 evaluation papers, 52 papers report effect sizes on either enrolment or attendance. Among them, only 43 of them report an error statistic and are eligible for inclusion in the meta-analysis. All journal articles that report an effect size also report a corresponding error statistic, and the publications that do not report an error statistic for effect sizes are mostly technical reports or policy briefs. I remove three papers that utilise *ex ante* simulations to evaluate programme impact and two papers that are duplicates (previous working versions of a published paper), leaving a total of 38 eligible papers in the final analysis sample.

The analysis sample contains papers between the years 2004 and 2022, and Table 1 reports the number of papers for each year of publication. 17 papers are journal articles and 11 are working papers. Four of the evaluation papers are technical reports and one is a conference paper. Of the remaining five papers, two are doctorate theses, two are masters' theses, and one is an undergraduate dissertation. More than three-quarters of the analysis sample, or 30 papers, report an effect size for enrolment, while 12 papers report an effect size for attendance. Ten papers evaluate a pilot UCT or a national UCT at its pilot phase. Only seven of the papers utilised random assignment; others utilised quasi-experimental approaches and relied on administrative data or national surveys in conducting the evaluation. More than half of the papers in the analysis sample evaluate UCTs conducted in Africa. Nine papers evaluate UCTs in Latin America and the Caribbean, and the remainder evaluates UCTs in the Middle East (four), South Asia (two), and Eastern Europe and Central Asia (two).

Table 1: Number of Papers in Analysis Sample by Publication Year



3.4 Coding Effect Sizes and Standard Errors

I follow Garcia and Saavedra (2017) in retrieving the "best" effect size estimate for each of the 38 evaluation papers for either enrolment or attendance. When the paper reports an overall programme effect for either enrolment or attendance, I record the estimate from the model with the most comprehensive set of control variables. All effect sizes are measured in terms of the percentage point change in the probability of being enrolled or attending school for a child whose household is in the treatment group compared to a child in the control group; hence, there is no need to convert the effect size estimates to facilitate comparison across papers.

Some papers report programme effects for multiple non-overlapping subgroups. For example, de Carvalho Filho (2012) reports separate effect sizes of the Old Age Pension in Brazil for boys and girls; Econometria Evaluation Team et al. (2020) report separate effect sizes of the Targeted Social Assistance in Georgia for different income groups; and Santana (2008) reports separate effect sizes of the South African Child Support Grant for different age groups. In such instances, I take one effect size estimate per subgroup from the model with the most comprehensive set of control variables and synthesise them into an average effect size using a fixed-effect meta-analysis model.

In some papers, t-statistics or p-values are reported instead of the standard error. I convert them into standard errors. The final list of effect sizes and their corresponding standard errors are tabulated in a spreadsheet.

3.5 Coding Programme and Country Characteristics

I then add new columns in the spreadsheet to code programme and contextual characteristics which are used as explanatory variables in the meta-regression. Following Baird et al. (2014) and Garcia and Saavedra (2017), two programme design characteristics are coded: the transfer amount as recorded by the authors and whether the programme is a pilot at the time of evaluation. To facilitate comparison, the recorded transfer amounts are converted to 2010 US dollars. The mean annual transfer amount is USD 407.05, with a standard deviation of USD 555.77. Most of the UCTs have an annual transfer amount of less than USD 1,000; however, the Multipurpose Cash Assistance to Syrian Refugees in Lebanon is an outlier with an annual transfer amount of USD 2,555.

In addition, I code two country-specific characteristics to test predictions from the theoretical framework in Churchill et al. (2021): the income per capita and the proportion of people living below the national poverty line, or the poverty headcount ratio. Data for these two variables are obtained from the World Bank. When data on the poverty headcount ratio is not available for the years that the UCT is implemented, I record data from the closest year when data is available. The mean income per capita in the analysis sample is USD 2,971.83, and the mean poverty headcount ratio is 41.7 percent.

3.6 Coding Risk of Bias

Finally, I add a categorical variable for the risk of bias. I follow the procedure in Baird et al. (2014) to determine the level of risk of bias of each paper across five categories. If a paper satisfies the requirement for a particular category, I code a “yes” under the column for the category. A paper has a high risk of bias if it fails to satisfy at least three of the categories.

The first category is selection bias and confounding in the programme design. To satisfy this category, any potential for bias in the process of allocation into the treatment group of the study must be eliminated. If the paper does not utilise random assignment, then any source of potential bias must be corrected with an appropriate quasi-experimental method. Most papers that evaluate the UCT using national survey data do not satisfy this category due to the lack of randomisation into treatment groups and potential selection of households in national surveys. Only 17 papers (44.7 percent) of the analysis sample satisfy this category.

To fulfil the second category of the absence of spillovers, crossovers, and contamination, studies with random assignment must address the issue of spillovers from the treatment to the control group by using either geographic or social separation. All but one paper satisfies this category.

The next category is outcome reporting. A paper satisfies this category if results for all relevant outcomes are reported, and there is no apparent selection in reporting outcomes. Nearly all papers (34 papers or 89.5 percent) satisfy this category.

The fourth category of analysis reporting evaluates whether the authors utilise a credible analysis method given the data available. This category is coded as "yes" if an exposition of the reason for using the method is given in the manuscript. If insufficient detail is provided to confirm that the most appropriate analysis method is used, the category is coded as "no." 29 papers (76.3 percent) satisfy this category.

The final category on other risks of bias is the most subjective of the five categories. It includes channels through which there is a possibility that the results reported by the paper are biased, such as retrospective collection of baseline data, use of an inappropriate instrument or a different instrument for the control and treatment groups, collection of information after different follow-up periods for control and treatment groups, and so on (Baird et al., 2014). 10 papers (26.3 percent) satisfy this category.

