The McGovern-Dole International Food for Education and Child Nutrition Program

School Feeding and Educational Outcomes in Developing Countries: A Systematic Review and Meta-Analysis

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1.0. Introduction

1.1. Background

The McGovern – Dole International Food for Education and Child Nutrition Program (MGD), one of the Foreign Agricultural Service's leading food assistance programs, helps support education, child development and food security in low-income, food-deficit countries throughout the world. The program is named in honor of former Ambassador and U.S. Senator George McGovern and former U.S. Senator Robert Dole for their efforts to encourage a global commitment to school feeding and child nutrition.

The key objective of the MGD program is to improve literacy of primary school-age children, especially for girls. By providing school meals, teacher training and related support, MGD projects help enhance school enrollment and academic performance. The program also funds supplementary activities that promote children's health and nutrition in an effort to further support children's school enrollment, attendance, and capacity to benefit from the educational instruction received.

The MGD program was first authorized in the Farm Security and Rural Investment Act of 2002 (P.L. 107-171). The 2014 Farm Bill reauthorized the program through 2018. USDA is currently funding 45 McGovern-Dole projects in 27 low-income, food-deficit countries throughout the world (McGovern-Dole, 2009) McGovern – Dole projects are conducted by non-profit charitable organizations, cooperatives, the United Nations World Food Program and other international organizations.

The present study is part of a broader evaluation and research effort to: (1) support the MGD program's ability to use rigorous evidence, evaluation and research in strategic decision-making to improve program outcomes; and (2) help the program identify key gaps in the knowledge base on what interventions are successful in improving literacy and reducing hunger. This study builds on three research efforts: a thorough intervention mapping analysis of the MGD program over a five-year period (2009-2013); a comprehensive annotated bibliography of the programmatic and policy topics of relevance to MGD program interventions; and a proposal for selecting research topics for three systematic reviews of the international literature on the impact of education program interventions in developing countries with particular relevance to the MGD program.

The first topic selected for a systematic review was school feeding and educational outcomes. The rationale for selecting this topic is threefold. First, meals served to children in school as well as take-home rations conditional on a child's school attendance are a central component of the MGD programs worldwide. In the MGD literacy results framework, increased access to food through school feeding results in reduced short-term hunger and increased economic incentives.

These in turn lead to improved literacy of school-age children through improved attentiveness as well as improved student attendance.

Second, the literature on school feeding in developing countries offers a relatively wide variety of experimental and quasi-experimental evidence providing causal impact outcomes, including impact evaluation studies on educational outcomes from which it is possible to draw conclusions about the likely impact on many of these educational outcomes. Most studies have focused on enrollment rates and school attendance, but several studies have also investigated the evidence on learning achievement and cognitive development.

Third, from this growing body of literature, it is possible to sketch a reasonable consensus on the effects of these interventions on the outcomes identified, draw lessons learned and corresponding policy implications, and identify areas for further investigation to help close the learning gap.

1.2. Organization of the Report

This report is divided into three major sections. Following this introduction, Section 2 describes the objective of the study and its methodology. Section 3 presents an in-depth discussion of the empirical evidence derived from the studies reviewed, including a detailed presentation of the findings and their limitations, and implications for future research. Detailed technical data used to derive findings are provided as annexes to the report.

2. 0. Objective and Methodology

2.1. Objective

The purpose of the present systematic review and meta-analysis is to investigate the likely causal impact of school feeding interventions on educational outcomes for pre-primary and primary-school-age children and its programmatic and policy implications, as reflected in the experimental and quasi-experimental literature on food-for-education programs in developing countries.

2.2. Methodology

2.2.1. School Feeding Defined

School feeding is defined as the provision of food to schoolchildren. Two categories of school feeding interventions were considered in this systematic review and meta-analysis: in-school meals and take-home rations. In-school meal programs make food available to children while they are at school. Food provided consists of either or a combination of the following forms: breakfast, snacks, and lunch. The three meals vary in both quantity of food provided and nutritional content. Since it is not always feasible or desirable to target individual students, school meals are provided to all students. Take-home rations are typically given to selected households conditional on their children's enrollment in school and a minimum level of attendance. Targeting criteria also include income and gender. Take-home rations may also be targeted to particularly vulnerable students, including the very poor, girls and children affected by HIV.

These two broad school-feeding modalities reflect MGD program interventions and the 2013 World Food Program school-feeding survey (World Food Program, 2013). An MGD intervention mapping analysis for the past five years (2009-2013) shows that all school feeding programs provide in-school meals to students in the targeted schools, except in 2012 when about 5 percent of programs provided only nutritional supplements as part of an effort to assess their effectiveness. Nearly 30 percent of programs provide both school meals and take-home rations. According to the World Food Program (2013) survey, there are at least 368 million pre-primary-, primary- and secondary-school children receiving food through schools around the world, with at least 43 countries with programs of more than one million children. The region with the largest number of beneficiaries is South Asia, followed by Latin America and the Caribbean.

2.2.2. Causal Pathways and Outcomes Considered

Within the framework of these two components of school feeding interventions, this systematic review includes only studies that investigate interventions linking school feeding to educational outcomes. Studies that focus exclusively on health and nutrition outcomes – as measured, for instance, by food energy consumption, anthropometry, and micronutrient status – are not included. When both health/nutrition and educational outcomes are investigated, the systematic review and meta-analysis is limited to educational outcomes.[1] Studies are therefore included only when they report results for at least one continuously measured educational outcome or a composite assessment.[2]

There are three main categories of educational outcomes that are commonly evaluated in educational interventions (see for instance, Adelman et al., 2008; Alderman et al., 2012; Lawson 2012; Petrossino, 2012; McEwan, 2014; Guerrero et al., 2012; Kremer et al., 2009). Annex 1 provides further illustration of the outcomes investigated in each study included in the meta-analysis). These three categories comprise:

1. School participation (includes enrollment, attendance/absenteeism, dropout, and grade/class repetition)
2. Learning achievement (includes standardized math and language test scores)

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[1] Health and nutrition interventions and their outcomes are analyzed in two separate meta-analyses funded by USDA under contract # D14PD01268.

[2] Dichotomous variables are variables with two categories (e.g., male/female, black/white, rich/poor). Although dichotomous variables may apply to educational outcomes (for instance, pass/fail, dropped out/didn’t drop out), they are reported in all the studies reviewed as continuous outcomes, typically in the form of rates of change between the pre-test data and the post-test data or the mean difference between the intervention group and the control group. Studies were therefore excluded from the analysis if they describe the results of the outcome without providing the quantitative data needed to conduct the meta-analysis.

3. Cognitive development (includes measures of verbal fluency, memory, and reasoning.)

Literacy is not commonly measured as an outcome, as it is a complex and dynamic concept that is subject to various interpretations and defined in multiple ways. Moreover, literacy has expanded from a simple process of acquiring basic cognitive skills, to using these skills in ways that contribute to socio-economic development, to developing the capacity for social awareness and critical reflection as a basis for personal and social change. Reflecting this complexity, UNESCO defines literacy as "a set of tangible skills — particularly the cognitive skills of reading and writing," and "the ability to use reading, writing and numeracy skills for effective functioning and development of the individual and the community"(UNESCO 2004). It should, however, be noted that the multi-dimensional nature of literacy in this definition is captured in at least two of the three categories of outcome measures (learning achievement, and cognitive development) used in the literature reviewed for this study.

The outcomes listed above reflect the MGD theory of change, which draws on standard theory and analysis in the technical sectors underlying the overall strategic objectives and intermediate results of the program. (Annex 2.) The MGD results framework has two strategic objectives (SOs): Improved Literacy of School Children; and Increased Use of Health and Dietary Practices. The two SOs are interrelated because Increased Use of Health and Dietary Practices leads to Improved Literacy of School Children via Improved School Attendance, one of SO1 Intermediate Results. Improved literacy of school-age children is achieved via three necessary and sufficient intermediate results (IRs): Improved Quality of Literacy Instruction; Improved Attentiveness; and Improved Student Attendance.

Increased Use of Health and Dietary Practices (SO2) improves student attendance (and therefore literacy) via reduced health-related absences. School feeding is a fundamental component of the MGD program for two reasons. First, increased access to food through school feeding improves student attendance via increased economic incentives.[3] Second, school feeding also improves student attentiveness (and therefore literacy) via reduced short-term hunger.

The MGD theory of change is in line with the research literature that outlines the major pathways through which school feeding programs may affect participants' education outcomes. Households are thought to base their children's schooling decisions on economic considerations by comparing the potential future benefits of schooling (e.g., higher earnings in adulthood and better marriage outcomes) with its costs. (See, for instance, Hanusek 1986; Schultz 1988; Alderman et al. 2008.)

Schooling costs include not only school fees, books and other supplies, but also the benefits forgone by the household when the children are not working on the family farm or business, by not earning additional income from work outside the household, or by not caring for a family

[3] Economic incentives in the MGD results framework are defined as "any number of activities that ease the economic burden of attending school for children… increased access to food through a school feeding program provides a strong incentive for children to attend school, especially girls. Other examples of incentives include subsidies for books or school uniforms, [and] transportation to school."

member. Since school feeding is a conditional non-cash transfer, the savings in food costs for the household help offset the cost of sending a child to school. Thus, school feeding is expected to increase school enrollment to the extent that the conditional in-kind transfer helps tilt the balance towards a higher benefit/cost ratio (Adelman et al., 2008; Alderman et al., 2012; Alderman & Bundy, 2012; Ravaillon 2000).

School feeding is expected to have an impact not only on school enrollment but also on attendance because children receive the meal only when they attend. Two other channels may affect school attendance (Alderman & Bundy, 2012; WFP, 2013; Adelman et al., 2008). First, since in-school meals relieve short-term hunger, they offer an incentive to attend school for children who would not otherwise attend because they are feeling hungry. Second, the longer-term nutrition effects of in-school meals are likely to improve attendance by reducing morbidity or incidence of disease and therefore the number of school days missed from illness. School feeding affects grade repetition and dropout rates through the attendance and nutrition channels.

While higher attendance offers more opportunity for learning, the nutrition improvements through school feeding may improve the physiological capacity for learning and higher school achievement. Hunger affects children's capacity to concentrate and learn but malnutrition affects their cognitive development through the cumulative effects of short-term metabolic and neurohormonal changes (Politt 1995; Jacoby et al. 1996; Kristjanson et al. 2007; Wesnes et al., 2012; Cooper et al., 2011). It is important to note, however, that increased enrollment and attendance can lead to overcrowded schools and lower teacher to student ratios, with potential adverse effects on learning and cognition. This pathway may help explain the insignificant or even negative effect of school feeding on learning and cognition in certain situations (Vermeersch and Kremer 2004; Powell et al. 1998).

Take-home rations affect educational outcomes through many of the same channels. However, since the entire household is indirectly targeted by the food transfer, and not only the school-going child, the effect is determined by how the food is redistributed among household members.[4] Another difference is that the in-school meal is consumed during school hours, with beneficial effect on concentration and learning.

2.2.3. Geographic Coverage

Only studies pertaining to developing countries are included.[5]

[4] Take-home rations are given to selected households conditional on their children's enrollment in school and a minimum level of attendance, but they are also targeted to vulnerable groups, including the very poor, girls and children affected by HIV. There is evidence to suggest (e.g., Islam & Hoddinott, 2009; Jacoby 2002) that interventions that target specific individuals in a household may be neutralized by reallocations of the resource away from the child. In which case, the entire household is de facto targeted by the food transfer and not only the targeted child.

[5] Developing countries are characterized as such based on the classification used in the International Monetary Fund World Economic Outlook for 2014.

2.2.4. Timeframe

The literature search was mainly, but not exclusively, based on studies published in 2000-2015. Studies conducted before 2000, but published in 2000-2015 were included. Earlier studies considered as pioneers and/or especially relevant were also considered.

2.2.5. Target Groups

Pre-primary and primary-school-age children are the focus of the investigation. Depending on data availability in the studies retained for review (based on the inclusion criteria spelled out in Section 2), target groups are differentiated by gender and, subject to data availability, by age group and grade.

2.2.6. Study Language

Searches were conducted in English, but studies were not excluded on the basis of language.

2.2.7. Evidence Considered and Estimation Methods

2.2.7.1. Screening Criteria

Only the empirical literature that contains the most rigorous evidence using the strongest methodology for identifying causal impacts was considered. Impact evaluations quantify the effects of programs on individuals, households, and communities. They show whether the changes observed are indeed due to the program intervention and not to other factors (see, for instance, Khandker et al. 2010). Impact evaluations are "analyses that measure the net change in outcomes for a particular group of people that can be attributed to a specific program using the best methodology available, feasible and appropriate to the evaluation question that is being investigated and to the specific context." (International Initiative for Impact Evaluation 2008). They "compare the outcomes of a program against a counterfactual that shows what would have happened to beneficiaries without the program. Unlike other forms of evaluation (such as 'performance evaluations'), they permit the attribution of observed changes in outcomes to the program being evaluated." (World Bank n.d.)

Attribution is different from association between the intervention and outcomes that may have been affected by other contextual factors. Evaluating the impact of an intervention hinges on a fundamental question: What would the situation have been if the intervention had not taken place. While descriptive monitoring leaves ample room for differing interpretations of how much the identified change can be attributed to the intervention, impact evaluations rely on more sophisticated methods to disentangle the net gains from that intervention.

Impact evaluations range from randomized designs to quasi-experimental models. There is consensus that the best evaluation method is the experimental design, in which beneficiaries (called intervention or treatment group) are randomly selected from a set of communities with similar characteristics. Subjects not randomly selected for the intervention form a counterfactual (called comparison or control group). Randomized controlled trials (RCTs), the gold standard by which scientific evidence is evaluated, can be designed in one of three ways:

- Double-blind trials: an experimental procedure in which neither the subjects nor the experimenters know which subjects are in the test and control groups during the actual course of the experiments.
- Single-blind trials: an experimental procedure in which the experimenters but not the subjects know the makeup of the test and control groups during the course of the experiments The control may be a standard practice, a placebo, or no intervention at all.
- Unblinded trial: an experimental procedure in which both the subject and the experimenter know who is in the test and control groups during the actual trial.

Ideally, all variables in an experiment will be controlled. In such a controlled experiment, if all the controls work as expected, it is possible to conclude that the results of the experiment are due to the effect of the variable being tested. More generally, experimental design enables the investigator to make claims of the following nature: The two situations were identical until the intervention was introduced. Since the intervention is the only difference between the two situations, the new outcome was caused by that intervention.

Quasi-experimental designs are used when all the necessary requirements to control influences of extraneous variables cannot be met, most particularly when randomization is not possible for political, ethical, or logistical reasons. When the subjects cannot be randomly assigned to either the experimental or the control group, or when the researcher cannot control which group will get the treatment, participants do not all have the same chance of being in the control or the experimental groups, or of receiving or not receiving the treatment. [6]

While RCTs have pre-test and post-test data for randomly assigned intervention and control groups, quasi-experimental design studies develop a counterfactual using a comparison group which has not been created by randomization. To develop the counterfactual, quasi-experimental studies use statistical techniques to create a comparison group that is matched with the intervention group in socioeconomic and other characteristics, or to adjust for differences between the two groups that might otherwise lead to inaccurate estimates. The goal of such statistical techniques is to simulate a randomized controlled trial.[7] Quasi-experimental methods include the following:

- Difference-in-Difference (or Double Difference): An increasingly popular method to estimate causal relationships, this technique compares the before-and-after difference for a group receiving the intervention to the before-after difference for those who did not.

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[6] Following the literature, the event for which an estimate of the causal effect is sought is called *treatment*. The *outcome* is what will be used to measure the effect of the treatment. The treatment and control groups do not necessarily need to have the same pre-intervention conditions. The two groups may well have different characteristics. However, many of those characteristics can reasonably be assumed to remain constant over time or at least over the course of an evaluation.

[7] For details on all these evaluation methods, see for instance Khandker et al. 2010; and Gertler et al. 2011.

- Matched comparisons: An analysis in which subjects in a treatment group and a comparison group are made comparable with respect to extraneous factors by individually pairing study subjects with the comparison group subjects.
- Instrumental variables: Have been used primarily in economic research, but have increasingly appeared in epidemiological studies. They are used to control for confounding and measurement error in observational studies, allowing for the possibility of making causal inferences with observational data and can adjust for both observed and unobserved confounding effects.
- Judgmental matching of comparison groups: A statistical method that involves creating a comparison group by finding a match for each person or site in the treatment group based on the researcher's judgment about what variables are important.
- Propensity score matching: Statistically creating comparable groups based on an analysis of the factors that influenced people's propensity to participate in a given program. The most common implementation of propensity score matching is one-to-one or pair matching, in which pairs of treated and untreated subjects are formed, such that matched subjects have similar values of the propensity score.
- Regression discontinuity: An analysis used to estimate program impacts in situations in which candidates are selected for treatment based on whether their value for a numeric rating exceeds a designated threshold or cut-off point. The analysis consists of comparing the outcomes of individuals below the cut-off point with those above the cut-off point.

2.2.7.2. Exclusion criteria

Studies that did not meet the inclusion criteria listed above (including studies that did not have a control group) were not considered.

2.2.7.3. Major Characteristics of the Studies Included in the Meta-Analysis

A detailed description of the major characteristics of the studies included in the meta-analysis is provided as Annex 1. The studies reviewed were identified through a systematic search. The search covered both general and specialist sources pertaining to education, economics, nutrition and health. They included electronic sources and journals, websites of research centers and gray publications (unpublished studies, including studies found through the World Bank, and the Abdul Latif Jameel Poverty Action Lab at MIT). Citation tracking and examination of the body of work of relevant influential authors were used to identify studies meeting the inclusion criteria used in this review. Electronic searches were conducted on papers cited in other papers already included in this review as well as cross-checking of references cited in other meta-analysis papers that included school feeding. Citation searches were also conducted using Google Scholar for related systematic reviews and relevant impact evaluations. Such impact evaluations and systematic reviews (and the citations therein) were screened for relevance using the screening criteria described above.

2.2.7.4. Statistical Analysis Methodology

Data in the studies reviewed were analyzed through meta-analysis.[8] Meta-analysis is the statistical combination of results from those separate studies. It can be used to generalize from the sample of studies based on different assumptions about the distribution of effects. Such a combination yields an overall effect size, a statistic (a quantitative measure) that summarizes the effectiveness of the interventions compared with their control interventions.[9]

The Comprehensive Meta-Analysis software, a computer program for meta-analysis, was used to estimate the overall impact of school feeding and pooled effect sizes. Following the international development meta-analysis literature, the random effects meta-analysis methodology was used to derive estimates. Unlike the fixed-effect meta-analysis, which assumes that the treatment effect is common across all studies and that differences in study findings are due to sampling error, or chance, only (Riley et al. 2011), random-effects meta-analysis estimates the average effect across studies, allowing for differences due to both chance and other factors which affect estimates -- such as study location, characteristics of the target population and length or intensity of the treatment. For this reason, the random-effects confidence interval in random-effects meta-analysis is wider than that estimated in a fixed-effect meta-analysis, reflecting a more conservative estimate as a result of the additional uncertainty around the estimate.

Study weights are also more balanced under the random-effects model than under the fixed-effect model. Under the fixed-effects model, it is assumed that the true effect size for all studies is identical, and the only reason the effect size varies between studies is sampling error (error in estimating the effect size). Therefore, when assigning weights to the different studies under the fixed-effect model it is assumed that we can largely ignore the information in the smaller studies because we have better information about the same effect size in the larger studies. By contrast,

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[8] According to the Campbell Collaboration -- an international research network that produces systematic reviews of the effects of social interventions in crime and justice, education, international development, and social welfare -- the objective of a systematic review is to "sum up the best available research on a specific question. This is done by synthesizing the results of several studies. A systematic review uses transparent procedures to find, evaluate and synthesize the results of relevant research. Procedures are explicitly defined in advance, in order to ensure that the exercise is transparent and can be replicated…Studies included in a review are screened for quality, so that the findings of a large number of studies can be combined." (Higgins 2014). This definition applies to any technical research topic. For instance, the U.S. Department of Health and Human Services defines the systematic review as "a critical assessment and evaluation of all research studies that address a particular clinical issue. The researchers use an organized method of locating, assembling, and evaluating a body of literature on a particular topic using a set of specific criteria." (http://effectivehealthcare.ahrq.gov/index.cfm/glossary-of-terms/?pageaction=showterm&termid=70;; accessed 5/9/2015).

[9] The effect size is a generic term for the estimate of effect of treatment for a study. It is a dimensionless measure of effect that is typically used for continuous data when different scales are used to measure an outcome and is usually defined as the difference in means between the intervention and control groups divided by the standard deviation of the control or both groups, where the standard deviation is defined as the spread or dispersion of a set of observations, calculated as the average difference from the mean value in the sample. (See, for instance, Cochrane Community, http://community.cochrane.org/; accessed 5/9/2015).

our objective under the random-effects model is not to estimate one true ("fixed") effect, but to estimate the mean of a distribution of effects to ensure that all these effect sizes are represented in the summary estimate.[10]

2.2.7.5. Limitations of the Analysis

2.2.7.5.1. Assessment of Publication Bias

The presence of bias in the extracted data was evaluated graphically by using the funnel plot and Egger's regression tests (Egger et al., 1997). To reduce publication bias (a situation that, for instance, may lead journals to prefer studies with positive effects), the search was broadened to the non-published "grey literature" that included conference proceedings, technical reports, dissertations, and theses. However, no attempt was made to assess publication bias through sensitivity analysis for outliers (defined as any study which differed markedly from the overall pattern) or through imputation of missing studies by using "trim and fill" analysis (Duval & Tweedie, 2000) -- a sensitivity analysis method that extends beyond the scope of this study.

Another method of assessing the potential for publication bias is to calculate the "fail-safe N," the number of studies whose effect size is zero or negative that would be needed to increase the P-value for the meta-analysis to above 0.05 (or any other selected threshold). However, the Cochrane Handbook for Systematic Reviews of Interventions notes that "this and other methods are not recommended for use in Cochrane reviews" (Higgins et al., 2014). (For additional information on publication bias, see Annex 1; for detailed funnel plots and Egger's regression texts associated with each pooled effect size estimated in this meta-analysis, see Annex 2.)

2.2.7.5.2. Assessment of Risk of Bias in Included Studies

The Cochrane Collaboration recommends a specific tool for assessing risk of bias in each included study and across studies. The assessment consists of a judgment and a support for that judgment for each entry in a "risk of bias" table, where each entry addresses a specific feature of the study. The judgment for each entry involves assessing the risk of bias as "low risk," "high risk," or "unclear risk," with the last category indicating either lack of information or uncertainty over the potential for bias. Assessment of risk of bias includes sequence generation (checking for possible selection bias), allocation concealment (checking for possible selection bias), blinding in RCTs (checking for possible performance and detection bias), incomplete outcome data

[10] This is equivalent to saying that we cannot discount a small study by giving it a very small weight (the way we would in a fixed-effect analysis). Since our objective is to estimate the mean effect in a range of studies -- and we do not want that overall estimate to be overly influenced by any one of them -- we cannot give too much weight to a very large study (the way we would in a fixed-effect analysis) and give too little weight to the estimate provided by a small study because that estimate contains information about an effect that no other study has estimated (See, for instance, http://www.meta-analysis.com/downloads/Meta-analysis%20Fixed-effect%20vs%20Random-effects%20models.pdf; accessed 6/10/2015).

(checking for possible attrition bias through withdrawals, dropouts or protocol deviations), selective reporting bias, and other sources of bias.

As for publication bias, a detailed assessment of risk of bias for each study included in the meta-analysis is beyond the scope of this investigation.

2.2.7.5.3. Heterogeneity and Stratified Analysis

We addressed heterogeneity,[11] by use of random-effects meta-analysis (see Section 1.3.7.4) and predefined subgroup analyses. We visually examined the forest plots[12] from the meta-analyses to look for any obvious heterogeneity among studies in terms of the size or the direction of treatment effect. We used the I^2 statistic test to quantify the level of heterogeneity among the studies in each analysis. We explored the identified heterogeneity by subgroups of participants, treatments, and outcomes. (Forest plots and I^2statistics for all interventions and outcomes measured can be found in Annex 2.) The stratified analysis focused on individual outcomes by type of school feeding (in-school meals and take-home rations) and gender. Further stratified analyses to control for certain treatment sub-categories and experimental samples are beyond the scope of this study. These include the effect of the following moderators[13] and d their impact:

- Study design and quality: RCTs vs. quasi-experimental design; for RCTs, masking of participants and outcome assessors, unit and method of allocation, and exclusion of participants after randomization or proportion of losses after follow-up; working papers vs. published papers; and quasi-experimental design method (for major quasi-experimental design methods, see Section 1.3.7.1).
- Geographic location of study population
- Rural and urban location

[11] Heterogeneity is used to describe the variation in, or diversity of, participants, interventions, and measurement of outcomes across a set of studies. In a statistical sense, it is used to describe the degree of variation in the effect estimates from a set of studies. It is also used to indicate the presence of variability among studies beyond the amount expected due solely to chance. Heterogeneity in meta-analysis is measured by I^2, a statistical expression of the inconsistency of the results in the studies reviewed. For example, a meta-analysis with $I^2 = 0$ means that all variability in effect size estimates is due to sampling error within studies. On the other hand, a meta-analysis with $I^2 = 50$ means that half of the total variability among effect sizes is caused not by sampling error, but by true heterogeneity between studies. According to the Cochrane Handbook (Higgins, 2014), a rough guide to the interpretation of I^2 is as follows:

0% to 40%: might not be important;
30% to 60%: may represent moderate heterogeneity;
50% to 90%: may represent substantial heterogeneity;
75% to 100%: considerable heterogeneity.

(http://handbook.cochrane.org/chapter_9/9_5_2_identifying_and_measuring_heterogeneity.htm)

[12] A forest plot is a graphical representation of the individual results of each study included in a meta-analysis, together with the combined meta-analysis result. The plot also allows researchers to see the heterogeneity among the results of the studies.

[13] Statistically, a moderating variable is one that affects the direction and/or strength of the relation between dependent and independent variables.

- Socio-economic status as defined in each study
- Age of children
- Grade of children
- Mode of school feeding management (e.g., extent of teacher involvement and community participation)
- Mode of school feeding administration (breakfast, lunch, snack or a combination thereof)
- Food type, quantity, and quality
- Study duration
- Sample size and power analysis (smaller experiments yield less precise estimates of treatment effects)

3.0. Empirical Evidence

This section examines the empirical evidence derived from the meta-analysis of the studies included in the investigation. (For a description of those studies and the outcomes measured, see Annex 1; a complete list of the studies is also provided in the bibliography.) As noted in Section 2.2, educational outcomes are divided into three categories: school participation, learning achievement, and cognitive development. School participation consists of enrollment, attendance, dropouts, and repetition; learning achievement has three components: math, language, and reading; and cognitive development includes memory, verbal achievement and reasoning.

As noted in Section 2.2.1, two categories of school feeding interventions were considered: in-school meals and take-home rations. While in-school meals (breakfast, snacks, lunch, or a combination thereof) are provided to all students while they are at school, take-home rations are given to selected households conditional on their children's enrollment in school and a minimum level of attendance. Take-home rations are also targeted to vulnerable groups, including the very poor, girls and children affected by HIV.

The meta-analysis first estimates the overall effect of school feeding across all school feeding modalities, educational outcomes, and target groups. To explore specific issues and answer programmatically relevant questions that would provide policymakers with potentially generalizable knowledge about school feeding, we next restrict the overall sample to sub-samples, and estimate pooled effect sizes for each category of outcomes and for each outcome within each category.[14] The sub-sample analysis is also used to explore effect sizes by type of school feeding (in-school meals and take-home rations) and by gender.[15]

For clarity and ease of presentation, the detailed findings are based on a series of tables derived from the forest plots and associated data presented as Annex 4 which, together with Annex 3,

[14] For a categorical subgroup variable, each subgroup should have a minimum of four studies (see, for instance, Tipton 2014 and Conn 2014).
[15] As noted in Section 2, other moderator variables could not be incorporated in the analysis.

includes detailed statistics of effect sizes such as standard errors, t-values, degrees of freedom, confidence intervals, statistical significance, heterogeneity statistics, funnel plots and Egger's tests.. The detailed findings are followed by summary and conclusions, limitations of those findings, and implications for future research.

3.1. Findings

In presenting findings, we first look at the overall effect of school-feeding, irrespective of the school-feeding modality on the pooled educational outcomes. Next, we explore whether different feeding modalities have different effects on the educational outcomes of interest. In doing so, we examine the separate effects of in-school meals and take-home rations on the pooled education outcomes. Since the effect of school feeding on educational outcomes may depend on the feeding modality and gender of the recipients, we estimate pooled effect sizes for the combined educational outcomes, by type of school feeding and gender. We then estimate, by type of school feeding and gender, the pooled effect sizes of school feeding for each outcome category and each component within each category. As described in Section 2.2.1, school feeding interventions are of two types: in-school meals and take-home rations. In-school meal programs make food available to children while they are at school and consist of breakfast, snacks, lunch or a combination thereof. Take-home rations are targeted to selected households or to particularly vulnerable students, including the very poor, girls and children affected by HIV.

Due to lack of data in the studies reviewed, only a subset of the outcome categories listed in Section 2.2.2 could be estimated.

3.1.1. Overall Effect Size

We first estimate an overall effect size across all school feeding modalities, educational outcomes, and target groups.

Finding 1: The overall effect of school feeding on the combined educational outcomes is positive, but very small

Table 3.1 shows the estimated combined mean impact of school feeding interventions on school participation (enrollment and attendance), learning achievement (math and language), and cognitive development (memory).[16] The overall effect size is 0.043 standard deviations[17], with a 95% confidence interval of (0.032, 0.054),[18] indicating that the impact of school feeding on

[16] Due to lack of data in the studies reviewed, dropout and repetition rates, reading, verbal achievement, and reasoning could not be included in the meta-analysis.
[17] The standard deviation is a measure of the dispersion of a set of data from its mean. The more spread apart the data, the higher the deviation. A standard deviation close to zero indicates that the data points tend to be very close to the mean.
[18] A confidence interval is a range of values such that there is a specified probability that the value of a parameter lies within that range. In our example, we are 99% confident that the 0.043 standard deviation falls between 0.032

educational outcomes as measured by the difference in outcomes between the treatment group and control group after the intervention (school feeding) is positive. As indicated by (***) in the table, this difference is statistically significant at the 99% level.[19]

It is important to note that, although positive, the effect size estimate (0.043) is very small.[20] This effect size is about the same effect size estimated in the only identified meta-analysis that included school feeding in developing countries (Conn, 2014). In that study, the effect size for school feeding is estimated at 0.059.[21]

Table 3.1: Overall Effect Size Estimate

Estimate	SE	P-value	95% CI.L	95% CI.U
0.043 (***)	0.005	0.00	0.032	0.054
(***) Significant at 99% level; (**) Significant at 95% level; (*) Significant at 90% level				
I^2 = 56.772				
Number of studies: 17 Number of effect sizes: 88				

3.1.2. Pooled Effect Sizes by Educational Outcome, Type of School Feeding, and Target Group

This section describes in more detail the effect sizes of school feeding by category of educational outcomes, by individual outcome within each category, and by school feeding modality and target group.

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and 0.054. Note that the significance level is reflected in the P-value as follows: P-value <0.01 means statistical significance at the 99% level; P-value <0.05 means statistical significance at the 95% level; P-value <0.1 means statistical significance at the 90% level.

