Schooling for All

Feasible Strategies to Achieve Universal Education

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Preface

Even before the COVID-19 pandemic, the world was nowhere close to being on track to meeting the ambitious targets of universal quality primary and secondary education for all. The pandemic has left the poorest children even further behind, and governments with even tighter fiscal constraints to reach the ambitious targets laid out in the United Nations Sustainable Development Goals.

Schooling For All: Feasible Strategies to Achieve Universal Education sets out a policy agenda for countries to address these challenges. At its core, it is an outline of how governments can rapidly provide the opportunities for all children to develop and learn. It is both an argument in favor of bigger fiscal investments in basic education and also a guide to prioritization bound by some sense of realism.

Personally, I take three main messages away from the volume.

First, despite ongoing debates about the effectiveness of education spending, there are several high-return education investments that ministries of education and finance can make, today. These begin with policies to expand access to secondary schooling, which remains quite limited in many low- and even middle-income countries, or extensions of the school day to provide more contact hours. Other examples include national school feeding programs that provide a midday meal for all children, starting with the most vulnerable. On the other hand, further increases in teachers' salaries relative to other professionals or reducing class sizes beyond a moderately large threshold do not appear to generate comparable returns for scarce education investment dollars.

None of these investments is new. They are at the heart of what countries have been doing for the past several decades to get more kids in school and learning. And that is also the point—they have a track record of results at scale in the environments that characterize most developing countries.

Second, over the next decade, innovation to improve learning at lower cost is essential. This is true both intrinsically and because budgets are tight. Through our collaboration with RTI on the Learning at Scale study we have documented several ways in which (cost-) effective systems might be set up to train and coach teachers or resource classrooms.

But, although the number of successful trials and small-scale programs is growing, there is as yet little evidence of their sustained impact at scale in the low- and middle-income country context. This is not a reason to focus less on them. Rather, it argues for continued research and development, and an iterative process of piloting and scaling. In the longer run, these kinds of difficult reforms to pedagogical practice or the integration of Ed Tech into classrooms will pay big dividends if they can be done correctly.

Third, the short-term, relatively easy wins discussed in this volume do not come for free. Education investment may be high return, but the aggregate costs add up to numbers that are likely to pose a challenge for most low-income, and many middle-income, countries. Reasonable projections suggest that we'll fall far short of what's needed to do everything in existing Sustainable Development Goal investment plans. *Schooling For All* makes the case that while it is in the interest of developing countries to take this budget constraint seriously, it is even more important that the international community recognizes the imperative of stepping up its support to supplement the education budget of those countries who are making the necessary efforts to both allocate their own resources and to spend them in ways that deliver good outcomes.

To the keen observer, this report may be a departure from much of CGD's earlier work on education, which implied that the access agenda was over and done with and the focus needed to shift primarily to reforming systems with the aim of raising test scores. In many ways, Schooling for All does represent an evolution of that line of thinking. This shift is the culmination of four years of work since we set up CGD's education program. During that time I've watched my colleagues wrestle with the evidence showing how difficult it has been to scale up new learning initiatives in low-income settings, but also the growing evidence on the success of more bread-and-butter education policies like the abolition of primary-school user fees which swept the globe in the 1990s and 2000s and school feeding programs that continue expanding today.

Yet in one important respect, this volume is a continuation of those earlier efforts. It is marked by the same kind of ambition, devotion to evidence, and sober realism about the economic and institutional constraints facing developing country education systems. At its core, it calls for governments and donors to do more of what they have shown they can credibly do to improve learning: expand schooling. And for the international community to recognize that even with the shortcomings that characterize education delivery in so many environments, sending and keeping girls and boys in school is a high payoff investment.

I hope you find *Schooling for All* as challenging and informative as I have.

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Editor’s Summary: All the Education Money Can Buy

Justin Sandefur

This report debates the case for specific public investments in education in low- and lower-middle-income countries, drawing on evidence of what has worked not just in small-scale experiments but historically and in large-scale national programs. Its messages are intended more for economic policymakers than educators, as they speak to what can be accomplished with fiscal instruments (money) and where trade-offs must be made. CGD does not take institutional positions. Each chapter is authored by a different set of CGD researchers (with some editorial steer), and each commentary is written by external contributors (who were promised space to disagree). This introduction tries to summarize the main arguments across all these contributions, noting points of consensus and ongoing debate—though inevitably with my own gloss.

Money matters

Economists have often shied away from debates about whether developing countries should spend more on education—other than to suggest it doesn’t matter. In 2018, for the first time, the World Bank dedicated its annual World Development Report to the subject of education (World Bank 2018). In the space of 11 chapters and 31 boxes spread across 239 pages, a single box was devoted to education budgets. The headline was that “public spending does not correlate strongly with learning,” and the short discussion concluded that “improvements in learning are unlikely when additional resources are allocated like past funding.”[1]

The core claim of this volume is that spending does matter, for both learning and other outcomes, and a lack of spending is the binding constraint to educational progress in many low- and lower-middle-income countries. We outline a list of shovel-ready investments in education, from abolishing user fees to extending the length of the school day, that have proven technically and politically feasible and have high returns. Of course, the fiscal realities of low- and lower-middle-income countries make some trade-offs unavoidable. The chapters that follow suggest, for instance, that money spent on school meals probably goes further than reducing class sizes. Ironically, it is exactly the kind of spending that the World Bank declared irrelevant—providing more schooling to more kids for more years—that has proven most successful at raising overall learning levels.

Historical and experimental evidence offers a list of educational investments that are technically feasible, politically popular, and effective

In the past 50 years, the developing world has witnessed dramatic improvement in enrollment and learning outcomes. There is a temptation to discount this historical accomplishment as an improvement in “mere” access to schooling, bemoaning the

1. Credit where credit is due: more recently during the COVID-19 pandemic, the World Bank and UNESCO have issued joint calls for increased investment in education systems: https://documents1.worldbank.org/curated/en/226481614027788096/pdf/Education-Finance-Watch-2021.pdf

low test-scores of South African eighth-graders (for instance) while ignoring the increased number of kids who make it to eighth grade. In Chapter 1, Lee Crawfurd, Susannah Hares, and I argue that this historical success should inform countries' forward-looking education policy agenda—especially in places where some of the core policies that did so much good in the 1990s and 2000s, like abolishing user fees and building schools in hard-to-reach areas, have not yet been fully implemented.

The historically proven route to more learning has been more schooling. The massive increase in educational access has also delivered a globally unprecedented increase in learning—with literacy rates in low- and lower-middle-income countries now higher than a half century ago by double-digit percentage points.

Randomized trials and quasi-experimental evaluations show how to do this: subsidize access, take care of kids' health and nutrition while in school, and don't expect huge gains from reducing class sizes or buying new books. A 2013 summary of this literature, published in *Science* by recent Nobel-laureate Michael Kremer, Conner Brannen, and Rachel Glennerster, has held up reasonably well. The abstract is worth quoting in full:

> *Across many different contexts, randomized evaluations find that school participation is sensitive to costs: Reducing out-of-pocket costs, merit scholarships, and conditional cash transfers all increase schooling. Addressing child health and providing information on how earnings rise with education can increase schooling even more cost-effectively. However, among those in school, test scores are remarkably low and unresponsive to more-of-the-same inputs, such as hiring additional teachers, buying more textbooks, or providing flexible grants. In contrast, pedagogical reforms that match teaching to students' learning levels are highly cost effective at increasing learning, as are reforms that improve accountability and incentives, such as local hiring of teachers on short-term contracts. Technology could potentially improve pedagogy and accountability. Improving pre- and post-primary education are major future challenges. (Kremer et al. 2013)*

Some items on this menu have proven technically and politically difficult for developing countries to scale up in public school systems. In the decade since the end of the trials summarized by Kremer and colleagues, development economists have increasingly recognized the difficulty of translating pilot projects into successful national policies. For instance, pedagogical reforms and accountability initiatives, while successful at trial stage, have often been abandoned or seen their impacts wane as they were taken to scale, encountering implementation failures and political resistance. But not everything falls apart when scaled up.

New public spending will do the most good where money is the binding constraint. Meta-analysis of impact evaluations shows that several categories of policies—which we characterize broadly as less technically demanding, more logistical interventions—show impacts that are more robust in large government-run programs. This includes things like school meals and extending the length of the school day. The core argument of Chapter 1 is that, if countries can spend more on education in the short-to-medium term, these things should be the first priority.

In contrast, the crucial task of improving pedagogical practices is less about money. In his commentary, Moses Oketch makes the case not to abandon the more difficult reforms, as governments pivot from a focus on schooling to school quality. By construction, that new direction will require new tools. But both Oketch and Rukmini Banerji note that, while spending money has worked well to improve access, it's less obvious that money can fix the pedagogical failures that Banerji and her colleagues at Pratham India have worked for decades to highlight. In her commentary, Banerji insists that a change in mindset is required across various actors at different levels of the system. Unless people buy into the idea of adjusting the curriculum to the level of the pupil and defining success in terms of learning outcomes, programmatic solutions are doomed to fail.

These are not contradictory agendas: one is a long-term project of research and development or complex system reform; the other is a short-term budget agenda. Most of the chapters that follow focus on this latter fiscal agenda, occasionally neglected by development economists.

Example #1: Free school meals enjoy broad support among education and social protection experts—yet remain limited in scope

Unlike many other policies shown to improve education outcomes, countries have shown they can make school meals work at scale. Many Indian states famously struggle to get teachers to turn up at school—a topic that development economists have fretted over intensely, experimenting with carrots and sticks but having limited success beyond small-scale trials. Yet nationwide, India's midday meals scheme manages to feed 100 million kids every day, and recent research shows benefits not just in the health and fertility of girls who received meals but also in the nutrition of their own children a generation later (Chakrabarti et al. 2021).

Coverage of school feeding programs has grown quickly in recent years but remains low. In Chapter 2 Biniam Bedasso notes that, as of 2019, only about 1 in 7 children in low-income countries receive meals at school, rising to under 1 in 3 in lower-middle-income countries. This is a policy ripe for expansion.

School meals highlight the limits of traditional comparative cost-effective metrics. The benefits of free school meals are sometimes underappreciated because they are spread across multiple outcomes: better nutrition, higher enrollment, and increased learning for kids who go to school. Combining these various benefits, analysis by the World Food Program suggests benefit-cost ratios between 5:1 and 6:1, where data are available.

The evidence on the benefits of school meals is strongest for nutrition and enrollment, and weakest for learning outcomes. As noted in both commentaries on this chapter (by Farzana Afridi and by Ugo Gentilini and Shwetlena Sabarwal), evidence from randomized controlled trials suggests there are cheaper ways to raise education outcomes. Information campaigns encouraging students to stay in secondary school get more bang for the buck than feeding them. But when viewed through a broader lens, considering not just social protection objectives but also arguments about the social contract between states and citizens and the types of large-scale programs that are politically sustainable, school meals look more and more attractive.

The main obstacle to expanding school meals is simply cost. Universal free school meals aren't cheap. Low-income countries spend less than 1 percent of their education budgets on meals (compared with about 2 percent in high-income countries). But that reflects their low coverage rates. Raising coverage to just the global median of 21 percent would eat up 5 percent of low-income countries' education budgets. We return to the question of financing in Chapter 6.

Example #2: The success of free primary education in the 1990s and 2000s provides a template for making free secondary school work

The abolition of user fees has been a key driver of educational progress in the developing world over recent decades. The wave of free primary-education reforms in sub-Saharan Africa and Southeast Asia in the 1990s and 2000s contributed to a massive expansion in education access and a marked global increase in literacy rates. In Chapter 3, Lee Crawfurd and Aisha Ali make the case to build on this success by extending fee abolition to secondary school as well.

Secondary enrollment rates remain low globally. In low-income countries, only about one-third of secondary-school-age children are in school; in lower-middle-income countries, the number is fewer than two-thirds. Hence the need for policy action and the room for potential impact are clear.

There is little doubt that, with straightforward complementary policies in place, fee abolition can boost enrollment. Published econometric studies of the effect of free primary education show that it increased average grade attainment by about one year—of course, with lots of variance across countries. While there's less direct econometric evidence on the more recent turn to free secondary schooling in lower-middle-income countries, experience from Ghana points to big impacts on enrollment. Crawfurd and Ali note that countries that made free primary education work combined fee abolition with school grants to offset the lost revenue to schools. Another complementary policy that has been crucial to boosting enrollment under free secondary schooling in Ghana, with potential lessons for other countries, is relaxation of exam requirements to enter public high school.

