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Impacts of Conditional Cash Transfer Programs on Educational Outcomes in Developing Countries

A Meta-analysis

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Abstract

We meta-analyze enrollment, attendance and dropout effect estimates from forty two references of conditional cash transfer program evaluations in fifteen developing countries. Average effect sizes for all outcomes in primary and secondary schooling are statistically different from zero. Average effect sizes for secondary enrollment, attendance and dropout are larger than those for primary. For all outcomes and schooling levels, there is considerable heterogeneity in effect sizes. Programs with more generous transfers have larger primary and secondary enrollment effects. Programs that condition benefit receipt on achievement and pay transfers less frequently than monthly, exhibit larger enrollment and attendance effects. Effect sizes are no different in programs with random assignment. We find evidence in support of publication bias and selective reporting.

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I. Introduction

Conditional cash transfer (CCT) programs have spread rapidly over the last decade in the developing world. CCT programs provide cash transfers to poor families that are contingent on children's educational and health investments, typically school attendance and regular medical checkups, with the goal of breaking the intergenerational cycle of poverty. As of 2010, all but two countries in Latin America and over 15 countries in Asia and Africa had a CCT program as part of their social protection systems. In Latin America alone, CCT programs benefit over one hundred and ten million people (The Economist, 2010).

In most of these countries, a rigorous impact evaluation – typically a treatment/control experimental or observational setup – has accompanied CCT program implementation. In fact, the positive results on schooling and health outcomes of early impact evaluations of pioneer programs such as *Oportunidades* in Mexico and *Bolsa Escola* in Brazil helped paved the way for the rapid expansion of these programs elsewhere.

Recent qualitative review studies of CCT evaluations (IEG, 2011; Fiszbein et al., 2009; Rawlings and Rubio, 2005) conclude that, on the whole, these programs have positive effects on schooling (enrollment, attendance, dropout) and health (vaccinations, medical check-up) outcomes. These reviews also indicate that there is substantial variation in effect sizes between countries and among different population groups within countries (for example gender, age or urban vs. rural residence).

While the basic structure of CCTs is essentially the same, specific design features vary considerably across programs and countries. Transfer amounts – even after accounting for differences in purchasing power – vary widely across programs. In Bangladesh, for example, the average household transfer is about one percent of household expenditures while in Nicaragua it is about twenty-nine percent (Fiszbein, et al., 2009). Programs also differ in how much they pay to different groups and when. While Mexico's *Oportunidades* pays students differently depending on school grade and gender, Brazil's *Bolsa Escola* makes no such differentiation. Some countries, recognizing potential educational resource constraints, complement household transfers with a supply-side intervention such as grants school construction or school grants, teachers bonuses or textbook provision.

Individual evaluations suggest that program design features such as transfer amounts (Filmer and Schady, 2009a), timing of payments (Barrera-Osorio, Bertrand, Perez-Calle and Linden,

2009) or whether there is a supply-side intervention (Filmer and Schady, 2009b; Glewwe and Olinto, 2004) explain certain patterns of treatment effect heterogeneity. No study to date, however, integrates quantitatively and in a systematic manner the available evidence on the effects of CCT programs on schooling outcomes nor attempts to statistically understand the factors and program characteristics that mediate the heterogeneity in reported program effects. The closest available study in scope is Manley, Gitter and Slavchevska (2011), which meta-analyzes the impact of CCT programs on nutritional status. Our main contribution to the CCT literature is, therefore, to integrate in a rigorous and systematic way the available evidence on CCT effects on educational outcomes, and shed light on which factors mediate heterogeneity in treatment effects.

From a literature search of over 25 electronic databases conducted in the spring of 2010, we surveyed 2,931 initial references containing the words "conditional cash transfer" or "conditional cash transfers" in either title, keyword or abstract (introduction if abstract not available). After screening out duplicate references, references that did not report effects on school enrollment, attendance or dropout and references that where either summary of other reports, reviews or commentaries, we narrowed down our sample to forty-two references covering CCT programs in fifteen developing countries, twenty-eight of which report effects on enrollment, nineteen on attendance and nine on dropout (some references report effects in more than one of these outcomes.) Two independent coders coded reference (such as authors, type of publication, sample size, effects, standard errors) and program (such as transfer amounts, conditions) characteristics. We computed inter-rater reliabilities for separate coding-protocol modules (program characteristics, study characteristics, enrollment effects, attendance effects, dropout effects) and in all cases obtained reliabilities of 85% or higher. We cross-validated each discrepancy by referring back to the corresponding reference. To statistically combine and analyze effect sizes we use various random and mixed effects models.

We find five key results. First, CCT average effect sizes on enrollment, attendance and dropout are all positive and statistically significant and larger in magnitude for secondary than for primary schooling. For instance, our primary schooling enrollment average effect size estimates are of around five percentage points. Relative to the average primary enrollment at baseline – which in our sample of studies is 84% – the average effect size represents a six-percent enrollment increase. In contrast, the average effect size for secondary enrollment – close to six percentage points – represents a ten percent enrollment effect. For attendance outcomes the difference is even more drastic. Relative to baseline attendance, the average effect size in primary is about three percent while for secondary is close to twelve percent. We find that CCTs reduce dropout rates by twice as much in secondary that in primary schooling. Also consistent with the observation that schooling effects might be larger in settings with lower initial conditions (see for example, Fiszbein et al., 2009) we find that, for both primary and secondary, effects on attendance are statistically larger when attendance baseline levels are lower.

Second, for all outcomes (enrollment, attendance and dropout) in all levels (primary and secondary) we find high levels of heterogeneity – in excess of seventy percent – in reported effects. Perhaps unsurprisingly, most of the heterogeneity stems from variation in effects across programs rather than within program.

Third, we find systematic patterns of bivariate associations between effects sizes and program characteristics. We find that more generous program transfer amounts (measured as percent of PPP-adjusted GDP per capita) are positively and significantly associated with larger primary and secondary enrollment effects (and in some models with larger secondary attendance effects as well), which suggests that more generous transfers might better compensate for the opportunity cost of sending children to school. This result challenges previous single-country evidence from Cambodia suggesting decreasing returns to transfer amount (Filmer and Schady, 2009a).

In both primary and secondary levels and for both enrollment and attendance outcomes, we find that the frequency of payment (monthly vs. less frequently) is negatively associated with the size of effects. Programs in which transfer payment is bi-monthy or quarterly tend to report larger effects than those in which payment is monthly. This result is consistent with single-program evidence from Bogotá (Barrera-Osorio, Bertrand, Perez-Calle and Linden, 2009) and with the idea that limited attention (Karlan, McConnell, Mullainathan and Zinman, 2011) or limited self-control (Ashraf, Karlan and Yin, 2006) can constrain families from saving.

Imposing conditions on achievement (such as not failing grades) beyond the standard attendance conditions is positively associated with larger secondary enrollment and attendance effects. This finding resonates with recent literature highlighting the importance of incentives in education (see for example Duflo, Hanna and Ryan, forthcoming; Bettinger, Kremer and Saavedra, 2010; Kremer, Miguel and Thornton, 2009).

We find significantly larger primary enrollment effects in programs that also attempt to expand supply through grants, infrastructure or other resources for schools. This result is consistent with single-program evidence from the Mexico's *Oportunidades* suggesting that school enrollment impacts were larger in areas with better school infrastructure and lower pupil-teacher ratios (Berhman, Parker and Todd, 2005) and with evidence from Colombia highlighting the importance of resource constraints in education (Saavedra, in press). We also find that secondary enrollment and attendance effect sizes are larger for CCT programs in Asia or Africa relative to those in Latin America. This finding is driven very large – of the order of twenty percentage points – secondary effect sizes for Cambodia's CESSP and JFPR programs.

Fourth, we find no systematic association between effect sizes and whether benefits were randomly assigned. All CCT programs target benefits to underprivileged groups. Such negative selection would suggest that observational evaluations might understate program impacts relative to evaluations that deal with selection through randomization. While we find that random assignment is positively, although statistically insignificantly associated with primary enrollment and attendance effect sizes, we also find that it is negatively associated with secondary enrollment (statistically insignificantly) and attendance (statistically significantly). This finding corroborates previous qualitative evidence by IGN (2011) indicating that among comparable CCT programs there are little differences between effects reported by experimental and observational evaluations.

Finally, we find some evidence indicative of publication bias and selective reporting. Published papers report larger effects for secondary enrollment and attendance. We find large heterogeneity in the number of effect estimates that each reference reports, with the median reference reporting between six and eleven effect estimates depending on the outcome and some references reporting more than twenty effect estimates. With the exception of primary enrollment estimates, funnel plots for all other outcomes and corresponding linear regression (Egger) tests also suggest asymmetry in reported effects.

We conclude that CCT programs are more effective in contexts in which initial enrollment and attendance conditions are relatively poor and for that reason, particularly effective in improving secondary schooling outcomes. When offered to primary school students, offering a more generous transfer amount and coupling transfers with additional resources such as infrastructure, textbook or teachers is associated with larger program effects. The relative effectiveness of CCTs for secondary schooling outcomes and additional costs related to transfer generosity and supply-side resources suggest that under tight budgetary conditions, targeting CCT investments to secondary level pupils is one simple way improve program cost-effectiveness.

In terms of design, we conclude that programs that impose conditions on school achievement such as not failing grades and that pay transfers less frequently are more effective than programs that simply condition on attendance. Methodologically we conclude by i) highlighting the similarities in effect sizes reported by experimental and observational evaluations, yielding credence to recent developments in the econometrics of program evaluation literature (see for example, Imbens and Wooldridge, 2009) and ii) advocating – as Duflo, Glennerster and Kremer (2007) do for randomized evaluations in development economics – for the importance of setting clear reporting standards for CCT impact evaluations given the popularity of these programs worldwide.

II. Literature Search

We search published and gray literature to find all available studies that report estimates of the impact of CCT programs on school enrollment, school attendance and/or school dropout.

We carried out the literature search in the spring of 2010. To minimize exclusion errors we began by searching for "conditional cash transfer" or "conditional cash transfers" in reference titles, abstracts or keywords in the following electronic databases: African Healthline, CAB Direct, Database of Abstracts of Reviews of Effectiveness, EBSCO, EconLit, Effective Practice and Organization of Care Group (EPOC), Eldis, British Library for Development Studies (BLDS), EMBASE, FRANCIS, Google Scholar, Healthcare Management Information Consortium, ID21, International Bibliography of the Social Sciences (IBSS), Internet Documents in Economics Access Service (Research Papers in Economics- IDEAS[Repec]), Inter-Science, Latin American and Caribbean Health Sciences Literature (LILACS), MEDCARIB, Medline, Pan American Health Organization (PAHO), POPLINE, ProQuest, Scielo, ScienceDirect, Social Science Research Network (SSRN), The Cochrane Central Register of Controlled Trials, Virtual

Library in Health (ADOLEC), WHOLIS (World Health, Organization Library Database) and World Bank.[1]

We retrieved all references in English or Spanish language regardless of geographic focus. We limited our search to published and unpublished studies, including refereed and non-refereed journals, working papers, conference proceedings, book chapters, dissertations, government reports, non-governmental reports and other technical reports. We did not include published comments, op-eds, summaries or media briefings.

To confirm that we had not left out studies, we cross-validated the initial literature search with the reference lists of Fiszbein et al.'s (2009) CCT review book and Milazzo's (2009) annotated bibliography on CCT programs. If we found a new reference from these two sources, we included it as long as it met the language and publication type restrictions above. This initial search procedure yielded 2,921 references, of which 1,341 were duplicate references (i.e. identical references) retrieved from more than one search engine (Table 1).

In the second stage, we asked two research assistants to independently flag studies that in the title, abstract (or introduction if no abstract was available) contained any of the following words: education, school, schools, enrollment, attendance, achievement, attainment, test-scores / test scores, drop-out/dropout/drop out, graduation, persistence, performance, retention, advancement, fail/ failing, pass/passing. The two principal investigators resolved any arising discrepancies from this process. With this filter we eliminated 342 references, keeping 1,248 for additional screening.

In the third stage, the two principal investigators independently read the abstract, introduction, methodological sections and tables of these 1,248 remaining and only retained references that met the following criteria:

1. *Intervention specification*: Reference must report CCT program effects on school enrollment, attendance or dropout. We understand CCT programs to be programs that provide monetary (i.e. not in kind) transfers to participant households in exchange of

[1] The complete list of search dates and articles retrieved from each database is available upon request.

compliance with program requirements (i.e. not unconditional), which may include health visits and school enrollment/attendance.

2. *Outcome variables*: Reference must report at least one impact and its associated standard error or t-statistic on school enrollment, attendance or dropout.
3. *Geographic focus*: Study must report impacts on a CCT implemented in a developing country (i.e. studies from the United States are excluded).
4. *Research design*: Study must use a treatment-comparison research design. The comparison group can be wait-list, or no treatment. One group pre-post designs are not eligible. Non-randomized studies are eligible only if they report relevant pre-treatment characteristics of treatment and comparison groups.

Inconsistencies – the bulk of which were errors on inclusion rather than exclusion – between the two researchers were then discussed and resolved by looking at the details of the manuscripts. We retained from this filter 48 references, 6 of which were an older version of a retained reference. Our sample of analysis therefore comprises 42 references, and closely matches the sample of CCT references in IGN (2011). Appendix Table A provides details of the references in the final analysis sample.

III. Coding of References in Analysis Sample

We created a coding protocol (available upon request) to capture in a hierarchical structure (i.e. effects in references, references in programs) the following information:

Program descriptors

Program targeting (both geographic and household targeting criteria); type of assignment to conditions (simple random assignment, random assignment after matching, stratification or blocking, nonrandom assignment); nature of the control group (whether the control group receives nothing from program or is on a waiting list); schooling conditionality (whether schooling conditionality is based on school enrollment, school attendance, grade promotion and/or other); school attendance conditionality (minimum school attendance required for schooling subsidy receipt); health conditionality (children attendance to regular health visits, children immunizations up to date, health visits for pregnant and breastfeeding women, mother's attendance to health education workshops or other); whether or not there is verification of school attendance and health conditions; member of the household that receives the subsidy (child, mother, father or both parents); amount of schooling and health subsidies (both in US dollars and/or domestic currency); frequency of payment of both schooling and health subsidies; whether the subsidy amounts vary by gender, grade, age or other characteristic. We also collected data on whether or not the program provides supply incentives for education and health.

Reference descriptors

Type of publication (journal article, book chapter, book, working paper, thesis or doctoral dissertation, conference paper/proceeding, government report, or technical report); publication year; country and language (English or Spanish); source of data; sample sizes at baseline and follow-up of both treatment and control groups; attrition rates for both treatment and control groups; whether or not baseline data were collected before households began receiving benefits; whether or not there is balance between treatment and control groups in all reported baseline characteristics.

Effects

Effect for school enrollment, school attendance and school drop-out, separately for primary and secondary schooling, unless effect sizes are reported for primary and secondary overall. For each outcome, we extracted information on mean and standard deviation at baseline, effect size (value, methodology of estimation, subgroup and sample size), standard error or t-statistic of the estimated effect, and time where the effect is measured.

We coded references as follows. Two trained research assistants (A and B) independently coded 17 of the 42 references in the sample using separate paper versions of the coding protocol. During this coding stage, coders where allowed to talk to each other and PIs to resolve questions. For the remaining 25 references, the principal investigators randomized the order in which to code them and coders where not allowed to talk to each other. We then randomly assigned research assistants C and D to separately input in Excel the 42 protocols of either assistant A or B.

With two separate versions of sample descriptors and effects information, we estimated various inter-rater reliabilities (IRR) for program-, reference- and effect-level variables, defined as the percent of coincidences over the total number of variables in the set. Reassuringly, we computed IRR’s of 84.8% for program-level variables, 89.5% for reference-level variables, 91.3% for enrollment effect sizes, 87.4% for attendance effect sizes and 96.9% for dropout effect sizes.

A principal investigator with the help of research assistant C, referred to the corresponding reference document for variables with detected inconsistencies and retrieved the correct response. Once we solved discrepancies one a case-by-case basis, we created a unified dataset for analysis.