Overall, 14 papers (36.8 percent) have a low risk of bias, and 15 papers (39.5 percent) have a middle risk of bias. Nine papers (23.7 percent) in the analysis sample have a high risk of bias and are excluded from the meta-analysis in the sensitivity analysis. This is to verify that the results are robust to exclusion of papers with a high risk of bias. A summary of the analysis sample is presented in Table 2. The spreadsheet containing information on all programmes and papers collected at each stage of the literature search is available online at https://bit.ly/UCTsynthesis2022.

Table 2: Summary Statistics of Analysis Sample

Sample of Paper ($N = 38$)	Number	%
Publication Type		
Journal Article	17	44.7
Working Paper	11	28.9
Technical Report	4	10.5
PhD Thesis	2	5.3
Masters Thesis	2	5.3
Undergraduate Dissertation	1	2.6
Conference Paper	1	2.6
Reports Effects On		
Enrolment	30	78.9
Attendance	12	31.6
Programme Characteristics		
Pilot programme	10	26.3
Random assignment	7	18.4
Risk of Bias		
Selection Bias and Confounding - Yes	17	44.7
Spillovers, Crossovers, and Contamination - Yes	37	97.4
Outcome Reporting - Yes	34	89.5
Analysis Reporting - Yes	29	76.3
Other Risk of Bias - Yes	10	26.3
Overall Risk of Bias - Low	14	36.8
Overall Risk of Bias - Middle	15	39.5
Overall Risk of Bias - High	9	23.7
Regional Distribution		
Africa	21	55.2
Latin America and the Caribbeans	9	23.7
Middle East	4	10.5
South Asia	2	5.3
Eastern Europe and Central Asia	2	5.3
	Mean	SD
Programme Characteristics		
Transfer Amount	407.05	555.77
Country Characteristics		
Income Per Capita	2,971.83	2,491.92
Poverty Headcount Ratio	41.7	16.3

4. Meta-Analysis Method

4.1 Obtaining the Summary UCT Effect Size

I use a random-effects model to synthesise the effect sizes of each paper i into a summary UCT effect size for enrolment and attendance respectively:

$$ES_{UCT} = \frac{\sum_i w_i ES_i}{\sum_i w_i} \tag{1}$$

In a random-effects model, the true effect sizes of each paper i are assumed to be different and distributed about a mean (Borenstein et al., 2010). This assumption is appropriate since the evaluations differ in the sample of participants, context, and programme design and implementation.

The weight in a random-effects meta-analysis model is given by:

$$w_i = \frac{1}{\hat{\sigma}_i^2 + \tau^2} \tag{2}$$

where $\hat{\sigma}_i^2$ is the within-study variance, or the square of the standard error reported in paper i, and τ^2 is the between-studies variance which can only be computed if the true effect sizes of all paper are known. Stata uses the DerSimonian and Laird (1986) method to obtain a sample estimate for it:

$$\hat{\tau}^2 = \frac{Q - (k-1)}{C} \tag{3}$$

where Q is the weighted sum of squares of the effect sizes reported by each paper i, $k-1$ is the degrees of freedom or the number of papers minus one, and C is a factor to standardise the estimate into the same index as the within-study variance.

$$Q = \sum_{i=1}^{k} w_i ES_i^2 - \frac{(\sum w_i ES_i)^2}{\sum w_i} \tag{4}$$

$$C = \sum_{i=1}^{k} w_i - \frac{\sum w_i^2}{\sum w_i} \tag{5}$$

All computations are run on Stata utilising the meta-analysis package.

4.2 Assessing Heterogeneity

The I^2 statistic indicates the extent of heterogeneity between studies. It is given by:

$$I^2 = \frac{\hat{\tau}^2}{\hat{\tau}^2 + \hat{\sigma}^2} \quad (6)$$

where $\hat{\sigma}^2$ is the meta-analysis error variance, which is computed by Stata along with the summary effect size.

The I^2 statistic has a straightforward interpretation – it indicates the percentage of all variability in effect size estimates that is due to heterogeneity (Higgins and Thompson, 2002). It follows a Chi-squared distribution with $n - 1$ degrees of freedom. The I^2 statistic is computed by Stata along with a corresponding p-value.

4.3 Exploring Heterogeneity

A meta-regression is a special case of weighted least squares regression with the effect size as the dependent variable and the effect size variances as inverse weights. The multivariate meta-regression allows exploration into specific factors, such as programme design and country-specific characteristics, that causes heterogeneity in effect sizes. It also allows me to test the predictions of the recent theoretical framework for UCTs (Churchill et al., 2021). The following meta-regression is estimated using Stata:

$$ES_i = \beta_0 + \beta_1 x_{1i} + \beta_2 x_{2i} + \beta_3 x_{3i} + \beta_4 D_{pilot} + e_i \quad (7)$$

where x_{1i} is the income per capita, x_{2i} is the poverty headcount ratio, x_{3i} is the transfer amount, D_{pilot} is a binary variable equal to one if the programme is a pilot programme instead of an established national programme, and e_i is the error term.

Based on the predictions of the model in Churchill et al. (2021), x_{1i} is expected to be positively correlated to effect size, while x_{2i} is expected to be negatively correlated with it. The conceptual framework of Garcia and Saavedra (2017) predicts that x_{3i} will be positively correlated with effect size, while the estimated coefficient on the binary variable D_{pilot} is predicted to be negative.

5. Results

5.1 Meta-Analysis Results

The summary UCT effect sizes for enrolment and attendance are reported in the first column of Table 3. The overall UCT impact on enrolment is 4.4 statistically significant percentage points, meaning that the probability that a child is enrolled in school is 4.4 percentage points higher among children in households being offered an unconditional cash transfer treatment compared to children in households not subjected to intervention. For attendance, the summary UCT impact is 2.9 statistically significant percentage points. To verify the robustness of these results, I exclude papers with a high risk of bias. The enrolment effect size increases to 6.2 percentage points while the attendance effect size increases slightly to 3.2 percentage points. Both effect sizes remain statistically significantly different from zero. The estimated between-studies variances τ^2 are reported in the fourth column. The I^2 statistics for all outcomes across all samples, reported in the fifth column, are close to 70 percent, implying that about 70 percent of all variability in effect size estimates is due to heterogeneity. Homogeneity is strongly rejected. The Chi-squared statistics for test of homogeneity and corresponding p-values are reported in the sixth column.