Expanding enrollment has historically not undermined learning levels. While Crawfurd and Ali present a review of the literature to bolster this claim, the commentaries by both Robert Osei and Pauline Rose express some skepticism, or at least caution. Osei notes that the potential for massive enrollment expansion to undermine resources per pupil is real and can't be dismissed without a big fiscal push.

Free schooling will often be progressive, according to the analysis in Chapter 4, though this is a contested point. Secondary enrollment in low- and lower-middle-income countries remains strongly associated with parental wealth. Crawfurd and Ali argue that the elasticity of new enrollment in response to fee abolition implied by earlier studies on primary schooling suggests that fee abolition will often be progressive—despite the subsidy to middle-class households whose children are already enrolled. Osei cites evidence from Ghana that free secondary has indeed been pro-poor (a slightly different criterion than progressivity). Rose's commentary remains skeptical.

In fiscal terms, low- and lower-middle-income countries can't afford the UN's Sustainable Development Goals for education

It is almost tautological to note that poor countries can't afford to do everything worth doing in education, as in other sectors. In Chapter 6, Jack Rossiter reviews past efforts to put a price tag on various global programs of improving education outcomes, such as reaching the UN's Sustainable Development Goal #4 for education, which includes free, universal, high-quality primary and secondary education. Some of these exercises produce eye-popping price tags, with several recommending a trebling of public expenditure on education in low- and lower-middle-income countries by 2030.

We should distinguish normal targets from positive forecasts: current spending trends suggest the ambitious goals laid out by various international bodies are likely out of reach. Rossiter presents simple extrapolations of current spending levels, based on the relationship between GDP growth and education spending by income level, and using IMF growth forecasts to project education spending forward to 2030. Those projections show, for instance, a shortfall of about $20 billion per annum by 2030 for low-income countries relative to the targets for domestic public spending on education set out by Gordon Brown's 2016 Education Commission report (Education Commission 2016), and a shortfall of about $300 billion per annum in lower-middle-income countries. These shortfalls already factor in large increases in international aid, which may or may not materialize.

Unit costs likely must come down. Rossiter concludes that the only realistic formula for meeting the kinds of ambitious goals laid out by the UN and other entities involves a significant reduction in expenditure per pupil as pupil numbers grow. That is, education must be done more cheaply if countries are going to fulfill the promise of reaching all children.

Of course, any new international action to soften this harsh reality remains welcome. In his commentary, Daouda Sembene acknowledges the inescapable realities laid out in this budget arithmetic, but he advocates greater focus on rooting out corruption and misallocation of education expenditure, and he issues a renewed plea for more international support. On the latter point, Sembene offers a more creative list of potential financing mechanisms, including carbon taxes, financial transaction taxes, debt relief, and allocation of the IMF's Special Drawing Rights.

For countries forced to contain education expenditure growth, teacher pay and staffing are an obvious place to look

Digging into the costing models underlying the calculations from UNESCO and the Education Commission, the big cost drivers in these projections are the assumptions made about teacher salary levels and pupil-teacher ratios. These models assume that all countries will reach the average of the current top 50 percent of countries in terms of pay and staffing levels by 2030.

Across-the-board increases in teacher salaries or staffing levels are an expensive way to increase education outcomes. Teacher salaries constitute 55 percent of education expenditure in low-income countries and 62 percent in lower-middle-income countries. In Chapter 4, Lee Crawfurd and Alexis Le Nestour make the case that "intensive margin" investments in teachers—that is, raising salaries for existing teachers or reducing class sizes in existing schools with business-as-usual pedagogical approaches—have little impact on learning outcomes.

Higher pay and smaller class sizes make little difference if pedagogy and teacher management are weak. On the basis of their meta-analysis, Crawfurd and Le Nestour argue that the effect of teacher salaries or class sizes on learning outcomes is likely contingent on good pedagogy and teacher management. Even where teacher management systems are functional, as scored by the World Bank, "average effects may be on the order of 0.05 standard deviations per $1000 PPP increase in pay."

Evidence of limited short-term impacts of salary increases on learning leaves open the question of whether higher salaries will attract more capable teachers over the longer term, as noted by Tessa Bold in her commentary. Interestingly, however, Chapter 4 shows that, in the handful of lower-middle-income countries where data are available, teachers are already recruited from the middle (or higher) of the test-score distribution among university graduates.

Getting teachers to underserved communities may be a higher priority than reducing class sizes in general. While Crawfurd and Le Nestour focus on "intensive margin" investments in new teachers to reduce class sizes, David Evans and Amina Mendez Acosta argue that "extensive margin" investments in teachers—recruiting teachers to staff new or understaffed schools in marginalized communities—are a better use of marginal salary expenditures. Speaking to Malawi's experience, Esme Kadzamira picks up on the points raised in Evans and Mendez Acosta's contribution, noting that inequitable allocation of teachers remains a challenge in some rural areas. She also argues that reforms to teachers' career structure, promotion opportunities, and other nonpecuniary benefits may be important to improving teacher motivation and teaching quality, at lower fiscal cost than unconditional salary increments.

Public-private partnerships have done better at expanding access to underserved populations than improving quality, but questions linger about the sustainability of those cost savings

Across the developing world, the share of children attending private schools has blossomed in recent decades—reaching a fifth of pupils at primary level in Nigeria, a third in Pakistan, and nearly a half in India. Faced with dysfunctional government schooling systems, many policymakers have taken renewed interest in outsourcing public education to the private sector. If you can't beat 'em, join 'em, as the saying goes.

Public-private partnerships (PPPs) have generated mixed evidence of lifting learning outcomes over traditional government schools. Reviewing the performance of PPPs across various dimensions in Chapter 5, Maryam Akmal, Rita Perakis, and Susannah Hares find weak and inconsistent evidence of learning gains in existing schools. For instance, Liberia's controversial outsourcing initiative that handed over government schools to private operators delivered only minimal learning gains at enormous per-pupil cost, while reducing enrollment and transitions to secondary school. In his commentary, Jishnu Das piles on. He focuses on five problems bedeviling current attempts to demonstrate gains from PPPs in education and asks whether it's time to turn back from this policy agenda.

Some PPPs have succeeded in expanding access where public schools are missing. Contrary to many popular conceptions of what outsourcing does, Akmal et al. find important examples of positive impacts on enrollment for marginalized groups. For instance, Uganda's secondary school PPP or the Sindh Education Fund in Pakistan may not have dramatically improved quality but extended access into underserved communities.

The strongest arguments for education PPPs are financial, not educational. Despite this mixed evidence, the financial imperative for engaging with the private sector remains compelling to many audiences. In his commentary, Moses Ngware notes this urge to leverage private capital for education investments. Viewed this way, education PPPs look more analogous to infrastructure PPPs that, Ngware notes, have a very checkered history in the developing world. He advocates a focus on schemes where money follows the student, to avoid some of the governance pitfalls associated with subsidizing specific firms.

Private schols offer cost savings not because they're more efficient but because they pay teachers less. That means sustaining the short-term cost savings from PPPs requires a politically difficult, two-tier wage structure for teachers. Teachers doing the same job and paid by the same government will often, quite understandably, militate for equal pay—as seen in India and Kenya.

Conclusion

Sometimes reading reports on topics like education in developing countries can feel like advocacy for "motherhood and apple pie," as Americans say. Yes, of course we all want these things. Where is the disagreement? For the sake of clarity, it is worth emphasizing what this report is not saying.

This report is not a template for fixing failed schools. Most of the volume is skeptical that new innovations in incentives and accountability for teachers will lead to dramatic improvements in learning outcomes at scale. The agenda of pay-for-performance contracts and outsourcing management of existing public schools remains largely unproven at scale in developing-country contexts. And even the less controversial (and in the opinion of most of the authors here, more promising) agenda around improving pedagogical practices in primary schools is largely offstage here: a topic for ministries of education to pursue internally, while we focus here on budgetary instruments.

This is also not just a plea for more money. It does not rest its case on an appeal for dramatic increases in public expenditure or international aid. Most of the authors gathered here would, I believe, support those things. But we have tried to focus the debate at the level of ambitious but feasible new investments, based on simple extrapolations of spending trends and GDP forecasts in low- and lower-middle-income countries.

Stepping back into motherhood-and-apple-pie mode, if I were trying to sell the messages of this report as broadly as possible, I would note that it is fundamentally a case for building on the amazing educational progress of the last half century in the developing world. It's a case for letting more kids finish primary and go on to secondary school, extending their instructional time, and providing them with free school meals. Crucially, these are all things that governments from Ghana to India have shown are possible to do nationwide even in imperfect schooling systems, and they are potentially affordable under current budget trends if expenditure growth is contained in other, lower-priority dimensions.

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Chapter 1. What Has Worked at Scale?

Many cost-effective education programs suffer implementation failures and political resistance when scaled up in government systems. But not all.

Lee Crawfurd, Susannah Hares, and Justin Sandefur

This chapter offers a reinterpretation of the evidence on cost-effective strategies to improve learning outcomes in low- and middle-income countries by filtering policies on one additional criterion: demonstrated effectiveness, at scale, in government school systems. Viewed through such a lens, policies that contributed to the success of the movement toward universal education and the concomitant increase in overall literacy over the past 50 years—such as school construction and abolishing user fees—merit renewed attention. Beyond this unfinished access agenda, filtering on scalability also highlights a number of policies to improve the pace of learning for existing students, including free school meals and extending the length of the school day. Countries where those policies are not already universal have for their consideration a set of feasible policy options to improve learning where money—rather than politically difficult reform or technically demanding innovation—is plausibly a binding constraint, and the return to a marginal dollar spent on education may be high.

1.1 Introduction

Imagine you are the minister of education in a lower-middle-income country with typical education statistics. Net primary enrollment is respectable, approaching 90 percent, but at the secondary level falls to under 60 percent, and test scores lag far behind those in rich countries (World Bank, n.d.; Patel and Sandefur 2020). Half of primary school students are still functionally illiterate (Le Nestour, Moscoviz, and Sandefur 2022).

So you schedule a meeting with the minister of finance, and try to persuade her to increase the education budget. She's skeptical. A few years back your predecessor made the same pitch for new money, and spent it all on laptops that didn't materialize, and when they did, didn't make a dent in classroom practice.[1] And just a few weeks ago, consultants submitted a report showing your multimillion-dollar teacher training program has had zero impact, killed by a thousand minor implementation failures.[2]

1. https://techmoran.com/2019/02/25/kenya-suspends-its-ksh-17-billion-one-laptop-per-child-programme/.
2. https://ageconsearch.umn.edu/record/316663/.

Why should the minister of finance think more money is going to fix things this time? What, specifically, could your ministry do with, say, a 10 percent budget increase that would actually move the needle on national education outcomes?

Before diving into the answers, note how this hypothetical story, while simplistic and contrived, reframes the question about what kind of evidence and what kind of education policy advice developing countries need.

The assumption implicit in most discussions of cost-effective education policies is that (1) the education budget is fixed, or "exogenously given" in econ-speak, but that (2) the ministry of education's capacity to implement complex programs is infinitely malleable with enough political will, trainings, and foreign technical assistance. Here, we flip those assumptions around. In the short term, there is no changing the political reality that you cannot, say, put all teachers on pay-for-performance contracts overnight, nor could most ministries of education even implement the national monitoring and evaluation required to do such a thing. "You go to war with the army you have," as the saying goes. But on the flip side, the ministry's budget allocation is not fixed at all, and is potentially open for negotiation if it can show viable spending options with the prospect of high returns. State capacity is fixed; the budget is negotiable. And budget negotiations require evidence of "shovel-ready" projects, where money is the key missing ingredient.

We argue here that many developing countries do indeed have at their disposal such high-return, shovel-ready investments in basic education where money is the binding constraint. The following sections lay out the evidence for that conclusion in two parts.

