

A systematic review of the evidence of the impact of eliminating school user fees in low-income developing countries



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List of abbreviations

3ie	International Initiative for Impact Evaluation
APHRC	African Population and Health Research Center (Kenya)
BRAC	Bangladesh Rural Advancement Committee
CCT	conditional cash transfer
CSP	Child Sponsorship Programme (ICS, Kenya)
DFID	Department for International Development (UK)
DHS	demographic and health survey
DSS	Demographic Surveillance System (Kenya)
EA	enumeration area
ERIC	Education Resources Information Center (USA)
ERP	Education Research Project (Kenya)
FFE	Food For Education (Bangladesh)
Fmg	Franc malagache (Madagascar)
FPE	free primary education
FSSAP	Female Secondary School Assistance (Bangladesh)
GDP	gross domestic product
GER	gross enrolment ratio
GNI	gross national income
HIV/AIDS	human immunodeficiency virus/acquired immune deficiency syndrome
HUD USER	Housing and Urban Development (US)
ICS	International Child Support
IDEAS	Internet Documents in Economics Access Service, Department of Economics, University of Connecticut (US)
IV	instrumental variables
JOLIS	Joint Bank-Fund Library Network
Ksh	Kenya shilling
LPM	linear probability model
MANOVA	multivariate analysis of variance
MIS	Management Information System (World Bank)
MLM	multi-level modelling
NCCAN	National Clearinghouse of Child Abuse and Neglect (US)
NGO	non-governmental organisation
OLS	ordinary least squares (regression)
PAIS	Public Affairs Information Services
PEDP	Primary Education Development Program (Tanzania)
PSM	propensity score matching
PTA	parent-teacher association
QED	quasi-experimental design
QMLE	quasi-maximum likelihood estimator (model)
RCT	randomised controlled trial
RDD	regression discontinuity design
REPEAT	Research on Poverty, Environment, and Agricultural Technology (Uganda)
RMANOVA	repeated measures analysis of variance
SES	socio-economic status
SMC	school management committee (Kenya)
SSSP	Secondary School Scholarship Program (Bangladesh)
UIHS	Uganda Integrated Household Survey
UNESCO	United Nations Educational, Scientific and Cultural Organization
UNHS	Uganda National Household Survey
UNICEF	United Nations Children's Fund
UPE	universal primary education
USAID	United States Agency for International Development

Abstract

Background

Low educational attainment in the developing world can be attributed in part to the private costs associated with sending children to public school (e.g. Bentaouet-Kattan and Burnett 2004, Filmer and Pritchett 1998). These household costs include lost work opportunities for children who attend school as well as direct costs in the form of school fees. Direct school user fees paid by households are common in developing countries and represent a percentage of all primary education costs ranging from 8 percent in Indonesia to 80 percent in Cambodia (Bentaouet-Kattan and Burnett 2004). Eighty-three percent of World Bank client countries surveyed in 2005 assessed user fees (Bentaouet-Kattan 2006). These costs, which include fees for books and uniforms, community and PTA (parent-teacher association) contributions, exam fees and tuition represent a large percentage of total household spending and are particularly burdensome for those families that face tough choices about which children to send to school and for how long (World Bank 2009a).

School fees abolition policies are supported by many development organisations and aid agencies and have been spurred worldwide by initiatives such as Education for All, the Millennium Development Goals, and the School Fee Abolition Initiative. Promises to abolish school fees are often politically motivated and featured in election campaigns, and may also be part of a wider policy reform, often including educational decentralisation (e.g. Kremer et al. 2003, Maikish 2010). School fees may be abolished through a 'big bang' approach, officially eliminating tuition fees (although other user fees often continue to be assessed at the school/community level), through a phasing-in approach by region or grade/age level, or through targeted exemptions aimed at vulnerable groups. Although surges in enrolment following the abolition of school fees in many developing countries have been demonstrated, there is little empirical evidence available to date on the impact of eliminating school fees on educational outcomes in developing nations (Grogan 2006).

We conducted a systematic review of studies of interventions in low-income developing countries that evaluated the elimination of school user fees paid by households - including the five fee categories identified by the World Bank (tuition, uniforms, textbooks, PTA contributions, other materials/activities).

Methods

Through extensive searching, including electronic keyword searches of bibliographic databases, handsearches of relevant journals, examinations of online holdings of international development organisations and research firms, citation chasing, examining grey literature, and contacting experts in the field, we identified studies that responded to the following question: *What is the evidence of the impact in the medium-longer term of the abolition of school fees in low-income developing countries?* Eligible studies had to meet the following criteria: The evaluation took place in a low-income developing nation as defined by the World Bank at the time of the intervention; the evaluation assessed the impact of eliminating primary or secondary public or private school fees. With the intent to conduct meta-analysis, we focused on identifying randomised controlled trials (RCTs) or quasi-experimental evaluations (QEDs) with some evidence that the groups being compared are equivalent. In addition, we examined for contextual information - but not for inclusion in effect size estimates -quasi-experiments

without pretest group equivalency, descriptive quantitative studies, and qualitative studies that shed light on implementation and context issues.

Each RCT or QED located in the search that appeared to be a possibility for inclusion was carefully reviewed by two authors and a structured abstract was prepared for each study, detailing the context, methodology and findings. For each study deemed eligible for inclusion following this screening process, a coding instrument was completed that included items in the following areas: Researcher and study characteristics, study methods and methodological quality, intervention and control conditions data, participants in the study, and outcome data. To evaluate study quality, we recorded details on three key implementation issues: How the groups were equated and whether any problems with equating were reported, information on attrition, and whether the programme experienced significant implementation or fidelity problems.

Results

We identified five rigorous experimental and quasi-experimental evaluations, as well as 31 quantitative and qualitative studies that did not meet our criteria for inclusion in effect size estimates but were examined to map the extent, types and quality of the evidence base in the topic area and to shed light on possible theory, implementation and context issues. Given the small number of studies that met our inclusion criteria and the variation amongst the studies, we elected to provide the results in a narrative fashion, rather than through meta-analysis.

Each of the five included interventions took place in sub-Saharan Africa. Three evaluations took place in Kenya, which abolished primary school tuition fees at the universal level in 2003, leaving households responsible for providing uniforms. The three Kenya studies (Duflo et al. [2006], Evans et al. [2009], Kremer et al. [2003]) evaluated programmes that provided free uniforms to schoolchildren. Grogan (2009) evaluated the impact of free universal primary education (UPE) implemented in Uganda in 1997, where parents had prior to the new law provided up to 90 percent of school expenditures. Finally, Baird et al. (2009) evaluated a secondary school fees elimination programme in Malawi. Outcomes reported across the evaluations included impacts on duration of enrolment, drop-out, grade advancement, re-enrolment, absenteeism, age at school entry and marriage and childbearing. All of the studies reported positive outcomes in one or more of these areas for the children involved. The 25 primary studies that evaluated school fees elimination interventions through descriptive quantitative (including quasi-experiments without equating of groups) and qualitative approaches highlight important issues about context, theory and implementation. The majority of these studies coincide with the findings from the five rigorous studies, reporting that school fees elimination interventions increased educational access, equity and student persistence. However, declines in educational quality were also reported in many of the studies.

Conclusions

The findings of this systematic review highlight the need for more rigorous empirical research to investigate the effects of various types of school fees elimination policies in low-income developing nations - particularly on the effectiveness of targeting policies to the most vulnerable groups - accompanying trade offs in education quality, and the extent to which fees abolition policies can be sustained over time without undue donor dependency. Research in this area is complicated by the fact that many countries have already implemented universal

free school tuition policies for all primary children, so an appropriate control group is difficult to identify and include in an evaluation. One possible solution to this challenge may involve utilising an interrupted time series design involving a single group (e.g. Bloom 2003). In addition, because UPE policies often do not eliminate all household contributions to schooling, additional impact evaluations of non-governmental or government-supported programmes targeting fees elimination for specific groups can shed more light on the true costs of education for households and the degree to which eliminating these costs can improve schooling and other outcomes for the most vulnerable groups. Longitudinal studies are needed to elucidate the longer-term impacts of fees elimination, including whether initial surges in enrolment are sustained over time and what the policies mean for future educational attainment, employment and other outcomes.

Research to determine the full household costs of education (including opportunity costs for boys and girls) for different socio-economic groups is key, as well as determining how much of the full cost of education households are willing and able to bear, given an acceptable level of education quality. Experimentation with different innovations, such as user fees on a sliding scale based on household ability to pay would be informative and could be researched empirically. Studies such as these can provide valuable information to countries that are considering abolishing school fees and can inform strategies for advance planning and targeting of reforms, including planning for efficient allocation of resources at the local level.

1. Introduction

1.1 Background

It is well established that education is a key to economic development and social welfare. Investments in education can yield returns in poverty reduction, improved health outcomes, and economic growth (Hannum and Buchmann 2004, Herz and Sperling 2003, UNESCO 2007). In addition, increased access to education can contribute to greater political participation and more equitable sharing of power (Birdsall 1999). Education for girls is viewed as particularly critical, given that research has shown that improvements in infant mortality rate, child nutrition and school enrolment are closely associated with maternal education (Birdsall et al. 2005, Herz and Sperling 2003, World Bank 2008). Yet, in 2008, 67 million children were out of school (UNESCO 2011). And in sub-Saharan Africa alone, 10 million children drop out of primary school each year (UNESCO 2011).

Low educational attainment in the developing world can be attributed in part to the private costs associated with sending children to public school (e.g. Bentaouet-Kattan and Burnett 2004, Filmer and Pritchett 1998). These household costs include lost work opportunities for children who attend school as well as direct costs in the form of school fees. Direct school user fees paid by households are common in developing countries and represent a percentage of all primary education costs ranging from 8 percent in Indonesia to 80 percent in Cambodia (Bentaouet-Kattan and Burnett 2004). Eighty-three percent of World Bank client countries surveyed in 2005 assessed fees for public schooling (Bentaouet-Kattan 2006). These costs, which include fees for books and uniforms, community and PTA (parent-teacher association) contributions, exam fees, and, less commonly, tuition, represent a large percentage of total household spending and are particularly burdensome for those families that face tough choices about which children to send to school and for how long (World Bank 2009a). School fees can be seen as a form of regressive taxation that disproportionately burdens the poor, including the most vulnerable, such as girls, child labourers and children living in remote areas (Bentaouet-Kattan 2006, World Bank 2009a).

1.2 School fees elimination interventions

During the independence movements of the 1960s, many developing nations made investments in free basic education to build capacity for more equitable involvement in economic growth and political participation, but in many cases these policies were abandoned during the 1980s, as governments found it increasingly difficult to provide for expanding school systems during times of low growth (World Bank 2009b). Currently, school fees abolition policies are supported by many development organisations and aid agencies and have been spurred worldwide by initiatives such as Education for All, the Millennium Development Goals, and the School Fee Abolition Initiative launched by UNICEF (United Nations Children's Fund) and the World Bank in 2005.

Promises to abolish school fees are often politically motivated and featured in election campaigns, and may be part of a wider policy reform, often including educational decentralisation (e.g. Kremer et al. 2003, Maikish 2010). Primary school fees are most commonly targeted for elimination and school fees abolition policies may or may not include private schools. School fees may be abolished through a 'big bang' approach, officially eliminating tuition fees (although other user fees often continue to be assessed at the school/community level), or through

a phasing-in approach by region or grade/age level, or through targeted exemptions aimed at vulnerable groups.

Several countries have experienced large increases in enrolment after abolishing school tuition fees. For example, in Kenya, primary school enrolments increased from 5.9 million in 2002 to 7.12 million in 2004; Timor Leste saw a 9.5 percent increase between 1999 and 2001 (Bentaouet-Kattan 2006 in World Bank 2009a). In the year following 'big-bang' fees abolition, Malawi and Uganda experienced enrolment increases of 51 percent and 68 percent respectively (World Bank 2009b). Tanzania saw a 33 percent increase using a phased-in approach (Bentaouet-Kattan 2006).

Although proponents claim that the abolition of user fees can reduce disparities and improve educational equity (e.g. Bentaouet-Kattan 2006), dramatic increases in enrolment may be accompanied by trade offs in educational quality (World Bank 2009b). In addition, abolishing school fees alone is not enough to extend access to the poorest and most marginalised groups facing barriers such as high opportunity or transportation costs, poor health and social conflict (Bentaouet-Kattan 2006, World Bank 2009a).

1.3 Prior research on school fees

The literature firmly establishes that user fees - which often constitute a large portion of overall education expenditures - inhibit school enrolment and persistence in developing countries (Bentaouet-Kattan 2006). However, precise information on the types and extent of user fees, which are often unofficially assessed and sporadically reported, is difficult to obtain. In 2001 and 2005, the World Bank conducted surveys of World Bank education task team leaders for 93 countries on the prevalence of school fees in their countries and found that 77 of the countries surveyed charged school fees (Bentaouet-Kattan 2006, Bentaouet-Kattan and Burnett 2004). The survey further found that five main types of fees are collected: Tuition fees, textbook charges, uniform fees, PTA/community contributions and other school-based activity fees. Of these fees, the most commonly collected were PTA/community contributions, which are also the most difficult to track, as they are often collected unofficially (Bentaouet-Kattan 2006). In addition, the survey found school fees to be a significant barrier to the transition of students from primary to secondary school, as secondary school fees are more prevalent and substantial than those assessed at the primary levels. However, the survey also identified a growing trend toward the elimination of school fees. For example, between 1994 and 1999, only three countries had implemented this policy, whilst between 2001 and 2005, 13 countries abolished school fees (at least in official policy) (Bentaouet-Kattan 2006).

Although surges in enrolment following the abolition of school fees in many developing countries have been demonstrated, as described above, relying on gross or net enrolment data provides a very limited picture of the impact of this policy. There is little empirical evidence available to date on the impact of eliminating school fees on educational outcomes in developing nations (Grogan 2006). Using national survey data and employing regression discontinuity¹ and difference-in-difference estimation techniques, Grogan (2006) conducted the first quantitative

¹ Regression discontinuity design (RDD) is a quasi-experimental design that can be used whenever assignment to treatment is made solely on the basis of a score. Because assignment can then be completely controlled, RDD is believed to be a powerful quasi-experimental alternative to randomized experiments, especially in situations in which randomization is not possible.

analysis of the effects of introducing free universal primary education (UPE) policy in an African country. The study found that the elimination of school fees increased the probability that an individual attended school before age 8 by about 10 percent, with similar findings for both boys and girls. However, the policy was also found to cause a 10 percent decrease in the probability that a publicly schooled child of a given age and socio-economic characteristics was able to complete a simple reading test.

Some research indicates that little of the central government education budget actually gets to the schools in some developing nations, sometimes due to corruption (e.g. Reinikka and Svensson 2007). Thus, some advocate for school fees to be charged at the local level both to supplement teachers' salaries and to purchase school equipment (e.g. Grogan 2006). One concern expressed in nations that have eliminated school fees is whether the resources will be available to support teacher payroll and other needs (e.g. Kenya 2008, OWN and Associates 2004).

Access to education by the poor and by other vulnerable groups, such as girls and orphans, has increased in some school fees elimination cases (e.g. Grogan 2009, Nicola 2010). However, rapid surges in enrolment can result in decreased educational quality. The extent to which this is the case may depend upon the extent of planning prior to fees elimination and the strategies put in place to cope with the surge in enrolment. For example, Ghana used a combination of measures, including training additional teachers and ensuring the provision of additional textbooks, and saw marked improvement in test scores (World Bank 2009b). In Malawi, on the other hand, massive growth in enrolment outstripped resources and led to a serious decline in school quality (World Bank 2009b).

To our knowledge, a systematic review of similar studies evaluating the impact of school fees abolition policies (including interventions such as establishing free UPE, eliminating textbook or uniform fees, or targeted fees exemptions for marginalised groups) has not been conducted.

1.4 Objectives

In this paper, we report on a systematic review of primary studies of interventions in low-income developing countries that evaluated the elimination of school user fees paid by households - including the five fee categories identified by the World Bank (tuition, uniforms, textbooks, PTA contributions, other materials/activities). These interventions may be carried out at the national level and provide for free UPE, or may be carried out by NGOs (non-governmental organisations) targeting specific regions, schools, or vulnerable groups. These interventions include programmes that provide fellowships or scholarships, provided they are explicitly intended to eliminate user fees for households.²

Through extensive searching - including handsearches, examining grey literature, and contacting experts in the field - and coding of the evaluative studies, we assess the breadth and quality of the literature base and synthesise the available empirical evidence of the effect of eliminating school fees in the five categories mentioned above. Outcomes coded include impacts on primary and secondary

² Conditional cash transfers (CCTs) – payments made to households conditional on students being enrolled in school – represent an intervention with goals and outcomes that overlap with school fees elimination. Comparing the effectiveness of CCTs to school fee elimination, while beyond the scope of the current review, is an interesting avenue for further research which will be partially explored through a systematic review funded by 3ie that we are currently conducting which is examining school enrolment interventions in developing nations.

school enrolment, gender parity in enrolment, drop-out, achievement and educational quality indicators.

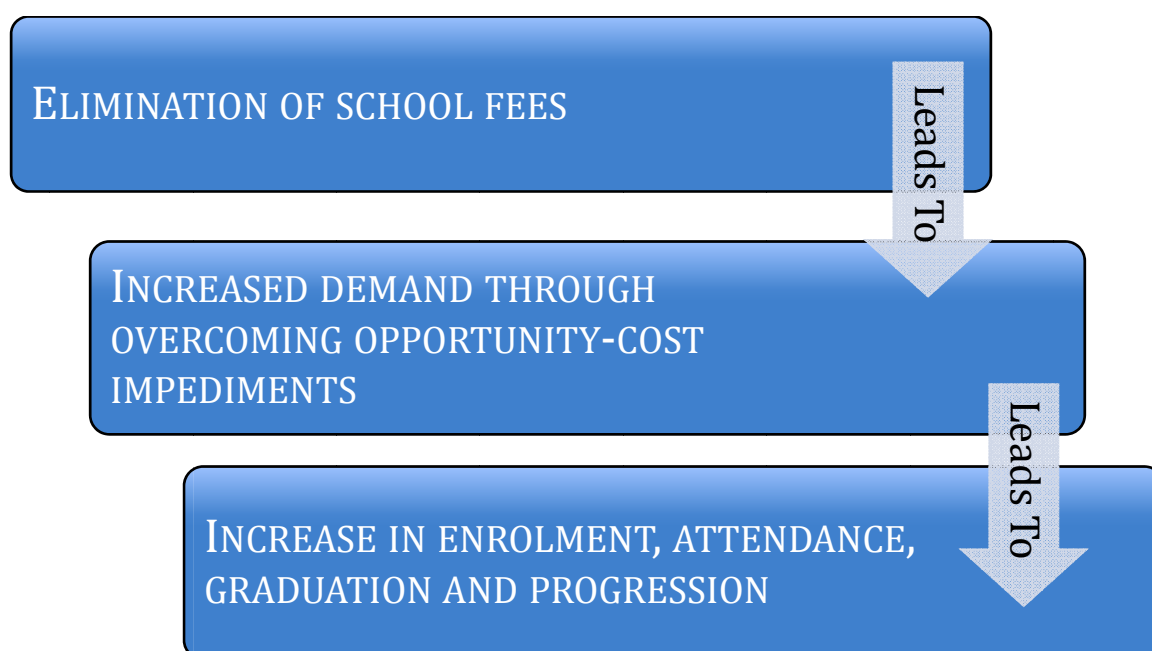
1.5 Conceptual framework

Costs, benefits, social norms and household characteristics all affect enrolment decisions. Households' schooling choices depend in large part on the costs and the perceived value of education. Besides the direct costs associated with schooling (e.g. tuition, books, uniforms), households may value present contributions through child labour and other household contributions (e.g. preparing food, tending animals/family members) more highly than the child's future earnings. And future earning potential may not be perceived to be significantly increased by education attainment, perhaps due to lack of information, limited social mobility or unavailable job markets.

Our conceptual framework is informed by demand-side economics of education financing, which establishes the negative price elasticity of demand for children's education (i.e. a change in price has a large impact on demand), particularly that of girls, in poor households in low-income countries (e.g. Birdsall and Orivel 1996, Gertler and Glewwe 1990, cited in Hillman and Jenker 2002). Although school fees can remove supply-side limitations, they exclude those households most unwilling or unable to pay the fees, due to demand-side constraints mentioned above such as opportunity costs of lost child labour, household contributions of children, low expectations by parents of returns to investing in education for their children, unavailable credit markets for financing education, and social norms that discourage school participation (Hillman and Jenker 2002).

Figure 1.1, below, displays the conceptual framework guiding our review. Simply put, we identify and code studies that test the hypothesis displayed in Figure 1.1 that reducing/eliminating user fees will increase demand for education, as demonstrated by increased enrolment and persistence, as well as decreased drop-out.

Figure 1.1 Conceptual framework



It is important to identify and understand the circumstances under which this hypothesis holds true, as well as those cases or particular groups for which it does not. Moderators affecting the impact of a fees elimination scheme may include school availability, cultural norms, gender, poverty level and perceived returns to education/opportunity costs. Additionally, unintended consequences that may accompany fees reduction, such as overcrowding or reduced educational quality, may affect the long-term impact of the intervention. Longitudinal studies can shed light on whether initial impacts are sustained over time.

2. Methods

2.1 Identifying and describing studies

For assessing the effectiveness of school fees elimination interventions, we only included studies with the following characteristics:

(1) *The evaluation took place in a country classified as a low-income economy by the World Bank (GNI [gross national income] of US\$995 or less) at the time the intervention being studied was implemented.*

(2) *The evaluation assessed the impact of eliminating primary or secondary public or private school fees (including tuition fees, fees for textbooks and other instructional materials, compulsory uniforms, PTA dues, community contributions to district education boards, and other school-based activity fees [exam fees, etc.]) whether at the universal level or the community/group, district, school or grade/age level. Thus, studies that eliminated fees for a specific school or district (and not the country) were considered eligible (e.g. Evans et al. 2009). We collected all outcome measures reported in the evaluations, such as impact on enrolment, persistence, achievement and school quality, and any cost-benefit information provided.³*

(3) *The evaluation study report was published or available by October 2010, without regard to language or publication type. We attempted to find English and non-English studies. In addition, we searched for published and unpublished studies (e.g. conference papers, dissertations, technical reports).*

(4) *Randomised controlled trials (RCTs) or quasi-experimental evaluations (QEDs) with some evidence that the groups being compared are equivalent. Our review includes evaluations that randomly assign entities (at any level) to intervention or control conditions. However, because randomisation is not possible in certain evaluation scenarios (e.g. universal fees elimination across an entire nation simultaneously), we also included evaluative studies that use quasi-experimental designs, provided that they offered evidence that the comparison groups were equated. Such quasi-experiments include regression discontinuity design (RDD), propensity scores, covariate matching, and other pre- or post-intervention matching. However, we did not consider studies in which a sample of persons in a nation before an intervention was matched to a sample in the same nation; although matching was used, it was not possible in these studies to control for the temporal effect (i.e. maturation, other historical factors, etc.).*

In addition, we examined for contextual background, but not for inclusion in effect size estimates:

(5) *Non-experimental studies and quasi-experiments without pretest group equivalency (e.g. before and after studies, such as those discussed above that looked at national rates).*

(6) *Descriptive quantitative studies.*

³ We did not include studies of vouchers, as the goal of voucher programmes is generally to increase school choice and school quality for children already enrolled by extending access to private schools and introducing competition among schools, while the goal of fees elimination programmes is generally to extend access to children of households which could otherwise not afford enrolment costs. However, we are conducting a systematic review of the impact of vouchers for a separate DFID project. See <http://www.dfid.gov.uk/R4D/Output/189456/Default.aspx>.

(7) Qualitative studies that shed light on possible theory, implementation and context issues.

2.2 Search strategy

Our goal was to identify both published and grey literature. Many of the databases in (1) below include grey literature (e.g. ERIC). The British Library indexes conference proceedings and makes these available in its 'Integrated Catalogue'. Internet searches will often identify literature that is made available on websites but not published in journals. Our contact with colleagues was designed to obtain more of the grey literature. To accomplish our goal, we used five major strategies:

(1) *Electronic searches of bibliographic databases.* We used available online resources and databases available at the University of Pennsylvania, including ERIC, PAIS International/Archive, Sociological Abstracts and World Bank documents (see Appendix 2.1 for a full list of databases searched), as well as broader searches of the internet. It is important to note that we conducted new searches for this DFID (UK Department for International Development) project, but also had the benefit of relying on completed searches for experiments and quasi-experiments for a larger project on school enrolment funded by 3ie.⁴

(2) *Handsearches of relevant journals.* Because electronic searches often miss relevant studies, we handsearched the tables of contents, and the abstracts when necessary, of all issues of five journals that publish studies relevant to this topic: *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 five journals were identified as the ones that published the most experimental and quasi-experimental research relevant to developing nations and education, from our larger review on school enrolment funded by 3ie.

(3) *Specific examinations of online holdings of international development organisations and research firms.* This included international agencies that either conduct or would be aware of possibly relevant evaluations in developing nations, including DFID and the United States Agency for International Development (USAID) (see Appendix 2.1 for list of organisations searched).

(4) *Citation chasing.* We checked the reference section of every retrieved report to determine whether any possible eligible evaluations were listed.

(5) *Contacting the 'informal college' of researchers in this area.* We contacted several researchers who are conducting or are aware of experimental and quasi-experimental studies relevant to education issues in developing nations so that they could alert us to any missed studies (see Appendix 2.2 for a list of persons contacted). These persons were lead or contributing authors to experimental or quasi-experimental research in the developing nation and education area, identified from our overarching review of school enrolment funded by 3ie. We choose not to reveal who responded as this might be embarrassing to our colleagues who may have been busy, on vacation, travelling, or otherwise occupied with other matters and unable to respond.