[19] A null hypothesis is the statement that school feeding has *no* impact on educational outcomes. For a null hypothesis to be rejected as false (i.e., that school feeding *does have* an impact on educational outcomes), the result has to be identified as being statistically significant (i.e., unlikely to have occurred due to sampling error alone or, equivalently, due to the unrepresentativeness of the sample). The probability of rejecting the null hypothesis (in this case rejecting the hypothesis that school feeding has *no* impact on educational outcomes) given that it is true, is most often set at 0.05 (95%), but can also be set at 0.01 (99%) or 0.10 (10%). Put differently, to determine whether a result is statistically significant at a given level, a researcher has to calculate a P-value, which is the probability of observing an effect given that the null hypothesis is true. The null hypothesis is rejected if the P-value is lower than the significance level -- which is the case here since the P-value (0.000) is lower than the significance level (0.01).

[20] Effect size magnitudes are typically interpreted on the basis of rules of thumb suggested by Cohen 1988. According to Cohen, an effect size of about 0.20 is considered “small,” of about 0.50 is considered “medium,” and of about 0.80 is considered “large.” Although these guidelines are broad categorizations, it has become standard practice for researchers to use them when interpreting effect size estimates. Thus, if the means for the treatment and control groups do not differ by 0.2 standard deviations or more, the difference is “trivial” or very small even if it is statistically significant.

[21] It should, however, be noted that the 0.059 figure applies only to sub-Saharan Africa) and to both school meals and supplements, and it is not statistically significant (the sample size was too small to assess the statistical significance of that estimate).

3.1.2.1. Pooled Effect Sizes for all Educational Outcomes, by Type of School Feeding and Target Group

Finding 2: The combined effect size of in-school meals and take-home rations on the combined educational outcomes is much higher for girls than for boys

As shown in Table 3.2, the combined effect size of in-school meals and take-home rations is much higher for girls (0.078 and statistically significant) than for boys (not statistically significant at any level).[22]

Finding 3: The effect size of school feeding on the combined educational outcomes is smaller for school meals than for take-home rations

Table 3.2 shows that the effect size for both take-home rations and in-school meals is statistically significant, the effect size for take-home rations (0.061) is nearly twice as large as the effect size for in-school meals (0.033).

Table 3.2: Mean Effect Size of School Feeding on the Combined Educational Outcomes, by School Feeding Modality and Gender

School feeding modality (#)	Effect size estimate	Number of effect sizes
In-school meals and take-home rations	0.043 (***)	88
Girls	0.078 (***)	17
Boys	0.033	17
In-school meals	0.033 (***)	59
Girls	0.054 (***)	8
Boys	0.020	8
Take-home rations	0.061 (***)	24
Girls	0.102 (***)	8
Boys	0.051 (***)	7

(***) Significant at 99% level; (**) Significant at 95% level; (*) Significant at 90% level
(#) Effect sizes too few to estimate effect size by type of school meals (breakfast, lunch, snack, and any combination thereof).

Finding 4: The effect of in-school meals on the combined educational outcomes is larger for girls than for boys

[22] Saying that the estimate is not statistically significant is equivalent to saying that the effect size is zero or that the treatment has no impact on the treatment group.

As shown in Table 3.2, the effect size for in-school meals is much higher for girls (0.054) than for boys (not statistically significant at any level).

Finding 5: The effect of take-home rations on the combined educational outcomes is larger for girls than for boys

As shown in Table 3.2, the effect size for take-home rations is twice as high for girls (0.102) than for boys (0.051).

3.1.2.2. Effect Size of School Feeding on School Participation

This section describes in the effect sizes of school feeding on school participation, the first category of educational outcomes. As noted earlier, school participation consists of school enrollment, attendance, dropout, and repetition rates. However, the investigation is limited to enrollment and attendance; dropout and repetition rates are not included in the analysis for lack of data in the studies reviewed. Outcomes are analyzed by school feeding modality and gender.

Finding 6: The effect of school feeding on school participation is positive for all school children regardless of sex

As shown in Table 3.3, the overall effect of school feeding (both in-school meals and take-home rations) for all children is positive (0.050, and statistically significant), and is nearly the same for girls (0.070) as for boys (0.067).

Finding 7: The effect of in-school meals on school participation is larger for girls than for boys

As shown in Table 3.3, the overall effect of in-school meals for all children is positive (0.061, and statistically significant). However the effect is larger for girls (0.045) than for boys (not statistically different from zero).

Finding 8: The effect of take-home rations on school participation is positive for all school children and is the same for girls as for boys

The effect of take-home rations on school participation is 0.061 for all school children (Table 3.3) and is nearly the same for girls (0.120) as for boys (0.122) in both size and statistical significance.

Table 3.3: Mean Effect Size of School Feeding on School Participation, by School Feeding Modality and Gender		
School feeding modality	**Effect size estimate**	**Number of effect sizes**
In-school meals and take-home rations	0.050 (***)	37
Girls	0.070 (***)	9
Boys	0.067 (**)	9
In-school meals	0.041 (***)	20
Girls	0.045 (*)	4
Boys	0.034	4
Take-home rations	0.061 (***)	12
Girls	0.120 (***)	4
Boys	0.122 (***)	4
(***) Significant at 99% level; (**) Significant at 95% level; (*) Significant at 90% level		

3.1.2.2.1: Effect Size of School Feeding on School Attendance

Finding 9: The combined effect of in-school meals and take-home rations on school attendance is very small, but higher for girls than for boys

As shown in Table 3.4, the combined effect of in-school meals and take-home rations is very small (0.078). The effect size for girls (0.108) is nearly twice as large as for boys (0.058).

Finding 10: The effect on school attendance is higher for take-home rations than for in-school meals

Table 3.4 demonstrates that the effect of take-home rations on attendance (0.121) is higher than for school meals, whose effect is not statistically different from zero.

Table 3.4: Mean Effect Size of School Feeding on School Attendance		
Item	**Effect size**	**Number of effect sizes**
Pooled effect	0.078 (***)	8
Girls	0.108 (**)	4
Boys	0.058 (*)	4
In-school meals only (#)	0.041	4
Take-home rations only (#)	0.121 (***)	4
(***) Significant at 99% level; (**) Significant at 95% level; (*) Significant at 90% level (#) Effect sizes too few to estimate effect size by gender		

Finding 11: The effect of take-home rations is higher for school attendance than for enrollment

As can be seen in Table 3.4 and Table 3.5, the effect of take-home rations for school attendance (0.121) is more than twice as high as for school enrollment (0.042). This outcome differential may be explained by two factors. First, take-home rations result in higher enrollment (Table 3.5). Second, take-home rations result in higher attendance because they are often offered in addition to in-school meals and are conditional on attendance.

3.1.2.2.2: Effect Size of School Feeding on School Enrollment

Finding 12: The pooled effect of school feeding on enrollment is very small but is nearly equal for girls and for boys

At 0.040 (Table 3.5), the pooled effect of school feeding on enrollment is very small. The pooled effect for girls (0.046 and significant at the 99% level) is slightly lower than for boys (0.051 and significant only at the 95% level).

Finding 13: The effect on school enrollment is higher for take-home rations than for in-school meals

As shown in Table 3.5, the effect on enrollment for take-home rations (0.042) is higher than for in-school meals (0.033).

Table 3.5: Mean Effect Size of School Feeding on School Enrollment

Item	Effect size	Number of effect sizes
Pooled effect	0.040 (**)	15
Girls	0.046 (***)	4
Boys	0.051 (**)	4
In-school meals only (#)	0.033 (*)	10
Take-home rations only (#)	0.042 (**)	6

(***) Significant at 99% level; (**) Significant at 95% level; (*) Significant at 90% level
(#) Effect sizes too few to estimate effect size by gender

3.1.2.3. Effect Size of School Feeding on Learning Achievement

This section describes the effect sizes of school feeding on learning achievement, which combines language achievement and math scores. Language and math scores were examined separately by school feeding modality and, when data are available, analyzed by gender.

Finding 14: The effect of school feeding on learning achievement is much smaller than its effect on school participation

A comparison of Table 3.3 and Table 3.6 demonstrates that the effect of school feeding on learning achievement (0.030) is much smaller than its effect on school participation (0.050).

Table 3.6: Mean Effect Size of School Feeding on Learning Achievement, by School Feeding Modality (#)

School feeding modality (#)	Effect size estimate	Number of effect sizes
In-school meals and take-home rations	0.030 (***)	36
In-school meals	0.023 (**)	31
Take-home rations	0.073 (**)	6

(***) Significant at 99% level; (**) Significant at 95% level; (*) Significant at 90% level
(#) Effect sizes too few to estimate effect size by gender

The effect size on language achievement (one of the two components of learning) is, as shown in Table 3.7, much lower (and not even statistically significant).

Table 3.7: Mean Effect Size of School Feeding on Language Achievement, by School Feeding Modality (#)

Item (#)	Effect size	Number of effect sizes
Pooled effect	0.013	21
School meals only	0.012	19
Take-home rations only	---	2

(***) Significant at 99% level; (**) Significant at 95% level; (*) Significant at 90% level
(#) Effect sizes too few to estimate effect size by gender
--- Effect sizes too few to estimate effect sizes separately

A similar observation can be made when comparing effects for school participation (Table 3.6) and math scores (Table 3.8). Two factors may account for this result. First, the effect on learning takes longer to materialize that can be observed during a particular study period, especially during the much shorter randomized controlled trials. Second, the pathway to higher learning achievement may be less direct than that mediated by enrollment or attendance since it also depends on the quality of education available. As suggested in Vermeersch and Kremer (2004) and Powell et al. (1998), increased enrollment and attendance may result in overcrowded schools and higher student to teacher ratio, with negative effects on learning outcomes. A related explanation is that administering the school feeding program may eat into the school day, especially when teachers are involved in managing the program.

Table 3.8: Mean Effect Size of School Feeding on Math Scores, by School Feeding Modality (#)		
Item	**Effect size**	**Number of effect sizes**
Pooled effect	0.055 (*)	15
School meals only	0.039 (*)	10
Take-home rations only	0.049 (**)	13

(***) Significant at 99% level; (**) Significant at 95% level; (*) Significant at 90% level
(#) Due to lack of data, verbal achievement and reasoning could not be included in the meta-analysis

3.1.2.4. Effect Size of School Feeding on Cognitive Development

This section describes the effect sizes of school feeding on cognitive development, by gender. Cognitive development combines verbal fluency, memory, and reasoning. However, since no data are available on reasoning and verbal fluency, only the effect on memory is examined.

Finding 15: School feeding has no effect on cognitive development when the combined effect of in-school meals and take-home rations on all school children is considered

The net effect of school feeding (in-school meals and take-home rations) is not statistically significant at any level when all school children are considered (Table 3.9). Such an outcome may be due to the fact that the link between nutrition and cognitive development is strongest in early years of life, and by the time students reach school age additional nutrients may not have an impact on cognitive development (Kristjansson et al. 2007; Vermeersch and Kremer 2004).

The effect of school feeding on cognitive development stands in contrast with its effect on school participation and learning achievement. Whereas school feeding has no effect on *cognitive development* when the combined effect of in-school meals and take-home rations on all school children is considered (Finding 15), the corresponding combined effect of in-school meals and take-home rations on all school children is positive for *school participation* (a statistically significant effect size of 0.050 in Table 3.3) and for *learning achievement* (a statistically significant effect size of 0.030 in Table 3.6).

Table 3.9: Mean Effect Size of School Feeding on Cognitive Development, by School Feeding Modality and Gender (^^)		
School feeding modality	**Effect size estimate**	**Number of effect sizes**
In-school meals and take-home rations	0.0603	15
Girls	0.081 (***)	6
Boys	-0.002	6
In-school meals	0.023	9
Take-home rations	0.056 (**)	6

(***) Significant at 99% level; (**) Significant at 95% level; (*) Significant at 90% level
(^^) The analysis for cognitive development was limited to memory improvement. Due to lack of data, verbal achievement and reasoning could not be included in the investigation.

Finding 16: Take-home rations have a positive effect on cognitive development but no effect on cognitive development is detected for in-school meals

Table 3.9 shows that take-home rations have a positive effect (0.056) on cognitive development but in-school meals have no effect on that educational outcome.

Finding 17: The effect of school feeding on cognitive development is positive for girls when the combined effect of in-school meals and take-home rations is disaggregated by gender

As shown in Table 3.9, the combined effect of in-school meals and take-home rations on cognitive development for girls is positive (0.081) when that effect is analyzed separately for girls and boys.

3.2. Conclusions, Limitation of Findings, and Future Research Directions

The objective of this meta-analysis was to investigate the likely causal impact of school feeding on educational outcomes for school-age children and its programmatic and policy implications, as reflected in the experimental and quasi-experimental literature on food-for-education programs in developing countries. This section summarizes the main findings of the study and points out the major limitations of those findings. Those limitations are due to several factors, including the limited number of studies eligible for inclusion in the meta-analysis, lack of detailed data on participants (e. g., gender, age, grade, household income), in-school feeding modalities (e.g., breakfast, lunch, snack), mode of school feeding management, and program implementation cost. Those limitations served as basis for suggesting possible areas for future research.

3.2.1. Conclusions

The meta-analysis examined the overall effect size of school feeding on educational outcomes, defined as school participation, learning achievement, and cognitive development. To assess the strength of the overall impact of school feeding, the study examined the overall effect size of school feeding across all educational outcomes. It then analyzed a pooled effect size separately for each outcome. When data were available, it also explored effect sizes by gender and type of school feeding: in-school feeding, take-home ration, or a combination thereof.

Several conclusions of the investigation should be emphasized:

- School feeding has a positive and statistically significant impact on educational outcomes. However, this conclusion should be tempered by the fact that the effect size is very small.

- Benefits are consistently stronger for girls, suggesting that school feeding may play a role in reducing gender disparity in developing countries where girls are often differentially excluded from education and where gender disparity otherwise remains a challenging task.
- School feeding has a positive and statistically significant impact on school participation. The impact is positive and statistically significant for both enrollment and attendance, indicating that school feeding serves as an incentive to get children into school and help keep them there. Another conclusion is that those benefits are stronger for girls.
- The impact of school feeding on learning achievement and cognitive function is lower than the impact on school participation. The lower result for learning achievement and cognitive development is not surprising for two reasons. First, the effect on learning takes longer time to materialize than can be observed during a particular study period, especially during the much shorter randomized controlled trials. Second, the pathway to higher learning achievement and cognitive development is less direct than that mediated by enrollment or attendance since it also depends on the quality of education available. From this perspective, school feeding programs may be more effective if combined with quality education programs, including an appropriate curriculum, quality teachers, high teacher to student ratios, and suitable textbooks. Higher learning achievement and cognitive development outcomes may also be negatively affected if teachers and education staff diverted part of their time to preparing and serving food to children, because this practice may tax the very system targeted for improvement.
- The effect on school enrollment, attendance, cognitive development, and learning achievement as measured by math scores is larger for take-home rations than for in-school meals.
- For optimal results, school feeding and quality education systems may need to be implemented in combination with supplementary services such as health and nutrition interventions. To take one example, malaria reduction in school-age children in Kenya resulted in a decline in the prevalence of anemia and a concomitant enhancement in performance on cognitive tests, but no measurable improvement in education outcomes was observed due to the lack of quality education inputs (Clarke et al. 2008). Such complex interactions suggest that school feeding may not be the best response to a developing education system, but that it may be a valuable tool in the range of instruments to achieve a more effective education system.

3.2.2. Limitations of the Findings

The results summarized above should be interpreted with caution for several reasons:

- Most of the evidence comes from a set of 11 countries which, while dispersed across Africa (4 countries), Asia (3 countries), and Latin America (4 countries), may not be representative of the population of other countries or other less-researched settings. As noted earlier in this report, there are nearly 400 million pre-primary-, primary- and secondary-school children receiving food through schools around the world, with programs of more than one million children in more than 40 countries. An MGD intervention mapping for 2009-2013 also shows that MGD interventions have covered 56 programs worldwide, of which 52 percent in Africa, 28 percent in Asia and 20 percent in Latin America. Those programs have been implemented by more than 20 organizations, and ranged from less than $5 million to more than $20 million and from less than 12 months to more than 36 months in duration.

- Due to lack of data, important moderator variables could not always be incorporated into the analysis, including effect sizes by age; grade; mode of in-school meals (breakfast, lunch, snack or a combination thereof); mode of school feeding management (notably extent of teacher involvement or community participation); and food type, quality and quantity.
- A number of other key moderator variables in the studies reviewed were either missing or not readily quantifiable to shed additional light on certain effect sizes. For instance, no quantifiable information was available to account for potential negative effects of a decrease in teacher to student ratios on learning or cognitive outcomes due to higher enrollment, or a decrease in instructional time due to higher involvement of teachers in school-feeding program management.
- The lack of sufficient information on the political and social context in the target school communities limits the possibility of replicating the program in similar contexts or the generalizability of the findings. In the absence of such data, this meta-analysis could not investigate a number of intervention features underlying the differential success in improving educational outcomes, nor could it explore the contextual barriers to, and facilitators of, the effectiveness of educational interventions. This limitation is all the more critical because knowing what works is not sufficient for policymakers, who also need to know how to make it work with different groups of people and in different institutional and economic contexts.
- The study showed that the impact of school feeding on educational outcomes is very small. However, since no other outcomes were considered in the analysis, school feeding may have had a higher impact on non-educational outcomes, such as health and nutrition – although available evidence (Alderman, H., & Bundy, D. 2012; Bundy et al. 2009; WFP 2013) suggests that school feeding is not a most likely response to malnutrition.
- Due to data availability, this study provided average effect size estimates without accounting for the resource inputs associated with each program. This is a significant gap because it is misleading to use effect size as the sole criterion for ranking interventions. Some programs may have been on average less effective but more cost-effective[23] than

[23] Cost-effectiveness is an evaluation method that examines the costs relative to the outcomes, or results, of interventions. It is critical to note that effectiveness must be measured by an outcome, not by an activity. For instance, just as the effectiveness of a smoking-cessation program cannot be measured by the number of smokers

others. Another aspect of cost-effectiveness is the long-term impact of the intervention on students and school communities.

3.2.3. Future Research Directions

A recent WFP review (WFP 2013) of school feeding programs in developing countries has pointed out the increasing political support for school feeding programs and demand for evidence-based guidance on their implementation. The limitations of the meta-analysis has also shown that though school feeding programs in developing countries are widely used, the evidence base for those programs needs to be strengthened.

Based on the limitations of the findings in this meta-analysis, the following major areas of focus in the school feeding learning agenda merit particular mention:

- Broadening the geographic focus to countries beyond the limited number of studies and countries in which most of the rigorous research has been conducted would strengthen the evidence base on school feeding and its educational outcomes.
- Additional impact evaluations are particularly needed across age groups and grades, and according to socioeconomic status and gender, so that trade-offs and returns to various combinations of treatment and outcomes can be estimated.
- The present analysis has shown that school feeding increases short-term measures of school participation, including enrollment and attendance, but has mixed effects on learning. Other studies (Fiszbein and Schady, 2009; Galiani and McEwan, 2013; Barham et al., 2012; Behrman et al., 2009) have shown that conditional transfer programs have yielded similar effects, suggesting that attending school may be a necessary but not sufficient condition to improved learning and that future research could combine school feeding (and more broadly, access-based interventions) with instructional interventions in schools such as textbooks, teacher training, curriculum upgrading, and similar treatment arms. Single or multi-armed studies combining school feeding with selected instructional interventions would show which combination of interventions are most likely to strengthen the educational outcomes of school feeding.
- Additional research on how food type, quantity and quality (e.g., fortified vs. non-fortified meals) would shed some light on whether and to what extent such characteristics may affect educational outcomes.
- Additional studies on the effect of school feeding on educational outcomes by mode of in-school meals (breakfast, lunch, snack or a combination thereof) would be helpful in identifying the effectiveness of each modality.
- Additional studies are needed that include quantifiable information to account for potential negative effects of a decrease in teacher to student ratios on learning or

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receiving smoking-cessation counseling, effectiveness of school-feeding cannot be measured by the number of children receiving in-school meals or take-home rations -- because there is no guarantee that the counseling or the school feeding will bring about results.

cognitive outcomes due to higher enrollment following the introduction of a new school feeding program, or a decrease in instructional time due to higher involvement of teachers in school-feeding program management.

- Including information in school-feeding research on the social and economic context in the target school communities would shed some light on how school feeding programs with the strongest educational outcomes can be replicated in similar settings and which contextual barriers may reduce their effectiveness.
- Additional studies on the longer-term impact of school feeding are needed to track the effect of school feeding on educational achievements and the economic productivity of children as they reach adulthood. Such evidence is currently lacking due in large part to the various difficulties associated with running experiments for an extended period of time.
- When conducting research on school feeding, it is critical to analyze the cost drivers of programs to gain better understanding of why costs are low in some countries and settings and high in others. Based on that analysis, guidance could be developed on how to estimate costs along the supply chain and optimize operations. A necessary step in that direction would be to design studies that fully detail the treatment itself, including implementation features and intervention inputs (such as school infrastructure improvements, staff resources, and meals provided) and their costs. Such information would stimulate the development of a cost-effectiveness methodology, cost-benefit ratios and associated metrics for selecting optimal interventions.

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Note: (*) denotes study included in the meta-analysis

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Annexes

Annex 1: Baseline Characteristics of the Studies Included in the Systematic Review and Meta-Analysis

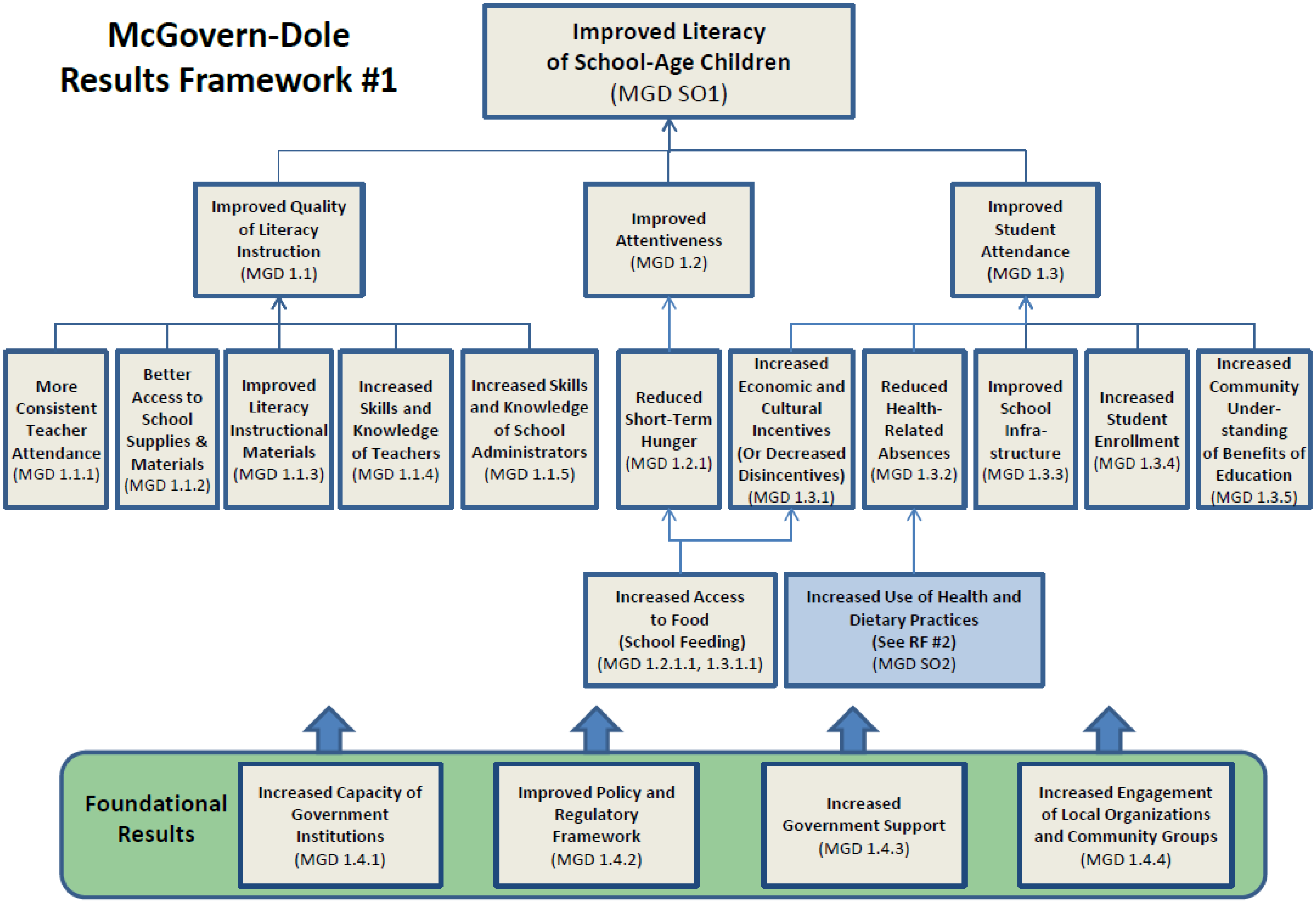
Author	Title	Type of Study	Location	Age/grade	Sample	Intervention	Educational Outcome Measures
Adelman et al., 2008	The Impact of Alternative Food for Education Programs on Learning Achievement and Cognitive Development in Northern Uganda	RCT	Northern Uganda	6-14 years	In-school meals (ISM), take-home ration (THR), and control groups. 2005 Baseline: 323 observations: 134 ISM; 103 THR; 86 control. 2007 Survey: 426 observations: 178 ISM; 143 THR; 105 control.	ISM; THR; nothing (control group). ISM=snack and lunch. THR= one month for students with 82% attendance. Duration: 18 months	Math, cognitive development
Afridi, 2011.	The Impact of School Meals on School Participation: Evidence from Rural India.	Staggered implementation of program; difference in differences;	Madhya Pradesh, rural India	Grade 1-5	39 treatment schools; 17 control schools.	Transition from THR monthly provision of free raw food grains to free daily cooked meals. Cooked meals cost is higher than raw food grains distribution Data from 2003-2004 used to determine whether transition to daily cooked meals improved school participation.	School attendance and enrollment
Ahmed, 2004	Impact of Feeding Children in School: Evidence from Bangladesh	RCT	Bangladesh, rural villages and Dhaka slums	6 -12 years	Four surveys: villages, schools, households, and communities. 4,453 households (3,193 program and 1,260 control) in villages and urban slums. 12 selected villages and urban slum-communities. 68 primary schools (34 program and 34 control) in the school survey.	ISM=mid-morning biscuit snack to 6,000 primary schools. Duration: baseline 2002; follow-up survey 2003	Enrollment, attendance
Ahmed & Del Ninno, 2002	Food for Education Program in Bangladesh: An Evaluation of Its Impact on Educational Attainment and Food Security	RCT	Bangladesh	6 -12 years	Household selection from 10 thanas; from each 2 intervention unions and 1 control union; from each 2 villages, from each village 10 households with school-age children. Intervention sample: 10,449 children in 70 schools in intervention unions. Control sample: 5,243 children in 40 schools in non-FFE unions.	THR. hhd with primary school-age children eligible for benefits if hhd has less than half acre, head is laborer, female, or has low-income occupation. Duration: surveys in 2000, but program on-going since 1993.	School enrollment, attendance, academic achievement
Alderman, 2010	The impact of Food for Education Programs on School Participation in Northern Uganda.	RCT	Norther Uganda; IDP refugee camps	6-17 years	31 out of 54 camps assigned to 3 groups: ISM 11 camps; THR 10 camps; control 10 camps. Households with children in camps also selected randomly in baseline survey. Gross enrollment 4,018 students; net enrollment 3,134.	THR vs. ISM vs. control. Experiment with timing of meals; and child/parent incentives. ISM group provided free snack and lunch. THR group received once per month dry ration equivalent in	Enrollment, attendance.

