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Meta-analysis of the effects of interventions on child labour

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Acronyms and abbreviations

CCT	Conditional cash transfer
CIA	Conditional independence assumption
CLEAR	Clearinghouse for Labor Evaluation and Research (United States)
DID	Difference-in-differences
ES	Effect size
ILO	International Labour Office/Organization
ITT	Intent-to-treat
IV	Instrumental variables
OR	Odds ratio
PMM	Propensity score matching
RCT	Randomized control trial
RDD	Regression discontinuity designs
ROBINS-I	Risk of Bias in Non-randomized Studies – of Interventions
RTA	Research to Action project (ILO)
SE	Standard error
TO	Treatment on the treated
UCT	Unconditional cash transfer

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1. Introduction

The latest International Labour Organization (ILO) global child labour estimates indicate that, despite important progress, there were still 160 million children in labour worldwide in 2020, many of whom were involved in hazardous work (79 million children) (ILO and UNICEF 2021). With the adoption of the Sustainable Development Goals, the international community has committed to ending child labour in all its forms by 2025 and eradicating forced labour by 2030. The headline figures emerging from the global estimates make it clear that a substantial acceleration of progress will be needed to meet these commitments. Moreover, the Covid-19 pandemic has amplified the drivers of child labour, including growing poverty and economic inequality. Consequently, understanding how social protection programmes and other interventions aimed at improving the welfare of poor households in developing countries work is a high priority for policymakers around the world. While narrative literature reviews have summarized and interpreted the literature on child labour (such as Edmonds 2009; Fors 2012; Dammert et al. 2018), this study combines the latest impacts on child labour, from existing evidence of different programmes, policies or activities (hereafter referred to as an intervention) reported between 2010 and March 2023 and covering a broad range of contexts and populations, to perform – to the best of the author's knowledge – the first meta-analysis focusing exclusively on child labour.[1]

The meta-analysis includes empirical studies that provide a quantitative assessment of treatment effects, rely on a control or comparison group, and report impacts of interventions on child labour as primary or secondary outcomes. It reviewed 614 studies on child labour and forced labour identified by the ILO's "Research to Action" (RTA) project, and an additional 40 recent studies to obtain a sample of 41 randomized and quasi-experimental impact evaluations for the meta-analysis (listed in Annex 1); 65.9 per cent of these are randomized control trials (RCT). As several outcomes and interventions are reported in these studies, 131 effect-size estimates were collected; of these, 86 refer to the child's probability of engaging in any type of activity, paid or unpaid, and 45 refer to hours of work.

Before moving into the methodology, discussing the definition of child labour is important. Child labour is any work that deprives children of their childhood, potential, and dignity, which harms physical and mental development. Three international Conventions have established legal grounds for national and international action against it: the ILO Minimum Age Convention, 1973 (No.138), the ILO Worst Forms of Child Labour Convention, 1999 (No.182), and the United Nations Convention on the Child's Rights. Child labour includes employment or work below the minimum legal working age; employment or work that is likely to harm the health and safety of children; and activities that interfere with their schooling. The studies included in the search do not restrict the definitions to those presented above. The search includes a broad range of outcomes (child labour, hazardous work, economically active children, among others), and age categories. Moreover, studies tend to use different definitions of child labour according to data availability, so that comparing estimates from the available studies can be challenging due to the heterogeneity in the outcome measures. For that reason, the study focuses on children's engagement in any type of economic activity and working hours as outcome measures. To facilitate comparison across studies, the impacts of individual studies were converted into odds ratios, which report changes in the odds of being engaged in any type of economic activity. When looking at hours of work, standardized mean impacts are reported.

1 Diverse topics covered in recent meta-analyses in economics focusing on developing countries include cash transfers and education (Baird et al. 2014), microcredit expansion (Meager 2019), and child mortality and water treatment (Kremer et al. 2023). Kabeer and Waddington (2015) analyse only the impacts of conditional cash transfers (CCTs) on child labour outcomes.

Based on comparable estimates for each study, a random effects meta-analysis was employed to combine the results of individual studies included in the review into an overall statistic effect size (ES), where each effect size is weighted by its inverse variance (Borenstein et al. 2021). An indicator of risk of bias based on the Cochrane Risk of Bias tool is also provided, and potential publication selection bias in the child labour literature is accounted for by providing a graphical inspection of funnel plot asymmetry and estimating the Egger test, as suggested by Borenstein et al. (2021).

The main findings of the study show that all interventions combined have an impact on participation in any type of activity of 2 per cent, compared to receiving no intervention based on 31 effect size estimates from 23 studies. This number, however, masks important differences between interventions that provide cash transfers and interventions that may affect the demand for adult work outside the household. When these two types of interventions are measured separately, the main results show that conditional cash transfers (CCTs) and unconditional cash transfers (UCTs) lower the odds of being engaged in any type of work activities by 10 and 11 per cent respectively, compared to no cash transfer programme, while interventions that are related to microfinance, training and capital increase the odds of being engaged in any type of work activities by 5 per cent.

A similar pattern is observed for hours of work. The average estimate shows that all interventions combined have an impact on hours of work in any type of activity of -0.04 standard deviations, suggesting that children who were exposed to an intervention experienced a decrease in hours worked on average, as compared to those who were not. Sub-group analysis by intervention category indicates that CCTs and UCTs lower the mean standardized work hours in any type of work activity by -0.09 standard deviations, and other programmes (public work programmes and health insurance) by -0.04 standard deviations. For other interventions, the effects are small and not statistically significant.

These results may reflect the different mechanisms that explain the changes in child labour. Conditional cash transfers increase the income of the household (income effect) and the opportunity cost of schooling (substitution effect) which affects the demand for child labour. Unconditional cash transfers are not attached to any specific behavioural condition to receive payment, and act as a pure income effect. The evidence shows that cash transfers have been proven effective in certain settings and for certain groups – such as in Honduras (Galiani and McEwan 2013), Mexico (Behrman, Parker, and Todd 2011; Tagliati 2022) and Morocco (Benhassine et al. 2015) – but ineffective in others, such as in Brazil (Cepaluni et al. 2022) and the Philippines (de Hoop et al. 2019). There is some evidence that school CCTs might be more effective in reducing child labour than in-kind transfers (Tagliati 2022), and they may also work better when combined with other programmes such as the provision of entrepreneurship skills (Del Carpio, Loayza, and Wada 2016). Microfinance, training and capital also affect household income, but they imply a behavioural change in adults; for instance, by investing in the family enterprise the demand for both adult and child labour may increase.

There are also other important sources of variability in the impact of different policies related to the child's gender (Islam and Choe 2013; Kazianga, de Walque, and Alderman 2012), whether the study examines the poorest or the less poor (Galiani and McEwan, 2013; Pellerano, Porreca, and Rosati 2020), agricultural or non-agricultural activities (de Hoop et al. 2018; Del Carpio, Loayza, and Wada 2016), and the timeframe of the policy (Edmonds and Shrestha 2014). However, due to a lack of statistical power, the study was able to estimate in a single coefficient these specific impacts.

This paper contributes to the meta-analysis literature on the effects of interventions on children's allocation of time in developing countries, where most reviews are focused on schooling outcomes (Baird et al. 2014; McEwan 2015; Evans and Yuan 2022). Most closely related to this paper is a meta-analysis of the effects of CCTs on children's work participation based on 10 studies published before 2010 (Kabeer et al. 2012), which reports an average reduction in children's work participation of 11 per cent due to CCTs. The present study extends this work beyond CCTs to include other types of intervention, and updates the search from 2010 onwards.

The remainder of this paper is structured as follows: section 2 discusses how the studies were identified and selected to create the dataset for analysis; section 3 describes the criteria for coding the individual estimates, calculation of effect size, and meta-analysis; section 4 presents the risk of bias analysis; and section 5 presents the discussion of the main results, risk of bias, and publication bias. Section 6 concludes.

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2. Criteria for considering studies for this review

The study began with a large database of child labour and forced labour studies identified by the ILO's Research to Action (RTA) project. The RTA bibliography aimed to classify and catalogue quantitative, qualitative or mixed methods peer-reviewed papers and reports published from 2010 to 2019 examining child labour and forced labour. The study designs are descriptive, relational or causal. The database consists of 614 studies that met the criteria defined by the ILO (see table 1).

