Chapter 2

Encouraging School Participation in Developing Countries: A Review

2.1 Introduction

In the year 2000 the millennium development goals were signed. One of the millennium development goals was to achieve universal primary education by 2015. Good progress towards achieving this goal has been made since then. In developing regions, primary enrollment rates increased from 83% in 2000 to 89% in 2008. However, despite this progress, the goal of universal primary education is unlikely to be reached by 2015 (UN 2010).\(^1\) Moreover, while primary enrollment rates have increased quite rapidly, secondary enrollment rates remain low in many parts of the developing world. Malawi, for instance, has nearly achieved universal access to primary education, but secondary enrollment rates (roughly 25%) still leave much to be desired.

These numbers suggest that, despite the progress that has been made, there still is a need for interventions that effectively and efficiently increase school participation in developing countries. This chapter reviews the latest evidence on the effectiveness of different types of interventions that aim to improve school participation. There are other reviews of this literature including Glewwe and Kremer (2006) and more recently Kremer and Holla (2009). This chapter builds on these earlier reviews and there is inevitably a degree of overlap with these studies. However, the scope of this chapter is broader than the latest review of the literature by Kremer and Holla (2009), as I also include evidence from

\(^1\)Achieving this goal has been particularly hard in Sub-Saharan Africa, which is home to nearly half of the world’s 69 million primary school aged children not in school (United Nations 2010).
non-randomized evaluations and incorporate evidence from several randomized evaluations (including chapters in this dissertation) that appeared after their review. Moreover, instead of purely describing what works, I pay particular attention to the optimal design of the different interventions.

In accordance with previous reviews of this literature, the evidence discussed in this review suggests that user fees form an important obstacle for school participation in developing countries. Malawi and Kenya, for instance, both experienced a surge in enrollment rates after they abolished school fees. The finding that school participation is strongly affected by school fees is in accordance with evidence presented by Holla and Kremer (2009), who discuss the literature on the pricing and uptake of health and education in developing countries. They point to a recurring finding in the literature: small user fees tend to have a strong negative impact on the uptake of services (including schooling) in developing countries.

The literature suggests that the provision of direct incentives in the form of cash transfers, such as the intervention discussed in this dissertation, can also be highly effective. And importantly, these interventions already appear to be effective at low transfer amounts.\(^2\) The most recent evidence discussed in this review also suggests that schooling conditions attached to the cash transfers are often hard to enforce and may have detrimental side effects. It may therefore not always be optimal to attach a schooling condition to cash transfers.

The review also discusses a series of interventions that attempt to directly tackle some of the other challenges that keep pupils in developing countries out of school. These challenges include malnutrition, poor health, limited information about the payoffs of school attendance, and poor school quality. The presented evaluation studies show that tackling each of these challenges can affect school participation to varying degrees. As I discuss in the concluding section of this chapter, the evidence suggests that the treatment of common health problems such as intestinal helminthes and the provision of information regarding the payoff of school attendance are the two most cost-effective interventions.

The review then discusses that implementing the discussed interventions on a large scale is not always straightforward. Inefficiency and corruption can make the interventions less effective and there may be unexpected and undesirable general equilibrium effects associated with the interventions. Moreover, in many developing countries the quality of

\(^2\)Moreover, school enrollment does not appear to be very elastic to transfers over and above the minimum amounts used in the available studies.
education is often poor and increased school participation does not necessarily result in equally strong increases in skill accumulation. However, as I discuss towards the end of this chapter, the evidence suggests that when increased school participation does result in skill accumulation there tend to be strong payoffs in terms of economic development. Additionally, many of the discussed interventions are associated with a wide range of other beneficial effects, including reduced HIV/AIDS transmission rates.

I identified the relevant papers for this literature review using two electronic bibliographical databases (Google Scholar and Econlit). I selected papers for inclusion in the review following a procedure similar to the one used by Adelman, Gilligan, and Lehrer (2008). According to this procedure, to be included, papers:

1. must focus on interventions aimed at increasing school participation;
2. must have primary or secondary school pupils as their primary subjects of investigation;
3. must clearly specify how they measure school participation (e.g. enrollment rates, attendance rates, dropout rates);
4. preferably have been subject to peer review;
5. preferably employ rigorous statistical techniques that allow for the identification of the causal impact of the intervention.

Point 5. merits further explanation and the following section of this chapter continues by discussing this issue in more detail. In section 2.3 the chapter then describes the evidence from rigorous evaluations of interventions that aim to increase school participation. Section 2.4 discusses the difficulties that can be encountered when scaling up some of the interventions discussed in section 2.3. Section 2.5 discusses what results may be expected from interventions that aim to increase school participation and section 2.6 concludes.

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3Not all papers in this chapter have been subject to peer review. Inclusion of papers not subject to peer review is by definition somewhat arbitrary. However, the papers discussed in this chapter that were not subject to peer review typically have strong mechanisms to identify treatment effects and have either appeared too recently for peer review or have been cited in other literature reviews or influential papers in the field.
Chapter 2. Encouraging School Participation

2.2 Rigorous Evaluations

Rigorous evaluations aim to establish the causal effect of interventions on specific outcomes (in this chapter school participation). To do so, rigorous evaluations must determine how the beneficiaries of an intervention would have fared in the absence of the intervention, an outcome that, by definition, cannot be observed. This section starts with a discussion of this so-called evaluation problem. Next, this section introduces several prominent strategies to solve the evaluation problem: field experiments, regression discontinuity designs, and non-experimental methods. This subsection is based on Adelman, Gilligan, and Lehrer (2008), Cameron and Trivedi (2005), Duflo, Glennerster, and Kremer (2008), Duflo and Kremer (2005), and Heckman, Ichimura, and Todd (1997; 1998).

2.2.1 The Evaluation Problem

In order to understand the evaluation problem, suppose that we want to identify the impact of a homogeneous binary intervention (treatment), such as a school feeding program. Individuals can be in one and only one of two states: either they receive the treatment (state 1) or they do not receive the treatment (state 0). This situation can be cast in the well known econometric notation of among others Heckman, Ichimura, and Todd (1997; 1998). Let the binary variable $D$ denote treatment, where $D = 1$ if an individual receives treatment and $D = 0$ if the individual does not receive treatment. Let $Y_1$ be the outcome if this individual receives treatment and let $Y_0$ be the outcome if this individual does not receive treatment. If both $Y_1$ and $Y_0$ could be observed then the counterfactual problem would be solved and the treatment effect for an individual would be given by $Y_1 - Y_0$. However, the individual treatment effect is not identified, because for each individual we only observe:

$$Y = DY_1 + (1 - D)Y_0.$$

This fundamental missing data problem can only be solved at the population level. At that level, the average gain from participation in the intervention (the average treatment effect on the treated, or ATET) is given by:

$$ATET = E[Y_1 \mid D = 1] - E[Y_0 \mid D = 1].$$
The first component of the ATET (the average outcome in the treatment group, \( E[Y_1 \mid D = 1] \)) can readily be observed. The second component (the population level counterfactual outcome \( E[Y_0 \mid D = 1] \)) is unobserved, but can be approximated when there is a valid comparison group.

A naïve estimate of the ATET would use the average outcome of individuals who do not participate in the intervention, \( E[Y_0 \mid D = 0] \), to approximate the counterfactual outcome \( E[Y_0 \mid D = 1] \). This naïve estimate, however, will be biased when there are systematic differences between individuals in the treatment and control group. Such systematic differences typically arise when individuals can self-select into the treatment and control group or when the intervention is targeted at a specific group of individuals (such as poor people in a specific geographical area). This selection bias is given by:

\[
B = E[Y_0 \mid D = 1] - E[Y_0 \mid D = 0].
\]

There are rigorous evaluation strategies that can be used to identify a valid unbiased population level counterfactual. Below I discuss the most prominent examples of these strategies.

### 2.2.2 Identifying the Population Level Counterfactual

In development economics, field experiments (also known as randomized experiments or randomized controlled trials) have become the preferred strategy to identify population level counterfactuals. Field experiments randomly assign individuals (or clusters of individuals such as pupils in schools or inhabitants of villages) either to the treatment or the control group. When the randomization is successful there are no systematic differences between the treatment and the control group. The expected outcome in absence of the intervention is then equal in the treatment and control group:

\[
E[Y_0 \mid D = 1] = E[Y_0 \mid D = 0]
\]

and differences between these two groups in the presence of the intervention can be attributed to the intervention (see Duflo, Glennerster, and Kremer, 2008). Chapters 3 and 4 of this dissertation employ this methodology.

In some cases, the treatment effect can be identified even when treatment / control status is not ex-ante randomly assigned by the researcher. One such possibility occurs
when other sources of exogenous variation in the treatment status can be used to identify
the population level counterfactual. The regression discontinuity design is a well known
variant of this family of so-called instrumental variable estimators and it is regularly ap-
plicated in the economics of education literature. The regression discontinuity design exploits
situations where the probability of being offered treatment jumps if an observed covariate
(typically called the forcing variable) exceeds a known threshold. Such thresholds are
usually the result of administrative rules. When individuals do not have precise control
over the forcing variable, the expected outcome in absence of the intervention is equal for
individuals with a forcing variable \(X\) just above and below the threshold \(c\):

\[
\lim_{x \downarrow c} E[Y_0 | X = x] = \lim_{x \uparrow c} E[Y_0 | X = x].
\]

Individuals just below the threshold then serve as a valid counterfactual for those just
above the threshold and can be used to obtain an estimate of the local treatment effect
at the threshold score (for detailed instructions on the implementation of the regression
discontinuity design see Imbens and Lemieux, 2008; Lee and Lemieux, 2009). Chapter 5
of this dissertation provides an application of the regression discontinuity design.

When there is no opportunity to exploit exogenous variation in treatment status, one
can try to eliminate selection bias by controlling for observed baseline characteristics of
program participants. When the observed baseline characteristics \(Z\) capture all relevant
differences between the treatment and the control group, this procedure too results in an
unbiased estimate of the population level counterfactual:

\[
E[Y_0 | D = 1, Z] = E[Y_0 | D = 0, Z].
\]

Controlling for observed baseline characteristics can be done in a regression framework or
using (propensity score) matching.\(^4\) A related way to control for baseline characteristics
is to compare changes in the outcome variable over time between the treatment and the
control group in a differences in differences framework.\(^5\)

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\(^4\)Propensity score matching compares each individual beneficiary of the intervention to a popu-
lation level counterfactual composed of non-beneficiaries with comparable baseline characteristics (for a
thorough discussion see Heckman, Ichimura, and Todd, 1997; 1998).

\(^5\)See, for instance, Cameron and Trivedi, 2005.
2.2.2.1 Beyond Intention-to-Treat Effects

The discussed techniques are primarily used to estimate intention-to-treat effects. Intention-to-treat effects consider all individuals who were offered treatment to be part of the treatment group, even when these individuals decided not to participate in treatment or were later dropped from the treatment group for other reasons. Intention-to-treat effects are an important outcome from a policy perspective, because they give an indication of the overall impact that can be expected when an intervention is implemented on a larger scale. Intention-to-treat effects, however, are usually not the only relevant outcome of interest. Policy makers will also be interested in spillover effects of the intervention on individuals who do not participate in treatment, heterogeneity in the uptake of treatment, and heterogeneity in treatment effects on the treated individuals. An understanding of these other program impacts can serve as a guide to policy makers when the interventions are rolled out on a larger scale. Many studies therefore provide information on a range of these impact indicators.

2.2.2.2 Complications

Although all of the discussed estimation techniques can in principle be used to identify treatment effects, they each have their downsides. The downsides of techniques that do not rely on exogenous variation in treatment status are most pronounced. They rely on the assumption that, conditional on the observed baseline characteristics, treatment is essentially randomly assigned. This assumption cannot be tested. Similarly, differences in differences estimation assumes that changes in the outcome variable would have been alike in the treatment and control group in absence of the intervention.

Instrumental variable estimation is not without limitations either. The regression discontinuity design, for instance, relies on the correct implementation of the assignment rules and the inability of individuals to sort endogenously around the discontinuity point (assumptions that can by and large be checked).\textsuperscript{6} Field experiments too rely on the correct implementation of the randomized assignment to the treatment and the control group. However, because the investigators are typically in control of this process, they will

\textsuperscript{6}Urquiola and Verhoogen (2009) provide an example of such endogenous sorting. The authors show that in Chile, which caps the maximum pupil teacher ratio by law, schools adjust fees when class sizes reach the maximum legal pupil teacher ratio in order to avoid adding an additional teacher. Hence, the discontinuities around the maximum class size cannot be automatically be used a source of plausibly exogenous variation in the pupil teacher ratio.
Because they provide a comparatively clean source of identification, field experiments have become the preferred source of identification in development economics. Nonetheless, field experiments too can have shortcomings. First, most field experiments are implemented on a rather small scale. Hence, regardless of the clean evaluation methodology, there may be concerns regarding the external validity of the results. Can the same results be expected when the intervention is implemented in another geographical area? Would large scale implementation of the intervention lead to adverse general equilibrium effects? Moreover, at times randomized implementation of an intervention may be considered unethical, too expensive, or simply impossible (Duflo, Glennerster, and Kremer, 2008, give the example of central bank independence, which cannot be randomized for obvious reasons).