Author	Title	Type of Study	Location	Age/grade	Sample	Intervention	Educational Outcome Measures
						energy and protein to ISM. Duration: Baseline survey (Oct-Dec 2005) compared with follow up (March-April 2007) survey. 18 months coverage.	
Buttenheim et al., 2011	Impact Evaluation of School Feeding Programs in Lao PDR	Difference in difference estimates at two levels: between districts and between take-up and non-take-up villages. Propensity-score matching to construct plausible control counterfactuals	Lao People's Democratic Republic	3-14 years. Potential spillover effects for older and younger siblings.	3 out of 7 districts randomly assigned to ISM, THR, and ISM+THR treatments. Longitudinal survey of 4,500 households with school-age children in villages in 4 sample districts. Villages that chose to participate had minimum criteria. 15 random households selected from villages. From a target of 4,500 hhd, interviews were conducted with 4,169 hhd in 263 villages. Follow-up survey attempted to cover same hhd as baseline survey but 11 villages had moved, for a loss of 119 hhd but 286 replacement hhds added in 2008 survey. Control district in neighboring province with similar ethnicity and geography.	ISM, THR, both, and control. THR. Duration: 2006 baseline and 2008 follow-up survey.	Enrollment
Cueto et al., 2008	Educational Impact of a School Breakfast Programme in Rural Peru.	Difference in differences treatment vs control group over time. Hierarchical Linear Model to analyze test data. Covariance analysis to compare outcomes.	Peru, rural highlands	Fourth grade.	11 schools in treatment group: 5 with separate classrooms for each grade and 6 with one or more grades per classroom. 9 control schools Total sample 350 fourth grade students: 169 in treatment group, and 181 in control group. Treatment schools in one province; control schools from near provinces to match treatment schools in altitude, bilingualism, demographics, and socio-economic status. The evaluation was designed after the school breakfast program was started, so it was not possible to assign students or schools randomly.	Breakfast served mid-morning during recess. Tests given after breakfast in treatment group, after recess in control group. Program started in 1996; evaluation in 1998.	Achievement tests for arithmetic, vocabulary, reading, memory, coding. attendance, enrollment and dropout rates
Gelli, A., Meir, U., & Espejo, F. (2007).	Does Provision of Food in School Increase Girls' Enrollment? Evidence from Schools in Sub-Saharan Africa	Retrospective cross-sectional study based on school-level survey data from 32 African countries	Sub-Saharan Africa, 32 different countries	Primary-school-age children	Schools divided by type and length of program. Out of 4,175 schools in sample, 593 had food program for 2 years, and the remaining 2,680 had it over 1 year and 903 not yet beneficiaries.	ISM and THR. Target population: food-insecure areas with poor access to education. Duration: Survey period October 2002 to February 2005.	Enrollment

Author	Title	Type of Study	Location	Age/grade	Sample	Intervention	Educational Outcome Measures
Ismail et al., 2014	Guyana's Hinterland Community-Based School Feeding Program	Longitudinal and cross-over samples. Compares group averages for treatment and control.	Guyana hinterland	Grades 2-6	Baseline data collected in 64 schools starting in 2007: 20 schools that began school feeding during the evaluation period formed the intervention group and the remaining 44 schools formed the control group.	Cooked lunches to intervention school children. Parents participated fully in food production and meal preparation and delivery. Duration: Baseline data collected starting 2007. ISM impact evaluation in 2007-2009.	Attendance, English and math
Kazianga et al., 2012	Educational and Child Labour Impacts of Two Food-for-Education Schemes: Evidence from a Randomised Trial in Rural Burkina Faso	RCT	Burkina Faso, northern (Sahelian region)	6-15 years	Initially 16 villages in ISM and 16 in THR, but after data problems analysis based on only 15 villages for each treatment and 12 in control. Surveyed random sample of 48 hhd around each school for total 2,208 hhd with 4,236 school-age children. 1,493 students in the ISM villages, 1,498 in THR villages, and 1,245 in control villages.	THR for girls with 90% attendance. Boys not eligible for THR. ISM lunch to both girls and boys. Duration: baseline June 2006; follow up June 2007 (one academic year).	Enrollment, attendance, academic performance, and cognitive development
Kazianga et al., 2009	Educational and Health Impacts of Two School Feeding Schemes: Evidence From a Randomized Trial in Rural Burkina Faso	RCT	Burkina Faso; northern rural Region with low school participation	6-15 years	15 schools to ISM program; 16 village/schools to THR; and 14 to control group. Random sample of 48 hhd per school, for a total of 2,208 hhd and 4,140 school age children.	ISM (lunch) and THR (for girls with 90% attendance). Duration: baseline survey in 2006. Follow-up survey in 2007 (one school year)	Enrollment, math
McEwan, 2013	The impact of Chile's School Feeding Program on Education Outcomes	Regression discontinuity design on school administrative data.	Chile, public schools system, nationwide	Grades 1-8; 6-17 years	8,727 schools. Vulnerability index for each school based on year 2000 report. Proportion of students within each school varies continuously with the vulnerability index, with higher proportions assigned to poorer schools with higher vulnerability scores.	Vulnerability index (0 to 100) used to assign school meal rations during 2001-2005 school years: Cut-off index used to identify discontinuities in regression estimates. Duration: Administrative records of Ministry of Education from 2001 to 2005.	Enrollment, attendance, math and language
Meng, X., & Ryan, J. (2007)	Does a Food for Education Program Affect School Outcomes? The Bangladesh Case.	Propensity score matching combined with difference-in-differences methodologies. Probit models to estimate probability of eligible households	Bangladesh	6 -13 years	Compare schooling outcomes between food for education (FFE) households and FFE-non-eligible households in the two FFE Unions (districts) and between FFE-eligible households in FFE unions and households in the non-FFE-unions. Total 400 households in intervention Unions: 209 hhd with 399 children are program eligible and 191 hhd with 336 children were non-eligible. Control in non-FFE unions: 200 households with 343 primary children used as counterfactuals of eligible households in FFE-unions.	Household eligible for FFE if head is female, has under 0.5 acres, is day laborer, or a low-income artisan. Subsidy: 15 kg of wheat or 12 kg of rice per month (20 kg wheat or 16 kg rice if more than 1 child) Duration: Data collected in 2000, after 7 years of program operation.	Attendance

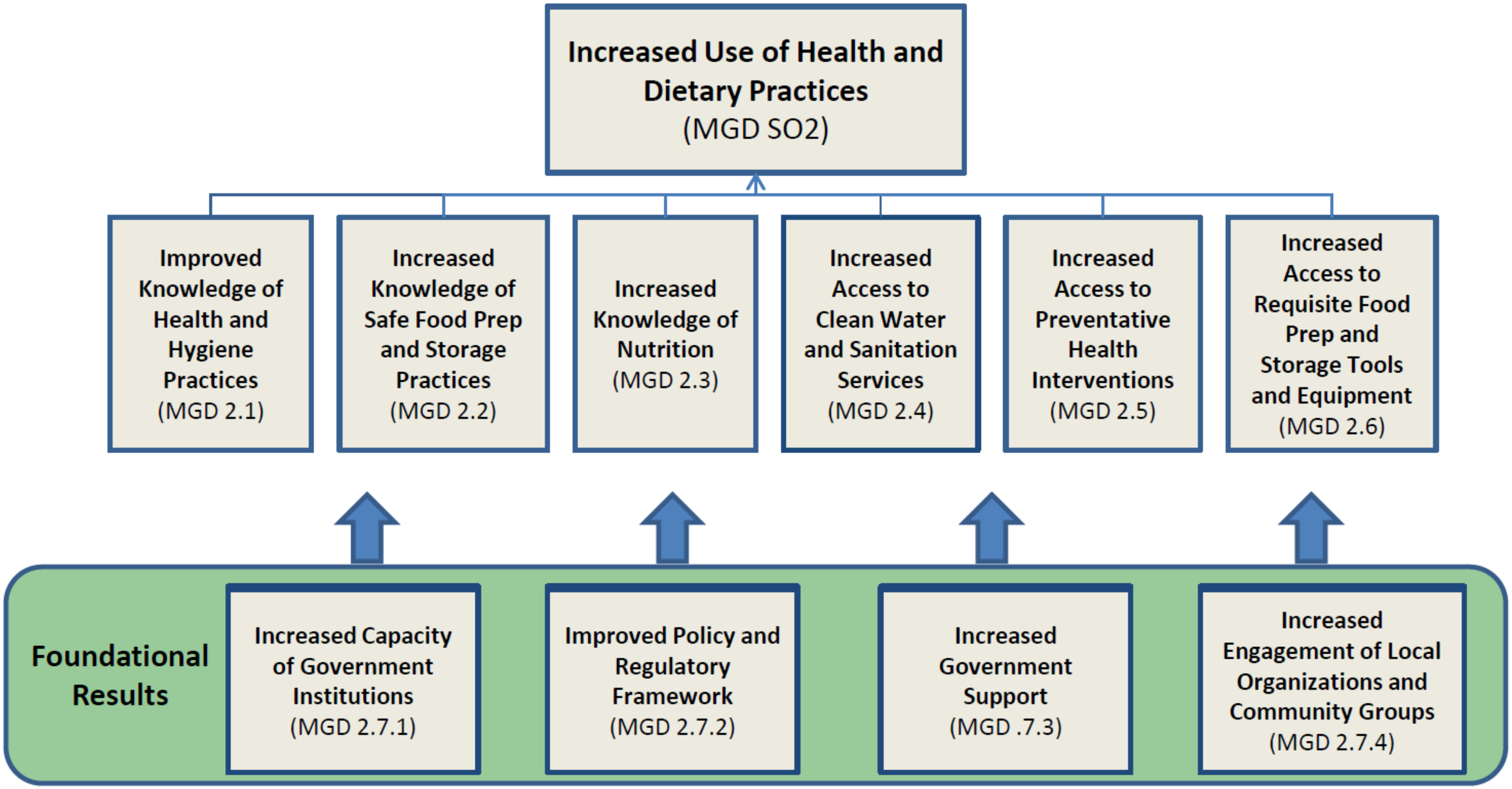
Author	Title	Type of Study	Location	Age/grade	Sample	Intervention	Educational Outcome Measures
Nielsen et al., 2010	WFP Cambodia School Feeding 2000–2010: A Mixed Method Impact Evaluation.	RCT Mixed methods; egressions using various functional specifications	Cambodia	Grades 4-6	Mixed methods approach combined household survey of randomly selected students in ISM, THR and control schools, with data from Ministry of Education annual reports. 108 total schools selected, of which 78 received ISM, THR, or both, and 30 in control group. 15-20 students randomly selected for follow up from each school, and their households surveyed. In total 2,014 households covered, 1,227 sixth graders given standardized tests.	ISM (early morning meal); THR targets poorest students; 37% of schools get combined ISM and THR. Control schools no school feeding. Duration: Ministry of Education annual reports for 2001-2009.	Enrollment, attendance, learning performance
Nkhoma et al. 2013	Early-Stage Primary School Children Attending a School in the Malawian School Feeding Program (SFP) Have Better Reversal Learning and Lean Muscle Mass Growth Than Those Attending a Non-SFP School	Single difference of average outcomes between samples in the SFP and non-SFP schools.	Malawi	6 -8 years	Samples of about 120 students were selected in two schools, one of which offered fortified ISM. 226 school children were followed for one academic year, 114 in the ISM school, and 112 in non-ISM. End survey covered 190 students (100 in ISM) as 36 dropped out (14 ISM and 22 non-ISM).	ISM (intervention), no ISM (control schools). Duration: 2010-2011academic year.	Cognitive tests for memory, reversal learning, and attention
Powell et al., 1998	Nutrition and Education: a Randomized Trial of the Effects of Breakfast in Rural Primary School Children.	RCT (testers were blind to children's' group assignment)	Jamaica rural schools	Grades 2-5	16 primary schools Total sample: 814 children in 16 primary schools. 408 children in treatment group received breakfast, 203 were undernourished (weight/age <= -1 SD) and 205 in the adequately nourished (weight/age > -1 SD). 406 pupils in control group: 204 undernourished and 202 adequately nourished). Children were matched for school and grade.	Breakfast served before class at separate rooms and times. Control children get quarter of an orange. Duration: Measurements done at start and end of 1994 school year.	Reading, spelling, arithmetic, attendance
Simeon, 1998	School Feeding in Jamaica: a Review of Its Evaluation	RCT (testers were blind to the subjects' treatment status)	Jamaica rural schools	First study: 12-13 years in Grade 7; second study: ages 9-10	First study, students in lowest 3 deciles in scholastic ability assigned to 3 groups: 44, received ISM; 33 syrup drink (placebo), and 38 got nothing. Second study, cross-over design with each child with own control: 90 children, 3 groups of 30: wasted; stunted; non-stunted.	First study, ISM=breakfast. Control got either placebo (syrup) or nothing. One year (Sep-Mar), first semester used as baseline. Second study, full dinner, next morning full breakfast on first visit and tea on second or vice versa.	Arithmetic, spelling, reading, attendance, cognitive tests
Vermeersch & Kremer, 2004	School Meals, Educational Achievement, and School Competition: Evidence from a Randomized Evaluation.	RCT	Kenya, western region.	4-6 years	25 of a pool of 50 schools were randomly selected for treatment, the other 25 were control. Within school, 30 children selected for testing, with replacements if student absent.	Pupils in treatment schools provided free school breakfast Duration: 2000 to 2002.	Enrollment, attendance, test scores, oral curriculum and written curriculum

Annex 2: The MGD Results Framework



A Note on Foundational Results: These results can feed into one or more higher-level results. Causal relationships sometimes exist between foundational results.

McGovern-Dole Results Framework #2



A Note on Foundational Results: These results can feed into one or more higher-level results. Causal relationships sometimes exist between foundational results.

Annex 3: Analysis of Publication Bias

Publication bias refers to the selective publication of studies with a particular outcome --- the greater likelihood that studies with positive results will be published, with the result that most treatments tend to be less effective in practice than the research suggests (see, for instance, Dickersin 1990 or Ferguson et al. 2012). Small studies are at the greatest risk of being lost because, with small samples, only very large effects are likely to be significant and those with small and moderate effects are likely to be unpublished. Large studies are likely to be published regardless of statistical significance.

Funnel plots and Egger tests (Egger et al. 1997) enable the quantification of publication bias. Funnel plots provide a graphical depiction of publication bias, based on the rationale that small studies are more likely to be unreported than large studies, a phenomenon referred to as the "file drawer problem." The y-axis, showing the standard error corresponding to sample size, is inverted with large studies measured at the top (see funnel plots below). The asymmetry in the plot, as highlighted by the lack of small sample studies which report findings below the average effect at the vertical line, suggests evidence for publication bias.

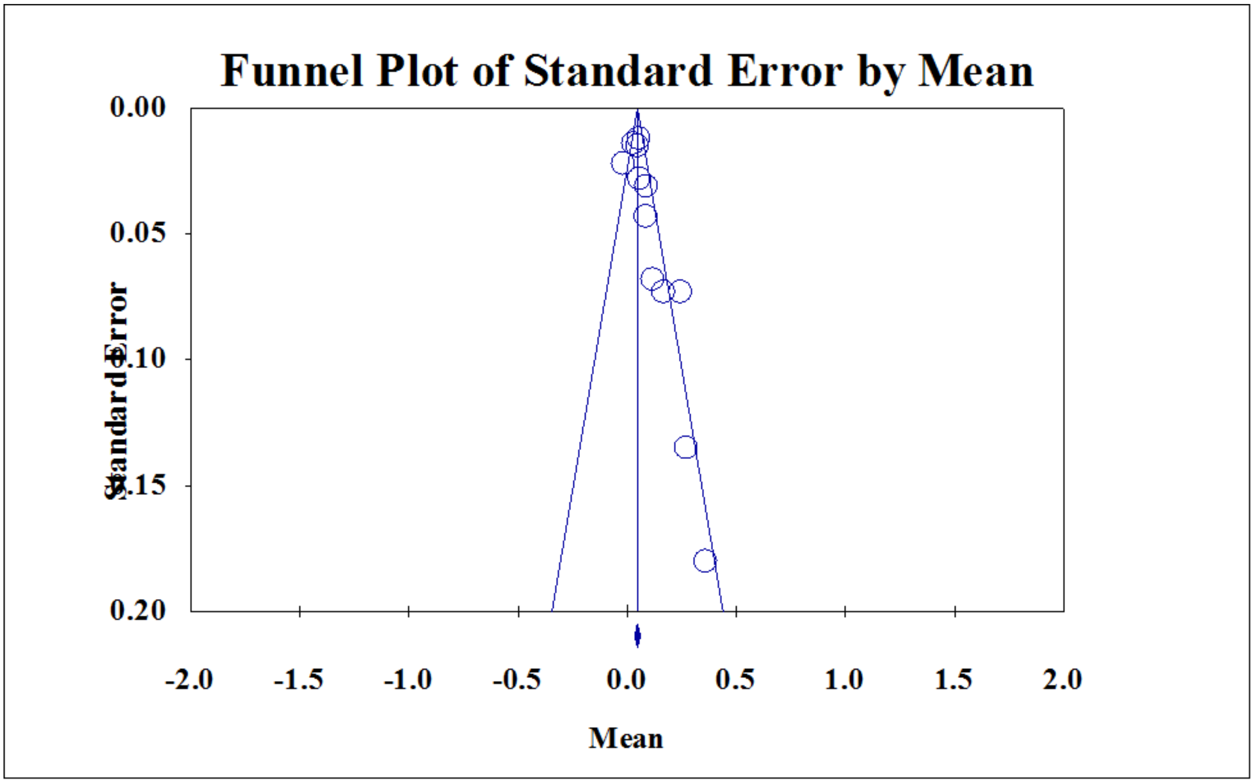
In the absence of publication bias the studies will be distributed symmetrically throughout the scatter plot. In the possible presence of bias, the bottom of the plot would tend to show a higher concentration of studies on one side of the plot than the other. The funnel plot can also be used to identify outliers -- observations that are numerically distant from the rest of the data. Identification of outliers in meta-analysis can be used to conduct sensitivity analysis (with and without outliers).

Given the difficulties in accurately assessing asymmetry by visual inspection, statistical tests are recommended. The most widely used statistical test is Egger's test. Egger's test is based on two variables: (i) normalized effect estimate (meta-analysis estimate divided by its standard error), and (ii) precision (reciprocal of the standard error of the estimate). The test is based on a simple linear regression to test for intercept $\beta 0=0$; i.e., the null hypothesis that intercept $b=0$ (or the null hypothesis that there is no funnel plot asymmetry). In this case the regression line will run through the origin. If the intercept b deviates from zero (the origin), the deviation provides a measure of asymmetry -- the larger the deviation from zero, the larger the asymmetry. (It is for this reason that Egger's test is also referred to as "Egger's test of the intercept.")

The following two plots are from a biased and unbiased analysis, as reflected in their corresponding funnel plots and Egger's test statistics.

Example of a biased analysis (effect of take-home rations on school attendance for all children described in this study):

- The effect sizes are not symmetrically distributed
- The Egger's test shows that the intercept (at 1.81745) is statistically different from zero (P-value = 0.01447)

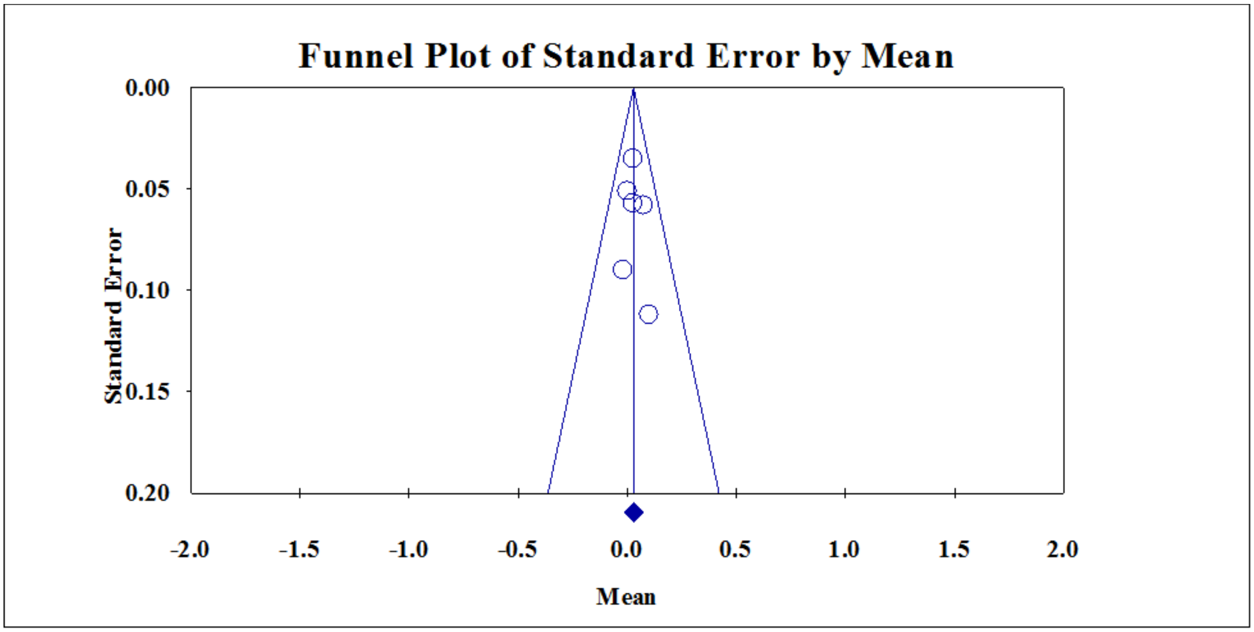


Egger's regression intercept

Intercept	1.81745
Standard error	0.61551
95% lower limit (2-tailed)	0.44600
95% upper limit (2-tailed)	3.18891
t-value	2.95274
df	10.00000
P-value (1-tailed)	0.00723
P-value (2-tailed)	0.01447

Example of an unbiased analysis (effect of in-school meals on cognitive development for all children described in this study):

- The effect sizes are symmetrically distributed
- The Egger's test shows that the intercept (at 0.32068) is not statistically different from zero (P-value = 0.68674)



Egger's regression intercept

Intercept	0.32068
Standard error	0.73907
95% lower limit (2-tailed)	-1.73132
95% upper limit (2-tailed)	2.37268
t-value	0.43390
df	4.00000
P-value (1-tailed)	0.34337
P-value (2-tailed)	0.68674

Assessing publication bias involves: (1) broadening the search to the non-published "grey literature" to reduce the bias; and (2) conducting sensitivity analysis. The present meta-analysis has made every attempt to minimize the publication bias by conducting a thorough search for non-published studies that included conference proceedings, technical reports, dissertations, and theses. Despite this effort, the funnel plots and Egger's tests presented in Annex 4 indicate that publication bias could not always be eliminated.

Assessing publication bias can also be conducted through imputation of missing studies by using "trim and fill" analysis -- a sensitivity analysis method that extends beyond the scope of this study. Another method of assessing the potential for publication bias is to calculate the "fail-safe N," the number of studies whose effect size is zero or negative that would be needed to increase the P-value for the meta-analysis to above 0.05. However, the Cochrane Handbook for Systematic Reviews of Interventions notes that "this and other methods are not recommended for use in Cochrane reviews" (Higgins et al. 2014).

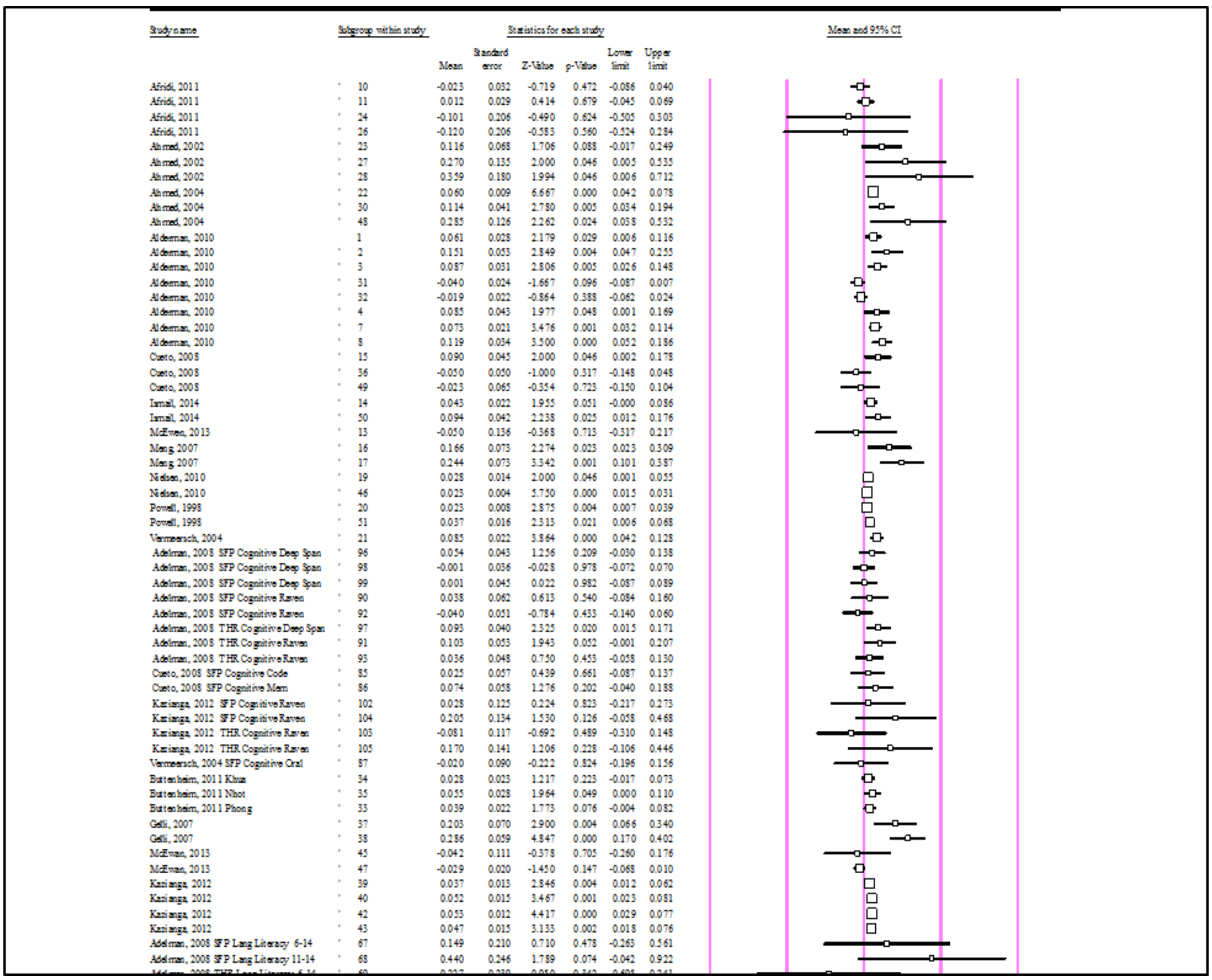
It is very important to note, however, that the presence of publication bias means that the pooled effect sizes may be overestimated and the response ratio effect size estimated by trim and fill corresponds to a reduction in average effect size. Since the school feeding effect sizes estimated in this meta-analysis are (when statistically significant) consistently "very small," the trim and fill analysis are expected to make those effect sizes even smaller --- with no major implications on the conclusions and learning agenda presented in this study.

Annex 4: Technical Data Used for Analysis: Forest Plots, Funnel Plots, Egger's Tests and Detailed Statistics

Data in this annex were used to derive the findings in Section 3.0 (empirical evidence) and Annex 1 (analysis of publication bias). The annex, which served as a basis for constructing the tables in Section 3.0, provides detailed statistics of effect sizes, including standard errors, t-values, degrees of freedom, confidence intervals, statistical significance, heterogeneity statistics, funnel plots and Egger's tests. Number of studies in the statistical tables below refers to the number of effect sizes, not the number of studies themselves.

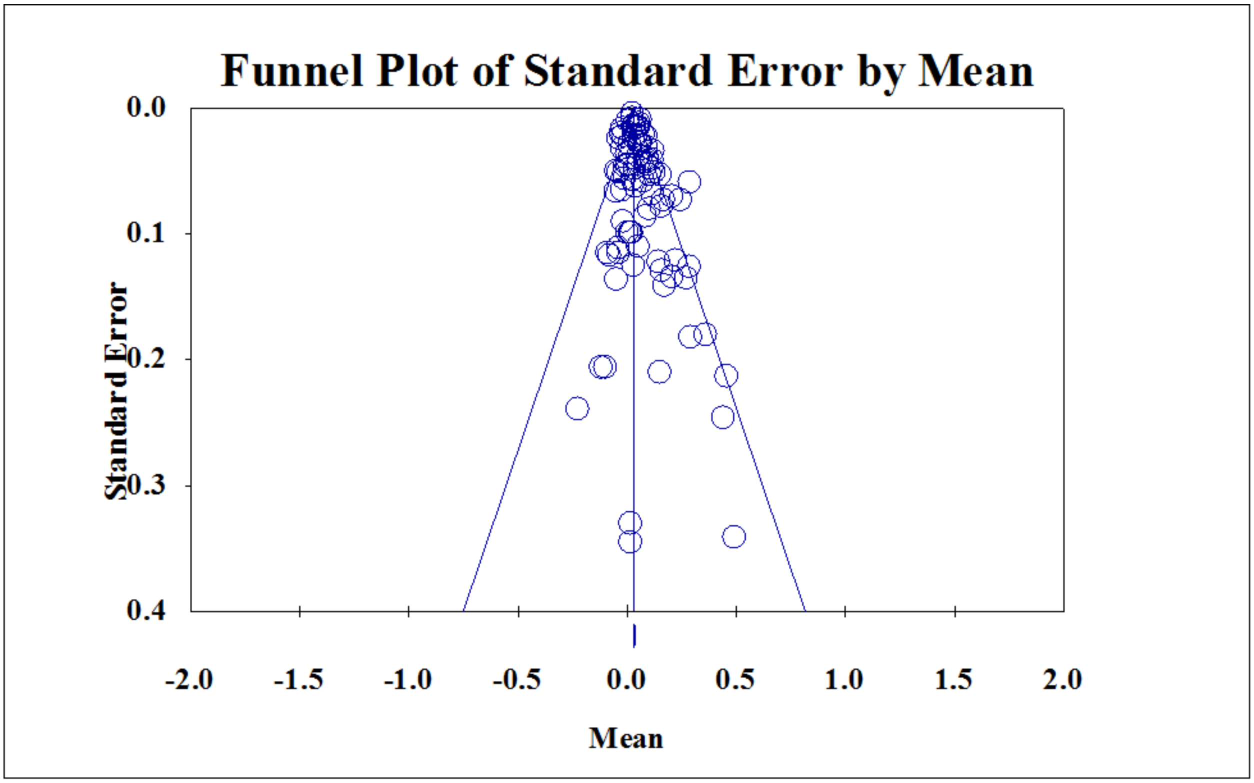
Pooled Effect Sizes for all Educational Outcomes, by Type of School Feeding and Target Group
All children

Forest plot



Effect size and significance				
Model	Number Studies	Point estimate	Significance	Standard error
Random effects	88.000	0.043	***	0.005

Heterogeneity				Tau-squared			
Q-value	df (Q)	P-value	I-squared	Tau Squared	Standard Error	Variance	Tau
201.257	87.000	-	56.772	0.001	0.000	0.000	0.028

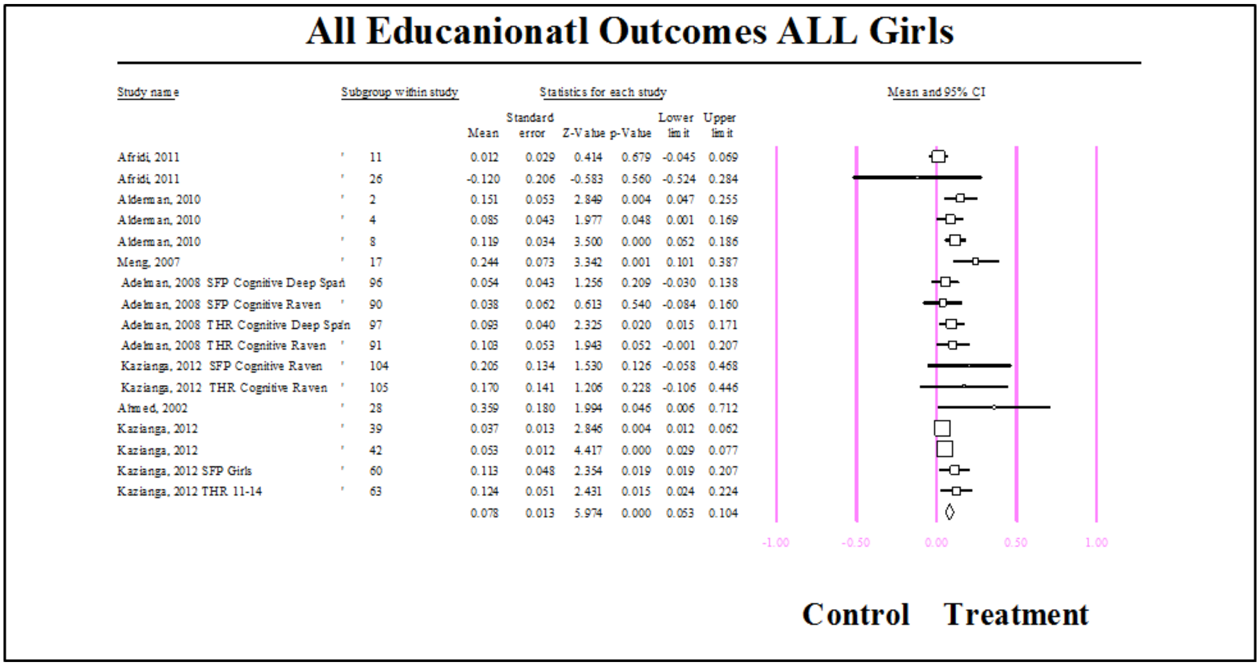


Egger's regression intercept

Intercept	0.64411
Standard error	0.20085
95% lower limit (2-tailed)	0.24484
95% upper limit (2-tailed)	1.04338
t-value	3.20699
df	86.00000
P-value (1-tailed)	0.00094
P-value (2-tailed)	0.00188

Pooled Effect Sizes for all Educational Outcomes, by Type of School Feeding and Target Group
Girls only

