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# Interventions in developing nations for improving primary and secondary school enrolments

## A systematic review

August 2013

Systematic Review 3

Education

3ie International Initiative for Impact Evaluation

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## About this review

*Interventions in developing nations for improving primary and secondary school enrolments: a systematic review,* was submitted in partial fulfilment of the requirements of grant SR1.9 issued under Systematic Review Window 1. This review and all of its appendixes are available on the 3ie website. 3ie is publishing this report as received from the authors; it has been formatted to 3ie style. This review has also been published by the Campbell Collaboration and is available here.

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# Interventions in developing nations for improving primary and secondary school enrolments: a systematic review

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# Summary

## Background

Education is considered critical to economic development and social welfare in developing nations. In light of compelling evidence that links expanded education systems and socioeconomic development while highlighting the importance of policies to offset inequality in access, governments and donor agencies have invested considerable funding to promote educational initiatives. Considerable funding for such initiatives has brought with it a concomitant increase in accountability and decision-makers want to know if the funds they have put toward such programs are having positive impact. Concurrently, there has also been a rise in impact evaluations in the developing world, particularly in education. Given the importance of education, particularly to outcomes in the most economically challenged nations, the amount of interventions that have been implemented to address education in developing nations, and the increase in relevant controlled impact evaluations, the need for a systematic review seems clear.

No systematic review of randomized controlled trials and quasi-experiments of strategies in developing nations to get children into school (enrollment) and keep them there (attendance, persistence, continuation) has yet been reported, nor has any looked at supplemental outcomes focused on learning. By systematically gathering and analyzing rigorous research about the program effects of primary and secondary school enrollment and completion policies, our review will hopefully provide evidence to inform the next wave of funding, intervention and evaluation efforts in this area.

## Objectives

For this project, our objectives were to respond to the following questions:

- Main Question: What are the effects of interventions implemented in developing countries on measures of students' enrollment, attendance, graduation, and progression?
- Supplemental Question: Within those studies that report the effects of an intervention on measures of students' enrollment, attendance, graduation or progression, what are the ancillary effects on learning outcomes as measured by students' test scores, grades, and other achievement measures?

## Search strategy

Five main strategies were used to identify eligible reports: (1) electronic searches of bibliographic databases; (2) hand searches of relevant journals; (3) examining the citations of every retrieved report; (4) contacting the "informal college" of researchers working in the area; and (5) searches of the internet and specialized holdings.

## Selection criteria

To be eligible, studies had to: (1) assess the impact of an intervention that included primary or secondary school outcomes (Kindergarten-12$^{th}$ grade in the U.S. context) relevant to the main research question; (2) use a randomized controlled trial (with or without baseline control), or a quasi-experimental approach in which baseline controls on main outcomes

were included; (3) be conducted in a country classified as a “low or middle income nation” by the World Bank at the time the intervention being studied was implemented; (4) include at least one quantifiable main outcome measure (enrollment, attendance, dropout, or progression); (5) be published or made available before December 2009, without regard to language or publication type; and (6) include data on participants from 1990 or beyond.

If a study satisfied those criteria, we then examined it for quantifiable measures relevant to the supplemental question on learning outcomes. These studies are not representative, of course, of all evaluations that included learning outcomes, but only of those studies that included learning outcomes along with at least one quantifiable outcome of enrollment, attendance, dropout and progression.

**Data collection and analysis**

A preliminary instrument was designed to extract data on substantive and methodological characteristics from each of the reports. Standardized mean difference (Cohen’s d) effect sizes were computed for the first effect reported in each study. Given the presumed heterogeneity of true effects in the population, analyses of effect sizes were estimated using random effects models. Main effects were analyzed for each main outcome reported: enrollment, attendance, dropout, and progression. Supplemental learning outcomes reported within the same studies were also coded; these included math, language, standardized assessment scores, and other achievement measures. Seven moderator analyses were also conducted.

**Results**

The sample includes 73 experiments and quasi-experiments. Across all interventions, the average effect size was positive in direction for all outcomes, and was largest for enrollment (*d*=.18; 95% CI[.13-.24]), attendance (*d*=.15, 95% CI [.10-.20]), progression (*d*=.13, 95% CI [.08-.18]), math (*d*=.16, 95% CI [.10-.23]) and language (*d*=.18, 95% CI [.12-.25]) outcomes. However, the results were not uniform across every study; given the large variation in programs, participants, settings and designs, there was no surprise that there was significant heterogeneity in effect sizes in these main analyses. Examining only outcomes of enrollment and attendance (n=59), studies that focused on new schools and other infrastructure interventions (*d*=.44, 95% CI [.40-.47]) reported the largest average effects. Studies that were conducted either in Europe or Central Asia (*d*=.58, 95% CI [.23-.93]), or East Asia and the Pacific (*d*=.36, 95% CI [.25-.48]), were also associated with larger average effects.

**Authors’ conclusions**

Based on the evidence presented in this report, interventions that address getting children into school and keeping them there have, on average, positive effects. This is also true of learning outcomes reported within those same studies. Although effects could be considered small, they represent 3-9% increases in positive outcomes compared to the control/comparison group in the studies. Policymakers would have to assess whether such outcomes are worth investments, given costs of implementation and how widespread the problem is that the intervention will address.

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# 1. Background

Education is critical to economic development and social welfare in developing nations. For example, the second Millennium Development Goal adopted by world leaders in 2000 is universal primary education for all boys and girls, while the third called for the elimination of gender disparities in education. Prioritizing education in such a way has several rationales. For one, investments in education are believed to yield returns in poverty reduction, improved health outcomes, and economic growth (Hannum & Buchmann, 2004; Herz & Sperling, 2003; UNESCO, 2007). In addition, increased access to education can lead to increased political participation and more equitable sharing of economic and political power (Birdsall, 1999). Education for girls is considered particularly critical, as improvements in the infant mortality rate, child nutrition, and school enrollment are closely associated with higher education among mothers (Birdsall, Levine, & Ibrahim, 2005; Herz & Sperling, 2003; World Bank, 2008). Yet, more than 100 million primary school-aged children are not in school and, of those that are, many—49 percent in Africa, for example—do not complete primary school (Birdsall, Levine, & Ibrahim, 2005). These and other data indicate wide variation in enrollment rates across developing country regions and by level of school (primary versus secondary; see Table 1).

**Table 1: 2007 Primary and Secondary School Net Enrollment Rates by Developing Nations Region (World Bank, 2012)**

| Region | Primary | Secondary |
|---|---|---|
| East Asia & Pacific | 93.5 | 68.4 |
| Europe & Central Asia | 93.2 | 82.1 |
| Latin America & Caribbean | 94.3 | 72.8 |
| Middle East & North Africa | 91.9 | 65.5 |
| South Asia | 85.8 | 48.1 |
| Sub-Saharan Africa | 74.0 | 26.2 |

Low educational attainment—or the inability of students to complete their primary and secondary school education—in the developing world is the combined result of children who do not enroll, children who do not progress, and children who drop out (World Bank, 2004). Children may not enroll or complete their schooling for a number of reasons. Research indicates that there are economic and other structural forces that present barriers. For example, in some countries, such as India, Mali, and Burkina Faso, school enrollment is very low due to the cost of schooling (both direct and opportunity costs), poor school infrastructure, teacher shortages, and safety and sanitation problems (Birdsall, Levine, & Ibrahim, 2005). In other areas, such as several Latin American countries, enrollment may be nearly universal, but retention and completion may be quite low for a myriad of reasons, including those mentioned above, as well as poor health of students or members of their households (Glewwe & Miguel, 2008; UNESCO, 2007), teacher absenteeism or malfeasance (World Bank, 2004), and curricula that do not match students' needs (Glewwe, Kremer, & Moulin, forthcoming). Value systems held within the country may also diminish the importance of enrolling children [particularly girls] in school (e.g., Academy of Educational Development Global Center, 2010; Brembeck, 1962), or parents may prioritize their children working to earn much needed immediate funds rather than attending school (Hillman & Jenker, 2004).

Furthermore, developing nations face significant school enrollment and completion disparities between segments of the population, such as between lower and upper income households, boys and girls, urban and rural dwellers, and combinations of these factors (Birdsall, Levine, & Ibrahim, 2005). For example, in India the gap in enrollment between boys and girls from the richest households is only 2.5 percent, whereas the gender disparity for children from the poorest households is 24 percent (Filmer, 1999 as cited in Birdsall, Levine, & Ibrahim 2005). In many African nations, rural rates of enrollment lag far behind the very modest national rates, particularly for rural girls, whose rate of enrollment is less than 15 percent in several countries (Birdsall, Levine & Ibrahim 2005). In addition, ethno-linguistic diversity, disabilities, and conflict situations in fragile states create further barriers to school participation in developing nations (Birdsall, Levine, & Ibrahim 2005).

In light of compelling evidence that links expanded education systems to socioeconomic development while highlighting the importance of policies to offset inequality in access to schooling (Fiszbein & Schady, 2009; UNESCO, 2007), and spurred by the donor community and such initiatives as the Millennium Development Goals and Education for All, governments in developing nations are, to varying degrees, making efforts to increase enrollment and equity. Building new schools to increase ease of access in remote areas is one intervention used in developing nations (e.g., Filmer, 2004). Other efforts include improving school infrastructure and safety and abolishing school fees, as well as implementing targeted policies to reach the most marginalized children. Such policies include school feeding programs, flexible schooling models for working children, school-based health interventions, and various types of financial subsidies and conditional cash transfer systems. For example, several Latin American governments and non- governmental partners have experimented with programs that transfer money directly to disadvantaged households—such as in rural, indigenous, migrant, or slum communities—in exchange for children's school enrollment and attendance (Fiszbein & Schady, 2009; UNESCO, 2007). In Asia, for example, such stipend programs encourage the transition of girls to secondary school (UNESCO, 2007).

In addition to expanding access to schooling and increasing enrollment and persistence, measuring learning achievement is an essential, although methodologically challenging, part of improving education in the developing world (Birdsall, Levine, & Ibrahim, 2005). Enrollment and persistence data are not necessarily good predictors of learning outcomes (ibid). That is, it is not enough merely to fill school spaces, but children must also learn if economic and social priorities are to be achieved. Increasing enrollment and increasing educational quality should go hand-in-hand, as poor children drop out with greater frequency when the quality of schooling is low (ibid). Many interventions, including those mentioned above, as well as teacher training and incentives, textbook provision, and health inventions such as deworming and providing nutritional supplements are undertaken with the goals of improving both enrollment and learning. In this review, data on achievement outcomes (e.g., test scores) were also collected and analyzed, even if the primary goal of the intervention was to increase student enrollment.

Considerable funding for initiatives to improve school enrollment has brought with it a concomitant increase in accountability. Donor agencies and governments want to know if the funds they have put toward such programs are having positive impact. This is not the only question they are asking of evaluation, but it is an important one. There is some frustration about the lack of knowledge about impact, as expressed in the 2006 Center for Global Development report:[1]

> *After decades in which development agencies have dispersed billions of dollars for social programs, and developing country governments and nongovernmental organizations (NGOs) have spent hundreds of billions more, it is deeply disappointing to recognize that we know relatively little about the net impact of most of these social programs.*

In recent years, there has been something akin to a "randomized revolution" in the developing world, as donors and governments are increasingly asking for impact evaluations that provide more credible estimates of effect (e.g., Duflo & Kremer, 2005; Kremer & Holla, 2009; Newman, Rawlings & Gertler, 1994). Evaluations of some of these recent policies and programs to increase school enrollment and persistence in developing nations include a number of randomized field trials and rigorous quasi-experimental studies. Randomized experiments evaluating conditional cash transfer programs in Latin America include the seminal Progresa experiment in Mexico, which gave educational grants to poor mothers in exchange for their children's good attendance. Communities were randomly assigned to intervention or control conditions, and positive impacts for school enrollment and other factors were demonstrated (Schultz, 2004). Similarly, in Ecuador, a lottery

provided cash vouchers to randomly selected families in exchange for enrolling their children in school; control families were placed in a "wait-list" condition until the study was completed. The early results were positive, increasing school enrollment by 10 percent and reducing child labor by 17 percent (Lopez-Calva, 2008). In addition, Filmer and Schady (2006) found that a scholarship program for girls in Cambodia making the transition from primary to secondary school had a large, positive effect on enrollment and attendance.

Randomized trials of school-based health interventions include a school feeding program in rural Peru, in which schools were randomized to implement a high- quality, ready-to-eat breakfast program or to a control group, with positive results for school enrollment and other outcomes, including test scores (Cueto & Chinen, 2008). Glewwe and Miguel's (2008) review randomized evaluations of school-based health interventions such as that of Miguel and Kremer (2004) found that absenteeism in Kenyan schools in which students received deworming treatment was 25 percent lower than in comparison schools, and that deworming increased schooling by 0.14 years, but without an accompanying increase in test scores. Recent randomized evaluations of other types of programs aimed at increasing enrollment, completion, and achievement include that of Glewwe, Kremer, and Moulin (2007), who found that providing textbooks to students in randomly selected rural primary schools in Kenya had no effect on dropout or repetition rate or on test scores. The Millennium Challenge Corporation funded Mathematica Policy Research to conduct a regression discontinuity study on the impact of school construction and other associated interventions on female student school enrollment in 132 communities in Burkina Faso; they reported positive results on enrollment and test scores when compared to the 161 communities not selected for the treatment (Levy, Sloan, Linden & Kazianga, 2009). All of these studies highlight the increased use of randomization and well-equated quasi-experiments in the developing world context.

[1] Thanks to the Coalition for Evidence-based Policy for identifying this quote in its email update of October 6, 2008.

Given the importance of education, particularly to outcomes in the most economically challenged nations, the number of interventions that have been implemented to address education in developing nations, and the increase in relevant controlled impact evaluations, the need for a systematic review seems clear. There have certainly been many good reviews relevant to education in the developing nation context (e.g., Fuller, 1987; Hanushek, 1995; Kremer & Holla, 2009).

To our knowledge, a systematic review of randomized controlled trials and quasi-experiments of strategies in developing nations to get children into school (enrollment) and keep them there (attendance, persistence, continuation) has not yet been reported, nor has any review looked at outcomes focused on learning within those same studies, although there are more focused reviews in progress with funding from the U.K. Department of International Development (e.g., school fees, by Morgan, Petrosino & Fronius, 2010; and teacher attendance by Cueto, Guerrero, Leon & Sigamuru, 2010). By systematically gathering and analyzing rigorous research about the program effects of primary and secondary school enrollment and completion policies, our review will hopefully provide evidence to inform the next wave of funding, intervention and evaluation efforts in this area.

## 2. Objectives

For this project, our objectives were to respond to the following main and supplemental questions:

- Main Question: What are the effects of interventions implemented in developing countries on measures of students' enrollment, attendance, graduation, and progression?
- Supplemental Question: Within those studies that report the effects of an intervention on measures of students' enrollment, attendance, graduation or progression, what are the ancillary effects on learning outcomes as measured by students' test scores, grades, and other achievement measures?

# 3. Methodology

## 3.1 Criteria for inclusion and exclusion of studies in the review

For this project, only evaluation studies that had the following characteristics were included. Included studies:

(1) *Assessed the impact of an intervention that included primary or secondary school outcomes (corresponding to Kindergarten-12th grade in the U.S. context or approximately age* 5-18*) relevant to the main research question.*

Programs designed to boost preschool, college, or university enrollment were not included, although one evaluation of the impact of an early intervention program on later primary/secondary enrollment was included (the Kagitcibasi, Sunar, & Bekman, 2001 evaluation of Mother-Child Education Program).

(2) *Used a randomized controlled trial, or a quasi-experiment with evidence of baseline control on a main outcome*

Our review includes evaluations that randomly assigned entities (at any level) to intervention or control conditions. The control or comparison group (the counterfactual) was either a condition that received no intervention or received the usual or standard practice. The rationale for including experiments seems well-founded, as randomized experiments, if implemented well, can provide statistically unbiased estimates of an intervention's effect and control for both known and unknown potential confounders (Boruch, 1997). We included randomized experiments regardless of whether the investigators reported baseline measurement and/or controls. This is because randomization theoretically distributes all known and unknown variables at baseline, whether they are measured or not (Boruch, 1997). We also included regression discontinuity designs in which a predefined cut-off score determined program eligibility and then program impact around the cut-off score. Regression discontinuity designs, theoretically, identify the sole reason for treatment selection (the cut-off score) and allow investigators to model it to identify an unbiased treatment effect (Cook, Shadish & Campbell, 2002).

However, because randomization or regression discontinuity is not always possible for evaluations of certain polices or programs, we also included quasi-experimental designs that employed controls for baseline or pretest differences on a main outcome. We based this decision on prior research examining the alignment of estimates from quasi-experiments to randomized experiments, finding that controls for baseline differences via matching or other processes were most important for achieving closer approximations to estimates from randomized controlled trials (RCTs; e.g., Bloom, Michalopoulos, Hill, & Lei, 2002; Glazerman, Levy & Myers, 2003).

Based on this guidance from the literature, quasi-experiments included here were one of the following two types: (1) studies that used matching methods such as propensity scores that explicitly included baseline measures of enrollment to "force" equity between groups on the baseline measure; or (2) designs that included a variety of methods such as a combination of propensity score or covariate matching with difference-in-difference techniques. Thus, quasi-experiments that relied solely on cross-sectional designs without baseline controls on at least one of the main outcomes of interest were excluded

(e.g., Cameron, 2002; Centre for Population and Development Activities (CEDPA), 2001; Walingo & Musamali, 2008; Yap, Sedlacek & Orazem, 2002).

Although randomized experiments are considered superior to quasi- experiments in terms of controlling all observable and unobservable confounders, there is conflicting literature on whether the estimates from quasi-experimental designs approximate those from randomized experiments (e.g., Oliver, et al., 2008). We include both types of studies and examine study design as a moderator in our later analyses.

(3) *Were conducted in a country classified as a "low or middle income nation" by the World Bank at the time the intervention being studied was implemented*.

The World Bank determines low and middle income nation status by calculating the Gross National Income (GNI) per capita, i.e., the average citizen's income. As of 2008, 151 nations were included in these categories. The categories are: low income ($975 or less); lower middle income ($976-$3,855); and upper middle income ($3,856-$11,905). Low and middle income nations are often referred to as "developing economies" and overlap considerably with the United Nations listing of "developing nations."

We used the World Bank listing for the time period closest to the start of the intervention. For the most part, there was very little fluctuation in the list, with the exception of nations that were newly created.

(4) *Included at least one quantifiable main outcome measure of school enrollment, attendance, dropout or progression*.

We had to be able to compute an effect size from the data reported in the evaluation or be able to acquire it from the principal investigators.

Unfortunately, several studies that met all of the criteria for this review did not provide the data necessary for the computation of an effect size (e.g., Clark, et al., 2005; Jalan & Glinskaya, no date; Kremer, Moulin, & Namunyu, 2003).

One important question is whether interventions that enroll or otherwise bring more children to school have any impact on learning (i.e., more students now strain existing resources, etc.). Thus, as a supplemental measure, we also collected information on learning outcomes (e.g., grades, test scores). However, if the report did not include one of the main outcomes of getting children into school and keeping them there (enrollment, attendance, graduation, and progression), it was not included. Thus, the data on learning outcomes should not be viewed as being representative of all educational interventions in developing nations, but only pertain to the programs and policies that included one of the main outcomes discussed above. This is the strategy used by Lipsey in his seminal study of juvenile delinquency prevention and treatment program evaluations (1992), and a more recent Campbell Collaboration review of dropout prevention and intervention programs (Wilson, et al., 2011).[2]

We did collect data on other intervention impacts, such as those on behavior, health, child labor, costs, equity, attitudes or satisfaction levels if these were reported, but very few evaluations (comparatively) did so, and they are not analyzed here.

Also note that our prioritization of enrollment, attendance, dropout and progression outcomes as main measures may not have matched how investigators in original studies

prioritized their outcome measures. For example, in a study of an educational reform, the investigators may have collected a wide range of measures about the schools, staff and students; outcomes such as attendance or progression may have been just one of many measures used to assess the overall impact of the reform.

(5) *Published or made available before December 2009, without regard to language or publication type*.

We searched for trials published up to and including December 2009. Studies published or available after January 2010 were not eligible, underscoring the importance of updating this review, given the increase in relevant studies. In concert with the International Initiative of Impact Evaluation (3ie) and Campbell Collaboration principles, we attempted to find English and non-English studies, and included published and unpublished studies (e.g., from conference papers, dissertations, technical reports). We also had some reports translated into English so that we could review them.

(6) *Included data on participants from 1990 or beyond*.

So as to be as relevant to current policy contexts as possible, we focused on studies that included data on participants from 1990 or later. Some studies may have published papers that used data from large scale administrative data sets generated decades earlier (e.g., Cutler, et al., 2009). Those studies were not included in the review.

## 3.2 Search strategy for identification of relevant studies

Our goal for the literature search was to identify relevant reports in both published and unpublished literature. See Appendices 8.1 and 8.2 for the complete search strategy. Many of the databases in Appendix 8.1 include the fugitive or grey literature (e.g., ERIC). We also included searches of the internet because such searches can identify reports that are made available at websites but are not published in journals. Our contact with colleagues was designed to get at more of the grey literature. To accomplish our search, we used five major strategies:

### *3.2.1 Electronic Searches of Bibliographic Databases*

Researchers searched available online resources and databases at the University of Pennsylvania and Bridgewater State College including ERIC, PAIS International/Archive, and Sociological Abstracts. Appendix 8.1 contains a complete list of databases that were searched, and Appendix 8.2 presents a full listing of the keywords used in each database.

### *3.2.2 Hand Searches of Relevant Journals*

Because electronic searches can miss relevant studies, we “hand searched” (i.e., visually inspected the table of contents and the articles) five journals: *Economic Development and Cultural Change, International Journal of Educational Development*, *Journal of Development Economics*, *World Bank Research Observer*, and the *World Bank Economic Review*. These journals had been identified in our early searches as being the most prolific in publishing evaluative studies relevant to this review. We hand searched every issue of each of the five journals through 2009.

[2] The authors only reported on dropout outcomes, but plan a future paper that focuses on the achievement measures reported in those same studies (i.e., those that included at least one quantifiable outcome of dropout).

### *3.2.3 Citation Chasing*

The reference section of every retrieved report was checked to determine whether any possible eligible evaluations were listed. We checked references for reports that were retrieved that were ineligible for this review, including syntheses, non- experimental studies, methodological papers, and descriptions of policies or interventions.

### *3.2.4 Contacting the "Informal College" of Researchers in this Area*

There is a network of researchers involved in conducting experimental and quasi-experimental studies relevant to developing nations. We identified the lead authors of such studies or relevant documents (e.g., reviews, non-evaluative studies), identified their email addresses from a *Google* search, and emailed them query letters. A full listing of all persons who were contacted is listed in Appendix 8.3.

Some of these researchers were extraordinarily helpful, such as Jere Behrman of the University of Pennsylvania and Paul Glewwe from the University of Minnesota.

### *3.2.5 Internet Searches and Specialized Holdings*

We also used the "advanced search" options in *Google* and *Google Scholar* for broad searches of the World Wide Web. This was supplemented by specialized searches of specific websites that could reference relevant holdings such as the Center for Population Development and Activities, the Massachusetts Institute of Technology's Poverty Action Lab, Yale University's Innovations for Poverty Center, the National Bureau for Economic Research (NBER), the Network for Policy Research, Review and Advice on Education and Training (www.norrag.org), the Network of Networks Impact Evaluation Initiative (NONIE), the World Health Organization (e.g., their Annotated Bibliography of Selected Research on Civil Society and Health), the Organization for Economic Co-operation and Development (OECD), the World Bank (especially their impact evaluation section), and the International Initiative for Impact Evaluation's (3ie) database of impact evaluations.

## 3.3 Keyword strategies for bibliographic databases

The databases listed in Appendix 8.1 can be somewhat idiosyncratic with regard to their contents. Thus, we believed the best strategy was to conduct a broad search of the available databases that erred on the side of sensitivity rather than specificity. In other words, we would rather have many titles and abstracts to sift through due to broader searches rather than potentially miss relevant citations because our search terms were drawn too narrowly.

As outlined in Appendix 8.2, our search strategies were of two major types. First, and for most databases, we developed a long list of keywords to identify three major study eligibility criteria: (1) keywords relevant to developing nations; (2) keywords relevant to the outcomes of enrollment, dropout, persistence, etc.; and (3) keywords relevant to experimental and quasi-experimental evaluations. These were used successfully in most databases; in a few instances, however, the yield was still so large that we instituted a (4) fourth criterion of keywords relevant to youth. Such searching is an iterative process. We modified our terms as we retrieved studies; Appendix 8.2 represents our results from our final pass through the databases. The second search strategy focused on databases that did not permit complex searches. In these, we searched by using one or a few keywords at a time.

## 3.4 Retrieving and final screening of studies

The search did identify a large number of citations and abstracts. Many citations were easily excluded because they were not relevant to the proposed review. One of the WestEd co-authors (Petrosino, Morgan or Fronius) reviewed citations and determined if the cited study should proceed to a second screening, i.e., was a potentially relevant study. If so, the full text documents of those potentially eligible studies were retrieved. When a full text report was received, one of these same authors read it to ensure that it met the minimum evaluation design requirement and included at least one outcome of school enrollment. If determined to be eligible, the lead author (Petrosino) reviewed the study again to ensure it met all inclusion criteria. Thus, in most cases, two independent persons reviewed a study for eligibility at the final screening stage, except when the lead author identified the study and confirmed its eligibility. Appendix 8.4 provides a list of studies excluded at the final screening stage.

## 3.5 Examples of studies included in the review

The Burkina Faso evaluation conducted by Mathematica Policy Research for the Millennium Challenge Corporation (Levy, et al., 2009) is an example of an evaluation that was included in our review. Using a regression discontinuity design, the study compares the results, at the village level, on girls' school enrollment for those villages that received new school construction and other interventions (via the BRIGHT program) versus those that did not. Results indicate that girls' enrollment increased 16-19% in the experimental villages. The Progresa/Opportunidades experiment in Mexico is another example of a study that was included in our review. In this study, cash transfers conditional on child attendance in school (and other conditions) were provided to households in experimental areas. A slew of reports have indicated that Progresa had positive impacts on school enrollment and attendance (e.g. Schultz, 2004).

## 3.6 Examples of studies not included in the review

One study that was not included in our review is that of Bobonis, Miguel, and Sharma (2006), which evaluated a health program that provided iron supplementation and deworming medicine to preschool-age children in poor urban areas of Delhi. Although this was a randomized evaluation in a developing nation that reported effects on school absenteeism, we did not include it in our review because it did not evaluate the impact of the health strategy on primary or secondary school enrollment, but on preschool attendance. Another example of a study that was not included in our review was conducted by Lockheed, Vail and Fuller (1986) in Thailand. They studied the impact of providing textbooks to Thai students. Although there was a question of whether they used a truly equated comparison group, the study was nonetheless excluded because it did not include any outcome of enrollment, but focused exclusively on academic achievement.

## 3.7 Extracting information from each study

We designed a coding instrument to guide us in extracting information from each study (see Appendix 8.5). Although the instrument contains several open-ended items, these were collapsed in some instances into a smaller number of categories to permit analyses. The instrument contains items that describe the characteristics of the researcher (e.g., field or discipline), the publication (i.e., type of document and year published), the setting or context

(country and classification of economy), the evaluation design (whether RCT or QED), methodological quality (i.e., how the study handled selection bias, the degree of attrition, and any program implementation compromises), the treatment condition, the control or comparison group, the participants (e.g., grade), and the outcomes (i.e., on enrollment and learning outcomes). Except for the coding reliability check discussed next, one person coded each study.

## 3.8 Coding reliability

To ensure that we achieved good coding reliability, the first three co-authors read and recorded information from a random sample of reports (12, or 17% of the final sample). We assessed coding reliability (i.e., inter-rater agreement) by using the percentage of agreement for each item, rather than reporting a global inter-rater reliability statistic. Items with lower rates of agreement (less than 80%) were investigated to determine the source of conflict. The authors held discussions to resolve disagreements and discuss coded items. For the most part, differences among authors stemmed from the varying levels of detail provided in study reports to respond to open-ended items. None of the items which are analyzed in this review had a rate of agreement lower than 80%.

## 3.9 Criteria for determination of independent findings

Our criteria for handling possible statistical dependencies were as follows:

*(1) One effect size per analysis*

Each study is represented by a single effect size in each analysis to prevent the analysis from being compromised by non-independence (multiple effect sizes from one study).

Evaluators reported their analyses at various time intervals and used various constructs that reflect school enrollment and persistence. For this review, to maintain just one effect size per analysis, we kept the four major outcomes distinct. That is, we analyzed the main outcomes of enrollment, attendance, dropout, and progression separately. We kept separate our analyses of the supplemental learning outcomes.

Few studies included more than one follow-up time interval. Consequently, we only report “first effects” and do not examine effects at additional follow- ups.

When studies included three or more groups in the design (e.g., multiple treatment groups), we only computed effect sizes for the treatment group that represented the strongest contrast with the control condition. In every case, this was the group that received the most intensive intervention, i.e., the most treatment components.

*(2) Our unit of analysis was the evaluation study, not the evaluation report.*

One perplexing issue we encountered in this review was the sheer number of reports and re-analyses using the same sample of data. In some instances, the same investigators, or other investigators obtaining the study data, published multiple articles on the effects of an intervention. For example, the aforementioned Progresa study has generated numerous reports or analyses using the same sample of data.

Our unit of analysis is the individual evaluation study and not the individual research article. When reports on the same study contained conflicting information, we contacted the original investigator(s) for resolution. The response to such questions was

understandably poor (given the lapse between the time a study was conducted and published and our contacting the study authors). Inconsistencies that would affect the conclusions about an intervention's impact were very rare.

If, however, a different set of investigators conducted a reanalysis using an improved study design (i.e., one that better handled possible selection bias), we used that study as the primary coding document. This happened on only one occasion (Meng & Ryan's 2004 reanalyses of the Food for Education intervention in Bangladesh).

*(3) An evaluation study was considered distinct if it used a different sample.*

Investigators sometimes published multiple articles or reports on the same intervention using *different samples*. Different study samples were coded as separate studies, even if the same general intervention was being investigated. For example, Kremer et al.'s (2003) study of decentralization occurred in two Kenyan cities (Busia and Teso). Because there were separate random assignments and separate samples, they are considered two different studies for purposes of this review. Although there may be a slight dependence in the estimates from the same multi-site study, the normal handling in meta-analysis is to treat these as separate and independent studies (Lipsey & Wilson, 2001).

*(4) The primary analysis or study design was the "most rigorous," or one that provided the most controls.*

If the same sample was being used in multiple designs, we focused on and coded the "strongest" or "most rigorous" design. For example, investigators may have conducted analyses using regression discontinuity design, regression controlled analyses, and difference-in-difference methods. To avoid dependencies in our analyses, only one of these designs should contribute to estimates of program impact. In this case, the strongest design methodologically was used. This was also true in those instances in which investigators reported Intent to Treat (ITT) analyses when using a randomized controlled trial, but also reported Instrumental Variables (IV) (with randomization as the instrument) or other Treatment on Treated (ToT) analyses. We always selected the more conservative ITT estimate.

Investigators sometimes reported on the results of multiple estimation models. We selected the model that included the most "controls" to compute the effect size estimates. If regression-adjusted estimates were reported for the experimental versus control groups, we relied on them for any quantitative synthesis because they theoretically reduced statistical "noise" that may have come from chance fluctuations or randomization violations (in the case of well implemented experiments) or uncontrolled variables (in the case of quasi-experiments).

*(5) Overall versus subgroup effects*

Another perplexing issue is that a few studies only reported outcomes by specific subgroups such as gender (male/female), school level (primary/secondary), type of geographic area (rural/urban) or grade level (1st-8th grades). This kind of specific subgroup effect can be very important to policy and practice decision-makers, but more difficult to handle in large reviews such as this one. In those few instances in which this occurred, we averaged effects over the included grades or across both boys and girls to obtain an overall effect for the intervention.

*(6) Individual level effects where possible*

Some studies reported analyses at multiple levels (e.g., Schultz, 2004), i.e., for schools or communities, households, and students. Our rule was to compute effect sizes for the analyses at the individual level, unless such data were not available in the original reports. In the latter instance, we computed effect sizes at the larger aggregate level (e.g., school) but conducted a post- hoc methodological analysis to compare differences in effect sizes when computing them based on using the sample sizes of individual students or from the larger aggregate samples.

## 3.10 Statistical procedures and conventions

Standardized mean differences (Cohen's *d)* were used as the effect size metric for all the main and supplemental outcomes of interest, which are appropriate for measuring group differences in mean levels of continuously measured outcomes (Lipsey & Wilson, 2001). All effect sizes were coded so that positive effect sizes represented better outcomes (e.g., higher enrollment, lower dropout). Standardized mean difference effect sizes were calculated as:

$$d = \frac{\bar{X}_{TX} - \bar{X}_{CT}}{s_{pooled}}$$

where the numerator is the difference in group means for the intervention and control groups, and the denominator is the pooled standard deviation for those groups. The variance of the standardized mean difference effect size was calculated as:

$$V_d = \frac{n_{TX}+n_{CT}}{n_{TX}*n_{CT}} + \frac{d^2}{2(n_{TX}+n_{CT})}$$

We chose the standardized mean difference (Cohen's *d*), as it is a very flexible effect size metric and many formulae are available to estimate effect size from information reported in study articles (e.g., regression coefficients, statistical test data, probability levels). Effect sizes and variances were calculated using David Wilson's online effect size calculator (http://www.campbellcollaboration.org/resources/effect_size_input.php). For example, one common transformation procedure we used was the logit transformation of binary proportions to standardized mean differences (Cohen's *d)*. The formula computes odds ratios for these data and then estimates *d* as follows:

$$d = \left[\ln\left(\frac{A*D}{B*C}\right)\right] * \frac{\sqrt{3}}{\pi}$$

where A and B are the counts of "successes" and "failures" in the treatment group, and C and D are the corresponding counts of "successes" and "failures" in the comparison group.

When primary studies reported results for aggregate groups as well as subgroups (e.g., male/female) we calculated the effect sizes and variances with standard formulae using the aggregate data only. When studies reported results only for subgroups but not for the aggregate groups, we calculated the aggregate effect size as the weighted mean across the

subgroups divided by the pooled standard deviation, as follows:

$$S_{Pooled} = \sqrt{TSS/(N-1)}$$

where TSS is the total sum of squares and N is the total sample size for all subgroups. These calculations ensured the statistical independence of all effect sizes included within a given analysis.

Appendix 8.10 presents the effect size and variance calculations for all studies, along with any notes regarding the effect size calculations. Because many of the included econometric studies used complex statistical models that adjusted for baseline and other covariates, the variances were rescaled when possible using the procedures outlined in Wilson (2011).

The data were entered into the Comprehensive Meta-Analysis (CMA) version 2 software program. We used CMA algorithms to statistically combine results from the evaluations. Forest plots generated by Stata are used to display the results from the effect sizes, including the effect size and 95% confidence intervals.

Because of the presumed heterogeneity in the true effects across interventions, samples, countries, and outcomes, we used random effects models in our statistical analyses. Random effects models tend to be more conservative than the fixed effects approach.

We report overall effects across all interventions on the four major outcomes (enrollment, attendance, dropout and progression) and on the four types of learning outcomes (math, language, standardized assessment scores, and other achievement measures). Table 2 presents how we organized outcomes across the studies.

**Table 2: Organization of Outcomes**

| **Main Outcomes (mandatory)** | | | |
|---|---|---|---|
| Enrollment<br>Enrollment<br>Participation | Attendance<br>Attendance<br>Absenteeism | Dropout Dropout<br>Retention Completion<br>Graduation | Progression<br>Progression to Next Grade Repetition |
| **Supplemental (if study had at least one primary outcome only)** | | | |
| Math Scores<br>Math | Language Scores<br>Reading<br>Verbal<br>Native Language | Standardized Assessment Scores<br>National Test Scores<br>Average Test Scores<br>Oral/Written Scores | Other Achievement<br>Grades<br>Classes Passed/Failed |

We then descriptively examine a number of moderators. These moderators are approached and interpreted descriptively rather than statistically, as they are often based on small numbers of studies (the “small cell” problem), and such analyses can be significant by chance if large numbers of variables are considered (the “capitalizing on chance” problem). Our analyses examined:

- *Broad intervention type*. This can be risky, as some interventions can be classified into more than one group. However, these groupings can be very persuasive in persuading readers about which bundle of interventions "work" (e.g., Greenleaf & Petrosino, 2008).
- *Specific intervention type*. An important policy question is whether developing nations and donor agencies are getting more "bang for the buck" using one particular approach or another. We compared the average effect sizes between the discrete types of interventions.
- *World Bank classification of economies.* We examined effect sizes by The World Bank three tiers of developing economies (low income, lower middle income, and upper middle income).
- *World Bank classification of developing regions*. We examined effect sizes by the World Bank classification of developing nations into six different regions of the world: East Asia and the Pacific, Europe and Central Asia, Latin America and the Caribbean, Middle East and North Africa, South Asia, and Sub-Saharan Africa. Note that our review did not identify any eligible studies from Middle East and North Africa.
- *Type of evaluation design.* An important question is whether the estimates from randomized experiments are substantially different from those reported in studies that used non-random assignment. This review examined the average effect size for the 52 randomized experiments and compared it to the average effect size for the 21 quasi-experiments in the sample.
- *Whether the intervention specifically targeted females or not*. Many interventions are specifically designed to increase female school enrollment. We examined effect sizes for those eight interventions that had exclusively female samples and compared it to the vast majority of studies those that focused on both boys and girls.
- *Whether the study included outcomes for primary or secondary school students*. We report effects for interventions that focused on primary schools, secondary schools, or included both types of schools.

We also report results for two different analyses related to methodology. The first was to examine how the effect sizes reported in the studies varied across dimensions of study quality (as rated "low," "moderate" or "high" by coders). The second was to determine the impact of our decision to use the individual sample sizes from the studies instead of the aggregate cluster sample sizes.

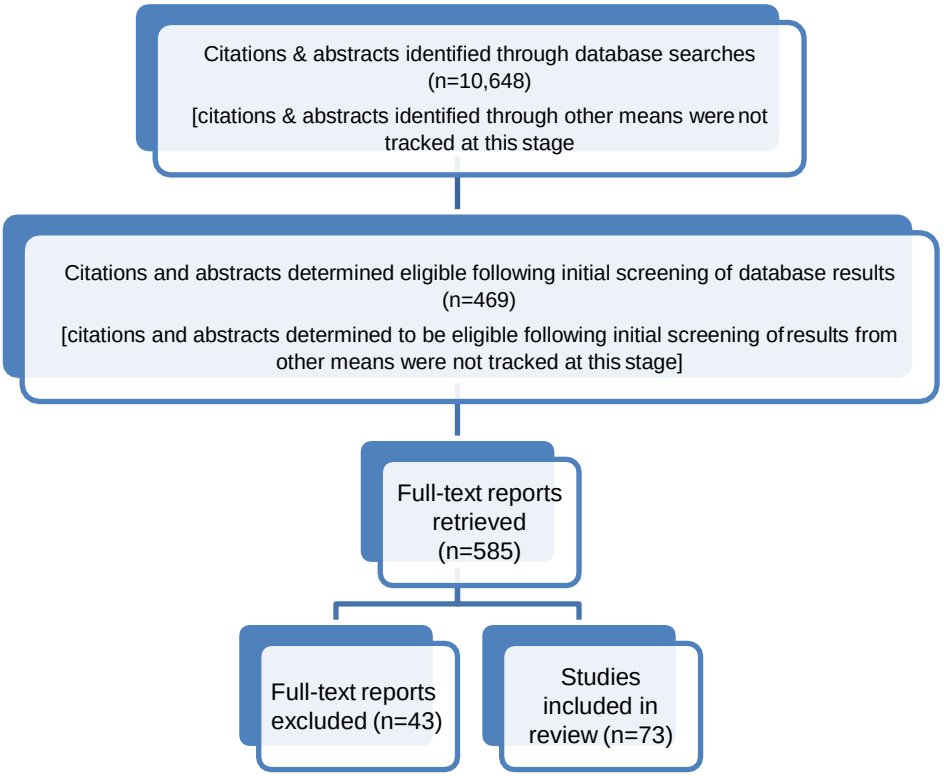
Finally, missing data were not a problem for the main and moderating analyses reported in this document.