The advantages and disadvantages of the different evaluation techniques have led to considerable debate in the development economics literature. Proponents of the randomized approach argue for expanding the use of randomized evaluations. Important arguments in favor of the use of randomized evaluations are (i) that they provide clean identification and thus overcome evaluation problems that are often encountered when using other evaluation procedures (Duflo and Kremer, 2005) and (ii) that they reduce the potential for publication bias (Duflo, Glennerster, and Kremer, 2008).

Critics, however, worry that a preference for specific evaluation techniques may result in a narrow focus on the questions that can be answered using these techniques. Deaton (2010), for instance, argues that researchers should shift their focus from (quasi-) experiments that seek to answer the question “what projects work?” to the more important question “what mechanisms explain why and when projects work?”. Ravallion (2009) concurs and notes that we can only learn valid lessons for scaling up the evaluated projects if we understand these mechanisms.

Given the potential shortcomings of all evaluation methods, I do not limit the remainder of this chapter to randomized evaluations (such as for instance Kremer and Holla, 2009, do). However, when evidence is available from multiple sources of identification I do devote most attention to evaluations that rely on exogenous variation in treatment status identification (usually field experiments or regression discontinuity designs). I only discuss evidence from observational studies when (quasi-) experimental evidence is non-existent. Moreover, when available, I discuss the evidence on why and when the different
interventions work.

2.3 What Works?

I now turn to approaches for which rigorous evaluations have shown that they can effectively raise school participation in developing countries. I start by discussing interventions that have the goal to directly encourage pupils to attend school and parents to send their kids to school. I then proceed to discuss interventions that are targeted at the school system and that increase school participation through changes in this system.

Before providing a detailed description of the studies describing these interventions, Tables 2.1 and 2.2 respectively summarize the average impact of interventions targeted at parents and pupils and interventions targeted at schools and the school system. The interventions targeted at parents and pupils contain 7 subcategories: school fee reductions, other cost reductions (for instance in the form of free school uniforms), conditional cash transfers, school-feeding programs, take home rations (which disburse food conditional on satisfactory school attendance), school health programs, and the provision of information to pupils regarding the expected financial returns to education. The interventions targeted at the school system contain 3 subcategories related to school quality (learning materials, teachers, and elite schools) and 3 subcategories that can be classified as alternative modes of service delivery (teacher incentives, citizen participation, and the provision of school vouchers that aim to increase access to private schools).

It is important to realize that the impact of the interventions reported in the different studies is not always immediately comparable. First, while most studies focus on primary schools, some studies focus on secondary education. Second, not all studies focus on the same outcome measure. Some studies use enrollment as an indicator of school participation, others studies use attendance. Finally, as the column labeled “Overall” indicates, the majority of the studies provides only a partial indicator of the overall impact of an intervention on school participation. The reason for this is that many studies investigate how an intervention affects pupils who are already attending school. These studies do not account for the fact that large scale implementation of the intervention may induce pupils who would otherwise not attend school to enroll.7

7Nearly all studies provide multiple estimates. I display only the most comprehensive estimates provided in the studies. When no results are available for boys and girls combined I show the results for boys and girls separately.
### Table 2.1: Impact of interventions that aim to improve school participation targeted at pupils and parents

<table>
<thead>
<tr>
<th>Country</th>
<th>Study</th>
<th>School type</th>
<th>Overall</th>
<th>Outcome</th>
<th>Identification</th>
<th>Impact</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>School fee reductions</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Malawi</td>
<td>Al Samarrai and Zaman (2007)</td>
<td>Primary</td>
<td>Yes</td>
<td>enrollment</td>
<td>Changes over time</td>
<td>50 pp</td>
</tr>
<tr>
<td>Kenya</td>
<td>Lucas and Mbiti (2010)</td>
<td>Primary</td>
<td>Yes</td>
<td>exam participation</td>
<td>Diff in diff</td>
<td>8 pp</td>
</tr>
<tr>
<td>Colombia</td>
<td>Barrera-Osorio, Linden, and Urquiola (2007)</td>
<td>Primary</td>
<td>Yes</td>
<td>enrollment</td>
<td>RD</td>
<td>3 pp</td>
</tr>
<tr>
<td>Colombia</td>
<td>Barrera-Osorio, Linden, and Urquiola (2007)</td>
<td>Secondary</td>
<td>Yes</td>
<td>enrollment</td>
<td>RD</td>
<td>6 pp</td>
</tr>
<tr>
<td><strong>Further cost reductions: free school uniforms</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Kenya</td>
<td>Duflo et al. (2006)</td>
<td>Primary</td>
<td>No</td>
<td>enrollment, girls</td>
<td>RCT</td>
<td>3 pp</td>
</tr>
<tr>
<td>Kenya</td>
<td>Duflo et al. (2006)</td>
<td>Primary</td>
<td>No</td>
<td>enrollment, boys</td>
<td>RCT</td>
<td>2 pp</td>
</tr>
<tr>
<td>Kenya</td>
<td>Evans, Kremer, and Ngatia (2009)</td>
<td>Primary</td>
<td>No</td>
<td>attendance</td>
<td>RCT</td>
<td>6 pp</td>
</tr>
<tr>
<td><strong>Conditional cash transfer programs</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mexico</td>
<td>Schultz (2004)</td>
<td>Primary, final grade</td>
<td>Yes</td>
<td>enrollment</td>
<td>RCT</td>
<td>9 pp</td>
</tr>
<tr>
<td>Various</td>
<td>Review of 13 studies by Fiszbein and Schady (2009)</td>
<td>Primary and Secondary</td>
<td>-</td>
<td>enrollment</td>
<td>Various</td>
<td>3 - 13 pp</td>
</tr>
<tr>
<td>Malawi</td>
<td>Baird, McIntosh, and Özer (2009)</td>
<td>Primary and Secondary</td>
<td>Yes</td>
<td>enrollment, girls</td>
<td>RCT</td>
<td>12 pp</td>
</tr>
<tr>
<td><strong>School feeding programs</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>Kenya</td>
<td>Omwami, Neumann, and Bwibo (2010)</td>
<td>Primary</td>
<td>No</td>
<td>attendance</td>
<td>RCT</td>
<td>0-4 pp</td>
</tr>
<tr>
<td>Jamaica</td>
<td>Powell et al. (1998)</td>
<td>Primary</td>
<td>No</td>
<td>attendance</td>
<td>RCT</td>
<td>2 pp</td>
</tr>
<tr>
<td>Philippines</td>
<td>Tan, Lane, and Lassibille (1999)</td>
<td>Primary</td>
<td>No</td>
<td>enrollment</td>
<td>RCT</td>
<td>No impact</td>
</tr>
<tr>
<td>Burkina Faso</td>
<td>Kazangi, De Walque, and Alderman (2008)</td>
<td>Primary</td>
<td>No</td>
<td>enrollment, girls</td>
<td>RCT</td>
<td>No impact</td>
</tr>
<tr>
<td>Burkina Faso</td>
<td>Kazangi, De Walque, and Alderman (2008)</td>
<td>Primary</td>
<td>Yes</td>
<td>enrollment, boys</td>
<td>RCT</td>
<td>No impact</td>
</tr>
<tr>
<td>Uganda</td>
<td>Alderman, Gilligan, and Lehrer (2010)</td>
<td>Primary</td>
<td>Yes</td>
<td>enrollment</td>
<td>RCT</td>
<td>9-14 pp</td>
</tr>
<tr>
<td>Uganda</td>
<td>Alderman, Gilligan, and Lehrer (2010)</td>
<td>Primary</td>
<td>Yes</td>
<td>attendance</td>
<td>RCT</td>
<td>9-14 pp</td>
</tr>
<tr>
<td><strong>Take home rations</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Burkina Faso</td>
<td>Kazangi, De Walque, and Alderman (2008)</td>
<td>Primary</td>
<td>Yes</td>
<td>enrollment, girls</td>
<td>RCT</td>
<td>6 pp</td>
</tr>
<tr>
<td>Uganda</td>
<td>Alderman, Gilligan, and Lehrer (2010)</td>
<td>Primary</td>
<td>Yes</td>
<td>enrollment</td>
<td>RCT</td>
<td>No impact</td>
</tr>
<tr>
<td>Uganda</td>
<td>Alderman, Gilligan, and Lehrer (2010)</td>
<td>Primary</td>
<td>Yes</td>
<td>attendance</td>
<td>RCT</td>
<td>9-14 pp</td>
</tr>
<tr>
<td><strong>School health programs (respectively deworming and deworming plus iron supplements)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>Kenya</td>
<td>Miguel and Kremer (2004)</td>
<td>Primary</td>
<td>No</td>
<td>school years per treated child</td>
<td>RCT</td>
<td>0.14</td>
</tr>
<tr>
<td>India</td>
<td>Bobonis, Miguel, and Puri-Sharma (2006)</td>
<td>Pre-school</td>
<td>No</td>
<td>attendance</td>
<td>RCT</td>
<td>6 pp</td>
</tr>
<tr>
<td><strong>Information on returns to education</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Dominican Republic</td>
<td>Jensen (2010)</td>
<td>Primary</td>
<td>No</td>
<td>years of schooling</td>
<td>RCT</td>
<td>0.2 years</td>
</tr>
<tr>
<td>Madagascar</td>
<td>Nguyen (2008)</td>
<td>Primary</td>
<td>No</td>
<td>attendance</td>
<td>RCT</td>
<td>3 pp</td>
</tr>
</tbody>
</table>

Notes: See Table 2.2 on following page.
Table 2.2: Impact of interventions that aim to improve school participation targeted at schools and the school system

<table>
<thead>
<tr>
<th>Country</th>
<th>Study</th>
<th>School type</th>
<th>Overall</th>
<th>Outcome</th>
<th>Identification</th>
<th>Impact</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>School quality: learning materials (respectively pedagogical materials, computer assisted learning, and textbooks)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Philippines</td>
<td>Tan, Lane, and Lassibille (1999)</td>
<td>Primary</td>
<td>No</td>
<td>enrollment</td>
<td>RCT</td>
<td>4 pp</td>
</tr>
<tr>
<td>India</td>
<td>Banerjee et al. (2007)</td>
<td>Primary</td>
<td>No</td>
<td>enrollment and attendance</td>
<td>RCT</td>
<td>No impact</td>
</tr>
<tr>
<td>Kenya</td>
<td>Glewwe, Kremer, and Moulin (2009)</td>
<td>Primary, lower grades</td>
<td>No</td>
<td>enrollment</td>
<td>RCT</td>
<td>No impact</td>
</tr>
<tr>
<td>Kenya</td>
<td>Glewwe, Kremer, and Moulin (2009)</td>
<td>Primary, final grade</td>
<td>No</td>
<td>entering secondary school</td>
<td>RCT</td>
<td>5 pp</td>
</tr>
<tr>
<td><strong>School quality: teachers (respectively remedial teachers and pupil teacher ratios)</strong></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>India</td>
<td>Banerjee et al. (2007)</td>
<td>Primary</td>
<td>No</td>
<td>enrollment and attendance</td>
<td>RCT</td>
<td>No impact</td>
</tr>
<tr>
<td>South Africa</td>
<td>Case and Deaton (1999)</td>
<td>Primary and Secondary</td>
<td>Yes</td>
<td>years of schooling</td>
<td>Natural exp.</td>
<td>0.5 years / 10 pupils</td>
</tr>
<tr>
<td><strong>School quality: elite schools</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Malawi</td>
<td>De Hoop (2011, Chapter 5 of this dissertation)</td>
<td>Secondary</td>
<td>No</td>
<td>exam participation</td>
<td>RD</td>
<td>20 pp</td>
</tr>
<tr>
<td>Romania</td>
<td>Pop-Eleches and Urquiola (2010)</td>
<td>Secondary</td>
<td>No</td>
<td>exam participation</td>
<td>RD</td>
<td>No impact</td>
</tr>
<tr>
<td>Trinidad and Tobago</td>
<td>Jackson (2010)</td>
<td>Secondary</td>
<td>No</td>
<td>exam participation</td>
<td>RD</td>
<td>1.5 - 3 pp</td>
</tr>
<tr>
<td><strong>Alternative modes of service delivery: teacher incentives</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Malawi</td>
<td>Glewwe, Ilias, and Kremer (2009)</td>
<td>Primary</td>
<td>No</td>
<td>enrollment</td>
<td>RCT</td>
<td>No impact</td>
</tr>
<tr>
<td><strong>Alternative modes of service delivery: citizen participation</strong></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>India</td>
<td>Banerjee et al. (2010)</td>
<td>Primary</td>
<td>Yes</td>
<td>enrollment, boys &amp; girls</td>
<td>RCT</td>
<td>No impact</td>
</tr>
<tr>
<td>India</td>
<td>Banerjee et al. (2010)</td>
<td>Primary</td>
<td>Yes</td>
<td>attendance, boys &amp; girls</td>
<td>RCT</td>
<td>No impact</td>
</tr>
<tr>
<td><strong>Alternative modes of service delivery: Vouchers</strong></td>
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<tr>
<td>Colombia</td>
<td>Angrist, Bettinger, and Kremer (2006)</td>
<td>Secondary</td>
<td>Yes</td>
<td>graduation rate</td>
<td>RCT</td>
<td>6 pp</td>
</tr>
</tbody>
</table>

Notes: Impacts displayed are at least significant at the 10% level.