IV. Sample Description

Programs in Sample

Table 2 presents a summary of CCT programs in our analysis sample. Our sample contains 42 references reporting effects for 19 programs in 15 countries. Sixty-three percent of programs (12 of them) are from Latin America, 32% are from Asian (6) and one is from Africa.

Table 2 demonstrates the degree of heterogeneity in program characteristics. For example, 68% of programs condition transfer-receipt on school attendance – which is typically 80% or more of the schooling reference period, while 32% impose additional conditions on school achievement such as grade promotion or school achievement as a requirement. In most programs, school officials verify student attendance.

There is also variation in payment frequency and whether transfer amounts vary for different target groups. Fifty two percent of programs pay educational transfers on a monthly basis and over forty percent pay transfers less frequently, either bi-monthly, quarterly or bi-annually. In almost 60% of programs all children regardless of age, grade or gender are entitled to the same transfer amount. In 30% of programs, however, transfers for girls differ from boys' or transfer amounts vary by grade or age.

We were interested in exploring the association between transfer amount and effect sizes, motivated by the finding in Filmer and Schady (2009) for Cambodia suggesting non-increasing returns to transfer amounts. To do so across countries, we opted to construct measures of monthly-equivalent average transfer amounts relative to PPP-adjusted GDP per capita.[2] This

[2] Another alternative would have been to use the transfer amount as a fraction of total household expenditures in the sample. Very few references reported expenditures, which is why we opted for transfer as a fraction of PPP-adjusted GDP per capita.

measure of transfer amount displays considerable variation across programs and across schooling levels. In the typical program, monthly schooling transfers for primary are 2.3% of PPP-adjusted GDP per capita, and the standard deviation is 2 percentage points. For secondary school, average transfer amount is 4% of PPP-adjusted-GDP per capita.

In over 70% of CCT programs, the demand-side transfer is unaccompanied by any sort of supply side intervention. In over 20% of programs in the sample, however, schools receive some form of support ranging from grants to infrastructure construction to textbook and other school inputs.

In most programs, assignment to treatment is not random and beneficiaries are usually selected using a variety of means tests. In 30% of programs, on the other hand, beneficiaries are selected randomly, most commonly after screening on the basis of geography or poverty. In close to 80% of programs the control group receives nothing, and in close to 20% controls are wait-listed.

Reference Characteristics

Table 3 shows reference-level characteristics of references in our analysis sample. Over fifty percent of references are working papers, less than 25% are journal articles and the remaining 25% are either government or technical reports and unpublished manuscripts/dissertations. Seventy six percent of references in our sample use program survey data to estimate program impacts, and the remaining use either census or household survey data or other data sources.

Sixty-seven percent of references in our analysis sample report effects on enrollment, primary, secondary or both. Forty-five percent report effects on attendance and 21% report effects on school dropout. (Some references report effects on more than one type of outcome.) We provide extensive details of each program and reference in our sample in Appendix Tables A and B, respectively.

Figure 1 shows the distribution of number of effects that each paper reports, separately by outcome and school level. For all outcomes and all levels, there is considerable heterogeneity in effect reporting, and all distributions have a long right tail. For primary enrollment, for example, conditional on reporting for the outcome, the median paper reports six effects, but the average reports ten, because four paper report 20 or more effects (different subgroups by age, grade, location or methodology). For secondary enrollment, the distribution is more symmetric conditional on reporting effects for this outcome: the median paper reports eleven effects and the average reports twelve, with four papers reporting more than twenty effects. For attendance, distributions of reported effects are fairly symmetrical, conditional on reporting. Conditional on reporting primary attendance outcomes, the median reference reports eight effects and the average nine, with two references reporting twenty-four or more effects. Conditional on reporting secondary attendance effects, median and mean number of reported effects is seven, with one reference reporting twenty-four effects. For primary dropout, conditional on reporting, the median paper reports six effects and the mean reports eight effects. One reference reports twenty-two primary dropout effects. Conditional on reporting secondary dropout effects, the median reference reports three effects, the mean reference reports five and one reference reports eighteen effects.

V. Methodological Approach to Combine and Analyze Effect Sizes

Universe of generalization

The most important decision in choosing the method for statistical inference in meta-analysis is the universe to which the study aims to generalize (Cooper et al., 2009). In our study we seek to make inferences beyond those CCT programs in our sample to gain knowledge about potential CCT impacts in different populations and under potentially different conditions. As such, CCT programs in our hypothetical universe of study might differ from those in our sample along three dimensions: i) study characteristics, including transfer amount, conditionality requirements and whether the nature of supply-side components; ii) true effect size parameter, and iii) effect-size estimates due to sampling variation.

For these reasons, given that we do not hold fixed study characteristics potentially related to effect sizes, the appropriate method for statistical inference in our case is a random-effects model (Cooper, Hedges and Valentine, 2009). Formally, let T_i denote the estimated effect size of study i, with population effect size θ_i and i=1,…..,k. Under a random-effects model, θ_i has a random component u_i in addition to sampling variation, e_i:

$$T_i = \theta_i + u_i + e_i \qquad (1)$$

The variability of $T_i - v_i^*$ – stems from variability arising from sampling variation v_i and variation of θ_i around its population mean τ_θ^2. In a fixed effects model $\tau_\theta^2 = 0$.

$$v_i^* = v_i + \tau_\theta^2 \qquad (2)$$

Estimating effect sizes

All educational outcome measures we focus on are dichotomous: enrollment, attendance and dropout. Estimates of T_i in our study are therefore either the post-treatment difference in the corresponding probability between treatment and comparison groups, $T_i = p_{it} - p_{ic}$, or a double difference (treatment v. control, before v. after).

Treatment-control contrasts are a natural measure in our context, and the way in which almost all references report program effects.[3] One potential concern with using mean contrasts to estimate average effect sizes with equations (1) and (2) is that mean contrasts tend to overstate τ_θ^2 -- the amount of heterogeneity arising from variation in effect sizes – in the sample because the possible values of the contrast when the group means are close to 0.5 is larger than those when the means are close to 0 or 1 (Fleiss and Berlin, 2009). The upward bias in $\hat{\tau}_\theta^2$ from using mean contrasts is empirically is inconsequential. For primary schooling, for example, baseline enrollment levels are quite similar across references in the sample, ranging from 71% to 98%. For secondary schooling, the range is larger, from 24% to 97%. Our heterogeneity estimate $\hat{\sigma}_\theta^2$ is, however, only slightly larger for secondary than for primary enrollment effect sizes.

Combining estimates of effect sizes

We pursue two approaches to combine effects sizes. We apply both approaches to estimate average effect sizes separately for each outcome and schooling level. In the first approach, we combine all of a reference effects in one reference-level average effect under a fixed-effects

[3] The only exceptions are Davis et al. (2002) which reports probit coefficients that we convert to (approximate) marginal effects by dividing the probit coefficient by 2.5 (Wooldridge, 2005), and Raymond and Sadoulet (2003) that report hazard ratios for dropout that we convert to percentage points using baseline hazards for each grade.

assumption.[4] Specifically, let T_{ij} denote the *j*'th effect estimate *j=1,2….., J* of study i, v_{ij} its associated variance and $w_{ij} = 1/v_{ij}$. Then the average study-level effect T_i is:

$$T_i = \frac{\sum_{j=1}^{J}(w_{ij}T_{ij})}{\sum_{j=1}^{J} w_{ij}} \qquad (3)$$

And it variance is:

$$v_i = \frac{1}{\sum_{j=1}^{J}(1/v_{ij})} \qquad (4)$$

Under a fixed-effects model, for the k studies in our sample, the overall mean effect size $\bar{T}$ is therefore:

$$\bar{T} = \frac{\sum_{i=1}^{k}(w_i T_i)}{\sum_{i=1}^{k} w_i} \qquad (5)$$

The variance of $\bar{T}$ is:

$$v = \frac{1}{\sum_{i=1}^{k}(1/v_i)} \qquad (6)$$

The homogeneity test to test whether σ_θ^2, the variation of θ_i around its population mean is zero is:

$$Q = \sum_{i=1}^{k} w_i\,(T_i - \bar{T})^2 \qquad (7)$$

which under the null hypothesis of fixed-effects (i.e. H_0: τ_θ^2=0) has a chi-square distribution with $k-1$ degrees of freedom. An unbiased estimate of τ_θ^2 is then obtained by computing:

$$\widehat{\tau_\theta^2} = [Q - (k-1)]/c \qquad (8)$$

where,

$$c = \sum_{i=1}^{k} w_i - \left[\sum_{i=1}^{k} w_i^2 / \sum_{i=1}^{k} w_i\right]$$

We then calculate the random-effects mean of T_i, $\bar{T}_*$ and its variance v_* by computing $v_i^* = v_i + \hat{\tau}_\theta^2$ and $w_i^* = 1/v_i^*$ and using them instead of v_i and w_i in equations (5) and (6). We do this procedure separately for each outcome (enrollment, attendance, dropout) and each

[4] When a reference reports effects for different follow-up periods – one year and two years after baseline data collection, for instance – we we compute separate average effect sizes for each measurement period. This occurred in the case of Duryea and Morrison (2004) who report primary attendance effects for two follow-up periods and for Skoufias and Parker (2001) who report primary and secondary attendance effects for three follow-up periods.

schooling level (primary and secondary). We estimate (5) and (6) using Restricted Maximum Likelihood and Method of Moments estimators.

In the second approach to estimate overall average effect sizes for each outcome and school level, we take all estimates from all papers and combine them directly in a random effects model. The second approach is useful for two reasons. The second approach allows us to test the sensitivity of the average effect size estimate to the fixed effects assumption. Although the fixed-effects assumption is arguably justified in our context by the fact that all effects in a given study correspond to the same underlying population and thus share the same institutional characteristics, it is important to test how it affects our estimates.

The second approach of directly combining all effect sizes in a random effects model also allows us to test for selective reporting using funnel plots and Egger linear regression tests that we explain in detail in the "analyzing effect sizes" section of the methodology, below.

Dealing with hierarchical dependence of effects

The effect size estimates in our application are not necessarily independent from each other because the random effects in our sample of studies are clustered. The clustered structure of the random effects arises from the nesting of more than one reference within a program, sharing the same program characteristics. We deal with this type of hierarchical dependence following the methods of Hedges, Tipton and Johnson (2010).

Specifically, we assume that our sample of $k = k_1 + k_2 + \cdots + k_m$ effect sizes is obtained by sampling m clusters of estimates (e.g. programs) and then sampling $k_l \geq 1$ estimates within the lth cluster. The vector of true effect sizes $\boldsymbol{\theta}$ may depend on a set of $p=1,\ldots,P<m$ covariates with a vector of associated coefficients $\boldsymbol{\beta} = (\beta_1, \ldots., \beta_P)'$, so that:

$$\boldsymbol{T} = \boldsymbol{X\beta} + \boldsymbol{u} + \boldsymbol{e} \qquad (9)$$

Because random effects from the same cluster are correlated, the covariance matrix of $u + e$ is a block-diagonal matrix of m $k_l \times k_l$ non-diagonal matrices:

$$\Sigma = Diag(\Sigma_1, \ldots, \Sigma_m) \quad (10)$$

Each cluster-specific covariance matrix l has a between-clusters variance component τ^2 and a between-studies-within-cluster variance component ω^2:

$$\Sigma_l = \omega^2 I_l + \tau^2 J_l + V_l \qquad (11)$$

with I_l a $k_l \times k_l$ identity matrix, J_l a $k_l \times k_l$ matrix of 1's and V_l a $k_l \times k_l$ diagonal matrix with the error variance v_i for each of the studies in cluster *l*. Note that the covariance structure need not be known *a priori*. Hedges, Tipton and Johnson (2010) propose to estimate ω^2 and τ^2 via method of moments and to use $\widehat{\omega}^2$ and $\hat{\tau}^2$ to compute efficient weights for study *i* as follows:

$$w^R{}_i = \frac{1}{v_i + \widehat{\omega}^2 + \hat{\tau}^2} \qquad (12)$$

Allowing for the hierarchical structure, we then compute the overall random-effects mean effect size as:

$$b_0 = \frac{\sum_{l=1}^{m} \sum_{i=1}^{k_l} w_i{}^R T_i}{\sum_{l=1}^{m} \sum_{i=1}^{k_l} w_i{}^R} \qquad (13)$$

with robust variance given by (8):

$$v^R = \frac{\sum_{l=1}^{m} \widetilde{w}_l^2 \,(T_l - b_0)^2}{\sum_{l=1}^{m} \widetilde{w}_l^2} \qquad (14)$$

where T_l is the unweighted mean of the estimates in the *l*th cluster, $\widetilde{w}_l^2$ is the total weight given to estimates in cluster *l*, and b_0 is the estimate of the overall mean from (13).

Analyzing effect sizes

To explore how study characteristics explain variability in effect sizes, we estimate bivariate random effects and robust (hierarchical) random-effects models in which the dependent variable is an effect (one per paper or all effects directly) and the explanatory variable is a program or reference characteristic, for instance, subsidy amount or whether the reference is published. We focus on bivariate associations because as we show in the following section, many program characteristics are highly collinear with each other and we have no a priori reason to include some of them and not others.

We employ two techniques to assess the extent to which publication bias and selective reporting are issues of potential concern in the CCT evaluation literature: funnel plots and Egger linear regression tests. The first is funnel plots in which we plot each impact estimate against the sample size used to calculate it. The intuition behind this test is straightforward. When sample sizes are small, there is likely a lot of variation in estimated effects around the overall (random effects) average effect size. As sample sizes increase, estimates on both sides of the overall effect will gradually converge to the overall effect, rendering a funnel-shaped plot of effect estimates. In the absence of publication bias and selective reporting, the funnel plot should look symmetrical and the number of effects should be evenly distributed around the overall effect (Sutton, 2009). The suppression of some effects that is associated with publication bias and selective reporting results in the plot being asymmetrical, with patchy spots of "missing effects."

Egger linear regression tests are a statistical formalization of the intuition behind funnel plots. In Egger tests, we regress standardized effect sizes against the reciprocal of the standard errors and a constant term. The constant provides a measure of asymmetry and thus we can test the null hypothesis of no asymmetry using a standard t-test on the constant term. None of these tests are "magic bullets," however. The funnel plot might be asymmetric if, for example, smaller studies take place under less rigorous conditions. Similarly, Egger's regression analysis has inflated type I errors in meta-analyses with dichotomous outcome variables. Moreover, asymmetry might be due to heterogeneity in effect sizes. For these reasons, we take the results from these tests as suggestive, not conclusive (Sutton, 2009.)

VI. Results

School Enrollment Average Effect Sizes

Figures 2 and 3 show the forest plots (distribution) of average effect sizes from all studies reporting enrollment effects on primary and secondary school, respectively. In all forest plot figures we report the average effect size per reference, combining all estimates into one using a fixed-effects model. In Table 4 we report primary and secondary average effect sizes both using one effect per reference and all effects directly.

We highlight three aspects of Figure 2. First, the overall random-effects average primary enrollment effect size is 5.1 percentage points, with a 95% confidence interval between 3.7 and 6.6 percentage points. Relative to the mean baseline primary enrollment of 84%, the average effect size represents a 6 percent enrollment increase. Second, with the exception of one reference reporting effects from the *SRMP* CCT program in Turkey, all reference-level average effects are positive and most are statistically distinguishable from zero. Third, there is ample variation in estimated effects across references, although most of it is across programs rather than within-program and across references. Reference-level effect sizes for Nicaragua's *Red de Protección Social* are an exception, however, ranging from close to 8 to 29 percentage points, and statistically positive. For Colombia's *Familias en Acción* and Brazil's *Bolsa Escola*, reference-level effects are, on the other hand, consistently small and generally statistically positive.