⁴ Unfortunately, the Campbell Collaboration Social, Psychological, Criminological, Education Trials Register was not functioning at the time of these searches. We did search the Cochrane Library for the 3ie project and did not find many eligible education studies in developing nations of any intervention type.

The searches were undertaken in two stages. First, we relied on searches completed for the larger 3ie project on school enrolment; that review includes experimental and quasi-experimental studies that were conducted in developing nations and includes school enrolment or participation outcomes. Second, a more focused search described below (and in Appendices 2.1 and 2.3) for studies relevant to school fees was undertaken. Although searches for both studies have been largely done by one researcher, decisions on the eligibility of each study were undertaken by the lead author (Morgan) and one co-author (Petrosino). No disagreements about the eligibility of studies were noted.

Using specific keywords to identify studies conducted between 1990 and 2010 in low-income developing countries that evaluate interventions involving the elimination of school fees, we searched 28 databases, websites, and journals (see Appendices 2.1 and 2.3 for keywords and sources searched). Keywords were selected based on a review of relevant school fees studies identified during the 3ie project (e.g. looking at the title and abstract to see what terms were used to identify school fees) and an examination of overarching review and overview papers in the area of school fees in developing nations. We reviewed all citations to determine if the cited study should proceed to a second screening, i.e. was a potentially relevant study. The full text documents of potentially eligible studies were retrieved and screened and entered into EPPI-Reviewer 4.0.

2.3 Coding

For each included study, we completed a coding instrument (Appendix 2.4) comprised of items in the following areas:

(1) *Researcher and study characteristics*: We extracted data about the type of publication the study was reported in and the setting and context in which the trial was conducted.

(2) *Study methods*: We extracted information about the randomisation, quasi-experimental assignment, and other methodological aspects of the evaluation. The level of assignment and whether the study included multiple analyses at different levels was also coded. In addition, we recorded details on the three key implementation issues:

- *How the groups were equated and whether any problems with equating were reported*. The integrity of a randomised experiment or a quasi-experiment largely rests on how faithfully the equating procedures were implemented. We recorded information about randomisation or the quasi-experimental matching or equating procedures that were used in the study. In randomised experiments, this includes how much of the originally randomly assigned sample actually received the treatment (slippage from the 'intention to treat' sample).
- *Whether the researchers reported a loss of participants from the initial assigned sample at the end of the study, how much attrition was reported, and whether the attrition differentially affected one group or the other*. Such attrition, if it is significant, can compromise the equating of groups, particularly if different types of people drop out from the intervention group than from other groups. We recorded specific information on the amount of attrition (if it occurred) and whether it was differential in nature.
- *Whether the programme experienced significant implementation and fidelity problems*. The first two issues deal with the implementation of the

evaluation. This issue deals with the implementation of the programme; there may be no observable programme impact because no 'real programme' was ever implemented. We recorded in descriptive and qualitative form any implementation problems noted by the investigators.

(3) Methodological quality: For reports of RCTs and quasi-experimental studies we examined and recorded the information described above to make a determination about methodological quality. For each study, we captured information about any issues with crossovers (persons receiving condition they were not assigned to), selection bias (e.g. breakdowns in randomisation or unusual unequal distributions in groups), loss of participants due to attrition or database matching issues, and intervention fidelity and implementation issues. We also rated each study according to our perception of whether the problems presented a threat to the findings reported in the study. These ratings were categorised as 'low', 'moderate', or 'high'. If there were no indicated problems, the threat to the study was rated as 'none'. It should be noted that these ratings are subjective, and they are based entirely on what is reported in the study documents. However, these ratings had good reliability across the three authors in our check of inter-rater reliability.

(4) Intervention and control conditions data: These items solicited detailed descriptions of the intervention and control condition, including the 'dosage' of the treatment being implemented, and the number of participants assigned to each group. In cases in which more than one treatment and control group was present, we selected the groups that experienced the greatest contrast between conditions, i.e. the most intensive intervention condition versus the least intensive control condition. We documented these decisions for full transparency.

(5) Participants in the study: These items solicited detail about the type of participants in the trials, including information on the country in which the study took place, the age, school level and gender targeted, whether an urban or rural setting was involved, and the socio-economic status (SES) of the students.

(6) Outcome data: We extracted information on reported outcomes including impacts on access to education, persistence, learning and equity. We also coded economic data, where present, and any other outputs or data on key 'mechanisms' that provided clues as to why the intervention did or did not have its intended impact. We also coded the time intervals of the various outcomes and subgroup effects.

It should be noted that the five impact studies included here are also part of the larger 3ie project on school enrolment. In that study, coding reliability was assessed with three co-authors independently coding 10 different eligible studies. Reliability was high in terms of factual accuracy across nearly all items; the major difference in comparing the three responses was the level of detail in responding to open-ended items.

2.4 Reviewing descriptive studies

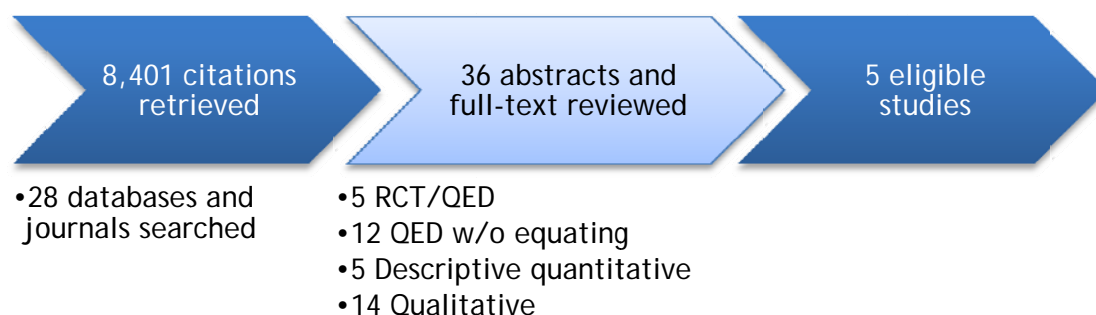
For descriptive studies to be reviewed for contextual information, they had to evaluate a fees elimination intervention and to include sufficient methodological detail as to be replicable. For the 17 quantitative (QEDs without evidence of equating and descriptive quantitative) reports identified in the searches, we completed structured cases (see Appendix 2.5), rather than the full coding document used for impact evaluations. These cases describe in narrative form important information from the descriptive studies including background/rationale,

setting/participants, intervention type/methodology, data collected/analyses conducted, key outcomes and practice/policy implications.

2.5 Results

As illustrated below in Figure 2.1, the 28 databases, along with searches of websites and journals, yielded 8,401 potentially relevant citations and abstracts (including duplicates). Most of these were eliminated after careful screening of the abstracts or full text. Reasons for elimination included not being evaluative studies of school fees elimination interventions and not being conducted in a lower-income country. We identified five rigorous experimental and quasi-experimental studies, as well as 12 quasi-experimental designs without equating of groups and 19 descriptive quantitative and qualitative studies. The quasi-experimental and descriptive studies did not meet our criteria for inclusion in effect size estimates but were examined to map the extent, types and quality of the evidence base in the topic area and to shed light on possible theory, implementation and context issues. Although we intended to quantitatively synthesise the results from the impact evaluations in a meta-analysis, given the small number of studies that met our inclusion criteria and the variation amongst the studies, we elected to provide the results in a narrative fashion. Given the limited number of rigorous primary studies, we should be very careful about generalising results from this systematic review.

Figure 2.1 Retrieval and screening results



3. Included impact studies

This section summarises the types, geographical locations, methodologies and reported outcomes of the interventions (see Table 3.1) before providing a more detailed description of each included empirical study. As described earlier, we searched for evaluative studies of interventions in low-income developing countries that evaluated the elimination of five types of school fees. As mentioned previously, in many cases primary school fees have been eliminated by governments at a universal level through a 'big bang' approach, the impact of which is very difficult to establish empirically through experimental or quasi-experimental means; this is because it is difficult to identify a valid control group, since everyone in the nation is receiving the 'treatment'. Table 3.2, below, shows the different interventions and the related policy levels (i.e. system level or localised/targeted). Only one included study (Grogan 2009) evaluated free UPE. Baird et al. (2009) evaluated the elimination of tuition fees, but through an NGO intervention targeted to secondary schoolgirls in a particularly poor district in Malawi. The remaining three studies evaluated NGO interventions that eliminated school uniform fees by providing free uniforms to targeted children. Such interventions, because they provide only partial coverage and usually have more demand than supply, lend themselves to evaluation through experimental means.

Each of the interventions took place in sub-Saharan Africa - a low-income developing region that since independence has focused on expanding access to education. Three of the included evaluations took place in Kenya, which abolished primary school tuition fees at the universal level in 2003, leaving households responsible for providing uniforms, which represent a substantial sum relative to per capita GDP (gross domestic product) (Kremer et al. 2003). The three Kenya studies evaluated programmes that provided free uniforms to schoolchildren. As mentioned above, Grogan (2009) evaluated the impact of UPE implemented in Uganda in 1997. Before this, parents in Uganda provided up to 90 percent of school expenditures. In Malawi, where primary education has been free since 1994 but fees are still assessed for secondary school, Baird et al. (2009) evaluated a fees elimination programme. This was the only included study that evaluated a school fees elimination intervention for secondary rather than primary school-age children.

Each of the included evaluations employed an RCT design, with the exception of the Grogan (2009) regression discontinuity study, and each study was determined to be of high methodological quality after assessing equating procedures, attrition and implementation fidelity. That is, we reviewed the study and concluded that the impact of any reported problems in these three methodological areas on the results was either "little" or "none".

More details on the findings of each of these evaluations follow, organised by type of intervention.

Table 3.1 Overview of included studies

Study	Intervention	Methodology	Policy level	Findings
Grogan (2009) Uganda	Free universal primary education	QED	System level	Increased likelihood of entering school before age 9
Baird et al. (2009) Malawi	Payment of school fees + cash transfer	RCT	Localised/targeted	Increased re-enrolment; decreased drop-out
Kremer et al. (2003) Kenya	Free uniforms	RCT	Localised/targeted	Increased persistence and grade advancement
Duflo et al. (2006) Kenya	Free uniforms	RCT	Localised/targeted	Decreased drop-out, teen childbearing, marriage
Evans et al. (2009) Kenya	Free uniforms	RCT	Localised/targeted	Reduced absenteeism; raised test scores

QED: Quasi-experimental evaluation; RCT: randomised controlled trial.

3.1 Free universal primary education

Of the five included evaluations, only Grogan (2009) attempted to evaluate the impact of the implementation of free UPE. She employed a RDD (coupled with difference-in-difference techniques) to estimate the impact of eliminating primary school fees in Uganda on the age at which children enter schooling - an important factor in subsequent attainment. Grogan estimated that school entry at ages above 8 is very strongly associated with early school drop-out in Uganda, and that free UPE had a positive effect of 3 percent on the probability of entering school before age 9. For girls who benefited from the elimination of school tuition fees before their ninth birthday, the probability of entering school before age 9 is 5 percent higher. In addition, the effects appear to be concentrated in rural areas, perhaps because school enrolment in urban areas was much higher than in rural areas prior to free UPE. Whilst demonstrating the positive effects of school tuition fees elimination on the timely enrolment of children in rural areas in Uganda, Grogan also points out that the sudden increase in enrolment in Uganda led to overcrowding and shortages of teachers and textbooks, highlighting the need for studies of the quality of learning outcomes under free UPE and on the impact on resources available at the school level.

3.2 Targeted tuition fees elimination

Although evaluations of conditional cash transfer (CCT) programmes - in which payments are made to households conditional on a child's school attendance - do not fall within the scope of this review (as the payments do not explicitly and directly eliminate school fees), Baird et al.'s (2009) evaluation of a CCT programme to boost secondary schooling amongst girls in Malawi fits our inclusion criteria. This programme experimented with different cash transfer amounts, recipients and delivery models, including a treatment group that received full payment of school fees directly to the schools, in addition to a small household cash transfer and a transfer directly to the girl, conditional upon school attendance. The two-year intervention was targeted to secondary schoolgirls in a particularly poor district in Malawi, and sought to examine the effect of conditionality, as well as the size and recipient of the transfer, on school enrolment. After one year, Baird and colleagues found strong enrolment impacts for the entire sample, and that the results were unresponsive to the size or conditionality of the transfer. The only variation was that impacts on enrolment were stronger when the transfer was made directly to the girl, but this was only significant when the transfer was conditional upon school attendance. Thus, Baird et al. conclude that the marginal increase in schooling rates achieved by doubling the total transfer to the household is not cost-effective and that the cost-effectiveness of monitoring conditionality is also in question.

3.3 Free uniforms

Three included evaluations looked at free uniforms interventions carried out by NGOs in poor primary schools in Kenya. Although primary school fees were abolished at the national level in Kenya in 2003, local school committees still assess some school fees and set other attendance requirements that cost parents money, such as uniforms. According to Duflo et al. (2006) a uniform costs about US\$6 in Kenya, a substantial expense for parents in a country where the GDP per capita is \$360. The authors of these studies find that the interventions increased school attendance and persistence. Other outcomes reported included decreased teen pregnancy and improved test scores. These studies are discussed in more detail below.

Kremer et al. (2003) conducted a randomised evaluation in 14 particularly poor primary schools in Kenya's Busia and Teso districts of an NGO programme that covered the major schooling costs of Kenyan households: Textbooks and classroom construction (typically paid for through local fundraising events, and uniforms required for school attendance. The evaluation considers the intervention through the lens of education decentralisation - a popular policy alternative in the developing world that dovetails with Kenya's long-established *harambee* system of local fundraising to finance community needs. Kremer et al. argue, however, that this system of partially decentralised education financing creates perverse incentives to construct too many schools, and for excessive spending on teachers, relative to non-teacher inputs, and setting of school fees at a level that deters participation. In other words, there is little incentive to attract additional students to a school because it would provide additional work for teachers and administrators without attracting additional resources because the school populations are typically much smaller than the threshold for additional government resources.

The intervention, which provided uniforms, textbooks and classroom construction to seven treatment schools, resulted in students in the treatment group remaining in school an average of 0.5 years longer after five years and advancing 0.3 grades

further than students in control schools (probably mainly, Kremer and colleagues postulate, as a result of the free uniforms). In addition, the classes in treatment schools grew by nearly nine students - a result that Kremer and his colleagues estimate was more than offset by the benefits of the inputs. They also estimate that the Kenyan Government could have financed the additional resources provided by the NGO without external funds through the savings that could be generated from an increase in class size much smaller than that generated by the programme. Kremer et al. also confirm the assertion of much of the school fees literature that lowering the price of schooling can significantly increase participation.

Duflo et al. (2006) evaluated a similar programme by the same NGO in 328 schools in two rural districts of western Kenya. However, in this programme, the uniform provision intervention was compared with three HIV/AIDS (human immunodeficiency virus/acquired immune deficiency syndrome) interventions, and marriage and childbearing outcomes were reported in addition to schooling outcomes. Duflo and her colleagues argue that since school tuition fees were abolished in Kenya in 2003, school uniforms represent the main financial barrier to primary school participation. Measured after two years, the provision of uniforms resulted in a 15 percent decrease in drop-out and a 10 percent decrease in teen childbearing. There were also reductions in the likelihood of being married of 12 percent for girls and 40 percent for boys. (Results of the other three interventions showed no impact on teen childbearing or retention of teacher training about HIV/AIDS, whilst informing girls about variations in HIV rates by age and gender led girls to avoid cross-generational partners and reduced childbearing rates. It was too soon to measure the impact of an essay and debate contest on condoms.) Duflo et al. conclude that reducing the cost of education represents an incentive for teenagers to stay in school and delay marriage and childbearing, and estimate that the uniform intervention cost at least US\$300 per pregnancy averted.

The third uniform intervention in Kenya included in our eligible studies was conducted by *Evans et al. (2009)* as the first randomised study of uniform provision that includes impact on student learning as measured by test scores. The intervention (implemented by the same NGO⁵ as the previous interventions described) involved 12 primary schools in Busia District and used a lottery to determine which children would receive a uniform each year. Evans and colleagues find that giving a uniform reduced absenteeism by 44 percent (62 percent for students who did not previously own a uniform), increased school participation by 0.064 years and raised test scores by one-quarter of a standard deviation. They also estimate that the cost of increasing school attendance for a child by one year is almost US\$91 - more expensive than some other school-based interventions such as de-worming, but less costly than CCTs.

⁵ In the three Kenya evaluations, the NGO supported the implementation of the programme, but the evaluators are independent from the NGO.

Table 3.2 Characteristics of included RCT/QED evaluations of school fees abolishing initiatives in low-income countries

Study	Country	Intervention	Group targeted	Sample size	Study design	Methodological threats to evaluation design	Follow-up period	Outcomes: School enrolment	Outcomes: Achievement	Outcomes: Other
Kremer et al. (2003)	Kenya	Free uniforms + text book funds and classroom construction	Particularly poor primary schools	14 schools (7 treatment; 7 control)	RCT	Low	5 years	Students in treatment schools remained enrolled 13% longer than comparison	Students in treatment schools had a 16% increase in grade advancement; no significant effect on test scores	Increased average treatment class size by 9 students; no significant effect on likelihood of being married
Duflo et al. (2006)	Kenya	Free uniforms	Grade 6 students in two rural districts	334 schools (163 treatment 171; control)	RCT	Low	3 years	15% decrease in likelihood of drop-out in treatment schools	N/A	For girls, 10% decrease in teen childbearing and 12% less likely to be married; for boys, 40% less likely to be married
Baird et al. (2009)	Malawi	Full payment of school fees + cash transfer to household and to girl conditional on attendance	Secondary schoolgirls aged 13-22 in a particularly poor district	3,805 girls (1,225 treatment; 2,580 control)	RCT	Low	1 year	Re-enrolment rate increased by 2.5 times for treatment girls and drop-out rate decreased from 11% to 6%	Self-reported English literacy improved for treatment girls	N/A

Study	Country	Intervention	Group targeted	Sample size	Study design	Methodological threats to evaluation design	Follow-up period	Outcomes: School enrolment	Outcomes: Achievement	Outcomes: Other
Evans et al. (2009)	Kenya	Free uniforms	Primary school standards 1-4 in a district	1,305 children (612 treatment; 693 control)	RCT	Low	3 years	Reduced school absenteeism by 44% (62% for children who did not already own a uniform)	Raised average test scores of recipients by 0.252 s.d.	N/A
Grogan (2009)	Uganda	Universal primary education (Government pays tuition fees)	All primary school-age children	8,206 children	QED (RDD)	Low	3-4 years	3% effect on probability of entering school before age 9 (results stronger for girls concentrated in rural areas)	N/A	No discontinuity in the probability of attending private school

RCT: randomised controlled trial; QED: quasi-experimental evaluation; RDD: regression continuity design.

4. Descriptive studies

This section summarises the 31 studies we identified that examined school fees elimination interventions through descriptive quantitative (including quasi-experiments without equating of groups) and qualitative approaches. (See Appendix 4.1 for a table summarising these studies). To be included in our review, these studies had to examine a school fees elimination intervention in low-income developing countries and to include sufficient methodological detail as to be replicable. Because these non-causal studies do not fit the criteria for inclusion in effect size estimates, we did not systematically code them as we did the five eligible evaluations. Rather, we reviewed them carefully to understand the key points, which we summarise below.

As with the five eligible evaluations, the interventions studied in the majority (27) of the studies took place in sub-Saharan Africa; the remaining four examined interventions that were implemented in Bangladesh. The reports were written between 1996 and 2010, with the majority written after 2002. Most of the studies (24) examined free UPE interventions; the remaining studies examined scholarships or fees waivers for girls (six), and a temporary waiving of schools fees following an economic blockade (one). Methodologies employed included quasi-experimental designs, descriptive quantitative studies, and qualitative approaches. Given the limitations in drawing causal inferences from these types of studies, care must be taken in interpreting the results. However, the majority of these studies do coincide with the findings from the five rigorous studies, reporting that school fees elimination interventions increased educational access, equity and student persistence. However, declines in educational quality were also reported in some of the studies. Readers should note that some of these studies were not designed to provide an evaluation of free UPE interventions. In those instances in which these descriptive studies were designed to evaluate a free UPE initiative, the design had significant internal validity threats to create questions about the causal impact of free UPE on the observed results.

4.1 Targeting of interventions

Our purpose in summarising these studies is to highlight important issues about context, theory and implementation. For example, these studies illuminate issues of concern in the implementation of school fees abolition initiatives. One important issue concerns effective targeting of the interventions and whether they meet the needs of those who are intended to benefit - poor and otherwise marginalised groups. Maikish (2010), comparing outcomes for deprived and non-deprived districts under free UPE in Ghana, reports divergent results, with poorer districts showing lower returns. She suggests that policy analysis should occur at the district level, rather than the national level, to highlight and address such differences. Nicola (2010) also notes differential outcomes for AIDS orphans, for whom enrolment does not increase at as high a rate as for non-orphans under free UPE in Tanzania, and calls for targeted school enrolment policies for orphans. Similarly, Sifuna (2005) claims that free UPE has not reached pastoralist groups in Kenya and that targeted and culturally appropriate outreach is needed to improve their education outcomes. Muyanga et al. (2010) argue that whilst free UPE in Kenya increased overall school enrolment, this was especially true for children from higher-income categories, indicating that the policy is not sufficient to reach the poorest and most marginalised groups. Identifying and addressing the factors that still prevent these groups from completing primary school is a key to continuing progress toward countries' school enrolment goals.

The studies of fees-waiver interventions for girls (Amin and Sedgh 1998, Arends-Kuenning and Amin 2004, Chapman and Mushlin 2008, Fuwa 2001, Khandker et al. 2003, USAID 1999) seem to suggest that households do respond to targeted incentives, even if they do not entirely cover all opportunity costs. However, even these incentives were not sufficient for all children across the studies, indicating the need to determine and address remaining barriers to education for these girls (Arends-Kuenning and Amin 2004).

4.2 Educational quality

Declining education quality under free UPE policies is highlighted in several of the descriptive studies. For example, Schmidt (2006) argues that the big increases in enrolment in Kenya may have been largely due to the publicity and large-scale outreach campaign that accompanied the announcement of free UPE. However, this enrolment gain has eroded over time, due to public perception of reduced education quality. She postulates that whilst free UPE may reduce the wealth bias in primary school attendance (poorer children, on average, are enrolling more), it may have increased the wealth bias in the attainment of a quality education, as wealthier households are taking their children out of the public sector schools and enrolling them in private schools. Other researchers have noted this change in the demographic makeup of public schools following free UPE implementation (e.g. Bold et al. 2009, Oketch et al. 2010, Tooley et al. 2008). Several other studies (e.g. Chimombo 2005, Deininger 2003, Nishimura et al. 2008) highlight specific quality issues under free UPE, such as overcrowding, inappropriate or insufficient allocation of funds, and teacher and supply shortages, and argue that sustained enrolment gains must necessitate concurrent improvements in education quality.

4.3 Role of the private sector

Declines in educational quality accompanying free UPE policies may lead to shifts in the share of enrolments between the public and private sectors. A phenomenon that has been noted in several sub-Saharan African countries that have officially abolished school fees is the 'mushrooming' of private schools, including low-fee/low-quality 'private schools for the poor'. Tooley et al. (2008), conducting exploratory qualitative research in an informal settlement in Nairobi, suggest that net enrolment may have actually declined under free UPE, as the official figures did not consider decreased enrolment in unregistered private schools serving the poor in slum areas. Interviews with school managers and parents suggested possible reasons for this decline. For example, losing even a small number of children to government schools after the implementation of free UPE forced many of these schools to close. Parents suggested that only the relatively more wealthy parents could afford to send their children to government schools under free UPE because of 'hidden costs', such as mandatory uniforms and PTA dues. In addition, parents viewed the educational quality of the slum private schools to be higher than that of the government schools under free UPE.

Oketch et al. (2010) build on this study by using an excess demand and differentiated demand framework to develop a logistic regression model to examine how slum and non-slum households react to free UPE policy. They found that 44 percent of students in their sample in two slums attend low-quality, low-fees private schools, despite free UPE. Oketch and colleagues argue that low government investment in slum areas leads to insufficient supply of public school spaces in such areas, driving poorer households to utilise low-fees private schools whilst wealthier slum households utilise the public system. On the other hand, under the differentiated demand model, wealthier households in non-slum areas

prefer high-quality private schools, due to perceptions of low quality in the state system. These exploratory studies highlight the need for more research to better understand the role of the private sector under free UPE policies, particularly that of unofficial private schools for the poor. At the very least, such schools should be identified and included in official counts of school enrolment. In addition, it is critical to understand the factors driving the behaviour of households of different socio-economic groups under school fees elimination policies and also to gain a better understanding of how much different groups are actually willing to pay for education to most effectively allocate resources. For example, the studies just discussed suggest that even some of the poorest slum households place a high value on education and are willing to pay to send their children to private schools if these are the only schools available to them or if they are perceived to be of higher quality than the government schools.

4.4 Sustainability

Central to this issue of education quality is the question of how to best finance school fees elimination policies, particularly so that they are sustainable. Many of the interventions examined, including national free UPE policies, were supported wholly or in part by external donors. Schmidt (2006) raises the question of whether the benefit of a small increase in enrolment is worth the cost of increased aid dependency, particularly if the increase is achieved at the cost of education quality. Many of the national free UPE policies have been implemented concurrently - and as an element of - education decentralisation, in which school funding decision making is devolved to the local level, with varying results in terms of efficiency and effectiveness of allocation (Maikish 2010). In most contexts, centralized free UPE policies do not appropriately or adequately allocate funds for various school-level expenses, necessitating local fundraising and the continued collection of fees from families (e.g. Al-Samarrai 2003, Kenya 2008, OWN and Associates 2004). Thus, curtailing the informal assessment of fees may not be feasible, and determining how much a community can afford to contribute to schools, and charging fees to households on a sliding scale based on ability to pay, have been raised as possible alternatives (Schmidt 2006).

