

## The effects of K-12 school enrollment policies in developing countries

3ie Synthetic Reviews – SR 004

Draft protocol

May 2009

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**Funding:** 3IE

**NOTE: THIS IS A DRAFT THAT WAS SUBMITTED BUT NOT YET APPROVED  
BY THE CAMPBELL COLLABORATION EDUCATION GROUP.**

## 1. Background

It has been well established that education is key to economic development and social welfare. Investments in education yield returns in poverty reduction, improved health outcomes, and economic growth (UNESCO, 2007; Hannum & Buchmann, 2004; Herz & Sperling, 2003). In addition, increased access to education contributes to increased political participation and more equitable sharing of economic and political power (Birdsall, 1999). Education for girls is particularly critical, as improvements in the infant mortality rate, child nutrition, and school enrollment are closely associated with maternal education (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).

Low educational attainment 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). In some countries, such as India, Mali, and Burkina Faso, school enrollment is very low, due to issues such as the cost of schooling (both direct and opportunity costs), poor school infrastructure, teacher shortages, and safety and sanitation problems (Birdsall, Levine, & Ibrahim, 2005). In others, such as many Latin American countries, enrollment may be nearly universal, but retention and completion may be quite low (*ibid*) 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).

Furthermore, developing nations face significant enrollment and completion disparities between segments of the population such as rich and poor, boys and girls, urban and rural dwellers, and combinations of these factors (Birdsall, Levine, & Ibrahim, 2005). For example, in India, the gender gap (favoring boys) for children from the richest households is only 2.5 percent, while 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 (*ibid*). In addition, ethno-linguistic diversity, disabilities, and conflict situations in fragile states create further barriers for school participation in developing nations (Birdsall, Levine, & Ibrahim 2005).

In light of compelling evidence that links expanded education systems and socioeconomic development while highlighting the importance of policies to offset inequality in access (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 school enrollment and equity. While building new schools to increase ease of access in remote areas is often prioritized, Filmer (2004), simulating the impact of lowering the average distance between children and schools, found little effect for improving enrollment or equity. 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 (UNESCO, 2007). In Asia, stipend programs encourage the transition of girls to secondary school (ibid).

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 Progresca school subsidies experiment in Mexico, which gave educational grants to poor mothers of children enrolled in school with 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 & 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 (Cueto & Chinen, 2008). Glewwe & Miguel (2008) review randomized evaluations of school-based health interventions such as that of Miguel and Krema (2004), which 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. Recent randomized evaluations of other types of programs aimed at increasing enrollment and completion 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. The Millennium Challenge Corporation is currently conducting a regression discontinuity study to assess the impact of school construction and other associated interventions on female student school enrollment in 132 communities in Burkina Faso versus 161 communities not selected for “treatment”, with the impact report to be issued shortly.

To our knowledge, a systematic review of randomized controlled trials and quasi-experiments of school enrollment and persistence strategies in developing nations has not yet been reported. By systematically gathering and analyzing rigorous research about the program effects of primary and secondary school enrollment and secondary school completion policies, our review will provide key evidence to inform the next wave of development efforts in this area.

## **2. Objectives**

For this project, we will be collecting studies that respond to the question: *What are the documented impacts of school enrollment policies and programs in developing nations on enrollment and persistence outcomes (e.g, primary and secondary school enrollment, attendance, retention, primary to secondary transition, and secondary school completion), and on learning outcomes (e.g., test scores, grades, etc.)?*

## **3. Methodology**

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

For this project, we will only include those studies that have the following characteristics:

(1) *Randomized controlled trials or quasi-experimental evaluations with evidence of equating.* Our review includes evaluations that randomly assign communities, families, or students to intervention or control conditions. Only well-implemented randomized experiments can provide statistically unbiased estimates of an intervention's effect (Boruch, 1997). There are comparatively very few randomized experiments in developing nations (Glewwe and Kremer, 2006). We will also include those evaluative studies that use quasi-experimental designs, provided that they offer evidence that the comparison groups were equated. Such quasi-experiments include regression discontinuity design, propensity scores, covariate matching, and other pre or post intervention matching. Although quasi-experiments cannot control for unknown or unmeasured confounding factors, there is conflicting literature on whether the estimates from such designs approximate those from randomized experiments (e.g., Oliver et al 2008). We include both types of studies and will include study design as a moderator in our later analyses.