Panel A of Table 4 shows primary enrollment average effect size estimates under different modeling choices. Importantly, average effect size estimates are very similar – between 5 and seven percentage points – whether we use one estimate per reference or all estimates, validating the fixed-effects methodology to compute a reference-level. Allowing for clustering of effects within program increases slightly the estimated standard error of the average effect size. Method of moments random-effects estimates – whether or not they account for clustering of estimates within programs – are more stable than Restricted Maximum Likelihood estimates. Under all model choices, we strongly reject the null hypothesis that the between-study variance component τ_θ^2 is zero and estimate that a very large share of variation in effect sizes, close to 90% or more, is due to "true" heterogeneity rather than sampling variation.

In the final column of Panel A of Table 4 we also include estimates of $\widehat{\omega}^2$ the within-program variance component, when we estimate the random-effects model allowing for arbitrary clustering of effects within program. Consistent with the visual evidence of Figure 2, this variance component is negligible, supporting the hypothesis that most of the variation in effect sizes is across rather than within program.

Figure 5 displays the forest plot of secondary enrollment average effect sizes, with one effect per reference that we estimate under a fixed-effects model. The average secondary enrollment effect is remarkably similar in percentage points to that of primary enrollment – 6 percentage points – although as a fraction of baseline enrollment it is notably larger, representing a 10-percent secondary enrollment increase.

The conclusion that CCT programs are more effective at increasing secondary than at increasing primary enrollment resonates with previous CCT review findings in Fiszbein et al. (2009). Like the primary enrollment forest plot, the secondary enrollment plot displays considerable effect-size variation, most of which is across CCT programs. While some CCT programs such as Ecuador's Bono de Desarrollo Humano show modest secondary enrollment improvements – around two percentage points – others like Cambodia's JFPR Scholarship and CESSP programs report average secondary enrollment impacts of close to twenty percentage points.

Panel B of Table 4 validates the main forest plot conclusions under different modeling assumptions. Estimates using one effect per reference or all effects are very comparable ranging between five and six percentage points. This result yields additional support to the fixed-effects assumption in combining multiple estimates from a single reference. For secondary enrollment we strongly reject the null hypothesis that variation is effect sizes is entirely attributed to sampling error and also estimate that most of this variation stems from variation across programs (in τ_θ^2) than within programs across references (in ω^2).

School Attendance Average Effect Sizes

Figure 4 displays the primary attendance effect size distribution (one effect per paper). Fewer references report primary attendance effects relative to those reporting primary enrollment. The average random-effects primary attendance effect is 2.5 percentage points – which off of a baseline attendance of 80% represents a three percent attendance effect – and is statistically significantly different from zero. A clear outlier is Nicaragua's *Red de Proteccion Social*, with reported average attendance effect of thirteen percentage points. For this program, as we noted earlier, primary enrollment effects are also notoriously large. With the exception of Uruguay's *Ingreso Ciudadano*, all primary attendance reference-level effects are positive and the majority statistically different from zero.

In Panel A of Table 5 we validate primary attendance effect estimates to modeling choices and conclude that the main reference-level random-effects estimate is robust to estimation methodology, and to the fixed-effects assumption since average effect sizes are quite similar whether we use one or all estimates per reference. In all cases, the average CCT primary attendance effect is positive and statistically different from zero and as is the case for enrollment, we: i) reject that variation in effect sizes is simply due to sampling variation and ii) most of this variation stems from between rather than within program across study sources.

Figure 5 displays the secondary attendance forest plot with one effect per reference computed using a fixed effects model. The CCT average secondary attendance effect is 8 percentage points. This effect represents a 12% increase in attendance relative to the average baseline secondary attendance level of 68%.

Figure 5 indicates that there is considerable heterogeneity in secondary attendance effects across programs. At one extreme stands Cambodia's CESSP with average secondary attendance effect sizes of twenty to thirty percentage points. (This program's evaluation also reports notoriously high secondary enrollment effects.) At the other extreme we find Malawi's CCT program with average secondary attendance effect sizes that although positive and statistically significant are small – close to half of a percentage point.

Panel B of Table 5 shows average secondary attendance effect sizes under different modeling choices; these results indicate that estimates of average secondary attendance effects are robust to modeling strategies including the fixed-effects assumption for combining reference estimates and whether or not we account for the hierarchical (clustering) structure of effects, which yields slightly larger standard errors for the average effect size. We strongly reject the null hypothesis that variation is effect sizes is simply sampling variation, and attribute more than ninety percent of variation to effect heterogeneity. As before, we conclude that the bulk of the heterogeneity is across programs as the estimate for ω^2 in the last column is negligible relative to the estimate of between-program heterogeneity.

School Dropout Average Effect Sizes

Compared to enrollment and attendance, few CCT references report dropout effects: nine for primary and six for secondary. This relatively low number of references reflects on the uncertainty with which we calculate average effects.

Figure 6 shows primary dropout's forest plot. The overall average dropout effect size for primary is negative one percentage point and statistically different from zero, although the 95% confidence interval is relatively wide. Nicaragua's *Red de Proteccion Social* and Brazil's *Bolsa Escola* have the largest effects on dropout reduction, while the evaluation of Ecuador's *Bono de Desarrollo Humano* suggests, if any, increases in dropout as a consequence of program participation.

Unlike enrollment and attendance, only two papers report baseline dropout rates so we opted for not reporting an average to avoid potential issues of sample selection in converting effect sizes to relative magnitudes. Average effect sizes are similar across different model specifications – ranging from negative one to negative two percentage point reductions – and are all distinguishable from zero, as Panel A of Table 6 suggests. Most of the variation in effect sizes is due to heterogeneity across programs.

The average secondary dropout effect of negative four percentage points is three times larger (in percentage points) than that for primary dropout, as Figure 7 indicates. Although we estimate the average secondary dropout effect with a high degree of uncertainty due to the fact that only six references report effects for this outcome, we still reject the null hypothesis that CCT programs do not affect secondary dropout outcomes. All reported secondary dropout effects are negative and statistically different from zero and those from Brazil's *Bolsa Escola* and Mexico's *Progresa* stand out as the largest effects in secondary dropout reduction, close to eight percentage points. Average effect size estimates for secondary dropout for *Progresa*, however, differ drastically across references.

In Panel B of Table 6 we estimate the overall secondary dropout effect size and conclude that the estimate is insensitive to model choices. All estimates of the overall dropout effect size are between negative three and four percentage points and all are statistically different from zero. Moreover, we strongly reject the hypothesis that variation in effect sizes is due to sampling variation and estimate that the majority of such variation is due to heterogeneity across programs.

<u>Meta-regression results</u>

Before we analyze the relationship between effect size estimates and mediator characteristics such as subsidy amount or frequency of payment, we report in Table 7 pairwise correlations between these possible mediators of effect size heterogeneity.

As Table 7 indicates, some program characteristics are highly correlated with each other. Not surprisingly, for example, the correlation between primary and secondary subsidy amounts is 0.89, suggesting that programs that provide generous transfers for primary schooling are also generous for secondary schooling. Similarly, there is a strong and negative correlation (-0.55) between paying transfers monthly (as opposed to less frequently) and whether the program conditions transfer on achievement in addition to attendance.

CCT programs that pay transfers to the mother of the household are programs that also vary subsidy amounts by grade or age. Latin American CCT programs rarely condition transfers on achievement as suggested by the strong negative correlation between these two attributes (-0.65). Interestingly, programs in which control-group units are on a waitlist to receive benefits in the future (as opposed to receiving nothing) are also programs that provide supply-side supplements such as grants to schools, infrastructure or teaching materials (correlation is 0.68).

Due to the high degree of collinearity between program characteristics, we pursue a bivariate meta-regression approach to analyzing effect sizes. We only pursue a meta-regression analysis for enrollment and attendance outcomes. We do not analyze in a meta-regression framework dropout effect size due to the small number of studies reporting effects for this outcome. Tables 8 through 11 reports bivariate correlations between effect sizes and effect mediators that we classify as program (e.g. subsidy amount), reference (e.g published) or contextual (e.g. Latin American) characteristics. For all outcomes we report results using, as before, variations of random-effects models including using the fixed-effect reference-level effect size or all effect sizes and accounting for the hierarchical dependence of effect sizes within programs.

Table 8 reports meta-regression results for primary enrollment effect sizes. Conditioning transfer receipt on achievement is not associated enrollment effect sizes. Primary enrollment effect sizes are significantly larger in programs with more generous transfer amounts. Primary enrollment effect sizes are larger in programs in which transfers are paid less periodically than monthly (statistically significant in specifications that use all estimates). Primary enrollment effect sizes are larger in programs that provide supplement school services with grants, infrastructure or other inputs. If elementary schools are operating at nearly-full capacity, as baseline enrollment rates indicates, it is not surprising that expanding supply might be a pre-condition to increase enrollment.

Programs that target beneficiaries geographically by using community-level proxy means tests have larger primary enrollment effects that non-targeted ones; the difference is statistically significant in specifications using all estimates from all references. The magnitude of the association, however, is comparable to that of specifications using the fixed-effects average per reference, suggesting that the latter approach might be slightly underpowered for this particular targeting mediator.

Programs in which control group members are placed in a waitlist to receive benefits in the future – as opposed to receiving nothing from the program – shows more positive effects, and the difference is statistically significant. For the case of primary school enrollment at least, this results challenges previous evidence indicating that anticipation bias – the change in behavior among control group members in anticipation of future benefit receipt – might lead to underestimating program effects (Attanasio et al., 2010).

Programs that select participants at random from a pool of eligible households display slightly larger primary enrollment effects, although differences in effect sizes with non-random assignment programs are insignificant and fairly small in magnitude in specifications that employ all estimates. Reference and contextual characteristics, such as whether the paper is published or whether the program is Latin American do not show systematic associations with primary enrollment effect sizes.

Table 9 shows meta-regression results for secondary enrollment. Programs that in addition to the usual attendance conditions also condition transfers on achievement (e.g. not failing a grade) are more effective at increasing secondary enrollment than programs that just condition on attendance. As is the case with primary enrollment, secondary enrollment effect sizes are larger in programs that provide more generous educational transfers, and the association is statistically significant in specifications that include all estimates.

Secondary enrollment effect sizes are smaller in programs that give the subsidy to the mother only and that vary subsidy amounts by grade or age. For secondary schooling we find no difference in effect sizes between programs that select beneficiaries at random and those that do not. In contrast with results for primary schooling, secondary effect sizes are smaller in programs that place control group units in a wait list. This result suggests that anticipation bias might be more of a concern, for the secondary enrollment margin.

Secondary enrollment effects are larger in published references. At the same time, effect sizes are negatively associated with the number of effects that a reference reports: papers that report more effects tend to report smaller effects, on average. Latin American programs are less effective than the rest at increasing secondary enrollment. Consistent with the forest plot evidence, Cambodian programs are highly effective at increasing secondary enrollment.

For primary attendance effect sizes, we the pattern of associations is less clear, in part because for this outcome our meta-analysis might be underpowered: only ten references in our sample report primary attendance effects (Table 10). As is the case with primary enrollment, however, we find some supporting evidence that programs in which transfers are paid less frequently than monthly also exhibit larger effect sizes. Similarly, primary attendance effect sizes are larger in contexts with lower baseline attendance levels (association is statistically significant).

Table 10 shows meta-regression results of secondary attendance average effect sizes. As is the case with secondary enrollment, conditioning transfer receipt on achievement is associated with larger secondary attendance effect sizes. Providing subsidies on a monthly basis (compared to less frequent) is associated with smaller effect sizes for both enrollment and attendance (in secondary). As is also the case with secondary enrollment effect sizes, secondary attendance effects are smaller in programs in which only the mother is entitled to receive the transfer.

Programs with random assignment yield larger secondary attendance effects than programs with non-random assignment of benefits. This result suggests that – at least for secondary attendance – it is not necessarily the case that effects are downward biased (relative to estimates based on random assignment) due to potential negative selection of beneficiaries.

Secondary attendance effects are significantly larger in published impact evaluation results and effects tend to increase over time. As is the case with secondary enrollment effect sizes, effects from Latin American CCT programs are smaller than those from programs elsewhere and the two CCT programs in Cambodia are notoriously effective at increasing secondary attendance, just as they are as increasing secondary enrollment.

Publication Bias and Selective Reporting

We have already reported some suggestive evidence of publication bias and selective reporting in CCT impact evaluation reports. For instance, effect sizes for both secondary enrollment and secondary attendance are significantly larger in published references than in unpublished ones. Similarly, we noted the wide degree of heterogeneity in the number of effects that references report: median number of reported effects ranges from six to eleven across outcomes and levels and some references report more than twenty effects.

In this section we report graphical and linear regression results from additional publication bias and selective reporting tests. We use two tests: funnel plots and linear regression Egger-type tests. Figures 8 through 13 display funnel plots separately for each outcome. Table 12 reports Egger tests for each outcome and level separately. Effects for primary enrollment do converge to the overall random effects average effect size, but the density of effects is not symmetric around the overall mean. Column 1 of Table 12 confirms this asymmetry: we strongly reject the null hypothesis that the constant is zero. Effects for secondary enrollment are also converge to the overall mean as sample size increases, but the funnel plot is considerably more symmetric than that for primary enrollment (Figure 9). Results in column 2 of Table 12 support the symmetry conclusion for secondary enrollment effects, as we cannot reject the null hypothesis that the constant is different from zero.

Effects for primary attendance converge to the overall mean as sample size increases (Figure 10). The funnel plot is visibly asymmetric, with a large patch of missing effects to the left of the overall mean. The funnel plot for secondary attendance effect sizes is also visibly asymmetric (Figure 11). Statistical analysis in columns 3 and 4 of Table 12 reject the hypothesis of funnel plot symmetry for both of these outcomes.

Figures 12 and 13 display funnel plots for primary and secondary dropout effects. Effects for both levels tend to converge to the overall effect size as sample size increases, but the are both visibly asymmetrical, with patches of missing positive effects (for instance, smaller reductions in dropout than the overall effect size). Results in columns 5 and 6 confirm the visual inspection of the funnel plots and for the case of primary dropout reject the null hypothesis of funnel plot symmetry. For secondary dropout, the magnitude of the constant is large (in standard deviation units) but the test is underpowered due to the small number of effects. Overall we conclude that for most outcomes – perhaps with the exception of secondary enrollment – there is suggestive evidence in support of publication bias and/or selective reporting. The heterogeneity in the number of effects that each paper reports provides additional support to this conjecture.

VII. Conclusion

We find that CCT programs appear to be more effective for secondary than for private schooling. Relative to baseline enrollment, the average effect size for secondary enrollment is ten percent, while for primary it is six percent relative to baseline enrollment. For attendance outcomes the difference is even more drastic. Relative to baseline attendance, the average effect size in primary is about three percent while for secondary is close to twelve percent. We find that CCTs reduce dropout rates by twice as much in secondary that in primary schooling.

We also find that effect sizes for all outcomes (enrollment, attendance and dropout) in all levels (primary and secondary) exhibit high degree of heterogeneity. Most of the heterogeneity stems from variation in effect estimates across programs rather across references of the same program.

We find that more generous program transfer amounts are positively and significantly associated with larger primary and secondary enrollment effects which, in particular, previous single-country evidence from Cambodia suggesting decreasing returns to transfer amount (Filmer and Schady, 2009a). We find that the frequency of payment (monthly vs. less frequently) is negatively associated with the size of effects. Programs in which transfer payment is bi-monthy or quarterly tend to report larger effects than those in which payment is monthly.

Imposing conditions on achievement (such as not failing grades) beyond the standard attendance conditions is positively associated with larger secondary enrollment and attendance effects. This finding resonates with recent literature highlighting the importance of incentives in education (see for example Duflo, Hanna and Ryan, forthcoming; Bettinger, Kremer and Saavedra, 2010; Kremer, Miguel and Thornton, 2009).

Consistent with previous evidence on country-level CCT impact evaluations (Berhman, Parker and Todd, 2005), programs that complement cash transfers with supply-side interventions have statistically larger effects on primary enrollment, but not for secondary enrollment. We hypothesize that this finding is driven by supply constraints being more binding in primary schooling where baseline enrollment levels are already quite high – over 80%.