1. **The developing world has seen a massive increase in literacy over recent decades as well as an unprecedented expansion in schooling with high economic returns**. But as we illustrate in the next section, the average *quality* of basic schooling in most low- and middle-income countries is dismal, and appears to have stagnated for decades. But unconditional literacy rates have boomed as the movement toward universal basic education has spread across the globe. Historical evidence suggests that the most proven, effective, scalable approach to improving educational outcomes may be to see this access agenda through—and focusing first on increasing access to secondary school. In some sense, expanding access is the only thing that has ever really worked, and the economic returns are high.
2. **Education policymakers can point to specific policy levers that have contributed to this progress, raising education outcomes at a national scale, even in imperfect government systems**. The impact evaluation literature is full of proven interventions to improve student learning that fell apart when rolled out at large scale, particularly within government systems in developing countries. Some of the most exciting interventions in education research have seen impacts taper off when taken to scale. Autopsies of these programs often point to "death by a thousand cuts": small compromises in the program design, myriad implementation failures, low adoption by teachers, and even political backlash leading to active resistance from teachers and staff. But that is not universally true. Our goal here is to point to policy levers that appear robust to the implementation environment. Focusing on policies that tend to survive and work at scale still leaves an actionable agenda for developing countries to improve education outcomes in the near term.

Note that the development literature discusses many promising education interventions that we do not include in our list here, but that we would encourage policymakers not to discard. Initiatives such as structured pedagogy, "teaching at the right level," or ed tech providing adaptive instruction have all shown big impacts on student learning in developing country settings. But as we show below, if we focus on the whole set of impact evaluation results rather than the most successful cases, the track record of these programs remains mixed at present or unproven at scale.

Crudely, we class these things as policies that ministries of education should explore, test, and refine. Our interest here is in policies that realistically lack nothing more than what a minister of finance can provide: money.

Based on a combination of recent historical evidence and meta-analysis of impact evaluations, our hypothetical minister of education has a potentially compelling budget justification to present to her ministry of finance colleagues. Familiar policies that have worked at scale in other low- and lower-middle income countries like abolishing user fees, providing universal school meals, or extending the school day are essentially shovel ready—that is, if the money were available they would stand a likely chance of success even at large scale in challenging environments.

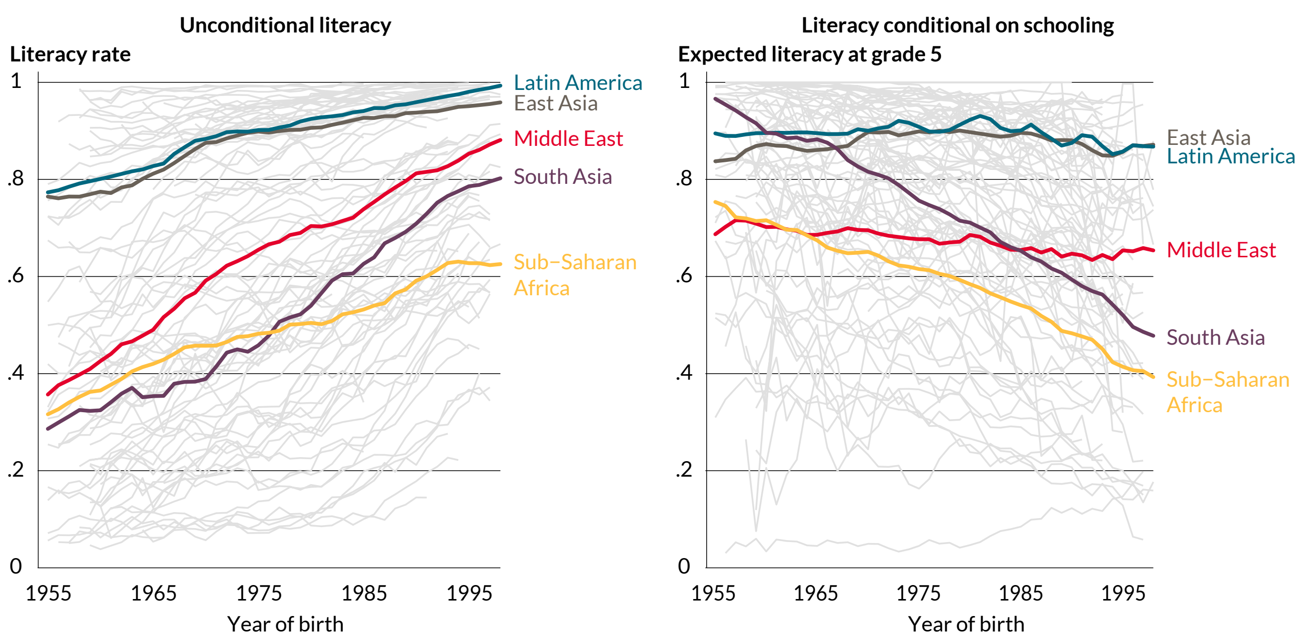
1.2 Building on a 50-year boom in global literacy

Over the past 50 years developing countries as a whole have dramatically expanded access to basic education. As shown in the left panel of Figure 1.1, taken from Le Nestour, Moscoviz, and Sandefur (2022), the immediate result of this schooling expansion has been a steady increase in literacy rates, especially for women. In South Asia, for instance, women born in the 1950s had only about a one-in-three chance of being literate at age 30, while women born in the 1990s had a three-in-four chance.

One premise of this chapter is that it's important not to lose track of what an astounding, historically unprecedented success the push for universal schooling has been. Across the developing world, literacy rates are converging to rich-country levels, and are now far higher than today's rich countries achieved at similar points in their own economic development.

Figure 1.1. **While, in most regions, women's literacy has improved dramatically overall, literacy for women with five years of schooling has stayed flat or declined**

Long-run trends in literacy, conditional and unconditional on schooling



Source: Le Nestour, Moscoviz, and Sandefur (2022) based on Demographic and Health Surveys (DHS) and Multiple Indicator Cluster Surveys (MICS) data.

Note: Literacy rates are estimated for women at age 30, born in the year shown on the horizontal axis. Women who attain secondary schooling are not tested and assumed to be literate. Each gray line shows one country's trajectory.

There is another, less optimistic way of looking at these trends though.

While overall literacy has blossomed, there is evidence that school quality has deteriorated as enrollment has expanded (see the right panel of Figure 1.1). In almost all of the countries where the underlying data are available from the US Agency for International Development (USAID) Demographic and Health Surveys, the literacy rate has gone up. But the literacy rate *among both men and women who went to school* has stagnated or declined. The end result is that in over half of the countries with data, more than half of women who completed five years of primary schooling cannot read a single sentence.

On the back of numbers like this, international organizations like the World Bank and UNESCO have declared a "learning crisis" in the developing world. The hope is that raising an alarm will spur education policymakers to undertake ambitious reforms to improve school quality. But there is a risk that that would mean abandoning what has worked (expanding access) in favor of what has not (trying to increase the pace of learning per year).

Both views have a solid factual basis. The policy question is whether a given country will do better by focusing attention on school quality reforms or by expanding access to more children for more years. The answer depends on more than these simple trends.

At a minimum we'd like to know (1) what are the returns to schooling (in its current imperfect form), and how much those returns would change if learning levels improved, and (2) the relative cost-effectiveness and feasibility, at scale, of policies to expand either access or learning. Let's look at each in turn.

1.3 Schooling pays big economic returns, even where learning levels are low

Labor economists typically measure the economic return to schooling as the percentage-point increment in wages associated with an additional year of schooling. Using a database of labor force surveys from dozens of countries, World Bank researchers found an average return of 12 percent for a year of schooling at the primary level, 7 percent at the secondary, and 15 percent at the tertiary (Montenegro and Patrinos 2014).

There is no truly comparable measure of test scores around the world, but World Bank researchers have also attempted to place different international and regional assessments on a common scale (Angrist et al. 2020). These harmonized learning outcomes are scaled to have an average in high-income countries of 500 and a standard deviation at the pupil level of 100 points.

Combining these two data sets, we compare the economic returns to schooling among countries with different levels of average test scores in Figure 1.2. In our analysis we find very little relationship between the quality of schooling in a country and the individual earning gains it delivers. There is perhaps some evidence of an increase in the return to secondary schooling at the highest test-score levels (mostly in rich countries). But returns to primary schooling show no clear relationship with test scores: if anything, wage returns are lower where test scores are higher. That is also the case for the return to tertiary education.

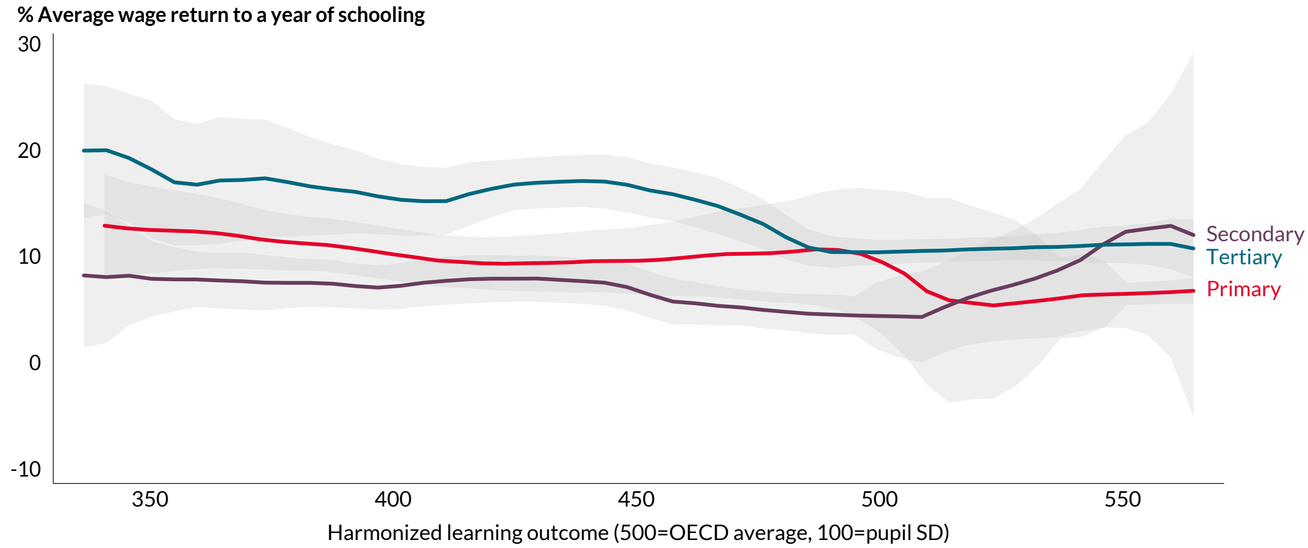
One should obviously resist any causal interpretation of these correlations. No sensible reader would infer that reducing test scores will increase the economic value of schooling. Many other factors differ across these countries including, not least, the relative scarcity of skills, which may drive up the wage return in countries where both schooling levels and test scores are lower.

But the simple descriptive fact remains: even in systems where schooling tends not to generate stellar learning outcomes, education pays. From an economic perspective, even rudimentary schooling may be a very good investment.

One possible objection is that the returns to schooling reported here (i.e., the vertical axis in Figure 1.2) are themselves not causal estimates.

Figure 1.2. **The returns to schooling are high, even where test scores are low**

Average wage return to schooling, by average country learning level



Note: Countries are the units of observation. Lines represent local polynomial regressions; shaded areas show 95% confidence intervals. The horizontal axis reports harmonized learning outcomes from the World Bank (WDI). The vertical axis reports Mincerian returns to schooling, as reported by Montenegro and Patrinos (2014) using harmonized labor market survey data. The samples at the primary, secondary, and tertiary levels include 42, 54, and 76 countries, respectively. SD = standard deviation.

There are lots of reasons more schooling might be correlated with higher wages, and even with other outcomes such as lower child mortality, even if schooling is actually useless. But while the omnibus estimates from Montenegro and Patrinos (2014) are vulnerable to this critique, a mountain of evidence from other studies suggests that the return to schooling in many developing countries is indeed causal. Those studies draw on natural experiments and policy reforms in places where enrollment has expanded in systems that produce fairly dire learning outcomes.