# 4. Results

## 4.1 Pipeline of studies

116 randomized experiments and quasi-experiments met our initial screening for eligibility. All of these were coded for study characteristics but not for effect sizes. We then conducted a second screening to again ensure that there was evidence of baseline control and that the original studies reported on participant data from 1990 or after (eligibility criterion 6). This left 81 studies. Six studies, as mentioned earlier, did not report outcome data that we could use to create a quantifiable effect size, and three studies used overlapping samples of schools and students (we selected the study that tested the strongest treatment and removed two from our review). This left 73 total studies in our final sample. The pipeline of studies is illustrated in Figure 1.

**Figure 1: Pipeline for Review Sample**




## 4.2 Descriptive statistics

The sample of studies was, as expected, very diverse. Studies were conducted in 27 different nations, with Kenya (N=12), India (N=9), Bangladesh (N=6), Colombia (N=5) and Jamaica (N=5) the most common. Not surprisingly, as Table 3 shows, most studies were conducted in the poorest developing nations (51% in Lower Income Countries or LICs); 34% were conducted in Lower Middle Income Countries (LMICs). All of the nations identified above except Colombia (which is defined as an Upper Middle Income Country [UMIC] economy) fall into those two classifications.

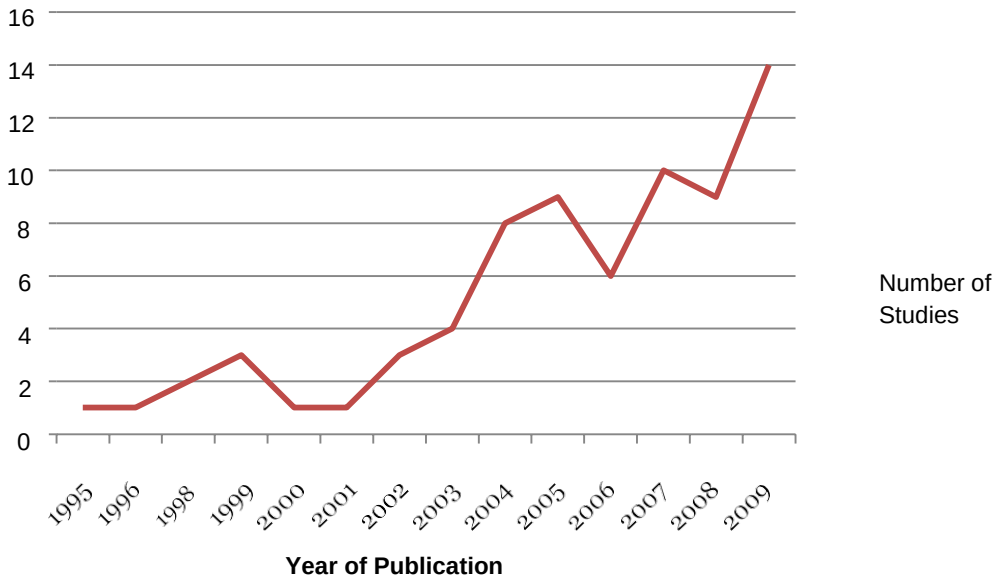
**Table 3: Included Studies by World Bank Classification of Economies**

| World Bank Classification of | N | Percent |
|---|---|---|
| | | |
| Lower Income Country (LIC) | 37 | 50.7 |
| Lower Middle Income Country (LMIC) | 25 | 34.2 |
| Upper Middle Income Country (UMIC) | 11 | 15.1 |
| **Total** | **73** | **100.0** |

Approximately 38 substantively different interventions were tested across these 73 studies; broadly, Conditional Cash Transfers (N=13), funding or grants to communities (N=5), school breakfasts or lunches (N=5), or remedial education or tutoring (N=5) were the most common. Appendix 8.6 includes the interventions and the studies in each category. Most of these programs targeted primary school-aged children (N=44, 60%), with 10 focusing exclusively on secondary school-aged children (14%). A minority of studies involved interventions that included both primary and secondary students (N=19, 26%). Nearly nine in ten studies (N=65, 89%) included both boys and girls in the intervention; the remainder focused exclusively on girls (N=8, 11%).

Fifty-two of the studies (71%) used randomization to assign participants to groups, and 21 (29%) used quasi-experimental procedures. This likely reflects the very strict processes we used to select quasi-experiments, and the "randomized revolution" discussed in the background to the review. These studies were published from 1995- 2009, and as Figure 2 indicates, there has been a large increase in the number of eligible studies since the early 2000s.

**Figure 2: Number of Included Studies by Year of Publication**




Most studies assigned individuals and larger aggregate units to treatment and control conditions. Most common were studies that assigned schools to treatment or control conditions (N=31; 43%), followed by the assignment of individuals to treatment and control

conditions (N=14, 19%). Some studies assigned villages to treatment and control conditions (N=13, 18%). As mentioned in the methodology section, this posed some problems in identifying the best way to compute effect sizes. Most of the studies included just one intervention and one control or comparison group (68%), but 14 (19%) used three or four groups in their designs. As mentioned earlier, when studies included more than one treatment group, we selected the condition that was most distinct from the control (e.g., the most intensive treatment group). This decision was based exclusively on the description of the intervention conditions in the study and not on the size of the treatment effects, and is an approach we have used in a prior Campbell Collaboration review (e.g., see Petrosino, Turpin-Petrosino & Guckenburg, 2010).

Most relevant impact studies identified in this review were conducted by economists (82%). It is common for economists to place working papers and reports online until publication in a journal. Indeed, only 21 studies (29%) were published in academic journals or books; the majority of reports (N=52, 71%) were working papers or reports by international organizations such as The World Bank. Our experience has been that most of these papers and reports find their way into journals eventually, although the time lapse can be extensive.

Most authors concluded that the intervention they studied had a positive impact on one of the main outcomes (N=42, 57%), but a large minority indicated no impact (N=21, 29%) and the remainder reported mixed results (N=10, 14%).

## 4.3 Broad intervention types and program theory

In this section, we provide brief descriptions of the five major types of interventions (and underscore our caution about doing so), and also provide the underlying rationale about why these bundles of interventions were expected to influence a main outcome of interest (e.g., Rogers, Petrosino, Huebner & Hacsi, 2000).

Unfortunately, most reports did not include an explicit program theory. Therefore, we provide an implicit program theory based on the information in the report. This was a strategy used by Dutch researchers in examining police practices (e.g., van der Knaap, Leeuw, Bogaerts & Nijssen 2007). With 73 studies and 36 interventions, we summarize these program theories for major “bins” (by size of bin) of interventions below. Appendix 8.8 provides a table of all studies, including the broad and specific types of interventions into which we classified them.

### *4.3.1 Economic (n=26)*

The largest category of interventions was economic. These included such interventions as conditional and unconditional cash and/or food transfers, vouchers for private school, microfinance opportunities in villages, school fee reduction or elimination, scholarships or fellowships to offset the costs of schooling, and materials such as uniforms to reduce household schooling costs.

Although such interventions are varied in terms of program components (including whether there are conditions and whether there is a health component), targeted participants and delivery, the general theories underlying why economic interventions should impact school enrollment outcomes fall into the following: (1) if program participation is based on meeting conditions, the economic benefit serves as an incentive that spurs parents to ensure youth in their households are enrolled in and attend school; (2) the economic benefit removes the

need for youth to work (at least during school hours) to provide household income, thereby eliminating a major barrier to child attendance; and (3) the benefit can also remove the costs of attending school such as fees or books, leading to parents being more willing to send their children to school.

Note that six programs offered fellowship or scholarship programs specifically for females (Filmer & Schady, 2006; Filmer & Schady, 2009; Kim, Alderman & Orazem, 1998; Kim, Alderman & Orazem, 1999; and Kremer, Miguel & Thornton, 2008[3]). In some countries, there is a stronger disincentive for parents to send their female children to school. Although the economic incentives mirror those discussed above, in at least one study, investigators posited that even boys' attendance may also increase because parents would request they escort their female siblings to school (Kim, et al., 1999).

### *4.3.2 Educational Programs and Practices (n=19)*

These were school or district level initiatives. These included such things as providing services or materials to students such as remedial education, computers, flip charts, textbooks, and English language training technology and software. They also included larger school-wide or district-wide initiatives, such as providing extra teachers (and reducing class size), providing incentives to teachers, monitoring teacher attendance, teacher training, empowering and funding parent school associations, and more comprehensive school reform and improvement efforts.

The underlying rationale for these programs is difficult to summarize given the diversity of programs, but the general gist is that these interventions seek to address student deficiencies or improve school quality. Students who are performing better in school or find school more interesting are more likely to attend, to stay, and to progress to the next grade. Similarly, as school and/or teacher quality increases, students may be more engaged, teachers may be more able to improve their pedagogy and address student needs, and students may find school more enjoyable and worthwhile and be more likely to attend and progress satisfactorily.

### *4.3.3 Health Care and Nutrition (n=14)*

These interventions include eight that tested health care interventions (and, on occasion, also included a preventive educational component) such as treatment for asthma, malaria, vitamin A deficiency, parasitic infections, and the ill effects of menstruation for pubescent girls. The underlying rationale about why these interventions will influence attendance is that children in certain impoverished nations will miss significant time at school due to illnesses or health conditions, and that treating the health conditions effectively will remove that barrier to school attendance. Six studies tested the effects of programs designed to provide nutritional meals to students, either in school or at their homes, and then tested whether the intervention improved one of the main outcomes of interest.

Theories about why health-related interventions would encourage youth to come to school were generally twofold. First, nutritional deficiencies hamper student attendance due to increased risk of illness, general fatigue, and inability to concentrate at school. For example, Jacoby, Cueto and Pollitt (1996) noted that overnight sleep leads to "decreased brain fuel"

[3] This report includes two eligible studies.

and providing a breakfast to students addresses this deficit. Indeed, Walker, Chang, Powell and Grantham-McGregor (2005) delivered their nutritional intervention early in the children's lives, noting that nutritional deficiency leads to stunting and influences proper development, including the ability to attend and succeed in school. A second rationale is that by providing nutritional meals at the school, parents have an incentive to send their youth to the campus. Kazianga, de Walque and Ackerman's (2008) intervention provided take home nutritional meals; this could only occur if the student showed up for school to receive the take home meal.

### *4.3.4 Building Schools and Infrastructure Improvements (n=7)*

This category included a wide range of interventions, including the construction of new schools (in conjunction with other components such as curriculum, textbooks, etc.), funding to municipalities to support infrastructure improvements (including to schools), and the building of new roads. The underlying assumptions about why these interventions should influence a main outcome are also varied, but include reducing the time a child has to spend to get to school, thereby removing a major impediment to youth enrollment and attendance. New roads would also increase household access to markets and thereby lead to increased household wealth, which would then remove the fiscal constraints discussed in the section on economic interventions. Addressing infrastructure of existing schools should result in more accessible buildings, leading to parents being more willing to send their youth to school and for children to stay there.

### *4.3.5 Providing Information or Training (n=7)*

This was also a very diverse category, and included such interventions as community-based programs that provide girls with opportunities to develop their skills (i.e., livelihood skills) or to reduce fertility (a major barrier to female participation in secondary school), providing information to parents about their children's learning or about the perceived benefits of education, community empowerment and participation, and parent training. Again, the underlying rationale for why these programs should influence main outcomes of interest is varied.

Decreasing the likelihood of female marriage and pregnancy would result in increased opportunity for schooling, particularly at the secondary level. Providing information to parents about perceived returns to education would counter myths and could lead more parents to keep their children in school. Empowering the community to hold public officials accountable for their school's performance may encourage the officials to improve schools, leading to the benefits discussed above under educational programs or practices. Providing parent training to young mothers could lead to cognitive improvements for their children and result in greater likelihood they will be enrolled in school and not fall behind.

Appendix 8.9 provides a table that outlines the barriers to school participation for youth, the interventions included under each broad type, key underlying assumptions or mechanisms by which the interventions should work, and the expected changes in main outcomes.

## 4.4 Meta- analysis

Using inverse variance random effects weights, we estimated the overall mean effect size *d* across studies separately for the different types of outcomes. Standardized mean differences (Cohen's *d*) are scaled in the analyses as positive if there was a positive impact

for intervention (e.g., if enrollment increased or dropout decreased), negative, such as -.10, if there was a negative impact (e.g., an increase in dropout or decrease in enrollment), and 0 if the effect for the intervention was identical for the treatment group and the control group (e.g., 95% enrollment rate in both groups).

Generally speaking, an effect size estimate of .10 reflects 1/10 standard deviation improvement for treatment participants compared to control participants.

Whether this is a substantively important policy or practice finding is sometimes difficult to determine, at least outside the policy context in which the review is being conducted. Rosenthal and Rubin (1982) attempted to make effect sizes more intuitive through the Binomial Effect Size Display (BESD), converting a standardized mean difference to percentage improvement of the treatment group compared to the control group. Thus, using BESD, and assuming a baseline enrollment rate of about 50% across treatment and control, a standardized mean difference of .10 would be interpreted as about 5% improvement in the intervention group (see Table 4). Whether this is a policy relevant finding would depend on how many participants the policy could impact across a jurisdiction, the cost of the intervention relative to another policy alternative, and other factors. Note that in the text in which we discuss the each of the analyses below, we round effect sizes to two decimal places.

**Table 4: BESD for Different Levels of STandardized Mean Differences (Cohen's D)**

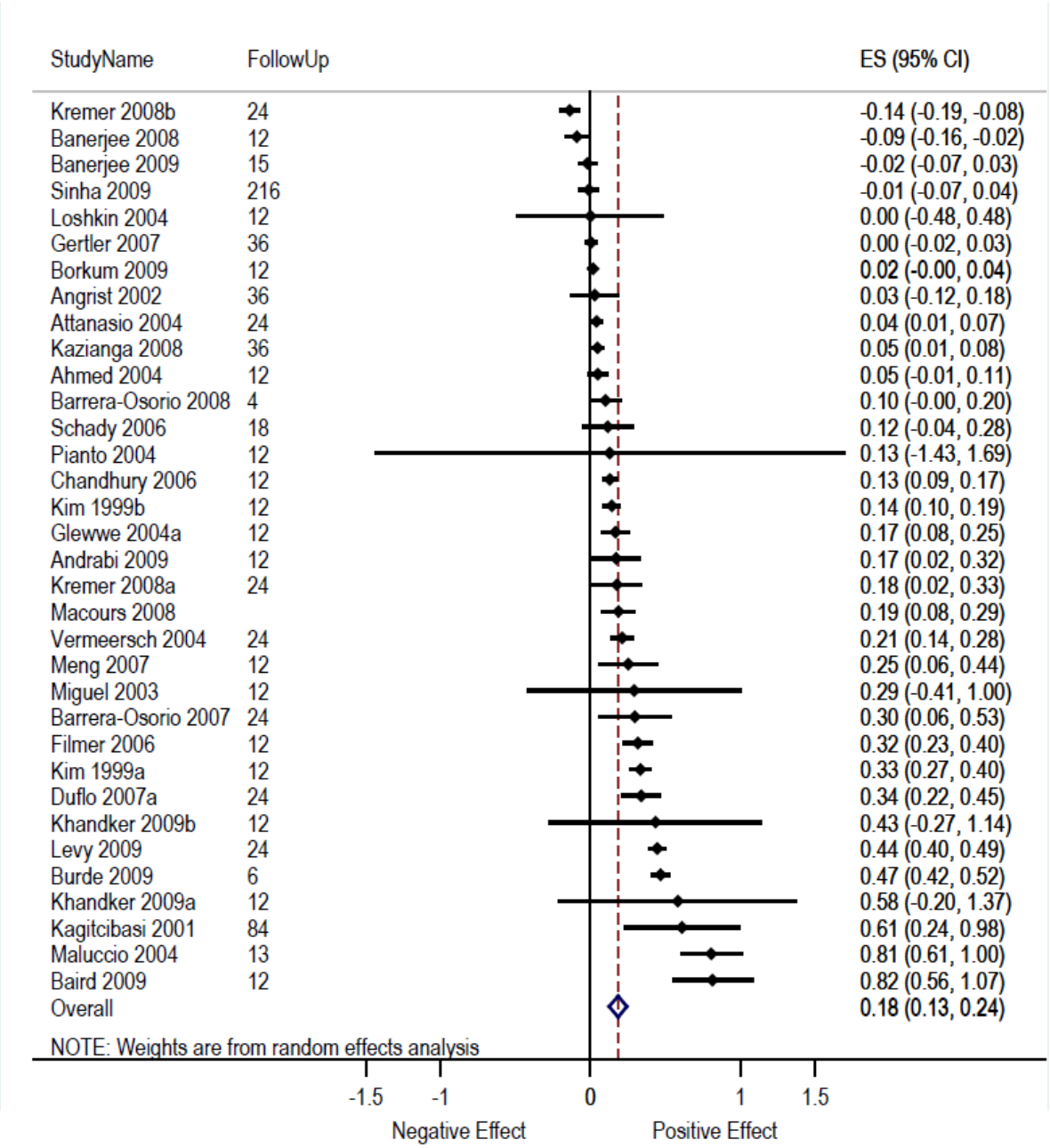
| Cohen's *d* | Percentage success Treatment | Percentage success Control | Difference in success rates |
|---|---|---|---|
| 0.00 | 50.0 | 50.0 | 0 |
| 0.10 | 52.5 | 47.5 | 5% |
| 0.20 | 55.0 | 45.0 | 10% |
| 0.30 | 57.4 | 42.6 | 14.8% |
| 0.40 | 59.8 | 40.2 | 19.6% |

Also note that for each analysis of overall intervention effects, we present heterogeneity data, including the $I^2$, the $tau^2$ (between studies) and the Q-test, all indicators of how well the mean effect represents the sample of studies in the analysis. As expected, given the variation in samples, interventions, countries, and design methods, the variability in effect size is also large.

## 4.5 Overall intervention effects, main analyses of enrollment and other outcomes

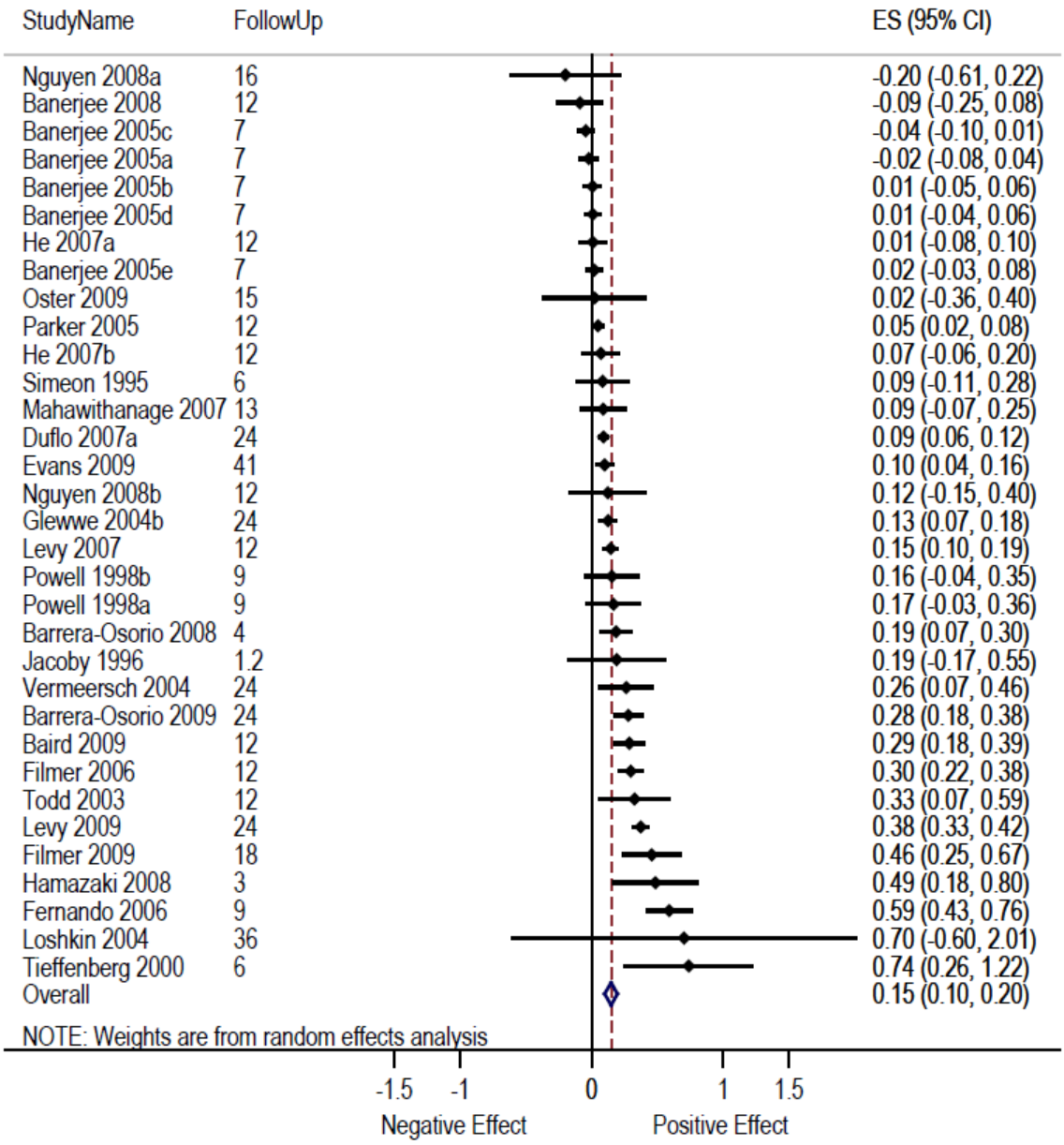
In this section, we report the average effects of the interventions on main enrollment and other relevant outcomes (attendance, dropout, and progression). In Figure 3, the results are presented for 34 studies that measured the impact of an intervention on an enrollment outcome. As mentioned earlier, this analysis includes only the first effect reported in the study; as indicated, the first follow-up measure ranged from 4 to 216 months. Collectively, the average treatment effect was positive (*d*=.18; 95% CI [.13-.24]), and ranged from -.14 to .82. Only five of the studies reported a negative effect on enrollment (to the left of zero). As expected, heterogeneity statistics indicate large variability across effect sizes (Q=875.94, df=33, p<.001; $I^2$=96.23; $tau^2$=.023).

**Figure 3: Main Effects on School Enrollment (n=34)**



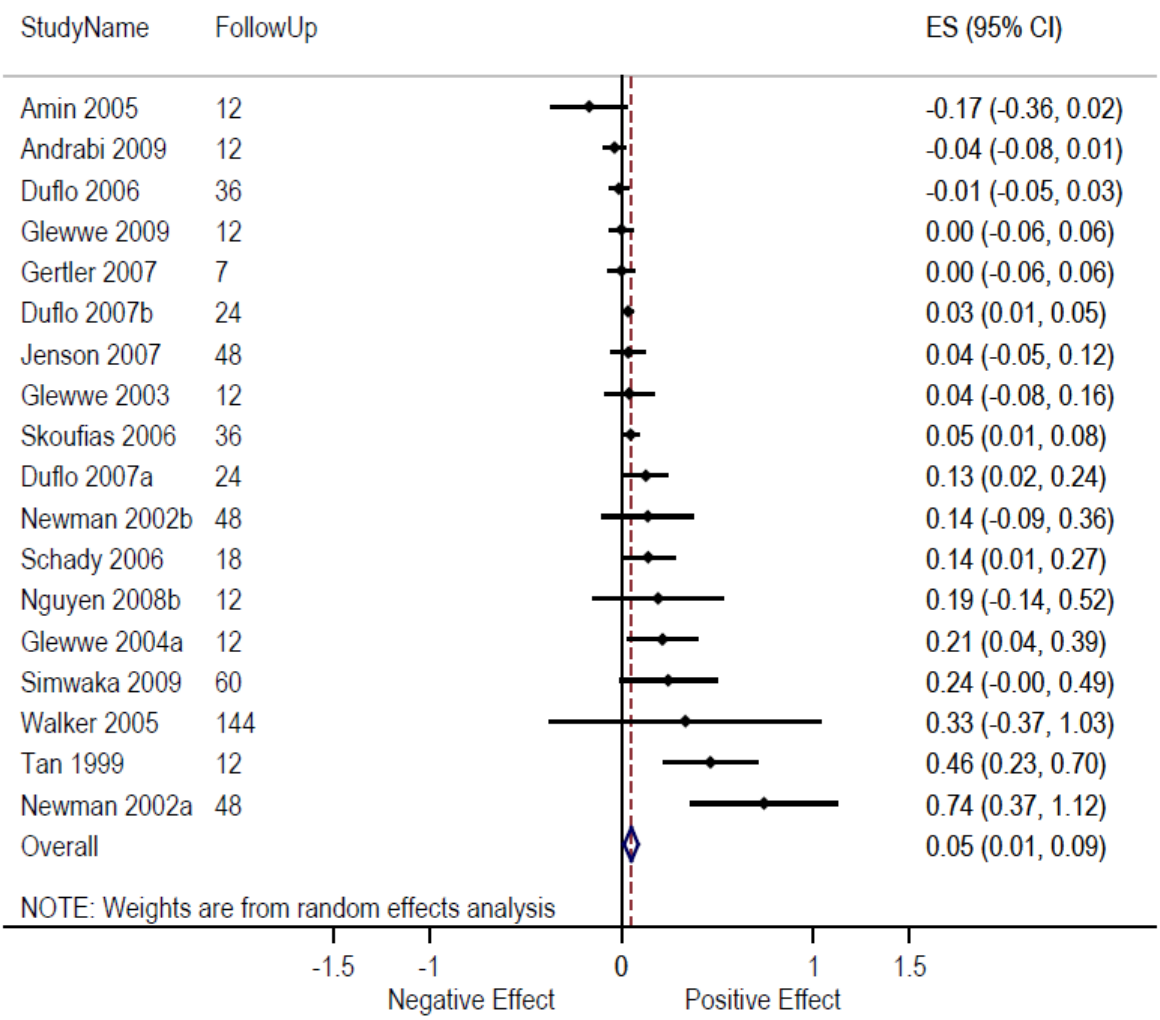

There were 33 studies that included at least one quantifiable measure of attendance in their analyses. These studies included measurement of attendance at a wide range of time intervals, ranging from 1.2 to 41 months. Figure 4 presents the effect sizes for these 33 studies. Similar to Figure 3 and the enrollment results, the overall effect was positive (*d*=.15, 95% CI [.10-.20]), ranging from -.20 to .74. Four studies reported negative results on attendance (to the left of zero). There was considerable heterogeneity in the effect sizes (Q= 341.64, df=32, p<.001; $I^2$=90.34; $tau^2$=.013).

**Figure 4: Main effects on School Attendance (n=33)**



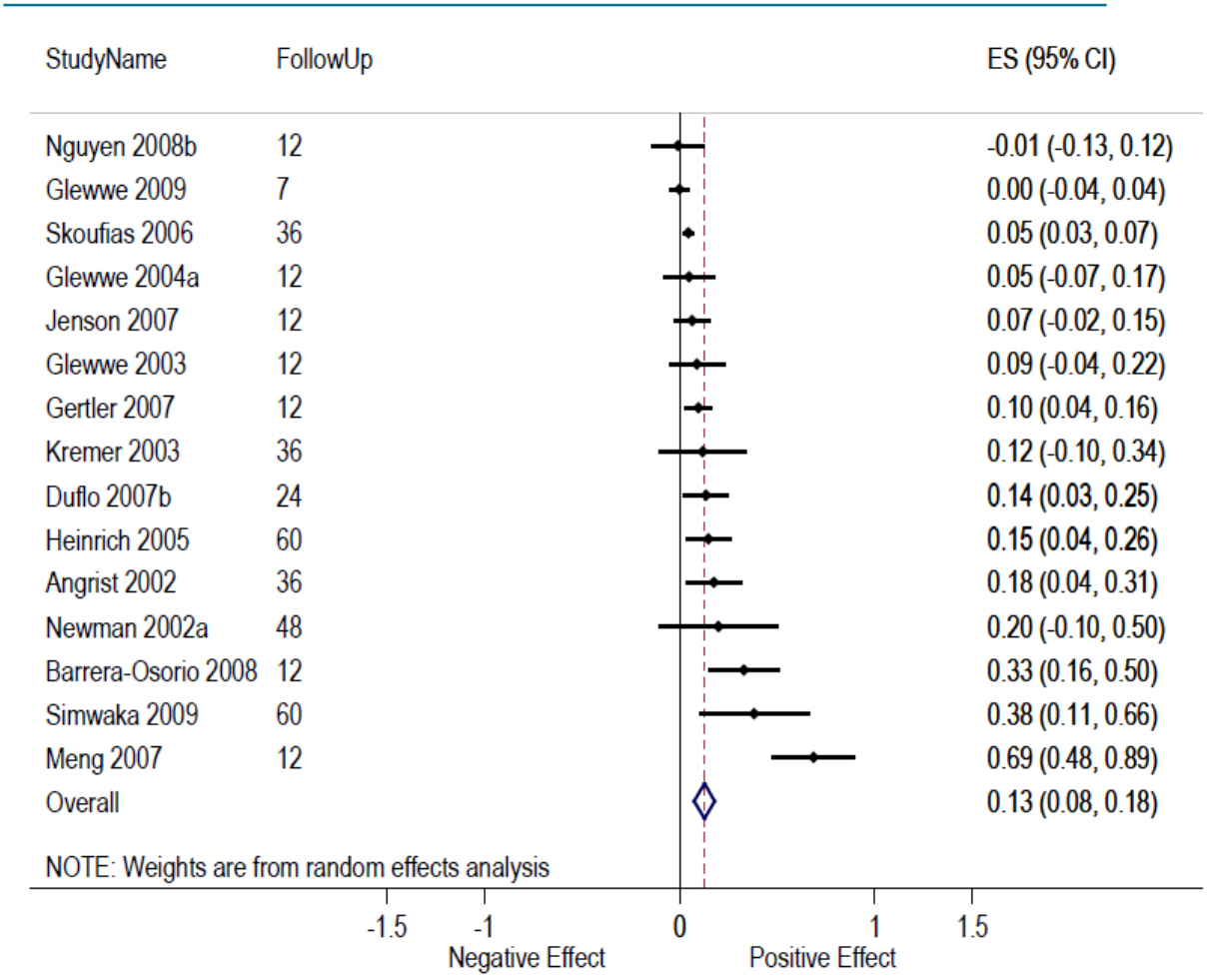

There were 18 studies that included at least one quantifiable measure of school dropout in their analyses. Timing of follow-up for the dropout outcomes varied greatly across these 18 studies, ranging from 7 to 144 months. Figure 5 presents the effect sizes for these studies. Compared to enrollment and attendance, the overall effect was positive but smaller (*d*=.05, 95% CI [.02-.09]), ranging from -.17 to .74. Three studies reported negative effects, i.e., an increase in school dropout (to left of zero). Again, there was considerable heterogeneity in the dropout effect sizes (Q= 61.08, df=17, p<.001; $I^2$=71.17; $tau^2$=.003).

**Figure 5: Main Effects on Dropout (N=18)**




There were 15 studies that included at least one quantifiable measure of progression in their analyses. Follow-up of outcomes for progression varied greatly across these studies, ranging from 7 to 60 months. Figure 6 presents the effect sizes for these studies. The overall effect was positive and similar to those reported for enrollment and attendance (*d*=.13, 95% CI [.08-.18]), ranging from -.01 to .69. Only one study reported a negative effect (to left of zero) on progression in school, but again there was considerable heterogeneity in the effect sizes (Q=77.66, df=14, p<.001; $I^2$=80.46, $tau^2$=.007).

**Figure 6: Main Effects on Progression (N=15)**




## 4.6 Overall intervention effects, supplemental analyses of learning outcomes

As mentioned earlier, we conducted meta-analyses of four distinct learning outcomes: math achievement, language achievement, standardized achievement tests, and other achievement measures (e.g., grades). Figure 7 presents the average effects for 25 studies that examined the impact of an intervention on math achievement. The average follow-up interval ranged from 1 to 144 months. The average effect of these interventions was positive (*d*=.16, 95% CI [.10-.23]), ranging from -.32 to .62. Six studies reported negative effects on math achievement (left of the zero), and there was heterogeneity in these math achievement effect sizes (Q 273.50, df=.24, p<.001; $I^2$=91.22, $tau^2$=.019).

**Figure 7: Supplemental Effects on Math Achievement (N=25)**

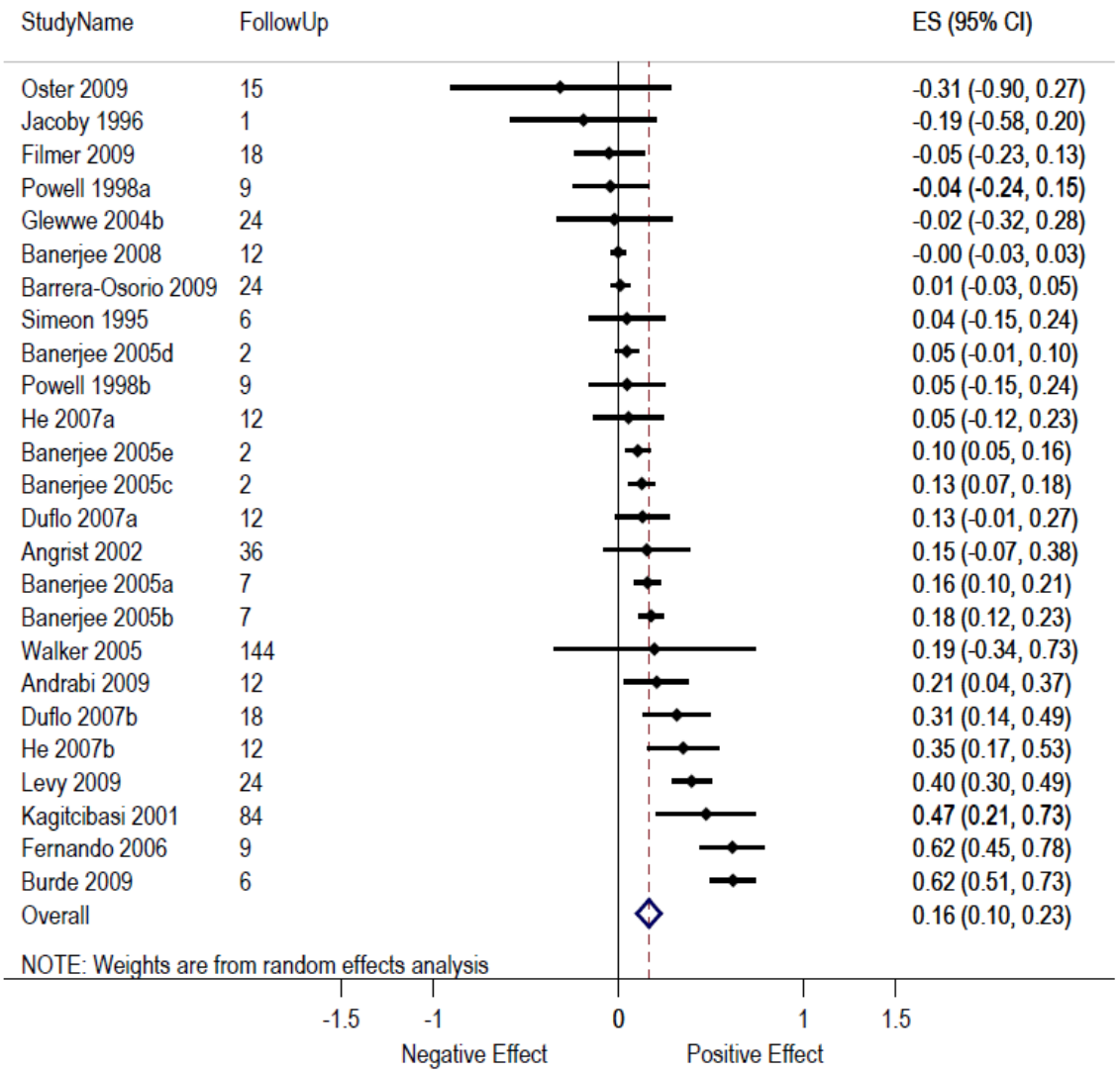



Figure 8 presents the average effects for 25 studies that examined the impact of an intervention on language achievement. Such outcomes included tests on native language performance and, in some instances, scores on English language tests.

These outcomes also included tests of reading or other comprehension measures. The average follow-up interval again had a wide range, from 1 to 144 months. As with the math achievement results, the average effect of these interventions was positive (*d*=.18, 95% CI [.12-.25]). Effect sizes ranged from -.09 to .66. Five studies reported negative effects for an intervention on language achievement (left of the zero), and there was considerable heterogeneity in the language achievement effect sizes (Q 325.60, df=.24, p<.001; $I^2$=92.62, $tau^2$=.019).

**Figure 8: Supplemental Effects on Language (N=25)**

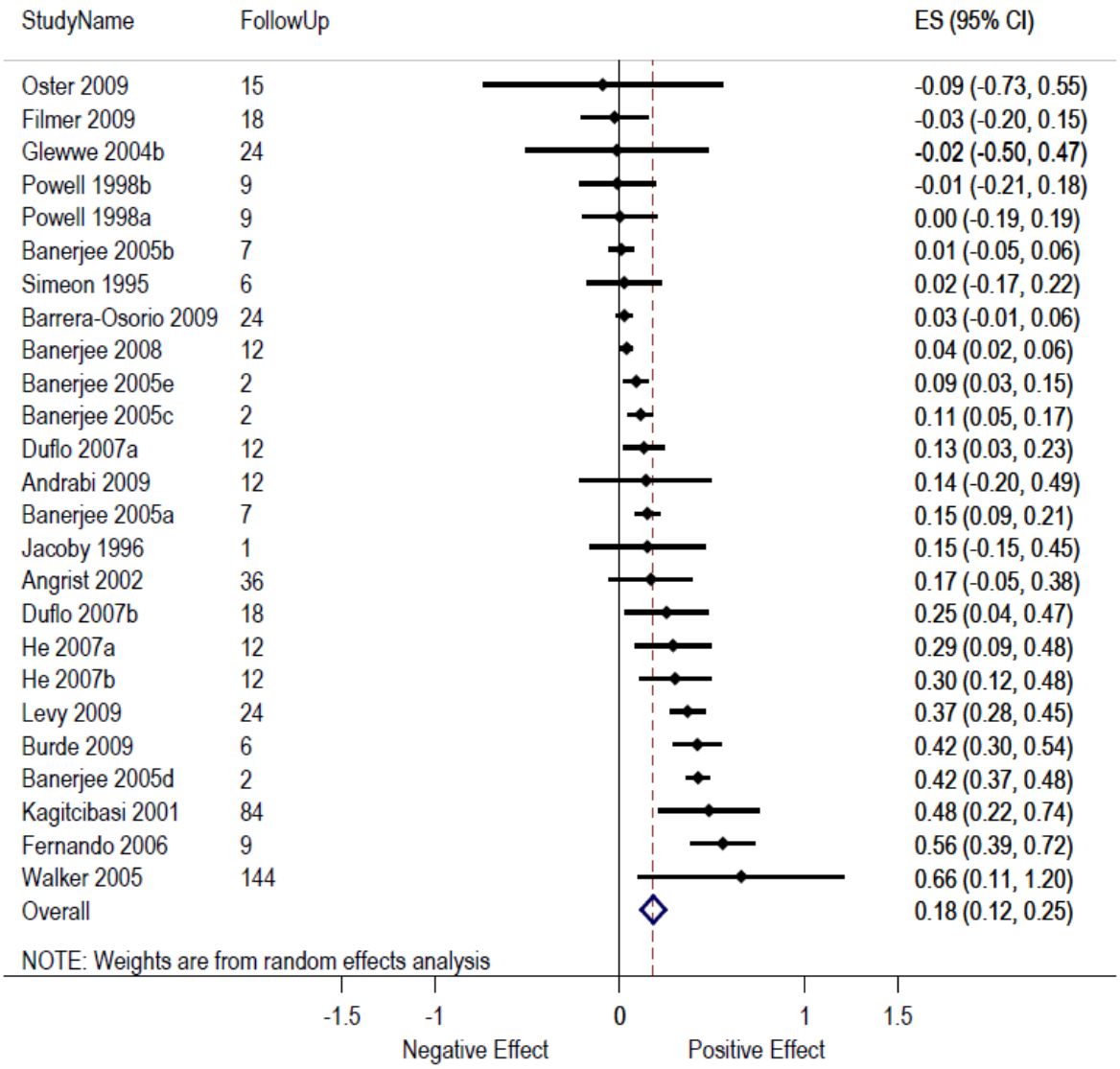



Figure 9 presents the average effects for 10 studies that examined the impact of an intervention on standardized achievement tests. These test scores were comprised of national or district tests and tended to include a range of subjects. The average follow-up interval was narrower than in prior analyses, ranging from 12 to 24 months. The average effect of the interventions on standardized achievement tests was positive but about one-third the size of prior effects on math- and language- specific tests (*d*=.06, 95% CI [-.02-.14]; Q 17.37, df=9, p=.043; $I^2$=48.19, $tau^2$=.006).