All of the studies in this table are discussed in more detail in the remainder of this chapter. The studies are ordered in the sequence in which they are discussed in the main text.

The abbreviations “Diff in diff”, “RD”, “RCT”, and “pp” respectively stand for “Differences in differences”, “Regression Discontinuity”, “Randomized Controlled Trial”, and “percentage points”.

The column labeled “School type” displays the type of school (primary or secondary) in which the intervention took place.

The column labeled “Overall” takes the value “Yes” if the impact estimate provides the overall impact of the intervention, including the impact on children who were not in school at the start of the intervention. The column takes the value “no” if the impact of the intervention provides only the impact of the intervention on pupils who were already in school.

The cash transfer intervention in Mexico (Schultz, 2004) had a weaker impact on pupils in other grades and did not induce children who were previously not in school to enroll.
The Malawi cash transfer intervention, which is the topic of Chapters 3 and 4 of this dissertation, is unique in this respect. It allows for a comparison of the impact of the intervention on children who are in school to the impact of the intervention on children who were not previously in school. As I discuss in below the difference between the two impact estimates is substantial.

Overall Tables 2.1 and 2.2 indicate that each of the different categories of interventions to be discussed in this review can have a substantial effect on school participation. However, as discussed above, these average impact estimates can hide a substantial degree of heterogeneity that can be important from a policy making perspective. The estimates can also hide serious unexpected general equilibrium effects and the estimates based on small randomized trials may run into institutional constraints when interventions are rolled out on a larger scale. I discuss each of these issues in more detail below.

The fact that different categories of interventions all appear to be effective implies that policy makers who wish to increase school participation will face a difficult choice between the different available alternatives. In order to make their choice they will, among other things, be interested in the cost-effectiveness of the different interventions. Towards the end of this chapter I provide some (ballpark) cost-effectiveness estimates.8

2.3.1 Interventions Targeted at Pupils and Parents

2.3.1.1 Cost Reductions

This first subsection focuses on the effectiveness of interventions that make education more accessible by cutting the costs incurred by households to let their children attend school.

School Fee Reductions To combat low enrollment rates, multiple Sub-Saharan countries have abolished school fees over the past decades. Malawi, the focus of the following chapters in this dissertation, is a prime example. In 1994 it abolished fees for all public primary schools as one of the first countries in Sub-Saharan Africa. Al-Samarrai and Zaman (2007) describe how, in the 5 years after the abolition, primary enrollment in Malawi surged by over 50% from 1.9 million pupils to 2.9 million students. Improvements in enrollment rates were strongest among Malawi’s poorest citizens and the initially large

8These cost-effectiveness estimates extend previous estimates available in J-Pal (2010)
differences in the primary enrollment rates of the poorest and richest quintile were virtually eliminated in the years after the abolition.

On a more negative note, the increased enrollment rate also put both Malawi’s primary and secondary schools under strain, as they had to cope with increasing shortages of nearly all human and physical resources. The government of Malawi did attempt to accommodate the rising number of primary school pupils by recruiting approximately 18,000 untrained teachers. However, the government did not manage to provide a sufficient number of trained teachers. By 1997, three years after the introduction of free primary education, the ratio of pupils to qualified teachers was 120 to 1, with potential detrimental consequences for the quality of education. Al-Samarrai and Zaman (2007) therefore suggest to carefully phase in future school fee abolition schemes and to combine them with sufficient investments in school quality.

In the years after Malawi abolished primary school fees, Kenya, Tanzania, Uganda, and Zambia followed suit. These countries experienced similarly strong increases in enrollment and similar shortages in human and physical capital in the years after the abolition of primary school fees (Kattan and Burnett, 2004; Riddell 2003). Although the increases in school participation in these countries are likely to be primarily caused by the abolition of school fees, Lucas and Mbiti (2010) recognize that there are potential endogeneity concerns. Other events and programs, for instance, may also have affected nationwide enrollment rates, thus making it impossible to attribute the entire increase in school participation to the abolition of school fees.

A study by Barrera-Osorio, Linden, and Urquiola (2007) does identify the causal effect of a fee reduction program introduced in 2004 by the municipal government of Bogota, Colombia, on school enrollment. To identify this causal effect, the authors exploit the procedure used to assign pupils into the fee reduction program. This procedure relies on a survey-based index to classify households into 6 vulnerability categories. Primary school pupils receive a 100% reduction in complementary service charges if they are in the highest vulnerability category. Given that primary schools typically do not charge other tuition fees, this reduction essentially implies that pupils in the highest vulnerability category can attend primary school free of charge. Secondary school pupils receive a 100% reduction in tuition fees and complementary charges if they are in the highest vulnerability category.

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9Although Al-Samarrai and Zaman do not indicate what this ratio was before the introduction of free primary indication, they do suggest that this finding is an indication of deteriorated quality of education in Malawi.
They receive a 50% reduction if they are in the second highest category. Regression discontinuity estimates show that the fee reduction program raises school enrollment rates by approximately 3 percentage points in primary schools (baseline enrollment of 89%) and by 6 percentage points among pupils in the second highest vulnerability category in secondary schools (baseline enrollment of 93%). Being in the first versus the second vulnerability category (e.g. moving from 50 to 100% fee reduction) in secondary school appears to have a modest beneficial impact on enrollment, but this effect is not robust to different specifications. The authors also separately investigate the impact of the intervention among pupils who were attending school at baseline and those who were not. Among pupils who were attending school at baseline the results are robust and roughly equivalent to those in the full sample. (Given that most were in school at baseline it is this group of pupils that drives the overall impact of the intervention on enrollment rates.) Possibly partly due to a smaller sample size, the results are not robust among pupils who were not in school at baseline. Among this group the impact of the intervention is sometimes negative and not always significant.

Lucas and Mbiti (2010) investigate how the abolition of primary school fees in Kenya in 2003 affected school participation. To do so, the authors use participation in the primary school exam for the years 2001 to 2007 as an indicator for school participation. While the authors do not exploit exogenous variation in the level of school fees to identify the impact of the abolition of school fees, the access to multiple years of data does allow them to get rid of several confounding factors, such as district and time fixed effects. Given that the primary school exam is administered in grade 8 the study looks at the effect of the intervention on those pupils who had already been in school and completed a minimum of 3 years of education when primary school fees were abolished. The results suggest that the abolition of school fees indeed resulted in improved school participation: in the years after the introduction of free primary education the average number of exam takers in primary schools increased by approximately 8 percentage points (or 2.5 students on a average of 31 exam takers per primary school). Lucas and Mbiti (2010) also show that the introduction of free primary education had a negative impact on the quality of education in Kenya’s primary schools. The authors test for the impact of free primary education on the pupil teacher ratio in public schools, a proxy for school quality, and show that it increased considerably. In the 3 years after the introduction of free primary education the ratio went up by 9 pupils per teacher (a 29% increase over the average ratio in 2001). The
top public schools experienced the largest increases in enrollment as well as deteriorated exam performance.

**Further Cost Reductions: Free School Uniforms** Even if countries abolish official public school fees, households often still incur substantial costs in order to make children attend school. Al-Samarrai and Zaman (2007), for example, explain that despite the abolition of primary school fees, public Malawian primary schools often still require parents to pay informal fees and contributions. Households may also have to invest in items like school uniforms and books, which can pose a substantial financial burden. Two randomized evaluations conducted in Kenya evaluate whether further reductions in the cost of school attendance result in improvements in school participation. Both of these interventions provide free school uniforms to pupils. This type of intervention is relevant, because many schools in developing countries require pupils to wear school uniforms. As the interventions were targeted at pupils who were in school at baseline they do not capture potential effects on pupils who were not in school at the start of the interventions.

Duflo et al. (2006) evaluate the impact of uniforms distributed to children in a random sub-sample of 163 schools out of a group of 328 primary schools in Kenya. The authors discuss that in Kenya, where primary school fees had been abolished before the start of the interventions, school uniforms are the most important remaining financial barrier to public primary education. Three years after the start of the intervention, boys and girls who had received free school uniforms were respectively 2 and 2.5 percentage points respectively less likely to have dropped out of school (mean dropout rates in the control group were 7.9% and 12.4% respectively).

Similarly, Evans, Kremer, and Ngatia (2009), evaluate the impact of an intervention in which school uniforms were distributed to a random subsample of children attending 12 schools in poor communities in Kenya. Attendance data were collected for these pupils through unannounced school visits. The authors again find that the intervention had a strong impact on school participation. The pupils who had been offered uniforms were 6.2 percentage points less likely to be absent during the unannounced school visits than pupils who had not been offered uniforms (absence rates at baseline were approximately 13%). This effect appears to have been stronger among pupils who did not have a uniform before the start of the program.
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2.3.1.2 Conditional Cash Transfers

Over the past decade, governments have increasingly started to adopt conditional cash transfers to encourage pupils from disadvantaged backgrounds to attend school. These programs provide a subsidy to the student or his/her household, on the condition that the student attends school on a regular basis. Mexico’s PROGRESA was the first conditional cash transfer scheme to be rigorously evaluated. The evidence presented in the evaluation studies of PROGRESA indicated that the program had a strong impact on a host of outcomes. Schultz (2004), for instance, showed that PROGRESA had a strong impact on pupils who had completed grade 6, the final grade of primary school. These pupils were 8.7 percentage points more likely to enter junior secondary school if their mother received one of PROGRESA’s educational conditional cash transfers (58% transferred to junior secondary school at baseline). However, the program did not appear to affect enrollment rates among pupils who had never been in school. And Gertler (2004) showed that PROGRESA had a beneficial influence on child health outcomes including morbidity, height, and anemia. The positive experiences with PROGRESA played a role the proliferation of conditional cash transfer schemes across many developing countries.

Some of these later cash transfer schemes aimed to improve school participation. Chapter 5 of Fiszbein and Schady (2009) reviews the studies that evaluate those cash transfer schemes. The review contains 13 studies: 5 randomized evaluations, 4 studies using a regression discontinuity setup, and 4 studies applying double-differencing, triple-differencing, or fixed effects methods. The review provides strong evidence for the ability of conditional cash transfers to affect school participation across many different contexts; each of the 13 studies finds a substantial and significant impact on school enrollment. The impact found in the randomized evaluations ranged from 3.3 percentage points in Honduras (baseline enrollment 66%) to 12.8 percentage points in Nicaragua (baseline enrollment 72%).

The randomly implemented conditional cash transfer intervention in Zomba, Malawi, (described in the introduction and the topic of chapters 3 and 4 of this dissertation, but not yet included in the review of Fiszbein and Schady, 2009) also resulted in strong increases in school participation. Baird, McIntosh, and Özler (2009) show that one year after the start of the intervention the average self-reported enrollment rate in the treatment group was a significant 12 percentage points higher in the treatment group than in the control group (control group enrollment 83%). Moreover, girls in the treatment group were a
significant 10 percentage points more likely to attend school regularly according to their teachers (control group attendance 67%). This impact represents the combined effect of the intervention on beneficiaries in primary and secondary school.

Interestingly, the conditional cash transfer intervention in Malawi can be used to explicitly divide the overall impact into the impact on individuals who were in school at the start of the intervention and the impact on those who were not in school. The effect on self-reported enrollment was particularly strong among girls who were not in school at baseline, as the intervention increased the likelihood that these girls were in school one year after the start of the intervention by 44 percentage points (17% in the control group). Self reported enrollment among girls who were in school at baseline was up by 4.6 percentage points (89% in the control group). These results indicate that studies that focus purely on pupils who are already in school may well miss a major part of the impact of the intervention under consideration.