- In Indonesia, where the median student scores below the 10th percentile for Vietnam on the Program for International Student Assessment (PISA), Duflo (2001) found that the surge in school construction between 1973 and 1976 still led to a significant increase in earnings of between 6.8 and 10.6 percent per extra year of schooling, and Breierova and Duflo (2004) showed it also led to a decline in both fertility and child mortality.
- In Nigeria, where just 8 percent (!) of adult women who left school after fifth grade can read a single sentence, Osili and Long (2008) show that one additional year of schooling nevertheless led to 0.26 fewer births in 2003, a result based on regional differentials in the timing of the introduction of universal primary education.
- In Uganda, where teachers are absent from the classroom 60 percent of the time, Keats (2018) found that Museveni's Universal Primary Education program launched in 1997 led to reduced overall fertility and fewer chronically malnourished children.
- In Ethiopia, where Singh (2014) showed that primary schools lag far behind those of India, Peru, and Vietnam in learning gains, Chicoine (2016) found that the extra 1.5 years of schooling that girls achieved after the abolition of user fees in stages between 1993 and 1996 still led to a significant decline in fertility driven by delays in sexual activity, marriage, and birth, as well as increased contraception usage.
- In Kenya, where Lucas and Mbiti (2014) found that even the best secondary schools don't add much additional "value" in terms of learning gains, Brudevold-Newman (2017) found that free secondary

education led to delayed childbirth and a shift from agricultural to skilled employment.

While arguably causal, all of the estimates above measure the private return to schooling—that is, how much schooling raises an individual's own earnings, rather than the returns to the economy as a whole or some other, broader social goal. There is a voluminous and largely inconclusive literature on the question of whether the private returns to schooling reflect human capital (and are thus, perhaps, a good measure of the social return to schooling as well) or some other mechanism. For example, they may signal preexisting ability, or they may even be indicative of a rent collected by educated individuals who gain privileged access to rationed formal-sector jobs (even if their productivity is no higher). Some recent studies point in the direction of human capital explanations:

- In one of the most thoroughgoing studies of this question, Khanna (2021) looks at India's flagship scheme to expand access to schooling in the 1990s and early 2000s, the District Primary Education Program. By comparing districts that were barely eligible or ineligible to participate in the program using a regression discontinuity design, Khanna is able to show not just how much an extra year of schooling pays an individual (an earnings increase of about 13.5 percent) but how much it increases earnings in a district as a whole (about 7 percent). In short, the answer to the question of how much of the private return to schooling in India is a genuine social return is about half.
- Arteaga (2018) uses an interesting natural experiment at Colombia's leading university, which in 2006 reduced the course requirements for a degree in economics or business without changing the selectivity of its admission standards. The result was that graduates under the new system saw 13 to 20 percent lower earnings, consistent with a significant role for genuine human capital acquisition in the returns to college in Colombia.

These micro studies suggest (indirectly) that schooling expansions are beneficial for an economy writ large even in cases (such as India's) where test scores are low.

Some macro evidence, however, suggests a stronger role for school quality than quantity in explaining aggregate economic growth. In a series of influential studies, Hanushek and coauthors found significant, positive, and putatively causal relationships between test scores and economic growth, but no such relationship between years of schooling and growth (Hanushek and Kimko 2000; Hanushek and Woessmann 2012).

In our view, cross-country regressions probably should not receive too much weight in the final assessment, especially when in tension with microeconometric results. The well-identified results from Khanna (2021) are perhaps the single strongest piece of evidence on the question of the aggregate effects of schooling expansions. On the other side of the ledger, microeconometric evidence on the impact of learning gains, per se, is much weaker. A handful of studies using the World Bank's STEP surveys, which embed a rich battery of cognitive skill questions in a standard labor market module, find statistically significant but relatively small effects of test scores on wages in a standard Mincerian specification (Valerio et al. 2016). Taken literally, these results imply that a year of schooling and one standard deviation in learning—which often takes two to three years to accumulate—have similar effects on wages. These STEP results lack any clear basis for causal inference though, and must be treated as only indicative associations.

In summary, standard measures of the labor market returns to schooling appear to indicate a big effect, even where school quality is low. There is strong evidence that such returns are causal, and some (albeit far from perfect) evidence that the gains of education expansion accrue not just to individual works but society at large. It is hard to draw firm conclusions about the potential economic gains from improvements in school quality (though they may be large). But even with business-as-usual school quality, allocating more public resources to expanded schooling in many developing countries remains a high-return investment.

1.4 New innovations to improve public schools often fail when scaled up

In a recent report, the World Bank's research department summarized key lessons from decades of research in development economics for development policy as follows:

> *The last decade has seen a large increase in the number of policy evaluations, with many pilot programs tested rigorously for their impacts. This is a boon to governments looking to practice evidence-based policy making. However, even when pilots and local development interventions have proven very successful, they have often been difficult to scale up in a cost-effective way to achieve development impact on a large scale. (Artuc et al. 2020)*

Or as economist Jason Kerwin (2021) put it more succinctly, "nothing scales."

This is a common finding across sectors (List 2022; List, Suskind, and Supplee 2021; Vivalt 2020), but perhaps nowhere in the development literature is this lesson more relevant than in education. Across all types of education interventions in low- and middle-income countries, effect sizes halve when study samples go from 500 to 5,000 (Evans and Yuan 2020).

To take one example, the "Jamaica model" of home visitation to promote child development is one of the more celebrated social policy experiments in the world (Araujo, Rubio-Codina, and Schady 2021). A randomized controlled trial (RCT) with 64 children, who have now been tracked for more than 30 years, found effect sizes of 0.88 standard deviations on overall measures of child development after 24 months. But a larger RCT of a similar program with about 720 children in Colombia found effects on an overall child development index of just 0.18 standard deviations after 18 months—still impressive, but far short of the Jamaica performance. Worse, those effects faded to null effects two years later. Finally, an even larger (nonrandomized) evaluation of a similar program in Peru with roughly 70,000 beneficiaries found overall effects of about 0.1 standard deviations on an aggregate index of child development.

There is a large and contentious literature on the scalability of preschool programs in the United States. The Perry preschool project in Michigan randomly assigned 58 children to high-quality preschool in 1962, with persistent impacts on later educational outcomes and adult life outcomes up to age 40 (Schweinhart et al. 2005). Similarly the Abecedarian project in North Carolina involved an RCT in 1972 of around 100 children given high-quality provision, with large impacts found on both adolescent test scores and later adult life outcomes up to age 35 (Carolina Abecedarian Project, n.d.). By contrast, results from the national Head Start program and large-scale universal programs have been more mixed (Elango et al. 2015). Similarly, parallel evaluations of a promising nongovernmental organization (NGO) kindergarten program in the Philippines found big impacts on primary school academic achievement, but those attenuated significantly when the government scaled it up (Bloem and Wydick 2021).

A fairly recent example is the impact of programs to encourage a growth mind-set in students (Ganimian 2020). Multiple small-scale experiments found surprisingly large effects from low-cost interventions, but when implemented at a reasonable scale in public secondary schools in Argentina, a similar intervention produced no impact on essentially any outcome.

Teacher coaching remains a promising category of education intervention (Kraft, Blazar, and Hogan 2018). But even within the United States, where one might expect high government implementation capacity, meta-analysis shows that average effect sizes of 0.18 standard deviations on achievement fall dramatically as programs reach larger scale.

Pay-for-performance contracts for teachers have generated significant improvements in student learning in randomized trials in Tanzania, Uganda, and Rwanda

(Obrero and Lombardi 2021). But an evaluation of a nationwide pay-for-performance system in Peru found precise null effects.

In many of these examples, the role of scale is hard to confidently disentangle from the role of context and other variation across programs. So it's important to unpack why scale matters.

Implementation fidelity and political economy. Cull and McKenzie note the case of contract teachers in Kenyan primary schools who raised math and reading scores in a small NGO pilot program but had zero impact when the government decided to hire 18,000 of them (Bold et al. 2018). To some degree, the Kenyan government may simply have been less capable of implementing the program well. But the shift from NGO to government provision as part of the scale-up also introduced new political economy elements. Organized resistance from civil service teachers ultimately doomed the government program.

Sensitivity to small tweaks. Sometimes programs must deliberately make compromises in the initial design when taken to scale. Many interventions don't appear to be very robust to small tweaks, and we just don't understand which tweaks matter. Kerwin and Thornton (2021) study a mother-tongue literacy program that had a very large impact on test scores. But when they tested "a modified program that tried to simulate how policymakers would reduce costs," impacts dropped off substantially.

The *scalability* of programs is critical when considering what interventions to recommend to a minister of finance. In 2020 the Global Education Evidence Advisory Panel convened jointly by the United Kingdom's Foreign, Commonwealth and Development Office (FCDO) and the World Bank identified seven "Smart Buy" approaches. Each approach has good evidence that it can be highly cost-effective. They do not, though, all have good evidence at scale. The average (median) country globally has 2,800 schools (Walter 2020). The vast majority of studies included in the Smart Buy report are well below this kind of national scale—including all studies on merit-based scholarships and level-adapted software and all but one study on information, targeted teaching, and structured lessons.

It is unclear whether the evidence cited in Figure 1.3 has been filtered to focus on studies with positive effects. Including null results is important for understanding scalability. As we show in the next section, many interventions (including some on this list) see effect sizes drop off markedly as scale increases.

All of which is not to say that these specific interventions are not scalable. Indeed we'll argue presently that some are quite promising in this respect, or have already been scaled by NGOs. Our point is simply that the current policy literature on "what works" in education has not reliably filtered the available evidence on consistent, demonstrated effectiveness at scale in government systems.

1.5 So what has worked at scale?

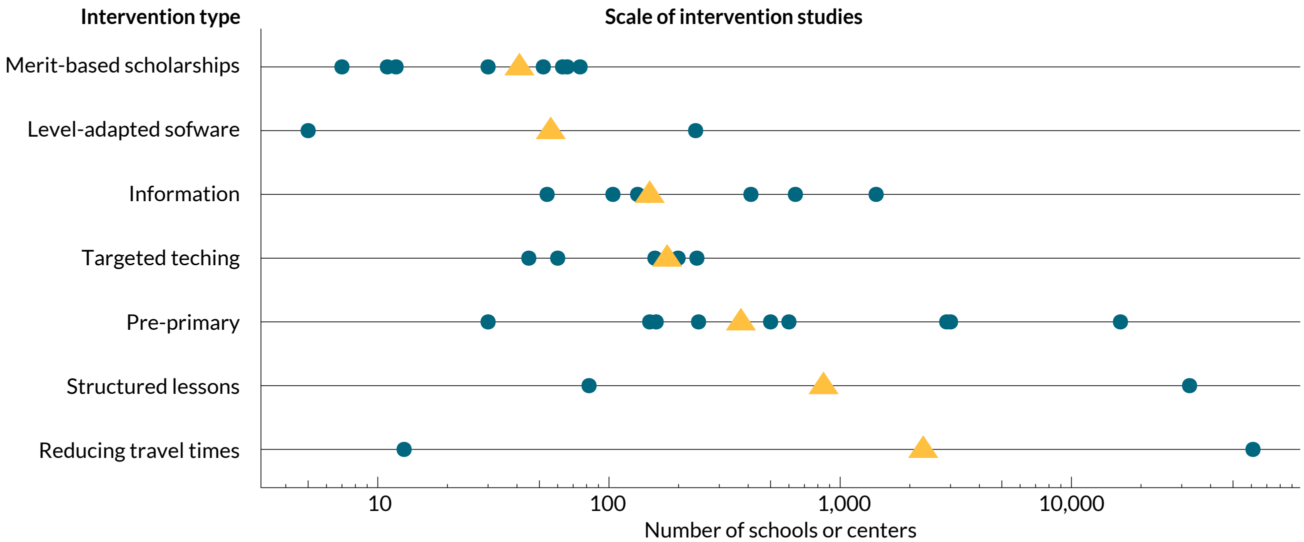
Which policies are likely to scale? Broadly we can think of two classes of policy.

Type A policies can be extremely effective when well implemented but are complex and difficult to get right. Often they require the judgment of highly skilled staff, who typically are in scarce supply. They generally ask teachers, principals, and district officials to learn and uniformly adopt new behaviors that are difficult to monitor and difficult for people to adhere to with high fidelity. Our conception of Type A policies here maps roughly onto things that Andrews, Pritchett, and Woolcock (2017) would call "implementation-intensive" services or "high discretion" activities, as opposed to mere "logistics." In short, Type A policies require highly capable organizations to implement effectively.