(2) *The evaluations have to have taken place in a country classified as a "developing nation"* (as defined by the United Nations) at the time the intervention being studied was implemented.

(3) *The evaluations have to assess the impact of a K-12 school enrollment strategy.* Programs designed to boost preschool, college, or university enrollment, or other strategies, will not be included.

(4) *The evaluations have to include at least one outcome measure of school enrollment or persistence.* Other measures, including other learning outcomes (e.g., grades, test scores, school disciplinaries) or impacts on health, child labor, costs, equity, or attitudes or satisfaction levels will also be collected provided the study includes at least one measure of school enrollment/persistence or learning.

(5) *The evaluation study report is published or available through March 2009, without regard to language or publication type.* We will also search for trials published up to and including March 2009, without regard to the start date of publication. However, we anticipate that most experiments in this area will have been published after 1960. In concert with Campbell principles, we will attempt to find English and non-English studies. In addition, we will include published and unpublished studies (e.g., from conference papers, dissertations, technical reports).

### **Example of studies that would be included in our review**

An experiment that would meet the study inclusion criteria is the aforementioned Progresca school subsidies experiment in rural Mexico, which gave educational grants to poor mothers of children enrolled in school with 85 percent attendance. During 1998-2000, the program was implemented randomly in 314 of the 495 poorest rural villages in central and southern Mexico. Positive impacts for school enrollment and other factors were demonstrated (Schultz, 2001).

### **Example of studies that would not be included in our review**

One study that would not be included in our review is that of Bobonis, Miguel, and Sharma (2006), who 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 would not include it in our review because it does not evaluate a K-12 enrollment strategy.

### **Search strategy for identification of relevant studies**

We will rely on three major strategies to identify relevant evaluations published up to 2009. These are:

- (1) *Electronic searches of bibliographic databases.* Researchers will use available online resources and databases at WestEd, the University of Pennsylvania, Boston Public Library, and the University of Massachusetts, including ERIC, EBSCO, British Education Index, PAIS International/Archive, Sage, Sociofile, and UNESDO/UNESBIB. See Appendix A for additional databases that will be searched.
- (2) *Google searches of the Internet, including online holdings of international development organizations and research firms.* This would also include international or national/federal agencies that either conduct or would be aware of possibly relevant evaluations in developing nations, including the World Health Organization, the U.K. Department for International Development (DfID), and the United States Agency for International Development (US-AID). It would also provide coverage of websites with great relevance to international development, including the Network for Policy Research, Review and Advice on Education and Training ([www.norrag.org](http://www.norrag.org)). Research firms such as RTI International (particularly its international education division at [http://www.rti.org/page.cfm/International\\_Education](http://www.rti.org/page.cfm/International_Education)) will also be part of this search strategy.
- (3) *Citation chasing.* The reference section of every retrieved report will also be checked to determine whether any possible eligible evaluations are listed. As noted in the eligibility criteria, we are not exclusively seeking English language reports. We will ask our colleagues from other nations for help in identifying any non-English studies. WestEd also has employees bilingual in Spanish, French, Japanese, and Chinese who can translate abstracts or full-text documents in non-English to determine their eligibility for this review.
- (4) *Contacting the "informal college" of researchers in this area.* Although randomized controlled trials and well-equated quasi-experiments are comparatively rare in developing countries, there is a network of researchers that are conducting or are aware of such studies. We will reach out to such researchers by email to query them about studies that may be relevant to our review. This includes representatives of organizations mentioned in (1) above.

### **Keyword strategies for bibliographic databases**

The databases in Appendix A can be somewhat idiosyncratic. In addition, the yield of randomized and strong quasi-experimental designs in developing nations is not expected to be large. Thus, we believe the best strategy is to conduct a broad search of the available databases that errs on the side of sensitivity rather than specificity. In other words, we would rather get many titles and abstracts to sift through rather than potentially miss relevant citations because our search terms were drawn narrowly.