Overall the evidence indicates that conditional cash transfers can serve as an effective tool to increase school participation. Policy makers who plan to implement a conditional cash transfer, however, have to make important choices regarding the design of the intervention. Most importantly, policy makers have to determine whom to target, what amounts to be dispensed, and what conditions to be imposed. The review by Fiszbein and Schady (2009) provides important insights into heterogeneous program effects that are of importance to policy makers when they determine the target group for their intervention. The cash transfer intervention in Zomba, Malawi, sheds more light on the other two crucial program parameters: the amounts to be disbursed and the conditions to be imposed. In the remainder of this subsection I discuss the lessons from these studies.

**Schooling Conditions** Turning first to schooling conditions, it is important to understand that conditional cash transfers have two main effects on school participation. First, they increase the income available to the household. To the extent that households are credit constrained this increase in income can affect their decisions to invest in education.\(^{10}\) Second, in addition to these income effects, the condition attached to the transfers changes the relative price of education and thus results in a substitution effect that can cause further increases school participation. This substitution effect can be used as a policy making tool to remedy suboptimal investments in education.

\(^{10}\)When households have access to the capital market the investment decision should be independent of the level of income available to the household.
Fiszbein and Schady (2009) discuss that investments in education can be suboptimal in two respects. First, the household’s investment in education may be below the optimal level from the child’s perspective. Reasons for these suboptimal levels of household investment include misguided beliefs about the returns to investments in schooling, conflicts of interest within the household that result in incomplete altruism, and procrastination. Second, if there are positive externalities from education, the private optimal level of investment in education may be below the social optimal level of investment in education.\textsuperscript{11}

Two recent studies suggest that treatment conditions can indeed be used as a tool to improve school participation. First, Baird et al. (forthcoming) show that, in the case of the cash transfer intervention in Malawi, the schooling condition did indeed lead to improved schooling outcomes. The intervention in Zomba contains a treatment group in which participating girls receive a conditional cash transfer, a treatment group in which participating girls receive an unconditional cash transfer, and a control group in which participating girls receive no transfer (this conditional cash transfer experiment was only conducted among girls who were in school at baseline). It thus provides the ideal experimental setup to identify the effect of the schooling condition on schooling outcomes. Baird et al. (forthcoming) show that increases in enrollment over the two years of the intervention were stronger in the conditional than in the unconditional treatment group. In the conditional treatment group teacher reported enrollment was up by roughly 0.5 terms (about 1 month and a half) in the two years that the intervention lasted and in the unconditional treatment group it was up by about 0.2 terms (enrollment in the control group was about 4.8 terms). The difference between the conditional and unconditional treatment is statistically significant (p-value = 0.011). Point estimates suggest that increases in attendance were also stronger in the conditional treatment group, but these differences are not statistically significant.

Second, a recent study by Kremer, Miguel, and Thornton (2009) examines a merit scholarship program randomly implemented in 64 out of 127 sampled primary schools in two districts in Kenya. The program awarded scholarships to the highest-scoring 15\% of grade 6 girls attending program schools. The program had a strong beneficial impact on

\textsuperscript{11}In addition, Fiszbein and Schady (2009) note that attached conditions can increase popular support for cash transfer schemes and thus lead to a favorable political economy equilibrium. The political situation in a given country, for instance, may make redistribution feasible only if it is tied to evidence of beneficiaries’ good behavior, even when there is no evidence that the condition itself would solve suboptimal private investment in children’s education.
test scores of girls in treatment schools. In addition there is suggestive evidence that the program had a modest beneficial impact on school participation in one of the two districts in which it was implemented, raising student attendance by 3.2 percentage points (mean attendance was 88%). In this case the treatment condition (scoring among the best 15%) thus appears to have resulted in improved school participation before the intervention resulted in any income effects.

Despite the evidence that conditions can be effective, the implementation of conditions must be considered carefully, first of all because conditions result in substantial operating costs, but also because they (i) are hard to enforce and (ii) may come at a cost to the beneficiary. I now proceed to describe each of these two issues in more detail. Turning first to the issue of enforcement, Shastry and Linden (2009) investigate a program that allocates grain to pupils in India conditional on their school attendance exceeding 80% (I discuss these so-called school feeding programs in greater detail below). Eligibility was determined based on attendance records taken by teachers. The authors of the study have access to these attendance records, but also to attendance records taken by a team of independent surveyors who made random school visits once a week. Comparing these two datasets, the authors find that teachers were strategically manipulating their attendance records to ensure that some of the students who did not meet the 80% requirement would receive the grain.\footnote{Clearly, this behavior on the part of teachers affects the incentives faced by pupils. This strategic manipulation of the records presents an econometric problem, as it introduces a bias in the school participation data. However, Shastry and Linden (2009) emphasize that strategic manipulation of the records is not necessarily a bad outcome in and of itself. Teachers may for instance use private information to target those pupils who would benefit most from receiving the grain.} On top of the difficulties in enforcing the treatment condition, the process of administering the treatment condition can itself affect the quality of teaching. Kazianga, De Walque, and Alderman (2008), for example, suggest that if teachers allocate part of their time to administering the program (a large part of which is enforcing the treatment conditions) the actual time available for teaching could decrease, thus lowering the quality of education.

In addition to the difficulties that may be encountered in enforcing the attached conditions, Chapter 4 of this dissertation shows that schooling conditions can come at a psychological cost to the beneficiaries of cash transfer interventions. The chapter investigates how the cash transfer intervention in Zomba affected the psychological well-being of beneficiaries (as measured by a standardized mental health instrument). The chapter shows that baseline schoolgirls who received unconditional cash transfers experienced
strong average reductions in psychological distress. Baseline schoolgirls receiving a conditional cash transfer, however, did not experience such strong reductions in psychological distress. The treatment condition thus appears to have imposed a mental burden on beneficiaries. This mental burden has not previously been mentioned in the literature on conditional cash transfers, but presents a potential argument against the use of treatment conditions.

**Cash Transfer Amounts** Within both the conditional and the unconditional treatment groups of the intervention in Malawi, amounts to be dispensed varied randomly and took integer values ranging from 5 US$ to 15 US$ per household per month.\(^{13}\) Baird, McIntosh, and Özler (2009) use this random variation in transfer sizes to show that the marginal impact of transfers beyond the minimum amount of 5 US$ on dropout and attendance rates is small in both the conditional and unconditional treatment groups. At the minimum value of 5 US$ the program induced 40.4% of the girls who were not in school at baseline to attend school (over and above the 17.2% of these girls who returned to school in the control group). Transferring additional money to these girls or their parents did not have a significant effect on school participation. Among girls who were in school at baseline additional conditional transfers beyond the minimum conditional amounts are also ineffective. However, additional unconditional transfers to the household increase enrollment by 0.08 terms per dollar (Baird, McIntosh, and Özler, forthcoming).

The limited impact of marginal transfers (i.e. transfers beyond a minimum amount) is in accordance with the findings in Filmer and Schady (forthcoming), who investigate the impact of transfer sizes on school attendance using data from a cash transfer intervention in Cambodia. For this program pupils within program schools were ranked according to the probability that they would drop out of school. The intervention aimed to encourage pupils who reached the end of primary school to attend secondary school. Pupils with a high probability of dropping out after primary school were offered a conditional transfer of 60 US$ a year, pupils with an intermediate probability of dropping out were offered a conditional transfer of 45 US$ a year, and pupils with a low probability of dropping out were not offered a transfer. The mechanism used to assign pupils to transfer groups allows Filmer and Schady (forthcoming) to identify the impact of higher transfer amounts using a regression discontinuity setup. The 45 US$ transfer raised school attendance by approximately 25 percentage points (from 44% in the group not receiving any transfers).

\(^{13}\)Amounts could be higher per household if there were multiple participating girls in one household.
At roughly 2 percentage points, however, the marginal impact of the additional 15 US$ transferred to pupils with a high probability of dropping out is small.

These results suggest that cash transfers are already effective at relatively low amounts and that increasing the amount transferred does not necessarily lead to strong improvements in school participation. That being said, there may well be a range along which marginal transfer amounts do induce meaningful changes in school participation. This range would be below the minimum transfer amount investigated by Baird, McIntosh, and Özler (2009) and Filmer and Schady (2010).

**Heterogeneous Program Effects** Knowledge of the dimensions along which program effects tend to be heterogeneous can help policy makers to determine the proper target groups for conditional cash transfer interventions. The review by Fiszbein and Schady (2009) indicates that there are two important dimensions along which the impact of conditional cash transfers on school participation may be expected to be heterogeneous. First, the impact of conditional cash transfers tends to be stronger in settings where enrollment rates are low or in grades where dropout rates are high. Grades in which students make the transition from primary to secondary school are a prime suspect. Second, program effects are typically stronger among pupils from poorer socioeconomic backgrounds. Filmer and Schady (2009) provide several potential explanations for this finding, including the fact that pupils from poorer socioeconomic backgrounds tend to have poorer pre-program schooling outcomes and that they are more likely to be credit constrained.

### 2.3.1.3 Food for Education Programs and School Health Programs

A third group of interventions attempts to improve school participation by providing school meals or health care. These programs are related to cash transfer programs, as they also provide a direct incentive for pupils to attend school. However, there is a conceptual difference with conditional cash transfer programs, as food for education programs have a stronger potential to improve the health and nutrition status of program benefi-
Chapter 2. Encouraging School Participation

...(continued from previous page)

ricies, both of which matter for school participation.\textsuperscript{14} \textsuperscript{15} (See Adelman, Gilligan, and Lehrer (2008) and Bundy et al. (2006) for a further discussion of the economic rationale behind the implementation of food for education and school health programs.) In this subsection I first discuss the literature that evaluates the effectiveness of food for education programs. I then proceed to briefly discuss two evaluations from the empirical economics literature showing that other health interventions can be similarly effective in raising school participation. For other recent reviews on the relationship between health and education / human capital formation see Bleakley (2010) and Eide and Schowalter (forthcoming).

**Food for Education Programs** School feeding programs have been used in developing countries for decades. Numerous randomized evaluations, some of them published in medical journals, evaluate the impact of school feeding programs on school attendance. Omwami, Neumann, and Bwibo (2010), for instance, randomized 12 primary schools in Kenya into 3 treatment groups and a control group each comprising 3 schools. In treatment group 1 the children received a meat snack, in treatment group 2 they received a milk snack, and in treatment group 3 they received a maize and beans snack. Only the meat snack had a statistically significant impact and increased school attendance by 4 percentage points (baseline attendance in control group was 94%). The authors, however, do not appear to use clustered standard errors and hence this result should be interpreted with care. Another study by Powell et al. (1998) randomizes children within a group of 16 Jamaican primary schools into a treatment group receiving a daily breakfast and a control group. Attendance rates in the treatment group increased by approximately 2.3 percentage points (average attendance in control group approximately 70%). A third study by Tan, Lane, and Lassibille (1999) investigates a sample of 30 primary schools in the Philippines. Out of this sample, 20 schools were randomized into 1 of 4 treatment

\textsuperscript{14}Alderman et al. (2001), for instance, look at the role of nutrition in school enrollment in Pakistan. The authors investigate how shocks in the prices of food crops experienced by children when they were of pre-school age affect their height-for-age Z-scores and subsequent school enrollment. They show that these food shocks and the resulting deterioration in anthropometrics have a substantial impact on schooling enrollment, especially among girls. According to their simulations a 0.5 improvement in female height-for-age Z-scores results in an increase of schooling enrollment of nearly 20%. Similarly, Alderman, Hoddinott, and Kinsey (2006) link civil war and drought shocks experienced by Zimbabwean children prior to age 3 to their pre-school nutritional status and number of grades completed as young adults.

\textsuperscript{15}In principle cash transfers and food for education programs (or school health programs) of equal value should not be expected to have an equivalent impact on nutrition and health status. The reason for this is that households may not spend the (entire) cash transfer on goods and services that result in improved health and nutrition status of the pupil.
groups: school feeding, the provision of pedagogical materials to teachers, and either of these interventions combined with regular parent teacher meetings. The authors find that the school feeding program (with or without parent teacher meetings) had no significant effect on school dropout rates.\(^\text{16}\) (I discuss the impact of the other treatment groups in more detail below.)

While these studies show that school feeding programs can have significant but modest positive effects on school attendance of pupils who are in school at baseline, they do not capture any increases in enrollment induced by the school feeding programs. Two recent randomized evaluations by Kazianga, De Walque, and Alderman (2008) and Alderman, Gilligan, and Lehrer (2010) do capture increases in enrollment by focusing on children living in the catchment areas of schools rather than children enrolled in school at baseline.\(^\text{17}\) Both of these studies compare the effectiveness of school feeding programs to that of take-home rations (a more recent innovation). The school feeding programs consisted of one or two meals served at school every day. The take-home rations consisted of a similar quantity of food provided to a pupil’s household every month, provided that the pupil attended school regularly.