Forest plot

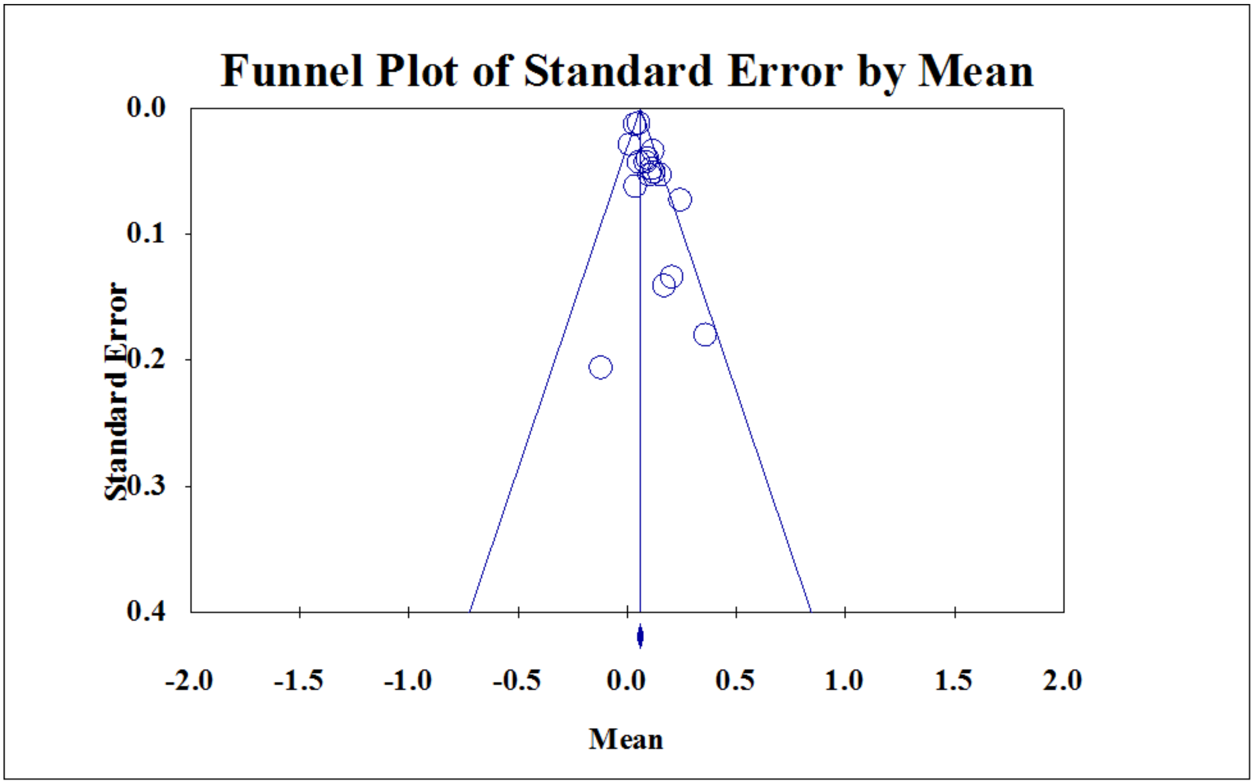


All Educanionatl Outcomes ALL Girls

Study name	Subgroup within study		Mean	Standard error	Z-Value	p-Value	Lower limit	Upper limit
Afridi, 2011	'	11	0.012	0.029	0.414	0.679	-0.045	0.069
Afridi, 2011	'	26	-0.120	0.206	-0.583	0.560	-0.524	0.284
Alderman, 2010	'	2	0.151	0.053	2.849	0.004	0.047	0.255
Alderman, 2010	'	4	0.085	0.043	1.977	0.048	0.001	0.169
Alderman, 2010	'	8	0.119	0.034	3.500	0.000	0.052	0.186
Meng, 2007	'	17	0.244	0.073	3.342	0.001	0.101	0.387
Adelman, 2008 SFP Cognitive Deep Span	'	96	0.054	0.043	1.256	0.209	-0.030	0.138
Adelman, 2008 SFP Cognitive Raven	'	90	0.038	0.062	0.613	0.540	-0.084	0.160
Adelman, 2008 THR Cognitive Deep Span	'	97	0.093	0.040	2.325	0.020	0.015	0.171
Adelman, 2008 THR Cognitive Raven	'	91	0.103	0.053	1.943	0.052	-0.001	0.207
Kazianga, 2012 SFP Cognitive Raven	'	104	0.205	0.134	1.530	0.126	-0.058	0.468
Kazianga, 2012 THR Cognitive Raven	'	105	0.170	0.141	1.206	0.228	-0.106	0.446
Ahmed, 2002	'	28	0.359	0.180	1.994	0.046	0.006	0.712
Kazianga, 2012	'	39	0.037	0.013	2.846	0.004	0.012	0.062
Kazianga, 2012	'	42	0.053	0.012	4.417	0.000	0.029	0.077
Kazianga, 2012 SFP Girls	'	60	0.113	0.048	2.354	0.019	0.019	0.207
Kazianga, 2012 THR 11-14	'	63	0.124	0.051	2.431	0.015	0.024	0.224
			0.078	0.013	5.974	0.000	0.053	0.104

Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	17.000	0.078	***	0.013

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
28.425	16.000	0.028	43.712	0.001	0.001	0.000	0.030



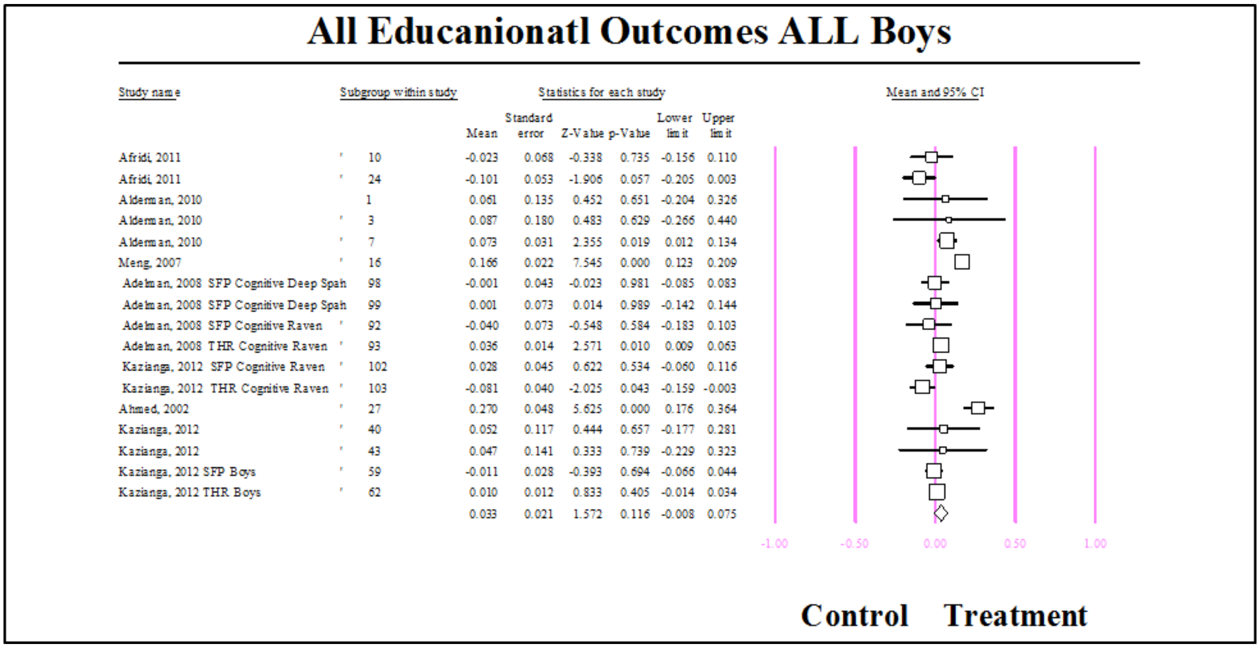
Egger's regression intercept

Intercept	1.26103
Standard error	0.39614
95% lower limit (2-tailed)	0.41668
95% upper limit (2-tailed)	2.10538
t-value	3.18332
df	15.00000
P-value (1-tailed)	0.00309
P-value (2-tailed)	0.00617

Pooled Effect Sizes for all Educational Outcomes, by Type of School Feeding and Target Group

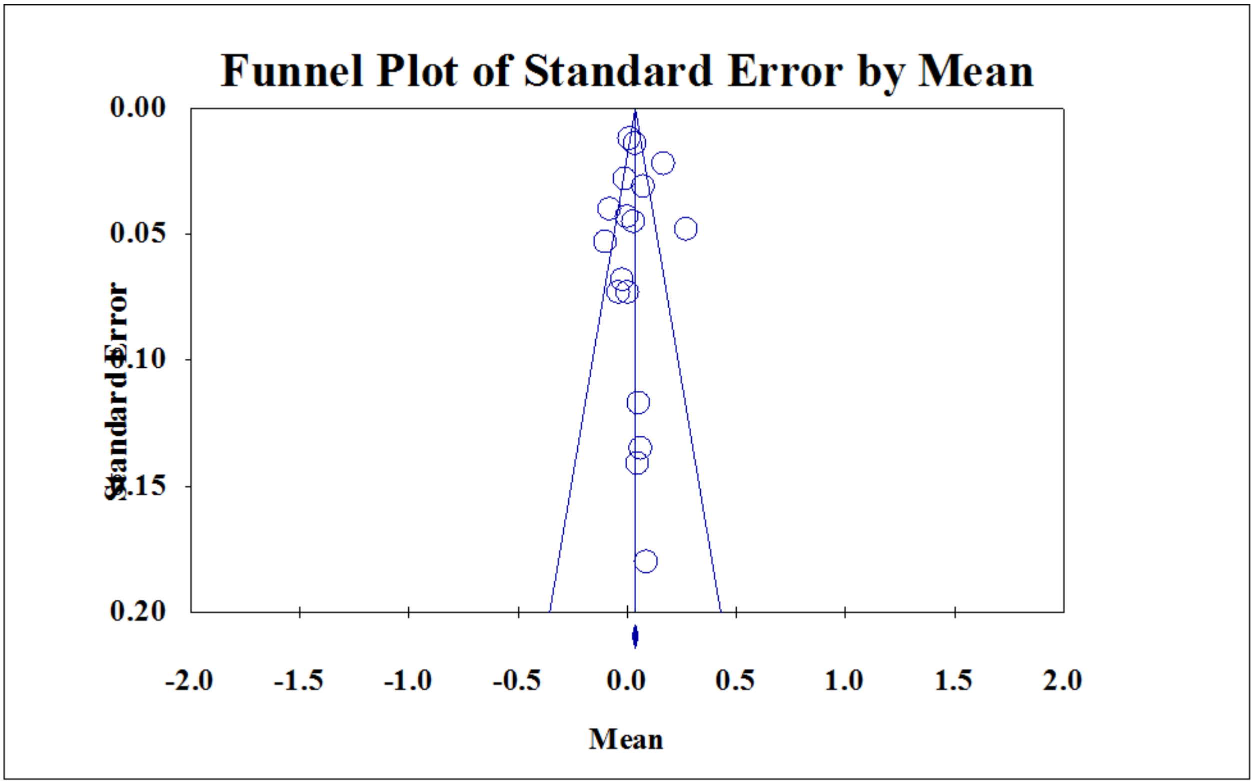
Boys only

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	17.000	0.033	-	0.021

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
85.833	16.000	0.000	81.359	0.005	0.003	0.000	0.067

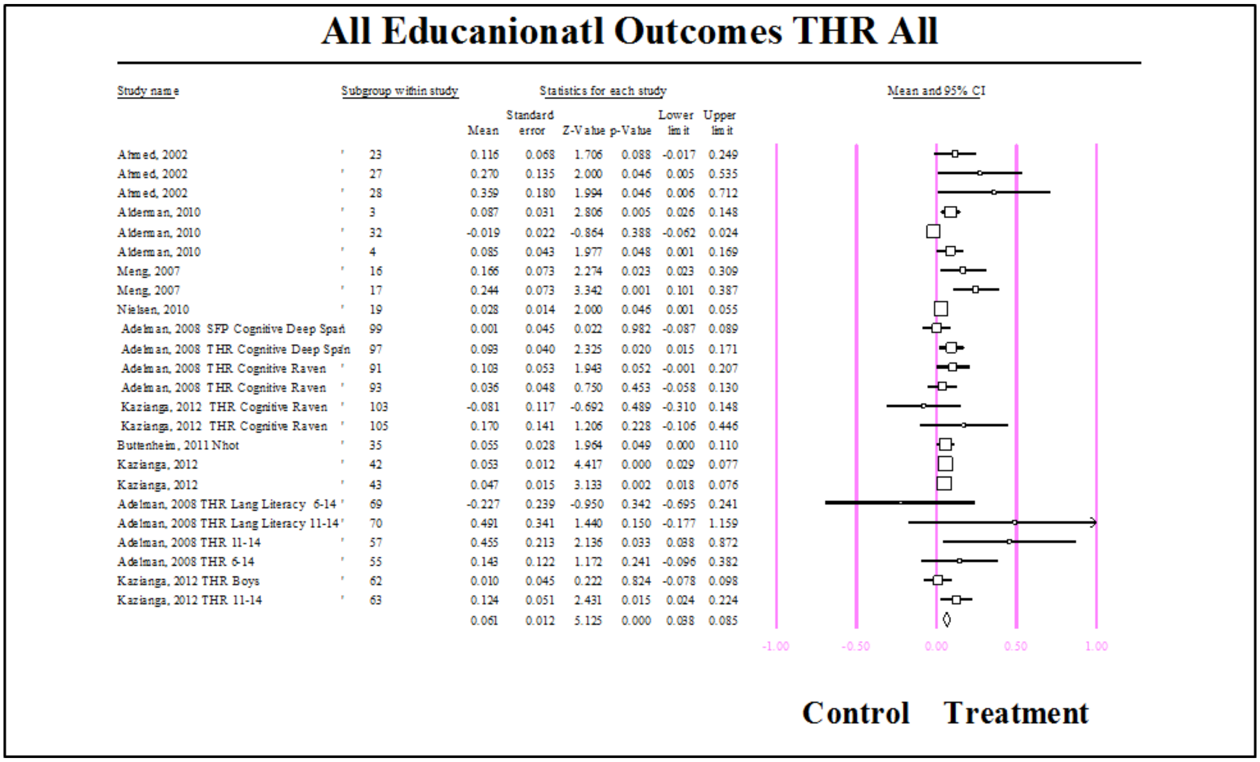


Egger's regression intercept

Intercept	0.00452
Standard error	0.92057
95% lower limit (2-tailed)	-1.95763
95% upper limit (2-tailed)	1.96667
t-value	0.00491
df	15.00000
P-value (1-tailed)	0.49807
P-value (2-tailed)	0.99615

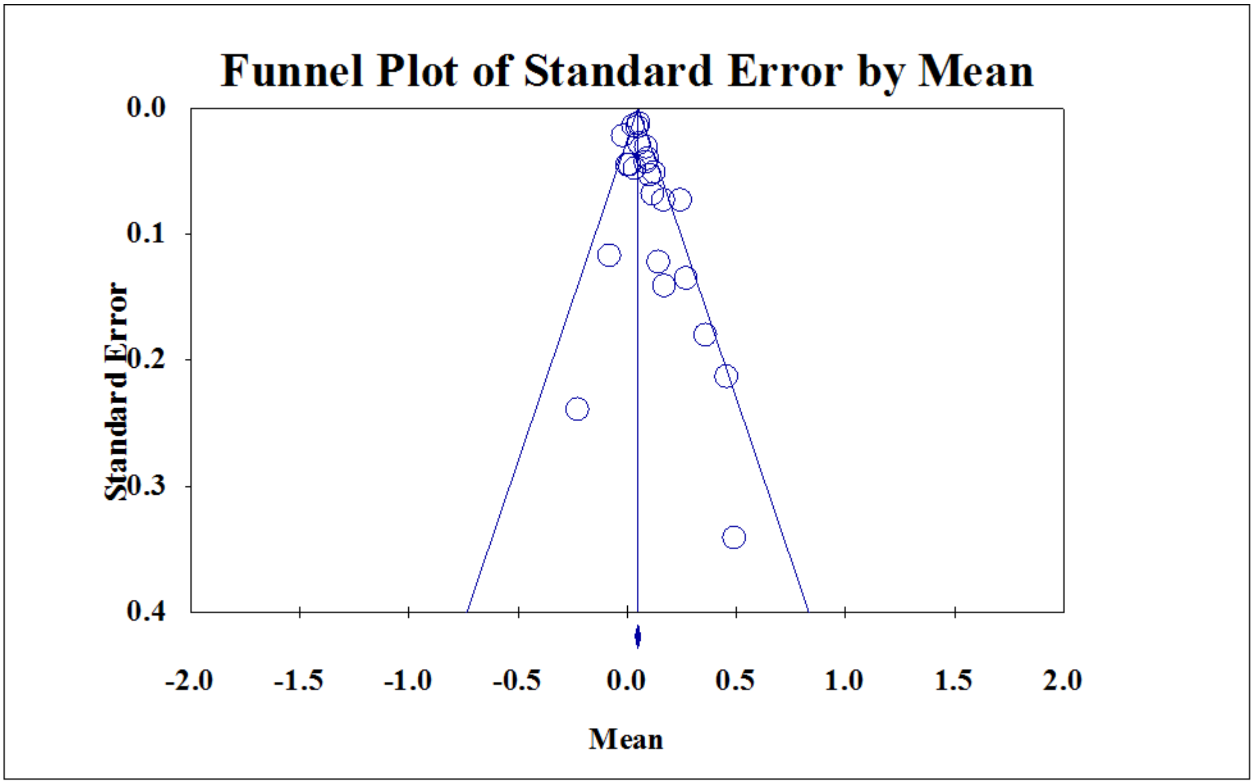
Pooled Effect Sizes for all Educational Outcomes, by Type of School Feeding and Target Group
Take-home rations only
All children

Forest plot



Effect size and significance				
Model	Number Studies	Point estimate	Significance	Standard error
Random effects	24.000	0.061	***	0.012

Heterogeneity				Tau-squared			
Q-value	df (Q)	P-value	I-squared	Tau Squared	Standard Error	Variance	Tau
46.053	23.000	0.003	50.057	0.001	0.001	0.000	0.033



Egger's regression intercept

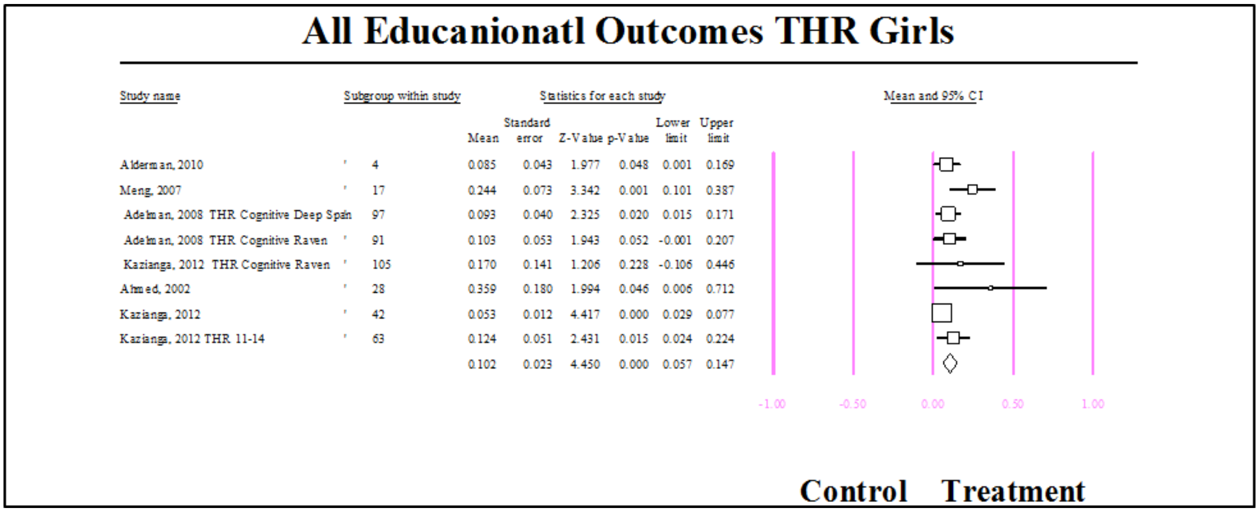
Intercept	1.03891
Standard error	0.38241
95% lower limit (2-tailed)	0.24583
95% upper limit (2-tailed)	1.83199
t-value	2.71670
df	22.00000
P-value (1-tailed)	0.00630
P-value (2-tailed)	0.01260

Pooled Effect Sizes for all Educational Outcomes, by Type of School Feeding and Target Group

Take-home rations only

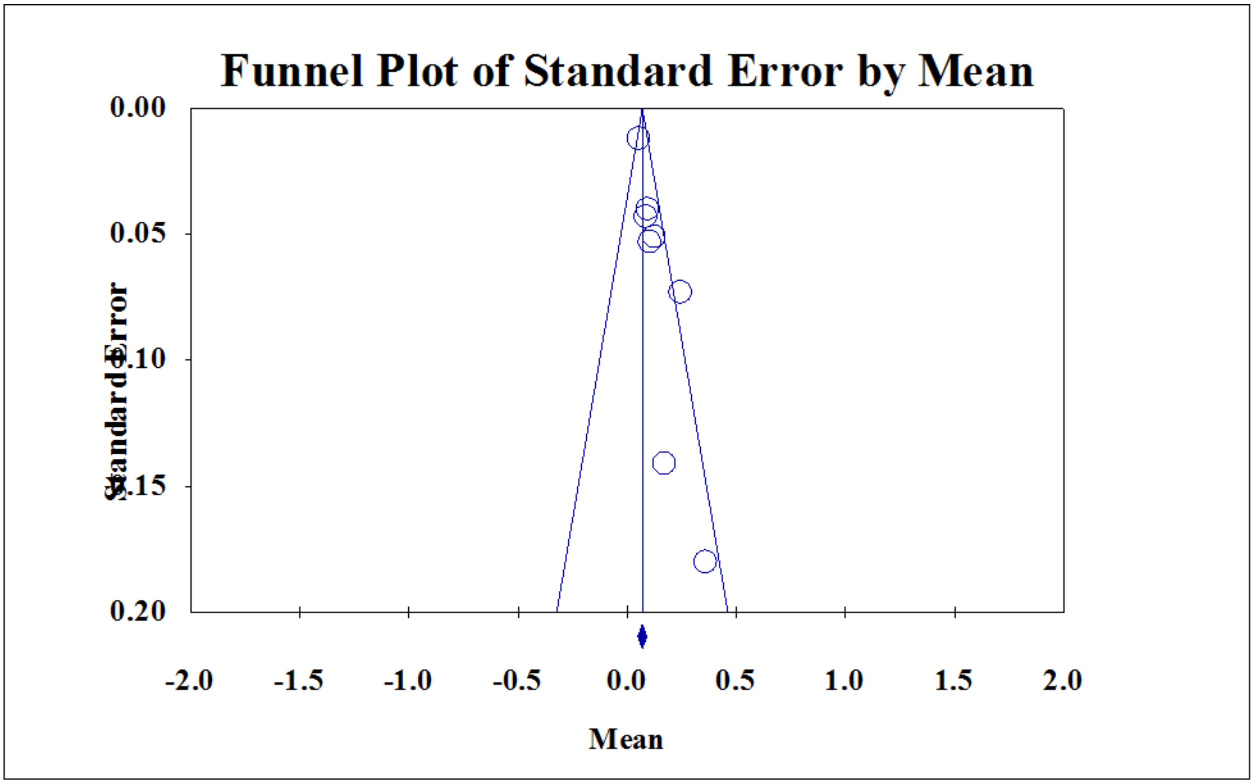
Girls only

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	8.000	0.102	***	0.023

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
12.701	7.000	0.080	44.884	0.002	0.002	0.000	0.039



Egger's regression intercept

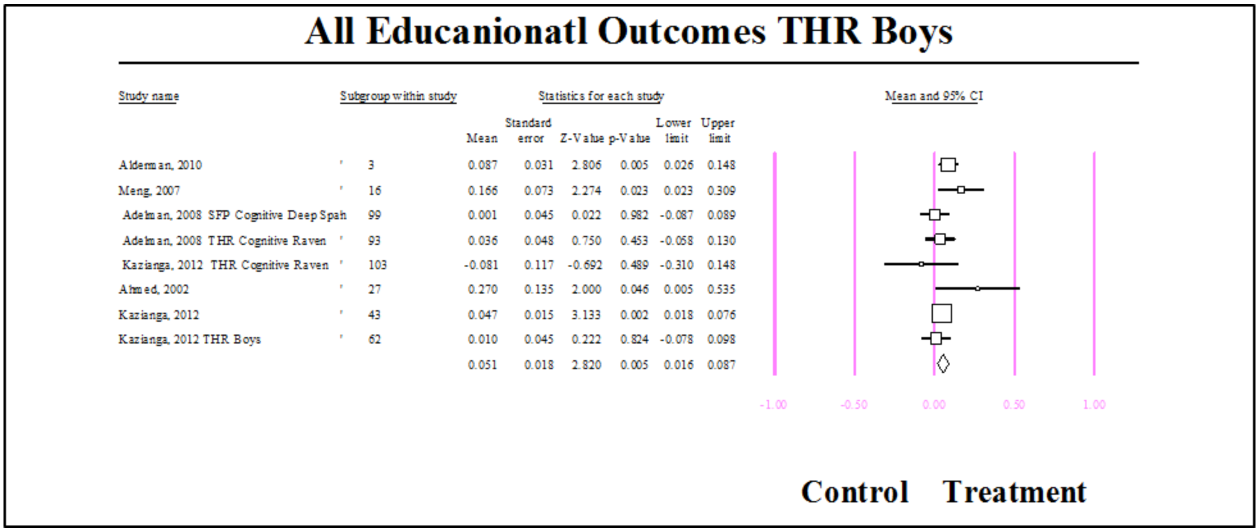
Intercept	1.65563
Standard error	0.32349
95% lower limit (2-tailed)	0.86407
95% upper limit (2-tailed)	2.44719
t-value	5.11798
df	6.00000
P-value (1-tailed)	0.00109
P-value (2-tailed)	0.00218

Pooled Effect Sizes for all Educational Outcomes, by Type of School Feeding and Target Group

Take-home rations only

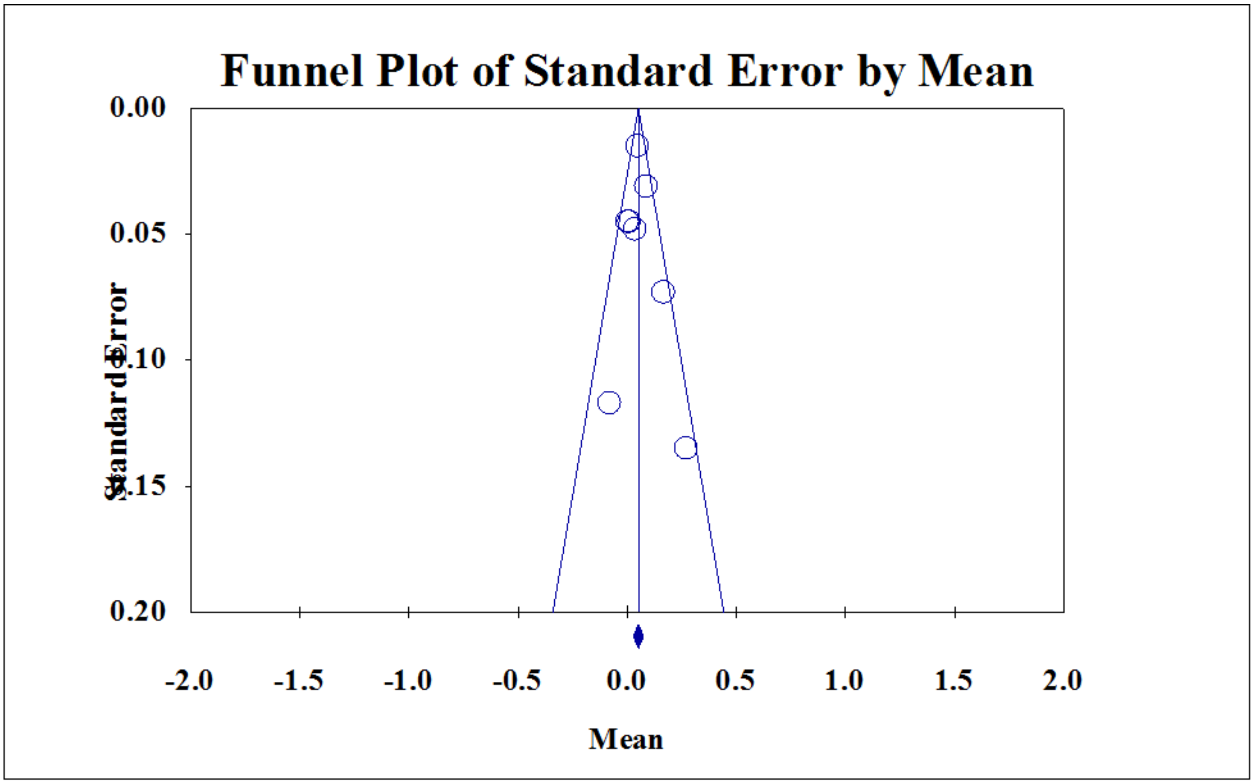
Boys only

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	8.000	0.051	***	0.018

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
9.960	7.000	0.191	29.717	0.001	0.001	0.000	0.026



Egger's regression intercept

Intercept	0.29998
Standard error	0.76063
95% lower limit (2-tailed)	-1.56121
95% upper limit (2-tailed)	2.16116
t-value	0.39438
df	6.00000
P-value (1-tailed)	0.35347
P-value (2-tailed)	0.70693

Pooled Effect Sizes for all Educational Outcomes, by Type of School Feeding and Target Group