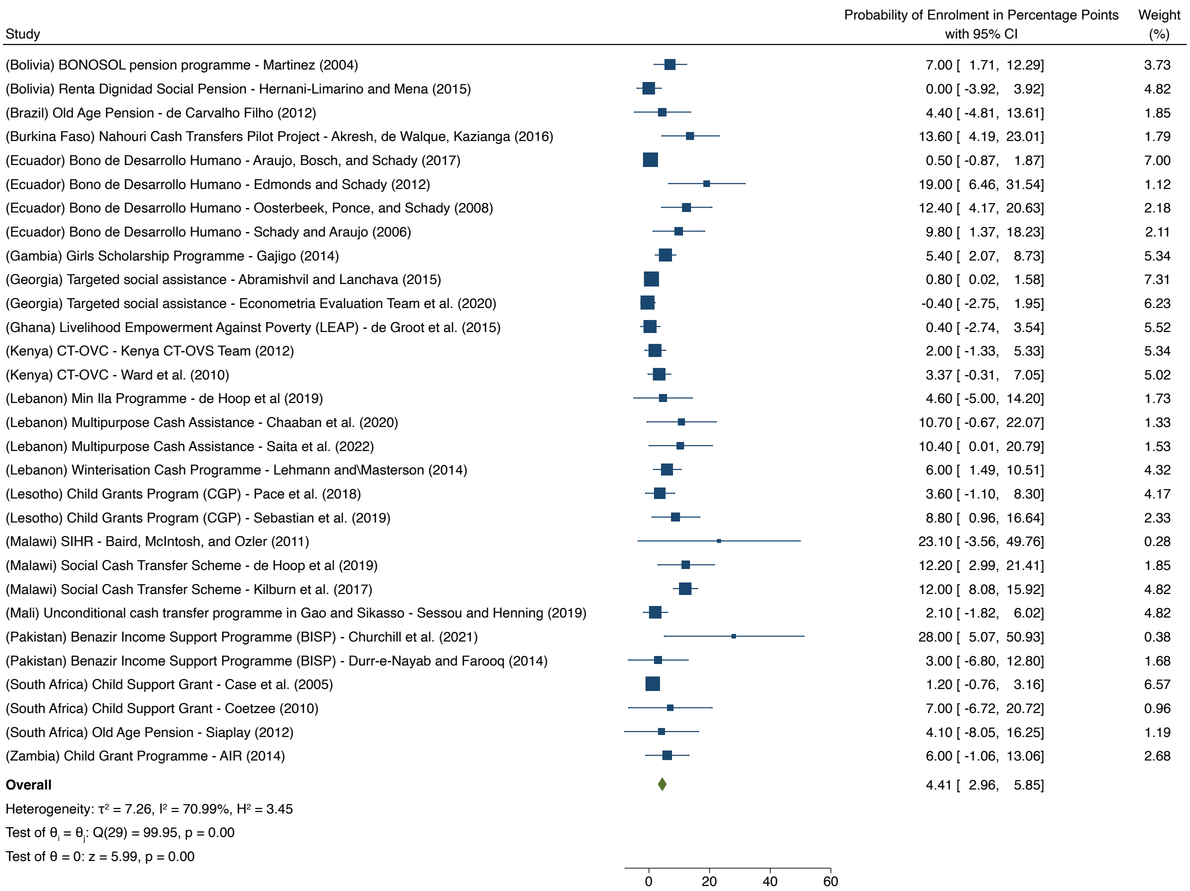
Table 3: Meta-Analysis Results

	Summary Effect Size	$p > \|z\|$	95% confidence interval	τ^2	I^2	Chi-squared statistic (p-value)	N
Enrolment	4.408	0.0000	[2.965, 5.852]	7.2595	70.99	99.95 (0.0000)	30
Attendance	2.880	0.0053	[0.854, 4.907]	5.2624	69.52	36.09 (0.0002)	12
Enrolment (high risk of bias studies excluded)	6.180	0.0000	[4.078, 8.281]	14.1201	70.88	75.54 (0.0000)	23
Attendance (high risk of bias studies excluded)	3.234	0.0341	[0.243, 6.224]	12.6171	70.64	30.66 (0.0003)	10

Note: Summary effect sizes, p-values, and 95-percent confidence intervals are computed using a random-effects meta-analysis model on Stata. The between-studies variance τ^2 is estimated using the DerSimonian-Laird method. I^2 statistics indicate the percentage of all variability in effect size estimates that is due to heterogeneity. Chi-squared statistics for test of homogeneity are presented with corresponding p-values. N denotes the number of papers used in the meta-analysis.

Figure 1 presents a forest plot of the UCT impact on enrolment, which lists all the UCT evaluation papers used in the meta-analysis, their reported effect sizes, corresponding 95-percent confidence intervals based on reported error statistics, and their weights in the meta-analysis. A positive summary UCT effect size is not surprising, as most of the papers report positive effects on school enrolment. The only exception is the Georgian Targeted Social Assistance evaluated by Econometria Evaluation Team et al. (2020), which reports a negative effect size that is not statistically significant. Figure 1 also reveals that studies with the smallest reported standard errors, and hence with the largest weights in the random-effects model, tend to find effect sizes close to zero. On the other hand, studies that find the largest effect sizes, namely Baird, McIntosh, and Özler (2011) and Churchill et al. (2021) have the largest standard errors, and hence have less weight in the meta-analysis.