We find no systematic association between effect sizes and whether benefits were randomly assigned. While we find that random assignment is positively, although statistically insignificantly associated with primary enrollment and attendance effect sizes, we also find that it is negatively associated with secondary enrollment (statistically insignificantly) and attendance (statistically significantly). This finding corroborates previous qualitative evidence by IGN (2011) indicating that among comparable CCT programs there are little differences between effects reported by experimental and observational evaluations.

Finally, we find some evidence indicative of publication bias and selective reporting. Published papers report larger effects for secondary enrollment and attendance. We find large heterogeneity in the number of effect estimates that each reference reports, with the median reference reporting between six and eleven effect estimates depending on the outcome and some references reporting more than twenty effect estimates. With the exception of primary enrollment estimates, funnel plots for all other outcomes and corresponding linear regression (Egger) tests also suggest asymmetry in reported effects.

We conclude that CCT programs are more effective in contexts in which initial enrollment and attendance conditions are relatively poor and for that reason, particularly effective in improving secondary schooling outcomes. When offered to primary school students, offering a more generous transfer amount and coupling transfers with additional resources such as infrastructure, textbook or teachers is associated with larger program effects. The relative effectiveness of CCTs for secondary schooling outcomes and additional costs related to transfer generosity and supply-side resources suggest that under tight budgetary conditions, targeting CCT investments to secondary level pupils is one simple way improve program cost-effectiveness.

In terms of design, we conclude that programs that impose conditions on school achievement such as not failing grades and that pay transfers less frequently are more effective than programs that simply condition on attendance. Methodologically we highlight the similarities in effect sizes reported by experimental and observational evaluations. From an impact evaluation policy perspective we advocate for setting clear reporting standards for CCT impact evaluations given the popularity of these around the world.

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Table 1. Reference screening procedure to obtain analysis sample

Phase 1	
Total references	2,931
Duplicates	1,341
No education-related words in abstract or title	342
Total eligible references phase 1	1,248
Phase 2	
Articles that did not meet inclusion criteria	
Intervention specification (unconditional transfer, scholarships, in-kind transfers)	24
Outcomes variables not related to education	146
Research design does not meet requirements	15
Other topic or type of document (policy briefs, comments, descriptive reports, reviews, etc)	1,015
Total ineligible references	1,200
Phase 3	
Old version of an eligible paper	6
Total eligible references	**42**

Table 2. Characteristics of CCT Programs in analysis sample

	Freq	%	n
Total number of programs		100	
Region [a]			
Latin America	12	63.2	
Asia	6	31.6	
Africa	1	5.3	
Education conditionality requirements			
School attendance only	5	26.3	
School enrollment and attendance	8	42.1	
Grade promotion or achievement	6	31.6	
Minimum school attendance for subsidy receipt [b] (mean, sd)	82.5	.04	14
Verification of school attendance			
Yes	9	47.4	
No	2	10.5	
No information reported	8	42.1	
Payment frequency			
Monthly	10	52.6	
Bimonthly	4	21.1	
Other	4	21.1	
No information reported	1	5.3	
Monthly average subsidy amount as a % of PPP- adjusted GDP per capita (mean, sd)			
Primary	2.3	2.0	13
Secondary	4.2	4.3	17
School subsidy amount varies by			
Gender	3	15.8	
Grade or age	3	15.8	
None	11	57.9	
Other [c]	2	10.5	
Supply incentives for education			
Yes	4	21.1	
No	14	73.7	
No information	1	5.3	
Type of assignment to conditions			
Random	6	31.5	
Non-random	13	68.4	
Nature of the control group			
Receives nothing from program	15	79.0	
Wait list, delayed entry	4	21.0	

[a] Programs that have changed their name are counted as different program because in some cases these changes were accompanied with changes in the program (these cases are: Bolsa Familia and Bolsa Escola in Brazil; Progresa and Oportunidades in Mexico; and CESSP and JFPR Scholarship Program in Cambodia.

[b] Percentage of time in school (month, every two months or school year).

[c] Dropout risk (CESSP program in Cambodia) and random (CCT for Schooling program in Malawi)

Table 3. Characteristics of references in analysis sample

Total number of references	42	
Publication type		
Journal article	10	23.8
Working paper	22	52.4
Government/technical reports	7	16.7
Unpublished	3	7.1
Source of data		
Program survey	32	76.2
National household survey	3	7.1
Census data	4	9.5
Other	3	7.1
Reports effects on		
Enrollment	28	66.7
Attendance	19	45.2
Dropout	9	21.4

See notes to Table 1 for reference screening procedure and Appendix Tables A and B for reference details.

Table 4. School enrollment effect sizes

Estimates used	Model Used to Compute Overall Average Effect Size	Number of Estimates	Average Effect Size	Standard Error of Average Effect Size	Between-Reference Variance (tau-squared)	Chi-square Test (2)	p-value of Chi-square Test	Percent of Total Variation in Effect Sizes Due to Heterogeneity	Within-Program Variance, Hierarchical (omega-squared)
				A. PRIMARY SCHOOL ENROLLMENT (Mean at baseline[a]: 0.840)					
One per ref. (1)	Random Effects, REML	19	0.055	0.016	0.005	735	0.000	97.55%	-
One per ref. (1)	Random Effects, MM	19	0.051	0.016	0.001	735	0.000	97.55%	-
One per ref. (1)	Random Effects, MM, Hierarchical	19	0.052	0.022	0.001	770	0.000	97.55%	0.000
All estimates	Random Effects, REML	187	0.071	0.009	0.010	1761	0.000	89.44%	
All estimates	Random Effects, MM	187	0.054	0.007	0.002	1761	0.000	89.44%	
All estimates	Random Effects, MM, Hierarchical	187	0.056	0.020	0.002	1761	0.000	89.44%	0.000
				B. SECONDARY SCHOOL ENROLLMENT (Mean at baseline[b]: 0.593)					
One per ref. (1)	Random Effects, REML	22	0.060	0.013	0.004	1302	0.000	98.39%	-
One per ref. (1)	Random Effects, MM	22	0.059	0.013	0.001	1302	0.000	98.39%	-
One per ref. (1)	Random Effects, MM, Hierarchical	22	0.059	0.014	0.001	1302	0.000	98.39%	0.000
All estimates	Random Effects, REML	258	0.049	0.005	0.005	2409	0.000	89.33%	
All estimates	Random Effects, MM	258	0.047	0.005	0.001	2409	0.000	89.33%	
All estimates	Random Effects, MM, Hierarchical	258	0.048	0.014	0.001	2409	0.000	89.33%	0.001

(1) computed from all estimates in paper under fixed effects assumption
(2) of null hypothesis that between-study variance is zero
[a] Simple average from papers that reported primary enrollment at baseline (n=12)
[b] Simple average from papers that reported secondary enrollment at baseline (n=10)

Table 5. School attendance effect sizes

Estimates used	Model Used to Compute Overall Average Effect Size	Number of Estimates	Average Effect Size	Standard Error of Average Effect Size	Between-Reference Variance (tau-squared)	Chi-square Test (2)	p-value of Chi-square Test	Percent of Total Variation in Effect Sizes Due to Heterogeneity	Within-Program Variance, Hierarchical (omega-squared)
				A. PRIMARY SCHOOL ATTENDANCE (Mean in sample= 0.804)[a]					
One per ref. (1)	Random Effects, REML	10	0.029	0.011	0.001	113.4	0.000	92.07%	-
One per ref. (1)	Random Effects, MM	10	0.025	0.009	0.0002	113.4	0.000	92.07%	-
One per ref. (1)	Random Effects, MM, Hierarchical	10	0.025	0.008	0.0002	113.4	0.000	92.07%	0.000
All estimates	Random Effects, REML	86	0.023	0.003	0.0003	317.8	0.000	73.25%	-
All estimates	Random Effects, MM	86	0.022	0.002	0.0002	317.8	0.000	73.25%	-
All estimates	Random Effects, MM, Hierarchical	86	0.023	0.003	0.000	317.8	0.000	73.25%	0.0003
				B. SECONDARY SCHOOL ATTENDANCE (Mean in sample=0.676)[b]					
One per ref. (1)	Random Effects, REML	18	0.082	0.021	0.007	4050	0.000	99.58%	-
One per ref. (1)	Random Effects, MM	18	0.081	0.021	0.0008	4050	0.000	99.58%	-
One per ref. (1)	Random Effects, MM, Hierarchical	18	0.081	0.026	0.002	4050	0.000	99.58%	0.000
All estimates	Random Effects, REML	131	0.095	0.010	0.012	4713	0.000	97.24%	-
All estimates	Random Effects, MM	131	0.079	0.008	0.0007	4713	0.000	97.24%	-
All estimates	Random Effects, MM, Hierarchical	131	0.086	0.028	0.002	4713	0.000	97.24%	0.000

(1) computed from all estimates in paper under fixed effects assumption

(2) of null hypothesis that between-study variance is zero

[a] Simple average from papers that reported primary attendance at baseline (n=9), [b] Simple average from papers that reported secondary attendance at baseline (n=9)

Table 6. School dropout effect sizes

Estimates used	Model Used to Compute Overall Average Effect Size	Number of Estimates	Average Effect Size	Standard Error of Average Effect Size	Between-Reference Variance (tau-squared)	Chi-square Test (2)	p-value of Chi-square Test	Percent of Total Variation in Effect Sizes Due to Heterogeneity	Within-Program Variance, Hierarchical (omega-squared)
				A. PRIMARY DROPOUT EFFECTS					
One per ref. (1)	Random Effects, REML	9	-0.013	0.008	0.001	3603	0.000	99.78%	-
One per ref. (1)	Random Effects, MM	9	-0.013	0.008	0.0002	3603	0.000	99.78%	-
One per ref. (1)	Random Effects, MM, Hierarchical	9	-0.013	0.008	0.0002	3603	0.000	99.78%	0.000
All estimates	Random Effects, REML	72	-0.024	0.005	0.0010	4283	0.000	98.34%	-
All estimates	Random Effects, MM	72	-0.023	0.004	0.0001	4283	0.000	98.34%	-
All estimates	Random Effects, MM, Hierarchical	72	-0.024	0.012	0.0002	4283	0.000	98.34%	0.000
				B. SECONDARY DROPOUT EFFECTS					
One per ref. (1)	Random Effects, REML	6	-0.036	0.014	0.001	1238	0.000	99.60%	-
One per ref. (1)	Random Effects, MM	6	-0.037	0.017	0.002	1238	0.000	99.60%	-
One per ref. (1)	Random Effects, MM, Hierarchical	6	-0.037	0.012	0.000	1238	0.000	99.60%	0.005
All estimates	Random Effects, REML	31	-0.039	0.009	0.002	1290	0.000	97.68%	-
All estimates	Random Effects, MM	31	-0.036	0.008	0.001	1290	0.000	97.68%	-
All estimates	Random Effects, MM, Hierarchical	31	-0.038	0.011	0.001	1290	0.000	97.68%	0.0004

(1) computed from all estimates in paper under fixed effects assumption

(2) of null hypothesis that between-study variance is zero

Table 7. Pairwise correlation coefficients between program characteristics

	Conditioned on achievement	Primary subsidy amt.	Secondary subsidy amt.	Payment made monthly	Subsidy age or grade variation	Subsidy paid to mother	Supply	Baseline prim. enrollment	Baseline sec. enrollment	Geographic targeting	Latin America	Random	Wait list
Conditioned on achievement	1.0												
Primary subsidy amt.	-0.33	1.0											
Secondary subsidy amt.	-0.11	0.89***	1.0										
Payment made monthly	-0.55**	0.29	0.13	1.0									
Subsidy age or grade variation	-0.07	-0.42	-0.18	-0.33	1.0								
Subsidy paid to mother	-0.49	-0.04	-0.19	-0.15	1.0***	1.0							
Supply	-0.09	0.11	-0.19	0.25	0.04	0.11	1.0						
Baseline prim. enrollment	0.30	-0.27	0.87	-0.05	0.29	0.24	-0.45	1.0					
Baseline sec. enrollment	-0.57		0.28	0.57	-0.34	-0.34	-0.34		1.0				
Geographic targeting	-0.28	-0.29	0.21	-0.20	0.41*	0.58**	0.40	-0.38	-0.34	1.0			
Latin America	-0.65***	0.33	-0.31	0.20	0.12	0.58*	0.15	-0.30	-0.57	0.13	1.0		
Random assign.	-0.22	0.16	0.31	0.15	-.07	-0.00	0.47**	-0.29	-0.32	0.42*	0.28	1.0	
Wait list	-0.07	0.03	-0.17	-0.06	0.34	0.56*	0.68***	0.31	-0.34	0.41*	0.13	0.21	1.0

*** p<0.01, ** p<0.05, * p<0.1

Table 8. Bivariate regressions for primary enrollment effect sizes

	One estimate per paper				All estimates			
	REML no clustering	MM no clustering	MM clustering	n	REML no clustering	MM no clustering	MM clustering	n
Program design characteristics								
Conditional on achievement (1=yes)	-0.036	-0.031	-0.031	19	-0.015	-0.001	-0.002	187
	(0.054)	(0.053)	(0.046)		(0.030)	(0.027)	(0.028)	
Monthly subsidy in primary (% of ppp-GDP/capita)	0.028 ***	0.028 ***	0.028 **	18	0.033 ***	0.024 ***	0.025	185
	(0.008)	(0.008)	(0.009)		(0.005)	(0.005)	(0.017)	
Payment is given monthly (vs. less frequent)	-0.043	-0.040	-0.040	19	-0.075 ***	-0.057 ***	-0.058 *	187
	(0.033)	(0.032)	(0.038)		(0.016)	(0.014)	(0.030)	
Subsidy varies by grade or age (1=yes)	-0.054	-0.051	-0.052	19	-0.049 ***	-0.031 **	-0.032	187
	(0.031)	(0.031)	(0.042)		(0.017)	(0.015)	(0.038)	
Only mother receives the subsidy (1=yes)	-0.019	-0.023	-0.023	10	-0.020	-0.023	-0.023	97
	(0.030)	(0.031)	(0.020)		(0.027)	(0.028)	(0.019)	
Supply component (1=yes)	0.071 **	0.07 **	0.071	18	0.082 ***	0.054 ***	0.061	186
	(0.031)	(0.031)	(0.046)		(0.018)	(0.017)	(0.066)	
Geographic targeting (1=yes)	0.056	0.052	0.053	19	0.06 ***	0.045 ***	0.047	187
	(0.036)	(0.035)	(0.032)		(0.018)	(0.016)	(0.027)	
Random assignment (1=yes)	0.048	0.044	0.045	19	0.024	0.006	0.007	187
	(0.032)	(0.031)	(0.041)		(0.018)	(0.015)	(0.035)	
Waiting list for control group (1=yes)	0.065 **	0.061 *	0.062	19	0.064 ***	0.046 ***	0.048	187
	(0.030)	(0.030)	(0.040)		(0.017)	(0.014)	(0.033)	
Reference characteristics								
Published (1=Yes)	-0.016	-0.015	-0.015	19	-0.036 *	-0.023	-0.025	187
	(0.038)	(0.037)	(0.026)		(0.019)	(0.016)	(0.029)	
Number of reported effects	0.002	0.002	0.002	19	0.003 **	0.002	0.002	187
	(0.002)	(0.002)	(0.002)		(0.001)	(0.001)	(0.002)	
Months between follow-up and baseline (/100)	-0.036	-0.033	-0.033	16	-0.089	-0.082	-0.083	127
	(0.056)	(0.051)	(0.038)		(0.057)	(0.053)	(0.049)	
Contextual characteristics								
Latin American country (1=yes)	0.036	0.031	0.031	19	0.015	0.001	0.002	187
	(0.054)	(0.053)	(0.046)		(0.030)	(0.027)	(0.028)	
Primary enrollment rate at baseline	-0.020	-0.013	-0.015	12	0.014	0.025	0.023	144
	(0.146)	(0.144)	(0.139)		(0.096)	(0.091)	(0.118)	

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 9. Bivariate regressions for secondary enrollment effect sizes