5. Conclusion

The five methodologically rigorous evaluations discussed previously show that the interventions studied – universally abolishing tuition fees, eliminating tuition fees for targeted groups, and providing free uniforms – did strongly increase school enrolment and positively impact other education and non-education outcomes, including age at school entry, persistence/grade advancement, attendance, re-enrolment and delayed marriage/childbearing. Follow-up periods were between one and five years, highlighting the need for longitudinal studies to assess whether gains are sustained in the medium-longer term.

Most of the studies also include some preliminary cost-benefit analysis. For example, Kremer et al. (2003) suggest that government could offset the costs of uniform provision through marginally increasing class size. Baird et al. (2009) find that strong enrolment impacts associated with eliminating tuition fees for secondary schoolgirls are not responsive to increasing the amount of an additional cash transfer made to the girls. Looking at the impact of delaying marriage/childbearing on school persistence, Duflo et al. (2006) estimate that the cost of the uniform provision intervention was at least US\$300 per pregnancy averted. Evans et al. (2009) estimate the cost of increasing persistence by one year through uniform provision to be around US\$91 per student.

The majority of the descriptive studies examine free UPE policies, which are very difficult to evaluate empirically. All of the studies find that these policies increased enrolment and, in many cases, increased equity by extending access to marginalised groups. However, for the most part, these studies evaluated the short-term effects of the policies, so we do not know to what extent enrolment and persistence remain high after the initial enthusiasm surrounding the new policy. They also raise serious questions about the quality of education under free UPE, the adequacy of budgetary allocations, and the long-term sustainability of the programmes. Some of the studies also suggest that fees abolition policies do not do enough to extend access to vulnerable groups, whether because school fees continue to be assessed locally, because public school spaces are unavailable in some areas, or for other reasons that need to be elucidated by further research in order to target interventions more effectively.

Due to the small yield of eligible impact evaluation studies, and the substantial diversity in samples, interventions, countries and other characteristics, we did not employ any analyses to generate an overall effect size, nor could we conduct analyses to examine the role of mediating (underlying causal chain) or moderating (subgroup) effects. Such analyses may have to wait for another generation of studies, so that more fine-grained results can be gleaned.

As impact and other empirical studies on this topic populate the literature, an updated version of this review should also incorporate more details on the political climate, educational structure and other factors that would assist readers in understanding the context in which these policies or programmes were undertaken and the evaluations done.

It is very true that these policy and programme initiatives took place in very different environments, and all at a different pace. In several instances, small-scale fees abolition initiatives were supported in local contexts by NGOs; in others, universal fees elimination was provided by a national policy on UPE. Generally speaking, the locally implemented and NGO-supported schemes lend themselves more readily to experimental design because not all children can be served. One question is whether any gains produced in the local implementation and NGO-run scheme will last once the initial enthusiasm and attention wane. The evaluations of

universal national policies, because all persons are subject to the new initiative, are not amenable to such randomised experimentation, but because they study the full roll-out of actual policy by government actors, may be viewed by some as being more policy relevant. Understanding the evidence generated by both types of interventions is necessary.

5.1 Future research

The findings of this systematic review highlight the need for more rigorous empirical research on the effects of various types of school fees elimination policies in low-income developing countries. As mentioned, research in this area is complicated by the fact that many countries have already implemented universal free school tuition policies for all primary children, so an appropriate control group is difficult to identify. One possible solution to this challenge may involve utilising an interrupted time series design involving a single group, provided that a long series of regularly collected data are available before and after the free UPE intervention was implemented (see Bloom 2003). In addition, because families often continue to pay some fees under free UPE policies, further impact evaluations of NGO- or government-supported programmes targeting full fees elimination for specific groups can shed more light on the true costs of education for households and the degree to which eliminating these costs can improve schooling and other outcomes for the most vulnerable groups. Policy effectiveness can be evaluated at the local level by comparing the success of different districts in implementing programmes (Maikish 2010).

To effectively target interventions and allocate resources, it is critical to understand the full household costs of education (including fees still assessed under free UPE policies and opportunity costs for boys and girls) for different socio-economic groups, as well as to determine how much of the full cost of education households from various policy target groups are willing and able to bear, given an acceptable level of education quality. For example, Gertler and Glewwe (1990), using a rigorous model of demand for schooling to calculate willingness to pay for secondary schools in rural Peru, found that even the poorest households were willing to pay fees high enough to cover operating costs of village schools. Several studies examined in this review show that parents do respond to incentives and that it is not necessary to eliminate all poverty to induce them to enrol their children (e.g. Arends-Kuenning and Amin 2004). The finding from studies discussed above that some poor slum households are utilising low-fees private schools highlights the importance of fully understanding the choices different groups make in terms of which schools to utilise and the motivations behind their choices, as well as policy opportunities for partnering with the private sector to improve school access and quality for the poor. Voucher programmes may be one way to leverage existing private sector resources to extend educational access and improve quality. Such programmes may be empirically evaluated as well.

On the other hand, even under incentive programmes, not all children attend school. To ensure that policies reach the most marginalised groups, research must be conducted to find out why incentives are not sufficient for some households and groups. The descriptive studies examined in this review highlight the importance, in addition to rigorous impact evaluations, of gathering qualitative data to contribute to a contextual understanding of behavioural responses to fees elimination policies and of different groups' perceptions of education quality in different sectors. Experimentation with different innovations, such as user fees on a sliding scale based on household ability to pay, would be informative and could be researched empirically.

In addition, longitudinal studies are needed to elucidate the longer-term impacts of fees elimination, including whether initial surges in enrolment are sustained over time, to what extent they are actually responses to reduced costs vs. public outreach, and what the policies mean for future educational attainment, employment and other outcomes. The success of any primary educational system could also be reflected in how students fare in secondary education and higher education and in obtaining gainful employment. Longer-term studies will be necessary to see if free UPE strategies have resulted in anything more than short-term gains for its recipients.

The available empirical evidence is not yet sufficient to guide policy decisions on which of the five types of school user fees would be most effectively targeted for future elimination interventions. Nor is it robust enough to clearly identify associated trade offs in educational quality, including impacts on resources available at the school level, or other unintended consequences of fees elimination. Whilst quality trade offs have been observed in countries that have implemented free UPE, they have not been established empirically in very many studies. A related area of interest for governments is the extent to which fees abolition policies can be sustained over time and the degree of donor dependency that these policies require.

Thus, some specific questions for future research might include:

- Does the removal of school fees encourage enrolment and persistence in the longer term? What are the longer-term impacts of school fees elimination policies?
- What are the unintended consequences of eliminating school fees? How might future policy mitigate these?
- Of the five types of school fees, which would be most cost-effective to reduce/eliminate?
- What groups are fees elimination policies not reaching? Why? How can policies effectively target these groups?
- How much are households with various background characteristics willing to pay for education and how does educational quality impact on this? How do households perceive and measure educational quality and how does this impact on their decisions? In the presence of school choice, how do households utilise the public and private sectors?

Rigorous impact studies on these topics can provide valuable information to countries that are considering abolishing school fees and can inform strategies for advance planning and targeting of reforms, including planning for efficient allocation of resources at the local level. For example, whilst sweeping universal reforms may be useful for political campaigns and result in initial surges in enrolment, other more targeted or phased-in models of fees elimination may be more effective and sustainable longer term. Pilot studies of such smaller-scale interventions could be evaluated through experimental means and provide important data for scaling-up of reforms. This type of information would also be very useful to the donor community that supports such interventions. In particular, government and non-governmental actors working in this area would benefit from specific impact data about the differential effects of various types of fees elimination policies on different groups (e.g. urban/rural, different socio-economic groups, girls/boys, AIDS orphans, baseline drop-outs), and on why these policies may not be sufficient to reach the most vulnerable children. This research is needed to most effectively design and target interventions.

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Appendices

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Appendix 2.1: Databases and organisations specifically searched for this DFID project

- DFID
- EBSCO Megafile
- Econlit
- Education Index
- Edis
- ERIC
- Google and Google Scholar
- IDEAS-Research Papers in Economics (<http://ideas.repec.org/>)
- Index to Current Urban Documents
- International Bibliography of Social Science
- ISI Web of Science
- JOLIS (World Bank, International Monetary Fund, International Finance Corporation)
- National Bureau of Economic Research Working Papers
- PAIS Archive
- PAIS International
- PolicyFile
- Proquest Dissertations and Theses
- Sociological Abstracts
- UNESCO (UNESDO and UNESBIB)
- USAID
- World Bank
- Worldwide Political Science Abstracts

Bibliographic databases that were searched for the 3ie project

- Academic Search Premiere
- British Library
- EBSCO Megafile
- Econlit
- Education Index
- Edis
- ERIC
- Expanded Academic ASAP
- Full-Text of Ovid Journals

- Google and Google Scholar
- Health Technology Assessment Register
- Housing and Urban Development (U.S.) “HUD USER”
- IDEAS-Research Papers in Economics (<http://ideas.repec.org/>)
- Index to Current Urban Documents
- International Bibliography of Social Science
- ISI Web of Science
- JOLIS (World Bank, International Monetary Fund, International Finance Corporation)
- Medline
- National Clearinghouse of Child Abuse and Neglect (NCCAN)
- PAIS Archive
- PAIS International
- Periodical Contents Index
- PolicyFile
- Proquest Dissertations and Theses
- Psychology and Behavioral Sciences Collection
- PsycInfo (includes PsychLit)
- Sage Family Studies Abstracts
- Sage Journals Management and Organizational Studies Full-Text Collection
- Sage Journals Political Science Full-Text Collection
- Sage Journals Sociology Full-Text Collection
- Sage Urban Studies Abstracts
- Social Service Abstracts
- Social Service Research Network
- Social Work Abstracts
- Sociological Abstracts
- UNESCO (UNESDO and UNESBIB)
- Worldwide Political Science Abstracts

Journals handsearched

- Economic Development and Cultural Change
- International Journal of Educational Development
- Journal of Development Economics
- World Bank Research Observer
- *World Bank Economic Review*

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Appendix 2.3: Search terms and results

Search terms

We used two search strategies to identify relevant studies. The first strategy identified studies conducted in low-income developing countries using the following search terms:

Afghanistan* OR Samoa* OR Bangladesh* OR Pakistan* OR Bengal* OR Benin* OR Dahomey* OR Burkin* OR Volta OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Africa* OR Chad* OR Comoros* OR Congo* OR Zaire* OR Somali* * OR Eritrea* OR Ethiopia* OR Gambia* OR Ghana* OR "Gold Coast" OR Guinea* OR Haiti* OR Kenya* OR Korea* OR Kyrgyz* OR Lao* OR Liberia* OR Madagascar* OR Malaw* OR Nyasaland* OR Mali* OR Mauritania* OR Mozambique* OR Myanmar* OR Burm* OR Niger* OR Rwanda* OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan* OR Tanzania* OR Togo* OR Uganda* OR Zambia* OR Zimbabwe* OR Asia* 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".

Then, to identify studies that evaluate interventions involving the elimination of school fees, we added the following search terms: fee* OR levies OR payment* OR tuition OR abolish OR eliminate OR exemption OR "free basic education" OR FBE OR "School Fee Abolition Initiative" OR SFAI OR "free primary education" OR FPE OR "free basic education" OR FBE OR "universal primary education" OR UPE OR "universal free primary education" OR textbook* OR due* OR contribution*

The specific search term combinations used and yield obtained for each database follow. Years searched were 1990-2010.

Source	Search terms	Yield	Hits
DFID	School fees	204	0
EBSCO	Afghanistan* OR Samoa* OR Bangladesh* OR Pakistan* OR Bengal* OR Benin* OR Dahomey* OR Burkin* OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Africa* OR Chad* OR Comoros* OR Congo* OR Zaire* OR Somali* OR Eritrea* OR Ethiopia* OR Gambia* OR Ghana* OR "Gold Coast" OR Guinea* OR Haiti* OR Kenya* OR Korea* OR Kyrgyz* OR Lao* OR Liberia* OR Madagascar* OR Malaw* OR Nyasaland* OR Mali* OR Mauritania* OR Mozambique* OR Myanmar* OR Burm* OR Niger* OR Rwanda* OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan* OR Tanzania* OR Togo* OR Uganda* OR Zambia* OR Zimbabwe* OR Asia* 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" AND "primary school" OR "secondary school" or school* OR educat* AND fee* OR levies OR payment* OR tuition* OR contribution* OR uniform* AND random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome*	629	7
Econlit	(Afghanistan* OR Samoa* OR Bangladesh* OR Pakistan* OR Bengal* OR Benin* OR Dahomey* OR Burkin* OR Volta* OR Burundi* OR Korea* OR Kyrgyz* OR Lao* OR Liberia* OR Madagascar* OR Malaw* OR Nyasaland*) or (Cambodia* OR Khmer* OR Kampuchea* OR Africa* OR Chad* OR Comoros* OR Congo* OR Zaire* OR Mali* OR Mauritania* OR Mozambique* OR Myanmar* OR Burm* OR Niger* OR Rwanda* OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan* OR Tanzania* OR Togo*) or (Somali* OR Eritrea* OR Ethiopia* OR Gambia* OR Ghana* OR "Gold Coast" OR Guinea* OR Haiti* OR Kenya* OR Uganda* OR Zambia* OR Zimbabwe* OR Asia* OR "developing nation" OR "developing region" OR "developing country")and AB=("primary school" OR "secondary school" or school* OR educat*) and AB=(fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*) and (random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome*)	160	4

Source	Search terms	Yield	Hits
Education FullText	(Afghanistan* OR Samoa* OR Bangladesh* OR Pakistan* OR Bengal* OR Benin* OR Dahomey* OR Burkin* OR Volta* OR Burundi* OR Korea* OR Kyrgyz* OR Lao* OR Liberia* OR Madagascar* OR Malaw* OR Nyasaland* or Cambodia* OR Khmer* OR Kampuchea* OR Africa* OR Mali* OR Mauritania* OR Mozambique* OR Myanmar* OR Burm* OR Niger* OR Rwanda* OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan* OR Tanzania* OR Togo* or Somali* OR Eritrea* OR Ethiopia* OR Gambia* OR Ghana* OR Chad* OR Comoros* OR Congo* OR Zaire* OR "Gold Coast" OR Guinea* OR Haiti* OR Kenya* OR Uganda* OR Zambia* OR Zimbabwe* OR Asia* OR "developing nation" OR "developing region" OR "developing country") and AB=("primary school" OR "secondary school" or school* OR educat*) and AB=(fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*)	193	3
Eldis	school* and fee* or pay* School*	54 500	2 0
ERIC	(Afghanistan* OR Samoa* OR Bangladesh* OR Pakistan* OR Bengal* OR Benin* OR Dahomey* OR Burkin* OR Volta* OR Burundi* OR Korea* OR Kyrgyz* OR Lao* OR Liberia* OR Madagascar* OR Malaw* OR Nyasaland*) or (Cambodia* OR Khmer* OR Kampuchea* OR Africa* OR Chad* OR Comoros* OR Congo* OR Zaire* OR Mali* OR Mauritania* OR Mozambique* OR Myanmar* OR Burm* OR Niger* OR Rwanda* OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan* OR Tanzania* OR Togo*) or (Somali* OR Eritrea* OR Ethiopia* OR Gambia* OR Ghana* OR "Gold Coast" OR Guinea* OR Haiti* OR Kenya* OR Uganda* OR Zambia* OR Zimbabwe* OR Asia* OR "developing nation" OR "developing region" OR "developing country") and AB=("primary school" OR "secondary school" or school* OR educat*) and AB=(fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*) and (random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome*)	488	9
Google	school fees	500	4
Google Scholar	School fees	500	5

Source	Search terms	Yield	Hits
IBSS2	(Afghanistan* or Samoa* or Bangladesh*) and AB=(school* or educat*) and (fee* or payment* or tuition*) and (impact* or effect* or outcome*)	3	0
	(Pakistan* or Bengal* or Benin*) and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	4	0
	(Dahomey* or Burkin* or Volta*) and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	0	0
	(Burundi* or Cambodia* or Khmer*) and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	0	0
	(Kampuchea* or Africa* or Chad*) and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	25	0
	(Comoros* or Congo* or Zaire*) and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	0	0
	(Gambia* or Ghana* or "Gold Coast*") and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	3	0
	(Guinea* or Haiti* or Kenya*) and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	8	1
	(Korea* or Kyrgyz* or Lao*) and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	1	0
	(Liberia* or Madagascar* or Malaw*) and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	4	1
	(Nyasaland* or Mali* or Mauritania*) and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	2	0
(Mozambique* or Myanmar* or Burm*) and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	3	0	

	(Niger* or Rwanda* or "Sierra Leone") and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	5	0
	(Togo or Uganda* or Zambia*) and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	2	1
	(Asia or "developing nation" or "developing country") and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	6	0
	("third world" or "low income" or impoverished) and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	5	0
	(Pacific or "Middle East") and AB=(school* or educat*) and AB=(fee* or payment* or tuition*) and (evaluat* or outcome* or impact*)	2	0

Source	Search terms	Yield	Hits
IDEAS	Search Terms: ("primary school" "secondary school" school*) + (fee* levies payment* tuition* contribution* uniform*) + (random* evaluate* experiment* control* trial* impact* effect* outcome*)	841	5
Index of Current Urban Documents	Afghanistan* OR Samoa* OR Bangladesh* OR Pakistan* OR Bengal* OR Benin* OR Dahomey* OR Burkin* OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Africa* OR Chad* OR Comoros* OR Congo* OR Zaire* OR Somali* OR Eritrea* OR Ethiopia* OR Gambia* OR Ghana* OR "Gold Coast" OR Guinea* OR Haiti* OR Kenya* OR Korea* OR Kyrgyz* OR Lao* OR Liberia* OR Madagascar* OR Malaw* OR Nyasaland* OR Mali* OR Mauritania* OR Mozambique* OR Myanmar* OR Burm* OR Niger* OR Rwanda* OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan* OR Tanzania* OR Togo* OR Uganda* OR Zambia* OR Zimbabwe* OR Asia* 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 Pacific* OR Middle East*	0	0
International Bibliography of the Social Sciences	Afghanistan* OR Samoa* OR Bangladesh* OR Pakistan* OR Bengal* OR Benin* OR Dahomey* OR Burkin* OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Africa* OR Chad* OR Comoros* OR Congo* OR Zaire* OR Somali* * OR Eritrea* OR Ethiopia* OR Gambia* OR Ghana* OR "Gold Coast" OR Guinea* OR Haiti* OR Kenya* OR Korea* OR Kyrgyz* OR Lao* OR Liberia* OR Madagascar* OR Malaw* OR Nyasaland* OR Mali* OR Mauritania* OR Mozambique* OR Myanmar* OR Burm* OR Niger* OR Rwanda* OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan* OR Tanzania* OR Togo* OR Uganda* OR Zambia* OR Zimbabwe* OR Asia* 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 Pacific* OR Middle East*	270	3

Source	Search terms	Yield	Hits
ISI Web of Knowledge	(Afghanistan OR Samoa OR Bangladesh OR Pakistan OR Bengal OR Benin OR Dahomey OR Burkina OR Volta OR Burundi OR Cambodia OR Khmer OR Kampuchea OR Africa) AND Topic=("primary school" OR "secondary school" OR school OR education) AND Topic=(fee OR levies OR payment OR tuition OR contribution OR uniform) AND Topic=(random OR evaluate OR experiment OR control OR trial OR impact OR effect OR outcome)	79	2
	(Chad OR Comoros OR Congo OR Zaire OR Somali OR Eritrea OR Ethiopia OR Gambia OR Ghana OR "Gold Coast" OR Guinea OR Haiti OR Kenya OR Korea OR Kyrgyz OR Lao OR Liberia OR Madagascar OR Malawi OR Nyasaland OR Mali OR Mauritania OR Mozambique OR Myanmar OR Burma OR Niger OR Rwanda OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan OR Tanzania) AND Topic=("primary school" OR "secondary school" OR school OR education) AND Topic=(fee OR levies OR payment OR tuition OR contribution OR uniform) AND Topic=(random OR evaluate OR experiment OR control OR trial OR impact OR effect OR outcome)	37	1
	(Togo OR Uganda OR Zambia OR Zimbabwe OR Asia 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 Pacific OR Middle East) AND Topic=("primary school" OR "secondary school" OR school OR education) AND Topic=(fee OR levies OR payment OR tuition OR contribution OR uniform) AND Topic=(random OR evaluate OR experiment OR control OR trial OR impact OR effect OR outcome)	36	1

Source	Search terms	Yield	Hits
JOLIS	"Afghanistan* OR Samoa* OR Bangladesh* OR Pakistan* OR Bengal* OR Benin* OR Dahomey* OR Burkin* OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Africa*" AND Keywords anywhere ""primary school" OR "secondary school" OR school* OR educat*" AND Keywords anywhere "fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*" AND Keywords anywhere "random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome**"	1	0
	"Chad* OR Comoros* OR Congo* OR Zaire* OR Somali* OR Eritrea* OR Ethiopia* OR Gambia* OR Ghana*" AND Keywords anywhere ""primary school" OR "secondary school" OR school* OR educat*" AND Keywords anywhere "fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*" AND Keywords anywhere "random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome**"	1	0
	"Gold Coast" OR Guinea* OR Haiti* OR Kenya* OR Korea* OR Kyrgyz* OR Lao* OR Liberia* OR Madagascar* OR Malaw* OR Nyasaland* OR Mali*" AND Keywords anywhere ""primary school" OR "secondary school" OR school* OR educat*" AND Keywords anywhere "fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*" AND Keywords anywhere "random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome**"	1	0
	"Mauritania* OR Mozambique* OR Myanmar* OR Burm* OR Niger* OR Rwanda* OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan* OR Tanzania* OR Togo*" AND Keywords anywhere ""primary school" OR "secondary school" OR school* OR educat*" AND Keywords anywhere "fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*" AND Keywords anywhere "random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome**"	1	0
	"Uganda* OR Zambia* OR Zimbabwe* OR Asia* OR "developing nation" OR "developing region" OR "developing country" OR "third world" OR "low income" OR impoverished" AND Keywords anywhere ""primary school" OR "secondary school" OR school* OR educat*" AND Keywords anywhere "fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*" AND Keywords anywhere "random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome**"	2	0
NBER	school, fees	1040	0

Source	Search terms	Yield	Hits
PAIS	(Afghanistan* OR Samoa* OR Bangladesh* OR Pakistan* OR Bengal* OR Benin* OR Dahomey* OR Burkin* OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Africa* OR Chad* OR Comoros* OR Congo* OR Zaire* OR Somali* * OR Eritrea* OR Ethiopia* OR Gambia* OR Ghana* OR "Gold Coast" OR Guinea* OR Haiti* OR Kenya* OR Korea* OR Kyrgyz* OR Lao* OR Liberia* OR Madagascar* OR Malaw* OR Nyasaland* OR Mali* OR Mauritania* OR Mozambique* OR Myanmar* OR Burm* OR Niger* OR Rwanda* OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan* OR Tanzania* OR Togo* OR Uganda* OR Zambia* OR Zimbabwe* OR Asia* 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 Pacific* OR Middle East*) AND ("primary school" OR "secondary school" OR school* OR educat*) AND (fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*) AND (random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome*)	94	3
PAIS Archive	SEARCH TERMS: (Afghanistan* OR Samoa* OR Bangladesh* OR Pakistan* OR Bengal* OR Benin* OR Dahomey* OR Burkin* OR Volta* OR Burundi* OR Cambodia* OR Khmer* OR Kampuchea* OR Africa* OR Chad* OR Comoros* OR Congo* OR Zaire* OR Somali* * OR Eritrea* OR Ethiopia* OR Gambia* OR Ghana* OR "Gold Coast" OR Guinea* OR Haiti* OR Kenya* OR Korea* OR Kyrgyz* OR Lao* OR Liberia* OR Madagascar* OR Malaw* OR Nyasaland* OR Mali* OR Mauritania* OR Mozambique* OR Myanmar* OR Burm* OR Niger* OR Rwanda* OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan* OR Tanzania* OR Togo* OR Uganda* OR Zambia* OR Zimbabwe* OR Asia* 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 Pacific* OR Middle East*) AND ("primary school" OR "secondary school" or school* OR educat*) AND (fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*) AND (random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome*)	0	0

Source	Search terms	Yield	Hits
PolicyFile	afghanistan* or samoa* or bangladesh* or pakistan* or bengal* or benin* or dahomey* or burkin* or volta* or burundi* or korea* or kyrgyz* or lao* or liberia* or madagascar* or malaw* or nyalaland* or cambodia* or khmer* or kampuchea* or africa* or mali* or mauritania* or mozambique* or myanmar* or burm* or niger* or rwanda* or "sierra leone" or "solomon islands" or tajikistan* or tanzania* or togo* or somali* or eritrea* or ethiopia* or gambia* or ghana* or chad* or comoros* or congo* or zaire* or "gold coast" or guinea* or haiti* or kenya* or uganda* or zambia* or zimbabwe* or asia* or "developing nation" or "developing region" or "developing country" and "primary school" or "secondary school" or school* or educat* and fee* or levies or payment* or tuition* or contribution* or uniform* Organization(s): "American Foreign Policy Council" "Amnesty International" "Asia Policy Point" "Asia Society" "Belfer Center for Science and International Affairs" "Berkeley Roundtable on the International Economy" "Carnegie Endowment for International Peace - Moscow Center" "Carnegie Endowment for International Peace - US" "Center for Asia-Pacific Policy, The RAND Corporation" "Center for Contemporary Arab Studies" "The Center for Education Reform" "Center for Evaluation and Education Policy" "Center for Global Development" "Center for International Governance" "Center for International Policy" "Center for International Security and Cooperation, Stanford University" "Center for International Studies, Massachusetts Institute of Technology" "Center for International Trade and Security, University of Georgia" "The Center for Strategic and International Studies" "Center for the Future of Children" "Council for Aid to Education" "Council on Foreign Relations" "East West Center" "Federal Institute for Russian, East European and International Studies" "Foundation For Middle East Peace" "Greenpeace" "Harvard Institute for International Development, Harvard University" "Human Rights Watch" "Institute of Development Studies" "International Center for Economic Growth" "International Institute for Strategic Studies" "International Labour Organization" "International Monetary Fund" "International Organization" "The Iraq Foundation" "John F Kennedy School of Government, Harvard University" "Kiel Institute for World Economy" "Korea Institute for International Economic Policy" "Latin American and the Caribbean Center, Florida International University" "The Middle East Institute" "Middle East Policy Council" "Middle East Research and Information Project" "Organization for Economic Cooperation and Development" "Overseas Development Council" "Overseas Development Institute" "Oxfam" "Pacific Institute for Studies in Development, Environment and Security" "Pacific Research Institute for Public Policy" "Peterson Institute for International Economics" "Philippine Institute for Development Studies" "Program on Education Policy and Governance,	181	0