In-school feeding

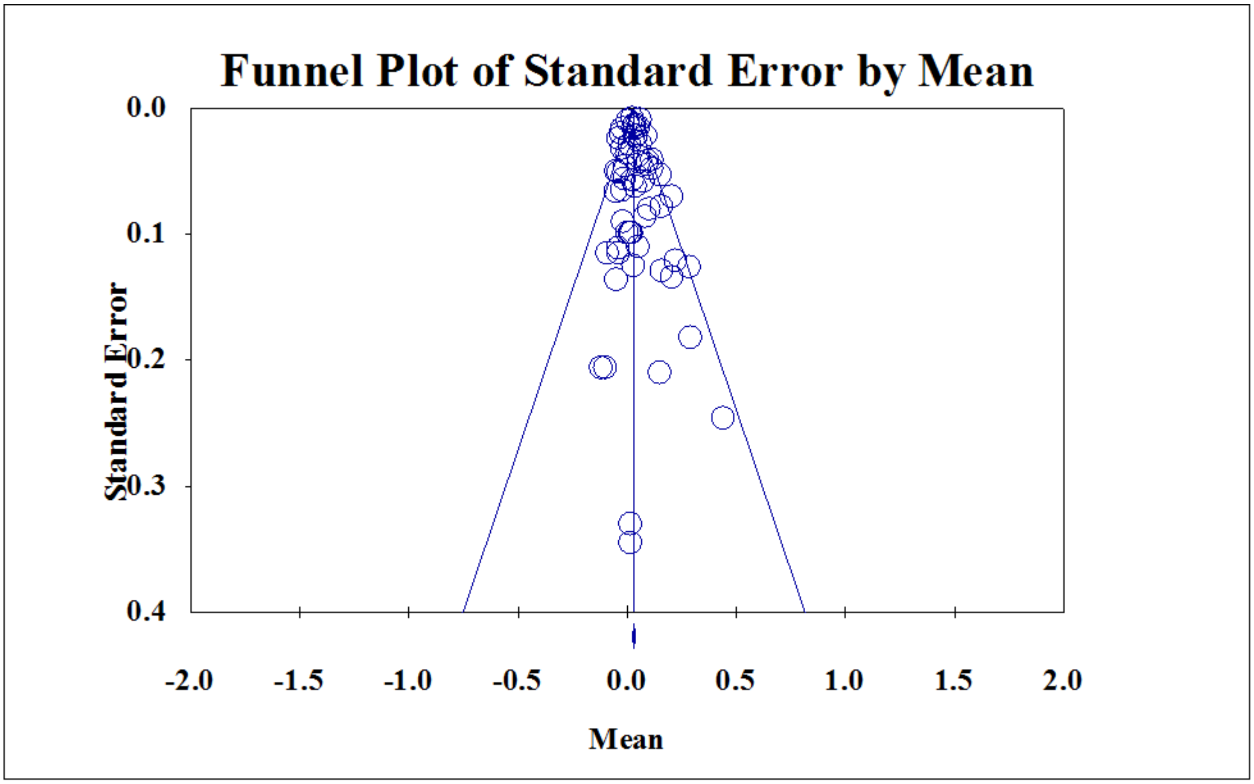
All children

Forest plot

Study name	Subgroup within study	Mean	Standard error	Z-Value	p-Value	Lower limit	Upper limit
Afridi, 2011	10	-0.023	0.032	-0.719	0.472	-0.086	0.040
Afridi, 2011	11	0.012	0.029	0.414	0.679	-0.045	0.069
Afridi, 2011	24	-0.101	0.206	-0.490	0.624	-0.505	0.303
Afridi, 2011	26	-0.120	0.206	-0.583	0.560	-0.524	0.284
Ahmed, 2004	22	0.060	0.009	6.667	0.000	0.042	0.078
Ahmed, 2004	30	0.114	0.041	2.780	0.005	0.034	0.194
Ahmed, 2004	48	0.285	0.126	2.262	0.024	0.038	0.532
Alderman, 2010	1	0.061	0.028	2.179	0.029	0.006	0.116
Alderman, 2010	2	0.151	0.053	2.849	0.004	0.047	0.255
Alderman, 2010	31	-0.040	0.024	-1.667	0.096	-0.087	0.007
Cueto, 2008	15	0.090	0.045	2.000	0.046	0.002	0.178
Cueto, 2008	36	-0.050	0.050	-1.000	0.317	-0.148	0.048
Cueto, 2008	49	-0.023	0.065	-0.354	0.723	-0.150	0.104
Ismail, 2014	14	0.043	0.022	1.955	0.051	-0.000	0.086
Ismail, 2014	50	0.094	0.042	2.238	0.025	0.012	0.176
McEwen, 2013	13	-0.050	0.136	-0.368	0.713	-0.317	0.217
Powell, 1998	20	0.023	0.008	2.875	0.004	0.007	0.039
Powell, 1998	51	0.037	0.016	2.313	0.021	0.006	0.068
Vermeersch, 2004	21	0.085	0.022	3.864	0.000	0.042	0.128
Adelman, 2008 SFP Cognitive Deep Span	96	0.054	0.043	1.256	0.209	-0.030	0.138
Adelman, 2008 SFP Cognitive Deep Span	98	-0.001	0.036	-0.028	0.978	-0.072	0.070
Adelman, 2008 SFP Cognitive Raven	90	0.038	0.062	0.613	0.540	-0.084	0.160
Adelman, 2008 SFP Cognitive Raven	92	-0.040	0.051	-0.784	0.433	-0.140	0.060
Cueto, 2008 SFP Cognitive Code	85	0.025	0.057	0.439	0.661	-0.087	0.137
Cueto, 2008 SFP Cognitive Mem	86	0.074	0.058	1.276	0.202	-0.040	0.188
Kazianga, 2012 SFP Cognitive Raven	102	0.028	0.125	0.224	0.823	-0.217	0.273
Kazianga, 2012 SFP Cognitive Raven	104	0.205	0.134	1.530	0.126	-0.058	0.468
Vermeersch, 2004 SFP Cognitive Oral	87	-0.020	0.090	-0.222	0.824	-0.196	0.156
Buttenheim, 2011 Phong	33	0.039	0.022	1.773	0.076	-0.004	0.082
Gelli, 2007	37	0.203	0.070	2.900	0.004	0.066	0.340
McEwan, 2013	45	-0.042	0.111	-0.378	0.705	-0.260	0.176
McEwan, 2013	47	-0.029	0.020	-1.450	0.147	-0.068	0.010
Kazianga, 2012	39	0.037	0.013	2.846	0.004	0.012	0.062
Kazianga, 2012	40	0.052	0.015	3.467	0.001	0.023	0.081
Adelman, 2008 SFP Lang Literacy 6-14	67	0.149	0.210	0.710	0.478	-0.263	0.561
Adelman, 2008 SFP Lang Literacy 11-14	68	0.440	0.246	1.789	0.074	-0.042	0.922
Ahmed, 2004 SFP Lang All Subjects	84	0.157	0.078	2.013	0.044	0.004	0.310
Ahmed, 2004 SFP Lang Bangla	64	0.049	0.038	1.289	0.197	-0.025	0.123
Ahmed, 2004 SFP Lang English	65	0.222	0.121	1.835	0.067	-0.015	0.459
Cueto, 2008 SFP Lang Reading G4	72	-0.013	0.056	-0.232	0.816	-0.123	0.097
Cueto, 2008 SFP Lang Reading G9	73	-0.040	0.115	-0.348	0.728	-0.265	0.185
Cueto, 2008 SFP Lang Vocab	81	-0.052	0.066	-0.788	0.431	-0.181	0.077
Cueto, 2008 SFP Lang Vocab SDM	82	-0.091	0.115	-0.791	0.429	-0.316	0.134
Ismail, 2014 SFP Lang English	66	0.083	0.086	0.965	0.334	-0.086	0.252
McEwan, 2013 SFP Language	80	-0.022	0.016	-1.375	0.169	-0.053	0.009
Powell, 1998 SFP Lang Reading <2SD	74	0.000	0.099	0.000	1.000	-0.194	0.194
Powell, 1998 SFP Lang Reading >2SD	75	0.013	0.099	0.131	0.896	-0.181	0.207
Powell, 1998 SFP Lang Reading Single Diff	76	0.004	0.010	0.400	0.689	-0.016	0.024
Powell, 1998 SFP Lang Spelling <2SD	77	0.019	0.099	0.192	0.848	-0.175	0.213
Powell, 1998 SFP Lang Spelling >2SD	78	0.020	0.099	0.202	0.840	-0.174	0.214
Powell, 1998 SFP Lang Spelling Single Diff	79	0.028	0.015	1.867	0.062	-0.001	0.057
Vermeersch, 2004 SFP Lang Oral	71	0.100	0.080	1.250	0.211	-0.057	0.257
Vermeersch, 2004 SFP Lang Written	83	0.050	0.110	0.455	0.649	-0.166	0.266
Adelman, 2008 SFP 11-14	56	0.290	0.182	1.593	0.111	-0.067	0.647
Adelman, 2008 SFP 6-14	54	0.158	0.129	1.225	0.221	-0.095	0.411
Powell, 1998 DID not ok	53	0.015	0.345	0.043	0.965	-0.661	0.691
Powell, 1998 DID ok	52	0.015	0.330	0.045	0.964	-0.632	0.662
Kazianga, 2012 SFP Boys	59	-0.011	0.046	-0.239	0.811	-0.101	0.079

Effect size and significance				
Model	Number Studies	Point estimate	Significance	Standard error
Random effects	59.000	0.033	***	0.007

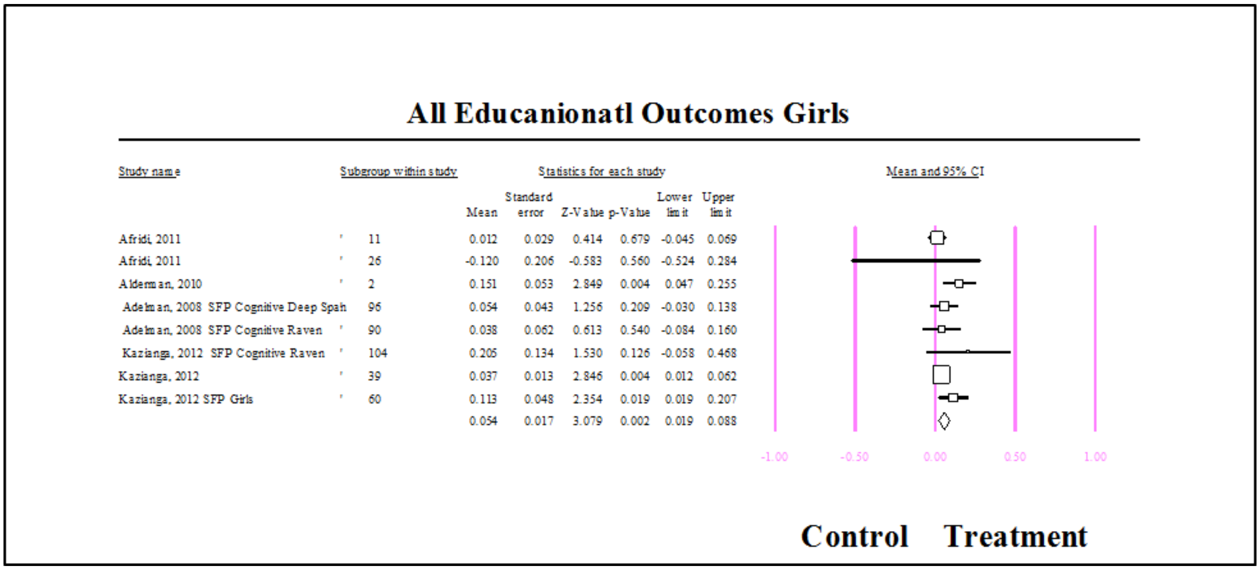
Heterogeneity				Tau-squared			
Q-value	df (Q)	P-value	I-squared	Tau Squared	Standard Error	Variance	Tau
114.635	58.000	0.000	49.404	0.001	0.000	0.000	0.027



Egger's regression intercept	
Intercept	0.28309
Standard error	0.25696
95% lower limit (2-tailed)	-0.23147
95% upper limit (2-tailed)	0.79766
t-value	1.10168
df	57.00000
P-value (1-tailed)	0.13762
P-value (2-tailed)	0.27523

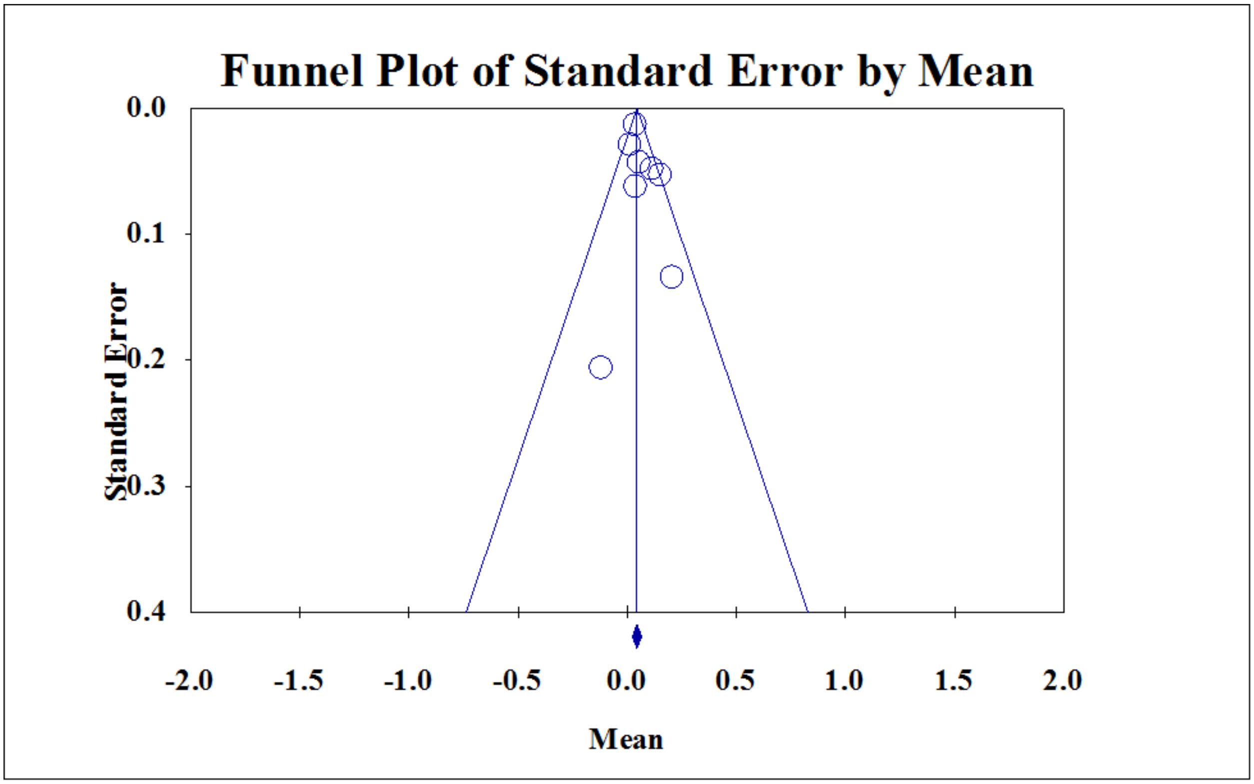
Pooled Effect Sizes for all Educational Outcomes, by Type of School Feeding and Target Group
In-school feeding
Girls only

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	8.000	0.054	***	0.017

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
9.790	7.000	0.201	28.498	0.001	0.001	0.000	0.025

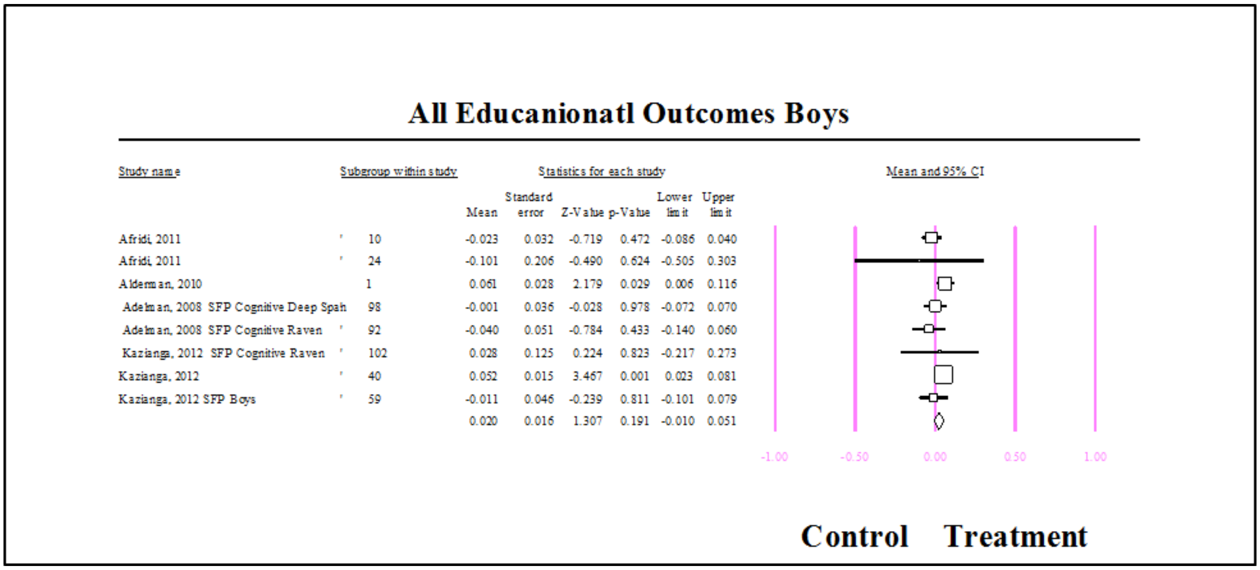


Egger's regression intercept

Intercept	0.72823
Standard error	0.63565
95% lower limit (2-tailed)	-0.82714
95% upper limit (2-tailed)	2.28360
t-value	1.14565
df	6.00000
P-value (1-tailed)	0.14778
P-value (2-tailed)	0.29557

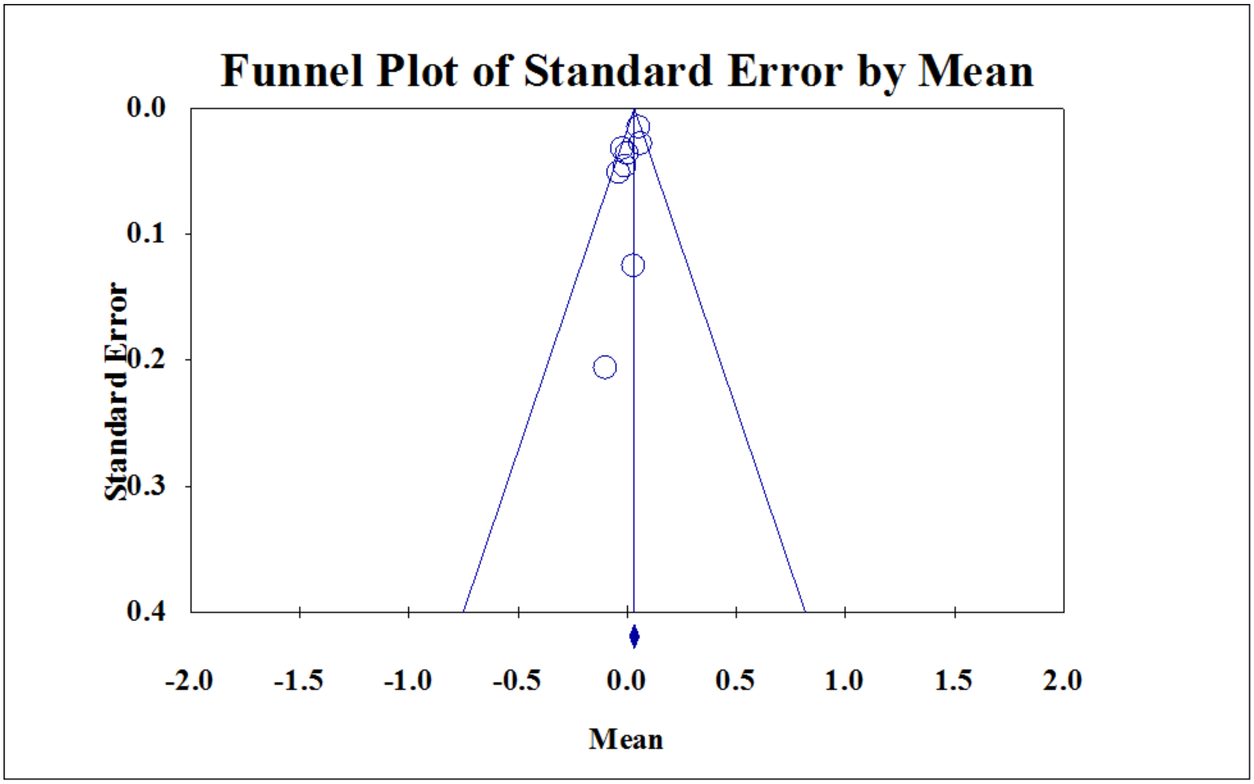
Pooled Effect Sizes for all Educational Outcomes, by Type of School Feeding and Target Group
In-school feeding
Boys only

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	8.000	0.020	-	0.016

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
9.927	7.000	0.193	29.485	0.001	0.001	0.000	0.023



Egger's regression intercept

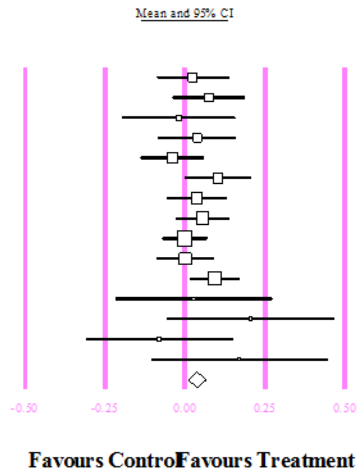
Intercept	-1.28141
Standard error	0.62853
95% lower limit (2-tailed)	-2.81937
95% upper limit (2-tailed)	0.25654
t-value	2.03875
df	6.00000
P-value (1-tailed)	0.04380
P-value (2-tailed)	0.08760

Effect Size of School Feeding on Cognitive Development
All children

Forest plot

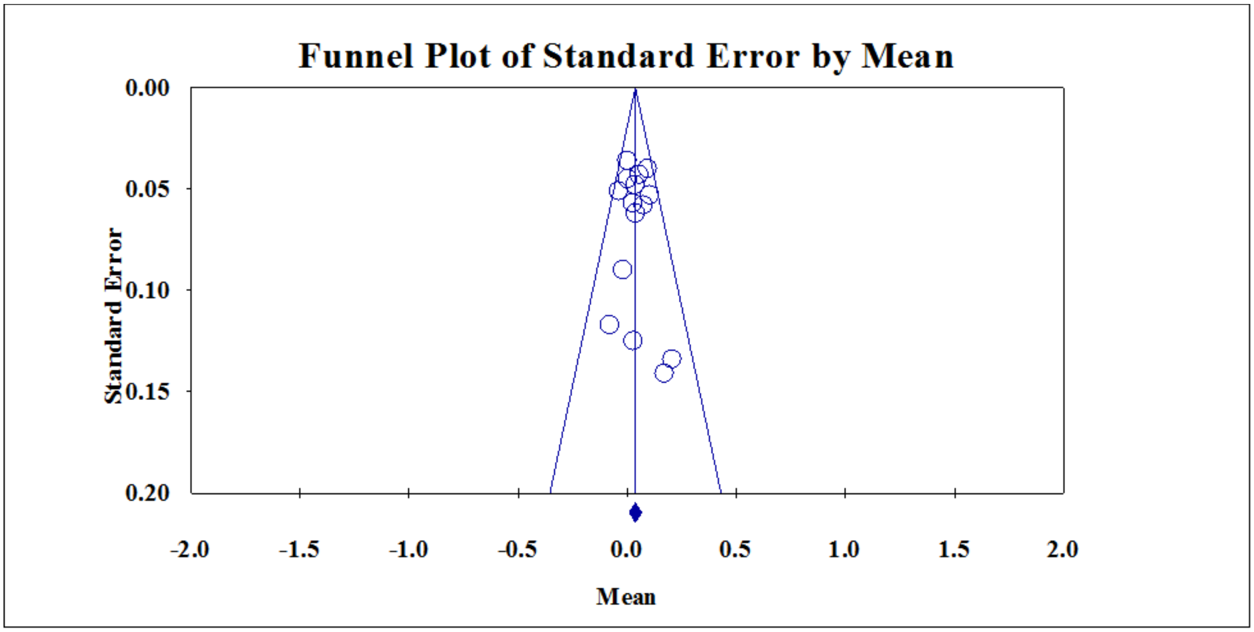
Effect of School Meals and Take-home Rations on Cognitive Development (Boys+Girls+Boys&Girls)

Study name	Subgroup within study	Mean	Standard error	Z-Value	p-Value	Lower limit	Upper limit
Cueto, 2008 SFP Cognitive Code	85	0.025	0.057	0.439	0.661	-0.087	0.137
Cueto, 2008 SFP Cognitive Mem	86	0.074	0.058	1.276	0.202	-0.040	0.188
Vermeersch, 2004 SFP Cognitive Oral	87	-0.020	0.090	-0.222	0.824	-0.196	0.156
Adelman, 2008 SFP Cognitive Raven	90	0.038	0.062	0.613	0.540	-0.084	0.160
Adelman, 2008 SFP Cognitive Raven	92	-0.040	0.051	-0.784	0.433	-0.140	0.060
Adelman, 2008 THR Cognitive Raven	91	0.103	0.053	1.943	0.052	-0.001	0.207
Adelman, 2008 THR Cognitive Raven	93	0.036	0.048	0.750	0.453	-0.058	0.130
Adelman, 2008 SFP Cognitive Deep Span	96	0.054	0.043	1.256	0.209	-0.030	0.138
Adelman, 2008 SFP Cognitive Deep Span	98	-0.001	0.036	-0.028	0.978	-0.072	0.070
Adelman, 2008 SFP Cognitive Deep Span	99	0.001	0.045	0.022	0.982	-0.087	0.089
Adelman, 2008 THR Cognitive Deep Spain	97	0.093	0.040	2.325	0.020	0.015	0.171
Kazianga, 2012 SFP Cognitive Raven	102	0.028	0.125	0.224	0.823	-0.217	0.273
Kazianga, 2012 SFP Cognitive Raven	104	0.205	0.134	1.530	0.126	-0.058	0.468
Kazianga, 2012 THR Cognitive Raven	103	-0.081	0.117	-0.692	0.489	-0.310	0.148
Kazianga, 2012 THR Cognitive Raven	105	0.170	0.141	1.206	0.228	-0.106	0.446
		0.037	0.014	2.544	0.011	0.008	0.065



Effect size and significance				
Model	Number Studies	Point estimate	Significance	Standard error
Random effects	15.000	0.037	**	0.014

Heterogeneity				Tau-squared			
Q-value	df (Q)	P-value	I-squared	Tau Squared	Standard Error	Variance	Tau
12.037	14.000	0.603	-	-	0.001	0.000	-

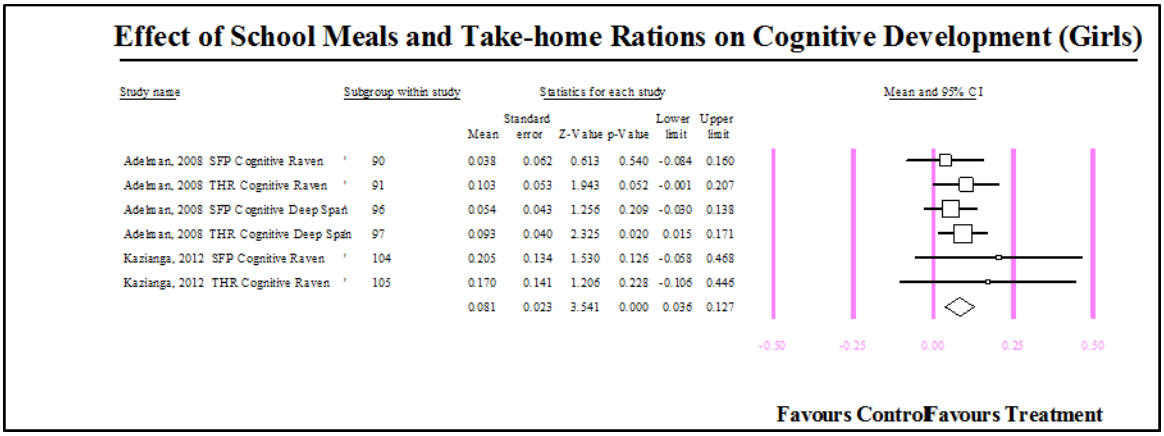


Egger's regression intercept

Intercept	0.34271
Standard error	0.67231
95% lower limit (2-tailed)	-1.10973
95% upper limit (2-tailed)	1.79514
t-value	0.50975
df	13.00000
P-value (1-tailed)	0.30939
P-value (2-tailed)	0.61877

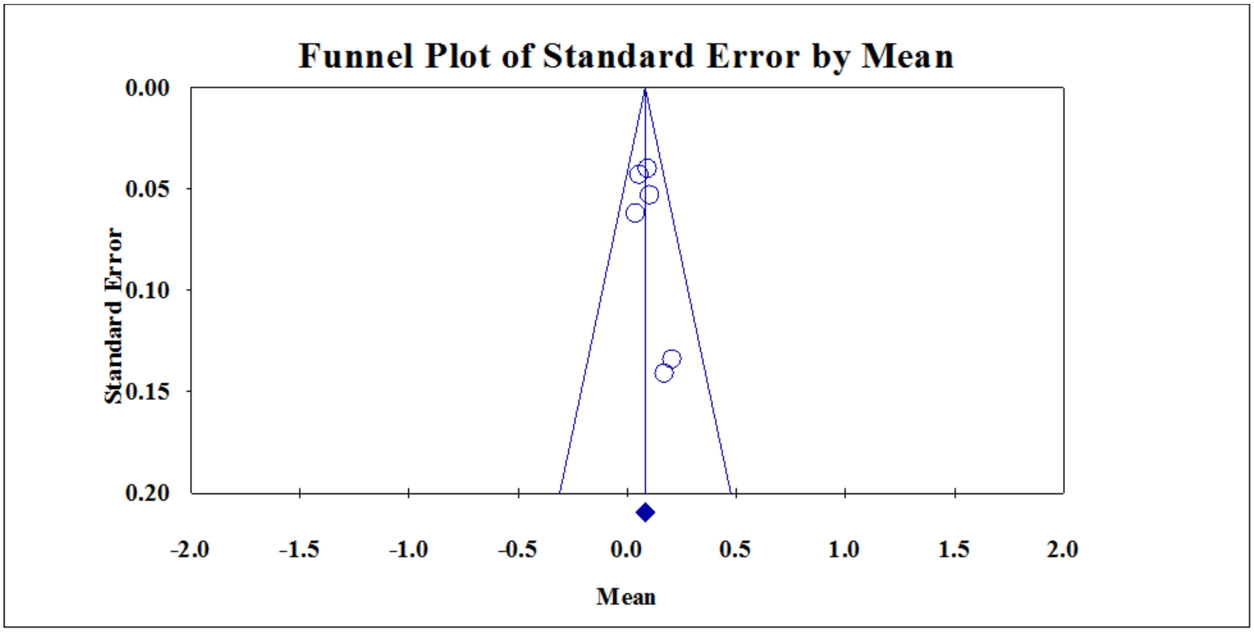
Effect Size of School Feeding on Cognitive Development
Girls only

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	6.000	0.081	***	0.023

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
2.392	5.000	0.793	-	-	0.002	0.000	-



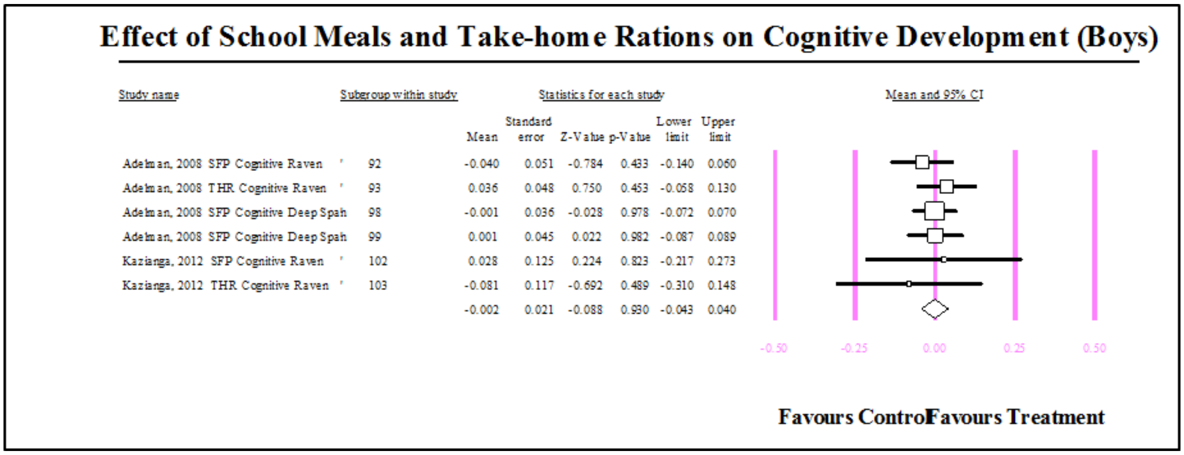
Egger's regression intercept

Intercept	0.96706
Standard error	0.63805
95% lower limit (2-tailed)	-0.80445
95% upper limit (2-tailed)	2.73857
t-value	1.51565
df	4.00000
P-value (1-tailed)	0.10209
P-value (2-tailed)	0.20419