	One estimate per paper				All estimates			
	REML no clustering	MM no clustering	MM clustering	n	REML no clustering	MM no clustering	MM clustering	n
Program design characteristics								
Conditional on achievement (1=yes)	0.057 **	0.062 **	0.062	22	0.048 ***	0.048 ***	0.049	258
	(0.027)	(0.027)	(0.040)		(0.013)	(0.012)	(0.051)	
Monthly subsidy in secondary (% of ppp- GDP/capita)	0.004	0.004	0.004	20	0.01 ***	0.009 ***	0.009 *	252
	(0.003)	(0.002)	(0.003)		(0.002)	(0.001)	(0.005)	
Payment is given monthly (vs. less frequent)	-0.024	-0.025	-0.025	22	-0.013	-0.017 *	-0.016	258
	(0.026)	(0.026)	(0.038)		(0.011)	(0.010)	(0.029)	
Subsidy varies by grade or age (1=yes)	-0.032	-0.032	-0.032	22	-0.029 ***	-0.024 **	-0.025	258
	(0.026)	(0.026)	(0.025)		(0.011)	(0.010)	(0.021)	
Only mother receives the subsidy (1=yes)	-0.049	-0.050	-0.050	17	-0.052 ***	-0.045 ***	-0.048	201
	(0.029)	(0.029)	(0.030)		(0.014)	(0.012)	(0.031)	
Supply component (1=yes)	-0.001	-0.002	-0.002	21	-0.016	-0.015	-0.016	257
	(0.032)	(0.032)	(0.021)		(0.016)	(0.014)	(0.028)	
Geographic targeting (1=yes)	-0.018	-0.019	-0.019	22	-0.016	-0.012	-0.013	258
	(0.026)	(0.026)	(0.029)		(0.011)	(0.010)	(0.023)	
Random assignment (1=yes)	-0.027	-0.029	-0.028	22	-0.011	-0.016	-0.014	258
	(0.026)	(0.026)	(0.028)		(0.011)	(0.010)	(0.024)	
Waiting list for control group (1=yes)	-0.025	-0.025	-0.025	22	-0.041 ***	-0.036 ***	-0.038 *	258
	(0.027)	(0.027)	(0.025)		(0.011)	(0.010)	(0.022)	
Reference characteristics								
Published (1=Yes)	0.038	0.039	0.039	22	0.06 ***	0.06 ***	0.060	258
	(0.033)	(0.033)	(0.051)		(0.013)	(0.012)	(0.042)	
Number of reported effects	-0.002	-0.002	-0.002	22	-0.002 ***	-0.002 ***	-0.002 **	258
	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)	
Months between follow-up and baseline (/100)	-0.040	-0.039	-0.039	15	-0.053	-0.064	-0.062	157
	(0.077)	(0.076)	(0.031)		(0.037)	(0.036)	(0.048)	
Contextual characteristics								
Latin American country (1=Yes)	-0.056 **	-0.059 **	-0.059 *	22	-0.057 ***	-0.055 ***	-0.056 *	258
	(0.024)	(0.023)	(0.028)		(0.011)	(0.010)	(0.032)	
Cambodia	0.178 ***	0.179 ***	0.179 ***	22	0.176 ***	0.185 ***	0.185 ***	258
	(0.024)	(0.024)	(0.018)		(0.017)	(0.016)	(0.010)	
Secondary enrollment at baseline	-0.001	-0.001	-0.001	9	0.001	0.001	0.001	126
	(0.002)	(0.002)	(0.001)		(0.001)	(0.001)	(0.001)	

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 10. Bivariate regressions for primary attendance effect sizes

	One estimate per paper				All estimates			
	REML no clustering	MM no clustering	REML no clustering	n	REML no clustering	MM no clustering	MM clustering	n
Program design characteristics								
Monthly subsidy in primary (% of ppp-GDP/capita)	0.005	0.001	0.002	10	-0.002	-0.002	-0.002	86
	(0.006)	(0.005)	(0.007)		(0.002)	(0.001)	(0.004)	
Payment is given monthly (vs. less frequent)	-0.032	-0.025	-0.026	9	-0.018**	-0.017**	-0.018*	78
	(0.024)	(0.020)	(0.020)		(0.007)	(0.007)	(0.008)	
Subsidy varies by grade or age (1=yes)	-0.027	-0.018	-0.018	10	-0.002	-0.001	-0.001	86
	(0.022)	(0.020)	(0.023)		(0.006)	(0.006)	(0.013)	
Only mother receives the subsidy (1=yes)	-0.014	-0.012	-0.013	8	-0.006	-0.006	-0.005	77
	(0.011)	(0.011)	(0.010)		(0.006)	(0.006)	(0.005)	
Supply component (1=yes)	0.018	0.010	0.011	10	0.011	0.009	0.011	86
	(0.023)	(0.019)	(0.023)		(0.009)	(0.008)	(0.026)	
Geographic targeting (1=yes)	0.021	0.016	0.016	10	0.010	0.009	0.010	86
	(0.028)	(0.023)	(0.016)		(0.006)	(0.006)	(0.011)	
Random assignment (1=yes)	0.018	0.010	0.011	10	0.011	0.009	0.011	86
	(0.023)	(0.019)	(0.023)		(0.009)	(0.008)	(0.026)	
Waiting list for control group (1=yes)	0.008	0.003	0.003	10	-0.000	-0.001	-0.000	86
	(0.024)	(0.021)	(0.021)		(0.005)	(0.005)	(0.008)	
Reference characteristics								
Published (1=Yes)	0.035	0.016	0.017	10	-0.005	-0.006	-0.005	86
	(0.027)	(0.025)	(0.046)		(0.009)	(0.008)	(0.026)	
Number of reported effects	-0.001	-0.001	-0.001	10	-0.000	-0.000	-0.000	86
	(0.001)	(0.001)	(0.001)		(0.000)	(0.000)	(0.000)	
Months between follow-up and baseline (/100)	0.482	0.452	N/A	6	0.101	0.090	0.103	56
	(0.349)	(0.337)			(0.083)	(0.078)	(0.161)	
Contextual characteristics								
Primary attendance at baseline	-0.136	-0.127	-0.127	7	-0.108 **	-0.104 **	-0.114	64
	(0.115)	(0.099)	(0.076)		(0.047)	(0.047)	(0.084)	

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Latin America dummy and conditionality on achievement are perfectly collinear with primary attendance

Table 11. Bivariate regressions for secondary attendance effect sizes

	One estimate per paper				All estimates			
	REML no clustering	MM no clustering	MM clustering	n	REML no clustering	MM no clustering	MM clustering	n
Program design characteristics								
Conditional on achievement (1=yes)	0.108 **	0.109 **	0.109	18	0.118 ***	0.092 ***	0.097	131
	(0.039)	(0.038)	(0.062)		(0.019)	(0.016)	(0.062)	
Monthly subsidy in secondary (% of ppp- GDP/capita)	0.001	0.001	0.001	16	0.007 ***	0.002	0.004	121
	(0.004)	(0.004)	(0.006)		(0.002)	(0.002)	(0.006)	
Payment is given monthly (vs. less frequent)	-0.097 **	-0.1 **	-0.099 *	18	-0.092 ***	-0.078 ***	-0.08 *	131
	(0.035)	(0.035)	(0.045)		(0.019)	(0.016)	(0.041)	
Subsidy varies by grade or age (1=yes)	-0.014	-0.016	-0.015	18	-0.048 **	-0.028	-0.032	131
	(0.045)	(0.046)	(0.043)		(0.020)	(0.018)	(0.040)	
Only mother receives the subsidy (1=yes)	-0.042	-0.042	-0.042	14	-0.079 ***	-0.051 ***	-0.059	113
	(0.052)	(0.052)	(0.058)		(0.022)	(0.019)	(0.053)	
Supply component (1=yes)	-0.043	-0.042	-0.043	17	-0.057 **	-0.042 *	-0.046	125
	(0.057)	(0.057)	(0.034)		(0.026)	(0.023)	(0.032)	
Geographic targeting (1=yes)	-0.063	-0.068	-0.064	18	-0.079 ***	-0.066 ***	-0.070	131
	(0.039)	(0.040)	(0.048)		(0.019)	(0.016)	(0.050)	
Random assignment (1=yes)	-0.076 *	-0.077 *	-0.077 *	18	-0.083 ***	-0.072 ***	-0.075 *	131
	(0.041)	(0.040)	(0.038)		(0.020)	(0.016)	(0.038)	
Waiting list for control group (1=yes)	-0.020	-0.023	-0.022	18	-0.043 *	-0.028	-0.031	131
	(0.051)	(0.052)	(0.038)		(0.024)	(0.022)	(0.030)	
Reference characteristics								
Published (1=Yes)	0.035	0.036	0.036	18	0.087 ***	0.066 ***	0.070	131
	(0.056)	(0.056)	(0.088)		(0.025)	(0.024)	(0.093)	
Number of reported effects	0.003	0.003	0.003	18	0.001	0.001	0.001	131
	(0.004)	(0.004)	(0.005)		(0.001)	(0.001)	(0.003)	
Months between follow-up and baseline (/100)	2.358 ***	2.446 ***	2.374 ***	10	2.338 ***	2.445 ***	2.407 ***	62
	(0.227)	(0.225)	(0.213)		(0.262)	(0.194)	(0.216)	
Contextual characteristics								
Latin American country (1=yes)	-0.072 *	-0.072 *	-0.072	18	-0.126 ***	-0.094 ***	-0.106*	131
	(0.040)	(0.040)	(0.062)		(0.019)	(0.017)	(0.056)	
Cambodia	0.206 ***	0.211 ***	0.209 ***	18	0.217 ***	0.218 ***	0.218 ***	131
	(0.023)	(0.021)	(0.026)		(0.016)	(0.012)	(0.020)	
Secondary attendance at baseline	-0.05 *	-0.049*	-0.042	8	-0.055 ***	-0.057 ***	-0.048	70
	(0.024)	(0.024)	(0.018)		(0.017)	(0.017)	(0.028)	

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 12. Egger's linear regression tests for publication bias and selective reporting

	Primary Enrollment (1)	Secondary Enrollment (2)	Primary Attendance (3)	Secondary Attendance (4)	Primary Dropout (5)	Secondary Dropout (6)
Constant (Asymmetry)	1.67	0.24	0.96	4.45	-3.27	-2.00
Standard Error	(0.29)	(0.23)	(0.39)	(0.45)	(0.85)	(1.26)
p-value	0.00	0.29	0.01	0.00	0.00	0.12
Number of Estimates	187	258	86	131	72	31

Notes: Each column reports estimates from a different regression in which the effect size divided by its standard error is regressed against the standard error and a constant term.

Figure 1. Distribution of effects reported in each reference in sample, by outcome and level

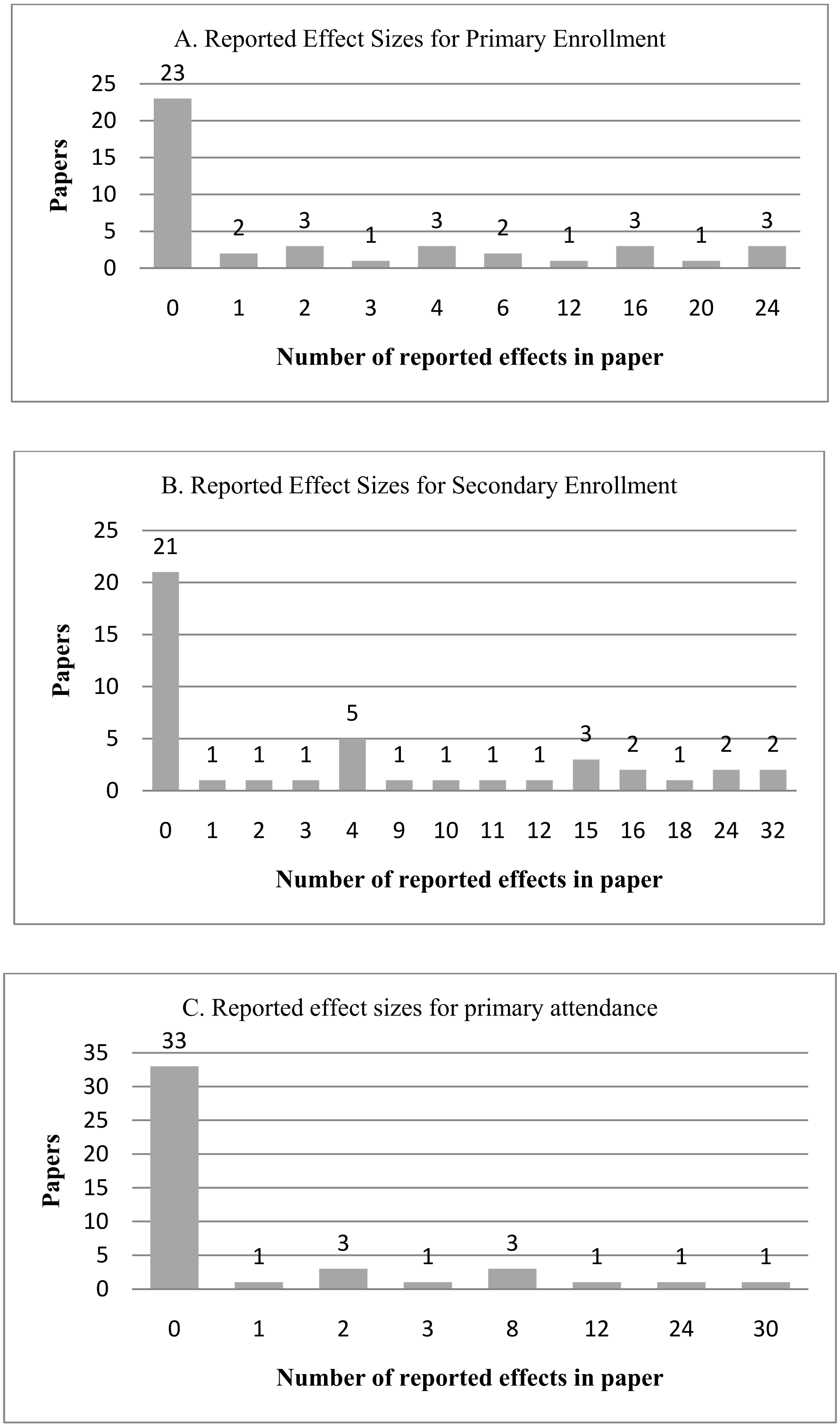
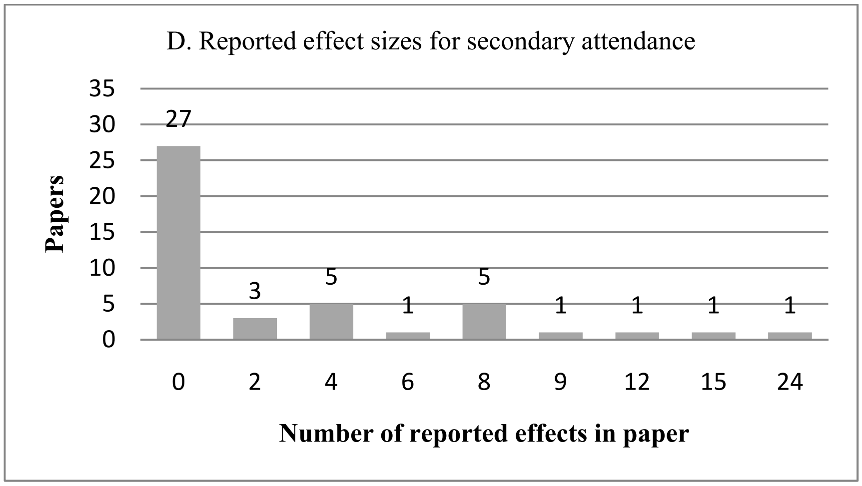
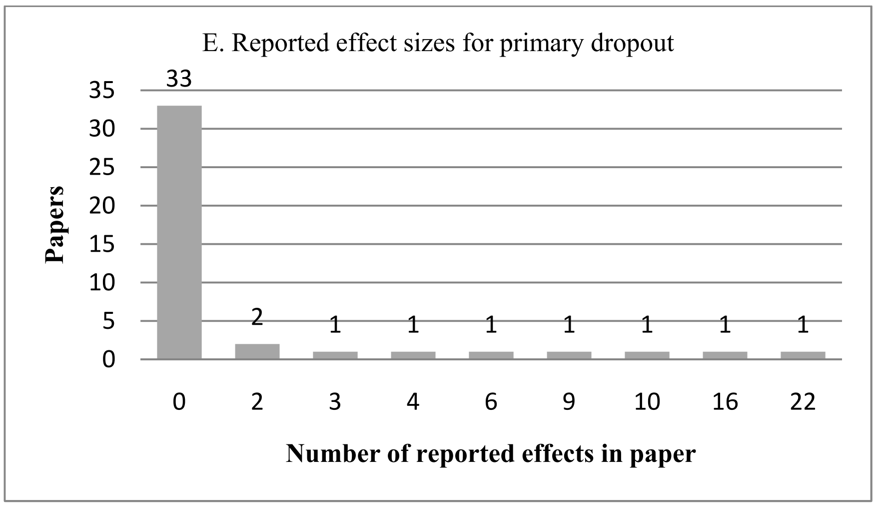


Figure 1. (cont.)