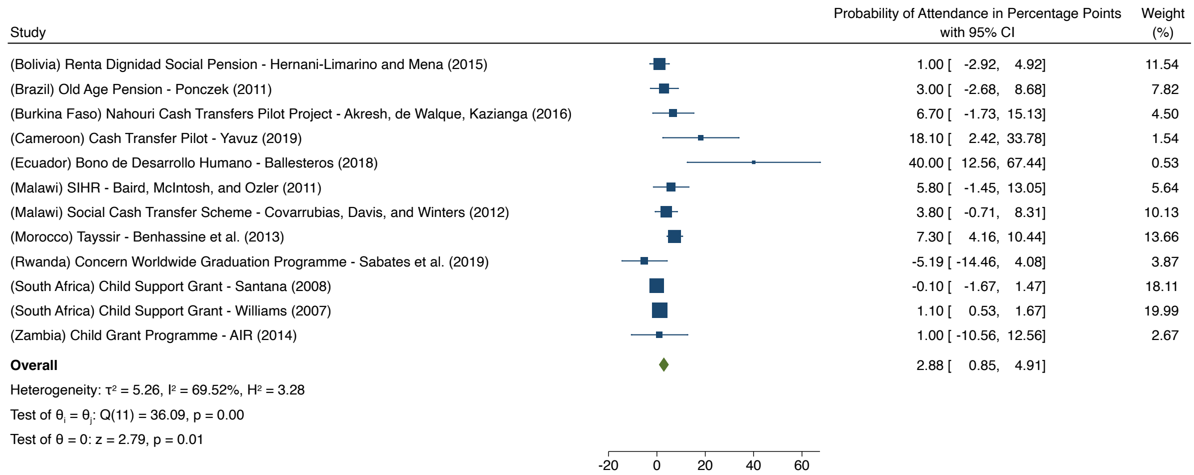
Figure 1: Forest Plot of Overall UCT Impact on Enrolment



Note: Blue squares indicate the reported effect sizes, and the size of the blue squares indicates the weight of the paper in the meta-analysis, which is inversely related to the variance. The green diamond is the summary effect size obtained from a random-effects DerSimonian-Laird meta-analysis model.

The forest plot for attendance is presented in Figure 2. Despite a smaller I^2, there is considerably greater variance in the attendance effect size. For example, Yavuz's (2019) evaluation of the cash transfer pilot in Cameroon reports an 18.1 percentage point increase in school attendance, and Ballesteros' (2018) evaluation of the *Bono de Desarrollo Humano* in Ecuador reports an even larger 40.0 percentage point effect. On the other hand, Sabetes et al. (2019) and Santana (2008) report negative effect sizes for the Rwandan Graduation Programme and South African Child Support Grant respectively. The negative effect sizes, however, are not statistically significant. Papers reporting the largest effect sizes again have the largest standard errors, meaning that their weight in the random-effects meta-analysis is small.

Figure 2: Forest Plot of Overall UCT Impact on Attendance



Papers with a high risk of bias are excluded in the forest plots presented in Figures 3 and 4.

Figure 3: Forest Plot of Overall UCT Impact on Enrolment (High Risk of Bias Studies Excluded)

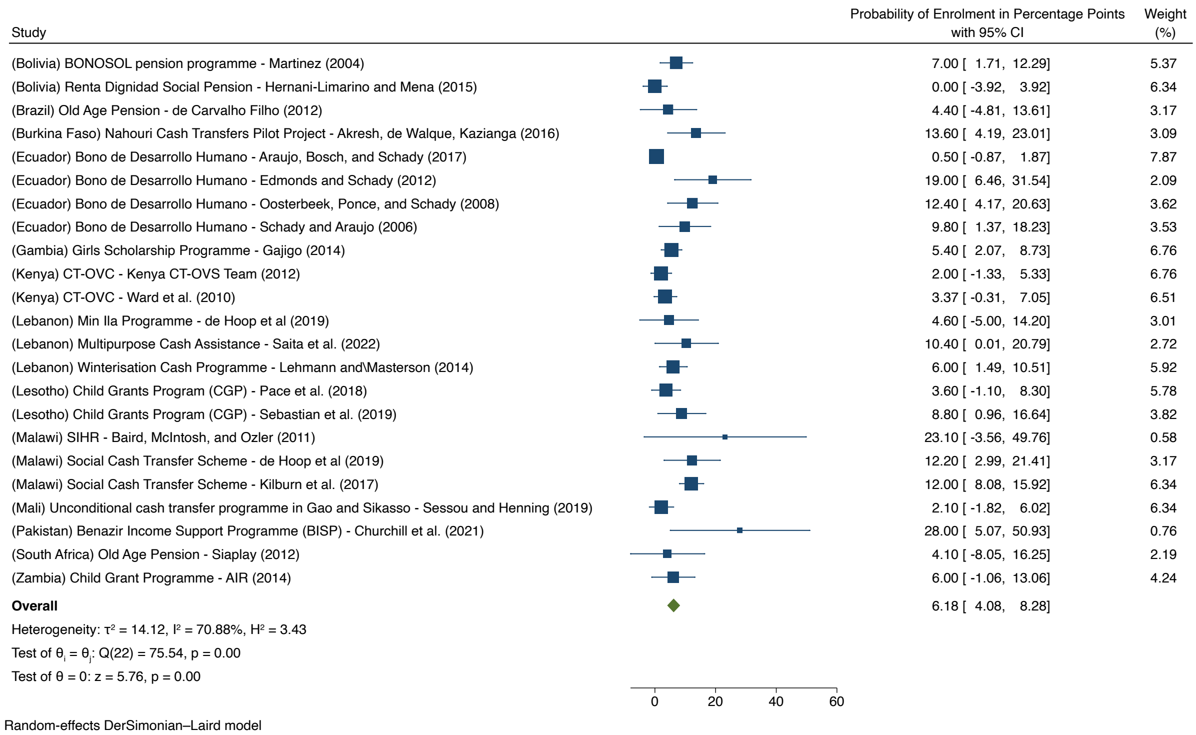
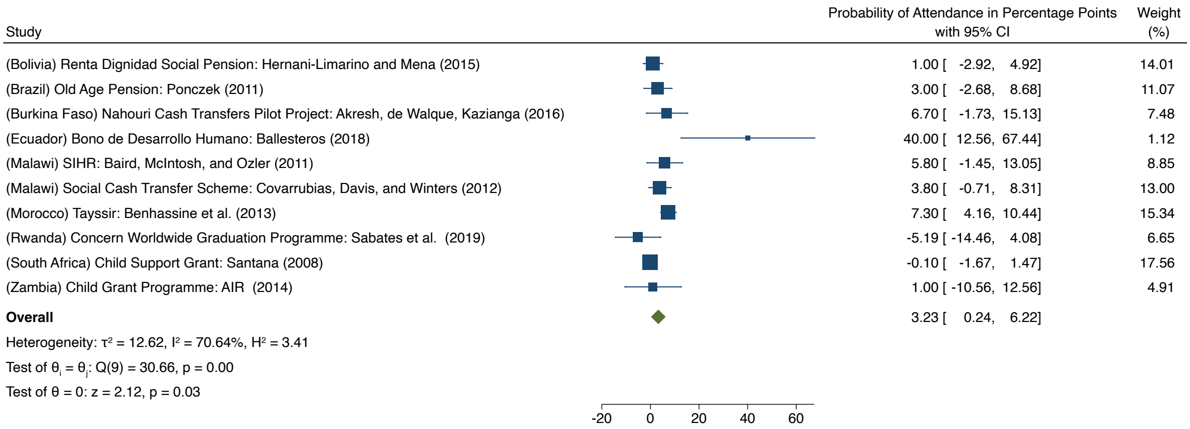