Effect Size of School Feeding on Cognitive Development

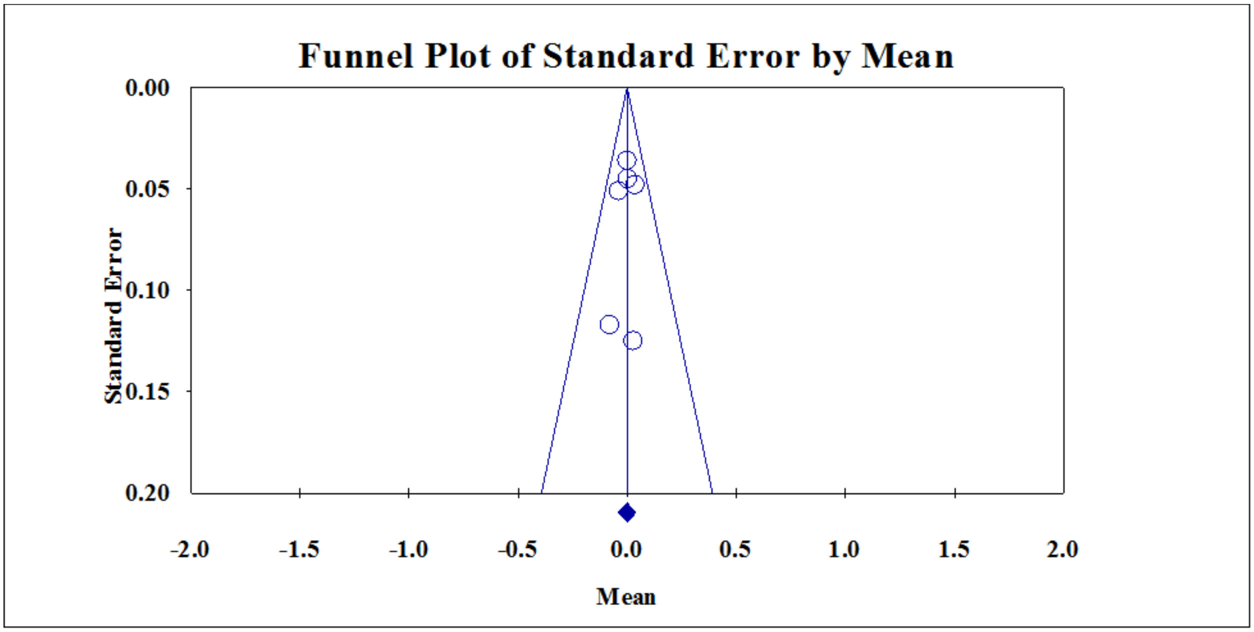
Boys only

Forest plot



Effect size and significance				
Model	Number Studies	Point estimate	Significance	Standard error
Random effects	6.000	(0.002)	-	0.021

Heterogeneity				Tau-squared			
Q-value	df (Q)	P-value	I-squared	Tau Squared	Standard Error	Variance	Tau
1.701	5.000	0.889	-	-	0.002	0.000	-

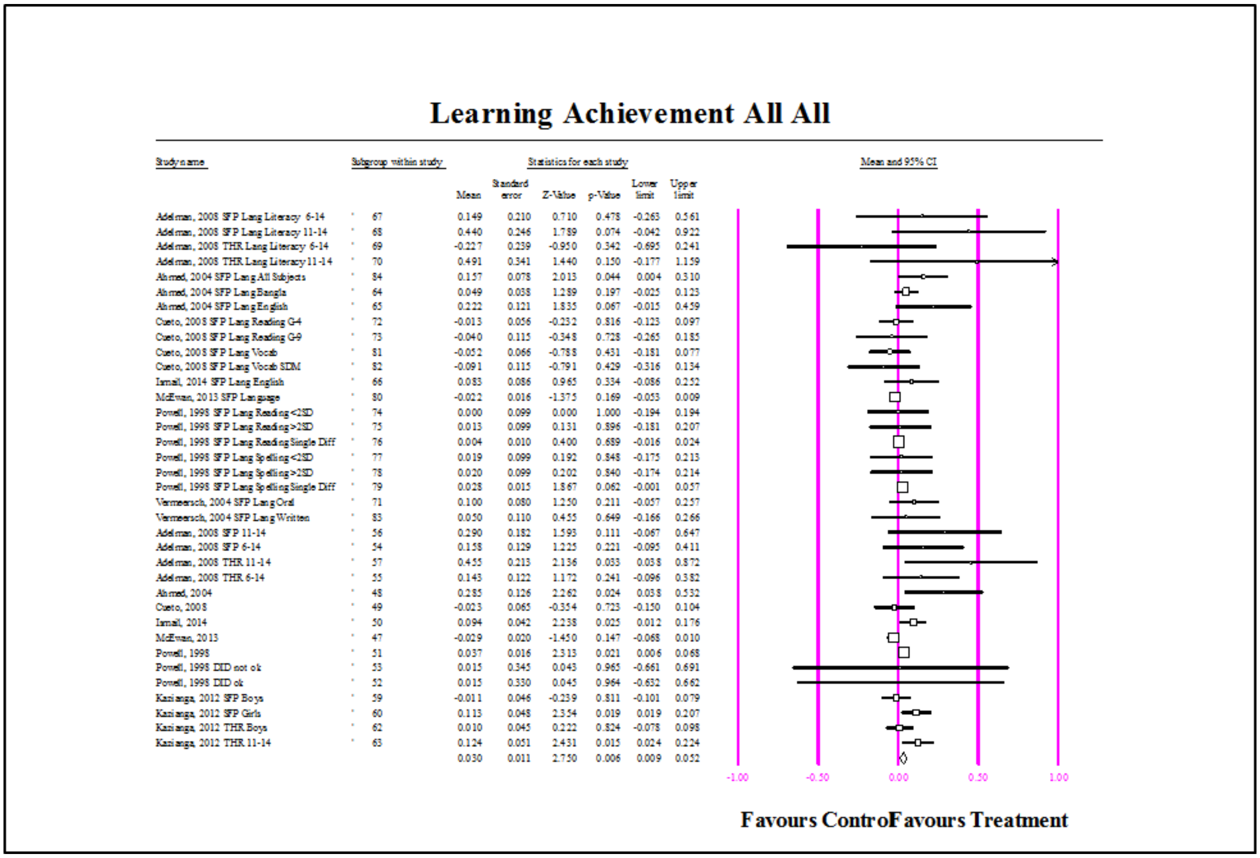


Egger's regression intercept

Intercept	-0.36391
Standard error	0.68506
95% lower limit (2-tailed)	-2.26595
95% upper limit (2-tailed)	1.53813
t-value	0.53121
df	4.00000
P-value (1-tailed)	0.31170
P-value (2-tailed)	0.62340

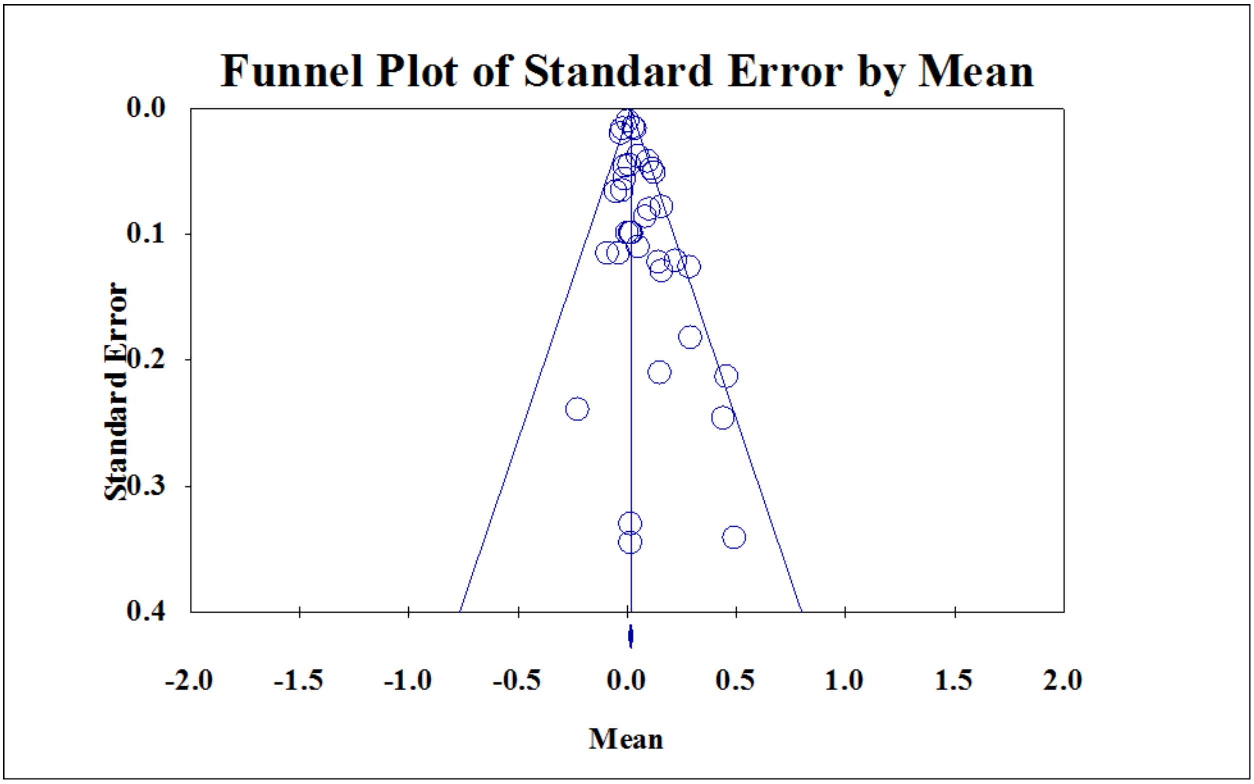
Effect Size of School Feeding on Learning Achievement (language and math)
All children

Forest plot



Effect size and significance				
Model	Number Studies	Point estimate	Significance	Standard error
Random effects	36.000	0.030	***	0.011

Heterogeneity				Tau-squared			
Q-value	df (Q)	P-value	I-squared	Tau Squared	Standard Error	Variance	Tau
58.107	35.000	0.008	39.767	0.001	0.001	0.000	0.031



Egger's regression intercept

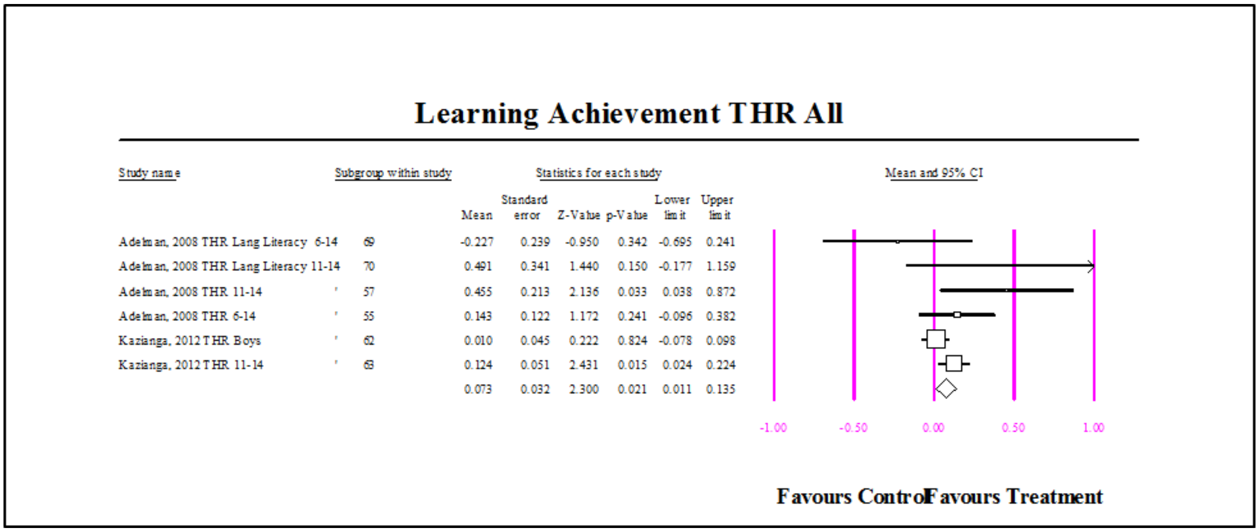
Intercept	0.78558
Standard error	0.25901
95% lower limit (2-tailed)	0.25921
95% upper limit (2-tailed)	1.31196
t-value	3.03299
df	34.00000
P-value (1-tailed)	0.00231
P-value (2-tailed)	0.00461

Effect Size of School Feeding on Learning Achievement (language and math)

Take-home rations only

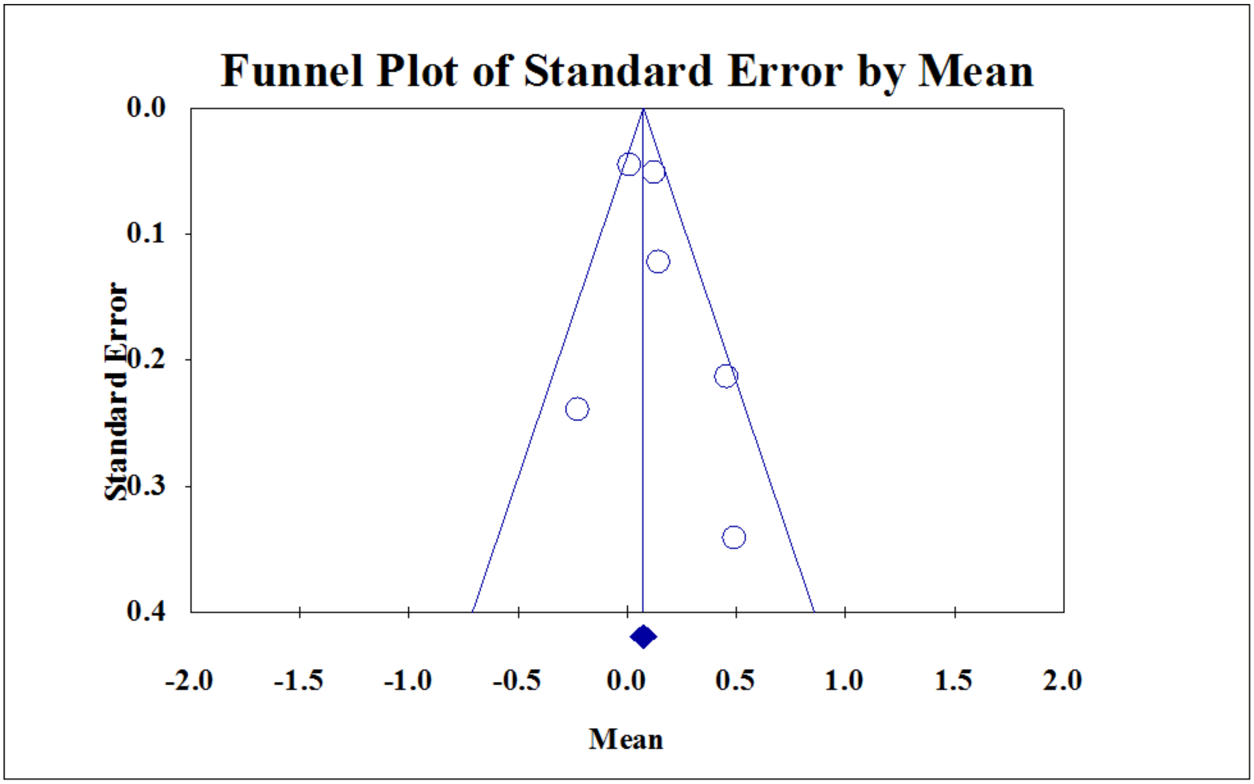
All children

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	6.000	0.099	*	0.059

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
9.584	5.000	0.088	47.829	0.008	0.012	0.000	0.088



Egger's regression intercept

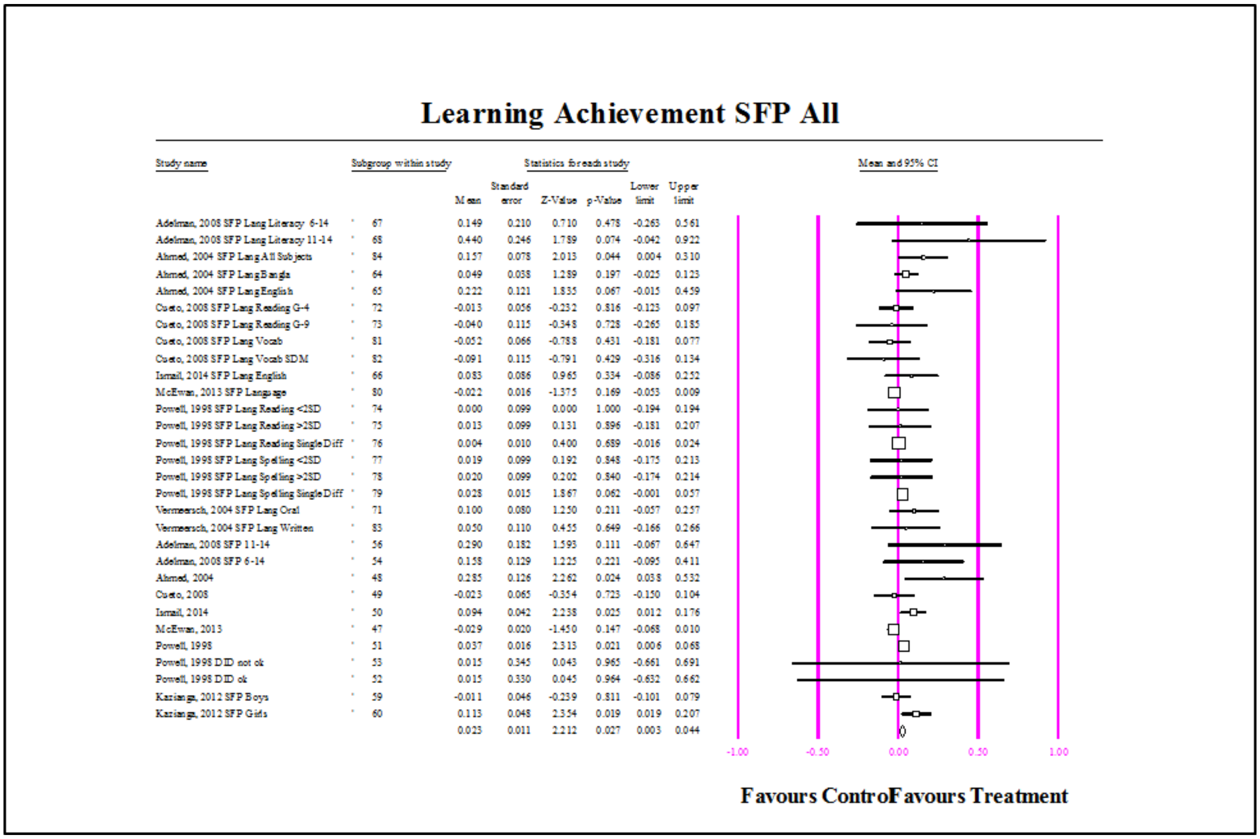
Intercept	0.90175
Standard error	0.95411
95% lower limit (2-tailed)	-1.74729
95% upper limit (2-tailed)	3.55078
t-value	0.94512
df	4.00000
P-value (1-tailed)	0.19906
P-value (2-tailed)	0.39811

Effect Size of School Feeding on Learning Achievement (language and math)

In-school meals only

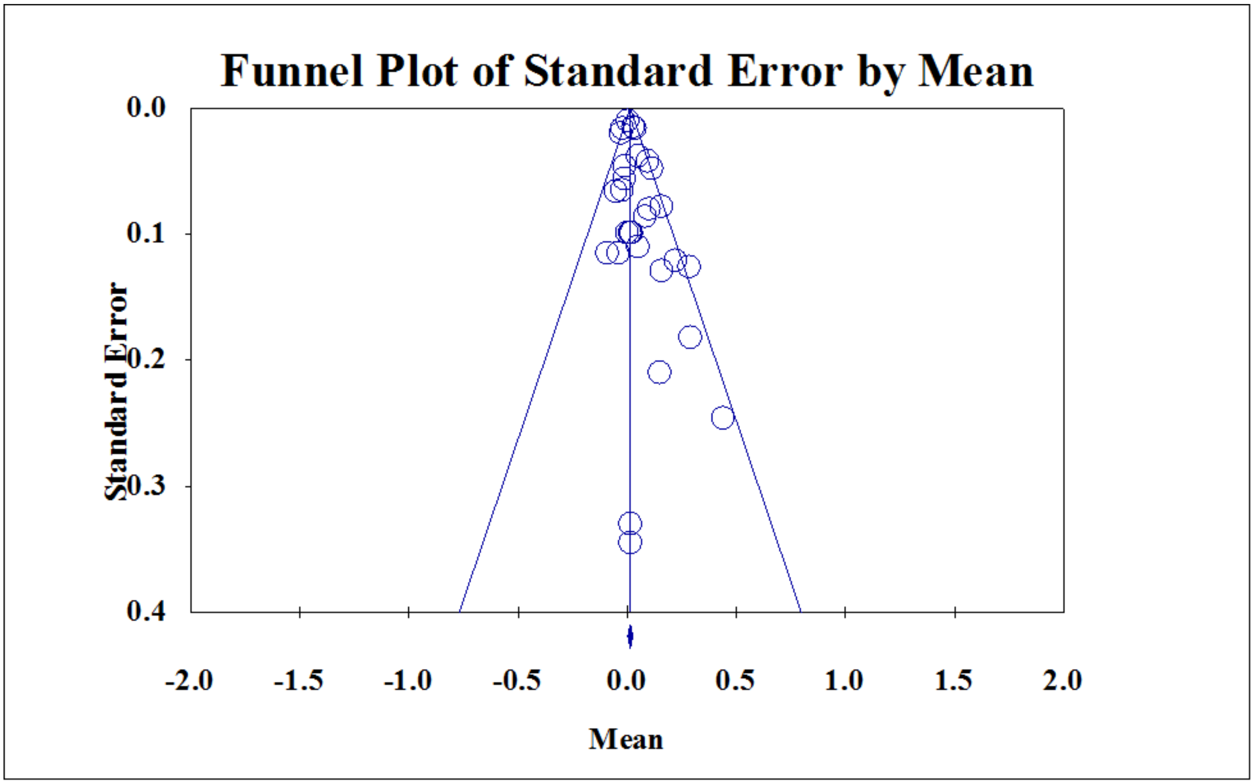
All children

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	30.000	0.023	**	0.011

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
45.055	29.000	0.029	35.634	0.001	0.001	0.000	0.026

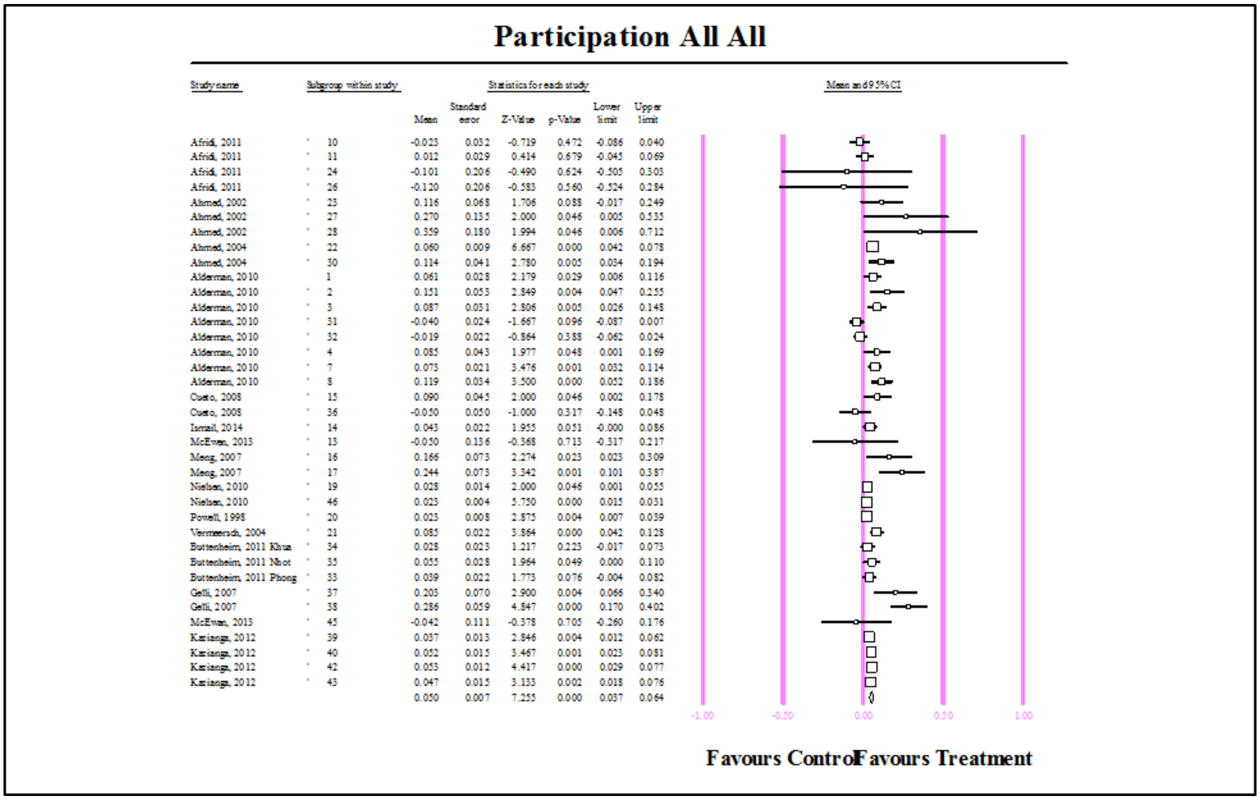


Egger's regression intercept

Intercept	0.70287
Standard error	0.28453
95% lower limit (2-tailed)	0.12003
95% upper limit (2-tailed)	1.28571
t-value	2.47026
df	28.00000
P-value (1-tailed)	0.00993
P-value (2-tailed)	0.01986

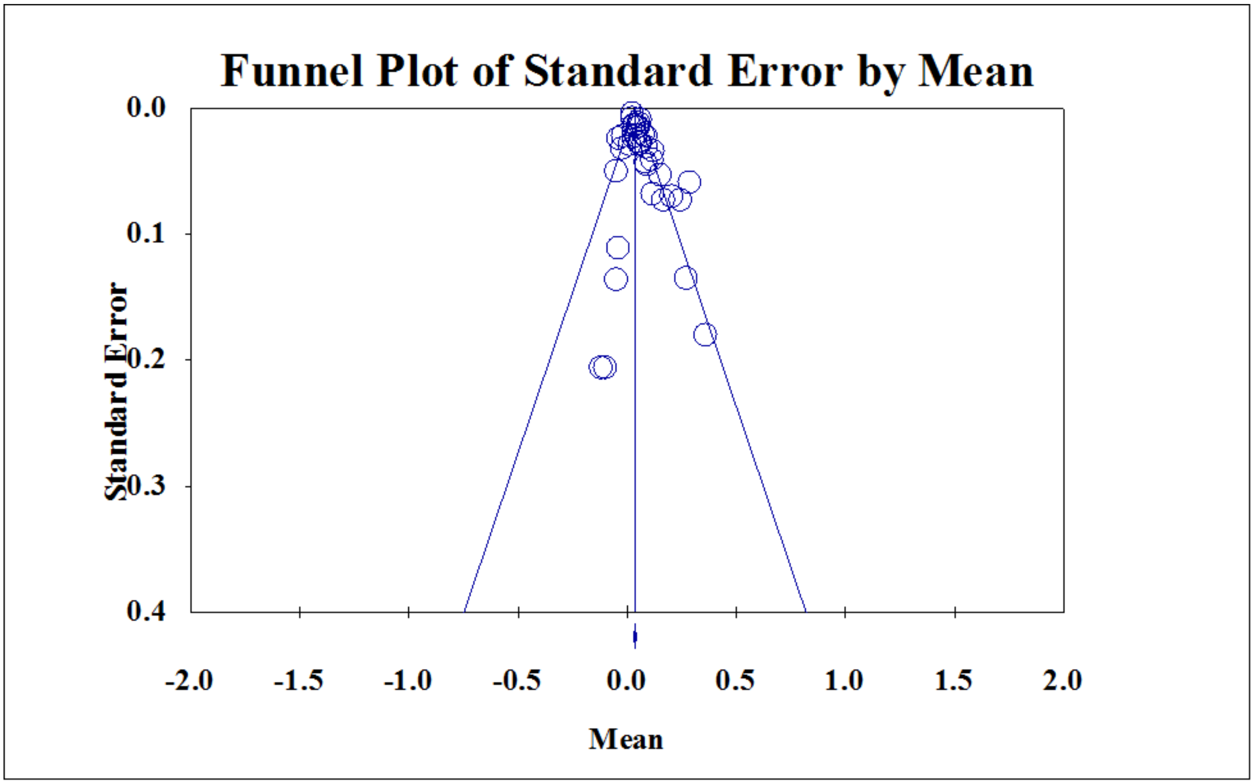
Effect Size of School Feeding on School Participation (enrollment and attendance)
All children

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	37.000	0.050	***	0.007

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
121.174	36.000	0.000	70.291	0.001	0.000	0.000	0.028

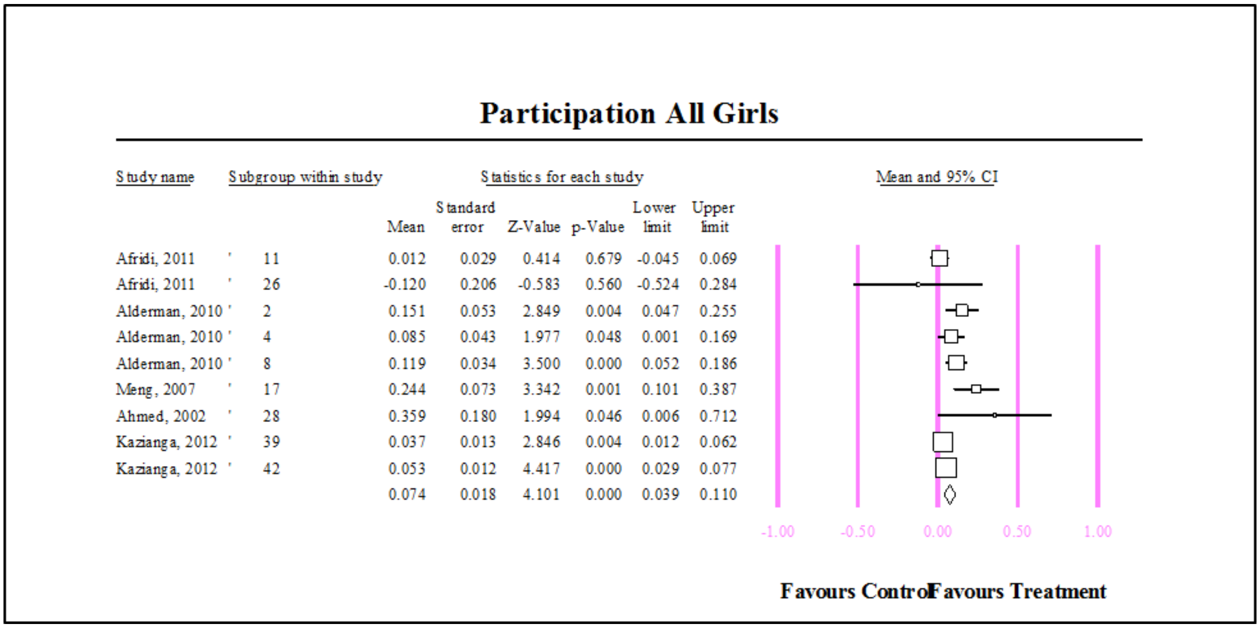


Egger's regression intercept

Intercept	1.13198
Standard error	0.37818
95% lower limit (2-tailed)	0.36424
95% upper limit (2-tailed)	1.89972
t-value	2.99326
df	35.00000
P-value (1-tailed)	0.00252
P-value (2-tailed)	0.00504