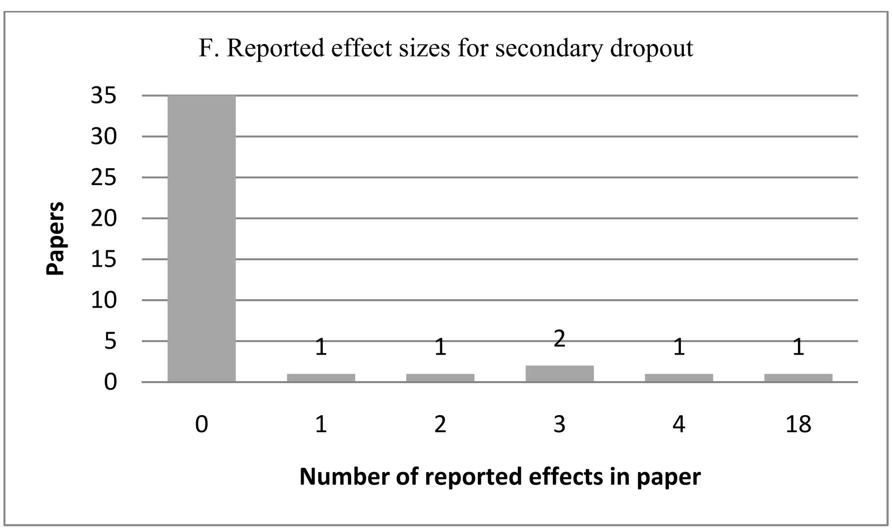
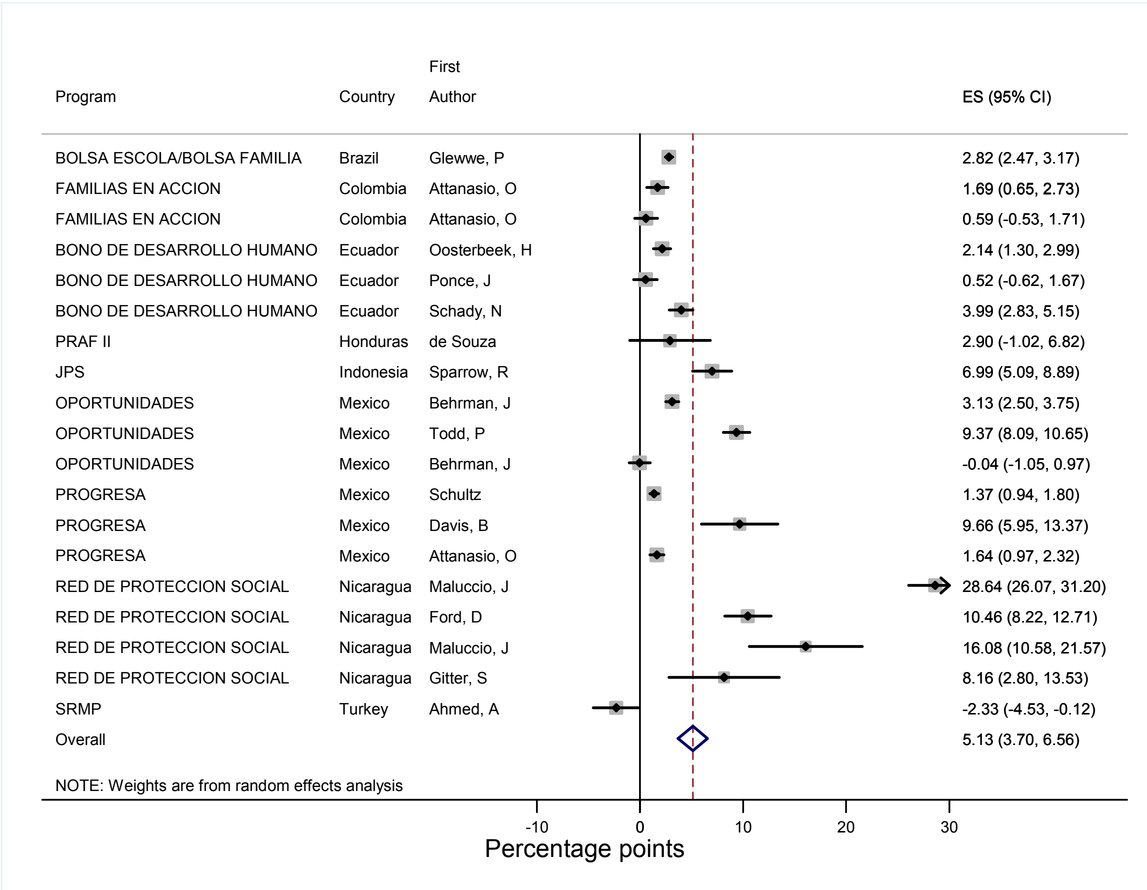
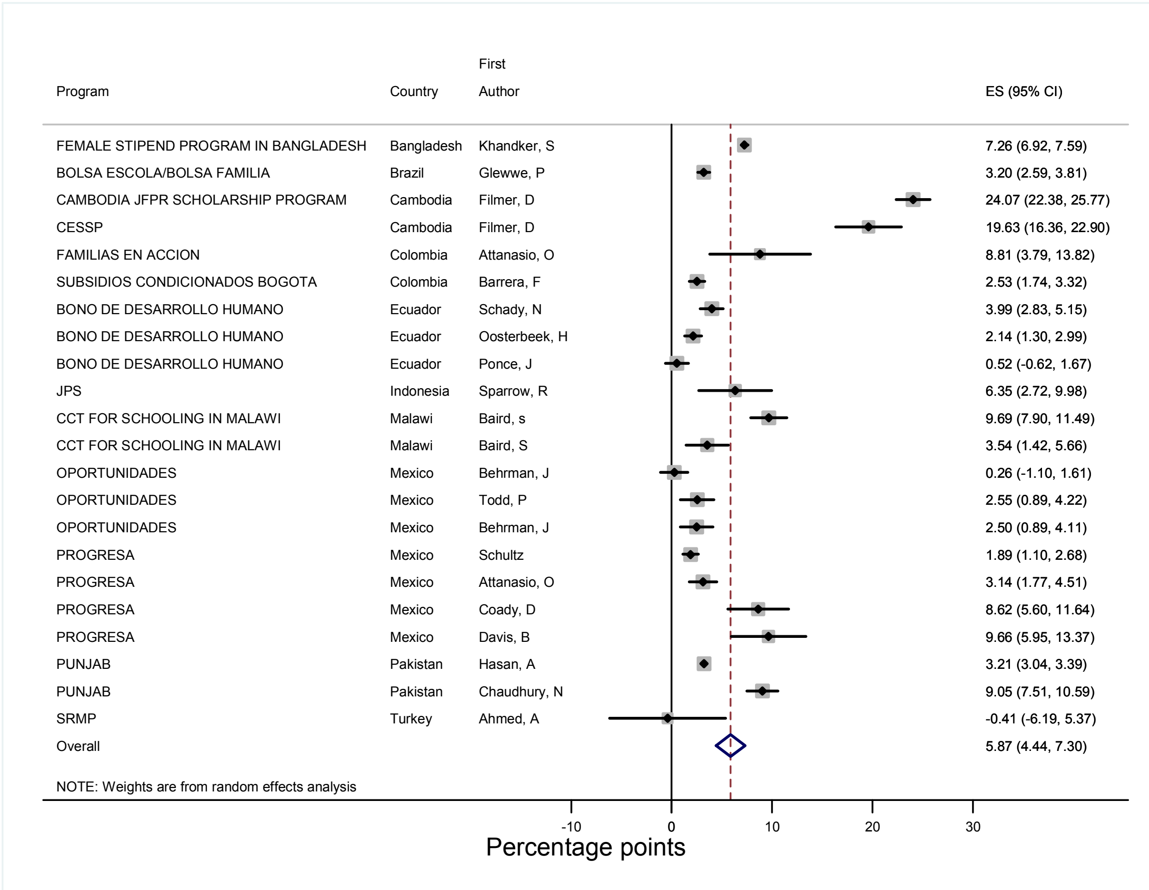


Figure 2. Forest plot of effect sizes on primary enrollment (one effect per paper)



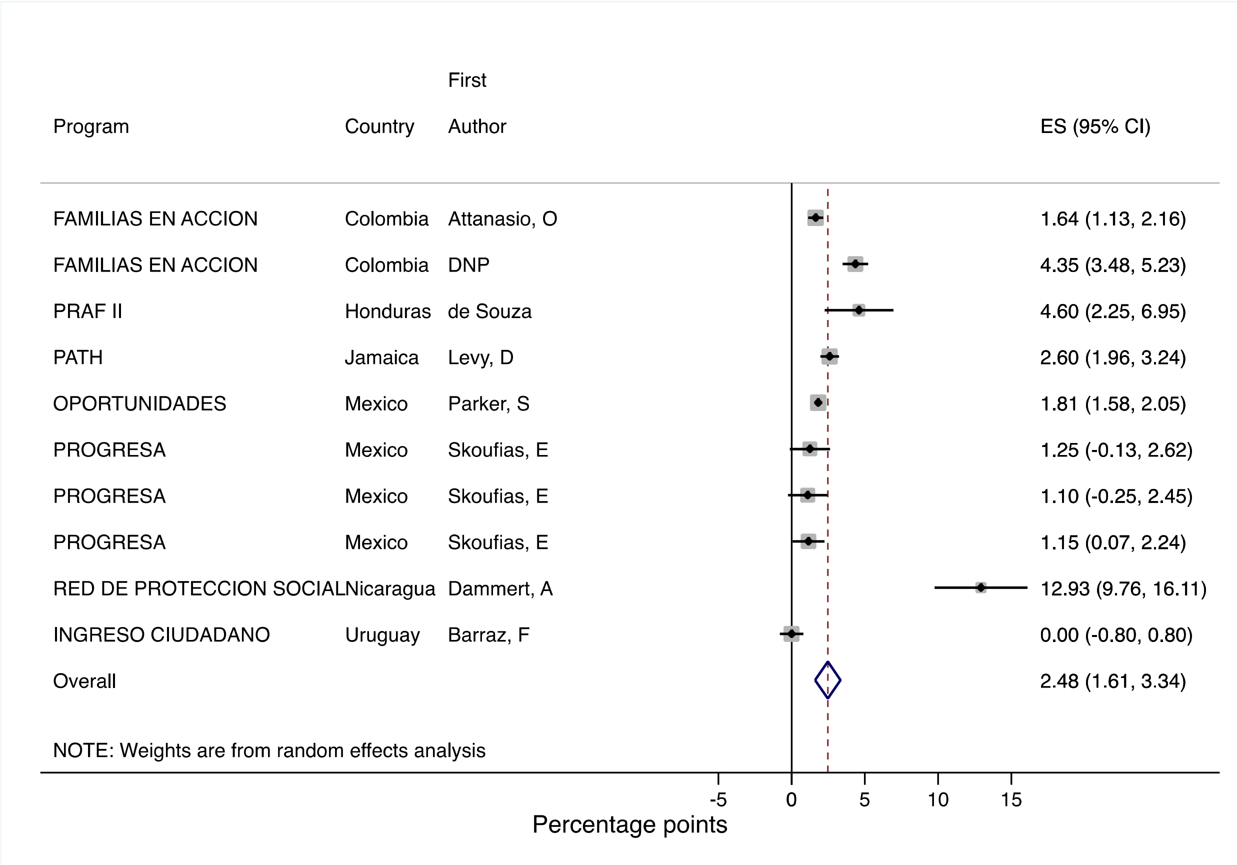
Notes: For each reference we compute one average effect size using a fixed effects model to combine all estimates in the reference. Mean baseline primary enrollment is 84%.

Figure 3. Forest plot of effect sizes on secondary enrollment (one effect per paper)



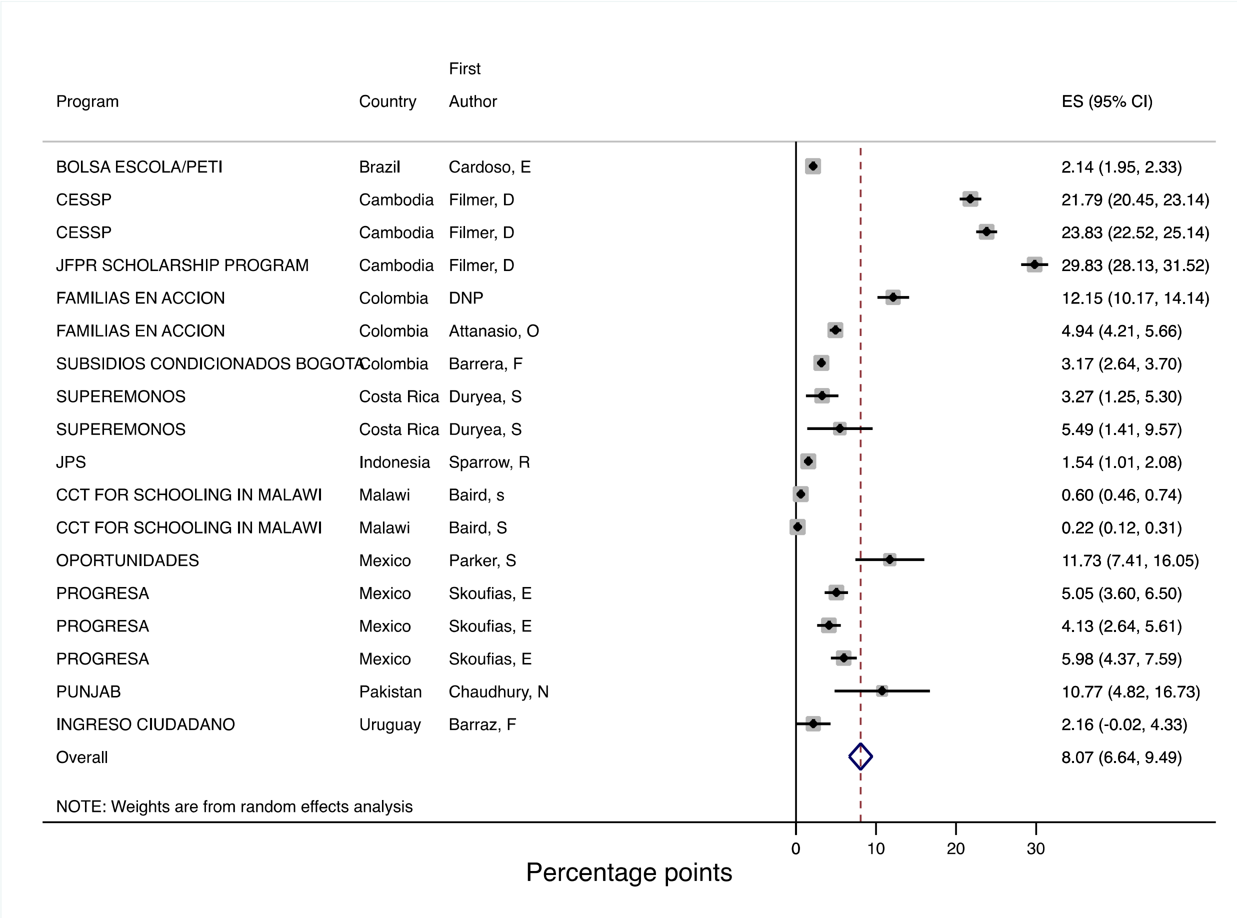
Notes: For each reference we compute one average effect size using a fixed effects model to combine all estimates in the reference. Overall random-effects average effect size is estimated using method of moments not accounting for hierarchical nesting of effects within programs. Mean baseline secondary enrollment is 59%.