Figure 4: Forest Plot of Overall UCT Impact on Attendance (High Risk of Bias Studies Excluded)



5.2 Meta-Regression Results

The results of the meta-regression are presented in Table 4. None of the moderating factors is statistically significant for both enrolment and attendance, even after excluding studies with a high risk of bias. This implies that the four moderating variables are unable to explain the between-studies variation in UCT effect sizes.

Table 4: Meta-Regression Results

	(1) Enrolment	(2) Attendance	(3) Enrolment (high risk of bias studies excluded)	(4) Attendance (high risk of bias studies excluded)
Pilot Dummy	1.091 (1.830)	4.295 (3.049)	−1.719 (2.597)	2.296 (5.143)
Transfer Amount (in USD 100)	0.258 (0.231)	0.155 (0.576)	−0.059 (0.363)	−0.303 (1.044)
Income Per Capita (in USD 1,000)	−0.379 (0.469)	0.140 (0.541)	−0.198 (0.708)	0.808 (1.215)
Poverty Headcount Ratio	0.050 (0.054)	−0.054 (0.073)	−0.019 (0.097)	-0.120 (0.135)
Constant	2.348 (3.081)	3.011 (4.822)	8.450 (5.507)	6.565 (8.328)
N	30	12	23	10

Note: Meta-regression is estimated using weighted least squares with the effect sizes as the dependent variable and the associated standard errors as inverse weights. Standard errors for meta-regression coefficients are reported in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

These findings are in line with two meta-analyses that have been conducted on the educational impact of cash transfer programmes. Garcia and Saavedra (2017) do not find evidence to support the prediction that greater transfer amounts lead to increased human capital production through increased school enrolment and attendance. They also find no support for the prediction that well-established programmes that raise permanent income have greater effect sizes. Similarly, Baird et al. (2014) find that neither transfer amount nor whether the programme is a pilot has a significant effect on the effect sizes of cash transfer programmes. With regard to country characteristics, the theoretical model in Churchill et al. (2021) predicts that the income effect of a cash transfer is stronger in countries with higher income per capita and weaker in countries with a higher poverty headcount ratio. There is no evidence to support these predictions in the meta-regression results.

To further investigate the moderating effect of whether a programme is a pilot, I conduct a subgroup meta-analysis for the pilot binary variable. Figures 5 and 6 present the subgroup forest plots for enrolment and attendance respectively. The overall effect size for enrolment in the pilot UCT group is only 0.2 percentage points greater than the non-pilot group, and this difference is not statistically significant. For attendance, the pilot group has an overall effect size of about 5.4 percentage points greater than the non-pilot group. The difference in attendance effect sizes between pilot and non-pilot programmes is significant, but this result must be interpreted with caution as the sample size for attendance is much smaller than that of enrolment, and the difference is not significant when other programme and contextual characteristics are controlled for in a multivariate meta-regression. In any case, there is limited evidence to support the conceptual framework in Garcia and Saavedra (2017), which argues that established national programmes raise permanent income and thus have a larger effect on schooling compared to pilot programmes.

Figure 5: Subgroup Analysis of UCT Impact on Enrolment (Pilot vs. Non-Pilot)