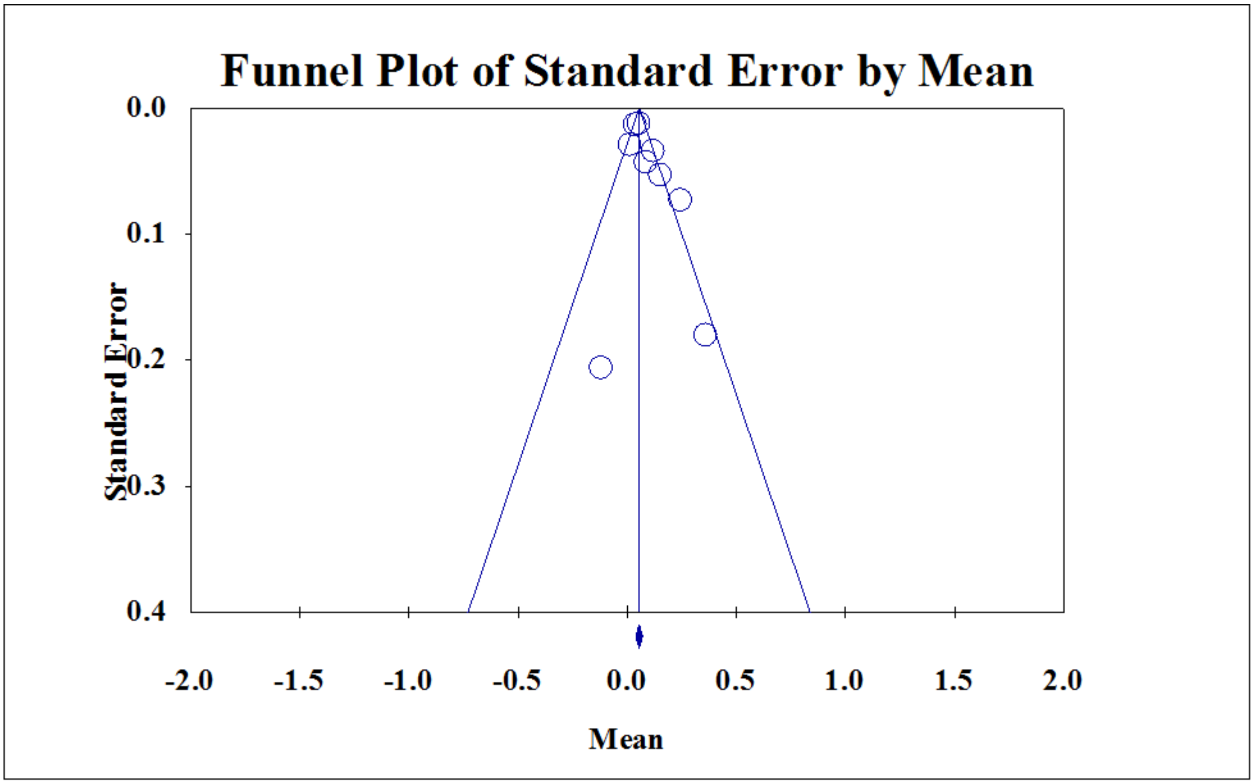
Effect Size of School Feeding on School Participation (enrollment and attendance) Girls only

Forest plot



Effect size and significance				
Model	Number Studies	Point estimate	Significance	Standard error
Random effects	9.000	0.074	***	0.018

Heterogeneity				Tau-squared			
Q-value	df (Q)	P-value	I-squared	Tau Squared	Standard Error	Variance	Tau
21.693	8.000	0.006	63.121	0.001	0.001	0.000	0.036

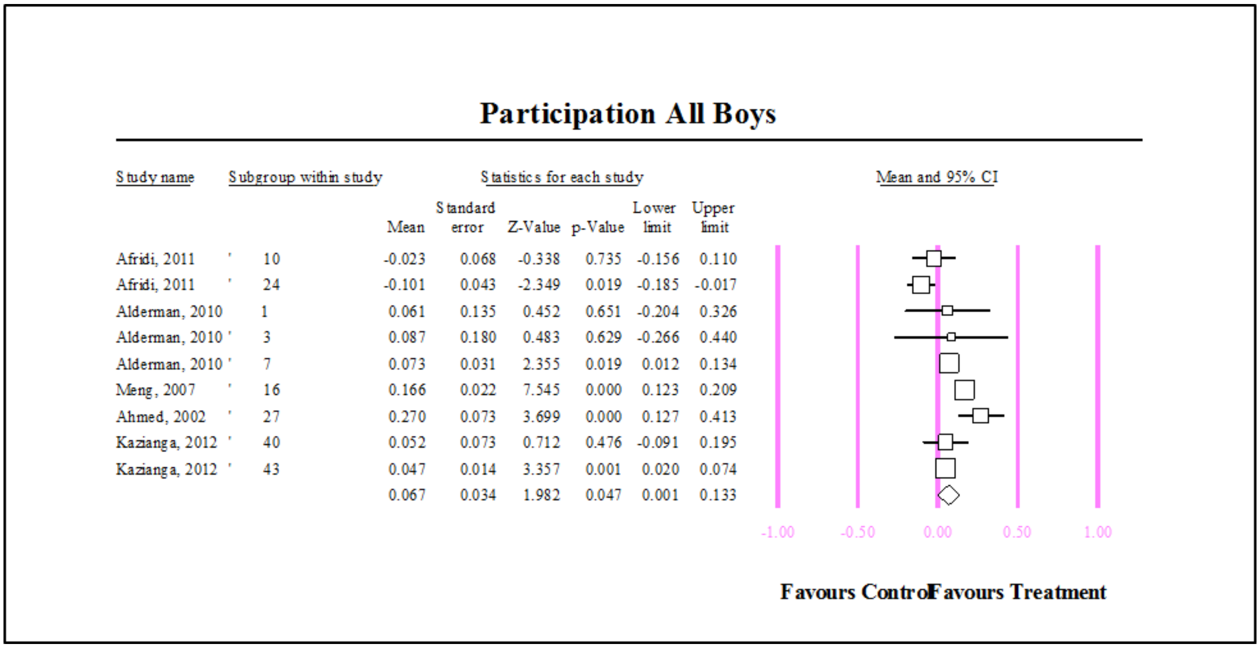


Egger's regression intercept

Intercept	1.39778
Standard error	0.74011
95% lower limit (2-tailed)	-0.35229
95% upper limit (2-tailed)	3.14785
t-value	1.88863
df	7.00000
P-value (1-tailed)	0.05044
P-value (2-tailed)	0.10088

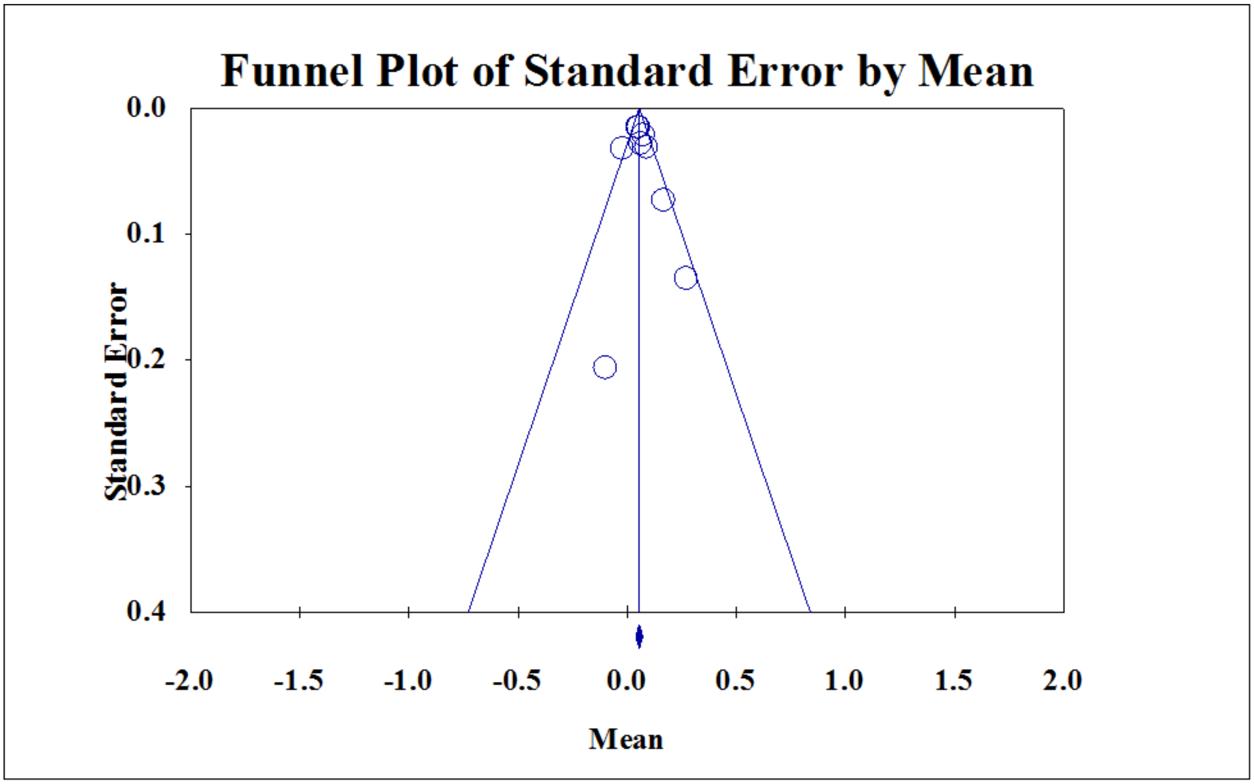
Effect Size of School Feeding on School Participation (enrollment and attendance)
Boys only

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	9.000	0.056	***	0.013

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
13.520	8.000	0.095	40.828	0.000	0.001	0.000	0.022

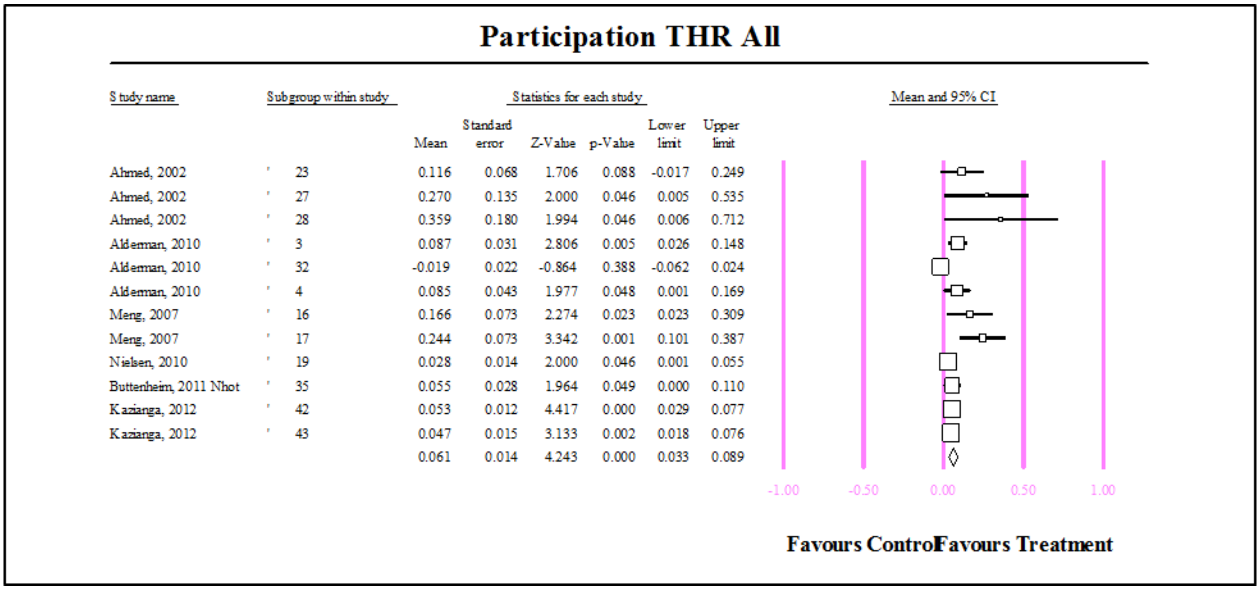


Egger's regression intercept

Intercept	0.60319
Standard error	0.82588
95% lower limit (2-tailed)	-1.34971
95% upper limit (2-tailed)	2.55609
t-value	0.73036
df	7.00000
P-value (1-tailed)	0.24444
P-value (2-tailed)	0.48889

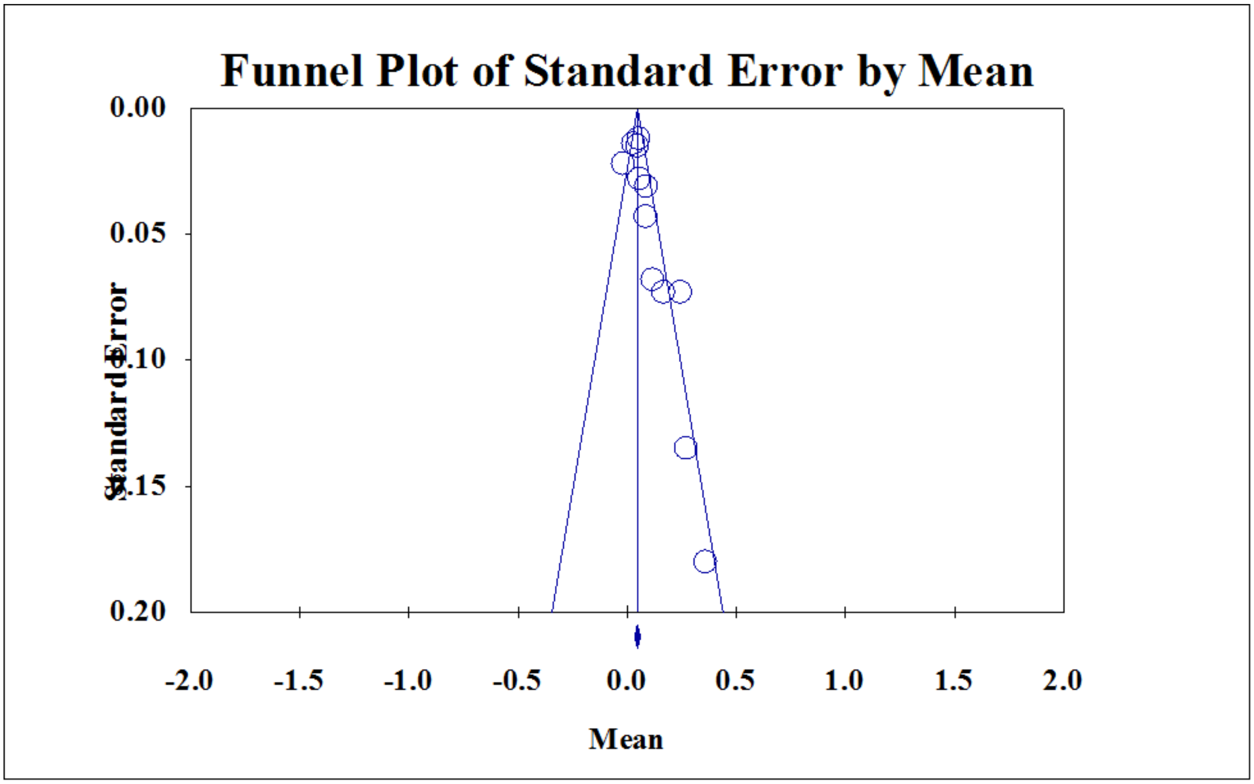
Effect Size of School Feeding on School Participation (enrollment and attendance)
Take-home rations only
All children

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	12.000	0.061	***	0.014

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
30.298	11.000	0.001	63.694	0.001	0.001	0.000	0.033



Egger's regression intercept

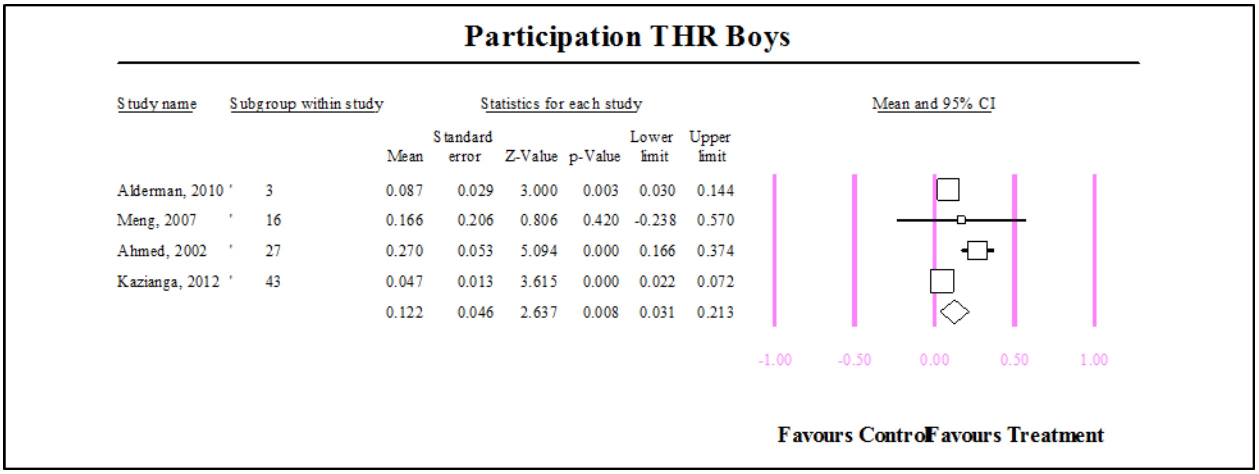
Intercept	1.81745
Standard error	0.61551
95% lower limit (2-tailed)	0.44600
95% upper limit (2-tailed)	3.18891
t-value	2.95274
df	10.00000
P-value (1-tailed)	0.00723
P-value (2-tailed)	0.01447

Effect Size of School Feeding on School Participation (enrollment and attendance)

Take-home rations only

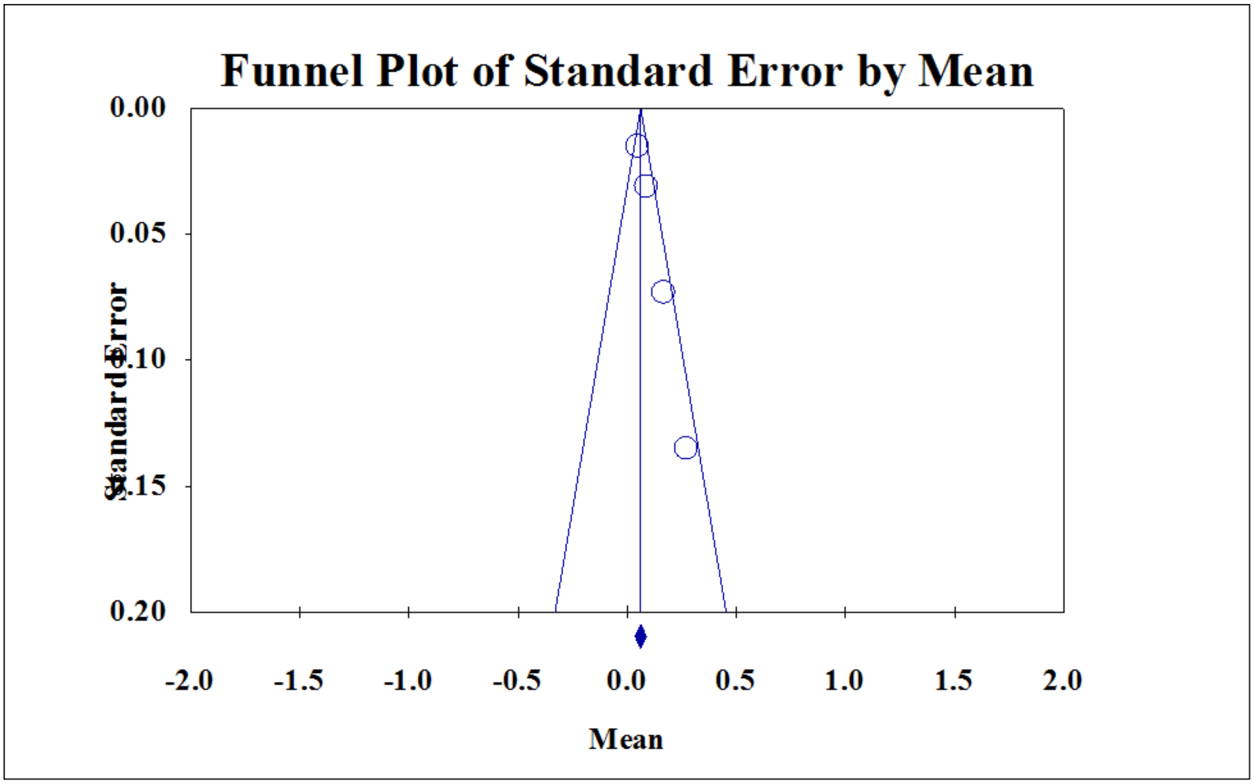
Boys only

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	4.000	0.085	***	0.029

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
6.037	3.000	0.110	50.310	0.001	0.003	0.000	0.038



Egger's regression intercept

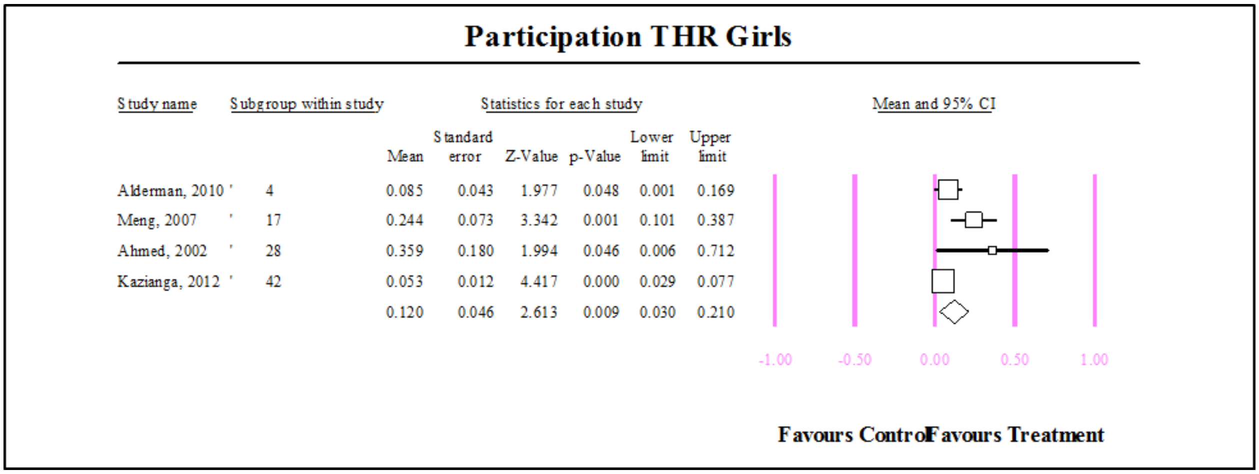
Intercept	2.00440
Standard error	0.15687
95% lower limit (2-tailed)	1.32943
95% upper limit (2-tailed)	2.67937
t-value	12.77722
df	2.00000
P-value (1-tailed)	0.00303
P-value (2-tailed)	0.00607

Effect Size of School Feeding on School Participation (enrollment and attendance)

Take-home rations only

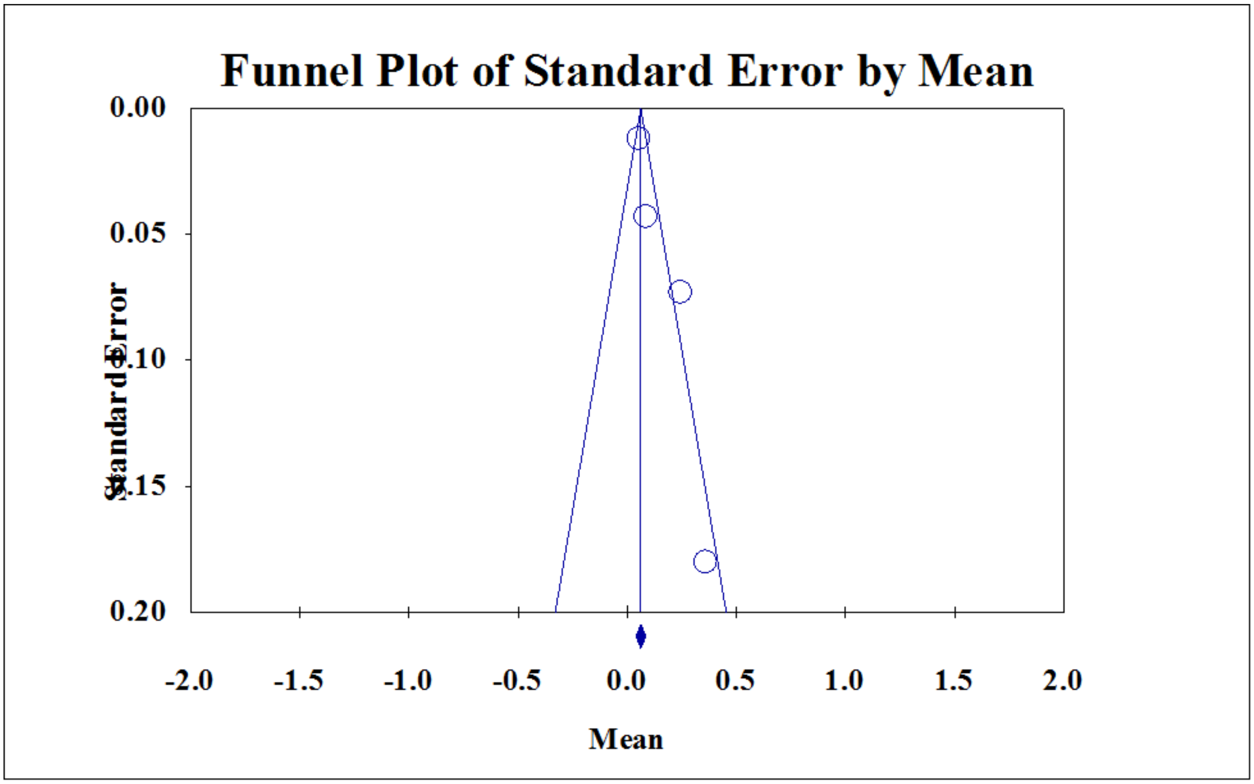
Girls only

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	4.000	0.120	***	0.046

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
9.781	3.000	0.021	69.328	0.005	0.007	0.000	0.070



Egger's regression intercept

Intercept	2.07690
Standard error	0.59876
95% lower limit (2-tailed)	-0.49937
95% upper limit (2-tailed)	4.65318
t-value	3.46864
df	2.00000
P-value (1-tailed)	0.03700
P-value (2-tailed)	0.07401

Effect Size of School Feeding on School Participation (enrollment and attendance)

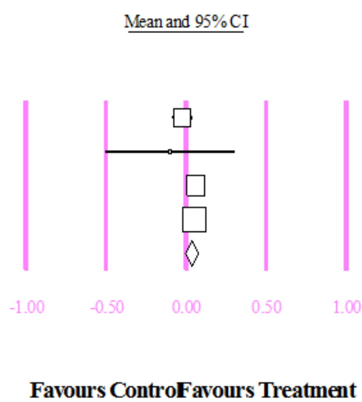
In-school feeding only

All children

Forest plot

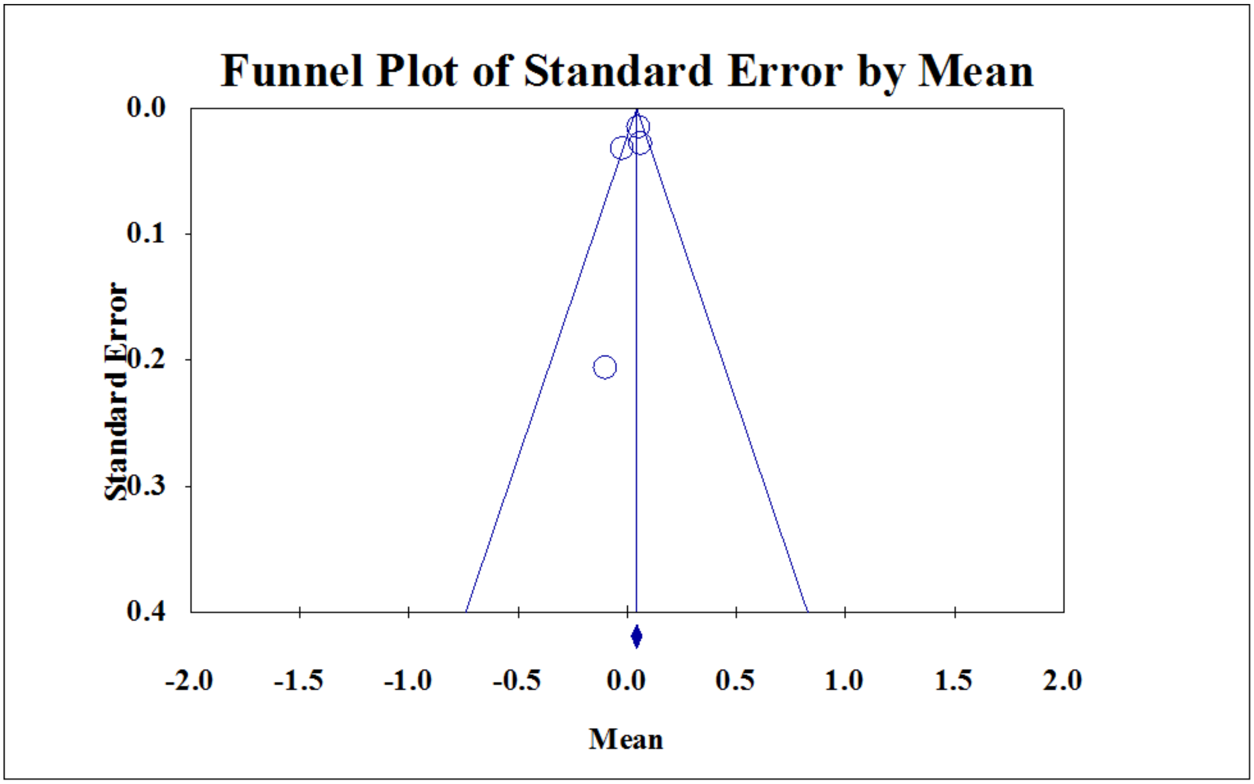
Participation SFP Boys

Study name	Subgroup within study	Mean	Standard error	Z-Value	p-Value	Lower limit	Upper limit
Afridi, 2011	' 10	-0.023	0.032	-0.719	0.472	-0.086	0.040
Afridi, 2011	' 24	-0.101	0.206	-0.490	0.624	-0.505	0.303
Alderman, 2010	1	0.061	0.028	2.179	0.029	0.006	0.116
Kazianga, 2012	' 40	0.052	0.015	3.467	0.001	0.023	0.081
		0.034	0.021	1.644	0.100	-0.007	0.075



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	4.000	0.034	-	0.021

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
5.512	3.000	0.138	45.576	0.001	0.001	0.000	0.027



Egger's regression intercept

Intercept	-1.23716
Standard error	1.28040
95% lower limit (2-tailed)	-6.74628
95% upper limit (2-tailed)	4.27196
t-value	0.96623
df	2.00000
P-value (1-tailed)	0.21793
P-value (2-tailed)	0.43587

Effect Size of School Feeding on School Participation (enrollment and attendance)