Although both school feeding programs and take-home rations provide food for education, these programs may not have the same impact on school participation. Before moving on to the results of the Kazianga, De Walque, and Alderman (2008) and Alderman, Gilligan, and Lehrer (2010) studies, I first explain why we may ex-ante expect the impact of these programs to differ. First, take-home rations can be shared with other household members, whereas school meals can only be consumed by the intended beneficiary. The impact of school meals on nutrition status of the beneficiary will thus differ from take-home rations to the extent that school meals result in a reallocation of food in the household away from the beneficiary and to the extent that take-home rations are shared within the household. Second, school meals directly encourage the beneficiary to attend school: by consuming school meals the beneficiary automatically complies with an

\(^{16}\)Point estimates suggest an improvement in the enrollment rate of roughly 2.5 percentage points in both schools that were assigned the school feeding program and in schools that were assigned the school feeding program combined with regular parent teacher meetings.

\(^{17}\)A randomized study by Vermeersch and Kremer (2004) also focuses on children living in the catchment areas of 50 preschools in Kenya. A school feeding program was randomly implemented in 25 of these schools. Vermeersch and Kremer (2004) show that the intervention resulted in an 8.5 percentage point increase in school attendance (control group attendance was 27.4%). However, a problem with this study appears to be that children who attended one of the 50 preschools and subsequently switched to a school outside the study sample were not tracked. If the switching rate is higher in control schools than in treatment schools, the estimated impact will therefore be biased upward.
implicit school attendance condition. Take-home rations, on the other hand, provide no strong incentive for the beneficiary to attend school, especially if the take-home rations are shared with other household members. However, take-home rations do provide an incentive for other household members to encourage the beneficiary to attend school to the extent that their own food consumption depends on it. Each of these issues may induce a differential impact of school feeding programs and take-home rations on school participation.

Kazianga, De Walque, and Alderman (2008) evaluate the effectiveness of school feeding and take-home ration programs in Burkina Faso. The study covers 45 primary schools that were randomized into a school feeding group (15 schools), a take-home rations group (16 schools), and a control group (14 schools). A random group of 48 households was sampled and surveyed at baseline and follow-up around these schools. The authors provide differences in differences estimates of the treatment effects and find that school meals and take-home rations significantly increased enrollment of girls by 4.9 and 5.6 percentage points respectively (the authors do not provide baseline percentages, nor do they state whether the difference between school meals and take-home rations is significant). No significant impact could be found for boys. The authors also show that both interventions increased absenteeism among girls by approximately one additional day per month. The authors argue that the increase in absenteeism could be due to the marginal students induced to enter schools by the programs. These students are likely to attend school less regularly, especially if these children are partly responsible for household chores.

Similar to the above study, Alderman, Gilligan, and Lehrer (2010) evaluate a randomly implemented school feeding and take-home ration program in refugee camps in Uganda. These refugee camps offer primary education in schools inside the camps. Randomization took place at the refugee camp level. A total of 31 camps were randomized into a school feeding group (11 camps), a take-home rations group (10 camps), and a control group (10 camps). Households with school-aged children were sampled from each of the camps to participate in a baseline and follow-up study. Neither of the two interventions had a significant effect on average enrollment, but school meals did significantly increase the enrollment of those pupils who were not in school at baseline by 8.9 percentage points. (An F-test does not reject the null-hypothesis that the impact of the two food for education programs on enrollment of pupils who were not in school at baseline is equivalent.) Moreover, in the full study sample both school meals and take-home rations
had a statistically equivalent and significant effect on school attendance measured through unannounced school visits. The estimated impact of school meals on attendance ranged from 8.5 to 14.6 percentage points and the impact of take-home rations ranged from 9.1 to 14.1 percentage points (self-reported baseline attendance was 86.2%).

**School Health Programs** School health programs are closely related to school feeding programs. These programs deliver health care through the school system and the resulting improved health status of pupils can lead to increased school participation. In this section I discuss two prominent studies on the impact of school health programs on school participation from the empirical economics literature.\(^{18}\)

First, I discuss an influential study by Miguel and Kremer (2004), who show that school-based provision of deworming treatment in Kenya had a substantial impact on school attendance. The intervention studied by Miguel and Kremer (2004) covered 75 primary schools that were randomly phased into the program in groups of 25 schools over three waves. Because the intervention was administered at the school level (rather than the individual level), the authors were able to account for externalities in the form of reduced disease transmission within schools. Moreover, exploiting the density of pupils attending treatment schools living in the neighborhood of pupils attending control schools, the authors also accounted for externalities across schools. The analysis focuses on the attendance of pupils who were listed in school registers at baseline. Given that some of these pupils may in fact have been dropouts at the time of the intervention, the study partly captures the impact of deworming on pupils who were out of school at baseline.

The first wave of the intervention resulted in a substantial reduction in helminth infection rates in treatment schools. The prevalence rate of moderate to heavy helminth infections was down by a significant 25 percentage points in treatment schools (52% in the control group). In addition, there was evidence of treatment externalities, as infection rates were down among pupils in treatment schools who did not receive treatment and among children living near but not attending treatment schools. The intervention also improved school participation. One year after the start of the intervention school attendance was up by approximately 7 percentage points in treatment schools (attendance rates in the control group range from roughly 60 to 80%).\(^{19}\) And the most comprehensive estimate

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\(^{18}\)A concise overview of the medical literature on health interventions and schooling outcomes can be found in Glewwe and Miguel (2008).

\(^{19}\)Actual point estimates of the differences in attendance between treatment and control schools differ by age groups and gender.
provided by the authors (including the direct effect of deworming as well as within-school and across-school externalities) indicates that deworming one child significantly increased school attendance by 0.14 years.²⁰

In a similar vein, Bobonis, Miguel, and Puri-Sharma (2006) show that an intervention providing iron supplementation and deworming drugs to young children in India resulted in a substantial increase in school participation rates. The intervention was randomly implemented in a random sample of 80 schools out of study sample of 200 pre-schools in Delhi.²¹ The study looks at the impact of the intervention on pupils who were in school at baseline and does not account for additional pupils who might enroll as a result of these school health programs. The authors show that the intervention had a mixed impact on health outcomes. Weight-for-height and weight-for-age Z-scores and body-mass index of treatment children improved substantially vis-à-vis the control group, but there were no differences in terms of height-for-age Z-scores, hemoglobin levels, or helminthes infections. School attendance rates in the treatment schools were up by 5.8 percentage points (average absence rates were 30%).

2.3.1.4 Information Provision

Above, I mentioned misguided beliefs about the returns to investments in education as a potential reason for low school participation rates. Limited knowledge of the private payoffs to schooling is of real concern. Based on survey data Jensen (2010), for instance, shows that eighth grade boys in the Dominican Republic severely underestimate private returns to secondary education.²² Conditional cash transfers try to remedy this problem by lowering the opportunity cost of attending school. A more direct approach, however, is to provide households or pupils with information on the returns to education. Two recent

²⁰Below, I discuss a later study by Kremer and Miguel (2007), which indicates that attempts to sustain the deworming intervention in the longer run were ineffective.
²¹Although the study offers clean identification because of its experimental design, sample attrition, which is roughly 25% after one year, is a cause of concern. The authors deal with this issue by showing that attrition rates are equally high in the treatment and control group and do not exhibit any strong correlation with pupils’ baseline characteristics. Moreover, they place bounds on the effect of differential attrition across the treatment and control group.
²²Jensen (2010) argues that in many developing countries it is nearly impossible for pupils to form accurate perceptions of the returns to education. Most schools do not have guidance counselors and earnings data are not collected on a regular basis. Under such circumstances, acquaintances observed by the students are the primary source of information. Typically, many of these acquaintances are not highly educated themselves and thus provide little information from which to infer returns. Moreover, the fact that the available information will be highly location specific further reduces the representativeness of the inferred perceptions.
studies show that such interventions can have a substantial impact on school participation of pupils who were in school at baseline.

First, Jensen (2010) uses a randomized experiment that provides information to pupils on the returns to education to show that indeed to a large extent inaccurate perceptions of returns to secondary education drive low rates of school attendance in the Dominican Republic. The experiment was conducted among randomly sampled pupils in the final grade of 150 primary schools. In half of these schools the sampled pupils were given information on returns to secondary education. The intervention had a strong short term impact on perceived returns to education and resulted in an additional 0.2 years of secondary education over the 4 years after the intervention. However, the impact of the intervention was heterogeneous along household income. While the intervention increased education of pupils from the richest households by 0.33 years it had a negligible effect on pupils from the poorest households. The latter finding suggests that provision of information to correct inaccurate perceptions of returns to education may not suffice to improve enrollment rates under all circumstances. In settings of severe poverty, direct constraints (which could include credit constraints and poor health) continue to play an important role in low school participation rates even when pupils are well informed about the expected returns to education.

Second, Nguyen (2008) employs a similar strategy to measure the impact of inaccurate knowledge of the returns to education on school participation in Madagascar. She extends the approach of Jensen (2010) by comparing the impact of the provision of earnings statistics to the impact of a role model sharing a success story related to their education. The intervention covered 640 primary schools that were randomly assigned to different combinations of the treatment modalities. Nguyen (2008) shows that the provision of earnings statistics effectively reduced the gap between perceived and actual returns to education, raised test scores by 0.2 standard deviations, and improved attendance rates by 3.4 percentage points (control group attendance is 85.6%). The impact of role models on any of these outcome measures was smaller than that of the provision of earnings statistics.

2.3.2 Interventions Targeted at Schools and the School System

The previous sections showed that interventions targeted directly at pupils (or parents) can be effective at improving school participation. Another category of interventions
targets schools and the education system at large to improve school participation. This section discusses the latter category of interventions. I start by discussing multiple studies which indicate that two prominent aspects of school quality (learning materials and teachers) appear to play only a limited role in school participation. I then discuss that, despite the apparently limited role for these aspects of school quality, a more comprehensive measure of school quality examined in Chapter 5 of this dissertation indicates that school quality nonetheless does matter. Next, I review the evidence on three alternative modes of service delivery that could potentially improve school participation: improved teacher incentives, community participation, and school vouchers.

### 2.3.2.1 School Quality

Improvements in the quality of education are an intuitively appealing approach to encourage school participation. When the quality of education is low pupils do not learn much and there is little reason for them to stay in school. And when schools are overcrowded and basic facilities such as sanitation and benches are missing, attending school may become so uncomfortable that students decide to drop out. Under such circumstances, improvements in school quality could potentially result in increased school participation. It is therefore perhaps surprising that the evidence for a relationship between school quality and school participation is mixed.

**Learning Materials** First, there is evidence from randomized interventions that the impact of improved learning materials available in schools is negligible or at best modest. In this sub-section I discuss the evidence from 3 randomized studies, all of which look at the impact of the improved learning materials on pupils who were in school at baseline. The first study is by Banerjee et al. (2007), who look at the impact of a randomly implemented computer assisted learning program in a subsample of a group of 122 primary schools in Vadadora, India. The intervention overlapped with a remedial teaching experiment that I describe in more detail below. For brevity I do not discuss the somewhat involved randomization procedure used to implement these overlapping treatment layers. The authors find that computer assisted learning did not affect dropout rates or attendance of pupils. However, the intervention did result in a substantial improvement in performance on a mathematics test.

A study by Glewwe, Kremer, and Moulin (2009) tests the impact of the provision of
textbooks (primarily mathematics and English) to pupils in a random sample of 25 out of 100 primary schools in two districts in Kenya. The provision of textbooks did not raise average test scores, but it did raise the test scores of the best pupils (i.e., pupils who did well on a test administered prior to the intervention). The provision of textbooks increased the number of pupils entering secondary school by 5 percentage points (38% in the control group), but did not affect enrollment in lower grades of primary school. The authors argue that the improved test scores of good pupils and increased entry into secondary school are related, since in Kenya only academically strong pupils go to secondary school.

Finally, the randomized study by Tan, Lane, and Lassibille (1999), also discussed in the section on food for education programs, tested the impact of 4 types of treatment (school feeding, the provision of pedagogical materials to teachers, and either of these interventions combined with regular parent teacher meetings) on school dropout rates and performance in primary schools in the Philippines. The authors find that the provision of pedagogical materials lowered dropout rates by a significant 4.3 percentage points (the dropout rate was 9.29% prior to the intervention). The provision of pedagogical materials in combination with parent teacher meetings resulted in a similar reduction in dropout rates, although this reduction was not statistically significant.