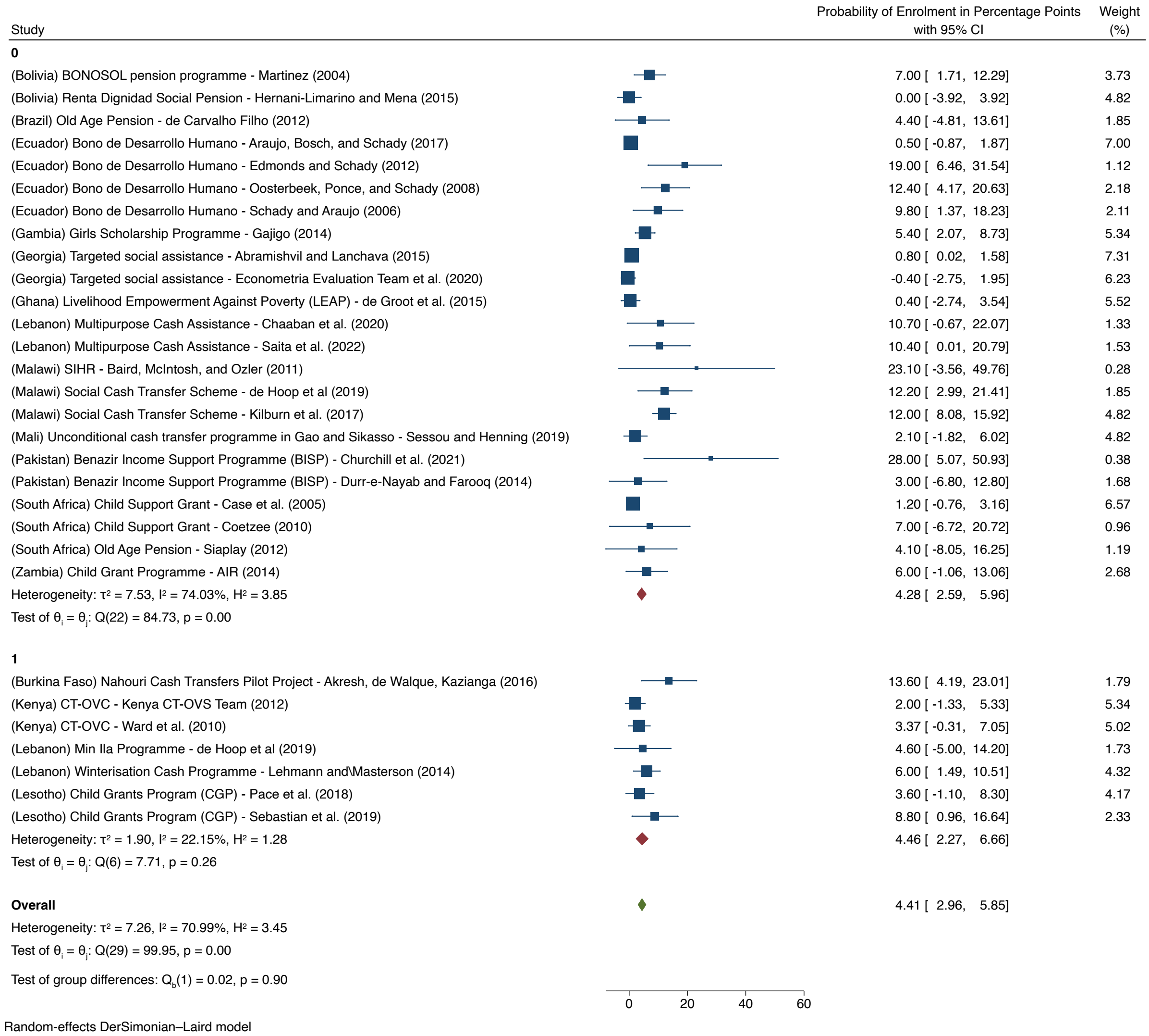
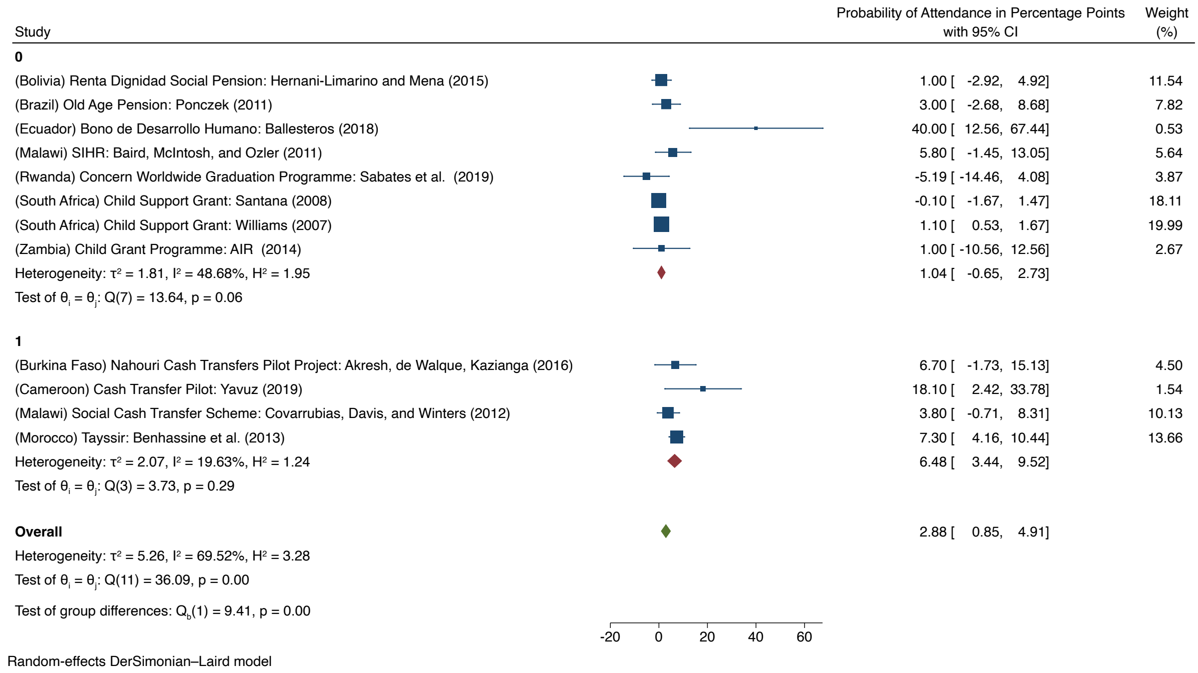


Figure 6: Subgroup Analysis of UCT Impact on Attendance (Pilot vs. Non-Pilot)



In Figure 7, I present bubble plots for the meta-regression on the three continuous moderating variables: transfer amount, income per capita, and the poverty headcount ratio. Bubble plots can only be produced for univariate meta-regression, and hence the estimated slope coefficients may differ from the multivariate meta-regression results in Table 4. The effect sizes on either enrolment or attendance are presented on the y-axes while the moderating variables are on the x-axes. The red lines are linear prediction lines estimated with the standard error of each paper used as inverse weights. The bubbles represent observations, or the effect sizes reported by the UCT evaluation papers, and a larger bubble means that the paper has a smaller standard error and a greater weight. The grey shaded area denotes the 95-percent confidence interval of the linear prediction line.