After an initial screening of the titles and abstracts, studies were selected that met the following criteria: causal studies where child labour, forced labour or human trafficking are an outcome of the empirical analysis; where the studied age group included children between the ages of 5 to 17, and studies that evaluated an intervention targeting child labour directly or as a secondary outcome. Eligible studies included experimental RCT and quasi-experimental designs (propensity score matching (PSM), instrumental variables (IV), difference-in-differences (DID), and regression discontinuity designs (RDD)).

Interventions related to education (such as stipends, scholarships and food programmes), health (such as health insurance and family health programmes), and safety net interventions (such as cash transfers) were included. Studies that focus on macro policies (such as trade, globalization, conflict, gold prices, and so on)or controlled laboratory experiments with no clear policy implications were excluded. Studies that were duplicates were also excluded (for example, when both the working paper and published paper were in the bibliography, only the published paper was included), as well as descriptive evaluation reports where no impact estimates were reported.

Table 1. RTA database search (as of November 2022)

Source	Number of records	Screened out by title	Excluded based on earlier research	Assessed for eligibility	Excluded by abstract	Full text assessed	Excluded based on full text	Included in the RTA bibliog-raphy
Google Scholar	8 550	7 470	419	661	156	505	290	215
ProQuest	8 326	5 300	308	2 718	2 504	214	192	22
ERIC	846	126	120	600	496	104	101	3
PubMed	422	17	80	325	100	225	221	4
Web of Science	1 676	617	0	1 059	541	518	224	294
Website Searches	7 767	7 217	47	503	263	240	164	76
Total	27 587	20 747	974	5 866	4 060	1 806	1 192	**614**

Source: ILO 2022: https://www.rtaproject.org/wp-content/uploads/2022/11/CLFLegm_method_en.pdf. More information available at https://rtaproject.org/.

Next, the database was updated, adding causal studies published from August 2019 to March 2023 to allow for studies published after the conclusion of the RTA reference period. A search was made to see if updated versions of the studies were available and if so, the most recent one was included. Forty studies not listed in the original RTA bibliography were also included (table 2). In total, 99 studies that matched the eligibility criteria and thus were eligible for inclusion in the meta-analysis were found.

▶ Table 2. Sample for meta-analysis

Phase 1: Total articles to be reviewed	**654**
Articles included in the RTA bibliography	614
Articles not listed in the original review	40
Phase 2: Ineligible references based on full-text assessment	**555**
Descriptive studies [1]	474
Descriptive evaluation report, no impact estimate	17
Research design does not meet requirements [2]	51
Duplicates	5
Review studies	1
No child labour outcome	6
Unable to access	1
Phase 3: Ineligible references for meta-analysis	**54**
No relevant child labour outcome [3]	7
Not a primary study	4
Research design does not meet the requirements for meta-analysis [4]	29
High risk of bias	18
Phase 4: Total eligible references for meta-analysis (as of 29 June 2023)	**41**

Notes: [1] Descriptive/assessment studies describe a setting where child labour or forced labour is present. [2] Research design does not meet requirements, including studies whose primary contribution is theoretical, qualitative studies, or studies focusing on macro policies. [3] Studies focus on outcomes that are not comparable with most studies, such as the proportion of children working in the village, the proportion of children engaged in child labour in the household, and papers looking only at children of a specific age group and gender. [4] No available information on baseline characteristics, mean treatment and control groups, p-values, or t-stats.

Finally, in the last phase, the full texts of the 99 articles and selected studies were reviewed, where the main outcome of interest was child participation in labour activities (paid or unpaid) or hours worked. For the meta-analysis, studies were excluded where the main outcome was not relevant, such as the proportion of children working at a village or house level (seven studies). Studies that were not primary studies were also excluded, for example, those replicating the impact effects of already published interventions (four studies) and those where the research design did not meet the requirements for meta-analysis (29 studies) – such as studies that did not provide p-values, t-stats, or standard errors to calculate confidence intervals or mean outcomes. Studies that were classified as having high risks of bias (18 studies) were also excluded. Given the heterogeneity in recall periods (such as a reference period of seven days, the previous month or the previous 12 months) and age group inclusion, studies based on these two variables were not excluded. Upon reviewing all the identified studies, it was found that 41 independent studies fully matched the criteria for inclusion in the meta-analysis (table 2). Section 4 describes the main characteristics of the selected studies.

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3. Meta-analysis

For each of the selected studies, this paper identifies whether they report child labour outcomes such as employment participation or worked hours. The estimates and standard errors, p-values, or t-stats are extracted and standardized to make them comparable, and the overall effect size across all studies calculated. This section explains in detail each of these steps.

3.1 Coding criteria

The unit of analysis in this study is the estimated impact of an intervention. A coding tool was developed to guide the data extraction process. Treatment effects estimates were coded across all studies eligible for meta-analysis, along with other parameters (standard errors, p-values, t-stats, number of observations, among others) and intervention characteristics such as type of design (RCT, DID, PSM, IV and RDD), study definition of child labour (such as engagement in any type of economic activity, work for payment, and so on.), age inclusion, the year the study was published, country and geographic region, among others.

The following criteria were used when coding the effect sizes: first, some studies report several outcomes for the same child: participation in wage activities, participation in unpaid activities, participation in any type of activities, participation in specific activities such as farming, herding, and so on. To make the studies more comparable to each other, the most conservative option is to include in the meta-analysis the impacts on participation and worked hours in any type of activity, whether paid or unpaid. While all outcomes provided in the studies were coded, only studies that report participation in any type of activities were included in the meta-analysis in the current version.

Second, some studies report multiple impact estimates using different methodologies. The estimate resulting from the specified preferred model and identification strategy as reported by the author was used. If no preference is specified, the intent-to-treat effects with most controls were taken into consideration. Third, some studies report effects only for sub-groups separately, for example, estimates disaggregated by boys and girls, rural and urban, or for separate years. In those cases, following Borenstein et al. (2021), the different estimates within the study were combined into one summary effect using a random-effects model.

Finally, the estimates of each treatment arm were coded separately when the study reports more than one type of intervention. Treatment arms may have different impacts on children: for example, an intervention that has two treatment arms, one where cash transfers are offered conditional on schooling attendance while the other offers a cash transfer that is conditional on schooling combined with investment in productive assets. These two experiments were coded as two separate interventions.

Overall, a total of 131 effect size estimates from the selected studies were coded, considering that individual studies typically look at multiple outcomes, measure outcomes at multiple time points, or include separate effect size estimates for age or sex.

3.2 Calculating effect sizes

For binary outcome variables, such as participation in labour activities, the standard practice is to calculate the risk ratios or odds ratios, which measure the ratio between two proportions, (Borenstein et al. 2021). However, calculating the effect size from regression estimates is not straightforward if it is considered that most studies control for observable characteristics and cluster standard errors. Following Baird et al. (2014), for each study the follow-up mean participation rate in the control group was coded, and the follow-up participation rate in the treatment groups was calculated, adjusted by covariates by adding the impact estimate from the regression model to the mean outcome in the control group. The natural logarithm of odds ratio (OR) (ln(OR)) was calculated, where OR equals the covariate-adjusted outcome in the treatment group divided by the outcome in the control group.

The standard error cannot be calculated using the standard formula since most studies cluster the standard errors to consider that treatment is assigned at the community or village level rather than the individual level or to consider survey strata. Thus, the ln (OR) was also converted into a standardized effect size, *ES*, dividing the ln (OR) by 1.814 (Baird et al. 2014). The standard error of *ES* was calculated using the standard error associated with the impact estimate from the regression model as follows: SE=ES/z, where z is the t-test associated with the treatment effect from the regression model. Thus, the reported estimates often are based on regression models controlling for a rich set of control variables and community fixed effects and clustering of standard errors.