	Kennedy School of Government" "Third World Network" "Transnational Foundation for Peace and Future Research" "Transnational Institute" "United Nations (UN)" "United Nations Research Institute for Social Development" "Walter H. Shorenstein Asia-Pacific Research Center" "The Walter H. Shorenstein Asia- Pacific Research Center (Shorenstein APARC)" "Washington Institute for Near East Policy" "Washington Office on Africa" "Washington Report on Middle East Affairs" "Watson Institute for International Studies" "Weatherhead Center for International Affairs" "Woodrow Wilson International Center for Scholars" "The World Bank" "World Peace Foundation" "World Policy Institute" Organization Type(s): "International" Subject(s): "Education" AND "International Development" Publication Date: from 01/01/1990 to 12/31/2010		
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Source	Search terms	Yield	Hits
Proquest Dissertations and Theses	(Afghanistan* OR Samoa* OR Bangladesh* OR Pakistan* OR Bengal* OR Benin* OR Dahomey* OR Burkin* OR Volta* OR Burundi* OR Korea* OR Kyrgyz* OR Lao* OR Liberia* OR Madagascar* OR Malaw* OR Nyasaland* or Cambodia* OR Khmer* OR Kampuchea* OR Africa*) and AB=("primary school" OR "secondary school" OR school* OR educat*) and AB=(fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*) and (random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome*)	502	2
	(Mali* OR Mauritania* OR Mozambique* OR Myanmar* OR Burm* OR Niger* OR Rwanda* OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan* OR Tanzania* OR Togo* OR Somali* OR Eritrea* OR Ethiopia* OR Gambia* OR Ghana*) AND (primary school OR secondary school OR school* OR educat*) AND (fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*) AND TEXT(random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome*) AND PDN(>1/1/1990) AND NOT ("African American")	49	2
	(Chad* OR Comoros* OR Congo* OR Zaire* OR "Gold Coast" OR Guinea* OR Haiti* OR Kenya* OR Uganda* OR Zambia* OR Zimbabwe* OR Asia* OR "developing nation" OR "developing region" OR "developing country") AND (primary school OR secondary school OR school* OR educat*) AND (fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*) AND TEXT(random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome*) AND PDN(>1/1/1990) AND NOT ("African American")	155	1
Sociological Abstracts	(Afghanistan* OR Samoa* OR Bangladesh* OR Pakistan* OR Bengal* OR Benin* OR Dahomey* OR Burkin* OR Volta* OR Burundi* OR Korea* OR Kyrgyz* OR Lao* OR Liberia* OR Madagascar* OR Malaw* OR Nyasaland*) or (Cambodia* OR Khmer* OR Kampuchea* OR Africa* OR Chad* OR Comoros* OR Congo* OR Zaire* OR Mali* OR Mauritania* OR Mozambique* OR Myanmar* OR Burm* OR Niger* OR Rwanda* OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan* OR Tanzania* OR Togo*) or (Somali* OR Eritrea* OR Ethiopia* OR Gambia* OR Ghana* OR "Gold Coast" OR Guinea* OR Haiti* OR Kenya* OR Uganda* OR Zambia* OR Zimbabwe* OR Asia* OR "developing nation" OR "developing region" OR "developing country") and AB=("primary school" OR "secondary school" OR school* OR educat*) and AB=(fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*) and (random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome*)	369	0
UNESCO	Thesaurus Term: "educational fees"	69	3

Source	Search terms	Yield	Hits
USAID	ANY NORTH AFRICA ONLY, ANY AFRICA SOUTH OF SAHARA, ANY EAST ASIA, AFGHANISTAN, BENIN, BURKINA FASO, BURMA, BURUNDI, CAMBODIA, CHAD, CONGO DR, CONGO PR, EQUATORIAL GUINEA, ERITREA, ETHIOPIA, GAMBIA, GHANA, GUINEA, HAITI, KOREA DPR, KYRGYZSTAN, LAOS, LIBERIA, MADAGASCAR, MALAWI, MALI, MAURITANIA, MOZAMBIQUE, NIGER, NIGERIA, PAKISTAN, RWANDA, SIERRA LEONE, SOMALIA, TAJIKISTAN, TANZANIA, TOGO, UGANDA, ZAMBIA, ZIMBABWE :: Any Evaluation :: Basic Ed	422	0
World Bank	"school fees" fee* OR levies OR payment* OR tuition OR abolish* OR eliminate* OR exemption OR "free basic education" OR FBE or "School Fee Abolition Initiative" OR SFAI or "free primary education" OR FPE OR "universal primary education" OR UPE OR "universal free primary education" OR textbook* OR due* OR contribution* OR uniform* Countries: Afghanistan, Samoa, Bangladesh, Pakistan, Bengal, Benin, Burkina Faso, Burundi, Cambodia, Chad, Africa, Comoros, Congo, Zaire, Somalia, Eritrea, Ethiopia, Gambia, Ghana, Guinea, Haiti, Kenya, Korea, Kyrgyz Republic, Lao, Liberia, Madagascar, Malawi, Rwanda, Sierra Leone, Solomon Islands, Tajikistan, Tanzania, Togo, Uganda, Zambia, Zimbabwe Topic: Education	362 537	1 1
Worldwide Political Science Abstracts	(Afghanistan* OR Samoa* OR Bangladesh* OR Pakistan* OR Bengal* OR Benin* OR Dahomey* OR Burkin* OR Volta* OR Burundi* OR Korea* OR Kyrgyz* OR Lao* OR Liberia* OR Madagascar* OR Malaw* OR Nyasaland*) or (Cambodia* OR Khmer* OR Kampuchea* OR Africa* OR Chad* OR Comoros* OR Congo* OR Zaire* OR Mali* OR Mauritania* OR Mozambique* OR Myanmar* OR Burm* OR Niger* OR Rwanda* OR "Sierra Leone" OR "Solomon Islands" OR Tajikistan* OR Tanzania* OR Togo*) or (Somali* OR Eritrea* OR Ethiopia* OR Gambia* OR Ghana* OR "Gold Coast" OR Guinea* OR Haiti* OR Kenya* OR Uganda* OR Zambia* OR Zimbabwe* OR Asia* OR "developing nation" OR "developing region" OR "developing country") and AB=("primary school" OR "secondary school" OR school* OR educat*) and AB=(fee* OR levies OR payment* OR tuition* OR contribution* OR uniform*) and (random* OR evaluate* OR experiment* OR control* OR trial* OR impact* OR effect* OR outcome*)	70	0
International Journal of Education Development	Handsearch		0
Economic Development and Cultural Change	Handsearch		0

Source	Search terms	Yield	Hits
World Bank Research Observer	Handsearch		0
Journal of Development Economics	Handsearch		0
World Bank Economic Review	Handsearch		0

Appendix 2.4: Coding instrument

DFID review: Impact of the abolition of school fees in low-income developing countries CODING INSTRUMENT

Coder:

- Claire Morgan
- Anthony Petrosino
- Trevor Fronius
- Other _____

I. RESEARCHER AND STUDY CHARACTERISTICS

What year was the primary document published?

What was the type of document?

- Book
- Book chapter
- Government report
- Technical report (reports by non-government research firms, e.g., Mathematica)
- NGO/IGO/IFI report (e.g., World Bank, Poverty Action Lab)
- Journal (peer reviewed)
- Dissertation
- Conference paper
- Other

What was the type of study?

- RCT
- QED with equating of groups
- Other

What was the methodology used? _____

What was the type of intervention?

- Universal free primary education
- Elimination of all school fees for targeted group
- Elimination of tuition fees for targeted group
- Elimination of uniform fees for targeted group
- Elimination of textbook fees for targeted group
- Other _____

Who implemented the policy/intervention?

- Government
- NGO/IGO/IFI
- Other _____

What is the quality of the study methodology?

- High
- Medium
- Low

In what country did the evaluation take place? _____

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)

What other information was provided on the context for the evaluation?

Baseline enrolment data: Males _____ Females _____

(Use enrolment 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 enrolment (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 enrolment (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?

- No treatment group
- Wait-list control
- Treatment as usual group
- Placebo
- Lesser dose of same treatment
- Entirely different treatment than experimental group
- 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 Pls using if providing income/wages)

V. OUTCOMES

SIMPLY INDICATE THE EDUCATION AND NON-EDUCATION OUTCOMES AND WHEN REPORTED (TIME INTERVAL)

<i>Education/learning outcome</i>	<i>Outcome measurement at what time intervals (only those in which data points are reported, e.g., 6 months, 12 months, etc.</i>	NON-EDUCATION OUTCOMES	<i>Outcome measurement at what time intervals (only those in which data points are reported, e.g., 6 months, 12 months, etc.</i>
<i>Enrolment</i>			
<i>Attendance</i>			
<i>Drop-out</i>			
<i>Test scores</i>			
<i>Grades</i>			
<i>Other (list each in a new row)</i>			

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

(include notes on scale and sustainability of intervention)

Appendix 2.5: Structured cases for 17 empirical studies in final screening

(* indicates primary impact evaluation used in the review)

*Alloush M (2010) Does free schooling fill the seats? Evaluating the changes in educational attainment associated with abolishing school fees in four African countries.*⁶

Background/rationale for the study

Africa lags behind rest of world by all measures of economic development. Primary completion rate was 63 percent in 2007. Education is especially low amongst the poorest and most rural households. Abolishing school fees has been a widely adopted intervention to increase access to education but few studies evaluate this policy. Ethiopia, Malawi, Tanzania and Ghana have instituted free primary education (FPE) policies in the past 20 years. These countries vary in wealth, size and baseline education indicators and these characteristics of the data should help formulate a stronger and broadly applicable analysis.

Ethiopia: Large and diverse eastern African country. Primary school completion was 47 percent in 2007. School fees for grades 1-10 were abolished in 1994 but took two years to fully implement.

Malawi: Small country in south-eastern Africa. Primary completion rate was 55 percent in 2007. School fees for grades 1-8 abolished in 1994.

Tanzania: south-eastern African country. Primary completion rate was 85 percent in 2007. School fees abolition came as part of a comprehensive poverty reduction plan in 2001; came with expansion policies. The country has also enjoyed steady growth and stability in recent years so this may contribute to better educational outcomes.

Ghana: Coastal West African country. Primary completion rate was 78 percent in 2007. Abolishing fees for grades 1-9 came as part of larger educational reform and poverty reduction plans. A pilot fees reduction programme was initiated in 40 poorest districts in 2003 and expanded in 2005.

Setting/participants

See above.

Intervention

See above: No further details about fees abolition.

Type of design

Takes advantage of sudden implementation of school fees elimination policies across all primary grade levels, allowing the authors to draw comparisons between people who just missed being eligible for free schooling, others who were eligible from the beginning of their schooling and another group that became eligible at different stages in their primary schooling.

Regressions: Uses a probit model to test hypothesis that abolition of school fees significantly increased the likelihood of completing primary education. OLS (ordinary least squares) regression evaluates the impact of the policy on years of schooling for individuals.

⁶ This structured abstract includes material excerpted from the report.

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

Surveys were conducted a few years after the school fees elimination policies were implemented so age of individuals could be used to identify a pseudo-treatment and control group based on the time they were eligible to be in primary school.

HOW WERE UNITS ASSIGNED TO GROUPS?

N/A

WAS THERE PRETEST OR BASELINE DATA?

No.

WHAT DATA WERE COLLECTED?

Four (one for each country) demographic and health surveys (DHS) on household characteristics, education, health, marital status, fertility preferences, mortality rates, nutrition, women's empowerment, knowledge of HIV/AIDS and employment.

Ethiopia: 2005 survey conducted under the Ministry of Health. 1994 survey used as a sampling frame. A nationally representative sample of 14,070 women (96 percent response rate) and 6,033 men (89 percent response rate) was interviewed.

Malawi: 2004 follow-up to previous surveys in 1992 and 2000 by the National Statistical Office. It used the 1998 Malawi census as a sampling frame. A nationally representative sample of 13,664 households (98 percent response rate), 11,698 women (96 percent response rate), and 3,261 men (84 percent response rate) was interviewed.

Tanzania: HIV/AIDS and Malaria Indicator Survey 2007-2008, which has all elements of standard DHS surveys but includes more specific information on HIV/AIDS and malaria. The survey was conducted by the National Bureau of Statistics. Used the 2002 population and housing census as a sampling frame. A nationally representative sample of 9,343 women (96 percent response rate) and 6,975 (88 percent response rate) men was interviewed.

Ghana: Survey conducted in 2008 by the Ghana Statistical Service. It was a follow-up to four previous DHS surveys. The 2000 Ghana Population and Housing Census was used as sampling frame. A nationally representative sample of 11,778 households was taken, and in half of these households 4,916 women (97 percent response rate) and 4,568 men (96 percent response rate) were interviewed.

For Ethiopia and Malawi, samples were restricted to individuals who were past primary school age at the time of the survey. Including younger individuals would have distorted the sample since their educational attainments are naturally lower. In Ghana and Tanzania, surveys were conducted very soon after the policy was implemented so it was difficult to test effects of the policy on primary completion and the authors used primary attainment of enrolment as the outcome.

WHAT KINDS OF ANALYSES WERE DONE?

Each dependent variable graphed over time to see if difference in enrolment decisions between different age groups is not just part of consistent trend over time. Trends take on logistic curve trend over time.

Regression analysis to identify the difference between the estimated trend over time without the policy and the actual trend. Controlled for household factors that literature has shown to have an effect on enrolment decisions.

Same regressions run in OLS with total years of schooling as dependent variable.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

Used individual cross-sectional survey data for each country collected several years after the abolition of school fees so the analysis is retrospective and cannot check for robustness of the model by adding several relevant controls. Limited ability to

remove variation from an error term increases the problem of correlation between dummy variables and the error. Analysis partly deals with omitted variable bias - the model allows detection of changes associated with implementation of the policy. The model will give biased estimates if there is an omitted variable that changed contemporaneously with the policy change and in turn affected the dependent variables. (Review of the economic/political history of time periods in each country shows the existence of such a change to be unlikely.)

Since children who were already of primary school age at the time of policy implementation were not fully exposed, there is a six- to eight-year (depending on length of primary education in each country) time lag between two main comparison groups (fully exposed vs not exposed), allowing the possibility of a significant change in control variables. This could affect dependent variables and weaken the causality chain.

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

For two countries with 10 or more years of time lag between when the policy was implemented and the survey administered, coefficients are positive and significant. People exposed to the policy were 25-30 percentage points more likely to complete primary school and 6-38 percentage points more likely to have ever enrolled in primary school and exposed people had 1.4-2.2 more years of education than those not exposed.

Ethiopia: People exposed to the policy were 31.5 percentage points more likely to complete primary education compared to those that just missed the policy and 19.5 percentage points more likely to ever enrol in school. Fully exposed people had 2.25 years more education than those that just missed exposure. Results were independent of time trend.

Malawi: People exposed to the policy were 9.1 percentage points more likely to complete primary school, 8.7 percentage points more likely ever to enrol, and have 0.8 more years of education than those who just missed exposure.

Tanzania: Not enough time had passed to allow students who were fully exposed to the policy to finish primary school. Those fully exposed were 14.9 percentage points less likely to have completed primary school but 12.8 percentage points more likely to have ever enrolled in school and had 0.527 more years of education than those just missed exposure.

Ghana: Survey was conducted 3-4 years after policy implementation so results are negative and significant for primary school completion and years of education, but 37.3 percentage points more likely to have ever enrolled in school.

Results also showed likelihood of finishing disaggregated stages of primary education (people exposed to the policy were significantly and more likely to finish each of the primary grades).

Authors tried to decrease the impact of the time lag by analysing the effect of the policy on children partially exposed to it by breaking the dummy into separate dummy variables for each grade. Results showed continuity in the effects of the policy: Children partially exposed were more likely to complete primary school than those that just missed exposure.

HOW LONG WERE PARTICIPANTS FOLLOWED?

N/A

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

FPE was associated with substantial increases in the likelihood of ever enrolling in school and completing primary school. It was also associated with significant increase in the years of schooling a child achieves. Even students partially exposed to the policy were more likely to complete primary school than those not exposed at all. The longer a person was exposed, the more likely was primary completion.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

Not given.

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

Further analysis in these countries about long-term effects of the policy would further fill gaps in literature. Seeing what the policy meant for future employment and health would provide answers about its effectiveness. But a large time lag would further complicate analysis and make it hard to disentangle the effects of the policy from other factors.

The paper does not address how school fees abolition affected quality of education.

*Arends-Kuenning M, Amin S (2004) School incentive programs and children's activities: The case of Bangladesh.*⁷

Background/rationale for the study

Growing literature documents an impact of CCTs on schooling but less is known about whether programmes change children's out of school activities and labour. We also do not know how programmes impact on work of other family members (who might have to work harder because others are in school). Bangladesh was one of first countries to implement these types of school incentive programmes. In 1990, 60 percent of girls and 68 percent of boys who were of primary-school age attended primary school. Previous interventions such as merit scholarships, flexible scheduling, free primary tuition and books, and building more schools, had limited success and gradually and cumulatively increased enrolment but did not address the cost of lost labour. This paper examines three programmes – Food For Education (FFE), BRAC (Bangladesh Rural Advancement Committee) schools and – the one we are interested in – the Secondary School Scholarship Program (SSSP) funded by the World Bank which is available to all girls who attend secondary school, waives all school fees and pays the girl a monthly stipend for attendance and not marrying. In rural Bangladesh, finding resources for schooling girls is less urgent than securing a good marriage.

This article reports on an investigation of time-use data collected in two Bangladeshi villages in 1992, 1995 and 1996; in-depth interviews conducted in 1995; responses to two village censuses conducted in 1992 and 1995; and data from an education survey conducted in 1996. The time-use data, which were collected by questioning respondents about the previous day's activities, illuminate children's time allocation in greater detail than do studies that define child labour based on household survey questions that ask about each child's normal activity in the previous week. The time-use data were collected before and after the schooling incentive programmes started in 1994, providing a natural experiment.

Setting/participants

Two villages typical of Bangladesh. School attainment was low, and the gender gap in school attainment was large before schooling incentive programmes were initiated.

Research questions

How do the school incentive programmes affect the amount of time that children spend studying and attending school, and do the effects differ by gender and age group?

Do school incentive programmes reduce children's time spent working in agriculture, in housework and in wage work?

Do married girls spend more time working in response to the increased time their unmarried sisters-in-law spend in school?

Intervention

The incentive consists of monthly stipends deposited in a girl's bank account if she attends secondary school during 65 percent of the days that school is in session and maintains a passing grade each year. The stipend ranges from US\$1 to \$2 per month, depending on the grade level. The programme also provides additional subsidies to secondary schoolteachers based on enrolment. In 1994, only girls enrolled in grades 6 and 9 were eligible to enter the programme. The programme

⁷ This structured abstract includes material excerpted from the report.

was expanded to include girls in grades 7 and 8 starting in 1996. Parents of girls in the programme are required to sign an agreement that their daughters will not be married before reaching 18 years of age. The programme is available to all girls in secondary school, regardless of economic status.

Type of design

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

Before/after; no control.

HOW WERE UNITS ASSIGNED TO GROUPS?

WAS THERE PRETEST OR BASELINE DATA?

Baseline survey in 1992.

WHAT DATA WERE COLLECTED?

Time-use data collected in 1992 from a sample of 240 households, containing a total of 890 individuals. The 240 households consisted of a 25 percent random sample of male-headed households and all female-headed households in two villages. All results are weighted so that the results are representative of the villages' populations. The data are longitudinal; the sample was drawn in 1992, and respondents were re-interviewed in 1995 and 1996. Each household member older than 5 years of age contributed observations and was asked to recall sequentially all activities conducted in the 24-hour period prior to the interview. Activities were recorded in daily charts and coded according to a detailed activity list. Supervisors checked forms carefully for accuracy and frequently revisited sample households.

Authors present two separate sets of results, one comparing August 1992 and August 1995 and another comparing May 1992 and May 1996, for three reasons. The first is associated with seasonality. The 1995 and 1996 surveys were chosen to cover a peak (May) and a low season (August) for agricultural labour demand and months when school is in session. Therefore, one can see how time allocation changes in response to the agricultural season and can examine one additional year of households' adaptation to the schooling incentive programmes. Second, some time-use data are missing in each round because the respondent was absent at the time of the interviews. The 1995 data are more complete and include fewer absences, so it is worthwhile to present the 1992 and 1995 comparisons. In 1996, more children were absent because the interviews occurred during a time of peak agricultural labour demand, and the time-use data were collected at the same time that an education survey was administered. Children did not want to take the exam that was part of the survey, and so they hid and refused to be interviewed. Third, only in 1996 did we know which children were participating in school incentive programmes because of the education survey administered in that year.

Data classified the children by gender and by two age groups: 6-10 and 11-19.

Because of sample attrition, the data are treated as a series of cross-sections rather than as a panel. Young adults aged 15-24 are likely to leave the household to marry if they are girls, or to emigrate to find work if they are boys. Limiting the analysis to observations from individuals who have valid observations in both periods would bias the results, because the young adults who stay in the village are different on both observable and unobservable dimensions from the young adults who leave. The differences would make those who stay more likely to be enrolled in school than those who leave. Therefore, observations are used from all of the young adults who were present in the sample household at the specified time regardless of whether they were present at other times. In addition, children age into and out of the sample. The unit of data collection is the household, so that

young women who marry into the household are included in subsequent data collection.

The village data describe a natural experiment. The schooling incentive programmes were started in 1994, after the May and August rounds of data collection in 1992. The programmes were national in scope, and no evidence suggests that they included the study villages because of a perceived high demand there for schooling.

WHAT KINDS OF ANALYSES WERE DONE?

Before/after, difference in means t tests.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

Small sample size and the lack of an experimental design prevent gauging the separate impact of each of the three programmes (but BRAC schools did not affect children in middle and higher grades because that programme was only for grades 1 and 2)

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

Changes in children's time for schooling (contribution of factors—enrolment rates, time spent in school and studying, changing grade distributions): Children spent more time in schooling activities in 1995 and 1996 than in 1992.

Results by age and gender: Girls aged 11-19 most impacted.

Time spent in work outside of school by type of work (household, agricultural, wage): Girls did not decrease housework time although they increased school time.

Marital status: Principal determinant of how adolescent girls spent time.

Note: It was not possible to separate the effects of SSSP from the FFE programme.

HOW LONG WERE PARTICIPANTS FOLLOWED?

Four years.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

Increases in enrolment, increases in the amount of time students spent on schooling activities and changes in the grade distribution all played a role in the increase in the time that children spent in school. Older girls, especially, experienced increased enrolments and changes in the grade distribution from lower to higher grades.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

Changes observed are consistent with the view that parents do indeed respond to incentives and it is not necessary to eliminate all poverty in order to induce children to attend school (payments are small and do not replace lost labour). On average, children did not work such long hours before the incentives that they would be prevented from attending school.

Adolescent girls, who received a scholarship and had their fees waived, increased their schooling more than did adolescent boys, who had no scholarship and had to pay fees. At the same time, adolescent boys increased the amount of time that they spent in wage work, suggesting that the incentives programmes might have created disincentives to send adolescent boys to school as compared with girls.

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

Despite the considerable success of FFE and SSSP, not all children attended school. Future research might examine why the incentives were insufficient for these vulnerable children.

*Bold T, Kimenyi M, Mwabu G, Sandefur J (2009) Free primary education in Kenya: Enrolment, achievement, and local accountability.*⁸

Note: This is a PowerPoint presentation for which there does not seem to be an accompanying paper.

Background/rationale for the study

Fees were abolished in Government schools in 2003. Funding for non-salary expenditure comes as a capitation grant from the Government. Teachers are recruited and paid centrally.

Increased centralisation has led to loss of local accountability. School management committees (SMC) have official governing authority for each school. SMCs no longer raise funds for schools so their governance power is undermined. The net effect on total funds of FPE is ambiguous due to the abolition of local fundraising.

There have been changes in pool of students owing to greater access with FPE so students may be of different SES, age and ability.

Setting/participants

Nation-wide sample.

Research questions

How has household educational expenditure changed with FPE?

Did abolishing fees drive students away? (how have public/private shares of enrolment have changed?)

Has FPE reduced inequality in educational attainment?

Type of design

Before/after regressions.

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

Looked at the same group before and after the intervention.

HOW WERE UNITS ASSIGNED TO GROUPS?

N/A

WAS THERE PRETEST OR BASELINE DATA?

Before/after

WHAT DATA WERE COLLECTED?

Data from two household surveys, one prior to and one after FPE, were combined:

- Welfare Monitoring Survey, 1997: 10,874 households nation-wide; 13,639 children aged 6-13.
- Kenya Integrated Household Budget Survey, 2006: 13,212 households nation-wide; 14,610 children aged 6-13.

Overlap between surveys: SES indicators (household consumption, assets, education of household head); child's current enrolment status, grade level, school type; expenditure on education, fees, and other categories.