Prominent examples of Type A policies include structured pedagogy, teacher coaching, or home visits for early child development (ECD). These are things that we know "work" but we also know can be hard to get right, as we show below. Other examples for which we don't have data to do systematic analysis here include

Figure 1.3. Much of the evidence for "Smart Buy" programs comes from small-scale pilots

Number of schools included in studies in Smart Buy Report, by intervention type



Note: The figure is the author's analysis based on the citations given in the World Bank and FCDO's Global Education Evidence Advisory Panel. We have not included studies here that do not have a control group, either experimentally or quasi-experimentally assigned. For six of the studies presented, we convert the units of the study into a similar approximate number of schools or centers.[3]

merit-based scholarships, teacher performance pay, and targeted teaching.

Type B policies might be less effective when well implemented, but they are more robust to weak implementation. They tend to focus on engineering solutions, building stuff or distributing goods in kind; they include rules that can be changed with the stroke of a pen and where adherence is easy to observe. Type B policies don't require lots of high-skilled technical staff to design or implement. They don't make big demands of teachers' time or require hard-to-monitor changes in their behavior.

Examples of Type B policies that we discuss here include policies such as building new schools in places where there are none, abolishing school fees, lengthening the school day, and providing school meals. Other examples for which we lack the data to go into detail here include unconditional cash transfers, construction of new preschool classrooms or ECD centers, information provision (e.g., on schools' performance), reducing travel times, health-related interventions such as deworming, and policies to reduce pollution. Intermediate cases exist as well, where more empirical evidence is probably required before judging the difficulty of implementation, such as the rollout of level-adapted software.

We test this framework with estimates of program effectiveness at different scales across these different policy types. We review existing studies on five policies: three Type A policies (structured pedagogy, teacher coaching, and ECD home visits) and two Type B policies (school meals and extending school hours) (see Figure 1.4). Where possible we rely on existing reviews to fix the sample of studies. For structured pedagogy we draw on the 12 studies from a review of evaluations

3. For Eble et al. (2021), the units of intervention were villages, each of which we present as being equivalent to one school. For Angrist et al. (2021), the units of intervention were households, and we present 100 as being equivalent to one school. For Muralidharan and Prakash (2017), the intervention reached all secondary schools in Bihar in 2006: we estimate this number to be 2,286, the total number of secondary schools in Bihar in 2020 according to a report by the Indian Ministry of Human Resource Development (Embibe 2020). For Hasan et al. (2019), the scale was reported in terms of villages, which we again present as being equivalent to one school. For Gormley, Phillips, and Anderson (2018), the number of preschools in Oklahoma for the mid-2000s is estimated as 2,888, the present figure for Oklahoma (GreatSchools 2020). For Perera and Aboal (2019), we estimated the number of preschools in the target period to be 500, calculated by dividing the approximately 100,000 preschool students in the target period by an estimated student per school ratio of 200 (Santiago et al. 2016).

of early grade reading programs (all early grade reading interventions between 2006 and 2017 that measure learning with an "early grade reading assessment" and have a control group; Graham and Kelly 2019).[4] For the Jamaica ECD model of home visits, we use the 11 studies from Jamaica and replicated in Bangladesh, Colombia, Peru, India, and China, reported in Araujo, Rubio-Codina, and Schady (2021). For teacher coaching, we use nine studies from a review of US evaluations (Kraft, Blazar, and Hogan 2018). For school meals and the school day, we conduct our own search for experimental or quasi-experimental studies that report effect sizes on learning and program scale. This search results in 11 studies on school meals and 11 on reforms to the length of the school day. These studies are included in the Appendix.

The meta-analysis of these studies is in line with the argument that skill-intensive policies can have bigger effects at small scale, but with effects that shrink with scale, and have rarely been tested at scale, whereas less skill-intensive policies have smaller effect sizes at small scale, but these effects are consistent at large scale.

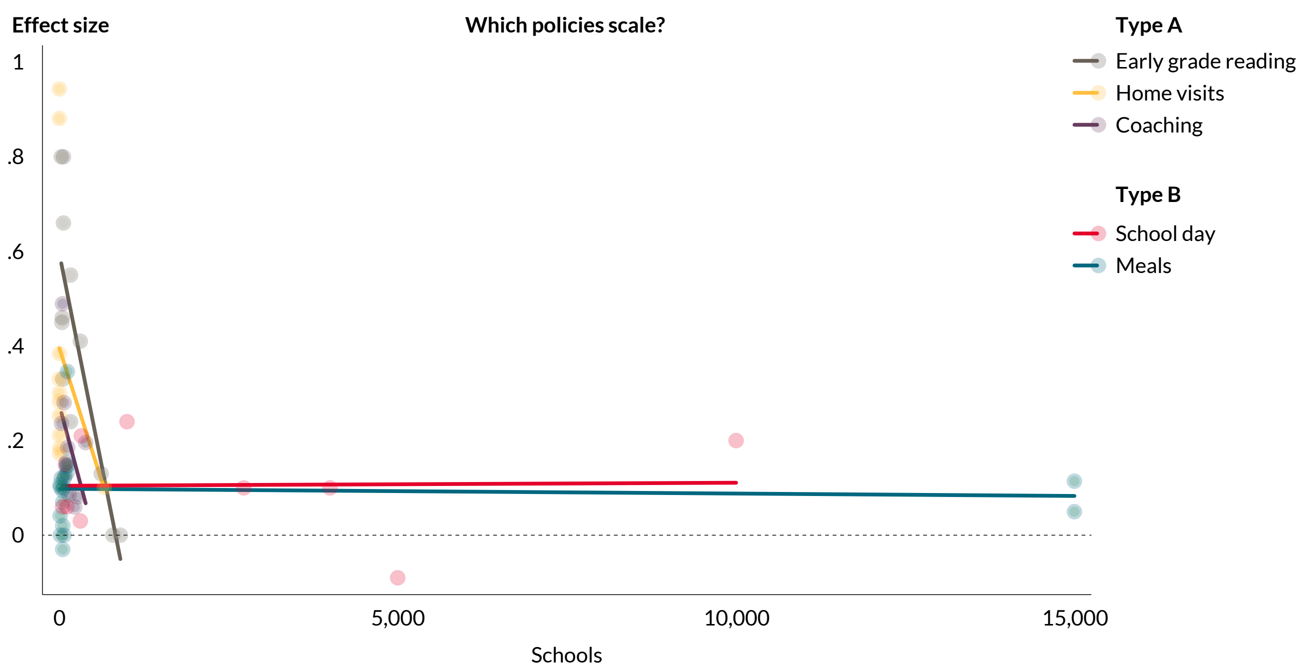
Among Type A programs, reforms to improve the quality of pedagogy in primary school are an obvious priority in pursuing foundational literacy and numeracy. But the track record of many such policies—for example, structured pedagogy programs for early grade reading and literacy (Graham and Kelly 2019) and "teaching at the right level" (Banerjee et al. 2017)—is somewhat mixed when taken to scale by governments in developing countries.

Figure 1.4 shows a clear attenuation of effects when structured pedagogy programs go to scale. But this pattern is only partially captured by the studies in the figure, which is restricted to evaluations with a control group as reported by Graham and Kelly (2019). For

Figure 1.4. Which interventions scale?

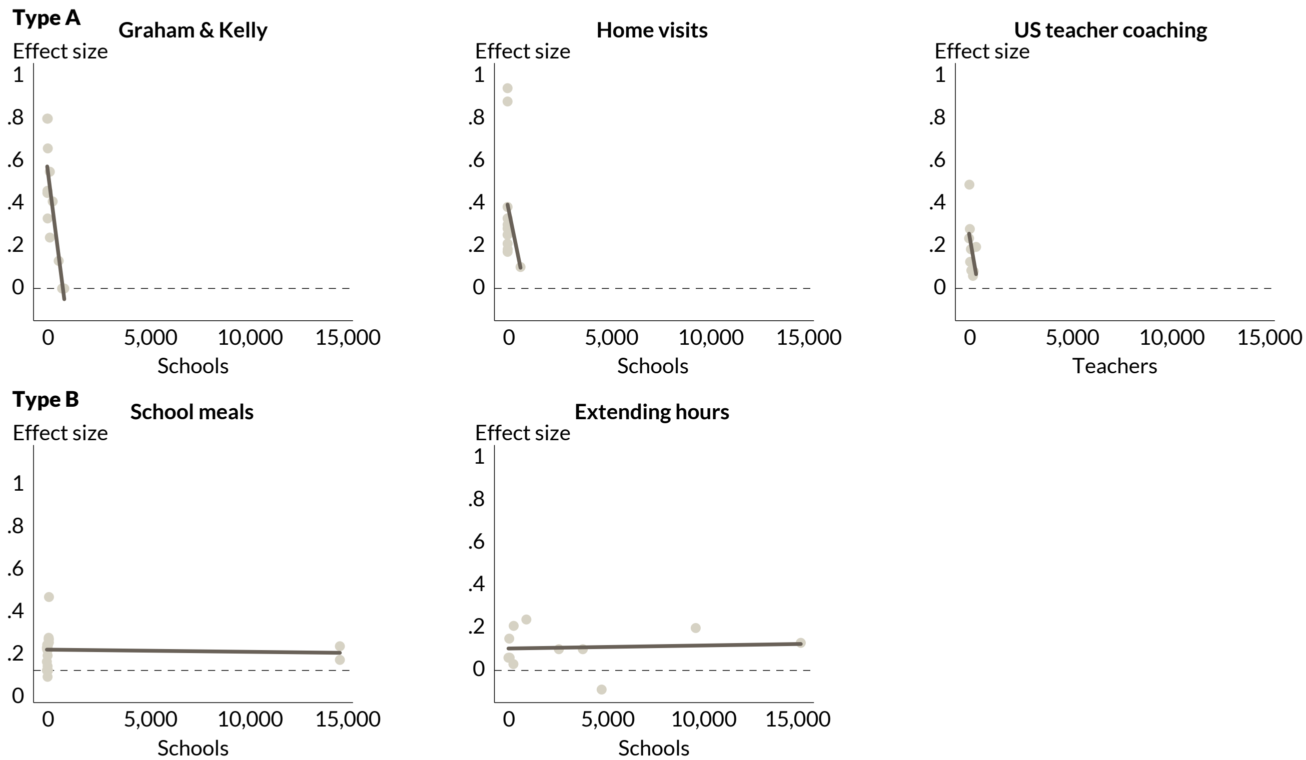
Meta-analysis of effects on learning from five different interventions

Panel A. Side-by-side comparison



4. Where possible, we rely on existing reviews here to tie our hands somewhat in the selection of studies. Particularly in the case of structured pedagogy, however, we know of some relevant studies (and may be unaware of others) conducted after Graham and Kelly (2019) was published. See Box 1.1 for a longer discussion.

Panel B. Same results broken out by policy



Note: The figure includes

- *12 studies on early grade reading programs (all early grade reading interventions between 2006 and 2017 that measure learning with an early grade reading assessment and have a control group, reported by Graham and Kelly 2019);*
- *11 studies of a home visitation program piloted in Jamaica (the Jamaica ECD model), and replicated in Bangladesh, Colombia, Peru, India, and China (reported in Araujo, Rubio-Codina, and Schady 2021)—as this was a home-based intervention, we convert to the same scale as the other studies by assuming that 100 households are equivalent to one school;*
- *9 studies on teacher coaching from the United States (reported in Kraft, Blazar, and Hogan 2018);*
- *19 results from 11 studies of the effect of school meals on learning outcomes, included in the Appendix; and*
- *11 evaluations of reforms to the length of the school day, included in the Appendix.*

instance, a celebrated USAID program in Kenya (originally known as Primr, and later Tusome) showed promising results in a randomized trial, before being rolled out nationwide. Almost by necessity, the evidence of impact at scale consists only of a before-and-after comparison, but those simple trends are very encouraging: the share of first-graders who were classified as "zero readers" fell from 53 percent at baseline in 2015 to 23 percent in 2016, before creeping back up to 34 percent by 2019—still a major improvement (Keaveney et al. 2021). But a similar USAID program to improve early grade reading in Liberia also showed big initial impacts (Piper and Korda 2011), yet a larger second phase saw multiple disruptions and disappointing results (Gove, Korda Poole, and Piper 2017).

Understanding how to reliably deliver the kinds of gains seen in the Kenyan program, and avoid the pitfalls encountered in Liberia and elsewhere, remains a priority area for research and development in the sector. RTI International's Learning at Scale project provides a good starting point: delving into the inner workings of eight large-scale programs in diverse contexts with solid evidence of sizable learning gains for primary students (Perakis and Stern 2021).