**Teachers** The evidence on the impact of teachers on school participation is inconclusive. Banerjee et al. (2007) evaluate the impact of a remedial teaching program randomly implemented in 122 primary schools in Vadadora (some of which also received the computer assisted learning program described above) and 74 primary schools in Mumbai, India. The intervention provided schools with (typically young female) remedial teachers who teach basic literacy and numeracy classes to weak third and fourth grade pupils. Similar remedial teaching programs have been implemented in many other primary schools in India. As in the case of the computer assisted learning intervention described above, remedial teaching had no impact on dropout rates or attendance, but did result in substantial improvements in the performance on standardized literacy and numeracy tests. Again, the intervention does not capture any effects on children who were not in school at baseline.

Case and Deaton (1999) look at the relationship between pupil teacher ratios and school participation among black students in South Africa during the apartheid era. The authors argue that the apartheid era can be interpreted as a natural experiment, as in those days black South African nationals had limited control over the quality of the

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schools their children were attending. They show that when the number of pupils per teacher increases by 10, the average years of schooling completed per pupil drops by half a year. However, the authors note that there is a potential endogeneity concern. Decisions regarding class-sizes of schools must have been made at some level in South Africa’s government and it is not impossible that these decisions were correlated with education outcomes. Schools in relatively affluent black neighborhoods attended by relatively advantaged pupils, for instance, may for some reason have been assigned more/less teachers and better/worse resources than other schools. In that case the results of Case and Deaton (1999) will be biased and can no longer be interpreted as causal.

Selective Secondary Education The school quality studies discussed thus far investigate the impact of only a single aspect of the school environment, such as learning materials or teachers, on school participation. These studies may well miss other crucial aspects of school quality. Chapter 5 of this dissertation identifies the effect of a more comprehensive measure of school quality on school participation. To identify this effect, I exploit a merit-based procedure on the basis of which pupils are selected into public secondary schools in Malawi. This procedure assigns the top performers on a national primary school exam to a group of elite public schools of comparatively high quality (i.e. with more trained teachers and better facilities). Second tier performers are selected into the remaining lower quality public schools. Third tier performers, approximately 60% of the pupils who annually complete primary school, are not assigned to public schools.

Many other Sub-Saharan countries apply such tracking policies in their secondary education systems. Similar tracking policies can also be encountered in the tertiary education sectors of many western countries, where students compete to enter selective colleges and universities. There is by now a considerable literature that looks at the payoff of attending such selective colleges and universities (see for instance Behrman, Rosenzweig, and Taubman 1996; Dale and Krueger, 2002; Hoekstra, 2009; Lindahl and Regnér, 2005). However, despite the fact that selection into selective secondary schools is common in Sub-Saharan Africa, little is known about the impact of selection into these relatively high quality schools on school outcomes.

In Chapter 5 of this dissertation I aim to fill this gap. I exploit the merit-based selection procedure in a regression discontinuity framework to identify the causal effect of selection

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23In the study of Case and Deaton, 1999, the pupil teacher ratio could be a proxy for a broader measure of school quality and thus capture the broader effect of school quality on school participation.
into an elite public secondary school on school participation (i.e. the local effect for those pupils who complete the primary school exam with scores close to threshold score for selection into an elite public school). The estimates suggest that selection into a selective secondary school, which places pupils in a learning environment with superior resources and peers, results in substantial increases in school participation. Two years after the selection takes place selected boys and girls are roughly 30 percentage points more likely to participate in the mandatory secondary school exam in the school into which they had been selected (at the cutoff point participation among pupils not selected is approximately 40%). I estimate that about two thirds of this increase in exam participation is due to a lower dropout rate in selective secondary schools, indicating that school quality matters for school participation.

The evidence I provide on selective education in Malawi contrasts with other recent regression discontinuity evidence. A study by Pop-Eleches and Urquiola (2010) shows that selective schools in Romania have no impact on exam participation. And Jackson (2010) shows that in Trinidad and Tobago selection into a selective school increases exam taking by only 1.5 to 3 percentage points (exam participation rates range from 65 to 90%, an average point estimate for pupils not selected into selective schools is not given). In Chapter 5 I argue that this difference is likely due to the different context of the studies. Trinidad and Tobago and Romania have secondary enrollment rates in the range of 75% (these rates are higher around the cutoff for selection into a selective secondary school), whereas Malawi has a net secondary enrollment rate of 25%.

2.3.2.2 Alternative Modes of Service Delivery

Direct investments in individual schools are not always the only or most efficient approach to improve school quality. Alternative modes of service delivery can be another cost-effective way to improve schooling outcomes. Devarajan and Reinikka (2004), for instance, mention community participation and the contracting out of service provision to private parties or NGOs as examples of alternative modes of delivery. In this section I discuss the evidence on the impact of alternative modes of service delivery on school participation.

Teacher Incentives As I discuss in more detail below, teachers in developing countries often face weak incentives to be present in school and to exert effort while in class. Glewwe, Ilias, and Kremer (2009) evaluate the impact of an intervention that attempted
to strengthen teacher incentives in primary schools in Kenya. The intervention was implemented in 50 schools that were randomly selected from a study sample of 100 schools. The intervention rewarded teachers in the treatment schools for the schooling outcomes of grade 4 to grade 8 pupils. Specifically, teachers were given prizes ranging in value from 21 to 43% of monthly teacher salaries for pupil participation in a government exam and for pupil performance on this exam. These rewards were based on school-level performance rather than teacher-level performance. School-level performance on the one hand increases opportunities for collaboration and social control among teachers, but on the other hand creates opportunities for free-riding. The intervention does not capture effects on pupils who were not in school at baseline.

Glewwe, Ilias, and Kremer (2009) show that the impact of the intervention on teacher performance was limited. Teacher absence did not decrease, pedagogical practices were not affected, but there was a substantial increase in exam preparation sessions. The authors show that the impact of the intervention on learning outcomes was also minimal. Participation in and performance on the government exam improved, but dropout rates and performance on another independent test were equivalent in the treatment and the control group. Although these disappointing results may be due to the setup of the intervention (i.e. limited duration, modest bonuses), the authors argue that the results suggest that it would be worthwhile to consider evaluating the impact of more fundamental reforms, such as community participation. As I now continue to discuss, Banerjee et al. (2010) provide an example of an intervention that attempts to gauge the impact of community participation.

**Community Participation** Banerjee et al (2010) evaluate an intervention that encouraged citizen involvement in elementary education in India. India already promotes citizen involvement through village education committees consisting of parents, the head teacher of the village school, and the head of the village government. Depending on the district, it is the responsibility of these committees to monitor the public school in the village. In practice, however, these committees do not take a very active role and committee members are often not aware of their responsibilities or the means available to the committee.

Banerjee et al. (2010) evaluate the impact of 3 interventions that encourage citizen involvement each implemented in a random subsample of 65 out of 280 villages in Juanpur district. The first intervention provided citizens with information on the roles and
responsibilities of the village education committee. The second intervention also provided this information and in addition trained volunteers to monitor the performance of the local school and to make the findings of their monitoring activities publicly available. The third intervention was equivalent to the second intervention, but in addition trained volunteers to hold reading camps in order to directly improve learning outcomes in their village. Households were randomized into the study sample at the village level, which allows the researchers to measure the impact of the intervention on children who were not in school at baseline.

The impact of the interventions is limited. There is evidence that the interventions improved citizens knowledge about the role of the village education committee and the state of education in their village. However, villagers did not act upon this information. The performance of village education committees did not improve and parental involvement in the schools was not affected. Accordingly, none of the three interventions affected enrollment or attendance, but the reading camp intervention did have modest impacts on reading ability of children.

School Vouchers School vouchers, yet another mode of delivery, have been evaluated particularly well. Vouchers form an alternative to direct funding of schools that potentially increases competition between private and public schools, (typically vouchers can be used at both private and public schools, thus lowering the relative cost of private education). Here, I discuss the evidence on the impact of a large-scale school voucher program in Colombia on school participation. The program was implemented in 1991 and provided vouchers that cover roughly half of the cost of attending private secondary schools in Colombia. Only pupils living in neighborhoods classified into the lowest socioeconomic strata could apply for vouchers and due to resource constraints even within this group not all applications could be granted. The vouchers were therefore distributed to pupils by lottery, a feature that two studies I now proceed to discuss exploit to identify the effect of vouchers on schooling outcomes.

Angrist et al. (2002) examine the impact of the program on secondary on school outcomes. Because only approximately 90% of lottery winners eventually used the voucher, the authors use lottery win/loss status as an instrument for scholarship use. Angrist et al. (2002) show that the short term benefits of the vouchers were improved performance on education tests and lower grade repetition rates (there was no evidence of any improvements in school attendance). The authors argue that three channels can account for
these improvements. First, the program induced some participants to shift from public to private schools. Second, the program induced some participants who planned to attend private school to shift to a more expensive private school. Third, vouchers increased the incentives for pupils to focus on school, because the vouchers could only be extended if pupils did not repeat a grade. Angrist, Bettinger, and Kremer (2006) show that, in the longer run, vouchers increased high school graduation by about 6 percentage points among a sample of 3542 pupils who entered the lottery in 1994 (mean graduation rate was 35%), a finding which I interpret as an indication of improved school participation.24

### 2.4 Scaling Up

The previous section discussed the effectiveness of interventions that aim to improve school participation. Some of these interventions were small scale randomized controlled trials. While it is important to have knowledge of the effectiveness of these interventions in small scale settings, their effectiveness in small scale settings in itself does not always suffice to guide government policy in developing countries. One reason is that, due to institutional constraints, many interventions that have been shown to be effective on a small scale may not be effective when they are implemented on a larger scale. Local capture is a prime example of such institutional constraints. Reinikka and Svensson (2004) studied the impact of local capture on the allocation of funds in public schools in Uganda. They find that, as a result of local capture, only a small fraction of funds disbursed through a capitation grant reached the schools for which it was intended. This section starts by discussing their results.

Then, this section proceeds by discussing that large scale interventions may not only be ineffective but may also result in potentially detrimental general equilibrium effects that are hard to detect in small scale studies (Duflo, Glennerster, and Kremer, 2008). One example of a general equilibrium effect has already been mentioned earlier: the abolition of school fees in Malawi and Kenya and the introduction of school meals in Kenya resulted in increased pressure on the schooling system. The available physical and human resources in the education sector had to be spread over an increasing number of pupils and as a result school quality deteriorated. This section discusses social stratification, another

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24Technically, it may be the case that pupils who do and pupils who do not receive a voucher spend an equal number of years in school. This could happen when increased high school graduation rates purely reflect improved learning of pupils and a resulting lower probability of repeating grades.
Section 2.4: Scaling Up

type of general equilibrium effect that is less predictable. The evidence for these general equilibrium effects comes from large scale school fee reductions and voucher programs discussed above.

2.4.1 Inefficiency and Corruption

Reinikka and Svensson (2004) illustrate that local capture can strongly reduce the effectiveness of public education programs using survey data from Uganda covering 250 sampled primary schools from 1991 to 1995. In their paper the authors assess the extent to which a capitation grant, a large scale public program to cover non-wage expenditures of schools, actually reached the schools for which it was intended. The results are ominous. On average, the sampled schools received only 13% of the funds disbursed by the government through this program. Local capture was stronger in schools with students from poorer socioeconomic backgrounds. Reinikka and Svensson (2004) assert that these results are not unique for Uganda and that other Sub-Saharan countries face similar problems.

In a follow-up study, Reinikka and Svensson (2005) show that publication of data on disbursed funds in Uganda’s national newspapers contributed to strong reductions in local capture in the years after 1995. The authors identify this effect using the distance to a newspaper outlet as an instrument for information available to head teachers about the funds disbursed by the government. Information available to head teachers was measured through a survey that asked teachers about their knowledge of the funds from the capitation grant that should be available to their school. The outcomes suggest that schools that were more strongly exposed to the newspaper campaign experienced stronger reductions in local capture. By 2001 the share of disbursed funds received by schools had increased from 13% to 82%.

Reinikka and Svensson (2005) also show that, while overall primary school enrollment in Uganda increased rapidly over this same period as a result of the introduction of free primary education, the increases in school enrollment were stronger in schools that were more strongly exposed to the information campaign.\textsuperscript{25} Reinikka and Svensson (2005) argue that this result is probably not caused by sorting into schools that receive more funds, as primary school pupils in Uganda have little choice with respect to the type of

\textsuperscript{25}Moreover, while being closer to a newspaper outlet resulted in significant changes in enrollment in the period after the start of the newspaper campaign, it was not associated with significant changes in enrollment in the period before the start of the campaign.
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Based on these results, the authors argue that reductions in local capture may improve school participation by ensuring that government funds actually reach the intended beneficiaries. While the authors do not explicitly discuss how increased funds result in increased enrollment, their findings suggest that improved school quality is the prime channel.