Effect sizes ranged from -.13 to .31. Three studies reported negative effects for an intervention on standardized achievement tests (left of the zero).

**Figure 9: Supplemental Effects on Standardized Achievement Test Scores (N=10)**

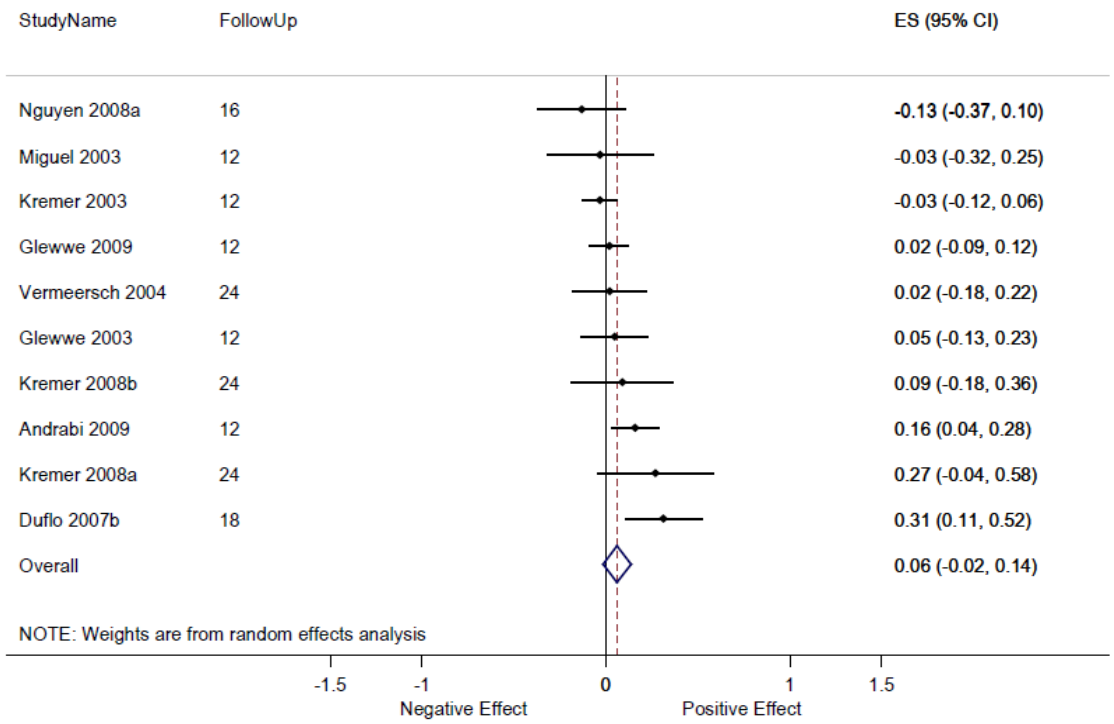
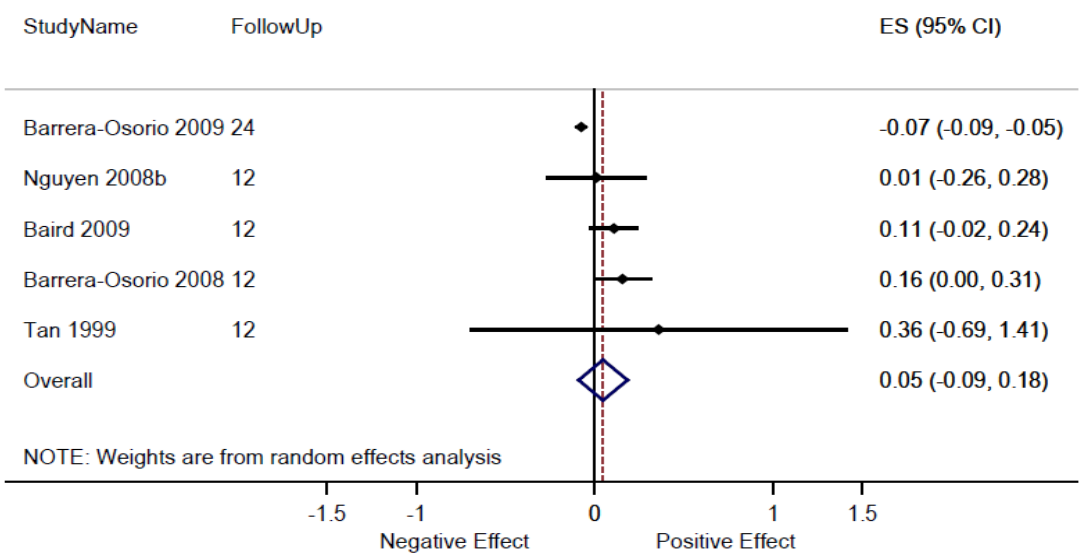



Figure 10 presents the average effects for 5 studies that examined the impact of an intervention on other achievement. This outcome included self-reported grades or achievement measures. Similar to the standardized achievement test analysis, the average follow-up interval was narrower than in prior analyses, ranging from 12 to 24 months. The average effect of the interventions on other achievement measures was positive but the smallest of all analyses reported so far (*d*=.05, 95% CI [-.09-.19]; Q =16.49, df=4, p=.002; $I^2$=75.68; $tau^2$=.014). Effect sizes ranged from -.07 to .36. One study reported negative effects for an intervention on other achievement (left of the zero).

**Figure 10: Supplemental Effects on Other Measures of Achievement (N=5)**



## 4.7 Moderating variable analyses

The meta-analyses reported above examined the distribution of effect sizes for the outcomes that respond to the main and supplemental questions driving this review. Because of the large number of studies considered, and the diversity of interventions, samples and settings, it is very likely that the effects varied across these different dimensions. This assumption was supported by the heterogeneity statistics presented in each of the prior analyses. In this section, we present analyses for seven moderating variables used to further explore and try to explain some of the observed heterogeneity in the effect sizes (broad type of intervention; specific type of intervention; World Bank classification of economies; World Bank region; type of evaluation design; whether the intervention targeted females or not; and whether the intervention included primary or secondary school students, or both).

For moderator analyses, we relied on the 59 studies that reported enrollment or attendance outcomes (for the eight studies that reported both a mean across the two was computed). The weighted mean effect sizes for these two outcome categories were similar (d=.18 for enrollment, d=.15 for attendance) and the Q-test for between-group heterogeneity for these two outcomes was not significant (Q=3.97, p=.14). Because not all included studies reported enrollment and attendance, and

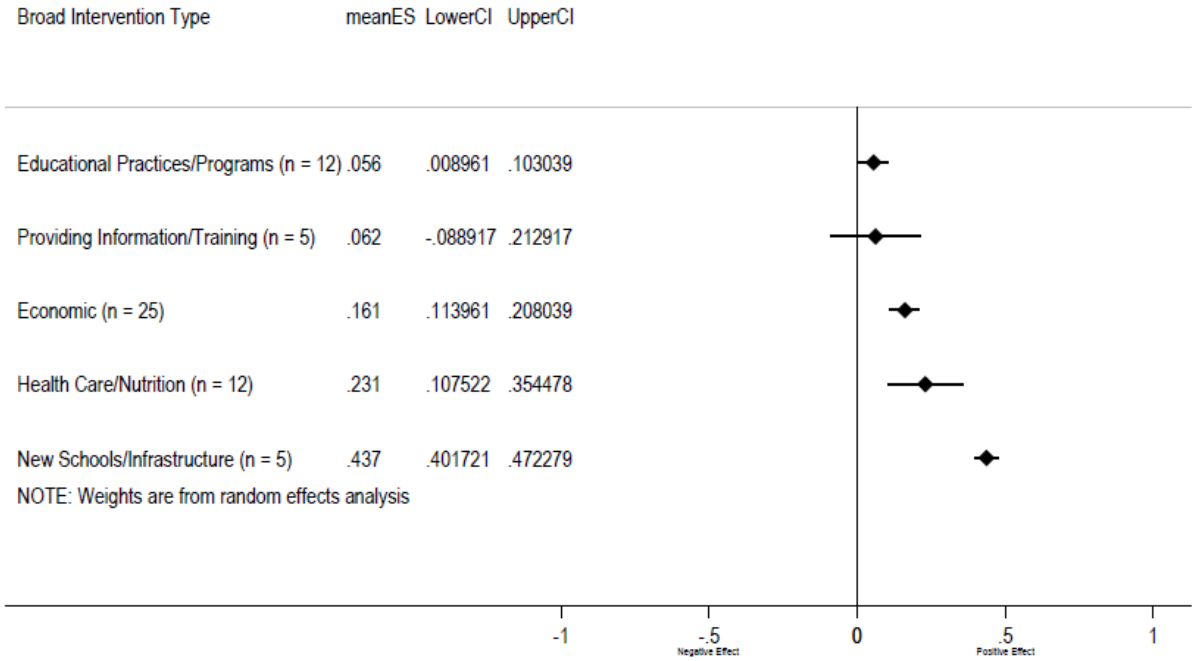
the means for enrollment and attendance were statistically similar, we combined the outcomes where needed to create a larger group of studies for moderator analysis, still retaining only one effect size per study for analysis. Again, for each analysis, we report heterogeneity statistics (Q) but caution that even within subgroups, there was large variation across studies in the types of interventions, participants, countries, designs, and other details.

### *4.7.1 Broad Intervention Type*

For this analysis, we grouped interventions into the broader intervention categories discussed above: (1) Economic; (2) Educational Programs/Practices; (3) Health Care/Nutrition; (4) New Schools/Infrastructure; and (5) Providing Information/Training. Figure 11 presents the results. The largest effects are reported across the five studies in the NewSchools/Infrastructure group (*d*=.44, 95% CI [.40-

.47]). This group of interventions was significantly larger than the next largest category of Health Care/Nutrition (d=.23, 95% CI [.11-.36]). The smallest effects were reported for those interventions in the Providing Information/Training group (*d*=.06, 95% CI [-.09-.05]) and the Educational Practices/Programs group (*d*=.06, 95% CI [.01-.10]). Note that interventions classified as Economic, Health Care/Nutrition or Providing Information/Training all had overlapping confidence intervals. The heterogeneity test of between group differences confirmed that these effects varied across intervention types, as there was more heterogeneity between groups than would be expected by chance (Q=195.25, df=4, p<.001).

**Figure 11: Effect Size by Broad Intervention Type (N=59)**



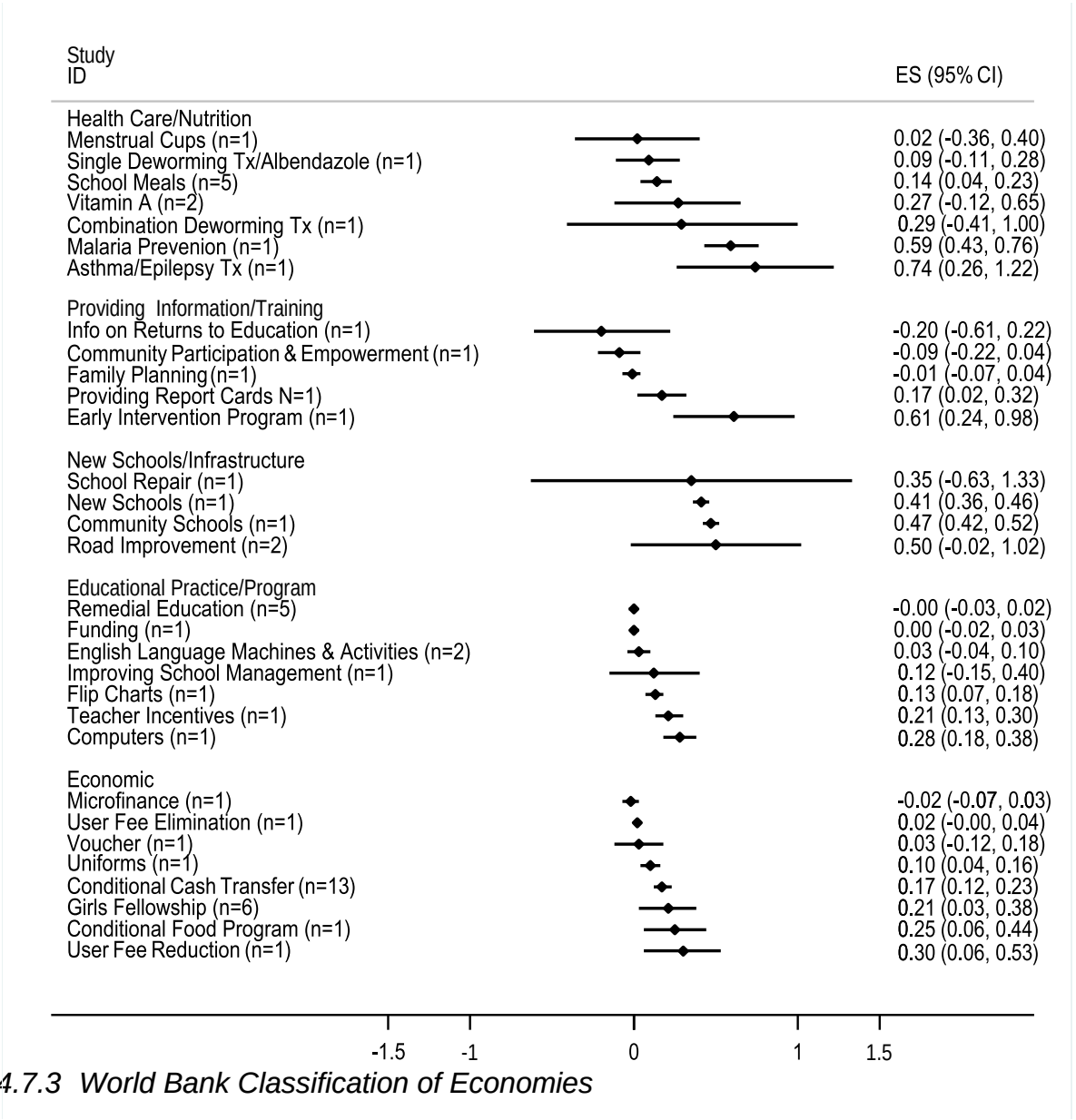

### *4.7.2 Specific Intervention Type*

The analysis reported above (and summarized in Figure 11) presented mean effect sizes across broad groupings of interventions. Another important policy and practice question is whether effect sizes varied across more specific intervention types.

Figure 12 presents the results across 31 interventions.[4] The interventions are listed in alphabetical order, and the number of studies in each intervention category is in parentheses, followed by an acronym indicating the broader intervention type it was assigned to. Most results were positive in direction. The five interventions reporting the largest average effects were asthma/epilepsy treatment (*d*=.74), early intervention (*d*=.61), malaria prevention (*d*=.59), road improvement (*d*=.50), and building new schools (*d*=.47). In this analysis, only four of the 31 specific interventions reported average negative effects (to the left of zero). These were providing returns on education (*d*=-.20), community participation and empowerment (*d*=-.09), family planning (*d*=-.01), microfinance (*d*=-.02). A heterogeneity test of between group differences confirmed that these effects varied across specific intervention types, as there was more heterogeneity between groups than would be expected by chance (Q=646.26, df=30, p=.00). Again, extreme caution should be used many of these types include but one study.

[4] Because we only used studies that reported enrollment or attendance for moderator analyses, five specific types of interventions evaluated in studies that only reported dropout or progression data are not included in Figure 12. These were: dropout prevention, funding, livelihood skills, preschool supplement, and teacher incentives.

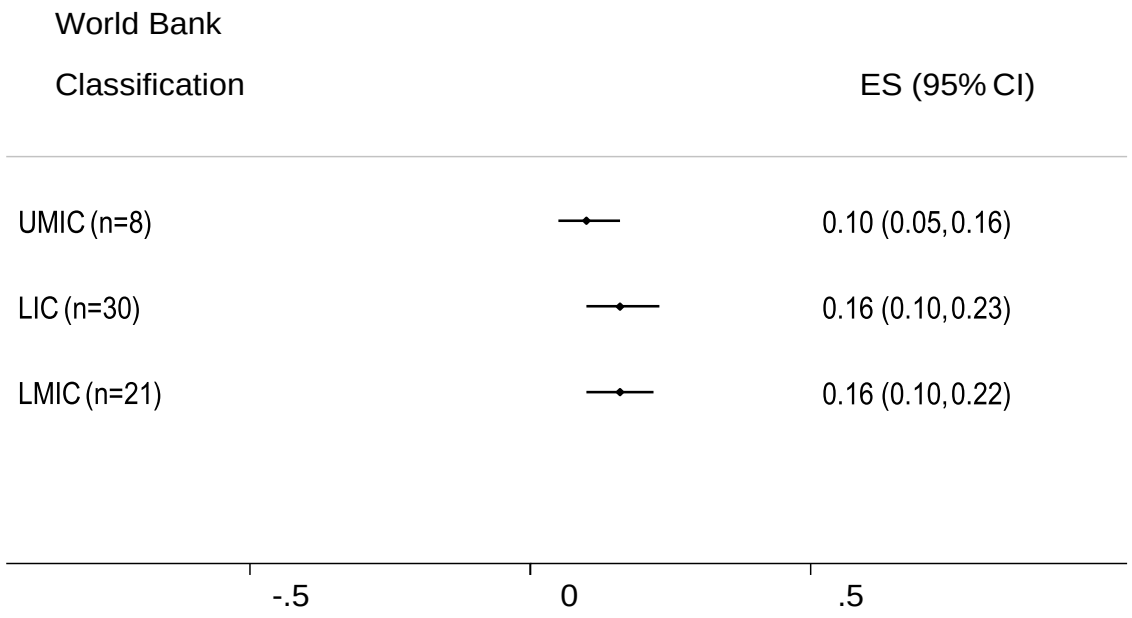
**Figure 12: Effect Size by Specific Intervention Type (N=59)**




### *4.7.3 World Bank Classification of Economies*

As mentioned earlier, interventions were implemented across a wide range of developing nations. In this analysis, we examined the average effect for interventions implemented in the three types of developing nations, as defined by the World Bank Classification of Economies (Lower Income Country, Lower Middle Income Country, and Upper Middle Income Country). As Figure 13 indicates, the average effects for the 30 LIC nations and 21 LMIC nations were .16; UMIC nations had a mean effect size of .10. The confidence intervals for all three categories overlapped, and there was no evidence of significant differences across the three categories (Q=2.29, df=2, p=.32).

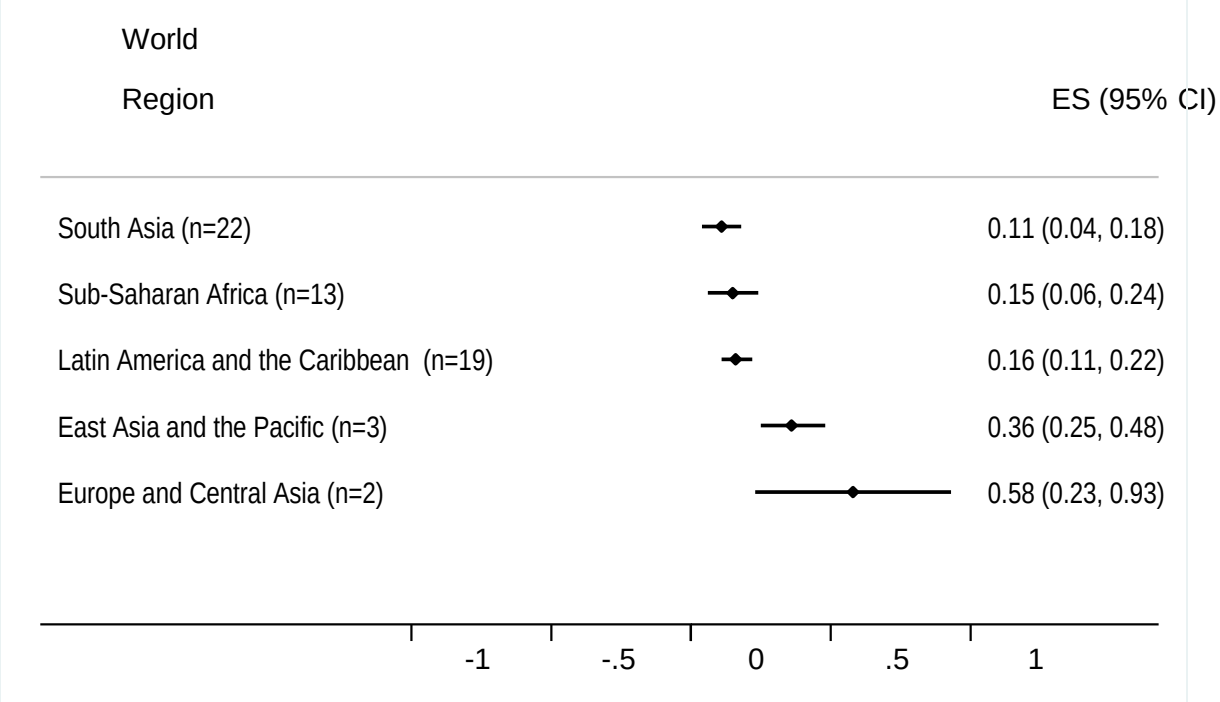
**Figure 13: Effect Size by World Bank Classification of Economies (N=59)**




### *4.7.4 World Bank Classification of Developing Regions*

The World Bank also groups nations by developing regions of the world. We used that grouping as a moderating variable to examine effects for the different regions. Figure 14 presents the results. The largest effects are shown for the two studies in Europe and Central Asia (*d*=.58) and the three studies in East Asia and the Pacific (d=36, 95% CI [.25-.48]). Overlapping confidence intervals and a test of between group differences indicated these two groupings were statistically different from each other (Q=19.33, df=4, p<.001).

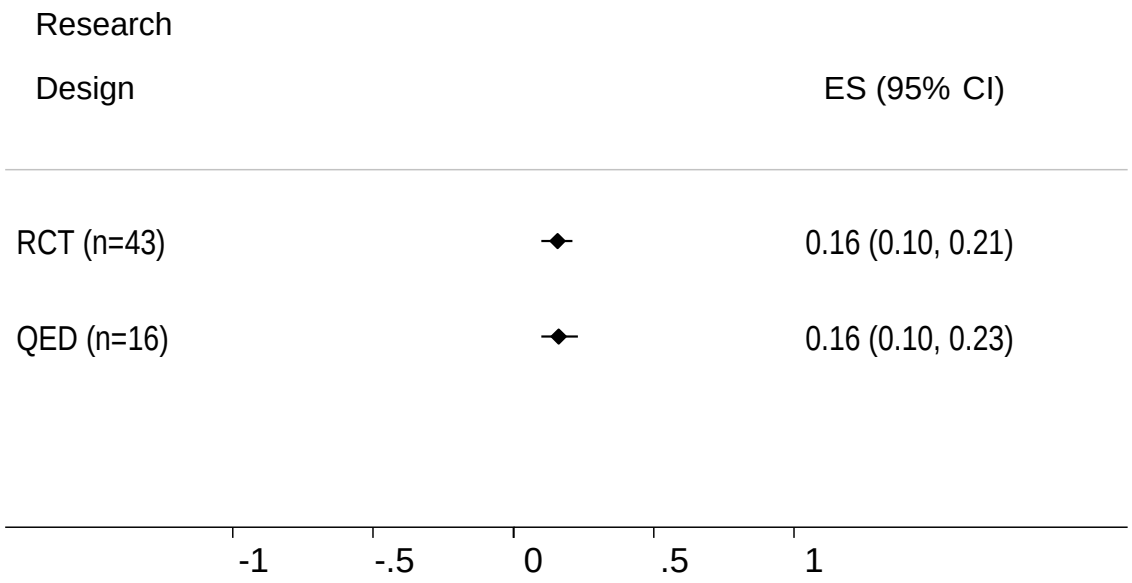
**Figure 14: Effect Size by World Bank Classification of Developing Regions (N=59)**



### *4.7.5 Type of Evaluation Design*

As discussed above, our sample was comprised of a large majority of randomized controlled trials (RCTs), likely reflecting our stringent eligibility criteria and screening of quasi-experimental designs (QEDs). In this analysis, we compare the average effect size for RCTs versus QEDs. As Figure 15 indicates, average effects for the different study designs were identical (*d*=.16). A test of between-group heterogeneity confirmed there was no evidence of a difference in mean effect sizes for RCTs versus QEDs (Q=.01, df=1, p=.92).

**Figure 15: Effect Size by Evaluation Design (N=59)[5]**



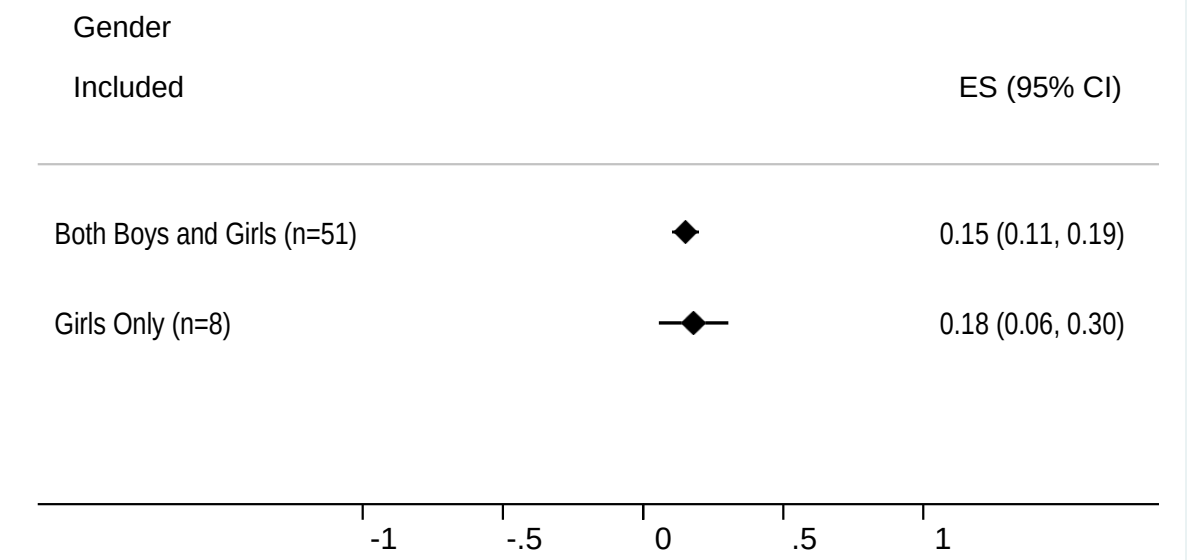

### *4.7.6 Whether intervention specifically targeted females or not[6]*

In recent years, there has been a strong emphasis by donor agencies and the governments of developing nations on specifically targeting females for educational initiatives. In this review, eight studies tested interventions that specifically targeted females (although some may have examined spillover effects on boys), including six that were scholarship/fellowship programs. Figure 16 examines the average effect for those eight studies, and compares it to the average effect for the 51 studies that tested interventions that included boys and girls. As the figure shows, the average effect for female-focused interventions was slightly larger (*d*=.18 to *d*=.15), but the there was no evidence of a difference in the two groups (Q=.194, df=1, p=.66).

[5] These effect sizes were rounded to two decimals; at three places, the effect sizes were .162 for QEDs and .157 for RCTs.

[6] Although we also wanted to examine this issue by analyzing the "percentage of female students" in the study, this information was missing in approximately half of the included studies.

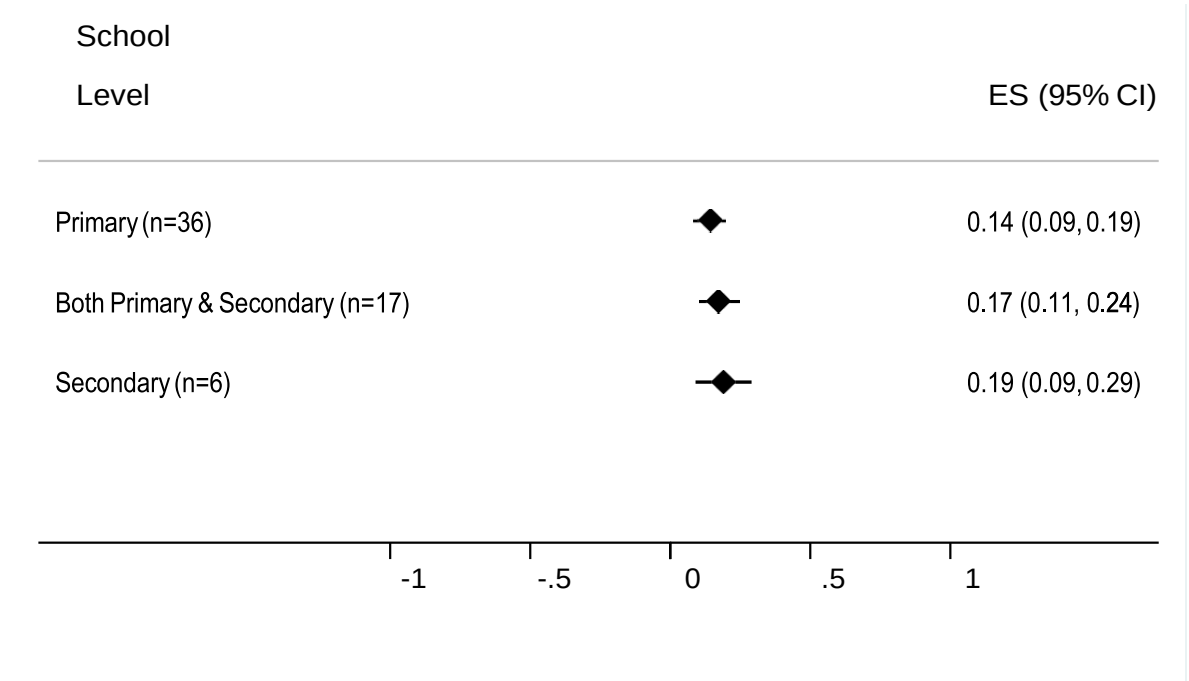
**Figure 16: Effect Size by whether Intervention Specifically Targeted Females or Not (N=59)**




### *4.7.7 Whether the study included outcomes for primary or secondary school students*

Another aspect of the wide diversity of the included studies is that some target primary school students, some target secondary students, and others include outcomes for students at both school levels. Figure 17 examines the average effects for interventions across these school levels. As the figure indicates, the effect sizes for studies including only primary student outcomes was .14 and those including only secondary student outcomes was .19. The average effect size for those interventions that included both types of students is .17. There was no evidence of a significant difference between the three types of studies (Q=.87, df= 2, p=.65).

**Figure 17: Effect Size by Level of School Attended by Students in Study (N=59)**



## 4.8 Methodological quality checks

### *4.8.1 Quality Scores*

For each study, we captured information about any issues with crossovers (persons receiving a treatment to which they were not assigned), selection bias (e.g., breakdowns in randomization or unusual unequal distributions in groups), loss of participants due to attrition or database matching issues, and intervention fidelity and implementation issues. The first three authors also rated each study according to their own perceptions of whether the problems presented a threat to the findings reported in the study. These ratings were categorized as "low," "moderate," or "high" threat. If there were no indicated problems, the threat to the study was rated as "none." It should be noted, however, that these ratings were subjective, and they were based entirely on what was reported in the study documents. However, these ratings had good reliability across the three first authors in our check of inter-rater reliability (generally, about 75% agreement across the 12 studies and 3 authors).

This would indicate that the three authors were both identifying the problems and rating the degree of threat to the conclusions in quite similar fashion.

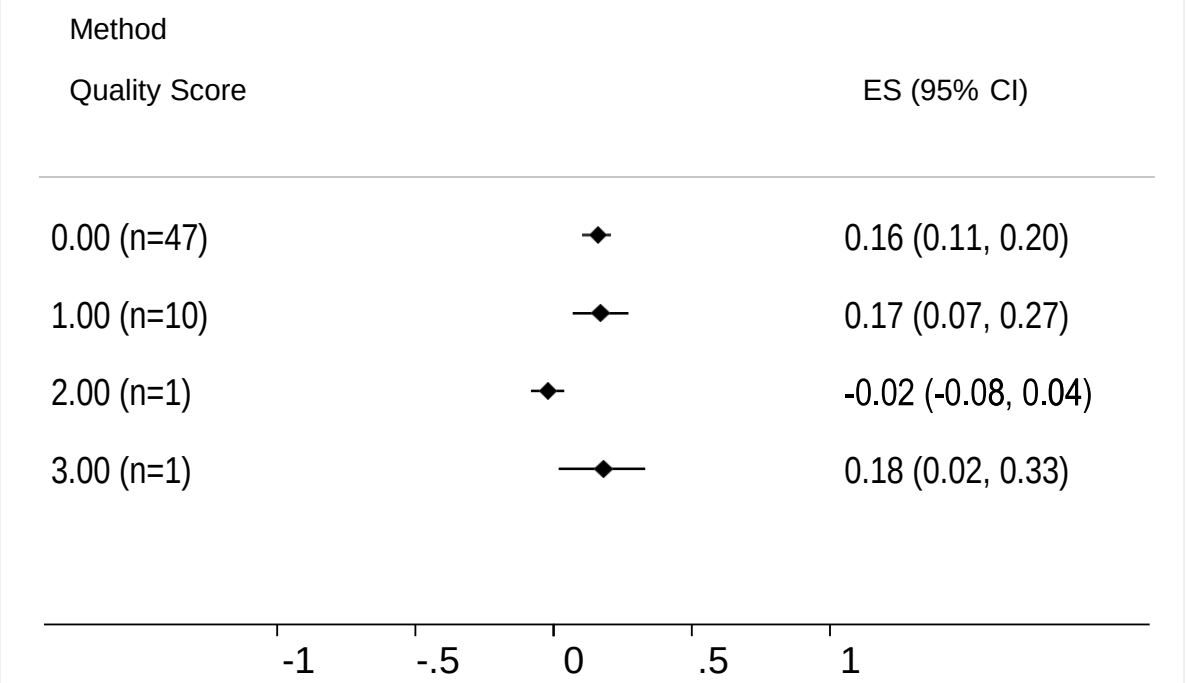
In the following analysis, we examined whether a rating of "moderate" or "strong" threat to the study's conclusions on the four methodological items influenced the average effect size across the studies. For example, a study that had none of the four items rated as a "moderate" or "strong" threat to validity received a "0." Likewise, a study that received a rating of a "moderate" or "strong" threat on all four of the methodological items was scored a "4." For this analysis, as with the moderating variable analyses, the 59 studies that provided enrollment or attendance outcomes were included (the eight studies that reported both outcomes were averaged).

Figure 18 presents the results of this analysis. The figure is instructive in several regards. First, the methodological problems in the largest majority of studies in this moderating analysis were rated as presenting little or no threat to study conclusions (N=57, 97%).[7]

Second, only two studies (3%) had two or three methodological problems rated as "moderate" or "strong" threats to study conclusions, scoring 2 or 3 on the Method Quality score. These findings are likely due to the especially strong designs of the RCTs and QEDs screened into this review. Figure 18 shows that the average effect size for studies that scored "0" was nearly identical (*d*=.16) to those studies that scored a "1" (*d*=.17). The larger average effect (*d*=.18) for studies scoring a "3" was based only on one study, as was the negative result for studies scoring a "2" (*d*=-.02). Confidence intervals overlapped for all categories except for the study that scored a "2." Heterogeneity statistics indicate that at least two levels of this variable (as it was coded here) were significantly different (Q= 23.79, df=3, p<.001), which is clearly driven by the mean negative effect size for the studies with poorer methodological quality ratings.

[7] Note the ratings for the overall sample: (1) for RCTs: 75% scored "0", 19% scored "1", 4% scored "2" and 2% scored "3"; (2) for QEDs:: 80% scored "0"; 15% scored "1"; 5% scored "2" and 0 scored "3".

**Figure 18: Effect Size by Methodological Quality Rating (N=59)**




### *4.8.2 Using Individual versus Aggregate Sample Sizes to Compute Effect Sizes*

As mentioned previously, when possible, we used sample sizes for the individual students in the studies rather than the sample sizes for the aggregate units that were randomly or quasi-experimentally assigned to conditions. So, for example, if a study randomly assigned 10 villages each to treatment and control conditions, and then reported analyses on enrollment using individual sample sizes, we used those individual sample sizes to compute effect sizes. All such studies took clustering into account when analyzing at the individual level, so no corrections for lack of clustering were applied to these data.

We conducted one post-hoc methodological check to see how different the average effect sizes were when using aggregate units of assignment versus using the sample sizes of individual students in the studies. Table 5 presents the results of a comparison for twelve studies (16% of the total review sample) in which the effect sizes for aggregate and individual sample sizes were computed. As the table indicates, the differences in the average effects and variances, at least in this analysis, were not substantial (*d*=.16 when using individual sample sizes, and *d*=.20 when using aggregate sample sizes). If this analysis holds true across all studies, the estimates of both effect size and variance using individual sample sizes would represent an underestimate for the studies compared to using aggregate sample sizes. It should be cautioned that these are unweighted effect sizes.