For continuous outcome variables, such as hours of work, the standard practice is to divide the mean difference in each study by that study's standard deviation to create the standardized mean difference known as Cohen's *d*. The standardized mean can be estimated using the mean difference between the treatment groups (Y_T) and a control group (Y_C) at the follow-up, as well as the pooled standard deviation for the treatment and control groups combined (S_{pooled}) as follows:

$$d = \frac{Y_T - Y_C}{S_{pooled}}$$

Following Borenstein et al. (2021), in cases where the pooled standard deviation was not directly reported in the study, it is calculated as follows:

$$S_{pooled} = SE\sqrt{\frac{n_T\, n_C}{n_T + n_C}}$$

where n_T and n_C are the sample sizes of the treatment and control group at follow-up and SE is the reported standard error of the estimated difference $Y_T - Y_C$.

3.3 Estimation of the summary effect size

Most meta-analyses are based on one of two statistical models, the fixed-effect model or the random-effects model. Under the fixed-effect model, the main assumption is that all studies in the analysis share the same true effect size, thus the summary effect is the estimate of this common effect size. Under the random-effects model, the main assumption is that the true effect size varies from study to study due to contextual and unobserved factors and the summary effect is the estimate of the mean of the distribution of effect sizes.

In this paper a random-effects meta-regression of all the individual estimated effects identified in the earlier section is employed. The random-effects model weights each study *i* point estimate by the inverse of the variance of the estimate, using the standard error associated with the estimate (see Borenstein et al. 2021 for a detailed explanation):

$$W_i = \frac{1}{V_{Yi} + T^2}$$

where V_{Y_i} is the variance of the estimated effect from study *i* (or the within-study variance) and T^2 is the between-studies variance. The between-study variance is estimated in this paper using a method of moments approach.

The weighted mean or summary effect (M) is then computed as the sum of the effect sizes Y_i multiplied by the weight of each study divided by the sum of the weights as follows:

$$M = \frac{\sum_{i=1}^{k} W_i Y_i}{\sum_{i=1}^{k} W_i}$$

The estimated variance and standard error of the summary effect (M) are calculated as:

$$V_M = \frac{1}{\sum_{i=1}^{k} W_i} \quad and \quad SE_M = \sqrt{V_M}$$

The prediction interval at the 95 per cent lower and upper limits for the weighted average effect across all studies is calculated as:

$$M \pm 1.96 \times SE_M$$

Thus, the random-effects meta-regression takes two sources of variation, the within-study error in estimating the effect in each study and the variation in the true effects across studies. Study weights are assigned to minimize both sources of variance.

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4. Risk of bias

Once the coding criteria for selecting the estimates is established, a critical appraisal of each study should be performed. The validity of each study design is subject to a range of biases that affect internal validity, statistical conclusion validity and external validity, among others (Waddington et al 2012). For example, RCTs have been increasingly used in the evaluation of the effectiveness of programme interventions. Simple randomization ensures that the allocation of individuals is not systematically biased by individuals' self-selecting into treatment. Thus, the treatment and control groups have similar observable and unobservable characteristics on average. While RCTs are considered the most rigorous method in impact evaluation, several things can go wrong in the randomization process and the implementation of the intervention which would cause biased treatment effects. For example, the recruitment of participants may not be random, data collectors or participants may not be blinded to the treatment status, or programme attendance and participation may be affected by high non-random attrition rates. Many of these issues are addressed in practical guidelines for conducting impact evaluations, such as Duflo, Glennerster, and Kremer (2007); Khandker, Koolwai, and Samad (2010); and government agencies such as the US Department of Labor Clearinghouse for Labor Evaluation and Research (CLEAR 2019) and the UK Foreign, Commonwealth, and Development Office (UK-DFID 2014).

The risk of bias tools used in economics usually focuses on internal validity in social experiments and quasi-experiments (Bose 2010; Eble et al. 2016). The focus is to assess the risk of bias in existing causal studies to determine whether the estimated effects are due to the intervention examined. The present paper follows the Cochrane Risk of Bias 2.0 tool for RCT, and the ROBINS-I tool (Risk of Bias in Non-randomized Studies – of Interventions) (Higgins et al. 2021) to assess individual causal studies (see the detailed explanation in Annex 2).[2]

The categories used to determine causal research available on child labour are as follows:

- **Low risk of bias:** The estimated effects are due to the intervention examined. RCTs receive a high rating if there are no threats to internal validity. Quasi-experimental studies receive a higher rating if they provide a clear counterfactual (what would have happened in the absence of the intervention).
- **Medium risk of bias:** The estimated effects are due to the intervention under study, but other confounding factors need to be considered.
- **High risk of bias:** That the estimated effects are due to the intervention cannot be ascertained with confidence, thus policy recommendations cannot be derived from these studies, which do not show clear pathways through which the intervention may affect children.

2 It should be noted that papers reviewed in this study were published before a pre-plan analysis was recommended by journals, so estimates were not adjusted by multiple hypothesis testing. This is particularly relevant for papers that look at the effects of interventions on child labour as secondary outcomes. Moreover, the assessment tool does not consider the recent advances in the econometrics of impact evaluation methods, given that the adjustments proposed in the new difference-in-differences literature were recommended in 2022 and most studies were published before that.

5

5. Results

5.1 Description of included studies

Out of the 41 identified studies that met our selection condition for inclusion in the meta-analysis, 27 were experimental (RCT) and 14 were non-experimental (DID, PSM, IV and RDD or a combination) (the 41 studies are listed in Annex 1, with bibliographical details in the Reference section). Studies on child labour generally examine the extensive margin of child labour (participation in labour activities) and/or the intensive margin of child labour (hours worked).

These 41 studies focus on 24 countries: 15 studies in Latin America and the Caribbean, 11 in South Asia, 10 in sub-Saharan Africa, four in East Asia and the Pacific, and one in the Middle East and North Africa. Most studies analyse an intervention located in a middle-income country: 23 studies in lower-middle-income and 10 studies in upper-middle-income countries.

5.2 Risk of bias

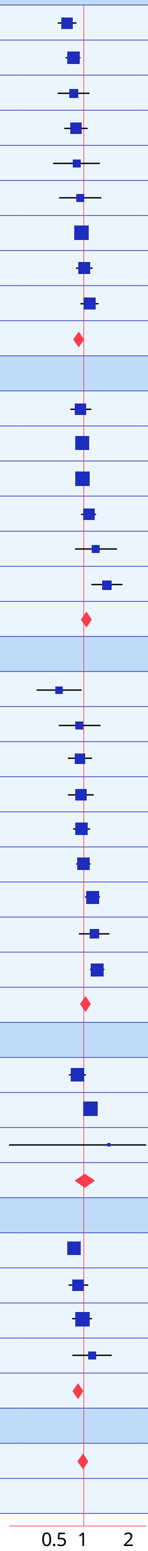
Overall, 28 studies (68.3 per cent) are categorized having a low risk of bias and 13 studies (31.7 per cent) are categorized as having a medium risk. Of the 28 low-risk bias studies, it is observed that 22 (78.6 per cent) used random assignment and six used quasi-experimental designs discussing all relevant features of the approach. Of the 13 medium-risk bias papers, five (38.5 per cent) used random assignment and eight used quasi-experimental designs (61.5 per cent). In some cases, RCT may be thought of as the most rigorous methodology, but high attrition rates and imbalances at the baseline affect the interpretation of the results.

5.3 Meta-analysis results

Tables 3 and 4 show the main results from the random-effect meta-analysis. Each forest plot shows the effect size for each study represented by a square and the 95 per cent confidence interval associated with each effect. The location of the square represents both the direction and the magnitude of the study effect, while the area of the square represents the weight assigned to that study in the meta-analysis. The summary effect or overall effect size is represented by a diamond at the bottom.