2.4.2 General Equilibrium Effects: Social Stratification

As mentioned above, large scale interventions may not only be ineffective due to institutional constraints, such as local capture, but may also result in unexpected general equilibrium effects. Here, I discuss social stratification, the clustering of pupils in different schools according to their socioeconomic background. One example of social stratification is given by Hsieh and Urquiola (2006), who investigate the impact of a voucher program in Chile. The vouchers were introduced nationwide in 1981 as part of a large program of market oriented reforms. Because a nationwide reform does not provide a clear counterfactual, the authors rely on differences between communities (in terms of market size and demand for private education) to identify the impact of the reform. The idea is that communities with larger education markets (typically urban communities) will experience stronger increases in competition between schools as a result of the voucher program than smaller communities. The identifying assumption is that these community characteristics are fixed over time, in which case changes between communities can be compared to measure the impact of the voucher program. Contrary to the evidence presented by Angrist et al. (2002) and Angrist, Bettinger, and Kremer (2006) for Columbia’s voucher program, Hsieh and Urquiola (2006) find no impact of the vouchers on pupil performance using this approach. However, the authors do find that Chile’s voucher program inadvertently resulted in social stratification, as middle-class students appear to have left the public school system. As a first piece of evidence, the authors show that, after the implementation of the voucher program, the relative educational attainment of parents of children in public schools was lower in districts with higher private

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26 Among others, Epple and Ferreyra (2008) argue that such general equilibrium effects of schooling interventions can be visible even outside schools. Schooling reform can, for instance, also affect housing prices and even the demographic composition of neighborhoods. The authors show that evidence of a school finance reform in Michigan is broadly in accordance with this prediction.

27 Nor do rankings of Chile vis-à-vis other countries on the Trends in International Mathematics and Science Study (a.k.a. TIMSS) suggest that pupil learning in Chilean schools improved after the introduction of the voucher program.
school enrollment. The authors also show that this effect was stronger in districts that experienced a stronger increase in private school enrollment.

Similarly, Lucas and Mbiti (2009) show that there is suggestive evidence that the abolition of school fees led to social stratification in Kenya’s primary schools. First, the free primary education program resulted in strong increases in school participation, especially among children from parents with lower educational attainment. These pupils primarily entered the public school system and thus widened the existing gap between socioeconomic backgrounds of pupils in public and primary schools. Second, there is evidence that demand for private education increased in districts that were more strongly affected by free primary education, suggesting that the intervention resulted in “rich flight” from the public schools.

2.5 What Results to Expect?

Increased school participation can hardly be considered to be a goal in itself. Rather, the goal is the benefit of school participation to pupils and society at large. So, if interventions that aim to increase school participation can be scaled up without diminished effectiveness due to inefficiency or corruption and without the presence of unexpected adverse general equilibrium effects, what benefits can we expect? To answer this question, a natural starting point for economists is to look at the impact of increased school participation on economic development. This section starts by discussing the available evidence on this relationship.

Economic development, however, is a rather narrow focus, as school participation can affect a broad range of outcomes beyond economic development. After discussing the impact of school participation economic development, this section therefore proceeds to discuss the available evidence on 2 of these other outcomes. I first discuss the impact of education on health with a particular focus on HIV/AIDS. I then discuss that, more generally speaking, interventions that improve school participation can have broad empowering effects on girls, helping them to invest in their future instead of spending their time working for short term benefits for the household.
2.5.1 Skill Accumulation and Economic Benefits

There is now a large literature that documents the relationship between school participation and economic development.\(^{28}\) Hanushek and Woessmann (2008) review this literature in detail and conclude that cognitive skill accumulation is the issue. The authors argue that school participation can have a strong impact on economic growth at large as well as individual earnings, but only when it results in increased cognitive skills.\(^{29}\) When school participation does not lead to increased cognitive skills no such impact on economic development should be expected.

Hanushek and Woessmann (2008) also explain that school participation itself does not necessarily improve cognitive skills. Especially in developing countries, where the quality of education can be poor, a strong relationship between school participation and increased cognitive skills is far from guaranteed. Hanushek and Woessmann (2008) illustrate this point using World Bank data on educational attainment and data from international achievement tests. Combining these data provides a clear view of how low school participation and low levels of cognitive skill accumulation in developing country schools form a double challenge.

First, these data show that there are vast differences in terms of schooling attainment. Almost all OECD member states have universal school attainment to grade 9. In Africa, the other extreme, only 13% of pupils finishes grade 9. But on top of these differences in school participation, there are also strong differences in pupil learning. The international student achievement tests indicate that roughly 80 to 95% of students from developed countries achieve basic skills in mathematics and science by grade 9. In developing countries this number is often below 50%.\(^{30}\) Low schooling attainment and low pupil learning together result in very low levels of skills accumulation in developing countries. Ghana serves as a good example. In this country 62% of pupils either never enters school or drops out before grade 9, 32% of pupils finishes grade 9 without reaching basic skills.

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\(^{29}\) Skill accumulation is a necessary but not a sufficient condition for economic development. The institutional context plays an important role in this relationship and without the right economic institutions the accumulation of cognitive skills may not result in increased economic development.

\(^{30}\) Since many developing countries do not participate in these tests, it is not possible to compare different regions or continents. However, given that the countries that participate in these tests are a self-selected sample, the average difference between the developing World and the developed World may well be more extreme.
and only 5% of pupils finishes grade 9 with basic skills. Developing countries thus face a double challenge as their pupils tend to drop out prematurely and a large percentage of pupils that stays in school does not attain even basic skills.

Kremer and Holla (2009) mention a variety of reasons that can explain the observed learning differential between developing countries and developed countries. These reasons can broadly be grouped into two categories. First, expenditure per pupil as a percentage of GDP (and thus also absolute expenditure) is much lower in developing countries than in developed countries. As a result of these lower expenditures the education sectors in many developing countries face a chronic lack of physical and human resources. Malawi, the focus of this dissertation, is a prime example. Chapter 5 shows that the majority of public secondary schools in Malawi have only 1 trained teacher per 125 pupils. Only 4% of these schools has toilets for pupils and only 22% of these schools has access to electricity.

Second, there are often distortions in the education systems of developing countries. Teacher incentives, for example, are often weak and as a result teacher absence rates are high. Chaudhury et al. (2006) report the results of a survey that tested for teacher presence during unannounced school visits in Bangladesh, Ecuador, India, Indonesia, Peru, and Uganda. Averaging across these countries, the authors find that 19% of teachers were absent during the unannounced visits. Moreover, in developing countries curricula are often tailored to the strongest students or to the needs of the elite. As a result, pupils from disadvantaged backgrounds may have difficulty following this curriculum and thus learn little.

Recognizing that making sure that pupils learn while they are in school is a key challenge, Kremer and Holla (2009) review the available evidence from randomized evaluations of interventions that aim to improve pupil learning. An extensive discussion of this evidence is beyond the scope of this chapter. However, it is of interest to note here that reductions of pupil teacher ratios or investments in non-teacher inputs appear to be relatively ineffective. Interventions that address distortions in education systems seem to be more effective and lead to much stronger increases in pupil learning (as measured by test scores).

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31 Where trained is defined as having any degree beyond secondary school.
32 The exact absence rates were 27% in Uganda, 25% in India, 19% in Indonesia, 14% in Ecuador, and 11% in Peru.
2.5.2 Other Benefits

While economists have primarily focused on the impact of education on economic development, there is also rigorous evidence on a series of other outcomes. In this section I discuss the impact of (projects that aim to increase) school participation on 3 of these outcomes: HIV/AIDS, female empowerment, and child labor. Contrary to the impact of school participation on economic development, there is evidence that the impact of interventions that aim to increase school participation on these outcomes may run not only through learning in school. Instead, it can also run through school participation, or through other elements of the interventions such as increased income (e.g. in the case of cash transfers) or improved nutrition (e.g. in the case of school feeding programs).

2.5.2.1 Health

Grossman (2006) provides an extensive review of the literature on the effect of education on non-market outcomes. A large part of this review focuses on the relationship between education and health, because, as Grossman (2006) argues, health and education are the two most important components of human capital. Most of the literature on this relationship discussed in Grossman (2006) is based on data from developed countries. This literature shows that years of formal schooling attained is the most important correlate of good health, more important than income and occupation. Moreover, a recent number of quasi experimental (instrumental variable) studies discussed in Grossman (2006) and more recently in Eide and Showalter (forthcoming) indicate that the effect of education on health is causal and potentially even stronger than the plain correlations seem to suggest.

The literature on the relationship between education and health in developing countries is more limited. However, there is a relatively large strand of recent literature that investigates the relationship between education and HIV/AIDS, one of the primary health risks in Sub-Saharan Africa. Most of this literature focuses on interventions that aim to increase school participation in Sub-Saharan Africa and I continue this section by discussing that literature.

I start by discussing the results of Dupas (2005) and Duflo et al. (2006), who evaluate the impact of an education intervention with multiple types of treatment on HIV/AIDS. The study sample consists of 70,000 pupils in 328 schools. In a random sample of 163 of these schools teachers were trained in Kenya’s HIV/AIDS curriculum. In a random sample of 163 schools the cost of education was reduced by distributing free school uniforms among
Section 2.5: What Results to Expect?

In a random sample of 71 schools pupils were given HIV prevalence information by age and gender. And finally, in a random sample of 182 schools pupils were encouraged to participate in debates and an essay contest about the use of condoms.

The authors look at the impact of each of these types of treatment on teenage childbearing (which is associated with HIV/AIDS prevalence), and knowledge, attitudes, and behavior regarding HIV/AIDS. The results varied substantially by treatment type. First, the authors find that the teacher training resulted in increased emphasis on HIV/AIDS in the school curriculum. Teachers in treatment schools, for instance, were 14 percentage points more likely to teach about HIV/AIDS (73% in control group). However, the authors find no evidence that teacher training affected any of the mentioned outcomes.

The other treatment types appear to have had a stronger impact on teenage pregnancy, knowledge, attitudes, and behavior. Reducing the cost of education through the distribution of school uniforms resulted in substantial changes in sexual behavior. Girls in schools in the treatment group were 2.3 percentage points less likely to indicate that they ever had sex (18% in control group) and 1.5 percentage points less likely to have started childbearing (15% in control group). Similarly, information about HIV prevalence by age and gender reduced childbearing by 1.7 percentage points (5.4% in control group). The debates and essay contests, on the other hand, appear to have had a stronger effect on HIV/AIDS knowledge. Girls in these treatment schools, for instance, were 7.9 percentage points more likely to mention condoms as a means to protect from HIV infection (32% in control group).

The cash transfer intervention in Malawi also had a substantial effect on pregnancy and risky sexual behavior (Baird, McIntosh, and Özler, 2010). The results were heterogeneous by baseline schooling status though. Girls who were not in school at baseline were 5.5 percentage points less likely to ever have been sexually active at follow-up when they had been offered a conditional cash transfer (42.0% in control group). No significant effects could be observed among girls who were in school at baseline.

Moreover, there is now evidence that the cash transfer intervention had a substantial impact on HIV prevalence rates themselves. A year and a half after the start of the intervention, the HIV prevalence was 1.2% among girls who had been offered a cash

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33 See also the section on cost reductions above.
34 The differences in control group childbearing rates between treatment groups are due to different years and samples under investigation in Dupas (2005) and Duflo et al. (2006).
35 The effect of the cash transfer intervention should be interpreted as the combined impact of school attendance and the income shock provided through the cash transfer program.
transfer and 3% among control girls. The HIV prevalence was thus a significant 60% lower in the treatment group. (The latter result has not yet been published, but was recently presented by Berk Özler at the 2010 annual meeting of the International AIDS Society.)

2.5.2.2 Female Empowerment and Child Labor

School participation can obviously have wide-ranging effects on people’s lives beyond economic development and health. The empowering effect of education on many aspects of the lives of women in developing countries is of particular relevance to this dissertation. A first indication for the potential empowering effects of education comes from the literature on female empowerment in theoretical economics. Chiappori, Iyigun, and Weiss (2009), for example, investigate the impact of increased education in a model of frictionless matching on the marriage market. Their model shows that increased female education not only increases women’s direct earning capacity, but also their ability to extract their share of the intra-marital surplus. And Iyigun and Walsh (2007) present an intra-household bargaining model that investigates the impact of female education on household decisions. Their model suggests that, by raising income, female education positively affects female bargaining power in the household. Higher female income, moreover, results in increased opportunity costs of rearing children and thus in a reduction in fertility.