Box 1.1. What has worked to improve reading pedagogy at scale?

Our analysis in the main text shows that programs to improve pedagogy in low- and lower-middle-income countries have struggled to scale up successfully. But it is important to note that exceptions do exist. And some of those exceptions come from new research conducted too recently to appear in the Graham and Kelly (2019) sample we relied on earlier.

So what has worked to improve pedagogy? The Learning at Scale program (Stern et al. 2021), led by RTI International in coordination with the Center for Global Development, scoured the world to find the best recent examples of programs with evidence of causal impact on literacy outcomes at large scale, and specifically those that got there by improving classroom teachers' effectiveness.

Selecting on the dependent variable (i.e., learning gains) complicates the interpretation here. Since the programs shown in the table were selected because they worked, they do not represent what policymakers can necessarily expect to achieve with similar interventions. In addition, the quality of evidence cited here for impact at scale varies quite a bit. Several of the programs were subject to rigorous evaluations at the pilot stage but have only indicative results (e.g., before-and-after comparisons without a control group) for the scaled-up program.

Nevertheless, these successful programs might offer clues about what has worked, and could be emulated elsewhere.

The Learning at Scale team looked across these programs for common distinguishing features.

Table B1.1. Eight successful, large-scale reading programs

Country	Program	Organization	Approx. number of schools	Impact on reading fluency in correct words per minute (cwpm)
Tanzania	EQUIP-T (2014–2020)	DFID/Cambridge Education and Mott MacDonald	5,000	+9 cwpm in Kiswahili
Ghana	Ghana Learning (2014–2020)	USAID/FHI360	7,000	+6 cwpm across languages in grades 1–2
Senegal	Lecture Pour Tous (2016–2021)	USAID/Chemonics	4,000	+13 to 18 cwpm across languages in grade 2
Nigeria	NEI Plus (2015–2021)	USAID/Creative Associates	7,000	+2 to 13 cwpm in Hausa in grades 2–3
Pakistan	PRP (2013–2020)	USAID/International Rescue Committee	24,000	+6 cwpm in Urdu in grades 1–2
India	Read India (2015–present)	Pratham	250,000	Two to three times the annual reading progress in public primary school
India	SERI (2015–2020)	USAID/Room to Read	2,000	+18 cwpm in Hindi in grade 2
Kenya	Tusome (2015–2021)	USAID/RTI	24,000	+12 cwpm in Kiswahili in grade 2

Source: Adapted from Stern et al. (2021), table 4.

Box 1.1. Continued

For starters, all the programs were run by NGOs or contractors paid by foreign donors—largely because few government programs collected the necessary impact evidence to be included.

During fieldwork with three of the programs, Stern et al. (2021) noted a "change in organizational culture" to focus on program implementation and setting clear expectations. The programs all combined training teachers, supplying teachers with necessary inputs, and ongoing follow-up to reinforce new teaching practices.

Substantively, Stern et al. note that seven of the eight programs can be characterized as structured pedagogy or "teaching at the right level" programs. Across three programs where researchers did follow-up interviews, teachers emphasized the importance of small-group practice in the training process and of guidance from coaches on how to teach. When asked what part of their instruction had had the biggest impact on student learning, teachers across multiple programs noted "more focus on letters, sounds, and/or blending."

Turning to Type B programs, school meals are a key example: too many children are hungry in school. Governments are pretty good at supplying school meals, and their provision can also increase enrollment and retention, particularly of girls (Gelli, Meir, and Espejo 2007). So it is puzzling that school feeding programs are not more central to education reform efforts. Alderman and Bundy (2012) note in the *World Bank Research Observer*, "numerous studies show that in-school feeding has a positive impact on school enrollment or participation in areas where initial indicators of school participation are low and . . . improved performance as measured by tests of achievement is often reported for [school feeding programs]." (See also Figure 1.4.) The last decade of evidence has added to the case that school feeding boosts both access and learning (Snilstveit et al. 2015; Evans and Mendez Acosta 2021).

Beyond its effectiveness, school feeding is an example of a program that is also feasible to implement with some fidelity in government systems. Experiences from India to Mozambique suggest that even weak states—sometimes with the help of aid donors—can successfully deliver meals to millions of kids. Writing about India's massive school feeding program, economist Abhijeet Singh (2013) notes: "Midday meals, which reach about 120 million children on every school day, are probably the most successful of all interventions in education that the Indian state has delivered in the past decade. On any school day, a quarter of teachers are absent from government schools (Kremer et al. 2005), only 45% of those in school are teaching, but in 87% of schools, a hot meal is served (ASER 2012)." Despite this, the latest World Food Programme (2020) state-of-feeding report suggests that low numbers of children are actually receiving school meals: 20 percent of children in Kenya and 10 percent in Rwanda, for example.

Even without feeding children *in* school, mass micronutrient fortification of staple foods has also been shown to improve learning, at scale (Tafesse 2022).

To cite another example, a raft of studies have examined national reforms to remove "double-shifting"[5] and extend the school day (e.g., Rosa et al. 2022 in Brazil, Cabrera-Hernandez 2020 in Mexico, and Orkin 2013 in Ethiopia). Doing so is not cheap—it needs the recruitment of many more teachers. But it can also double the amount of time that children have for

5. Double-shifting is the practice of running two shifts each day at a school. Each child attends only the morning or afternoon shift. This allows more children to benefit from the same buildings and teachers, but limits the amount of school time that each child gets.

learning overnight, and effectively, at a huge national scale. More time in school has also had documented later life labor market benefits (Dominguez and Ruffini 2021). This kind of foundational investment may also be necessary for later more cost-effective interventions. As discussed in a later chapter in this volume, larger classes carry a small cost in terms of quality (Crawfurd and Le Nestour 2022), and so may be a price worth paying for increasing the total amount of time each child is able to attend.

Finally, we should flag that one reasonable pushback on the type of analysis we present in Figure 1.4 is that it ignores the time dimension, and learning from one study to the next. Banerjee et al. (2017) relate how a series of five randomized trials across multiple Indian states—with greater and lesser degrees of success—helped refine Pratham's Teaching at the Right Level (TaRL) approach into a final, scalable Haryana model. Viewed this way, early missteps, null results, and the apparent sensitivity of impacts to specific program design features should not be seen as an indication of a lack of robustness, but rather steps toward a more robust end result. (See Rukmini Banerji's comment in this volume for more on Pratham's experience.)

For now though, we interpret Pratham's learning journey as evidence of just how difficult it can be to get pedagogy reforms right, and the kind of local adaptation and experimentation that is required. That's exactly what Pratham and J-PAL are currently doing through their TaRL in Africa initiative, piloting various approaches in multiple new countries. That gradualist approach feels wise, rather than asking the minister of finance for a big budget line to rush these programs to scale.

1.6 Conclusion

Return now to our hypothetical meeting where the minister of education is pitching the minister of finance for a budget increase. What can she offer her counterpart as a credible plan to deploy new resources to improve educational outcomes? Recent historical experience, labor market data, and the impact evaluation literature on education programs point to a menu of options.

She can point out that expanded access to basic education has dramatically improved literacy outcomes over the past several decades. Even where the quality of that education leaves much to be desired, the economic returns remain high. From both an aggregate efficiency perspective and an equity perspective, our hypothetical minister can make a strong case for building on these successes at the primary level and pushing this access agenda to the secondary level, especially if she works in a country where secondary (and less commonly, primary) completion rates remain low.

Beyond that access agenda, our fictional minister also has before her a menu of "shovel-ready" investments to improve outcomes for existing students who are being failed by the education system. Granted, the education policy literature is littered with examples of promising programs that fell apart when taken to scale. But that literature also points to a clear set of policies that have demonstrated meaningful impacts at large scale within government systems in low- and middle-income countries.

That agenda consists of policies well within the current capabilities of ministries of education in even the poorest countries: for example, free provision of school meals and extending the school day to increase instructional time. Other options must be treated with caution. Programs like adaptive computer learning have shown big impacts at small scale and, after some teething problems, are showing smaller but still significant impacts at scale. And programs involving remedial education with teaching assistants have shown positive impacts across multiple countries, despite some failures. The crucial research frontier is to identify why the program has scaled up successfully in some contexts and not in others.

Finally, our hypothetical minister will of course need to tailor all of this advice to her specific context, and choose carefully what is and isn't relevant in her country. As we said at the outset, policies such as abolishing user fees, providing school meals, and extending the school day offer feasible alternatives where they are not already in place. But many upper-middle-income countries have already exhausted these alternatives, and must face the more technically challenging task of improving pedagogy and learning rates for a given student population. Importantly, this is also largely true of India, home to about a fifth of the world's extreme poor, where government primary and secondary schools are already free and those schools successfully feed more than 100 million children every day. (That said, national net secondary enrollment in India is only 75 percent, so there may still be relatively low-hanging fruit in terms of an access agenda in some states.)

The following chapters examine various policies on this list in much greater detail, as well as some others that the minister of finance might ask about but should probably be resisted. But the bottom line we hope readers take away is that the last half-century of educational expansion in the developing world has been a roaring success. Countries should look to build on that success, and draw policy lessons from the drivers of recent progress. Rather than accepting an agenda focused exclusively on finding new efficiencies and cost-savings, ministries of education possess solid evidence to argue that more expenditure on some familiar, well-tested spending categories is an investment with large and reliable economic returns.

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Chapter 1 Appendix

Studies included in Figure 1.4

Studies on school meals

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Comment: The pivot from every child in school to every child learning requires a new mind-set

Rukmini Banerji

In the last two decades or more, low-income countries, especially in Asia and Africa, have made tremendous progress in bringing children to school. In almost all countries of the Global South, over time, at least until COVID-19, the average number of years of schooling for a student has been rising. The real challenge now is how to translate years of schooling into years of effective learning.

In the efforts to universalize schooling and ensure learning for all, there have been a wide spectrum of reform initiatives. Looking across these innovations and experiences, what have we learned about what can be scaled up? Why do some scale-ups succeed and why do some scale-ups fail?

But before grappling with the question of what works at scale, it is worth reminding ourselves that education systems are in the middle of a significant shift in priorities. Although many countries know what to do to bring every child into school, every child learning well has been much harder to do. Broadly speaking, expenditure on inputs like school buildings, other infrastructure, teachers, and textbooks works well for increasing access. However, increased learning outcomes are not as straightforward to implement.

The question of children's learning can be roughly thought of in two categories: first, what is to be done in the early years of schooling (for example, grades 1 and 2) so that children can acquire basic learning skills like reading and basic arithmetic early in their educational career? Second, how do we deal with the fact that a majority of children in grades 3 and above are well below expected levels? For those children, foundational skills have to be built before moving to what is expected of them in each grade as per curricular expectations.

Let us take the case of India as an illustration. Only in 2020, with the launch of the New Education Policy, was it acknowledged that many children enter formal schooling with a disadvantage. If a child has not been exposed to any preschool or early childhood education, then she or he arrives in grade 1 without adequate readiness for school. Often, the curriculum for grades 1 and 2 focuses on academic competencies without taking into consideration a breadth of skills that are needed for getting a child ready for school and ready to learn.[1] NIPUN Bharat, or the new Foundational Literacy and Numeracy (FLN) mission, is focusing on how to rework teaching/learning activities in these early grades so that children can have a good start to learning. Moves are also afoot to better link early childhood

1. The breadth of skills refers to cognitive skills, pre-math, and oral language capabilities as well as a range of socio-emotional skills

programs to school initiatives in the early years. In the next few years, as each cohort moves through the school system, we will be able to assess whether these efforts are bearing fruit.

What about the "learning deficit" for children who have been in school for several years but have not acquired the basics? This issue is much harder to tackle. Until the Annual Status of Education Report (ASER) started in 2005, there were no simple assessments or learning outcome data for primary school children that focused on foundational skills. ASER data pointed to several features of the education system in India—children's learning levels were low (a majority were several years behind where they should be), there was wide variation in learning levels in the same grade (whether in the same class or across states), and over time learning trajectories were flat. Acknowledging that there indeed was a "learning crisis" was tantamount to accepting that "business as usual" had not worked effectively for most children until now.