**Table 5: Comparison of Average Effect Sizes Using Individual N's and Aggregate N's (n=12 Studies, 51 Effect Sizes)**

| Used in the | Standardized Mean Effect Size | Variance (v) |
|---|---|---|
| Individual N's | .16 | .0026 |
| Aggregate N's | .20 | .0067 |

## 4.9 Publication bias

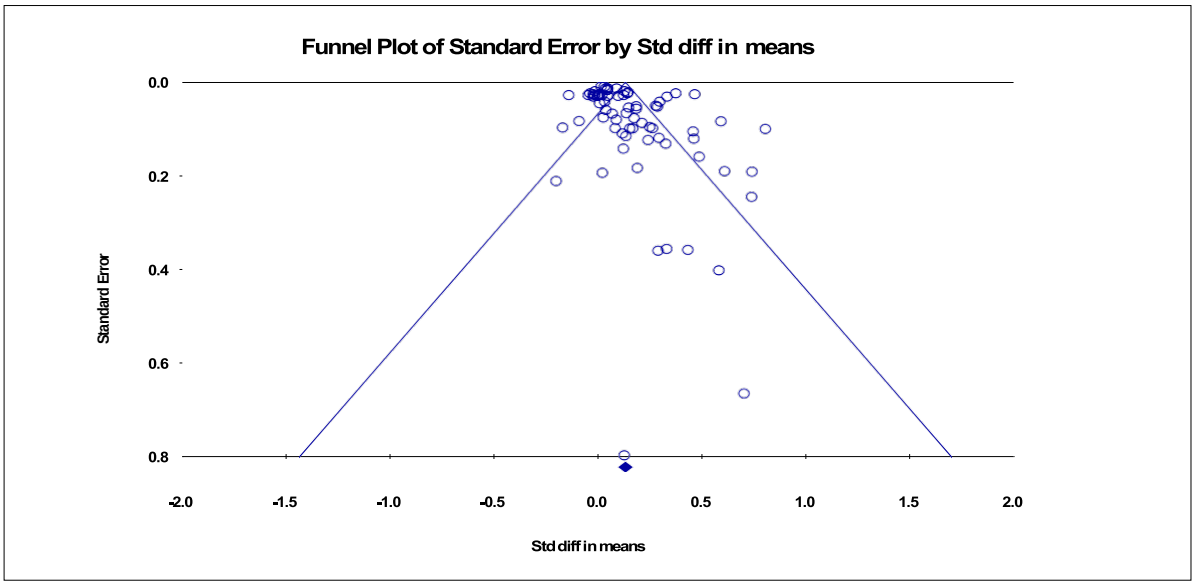
Publication bias refers to bias that can occur if unpublished or difficult to locate studies produce systematically different results than those reported in published or easy to locate studies. Presumably, smaller sample size studies are less likely to be published than those with larger sample sizes, as are studies with null (i.e., non- significant) or negative findings.

The studies in this review include a large number of unpublished documents. As mentioned earlier in the section on "descriptive statistics," approximately seven in ten (71%) studies were located outside of academic journals or books. Because most of the studies included in this review reported findings in both unpublished and published formats (e.g., technical reports and journal articles), it is unlikely that publication bias is an issue in this meta-analysis.

It should also be noted that researchers in developmental economics have conducted a large percentage of the studies in this review sample. One tradition in the field of economics is that unpublished papers are often made available online due to the long time lag in getting published in journals. However, many of these are eventually published in economics journals, as we found during our search and retrieval process for this review. Therefore, papers may only be temporarily "unpublished." Second, researchers in economics may provide a series of working papers on their research and also publish a shorter version of the study in a journal. We only coded type of publication on the primary document we used for the study. In some cases, this was the working paper instead of the journal article, which might have provided more details than can be published in the typical journal article. Thus, the results may indeed have been published in a peer-reviewed journal—but because our primary document in the study was an unpublished work, we would have listed this as "unpublished." Finally, it is not entirely transparent what "unpublished" means in the electronic digital age, when documents that are not controlled by commercial publishers are easily obtained via internet searches.

With those caveats aside, to assess the possibility of publication bias we visually examined a funnel plot, conducted an Egger regression test for funnel plot asymmetry (1997), and conducted a trim and fill analysis (2000). The funnel plot (Figure 19) was relatively symmetric, due in large part to the fact that very few studies had large standard errors (regardless of the direction or magnitude of the effect size). However, there were no studies in the meta-analysis with large standard errors and null/negative effect sizes. Results from the Egger and colleagues' (Egger, Davey Smith, Schneider, & Minder, 1997) test for funnel plot asymmetry indicated a significant positive association between the effect size and standard error (b = 1.89, p = .003, 95% CI [.68, 3.11]), indicating possible evidence of publication bias (see Appendix 8.7). Trim and fill analysis (Duval & Tweedie, 2000) indicated that even after trimming and filling 24 hypothetical effect sizes, the random effects mean effect size was still positive and statistically significant (see Appendix 8.7). Although there was some evidence of asymmetry in the funnel plot, we conclude that any possible bias is unlikely to have had an appreciable effect on the substantive conclusions of the meta-analysis. Most of the studies included in the meta-analysis were reported in published and unpublished formats, and there simply are not many small sample size studies in this field of study (regardless of effect direction/magnitude). Thus it is likely that any observed asymmetry in the funnel plot is due to this literature being dominated by large-scale trials, rather than publication bias per se.

**Figure 19: Funnel Plot of Standard Error by Standardized Difference in Means (n=73)**



# 5. Conclusions

In this review, we identified 73 experimental and quasi-experimental studies that examined the impact of an intervention on at least one main outcome of school enrollment, attendance, dropout, or progression in a developing nation. We also examined the effects of the interventions on supplemental learning outcomes of math achievement, language achievement, standardized achievement tests, and other achievement. Table 6 summarizes the results of our analyses. In sum, average effects across the four main outcomes were all positive and statistically significant, although effects on enrollment, attendance, and progression were larger than those on dropout. Results indicated positive and statistically significant effects for two of the four supplemental outcomes (math and language); but there was no evidence of effects on standardized test scores or achievement outcomes. Although most of the findings indicated beneficial intervention effects on the outcomes of interest, these effects were relatively small in magnitude. As shown in Table 6, based on the BESD, the average intervention effect on enrollment (d = .18) is equivalent to a 9% improvement in enrollment for intervention groups. Thus, despite the statistical significance of the findings for the main outcomes, most of the effects are equivalent to about 3-9% improvements in the intervention versus control groups.

**Table 6: Summary of Average Effect Sizes for Overall Intervention Effects (by order of effect size magnitude)**

| Outcome | Standardized Mean Effect (d) N of effect sizes | BESD (Percentage Improvement in Treatment |
|---|---|---|
| **MAIN :** | | |
| Enrollment | .18 (34) | 9% |
| Attendance | .15 (33) | 8% |
| Progression | .13 (15) | 7% |
| Dropout | .05 (18) | 3% |
| **SUPPLEMENTAL** | | |
| Languag | .18 (25) | 9% |
| Math | .16 (25) | 8% |
| Standardized Assessment | .06 (10) | 3% |
| Other | .05 (5) | 3% |

Although we did not explicitly look at gains or losses in supplemental outcomes compared to main outcomes (i.e., did increases in enrollment result in decreases in test scores), the average effect sizes for the supplemental outcomes of language and math were as large or

**Table 7: Average Effect Size Across Levels of Moderating Variables (by order of effect size magnitude) *indicated a significant moderator**

| Outcome | Standardized Mean Effect |
|---|---|
| **Broad Intervention Type*** | |
| New Schools/Infrastructure | .44 |
| Health Care/Nutrition | .23 |
| Economic | .16 |
| Educational Practice/Programs | .06 |
| Providing Information/Training | .06 |
| **World Bank Classification of** | |
| LIC | .16 |
| LMIC | .16 |
| UMIC | .10 |
| **World Bank Classification of Developing Regions:*** | |
| Europe and Central Asia | .58 |
| East Asia and the Pacific | .36 |
| Latin America and the Caribbean | .16 |
| Sub-Sahara Africa | .15 |
| South Asia | .11 |
| **School Level:** | |
| Secondary | .19 |
| Both | .17 |
| Primary | .14 |
| **Broad Design:** | |
| RCT | .16 |
| QED | .16 |
| **Gender:** | |
| Girls Only | .18 |
| Boys and Girls | .15 |

larger as they were for the main outcomes of enrollment, attendance, dropout and progression. This provides some early evidence, at least, that increases in enrollment and attendance in the schools studied did not necessarily overwhelm resources, swamp the quality of teaching, or inhibit learning. Nonetheless, there was no evidence of effects on other standardized assessment scores or achievement measures; these analyses, however, were based on fewer studies and may have been underpowered to detect such effects.

The average effects reported in this review should be tempered by noting the diversity of studies, samples, countries, interventions, and measures in the 73 studies synthesized here. Figures 11 and 12 presented the average effects for five broad groups and for 31 specific types of interventions evaluated in this sample of studies. They present some early indications, comparatively, about the effects of the different intervention strategies on the outcomes. Several cautions are in order, however. First, in many specific intervention categories, only one or two studies have been reported. Second, our analyses focused only on main outcomes of enrollment, attendance, dropout, and progression (and then examined supplemental outcomes of learning); there may be other very important outcomes for child employment, health, and other school outcomes (e.g., teacher attendance and efficacy) that we do not summarize here.

With those caveats aside, results from moderator analyses indicated variability in effects across different broad categories of interventions. Namely, interventions that are assumed to have a more direct pathway to addressing the underlying barrier to influencing school enrollment, attendance, dropout, or progression outcomes seem to be more effective. Programs grouped in this review as "New Schools/Infrastructure," had the largest average effects (d = .44, see Table 7). These interventions dealt with improving school or community infrastructures in an attempt to boost enrollment, such as building new schools, repairing schools, and improving roads/access to schools. Other interventions grouped in the Economic, Health Care/Nutrition or Providing Information/Training categories showed smaller effects, and were not statistically different from each other. To be fair, the direct goals of interventions in the categories such as Providing Information/Training or Educational Programs/Practices may have been improving school management or addressing student learning deficiencies; outcomes such as attendance and progression may have been more distal to program goals, and thus just a few of many outcomes the investigators in those studies collected and reported to assess impact. Despite the variability of effects across intervention types, it should be noted that no data were provided here about the possible additive effects of providing multimodal interventions.

Apart from the average effects for broad interventions, we also examined six other moderating variables. These are also summarized in Table 7 (except for specific intervention type, which is provided in Figure 11). There was no evidence that World Bank Classification of economy, school level, research design, or gender of participants moderated the effects of intervention on enrollment outcomes. However, there were differences in intervention effects according to the World Bank Classification of Developing Regions. Results indicated that studies conducted in Europe and Central Asia and East Asia and the Pacific had the largest average effects whereas those in Sub-Saharan Africa and Southeast Asia exhibited the smallest (but nonetheless statistically significant) effects. It should be cautioned, however, that these two regions had the fewest yield of studies in this analysis (2 and 3, respectively).

## 5.1 Implications for practice and policy

One key question is what can policy and practice decision-makers take from these results? For one, the results provide some early data on "best bets." In other words, the average effect of the interventions represented here will likely hover around a 9% increase in student enrollment, a 7-8% increase in student attendance (or decrease in absenteeism), a 2-3% decrease in student school dropout, and a 6-7% increase in students making adequate

progress in their education (all things being equal, and making assumptions about the baseline). Likewise, the evidence indicated 9% increases in math and language achievement, and negligible improvement on standardized achievement tests and other achievement outcomes.

Whether these improvements are sizable enough to warrant further investments depends on more information than is currently available here, including costs of interventions and alternatives. Although we coded information on economic data when available (i.e., when it was contained in the main evaluation report or in another publication on the study), it was reported too infrequently and in such varying levels of detail that we could not synthesize it in any defensible fashion. Policy-makers and practitioners should therefore consider the costs associated with implementing different types of interventions relative to the potential gains they can expect in educational outcomes. Other important considerations would be the effect of these interventions on other individual outcomes such as morbidity and mortality, as well as larger community level outcomes that may be improved from certain types of infrastructure-building interventions. Of course, decision-making about investments in education also depends on the scope of the problem in the jurisdiction; for example, health care/nutrition treatments would only be feasible in those regions in which illness or nutritional deficiency is a significant barrier to student attendance. Researchers and practitioners interested in implementing such interventions must therefore be attuned to the needs of the community.

Another issue for practitioners to consider is the extent of the enrollment problem in the region, nation, and within the country itself. As indicated in Table 1 in the background section, enrollment rates vary by level of school (primary and secondary) and for different regions of the world. Furthermore, enrollment rates can greatly vary within a specific country, particularly by gender and socioeconomic status (Birdsall, Levine & Ibrahim, 2005). The percentage improvements noted in Table 6 (BESD column) have to be considered in light of these regional and socio- demographic differences in baseline enrollment rates. A 3% improvement might be considered more modest in a nation in which only 50% of youth enroll in primary school, as opposed to a nation in which 90% enroll.

## 5.2 Future research directions

The studies represented in this report cover literature from 1990-2009. Given the rate of production of eligible studies (judging by Figure 2), this review will need to be updated very quickly to take any new evaluations into account. In addition, it may prove fruitful to include learning outcomes as criteria for inclusion along with enrollment measures. This would expand the size and scope of the review, but might result in more policy relevant and useful data for stakeholders.

There is much to learn from these studies about evaluation design. Although the effect sizes for experiments did not differ when compared to quasi-experiments, this is possibly due to the stringent criteria for including non-randomized evaluation studies employed here. Methodological research could expand the review to include all types of quasi-experiments and examine which classes of designs seem to approximate the average effect for randomized experiments.

An important area for future reviews will be to examine whether school enrollment interventions in developing nations also influence other individual and community level

outcomes. For instance, 14 of the studies included in this review focused on providing health and nutritional support for students and thus likely impact numerous student health outcomes as well as their school attendance outcomes.

Examining these supplementary effects will be important for researchers, policy- makers, and practitioners interested in implementing programs with a wide impact on multiple outcomes. Interventions that improve multiple outcomes at once may be more cost-effective and appealing in developing countries with limited resources.

In addition to these implications for future systematic reviews and meta-analyses, this review also has implications for the conduct of primary research studies investigating the effect of interventions on primary and secondary school enrollment in developing countries. Most critical is that descriptive validity, i.e., how the studies are reported, will need to improve to facilitate subsequent systematic reviews. It was difficult, at times, to determine sample sizes of students and youths in the study, particularly how many were included in clusters of schools or other aggregate units that were randomly assigned, and how many remained in the study at the analysis stage.

# 6. Other Topics

## 6.1 Acknowledgements

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## 6.2 Plans for updating the review

We plan to update this review in 36 months in concert with Campbell Collaboration (C2) guidelines, conditional on obtaining funding.

## 6.3 Statement concerning conflict of interest

We do not have any conflict of interest regarding school enrollment policies. None of the authors has any financial or other personal interest in the results of this review.

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# Appendices

## List of bibliographic databases searched

ACADEMIC SEARCH PREMIERE

ACCESSUN (UNITED NATIONS DOCUMENTS)

ACCESSUNDP (UNITED NATIONS DEVELOPMENT PROGRAM DOCUMENTS) AGRICOLA

C2SPECTR

CENTER FOR AGRICULTURAL BIOSCIENCE (CAB) ABSTRACTS/OVID COCHRANE CONTROLLED TRIALS REGISTER

CRIMINAL JUSTICE ABSTRACTS ECONLIT

EMBASE ERIC

GLOBAL HEALTH HEALTHSTAR

INTERNATIONAL BIBLIOGRAPHY OF THE SOCIAL SCIENCES MEDLINE

PUBLIC AFFAIRS INFORMATION SERVICE (PAIS) AND PAIS INTERNATIONAL POLICY FILE

PROQUEST THESES AND DISSERTATIONS PSYCHINFO

SOCIAL SERVICE ABSTRACTS SOCIAL WORK ABSTRACTS SOCIOLOGICAL ABSTRACTS

UN-DOCS (GENERAL UNITED NATIONS DOCUMENTS) WORLDWIDE POLITICAL SCIENCE ABSTRACTS

## Search strategy

These are the major search terms referenced in many search strategies below: developing nations; evaluation; enrollment outcomes; youths. Unless otherwise noted, these search terms refer to the following search queries:

**Developing nations:**

Afghanistan* OR Albania* OR Samoa* OR Angola* OR Argentina* OR Armenia* OR Azerbaijan* OR Bangladesh* OR Pakistan* OR Bengal* OR Belarus* OR Byelorussia* OR Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* OR Bolivia* OR Bosnia* OR Herzegovina* OR Botswana* OR Bechuanaland* OR Brazil* OR Bulgaria* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Cameroon* OR Verde* OR Africa* OR "Ubangi-Shari" OR Chad* OR Chile* OR China* OR Colombia* OR Comoros* OR Congo* OR Zaire* OR Rica* OR "Cote d'Ivoire" OR "Ivory Coast" OR Cuba* OR Djibouti* OR Somali* OR "Afars Issas" OR Domini* OR "Santo Domingo" OR Ecuador* OR Egypt* OR Salvador* OR Eritrea* OR Ethiopia* OR Abyssinia* OR Fiji* OR Gabon* OR Gambia* OR Georgia* OR Ghana* OR "Gold Coast" OR Grenada* OR Guinea* OR Guyana* OR Guiana* OR Haiti* OR India* OR Indonesia* OR "East Indies" OR Iran* OR Persia* OR Iraq* OR Jamaica* OR Jordan* OR Kazakhstan* OR Kenya* OR Kiribati* OR "Gilbert Islands" OR Korea* OR Kosovo* OR Kyrgyz* OR Lao* OR Latvia* OR Leban* OR

Lesotho* OR Basutoland* OR Liberia* OR Libya* OR Lithuania* OR Macedonia* OR Madagascar* OR Malaw* OR Nyasaland* OR Malaysia* OR Malaya* OR Maldives* OR Mali* OR Sudan* OR "Marshall Islands" OR Mauritania* OR Mauritius* OR Mayotte* OR Mexic* OR Micronesia* OR Moldova* OR Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* OR Morocc* OR Mozambique* OR Myanmar* OR Burm* OR Namibia* OR Nepal* OR Nicaragua* OR Niger* OR Nigeria* OR Palau* OR Panama*OR Paraguay* OR Peru* OR Philippin* OR Fillipino* OR Poland* OR Polish* OR Romania* OR Russia* OR Rwanda* OR "Sao Tome Principe" OR Senegal* OR Serbia* OR Seychelles* OR "Sierra Leone" OR "Solomon Islands" OR Lanka* OR Ceylon* OR "St. Kitts" OR Nevis* OR Lucia* OR "St. Vincent" OR Grenadines* OR Suriname* OR Swaziland* OR Arab* OR Syria* OR Tajikistan* OR Soviet* OR Tanzania* OR Thai* OR Siam* OR Timor* OR Togo* OR Tonga* OR Tunisia* OR Turk* OR Uganda* OR Ukraine* OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides OR Venezuela* OR Vietnam* "West Bank" OR Gaza* OR Yemen* OR Zambia* OR Zimbabwe* OR Bogota* OR Asia* OR "Latin America" OR "developing nation" OR "developing region" OR "developing country" OR "third world nation" OR "third world country" OR "third world region" OR "low income nation" OR "low income country" OR "low income region" OR "impoverished country" OR "impoverished region" OR "impoverished region"

**Evaluation:**
evaluat* OR random* OR experiment* OR controlled OR "control group" OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR "study design" OR rigorous

**Enrollment outcomes:**
schooling OR enrol* OR attend* OR absent* OR absence* OR dropout* OR "drop- out" OR "drop-outs" OR "grade repetition" OR "repeat grade" OR "complete grade" OR "test score" OR "grade completion" OR "standardized test" OR matricul*

**Youths:**
youth* OR child* OR student* OR adolescent* OR teen* OR boy* OR girl* OR pupil* OR youngster* OR juveniles OR minors OR kids\

***Academic Search Premiere***

(USED 4 MAJOR SEARCHES: Developing nations;

evaluation; enrollment outcomes; youths) 104 yield:0hits; tried broader search term with first 3 (not youth terms) and used a variety of NOT

statements (BIOLOGY; GEOLOGY; INFANTS; AFRICAN AMERICANS; HIGHER EDUCATION; OCEANS; CHEMISTRY; MAMMALS; ANIMALS; ELECTRIC)

window to 1044 Yield/0 hits

***Accessun***

used enrollment terms with evaluation terms (one in title, other in all field, then reverse) through 2009

***Accesundp***

(74/0 - used enrollment terms with evaluation terms in all fields) through 2009

schooling OR enrol* OR attend* OR absent* OR absence* OR dropout* OR "drop- out" OR "drop-outs" OR "grade repetition" OR "repeat grade" OR "complete grade" OR "test score" OR "grade completion" OR "standardized test" OR matricul*

AND

evaluat* OR random* OR experiment* OR controlled OR "control group" OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR "study design" OR rigorous

***Agricola***

1 (Afghanistan* or Albania* or Samoa* or Angola* or Argentina* or Armenia*

or Azerbaijan* or Bangladesh* or Pakistan* or Bengal* or Belarus* or Byelorussia* or Belize* or Hondur* or Benin* or Dahomey* or Bhutan* or Bolivia* or Bosnia* or Herzegovina* or Botswana* or Bechuanaland* or Brazil* or Bulgaria* or Burkin* or Volta or Volta* or Burundi* or Cambodia* or Khmer* or Kampuchea* or Cameroon* or Verde* or Africa* or "Ubangi-Shari" or Chad* or Chile* or China* or Colombia* or Comoros* or Congo* or Zaire* or Rica* or "Cote d'Ivoire" or "Ivory Coast" or Cuba* or Djibouti* or Somali* or "Afars Issas" or Domini* or "Santo Domingo" or Ecuador* or Egypt* or Salvador* or Eritrea* or Ethiopia* or Abyssinia* or Fiji* or Gabon* or Gambia* or Georgia* or Ghana* or "Gold Coast" or Grenada* or Guinea* or Guyana* or Guiana* or Haiti* or India* or Indonesia* or "East Indies" or Iran* or Persia* or Iraq* or Jamaica* or Jordan* or Kazakhstan* or Kenya* or Kiribati* or "Gilbert Islands" or Korea* or Kosovo* or Kyrgyz* or Lao* or Latvia* or Leban* or Lesotho* or Basutoland* or Liberia* or Libya* or Lithuania* or Macedonia* or Madagascar* or Malaw* or Nyasaland* or Malaysia* or Malaya* or Maldives* or Mali* or Sudan* or "Marshall Islands" or Mauritania* or Mauritius* or Mayotte* or Mexic* or Micronesia* or Moldova* or Moldavia* or Mongolia* or Montenegro* or Yugoslavia* or Morocc* or Mozambique* or Myanmar* or Burm* or Namibia* or Nepal* or Nicaragua* or Niger* or Nigeria* or Palau* or Panama*OR Paraguay* or Peru* or Philippin* or Fillipino* or Poland* or Polish* or Romania* or Russia* or Rwanda* or "Sao Tome Principe" or Senegal* or Serbia* or Seychelles* or "Sierra Leone" or "Solomon Islands" or Lanka* or Ceylon* or "St Kitts" or Nevis* or Lucia* or "St Vincent" or Grenadines* or Suriname* or Swaziland* or Arab* or Syria* or Tajikistan* or Soviet* or Tanzania* or Thai* or Siam* or Timor* or Togo* or Tonga* or Tunisia* or Turk* or Uganda* or Ukraine* or Uruguay* or Uzbekistan* or Vanuatu* or Hebrides or Venezuela* or "Vietnam* West Bank" or Gaza* or Yemen* or Zambia* or Zimbabwe* or Bogota* or Asia* or "Latin America" or "developing nation" or "developing region" or "developing country" or "third world nation" or "third world country" or "third world region" or "low income nation" or "low income country" or "low income region" or "impoverished country" or "impoverished region" or "impoverished region").ab.

2 limit 1 to yr="1860 - 2009"

3 (schooling or enrol* or attend* or absent* or absence* or dropout* or "drop-out" or "drop-outs" or "grade repetition" or "repeat grade" or "complete grade" or "test score" or "grade completion" or "standardized test" or matricul*).ab.

4 limit 3 to yr="2009"

5 2 and 4

6 (evaluat* or random* or experiment* or controlled or "control group" or comparison* or propensity or discontinuity or match* or lotter* or "study design" or rigorous).ab.

7 limit 6 to yr="2009"

8 5 and 7

9 limit 8 to (yr="2009" and (book or book chapter or collection or conference publication or electronic or festschrift or government publications or journal article or serial))

10 (youth* or child* or student* or adolescent* or teen* or boy* or girl* or pupil* or youngster* or juveniles or minors or kids).ab.

***C2-spectr***

Afghanistan* OR Albania* OR Samoa* OR Angola* OR Argentina* OR Armenia* OR Azerbaijan* OR Bangladesh* OR Pakistan* OR Bengal* OR Belarus* OR Byelorussia* OR Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* OR Bolivia* OR Bosnia* OR Herzegovina* OR Botswana* OR Bechuanaland* OR Brazil* OR Bulgaria* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Cameroon* OR Verde* OR Africa* OR "Ubangi-Shari" OR Chad* OR Chile* OR China* OR Colombia* OR Comoros* OR Congo* OR Zaire* OR Rica* OR "Cote d'Ivoire" OR "Ivory Coast" OR Cuba* OR Djibouti* OR Somali* OR "Afars Issas" OR Domini* OR "Santo Domingo" OR Ecuador* OR Egypt* OR Salvador* OR Eritrea* OR Ethiopia* OR Abyssinia* OR Fiji* OR Gabon* OR Gambia* OR Georgia* OR Ghana* OR "Gold Coast" OR Grenada* OR Guinea* OR Guyana* OR Guiana* OR Haiti* OR India* OR Indonesia* OR "East Indies" OR Iran* OR Persia* OR Iraq* OR Jamaica* OR Jordan* OR Kazakhstan* OR Kenya* OR Kiribati* OR "Gilbert Islands" OR Korea* OR Kosovo* OR Kyrgyz* OR Lao* OR Latvia* OR Leban* OR Lesotho* OR Basutoland* OR Liberia* OR Libya* OR Lithuania* OR Macedonia* OR Madagascar* OR Malaw* OR Nyasaland* OR Malaysia* OR Malaya* OR Maldives* OR Mali* OR Sudan* OR "Marshall Islands" OR Mauritania* OR Mauritius* OR Mayotte* OR Mexic* OR Micronesia* OR Moldova* OR Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* OR Morocc* OR Mozambique* OR Myanmar* OR Burm* OR Namibia* OR Nepal* OR Nicaragua* OR Niger* OR Nigeria* OR Palau* OR Panama*OR Paraguay* OR Peru* OR Philippin* OR Fillipino* OR Poland* OR Polish* OR Romania* OR Russia* OR Rwanda* OR "Sao Tome Principe" OR Senegal* OR Serbia* OR Seychelles* OR "Sierra Leone" OR "Solomon Islands" OR Lanka* OR Ceylon* OR "St. Kitts" OR Nevis* OR Lucia* OR "St. Vincent" OR Grenadines* OR Suriname* OR Swaziland* OR Arab* OR Syria* OR Tajikistan* OR Soviet* OR Tanzania* OR Thai* OR Siam* OR Timor* OR Togo* OR Tonga* OR Tunisia* OR Turk* OR Uganda* OR Ukraine* OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides OR Venezuela* OR Vietnam* "West Bank" OR Gaza* OR Yemen* OR Zambia* OR Zimbabwe* OR Bogota* OR Asia* OR "Latin America" OR "developing nation" OR "developing region" OR "developing country" OR "third world nation" OR "third world country" OR "third world region" OR "low income nation" OR "low income country" OR "low income region" OR "impoverished country" OR "impoverished region" OR "impoverished region"

***Cab abstracts***

1 (Afghanistan* or Albania* or Samoa* or Angola* or Argentina* or Armenia*

or Azerbaijan* or Bangladesh* or Pakistan* or Bengal* or Belarus* or Byelorussia* or Belize* or Hondur* or Benin* or Dahomey* or Bhutan* or Bolivia* or Bosnia* or Herzegovina* or Botswana* or Bechuanaland* or Brazil* or Bulgaria* or Burkin* or Volta or Volta* or Burundi* or Cambodia* or Khmer* or Kampuchea* or Cameroon* or Verde* or Africa* or "Ubangi-Shari" or Chad* or Chile* or China* or Colombia* or Comoros* or Congo* or Zaire* or Rica* or "Cote d'Ivoire" or "Ivory Coast" or Cuba* or Djibouti* or Somali* or "Afars Issas" or Domini* or "Santo Domingo" or Ecuador* or Egypt* or Salvador* or Eritrea* or Ethiopia* or Abyssinia* or Fiji* or Gabon* or Gambia* or Georgia* or Ghana* or "Gold Coast" or Grenada* or Guinea* or Guyana* or Guiana* or Haiti* or India* or Indonesia* or "East Indies" or Iran* or Persia* or Iraq* or Jamaica* or Jordan* or Kazakhstan* or Kenya* or Kiribati* or "Gilbert Islands" or Korea* or Kosovo* or Kyrgyz* or Lao* or Latvia* or Leban* or Lesotho* or Basutoland* or Liberia* or Libya* or Lithuania* or Macedonia* or Madagascar* or Malaw* or Nyasaland* or Malaysia* or Malaya* or Maldives* or Mali* or Sudan* or "Marshall Islands" or Mauritania* or Mauritius* or Mayotte* or Mexic* or Micronesia* or Moldova* or Moldavia* or Mongolia* or Montenegro* or Yugoslavia* or Morocc* or Mozambique* or Myanmar* or Burm* or Namibia* or Nepal* or Nicaragua* or Niger* or Nigeria* or Palau* or Panama*OR Paraguay* or Peru* or Philippin* or Fillipino* or Poland* or Polish* or Romania* or Russia* or Rwanda* or "Sao Tome Principe" or Senegal* or Serbia* or Seychelles* or "Sierra Leone" or "Solomon Islands" or Lanka* or Ceylon* or "St Kitts" or Nevis* or Lucia* or "St Vincent" or Grenadines* or Suriname* or Swaziland* or Arab* or Syria* or Tajikistan* or Soviet* or Tanzania* or Thai* or Siam* or Timor* or Togo* or Tonga* or Tunisia* or Turk* or Uganda* or Ukraine* or Uruguay* or Uzbekistan* or Vanuatu* or Hebrides or Venezuela* or "Vietnam* West Bank" or Gaza* or Yemen* or Zambia* or Zimbabwe* or Bogota* or Asia* or "Latin America" or "developing nation" or "developing region" or "developing country" or "third world nation" or "third world country" or "third world region" or "low income nation" or "low income country" or "low income region" or "impoverished country" or "impoverished region" or "impoverished region") (1411968)

2 limit 1 to yr="1883 - 2009" (1390935)

3 (schooling or enrol* or attend* or absent* or absence* or dropout* or "drop-out" or "drop-outs" or "grade repetition" or "repeat grade" or "complete grade" or "test score" or "grade completion" or "standardized test" or matricul*).ab. (176507)

4 limit 3 to yr="1883 - 2009" (173103)

5 2 and 4 (39745)

6 (evaluat* or random* or experiment* or controlled or "control group" or comparison* or propensity or discontinuity or match* or lotter* or "study design" or rigorous).ab. (1628898)

7 limit 6 to yr="1883 - 2009" (1595303)

8 5 and 7 (11464)

9 (youth* or child* or student* or adolescent* or teen* or boy* or girl* or pupil* or youngster* or juveniles or minors or kids).ab. (175152)

10 limit 9 to yr="1883 - 2009" (171433)

11 8 and 10 (1958)

12 limit 11 to (abstract only or book or book chapter or conference paper or journal article or thesis) (1816)

1312 and "school children".sa_suba. (191)

14 from 13 keep 17, 52, 144, 174, 182-183 (6)

***Cochrane controlled trials register***

#1(Afghanistan* OR Albania* OR Samoa* OR Angola* OR Argentina* OR Armenia* OR Azerbaijan* OR Bangladesh* OR Pakistan* OR Bengal* OR Belarus* OR Byelorussia* OR Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* OR Bolivia* OR Bosnia* OR Herzegovina* OR Botswana* OR Bechuanaland* OR Brazil* OR Bulgaria* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Cameroon* OR Verde* OR Africa* OR "Ubangi-Shari" OR Chad* OR Chile* OR China* OR Colombia* OR Comoros* OR Congo* OR Zaire* OR Rica* OR "Cote d'Ivoire" OR "Ivory Coast" OR Cuba* OR Djibouti* OR Somali* OR "Afars Issas" OR Domini* OR "Santo Domingo" OR Ecuador* OR Egypt* OR Salvador* OR Eritrea* OR Ethiopia* OR Abyssinia* OR Fiji* OR Gabon* OR Gambia* OR Georgia* OR Ghana* OR "Gold Coast" OR Grenada* OR Guinea* OR Guyana* OR Guiana* OR Haiti* OR India* OR Indonesia* OR "East Indies" OR Iran* OR Persia* OR Iraq* OR Jamaica* OR Jordan* OR Kazakhstan* OR Kenya* OR Kiribati* OR "Gilbert Islands" OR Korea* OR Kosovo* OR Kyrgyz* OR Lao* OR Latvia* OR Leban* OR Lesotho* OR Basutoland* OR Liberia* OR Libya* OR Lithuania* OR Macedonia* OR Madagascar* OR Malaw* OR Nyasaland* OR Malaysia* OR Malaya* OR Maldives* OR Mali* OR Sudan* OR "Marshall Islands" OR Mauritania* OR Mauritius* OR Mayotte* OR Mexic* OR Micronesia* OR Moldova* OR Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* OR Morocc* OR Mozambique* OR Myanmar* OR Burm* OR Namibia* OR Nepal* OR Nicaragua* OR Niger* OR Nigeria* OR Palau* OR Panama*OR Paraguay* OR Peru* OR Philippin* OR Fillipino* OR Poland* OR Polish* OR Romania* OR Russia* OR Rwanda* OR "Sao Tome Principe" OR Senegal* OR Serbia* OR Seychelles* OR "Sierra Leone" OR "Solomon Islands" OR Lanka* OR Ceylon* OR "St. Kitts" OR Nevis* OR Lucia* OR "St. Vincent" OR Grenadines* OR Suriname* OR Swaziland* OR Arab* OR Syria* OR Tajikistan* OR Soviet* OR Tanzania* OR Thai* OR Siam* OR Timor* OR Togo* OR Tonga* OR Tunisia* OR Turk* OR Uganda* OR Ukraine* OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides OR Venezuela* OR Vietnam* "West Bank" OR Gaza* OR Yemen* OR Zambia* OR Zimbabwe* OR Bogota* OR Asia* OR "Latin America" OR "developing nation" OR "developing region" OR "developing country" OR "third world nation" OR "third world country" OR "third world region" OR "low income nation" OR "low income country" OR "low income region" OR "impoverished country" OR "impoverished region" OR "impoverished region"):ab, from 1800 to 2009 in Clinical Trials

#2(schooling OR enrol* OR attend* OR absent* OR absence* OR dropout* OR "drop-out" OR "drop-outs" OR "grade repetition" OR "repeat grade" OR "complete grade" OR "test score" OR "grade completion" OR "standardized test" OR matricul*):ab, from 1800 to 2009 in Clinical Trials

#3(youth* OR child* OR student* OR adolescent* OR teen* OR boy* OR girl* OR pupil* OR youngster* OR juveniles OR minors OR kids): ab

#4(#1 AND #2 AND #3), from 1800 to 2009

#5(evaluat* OR random* OR experiment* OR controlled OR "control group" OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR "study design" OR rigorous):ab in Clinical Trials

#6(#4 AND #5), from 1800 to 2009

#7(evaluat* OR random* OR experiment* OR controlled OR "control group" OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR "study design" OR rigorous):ab not "African American" OR "African Americans" OR "African-American" OR "African-Americans" OR "Asian American" OR "Asian Americans" OR "Mexican American" OR "Mexican Americans" OR "American Indian" OR "American Indians" OR Indiana:ab, from 1800 to 2009 in Clinical Trials

#8(#4 AND #7), from 1981 to 1990

#11 school* near/7 attend* in Clinical Trials 290 edit delete #2 school near/7 absen* in Clinical Trials

delete #3 school near/7 enrol* in Clinical Trials 109 edit delete #4 school near/7 drop* in Clinical Trials

delete #5 school near/7 persist* in Clinical Trials 40 edit delete #6 school near/7 repeat* in Clinical Trials

delete #7 school near/7 repet* in Clinical Trials 2 edit delete #8 school near/7 complet* in Clinical Trials

delete #9 school near/7 graduat* in Clinical Trials 1519 edit delete #10 school near/7 finish* in Clinical Trials 1 edit delete #11 (#1 OR #2 OR #3 OR #4 OR #5 OR #6 OR #7 OR #8 OR #9 OR #10), from 1991 to 2009

#12 (Afghanistan* OR Albania* OR Samoa* OR Angola* OR Argentina* OR Armenia* OR Azerbaijan* OR Bangladesh* OR Pakistan* OR Bengal* OR Belarus* OR Byelorussia* OR Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* OR Bolivia* OR Bosnia* OR Herzegovina* OR Botswana* OR Bechuanaland* OR Brazil* OR Bulgaria* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Cameroon* OR Verde* OR Africa* OR "Ubangi-Shari" OR Chad* OR Chile* OR China* OR Colombia* OR Comoros* OR Congo* OR Zaire* OR Rica* OR "Cote d'Ivoire" OR "Ivory Coast" OR Cuba* OR Djibouti* OR Somali* OR "Afars Issas" OR Domini* OR "Santo Domingo" OR Ecuador* OR Egypt* OR Salvador* OR Eritrea* OR Ethiopia* OR Abyssinia* OR Fiji* OR Gabon* OR Gambia* OR Georgia* OR Ghana* OR "Gold Coast" OR Grenada* OR Guinea* OR Guyana* OR Guiana* OR Haiti* OR India* OR Indonesia* OR "East Indies" OR Iran* OR Persia* OR Iraq* OR Jamaica* OR Jordan* OR Kazakhstan* OR Kenya* OR Kiribati* OR "Gilbert Islands" OR Korea* OR Kosovo* OR Kyrgyz* OR Lao* OR Latvia* OR Leban* OR Lesotho* OR Basutoland* OR Liberia* OR Libya* OR Lithuania* OR Macedonia* OR Madagascar* OR Malaw* OR Nyasaland* OR Malaysia* OR Malaya* OR Maldives* OR Mali* OR Sudan* OR "Marshall Islands" OR Mauritania* OR Mauritius* OR Mayotte* OR Mexic* OR Micronesia* OR Moldova* OR Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* OR Morocc* OR Mozambique* OR Myanmar* OR Burm* OR Namibia* OR Nepal* OR Nicaragua* OR Niger* OR Nigeria* OR Palau* OR Panama*OR Paraguay* OR Peru* OR Philippin* OR Fillipino* OR Poland* OR Polish* OR Romania* OR Russia* OR

Rwanda* OR "Sao Tome Principe" OR Senegal* OR Serbia* OR Seychelles* OR "Sierra Leone" OR "Solomon Islands" OR Lanka* OR Ceylon* OR "St. Kitts" OR Nevis* OR Lucia* OR "St. Vincent" OR Grenadines* OR Suriname* OR Swaziland* OR Arab* OR Syria* OR Tajikistan* OR Soviet* OR Tanzania* OR Thai* OR Siam* OR Timor* OR Togo* OR Tonga* OR Tunisia* OR Turk* OR Uganda* OR Ukraine* OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides OR Venezuela* OR Vietnam* "West Bank" OR Gaza* OR Yemen* OR Zambia* OR Zimbabwe* OR Bogota* OR Asia* OR "Latin America" OR "developing nation" OR "developing region" OR "developing country" OR "third world nation" OR "third world country" OR "third world region" OR "low income nation" OR "low income country" OR "low income region" OR "impoverished country" OR "impoverished region" OR "impoverished region"):ti,ab,kw, from 1991 to 2009 in Clinical Trials

#13 (#11 AND #12), from 1991 to 2010

***Criminal justice abstracts***

(82/0) Used nation terms with evaluation terms with school enrollment terms through 2009

Afghanistan* OR Albania* OR Samoa* OR Angola* OR Argentina* OR Armenia* OR Azerbaijan* OR Bangladesh* OR Pakistan* OR Bengal* OR Belarus* OR Byelorussia* OR Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* OR Bolivia* OR Bosnia* OR Herzegovina* OR Botswana* OR Bechuanaland* OR Brazil* OR Bulgaria* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Cameroon* OR Verde* OR Africa* OR "Ubangi-Shari" OR Chad* OR Chile* OR China* OR Colombia* OR Comoros* OR Congo* OR Zaire* OR Rica* OR "Cote d'Ivoire" OR "Ivory Coast" OR Cuba* OR Djibouti* OR Somali* OR "Afars Issas" OR Domini* OR "Santo Domingo" OR Ecuador* OR Egypt* OR Salvador* OR Eritrea* OR Ethiopia* OR Abyssinia* OR Fiji* OR Gabon* OR Gambia* OR Georgia* OR Ghana* OR "Gold Coast" OR Grenada* OR Guinea* OR Guyana* OR Guiana* OR Haiti* OR India* OR Indonesia* OR "East Indies" OR Iran* OR Persia* OR Iraq* OR Jamaica* OR Jordan* OR Kazakhstan* OR Kenya* OR Kiribati* OR "Gilbert Islands" OR Korea* OR Kosovo* OR Kyrgyz* OR Lao* OR Latvia* OR Leban* OR Lesotho* OR Basutoland* OR Liberia* OR Libya* OR Lithuania* OR Macedonia* OR Madagascar* OR Malaw* OR Nyasaland* OR Malaysia* OR Malaya* OR Maldives* OR Mali* OR Sudan* OR "Marshall Islands" OR Mauritania* OR Mauritius* OR Mayotte* OR Mexic* OR Micronesia* OR Moldova* OR Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* OR Morocc* OR Mozambique* OR Myanmar* OR Burm* OR Namibia* OR Nepal* OR Nicaragua* OR Niger* OR Nigeria* OR Palau* OR Panama*OR Paraguay* OR Peru* OR Philippin* OR Fillipino* OR Poland* OR Polish* OR Romania* OR Russia* OR Rwanda* OR "Sao Tome Principe" OR Senegal* OR Serbia* OR Seychelles* OR "Sierra Leone" OR "Solomon Islands" OR Lanka* OR Ceylon* OR "St. Kitts" OR Nevis* OR Lucia* OR "St. Vincent" OR Grenadines* OR Suriname* OR Swaziland* OR Arab* OR Syria* OR Tajikistan* OR Soviet* OR Tanzania* OR Thai* OR Siam* OR Timor* OR Togo* OR Tonga* OR Tunisia* OR Turk* OR Uganda* OR Ukraine* OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides OR Venezuela* OR Vietnam* "West Bank" OR Gaza* OR Yemen* OR Zambia* OR Zimbabwe* OR Bogota* OR Asia* OR "Latin America" OR "developing nation" OR "developing region" OR "developing country" OR "third world nation" OR "third world country" OR "third world region" OR "low income nation" OR "low income country" OR "low income region" OR "impoverished country" OR "impoverished region" OR "impoverished region"

AND

evaluat* OR random* OR experiment* OR controlled OR "control group" OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR "study design" OR rigorous

AND

schooling OR enrol* OR attend* OR absent* OR absence* OR dropout* OR "drop- out" OR "drop-outs" OR "grade repetition" OR "repeat grade" OR "complete grade" OR "test score" OR "grade completion" OR "standardized test" OR matricul*

***Econlit***

3 searches based on same search terms with additional descriptors listed at bottom.