We will use two different search strategies for these databases, depending on the focus of the bibliographic database. If the database is focused on education (such

as *ERIC* or *ProQuest Education Journals*), we will use broad searches that identify evaluation studies conducted in developing nations. To do this, we will use the following keywords (and their derivatives) to find outcome studies: "random," "experiment," "control," "evaluate," "trial," "impact," "effect," and "outcome." Second, we will combine those keywords with ones that focus the search on developing nations, such as the use of terms like "developing," "third world," and "impoverished" with "nation," "country," or "region." In addition, we will use the names of specific developing nations, such as India, Mexico, etc., and the names of regions, such as Africa, Latin America, and Asia. Such searching is an iterative process and we will modify as we retrieve studies. This strategy may produce a number of false positives, but our experience is that examining the abstracts is not time consuming and researchers can go through them quite quickly.

If the database is not focused on education (e.g., *IngentaConnect* or *Sociological Abstracts*), the above strategy must then be supplemented by either a classification code or a third set of keywords that identifies education literature. In some databases, a classification code exists; for example, in *Sociological Abstracts (Sociofile)*, one can limit the abstracts to those dealing with "sociology of education". But in many of others, there is no classification code. In those instances, we will use keywords (and their derivatives) related to education, for example: "primary school," "secondary school," "school enrollment," (and its UK spelling, "enrolment"), and "schooling."

The appendices to our final review report will carefully document all keywords used for each database to permit replication.

### **Retrieving and Final Screening of Studies**

Search methods will identify a large number of citations and abstracts. Many of these will be easily excluded as not being relevant to the proposed review. In some cases, however, they will identify potentially eligible studies. The full text documents of those potentially eligible studies will be retrieved and screened by the first two authors before the study can be formally included in the review. Fortunately, with the advent of the Internet and full-text electronic journal access, we will be able to rapidly retrieve the reports to do a more thorough reading. When a full text report is received, we will scan it to ensure that it includes randomization or quasi-experimental equating of study subjects and includes at least one outcome of school enrollment or persistence. If the first two Investigators do not agree on the inclusion of a particular study, it will be excluded and documented in the final report.

### **Extracting Information from Each Study**

We have designed a preliminary instrument to guide us in recording information from each study (see Appendix B). Although the instrument contains several open-ended items, these will be collapsed when appropriate into a smaller number of categories to permit further analysis. For example, items such as "how equating was performed" can be collapsed into three or four larger categories representing the most frequent responses (e.g., discontinuity, covariate matching, propensity score, post-hoc statistical matching) and an "other" response that captures all those responses that do not fit into the most common methods of equating in this set of studies.

The instrument has items in the following areas:

*Researcher, Study and Contextual Characteristics:*

Study reports can be used to provide information about the publication and characteristics about the experiment and the context. For example, we will extract data about the type of publication the study was reported in and the setting in which the trial was conducted. If the documents provide information on the context in which the study takes place, we will also include it.

*Study Methods and Methodological Quality:*

We will extract information about the randomization, quasi-experimental assignment, and other methodological aspects of the evaluation. It is especially critical that information about two key issues in the implementation of an evaluation be extracted from each study report:

- a) *How the groups were equated and whether any problems with equating were reported.* The integrity of a randomized experiment or a quasi-experiment largely rests on how faithfully the equating procedures were implemented. We will code information about randomization and the quasi-experimental matching or equating procedures that were used in the study.
- b) *Whether the researchers report a loss of participants from the initial assigned sample at the end of the study.* Such attrition, if it is significant, can comprise the equating of groups, particularly if different types of people drop out from the intervention than dropped out from the other conditions.

*Intervention and Control Conditions data:*

These items will solicit detailed descriptions of the intervention and control condition, and the number of participants assigned to each. We anticipate that the evaluations in this review sample will be comprised of a single intervention and a single control group. In the rare occasion that this is not the case, we will select the most policy relevant groups to include to compute our experimental versus control condition contrast. In most cases, it will be the groups with the strongest contrast, i.e., the most intensive intervention condition versus the control condition. We recognize the importance of documenting these decisions for full transparency.

*Participants in the Trial data:*

These items solicit detail about the type of participants in the trials, including information on the country where the study took place, the nationality of the participants, the age and school level targeted, gender, whether an urban or rural setting was involved, and the socioeconomic status of the students.

*Outcome data:*

For each eligible study (each eligible study will have, at minimum, one outcome measure of enrollment or persistence), we will extract information on reported outcomes including impacts on learning, health, child labor, costs, and equity. We will also code any other outputs or data on key "mechanisms" that would provide clues as to why the intervention did or did not have its intended impact.