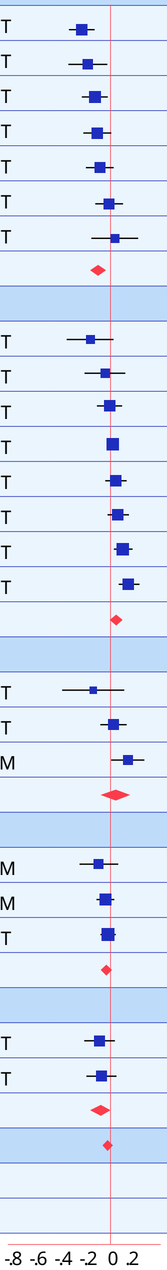
▶ Table 3. Random effects forest plot of engagement in any type of activity estimates

Study	Intervention	Causal/ Design	Odds ratios with 95% CI	Weight (%)
CCT				
Galiani and Mc Ewan (2013)	CCT + investment in education	RCT	0.71 [0.58, 0.87]	3.42
Attnasio et al. (2015)	CCT	DID	0.80 [0.69, 0.93]	4.15
Vanelas and Niño-Zarazúa (2019)	CCT	DID	0.81 [0.58, 1.11]	2.15
Galiani and Mc Ewan (2013)	CCT	RCT	0.85 [0.67, 1.07]	3.03
Barrera-Osorio et al. (2011)	CCT	RCT	0.85 [0.53, 1.36]	1.26
Barrera-Osorio et al. (2011)	CCT + Savings	RCT	0.93 [0.60, 1.44]	1.41
Behrman et al. (2011)	CCT	RCT	0.95 [0.90, 1.00]	5.36
Tagliati (2022)	CCT	RCT	1.01 [0.86, 1.19]	3.95
de Hoop et al. (2019)	CCT	RCT	1.13 [0.94, 1.36]	3.67
Heterogeneity: $T^2 = 0.01$, $I^2 = 64.56\%$, $H^2 = 2.82$			0.90 [0.81, 1.00]	
Enrerpreneurship, microfinance, capital				
Karlan and Valdivia (2011)	Microfinance	RCT	0.93 [0.76, 1.14]	3.41
de Hoop et al. (2018)	Female Capital and Training	RCT	0.97 [0.87, 1.07]	4.80
Angelucci et al. (2015)	Microcredit	RCT	0.98 [0.94, 1.01]	5.44
Edmonds and Theoharides (2020)	Asset Transfer	RCT	1.11 [0.95, 1.29]	4.08
Baland et al. (2020)	Microfinance	RCT	1.27 [0.83, 1.96]	1.43
Islam and Choe (2013)	Microcredit	IV	1.60 [1.16, 2.20]	2.17
Heterogeneity: $T^2 = 0.01$, $I^2 = 74.66\%$, $H^2 = 3.95$			1.05 [0.94, 1.18]	
In-kind transfers				
Edmonds and Shrestha (2014)	Stipend	RCT	0.60 [0.37, 0.96]	1.26
Edmonds and Shrestha (2014)	Scholarship	RCT	0.91 [0.59, 1.40]	1.44
Kazianga et al. (2012)	Take-home ratios	RCT	0.92 [0.72, 1.18]	2.87
Galiani and Mc Ewan (2013)	Investment in education	RCT	0.94 [0.72, 1.22]	2.65
Aurino et al. (2019)	School feeding	DID	0.95 [0.80, 1.12]	3.87
Tagliati (2022)	Food Transfer	RCT	0.99 [0.87, 1.12]	4.41
Aurino et al. (2019)	Any aid	DID	1.19 [1.03, 1.37]	4.26
Kazianga et al. (2012)	School meals	RCT	1.24 [0.92, 1.67]	2.34
Aurino et al. (2019)	General Foof Distribution	DID	1.32 [1.15, 1.50]	4.40
Heterogeneity: $T^2 = 0.02$, $I^2 = 67.07\%$, $H^2 = 3.04$			1.03 [0.91, 1.17]	
Other				
Landmann and Frölich (2015)	Health Insurance	RCT	0.88 [0.75, 1.03]	4.00
Bharadwaj, Lakdawala and Li (2020)	Labor Regulation	DID	0.13 [1.08, 1.19]	5.38
Richa and Soares (2010)	Family Health Program	DID	0.76 [0.27, 11.62]	0.10
Heterogeneity: $T^2 = 0.03$, $I^2 = 79.78\%$, $H^2 = 4.95$			1.02 [0.80, 1.30]	
UCT				
Edmonds and Schady (2012)	UCT	RCT	0.82 [0.73, 0.92]	4.57
Handa et al. (2016)	UCT	RCT	0.90 [0.74, 1.09]	3.51
Pellerano et al. (2020)	UCT	RCT	0.95 [0.78, 1.14]	3.60
Chong and Yáñez-Pagans (2019)	UCT	RDD	1.18 [0.79, 1.75]	1.62
Heterogeneity: $T^2 = 0.00$, $I^2 = 24.06\%$, $H^2 = 1.32$			0.89 [0.80, 0.99]	
Overall				
Heterogeneity: $T^2 = 0.02$, $I^2 = 80.59\%$, $H^2 = 5.15$			0.98 [0.92, 1.04]	
Test of group differences: $Q_b(4) = 7.68$, $p = 0.10$				



▶ Table 4. Random effects forest plot of hours of work in any type of activity estimates

Study	Intervention	Causal/ Design	Standardized Difference with 95% CI	Weight (%)
CCT				
Del Carpio et al. (2016)	CCT + Training	RCT	-0.19 [-0.26, -0.12]	4.72
Benhassine et al. (2015)	CCT	RCT	-0.14 [-0.25, -0.03]	3.47
Del Carpio et al. (2016)	CCT + Business Grant	RCT	-0.11 [-0.19, -1.04]	4.74
Barrera-Osorio et al. (2011)	CCT	RCT	-0.10 [-0.18, -1.01]	4.45
Barrera-Osorio et al. (2011)	CCT + Savings	RCT	-0.07 [-0.16, 1.01]	4.45
Tagliati (2022)	CCT	RCT	-0.01 [-0.09, 1.07]	4.59
Banerjee et al. (2012)	CCT	RCT	0.03 [-0.11, 1.16]	2.80
Heterogeneity: $T^2 = 0.01$, $I^2 = 58.66\%$, $H^2 = 2.42$			-0.09 [-0.14, -1.04]	
Entrepreneurship, microfinance, capital				
Attanasio et al. (2015)	Microcredit	RCT	-0.14 [-0.30, 0.02]	2.31
Karlan and Valdivia (2011)	Microfinance	RCT	-0.05 [-0.17, 0.07]	3.20
Banerjee et al. (2015)	Microcredit	RCT	-0.02 [-0.10, 0.05]	4.71
Tarozzi et al. (2015)	Microcredit	RCT	-0.01 [-0.04, 0.03]	6.06
Karlan and Linden (2022)	Commitment Devices - Savings	RCT	0.01 [-0.05, 0.08]	5.13
Berry et al. (2018)	Financial Education	RCT	0.03 [-0.03, 0.08]	5.36
Berry et al. (2018)	Financial Education + social	RCT	0.06 [-0.00, 0.11]	5.36
Hossain (2023)	Microcredit	RCT	0.09 [0.03, 0.15]	5.22
Heterogeneity: $T^2 = 0.00$, $I^2 = 48.25\%$, $H^2 = 1.93$			0.01 [-0.02, 0.05]	
In-kind transfers				
Edmonds and Shrestha (2014)	In-kind Stipend	RCT	-0.12 [-0.31, 0.07]	1.85
Tagliati (2022)	In-kind transfers	RCT	0.00 [-0.08, 0.08]	4.59
Datt and Uhe (2019)	In-kind transfers	PSM	0.09 [-0.01, 0.19]	3.92
Heterogeneity: $T^2 = 0.00$, $I^2 = 53.13\%$, $H^2 = 2.13$			0.01 [-0.08, 0.11]	
Other				
Hoddinott et al. (2010)	Public Work Programme (PWP)	PSM	-0.09 [-0.21, 0.03]	3.37
Strobl (2017)	Health Insurance	PSM	-0.05 [-0.10, 0.00]	5.56
Landmann and Frölich (2015)	Health Insurance	RCT	-0.03 [-0.08, 0.01]	5.77
Heterogeneity: $T^2 = 0.00$, $I^2 = 0.09\%$, $H^2 = 1.00$			-0.04 [-0.08, 0.01]	
UCT				
Edmonds and Schady (2012)	UCT	RCT	-0.09 [-0.18, 0.00]	4.16
Pellerano et al. (2020)	UCT	RCT	-0.07 [-0.16, 0.02]	4.21
Heterogeneity: $T^2 = 0.00$, $I^2 = 0.00\%$, $H^2 = 1.00$			-0.08 [-0.14, -0.02]	
Overall				
Heterogeneity: $T^2 = 0.00$, $I^2 = 72.86\%$, $H^2 = 3.68$			-0.04 [-0.07, -0.01]	
Test of group differences: $Q_b(4) = 16.52$, $p = 0.00$				