AB=(Afghanistan* OR Albania* OR Samoa* OR Angola* OR Argentina* OR Armenia* OR Azerbaijan* OR Bangladesh* OR Pakistan* OR Bengal* OR Belarus* OR Byelorussia* OR Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* OR Bolivia* OR Bosnia* OR Herzegovina* OR Botswana* OR Bechuanaland* OR Brazil* OR Bulgaria* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Cameroon* OR Verde* OR Africa* OR "Ubangi-Shari" OR Chad* OR Chile* OR China* OR Colombia* OR Comoros* OR Congo* OR Zaire* OR Rica* OR "Cote d'Ivoire" OR "Ivory Coast" OR Cuba* OR Djibouti* OR Somali* OR "Afars Issas" OR Domini* OR "Santo Domingo" OR Ecuador* OR Egypt* OR Salvador* OR Eritrea* OR Ethiopia* OR Abyssinia* OR Fiji* OR Gabon* OR Gambia* OR Georgia* OR Ghana* OR "Gold Coast" OR Grenada* OR Guinea* OR Guyana* OR Guiana* OR Haiti* OR India* OR Indonesia* OR "East Indies" OR Iran* OR Persia* OR Iraq* OR Jamaica* OR Jordan* OR Kazakhstan* OR Kenya* OR Kiribati* OR "Gilbert Islands" OR Korea* OR Kosovo* OR Kyrgyz* OR Lao* OR Latvia* OR Leban* OR Lesotho* OR Basutoland* OR Liberia* OR Libya* OR Lithuania* OR Macedonia* OR Madagascar* OR Malaw* OR Nyasaland* OR Malaysia* OR Malaya* OR Maldives* OR Mali* OR Sudan* OR "Marshall Islands" OR Mauritania* OR Mauritius* OR Mayotte* OR Mexic* OR Micronesia* OR Moldova* OR Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* OR Morocc* OR Mozambique* OR Myanmar* OR Burm* OR Namibia* OR Nepal* OR Nicaragua* OR Niger* OR Nigeria* OR Palau* OR Panama*OR Paraguay* OR Peru* OR Philippin* OR Fillipino* OR Poland* OR Polish* OR Romania* OR Russia* OR Rwanda* OR "Sao Tome Principe" OR Senegal* OR Serbia* OR Seychelles* OR "Sierra Leone" OR "Solomon Islands" OR Lanka* OR Ceylon* OR "St. Kitts" OR Nevis* OR Lucia* OR "St. Vincent" OR Grenadines* OR Suriname* OR Swaziland* OR Arab* OR Syria* OR Tajikistan* OR Soviet* OR Tanzania* OR Thai* OR Siam* OR Timor* OR Togo* OR Tonga* OR Tunisia* OR Turk* OR Uganda* OR Ukraine* OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides OR Venezuela* OR Vietnam* "West Bank" OR Gaza* OR Yemen* OR Zambia* OR Zimbabwe* OR Bogota* OR Asia* OR "Latin America" OR "developing nation" OR "developing region" OR "developing country" OR "third world nation" OR "third world country" OR "third world region" OR "low income nation" OR "low income country" OR "low income region" OR "impoverished country" OR "impoverished region" OR "impoverished region") and AB=(evaluat* OR random* OR experiment* OR controlled OR "control group" OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR "study design" OR rigorous) and AB=(school* OR student* OR teacher* OR classroom* OR enrol* OR attend* OR absent* OR absence* OR dropout* OR "drop-out" OR "drop- outs" OR "grade repetition" OR "repeat grade" OR "complete grade" OR "test score" OR "grade completion" OR "standardized test") and DE=(analysis of education)

Your Comments: this is for the search (not de="analysis of education), and through 2004

Your Comments: Used descriptor="analysis of education" to reduce from 1009 to 195

Your Comments: This covers 2005-2009; run search again using pre-2005 to examine the remaining 500 citations. It looks like "blank" descriptor

fields and fields with "Education Government policy" and "analyzing human development" are good limiters

***Embase***

#12 #10 AND #11

#11 'schooling' /exp OR enrol* OR attend* OR absent* OR absence* OR dropout* OR 'drop-out' OR 'drop-outs' OR 'grade repetition' OR 'repeat grade' OR 'complete grade' OR 'test score' OR 'grade completion' OR 'standardized test' OR matricul* AND ([article]/lim OR [article in press]/lim OR [conference abstract]/lim OR [conference paper]/lim OR [letter]/lim OR [note]/lim) AND ([preschool]/lim OR [school]/lim OR [child]/lim OR [adolescent]/lim) AND [humans]/lim AND [embase]/lim AND [<1966-2010]/py

#10 #1 AND #7 AND #9

#9 school* OR student* e AND ([article]/lim OR [article in press]/lim OR [conference abstract]/lim OR [conference paper]/lim OR [letter]/lim OR [note]/lim) AND ([preschool]/lim OR [school]/lim OR [child]/lim OR [adolescent]/lim) AND [humans]/lim AND [embase]/lim AND [<1966-2010]/py 248473

#8 #5 AND #7

#7 evaluat* OR random* OR experiment* OR controlled OR 'control group' OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR 'study design' OR rigorous :ab AND ([article]/lim OR [article in press]/lim OR [conference abstract]/lim OR [conference paper]/lim OR [letter]/lim OR [note]/lim) AND ([preschool]/lim OR [school]/lim OR [child]/lim OR [adolescent]/lim) AND [humans]/lim AND [embase]/lim AND [<1966-2010]/py

#6 evaluat* OR random* OR experiment* OR controlled OR 'control group' OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR 'study design' OR rigorous :ab AND ([article]/lim OR [article in press]/lim OR [conference abstract]/lim OR [conference paper]/lim OR [letter]/lim OR [note]/lim) AND ([preschool]/lim OR [school]/lim OR [child]/lim OR [adolescent]/lim) AND [humans]/lim AND [embase]/lim AND [<1966-2010]/py

#5 #1 AND #4

#4 'student' /exp OR student OR 'school' /exp OR school AND [<1966-2010]/py 2616073

#3 #1 AND #2

#2 school e AND ([article]/lim OR [article in press]/lim OR [conference abstract]/lim OR [conference paper]/lim OR [letter]/lim OR [note]/lim) AND ([preschool]/lim OR [school]/lim OR [child]/lim OR [adolescent]/lim) AND [humans]/lim AND [embase]/lim AND [<1966-2010]/py

#1 developing AND countries :ab AND ([article]/lim OR [article in press]/lim OR [conference abstract]/lim OR [conference paper]/lim OR [letter]/lim OR [note]/lim) AND ([preschool]/lim OR [school]/lim OR [child]/lim OR [adolescent]/lim) AND [humans]/lim AND [embase]/lim AND [<1966-2010]/py

***Eric***

Query: (DE=(foreign or developing) and AB=(evaluat* OR random* OR experiment* OR controlled OR "control group" OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR "study design" OR rigorous) and AB=(schooling OR enrol* OR attend* OR absent* OR absence* OR dropout* OR "drop-out" OR "drop- outs" OR "grade repetition" OR "repeat grade" OR "complete grade" OR "test score" OR "grade completion" OR "standardized test" OR matricul*) and AB=( Afghanistan* OR Albania* OR Samoa* OR Angola* OR Argentina* OR Armenia* OR Azerbaijan* OR Bangladesh* OR Pakistan* OR Bengal* OR Belarus* OR Byelorussia* OR Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* OR Bolivia* OR Bosnia* OR Herzegovina* OR Botswana* OR Bechuanaland* OR Brazil* OR Bulgaria* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Cameroon* OR Verde* OR Africa* OR "Ubangi-Shari" OR Chad* OR Chile* OR China* OR Colombia* OR Comoros* OR Congo* OR Zaire* OR Rica* OR "Cote d'Ivoire" OR "Ivory Coast" OR Cuba* OR Djibouti* OR Somali* OR "Afars Issas" OR Domini* OR "Santo Domingo" OR Ecuador* OR Egypt* OR Salvador* OR Eritrea* OR Ethiopia* OR Abyssinia* OR Fiji* OR Gabon* OR Gambia* OR Georgia* OR Ghana* OR "Gold Coast" OR Grenada* OR Guinea* OR Guyana* OR Guiana* OR Haiti* OR India* OR Indonesia* OR "East Indies" OR Iran* OR Persia* OR Iraq* OR Jamaica* OR Jordan* OR Kazakhstan* OR Kenya* OR Kiribati* OR "Gilbert Islands" OR Korea* OR Kosovo* OR Kyrgyz* OR Lao* OR Latvia* OR Leban* OR Lesotho* OR Basutoland* OR Liberia* OR Libya* OR Lithuania* OR Macedonia* OR Madagascar* OR Malaw* OR Nyasaland* OR Malaysia* OR Malaya* OR Maldives* OR Mali* OR Sudan* OR "Marshall Islands" OR Mauritania* OR Mauritius* OR Mayotte* OR Mexic* OR Micronesia* OR Moldova* OR Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* OR Morocc* OR Mozambique* OR Myanmar* OR Burm* OR Namibia* OR Nepal* OR Nicaragua* OR Niger* OR Nigeria* OR Palau* OR Panama*OR Paraguay* OR Peru* OR Philippin* OR Fillipino* OR Poland* OR Polish* OR Romania* OR Russia* OR Rwanda* OR "Sao Tome Principe" OR Senegal* OR Serbia* OR Seychelles* OR "Sierra Leone" OR "Solomon Islands" OR Lanka* OR Ceylon* OR "St. Kitts" OR Nevis* OR Lucia* OR "St. Vincent" OR Grenadines* OR Suriname* OR Swaziland* OR Arab* OR Syria* OR Tajikistan* OR Soviet* OR Tanzania* OR Thai* OR Siam* OR Timor* OR Togo* OR Tonga* OR Tunisia* OR Turk* OR Uganda* OR Ukraine* OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides OR Venezuela* OR Vietnam* "West Bank" OR Gaza* OR Yemen* OR Zambia* OR Zimbabwe* OR Bogota* OR Asia* OR "Latin America" OR "developing nation" OR "developing region" OR "developing country" OR "third world nation" OR "third world country" OR "third world region" OR "low income nation" OR "low income country" OR "low income region" OR "impoverished country" OR "impoverished region" OR "impoverished region") and LV=(Elementary OR Secondary OR Grade OR Primary OR "High Schools" OR Intermediate OR Junior OR Middle OR Kindergarten)) and(PT=(books OR works OR dissertations OR theses OR analyses OR articles OR evaluative OR research)) and NOT (AB=Indiana OR "African-American" OR "African-Americans")

***Global health***

1 (Afghanistan* or Albania* or Samoa* or Angola* or Argentina* or Armenia*

or Azerbaijan* or Bangladesh* or Pakistan* or Bengal* or Belarus* or Byelorussia* or Belize* or Hondur* or Benin* or Dahomey* or Bhutan* or Bolivia* or Bosnia* or Herzegovina* or

Botswana* or Bechuanaland* or Brazil* or Bulgaria* or Burkin* or Volta or Volta* or Burundi* or Cambodia* or Khmer* or Kampuchea* or Cameroon* or Verde* or Africa* or "Ubangi-Shari" or Chad* or Chile* or China* or Colombia* or Comoros* or Congo* or Zaire* or Rica* or "Cote d'Ivoire" or "Ivory Coast" or Cuba* or Djibouti* or Somali* or "Afars Issas" or Domini* or "Santo Domingo" or Ecuador* or Egypt* or Salvador* or Eritrea* or Ethiopia* or Abyssinia* or Fiji* or Gabon* or Gambia* or Georgia* or Ghana* or "Gold Coast" or Grenada* or Guinea* or Guyana* or Guiana* or Haiti* or India* or Indonesia* or "East Indies" or Iran* or Persia* or Iraq* or Jamaica* or Jordan* or Kazakhstan* or Kenya* or Kiribati* or "Gilbert Islands" or Korea* or Kosovo* or Kyrgyz* or Lao* or Latvia* or Leban* or Lesotho* or Basutoland* or Liberia* or Libya* or Lithuania* or Macedonia* or Madagascar* or Malaw* or Nyasaland* or Malaysia* or Malaya* or Maldives* or Mali* or Sudan* or "Marshall Islands" or Mauritania* or Mauritius* or Mayotte* or Mexic* or Micronesia* or Moldova* or Moldavia* or Mongolia* or Montenegro* or Yugoslavia* or Morocc* or Mozambique* or Myanmar* or Burm* or Namibia* or Nepal* or Nicaragua* or Niger* or Nigeria* or Palau* or Panama*OR Paraguay* or Peru* or Philippin* or Fillipino* or Poland* or Polish* or Romania* or Russia* or Rwanda* or "Sao Tome Principe" or Senegal* or Serbia* or Seychelles* or "Sierra Leone" or "Solomon Islands" or Lanka* or Ceylon* or "St Kitts" or Nevis* or Lucia* or "St Vincent" or Grenadines* or Suriname* or Swaziland* or Arab* or Syria* or Tajikistan* or Soviet* or Tanzania* or Thai* or Siam* or Timor* or Togo* or Tonga* or Tunisia* or Turk* or Uganda* or Ukraine* or Uruguay* or Uzbekistan* or Vanuatu* or Hebrides or Venezuela* or "Vietnam* West Bank" or Gaza* or Yemen* or Zambia* or Zimbabwe* or Bogota* or Asia* or "Latin America" or "developing nation" or "developing region" or "developing country" or "third world nation" or "third world country" or "third world region" or "low income nation" or "low income country" or "low income region" or "impoverished country" or "impoverished region" or "impoverished region")

2 limit 1 to yr="1883 - 2009"

3 (schooling or enrol* or attend* or absent* or absence* or dropout* or "drop-out" or "drop-outs" or "grade repetition" or "repeat grade" or "complete grade" or "test score" or "grade completion" or "standardized test" or matricul*).ab.

4 limit 3 to yr="1883 - 2009"

5 2 and 4

6 (evaluat* or random* or experiment* or controlled or "control group" or comparison* or propensity or discontinuity or match* or lotter* or "study design" or rigorous).ab.

7 limit 6 to yr="1883 - 2009"

8 5 and 7

9 (youth* or child* or student* or adolescent* or teen* or boy* or girl* or pupil* or youngster* or juveniles or minors or kids).ab.

10 limit 9 to yr="1883 – 2009"

11 8 and 10

12 limit 11 to (abstract only or book or book chapter or conference paper or journal article or thesis) (1816)

13 12 and "school children".sa_suba.

14 from 13 keep 17, 52, 144, 174, 182-183

***Healthstar***

1 (Afghanistan* or Albania* or Samoa* or Angola* or Argentina* or Armenia*

or Azerbaijan* or Bangladesh* or Pakistan* or Bengal* or Belarus* or Byelorussia* or Belize* or Hondur* or Benin* or Dahomey* or Bhutan* or Bolivia* or Bosnia* or Herzegovina* or Botswana* or Bechuanaland* or Brazil* or Bulgaria* or Burkin* or Volta or Volta* or Burundi* or Cambodia* or Khmer* or Kampuchea* or Cameroon* or Verde* or Africa* or "Ubangi-Shari" or Chad* or Chile* or China* or Colombia* or Comoros* or Congo* or Zaire* or Rica* or "Cote d'Ivoire" or "Ivory Coast" or Cuba* or Djibouti* or Somali* or "Afars Issas" or Domini* or "Santo Domingo" or Ecuador* or Egypt* or Salvador* or Eritrea* or Ethiopia* or Abyssinia* or Fiji* or Gabon* or Gambia* or Georgia* or Ghana* or "Gold Coast" or Grenada* or Guinea* or Guyana* or Guiana* or Haiti* or India* or Indonesia* or "East Indies" or Iran* or Persia* or Iraq* or Jamaica* or Jordan* or Kazakhstan* or Kenya* or Kiribati* or "Gilbert Islands" or Korea* or Kosovo* or Kyrgyz* or Lao* or Latvia* or Leban* or Lesotho* or Basutoland* or Liberia* or Libya* or Lithuania* or Macedonia* or Madagascar* or Malaw* or Nyasaland* or Malaysia* or Malaya* or Maldives* or Mali* or Sudan* or "Marshall Islands" or Mauritania* or Mauritius* or Mayotte* or Mexic* or Micronesia* or Moldova* or Moldavia* or Mongolia* or Montenegro* or Yugoslavia* or Morocc* or Mozambique* or Myanmar* or Burm* or Namibia* or Nepal* or Nicaragua* or Niger* or Nigeria* or Palau* or Panama*OR Paraguay* or Peru* or Philippin* or Fillipino* or Poland* or Polish* or Romania* or Russia* or Rwanda* or "Sao Tome Principe" or Senegal* or Serbia* or Seychelles* or "Sierra Leone" or "Solomon Islands" or Lanka* or Ceylon* or "St Kitts" or Nevis* or Lucia* or "St Vincent" or Grenadines* or Suriname* or Swaziland* or Arab* or Syria* or Tajikistan* or Soviet* or Tanzania* or Thai* or Siam* or Timor* or Togo* or Tonga* or Tunisia* or Turk* or Uganda* or Ukraine* or Uruguay* or Uzbekistan* or Vanuatu* or Hebrides or Venezuela* or "Vietnam* West Bank" or Gaza* or Yemen* or Zambia* or Zimbabwe* or Bogota* or Asia* or "Latin America" or "developing nation" or "developing region" or "developing country" or "third world nation" or "third world country" or "third world region" or "low income nation" or "low income country" or "low income region" or "impoverished country" or "impoverished region" or "impoverished region") ab.

2 limit 1 to (humans and yr="1903 - 2009")

3 (schooling or enrol* or attend* or absent* or absence* or dropout* or "drop-out" or "drop-outs" or "grade repetition" or "repeat grade" or "complete grade" or "test score" or "grade completion" or "standardized test" or matricul* or graduat*).ab. 4 limit 3 to (humans and yr="1903 - 2009")

5 2 and 4

6 (evaluat* or random* or experiment* or controlled or "control group" or comparison* or propensity or discontinuity or match* or lotter* or "study design" or rigorous).ab.

7 limit 6 to (humans and yr="1903 - 2009")

8 5 and 7

9 ((youth* or child* or student* or adolescent* or teen* or boy* or girl* or pupil* or

youngster* or juveniles or minors or kids) not ("African Americans" or "African American" or "American Indians" or"American Indian" or "Mexican American" or "Mexican Americans" or Indiana or "New Mexico" or "Asian American" or "Asian Americans")).ab. (558780)

10 limit 9 to (humans and yr="1903 - 2009")

118 and 10

12 limit 11 to (humans and ("all child (0 to 18 years)" or "preschool child (2 to 5 years)" or "child (6 to 12 years)" or "adolescent (13 to 18 years)") and yr="1903 - 2009" and (clinical trial, all or comparative study or controlled clinical trial or evaluation studies or festschrift or government publications or journal article or multicenter study or randomized controlled trial or technical report) and humans) 13 ((((((((((((((school* adj10 attend*).ab. or school*.mp.) adj10 absen*.ab.) or school*.mp.) adj10 graduat*.ab.) or school*.mp.) adj10 repe*.ab.) or school*.mp.) adj10 enrol*.ab.) or school*.mp.) adj10 drop*.ab.) or school*.mp.) adj10 complet*.ab.) or school*.mp.) adj10 matric*.ab.) or school*.mp.) adj10 test.af. [mp=title, original title, abstract, name of substance word, subject heading word] 14 limit 13 to (humans and yr="1903 - 2009")

15 (school* adj10 attend*).ab. 16 (school* adj10 absen*).ab. 17 (school* adj10 enrol*).ab. 18 (school* adj10 graduat*).ab. 19 (school* adj10 repe*).ab.

20 (school* adj10 test*).ab. 21 (school* adj10 complet*).ab. 22 (school adj10 drop*).ab.

23 (school* adj10 matric*).ab.

24 15 or 16 or 17 or 18 or 19 or 20 or 21 or 22 or 23

25 12 and 24

26 limit 25 to (humans and ("all child (0 to 18 years)" or "preschool child (2 to 5 years)" or "child (6 to 12 years)" or "adolescent (13 to 18 years)") and yr="1903 - 2009" and (clinical trial, all or comparative study or controlled clinical trial or evaluation studies or government publications or journal article or randomized controlled trial or technical report))

27 from 26 keep 42,239

***International bibliography of the social sciences***

KW=( Afghanistan* or Albania* or Samoa* or Angola* or Argentina* or Armenia* or Azerbaijan* or Bangladesh* or Pakistan* or Bengal* or Belarus* or Byelorussia* or Belize* or Hondur* or Benin* or Dahomey* or Bhutan* or Bolivia* or Bosnia* or Herzegovina* or Botswana* or Bechuanaland* or Brazil* or Bulgaria* or Burkin* or Volta or Volta* or Burundi* or Cambodia* or Khmer* or Kampuchea* or Cameroon* or Verde* or Africa* or "Ubangi-Shari" or Chad* or Chile* or China* or Colombia* or Comoros* or Congo* or Zaire* or Rica* or "Cote d'Ivoire" or "Ivory Coast" or Cuba* or Djibouti* or Somali* or "Afars Issas" or Domini* or "Santo Domingo" or Ecuador* or Egypt* or Salvador* or Eritrea* or Ethiopia* or Abyssinia* or Fiji* or Gabon* or Gambia* or Georgia* or Ghana* or "Gold Coast" or Grenada* or Guinea* or Guyana* or Guiana* or Haiti* or India* or Indonesia* or "East Indies" or Iran* or Persia* or Iraq* or Jamaica* or Jordan* or Kazakhstan* or Kenya* or Kiribati* or "Gilbert Islands" or Korea* or Kosovo* or Kyrgyz* or Lao* or Latvia* or Leban* or Lesotho* or Basutoland* or Liberia* or Libya* or Lithuania* or Macedonia* or Madagascar* or Malaw* or Nyasaland* or Malaysia* or Malaya* or Maldives* or Mali* or Sudan* or "Marshall Islands" or

Mauritania* or Mauritius* or Mayotte* or Mexic* or Micronesia* or Moldova* or Moldavia* or Mongolia* or Montenegro* or Yugoslavia* or Morocc* or Mozambique* or Myanmar* or Burm* or Namibia* or Nepal* or Nicaragua* or Niger* or Nigeria* or Palau* or Panama*OR Paraguay* or Peru* or Philippin* or Fillipino* or Poland* or Polish* or Romania* or Russia* or Rwanda* or "Sao Tome Principe" or Senegal* or Serbia* or Seychelles* or "Sierra Leone" or "Solomon Islands" or Lanka* or Ceylon* or "St Kitts" or Nevis* or Lucia* or "St Vincent" or Grenadines* or Suriname* or Swaziland* or Arab* or Syria* or Tajikistan* or Soviet* or Tanzania* or Thai* or Siam* or Timor* or Togo* or Tonga* or Tunisia* or Turk* or Uganda* or Ukraine* or Uruguay* or Uzbekistan* or Vanuatu* or Hebrides or Venezuela* or "Vietnam* West Bank" or Gaza* or Yemen* or Zambia* or Zimbabwe* or Bogota* or Asia* or "Latin America" or "developing nation" or "developing region" or "developing country" or "third world nation" or "third world country" or "third world region" or "low income nation" or "low income country" or "low income region" or "impoverished country" or "impoverished region" or "impoverished region") and KW=(evaluat* OR random* OR experiment* OR controlled OR "control group" OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR "study design" OR rigorous) and KW=(school* OR student* OR teacher* OR classroom* OR enrol* OR attend* OR absent* OR absence* OR dropout* OR "drop out" OR "drop-outs" OR "grade repetition" OR "repeat grade" OR "complete grade" OR "test score" OR "grade completion" OR "standardized test" OR matricul*) and not AB=(Indiana OR "African-American") and DE=develop*

Search included: country terms, methods terms, outcome terms, limited by "not Indiana or "African-American"; descriptor included develop*

***Medline***

(((schooling or enrol* or attend* or absent* or absence* or dropout* or "drop-out" or "drop-outs" or "grade repetition" or "repeat grade" or "complete grade" or "test score" or "grade completion" or "standardized test" or matricul*) and (Afghanistan* or Albania* or Samoa* or Angola* or Argentina* or Armenia* or Azerbaijan* or Bangladesh* or Pakistan* or Bengal* or Belarus* or Byelorussia* or Belize* or Hondur* or Benin* or Dahomey* or Bhutan* or Bolivia* or Bosnia* or Herzegovina* or Botswana* or Bechuanaland* or Brazil* or Bulgaria* or Burkin* or Volta or Volta* or Burundi* or Cambodia* or Khmer* or Kampuchea* or Cameroon* or Verde* or Africa* or "Ubangi-Shari" or Chad* or Chile* or China* or Colombia* or Comoros* or Congo* or Zaire* or Rica* or "Cote d'Ivoire" or "Ivory Coast" or Cuba* or Djibouti* or Somali* or "Afars Issas" or Domini* or "Santo Domingo" or Ecuador* or Egypt* or Salvador* or Eritrea* or Ethiopia* or Abyssinia* or Fiji* or Gabon* or Gambia* or Georgia* or Ghana* or "Gold Coast" or Grenada* or Guinea* or Guyana* or Guiana* or Haiti* or India* or Indonesia* or "East Indies" or Iran* or Persia* or Iraq* or Jamaica* or Jordan* or Kazakhstan* or Kenya* or Kiribati* or "Gilbert Islands" or Korea* or Kosovo* or Kyrgyz* or Lao* or Latvia* or Leban* or Lesotho* or Basutoland* or Liberia* or Libya* or Lithuania* or Macedonia* or Madagascar* or Malaw* or Nyasaland* or Malaysia* or Malaya* or Maldives* or Mali* or Sudan* or "Marshall Islands" or Mauritania* or Mauritius* or Mayotte* or Mexic* or Micronesia* or Moldova* or Moldavia* or Mongolia* or Montenegro* or Yugoslavia* or Morocc* or Mozambique* or Myanmar* or Burm* or Namibia* or Nepal* or Nicaragua* or Niger* or Nigeria* or Palau* or Panama*OR Paraguay* or Peru* or Philippin* or Fillipino* or Poland* or Polish* or Romania* or Russia* or Rwanda* or "Sao Tome Principe" or Senegal* or Serbia* or Seychelles* or "Sierra Leone" or "Solomon Islands" or Lanka* or Ceylon* or "St Kitts" or Nevis* or Lucia* or "St Vincent" or Grenadines* or Suriname* or Swaziland* or

Arab* or Syria* or Tajikistan* or Soviet* or Tanzania* or Thai* or Siam* or Timor* or Togo* or Tonga* or Tunisia* or Turk* or Uganda* or Ukraine* or Uruguay* or Uzbekistan* or Vanuatu* or Hebrides or Venezuela* or "Vietnam* West Bank" or Gaza* or Yemen* or Zambia* or Zimbabwe* or Bogota* or Asia* or "Latin America" or "developing nation" or "developing region" or "developing country" or "third world nation" or "third world country" or "third world region" or "low income nation" or "low income country" or "low income region" or "impoverished country" or "impoverished region" or "impoverished region")) not ("African American" or "African Americans" or "African-American" or "African-Americans" or "Mexican American" or "Mexican Americans" or "American Indian" or "American Indians" or "Asian American" or "Asian Americans")).ab.

1902-1970: (Using search above with year and "human" limits)

1971-1980: (Using search and "humans" and pt=RCT,CCT, comparative study, Evaluation study, Clinical trial (all), multicenter study)

1981-1990: (Using search and humans and pt= above; and subject=preschool and school aged children)

1991-2000: included all of above and "Medical Subject Heading"= School* OR Student*

2001-2009: included all of above and MESH = School* OR STUDENT* OR Absenteeism*

**Pais/pais international**

2 SEARCHES

SEARCH 1:

AB=(Afghanistan* OR Albania* OR Samoa* OR Angola* OR Argentina* OR Armenia* OR Azerbaijan* OR Bangladesh* OR Pakistan* OR Bengal* OR Belarus* OR Byelorussia* OR Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* OR Bolivia* OR Bosnia* OR Herzegovina* OR Botswana* OR Bechuanaland* OR Brazil* OR Bulgaria* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Cameroon* OR Verde* OR Africa* OR "Ubangi-Shari" OR Chad* OR Chile* OR China* OR Colombia* OR Comoros* OR Congo* OR Zaire* OR Rica* OR "Cote d'Ivoire" OR "Ivory Coast" OR Cuba* OR Djibouti* OR Somali* OR "Afars Issas" OR Domini* OR "Santo Domingo" OR Ecuador* OR Egypt* OR Salvador* OR Eritrea* OR Ethiopia* OR Abyssinia* OR Fiji* OR Gabon* OR Gambia* OR Georgia* OR Ghana* OR "Gold Coast" OR Grenada* OR Guinea* OR Guyana* OR Guiana* OR Haiti* OR India* OR Indonesia* OR "East Indies" OR Iran* OR Persia* OR Iraq* OR Jamaica* OR Jordan* OR Kazakhstan* OR Kenya* OR Kiribati* OR "Gilbert Islands" OR Korea* OR Kosovo* OR Kyrgyz* OR Lao* OR Latvia* OR Leban* OR Lesotho* OR Basutoland* OR Liberia* OR Libya* OR Lithuania* OR Macedonia* OR Madagascar* OR Malaw* OR Nyasaland* OR Malaysia* OR Malaya* OR Maldives* OR Mali* OR Sudan* OR "Marshall Islands" OR Mauritania* OR Mauritius* OR Mayotte* OR Mexic* OR Micronesia* OR Moldova* OR Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* OR Morocc* OR Mozambique* OR Myanmar* OR Burm* OR Namibia* OR Nepal* OR Nicaragua* OR Niger* OR Nigeria* OR Palau* OR Panama*OR Paraguay* OR Peru* OR Philippin* OR Fillipino* OR Poland* OR Polish* OR Romania* OR Russia* OR Rwanda* OR "Sao Tome Principe" OR Senegal* OR Serbia* OR Seychelles* OR "Sierra Leone" OR "Solomon Islands" OR Lanka* OR Ceylon* OR "St. Kitts" OR Nevis* OR Lucia* OR "St. Vincent" OR Grenadines* OR Suriname* OR Swaziland* OR Arab* OR Syria* OR Tajikistan* OR Soviet* OR Tanzania* OR Thai* OR Siam* OR Timor* OR Togo* OR Tonga*

OR Tunisia* OR Turk* OR Uganda* OR Ukraine* OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides OR Venezuela* OR Vietnam* "West Bank" OR Gaza* OR Yemen* OR Zambia* OR Zimbabwe* OR Bogota* OR Asia* OR "Latin America" OR "developing nation" OR "developing region" OR "developing country" OR "third world nation" OR "third world country" OR "third world region" OR "low income nation" OR "low income country" OR "low income region" OR "impoverished country" OR "impoverished region" OR "impoverished region") and AB=(evaluat* OR random* OR experiment* OR control* OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR “study design” OR rigorous) and AB=(school* OR student* OR teacher* OR classroom* OR enrol* OR attend* OR absent* OR absence* OR dropout* OR “drop- out” OR “drop-outs” OR “grade repetition” OR “repeat grade” OR “complete school” OR “complete grade” OR “test score”)

SEARCH 2:

AB=(Afghanistan* OR Albania* OR Samoa* OR Angola* OR Argentina* OR Armenia* OR Azerbaijan* OR Bangladesh* OR Pakistan* OR Bengal* OR Belarus* OR Byelorussia* OR Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* OR Bolivia* OR Bosnia* OR Herzegovina* OR Botswana* OR Bechuanaland* OR Brazil* OR Bulgaria* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Cameroon* OR Verde* OR Africa* OR "Ubangi-Shari" OR Chad* OR Chile* OR China* OR Colombia* OR Comoros* OR Congo* OR Zaire* OR Rica* OR "Cote d’Ivoire" OR "Ivory Coast" OR Cuba* OR Djibouti* OR Somali* OR "Afars Issas" OR Domini* OR "Santo Domingo" OR Ecuador* OR Egypt* OR Salvador* OR Eritrea* OR Ethiopia* OR Abyssinia* OR Fiji* OR Gabon* OR Gambia* OR Georgia* OR Ghana* OR "Gold Coast" OR Grenada* OR Guinea* OR Guyana* OR Guiana* OR Haiti* OR India* OR Indonesia* OR "East Indies" OR Iran* OR Persia* OR Iraq* OR Jamaica* OR Jordan* OR Kazakhstan* OR Kenya* OR Kiribati* OR "Gilbert Islands" OR Korea* OR Kosovo* OR Kyrgyz* OR Lao* OR Latvia* OR Leban* OR Lesotho* OR Basutoland* OR Liberia* OR Libya* OR Lithuania* OR Macedonia* OR Madagascar* OR Malaw* OR Nyasaland* OR Malaysia* OR Malaya* OR Maldives* OR Mali* OR Sudan* OR "Marshall Islands" OR Mauritania* OR Mauritius* OR Mayotte* OR Mexic* OR Micronesia* OR Moldova* OR Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* OR Morocc* OR Mozambique* OR Myanmar* OR Burm* OR Namibia* OR Nepal* OR Nicaragua* OR Niger* OR Nigeria* OR Palau* OR Panama*OR Paraguay* OR Peru* OR Philippin* OR Fillipino* OR Poland* OR Polish* OR Romania* OR Russia* OR Rwanda* OR "Sao Tome Principe" OR Senegal* OR Serbia* OR Seychelles* OR "Sierra Leone" OR "Solomon Islands" OR Lanka* OR Ceylon* OR "St. Kitts" OR Nevis* OR Lucia* OR "St. Vincent" OR Grenadines* OR Suriname* OR Swaziland* OR Arab* OR Syria* OR Tajikistan* OR Soviet* OR Tanzania* OR Thai* OR Siam* OR Timor* OR Togo* OR Tonga* OR Tunisia* OR Turk* OR Uganda* OR Ukraine* OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides OR Venezuela* OR Vietnam* "West Bank" OR Gaza* OR Yemen* OR Zambia* OR Zimbabwe* OR Bogota* OR Asia* OR "Latin America" OR "developing nation" OR "developing region" OR "developing country" OR "third world nation" OR "third world country" OR "third world region" OR "low income nation" OR "low income country" OR "low income region" OR "impoverished country" OR "impoverished region" OR "impoverished region") and AB=(evaluat* OR random* OR experiment* OR control* OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR “study design” OR rigorous) and AB=(school* OR student* OR teacher* OR classroom* OR enrol* OR attend* OR absent* OR absence* OR dropout* OR drop-out* OR “grade repetition” OR “repeat grade” OR “complete school” OR “complete grade” OR “test score”)

**Policy file**

Using region qualifiers/through dec 31 2009/enrollment outcomes; try to reduce-- used evaluation/RCT terms in title;/REVERSED; FINAL SEARCH WAS "EDUCATION" AS SUBJECT; NATIONS IN TITLE; AND ENROLLMENT IN KEYWORDS

**Proquest theses and dissertations**

No documents found for: (schooling or enrol* or attend* or absent* or absence* or dropout* or "drop-out" or "drop-outs" or "grade repetition" or "repeat grade" or "complete grade" or "test score" or "grade completion" or "standardized test" or matricul* or graduat*) AND (random* or experiment* or controlled or "control group" or propensity or discontinuity or lotter*) AND (Grenada* OR Guinea*) AND (Russia*) AND PDN(>1/1/1992) AND PDN(<12/31/2009) AND NOT SU(Biochemistry OR Biology)

Afghanistan* OR Albania* 5/0 OR Samoa* OR Angola* 2/0 Argentina* 8/0

Armenia* OR Azerbaijan* OR Bangladesh* 16/0 Pakistan* OR Bengal* OR Belarus* OR Byelorussia* 17/0

Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* 12/0 Bolivia* 8/0

Bosnia* OR Herzegovina* OR Botswana* 5/0 Bechuanaland* OR Brazil* 34/0

Bulgaria* OR Burkin* 5/0

Volta OR Volta* and not voltage: 2/0 Burundi* OR Cambodia* OR Khmer* 2/0 Kampuchea* OR Cameroon* 2/0

Verde* OR Africa* 126/0

"Ubangi-Shari" OR Chad* OR Chile* 37/0 China* 142/0

Colombia* OR Comoros* OR Congo* OR Zaire* 29/1 "Costa Rica" OR "Costa Rican" 14/0