Why were children not able to reach grade-level expectations? Several explanations are possible—one set of explanations included possibilities such as teachers not equipped to teach effectively, children not ready for learning, and families not able to support children's learning. But another explanation would point to the curriculum. Can the situation that we see today be interpreted as a "negative consequence of overambitious curriculum" or a result of "teaching to the top of the class"? If so, any effort to improve learning outcomes must focus not on an age-grade syllabus but on Teaching at the Right Level (TaRL). In fact, it could be the one-size-fits-all curriculum often prescribed for the entire country that is the real problem.

Coming back to the difficulties of embedding TaRL into government systems in India, it could be the case that it has been a failure of changing mind-sets (away from syllabus). Even after a large-scale TaRL effort is undertaken, the system returns quickly to its age-grade curriculum as an anchor or as a security blanket. Undoubtedly, implementation could be and can be better on many counts, but if the actual problem has not been understood properly and accepted, initiatives such as TaRL cannot be successful.

Moving away from the specific case of TaRL or the context of India, there are several other points to think about in the relationship between research and evidence on the one hand and policy and practice on the other. Context and conditions matter for any implementation, but so do the starting point and who fuels the change. Did the original experiment start small (and was it designed for that scale)? Later, after a successful demonstration, was there an effort to scale it up? What would happen if the original scale were large and with each round of implementation, improvements and adjustments were made? In low-income countries and within centralized education systems, is there internal experimentation capability for iteration? Over time are such countries investing in adaptive capability? Or will the evidence and innovations continue to come from researchers and consultants and donor agencies?

A final comment: The appropriate methodology for meeting acceptable standards of rigorous evidence is costly, and the skills for carrying out such studies often lie with researchers in western countries. Do both of those conditions constrain the scale at which evidence can be generated? Are alternative methods being developed that can study a system at scale and provide key inputs for improvement? For Pratham and TaRL in India, we have benefited hugely from a series of randomized controlled trials done in partnership with J-PAL over two decades. These studies were layered onto a long-run effort for improving basic learning outcomes that continuously tweaked and adapted different features of the approach and implemented it in many different conditions and contexts. Instead of being seen as a one-off research study that may provide input for a large-scale implementation, the ongoing efforts—research and implementation—together represent an ongoing endeavor to improve children's futures. Perhaps that is the missing element for strengthening scaling up initiatives.

Comment: Scalability cannot be the sole criterion for policy decision making

Moses Oketch

Crawfurd, Hares, and Sandefur raise several important issues related to what works at scale in terms of education policy and interventions. My comments here focus on two key issues, of relevance to low- and lower-middle-income countries: (1) the need for renewed attention to the unfinished business of access and (2) the complexity of scaling pedagogic reforms shown to work in the pilot phase.

First, concerning the need for renewed attention to the unfinished business of access. The focus on learning instead of schooling is central to realizing Sustainable Development Goal 4. Without learning, there is little point in going to school; without children going to school, it is nearly impossible to organize formal learning at scale. Parents expect schooling to lead to learning. However, the "learning crisis" changes the terms of the debate since it is largely presented and interpreted as an argument and movement that is against continued expansion in enrollment and attainment. As Pritchett and Sandefur (2020) have noted, it will require both universal schooling and a dramatic improvement in learning profiles to achieve the SDG targets. However, dramatic improvement in learning is not going to happen easily and analysis comparing countries' learning profiles does not show decisively what works, but the clear message in the learning crisis movement is that schooling itself without evidence that it also generates sufficient learning is not good enough. In order to dramatically improve learning, Crouch, Kaffenberger, and Savage (2021) have argued that there is a need to focus on systems improvement, and to use foundational learning as the guiding principle to ratchet up learning. However, Crawfurd, Hares, and Sandefur challenge that view, at least in low- and lower-middle-income countries with space to expand access. They note that pedagogy reforms can be difficult to implement and there is not enough evidence of their scalability in government school systems. Expecting imperfect government systems to dramatically improve learning has proven to be a tall order. So, they argue that expanding access, providing school meals, and extending school time are policies that have worked at scale, and focusing on these is an actionable agenda that can raise learning outcomes in the near time. Their main message is that education pays even where schooling does not generate stellar learning outcomes; therefore, from an economic standpoint, even rudimentary schooling may be a very good investment. The main theme of their argument is that scalability of a program has to be critical when interventions are presented to a minister of finance. While this is a reasonable argument, it is also very narrow. I would argue that scalability cannot be the sole criterion for policy decision making, although it may be one of the considerations.

Second, concerning the complexity of pedagogic reforms and scalability, there is general agreement that learning needs to improve dramatically in low- and lower-middle-income countries, and even Crawfurd, Hares, and Sandefur do not argue against this. Most commonly for low- and lower-middle-income countries, there is agreement that something must be done about the learning crisis. The success in schooling should not be allowed to go to waste. Thus, I would argue that the choice of Type A policies—those that are more effective when well implemented but require highly skilled staff (these policies include structured pedagogy, teacher coaching, or home visits for early child development)—versus Type B policies—those that might be less effective when implemented in terms of improving learning but are more robust to weak implementation (these include school-building, lengthening the school day, or providing school meals)—is a false choice. Instead, both Type A and Type B policies are needed in a country, as addressing the learning crisis requires education system improvement. I have argued elsewhere (Oketch 2019), as Crawfurd, Hares, and Sandefur do in their chapter, that measures of performance, efficiency, and effectiveness often embedded and dominant in randomized control trial (RCT) studies do not provide explanations of how and why an education system "is where it is" or of "what works" to improve it. But, I would also argue that RCT-based studies may offer some insight at small scale on potential mechanisms of change, which can help to identify where the "blockage" lies at the macro level, even when these pilots have not proven scalable. So, while scalability may be a useful criterion for policies and indeed critical, there are still many systems-improving lessons that can be learnt from pilots and projects that haven't proven scalable. What I would argue against, and where I agree with Crawfurd, Hares, and Sandefur's argument, is that RCTs and pilot projects that have demonstrated success should not crowd out those programs that have already shown success when rolled out at scale, and a gradualist approach rather than a big bang rollout of pedagogy reforms might be a wise approach to present to a minister of finance. It is not obvious that systems that have improved learning have done so through relying on pilots, but they have certainly learnt gradually or in an evolutionary manner.

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Chapter 2. Feed All the Kids

Feeding kids may look expensive by standard value-for-money metrics, but it promotes equity in outcomes beyond just test scores.

Biniam Bedasso

The central appeal of school feeding is that it allows policymakers to target multiple objectives with a relatively straightforward and scalable intervention delivered on school premises. A review of 11 experimental and quasi-experimental studies from low- and middle-income countries reveals that school feeding contributes to better learning outcomes at the same time as it keeps vulnerable children in school and improves gender equity in education. Although school feeding might appear cost-ineffective compared with specialized education or social protection interventions, the economies of scope it generates are likely to make it a worthwhile investment particularly in food-insecure areas. Given the existing gap in coverage in low-income countries, financing might be a key constraint in the very places where school feeding tends to have the largest marginal returns.

2.1 Introduction

Ensuring that children who would otherwise go hungry or be undernourished are fed is a noble cause in its own right. As such, the most important question is not whether school feeding is desirable as an end in itself. There is already evidence that school feeding can have significant long-term benefits in the form of lifetime income particularly for children from poor households exposed to such programs for a reasonably long period (Lundborg, Rooth, and Alex-Petersen 2021). The material question, particularly for education policymakers, is whether the educational benefits of school feeding are strong enough to justify its existence as a separate intervention instead of (or on top of) specialized interventions such as cash transfers that might be better positioned to achieve the social protection and nutrition objectives. This is often followed by a question on the cost-effectiveness of school feeding programs vis-à-vis comparable interventions aiming to achieve the multiple objectives of school feeding jointly or separately.

This chapter reviews the existing evidence on the impact of school feeding on education in low- and middle-income countries with particular attention to learning outcomes. It also attempts to put the discussion in the context of the range of policy objectives countries are actually trying to achieve through school feeding. Additionally, the chapter also looks at the fledgling evidence base on the cost-effectiveness of school feeding programs. Finally, it discusses the more practical aspects of scalability, financing, and implementation capacity.

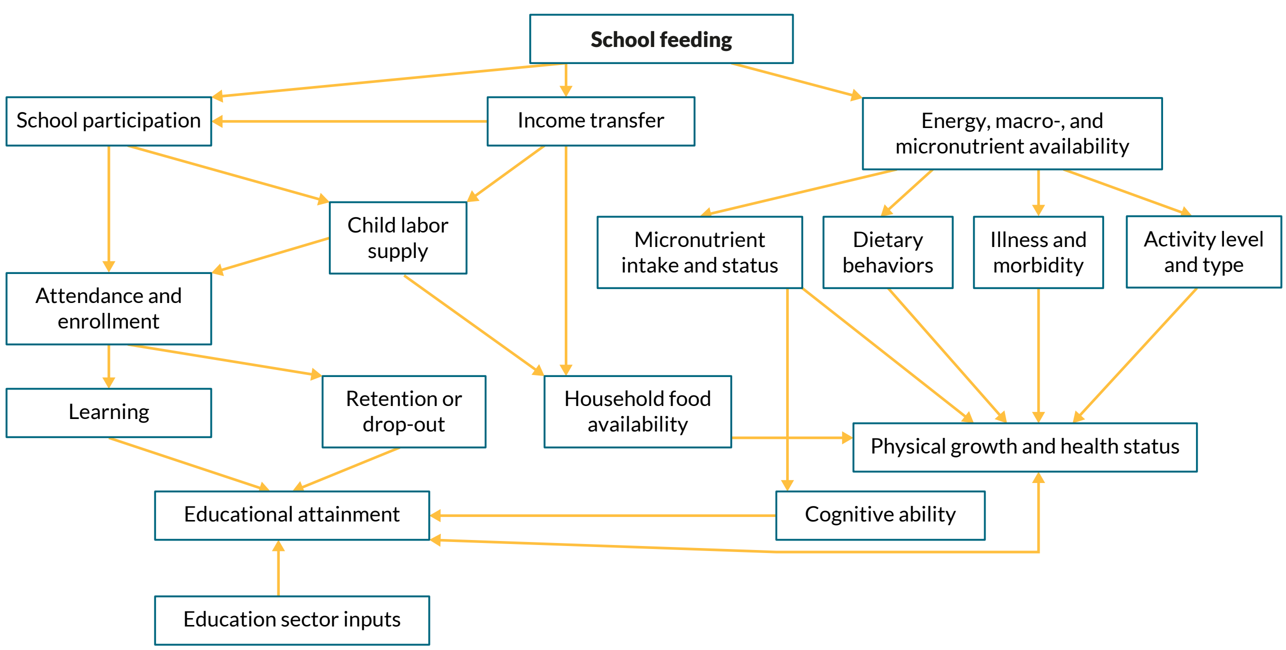
2.2 Countries invest in school feeding programs to achieve multiple objectives

School feeding programs have emerged as one of the most common social policy interventions in a wide range of developing countries over the past few decades. Before the disruptions caused by the COVID-19 pandemic, nearly half the world's schoolchildren, about 388 million, received a meal at school every day (WFP 2020). As such, school feeding is regarded as the most ubiquitous instrument of social protection in the world employed by developing and developed countries alike. But school feeding is also a human capital development tool. The theory of change for school feeding programs is rooted in the synergistic relationship between childhood nutrition, health, and education underscored in the integrated human capital approach (Schultz 1997). The stock of human capital acquired as an adult—a key determinant of productivity—is supposed to be a function of the individual and interactive effects of schooling, nutrition, health, and mobility. There is also a moral argument in favor of instituting school feeding programs predicated on a rights-based approach regardless of the economic returns of public spending in this area (see, for example, FAO 2019).

There are multiple channels through which school feeding programs can potentially affect individual human capital development and aggregate welfare outcomes. Figure 2.1 maps various ways through which school feeding contributes to outcomes in education, health, and social protection. Beyond the primary objectives in those areas, the secondary objectives of school feeding programs could include broader goals such as stimulating local agriculture. In this context, policymakers could aim to achieve multiple social policy objectives through the provision of school meals. As such, it is important to have perspective on the stated objectives governments of developing countries are trying to achieve with such programs.