School-level data: Census of all primary schools spanning the period before and since FPE. Standardised test scores and data from the Education Management Information System (panel of schools 1998-2005; average of 15,000 schools per

⁸ This structured abstract includes material excerpted from the report.

year; includes data on enrolment by grade, age, gender; and experience/qualifications of teachers).

WHAT KINDS OF ANALYSES WERE DONE?

OLS/LPM (linear probability model) regression: Cost of schooling in Kenya before and after FPE, distinguishing between fees and other expenditures, and emphasising the contrast between the public and private system.

Also investigated: Whether the relationship between socio-economic indicators and enrolment status has weakened since the introduction of FPE.

Fixed effects regressions conducted to show the rise of private sector and decline of public performance.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

None.

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

Since FPE, fees for public primary schools have fallen to zero, and total costs have nearly halved. The cost of private education has more than doubled - it is now more than 20 times more expensive than public education.

Inequality in education access has declined by preferred measure. However, enrolment rates for public schools have actually declined; as poor students have come in, richer students have fled to private schools in greater or equal measure. The net effect is an accelerated decline in the average SES of public school students. This coincides with a rise in pupil:teacher ratios (due to a hiring freeze plus an influx of overage students).

There is a large performance gap between private and public schools. The flight to private schools is associated with a fall in public school performance.

HOW LONG WERE PARTICIPANTS FOLLOWED?

Nine years.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

See above.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

Not given.

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

Not given.

*Deininger K (2003) Does cost of schooling affect enrolment by the poor? Universal primary education in Uganda.*⁹

Background/rationale for the study

UPE in Uganda was implemented rapidly and aimed to eliminate the cost of primary education for up to four children per household. Few studies have investigated the impact of large-scale school expansion programmes - other impediments may preclude human capital acquisition by the poor even if financial constraints are removed. There is also a need to look at the trade off between access and equity, with implications for sequencing reforms and distribution of benefits (e.g. increased access favours the poor but increased quality favours the already enrolled).

Setting/participants

Nation-wide sample.

Research questions

To what extent can a well-publicised programme that reduces costs of acquiring education contribute to higher levels of human capital acquisition?

How much of it is due to cost and does the restructuring of existing patterns of educational spending provide a realistic option for refocusing the incidence of educational spending, especially in Africa where misalignments appear to be most severe?

What does the evidence reveal on the relative balance between increasing access and increasing quality in the sequencing of educational reforms?

Intervention

UPE took effect in Uganda in 1997, eliminating the cost of primary schooling for up to four children per family, two of which had to be girls. It abolished direct and indirect contributions and fees for uniforms. It was combined with a strong dissemination campaign, highly decentralised mode of implementation, and far-reaching restructuring of public spending in education. There was also an accompanying media campaign for girls' education and against early marriage. Since the early 1990s, the overall education budget has increased from 1.6 percent to 3.8 percent of GDP. Compared to less than 40 percent in the early 1990s, 70 percent of the education budget was allocated to basic education in 1997/98.

Type of Design

Before/after regressions.

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

Looking at same group before and after intervention.

HOW WERE UNITS ASSIGNED TO GROUPS?

WAS THERE PRETEST OR BASELINE DATA?

Baseline survey before intervention took place.

WHAT DATA WERE COLLECTED?

Relied on two large household surveys, including a community module, undertaken before and after the intervention. The nationally representative 1992 Uganda Integrated Household Survey (UIHS) provides data for slightly fewer than 10,000 households with almost 50,000 individuals, 17,126 of which were between 6 and 18

⁹ This structured abstract includes material excerpted from the report.

years of age. It is complemented with information on about 6,000 households (with more than 30,000 individuals, and 14,868 in the 6-18 age group) from the 1999-2000 Uganda National Household Survey (UNHS).

WHAT KINDS OF ANALYSES WERE DONE?

t tests.

Cross-section estimates (regressions). Estimate regression for different age groups for primary and secondary attendance plus personal and household characteristics.

Regressions to assess costs of school enrolment at different levels.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

Survey data alone do not allow ruling out the possibility that primary attendance increased gradually during the period and can thus not be attributed to the implementation of UPE. This is of concern as the 1992 data were obtained five years before UPE. To deal with this issue the survey-based figures on attendance were complemented with official statistics on enrolment collected annually by the Ministry of Education. Evidence from both sources consistently points toward a large and discontinuous increase in enrolment in 1997, the year of introduction of UPE. Official figures suggest an increase that is higher by between 20 percent and 30 percent than the survey data (using the expansion factors).

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

Descriptive statistics showing before/after primary and secondary enrolment by children who should have started before UPE was in place and those who were aged 6-8 and started when it was in place. Also information provided about enrolment by age classes and reasons for non-enrolment in primary school by region and urban/rural. In addition, information on per capita income distribution and for girls given.

Regressions confirm descriptive statistics, i.e. a significant expansion of primary schooling, a reduction in the role of income as a constraint on enrolment, and elimination of gender bias. All three regressions for primary attendance show a highly significant and positive time trend, implying that the probability of primary attendance increased for everybody in the sample. But there was no large expansion in secondary enrolment.

Analysis of the cost of education showed that progressivity of educational spending has clearly increased, leading to increased educational opportunities. But the dramatic increase in primary attendance has not been accompanied by a commensurate increase in the number of teachers, leading to a big increase in student:teacher ratios. Quality of school buildings and furniture, two variables that are to a large extent affected by community-level inputs, increased considerably during the period of concern, pointing toward the scope of the reforms to alter incentives at the local level and prompt in-kind contributions (as well as possibly other parental inputs) into the education process.

Comparison of the cost of primary schooling and secondary and higher education indicate that whilst the cost of primary education has decreased significantly, that of higher education has increased.

HOW LONG WERE PARTICIPANTS FOLLOWED?

Seven years.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

UPE has greatly reduced the wealth bias that had characterised access to primary education in 1992; helped to establish gender equality by increasing girls' access to

primary education; and reduced the incidence of cost-related drop-outs from primary school. The strong evidence for a significant increase in delayed enrolment by children in the 12-18 age bracket provides an indication that, even though not all of the impact of UPE can be explained via the cost of education, such factors did indeed have an important impact.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

Implementation of UPE has resulted in a significant reduction of the costs of school attendance which has opened the door to primary enrolment for poor rural households who previously appear to have been excluded. In addition to their potentially large impact on the poor, the budgetary cost of implementing such programmes can be reduced by changes in the overall cost structure of education which combines cost reductions at the primary level with increased cost recovery at higher levels.

But holding constant for school quality, the decrease in cost of schooling disappears. Although the out-of-pocket costs of primary schooling have decreased, this suggests that, at least in the short term, and consistent with earlier findings, one consequence of the increased inflow of students into the system was a reduction of quality. It is thus impossible to reject the hypothesis that, in quality-adjusted terms, there has been little change in the cost of primary education. Even though it is a major achievement to reduce the cash cost of education and thus increases in attendance have been made, the next step of transforming this into higher-quality education still remains to be accomplished.

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

Not given.

*Fafchamps M, Minten B (2003) Public service provision, user fees, and political turmoil.*¹⁰

Background/rationale for the study

Following an electoral dispute, the central highlands of Madagascar were subjected to an economic blockade during January - June 2002. After the blockade ended, school fees were progressively eliminated. By June 2003, all provinces had eliminated school fees and user fees in health care. The blockade and subsequent removal of user fees provides a natural experiment from which to examine the resilience of public services to massive shocks and the effect of a removal of user fees on public service provision.

This paper is unique because massive changes took place over a very short time period so location-specific factors affecting demand and supply of public services can be regarded as constant and can be controlled for using fixed effects. Under normal conditions, short panels convey little information because changes occurring over a short period are typically small in magnitude so measurement error weakens econometric results. This is less true in this case.

During the crisis working hours in schools were reduced. Some 56 percent of schools experienced interruptions in teaching. Classes were cancelled for 58 days on average. After the end of the strikes, the teaching schedule was reorganised to address transportation problems caused by fuel shortages, i.e. hours of class per day were reduced. The pass rate for standardised exams showed a 13 percent decline and absenteeism rose by 20 percent (caused by child labour - although demand for this always increases during April/May in rural areas - and inability to pay school expenses during the economic crisis).

Setting/participants

Madagascar: Island country of 16 million people off the East coast of Africa.

Intervention

The new president announced that the Government would waive tuition fees for the 2002/03 school year. Each student inscription would lead to an extra allocation of 10,000 Fmg (Franc malagache) toward the school. (But funds did not arrive promptly in all areas and a significant number of schools asked parents to pay tuition fees to deal with the liquidity problem.)

Type of design

Before/after regressions.

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

Looked at the same group before and after intervention.

HOW WERE UNITS ASSIGNED TO GROUPS?

N/A

WAS THERE PRETEST OR BASELINE DATA?

Baseline survey before the crisis.

WHAT DATA WERE COLLECTED?

Data on public service provision were collected immediately before the crisis in December 2001, at the height of the blockade in June 2001, and during the period of gradual removal of user fees in November 2002. Also from surveys to evaluate

¹⁰ This structured abstract includes material excerpted from the report.

the effects of the political turmoil, the policy changes on the functioning of the economy and the delivery of social services. Most questions asked for recall about periods before, during, and following crisis.

Two primary schools were sampled in each commune - one central and one remote - with a total of 284 schools visited.

WHAT KINDS OF ANALYSES WERE DONE?

Fixed effects regression: Variation across the sample in changes across the period used to isolate the respective effects of supply and demand shifts. Student enrolment used as a measure of quantity of education services consumed by a population (in logs, as well as school fees).

Instrumenting regressions: Re-estimated model with additional controls for demand and supply shifters. Instrument number of books and indicator of school quality.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

None,

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

Descriptive data show: Primary school enrolment in 2002/03 increased dramatically - by 15 percent over the previous year. The number of first-grade students increased 20 percent. School staff reported that the increase was due to a decrease in school costs. Twenty percent of schools had to refuse students owing to lack of space, so enrolment could have been even higher.

Fixed effects: Coefficients of the time dummies suggest that the blockade and subsequent recovery had large effects on the provision of public services across the sample. Elimination of school fees was associated with a 6.5 percent increase in school enrolment. The order of magnitude of this elasticity may seem small, but measures a short-term response to a temporary measure; if abolishment were made permanent a larger response would be expected. Also, older children who had never been to school or dropped out at a young age are unlikely to re-enter school in response to a temporary change in fees.

Instrumenting regressions: Blockade was associated with a 3 percent drop in school enrolment and elimination of fees resulted in a 9 percent increase in enrolment. Free distribution of books raised school enrolment but not significantly. Instrumented results by and large resembled uninstrumented ones.

HOW LONG WERE PARTICIPANTS FOLLOWED?

One year.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

There was a significant increase in enrolment after elimination of user fees.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

The education sector exhibited the least effects of the shock (compared to health and other sectors). The Government adjusted its policy in face of the shock. The study shows high resilience of service delivery. Most of the drop in demand was due to lack of income and an increase in poverty with rising opportunity costs.

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

In order to mitigate effects of macro-economic shocks on access to public services, more attention should be paid to the demand side whilst keeping in mind that long-term financial viability of public service delivery is essential to ensure continued provision.

Khandker S, Pitt M, Fuwa N (2003) Subsidy to promote girls' secondary education: The female stipend program in Bangladesh.¹¹

Background/rationale for the study

The female share of secondary enrolment was 38 percent in 1994, the initial year of programme intervention. In 1991, only 5 percent of rural girls completed tenth grade compared to 12 percent of boys.

The nation-wide stipend programme implemented by the Government of Bangladesh with donor support started in the early 1990s to increase rural female enrolment at the secondary level, improve secondary completion rate for girls, and increase female age at marriage.

Setting/participants

The intervention was targeted to girls in grades 6-10 in 460 rural districts starting in 1994. Once a school participates in the programme, all girls satisfying criteria receive the intervention.

Intervention

Full tuition is paid directly to the school and a subsidy covering up to 50 percent of other fees (books, uniforms, etc.) is deposited in girls' bank accounts, subject to criteria of 45 percent of class-level test scores, attending 75 percent of school days, and staying unmarried.

Type of design

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

No real comparison group: Study compared girls to boys and used variation in programme phase-in as sources of parameter identification.

HOW WERE UNITS ASSIGNED TO GROUPS?

WAS THERE PRETEST OR BASELINE DATA?

No pretest. Baseline data came from first survey round.

*WHAT DATA WERE COLLECTED?

Two data sources:

- Management Information System (MIS) of the World Bank-funded Female Secondary School Assistance Project (FSSAP) provides enrolment data for FSSAP schools by grade and gender since the stipend programme was introduced in 1994.
- Published Government statistics on nation-wide school enrolment collected by the Ministry of Education.

FSSAP data are available from 118 districts (out of 460) in rural Bangladesh (includes only female-only and co-educational schools).

In addition, a 1991/92 survey was conducted in households and schools in 32 randomly drawn subdistricts throughout Bangladesh (out of 460 subdistricts covered by a subsidy programme). In some 1,800 households from 87 villages in 29 subdistricts, information was collected on labour supply, income, employment, expenditure, borrowing and savings. A school-level survey covered 687 primary, secondary and post-secondary schools attended by members of the households

¹¹ This structured abstract includes material excerpted from the report.

surveyed (information was collected on student enrolment by gender and class and on number of teachers and their gender and qualifications, etc.).

In a 1998/99 follow-up survey, the same set of households and schools plus an additional 180 households selected from nine villages drawn randomly from three new subdistricts (from south-eastern Bangladesh which had been excluded from the first survey because of a cyclone). The re-survey also added more households from the original 87 villages covering a total of 2,599 households and 889 schools.

Programmes were not in operation in any village during the first round but operated in every village during the second survey round. The only source of programme variation is the date of its introduction between the two rounds. (Programme duration was variable reflecting the date at which any school serving children of a village became associated with FSSAP.)

WHAT KINDS OF ANALYSES WERE DONE?
ixed effects regression.

*WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

FSSAP school enrolment increases are probably only slightly inflated in the initial years by the transfer of girls from non-FSSAP schools to FSSAP schools. By 1998, 98 percent of all rural secondary schools that enrol girls were part of this programme.

The predecessor to the FSSAP programme for girls in grades 6-8 is likely to have increased the number of girls 'at risk' for continued secondary schooling at time the FSSAP programme was being introduced. So the effect estimated in the present study is certainly an underestimate of the full effect of the school subsidy programmes at all levels of schooling on secondary school enrolments during the period of the study. Also, households may have anticipated the broadening of the predecessor programme to other grades and kept their daughters in school.

Possible endogeneity of the date of programme introduction across the villages is noted above. Fixed effect conditional logit models are estimated to sweep out village-level heterogeneity that may both affect the determinants of the dependent variable and individual school enrolment, and the timing of the introduction of the FSSAP programme into villages. These data are sufficient to estimate the marginal effects of FSSAP on school enrolments but insufficient to identify the average effects of the programme.

For school-level estimate, the unit of analysis is the school rather than the decision-making unit - the household. Schools may be expanding beyond the trend effect in response to the programme and to the extent that school enrolment decisions were constrained by school capacity, this is included as a programme effect.

New secondary schools opened during this period so the study may underestimate programme effects (only schools in operation in 1994 are included in the panel).

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

Girls' school enrolment and grade attainment well exceeded that of boys at the end of the 1991-98 period. Boys aged 11-18 years in the sample had a secondary enrolment rate of 47 percent in 1991/92 and 58 percent in 1998/99. Girls of the same cohort had rates of 44 percent in 1991/92 and 60 percent in 1998/99. Mean years of schooling for girls increased from 3.0 to 4.4 years, and for boys from 3.2 to 4.1 years. The transition to secondary school for girls grew from 27 percent to 45 percent and for boys from 27.5 percent to 40 percent. But secondary completion rate declined for girls and increased slightly for boys over the period.

The duration of the FSSAP programme in the village does not have a significant effect on school enrolment for either gender when village-level heterogeneity is not controlled for but is positive and significant for girls when using village fixed effects.

The effect of the secondary stipend on school enrolment may depend on child age. In model 2, dropping children of 11 and 12 years from the sample leads to an appreciable rise in the estimated effect of stipend duration. With the limited variation on stipend duration available in these data, authors allow for age-varying stipend effects through an age-stipend interaction term without a level stipend variable.

Although the analysis of household survey data demonstrates positive effects of the stipend programme, parameter identification rests on the limited variation in programme duration across the villages. Results would be strengthened with confirmatory results from an analysis based on a different dataset, so authors make use of aggregate school-level information from MIS data (schools included in this dataset provided annual information from 1994 to 1998 only). The basic source of identification is the same - not all schools were enrolled in the stipend programme at the same time - but another source of variation is that not all grades were treated in the same year. Both sources of variation observed at school level are exploited to estimate the programme's effect in terms of intra-school variation in programme exposure.]

Results using MIS school-level data show that the duration of the stipend programme has a significant positive impact on female class enrolment and a significant negative effect on male class enrolment in FSSAP schools. One additional year of programme exposure increases female class enrolment rate by 8 percent and reduces male class enrolment rate by 29 percent (although boys could have switched schools, so it only shows that gender composition of co-educational schools was strongly affected).

School enrolment benefits accrue disproportionately to girls from households with larger quantities of owned land.

HOW LONG WERE PARTICIPANTS FOLLOWED?

Seven years.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

The programme increased girls' secondary education substantially but had no discernable impact on schooling of boys.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

The programme is very costly to operate, requires donor support and may not be feasible to sustain.

The need to consider how the programme can best be targeted - perhaps toward the land-poor?

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

Maikish A (2010) *Universal primary education and decentralization in Ghana*.¹²

Background/rationale for the study

Ghana 2002–2008 saw implementation of a Government-initiated capitation grant scheme to bolster the policy of providing universal tuition-free basic education through a decentralised governance framework transferring responsibility for the provision of education to the local level.

The paper uses a mixed methods approach: it analyses recent Ghanaian education policy and strategy documents as well as legal documents and data from qualitative fieldwork in five districts and the national headquarters of the Ghana Education Service and Ministry of Education to provide context. It measures the impact of the capitation grant scheme on education enrolment and quality in Ghana through growth model analyses on educational census since 2002. It compares and contrasts two districts that were identified by quantitative study as showing above-average enrolment growth with two districts that experienced below-average enrolment through qualitative fieldwork in each of the four districts as well as in the national headquarters of the Ghana Education Service and Ministry of Education.

Setting/participants

Universal policy. Looked at data from 110 districts throughout Ghana.

Research questions

Following the implementation of Ghana's capitation grant scheme,

Is there an overall change in: (a) Net student enrolment? (b) Pupil:teacher ratios?

Does district deprivation status moderate the change in: (a) Net student enrolment? (b) Pupil:teacher ratios?

Controlling for district deprivation status, (a) Does net student enrolment differ across districts? (b) Do pupil:teacher ratios differ across districts?

Intervention

Type of design

MANOVA (multivariate analysis of variance) and MLM (multilevel modelling).

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

HOW WERE UNITS ASSIGNED TO GROUPS?

WAS THERE PRETEST OR BASELINE DATA?

Baseline year was 2002.

WHAT DATA WERE COLLECTED?

Study utilises student enrolment, population and teacher enrolment data compiled by the Ghana Ministry of Education, Science and Sports in collaboration with the UNESCO (United Nations Educational, Scientific and Cultural Organization) Institute for Statistics and aggregated at district level (N=110) clustered into geographic regions (N=10). Districts were studied over a five-year time period. The predictor *deprived* was used to indicate whether a district was categorised as deprived in 2004, and *treatment* to indicate whether a district received capitation grant in a given year.

¹² This structured abstract includes material excerpted from the report.

WHAT KINDS OF ANALYSES WERE DONE?

The growth of the net enrolment ratio in basic school (grades 1 through 6 measured from 2002/03 to 2006/07 in Ghana.

To measure quality, the pupil:teacher ratio was used: Total student enrolment at the basic school level divided by total number of teachers at that level.

RMANOVA (repeated measures analysis of variance) was used to measure whether there are systematic mean changes across time that are not due to chance in terms of both overall net enrolment and pupil:teacher ratios.

MLM was used to investigate whether change over time for both overall net enrolment and pupil:teacher ratios varied by individual districts and not merely at aggregate mean level across districts. It was then determined whether individual differences amongst districts systematically related to known measurable variables such as deprivation status and district size.

In addition: Covariance structure was tested for random effects. Normality assumptions were checked. Region effects were tested. Post-hoc tests were run.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

It was acknowledged that there were limitations in using enrolment data instead of, for example, attendance data and in using pupil:teacher ratio as a quality indicator. Although it might be ideal to include school-level and/or household-level data in analysis but such longitudinal data were unavailable.

Districts varied in initial enrolment rates in 2003 as evidenced by their different Y-intercepts and also had different rates of growth in net enrolment rates across time as evidenced by their trajectories' varying slopes, which violates one of the major assumptions underlying RMANOVA, that of sphericity, and further supports the use of MLM, which not only does not rely on this assumption but allows for the researcher to probe into why such variation occurs. There is also intercept and slope variation across individual districts in regard to pupil:teacher ratios, although perhaps to a lesser extent, suggesting again the use of MLM to investigate in greater detail. So the primary method of analysis is MLM, which allows investigation of whether there are systematic changes across time at an aggregated mean level and also whether there are individual district-level differences in change across time, and whether such differences relate systematically to known measurable variables.

Because the deprived districts received the Scheme in 2004/05 whilst the non-deprived districts received it in 2005/06, in order to utilise RMANOVA, a method that does not accommodate to time-varying predictors, it was necessary to create a new variable, 'years in programme', to measure the number of years a district had participated in the programme. In creating this new variable, it was necessary to eliminate data and lose, what might be, important information from the year 2004/05 for the non-deprived districts (their pre-treatment year) and from the year 2006/07 for the deprived districts. In all, the need to equate districts in terms of their number of years in the programme resulted in a loss of 110 data points, and a consequent loss in power. By contrast, the MLM method does allow for time-varying predictors and avoids the necessity of such loss of information.

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

Whether the mean net enrolment ratio and pupil:teacher ratio of a district varied significantly as a function of the number of years that district participated in the capitation scheme.

The net enrolment ratio increased overall as result of the capitation scheme. The net enrolment ratio for the non-deprived group exceeded that for the deprived group in every year following the inception of the programme. Compared to the non-deprived group, the growth trajectory of the deprived group increased more slowly initially, and then more rapidly in the last year studied.

Despite an initial spike in pupil:teacher ratio post-treatment, the longer-term growth in the pupil:teacher ratio in non-deprived and deprived districts showed an improvement, on average. But improvement in non-deprived districts was significantly better than that in deprived districts.

HOW LONG WERE PARTICIPANTS FOLLOWED?

Five years.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

The per-pupil funding scheme aimed at increasing enrolment in Ghana was successful overall. But widely differing returns to treatment were found in deprived and non-deprived districts. Deprived districts, on average, showed lower returns to treatment than non-deprived districts. Also, whilst localities were successful at getting students into school they did not appear to be as successful at supplying the teachers and classrooms needed to educate the increased influx of students at first. Districts, however, seemed able to adjust the teacher supply to compensate for an increasing number of students in the longer run. Non-deprived districts were more successful at this than deprived districts.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

Differing returns realised by each district in Ghana strongly suggest that policy analysis needs to occur at the district level as national-level analyses mask large differences at the local level.

Deprived districts, on average, show lower returns to treatment than non-deprived districts. Accordingly, in subsequent efforts to promote growth in net enrolment in Ghana, policy-makers need to be more sensitive in designing the scheme for deprived districts so as to bring their gains into line with those of non-deprived districts. Also, not all districts show gains; the returns to each district vary greatly. Accordingly, officials must respond to the nature of these district differences to improve upon the effectiveness of this decentralised, universal education funding scheme. This study indicates that enrolment gains reaped from decentralisation in one locality may be different or offset by losses in another.

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

It is critical that future quantitative data analyses evaluate the effectiveness of the education policy variable at the level at which the policy is targeted, which in Ghana and many other countries in the developing world is the local level. Doing so not only helps to construct a robust picture of true gains, but also has the potential for suggesting follow-up research that could explicate why certain districts are successful in implementing these programmes whilst others are not.

*Muyanga M, Olwande J, Mueni E, Wambugu S (2010) Free primary education in Kenya: An impact evaluation using propensity score methods.*¹³

Background/rationale for the study

A Government-initiated FPE programme began in January 2003. Before this parents covered 35 percent of total costs in primary schools and also were responsible for supplying instructional materials to schools.

There was a big improvement in primary school enrolment after FPE was implemented. GER (gross enrolment ratio) increased from 92 percent to 104 percent. Enrolment of girls grew by 17 percent and boys by 18 percent. This presented challenges including overcrowding, an increased pupil:teacher ratio from 35:1 to 43:1, growth in classroom sizes from an average of 40 to 120 students - and some had to study outside - and shortages of desks and supplies. The quality of education under such circumstances was questionable.

Is programme sustainable? It is supported by the World Bank, DFID and other donors so it is not clear if it would be affordable if it relied solely on domestic resources.

Setting/participants

Implemented nationally for all primary school-aged children.

Intervention

The programme makes a capitation payment to all public primary schools amounting to 1020 Kenya shillings (Ksh) (US\$14.57) per child per year. Of this, 36 percent goes to a general purpose account (wages for support staff, repairs/maintenance, utilities, etc.) and 64 percent goes to an instructional materials account. (Funds are managed by school management committees - SMCs - who have argued that payment allocation for repairs/maintenance is not adequate. If parents wish to charge additional levies, they have to send a request through a bureaucratic and tedious process.)

Research questions

The authors analyse trends in key primary education outcome indicators (school enrolment rates, grade progression, transition to secondary) before and after FPE implementation; identify correlates of outcome indicators; and examine the pro-poorness of the FPE transfer.

Type of design

Before/after PSM (propensity score matching).

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

The only untreated pool from which a comparison sample can be drawn is the eligible population from the period before programme implementation to give a before/after design. *How were units assigned to groups?