5.3.1 Employment

When looking at employment, the effect size is reported in changes in the odds of being engaged in child labour. The size of each square reflects the weight associated with the corresponding study when computing the overall effect size: studies with good precision are assigned more weight while studies with relatively less precision are assigned less weight. As explained above, a random-effects model was used, where the individual impacts could vary from study to study given the differences in the implementation of the intervention, sample size and characteristics of the sample, among others.[3] The results are presented based on the most conservative approach that only considers participation in any type of activities and studies that were classified as low or medium risk of bias.[4]

The main results show that the summary effect size is 0.98 (95 per cent CI 0.92-1.04), meaning that the odds of children being engaged in child labour are 2 per cent lower among children in participating households based on 31 effect size estimates from 23 studies. It may be concluded that the interventions analysed in this paper have a small impact on child labour, but this would be misleading. Instead of considering the overall effect, the focus should be first on cash transfers, labelled as schooling CCT and UCT in table 3. The overall effect size of schooling CCT is 0.90 (95% CI 0.81-1.00), meaning that the odds of children being engaged in child labour are 10 per cent lower among children in households offered conditional cash transfers compared with children in households who were not offered participation in a cash transfer intervention. Similarly, the overall effect size of unconditional cash transfers is 0.89 (95 per cent CI 0.80-0.99), which means that the odds of children engaging in child labour are 11 per cent lower among children in households offered unconditional cash transfers. Effect sizes are statistically significant at the 99 per cent level (p-value < 0.001).

School CCTs seemed to be effective at reducing child labour in a few studies and settings – for example, in Mexico (Behrman, Parker, and Todd 2011; Tagliati,2022); in Morocco (Benhassine et al. 2015); and in Honduras (Galiani and McEwan 2013). This type of intervention seems to contribute more effectively to a decrease in paid employment compared to children in in-kind-recipient households in Mexico (Tagliati 2022). School CCTs may also reduce household chores and work in family businesses in Morocco (Benhassine et al. 2015).

However, in some cases, school CCTs either had no impact on child labour – for example, in the Plurinational State of Bolivia and Brazil (Canelas and Niño-Zarazúa 2019; Cepaluni et al. 2022) – or increased it, in the case of Burkina Faso, Indonesia and the Philippines (de Hoop et al. 2019). School CCTs may also allow children to move away from agricultural to non-agricultural activities in Mexico (Behrman, Parker, and Todd 2011), and the poorest seem to benefit more than the less poor in Honduras and the Philippines (Galiani and McEwan 2013).

Similarly, there is evidence that unconditional cash transfers may be able to reduce child labour in Ecuador, Lesotho, Pakistan and Zambia (Awaworyi Churchill et al. 2021; Edmonds and Schady 2012; Handa et al. 2016; Pellerano, Porreca, and Rosati 2020). However, also in Ecuador, it was identified that unconditional cash transfers may increase the probability that boys in rural areas engage in child labour (Chong and Yáñez-Pagans 2019). In addition, different extracts of the population are likely to behave differently. For example, while the poorest households in Lesotho do not increase investment in children's human capital, the relatively poorer households reduce child labour and increase education (Pellerano, Porreca, and Rosati 2020). The timeframe of the impact of the intervention may vary as well. In Pakistan, it was found that the policy intervention was able to reduce child labour in the medium to long term but not immediately after the policy was implemented (Awaworyi Churchill et al. 2021).

3 A random effects model was used ("metan" or "meta forestplot"'commands in Stata) to produce forest plots and "metafunnel" to produce funnel plots.

4 Six papers were excluded where the dependent variable was not comparable to other studies (Avitabile, Cunha, and Cohn (2019), Awaworyi Churchill et al. (2021), Bandiera et al. (2013), Carvalho Filho (2012), de Hoop and Rosati (2014), and Olken, Onishi, and Wong (2011).

When other types of intervention are examined, it is observed that the overall effect size becomes higher than one. For instance, for interventions related to entrepreneurship, microfinance and capital, the estimated effect size is 1.05 (95 per cent CI 0.94-1.18), meaning that the odds of children being engaged in child labour are 5 per cent higher among children in households offered these programmes compared with children in households who were not offered participation. Effect sizes are statistically significant at the 99 per cent level (p-value < 0.001).

Most of the interventions classified as "entrepreneurship, microfinance, capital"" either had no impact on child labour or increased it slightly. For example, there is some suggestion that these programmes did not increase child labour in Ethiopia (Tarozzi, Desai, and Johnson 2015), India (Banerjee et al. 2015), Mexico (Angelucci, Karlan, and Zinman 2015), Mongolia (Attanasio et al. 2015), Peru (Karlan and Valdivia 2011), and Uganda (Karlan and Linden 2022). However, in other settings, an increase in child work was observed after these interventions were implemented –for example, in Bangladesh (Hossain 2023; Islam and Choe 2013); Ghana (Berry, Karlan, and Pradhan 2018), and Philippines (Edmonds and Theoharides 2019). An exception seems to have occurred in Nicaragua, where a mix of cash and capital granted to women in poor rural communities led to a decline in household chores and a reduction in the number of children that were only working (de Hoop et al. 2018).

The studies that examine in-kind stipends and other transfers (scholarships, education transfers given to parent associations, money for school expenses) suggest that these interventions may not be as effective as CCTs to reduce child labour in Mexico (Tagliati 2022), although there is some evidence that they can also contribute to this objective in Burkina Faso (Aurino et al. 2019) and Nepal (Datt and Uhe 2019; Edmonds and Shrestha 2014), in addition to shifting occupation from farm to non-farm activities also for Burkina Faso (Kazianga, de Walque, and Alderman 2012). Girls seem to have benefited more than boys in Burkina Faso and Nepal (Aurino et al. 2019; Datt and Uhe 2019; Kazianga, de Walque, and Alderman 2012). However, the positive outcomes of these programmes may last only while they are in place (Edmonds and Shrestha 2014). The estimated effect size is 1.03 (95 per cent CI 0.91-1.17), meaning that the odds of children being engaged in child labour are 3 per cent higher among children in households offered these programmes compared with children in households who were not offered participation.

Finally, the interventions classified as "other" encompass those in health, labour regulation, and public works programmes. The estimated effect size is 1.02 (95 per cent CI 0.80-1.30), meaning that the odds of children being engaged in child labour are 2 per cent higher among children in households offered these programmes compared with children in households who were not offered participation. The evidence is mixed: children belonging to households granted health insurance may work less in Rwanda (Strobl 2017), and general health programmes may decrease child labour in Pakistan (Landmann and Frölich 2015), although this is not confirmed in all settings (Rocha and Soares 2010, in the case of Brazil). Regulation of child labour per se may not be as effective and may even increase child labour (Bharadwaj, Lakdawala, and Li 2020, in India). Public works seem to help reduce child labour, although boys may benefit more than girls in these programmes in Ethiopia (Hoddinott, Gilligan, and Taffesse 2010).

5.3.2 Hours of work

For continuous variables, such as hours of work, the effect size is reported in standardized means. The size of each square reflects the weight associated with the corresponding study when computing the overall effect size: studies with relatively good precision are assigned more weight while studies with relatively less precision are assigned less weight. The results are presented based on the most conservative approach that considers only participation in any type of activity.