Figure 4. Forest plot of effect sizes on primary attendance (one effect per paper)



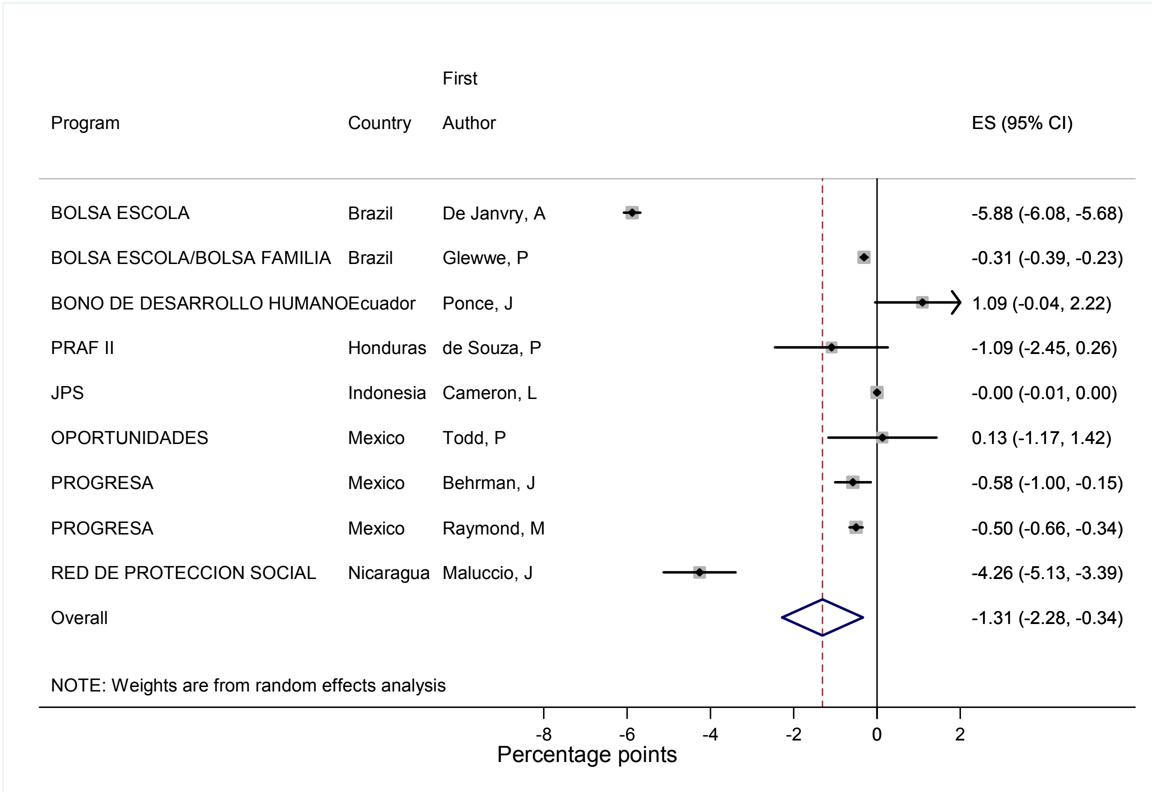
Notes: For each reference we compute one average effect size using a fixed effects model to combine all estimates in the reference. Skoufias and Parker (2001) (First Author Skoufias for Progresa) reports effects for three post-treatment periods and we compute one effect size per measurement period. Overall random-effects average effect size is estimated using method of moments not accounting for hierarchical nesting of effects within programs. Mean baseline primary attendance in sample is 80%.

Figure 5. Forest plot of effect sizes on secondary attendance (one effect per paper)



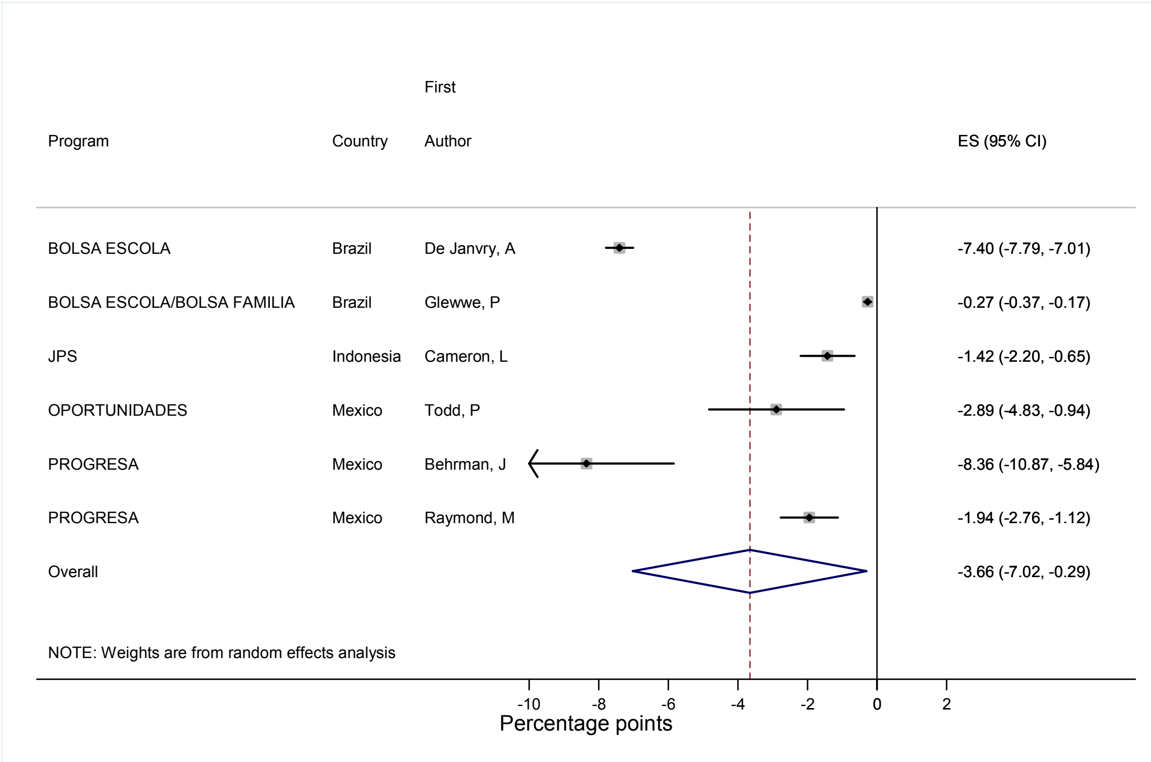
Notes: For each reference we compute one average effect size using a fixed effects model to combine all estimates in the reference. Duryea and Morrison (2004) reports effects for two post-treatment periods and we compute one effect size per measurement period. Skoufias and Parker (2001) (First Author Skoufias for Progresa) reports effects for three post-treatment periods and we compute one effect size per measurement period. Overall random-effects average effect size is estimated using method of moments not accounting for hierarchical nesting of effects within programs. Mean baseline secondary attendance in sample is 68%.

Figure 6. Forest plot of effect sizes on primary dropout (one effect per paper)



Notes: For each paper we compute one average effect size using a fixed effects model to combine all estimates in the paper. Overall random-effects average effect size is estimated using method of moments not accounting for hierarchical nesting of effects within programs.

Figure 7. Forest plot of effect sizes on secondary dropout (one effect per paper)



Notes: For each paper we compute one average effect size using a fixed effects model to combine all estimates in the paper. Overall random-effects average effect size is estimated using method of moments not accounting for hierarchical nesting of effects within programs.

Figure 8. Funnel plot of standard error of effect on reported primary enrollment effect size (all effects from all references)

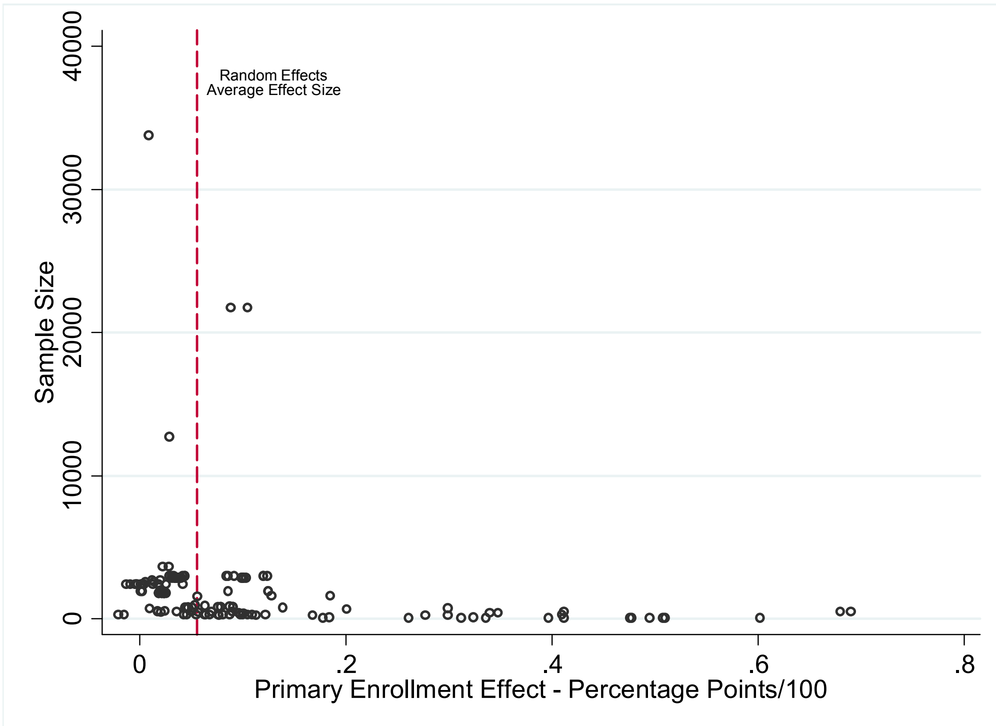


Figure 9. Funnel plot of standard error of effect on reported secondary enrollment effect size (all effects from all references)

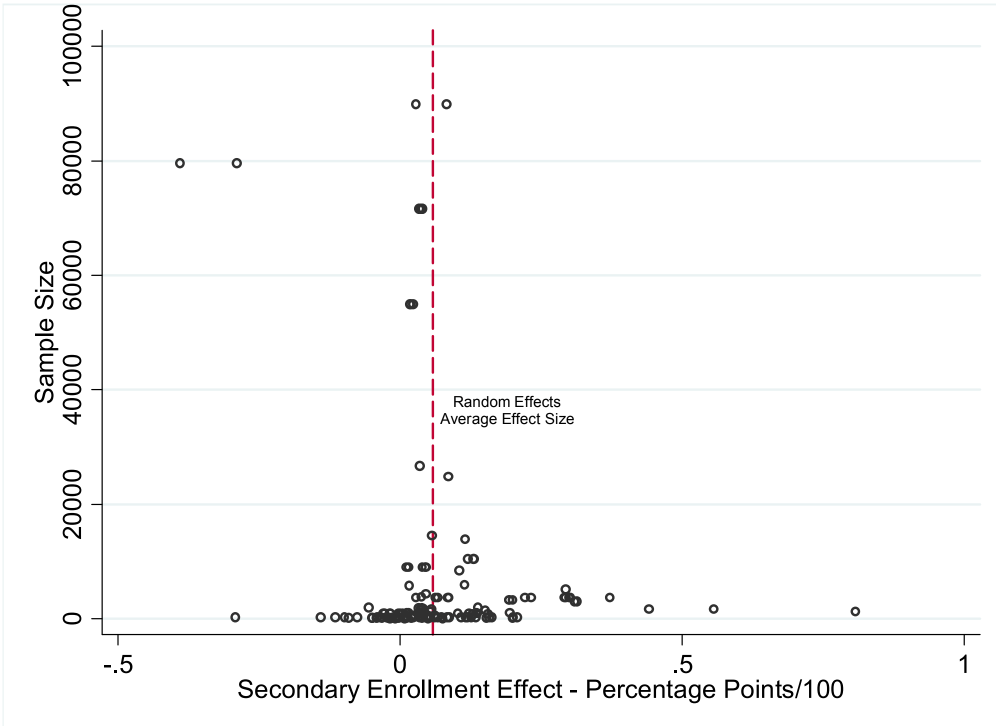


Figure 10. Funnel plot of standard error of effect on reported primary attendance effect size (all effects from all references)

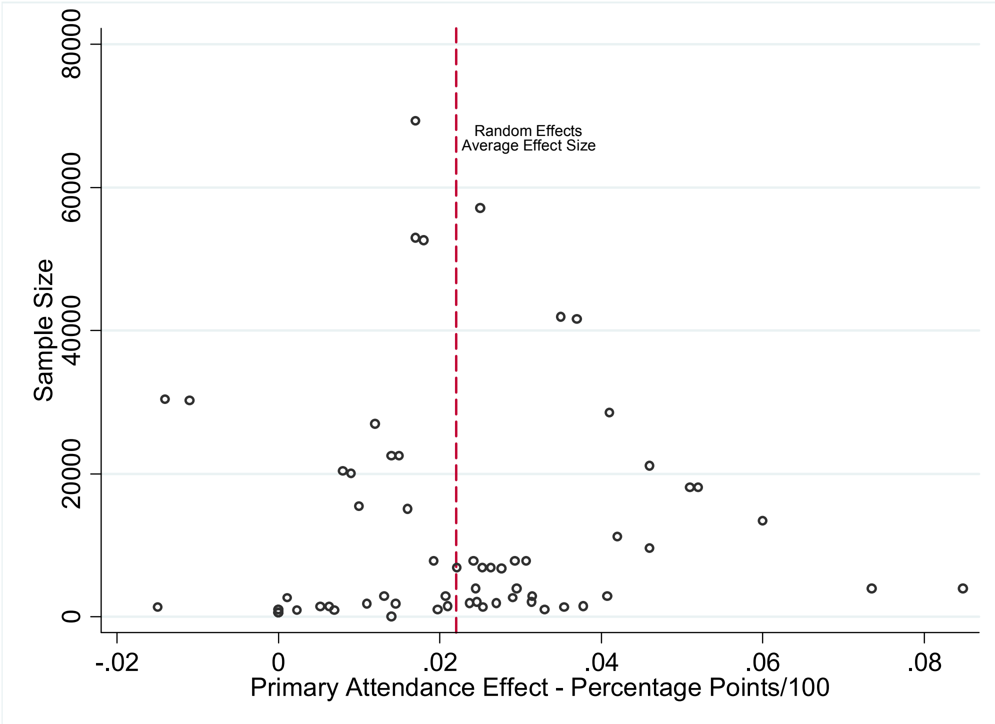


Figure 11. Funnel plot of standard error of effect on reported secondary attendance effect size (all effects from all references)