Ivoire OR "Ivory Coast" 0/0

Cuba* OR Djibouti* OR Somali* 13/0 "Afars Issas" 0/0

Domini* 27/0

"Santo Domingo" OR Ecuador* OR Egypt* 19/0 Salvador* OR Eritrea* OR Ethiopia* 20/0 Abyssinia* OR Fiji* OR Gabon* OR Gambia* 5/0 Georgia* 1/0

Ghana* OR "Gold Coast" 14/0 Grenada* OR Guinea* 20/0

Guyana* OR Guiana* OR Haiti* 10/0 India* 103/1

Indonesia* OR "East Indies" OR Iran* 26/0 Persia* OR Iraq* OR Jamaica* OR Jordan* 40/0 Kazakhstan* OR Kenya* 40/0

Kiribati* OR "Gilbert Islands" OR Korea* 55/0 Kosovo* OR Kyrgyz* OR Lao* OR Latvia* 6/0

Leban* OR Lesotho* OR Basutoland* OR Liberia* 6/0 Libya* OR Lithuania* OR Macedonia* OR Madagascar* 2/0 Malaw* OR Nyasaland* OR Malaysia* OR Malaya* 26/0 Maldives* OR Mali* OR Sudan* 33/0

"Marshall Islands" OR Mauritania* OR Mauritius* 2/0 Mexic* 101/0

Mayotte* OR Micronesia* OR Moldova* 0/0

Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* 3/0 Morocc* OR Mozambique* OR Myanmar* 3/0

Burm* OR Namibia* OR Nepal* OR Nicaragua* 27/1 Niger* OR Nigeria* OR Palau* OR Panama* 26/0 Paraguay* OR Peru* OR Philippin* OR Fillipino* 44/0 Poland* OR Polish* OR Romania* 23/0

Russia* 26/0

Rwanda* OR "Sao Tome Principe" 4/0 Senegal* 6/0

Serbia* OR Seychelles* OR "Sierra Leone" 1/0 "Solomon Islands" OR Lanka* OR Ceylon* 4/0 "St. Kitts" OR Nevis* OR Lucia* 6/0

"St. Vincent" OR Grenadines* OR Suriname* 6/0 Swaziland* 0/0

Arab* 72/0

Syria* OR Tajikistan* 9/0 Soviet* 17/0

Tanzania* OR Thai* OR Siam* OR Timor* 55/0 Togo* OR Tonga* OR Tunisia* 3/0

Turk* OR Uganda* OR Ukraine* 56/0

OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides 2/0 Venezuela* OR Vietnam* 37/1

"West Bank" OR Gaza* 3/0

Yemen* OR Zambia* OR Zimbabwe* OR Bogota* 18/0 Asia* 146/0

"Latin America" 13/0 "developing nation" 4/0 "developing region" 0/0

"developing country" 69/0 "third world nation" 0/0 "third world country" 1/0 "third world region" 0/0 "low income nation" 0/0 "low income country" 1/0 "low income region" 0/0 "impoverished country" 0/0 "impoverished nation" 0/0 "impoverished region" 0/0

***Psycinfo***

(((schooling or enrol* or attend* or absent* or absence* or dropout* or "drop-out" or "drop-outs" or "grade repetition" or "repeat grade" or "complete grade" or "test score" or "grade completion" or "standardized test" or matricul*) and (Afghanistan* OR Albania* OR Samoa* OR Angola* OR Argentina* OR Armenia* OR Azerbaijan* OR Bangladesh* OR Pakistan* OR Bengal* OR Belarus* OR Byelorussia* OR Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* OR Bolivia* OR Bosnia* OR Herzegovina* OR Botswana* OR Bechuanaland* OR Brazil* OR Bulgaria* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Cameroon* OR Verde* OR Africa* OR "Ubangi-Shari" OR Chad* OR Chile* OR China* OR Colombia* OR Comoros* OR Congo* OR Zaire* OR Rica* OR "Cote d'Ivoire" OR "Ivory Coast" OR Cuba* OR Djibouti* OR Somali* OR "Afars Issas" OR Domini* OR "Santo Domingo" OR Ecuador* OR Egypt* OR Salvador* OR Eritrea* OR Ethiopia* OR Abyssinia* OR Fiji* OR Gabon* OR Gambia* OR Georgia* OR Ghana* OR "Gold Coast" OR Grenada* OR Guinea* OR Guyana* OR Guiana* OR Haiti* OR India* OR Indonesia* OR "East Indies" OR Iran* OR Persia* OR Iraq* OR

Jamaica* OR Jordan* OR Kazakhstan* OR Kenya* OR Kiribati* OR "Gilbert Islands" OR Korea* OR Kosovo* OR Kyrgyz* OR Lao* OR Latvia* OR Leban* OR Lesotho* OR Basutoland* OR Liberia* OR Libya* OR Lithuania* OR Macedonia* OR Madagascar* OR Malaw* OR Nyasaland* OR Malaysia* OR Malaya* OR Maldives* OR Mali* OR Sudan* OR "Marshall Islands" OR Mauritania* OR Mauritius* OR Mayotte* OR Mexic* OR Micronesia* OR Moldova* OR Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* OR Morocc* OR Mozambique* OR Myanmar* OR Burm* OR Namibia* OR Nepal* OR Nicaragua* OR Niger* OR Nigeria* OR Palau* OR Panama*OR Paraguay* OR Peru* OR Philippin* OR Fillipino* OR Poland* OR Polish* OR Romania* OR Russia* OR Rwanda* OR "Sao Tome Principe" OR Senegal* OR Serbia* OR Seychelles* OR "Sierra Leone" OR "Solomon Islands" OR Lanka* OR Ceylon* OR "St. Kitts" OR Nevis* OR Lucia* OR "St. Vincent" OR Grenadines* OR Suriname* OR Swaziland* OR Arab* OR Syria* OR Tajikistan* OR Soviet* OR Tanzania* OR Thai* OR Siam* OR Timor* OR Togo* OR Tonga* OR Tunisia* OR Turk* OR Uganda* OR Ukraine* OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides OR Venezuela* OR Vietnam* "West Bank" OR Gaza* OR Yemen* OR Zambia* OR Zimbabwe* OR Bogota* OR Asia* OR "Latin America" OR "developing nation" OR "developing region" OR "developing country" OR "third world nation" OR "third world country" OR "third world region" OR "low income nation" OR "low income country" OR "low income region" OR "impoverished country" OR "impoverished region" OR "impoverished region") not ("African American" or "African Americans" or "African-American" or "African-Americans" or "Mexican American" or "Mexican Americans" or "American Indian" or "American Indians" or "Asian American" or "Asian Americans")).ab.

1902-1970 (Using search above with year and "human" limits)

1971-1980: (Using search and "humans" and pt=RCT, CCT, comparative study, Evaluation study, Clinical trial (all), multicenter study)

1981-1990: (Using search and humans and pt= above; and subject=preschool and school aged children)

1991-2000: included all of above and "Medical Subject Heading"= School* OR Student*

2001-2009: included all of above and MESH = School* OR STUDENT* OR Absenteeism*

***Social service abstracts***

Used enrollment terms+developing nations+evaluation terms, limited to through 2009; pt including book, dissertation, journal article, working paper)

***Social work abstracts***

1 (Afghanistan* OR Albania* OR Samoa* OR Angola* OR Argentina* OR Armenia* OR Azerbaijan* OR Bangladesh* OR Pakistan* OR Bengal* OR Belarus* OR Byelorussia* OR Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* OR Bolivia* OR Bosnia* OR Herzegovina* OR Botswana* OR Bechuanaland* OR Brazil* OR Bulgaria* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Cameroon* OR Verde* OR Africa* OR "Ubangi-Shari" OR Chad* OR Chile* OR China* OR Colombia* OR Comoros* OR Congo* OR Zaire* OR Rica* OR "Cote d'Ivoire" OR "Ivory Coast" OR Cuba* OR Djibouti* OR Somali* OR "Afars Issas" OR Domini* OR "Santo Domingo" OR Ecuador* OR Egypt* OR Salvador* OR Eritrea* OR Ethiopia* OR Abyssinia* OR Fiji* OR Gabon* OR Gambia* OR Georgia* OR Ghana* OR "Gold Coast" OR Grenada* OR Guinea*

OR Guyana* OR Guiana* OR Haiti* OR India* OR Indonesia* OR "East Indies" OR Iran* OR Persia* OR Iraq* OR Jamaica* OR Jordan* OR Kazakhstan* OR Kenya* OR Kiribati* OR "Gilbert Islands" OR Korea* OR Kosovo* OR Kyrgyz* OR Lao* OR Latvia* OR Leban* OR Lesotho* OR Basutoland* OR Liberia* OR Libya* OR Lithuania* OR Macedonia* OR Madagascar* OR Malaw* OR Nyasaland* OR Malaysia* OR Malaya* OR Maldives* OR Mali* OR Sudan* OR "Marshall Islands" OR Mauritania* OR Mauritius* OR Mayotte* OR Mexic* OR Micronesia* OR Moldova* OR Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* OR Morocc* OR Mozambique* OR Myanmar* OR Burm* OR Namibia* OR Nepal* OR Nicaragua* OR Niger* OR Nigeria* OR Palau* OR Panama*OR Paraguay* OR Peru* OR Philippin* OR Fillipino* OR Poland* OR Polish* OR Romania* OR Russia* OR Rwanda* OR "Sao Tome Principe" OR Senegal* OR Serbia* OR Seychelles* OR "Sierra Leone" OR "Solomon Islands" OR Lanka* OR Ceylon* OR "St. Kitts" OR Nevis* OR Lucia* OR "St. Vincent" OR Grenadines* OR Suriname* OR Swaziland* OR Arab* OR Syria* OR Tajikistan* OR Soviet* OR Tanzania* OR Thai* OR Siam* OR Timor* OR Togo* OR Tonga* OR Tunisia* OR Turk* OR Uganda* OR Ukraine* OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides OR Venezuela* OR Vietnam* "West Bank" OR Gaza* OR Yemen* OR Zambia* OR Zimbabwe* OR Bogota* OR Asia* OR "Latin America" OR "developing nation" OR "developing region" OR "developing country" OR "third world nation" OR "third world country" OR "third world region" OR "low income nation" OR "low income country" OR "low income region" OR "impoverished country" OR "impoverished region" OR "impoverished region") .ab.

2 limit 1 to yr="1968 - 2009"

3 (schooling or enrol* or attend* or absent* or absence* or dropout* or "drop-out" or "drop-outs" or "grade repetition" or "repeat grade" or "complete grade" or "test score" or "grade completion" or "standardized test" or matricul*).ab.

4 limit 3 to yr="1968 - 2009"

5 2 and 4

6 (evaluat* or random* or experiment* or controlled or "control group" or comparison* or propensity or discontinuity or match* or lotter* or "study design" or rigorous).ab.

7 limit 6 to yr="1968 - 2009"

8 8 5 and 7

***Sociological abstracts***

(AB=(evaluat* OR random* OR experiment* OR controlled OR "control group" OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR "study design" OR rigorous) and AB=(schooling OR enrol* OR attend* OR absent* OR absence* OR dropout* OR "drop-out" OR "drop-outs" OR "grade repetition" OR "repeat grade" OR "complete grade" OR "test score" OR "grade completion" OR "standardized test" OR matricul*) and AB= (Afghanistan* OR Albania* OR Samoa* OR Angola* OR Argentina* OR Armenia* OR Azerbaijan* OR Bangladesh* OR Pakistan* OR Bengal* OR Belarus* OR Byelorussia* OR Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* OR Bolivia* OR Bosnia* OR Herzegovina* OR Botswana* OR Bechuanaland* OR Brazil* OR Bulgaria* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Cameroon* OR Verde* OR Africa* OR "Ubangi-Shari" OR Chad* OR Chile* OR China* OR Colombia*

OR Comoros* OR Congo* OR Zaire* OR Rica* OR "Cote d'Ivoire" OR "Ivory Coast" OR Cuba* OR Djibouti* OR Somali* OR "Afars Issas" OR Domini* OR "Santo Domingo" OR Ecuador* OR Egypt* OR Salvador* OR Eritrea* OR Ethiopia* OR Abyssinia* OR Fiji* OR Gabon* OR Gambia* OR Georgia* OR Ghana* OR "Gold Coast" OR Grenada* OR Guinea* OR Guyana* OR Guiana* OR Haiti* OR India* OR Indonesia* OR "East Indies" OR Iran* OR Persia* OR Iraq* OR Jamaica* OR Jordan* OR Kazakhstan* OR Kenya* OR Kiribati* OR "Gilbert Islands" OR Korea* OR Kosovo* OR Kyrgyz* OR Lao* OR Latvia* OR Leban* OR Lesotho* OR Basutoland* OR Liberia* OR Libya* OR Lithuania* OR Macedonia* OR Madagascar* OR Malaw* OR Nyasaland* OR Malaysia* OR Malaya* OR Maldives* OR Mali* OR Sudan* OR "Marshall Islands" OR Mauritania* OR Mauritius* OR Mayotte* OR Mexic* OR Micronesia* OR Moldova* OR Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* OR Morocc* OR Mozambique* OR Myanmar* OR Burm* OR Namibia* OR Nepal* OR Nicaragua* OR Niger* OR Nigeria* OR Palau* OR Panama*OR Paraguay* OR Peru* OR Philippin* OR Fillipino* OR Poland* OR Polish* OR Romania* OR Russia* OR Rwanda* OR "Sao Tome Principe" OR Senegal* OR Serbia* OR Seychelles* OR "Sierra Leone" OR "Solomon Islands" OR Lanka* OR Ceylon* OR "St. Kitts" OR Nevis* OR Lucia* OR "St. Vincent" OR Grenadines* OR Suriname* OR Swaziland* OR Arab* OR Syria* OR Tajikistan* OR Soviet* OR Tanzania* OR Thai* OR Siam* OR Timor* OR Togo* OR Tonga* OR Tunisia* OR Turk* OR Uganda* OR Ukraine* OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides OR Venezuela* OR Vietnam* "West Bank" OR Gaza* OR Yemen* OR Zambia* OR Zimbabwe* OR Bogota* OR Asia* OR "Latin America" OR "developing nation" OR "developing region" OR "developing country" OR "third world nation" OR "third world country" OR "third world region" OR "low income nation" OR "low income country" OR "low income region" OR "impoverished country" OR "impoverished region" OR "impoverished region") and not AB= ("African American" or "African Americans" or "African-American" or "African- Americans" or "Mexican American" or "Mexican Americans" or "American Indian" or "American Indians" or "Asian American" or "Asian Americans") and PT=(book OR paper OR dissertation OR article)) and (DE=(foreign or develop*))

INCLUDED countries/methods/enrollment/pub type/descriptors=foreign and develop*/through 2009

***Un-docs***

(Subject searches: absenteeism; academic achievement; development research; education research; evaluation research; evaluation=subject and enrollment keywords; programme evaluation=subject and enrollment keywords; project

Evaluation = subject and enrollment keywords; school attendance; secondary education; primary education)

***Worldwide political science abstracts***

AB=(evaluat* OR random* OR experiment* OR controlled OR "control group" OR comparison* OR propensity OR discontinuity OR match* OR lotter* OR "study design" OR rigorous) and AB=schooling and AB=(Afghanistan* OR Albania* OR Samoa* OR Angola* OR Argentina* OR Armenia* OR Azerbaijan* OR Bangladesh* OR Pakistan* OR Bengal* OR Belarus* OR Byelorussia* OR Belize* OR Hondur* OR Benin* OR Dahomey* OR Bhutan* OR Bolivia* OR Bosnia* OR Herzegovina* OR Botswana* OR Bechuanaland* OR Brazil* OR Bulgaria* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR

Khmer* OR Kampuchea* OR Cameroon* OR Verde* OR Africa* OR "Ubangi-Shari" OR Chad* OR Chile* OR China* OR Colombia* OR Comoros* OR Congo* OR Zaire* OR Rica* OR "Cote d'Ivoire" OR "Ivory Coast" OR Cuba* OR Djibouti* OR Somali* OR "Afars Issas" OR Domini* OR "Santo Domingo" OR Ecuador* OR Egypt* OR Salvador* OR Eritrea* OR Ethiopia* OR Abyssinia* OR Fiji* OR Gabon* OR Gambia* OR Georgia* OR Ghana* OR "Gold Coast" OR Grenada* OR Guinea* OR Guyana* OR Guiana* OR Haiti* OR India* OR Indonesia* OR "East Indies" OR Iran* OR Persia* OR Iraq* OR Jamaica* OR Jordan* OR Kazakhstan* OR Kenya* OR Kiribati* OR "Gilbert Islands" OR Korea* OR Kosovo* OR Kyrgyz* OR Lao* OR Latvia* OR Leban* OR Lesotho* OR Basutoland* OR Liberia* OR Libya* OR Lithuania* OR Macedonia* OR Madagascar* OR Malaw* OR Nyasaland* OR Malaysia* OR Malaya* OR Maldives* OR Mali* OR Sudan* OR "Marshall Islands" OR Mauritania* OR Mauritius* OR Mayotte* OR Mexic* OR Micronesia* OR Moldova* OR Moldavia* OR Mongolia* OR Montenegro* OR Yugoslavia* OR Morocc* OR Mozambique* OR Myanmar* OR Burm* OR Namibia* OR Nepal* OR Nicaragua* OR Niger* OR Nigeria* OR Palau* OR Panama*OR Paraguay* OR Peru* OR Philippin* OR Fillipino* OR Poland* OR Polish* OR Romania* OR Russia* OR Rwanda* OR "Sao Tome Principe" OR Senegal* OR Serbia* OR Seychelles* OR "Sierra Leone" OR "Solomon Islands" OR Lanka* OR Ceylon* OR "St. Kitts" OR Nevis* OR Lucia* OR "St. Vincent" OR Grenadines* OR Suriname* OR Swaziland* OR Arab* OR Syria* OR Tajikistan* OR Soviet* OR Tanzania* OR Thai* OR Siam* OR Timor* OR Togo* OR Tonga* OR Tunisia* OR Turk* OR Uganda* OR Ukraine* OR Uruguay* OR Uzbekistan* OR Vanuatu* OR Hebrides OR Venezuela* OR Vietnam* "West Bank" OR Gaza* OR Yemen* OR Zambia* OR Zimbabwe* OR Bogota* OR Asia* OR "Latin America" OR "developing nation" OR "developing region" OR "developing country" OR "third world nation" OR "third world country" OR "third world region" OR "low income nation" OR "low income country" OR "low income region" OR "impoverished country" OR "impoverished region" OR "impoverished region") and not AB= ("African American" or "African Americans" or "African-American" or "African- Americans" or "Mexican Americans" or "American Indian" or "American Indians")

## Contacted authors

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| James Ohls | Mathematica policy research | United States |
| Peter Orazem | Iowa State University | United States |
| Laura Rawlings | World Bank | Latin America/Carib |
| Gloria Rubio | Sedesol | Mexico |
| Yasuyuki Sawada | University of Toyko | Japan |
| Norbert Schady | World Bank | Latin America/Carib |
| Andre Portela Souza | Fundacao Getulio Vargas | Brazil |
| Kartini Shastry | Wellesley College | United States |
| Jos Vaessen | Maastricht University | Netherlands |
| Emiliana Vegas | World Bank | United States |
| Nick York | Department for International Development – | (UK) United Kingdom |

## Studies excluded at final screening (with reason for exclusion in parentheses)

Banerjee, A., Cole, S., Duflo, E., & Linden, L. (2005). *Remedying Education: Evidence from Two Randomized Experiments in India*. BREAD Working Paper 109. Duke University, Department of Economics (This report actually includes seven randomized experiments; two were not included because insufficient data were not provided to permit computation of an effect size).

Barrera-Osorio, F. (2006). *The Impact of Private Provision of Public Education: Empirical Evidence from Bogota's Concession Schools.* Policy Research Working Paper, Impact Evaluation Series 4121. Washington, DC: World Bank. (No baseline control for main outcome).

Borraz, F. & Gonzalez, N. (2009). Impact of the Uruguayan conditional cash transfer program. *Cuadernos De Economia,* 46, 243-271. (No baseline control on attendance).

Cardoso, E. & Souza, A.P. (2004). *The Impact of Cash Transfers on Child Labor and School Attendance in Brazil.* Nashville, TN: Vanderbilt University, Department of Economics (Working Paper (04-W07). (No baseline control on attendance).

Cameron, L. (2002). *Did Social Safety Net Scholarships Reduce Drop-Out Rates During the Indonesian Economic Crisis?* Unpublished paper. (No baseline control for dropout)

Centre for Population and Development Activities (2001). *Adolescent Girls in India Choose a Better Future: An Impact Assessment.* Washington, DC: CEPDA. (No baseline control for enrollment or graduation).

Chase, R.S. (2002). Supporting communities in transition: The impact of the Amenian Social Investment Fund. *World Bank Economic Review*, 16(2), 219-240. (No baseline control of enrollment)

Chase, R.S. & Sherburne-Benz, L. (2001). *Household Effects of African Community Initiatives: Evaluating the Impact of the Zambia Social Fund*. Unpublished Report. (No baseline control of attendance).

Chatterji, M., Hutchinson, P., Murray, N., Buek, K., Mulenga, Y., & Ventimiglia, T. (2009). Evaluating the impact of community-based interventions on schooling outcomes among orphans and vulnerable children in Lusaka, Zambia. Chapel Hill, North Carolina: Carolina Population Center, University of North Carolina at Chapel Hill (Measure Evaluation Working Paper Series, WP-09-110). (No baseline control on enrollment, attendance or progression).

Chaturvedi, S., Srivastave, B., Singh, J. & Prasad, M. (1987). Impact of six years exposure to ICDS scheme on psycho-social development. *Indian Pediatrics: Journal of the Indian Academy of Pediatrics,* 24(2), 153-160. (Data collected prior to 1990; fails to meet data collection of 1990-2009 eligibility screen).

Chemin, M. (2008). The benefits and costs of microfinance: Evidence from Bangladesh. *Journal of Development Studies,* 44(4), 463-484. (No baseline control for enrollment of boys and girls).

Clark, N., Gong, M.M., Kacirott, N., Yu, J., Wu, G., Zeng, Z., & Wu, Z (2005). A trial of

asthma self-management in the Beijing schools. *Chronic Illness*, 1:31-38. (Insufficient data for computing effect size).

Colbourne, M. J. (1955). The effect of malaria supression in a group of Accra school children. *Transactions of the Royal Society of Tropical Medicine and Hygiene*, 49(4), 356-369. (Data collected in 1953-1954; does not meet screening criteria of using post-1990 data).

Cueto, S. & Chinen, M. (2008). Educational impact of a school breakfast programme in rural Peru. *International Journal of Educational Development*, 28, 132-

148. (No evidence of baseline equivalence on outcome variables of attendance, dropout and enrollment).

De Oliviera, A. (no date). *An Evaluation of the Bolsa Familia Program in Brazil: Expenditures, Education and Labor Outcomes.* Unpublished Report. (No baseline control for attendance, dropout, or continuation to next grade).

Di Gropello, E. & Marshall, J.H. (2005). Teacher effort and schooling outcomes in rural Honduras. In E. Vegas (Ed.) *Incentives to Improve Teaching. Lessons from Latin America* (305-357). Washington, DC: World Bank. (No baseline control on absenteeism, dropout, or repetition).

Duflo, E., Dupas, P., & Kremer, M. (2009). *Additional Resources versus Organizational Changes in Education: Experimental Evidence from Kenya.* Unpublished Report. (This study excluded because it used overlapping students and schools as RCT in review sample).

Duflo, E., Dupas, P., & Kremer, M. (2008). *Peer Effects and the Impact of Tracking: Evidence from a Randomized Evaluation in Kenya.* Unpublished Report.

Duryea, S. & Morrison, A. (2004). *The Effect of Conditional Transfers on School Performance and Child Labor: Evidence from an Ex-Post Impact Evaluation in Costa Rica*. Working Paper 505. Washington, DC: Inter- American Development Bank. (No baseline control of attendance).

Edmonds, E. V. (2004). *Does Illiquidity Alter Child Labor and Schooling Decisions? Evidence from Household Responses to Anticipated Cash Transfers in South Africa.* Cambridge, MA: National Bureau of Economic Research (Investigator initiated regression discontinuity; not based on actual program rules).

Erulkar, A. S. & Muthengi, E. (2009). Evaluation of Berhane Hewan: A program to delay child marriage in rural Ethiopia. *International Perspectives on Sexual and Reproductive Health*, 36(1), 6-14. (Covariate matching on very few variables, no evidence provided of group equivalence on baselines of enrollment or years of schooling outcomes).

Graeff-Martins, A., Oswald, S.S., Comassetto, J.O., Kieling, C., Goncalves, R., & Rohde, L.A. (2006). A package of interventions to reduce school dropout in public schools in a developing country. A feasibility study. *European Child & Adolescent Psychiatry,* 15, 442-449. (Random assignment of only one school to each group; no evidence of equivalence).

Grogan, L. (2008). Universal primary education and school entry in Uganda. *Journal of African Economies,* 18(2), 183-211. (Investigator initiated regression discontinuity; not based on actual program rules).

Jalan, J. & Glinskaya, E. (no date). *Improving Primary School Education in India: An Impact Assessment of DPEP-Phase I*. No Date. Unpublished Report. (Insufficent data to compute effect size).

Jimenez, E. & Sawada, Y. (1999). Do community-managed schools work? An evaluation of El Salvador's EDUCO program. *World Bank Economic Review,* 13(3), 415-441. (No baseline collection of absenteeism or dropout).

Kremer, M., Moulin, S. & Namunyu, R. (2003). *Decentralization: A Cautionary Tale*. Cambridge, MA: Poverty Action Lab (Poverty Action Lab Paper Number 10). (Insufficient data to compute effect sizes).

Litschig, S. (2007). *Intergovernmental Transfers and Elementary Education: Quasi-Experimental Evidence from Brazil.* New York: Columbia University, Department of Economics (Author requested that we used 2010 publication and not the 2007 report; 2010 publication does not meet pre-2010 publication eligibility criteria).

Maluccio, J., Hoddinott, J., Behrman, J.R., Martorell, R., Quisumbing, A.R., & Stein,

A.D. (2009). The impact of improving nutrition during early childhood on education among Guatemalan adults. *Economic Journal,* 119, 734-763. (Only randomized two villages to each condition; no baseline controls).

Mensch, B.S., Grant, M.J., Sebastian, M.P., Hewitt, P.C., & Huntington, D. (2004).

*The Effect of a Livelihoods Intervention in an Urban Slum in India: Do Vocational Counseling and Training Alter the Attitudes and Behavior of Adolescent Girls?* New York: Population Council. (No baseline control of enrollment or completion of primary school).

Mizala, A. & Urquiola, M. (2009). *School Markets: The Impact of Information Approximating School Effectiveness.* Documents de Trabajo. Serie Economia Centro de Economia Aplicada, Universidad de Chile. (This study was focused on the “market share” of private schools folloing the introduction of the SNED program).

Montgomery, H. (2005). *Meeting the Double Bottom Line –The Impact of Khushhali Bank’s Microfinance Program in Pakistan.* Unpublished Report. (Statistical controls with no control for baseline of enrollment or attendance).

O'Reilly, C., Freeman, C., Ravani, M., Migele, J., Mwaki, A., Ayalo, M., Ombeki, S., Hoekstra, R.M., & Quick, R. (2008). The impact of a school-based safe water and hygiene programme on knowledge and practices of students and their parents: Nyanza Province, western Kenya, 2006. *Epidemiology and Infection*, 136, 80-91. (No baseline control on absenteeism).

Powell, C., Grantham-McGregor, S.M., & Elston, M. (1983). An evaluation of giving the Jamaican government school meal to a class of children. *Human Nutrition: Clinical Nutrition,* 37C, 381-388. (Data collected from 1976-1977; does not meet 1990- data collection screen).

Pradhan, M. & Rawlings, L. (2002). The impact and targeting of social infrastructure investments: Lessons from the Nicaraguan Social Fund. *World Bank Economic Review*. 16(2), 275-295. (No baseline control on enrollment, attendance, repetition or progression).

Psacharopoulos, G., Rojas, C., & Velez, E. (1992). *Achievement Evaluation of Colombia's Escuela Nueva. Is Multigrade the Answer?* Policy Working Paper 896. Washington, DC: World Bank. (Data from 1987, does not meet eligibility criteria of data from 1990-).

Ravillion, M. & Wodon, W. (2000). Does child labour displace schooling? Evidence on behavioural responses to an enrollment subsidy. *Economic Journal,* 110, 158-175. (This is one of several reanalyses of the Bangladesh Food for Education program evaluation. The program evaluation is already represented in this review by the Meng and Ryan (2007) analysis which used both propensity score matching and difference-in-difference analyses).

Smuts, M. (2006). Effects of a fish flour-enriched spread on cognition and absenteeism in schoolchildren: a randomised controlled trial. Polysaturated Fatty Acids and Cognition in Children. Proceedings of the Unilever Congress Workshop, International Society for the Study of Fatty Acids and Lipids Congress; Cairns, Australia. Vlaardingen, The Netherlands: Unilever Food and Health Research Institute. (Did not report sufficient data to compute effect size).

Yap, Y., Sedlacek, G., & Orazem, P.F. (2002). Limiting Child Labor Through Behavior-Based Income Transfers: An Experimental Evaluation of the PETI Program in Rural Brazil. Unpublished Report. (No baseline collection of data on average time spent in school or progression [grade for age]).

Walingo, M. & Musamali, B. (2008). Nutrient intake and nutritional status indicators of participant and nonparticipant pupils of a parent-supported school lunch program in Kenya. Journal of Nutrition Education and Behavior, 40, 298-304. (No baseline collection of data on school attendance).

Walker, I., Ordonez, I. & Rodriguez, F. (1999). Ex-Post Evaluation of the Honduran Social Investment Fund. Washington, DC: World Bank; 1999. (No baseline control on enrollment or progression).

Zhang, H. (2009). Magnet Schools and Student Achievement: Evidence from a Randomized Natural Experiment in China. Unpublished paper. (this study tested an intervention to open up access to elite high schools for regular high school students, and examined admission to elite high schools and admission to regular high schools as outcomes).

## Coding instrument

**C2 Review: School Enrollment in Developing Nations**

**BRIEF VERSION OF CODING INSTRUMENT**

**Coder:**

- ❑ Claire Morgan
- ❑ Anthony Petrosino
- ❑ Trevor Fronius
- ❑ Bob Boruch
- ❑ Other _____

**I. Researcher and study characteristics**
**What year was the primary document published?**

_____

**What was the type of document?**

o Book

o Book Chapter

o Government Report

o Technical Report (reports by non-Govt. research firms, e.g. Mathematica)

o NGO Report (e.g., World Bank, Poverty Action Lab)

o Journal (peer reviewed)

o Dissertation

o Conference Paper

o Other

**In what country did the evaluation take place?**

_____

o Lower Income (LIC)

o Middle Income (MIC)

o Upper Middle Income (LMIC)

What was the setting for the evaluation?

_____

_____

_____

**Who conducted the evaluation?** (e.g., medical researchers, economists, etc. May be an assumption based on the affiliation)

Baseline enrollment data: Males_____ Females

(Use enrollment rates as close in proximity to intervention setting as possible, but if only national rates available, use those)

## II. Study methods and methodological quality

**What method of assignment was used to assign or form groups?**

- ❑ Random Assignment
- ❑ Non-Random Assignment
- ❑ Combination of Random Assignment and Non-Random Assignment (e.g., randomization only after oversubscription of available "spots")

**If non-random assignment, what procedure was used to assign or form groups?**

- ❑ Regression Discontinuity Design
- ❑ Statistical Matching
- ❑ Other (Indicate:_____)

**If statistical matching used, what procedure was used to match?**

- ❑ Propensity Scores
- ❑ Covariate matching
- ❑ Other (Indicate:_____)

**At what level was assignment made?**

- ❑ Village/Neighborhood
- ❑ School
- ❑ Classroom
- ❑ Household
- ❑ Individual
- ❑ Other (Indicate:_____)

# Methodological Threats to Evaluation Design

| **Threat** | **Did it exist?** | **How extensive? (Percentage of sample)** | **What did authors do to address?** | **Rate the Threat to Evaluation Findings about Enrollment (None/Low/Moderate/High)** |
|---|---|---|---|---|
| Crossovers | YES/NO | | | |
| Attrition from Original Study Sample | YES/NO | | | |
| Attrition of Students from Larger Aggregate Unit Assignment | YES/NO | | | |
| Differential Attrition | YES/NO | (Percentage difference between groups) | | |
| GROUP INEQUITY AT PRETEST | | | | |
| **Number of variables examined** | **Number of statistically significant differences** | **What did authors do to address?** | | **Rate the threat to evaluation findings about enrollment (None/Low/Moderate/High)** |
| | | | | |

## III. Intervention and control conditions

Describe the intervention group below, with particular attention to the "dosage" of the treatment:

*How many cases were randomized or assigned to this group?*

Program Implementation/Fidelity

| Program Implementation Issues Mentioned by Authors (Not Possible but Actual) | What did authors do to address? | Rating |
|---|---|---|
| | | |
| | | |
| | | |

Please provide simple program theory (or mechanisms for why the intervention should work):

What is the control or comparison condition?

o No Treatment Group

o Wait-List Control

o Treatment as Usual Group

o Placebo

o Lesser dose of the same treatment

o Entirely different treatment than what Experimental got

o Other_____

(Indicate)

**Describe the control or comparison condition (including "dosage" and where it came from if applicable):**

*How many cases were randomized or assigned to this group?*

**IV. Participants in the study**

**Type of school**

_____

**Age/school level/grade**

_____

**Percentage of participants that were female**

_____

**Poverty/SES** (indicate currency PIs using if providing income/wages)

_____

_____

***V. OUTCOMES (You can also XEROX OUTCOME DATA OR OTHERWISE INCLUDE IN CODING SHEET)***

**What was the overall conclusion or investigator-reported result (IRR) for enrollment?**

- ❑ Positive
- ❑ Null or no effect
- ❑ Negative effect
- ❑ Mixed-Can't discern

## Simply indicate the education and non-education outcomes and when reported (time interval)

| **Education/Learning Outcome** | **Outcome Measurement at What Time Intervals (only those in which data points are reported, e.g., 6 months, 12 months,** | **NON-EDUCATION OUTCOMES** | **Outcome Measurement at What Time Intervals (only those in which data points are reported, e.g., 6 months, 12 months, etc.)** |
|---|---|---|---|
| Enrollment | | | |
| Attendance | | | |
| Dropout | | | |
| Test Scores | | | |
| Grades | | | |
| Other (List each in a new row) | | | |

**Were subgroup effects for treatment reported?** (Yes/No)

***If so: List:***

**Was any cost-benefit or economic analysis reported? (Yes/No)**

**Indicate outcome of economic analysis:**

- Program Group is more efficient option
- Comparison/Control Group is more efficient option
- Program Group is more efficient than policy alternatives
- Policy Alternatives are more efficient than program group
- No clear distinction between the two groups

**Any other comments on the program or evaluation**

## Type of intervention and number of included studies

| *Type of Intervention* | N of Studies | Citations |
|---|---|---|
| Albendazole | 1 | Simeon, et al. 1995 |
| Asthma/Epilepsy Treatment | 1 | Tieffenberg, et al. 2000 |
| Community Participation and Empowerment | 1 | Banerjee, et al. 2008 |
| Community Schools | 1 | Burde & Linden 200 |
| Computers | 1 | Barrera-Osorio & Linden 2009 |
| Conditional Cash Transfer | 14 | Ahmed, et al. 2009; Attanasio, et al. 2004; Baird, et al. 2009;Barrera- Osorio, et al. 2008; Chandhury & Parajuli 2006; Glewwe & Olinto 2004; Heinrich 2005; Levy & Ohls 2007; Macours & Vakis (2008); Malucci & Flores 2004; Parker, et al. 2005, Pianto & Soares 2004; Schady & Araujo 2006; Todd & Wolpin 2003 |
| Conditional Food Program | 1 | Meng & Ryan 2007 |
| Deworming | 1 | Miguel & Kremer 2003 |
| Dropout Prevention Program | 1 | Tan, et al. 1999 |
| Early Intervention Program | 1 | Kagitcibasi, et al. 2001 |
| English Language Machines and Activities | 2 | He, et al. 2007 (2 studies in report) |
| Family Planning | 1 | Sinha, et al. 2009 |
| Flip Charts | 1 | Glewwe, et al. 2004 |
| Funding | 5 | Duflo, et al. 2007b; Gertler & Rubio-Cordina 2007; Newman, et al. 2002 (2 studies in this report); Skoufias & Shapiro 2006 |

| | | |
|---|---|---|
| Girls Fellowships | 6 | Filmer & Schady 2006; Filmer & Schady 2009; Kim, et al. 1998; Kim, et al. 1999; Kremer, et al. 2008 (2 studies in this report) |
| Improving School Management | 1 | Nguyen 2008 (2 studies in this report—see providing returns on information) |
| Livelihood Skills | 1 | Amin & Suran 2005 |

| ***Type of Intervention*** | **N of Studies** | **Citations** |
|---|---|---|
| Malaria Prevention | 2 | Fernando, et al. 2006; Simwaka, et al. 2009 |
| Menstrual Cups | 1 | Oster, et al. 2009 |
| Microfinance | 1 | Banerjee, et al. 2009 |
| New Schools | 1 | Levy, et al. 2009 |
| Preschool Nutritional Supplement | 1 | Walker, et al. 2005 |
| Providing returns on information | 2 | Jenson 2007; Nguyen 2008 |
| Providing report cards | 1 | Andrabi, et al. 2009 |
| Remedial Tutoring | 5 | Banerjee, et al. 2005 (5 studies in this report) |
| Road Improvement | 2 | Khandker 2009 (2 studies in this report) |
| School Meals | 5 | Jacoby, et al. 1996; Kazianga, et al. 2008; Powell, et al. 1998 (2 studies in this report); Vermeersch & Kremer 2004 |
| School Repair | 1 | Loshkin & Ymetsov 2004 |
| Teacher Incentives | 3 | Duflo, et al. 2007a; Glewwe, et al. 2003; Kremer & Chen 2003 |
| Teacher Training | 1 | Duflo, et al. 2006 |
| Textbooks | 1 | Glewwe, et al. 2009 |
| Uniforms | 1 | Evans, et al. 2009 |
| User Fee Elimination | 1 | Borkum 2009 |
| User Fee Reduction | 1 | Barrera-Osorio, et al. 2007 |
| Vitamin A | 2 | Hamazaki, et al. 2008; Mahawithanage, et al. 2007 |
| Voucher | 1 | Angrist, et al. 2002 |

## Publication bias statistics

### Egger's regression intercept

| | |
|---|---|
| Intercept | 1.89319 |
| Standard error | 0.60944 |
| 95% lower limit (2-tailed) | 0.67799 |
| 95% upper limit (2-tailed) | 3.10838 |
| t-value | 3.10642 |
| df | 71.00000 |
| P-value (1-tailed) | 0.00136 |
| P-value (2-tailed) | 0.00272 |

### Duval and Tweedie's trim and fill

| | | Fixed Effects | | | Random Effects | | | Q Value |
|---|---|---|---|---|---|---|---|---|
| | Studies Trimmed | Point Estimate | Lower Limit | Upper Limit | Point Estimate | Lower Limit | Upper Limit | |
| **Observed values** | | 0.06866 | 0.06167 | 0.07566 | 0.13282 | 0.10205 | 0.16359 | 966.14484 |
| **Adjusted values** | 24 | 0.04100 | 0.03427 | 0.04773 | 0.04886 | 0.01391 | 0.08381 | 1832.69983 |