The main result shows that interventions analysed in this paper tend to reduce the extensive margin of work. The estimated effect size is negative and statistically significant on average: the estimated effect size is -0.04 standard deviations (95 per cent CI -0.07, -0.01) based on 23 impact estimates from 19 independent studies. Effects across studies, however, were heterogenous (I-squared = 73 per cent). Sub-group analysis by intervention category indicated that CCT reduced hours of work by -0.09 standard deviations (95 per cent CI

-0.14, -0.04). There is relatively strong evidence that school CCTs may decrease time spent on child labour. For example, in Nicaragua working hours in both total labour and farm work were reduced when a CCT combined with training and business grant were implemented (Del Carpio, Loayza, and Wada 2016). In Morocco, time spent working in the household business, farm and outside the household decreased after a small cash transfer made to fathers of school-aged children was granted in poor rural communities (Benhassine et al. 2015). In Colombia, different types of incentives to encourage children to increase school attendance led to a decrease in time spent working (Barrera-Osorio et al. 2011). In some contexts, CCTs do not seem to be effective in reducing time spent working – for instance in the Plurinational State of Bolivia (Canelas and Niño-Zarazúa 2019), India (Banerjee et al. 2012) and the Philippines (de Hoop et al. 2019).

Likewise, unconditional cash transfers (UCT) reduced hours of work by -0.08 standard deviations (95 per cent CI -0.14, -0.02), however, there is mixed evidence for the effectiveness of UCTs in reducing time spent working. While in Pakistan the average working hours declined after the intervention (Awaworyi Churchill et al. 2021), in Ecuador time spent in unpaid household services increased, although overall time spent working declined (Edmonds and Schady 2012). This type of intervention has also proved ineffective in Lesotho (Pellerano, Porreca, and Rosati 2020). The overall effect of other programmes (public work and health insurance programmes) is a decrease of 0.04 standard deviations (95 per cent CI -0.08, -0.01).

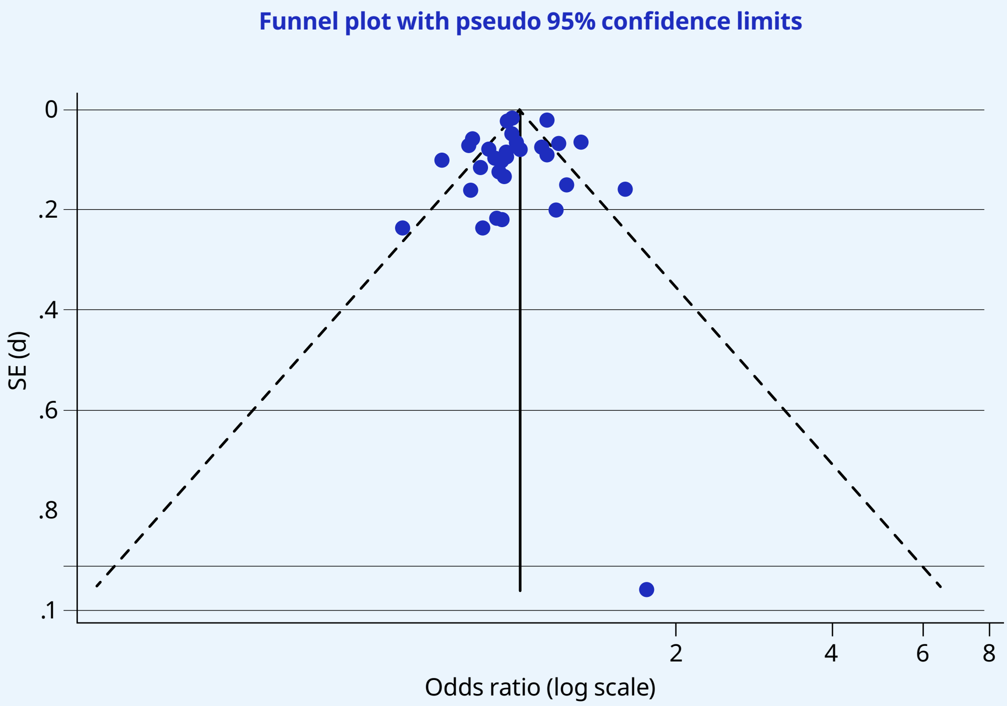
Effects were small and not statistically significant for entrepreneurship, microfinance and capital (0.01 SD, 95 per cent CI -0.02, 0.05) and in-kind transfers (0.01 SD, 95 per cent CI -0.08, 0.11). In many settings, the number of hours of work performed by children was not impacted by interventions related to entrepreneurship, microfinance and capital. For example, no change was observed in Ethiopia (Tarozzi, Desai, and Johnson 2015), India (Baland, Demont, and Somanathan 2020, Nicaragua (de Hoop et al. 2018), Peru (Karlan and Valdivia 2011) or Uganda (Karlan and Linden 2022). However, in other contexts, the time spent on child labour increased. In Bangladesh, the number of hours devoted to self-employment increased after a microcredit programme targeted at agriculture (Hossain 2023). In Ghana, the number of days worked increased for children exposed to a financial literacy programme that encouraged saving (Berry, Karlan, and Pradhan 2018). Still, in other jurisdictions, these interventions reduced the number of hours worked. For example, in Mongolia, the total hours worked by children decreased after a joint-liability microcredit programme was targeted at women (Attanasio et al. 2015).

5.4 Publication bias

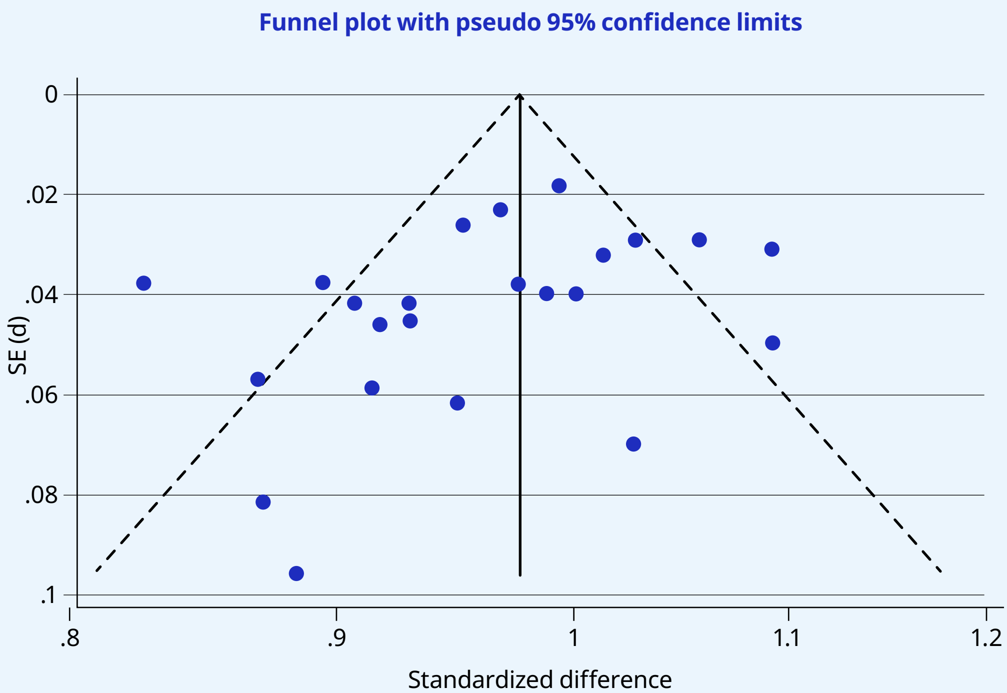
Publication bias refers to the potential behaviour of researchers to favour statistically significant results and not report estimates that do not pass the test for conventional levels of statistical significance. One way to examine whether publication bias is an issue is through the analysis of the funnel plot, where the effect size is drawn against the variance (Borenstein et al. 2011). If there is no evidence of publication bias, then we would expect the studies to be distributed symmetrically about the mean effect size, since the sampling error is random. If there is evidence of publication bias, then we would expect to see an asymmetric funnel plot, suggesting that there are some missing studies under the rationale that estimates with large standard errors (especially in small studies) are more likely to be unreported than large studies (Waddington et al. 2021). Figures 1 and 2 show funnel plots for employment and hours of work separately, where each dot represents an individual study and the solid line crosses the horizontal axis at the overall effect size estimate. The triangular area indicates the 95 per cent confidence interval.