Looked at the same group before and after intervention.

WAS THERE PRETEST OR BASELINE DATA?

Baseline was a 2000 survey.

WHAT DATA WERE COLLECTED?

Panel data of school-age children drawn from interviews with 1500 rural households. Interviews conducted before FPE was introduced in 1997 and 2000 and after the programme was implemented, in 2004 and 2007.

¹³ This structured abstract includes material excerpted from the report.

The same households were interviewed in these four waves. Districts were classified into seven agro-regional zones and households sampled through standard proportional sampling aided by national census data. Households sampled span 24 districts, 39 divisions, and 120 villages. The questionnaire remained relatively stable over years but information on schooling was only well captured in 2000, 2004 and 2007.

There were three main outcome indicators:

- Primary school enrolment: Child of school-going age (6-13), in or out of school during the year of survey. Estimated for 2000 (before FPE) and 2004, 2007 (after FPE).
- Primary school grade progression: Average years spent by a pupil in one grade between two survey periods. Progression measured as the difference between grades achieved in 2000 and 2004 and between 2004 and 2007. Index constructed and average grade progression of 2000/04 and 2004/07 compared.
- Secondary school enrolment: Whether a child of secondary school age who had completed primary school was in or out of school during the year of the survey.

Child-level variables assessed: Age, gender, relationship to caregiver and health.

Household-level variables: Age of household head, health, dependency ratio, distance to nearest school and per capita household income.

Regional variables: control for regional inequalities in incomes and opportunities.

WHAT KINDS OF ANALYSES WERE DONE?

Regressions: Estimate pooled model used to examine correlates of primary schooling performance indicators and probit model dichotomous dependent variables. QMLE (quasi-maximum likelihood estimator) model used for primary school progression.

PSM: Because the FPE programme is mandatory and introduced nationally, the study focused on estimating the average treatment effect of the programme over time. Authors performed a matching process in two steps: First they used standard logistic regression to generate propensity scores for each observation in the treatment and non-treatment samples. Second, they conducted one-to-one matching without replacement (nearest-neighbour matching), choosing for each treatment group member (observations in 2004, 2007) the comparison group member (observations in 2000) with the closest estimated propensity score. If several comparison group members matched a given treatment group member equally well, one group member was chosen randomly. Comparison group members were dropped from analysis if they were not a best match for any treatment group member. There was a set trimming level of 2 percent.

Benefit incidence analysis: Combined the cost of providing public services with information on their use to show how benefits of Government spending are distributed across the population. Study examined the average benefit incidence of FPE programme per capita transfers across income quintiles.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

PSM controls for differences in the profiles of the two groups (before and after) but will not automatically allow for programme effects to be differentiated from temporal effects. Unobservable factors may be important and these data are unavailable, but PSM is the only technique available in this case, where experimental data are absent.

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

Results from probit regression were significant and increased in probability for primary enrolment in 2007 relative to the base year (2004), controlling for other observable factors. Compared to the 2004 base year (one year into FPE), there was a lower probability of enrolment in 2000. Significant correlates of primary school enrolment at child, household and regional levels are identified.

QMLE of primary school progression was significant. The coefficient of programme dummy was negative, and significant at the 1 percent level, showing a decrease in grade progression in the period 2004-07 relative to 2000-04, controlling for other observables. The authors identify child-, household- and regional-level factors as predicting grade progression.

Results of probit regression of secondary school transition indicated that the probability of a child being in secondary school was lower in 2000 than in 2004, but not significant. After FPE, the probability of a child being in secondary school was higher in 2007. The authors identify important correlates at child, household and regional levels.

PSM results showed that primary school enrolment increased significantly from 82 percent in 2000 to 86 percent in 2004 to 89 percent in 2007. Enrolment increased across all income groups but was higher for higher-income groups. Grade progression slightly declined in the period under review. Secondary transition rates were about one child out of every three both before and after FPE. Transition rates for poorer children worsened after FPE. There were gender and regional disparities in secondary enrolment rates.

Benefit incidence analysis results indicated a shift in distribution of primary school enrolment across income quintiles. Poorer households had comparatively more children attending primary school than wealthy ones. The poorest 20 percent captured more than twice the Government expenditure on FPE so the programme is pro-poor (poorer households tend to have more children). Poor households have more children out of school.

HOW LONG WERE PARTICIPANTS FOLLOWED?

Seven years.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

Primary and secondary enrolment rates have improved significantly but grade progression has worsened. Government spending on FPE was found to be pro-poor.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

Declining grade progression could indicate declining quality of education. There is a need to improve infrastructure and recruit teachers. Secondary school rates remain low and Government intervention is needed at secondary level. FPE is a step in the right direction and needs to be sustained.

Factors that prevent children from poor backgrounds from attending primary school go beyond the ability to pay fees. Constraints also still exist hindering children from poorer households from transitioning to secondary school. There is a need for pragmatic interventions to combat factors beyond direct schooling costs.

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

An inquiry is needed into relevant hindrances to primary school enrolment so interventions can be targeted.

*Nicola E (2010) Has universal primary education improved school enrolment of AIDS orphans? A study of the Kagera Region of Tanzania from 1994-2004.*¹⁴

Background/rationale for the study

The study investigated whether enrolment rates in Tanzania, specifically for HIV/AIDS orphans, have improved as a result of the Government's implementation of free UPE and education sector enhancements (enhanced teacher recruitment, building schools) through the Primary Education Development Program (PEDP).

Loss of a parent has been shown to have significant detrimental impacts on schooling and health, and studies call for expanded schooling opportunities to mitigate negative effects of orphanhood.

The HIV/AIDS prevalence rate amongst adults aged 15-49 in Tanzania was 7.1 percent in 1994, peaked at 7.5 percent in 1996-97 and showed a decline in 2004 at 6.5 percent. It is the leading cause of death amongst adults in Tanzania. The Tanzanian population is young with ~45 percent of the population under age 15.

In 2004 the percentage of children enrolled on time at age 7 was below 40 percent. Withholding children from primary school has been shown to be a coping method for families affected by the loss of an adult to HIV/AIDS. Younger children (ages 7-10) who experience the loss of parent typically experience an impact on enrolment whilst older children (7-14) remain enrolled.

Setting/participants

Orphans who have lost at least one parent before age 20. Typically they live in poorer homes with adults less likely to care for them than non-orphan peers.

Tanzania has offered FPE for all since 2001. In 1994, before PEDP, enrolment (ages 7-20) was 53.6 percent for non-orphans and 51.9 percent for orphans (an insignificant difference: Provides no initial biases resulting from baseline enrolment rates).

The study conducted in Kagera Region, north-west Tanzania: population approximately 1.9 million people; GDP per capita in the region in 2002 was US\$200.

Intervention

PEDP, which began in 2001 with support of World Bank, EuropeAID and DFID, involved country-wide elimination of school fees (beginning at age 7 and lasting seven years) plus building schools and enhanced recruitment of teachers (no details are provided on the particulars of these interventions). It was designed for all Tanzanian children and targeted specifically at the poor. Orphans' lower enrolment rates have been attributed to costs related to schooling and caretaking so diminished school costs through PEDP may increase enrolment for orphans.

In Kagera, fees were removed from each Government-run school. Given the universal removal of school fees in 2001 and the similar enrolment rates in 1994 between orphans and non-orphans, PEDP's appearance shortly thereafter in 2002 provides the opportunity to evaluate this 'experiment' with the data collected in 1994 and 2004.

Research questions

Did enrolment rates for orphans increase overall between 1994 and 2004 and in relation to non-orphans?

¹⁴ This structured abstract includes material excerpted from the report.

What are the effects of PEDP on on-time enrolment?

Type of design

Study compares Kagera Health and Development Survey panel data samples collected from 1994 and 2004 which cover the same cohort of individuals organised by cluster and household.

Uses OLS and difference-in-difference models.

Also adopts a three-state econometric plan. This uses a difference-in-difference approach, and is a pooled model including a 2004 year dummy variable and OLS regressions disaggregating by gender and orphan status. The dependent variable in each equation is an indicator variable, equalling one if an individual is currently enrolled in school and zero if not enrolled.

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

Study compares enrolment rates of orphans and non-orphans in a difference-in-difference model using a pooled dataset of all observations for children aged 7-20 in 1994 and 2004.

HOW WERE UNITS ASSIGNED TO GROUPS?

All observations used. Two groups: Orphans and non-orphans.

WAS THERE PRETEST OR BASELINE DATA?

No pretest. Baseline was 1994 survey.

WHAT DATA WERE COLLECTED?

Sampling covered more than 800 households in over 50 communities, and over 13,000 observations. The survey covered high-mortality and low-mortality rate communities.

Data were disaggregated into additional subgroups of orphans/non-orphans, and boys/girls by orphan status.

WHAT KINDS OF ANALYSES WERE DONE?

Stage 1: Difference-in-difference model using pooled dataset of all observations for children aged 7-20 in 1994 and 2004 to compare non-orphans and orphans in those years to highlight impacts of PEDP on enrolment rates.

Stage 2: Pooled regressions conducted including a year 2004 dummy variable using a pooled dataset including all observations from 1994 and 2004. An initial aggregate equation was used to determine overall enrolment and then disaggregated equations compared non-orphans with orphans and disaggregated by orphan status and gender.

Stage 3: Multivariate regressions - OLS regressions highlighted effects on enrolment by orphan status and gender. Equations were run for 1994 and 2004 and then compared. Subsequent equations were run for non-orphans and orphans, using subsamples comprised of each group. Results helped demonstrate changes related specifically to gender impact for orphans/non-orphans and age, i.e. the issue of timely enrolment.

Individual controls: Gender, age, orphan status, height and presence of mother/father in household.

Household controls: Gender/age of household head, household size and quality of roofing material.

Cluster-level characteristics: Average cluster distance to school plus a cluster variable to control for fixed effects in each of the study's 51 clusters.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

The re-interview rate between 1994 and 2004 was 91 percent. Attrition may provide a source of bias as many moved away or died but the 2004 sample included additional observations and the model does not track individuals but analyses a cohort aged 7–20 in 1994 and a cohort aged 7–20 in 2004 to help reduce bias.

Interviews occurred over a 10-year span and people interviewed may no longer be part of same 'household'. so data are limited. The model helps eliminate bias resulting from unobserved fixed effects.

Other sources of bias include advancements in HIV/AIDS treatment between 1994 and 2004. The author sought to diminish this effect by including a height variable. Developments in health were reflected in a reduction in number of orphans between the two survey years, which could understate improvements to enrolment. Bias may also result from age differentials between orphans and non-orphans (little difference in 1994, but in 2004 orphans have a higher average age than non-orphans and this may limit their enrolment rate since secondary schooling is less available and older children may be working—authors control for age in regressions). Also, bias may result from individuals previously enrolled and benefiting from a smaller class size being less likely to attend or moving to another school to avoid the influx of poor students. (A 2006 PEDP report by the Government states that over 43,000 students dropped out in 2005 owing to overcrowding and an insufficient number of teachers and classrooms). Finally, omitted variable bias may exist as R-squared calculations are low for each equation (never exceed 0.50).

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

Enrolment of all children was 16.2 percentage points higher in 2004 than in 1994 and statistically significant. Orphans' enrolment improved by 4.2 percentage points but was not significant. Non-orphans enrolment increased by 20.5 percentage points Non-orphan enrolment increased by 16.3 percentage points more than orphans' enrolment, creating a widened enrolment gap in 2004 which was smaller and not significant in 1994. Examining the subsamples appears to indicate a younger cohort in 2004 than in 1994, but the orphan subset was older than in 1994 and the non-orphan subset younger. The small difference in orphan enrolment rates may be affected by the subset consisting of older students.

Comparing non-orphan girls with orphan girls, there is a considerable and significant increase in non-orphan enrolment (23.6 percentage points) whilst the orphan girls' 1 percentage point increase is not significant. The difference in enrolment between non-orphan and orphan girls in 1994 is not significant, whilst the difference is significant in 2004, highlighting the enrolment gap. Non-orphan boys' enrolment increased by 17.4 percentage points and was significant whilst orphan boys' enrolment increased 6.7 percentage points and was not significant. There was a decrease of 14.2 percentage points in orphan status (perhaps from people moving, becoming too old to be in the sample, or improved health resulting in fewer orphans). There were small increases in male household head and good roofing materials, and a decrease in household size (quality roofing and smaller household size were more likely for non-orphans).

On-time enrolment (presence in school at age 7) increased 52.5 percentage points for all groups and 8.7 percentage points for orphans. Enrolments rates amongst orphan boys increased more than for orphan girls. The effect of wealth on enrolment overall decreased by 8.1 percentage points, and by 8.6 percentage points for orphans.

Stage 1 difference-in-difference: Result shows a highly significant negative difference-in-difference between orphans and non-orphans in 1994 and 2004, and indicates a widened enrolment gap.

Stage 2 pooled regressions: Results show a highly significant 14.8 percent overall higher enrolment in 2004; non-orphans highly significant at 17.9 percent; non-orphan girls (20.8 percent), non-orphan boys (14.9 percent) and orphan boys (14.9 percent) significant; orphans overall (5.6 percent) and orphan girls (-4.7 percent) not significant.

Stage 3 multivariate regressions: Coefficients on orphan status are not significant in either year; the gender coefficient is positive and significant at the 5 percent level in 2004. Age dummy variables for ages 7-18 are significant at the 1 percent level in 2004 and for ages 9-17 in 1994. There are positive indications of enrolment at each age level in 2004 and an increase in probability of enrolment is present at each age except 17. In both years, mother in the household has a significant effect on enrolment, as does a good roof.

Disaggregated regressions comparing orphans and non-orphans show non-orphans in 2004 see positive impacts on enrolment at ages 7-18 and orphans at ages 9-16. Improvements in the magnitude of probability of enrolment is higher for non-orphans. On-time enrolment for non-orphans increases 73 percentage points

HOW LONG WERE PARTICIPANTS FOLLOWED?

Ten years.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

There were overall gains for enrolment, but for orphans, there was a significant increase only for orphan boys show.

The non-orphan girls' significant 23.6 percentage point increase in enrolment and 20.8 percentage point higher probability of enrolment in 2004 may be cost-effective (scholarships and cash transfers for non-orphan girls may not be necessary).

PEDP appears to have increased on-time enrolment (although wealth effects also improved).

Orphan boys have been affected more positively than orphan girls.

Overall broadened access and better on-time enrolment was captured predominantly by non-orphans. PEDP is insufficient to address orphans' enrolment rate.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

Enhancing PEDP with additional policies designed to target orphans is vital to ensure national policies do not leave HIV/AIDS orphans behind. Strategies could include cash transfers to male household heads; and continuing to improve access through building schools and teacher recruitment.

Interventions on behalf of orphan girls are imperative.

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

The results indicate a need to find out whether other interventions such as those mentioned above lead to improved outcomes for orphans; and whether improved enrolment leads to higher earning potential and a reduction in poverty.

*Nishimura M, Yamano T, Sasaoka Y (2008) Impacts of the universal primary education policy on educational attainment and private costs in rural Uganda.*¹⁵

Background/rationale for the study

Before UPE, families contributed more than 80 percent of the costs of education. Uganda was one of the first sub-Saharan African countries to adopt the UPE policy in 1997 and experienced a robust increase of primary enrolment from 2.8 million in 1997 to 7.6 million in 2004. The evidence of its actual effect, however, is mixed. Whilst studies indicate that the UPE policy effectively improved access to primary education for children of poor families by removing tuition for public primary education others reveal that various fees are still charged under the UPE policy. For instance, a Government report shows that 55 percent of primary drop-outs left school due to the costs of schooling. These existing studies, however, conducted research a few years after the implementation of UPE, and there has been no empirical study in recent years. Since the aim of the UPE policy was primarily to increase the overall educational attainments of children, it is important to examine the impacts of the UPE policy beyond school enrolment. This study looks at overall primary attainments. It uses data collected six years after implementation of UPE to estimate impacts on delayed enrolment and completion of upper grades of primary school.

Deininger's (2003) study was undertaken too soon after implementation of UPE to evaluate impacts on overall attainment and did not examine the impact on delayed enrolments.

Setting/participants

The intervention was nation-wide for primary students; data are from rural Uganda.

Research questions

1. How much did the UPE increase the grade completion rates in primary education in rural Uganda?
2. How much did the UPE decrease delayed enrolments in rural Uganda?
3. What factors determine private spending on education under UPE in rural Uganda?

Intervention

In 1997 the Government pledged to meet the costs of schooling for four children per family, which was amended to benefit all children in 2003, whilst parents meet the costs of school uniforms, meals, exercise books, local materials for building classrooms and physical labour. Overall the education budget increased from 1.6 percent to 3.8 percent of GDP, with the share of the primary education subsector of the total education expenditure increased from 40 percent in 1996 to 65 percent in 2004. The number of primary schoolteachers increased by 41 percent from 103,331 in 1997 to 145,703 in 2004 and the number of schools also increased by 41 percent from 10,490 in 1997 to 14,816 in 2004. Decentralisation of responsibilities and massive publicity campaigns with a particular focus on girls' education were undertaken and the co-ordination amongst donors was strengthened in targeting investments in primary education. Since then, each school has been receiving the

¹⁵ This structured abstract includes material excerpted from the report.

capitation grant based on the number of pupils in the school and subsequently spending the grant by following a guideline.

Type of design

Before/after regressions.

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

N/A

HOW WERE UNITS ASSIGNED TO GROUPS?

N/A

WAS THERE PRETEST OR BASELINE DATA?

Baseline survey before intervention took place

WHAT DATA WERE COLLECTED?

Data collected from 940 households in rural Uganda surveyed in 2003 as part of the Research on Poverty, Environment, and Agricultural Technology (REPEAT) project. The survey was conducted in August-October 2003. It covered 94 local councils (the lowest administrative level), which are located across most regions in Uganda, except the North where security problems exist. From each local council area, 10 households were selected, resulting in a total of 940 households and 3,121 school-age children aged 6-18. Of them, 72.5 percent attended primary school, 12.7 percent attended secondary school, and 16.6 percent did not attend school at the time of survey. The net enrolment rate, which is the proportion of school-aged children attending school over the total number of school-aged children, is 86.1 percent for boys and 86.9 percent for girls at the primary level and 27.6 percent for boys and 29.5 percent for girls at the secondary level.

WHAT KINDS OF ANALYSES WERE DONE?

Probit model regressions.

Pre- and post-UPE cohorts compared separately for boys and girls. Late-teens aged 15-19 in 2003 (thus aged 9-13 in 1997) were selected as the post-UPE cohort because they were of primary school age when the UPE policy was adopted in 1997. The comparison group of young adults aged 20-24 in 2003 (thus aged 14-18 in 1997) was chosen as the pre-UPE cohort because they were too old to receive the full benefits of the UPE policy.

The education cost per pupil calculated by obtaining the average per pupil spending on education from households that have children either in primary or in secondary school. Then the average per capita spending was calculated for the primary and secondary levels and a ratio of primary to secondary education costs at 1:8.7 obtained. This ratio was applied to the education spending of households that have children in both primary and secondary schools.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

Not given.

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

Determinants of enrolment: For girls aged 6-12, only age of the child and mother's education were significant. For boys aged 6-12, younger children tended to be out of school but parental education did not impact. Amongst children aged 13-18, socio-economic factors had strong impact (some attended secondary schools, which are not free). Catholic religion was a negative factor for girls and age of household head was also significant, but not for boys.

Determinants of delayed enrolment: UPE reduced the delayed enrolment by 24.3 percentage points for girls and 25.8 percentage points for boys. Socio-economic

factors influence delayed enrolment in primary school, and the UPE policy is not sufficient to eliminate delayed enrolment by itself.

Completion of fourth and fifth grade: UPE policy has large positive impacts on the completion rates of primary education up to the fifth grade for female students but only up to fourth grade for male students, and the sizes of the impacts are larger for female students than male students.

Determinants of education expenditure: Overall primary to secondary cost ratio was calculated as 1:9.8. The proportion of education expenditure out of the total household expenditure was 2.7 percent for each primary pupil and 15.5 percent for each secondary pupil. Households with younger household heads spend less on primary education than those with older household heads and households with only girls spend less.

HOW LONG WERE PARTICIPANTS FOLLOWED?

N/A

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

UPE has decreased delayed enrolments and increased grade completion rates up to the fifth grade and its effects are especially large amongst girls in poor households. Yet, schools in Uganda still face further challenges in terms of low internal efficiency and the unequal quality of education.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

There should be more than just the one demand-side policy intervention of reducing the school tuition fees in public primary education to achieve UPE. Internal inefficiency, such as delayed enrolment and repetition, remains a major problem in primary education in Uganda. Thus, further policy interventions will be necessary to deal with the reasons for internal inefficiency. Supply-side policy interventions, such as providing enough school facilities in a neighbourhood, or demand-side policy interventions, such as improving parental awareness, should follow the abolition of school tuition.

Quality improvements will be essential for retaining pupils at upper grades.

Low completion rates in upper grades suggest high indirect costs for older children. More comprehensive rural development strategies should increase the benefits from primary education so that the expected benefits exceed the total costs (direct and indirect) of education.

A funding scheme for primary schools could provide better incentives for them to reduce internal inefficiency (a capitation grant could encourage schools to retain students).

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

Not given.

Oketch M, Mutisya M, Ngware M, Ezeh A (2010) Why are there proportionately more poor pupils enrolled in non-state schools in urban Kenya in spite of FPE policy?¹⁶

Background/rationale for the study

One of the conundrums of FPE policy in several countries in sub-Saharan Africa is the 'mushrooming' of fee-paying private schools. Several researchers have become interested in studying this phenomenon and have raised the question: Does FPE meet the needs of the poor? Emerging voices amongst this group of researchers suggest that the impact of FPE, particularly in meeting the educational needs of the poor, may be over stated in situations where the poor still utilise what is referred to as 'private schools for the poor'. The concerns expressed by those voicing this point of view stem from the question of why the poor choose fee-paying (low-quality) private schools when there is, presumably, a free state school? To respond to this question and to contribute to this debate, this paper adopts the excess demand and differentiated demand frameworks to analyse how slum (poor) and non-slum (non-poor) parents utilise education in Nairobi, Kenya, following the implementation of the FPE policy in 2003.

A total of 44 percent of pupils in the sample in the two slums attend poor-quality fee-charging private schools, in spite of the existing policy of FPE in Kenya.

In the case of Kenya, the introduction of the FPE policy has exacerbated perceptions of low-quality in state provision, particularly when parents see larger class sizes and when schools no longer select the pupils whom they admit.

One argument put forward is that it is 'excess demand' which drives poorer parents to low-quality private schools, but it is differentiated demand which is driving non-slum parents to choose private schooling for their children over free public schools. In the slums, public spending on education is low and so parents will find alternative ways of educating their children, when their preferred route of free state schooling is unavailable to them. In contrast, wealthy parents in the non-slum will bypass the free state system because they have a preference for the fee-charging high-quality private sector.

The FPE policy announcement triggered interest in education and has led to excess demand from the slum residents who would prefer to send their children to the free public schools, but are unable to find a place. The excess demand phenomenon helps to explain the anomaly of why poor parents in the slums send their children to low-quality private schools for which they pay fees, when presumably there should be free public primary education. Equally, it may explain why comparatively wealthier parents in the non-slum settlements send their children to free public primary schools under the FPE policy.

Setting/participants

The intervention was nation-wide for primary students; data are from slum and non-slum areas of Nairobi.

Research questions

Why are poor parents paying for poor-quality education, when they could be getting fee-free schooling in the state sector?

¹⁶ This structured abstract includes material excerpted from the report.

Intervention

This study examines the consequences of UPE in Kenya in both slum and non-slum areas. UPE in Kenya provided for elimination of school fees for public schools but, consequently, there was great concern that quality of public education had decreased, leading to many parents enrolling their children in private schools when possible.

Type of design

Logistic regression and multilevel random effects modeling.

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

Slum and non-slum areas and different wealth quintiles compared.

HOW WERE UNITS ASSIGNED TO GROUPS?

N/A

WAS THERE PRETEST OR BASELINE DATA?

No.

WHAT DATA WERE COLLECTED?

Data collected in 2005 by the African Population and Health Research Center (APHRC) in two slum and two non-slum settlements in Nairobi. APHRC collected data on schooling outcomes - enrolment, transition, drop-out and progression - amongst all children aged 5-19 years from 2005 onwards and living in the study area regardless of whether they were in school at the time of visit. Data were also collected on school characteristics and household characteristics and schooling expenses.

Since data were collected for the first time in 2005, the children were asked to reconstruct their schooling history for the five years before 2005 (i.e. there was a five-year lag), which dated from 2000 to 2004. The lag depended on the age of the child in 2005: For children aged 6-9, schooling information was lagged for one, two, three and four years plus the current schooling year (2005), respectively; whilst for those aged 5, schooling information was only collected for the current schooling year, and those aged over 9 had schooling information recorded for six years. For those enrolled or who had attended school for some time during the year, schooling information such as level, grade, school type (public or private) and location of the school was collected.

A total of 13,257 individuals aged 5-19 years in different levels of schooling were captured in 2005. Analysis for this paper was restricted to children enrolled in primary schools that are located within Nairobi region; a total of 7,295 were eligible out of the 7,940 pupils enrolled in primary schools in the sample. Household characteristics data collected through DSS (Demographic Surveillance System) (slum) and ERP (Education Research Project) (non-slum) were used to calculate the wealth index of the households where these pupils lived.

WHAT KINDS OF ANALYSES WERE DONE?

To assess the adjusted effect of the household wealth index on attending private or public schools within the study site, a logistic regression model was fitted. The dependent variable is the type of school (public or private) whilst the main independent variable is the household wealth index; other explanatory variables: Household size, average household educational attainment and household gender. Three different models were fitted, one for all study sites (model 1) and separate ones for the two study sites (slum - model 2 and non-slum - model 3).