## Supplement 1: List of included studies and details

| Study | Country | Specific Intervention (Broad Group) | Group Targeted | Sample Size (Assignment Level) | Study Design | Methodological Quality Score |
|---|---|---|---|---|---|---|
| Ahmed, et al. (2009) | Bangladesh | Household enterprise development (Economic) | Ultra poor households | 5,626 households (2,633 treatment; 2,993 control) | QED (PSM) | 0 |
| Amin & Suran (2005) | Bangladesh | Livelihood skills and self-esteem/ leadership training (Providing Information/Training) | Girls in 14 rural districts | 90 villages (75 treatment; 15 control) | QED (PSM) | 1 |
| Andrabi, et al. (2009) | Pakistan | Student and school report cards provided to parents (Providing Information/Training) | Grade 3 students in rural villages | 112 villages (56 treatment; 56 control) | RCT | 0 |
| Angrist, et al. (2002) | Colombia | Private school vouchers (Economic) | Students in grades 6-9 nationwide | 1,176 (593 treatment; 583 control) | RCT | 0 |
| Attanasio, et a.l (2004) | Colombia | Conditional subsidy (Economic) | Poorest rural families | 6,773 households (3,387 treatment; 3,386 control) | QED (PSM) | 0 |
| Baird, et al (2009) | Malawi | Conditional cash transfer (Economic) | Secondary-school girls in particularly poor district | 3805 girls (1,225 treatment; 2,580) | RCT | 0 |
| Banerjee (2005a) | India | Remedial tutoring (Educational Programs/Practices) | Grade 3 students in urban slums | 77 schools (38 treatment; 39 control) | RCT | 2 |
| Banerjee (2005b) | India | Remedial tutoring (Educational Programs/Practices) | Grade 3 students in urban slums | 122 schools (61 treatment; 61 control) | RCT | 1 |

| | | | | | | |
|---|---|---|---|---|---|---|
| Banerjee (2005c) | India | Remedial tutoring (Educational Programs/Practices) | Grade 4 students in urban slums | 97 schools (48 treatment; 49 control) | RCT | 0 |
| Banerjee (2005d) | India | Remedial tutoring (Educational Programs/Practices) | Grade 3-4 students in urban slums | 96 schools (48 treatment; 48 control) | RCT | 1 |
| Banerjee (2005g) | India | Remedial tutoring (Educational Programs/Practices) | Grade 3 students in urban slums | 77 schools (39 treatment; 38 control) | RCT | 0 |
| Banerjee (2008) | India | Community empowerment training (creating education report cards and organizing reading camps) (Providing Information/Training) | Communities | 150 villages (65 treatment; 85 control) | RCT | 0 |
| Banerjee (2009) | India | Microfinance (Economic) | Households in urban slums | 104 slums (52 treatment; 52 control) | RCT | 0 |
| Barrera-Osorio (2008) | Colombia | Conditional cash transfer (Economic) | Lowest-income grade 9-11 students | 2,544 students 1,140 treatment; 1,440 control) | RCT | 0 |
| Barrera-Osorio, et al. (2007) | Colombia | School fees reduction (Economic) | Lowest-income students | | QED (RDD) | 0 |
| Barrera-Osorio, et al. (2009) | Colombia | Provided computers and training for teachers (Educational Programs/Practices) | Grades 1-9 | 97 schools (48 treatment; 49 control) | RCT | 0 |
| Borkum (2009) | South Africa | Waiving school tuition fees (Economic) | Lowest-income communities | 12,287 students (7,290 treatment; 7,997 control) | QED (RDD) | 0 |
| Burde & Linden (2009) | Afghanistan | Community schools (New Schools/Infrastructure) | Rural villages | 31 villages (13 treatment; 18 control) | RCT | 0 |

| | | | | | | |
|---|---|---|---|---|---|---|
| Chandhury, et al (2006) | Pakistan | Conditional cash transfer (Economic) | Girls in low-literacy rural districts | 68 districts (30 treatment; 38 control) | QED (RDD) | 0 |
| Duflo, et al. (2006) | Kenya | HIV/AIDS education + free uniforms (Educational Programs/Practices) | Grade 6 students in two rural districts | 328 schools (240 treatment; 88 control) | RCT | 0 |
| Duflo, et al. (2007b) | Kenya | Teacher attendance monitoring + financial incentives (Educational Programs/Practices)0 | Non-formal education centers in tribal villages | 113 schools (57 treatment; 56 control) | RCT | 0 |
| Duflo, et al. (2009) | Kenya | Extra teacher + local school committee (Educational Programs/Practices) | Grade 1 | 121 schools (61 treatment; 60 control) | RCT | 0 |
| Evans, et al. (2009) | Kenya | Free uniforms (Economic) | Primary 1-4 | 1,305 students (612 treatment; 693 control) | RCT | 0 |
| Fernando, et al. (2006) | Sri Lanka | Malaria prevention (Health Care/Nutrition) | Primary grades 1-5 | 587 students (295 treatment; 292 control) | RCT | 1 |
| Filmer & Schady, (2006a) | Cambodia | Scholarships (Economic) | Poor lower secondary students | 3,225 households | QED (RDD) | 0 |
| Filmer & Schady, (2006b) | Cambodia | Scholarships (Economic) | Poor lower secondary girls | 3,623 girls (2,765 treatment; 858 control) | QED (PSM) | 0 |
| Gertler (2007) | Mexico | Funds for parent associations (Educational Programs/Practices) | Highly disadvantaged rural communities | 6,038 schools (2,580 treatment; 3,458 control) | Cohort design with statistical controls | 0 |

| | | | | | | |
|---|---|---|---|---|---|---|
| Glewwe, et al (1998) | Kenya | Textbook provision (Educational Programs/Practices) | Primary schools | 100 schools (25 treatment; 75 control) | RCT | 1 |
| Glewwe, et al. (2003) | Kenya | Teacher incentives for student achievement (Educational Programs/Practices) | Rural primary schools | 100 schools (50 treatment; 50 control) | RCT | 0 |
| Glewwe, et al. (2004a) | Kenya | Flipcharts (Educational Programs/Practices) | Rural primary schools | 178 schools (89 treatment; 89 control) | RCT | 0 |
| Glewwe, et al. (2004b) | Honduras | Conditional cash transfer (Economic) | Poor primary schools | 70 villages (50 treatment; 20 control) | RCT | 1 |
| Hamazaki, et al. (2008) | Indonesia | Fish oil supplement (Health Care/Nutrition) | Primary school | 233 students (116 treatment; 117 control) | RCT | 0 |
| He, et al. (2007a) | India | PicTalk machine for learning English (Educational Programs/Practices) | Primary schools | 97 schools (49 treatment; 48 control) | RCT | 0 |
| He, at al. (2007b) | India | PicTalk machine for learning English (Educational Programs/Practices) | Primary schools | 121 schools (60 treatment; 61 control) | RCT | 0 |
| Heinrich (2005) | Argentina | Conditional cash transfer (Economic) | Students entering grades 8 and 9 | 3,490 students (2,497 treatment; 993 control) | QED (PSM) | 2 |
| Jacoby, et al (1996) | Peru | School breakfast (Health Care/Nutrition) | Poor rural provinces | 10 schools (5 treatment; 5 control) | RCT | 0 |
| Jensen (2007) | Dominican Republic | Provision of information about returns to education (Providing Information/ Training) | Grade 8 students | 150 schools | RCT | 0 |

| | | | | | | |
|---|---|---|---|---|---|---|
| Kagitcibasi, et al. (2001) | Turkey | Early intervention program for mothers and children (Providing Information/ Training) | Poor households | 255 mothers (90 treatment; 165 control) | RCT | 0 |
| Kazianga, et al. (2008) | Burkina Faso | School feeding (Health Care/Nutrition) | Very poor primary and secondary schools | 46 schools | RCT | 0 |
| Khandker (2009a) | Bangladesh | Improving rural roads (New Schools/Infrastructure) | Poor rural villages | 38 villages (26 treatment; 12 control) | QED (DID) | 0 |
| Khandker (2009b) | Bangladesh | Improving rural roads (New Schools/Infrastructure) | Poor rural villages | 105 villages (12 treatment; 93 control) | QED (DID) | 0 |
| Kim, et al (1998) | Pakistan | Private school established + fellowships for girls (Economic) | Underserved neighborhoods in the | 30 neighborhoods (10 treatment; 20 control) | RCT | 0 |
| Kim, et al (1999) | Pakistan | Girls' fellowship program (Economic) | Primary schools in region with very low female enrollment | 36 villages (30 treatment; 6 control) | RCT | 0 |
| Kremer & Chen (2001) | Kenya | Early childhood education program (Educational Programs/Practices) | Very poor rural areas | 100 schools (50 treatment; 50 control) | RCT | 1 |
| Kremer, et al (2008a) | Kenya | Girls' merit scholarship (Economic) | Rural primary schools | 69 schools (34 treatment; 35 control) | RCT | 3 |
| Kremer, et al (2008b) | Kenya | Girls' merit scholarship (Economic) | Rural primary schools | 58 schools (30 treatment; 28 control) | RCT | 0 |
| Levy & Ohls (2007) | Jamaica | Conditional cash transfer (Economic) | Poor households nationwide | 5,000 households (2,500 treatment; 2,500 control) | QED (RDD) | 0 |
| Levy, et al (2009) | Burkina Faso | Girl-friendly schools (New Schools/Infrastructure) | Rural villages with lowest female enrollment rates | 293 villages (132 treatment; 161 control) | QED (RDD) | 0 |
| Lokshin & Yemtshov (2004) | Georgia | School infrastructure rehabilitation (New Schools/Infrastructure) | Nationwide | 106 villages (61 treatment; 45 control) | QED (PSM & DID) | 1 |

| | | | | | | |
|---|---|---|---|---|---|---|
| Macours & Vakis (2008) | Nicaragua | Conditional cash transfer (Economic) | Extremely poor municipalities in drought region | 2,000 households (1,000 treatment; 1,000 control) | RCT | 0 |
| Mahawithanage, et al (2007) | Sri Lanka | Vitamin A supplement (Health Care/Nutrition) | Two rural primary schools | 659 students (322 treatment; 337 control) | RCT | 0 |
| Maluccio & Flores (2004) | Nicaragua | Conditional cash transfer (Economic) | Poor primary students | 42 villages (21 treatment; 21 control) | RCT | 0 |
| Meng & Ryan (2007) | Bangladesh | Food for education (Economic) | Very poor primary students | 888 households (209 treatment; 679 control) | QED (PSM) | 1 |
| Miguel & Kremer | Kenya | De-worming (Health Care/Nutrition) | Poor rural primary schools | 75 schools (25 treatment; 50 control) | RCT | 0 |
| Newman, et al (2002a) | Bolivia | School infrastructure funding (New Schools/Infrastructure) | Poor rural area | 72 schools (35 treatment; 37 control) | RCT | 0 |
| Newman, et al (2002b) | Bolivia | School infrastructure funding (New Schools/Infrastructure) | Poor rural area | 70 schools (37 treatment; 33 control) | QED (PSM) | 0 |
| Nguyen (2008a) | Madagascar | Provision of information on returns to education (Providing Information/Training) | Rural primary schools | 160 schools (80 treatment; 80 control) | RCT | 0 |
| Nguyen (2008b) | Madagascar | School capacity-building (Educational Programs/Practices) | Rural primary schools | 606 schools (303 treatment; 303 control) | RCT | 0 |
| Oster, et al (2009) | Nepal | Menstrual cups (Health Care/Nutrition) | Girls in grades 7-8 | 198 girls (99 treatment; 99 control) | RCT | 0 |
| Parker, et al (2005) | Mexico | Conditional cash transfer (Economic) | Urban households | 3,885 households (1,779 treatment; 2,106 control) | QED (PSM) | 0 |

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| Pianto & Soares | Brazil | Conditional cash transfer (Economic) | Nationwide | 76 municipalities (38 treatment; 38 control) | QED (PSM) | 0 |
| Powell, et al (1998a) | Jamaica | School breakfast (Health Care/Nutrition) | Rural primary schools (under- nourished) | 407 students (203 treatment; 204 control) | RCT | 0 |
| Powell, et al (1998b) | Jamaica | School breakfast (Health Care/Nutrition) | Rural primary schools (adequately nourished) | 405 students (200 treatment; 205 control) | RCT | 0 |
| Schady & Araujo (2006) | Ecuador | Conditional cash transfer (Economic) | Poor primary students | 1,391 households | RCT | 1 |
| Simeon, et al (1995) | Jamaica | De-worming (Health Care/Nutrition) | Primary school students | 407 students (206 treatment; 201 control) | RCT | 0 |
| Simwaka, et al (2009) | Malawi | Malaria treatment (Health Care/Nutrition) | Primary school students | 237 schools (96 treatment; 141 control) | QED (PSM) | 0 |
| Sinha, et al | Bangladesh | Family planning | Low-income villages | 139 villages | RCT | 1 |
| (2005) | | | | (70 treatment; 69 control) | | |
| | | (Providing Information/Training) | | | | |
| Skoufias & Shapiro (2006) | Mexico | School improvement grants (Educational Programs/Practices) | Primary schools nationwide | 67,234 schools (1,767 treatment; 65,467 comparison) | QED (PSM) | 0 |
| Tan, et al (1999) | Philippines | Dropout prevention (Educational Programs/Practices) | Poor primary schools | 1,182 students (629 treatment; 553 control) | RCT | 2 |
| Tieffenberg, et al (2000) | Argentina | Asthma and epilepsy training (Health Care/Nutrition) | Low SES children | 355 children (230 treatment; 125 control) | RCT | 1 |
| Todd & Wolpin | Mexico | Conditional cash transfer (Economic) | Poor rural households | 18, 796 households | RCT | 0 |
| Vermeersch & Kremer | Kenya | School meals (Health Care/Nutrition) | Poor preschool and grades 1-2 students | 50 schools (25 treatment; 25 control) | RCT | 1 |

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| Walker, et al (2005) | Jamaica | Nutritional supplementation and psychological stimulation (Health Care/Nutrition) | Children age 9-24 months | 54 students (27 treatment; 27 control) | RCT | 0 |

## Supplement 2: Theory of Change for Broad intervention groups

| BROAD INTERVENTION TYPE | UNDERLYING BARRIER TO SCHOOL PARTICIPATION | TYPE OF INTERVENTIONS | KEY MECHANISM | PRIMARY OUTCOMES |
|---|---|---|---|---|
| **ECONOMIC (FINANCIAL BENEFITS/INCENTIVES PROVIDED TO HOUSEHOLDS)** | COSTS OF GOING TO SCHOOL; CHILD NEEDED AT HOME OR TO WORK TO SUPPLEMENT INCOME; FINANCIAL BENEFITS OF EDUCATION NOT RECOGNIZED | CONDITIONAL AND UNCONDITIONAL CASH AND/OR FOOD TRANSFERS; FEE REDUCTION OR ELIMINATION; VOUCHERS; PROVIDING UNIFORMS; MICROFINANCE LOANS; FELLOWSHIPS AND SCHOLARSHIPS | REMOVAL OF FISCAL BARRIER TO STUDENT PARTICIPATION; ECONOMIC BENEFIT IS INCENTIVE TO SEND YOUTH TO SCHOOL; ECONOMIC BENEFIT OUTWEIGHS YOUTH INCOME BY WORKING | INCREASES IN ENROLLMENT, ATTENDANCE, AND PROGRESSION; DECREASE IN SCHOOL DROPOUT |
| **EDUCATIONAL PROGRAMS/PRACTICES** | STUDENT DEFICIENCIES OR POOR SCHOOL QUALITY INHIBHT STUDENT ENGAGEMENT AND LEARNING | TEACHER INCENTIVES; TEACHER TRAINING; TEXTBOOKS, FLIPCHARTS; IMPROVING SCHOOL MANAGEMENT; SCHOOL FUNDING; DROPOUT PREVENTION PROGRAM; ENGLISH LANGUAGE TEACHING TECHNOLOGY; COMPUTERS; REMEDIAL TUTORING | IMPROVING SCHOOL QUALITY AND/OR ADDRESSING STUDENT DEFICIENCIES WILL LEAD TO GREATER STUDENT ENGAGEMENT; PARENTS WILL SEE MORE BENEFIT TO SENDING YOUTHS TO SCHOOL. | INCREASES IN ATTENDANCE, PROGRESSION, AND DECREASED DROPOUT. |

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| **HEALTH CARE/NUTRITION** | ILLNESS OR NUTRITIONAL DEFICIENCY KEEPS YOUTH OUT OF SCHOOL | VITAMIN A; MALARIA PREVENTION/TREATMENT; PRESCHOOL NUTRITIONAL SUPPLEMENT; SCHOOL MEALS; MENSTRUAL CUPS; DEWORMING; ASTHMA/EPILEPSY TREATMENT | REDUCING ILLNESS AND IMPROVING NUTRITION MEANS YOUTH ARE HEALTHY ENOUGH TO COME TO SCHOOL; PARENTS AND YOUTH HAVE INCENTIVE FOR YOUTH TO GO TO SCHOOL TO RECEIVE MEAL (SCHOOL BREAKFAST/LUNCH) | IMPROVED ATTENDANCE AND PROGRESSION; REDUCED DROPOUT |
| **NEW SCHOOLS/INFRASTRUCTURE** | SCHOOLS ARE TOO FAR AWAY OR IN DISREPAIR, DISCOURAGING YOUTH PARTICIPATION; PARENTS AFRAID TO SEND YOUTH TO SCHOOL FOR SAFETY REASONS; INABILITY TO ACCESS MARKETS PROVIDES ECONOMIC BARRIER TO HOUSEHOLD WEALTH | BUILDING NEW SCHOOLS IN COMMUNITIES; PROVIDING INFRASTRUCTURE FUNDING FOR NEW ROADS OR SCHOOL REPAIRS | SCHOOLS CLOSE ENOUGH FOR STUDENTS TO ATTEND EASILY; NEW ROADS IMPROVE PARENT ACCESS TO MARKETS AND ABILITY TO GET WORK AND OVERCOME FISCAL BARRIERS (SEE ECONOMIC ABOVE); SCHOOL CONDITIONS IMPROVE AND PARENTS/ MORE INCLINED TO SEND YOUTH | INCREASED ENROLLMENT, ATTENDANCE AND PROGRESSION ; DECREASED DROPOUT |

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|---|---|---|---|---|
| **PROVIDING INFORMATION/ TRAINING** | PARENTS/YOUTH UNAWARE OF BENEFITS OF YOUTH ATTENDING SCHOOL; PARENTS UNAWARE OF SCHOOL PERFORMANCE; PREGNANCY INHIBITS FEMALE COMPLETION OF SCHOOL; COMMUNITIES DISORGANIZED AND UNABLE TO HOLD SCHOOLS ACCOUNTABLE FOR EDUCATION OF YOUTH | PROVIDING LIVELIHOOD SKILLS; FAMILY PLANNING; PARENT TRAINING; PROVIDING INFORMATION ON PERCEIVED BENEFITS; PROVIDING REPORT CARDS; ORGANIZING AND EMPOWERING COMMUNITIES | FEMALES WHO ARE EMPOWERED AND NOT PREGNANT WILL BE MORE LIKELY TO PURSUE EDUCATION AND EMPLOYMENT; PARENTS AND YOUTH THAT UNDERSTAND BENEFITS TO EDUCATION MAY BE MORE LIKELY TO ENSURE SCHOOL PARTICIPATION; COMMUNITIES THAT WORK TOGETHER WELL CAN HOLD SCHOOLS ACCOUNTABLE; PARENTS AND SCHOOLS THAT RECEIVE INFORMATION ON SCHOOL PERFORMANCE WILL BE BETTER ABLE TO PUSH FOR QUALITY IMPROVEMENTS | INCREASED ENROLLMENT, ATTENDANCE, PROGRESSION AND DECREASED DROPOUT |

## Supplement 3: Effect Size Data Computed from Included Studies

| Study ID (Cite) | Education Outcome | Follow-up Interval | Type Data [ES Method Used] | Treatment Analysis N | Control Analysis N | Effect Size d | Variance | Notes |
|---|---|---|---|---|---|---|---|---|
| **2010001 (Hamazaki et al 2008)** | Absenteeism | 3 Months | Binary proportions [LOGIT] | 27/116 | 49/117 | .d= 4882 | v=0.025461 | |
| **2010002 (Mahawithanage et al. 2007)** | Health-Related Absenteeism | 13 Months | Means/Standard Deviations (Had to average 4 health conditions) [Mean of 4 Cohen's d's] | 297 | 316 | .148+.165+.065+0/4=.09/Cohen's d for means and SDs<br><u>Cough and Cold</u> Treatment 5,8 (SD 5.6); Control 6.7 (SD 6.5) d=.148 v=0.0065<br><u>Headache</u><br>Treatment 0.2 (SD 0.5); Control<br>0.3 (SD 0.7) d=.164 v=0.0066<br><u>Stomach Ache</u> Treatment 0.7 (SD 1.5); Control 0.8 (SD 1.6) d=.064 v=0.0065<br><u>Other Illness</u><br>Treatment 1.6 (SD 3.3); Control<br>1.6 (SD 3.8) d=0 v=0.0065 | .0065+.0066+.0065+.0065/4=.0065 | Had to average 4 health condition effect sizes and variance. |
| **2010100 (Walker et al. 2005)** | Dropout | 13-16 Years | Binary Proportions [Logit] | 5/27 | 8/27 | d= .3323 | v=0.12676 | |

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|---|---|---|---|---|---|---|---|---|
| | Test Scores, Math | 13-16 Years | Means/SDs [Cohen's d] | 27 | 27 | d=.1943 | v= 0.0746 | |
| | Test Scores, Reading | 13-16 Years | Means/SDs [Cohen's d] | 27 | 27 | Mean d=.65555 Sentence d=0.5695<br><br>Context d= .7426 | Sentence v=0.0771<br>Context v= .0792 | Averaged sentence completion and context comprehension into one Reading score. |
| **2010034 (Powell et al. 1998a)** | Attendance | 9-10 Months | Means/SDs [mean gain scores divided by the pooled pretest standard deviation] | 203 | 204 | d=.1684 | v=.0099 | Note that there is a multilevel analysis that presents coefficient and SE for treatment but it lumps both RCTs (nourished and undernourished groups) together. |
| | Math | 9-10 Months | Mean/SDs [mean gain scores divided by the pooled pretest standard deviation] | 203 | 204 | d=-.0434 | v=0.0098 | See above. |
| | Reading | 9-10 Months | Means/SDs [mean gain scores divided by the pooled pretest standard deviation] | 203 | 204 | d=0 | v=.0098 | See above. |

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| **2010035 (Powell et al. 1998b)** | Attendance | 9-10 Months | Means/SDs [mean gain scores divided by the pooled pretest standard deviation] | 201 | 205 | d=.1553 | v=.0099 | See above for 2010034. |
| | Math | 9-10 Months | Means/SDs [mean gain scores divided by the pooled pretest standard deviation] | 201 | 205 | d=.0458 | v=.0099 | See above. |
| | Reading | 9-10 Months | Means/SDs [mean gain scores divided by the pooled pretest standard deviation] | 201 | 205 | d=-.0146 | v=.0099 | See above. |
| **2010025 (Simeon et al 1995)** | Attendance | 6 Months | Means/SDs [mean gain scores divided by the pooled pretest standard deviation] | 206 | 201 | d=.085 | v=.0098 | |

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| | Math | 6 Months | Means/SDs [mean gain scores divided by the pooled pretest standard deviation] | 206 | 201 | d=.0441 | v=.0098 |
| | Reading | 6 Months | Means/SDs [mean gain scores divided by the pooled pretest standard deviation] | 206 | 201 | d=.0231 | v=.0098 |

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| **2010030 (Tieffenberg et al. 2000)** | School Absenteeism | 6 Months | p-value | 38<br><br>41 | 28<br><br>39 | d=.8979 (asthma) d=.5823 average d=.7401 | v=0.0681 v=0.0521 average v=.0601 | PIs only report pre and post mean number of absences per 100 days, and a p-value. They use Mann-Whitney- Wilcoxon test for significance.<br><br>Note: Results are reported for asthma and epilepsy patients separately; we average results at 6 months.<br><br>The Mann- Whitney-Wilcoxon test is a non-parametric version of the t- test, and it gives p-values that are roughly comparable to the t-test.<br>Treating it as a p-value from a t will give an effect size that has the same p- value as this test and is reasonable but not exactly correct. |
| | School Absenteeis | 12 months | p-value | 53 | 45 | d=.4492 | v=0.0421 | See above note. |

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| **2010040 (Fernando et al. 2006)** | Absenteeism due to Malaria | 9 Months | Means/SDs [Cohen's d] | 295 | 292 | d=.5931 | v=.0071 | |
| | Math | 9 Months | Means/SDs [Cohen's d] | 295 | 292 | d=.6156 | v=.0071 | |
| | Language | 9 Months | Means/SDs [Cohen's d] | 295 | 292 | d=.5561 | v=.0071 | |
| **2010014 (Jacoby et al. 1996)** | Attendance | 6 Weeks (1.2 Months) | Compute ES from binary proportion at post test and subtract d from binary proportion at posttest [Using Logit method] | 233 | 169 | d=.1915 | v=.0337 | This is using individual level N's. Averaged variance across pretest and posttest.<br><br>Using cluster N's of 5 in each group produce identical d but a variance of 1.333. |
| | Math | 1.2 Months | See (2) below | 233 | 169 | d=-.1900 | v=.0388 | |
| | Reading | 1.2 Months | See (2) below | 233 | 169 | d=.1496 | v=.0238 | |
| **2010037 (Amin and Suran, 2005)** | Schooling retention rate | 12-24 Months | Proportions [Logit] | 276 | 265 | d=-.1671 | v=.0095 | Village level, but does not appear to take clustering into effect. If we use groups (75/15 villages), d is same but v=.1050. |

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| **2010061 (Ahmed et al. 2004)** | Net primary school enrolment of children 6-11 | 36 Months | Compute ES from binary proportion at post test and subtract d from binary proportion at posttest [Using Logit method] | 2633 | 2993 | d=.0491 | V=.0009 | Similar to ES (.03) for computing using t-test for difference-in-differences. Using difference in proportions indicates ES of .10. Treat household as individual level. |
| **2010096 (Banerjee et al. 2009)** | All children in school | 15-18 Months | Simple regression coefficient formula | 2720 | 2719 | d=-.0211 | v=.0007 | By using treatment S/E and coefficient to compute d, this should take clustering at the larger level into account. Using larger aggregate N's produce d ES of -.15 and v=.0394. Assume near equal number of participants in each group. These effects are similar to those computed from simple means and standard deviations using individual N's. |

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| | Girls in school | 15-18 Months | Simple regression coefficient formula | 2029 | 2029 | d=-.0385 | v=.0009 | Assume equal number of participants in the groups.<br><br>Using aggregate N's produces d=-.2410 and v=.0407. |
| **2010114 (Sinha et al. 2009)** | Boys' Enrollment | 18 YEARS | [Probit] | 635 | 700 | d=-.011 | v=.0011 | For variance, use d/z standard error. |
| | Girls' | 18 | [Probit] | 592 | 573 | d=-.012 | v=.0005 | See above. |
| | Enrollment | YEARS | | | | | | |
| **2010027 (Filmer & Schady 2006)** | Enrolled at JFPR School | 12 Months | [Simple regression method]. We are assuming that other variables are not included in these models. This seems to be the case for the final column. | 2360 | 705 | d=.3169 | v=.0019 | 2008 *Economic & Cultural Change* article indicates that 6 methods for estimating program impact are very similar. We selected nearest neighbor propensity score matching with trimmed sample (7.7-92.3 percentile of PS score) for ES from the 2008 paper.<br>One issue: if we were to compute basic differences in binary proportions, the d would be much larger. |

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| | Attending JFPR School on day of visit | 12 Months | Simple regression method | 2360 | 705 | d=.2986 | v=.0018 | See above. |
| **2010104 (Khandker, 2009) RDP** | School Enrollment | 12 Months | Binary Proportions [Logit] | 989 | 123 | d=.6725 | v=.1660 | Assume split in sample size similar to split in households (89% in treatment). |
| | School Enrollment | 12 Months | Binary Proportions [Logit] | 991 | 124 | d=.4936 | v=.1569 | See above. |
| **2010105 (Khandker, 2009) RRMIMP** | School Enrollment | 12 Months | Binary Proportions [Logit] | 680 | 292 | d=.4176 | v=.1481 | Assume split in sample size similar to split in households (70% in treatment). |
| | School Enrollment | 12 Months | Binary Proportions [Logit] | 670 | 287 | d=.4512 | v=.1084 | See above. |
| **2010103 (Kremer & Chen 2003)** | Grade progression to Grade 1 | 36 Months | Binary proportions [Probit] | 2610 | 2609 | d=.12 | v=.0121 | Assume 50/50 split in sample size. Use $S/E^2$ of the probit as variance. |
| | Oral Test | 12 Months | Simple Regression (coefficient/SE) | 901 | 900 | d=-.0330 | v=.0022 | Assume 50/50 split. |
| | Written Test | 12 Months | Simple Regression | 901 | 900 | d=-.0300 | v=.0022 | See above. |
| | | | (coefficient/SE) | | | | | |
| | Oral Test | 24 Months | Simple Regression (coefficient/SE) | 733 | 732 | d=-.0664 | v=.0026 | See above. |

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| | Written Test | 24 Months | Simple Regression (coefficient/SE) | 732 | 732 | d=-.0871 | v=.0288 | See above. |
| | Oral Test | 36 Months | Simple Regression (coefficient/SE) | 1032 | 1031 | d=-.0556 | v=.0019 | See above. [Note that S/E for coefficient is .010, but it is likely a typo given the text (we used .110.).] |
| | Written Test | 36 Months | Simple Regression (coefficient/SE) | 1095 | 1094 | d=-.0301 | v=.0018 | See above. |
| **2010055 (Tan et al. 1999)** | Dropout | 12 Months | Pretest and Posttest binary proportions (Logit); subtract pretest from posttest | 1356 | 1279 | d=.4627 | v=.0147 | Selected the strongest contrast (intervention with all treatments). Use posttest N's. Effects for individual level N's. If School Ns of 5 treatment and 10 control used, sum of v at pretest and posttest is 3.169. |
| | Student Achievement | 12 Months | Use formula for Z-scores of a probability associated with baseline to posttest change in test scores. | 1356 | 1279 | d=.3600 | V=.2848 | |

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| **2010054 (Malucci o & Flores, 2004)** | School Enrollment, Grades 1-4 | 13 Months | Binary Proportions [Logit]; subtract pretest from Posttest | 896 | 869 | d=.8062 | v=.0101 | Use sum of variance. (pretest and posttest). Sum of variance using village N's of 21/21 is .4391. |
| | | 24 Months | Binary Proportions [Logit]; subtract pretest from Posttest | 806 | 872 | d=.6528 | v=.0107 | See above. Same variance as above if village N's are used. |
| | Matriculation for 7-13 Year Olds who have not completed 4th grade | 13 Months | Binary Proportions [Logit]; subtract pretest from Posttest | 880 | 852 | d=.8181 | v=.0118 | See above. Sum of variance changes to .3587 if village N's of 21/21 are used. |
| **2010048 (Attanasio et al. 2004)** | Enrollment, Rural youth, ages 8-13 | 24 Months | Probit | 3387 | 3386 | d=.0270 | v=.0001 | Used household N's for all analyses. PIs also reported analyses of treatment versus control areas. |
| | Enrollment, Rural Youth 14-17 | 24 Months | Probit | 3387 | 3386 | d=.0711 | v=.0005 | See above. |
| | Enrollment, Urban Youth 8-13 | 24 Months | Probit | 3387 | 3386 | d=.0138 | v=.00002 | See above. |

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| | Enrollment, Urban Youth 14-17 | 24 Months | Probit | 3387 | 3386 | d=.0478 | v=.0001 | See above. |
| **2010021 (Evans et al 2009)** | Attendance | 41 Months | Binary proportions [Logit, subtracting pretest d from post-test d] | 612 | 693 | d=.0990 | v=.0009 | Used ITT estimates and not IV estimates, which were much larger. Also note that PIs did not provide means and SDs for dependent variable for test scores in ITT analysis, so they are not included here. Variance is computed by dividing the coefficient by SE to produce t; divide d by t to produce S/E of the d estimate; square it to produce v. |
| **2010043 (Burde and Linden, 2009)** | Formal School Enrollment, Fall 2007 | 6 Months | PIs reported ES | 414 | 391 | d=.467 | v=.0007 | Use N of households with children. Also used differences rather than differences with controls. |
| | Formal School Enrollment, Winter 2008 | 8 Months | PIs reported ES | 399 | 395 | d=.410 | v=.0006 | See above. |
| | Math | 6 Months | PIs reported ES | 721 | 653 | d=.620 | v=.0033 | See above. |

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| | Math | 8 Months | PIs reported ES | 721 | 653 | d=.667 | v=.0033 | See above. |
| | Dari | 6 Months | PIs reported ES | 722 | 679 | d=.418 | v=.0036 | See above. |
| | Dari | 8 Months | PIs reported ES | 722 | 679 | d=.456 | v=.0037 | See above. |
| **2010019 (Kim et al. 1999)** | Enrollment (using cross sectional data). Model 1 | 12-24 Months | Probit | 1332 | 827 | d=.114 | v=.0005 | Use proportionate N's of students based on proportions of villages and schools assigned to treatment and control. Longitudinal data are essentially pre- post with no comparison group. |
| | Enrollment (using cross sectional data). Model 2 | 12-24 Months | Probit | 1332 | 827 | d=.151 | v=.0005 | See above |
| | Enrollment (using cross sectional data). Model 3. | 12-24 Months | Probit | 904 | 561 | d=.168 | v=.0007 | See above |
| | Enrollment (using cross sectional data). Model 1 | 12-24 Months | Probit | 1182 | 734 | d=-.120 | v=.0005 | See above. |

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| | Enrollment (using cross sectional data). Model 2. | 12-24 Months | Probit | 1182 | 734 | d=-.101 | v=.0005 | See above. |
| | Enrollment (using cross sectional data). Model 3. | 12-24 Months | Probit | 777 | 483 | d=-.087 | v=.0008 | See above. |
| **2010022 (Glewwe et al. 2009)** | Stayed or promoted, Grades 3-7 | 7-9 Months | Binary Proportions [Logit] | 5009 | 4838 | d=0 | v=.0004 | Used proportionate number of students (25% in treatment; 75% in control) based on proportion of schools in groups. |
| | Finished primary, no secondary, Grade 8 | 7-9 Months | Binary Proportions [Logit] | 447 | 440 | d=-.2149 | v=.0059 | See above. |
| | Entered secondary school, Grade 8 | 7-9 Months | Binary Proportions [Logit] | 447 | 440 | d=.1145 | v=.0057 | See above. |
| | Stayed, repeated, grades 3-7 | 7-9 Months | Binary Proportions [Logit] | 5009 | 4838 | d=0 | v=.0007 | See above. |
| | Stayed, repeated, grade 8 | 7-9 Months | Binary Proportions [Logit] | 447 | 440 | d=.0866 | v=.0107 | See above. |

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| | Dropped out, grades 3-7 | 7-9 Months | Binary Proportions [Logit] | 5009 | 4838 | d=0 | v=.0008 | See above. |
| | Dropped out, grade 8 | 7-9 Months | Binary Proportions [Logit] | 447 | 440 | d=.6169 | v=.0924 | See above. |
| | Transferred out, Grades 3-7 | 7-9 Months | Binary Proportions [Logit] | 5009 | 4838 | d=.0710 | v=.0015 | See above. |
| | Transferred out, Grade 8. | 7-9 Months | Binary Proportions [Logit] | 447 | 440 | d=.2352 | v=.0300 | See above. |
| | Normalized test scores | 12 months | Simple regression method | 5661 | 5660 | d=.0180 | v=.0028 | Assume 50/50 split in sample. Treat as ES, and use SE squared as variance. |
| | Normalized test scores | 24 Months | Simple regression method | 3677 | 3677 | d=-.0460 | v=.0050 | See above. |

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| **2010005 (Schady & Araujo, 2006)** | Child enrolled in school in follow-up survey | 18 Months | Binary proportions [Logit] | 1501 | 1500 | d=.0972 | v=.0017 | Assumed 50/50 split in sample size.<br>PIs reported three estimation models, each with additional controls. Effects were similar across all three.<br>We computed estimates from "extended ontrols" model which had large number of covariates and included canton fixed effects.<br>Standardized variance by dividing coefficient by SE to produce t; divide d by t to produce SE of that d and squaring to produce variance.<br>Variance computed by square of S/E of treatment coefficient. |
| | Child dropped out of school between baseline and follow-up | 18 Months | Binary proportions [Logit] | 1105 | 1104 | d=.1387 | v=.0045 | See above. |

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| | Child enrolled in school between baseline and follow-up | 18 Months | Binary proportions [Logit] | 333 | 333 | d=.1357 | v=.0116 | See above. |
| **2010033 (Barrera-Osorio & Linden, 2009)** | Attended school last year | 24 Months | Binary proportions [Logit] | 4327 | 3889 | d=.2352 | v=.0002 | PI analyzed by individual. Variance computed by squaring the S/E of the difference between treatment and control groups. |
| | Did not attend school last week | 24 months | Binary proportions [Logit] | 4327 | 3889 | d=.3241 | v=.0049 | See above. |
| | Mean days not attending | 24 Months | Means/SDs [Cohen's d] | 4327 | 3889 | d=.0256 | v=.0571 | See above. |
| | Mean grade report | 24 Months | Means/SDs [Cohen's d] | 4327 | 3889 | d=-.0262 | v=.0036 | See above. |
| | Failing grade | 24 Months | Binary proportions [Logit] | 4327 | 3889 | d=-.0735 | v=.00001 | See above. |
| | Percentage of correct test items in MATH | 24 Months | Means/SDs [Cohen's d] | 2757 | 2744 | d=.0102 | v=.0004 | See above. |
| | Percent of correct test items in SPANISH | 24 months | Means/SDs [Cohen's d] | 2757 | 2744 | d=.0254 | v=.0003 | See above. |

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| **2010107 (Glewwe et al. 2004)** | Test absenteeism (combined and averaged data from six different tests) | 24 months | Binary Proportions [Logit] | 19650 | 19649 | d=.1261 | v=.0008 | Assume 50/50 split in sample size to match 75 schools in each group. We took the average d and v across multiple test administrations. Also reporting an average sample size as they changed per administration. |
| | Math test (Normalized scores) | 24 Months | Mean ES reported | 10221 | 10220 | d=-.0212 | v=.0236 | See above. Took scores with past performance controls included. $v=SE^2$ |
| | English test (Normalized scores) | 24 Months | Mean ES reported | 10217 | 10216 | d=-.0100 | v=.1152 | See above. |
| | KiSwahili test (Normalized scores) | 24 Months | Mean ES reported | 10224 | 10224 | d=-.0146 | v=.0054 | See above. |
| **2010016 (Kremer et al 2008) TESO** | Average student school participation | 24 Months | Binary Proportion [Logit] | 681 | 629 | d=-.1378 | v=.0008 | Used sample size proportionate to percentage of schools assigned to each group (52% schools assigned to treatment). Only restricted and longitudinal samples combined reported. |

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| | Average student school participation | 24 Months | Binary proportion [Logit] | 799 | 733 | d=-.1203 | v=.0058 | See above. |
| | Normalized test scores (all subjects) | 24 Months | Mean ES reported. | 778 | 718 | d=.0900 | v=.0196 | Used ITT sample only. See above on sample size. $v=SE^2$ |
| | Normalized test scores (all subjects) | 24 Months | Mean ES reported. | 864 | 863 | d=.0400 | v=.0196 | Used ITT sample only. See above on sample size. $v=SE^2$ |
| **2010016 (Kremer et al 2008) BUSIA** | Average student school participation | 24 Months | Binary Proportion [Logit] | 1057 | 976 | d=.1757 | v=.0060 | Used sample size proportionate to percentage of schools assigned to each group (52% schools assigned to treatment). Only restricted and longitudinal samples combined reported. |
| | Average student school participation | 24 Months | Binary proportion [Logit] | 1155 | 1066 | d=.0264 | v=.0043 | See above. |
| | Normalized test scores (all subjects) | 24 Months | Mean ES reported. | 1095 | 1011 | d=.2700 | v=.0256 | Used ITT sample only. See above on sample size. $v=SE^2$ |