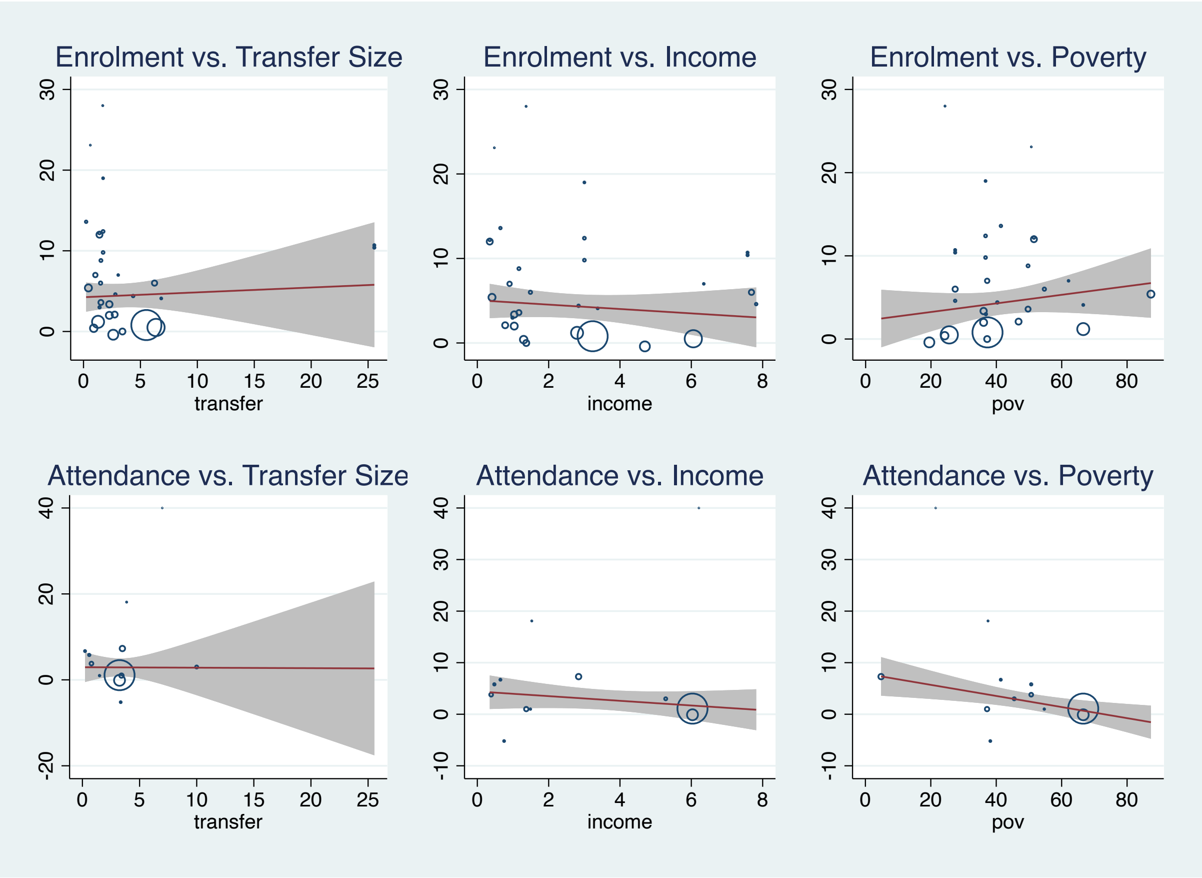
The panel on the top-left in Figure 7 shows a slight positive correlation between transfer amount and the effect size on enrolment. This, however, may be affected by an outlier. All studies have a transfer amount of less than USD 1,000 except for Lebanon's Multipurpose Cash Assistance, which had an annual transfer amount of USD 2,555. After dropping the outlier, the sign of the coefficient on transfer amount changes, but the results remain statistically insignificant. The relationship between transfer amount and the effect size on attendance is unclear from the bubble plot on the lower-left panel.

The middle panels in Figure 7 show a slight negative relationship between income per capita and both the enrolment and attendance effect sizes, contradicting the theoretical prediction that UCT impact is greater when the economy has higher average levels of income (Churchill et al., 2021). The downward-sloping prediction line implies that UCTs are most effective in poorer countries; however, this result is not statistically significant.

In the right panels in Figure 7, the coefficients on the poverty headcount ratio are different for enrolment and attendance. There is an upward-sloping prediction line for enrolment, but a downward-sloping one for attendance. The latter is consistent with the prediction that UCTs are more effective in raising schooling when the economy has a smaller fraction of poor households, such that the income effect on each individual household is greater. The results are again statistically insignificant.

Two possible explanations exist for the lack of statistical significance in the meta-regression. Firstly, it is possible that there is no relationship between the proposed moderating factors and UCT effect sizes, and the heterogeneity in UCT impact is due to other omitted factors. Secondly, the number of studies included in the meta-regression may be too small for any systematic relationship between the effect sizes and the moderating factors to emerge.