Model for cost of schooling: The dependent variable is the cost of schooling which was calculated using four items: Transport to and from school, tuition fees,

supplies expenses for items such as text books and exercise books, and examination fees.

Since the data are at two levels – pupil and household levels – and since individual effects are randomly distributed, a multilevel random effects model was fitted.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

Not given.

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

The trend in primary school enrolment in the two slums before the FPE policy and after the policy was introduced in 2003 is reported. FPE led to a surge in enrolment in state schools in the slums. But the trend started to change in 2005 and, as of 2007, 43.27 percent of slum pupils were not enrolled in the state system. This trend is very different from that of the two non-slums where the impact of FPE in the utilisation of private and public schools has not been dramatic. Although there has been a slight increment in the use of private schools, more pupils utilise state schools compared to those living in the two slums.

An odds ratio is given from logistic regression models of enrolling in a public school overall and by area of residence. After adjusting for other covariates, pupils from the least-poor wealth quintile are twice as likely to enrol in public schools compared to those from the poorest households in model 1. The least-poor households in the slum settlements (model 2) have statistically significant higher odds of sending their children to public schools compared to the poorest. However, those in the third wealth quintile amongst the slum households have insignificantly increased odds of enrolling in public school compared to the poorest households. Amongst the non-slum dwellers (model 3), the odds of enrolling in a public school decreases with increase in wealth. The odds of enrolling in a public school amongst the non-slum dwellers if a household is ranked as least-poor decreases by 84 percent compared to the poorest households in the same settlements.)

The cost of schooling after the FPE policy was implemented reduced significantly amongst the public schools.

HOW LONG WERE PARTICIPANTS FOLLOWED?

Participants reconstructed their schooling history for the previous five years.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

There is a generally unacceptably high level of utilisation of private schools in the slums by the poorest because there are fewer public schools in the slums (i.e. supply constraint). It is also evident that there is high public school utilisation by the least-poor slum residents. This is confirmed further by the logit model where it is evident that the odds of the poor attending public school is lower in the slums compared with those in higher wealth quintiles in the slums. In the non-slums, it is the wealthier quintiles that are more likely to send their children to private schools. These results confirm that in the slums, excess demand is associated with the mushrooming of low-fees private schools. This is part of the answer to the question of why the poor are paying for low-quality education when there is free schooling in the state sector.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

In the non-slum context, the public system is seen to offer low-quality education which has triggered the supply of what is perceived as a high-quality private system. For the wealthy in the non-slums, FPE offers them choice. For the slum

residents, there is limited choice as the mushrooming of private schools for the poor merely fills that gap left by the insufficient supply of school places in the public system. For the slum residents, public schools and private schools are perfect substitutes.

Why the poor attend private schools when there is a policy of FPE in Kenya is explained by excess demand as a result of low public expenditure in education in the slums. In the slums, those who utilise the private sector are those who have been involuntarily excluded from the state system. In contrast, in the non-slums, those utilising the private sector are those with a preference for the private sector over the state system.

FPE has obviously led to improved participation and it is difficult to argue that it has not met the needs of the poor. What is however evident from the data in this study is that an unacceptable proportion of the poor still utilise private schools, not necessarily because they prefer to, but because they have not been included in the state system through lack of supply. A first step by the Government is to find ways of improving the supply of state schools of acceptable standard in the slums. Moreover, unequal treatment of the unequal may be necessary to improve educational opportunities and equity that supply-side policy, such as FPE, which treats the unequal equally, has not been able to achieve.

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

Not given.

*Schmidt D (2006) The impact of free primary education on enrolment in Kenya.*¹⁷

Background/rationale for the study

When the price of schooling goes down, lower-income households will increase their demand for schooling by more than their wealthier counterparts. Therefore, eliminating school fees reduces the wealth bias of access to primary education. On the other hand, theories of school enrolment also outline the importance of the perception of school quality in schooling decisions. If a decrease in school fees is complemented by a decrease in quality, then the positive incentive of reduced cost will be mitigated by the negative incentive of reduced quality: If less money means poorer quality, then the benefits of dropping user fees could be negligible. The evidence also suggests the importance of other contextual factors in schooling choices, like social norms and opportunities available in the labour market. Thus the single policy of eliminating school fees is unlikely to be sufficient to achieve UPE.

This paper focuses on the demand effect of eliminating primary school fees to help to determine whether the solution to under-enrolment can result primarily from Government policies on education - like the elimination of fees - or whether it will also depend on attention to broader factors like poverty alleviation and job creation. It uses a longitudinal dataset to estimate the effect of school fees over time for three slum neighbourhoods and two middle-income neighbourhoods in Nairobi, Kenya.

President Kibaki eliminated school fees in January 2003, instituting FPE and promising that no child would be kept out of school for lack of a uniform. But students were still concerned about being ostracised for not having a uniform and other fees such as school lunch and transportation. Nevertheless, 1.5 million new children enrolled. In total, 80 percent of funding for FPE comes from domestic sources and the rest from donors.

Setting/participants

GNI per capita in Kenya in 2000 was US\$430; the fertility rate was five births per woman; school fees were \$5-12 and uniforms \$6-13.

The study focused on three slums and two middle-income neighbourhoods in Nairobi. In Nairobi, the net enrolment rate in 2003 was 44.9 percent for boys and 42.1 percent for girls. Some 40-60 percent of Nairobi's 2.5 million population live in slums. The paper looks at patterns of enrolment in all types of schools (Government, religious, private, etc.)

Research questions

What has happened to primary school enrolments - and thereby progress toward UPE - since the elimination of primary school fees?

Are there differential changes in enrolment based on the neighbourhoods in which children live?

Are there other changes in enrolment behaviour that might suggest changes in school quality following the elimination of primary school fees?

Type of design

Retrospective before/after.

¹⁷ This structured abstract includes material excerpted from the report.

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

No control group.

HOW WERE UNITS ASSIGNED TO GROUPS?

N/A

WAS THERE PRETEST OR BASELINE DATA?

Retrospective

WHAT DATA WERE COLLECTED?

Data obtained from a child update questionnaire collected by the African Population and Health Research Centre (APHRC) in 2005 containing information on schooling status of 10,000 students and asking for retrospective information on enrolment for each of six school years from 2000 to 2005. It also included data on class level and type of school. The survey was conducted by speaking with parents of children aged 5-11 and speaking with the child if aged 12-19. The random sample was representative of slum households in Nairobi.

WHAT KINDS OF ANALYSES WERE DONE?

OLS/LPM: five separate regressions for each of the five sites were performed to distinguish differential effects based on site and avoid introducing omitted variable bias. Regression was estimated separately for all students of primary school age (5-14), young students (5-9), older students (10-14) and oldest students who were not of primary age (15-19); it was corrected for clustered standard errors.

(Maximum-likelihood probit models were also run and found to yield largely identical results.)

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

Many of the variables identified in the literature as important determinants of household schooling decisions could not be controlled for: No variables for household wealth or parental education level, and school quality and opportunity costs of schooling could not be controlled for. Also policies and events other than FPE that are not accounted for here could have had significant impacts on primary school enrolments between 2000 and 2005. Finally, there may be tendency in the sample to misrepresent retrospective info.

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

Descriptive data show Increase in enrolment in 2003 and declines in 2004 and 2005. Enrolment rates were lower in slum than non-slum areas. The share of Government school enrolments declined over time (the drop was particularly steep for slum areas immediately after implementation of FPE and in lower grades there was a pronounced rise in public schooling).

OLS model results are provided for each site and by age group and gender.

Children most likely to enter school as a result of the policy change in 2003 were in the youngest age group (5-9) and there was a delayed positive impact in 2004 and 2005 amongst 10- to 14-year-olds. There was no delayed enrolment amongst older students (aged 15-19) who had not yet finished primary school as a result of the elimination of fees. The policy does not appear to have had a lasting impact on increasing enrolments over time. For young children, in fact, the probability of enrolment declined significantly even relative to the years prior to the policy change. There was little if any impact of FPE on children older than primary age.

HOW LONG WERE PARTICIPANTS FOLLOWED?

Participants asked to reconstruct schooling history for the previous five years.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

The policy reduced wealth bias in primary education.

Most of the students who enrolled in school as a result of FPE were young students who otherwise would not have entered, and some students may have remained in school longer than they would have had there been fees. Declining enrolment amongst children above primary school age suggests that the main effect of FPE was that families no longer postponed putting their children into school.

It may have been initial publicity 'hype' that influenced increased enrolment at first, and this then died down.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

A relatively slight increase in enrolments, along with the flight of students from Government schools, suggests that the quality of the fee-free schools should not have been dramatically impacted by increasing class sizes. However, other factors could have impacted quality, such as a change in the demographics of students attending Government schools. If families perceive a decline in school quality, we expect their enrolment decisions to change.

The study shows evidence of families switching students from public schools to private schools, where fees are still collected, which is a possible indication that such families believed that school quality was declining. Parents with multiple children in school may send some of their children to private schools rather than public schools since the elimination of fees in Government schools has freed more of the household's money for schooling purposes. Although FPE may have decreased wealth biases in primary enrolments, at least initially, it may have simultaneously increased the wealth bias in the attainment of a quality education.

The study suggests that in Nairobi, benefits of eliminating primary school fees in this regard have been overstated and suggests the importance of analysing policy impacts within varied contexts. Even across slums within Nairobi, this study found the policy to have different effects. Likewise, the effects in an urban area like Nairobi are expected to be very different from the effects in more rural areas. Careful consideration should therefore be given to the differential impacts that may occur as the result of a policy such as eliminating school fees, and future research should aim to further examine these differential effects.

It is also important to the costs of implementing a policy like FPE. Today, Kenya's education system is more aid dependent even as it lags behind its goal of UPE. Meanwhile, many parents are choosing to pay to send their children to private schools, revealing the ability and willingness of many to pay for education. Policy-makers should carefully consider whether the benefit of a very small increase in primary school enrolment is worth the costs of increased aid dependency and possible decreased school quality.

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

Further research should aim to determine the full costs and benefits of eliminating school fees, including a more detailed analysis of its impact on teachers and individual school finances. Schemes in which students are charged fees based on their ability to pay, such as that employed in South Africa, might also be considered as possible alternatives to the complete elimination of fees. The role that non-government schools play in educating the poor should also receive more attention in future research.

It still remains to be seen how much of the positive impact of fees reductions on enrolment can be attributed to the publicity push when fees were eliminated versus how much can be attributed to an actual reduction in the cost of schooling.

Researchers should work to determine whether other models of abolishing fees are more effective than a sudden, sweeping reform. More research is warranted to determine the extent to which school fees abolishment can help, the mechanisms through which it differentially affects populations within a country, and the costs it imposes on the financial system and the quality of schooling.

Baird S, McIntosh C, Ozler B (2009) Designing cost effective cash transfer programs to boost schooling among young women in Sub-Saharan Africa.¹⁸*

Background/rationale for the study

Cash transfers reduce costs of schooling and may therefore encourage enrolment. Transfers made conditional on school attendance may further encourage attendance. Also, the recipient of the transfer may alter the effect. For example, education outcomes may improve more if the transfer goes directly to the girl rather than to the parent/household so the girl has more incentive to attend.

But evidence on CCTs are mainly from Latin America and we need evidence from sub-Saharan Africa. The evidence base needed by a Government to decide *how* to design a new CCT programme is severely limited in some critical dimensions. This paper presents one-year schooling impacts from a CCT experiment amongst teenage girls and young women in Malawi, which was designed to address these shortcomings: Conditionality status, size of separate transfers to the schoolgirl and the parent, and village-level saturation of treatment were all independently randomised.

Setting/participants

Malawi, - a small, country in southern Africa with most people living from subsistence farming supplemented by small-scale income-generating opportunities, is poor even by African standards. The study took place in Zomba, a highly populated district in southern Malawi, where distances from the district capital (Zomba town) are relatively small. It has a high rate of school drop-outs and low educational attainment. The biggest reason for drop-out from school is financial. Unlike many other districts, Zomba has the advantage of having a true urban centre as well as rural areas; the study sample was stratified to get representative samples from urban areas (Zomba town), rural areas near Zomba town, and distant rural areas in the district to analyse the heterogeneity of the impacts by urban/rural areas. Southern Malawi, which includes Zomba, is poorer, has lower levels of education, and higher rates of HIV than central and northern Malawi.

The intervention was for girls in public schools, aged 13-22; from standard 7 through to form 4.

Intervention

Girls were classified at baseline as either schoolgirls or drop-outs. Different groups received different variations of the treatment. In 46 enumeration areas (EAs) (a randomly determined share of) schoolgirls received *conditional* transfers; in 27 EAs schoolgirls received *unconditional* transfers; all baseline drop-outs received conditional transfers in the 88 treatment EAs. (The randomly determined shares of schoolgirls in the study sample that were treated were 33 percent, 66 percent or 100 percent)

The offer consisted of a household transfer and a transfer directly to the girl, as well as full payment of school fees for conditional girls in secondary school (primary school is free through to standard 8).

Note: We are concerned only with the conditional transfers since school fees were paid directly.

¹⁸ This structured abstract includes material excerpted from the report.

Treatment also varied by amount of transfer and by the share of the transfer going to the individual as opposed to the household. The household (or parental) transfer size was randomised *across* treatment EAs, and the size of the transfer that was made directly to the girl was independently randomised at the individual level *within* EAs. The household amount was randomly varied across EAs from US\$4/month to \$10/month, with all recipients in a given EA receiving the same amount. To determine the individual transfer amount, girls participated in a lottery where they picked bottle caps out of an envelope to win an amount between \$1/month and \$5/month.

As part of the offer, a detailed information sheet was given to each household that detailed the quantity of transfers that each household and girl would receive, as well as the conditions of the contract. The *conditional* offer sheet for secondary school CCT recipients stated that their school fees would be paid in full directly to the school. The contract was then signed by both the recipients (guardian and core respondent) and the firm delivering the funds.

Monthly school attendance of all the CCT recipients was checked and payment for the following month was withheld for any student whose attendance was below 75 percent of the number of days school was in session for the previous month (but no one was ejected from the programme).

Type of design RCT.

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?
See below.

HOW WERE UNITS ASSIGNED TO GROUPS?

In all, 176 EAs were randomly sampled out of a total of 550 EAs using three strata in the study district of Zomba. Each of these 176 EAs were then randomly assigned treatment or control status. Furthermore, *baseline schoolgirls* in each treatment EA were randomly assigned to receive either *conditional* or *unconditional* transfers. Second, two separate transfers were made to the household in which the target beneficiary lived. The household (or parental) transfer size was randomised *across* treatment EAs, and the size of the transfer that was made directly to the girl was independently randomised at the individual level *within* EAs, which allows estimation of the elasticity of outcomes with respect to transfer size. 'Pure' income elasticity can be estimated by restricting the analysis to only those receiving *unconditional* transfers. In addition, because these two transfer sizes are independently randomised, there is experimental identification over the impact of the *split* of the transfers, conditional on the total transfer size. The study investigated whether, for a given cost, impacts can be improved by altering the recipient of the transfer. Finally, the percentage of girls assigned to the treatment group was randomised at the EA level, and hence the survey includes a group of randomly selected 'within village controls' who did not receive the treatment. The second control group was used to exploit the direct randomisation of treatment saturations to test for the presence of spillover effects within villages.

WAS THERE PRETEST OR BASELINE DATA?

Baseline data collection was conducted in the autumn of 2007. The sample was randomly drawn (using the above eligibility criteria) using data from a full listing exercise: Weight estimates were used to represent the entire eligible population in the 176 study EAs. A baseline survey was implemented after the listing exercise and before the selection of treatment status.

WHAT DATA WERE COLLECTED?

Results were based on the first two rounds of a household survey covering 3,805 girls and young women, between the ages of 13 and 22, and never married as of baseline. The annual household survey consisted of a multi-topic questionnaire administered to the households in which the selected sample respondents reside. The survey consisted of two parts: One administered to the head of the household and another administered to the core respondent, i.e. the sampled girl from the target population. The former collected information on the household roster, dwelling characteristics, household assets and durables, shocks and consumption. The core respondent survey provided information about her family background, her education and labour market participation, her health, her dating patterns, her sexual behaviour, her marital expectations, her knowledge of HIV/AIDS, her social networks, as well as her own consumption of girl-specific goods (such as soaps, mobile phone airtime, clothing, braids, sodas and alcoholic drinks). Community characteristics were also collected in a separate, short community questionnaire. This paper utilises data from the baseline survey (October 2007 - February 2008) and follow-up data (October 2008 -February 2009) to analyse the one-year impact of the programme on self-reported school enrolment and literacy.

An independent school survey was also conducted in every school in Zomba attended by any of the core respondents in the study sample, and collected data on, *inter alia*, each student's attendance and her grade progression separately for each school term.

WHAT KINDS OF ANALYSES WERE DONE?

Self-reported attendance and English literacy: Difference-in-difference regression estimated using individual fixed effects, thereby explaining changes in educational outcomes with a dummy for the second round and a dummy that only switches on for the relevant treatment group. The regressions were weighted to be representative of the study EAs. Standard errors were clustered at the EA level to account for the design effect.

In order to isolate the effect of the transfer split, a difference-in-difference regression was run using only treatment girls (because this split is undefined in the control). The total transfer size was then included to allow for any way in which the different total amounts of household and individual transfers might enter the ratio interaction analysis. The transfer size was interacted with the dummy for conditionality.

Difference-in-difference regression examined spillover effects.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

A 7 percent attrition was noted but not reported as a problem; regressions investigating differential attrition across treatment and control show that tracking was balanced perfectly across treatment and control groups.

Treatment status as originally assigned out of the baseline data was used for the entire analysis, because certain mistakes in treatment assignment were only uncovered through the process of attempting to make offers, and so correcting these mistakes in the treatment group only could have led to imbalance between treatment and control. Therefore estimates should be thought of as the 'intention-to-treat' effect of the original assignment to a treatment category.

Balance tests were performed for a battery of baseline covariates over every dimension of the randomisation (overall balance, balance within drop-outs and schoolgirls, conditionality, transfer amounts, spillover saturations). These tests take into account the design effects arising from the EA-level randomisation by clustering standard errors at the EA level. Overall, very few violations of balance

were detected: of 49 tests for balance, three were significant at the 5 percent level and none at the 1 percent level, indicating a rejection rate very much in line with what authors expected from fully random comparisons. The one attribute that appeared somewhat problematic was the indicator for female-headed households, with a slightly lower treatment rate amongst schoolgirls and amongst the within-village controls, indicating the presence of some village-level heterogeneity.

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

Self-reported school attendance: Pronounced one-year improvement in the treatment relative to the control. Both for attendance and for English literacy, baseline drop-outs experienced treatment effects that were larger in magnitude than baseline schoolgirls.

How treatment effects for attendance and English literacy differ according to the highest grade completed at baseline was also reported.

From the school survey, self-reported attendance data were found to be fairly accurate, and impacts estimated using data from the school survey are qualitatively very similar: The cross-sectional data from the Round 2 school survey were used to measure the extent to which the treatment improved the probability that a girl attended school regularly during all three terms in 2008 and whether she successfully completed her current grade – according to her teacher.

Impact of transfer sizes and splits: No evidence that an increase in the total transfer size had a strong marginal impact on school attendance over the receipt of the minimum transfer size (\$5/month for the parents and the student *combined*) in any treatment group.

Impacts seemed, in general, more responsive to *individual* transfer amounts, but were significant only when individual transfer sizes were increased amongst *conditional* schoolgirls. For example, amongst conditional schoolgirls, each \$1 transferred to the girl seemed to reduce her likelihood of drop-out by 1.3 percentage points, implying a reduction in drop-out of more than 50 percent if the girl were receiving the highest *individual* transfer amount of \$5.

Impact of conditionality: No significant one-year impacts of conditionality on schooling and literacy were found.

No evidence of spillover effects on five closest friends, classrooms and within-village controls were found.

HOW LONG WERE PARTICIPANTS FOLLOWED?

Effects after the first year of a two-year intervention reported.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

The programme had large impacts on school attendance: *Re-enrolment rate* amongst those who had already dropped out of school before the start of the programme increased by 2.5 times and the *drop-out* rate amongst those in school at baseline decreased from 11 percent to 6 percent. These impacts were, *on average*, similar in the conditional and the unconditional treatment arms. Although most schooling outcomes examined here were unresponsive to variation in the size of the transfer to the parents, higher transfers given directly to the schoolgirls were associated with significantly improved school attendance and progress, but only if the transfers were conditional on school attendance. There were no spillover effects within treatment communities after the first year of programme implementation.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

Policy-makers looking to design cost-effective cash transfer programmes targeted toward young women should note the relative insensitivity of these short-term programme impacts with respect to conditionality and total transfer size. Increase in schooling rates achieved by doubling the total transfer to the household does not seem cost-effective. Similarly, monitoring school attendance to enforce the conditionality is costly and the cost-effectiveness of imposing a schooling conditionality for cash transfer programmes needs to be examined more carefully in light of the income effects detected here. Policy-makers may also consider making at least some of the transfers directly to the target beneficiary in this context.

Results conform to a large body of evidence showing that the dramatic influence of CCT programmes on attendance is not accompanied by similarly large improvements in *learning*.

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

CCTs can generate impacts across a much broader range of baseline schooling status when individuals who had already dropped out as of baseline are included and examined.

Whether these attainment gains are resulting in improvements in relevant learning areas or not will be assessed when the authors of this study conduct tests in mathematics, reading comprehension and problem solving/life skills amongst the entire study sample in early 2010.

They have not experimented with conditionality amongst *baseline drop-outs*.

To make an informed decision on whether to 'condition' transfers or not, there is a need to examine a broader set of outcomes - not only with respect to schooling (e.g. actual learning), but also other relevant outcomes for this target population, such as early marriage, teenage pregnancy, risk of HIV infection.

The one-year results presented here may not be the same as those after the second and final year of this CCT experiment.

Duflo E, Dupas P, Kremer M, Sinei S (2006) Education and HIV/AIDS prevention: Evidence from a randomized evaluation in Western Kenya.¹⁹*

Background/rationale for the study

Schools offer opportunity to teach children aged 5-14 in the 'window of hope' about HIV/AIDS. But is this effective? This paper reports results from a randomised evaluation comparing three school-based HIV/AIDS interventions in Kenya: (1) Training teachers in the Kenyan Government's HIV/AIDS-education curriculum; (2) Encouraging students to debate the role of condoms and to write essays on how to protect themselves against HIV/AIDS; and (3) Reducing the cost of education (our study focuses on one aspect: Provision of free uniforms).

Since school fees were abolished in Kenya in 2003, school uniforms are the main direct financial barrier to access to education at the primary level. A uniform costs about US\$6, a substantial expense for parents in a country where the GDP per capita is \$360.

Setting/participants

Two rural districts of western Kenya: Bungoma and Butere-Mumias. A sample of 328 schools, each student in grade 6. Some 7 percent of Kenyan adults are infected with HIV. Measured prevalence levels amongst young women rise quickly with age.

Intervention

Four interventions took place in the same area: (1) Training teachers in the HIV/AIDS education curriculum designed for primary schools by the Kenyan Government; (2) Encouraging students to debate the role of condoms and to write essays about how they can protect themselves from HIV/AIDS; (3) Informing teenagers about variation in HIV rates by age and gender; and (4) Reducing the cost of education by providing free uniforms.

Between February and July 2003, ICS (International Child Support) distributed a free school uniform to each student who was enrolled in grade 6 in January 2003. In total, about 10,000 uniforms were distributed. In the Fall of 2004, ICS distributed a second uniform to the same students if they were still enrolled in the same school. It was announced at the onset of the programme that students still enrolled in school would be eligible for a second uniform.

Type of design

RCT.

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

Randomisation for teacher training was done by the generation of a random number, after stratifying by the geographical division of the school, average performance of the school on the Kenya Certificate of Primary Education exam of 2001, and the gender ratio amongst upper primary students. In addition, 163 schools were randomly selected to receive uniforms, after stratifying by whether or not the school was receiving training reinforcement on HIV education and by geographical location, school achievement and gender ratio.

HOW WERE UNITS ASSIGNED TO GROUPS?

WAS THERE PRETEST OR BASELINE DATA?

Baseline statistics confirm no significant difference in observable school characteristics across groups at the start of the programme.

¹⁹ This structured abstract includes material excerpted from the report.

In order to avoid creating incentives for students to transfer between schools, ICS field officers visited all the schools to collect baseline enrolment data before announcing the programme; only children enrolled at the time of the baseline were eligible for the uniform programme.

WHAT DATA WERE COLLECTED?

In order to determine if reducing the cost of school reduced drop-out rates, ICS field officers conducted six school visits, during which they conducted a roll call using the list of students enrolled at the baseline, and enquired about the whereabouts of the absent students: Are they still enrolled in this school? Have they changed school? Have they dropped out?

Information on childbearing and marital status was obtained for all girls in the sample during six visits to each sampled primary school in the 36-month period following the intervention. At each visit, the list of all students in the baseline sample was read aloud to pupils enrolled in upper grades at the time of the visit, and for each of the baseline students the following questions were asked: Is 'Mary' still in school? If yes, in what grade? In what school? Does she still live in the area? Is she married? Does she have any children? If so, how many? Is she pregnant?

WHAT KINDS OF ANALYSES WERE DONE?

OLS regression.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

Childbearing is an imperfect proxy for risky sexual behaviour.

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

Drop-out and childbearing rates: Reducing the cost of education helped students stay in school longer and, consequently, decreased the likelihood that they would get married and have children.

HOW LONG WERE PARTICIPANTS FOLLOWED?

Follow-up at two years.

***WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?**

Reducing the cost of education by paying for school uniforms reduced drop-out rates and the incidence of teen childbearing.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

Reducing the cost of education costs at least \$300 per pregnancy averted, and the teacher training costs at least \$525 per pregnancy averted.

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

Definitive assessment of the impact of these programmes will require biomarker tests.