Figure 1 suggests that the funnel plot is not asymmetric when analysing employment in any type of activity. This visual impression is confirmed by Egger's test for small-study random effects which yields a p-value of 0.9081. However, for hours of work (figure 2) a tendency towards the left is observed, which represents studies that reported negative effects with a low level of precision, as measured by the standard error. This asymmetry is confirmed by Egger's test p-value of 0.0825.

▶ **Figure 1. Funnel plot for all interventions: Employment**



▶ **Figure 2. Funnel plot for all interventions: Hours**



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6. Conclusions

A meta-analysis was performed on a comprehensive set of interventions reported from January 2010 to March 2023 to provide a quantitative assessment of the impacts of several interventions on child labour. The research covered 614 studies on child labour and forced labour identified by the ILO Research to Action (RTA) project and 40 updated studies to obtain a sample of 41 randomized and quasi-experimental impact evaluations for the meta-analysis. The results suggest that cash transfers (both conditional and unconditional) seem to be quite successful in decreasing the probability of engaging in economic activities and working hours among children in developing countries. The summary effect size from other interventions is small and, in some cases, not statistically significant. Different factors explain these results: for instance, conditional cash transfers increase the income of the household (income effect) and the opportunity cost of schooling (substitution effect) which affects the demand for child labour. Unconditional cash transfers are not attached to any specific behavioural condition to receive payment, and act as a pure income effect without changing parental time in the household or the demand for child labour in household enterprises. Other types of intervention do not target children directly; thus, we expect the impact on children to be smaller depending on the household behaviour. Moreover, these types of intervention may be underpowered to detect an effect on child labour. At the same time, given the popularity of CCTs, there is more experimental evidence on the impacts of CCTs on child labour, which provides more robust impacts.

Notwithstanding the important contributions of this meta-analysis, there are some limitations concerning the research methods and search strategy applied. First, sufficient studies were lacking to synthesize whether the policy interventions had differential effects by sex and to analyse their impacts on household chores as an outcome of interest. Second, the studies included in the review allowed measurement of the partial equilibrium effects of the interventions by comparing the mean outcomes in treatment groups to those of the untreated control or comparison groups; but it was not possible to incorporate another dimension – the cost of the interventions – since very few papers report cost or provide a benefit-cost analysis of the interventions. Third, although care was taken in collecting the coefficients of the declared preferred specification, which is usually also the more stringent one, it must be acknowledged that the size of the treatment effect in non-experimental studies is dependent on the covariates included (Borenstein et al. 2021). Finally, most of the variability around the estimates is unexplained; this should be pursued as the next step.

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Annexes

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Annexes

Annex 1. Characteristics of the 41 studies included, 2010–2023

ID	Authors (year)	Country	Intervention type	Study design	Age group	Child labour measurement	Risk of bias
19	Rocha and Soares (2010)	Brazil	Health	DID	10-17	Whether the child worked in the previous week	Medium
57	Hoddinott et al. (2010)	Ethiopia	Public work programme	PSM	6-16	Hours worked in domestic chores, agriculture, and total hours	Medium
72	Barrera Osorio et al. (2011)	Colombia	School CCT	RCT	6-10	Hours worked in the last week in primary activity	Low
107	Karlan and Valdivia (2011)	Peru	Microcredit	RCT	6-15	Whether the child works; hours of work outside the house and household chores	Medium
112	Banerjee et al. (2012)	India	CCT	RCT	6-17	Time spent working and time spent doing household chores	Low
136	Edmonds and Schady (2012)	Ecuador	UCT	RCT	11-16	Paid employment, unpaid economic activity, unpaid household services, or any other work (intensive and extensive margins)	Low
144	Kazianga et al. (2012)	Burkina Faso	School meals; take-home ratios	RCT	6-15	Whether the child does any type of labour, child productive labour, farm labour, non-farm labour, and household chores	Low
211	Islam and Choe (2013)	Bangladesh	Microcredit	IV	7-16	Performs any economic activity	Medium
227	Galiani and McEwan (2013)	Honduras	School CCT	RCT	6-12	Whether the child works outside the home or only in the home	Low
229	Tarozzi et al. (2015)	Ethiopia (excludes Eritrea)	Microcredit	RCT	10-15	Average hours worked per week on self-employment and outside activities	Low
232	Bharadwaj et al.(2020)	India	Labour regulation	DID	10-17	Hours of work per week on any economic activity, employment in banned and non-banned occupations, unpaid economic activity, unpaid household services	Low

ID	Authors (year)	Country	Intervention type	Study design	Age group	Child labour measurement	Risk of bias
269	de Hoop and Rosati (2014)	Burkina Faso	Education	RDD	7-12	Work for pay outside the household; work outside the household; farming; tending animals; work in the family business or selling goods in the street	Low
283	Edmonds and Shrestha (2014)	Nepal	In-kind stipend; scholarships	RCT	10-16	Whether the child wove carpet in the past seven days, 30 days, and 12 months, and how many hours in the past seven days	Low
314	Angelucci et al. (2015)	Mexico	Microcredit	RCT	4-17	Whether the child participated in any economic activity; the fraction of 4-17-year-olds working	Low
315	Attanasio et al. (2015)	Mongolia	Microcredit	RCT	6-15	Hours worked by the child in the past 7 days in self-employment activities and other household self-employment activities. Total hours worked in any household business and outside activities	Medium
316	Benhassine et al. (2015)	Morocco	CCT	RCT	6-15	Minutes spent the day before the survey on household chores; the same for working on household business/ farms/outside	Low
337	Landmann and Frölich (2015)	Pakistan	Health insurance	RCT	5-17	Whether the child works or not, weekly hours of work, and whether the child engages in a hazardous occupation	Medium
387	Handa et al. (2016)	Zambia	UCT	RCT	7-14	Whether the child engages in paid work, unpaid work, or both	Medium
429	Del Carpio et al. (2016)	Nicaragua	CCT	RCT	8-15	Total labour (hours in the previous week)	Low
437	Karlan and Linden (2022)	Uganda	Commitment devices - savings	RCT	10-12	Total annual hours worked	Low
486	Strobl (2017)	Rwanda	Health	PSM	7-15	Child labour is the total hours worked in the last seven days, including both economic activities and household chores (for example gathering wood, fetching water, or cooking).	Medium

ID	Authors (year)	Country	Intervention type	Study design	Age group	Child labour measurement	Risk of bias
487	de Hoop et al. (2019)	Philippines	School CCT	RCT	10-14	Any work: work for pay and outside own household; work for pay inside own household; work without pay inside own household. Number of days worked, and days worked for pay outside own household	Low
539	de Hoop et al. (2018)	Nicaragua	Capital and training	RCT	6-15	Any work in the past 12 months; any household chores in the past seven days. Usual hours of work per week and hours of household chores in the past seven days	Low
565	Berry et al. (2018)	Ghana	Financial education	RCT	12-14	Whether the child is engaged in paid work	Low
600	Datt and Uhe (2019)	Nepal	Education	PSM	8-16	Hours spent on economic work and extended economic work	Medium
608	Edmonds and Theoharides (2020)	Philippines	Productive asset transfer	RCT	12-17	Child works for pay; the child is economically active; the child is in hazardous child labour; the child is in child labour	Low
856	Awaworyi Churchill et al. (2021)	Pakistan	UCT	RDD	5-14	Whether the child participated in any economic activity; the fraction of 5-14-year-olds working	Low
861	Avitabile et al. (2019)	Mexico	CCT; in-kind food transfer	RCT	10-14	The child worked in the week before the interview	Low
868	Canelas and Niño-Zarazúa (2019)	Plurinational State of Bolivia	School CCT	DID	7-17	Work participation in any activity	Medium
869	Aurino et al. (2019)	Burkina Faso	In-kind stipend/ transfer	DID	7-16	Any work activity and farm labour	Low
873	Chong and Yañez-Pagans (2019)	Plurinational State of Bolivia	UCT	RDD	7-17	Child works, paid or unpaid market and agriculture work	Medium
877	Pellerano et al. (2020)	Lesotho	UCT	RCT	7-18	Participation in economic activities during the last 12 months prior to the interview and the number of hours and days worked during the last seven days before the interview	Low
883	Cepaluni (2022)	Brazil	School CCT	IV	4-14	Child works in economic activity	Medium
887	Tagliati (2022)	Mexico	CCT	RCT	12-16	Paid and unpaid work	Low