Figure 7: Meta-Regression Bubble Plots



Note: Effect sizes for enrolment and attendance in percentage points are graphed on the y-axes, while the individual moderating variables are graphed on the x-axes. Bubbles indicate the observations (effect sizes), and the size of the bubbles corresponds to the weight of the observation (inversely related to its standard error). The red lines denote a linear prediction line, and the grey shaded areas denote the corresponding 95-percent confidence interval. Transfer size is in units of USD 100 and income per capita is in units of USD 1,000.

6. Conclusion

In this review, I systematically search the impact evaluation literature to integrate the most recent and comprehensive available evidence for the educational impact of UCTs. To answer the question of whether UCTs have a positive impact on school enrolment and attendance, I quantitatively synthesise the effect sizes reported by 38 evaluations of 22 programmes in 18 countries.

The key finding is that there is a statistically significant positive impact of UCTs on both enrolment and attendance, and this impact is robust to the exclusion of studies with a high risk of bias. Using a random-effects meta-analysis, I find that the summary UCT effect size on enrolment is 4.4 percentage points, meaning that the probability that a child is enrolled in school is 4.4 percentage points higher among children in households being offered a UCT. The summary UCT effect size on attendance is 2.9 percentage points. After excluding papers with a high risk of bias, the summary effect sizes for both outcomes remain positive and statistically significant. These results suggest that UCTs are an effective social intervention for policymakers aiming to improve immediate schooling outcomes.

Across both outcomes and samples, homogeneity is rejected, implying that there is significant between-studies variation in UCT effect sizes. I attempt to explain this heterogeneity using a multivariate meta-regression with two programme design factors, namely the transfer amount and whether the programme is a pilot, and two country-specific factors, namely income per capita and the proportion of people living in poverty, as explanatory variables. The meta-regression allows me to explore the moderating effects of these four study-level characteristics and test theoretical predictions on the interplay between these factors and the effect of an income increase on household investment in education through increased school enrolment and attendance.

As with prior meta-analyses in Baird et al. (2014) and Garcia and Saavedra (2017), I fail to find any statistically significant moderating effect of the two programme characteristics: the transfer amount and whether the programme is a pilot. This finding, taken with the results of previous research, implies that perhaps policymakers should not be concerned about whether greater transfer sizes are necessary to improve schooling or whether there are advantages of implementing nationwide programmes over smaller-scale pilot programmes. Neither do country-specific characteristics such as income per capita and the poverty headcount ratio explain the heterogeneity in UCT effect sizes, lending no support to the recently developed theoretical framework which seeks to explain UCT educational impact through an income effect on households (Churchill et al., 2021).

The key limitation of this study is coverage. The World Bank (2015) reports that UCTs are present in 113 countries worldwide, but I only find evaluations of 90 programmes in 64 countries. Among them, only 22 programmes in 18 countries report educational effects. While this is a significant increase from five UCT evaluations found by Baird et al. (2014) and shows that there has been an increase in UCT evaluations in recent years, a larger sample of studies is still needed to claim that the results are representative of the population of UCTs. Wider coverage of UCTs will also allow further examination into systematic relationships between UCT effectiveness and programme or contextual characteristics.

With an increasing number of impact evaluations of cash transfer programmes, a key policy question is whether a programme will have the same impact outside of the experimental setting in which it is evaluated. Rodrik (2008) highlights this external validity problem in micro-development economics, where the causal impact of a particular policy or programme demonstrated in a randomised evaluation does not necessarily generalise to settings outside of the experiment. A meta-analysis offers a way to overcome this problem. By combining the impact estimates from many different studies of a particular treatment on a common measured outcome, researchers can infer generalised causal associations between treatment and outcome, or in this case the causal association between UCTs and school enrolment or attendance, across different settings (Matt, Brewer, and Sklar, 2010). This review offers three recommendations for future evaluations to enhance our understanding of UCTs and to allow research synthesis methods to be helpful to researchers.

First, there is a need for a greater number of evaluations, which can be achieved by incorporating monitoring and evaluation into national social safety net programmes. In the literature search, I find that many national social safety net programmes in developing countries, such as social pensions and benefits, public assistance to vulnerable groups, and child and family allowances are not evaluated. Exceptions exist, for example, the Old Age Pension in Brazil, the Targeted Social Assistance in Georgia, and the Child Support Grant in South Africa, all of which are included in the meta-analysis in this review. Building monitoring and evaluation capabilities for national social safety net programmes is a low-hanging fruit to increase the evidence base for UCTs and promote evidence-based policymaking.

Second, improvements in the standardisation of reporting will greatly assist researchers in conducting systematic reviews and meta-analyses. In the review, all journal articles report an error statistic, but technical reports and policy briefs tend to omit it. For studies to be eligible for inclusion in a meta-analysis, effect sizes must be accompanied by a corresponding error statistic. Furthermore, analysis reporting and explanation must be thorough so that researchers conducting research synthesis can code effect sizes without the need to make any assumption on the interpretation of the impact estimate. If an evaluation is conducted for several subgroups in the target population, authors should report an overall impact estimate to facilitate comparisons across studies. A final best practice that should be emulated in future evaluations is the reporting of study-level characteristics, such as programme design features, baseline outcome measurements, and other variables in the experimental setting so that these can be used as explanatory variables in a meta-regression to study heterogeneity in UCT impact.

Third, exploration of UCT effects on longer-term outcomes such as learning, employment, and earnings is a worthwhile undertaking for future empirical research. Considering that UCTs are a relatively recent invention, much of the evidence, including those in this review, focuses on the immediate measurable outcomes, such as enrolment and attendance. Researchers are increasingly interested in whether cash transfer programmes have a sustaining positive impact over the long run. For example, a recent follow-up study on Mexico's *PROGRESA* find improved economic outcomes for women due to childhood exposure to the CCT (Parker and Vogl, 2018). Whether UCTs have a similar long-run impact remains an open question that should be answered by future research.

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