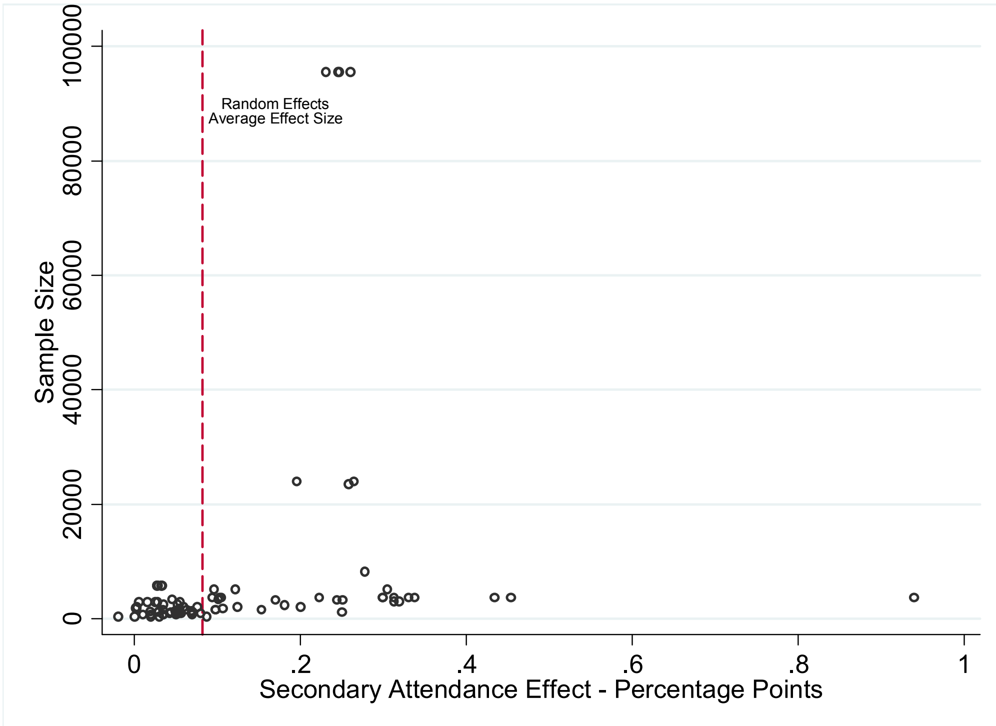


Figure 12. Funnel plot of standard error of effect on reported primary dropout effect size (all effects from all references)

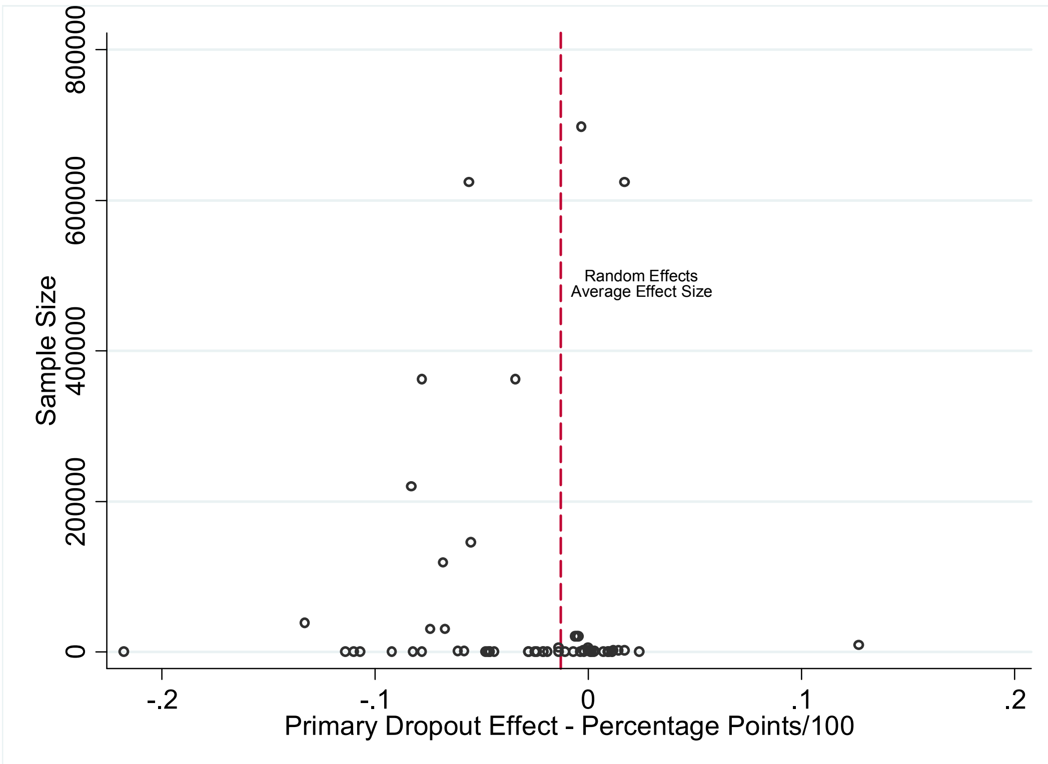
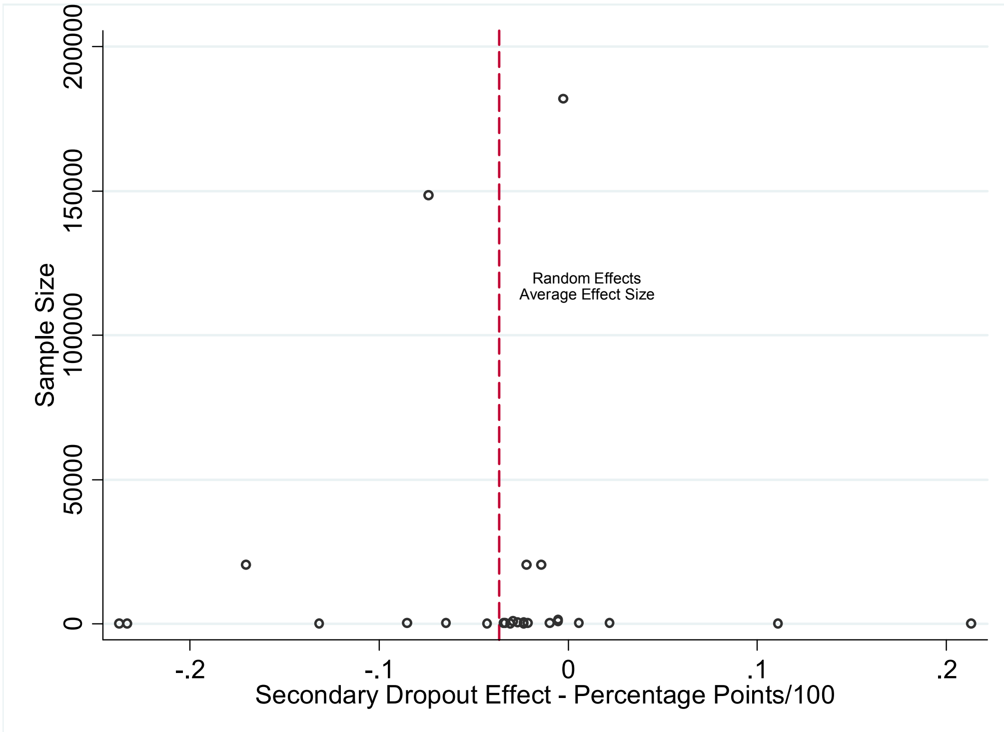


Figure 13. Funnel plot of standard error of effect on reported secondary dropout effect size (all effects from all references)



Appendix Table b. Characteristics of references in final analysis sample

Country	Program name	First author	Year	Publication type	Source of data	Sample size[a]	Reports effects on		
							Enrollment	Attendance	Dropout
Bangladesh	Female Stipend Program	Khandker, S.	2003	Working paper	Household survey and school data	89,861	Yes	No	No
Brazil	Bolsa Escola	De Janvry, A.	2006	Working paper	Administrative data	624,077	No	No	Yes
Brazil	Bolsa Escola/Bolsa Familia	Glewwe, P.	2008	Conference paper	Census data	699,255	Yes	No	Yes
Brazil	PETI/Bolsa Escola/Renda Minima	Cardoso, E.	2004	Working paper	Census data	428,740	No	Yes	No
Cambodia	CESSP	Filmer, D.	2009	Working paper	Program survey	3,225	Yes	Yes	No
Cambodia	CESSP	Filmer, D.	2009	Working paper	Program survey	95,493	No	Yes	No
Cambodia	JFPR Scholarship Program	Filmer, D.	2008	Journal article	Program survey	5,138	Yes	Yes	No
Colombia	Familias en Acción	Attanasio, O.	2010	Journal article	Program survey	3,648	Yes	No	No
Colombia	Familias en Acción	Attanasio, O.	2004	Technical Report	Program survey	2,691	No	Yes	No
Colombia	Familias en Acción	Attanasio, O.	2004	Government report	Program survey	3,935	Yes	No	No
Colombia	Familias en Acción	National Planning Department	2006	Government report	Program survey	3,935	No	Yes	No
Colombia	Subsidios Condicionados a la Asistencia Escolar en Bogotá	Barrera, F.	2009	Working paper	Program survey	8,980	Yes	Yes	No
Costa Rica	Superémonos	Duryea, S.	2004	Working paper	Program survey	1,109	No	Yes	No
Ecuador	Bono de Desarrollo Humano	Oosterbeek, H.	2008	Working paper	Program survey	3,004	Yes	No	No
Ecuador	Bono de Desarrollo Humano	Ponce, J.	2006	Working paper	Program survey	2,384	Yes	No	Yes
Ecuador	Bono de Desarrollo Humano	Schady, N.	2008	Journal article	Program survey	2,875	Yes	No	No
Honduras	PRAF II	De Souza	2005	Doctoral dissertation	Program survey	12,741	Yes	Yes	Yes
Indonesia	JPS	Cameron, L.	2009	Journal article	National household survey	5,358	No	No	Yes
Indonesia	JPS	Sparrow, R.	2007	Journal article	National household survey	120,022	Yes	Yes	No
Jamaica	PATH	Levy, D.	2007	Technical report	Program survey	7,751	No	Yes	No

Country	Program name	First author	Year	Publication type	Source of data	Sample size[a]	Reports effects on		
							Enrollment	Attendance	Dropout
Malawi	CCT for Schooling	Baird, S.	2009	Working paper	Program Survey	5,914	Yes	Yes	No
Malawi	CCT for Schooling	Baird, S.	2010	Working paper	Program survey	1,832	Yes	Yes	No
Mexico	Oportunidades	Behrman, J.	2004	Technical report	Program survey	1,796	Yes	No	No
Mexico	Oportunidades	Behrman, J.	2005	Working paper	Program survey	1,013	Yes	No	No
Mexico	Oportunidades	Parker, S.	2006	Working paper	Program survey	69,261	No	Yes	No
Mexico	Oportunidades	Todd, P.	2005	Technical report	Program survey	1,994	Yes	No	Yes
Mexico	Progresa	Attanasio, O.	2005	Working paper	Program survey	N/A	Yes	No	No
Mexico	Progresa	Behrman, J.	2005	Journal article	Program survey	75,000	No	No	Yes
Mexico	Progresa	Coady, D.	2002	Working paper	Program survey	N/A	Yes	No No	
Mexico	Progresa	Davis, B.	2002	Working paper	Program survey	21,709	Yes	No	No
Mexico	Progresa	Raymond, M.	2003	Working paper	Program survey	20,541	No	No	Yes
Mexico	Progresa	Schultz, P.	2004	Journal article	Program survey	33,795	Yes	No	No
Mexico	Progresa	Skoufias, E.	2001	Working paper	Program survey	27,845	No	Yes	No
Nicaragua	Red de Protección Social	Dammert, A.	2009	Journal article	Program survey	1,745	No	Yes	No
Nicaragua	Red de Protección Social	Ford, D.	2007	Doctoral dissertation	Program survey	1,946	Yes	No	No
Nicaragua	Red de Protección Social	Gitter, S.	2009	Journal article	Program survey	1,561	Yes	No	No
Nicaragua	Red de Protección Social	Maluccio, J.	2009	Working paper	Program survey	1,227	Yes	No	Yes
Nicaragua	Red de Protección Social	Maluccio, J.	2005	Technical report	Program survey	1,594	Yes	Yes	No
Pakistan	PUNJAB	Chaudury, N.	2006	Working paper	Census data	5,164	Yes	Yes	No
Pakistan	PUNJAB	Hasan, A.	2010	Working paper	Census data	71,620	Yes	No	No
Turkey	SRMP	Ahmed, A.	2006	Working paper	Program survey	2,905	Yes	No	No
Uruguay	Ingreso Ciudadano	Barraz, F.	2009	Journal article	National household survey	1,011	No	Yes	No

[a]Maximum sample size to compute effect sizes or sample size reported in the text (if no sample size reported in effect sizes results).

Appendix Table B. Programs characteristics

Country	Program name	Year program started	Conditionality	Minimum attendance rate (%)	Conditions verification	Transfer amount[a]		Payment frequency	Subsidy received by	Subsidy varies by	Supply compo-nent	Random Assign-ment
						Primary	Secondary					
Bangladesh	Female Stipend Program	1994	Attendance, academic proficiency and remain unmarried	75	Yes	Not applicable	1.42	Monthly	Student	Grade	Yes	No
Brazil	Bolsa Escola	2001	Attendance	85	N/A	0.77	0.77	Monthly		None	No	No
Brazil	Bolsa Escola/Bolsa Familia	1995	Enrollment and attendance	85	N/A	1.05	1.05	Monthly	N/A	None	N/A	No
Cambodia	CESSP	2004	Enrollment, attendance and grade promotion	N/A	Yes	Not applicable	10.01	3 times per year	Parents	Dropout risk	No	No
Cambodia	JFPR Scolarship Program	2005	Enrollment, attendance and grade promotion	N/A	N/A	Not applicable	8.95	3 times per year	Parents	None	No	No
Colombia	Familias en Accion	2001	Enrollment and attendance	80	N/A	1.10	2.21	Bimonthly	Mother	Age	No	No
Colombia	Subsidios Condicionados a Asistencia Escolar en Bogotá	2005	Attendance, grade promotion, graduation and enrollment in higher education institution	80	Yes	Not applicable	2.46	Bimonthly plus lump-sum at the end of school year or upon graduation[b]	Parents	None	No	Yes
Costa Rica	Superémonos	2001	Enrollment and attendance	N/A	Yes	4.47	4.47	Monthly	N/A	None	No	No
Ecuador	Bono de Desarrollo Humano	2004	Enrollment and attendance	90	No	3.08	3.08	Monthly		None	No	Yes
Honduras	PRAF II	1998	Enrollment and attendance	85	No	2.06	Not applicable	Monthly	Parents	None	Yes	Yes
Indonesia	JPS	1998	Enrollment and passing grades	N/A	N/A	0.39	0.98	3 times per year	Student	Grade	No	No

Country	Program name	Year program started	Conditionality	Minimum attendance rate (%)	Conditions verification	Transfer amount[a]		Payment frequency	Subsidy received by	Subsidy varies by	Supply compo-nent	Random Assign-ment
						Primary	Secondary					
Jamaica	PATH	2001	Attendance	85	Yes	1.11	1.11	N/A	Parents	None	No	No
Malawi	CCT for schooling	2007	Enrollment and attendance	75	Yes	Not applicable	17.3	Monthly	Parent or guardian and student[c]	Randomly	No	Yes
Mexico	Oportunidades	2002	Attendance	85	N/A	1.21	3.92	Bimonthly	Mother	Gender and grade	No	No
Mexico	Progresa	1997	Attendance	85	N/A	1.05	2.49	Monthly	Mother	Gender and grade	Yes	Yes
Nicaragua	Red de Protección Social	2000	Enrollment and attendance	85	Yes	5.23	Not applicable	Bimonthly	N/A	None	Yes	Yes
Punjab	Pakistan	2004	Attendance	80	N/A	Not applicable	2.28	Monthly	Student	None	No	No
Turkey	SRMP	2004	Attendance and not repeating a grade more than once	80	Yes	1.56	2.62	Bimonthly	N/A	Gender and grade	No	No
Uruguay	Ingreso Ciudadano	2005	Enrollment and attendance	N/A	Yes	6.94	6.94	Monthly	N/A	None	No	No

[a] As percentage of PPP-adjusted GDP/capita.

[b] This program was part of an experiment that included 3 different treatments that varied in the timing of subsidy delivery: (1) a subsidy with bimonthly payments conditioned on attendance, (2) subsidy with bimonthly payments conditioned on attendance and a lump sum at the end of the school year conditioned on school enrollment the following year, and (3) a subsidy with bimonthly payments conditioned on attendance and a lump sum upon graduation and enrollment in a higher education institution.

[c] The program included two transfers: one to the household and another one to the student (girl).