### **Handling multiple reports on the same experiment**

Note that investigators may publish several articles on the same study. Our unit of analysis is the individual evaluation and not the individual research article, and so it is reasonable to extract information from all documents to complete the coding instrument for one experiment. When reports on the same study contain conflicting information, we will employ a number of strategies, including contacting the original investigator(s) for resolution.

### **Criteria for determination of independent findings**

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

### **Details of study coding categories**

To ensure that we achieve good coding reliability, we will have two of the co-authors read and record information from all reports. This should not be difficult given that our anticipated yield is likely going to be less than 20 studies. We will assess 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. This will avoid inflating reliability measures with study characteristics that generally achieve perfect agreement (e.g. year of publication) with those that do not. Items with lower rates of agreement (less than 80%) will be investigated to determine the source for conflict. The authors will meet to resolve disagreements and discussing coded items. We will drop those items from our database in which resolution could not be reached, as well as items that lack clear interpretation.

### **Statistical procedures and conventions**

The data will be entered into the Comprehensive Meta-Analysis (CMA), version 2. We will use CMA to statistically combine results from the evaluations, if appropriate to do so.

If quantitative analysis is appropriate, we will compute odds ratios as the most common type of outcome in evaluation reports are prevalence rates, usually expressed dichotomously such as failure or success (e.g., percentage of each group dropping out or staying in school). A forest plot will be used to display the results from the odds ratios. Our first analysis, therefore, will be an overall display of the odds ratios—using the success/failure rates from the studies—in the form of a forest plot.

We will assume random effects models, which tends to be more conservative than the fixed effects approach, in weighting intervention effects across the randomized trials to compare outcomes. Given the paucity of follow-up data in many studies, however, it is likely that we will report on the “first-effect” only. Given that we do not expect to find a large number of eligible studies, moderator analyses are not anticipated. A small number of total studies could lead us to reject a potentially important moderator because of insufficient statistical power. We will also examine, even qualitatively, a comparison of RCTs versus QEDs, and published versus unpublished reports.

Our final report will also include a detailed narrative review of each included evaluation study. This narrative treatment will then be followed by the statistical analysis and forest plot.

### **Treatment of qualitative research**

We will use qualitative data, if reported, to provide information on context. Of special interest to us is any information on the specific education conditions in the country and region in which the study was conducted. We will use these qualitative data in conjunction with any quantitative data, including data on education, behavioral, or other indicators that could shed light on the research context. We will do this as we describe the findings in each study in narrative fashion first, before we describe our quantitative analyses and results.

### **Treatment of economic data**

We will report on any economic data included in the primary studies that are included in the review. This includes information on the costs of the program, any analysis of the cost-effectiveness of the intervention (e.g., the cost per child enrolled) and cost-benefit studies (e.g., the sum costs and benefits of the program). It is important that this information be linked in some way to the primary outcome studies so that it can be retrieved.

## **4. Study Team**

**Anthony Petrosino**, Ph.D., is Senior Research Associate at Learning Innovations at WestEd, and Associate Director of Research for the Regional Education Laboratory, Northeast and Islands. Anthony has worked on a number of projects during the past 20 years to identify, retrieve, appraise, analyze and report on separate but similar studies. For example, he was one of the founding members of the Campbell Collaboration, assisting in the development of its first trials register (C2-Spectr), co-authoring its pilot review (on "Scared Straight" and other juvenile awareness programs), and serving as Founding Coordinator for its Crime and Justice Group. A version of the "Scared Straight" review received the prestigious Pro Humanitate Literary Award from the Center for Child Welfare Policy of the North American Resource Center for Child Welfare. Although most of his training and experience has been in the justice area, he has more recently been working in education, and has co-authored government reports on the school dropout issue and the use of interim assessment in low-performing schools in Massachusetts.