However, the empirical evidence on the relationship between education and empowerment within marital relationships is scant. The available empirical studies typically focus on the empowering effects of interventions targeted at women who are already married and have thus completed most of their schooling. Chapter 3 of this dissertation

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36There is also non-experimental evidence on the relationship between education and HIV/AIDS in Sub-Saharan Africa. De Walque (2009) uses data from demographic and health surveys from 5 countries to investigate the socioeconomic determinants of HIV/AIDS infection. He finds that education is associated with both beneficial and detrimental effects on HIV/AIDS related knowledge and behavior. On the one hand, education predicts condom use, counseling and testing, and knowledge about HIV/AIDS. On the other hand, education predicts higher levels of infidelity and lower levels of abstinence.

De Walque (2007) goes one step further and uses a panel dataset that follows the population of a group of Ugandan villages over a period of 12 years, starting in 1989. De Walque shows that in 1989, among young individuals (aged 18-29), there was no relationship between education and HIV/AIDS. However, 11 years later, when the government of Uganda had rolled out a strong HIV/AIDS campaign in primary and secondary schools, there was a strong negative relationship between education and HIV/AIDS among young women. De Walque (2007) shows that education also predicts condom use of both men and women, is negatively related to the number of sexual partners of women, and positively related to the number of sexual partners of men.

37There are obvious endogeneity concerns that complicate the identification of the causal effect of education on empowerment.
contributes to the empirical literature by showing that the cash transfer intervention in Malawi had important contemporaneous effects beyond increased school participation that may well empower younger unmarried women in the long run. The intervention, for instance, affected social patterns such as early marriage, and pregnancy that hamper the development of school aged girls.

Moreover, in accordance with the strong increase in school participation, the girls who were not in school before the start of the intervention indicated that the intervention had a considerable influence on their daily time use. At follow-up they were 17 percentage points less likely to indicate that household chores were their primary daily activity (52% in control group) and 12 percentage points less likely that labor was their primary daily activity (30% in control group). These results suggest that the intervention helped reduce child labor by encouraging them to spend more time investing in their future.

Kazianga, De Walque, and Alderman (2008) show that the take-home rations distributed in Burkina Faso (see also above) had a similar impact on child labor among girls. In this gender group, treatment lowered both participation in farm labor and non-farm labor by 9 percentage points. The impact of school meals and take-home rations on household chores, on the other hand, is mixed. Girls receiving school meals and take-home rations, for instance, were respectively 8 and 10 percentage points more likely to participate in fetching water. But girls receiving school meals were 8 percentage points less likely to tend for their younger siblings (control group percentages not provided).  

2.6 Cost Effectiveness and Conclusion

This chapter discussed the available evidence on the effectiveness of a wide range of interventions that aim to increase school participation. The presented evidence shows that there is a variety of ways in which school participation can be promoted. Given this large number of potentially effective interventions and the fact that available funds are limited, policy makers who wish to increase school participation will be interested in the

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38 This result is at odds with earlier evidence on the impact of a food for education program in Bangladesh on child labor in Ravallion and Wodon (2000). This food for education project was not randomly implemented, but used a combination of geographic targeting and individual targeting within selected geographical areas. Relying on a series of controls to correct for endogeneity due to geographic targeting and using geographic placement as an instrument to correct for endogeneity due to individual targeting, the authors show that (at the average stipend size) the intervention increased school participation of girls by 18 percentage points (73% among non-participants in the food for education program). However, it reduced child labor among this group by only 2 percentage points (12% among non-participants in the food for education program).
comparative cost effectiveness of the different interventions.

Table 2.3 below gives an indication of the cost-effectiveness of some of the different interventions discussed in this chapter. Part of these estimates comes from the Poverty Action Lab (J-PAL) at MIT. The remaining cost estimates are based on my own calculations. In my calculations, I attempted to stay as close as possible to the methodology for the calculation of cost effectiveness used by J-PAL (see J-PAL, 2010). My estimates, however, are sometimes based on cruder assumptions described in the appendix and accordingly should be interpreted with care.

The estimates in Table 2.3 show that there is wide variation in the cost effectiveness of the different interventions. The provision of information on the payoffs from staying in school in Madagascar (Nguyen, 2008) and the deworming campaign in Kenya (Miguel and Kremer, 2004) appear to be the two most cost-effective interventions. Their cost per additional year of schooling is an order of magnitude smaller than that for the third most cost effective intervention, the provision of iron and deworming medication in India.

The abolition of school fees in Malawi also appears to have been a relatively cost effective intervention. The estimates of Al-Sammarai and Zaman (2007) suggest that 5 years after the introduction of free primary education, the intervention had raised primary enrollment by approximately 50% at a cost of about 32 US$ per additional year of education. The estimates of Lucas and Mbiti (2010) suggest that, at approximately 110 US$ per additional year of education, the cost-effectiveness of free primary education in Kenya was lower. However, as the latter estimate only represents the cost per additional year of schooling shortly after the start of the intervention, the comparison is likely to be skewed in favor of the Malawi study.

Interventions that provide direct incentives for students to attend school (including school meals, take home rations, and cash transfers) are more expensive. Their costs per added year of schooling range from nearly 82.50 US$ for school meals in Burkina Faso (Kazianga, de Walque, and Alderman, 2004) to 245 US$ for take-home rations in Uganda (Alderman, Gilligan, and Lehrer, 2010). The cost-effectiveness estimate for the cash transfer intervention in Malawi (183 US$) is for girls who were not in school at baseline (i.e. the most optimistic cost effectiveness estimate).

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Table 2.3: Cost-effectiveness estimates of interventions that aim to increase school participation

<table>
<thead>
<tr>
<th>Type of intervention</th>
<th>Cost per additional year of schooling per student (2009 US$)</th>
<th>Study</th>
<th>Country</th>
<th>Cost-effectiveness calculations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Information on returns to education</td>
<td>2.50</td>
<td>Nguyen (2008)</td>
<td>Madagascar</td>
<td>J-PAL</td>
</tr>
<tr>
<td>Iron and deworming</td>
<td>25.00</td>
<td>Bobonis et al. (2006)</td>
<td>India</td>
<td>J-PAL</td>
</tr>
<tr>
<td>Abolition of school fees</td>
<td>32.00</td>
<td>Al-Samarrai and Zaman (2007)</td>
<td>Malawi</td>
<td>Own calculations</td>
</tr>
<tr>
<td>Merit scholarships</td>
<td>71.50</td>
<td>Kremer et al. (2009)</td>
<td>Kenya</td>
<td>J-PAL</td>
</tr>
<tr>
<td>Take home rations</td>
<td>82.50</td>
<td>Kazianga et al. (2009)</td>
<td>Burkina Faso</td>
<td>Own calculations</td>
</tr>
<tr>
<td>School uniforms</td>
<td>100.00</td>
<td>Evans et al. (2009)</td>
<td>Kenya</td>
<td>J-PAL</td>
</tr>
<tr>
<td>Cash transfers</td>
<td>183.00</td>
<td>Baird et al. (2009)</td>
<td>Malawi</td>
<td>Own calculations</td>
</tr>
<tr>
<td>Take home rations</td>
<td>245.00</td>
<td>Alderman et al. (2010)</td>
<td>Uganda</td>
<td>Own calculations</td>
</tr>
</tbody>
</table>

Although useful, the cost-effectiveness calculations should be interpreted with care as results may not generalize across geographic locations or demographic subgroups. For instance, as the J-PAL estimates indicate, while the deworming program in Kenya added years of schooling at a cost of only 3.50 US$, the discussed provision of iron and deworming medication in India added years of schooling at roughly 7 times that price. Clearly, cost-effectiveness depends on the local situation. In areas where helminthes infections are highly prevalent, for example, deworming drugs will have a larger effect on school participation than in areas where prevalence rates are lower. All of these cost effectiveness estimates should therefore be treated as only very rough indications.

It should also be noted that many of the interventions have additional benefits that are not accounted for in the cost effectiveness estimates. The cash transfer intervention in Malawi for instance, reduced the incidence of HIV/AIDS, and resulted in a wide range of other gains, such as improved mental health and female empowerment described in the next chapters of this dissertation. Such gains are not reflected in the cost effectiveness estimates, but should be taken into account when policy makers wish to pursue a broader range of policy goals. It is also essential to note that the cost-effectiveness estimates capture only the essentially intermediate outcome of school participation and not the ultimate outcome of pupil learning.

Finally, it is important to mention that the opportunities for sustainable interventions (which require only startup funding and from then on rely purely on the support of the intervention’s beneficiaries) appear to be limited in the most impoverished countries. The results on the relationship between school fees and school participation show that even the introduction of minor fees leads to strong reductions in school participation. Evidence presented by Holla and Kremer (2009), moreover, suggests that the same holds for health interventions that could be instrumental in increasing school participation.

Kremer and Miguel (2007) provide a clear example of the limited opportunities for interventions that recover their costs through user fees. The authors describe how the NGO that implemented the Kenyan deworming program described earlier in this chapter attempted to move from fully subsidized deworming treatment, to recovery of part of the program costs through the introduction of user fees. The introduction of user fees was implemented randomly in a subsample of the schools targeted by the deworming project and had a dramatic impact on the uptake of helminthes medication. In schools subject to user fees the use of the medication was 80% lower than in schools where the
medication was provided for free. Encouraging school participation through any of the described interventions will, therefore, require continuing investments by governments for many years.

All in all, the evidence suggests that each of the described interventions can effectively increase school participation. However, there is great variation in the cost-effectiveness of the different interventions. It would be wise for policy makers who wish to improve school participation to carefully examine which local constraints keep pupils out of school. Such analysis will help in choosing the intervention that will most effectively and efficiently improve school participation. Future research providing more information on the heterogeneity of the effects of the different interventions along socioeconomic dimensions would be useful in this respect, as it could be used to target those pupils who will benefit most from the interventions.

A1. Cost-Effectiveness Calculations

This appendix describes the calculations for my cost-effectiveness estimates presented in Table 2.3.

Cost-Effectiveness Estimate for Abolition of School Fees in Malawi: Average monthly primary school fees were approximately 1 US$ in 1994 prices. In 1994 there were 1.9 million students in school. The lost annual school fee revenue thus amounts to 22.8 million US$ in 1994 prices. The intervention resulted in an enrollment increase of 1 million students after 5 years and I assume this increase is permanent and was caused by the intervention. The intervention thus cost 22.8 US$ per pupil in 1994 prices, which is 32.03 US$ in 2009 prices (using average GDP deflators from the World Bank Data Finder). This estimate does not include additional expenditures by the government of Malawi and donors to reduce deteriorations in school quality due to the increase in enrollment to a minimum and thus underestimates the full cost of expanding access to primary education in Malawi.

Cost-Effectiveness Estimate for Take-Home Rations in Burkina Faso: Kazianga, De Walque, and Alderman (2008) provide no estimates for the cost of the intervention. However, Gelli et al. (2009) estimate that the average cost of a combined school feeding and take home ration program administered by the World Food Program in Burkina
Faso was 7.7 US$ per pupil per year (base year not given). I assume that administering just take home rations would cut the cost of this program by 40%. The cost of the take home ration per pupil would then be 4.62 US$. Enrollment increased by 5.6 percentage points due to the take home rations, which implies that the cost of one additional year of schooling would be 82.50. Given that Gelli et al. (2009) provide no base year this cost estimate cannot be corrected for inflation and is therefore an underestimate.

Cost-Effectiveness Estimate for Cash Transfers in Malawi: At the minimum transfer amount of 5 US$ in 2008 prices the school participation rate of pupils who were not in school at baseline increased by 40.4 percentage points. I currently have no data on the program costs and I assume that the average cost to administer the transfers was an additional 1 US$ per girl per month in 2008 prices. There is no evidence of positive spillover effects of the intervention on other pupils. The intervention thus cost 180 US$ per additional school year in 2008 prices, which is 183.06 US$ in 2009 prices (using average GDP deflators from the World Bank Data Finder). This is the most optimistic estimate, as the cost per additional year of schooling is higher for girls who were in school at baseline and for girls who received higher transfer amounts.

Cost-Effectiveness Estimate for Take-Home Rations in Uganda: Alderman, Gilligan, and Lehrer (2010) provide no estimates for the cost of the intervention, but Gelli et al. (2009) estimate that the average cost of a combined school feeding and take home ration program administered by the World Food Program in Uganda was 36.77 US$ (base year not given). I assume that administering just take home rations would cut the cost of the program by 40%. The cost of the take home ration per pupil would then be 22.06 US$. Enrollment increased by 9 percentage points due to the take home rations, which implies that the cost of one additional year of schooling would be 245.13 US$. Given that Gelli et al. (2009) provide no base year this cost estimate cannot be corrected for inflation and is therefore an underestimate.