Evans D, Kremer M and Ngatia M (2009) The impact of distributing free uniforms on children's education in Kenya.²⁰*

Background/rationale for the study

This is the first study that randomises uniform provision alone amongst primary school students and the first that includes impact on student learning as measured by test scores.

In January 2003, a new Government policy in Kenya provided not only fees but also basic textbooks and notebooks. This led to dramatic increases in school participation. However, schools still required students to wear uniforms. Historically, students who did not pay their school fees or those who did not wear uniforms could be sent away from school. In recent years, several prominent officials in the Kenyan Government have said that head schoolteachers should not dismiss children who fail to wear a school uniform. Anecdotal evidence suggests that students are less likely to be sent away from school for failure to wear a uniform after 2002 than previously, but that students still feel stigmatised by the failure to wear a uniform and may be reprimanded by teachers.

Setting/participants

Twelve primary schools in Busia district, the westernmost district of Kenya's Western Province. Busia District borders Uganda and is located just north of Lake Victoria.

Intervention

Sponsored children received a new uniform in June/July 2002 (and also received a new uniform in 2003 and 2004 but this analysis looks only at the impact of initial uniform receipt). Some children also received occasional letters or gifts of very low value from their sponsors. Schools chosen by ICS received a pair of nurses to visit each school several times a year and provided basic first aid to any child (sponsored or not) or local adult who requested it. An agricultural representative organised student clubs to grow crops on the school grounds. In fall 2002, each school received a sizeable grant for classroom construction, desks and books.

Type of design

RCT.

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

In January 2002, ICS organised a census of children in standards 1-4 of the 12 selected schools. Based on that census, ICS selected all children who had experienced deaths of one or both parents (orphans) to automatically receive sponsorship. It then used a lottery to randomly select the remaining beneficiaries (only examine impacts for non-orphans since orphans are not selected randomly). Next, a field representative from ICS went to the 12 schools to enrol those children selected for sponsorship into the programme. For enrolment in the programme, a child had to be present for a photograph to be taken and a small information card to be filled in. If a child intended for treatment was not present, then a replacement was selected from a list. If that replacement was not present, another was chosen. Because of that, some children initially assigned to the treatment group were ultimately assigned to the comparison group and vice-versa (so use treatment assignment was used as an instrumental variable (IV) to estimate the effect of treatment on the treated. In June 2002, uniforms were distributed to all sponsored children who were still in school. Sponsored children received a new

²⁰ This structured abstract includes material excerpted from the report.

uniform each year in June or July. If a sponsored child dropped out of school, the NGO selected a replacement child, usually a child who had experienced the death of one or both parents but had not been sponsored previously (either because they transferred to the school after the initiation of the programme, or because they lost a parent subsequent to the school census).

HOW WERE UNITS ASSIGNED TO GROUPS?

See above

WAS THERE PRETEST OR BASELINE DATA?

Children who won the lottery were compared with children who did not win the lottery for the 768 children in school on the day of the 2002 pupil questionnaire. Few significant differences and certainly no patterns of difference were discerned. Children who received uniforms in June 2002 were compared to those who did not, again for the 768 children in school on the day of the questionnaire; no significant differences were found.

*WHAT KINDS OF ANALYSES WERE DONE?

First stage regressions: What is the probability that a child, having been randomised into the project, was actually registered or went on to receive a uniform? (Being randomised into the programme does act as an appropriate IV for actual enrolment and later uniform receipt.)

IV regressions: Whether initial registration into the programme had any effect on children's school attendance in the six months previous to distribution of uniforms (insignificant positive impact on attendance).

Intent-to-treat regression, measuring the simple impact of being randomised into the project on attendance after uniform distribution (shows effect of 3.5 percentage points)

IV estimates of the impact of registration into the programme and of actually receiving a uniform in 2002 on attendance after uniform distribution. (Measured impact of programme enrolment was a 6.2 percentage point increase in attendance, and that of receiving a uniform was 6.4 percentage points. This was a major reduction in absenteeism from a baseline school attendance level of 85 percent.)

OLS estimate of receiving a uniform on school attendance (5.5 percentage point increase).

Non-parametric bounds on test scores were constructed to address non-random sample attrition. (The bounds of the effect in 2003 were reasonably tight and suggestive of positive test score effects of the programme. The bounds for the effect in 2004 were also reasonably narrow, and whilst they did contain zero were suggestive of a positive treatment effect.)

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

Names were matched manually and with the aid of a computer program. Some pupils in the various other datasets did not match with any pupil in the attendance dataset. This may stem from the fact that sometimes children have several names and may only give a subset of those in one data-gathering exercise and a non-overlapping subset in another exercise.

Sponsored children had their picture taken and were singled out, and that kind of attention could conceivably have self-esteem impacts that could affect school attendance. This could work together with moral support offered by sponsors to a few students. Estimates could not differentiate between these effects.

A potential concern was that these results would be positively biased if unsponsored students who transferred from CSP (Child Sponsorship Programme) schools to non-programme schools were listed in the attendance data as having dropped out. This kind of misclassification did not seem to be a significant problem given that students in CSP schools came from nearby villages, so the pupils seemed very certain of their classmates' (and neighbours') whereabouts. The enumerators, upon encountering a child who was absent on the date of the attendance check, would ask her classmates where the child was and whether or not she had dropped out. Further, given the school-wide benefits of the CSP, students had little incentive to transfer schools.

Test score data were collected after the conclusion of the programme and were not complete for all years and for all schools. There was also significant attrition in the test score data, largely due to imperfect matching between the school administrative records and our student data.

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

See above, under analyses.

Results by gender and age.

No significant effects on siblings.

Test scores: Uniform provision was associated with a significantly lower likelihood of having missing test score data, implying in and of itself that the programme increased the likelihood that children stayed in school to participate in exams. The programme appears to have had a positive impact on test scores in 2003, raising average test scores of recipients by one-quarter of a standard deviation.

HOW LONG WERE PARTICIPANTS FOLLOWED?

Three years.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

Strong positive impacts: Giving a uniform reduced school absenteeism by 6.4 percentage points (43 percent) from a base of 15 percent school absenteeism. The effect was 4.3 percentage points larger for students who had a uniform at the baseline. The programme also had a positive impact on test scores immediately after inception, raising average test scores of recipients by 0.252 standard deviations.

The average effect of the programme was an increase in school participation of 0.064 years per treated child.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

The cost of increasing school attendance by one year is US\$5.82/0.064, or \$90.94 (more expensive than some interventions like deworming but cheaper than CCTs).

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

Grogan L (2009) *Universal primary education and school entry in Uganda**.²¹

Background/rationale for the study

In Uganda, UPE was introduced for all primary grades simultaneously in 1997 and enrolment immediately increased by 58 percent. (But other costs of schooling, such as transportation and uniforms, remained the responsibility of families.) Parental contributions were providing up to 90 percent of recurrent and capital expenditures made by schools just prior to the elimination of school fees. Because school fees were eliminated before infrastructural improvements in the school system had been carried out, the access shock created by the elimination of fees resulted in a substantial initial decrease in resources available per pupil and a large increase in the pupil:teacher ratio.

This paper focuses on one margin on which the elimination of school fees might be expected to have had an impact: Age at which children enter schooling. This is a very important margin in the Ugandan case. As will be demonstrated, school entry at ages above 8 is very strongly associated with early school drop-out. The Uganda DHS (demographic and health survey) and DHS EdData surveys, which were undertaken in late 2000 and in the first half of 2001, together comprise one of the first sources of data with which an examination of potential effects of school fees elimination on this margin was feasible.

Setting/participants

The intervention was universal across Government-aided primary schools.

Intervention

For all primary school students attending Government-aided schools, Government pays tuition fees at the rate of 5000 Ugandan shillings per pupil per annum in the first three years of schooling, and 8100 Ugandan shillings for the fourth to the seventh classes. (Other costs of schooling, such as transportation and uniforms, remain the responsibility of families.)

Type of design

RDD.

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

HOW WERE UNITS ASSIGNED TO GROUPS?

RDD. Two key variables used for identification in the regression discontinuity estimates: A continuous, linear variable for the year of birth (YBIRTH) and a dummy variable indicating whether or not school fees had been eliminated before a child attained the age of 9 (UPELEQ8). The first of these variables (YBIRTH) accounts for any potential secular, linear trend across cohorts in the probability of entering school before age 9. Given that both the Ugandan Government and UNICEF had been working for several years to increase school enrolment by the time school fees were eliminated, allowing for such a secular trend seemed appropriate for the study. The second variable, UPELEQ8, assigns sample members to treatment and control groups on the basis of the year of birth. Whilst the assignment to the treatment or control group might here be considered exogenous to the individual, controls for observable personal and household characteristics were also included in the regression.

²¹ This structured abstract includes material excerpted from the report.

WAS THERE PRETEST OR BASELINE DATA?

No.

WHAT DATA WERE COLLECTED?

From 2000 DHS and 2001 DHS EdData surveys. The main DHS survey is a stratified random sample of Ugandan households conducted in late December 2000, and January and February 2001. After employing appropriate weights, the survey can be considered representative at the national level.

The primary purpose of this survey, common to most DHS surveys, is to provide information on education, nutrition, child and adult mortality, fertility, maternal and child health and knowledge of HIV-AIDS. Health questionnaires were administered to women and men, and detailed information on the living circumstances of each household was recorded. Within six months of the completion of the main DHS survey, the specially constructed Ugandan EdData survey was administered. Households containing individuals aged 5-18 in the main survey were targeted for this second survey. The EdData survey collected information on the age of children at the beginning and end of their schooling, educational attainment and reasons for non-attendance. From parents and guardians, the information on their knowledge of UPE was collected. Adults were asked to give their assessments of the qualities and failings of the schools in the local area. Using the sample weights constructed for the second survey, the sample is representative of Uganda as a whole.

WHAT KINDS OF ANALYSES WERE DONE?

Probit marginal effects of regressions examined the probability that individuals born in 1982 or 1983 have completed at least seven years of schooling at the time of the EdData survey, conditional on having started school by age 10.

Estimates in which dummies representing age at school start were then replaced by a single dummy indicating whether or not the child began school before age 9.

Probit regression examined the probability that an individual began attending school before age 9, by birth year.

RDD estimated the effects of being in a UPE-affected cohort on the probability of school entry before age 9.

Analysis included checking for discontinuity in the probability of attending private school associated with school fees elimination.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

None.

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

The probability of beginning school before age 9.

The probability that the school attended is private.

HOW LONG WERE PARTICIPANTS FOLLOWED?

For 3-4 years.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

There have been significant positive effects of school fees elimination on the timely enrolment of girls and children living in rural areas of Uganda.

The discontinuous, positive effect of school fees elimination on the probability of entering school before age 9 was about 3 percent. The effects of school fees elimination on this margin were concentrated on females. Amongst girls for whom school fees were eliminated before the ninth birthday, the probability of entering

school before this age was 5 percent higher. No such effect of UPE was found for boys. The effect of school fees elimination appeared to be concentrated in rural areas. In rural areas, which contain two-thirds of Uganda's population, a 3.4 percent increase in the probability of attending school before age 9 was associated with the advent of UPE in January 1997. In urban areas, no significant jump in this probability was associated with the introduction of UPE. This may reflect the fact that school enrolment in urban areas was much higher than in rural areas before the elimination of school fees. There is also some anecdotal evidence that urban schools continued to charge fees after 1997.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

There is a need to gather longitudinal data at the individual level on the educational trajectories of students and on the quality of learning outcomes under UPE. In order to ascertain the specific effects of the elimination of school fees on AIDS orphans, a large and particularly disadvantaged socio-economic group in Uganda, data must be collected which include specific questions on the timing of parental sickness and death. There is also a need for survey data which permit an examination of the effects of the elimination of school fees on the resources available at the school level in Uganda.

Kremer M, Moulin S, Namunyu R (2003) Decentralization: A cautionary tale^{}.²²*

Background/rationale for the study

Kenya's education system blends substantial centralisation with elements of local control and school choice. This paper argues that the system creates incentives for local communities to build too many small schools; to spend too much on teachers relative to non-teacher inputs; and to set school fees that exceed those preferred by the median voter and prevent many children from attending school. Moreover, the system renders the incentive effects of school choice counterproductive by undermining the tendency for pupils to switch into the schools with the best headmasters.

Harambee: Local communities raise funds for schools and other local public goods. Under the education system Kenya established after independence, local harambee fundraisers typically cover initial capital costs for new schools. School fees set by local school committees and collected by headmasters cover most non-teacher recurrent costs, such as chalk, classroom maintenance and teachers' textbooks. Once local communities establish schools, the central Government assigns teachers to schools and pays their salaries. Outside-donors supplement Kenyan finance, sometimes providing additional resources that are targeted to poor or poorly performing schools.

Whilst local school committees bear only a small fraction of the cost of reducing class size by building additional schools and reducing the number of pupils per teacher, they bear the full cost of non-teacher inputs, such as textbooks. Both headmasters and parent representatives on school committees have incentives to set fees and other attendance requirements, such as uniform requirements, at levels that deter the poorest households from participating in school and that are greater than the median voter would prefer. Aside from school fees, uniforms are a key school attendance requirement. Pupils in Kenyan schools are required to purchase uniforms, which cost about US\$6, a substantial sum relative to per capita GDP, which is \$340.

This paper presents evidence from a randomised evaluation of a programme that paid for textbooks, classroom construction, and the uniforms that parents in Kenyan schools are required to purchase, which constitute the major cost to parents of sending their children to school.

Setting/participants

Fourteen particularly poor public primary schools in Kenya's Busia and Teso districts, a densely settled agricultural region on the border of Uganda. There is a dense concentration of schools with low enrolment (i.e. not cost-effective) and considerable school choice in the region. For the selected schools, both schools and performance were poorer than the average for schools in the area.

Intervention

All children were provided with uniforms in the first three years of the programme. In the fourth and fifth years, half of the grades were provided uniforms in each year (students that received uniforms in Year 4 did not receive uniforms in Year 5). ICS gave programme schools an extra \$3.44-worth of textbooks per student in an average year. ICS built 10 classrooms in each programme school over the course of five years, with two classrooms being built every year after the first year. None of

²² This structured abstract includes material excerpted from the report.

the classrooms built were ready to use until Year 2. Beginning in Year 3, ICS started providing a Christmas party to treatment schools.

Medical treatment and training were provided for both treatment and comparison schools. These included monthly visits from a nurse and basic medical supplies such as aspirin, bandages and malaria medicine. Since these benefits accrued to both treatment and comparison schools, the researchers did not evaluate their impact. Both treatment and comparison teachers also received gifts, such as soap or blankets, in appreciation for their cooperation.

Type of design

RCT.

HOW WAS THE COMPARISON/CONTROL GROUP FORMED?

ICS selected 14 particularly poor schools as candidates for the CSP (Child Sponsorship Programme). Schools were then randomly divided into programme and comparison groups. Schools were matched into pairs, based on geographic division and on school size within divisions. Within each pair of schools, school assignment to the treatment or comparison group was decided by a coin toss.

HOW WERE UNITS ASSIGNED TO GROUPS?

See above

WAS THERE PRETEST OR BASELINE DATA?

Researchers had only limited data on the schools from the period prior to the programme, but programme and comparison groups seemed similar in terms of their test scores and their SES. Programme students seemed to score slightly higher than comparison students on tests administered before the intervention, but these estimates are not statistically significant. There was also no significant difference in SES, as estimated from a survey questioning students about whether they have shoes, a watch or a house with a metal roof. Before the intervention, programme schools had 9 percent more students than comparison schools.

WHAT DATA WERE COLLECTED?

Enrolment data from Years 0-3 came from the school register records which schools themselves maintain. For Years 4 and 5, ICS conducted unannounced school visits in order to see who was actually present in school on a given day, for the purposes of keeping the list of enrolled students updated and measuring attendance. These data are probably more accurate, since schools receiving a lot of transfer students may have delayed listing them on the register, either because of procedural delays or because ICS was pressuring them not to accept too many transfer students.

Beginning in Year 1, yearly exams were administered to students enrolled in grades 3 through 8 at the end of each school year. The test scores were normalised by subtracting the mean score in comparison schools and dividing by the standard deviation of the scores in the comparison schools, so that the comparison schools have a mean score of 0 and a standard deviation of 1.

ICS administered its own test in Year 6 which was the same for all grades in order to correct for the problem of grade repetition.

WHAT KINDS OF ANALYSES WERE DONE?

Regressions by grade including school random effects and gender controls.

Other regressions including cohort dummies, gender dummies, cohort gender interaction dummies, and school random effects.

Regressions of programme effects on test scores.

WHAT METHODOLOGICAL PROBLEMS WERE REPORTED?

The sample size was too small and there was too much selective attrition in the sample to accurately estimate the programme's effect on test scores, but it seems likely that any effect was modest. An estimate that tried to correct for selective attrition suggests the overall effect on learning was small and positive.

Although the model suggests that both transferring resources from teachers to non-teacher inputs and transferring resources from teachers to lowering the cost of school would improve welfare, only one experiment is reported and it is not possible to separately determine the effect of each change. But the authors could evaluate the combined expenditure reallocation created by the CSP.

Results

WHAT WERE THE KEY OUTCOMES THAT WERE REPORTED?

Programme effect on class size: There was an increased class size both because students in programme schools remained enrolled longer and because many students transferred in from neighbouring schools.

Impact on years of schooling and grade attainment by gender/age: As of five years after the programme began, students in comparison schools remained enrolled for 3.8 years, whilst those in treatment schools had remained enrolled for 4.3 years, a 0.5-year or 13 percent increase. Students in comparison schools advanced 1.9 grades, whilst students in treatment schools advanced 2.2 grades, for a 0.3-grade or 16 percent increase. Effects were generally larger for younger cohorts that were exposed to the programme for a longer time. Point estimates suggest that the effects of the programme on grade advancement and enrolment increased over time, but that there was a gradual decline in the rate of increase of the programme effect.

The authors attempted to track a few non-educational, long-term outcomes: They conducted a follow-up survey in August 2001 on the cohort of pupils who were in grade 4 in 1994. They found information on 474 of the original 574 students. At that point, 42 percent of girls from comparison schools were married, whilst only 30 percent of girls from programme schools were married. This effect was not statistically significant, given the sample size (t-value: -1.49). There was no significant effect on the likelihood boys were married or on the number of children the former students had. Additional cohorts are currently being followed up.

Programme effects on test scores: Evidence presented suggests that textbook provision and larger classes had roughly offsetting effects on test scores, but it was not possible to determine whether textbook provision had a large positive impact that was offset by a large negative impact from larger class sizes or whether both impacts were small.

HOW LONG WERE PARTICIPANTS FOLLOWED?

Impacts examined at five years or more - see above.

WHAT WAS THE CONCLUSION ABOUT THE EFFECTIVENESS OF FEES ELIMINATION?

The programme led to a sharp reduction in drop-out rates and a large inflow of students from nearby schools into programme schools, thus increasing the class size in programme schools by approximately nine students. No significant effect of the combination of higher pupil:teacher ratios and more non-teacher inputs on test scores was found. Evidence from transfers suggests that, overall, parents preferred the combination of lower fees, more non-teacher inputs, and sharply higher pupil:teacher ratios associated with the programme.

Practice/policy implications

WHAT WERE THE MAJOR LESSONS LEARNED FOR POLICY AND PRACTICE?

The Kenyan Government could have financed the textbooks, classroom construction and uniforms provided by the CSP programme without external funds, using the savings that would be generated from an increase in class size much smaller than that associated with the CSP.

The main reason programme schools retained pupils and attracted transfers was probably the financial benefit of free uniforms.

Randomised evaluation suggests that lowering the cost of education can dramatically increase school participation. Widespread school choice also implies that programmes like CSP, which provide a lot of assistance to a few targeted schools, may lead to dissipation of programme benefits as people walk from distant areas to take advantage of the programme. Targeting larger geographic areas may not lead to much costly movement, but targeting individual schools apparently can, at least in rural Kenya.

WHAT WERE THE MAJOR IMPLICATIONS FOR FUTURE RESEARCH?

Not given.

Appendix 4.1: Descriptive quantitative and qualitative studies

Study	Country	Intervention	Methodology	Outcomes
Alloush (2010)	Ethiopia, Malawi, Tanzania, Ghana	Free UPE	QED without equating of groups	People exposed to the policy were more likely to enrol in school and to complete primary school.
Maikish (2010)	Ghana	Free UPE	QED without equating of groups	Per-pupil funding scheme was successful overall, but widely differing returns by deprived and non-deprived districts.
Nicola (2010)	Tanzania	Free UPE	QED without equating of groups	Children enrolled at rate 16.2 percentage points higher but widened enrolment gap between non-orphans and orphans.
Muyanga et al. (2010)	Kenya	Free UPE	QED without adequate controls	Primary school enrolment increased significantly across all income groups but more so for higher-income groups. Grade progression slightly declined and transition rates for poorer children worsened.
Bold et al. (2009)	Kenya	Free UPE	QED without equating of groups	Decline in inequality of access but accelerated decline in SES and performance in public schools as richer students flee to private schools.
Nishimura et al. (2008)	Uganda	Free UPE	QED without equating of groups	UPE decreased delayed enrolment and increased grade completion and gains have been greatest amongst girls in poor households.
Schmidt (2006)	Kenya	Free UPE	QED without equating of groups	Most of the students who enrolled in school as a result of UPE were young students who otherwise would not have entered and some students may have remained in school longer than they would have had there been fees. Declining enrolment amongst children above primary school age suggests that the main effect of UPE was that families no longer postponed putting their children into school.

Study	Country	Intervention	Methodology	Outcomes
Deininger (2003)	Uganda	Free UPE	QED without equating of groups	UPE greatly reduced the wealth bias and increased girls' access to primary education and reduced the incidence of cost-related drop-outs from primary school.
Fafchamps and Minten (2003)	Madagascar	School fees waived temporarily in a region following an economic blockade	QED without equating of groups	Between 6.5% and 9% increase in school enrolment.
Oketch et al. (2010)	Kenya	Free UPE	Descriptive (multilevel modelling)	In the slums, excess demand for school places leads to mushrooming of low-fees private schools.
Oumer (2009)	Ethiopia	Free UPE	Descriptive (school case studies)	Access to education has improved but budget allocation is far behind and the influx of students has led to deterioration in quality and efficiency.
Inoue and Oketch (2008)	Malawi, Ghana	Free UPE	Descriptive (Lorenz curves and Gini coefficients to measure equity)	Malawi reduced income-based, but not gender-based, disparities in both enrolment and attainment. There was no positive impact on equity in Ghana.
Al-Samarrai and Zaman (2007)	Malawi	Free UPE	Descriptive (benefit incidence methodology)	Enrolment increased dramatically and gains were greatest for poor socio-economic groups.
Castro-Leal (1996)	Malawi	Free UPE	Descriptive (benefit incidence analysis)	Increased the equity of public spending in education by increasing the total allocation of public resources to primary education whilst easing the constraints on the demand for education faced by poor households.
Nishimura et al. (2009)	Ghana, Kenya, Malawi, Uganda	Free UPE	Qualitative	Effective policy implementation would require considerable consultation with key stakeholders and a baseline survey that will enable systematic implementation and consideration of equity.

Study	Country	Intervention	Methodology	Outcomes
Vavrus and Moshi (2009)	Tanzania	Free UPE	Qualitative	Policy has poorly-defined features and limited impact on overall financial burden for households.
Kenya (2008)	Kenya	Free UPE	Qualitative	Initial surges in enrolment followed by retention issues and funding/quality concerns.
Tooley et al. (2008)	Kenya	Free UPE	Qualitative	Enrolment increased in Government schools, but much larger increase in private schools for the poor.
Chimombo (2005)	Malawi	Free UPE	Qualitative	Increased access to schooling has been achieved at the expense of the quality of education.
Sifuna (2005)	Kenya	Free UPE	Qualitative	Little impact on access and participation of pastoralist communities.
Avenstrup (2004)	Kenya, Lesotho, Malawi, Uganda	Free UPE	Qualitative	Increased access to and resources for education, accompanied by 'access shock' and questions about quality and sustainability.
OWN and Associates (2004)	Kenya	Free UPE	Qualitative	Capitation grant inadequate to cover education costs. Quality of education and retention of students is an issue.
Al-Samarrai (2003)	Botswana, Malawi, Uganda	Free UPE	Qualitative	Improved access to education for the poor but lack of a relationship across countries between public spending and education outcomes.
Sumra (2003)	Tanzania	Free UPE	Qualitative	Infrastructure and other resources have not kept up with enrolment. Confusion about what fees are still required and how schools can use funds.
Makori (n.d.)	Kenya	Free UPE	Qualitative	UPE increased access by disadvantaged children and reduced repetition but was accompanied by quality issues.
Khandker et al. (2003)	Bangladesh	Targeted scholarships for girls	QED without equating of groups	Programme increased girls' secondary education substantially but had no discernable impact on schooling of boys.

Study	Country	Intervention	Methodology	Outcomes
Fuwa (2001)	Bangladesh	Targeted scholarships for girls	QED without equating of groups	Increased female enrolment by 2% above prevailing trend rate of increase and negative impact on male enrolment.
Arends-Kuenning and Amin (2004)	Bangladesh	Targeted scholarships for girls	QED without equating of groups	Increases in enrolment, increases in the amount of time students spent on schooling activities, and changes in the grade distribution all played a role in explaining the increase in the time that children spent in school. Older girls, especially, experienced increased enrolments and changes in the grade distribution from lower to higher grades.
Chapman and Mushlin (2008)	Sierra Leone, Djibouti	Targeted scholarships for girls	Qualitative	Scholarships often created significant tensions between award recipients and non-recipients and amongst their families that frequently isolated recipient girls and led to their harassment by non-recipients.
Amin and Sedgh (1998)	Bangladesh	Targeted scholarships for girls	Qualitative	Increased school enrolment and delayed marriage.
USAID (1999)	Malawi	Fees waivers for girls	Qualitative	Increased girls' primary enrolments and persistence but quality improvements lag and sustainability of programme in doubt.

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