**Claire Morgan**, M.A., is a Research Associate at Learning Innovations at WestEd. She brings rich experience and sensitivity to issues facing developing nations. Morgan has lived and worked in Mexico, Central America, and the South Pacific, and has considerable experience conducting research in international issues and among marginalized populations, including a study of the education and work experiences of Tongan immigrants, action research around non-formal education of Latino immigrants, and current work on English language learners, education policy issues in Puerto Rico, and the achievement of Hispanic immigrant students in the U.S. Virgin Islands. Morgan serves as the lead researcher for Puerto Rico and the U.S. Virgin Islands for the U.S. Department of Education-funded Regional Educational Laboratory (REL) Northeast & Islands. In this capacity, she consults with the Departments of Education of Puerto Rico and the U.S. Virgin Islands and provides research and technical assistance to address their education policy priorities. Morgan's other research and evaluation work includes evaluation of National Science Foundation (NSF) university-school partnerships, evaluations of the federally-mandated Supplemental Educational Services (SES) program for the Massachusetts Department of Education and of the English to Speakers of Other Languages (ESOL) program for Prince Georges County, Maryland, and an evaluation of a language-minority community schools initiative in Quebec, Canada. Prior to joining WestEd, Morgan, who is fluent in Spanish, developed and

directed a community-based adult education program for Nuestra Casa, a nonprofit organization serving Latino immigrants in Northern California. This initiative, a partnership with an underserved school district serving a large Hispanic student population, increased parent participation in schools and contributed to community development. In addition, she completed an internship at International Development Exchange (IDEX) in San Francisco, providing support to micro-ventures in Latin America. Morgan is currently directing the expansion of a nonprofit Latina women's work cooperative that she co-founded in 2006. She received an MA in International Education Administration and Policy Analysis from Stanford University. She is particularly skilled in designing and conducting quantitative and qualitative research and evaluation, including instrument development and coding, literature reviews, interviews and observations, and analyzing demographic and achievement data.

**Robert Boruch**, Ph.D., is University Trustee Chair Professor of Education and Professor of Statistics (Wharton School) at the University of Pennsylvania. He has also served as faculty in the Fels Center for Government and at the Annenberg School Statistical Institutes. He is principal investigator for the What Works Clearinghouse (US Department of Education) and co-chairs the Steering Group of the international Campbell Collaboration. Dr. Boruch is a leading expert in experimental design research methods and related science policy on estimating effects of interventions. He has advised governments, private foundations, and research firms on randomized field trials in education, criminal justice, employment and training, and social welfare in the US and in other countries. His earliest contributions, during the 1970s, included service as advisor on the Cali Colombia randomized trials on cultural enrichment programs and the Nicaraguan trials on radio based mathematics education. Boruch chaired the National Academy of Sciences Committee on Evaluation of AIDS prevention programs in the 1980s and contributed to the WHO committee on the topic during the same period. He has authored numerous books and peer reviewed articles on related subjects. The most recent products include Evidence Matters: Randomized Trials in Education Research (2001), edited by Mosteller and Boruch (Brookings Institution Press), and a special edition of the *Annals of the American Academy of Political and Social Sciences* on place randomized trials in developed and developing countries (May 2005, volume 599) which covers health, crime and justice, welfare, housing, and education. Boruch has been leader in institutes on generating better evidence for the US National Academy of Sciences and the Israel Academy of Sciences and Humanities, the Campbell Collaboration, workshops/seminars for the World Bank' IPDET, and in other venues. Boruch is an elected Fellow of the American Statistical Association, the Academy of Experimental Criminology, and the American Academy of Arts and Sciences, and is a Lifetime Associate member of the National Academy of Sciences (US). He has received awards for the work from the American Evaluation Association (Myrdal Award), American Educational Research Association, the Campbell Collaboration, and the Policy Studies Organization.

## 5. Timeframe

Task to be Completed	Target Date for Completion
Search for published and unpublished studies	June 2009
Data extraction from research reports	July 2009
Statistical analyses	August 2009
Preparation of draft final review	September 2009

## **6. Plans for updating the review**

We plan to update this review in 36 months, in concert with C2 guidelines.

## **7. Acknowledgments**

We would like to thank International Initiative for Impact Evaluation (3ei) of the Global Development Network (GDN) for their support of this project.

We thank the following persons for their helpful comments on this protocol (in alphabetical order): Mary Cazabon, Sarah Guckenbug, Sue Henderson, Daniel Mello, Eliza Spang, Hugh Waddington and Howard White.

## **8. Statement concerning conflict of interest**

We do not have any conflicts 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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