ID	Authors (year)	Country	Intervention type	Study design	Age group	Child labour measurement	Risk of bias
888	Baland et al. (2020)	India	Microfinance	RCT	12-17	The child is involved in productive (income-earning) activities (both in and out of the household) and domestic chores (including childcare, and fuel, wood, and water collection)	Low
892	Behrman et al. (2011)	Mexico	School CCT	RCT	9-15	The proportion of children working in the household, probability of working, and probability of participating in agricultural work	Low
894	Olken et al. (2011)	Indonesia	Multiple conditionality - CCT	RCT	6-15	Hours of wage work, hours of household work, a dummy for both	Medium
899	Bandiera et al. (2013)	Bangladesh	Entrepreneurship	DID	6-11	Hours devoted to self-employment; wage labour	Low
895	Carvalho Filho (2012)	Brazil	Other	DID	10-14	Children work for pay	Low
903	Banerjee et al. (2015)	India	Microcredit	RCT	5-15	Hours worked per child over the past 7 days	Low
999	Hossain (2023)	Bangladesh	Microcredit	RCT	5-14	Household uses child labour; the number of hours per week	Low

Annex 2. The risk of bias tool: A detailed description

Methodology

1. Randomized evaluation

- Baseline data available.
- Comparable treatment and control groups before the intervention.
- Low attrition rates.
- High take-up rates/compliance, if applicable (stated in the analysis if the estimates are intent-to-treat (ITT), treatment on the treated (TO)).
- How was the sample size determined? Power calculations/random sample.
- Eligibility criteria for participants and clusters (villages, schools, etc.).
- No confounding factors affecting the intervention (such as policy changes, issues raised at the implementation phase, etc.).
- Some details on the randomization process were provided: Who performed the randomization? Were the randomization criteria fixed over time? Was the subject aware of the randomization process?
- Unit of randomization: village, household, school. Does the regression account for the level of randomization?

2. Quasi-experimental evaluation

- More than two periods, pre- and post-periods.
- Panel data composed of the same individuals or repeated cross-sections.
- Low attrition rates, high take-up rates, if applicable (stated in the analysis if the estimates are intent-to-treat (ITT).
- The study shows that the treatment group is comparable to the comparison group absent treatment (for example, DID parallel trends assumption – placebo tests).
- The study shows that beneficiaries of the intervention did not self-select into participating in the programme. There is a clear description of the selection of participants and whether there were any deviations from the selection method over time and in the field
- If the level of analysis differs from the level of randomization, the estimates should account for this (standard errors, control variables).
- No confounding factors affecting the intervention (such as policy changes, issues raised at the implementation phase, etc.).
- If the study uses matching methods (PSM), the matching design must include a rich set of control variables at baseline, provide evidence of support group and common independence assumption, and robustness tests to econometric specifications of the bandwidth selection and matching method. The selection rule of beneficiaries should be clearly defined.

- If the study uses regression discontinuity design (RDD), the allocation should be made based on a pre-determined discontinuity. Individuals cannot affect the assignment variable which is based on clearly stated eligibility rules. Robustness checks to the bandwidth selection should be presented.
- If the study uses instrumental variables (IV) including two-stage least squares and Heckman two-step correction: IV is exogenously generated (natural experiment, etc.), evidence of a strong instrument (F-statistic should be higher than 10), and clearly stated discussion on whether the exclusion restriction is satisfied.

Scoring tool

An individual study receives a score of

- Low risk of bias: "Yes" for four or five categories
- Medium risk of bias: "Yes" for three categories
- High risk of bias: "Yes" for two or fewer categories

Target child labour directly/ education or secondary child labour outcome=1	Coded as 1 when the study examines a policy or problem that tackles child labour directly or when child labour is a secondary outcome (such as through education or cash transfers).

Randomized control trial (RCT) (maximum points = 4)	
Baseline available/balanced samples	Coded as 1 when the study shows a table of baseline characteristics and whether there are any statistically significant differences between treatment and control groups. If there is any, the paper should take the imbalance into account.
Attrition lower than 10%	Coded as 1 when the attrition rate is lower than 10% in all phases of the project and both the overall sample attrition rate and the difference in sample attrition rates between the intervention and comparison groups are considered.
Take-up/compliance	Coded as 1 when take-up was measured, and stated steps were taken to mitigate low compliance.
Details of the randomization process and sample size selection (power calc., random sample	Coded as 1 when the paper discusses how randomization was achieved, and by what method (simple randomization, block randomization, stratified randomization, etc.). Also, when the paper shows the criteria adopted to calculate the sample size necessary to make the necessary inference to answer the research questions, and whether the sample was randomly selected.

Difference-in-differences (DID) (maximum points = 4)	
Pre-/post-periods (at least one year pre-/post-)	Coded as 1 when the paper shows the outcomes of interest at least one year before and one year after the intervention.
Show parallel trends, placebo tests	Coded as 1 when the paper shows that the parallel trends assumption is observed, that is, the untreated units provide the appropriate counterfactual of the trend that the treated units would have followed if they had not been treated. Graphs and placebo tests are valid alternatives. If the authors do not attempt to show equivalence by one of these methods, or if the trends do appear to differ, they must adequately control for time-varying characteristics that might affect the outcomes.
Robustness checks	Coded as 1 when the paper shows the results of several robustness checks that aim to show the method chosen is valid and solves the common pitfalls of DID (omitted variable bias, measurement errors, etc.).
Clearly stated eligibility rules	Coded as 1 when the paper shows that the criteria adopted to obtain the counterfactual are based on theory or institutional knowledge.

Cross-sectional matching (maximum points = 4)	
Rich set of control variables correlated with T and outcome (baseline)	Coded as 1 when the paper clearly shows the control variables used in the regressions and which variables were used to create the counterfactual. If the matching analysis does not attempt to match all key control variables, or if the matching process was unsuccessful on one or more of these variables, the regression analysis must include them
Common support/CIA	Coded as 1 when the paper shows the common support (comparison of "comparable" units) and discusses the conditional independence assumption (CIA) (there is "selection on observables" and participation is independent of outcomes once observable characteristics (X) are controlled for.
Robustness checks on bandwidth selection and matching method	Coded as 1 when the paper shows the results of several robustness checks that aim to show the adequacy of the bandwidth selection and the adequacy of the matching method chosen.
Clearly stated eligibility rules	Coded as 1 when the paper shows that the criteria adopted to obtain the counterfactual are based on theory or institutional knowledge.

Regression discontinuity design (RDD) (maximum points = 4)	
Allocation is made based on a pre-determined discontinuity	Coded as 1 when the paper clearly shows the control variables used in the regressions and which variables were used to create the counterfactual. If the matching analysis does not attempt to match all key control variables, or if the matching process was unsuccessful on one or more of these variables, the regression analysis must include them.
Individuals cannot affect the assignment variable	Coded as 1 when the paper clearly shows that individuals cannot change groups (treatment/control) before the implementation of the intervention.
Clearly stated eligibility rules	Coded as 1 when the paper shows that the criteria adopted to obtain the counterfactual are based on theory or institutional knowledge.
Robustness checks	Coded as 1 when the paper shows the results of several robustness checks that aim to show the method chosen is valid and solves the common pitfalls of RDD (omitted variable bias, measurement errors, etc.).

Instrumental variables (IV) (maximum points = 4)	
Instrument is exogenous	Coded as 1 when the instrument is exogenously generated (for example, natural experiment or random assignment of participants to T/C).
The F-test in the first stage regression is higher than 10	Coded as 1 when the first stage regression (instrument-covariates) has an F-statistic equal to or above 10 (rule of thumb in the literature).
Includes relevant control for confounding, and none of the controls is likely affected by participation	Coded as 1 when the paper includes other relevant control variables to account for confounding; none of these controls will likely be affected by participation/ eligibility in the programme.
Discussion on whether the exclusion restriction is satisfied	Coded as 1 when the paper discusses the exclusion restriction based on theory or institutional knowledge, that is, that the instrument does not directly affect child labour or forced labour.

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