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| | Normalized test scores (all subjects) | 24 Months | Mean ES reported. | 1212 | 819 | d=.1000 | v=.0400 | Used ITT sample only. See above on sample size. $v=SE^2$ |
| **2010051 (Duflo et al. 2006A)** | Dropout before | 36 Months | Binary proportions | 23036 | 12404 | d=-.0510 | v=.0005 | Initial design included six |
| | completing primary school | | [Logit] | | | | | groups, but in analysis, PIs analyze teacher training separately (as delivered in any one of three groups) against the comparison group. {Reducing the cost of education program was only delivered in grade 6). |
| | Dropout before completing primary school | 36 Months | Binary proportions [Logit] | 22587 | 12161 | d=.0258 | v=.0003 | Initial design included six groups, but in analysis, PIs analyze teacher training (as delivered in any one of three groups) against the control group. |
| **2010097** | Dropout rate | 4 Years | Binary | 486 | 513 | d=.7419 | v=.0357 | Computed |

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| **(Newman et al. 2002)** | based on household data | | proportions [Logit, subtracting pretest d from post-test d] | | | | | sample sizes for households using same proportion as schools assigned to groups (.486 to treatment). Sum of variance at pretest and posttest. |
| | Grade repetition rate | 4 Years | Binary proportions [Logit] | 486 | 513 | d=.2010 | v=.0232 | See above. Pretest difference was 0. |
| **2010098 (Newman et al. 2002)** | Dropout rate based on household data | 4 Years | Binary proportions [Logit] | 476 | 436 | d=.1364 | v=.0134 | Computed sample sizes for households using same proportion as schools assigned to groups (.528 to treatment). |
| **2010003 (Simwaka et al. 2009)** | Dropping out | 5-6 Years | Binary proportions [Logit] | 422 | 229 | d=.2429 | v=.0154 | Used proportion data from randomly selected sample of schools and individual student analyses. |
| | Students transferring out of school | 5-6 Years | Binary proportions [Logit] | 422 | 229 | d=.0987 | v=.0240 | See above. |
| | Repeating grade | 5-6 Years | Binary proportions [Logit] | 422 | 229 | d=.3835 | v=.0192 | See above. |
| **2010049 (He et al. 2007)** | Attendance | 12 Months | Binary proportions [Logit] | 2699 | 2618 | d=.0104 | v=.0021 | Used individual sample sizes as reported in the evaluation document. |

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| | English Test, Total | 12 Months | Mean ES reported by PIs | 2699 | 2618 | d=.287 | v=.0102 | See above. |
| | Math Test, Total | 12 Months | Mean ES reported by PIs | 2699 | 2618 | d=.055 | v=.0082 | See above. |
| **2010050 (He et al. 2007)** | Attendance | 12 Months | Binary proportions [Logit] | 2413 | 2458 | d=.0713 | v=.0046 | Used individual sample sizes as reported in the evaluation document. In this RCT, PIs created four groups; we selected the most intensive intervention that combined both programs. |
| | English Test, Total | 12 Months | Mean ES reported by PIs | 2413 | 2458 | d=.299 | v=.0088 | See above. |
| | Math Test, Total | 12 Months | Mean ES reported by PIs | 2413 | 2458 | d=.350 | v=.0086 | See above. |
| **2010091 (Loshkin and Yemtsov, 2004)** | Child enrolled in school | 3 Years | PI reports same outcome and p=.50, two tailed test | 37 | 45 | d=0 | v=.0598 | Because baseline data not reported, only one outcome computed (two- tailed, p=.50 means that d=0). To compute v in this instance, we used the following. There were 37 villages compared to 45 villages. We used village level variance. Note that other |

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| | | | | | | | | outcomes were positive in direction but we could not compute ES from the data. |
| | Percent missing more than 30 days of school. | 3 years | Binary proportions [Logit] | 37 | 45 | d=.7038 | v=.4414 | Use pretest proportion for all villages. See above. |
| **2010069 (Kagitcibasi, et al. 2001)** | Still in school | 7 Years | Binary proportions [Logit] | 90 | 165 | d=.6104 | v=.0363 | Using Practical Effect Size Calculator to compute variance. |
| | | Math | Means/SDs [Cohen's d] | 90 | 165 | d=.4743 | v=.0176 | See above. |
| | | Turkish | Means/SDs [Cohen's d] | 90 | 165 | d=.4811 | v=.0176 | See above. |
| **2010065 (Nguyen, 2008a)** | Attendance | 16 Months | Binary proportions [Logit] | 5830 | 5829 | d=-1985 | v=.0447 | Used most intensive condition: Statistics and Role Model. PIs analyzed by school; we extrapolated by 66.24 students per school to produce individual N's. 50/50 split given that 160 schools randomized. Computed v using coefficient/SE=t, divide d by t, square. |

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| | Test Scores | 16 Months | PIs reported standardized mean effects | 5830 | 5829 | d=-.1320 | v-.0143 | See above. |
| **2010066 (Nguyen, 2008b)** | Attendance (surveyor reported) | 12 months | Binary Proportions [Logit] | 72720 | 72720 | d=-.0393 | v=.0304 | We selected the most intensive intervention which combined all district and school treatments. PI reports number of schools. We extrapolate number of schools to students by multiplying by 240 average enrolled per school. Only used proportion data due to schools as unit assigned and analyzed. {did not use PI reported ES for test scores as these were based on schools}. Computed v from t of treatment coefficient (coefficient/SE), divide d by t to get SE of d and square to get v. |
| | Attendance (classroom level count) | 12 months | Binary Proportions [Logit] | 72720 | 72720 | d=.1133 | v=.0121 | See above. |

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| | Attendance (Director's records) | 12 months | Binary Proportions [Logit] | 72720 | 72720 | d=.2988 | v=.0185 | See above. |
| | Grade repetition year 1 | 12 months | Binary proportions [Logit] | 72720 | 72720 | d=-.0064 | v=.0041 | See above. |
| | Grade repetition year 2 | 24 months | Binary proportions [Logit] | 72720 | 72720 | d=-.1762 | v=.0043 | See above. |
| | Exam Pas Rate | 12 months | Binary proportions [Logit] | 72720 | 72720 | d=.0104 | v=.0189 | See above. |
| | Net exit from all grades (dropout rate) | 12 Months | Binary proportions [Logit] | 72720 | 72720 | d=.1900 | v=.0281 | See above. |
| **2010008 (Kim et al. 1999)** | Probability of Enrollment, 1995 | 12 Months | Probit | 781 | 697 | d=.334 | v=.0010 | PIs do report a variety of methods of analyzing the data. All show positive and fairly sizable differences in enrollment in favor of the program. We use probit as that includes other statistical controls and takes clustering into account.<br><br>Computed v by dividing d by z- score and squaring. |

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| | Probability of Enrollment, 1996 | 24 Months | Binary proportions [Logit, subtracting pretest d from post-test d] | 781 | 697 | d=.399 | v=.0016 | See above. |
| | Probability of Enrollment, 1995 | 12 Months | Binary proportions [Logit, subtracting pretest d from post-test d] | 529 | 661 | d=.224 | v=.0018 | See above. |
| | Probability of Enrollment, 1996 | 24 Months | Binary proportions [Logit, subtracting pretest d from post-test d] | 529 | 661 | d=.268 | v=.0023 | See above.<br><br>Used cohort analysis sample (which was analysis sample) more conservative than age |
| **2010068 (Jenson 2007)** | Returned Next Year | 12 Month | Simple regression method | 1125 | 1125 | d=.0657 | v=.0018 | Assume 50/50 split in sample. |
| | Completed Secondary School | 4 Years | Simple regression method | 1125 | 1125 | d=.0351 | v=.0018 | See above. |

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| | Years of Schooling | 4 Years | Simple regression method | 1037 | 1037 | d=.0743 | v=.0019 | See above. |
| **2010044 (Duflo, et al. 2007)** | Child enrolled in government school | 24 Months | Binary proportions [Logit] | 1136 | 1061 | d=.3376 | v=.0035 | |
| | Child dropped out of school | 24 Months | Binary proportions [Logit] | 1136 | 1061 | d=.0717 | v=.0036 | |
| | Child left non-formal education center | 24 Months | Binary proportions [Logit] | 1136 | 1061 | d=.1843 | v=.0023 | This outcome is a positive as the program is designed to move children out of NFE Centers to government schools. |
| | Attendance for children | 24 Months | Binary proportions | 12956 | 10737 | d=.0919 | v=.0002 | |
| | who did not leave NFE | | [Logit] | | | | | |
| | Math, Mid- test | 12 Months | PI reported ES | 984 | 909 | d=.13 | v=.0049 | Only total N provided so used same proportions as above (.48 in control).<br>Used all children results with covariate controls.<br>Computed v using d/t method. |

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| | Language, Mid-test | 12 Months | PI reported ES | 984 | 909 | d=.13 | v=.0025 | See above. |
| | Math, Post-test | 24 Months | PI reported ES | 915 | 845 | d=.17 | v=.0100 | See above. |
| | Language, Post-test | 24 Months | PI reported ES | 915 | 845 | d=.13 | v=.0049 | See above. |
| **2010093**<br><br>**(Macours and Vakis, 2008)** | Assisting to school | Unknown | Binary proportions [Logit] | 3167 | 3166 | d=.2299 | v=.0022 | PI reports all beneficiaries together and we use that since it is hard to distinguish which intervention is most intensive.<br><br>There were 1000 households in treatment and 1000 in control; applied 50/50 split in student analysis.<br><br>No baseline data reported for “self reported better grades this year than last year.” Also, days absent is a mean and insufficient data provided for computation.<br><br>Used d/t method to compute v. |

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| | Early enrollment inn primary school | Unknow | Binary proportions [Logit] | 1753 | 1753 | d=.1431 | | v=.0032 | See above. |
| **2010052 (Baird et al. 2009)** | Enrollment | 12 months | Binary proportions [Logit] | 1346 | 1346 | d=.8152 | | v=.0173 | Assumed 50/50 split in sample based on proportion of areas in each group.<br>Used d/t method for computing v. |
| | School attendance | 12 months | Binary proportions [Logit] | 1437 | 1437 | d=.2878 | v=.0028 | | See above. |
| | Passed grade | 12 months | Binary proportions [Logit] | 1437 | 1437 | d=.1083 | v=.0043 | | See above. |
| **2010036 (Filmer and Schady, 2006)** | Attending on day of visit | 18 Months | Binary proportions [Logit].<br>Compared to baseline data in region on enrollment (used upper end of 36%). | 549 | 549 | d=.4591 | v=.0112 | | Used Regression Discontinuity Design quartic results from within 10 ranks of cut-off.<br>Similar to other estimates.<br>No other baseline data to compute d from.<br>Assumed 50/50 split in sample.<br>Used d/t method to compute v. |

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| | Normalized math test | 18 Months | PIs report ES | 549 | 549 | d=-.052 | v=.0084 | See above. |
| | Normalized vocabulary test | 18 Months | PIs report ES | 549 | 549 | d=-.029 | v=.008 | See above. |
| **2010017 (Levy, et al. 2009)** | Enrollment as reported in household survey | 24 Months | Binary proportions [Logit] | 9352 | 8632 | d=.4449 | v=.0006 | Estimated student sample size based on proportion of households in each group (48% in control). Used regression discontinuity estimation model highlighted by PIs (#3) but results similar for other specifications. Used ES Calculator to produce v. |
| | Child found in school during day of visit | 24 Months | Binary proportions [Logit] | 9352 | 8632 | d=.3760 | v=.0006 | See above. |
| | Normalized Math score | 24 Months | PIs reported ES | 9352 | 8632 | d=.395 | v=.0024 | See above on sample size. Used d/t method to compute v. |
| | Normalized French score | 24 Months | PIs reported ES | 9352 | 8632 | d=.366 | v=.0018 | See above on sample size. Used d/t method to compute v. |

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| **2010071 (Levy, et al. 2007)** | Attendance during reference period | 12 months | Cohen's d [Means/Sds] | 3783 | 3956 | d=.1417 | v=.0005 | Used standard deviation of the control group at baseline as SD for both groups.<br>Computed v using ES calculator. |
| | Attendance during typical period | 12 months | Cohen's d [Means/Sds] | 3783 | 3956 | d=.1528 | v=.0005 | See above. |
| | Parent self- report of child progressing to next grade | 12 months | Binary proportions [Logit] | 3783 | 3956 | d=.1078 | v=.0013 | Used ES Calculator to compute v.<br>PIs report regressions but those do not permit computation of ES. |
| | Parent self-report that their children doing better or much better than prior year | 12 months | Binary proportions [Logit] | 3783 | 3956 | d=-.0337 | v=.0007 | See above. |

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| | Parent self- report of their child's attendance at school is "more frequent" | 12 months | Binary proportions [Logit] | 3788 | 3589 | d=.7358 | v=.0012 | See above. |
| **2010007 (Barrera-Osorio, et al. 2007)** | Enrollment, grades 1-9 | 24 Months | Binary proportions [Logit] | 19823 | 19823 | d=.1369 | v=.0004 | Analysis sample here is 1 point above or below cut-off (similar except for high school estimate).<br>Assume 50/50 split in sample.<br>Used d/t method to compute v. |
| | Enrollment, grades 10-11, lowest income | 24 Months | Binary proportions [Logit] | 6975 | 6974 | d=.0254 | v=.0005 | See above. |
| | Enrollment, grades 10-11, higher income | 24 Months | Binary proportions [Logit] | 4049 | 4049 | d=.6169 | v=.0412 | See above. Also note that controlled regression analysis led to a percentage point increase to a result higher than 100%. We used .99 in the effect size calculator to compute. |

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| **2010064**<br>**(Banerjee, et al. 2008)** | Student out of school | 12 Months | Binary proportions | 6400 | 9209 | d=-.0918 | v=.0012 | We compared treatment 3 which is most intensive with the control group.<br>We used proportion of villages in treatment and control and sample size for test N's as our sample sizes.<br>PIs also reported "log" enrollment but this is based on a log of the enrollment rate at the aggregate level to correct for a positive skew, and is not useable in the effect size determination. |
| | Fraction of boys present in government schools | 12 Months | Binary proportions | 3200 | 4605 | d=-.1170 | v=.0081 | See above. |
| | Fraction of girls present in government schools | 12 Months | Binary proportions | 3200 | 4605 | d=-.0596 | v=.0059 | See above. |

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| | Math: could subtract or divide | 12 months | Binary proportions [Logit] | 6393 | 9199 | d=-.0023 | v=.0002 | See above. |
| | Reading: could read stories | 12 months | Binary proportions [Logit] | 6400 | 9209 | d=.0375 | v=.0001 | See above. |
| **2010011 (Oster et al. 2009)** | Attendance, official records | 15 Months | Binary proportions [Logit] | 99 | 99 | d=-.0314 | v=.0499 | PIs used N of observations; we use N of individuals.<br>Used v from ES Calculator.<br>Note that this effect includes only girls affected by their period. |
| | Attendance, official records | 15 Months | Binary proportions [Logit] | 99 | 99 | d=-.0314 | v=.0499 | See above. |
| | Attendance, Unofficial count | 15 Months | Binary proportions [Logit] | 99 | 99 | d=-.3284 | v=.0429 | See above. |
| | Attendance, time diary | 15 Months | Binary proportions [Logit] | 99 | 99 | d=.0548 | v=.0254 | See above. |
| | Math, normalized score | 15 Months | PI reported ES | 99 | 99 | d=-.315 | v=.0900 | See above.<br>Computed v using d/t method. |

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| | Nepali, normalized score | 15 Months | PI reported ES | 99 | 99 | d=.011 | v=.0697 | See above. |
| | English, normalized score | 15 Months | PI reported ES | 99 | 99 | d=-.197 | v=.1422 | See above. |
| **2010085 (Pianto & Soares, 2004)** | Enrollment of 7-14 year olds | 1-2 Years | Binary Proportions [Logit, subtracting pretest d from post-test d] | 38 | 38 | d=.1299 | v=.4184 | All matched municipalities that entered 1997-1999 and Had household sample sizes of 800 or less.<br>Computed v as sum of pretest and posttest v. |
| | | | | 17 | 17 | d=.1274 | v=.8493 | Only those matched municipalities that entered in 1998 and had household sample sizes of 800 or less.<br>Computed v as sum of v at pretest and posttest. |
| **2010020 (Vermeersch & Kremer, 2004)** | Child level school participation | 2 years | Binary proportions [Logit] | 2555 | 2555 | d=.2106 | v=.0012 | Assume 50/50 split in the student N's (due to 50/50 split in randomized schools).<br>Computed v ES Calculator |

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| | Probability of being found once in school | 2 years | Probit | 2099 | 2098 | d=.264 | v=.0098 | Computed v using d/t method. |
| | Test scores: Oral Cognitive | 2 years | PI reported ES. | 675 | 675 | d=.-.03 | v=.0081 | Computed v using d/t method. |
| | Test Scores: Oral Curriculum | 2 years | PI reported ES. | 679 | 678 | d=.07 | v=.0081 | Computed v using d/t method. |
| | Test Scores: Written Curriculum | 2 years | PI reported ES. | 663 | 663 | d=.02 | v=.0144 | Computed v using d/t method. |
| **2010067 (Miguel & Kremer, 2003)** | School level participation | 1 year | Binary proportions [Logit] | 25 | 25 | d=.3156 | v=.1232 | Took average of two comparison groups (used N of 50 for control). Could not use student level N's for this analysis. (Later student level analysis did not provide necessary data to compute d). |
| | School level participation | 1 year | Binary proportions [Logit] | 25 | 50 | d=.2306 | v=.1425 | See above. |
| | School level participation | 1 year | Binary proportions [Logit] | 25 | 50 | d=.2805 | v=.1335 | See above. |

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| | School level participation | 1 year | Binary proportions [Logit] | 25 | 50 | d=.3013 | v=.1254 | See above. |
| | School level participation | 2 years | Binary proportions [Logit] | 25 | 25 | d=.1286 | v=.0866 | Took average of two comparison groups (used N of 50 for control). Could not use student level N's for this analysis. (Later student level analysis did not provide necessary data to compute d). |
| | School level participation | 2 years | Binary proportions [Logit] | 25 | 50 | d=.0902 | v=.0770 | See above. |
| | School level participation | 2 years | Binary proportions [Logit] | 25 | 50 | d=.1734 | v=.0864 | See above. |
| | School level participation | 2 years | Binary proportions [Logit] | 25 | 50 | d=.1084 | v=.0845 | See above. |
| | ICS Exam Score | 1 year | PI reported ES | 12479 | 12479 | d=-.032 | v=.0211 | Individual level test data clustered within schools.<br>Computed v using d/t method. |
| | ICS Exam Score | 2 years | PI reported ES | 9536 | 9536 | d=.001 | v=.0001 | See above. |

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| **2010053**<br><br>**(Kazianga et al. 2008)** | Program Impact on New Enrollment 6-15 year olds | 1 Year | Binary proportions [Logit] | 3294 | 3293 | d=.0475 | v=.0003 | Two interventions; used school feeding scheme as the treatment rather than take home rations as treatment.<br><br>Assume 50/50 split in individual N's.<br><br>We averaged the treatment and control baseline enrollment data and then used the estimate of impact as the difference to produce d.<br><br>Only captured data on boys and girls, ages 6-15 (other analyses reported further differentiate by age: 6-12; 13-15).<br><br>Used d/t to produce v. |
| | Program Impact on New Enrollment 6-15 year olds | 1 Year | Binary proportions [Logit] | 1269 | 1268 | d=-,0289 | v=.0078 | See above. |

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| | Program Impact on New Enrollment 6-15 year olds | 1 Year | Binary proportions [Logit] | 2025 | 2025 | d=.1324 | v=.0014 | See above. |
| **2010099**<br>**(Barrera-Osorio, et al. 2008)** | Verified attendance, grades 9-11 | 4-12 months | Binary proportions [Logit] | 1140 | 1404 | d=.1861 | v=.0033 | There were three treatment groups; we selected the “tertiary treatment” which has a larger overall worth than the other two interventions (traditional CCT or “savings” treatment).<br>Used ES Calculator to compute v. |
| | Enrollment | 4-12 months | Binary proportions [Logit] | 1140 | 1404 | d=.0992 | v=.0026 | See above. |
| | Self-reported enrollment | 4-12 months | Binary proportions [Logit] | 1140 | 1404 | d=.6101 | v=.0851 | See above. |
| | Self-reported attendance | 4-12 months | Binary proportions [Logit] | 1140 | 1404 | d=.2514 | v=.0145 | See above. |

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| | Graduated | 12 months | Binary proportions [Logit] | 1140 | 1404 | d=.3316 | v=.0076 | See above. |
| | Passed grade | 12 months | Binary proportions [Logit] | 1140 | 1404 | d=.1568 | v=.0063 | See above. |
| **2010109 (Glewwe, et al. 2003)** | Dropout | 12 months | Binary proportions [Logit] | 7200 | 8024 | D=.0401 | v=.0036 | Used d/t method to compute v. |
| | Dropout | 24 months | Binary proportions [Logit] | 7492 | 8226 | d=.0401 | v=.0036 | See above. |
| | Dropout | 36 months | Binary proportions [Logit] | 4531 | 4948 | d=.0097 | v=.0001 | See above. Also, we computed N's based on year 1 and year 2 percentage of treatment participants (.478), as only total participant N provided. |
| | Grade Repetition | 12 months | Binary proportions [Logit] | 5039 | 5503 | d=.0895 | v=.0047 | See above. |
| | Grade repetition | 24 months | Binary proportions [Logit] | 4258 | 4649 | d=.0864 | v=.0059 | See above. |

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| | Test score on government exam | 12 months | PI reported ES | 24232 | 26462 | d=.0052 | v=.0092 | See above, except compute v by multiplying S/E. |
| | | 24 months | PI reported ES | 26004 | 28397 | d=.1440 | v=.0075 | See above, except compute v by multiplying S/E. |
| | | 36 months | PI reported ES | 16014 | 17488 | d=.0900 | v=.0073 | See above, except compute v by multiplying S/E. |
| | Score on NGO exam | 12 months | PI reported ES | 19072 | 20828 | d=.0920 | v=.0073 | See above, except compute v by multiplying S/E. |
| | | 24 months | PI reported ES | 8956 | 9780 | d=.0240 | v=.0102 | See above, except compute v by multiplying S/E. |
| **2010083 (Meng & Ryan, 2007)** | School participation rate | 1-7 years | t-test with unequal sample sizes | 355 | 155 | d=0.2513 | v=0.0093 | Used comparisons for FFE eligible vs. would have been eligible in non-FFE unions. Used estimates from matches using caliper = .01.<br><br>Estimates of d are similar when using caliper = . 005. Estimate of d is smaller than the d (.34) computed using difference-in-differences. |

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| | Years of schooling | 1-7 years | t-test with unequal sample sizes | 291 | 144 | d= 0.6867 | v= 0.0109 | See above.<br>Estimates of d are similar when using caliper =<br>.005. Estimate of d is larger than the d (.47) computed using difference-in-differences. |
| **2010028**<br>**(Borkum, 2009)** | School enrollment (logged) | 12 months | Means and standard deviations | 6630 | 4607 | d = 0.005 | v = 0.000039 | Use proportionate N's of students based on proportions of total Ns observed (59%/45%).<br>We used the coefficient from the matched model using 3 nearest neighbors with bias adjustment. Results are similar using the results from other matched models.<br>Variance is computed by dividing the coefficient by SE to produce t; divide d by t to produce S/E of the d estimate; square it to produce v. |
| | School enrollment (logged) | 12 months | Means and standard deviations | 1461 | 1785 | d = 0.0354 | v = 0.00015 | See above. |

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| **2010113**<br>**(Parker, Todd, & Wolpin, 2005)** | School attendance | 12 months | Unstandardized regression coefficient | 2615 | 1733 | d = 0.0499 | v =0 .0002 | We chose the Intervention Eligible and<br>Non-intervention Eligible groups.<br>We used the coefficient from the difference- in-difference intrafamily model emphasized by PI. Used pooled baseline standard deviation. Used household level Ns. Variance is computed by dividing the coefficient by SE to produce t; divide d by t to produce S/E of the d estimate; square it to produce v. |
| **2010010**<br>**(Chadhury & Parajuli, 2006)** | Percent enrolled | 12 months | T-test, unequal sample sizes | 4026 | 6421 | d = 0.1301 | v = 0.0004 | We used coefficient from the DD strategy and RDD approach from which an ES could be calculated. |
| | Number enrolled | 12 months | T-test, unequal sample sizes | 4026 | 6421 | d=0.155 | v=0.0004 | We used coefficient from the DD strategy and RDD approach from which an ES could be calculated. |

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| **2010084**<br><br>**(Gertler, Patrinos, & Rubio-Codina, 2007)** | School failure rate | 12-48 months | Unstandardized regression coefficient | 1825 | 2793 | d= 0.0863 | v= 0.001 | PIs reported eight estimation models. Effects were similar across all eight. We computed estimates from “Q34A” model that used a comparison group matched to targeting index, used one treatment indicator, and controlled for other educational interventions present in the schools.<br><br>Effects for school level Ns and SDs. If Individual Ns of 9125 and 13965 used, v changes to .0002.<br><br>Variance is computed by dividing the coefficient by SE to produce t; divide d by t to produce S/E of the d estimate; square it to produce v. |
| | School repetition rate | 12-48 months | Unstandardized regression coefficient | 1825 | 2793 | d= 0.1093 | v= 0.001 | See above. |

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| | Dropout rate | 12-48 months | Unstandardized regression coefficient | 1825 | 2793 | d=0 | v= 0.0009 | See above. |
| | Total enrollment | 12-48 months | Unstandardized regression coefficient | 1825 | 2793 | d= 0.0049 | v= 0.00002 | See above. |
| **2010004 (Angrist et al., 2002)** | Currently in private school | 36 months | Binary proportions | 1147 | 562 | d=0.3679 | v=0.003427 | Estimates for Bogota 1995 cohort only. Used estimates from model with basic controls and 19 barrio controls. |
| | Currently in school | 36 months | Binary proportions | 1147 | 562 | d=0.0279 | v=0.005803 | See above. |
| | Finished 6th grade | 36 months | Binary proportions | 1147 | 562 | d=0.2655 | v=0.017699 | See above. |
| | Finished 7th grade | 36 months | Binary proportions | 1147 | 562 | d=0.1344 | v=0.006613 | See above. |
| | Finished 8th grade | 36 months | Binary proportions | 1147 | 562 | d=0.2391 | v=0.003658 | See above. |
| | Ever repeated a grade (after lottery) | 36 months | Binary proportions | 1147 | 562 | d=0.1775 | v=0.004964 | See above. |

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| | Highest grade completed | 36 months | Means and standard deviations | 1147 | 562 | d=0.125 | v=0.0027 | See above. Used control group standard deviation. |
| | Repetitions of 6th grade | 36 months | Means and standard deviations | 1147 | 562 | d=0.13 | v=0.0027 | See above. |
| | Total repetitions since lottery | 36 months | Means and standard deviations | 1147 | 562 | d=0.126 | v=0.0027 | See above. |
| | Years in school since lottery | 36 months | Means and standard deviations | 1147 | 562 | d=0.0326 | v=0.0027 | See above. |
| | Test Scores, Math + Reading + Writing | 36 months | Unstandardized regression coefficient | 143 | 139 | d=0.2065 | v=0.012 | Assumed Ns proportionate to those in total Bogota 1995 sample. Used control group standard deviation. Estimates from OLS model with covariates. Variance is computed by dividing the coefficient by SE to produce t; divide d by t to produce S/E of the destimate; square it to produce v. |
| | Test Scores, Math | 36 months | Unstandardized regression coefficient | 143 | 139 | d=0.1537 | v=0.013 | See above. |

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| | Test Scores, Reading | 36 months | Unstandardized regression coefficient | 144 | 139 | d=0.2044 | v=0.013 | See above. |
| | Test Scores, Writing | 36 months | Unstandardized regression coefficient | 144 | 139 | d=0.1285 | v=0.011 | See above. |
| | Test Scores, Language | 84 months | Unstandardized regression coefficient | 624 | 599 | d=0.1253 | v=0.003 | Assumed Ns proportionate to those in total Bogota 1995 sample. Estimates from OLS model with score > 0. Variance is computed by dividing the coefficient by SE to produce t; divide d by t to produce S/E of the d estimate; square it to produce v. |
| | Test Scores, Math | 84 months | Unstandardized regression coefficient | 624 | 599 | d=0.0817 | v=0.004 | See above. |

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| **2010031**<br>**(Skoufias & Shapiro, 2006)** | School dropout rate | 36 months | Unstandardized regression coefficient | 1767 | 65457 | d=0.0472 | v=0.0003 | PIs reported five estimation models. We computed estimates from the PI's preferred DID model with PSM.<br>Selected the strongest contrast (3 years of intervention vs. no years of intervention).<br>Variance is computed by dividing the coefficient by SE to produce t; divide d by t to produce S/E of the d estimate; square it to produce v. |
| | School failure rate | 36 months | Unstandardized regression coefficient | 1767 | 65457 | d=0.0386 | v=0.00007 | See above. |
| | School repetition rate | 36 months | Unstandardized regression coefficient | 1767 | 65457 | d=0.0539 | v=0.0001 | See above. |

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| **2010060**<br>**(Heinrich, 2005)** | Number of years attended full school year | 60 months | Unstandardized regression coefficient | 1916 | 670 | d=0.15 | v=0.003 | Selected the intervention for which SD was calculable based on multi-category frequencies (any scholarship receipt).<br>Variance is computed by dividing the coefficient by SE to produce t; divide d by t to produce S/E of the d estimate; square it to produce v. |
| **2010076**<br>**(Banerjee et al, 2005)** | Attendance | 7 months | T-test, unequal sample sizes | 2463 | 1786 | d = -0.0199 | v = 0.001 | This study ID refers to Mumbai Grade 3, Year 1 Cohort<br>Authors reported two attendance outcomes (researcher, teacher observation); we chose the first researcher report. |
| | Math scores | 7 months | Means and standard deviations | 2417 | 2027 | d = 0.156 | v = 0.0009 | This study ID refers to Mumbai Grade 3, Year 1 Cohort<br>Used control group SD. |
| | Verbal scores | 7 months | Means and standard deviations | 2417 | 2027 | d = 0.149 | v = 0.0009 | This study ID refers to Mumbai Grade 3, Year 1 Cohort<br>Used control group SD. |

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| **2010080**<br>**(Banerjee et al, 2005)** | Attendance | 7 months | T-test, unequal sample sizes | 2499 | 2836 | d = 0.0088 | v = 0.0008 | This study ID refers to Mumbai Grade 3, Year 2 Cohort |
| | Math scores | 7 months | Means and standard deviations | 2337 | 2731 | d = 0.176 | v = 0.0008 | This study ID refers to Mumbai Grade 3, Year 2 Cohort<br>Used control group SD. |
| | Verbal scores | 7 months | Means and standard deviations | 2337 | 2731 | d = 0.067 | v = 0.0008 | This study ID refers to Mumbai Grade 3, Year 2 Cohort<br>Used control group SD. |
| **2010077**<br>**(Banerjee et al, 2005)** | Attendance | 7 months | T-test, unequal sample sizes | 2593 | 2535 | d = -0.0441 | v = 0.0008 | This study ID refers to Vadodara Balsakhi Grade 3, Year 1 Cohort |
| | Math scores | 2 months | Means and standard deviations | 2285 | 2174 | d = 0.125 | v = 0.0009 | This study ID refers to Vadodara Balsakhi Grade 3, Year 1 Cohort<br>Used control group SD. |
| | Math scores | 7 months | Means and standard deviations | 2122 | 2108 | d = 0.18 | v = 0.0009 | This study ID refers to Vadodara Balsakhi Grade 3, Year 1 Cohort<br>Used control group SD. |

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| | Verbal scores | 2 months | Means and standard deviations | 2285 | 2174 | d = 0.111 | v = 0.0009 | This study ID refers to Vadodara Balsakhi Grade 3, Year 1 Cohort<br><br>Used control group SD. |
| | Verbal scores | 7 months | Means and standard deviations | 2122 | 2108 | d = 0.159 | v = 0.0009 | This study ID refers to Vadodara Balsakhi Grade 3, Year 1 Cohort<br><br>Used control group SD. |
| **2010079**<br><br>**(Banerjee et al, 2005)** | Attendance | 7 months | T-test, unequal sample sizes | 3131 | 2892 | d = 0.0098 | v = 0.0007 | This study ID refers to Vadodara Balsakhi Grade 3, Year 2 Cohort |
| | Math scores | 2 months | Means and standard deviations | 2843 | 2608 | d = 0.454 | v = 0.0008 | This study ID refers to Vadodara Balsakhi Grade 3, Year 2 Cohort<br><br>Used control group SD. |
| | Math scores | 7 months | Means and standard deviations | 3027 | 2792 | d = 0.439 | v = 0.0007 | This study ID refers to Vadodara Balsakhi Grade 3, Year 2 Cohort<br><br>Used control group SD. |
| | Verbal scores | 2 months | Means and standard deviations | 2843 | 2608 | d = 0.423 | v = 0.0008 | This study ID refers to Vadodara Balsakhi Grade 3, Year 2 Cohort Used control group SD. |

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| | Verbal scores | 7 months | Means and standard deviations | 3027 | 2792 | d = 0.247 | v = 0.0007 | This study ID refers to Vadodara Balsakhi Grade 3, Year 2 Cohort<br><br>Used control group SD. |
| **2010078**<br><br>**(Banerjee et al, 2005)** | Attendance | 7 months | T-test, unequal sample sizes | 2389 | 2595 | d = 0.0215 | v = 0.0008 | This study ID refers to Vadodara Balsakhi Grade 4, Year 1 Cohort |
| | Math scores | 2 months | Means and standard deviations | 2175 | 2402 | d = 0.104 | v = 0.0009 | This study ID refers to Vadodara Balsakhi Grade 4, Year 1 Cohort<br><br>Used control group SD. |
| | Math scores | 7 months | Means and standard deviations | 1962 | 2234 | d = 0.162 | v = 0.001 | This study ID refers to Vadodara Balsakhi Grade 4, Year 1 Cohort<br><br>Used control group SD. |
| | Verbal scores | 2 months | Means and standard deviations | 2175 | 2402 | d = 0.089 | v = 0.0009 | This study ID refers to Vadodara Balsakhi Grade 4, Year 1 Cohort<br><br>Used control group SD. |
| | Verbal scores | 7 months | Means and standard deviations | 1962 | 2234 | d = 0.086 | v = 0.001 | This study ID refers to Vadodara Balsakhi Grade 4, Year 1 Cohort<br><br>Used control group SD. |

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| **2010041 (Glewwe & Olinto, 2004)** | Enrollment | 12 months | Binary proportions | 1906 | 1858 | d= 0.1677 | v= 0.0018 | Selected the strongest contrast (SDIF Supply + Demand intervention vs. control).<br>Use proportionate N's of students based on proportions of total Ns observed.<br>Used estimates from PI preferred diff in diff model. |
| | Enrollment | 24 months | Binary proportions | 2225 | 2160 | d= 0.1996 | v= 0.001915 | See above. |
| | Dropping out | 12 months | Binary proportions | 1424 | 1382 | d= 0.214 | v= 0.007786 | Used estimates from cross-sectional diff model. |
| | Dropping out | 24 months | Binary proportions | 1491 | 1447 | d= 0.0609 | v= 0.003816 | See above. |
| | Grade promotion | 12 months | Binary proportions | 1424 | 1382 | d= 0.0504 | v= 0.003966 | See above. Used estimates from cross- sectional diff model that provided baseline data needed to calculate d. |
| **2010112 (Todd & Wolpin, 2003)** | Attendance | 12 months | 2x2 frequency table | 1278 | 891 | d= 0.4916 | v= 0.027613 | Used 1998 posttest differences in attendance aggregated across gender. |

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| | Attendance | 12 months | 2x2 frequency table | 485 | 358 | d= 0.164 | v= 0.007394 | See above. |
| **2010042** **(Andrabi, Das, & Khwaja, 2009)** | Test Scores, Average | 12 months | Unstandardized regression coefficient | 56 | 56 | d = 0.1596 | v =0.004 | Used school level SD from average score at pretest.<br>Variance is computed by dividing the coefficient by SE to produce t; divide d by t to produce S/E of the d estimate; square it to produce v.<br>Effects for school level Ns. If individual Ns of 4933 treatment and 4934 control used, d remains the same, v changes to 0.0004 |
| | Test Scores, English | 12 months | Unstandardized regression coefficient | 56 | 56 | d = 0.1399 | v =0.005 | See above. |
| | Test Scores, Urdu | 12 months | Unstandardized regression coefficient | 56 | 56 | d = 0.1413 | v =0.057 | See above. |

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| | Test Scores, Math | 12 months | Unstandardized regression coefficient | 56 | 56 | d = 0.2062 | v =0.007 | See above. |
| | Test Scores, Average | 24 months | Unstandardized regression coefficient | 56 | 56 | d = 0.1722 | v =0.004 | See above. |
| | Enrollment, number enrolled | 12 months | Unstandardized regression coefficient | 56 | 56 | d = 0.1723 | v =0.006 | See above. |
| | School dropout | 12 months | Binary proportions | 56 | 56 | d = -0.0354 | v =0.0006 | Assumes baseline dropout rate of 9%. |

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| **2010044 (Duflo, Dupas, & Kremer, 2007)** | Test scores, Total score | 18 months | Unstandardized regression coefficient | 1813 | 3308 | d = 0.3135 | v = 0.011 | We used the strongest intervention contrast of non- ETP students vs. achievement tracking with SBM.<br><br>Effects for individual level Ns. If school Ns of 70/70 used, v changes to 0.03.<br><br>Variance is computed by dividing the coefficient by SE to produce t; divide d by t to produce S/E of the d estimate; square it to produce v. |
| | Test scores, Mathematics | 18 months | Unstandardized regression coefficient | 1813 | 3308 | d = 0.3135 | v = 0.008 | See above. |
| | Test scores, Literacy | 18 months | Unstandardized regression coefficient | 1813 | 3308 | d = 0.2518 | v = 0.012 | See above. |
| | Dropped out | 24 months | Unstandardized regression coefficient | 3381 | 6731 | d = 0.0333 | v = 0.000003 | See above. |

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| | Grade promotion | 24 months | Unstandardized regression coefficient | 3381 | 6731 | d = 0.1383 | v = 0.003 | See above. |

1. Technically, there are formulae available to increase precision of this approach, but these have not become standard practice within meta-analysis because there is no reasonable basis for estimating covariances. The approach here has been the consistent with existing practice, and is more conservative (assumes more variability in the ES than there probably is).
2. *In this instance, PIs report means and standard deviations at pretest and posttest on math and reading tests, and an analysis of covariance that includes controls and takes clustering into account. We first compute a pooled standard deviation by squaring each pretest and posttest SD and multiplying it by N-1 (232 for treatment and 168 for control). We divide this by the sum of these four N-1 samples, take the square root of that sum, and that provides the pooled SD. Dividing the parameter estimate by this pooled standard deviation provides an estimate of d. To compute the standard error, we divide d by t (the square root of F is t), and squaring the result provides an estimate of variance.*

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