In-school feeding only

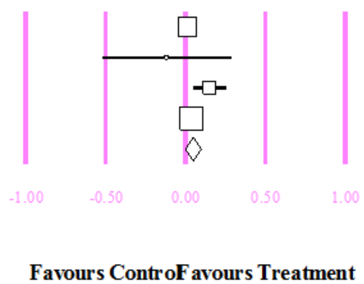
Girls only

Forest plot

Participation SFP Girls

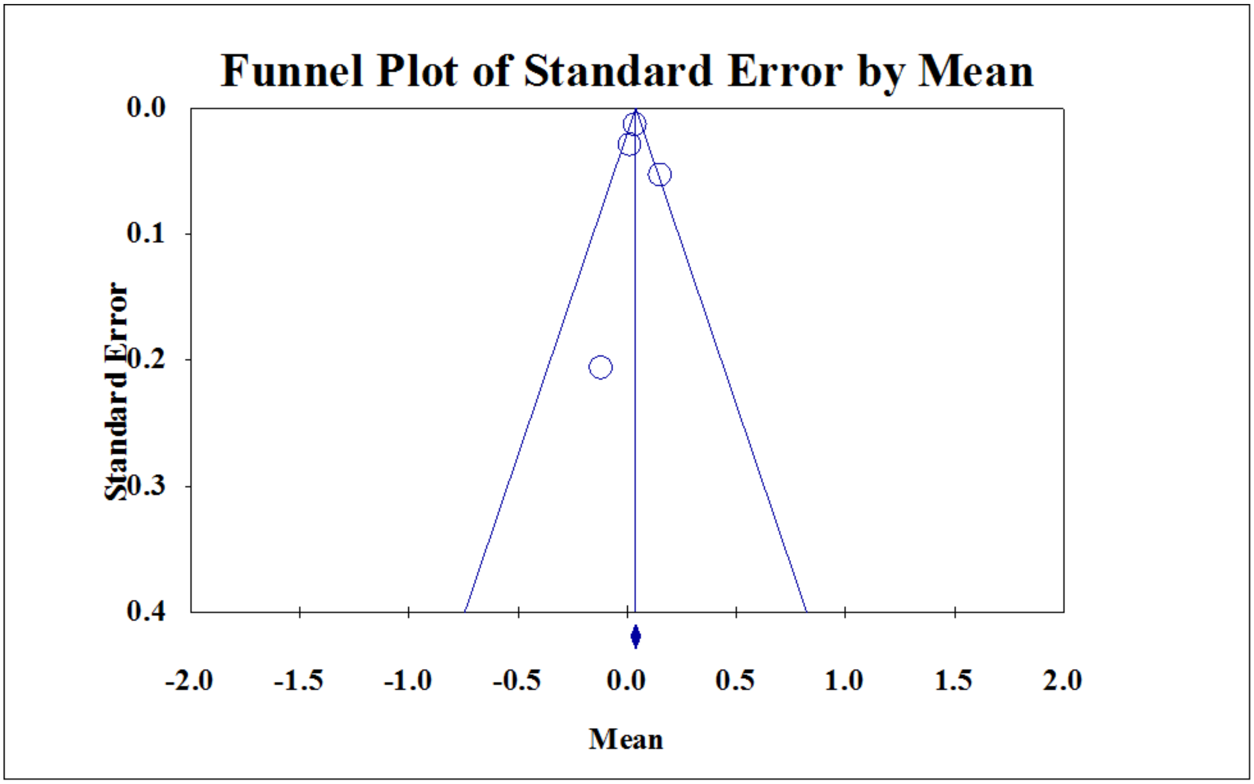
Study name	Subgroup within study	Mean	Standard error	Z-Value	p-Value	Lower limit	Upper limit
Afridi, 2011 '	11	0.012	0.029	0.414	0.679	-0.045	0.069
Afridi, 2011 '	26	-0.120	0.206	-0.583	0.560	-0.524	0.284
Alderman, 2010 '	2	0.151	0.053	2.849	0.004	0.047	0.255
Kazianga, 2012 '	39	0.037	0.013	2.846	0.004	0.012	0.062
		0.045	0.025	1.779	0.075	-0.005	0.095

Mean and 95% CI



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	4.000	0.045	*	0.025

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
5.944	3.000	0.114	49.527	0.001	0.002	0.000	0.034



Egger's regression intercept

Intercept	0.25471
Standard error	1.36809
95% lower limit (2-tailed)	-5.63172
95% upper limit (2-tailed)	6.14113
t-value	0.18618
df	2.00000
P-value (1-tailed)	0.43474
P-value (2-tailed)	0.86948

Effect Size of School Feeding on School Participation (enrollment and attendance)

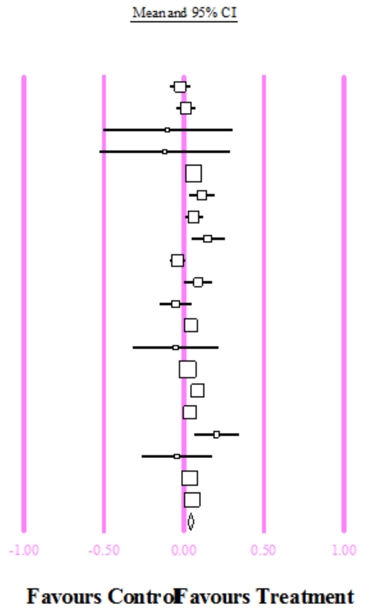
In-school feeding only

All children

Forest plot

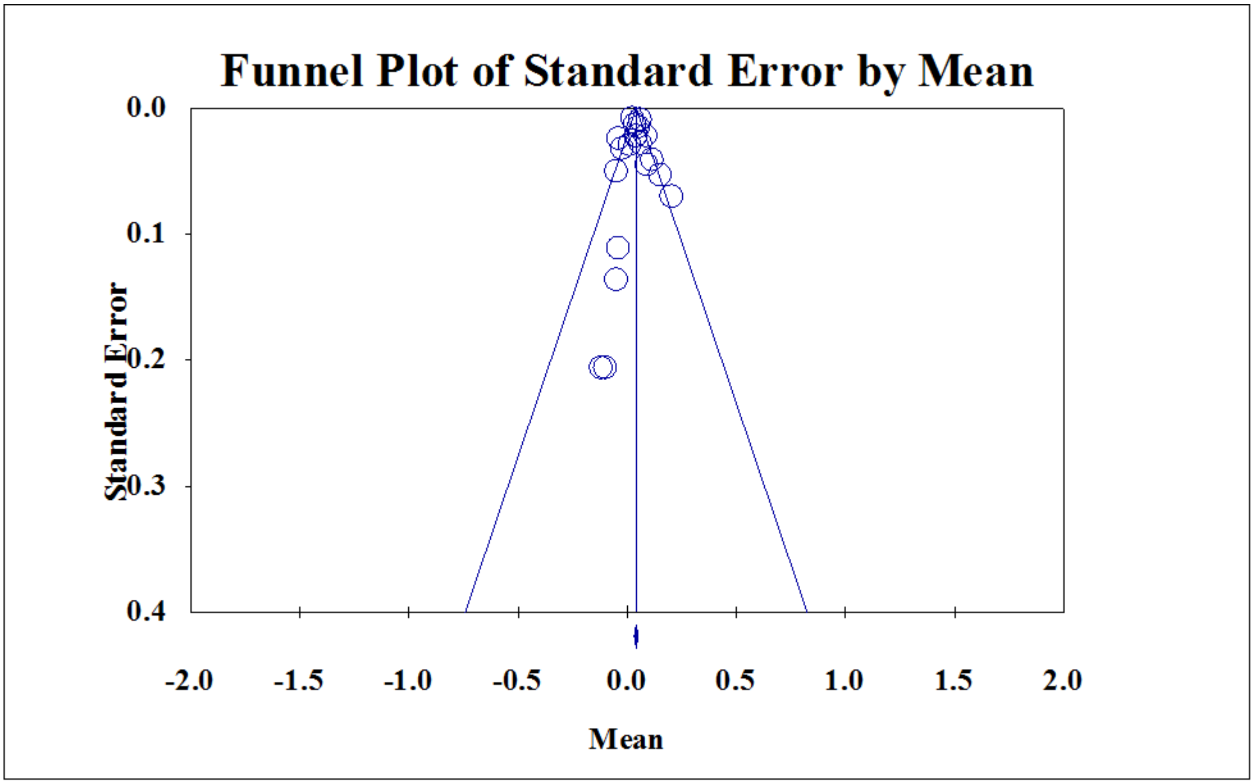
Participation SFP All

Study name	Subgroup within study	Mean	Standard error	Z-Value	p-Value	Lower limit	Upper limit
Afridi, 2011	10	-0.023	0.032	-0.719	0.472	-0.086	0.040
Afridi, 2011	11	0.012	0.029	0.414	0.679	-0.045	0.069
Afridi, 2011	24	-0.101	0.206	-0.490	0.624	-0.505	0.303
Afridi, 2011	26	-0.120	0.206	-0.583	0.560	-0.524	0.284
Ahmed, 2004	22	0.060	0.009	6.667	0.000	0.042	0.078
Ahmed, 2004	30	0.114	0.041	2.780	0.005	0.034	0.194
Alderman, 2010	1	0.061	0.028	2.179	0.029	0.006	0.116
Alderman, 2010	2	0.151	0.053	2.849	0.004	0.047	0.255
Alderman, 2010	31	-0.040	0.024	-1.667	0.096	-0.087	0.007
Cueto, 2008	15	0.090	0.045	2.000	0.046	0.002	0.178
Cueto, 2008	36	-0.050	0.050	-1.000	0.317	-0.148	0.048
Ismail, 2014	14	0.043	0.022	1.955	0.051	-0.000	0.086
McEwen, 2013	13	-0.050	0.136	-0.368	0.713	-0.317	0.217
Powell, 1998	20	0.023	0.008	2.875	0.004	0.007	0.039
Vermeersch, 2004	21	0.085	0.022	3.864	0.000	0.042	0.128
Butterheim, 2011 Phong	33	0.039	0.022	1.773	0.076	-0.004	0.082
Gelli, 2007	37	0.203	0.070	2.900	0.004	0.066	0.340
McEwan, 2013	45	-0.042	0.111	-0.378	0.705	-0.260	0.176
Kazianga, 2012	39	0.037	0.013	2.846	0.004	0.012	0.062
Kazianga, 2012	40	0.052	0.015	3.467	0.001	0.023	0.081
		0.041	0.009	4.405	0.000	0.023	0.059



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	20.000	0.041	***	0.009

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
50.429	19.000	0.000	62.323	0.001	0.001	0.000	0.027

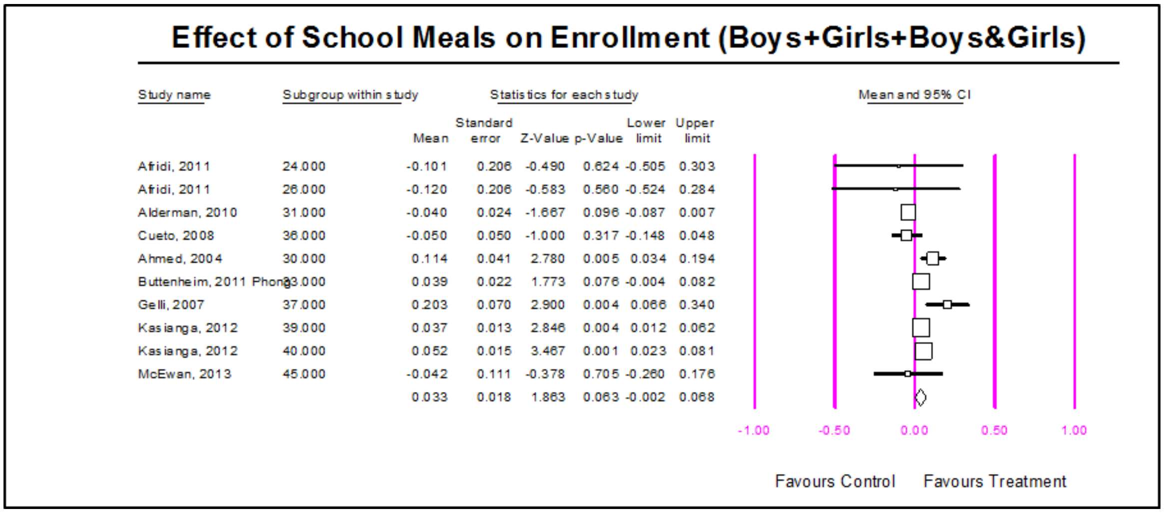


Egger's regression intercept

Intercept	0.00665
Standard error	0.58684
95% lower limit (2-tailed)	-1.22626
95% upper limit (2-tailed)	1.23955
t-value	0.01133
df	18.00000
P-value (1-tailed)	0.49554
P-value (2-tailed)	0.99109

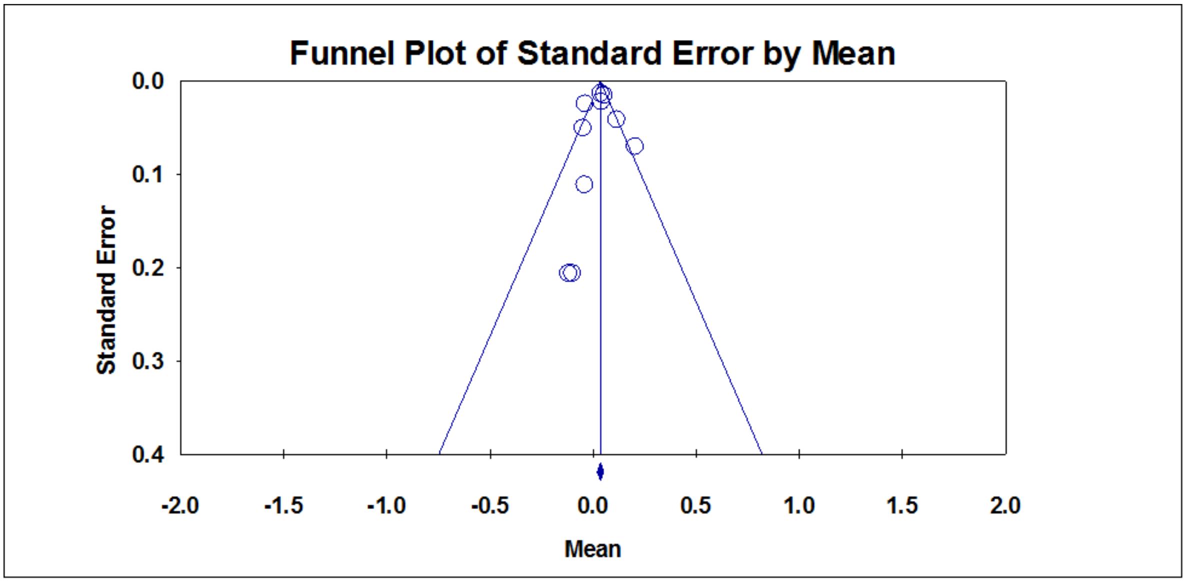
Effect Size of School Feeding on School Enrollment
All children

Forest plot



Effect size and significance				
Model	Number Studies	Point estimate	Significance	Standard error
Random effects	10.000	0.033	*	0.018

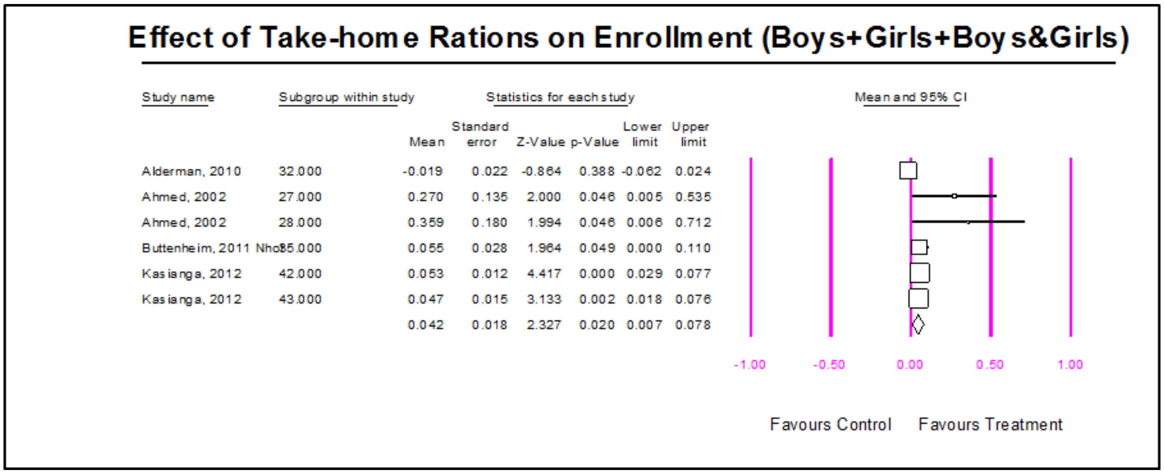
Heterogeneity				Tau-squared			
Q-value	df (Q)	P-value	I-squared	Tau Squared	Standard Error	Variance	Tau
24.953	9.000	0.003	63.932	0.001	0.001	0.000	0.037



Funnel Plot of Standard Error by Mean
Standard Error
0.0
0.1
0.2
0.3
0.4
-2.0
-1.5
-1.0
-0.5
0.0
0.5
1.0
1.5
2.0
Mean

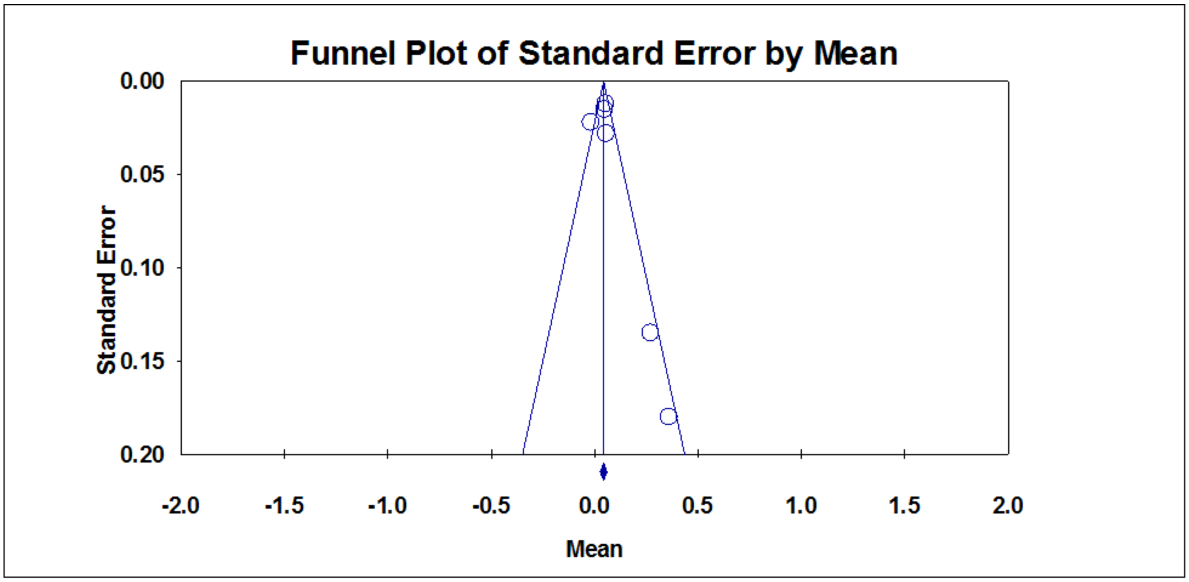
Effect Size of School Feeding on School Enrollment
Take-home rations only
All children

Forest plot



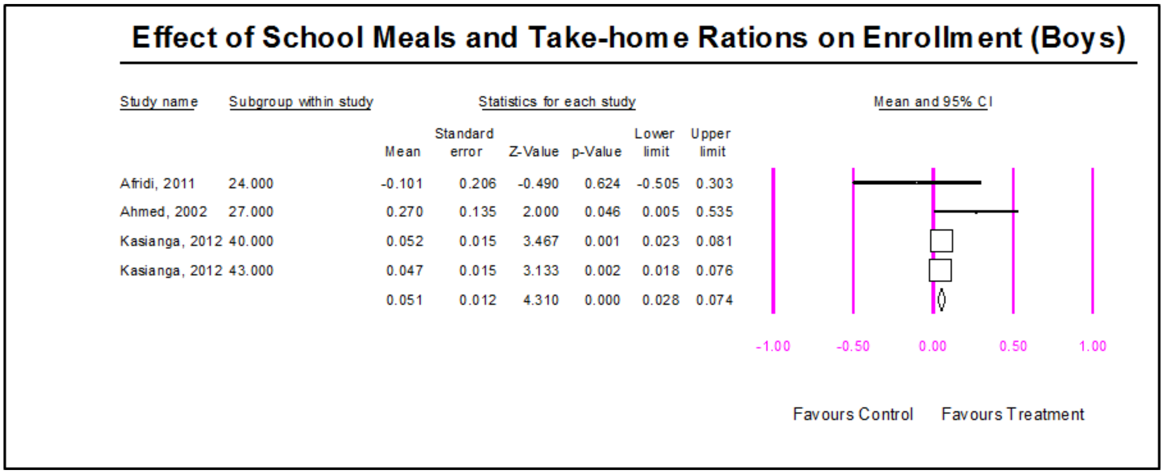
Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	6.000	0.042	**	0.018

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
14.800	5.000	0.011	66.216	0.001	0.001	0.000	0.032


Funnel Plot of Standard Error by Mean
Standard Error
0.00
0.05
0.10
0.15
0.20
-2.0
-1.5
-1.0
-0.5
0.0
0.5
1.0
1.5
2.0
Mean

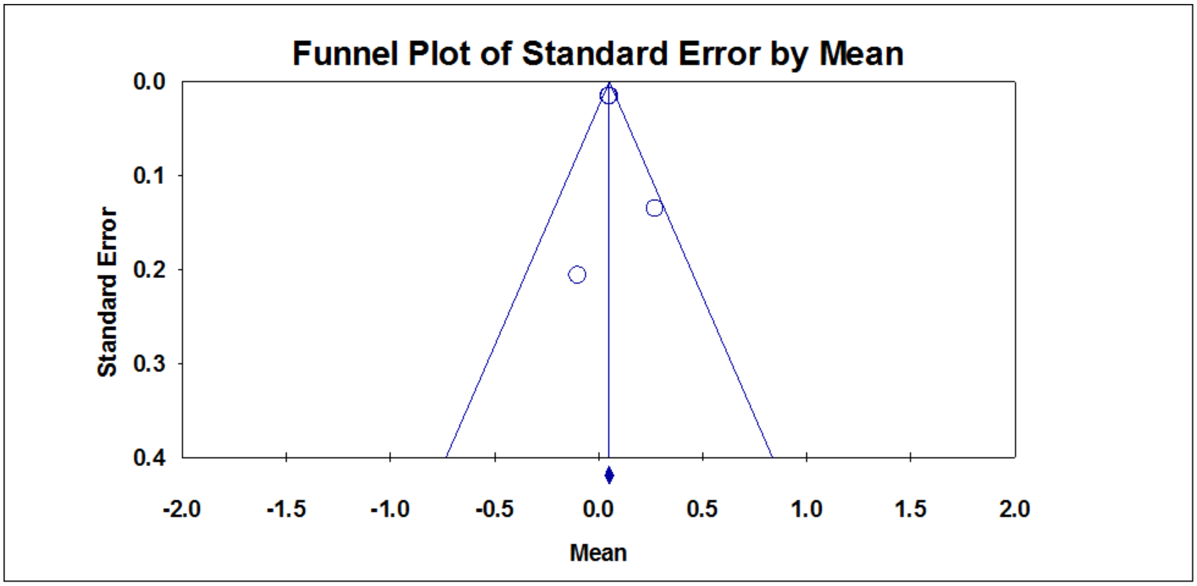
Effect Size of School Feeding on School Enrollment
Take-home rations only
Boys only

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	4.000	0.051	***	0.012

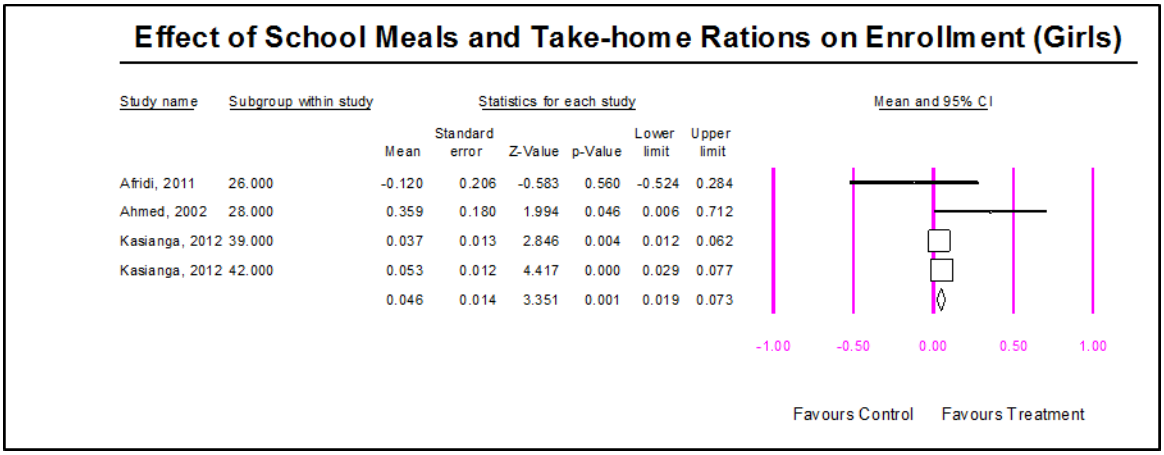
Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
3.249	3.000	0.355	7.662	0.000	0.001	0.000	0.007



Funnel Plot of Standard Error by Mean
Standard Error
0.0
0.1
0.2
0.3
0.4
-2.0
-1.5
-1.0
-0.5
0.0
0.5
1.0
1.5
2.0
Mean

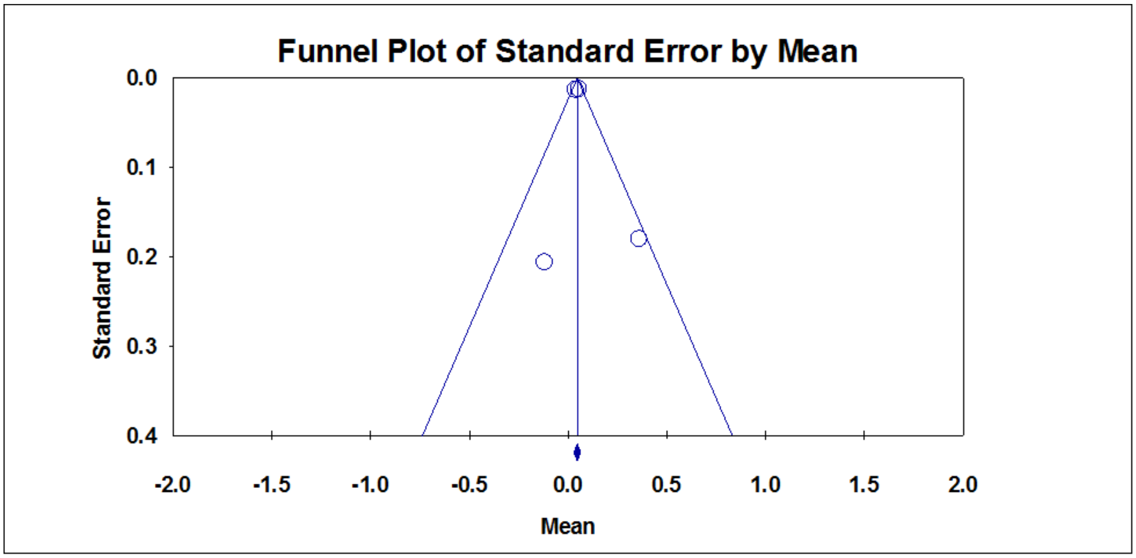
Effect Size of School Feeding on School Enrollment
In-school meals and take-home rations
Girls only

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	4.000	0.046	***	0.014

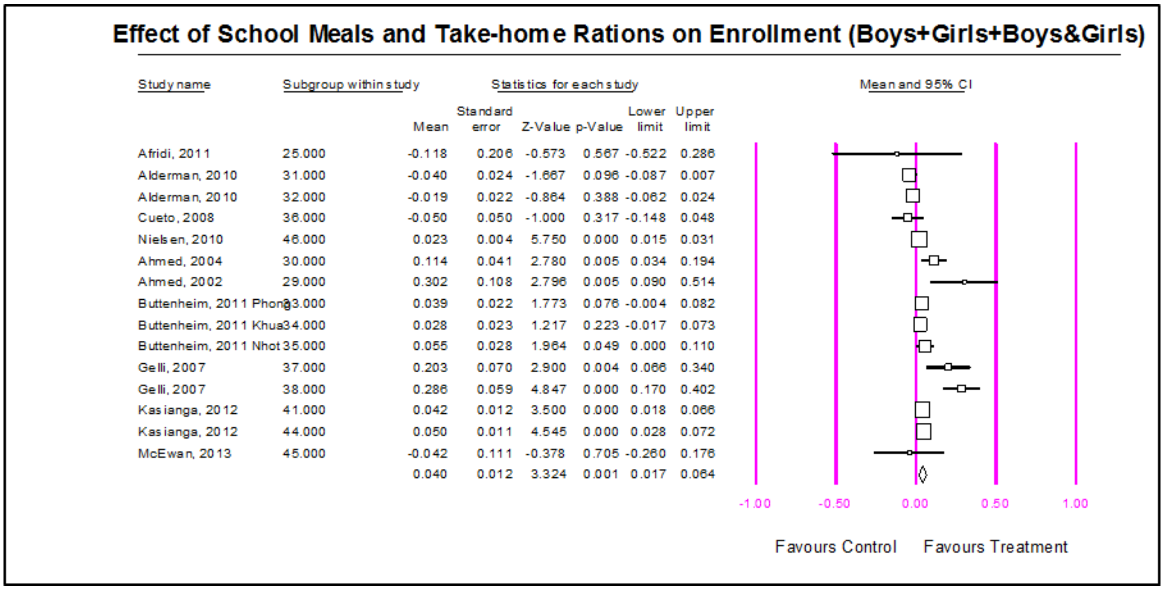
Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
4.493	3.000	0.213	33.223	0.000	0.001	0.000	0.015



Funnel Plot of Standard Error by Mean
Standard Error
0.0
0.1
0.2
0.3
0.4
-2.0
-1.5
-1.0
-0.5
0.0
0.5
1.0
1.5
2.0
Mean

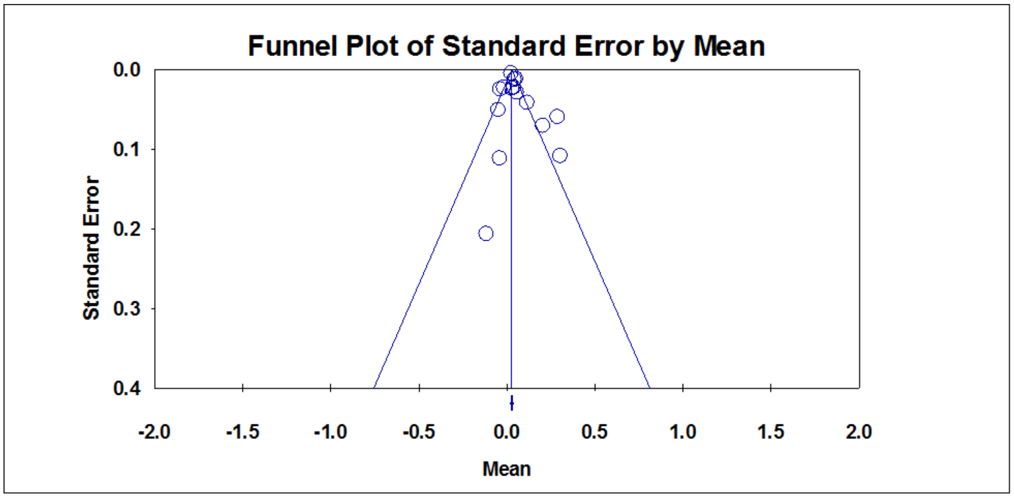
Effect Size of School Feeding on School Enrollment
In-school meals and take-home rations
All children

Forest plot



Effect size and significance				
Model	**Number Studies**	**Point estimate**	**Significance**	**Standard error**
Random effects	15.000	0.040	***	0.012

Heterogeneity				Tau-squared			
Q-value	**df (Q)**	**P-value**	**I-squared**	**Tau Squared**	**Standard Error**	**Variance**	**Tau**
60.212	14.000	0.000	76.749	0.001	0.001	0.000	0.033


Funnel Plot of Standard Error by Mean
Standard Error
0.0
0.1
0.2
0.3
0.4
-2.0
-1.5
-1.0
-0.5
0.0
0.5
1.0
1.5
2.0
Mean