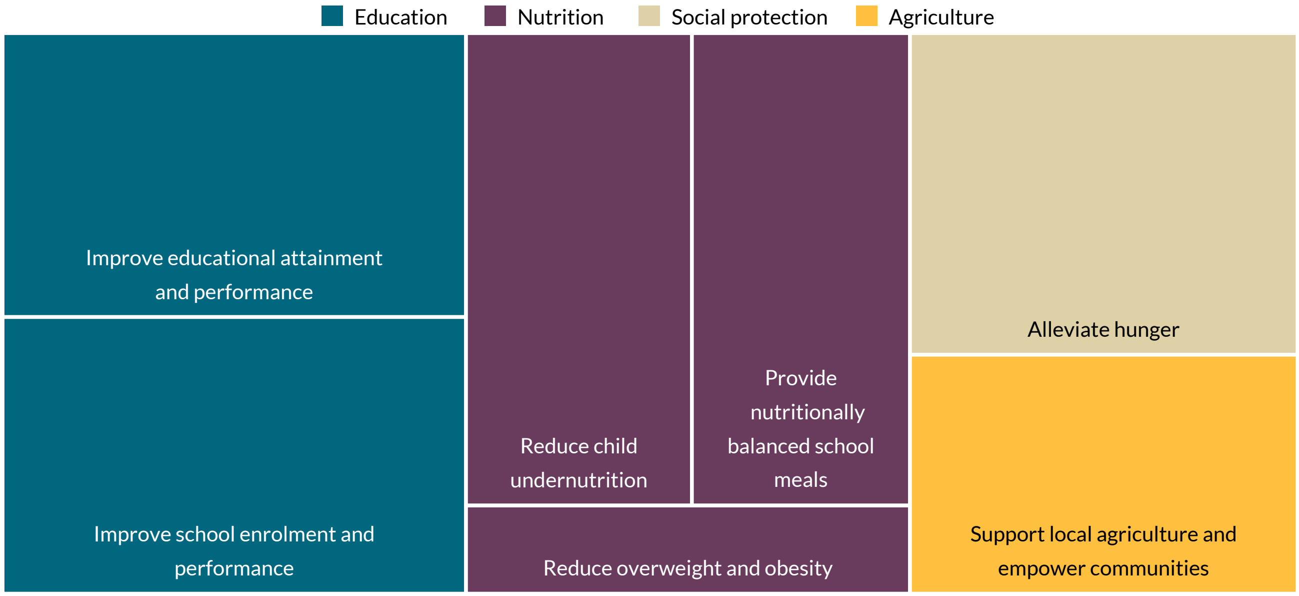
A survey of government officials in 41 Sub-Saharan Africa countries conducted by the Food and Agriculture Organization of the United Nations (FAO) in 2016 shows that a great majority of the countries implement school feeding programs to help achieve education objectives. Figure 2.2 shows the composition of objectives identified by interviewed policymakers. Among 11 objectives organized under four major themes, improving educational attainment and performance stands out as the single most common objective identified by all but one country. Improving school enrollment and performance is second with 36 out of 38 countries identifying it as an objective they are trying to achieve with school meals. This means policymakers aim to address both

Figure 2.1. School feeding pathways to shaping child and adolescent development



Source: Drake et al. (2017).

Figure 2.2. Policy objectives of school feeding programs in 38 African countries



Source: Author's compilation based on FAO (2018).

Note: The multicolored blocks represent stated objectives for school feeding programs under four main themes. The size of the blocks is proportional to the number of countries that have identified that particular objective.

enrollment and attainment through school feeding programs. Alleviating hunger follows the education objectives as the third most prominent objective and is representative of the social protection side of the policy outcomes. Of the nutrition objectives, reducing child undernutrition is the most popular, identified by 30 countries, as opposed to reducing overweight and obesity, which is mentioned by only 11 countries.

A more recent and larger survey of officials in 85 countries around the world also finds that 93 percent of respondent countries have designed school meal programs to meet education goals (GCNF 2019). Next to education, nutrition and/or health goals are cited by 88 percent of countries as the objectives school meal programs are intended to meet. Finally, 73 percent of countries consider social protection goals as one of the reasons to institute such programs.

2.3 School feeding keeps children in school and helps them learn better

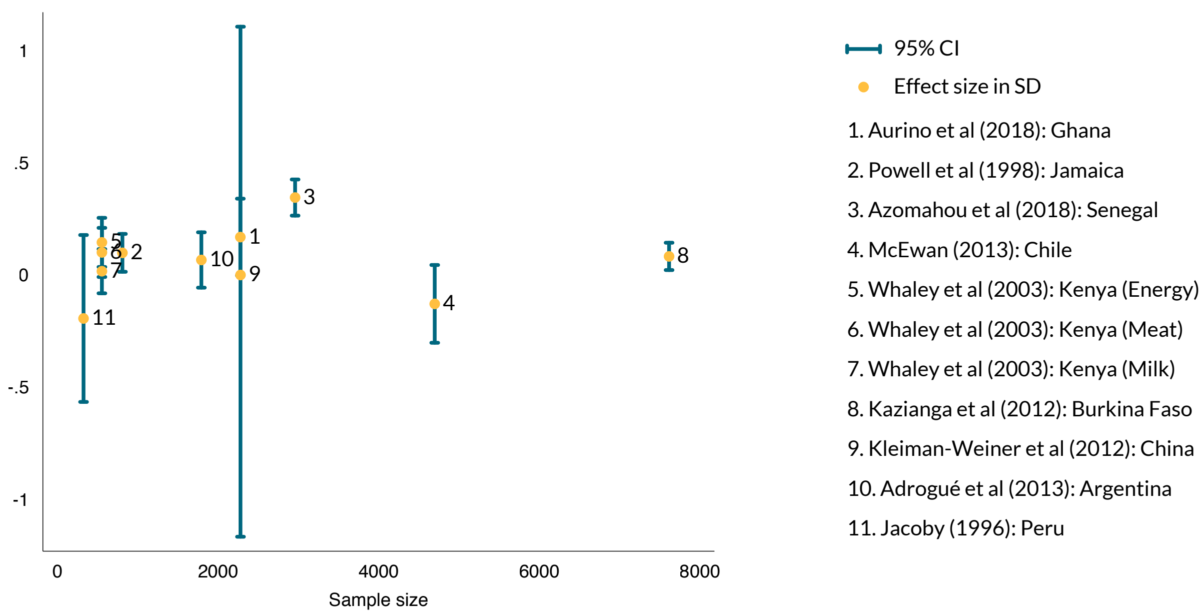
A growing body of empirical research and program evaluation endeavors to document the impact of school feeding programs on education as well as nutrition and health outcomes of beneficiaries. Needless to say, the impact of school meals on both access and final outcome variables is mixed. A recent synthesis of program evaluations shows that the strongest positive impact of school feeding is on school enrollment in food-insecure areas (UNESCO 2019). Based on a review of the 20 evaluations included in the UNESCO synthesis, enrollment is an outcome in which the most consistent positive effect across different contexts is registered. Not surprisingly, the marginal impact of school feeding interventions on school participation is lower where enrollment rates are already high.

Impacts on learning outcomes

Ultimately, higher levels of school participation are expected to contribute to better final outcomes including learning. Conceptually, the impact of school meals on learning operates through both improvements in school attendance and better learning efficiency while in school (Adelman, Gilligan, and Lehrer 2008). Recent decades have seen more rigorous evidence on the impact of school feeding programs on learning outcomes in developing countries. Bashir et al. (2018) reviewed the available evidence comparing the relative impacts of 15 types of interventions. The results show that school feeding interventions have a moderate effect on learning outcomes when all low- and middle-income countries are considered. However, in the context of Sub-Saharan Africa countries, the effect of school feeding is surpassed only by structured pedagogy and extra time. In the realm of social protection interventions that also serve the purpose of promoting schooling, school feeding outperforms cash transfers across the board in terms of improving learning outcomes (Bashir et al. 2018). This may not come as a surprise given that cash transfer programs "may relax an economic constraint to access for the children but do not directly affect the learning process beyond that" (Evans and Acosta 2021, 14–15).

Figure 2.3 displays a compilation of the effect sizes on math scores of school feeding interventions in a cross section of low- and middle-income countries implemented at various points during the past three decades. The inclusion criteria for the review are as follows: (1) the study uses experimental (randomized control trial) or quasi-experimental (controlled, before-and-after) methodology; (2) the country was classified as a low- or middle-income country at the time of the intervention; (3) the intervention consists of providing school meals (as opposed to supplements or take-home rations); and (4) math scores are measured as one of the outcome variables. There is significant heterogeneity in both the magnitude and statistical precision of the effects of interventions. Different types of food items may have diverging impacts as shown in the case of the various arms of the trial in Kenya (Whaley et al. 2003). But there appears to be no correlation between effect size or statistical significance and sample size. This could be interpreted as suggesting that there is no indication of effectiveness of interventions diminishing with scale.

Figure 2.3. Compilation of effect sizes of school feeding on math scores



Source: Author's compilation.

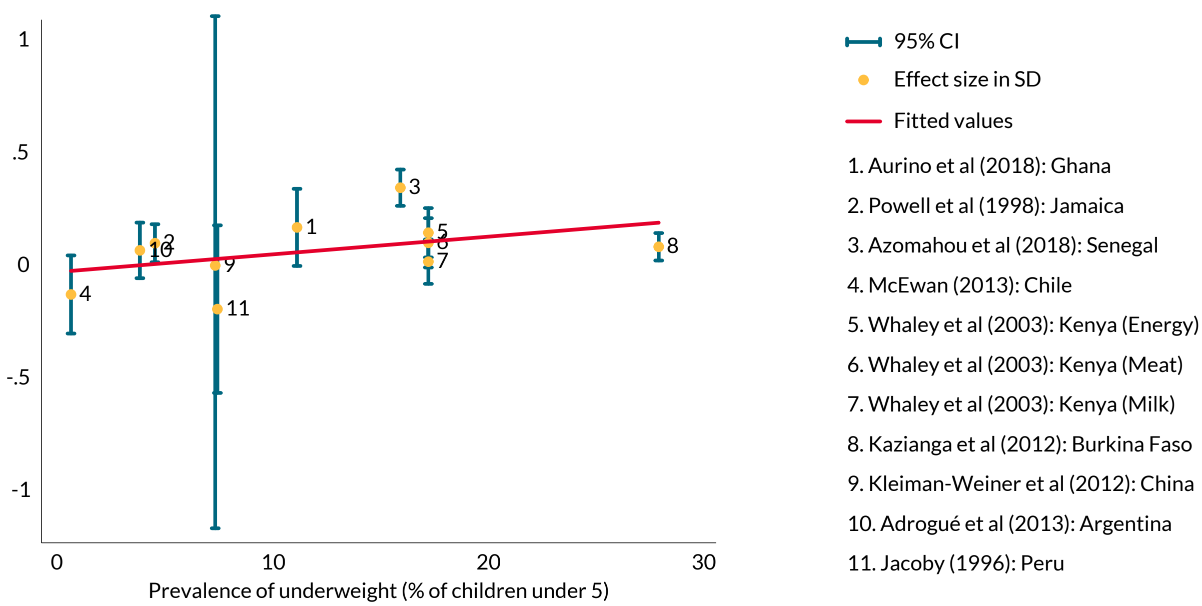
School feeding programs are in operation across the world from advanced countries to low-income and fragile states. As such, the objectives high-income countries intend to achieve with school meal programs can be different from those low-income countries set as a priority. Food-insecure countries in particular may be able to use these interventions to improve a number of outcomes in nutrition, health, and education. Figure 2.4 shows that there is a slight correlation between the percentage of under-5-year-olds who are underweight, a measure that can serve as a proxy for overall food insecurity, and the effect of school feeding programs on math scores. This could mean that school meals have a higher impact on learning in places where children are more likely to come to school hungry.

Learning gains caused by interventions such as school feeding may need some time to accumulate. Unfortunately, many evaluations, no matter how methodologically sound, do not cover a time period long enough to capture the cumulative gains in learning outcomes. Among the few studies that consider the time dimension of school feeding interventions, Chakraborty and Jayaraman (2019) find that "children with up to five years of exposure have reading test scores that are 18 percent higher, and math test scores that are 9 percent higher than students with less than a year of exposure" (250).

Impacts on girls and vulnerable populations

The theory of change of school feeding interventions implies that the impact on education outcomes is likely to be heterogeneous across various groups with larger effects expected on groups that would otherwise be excluded from schooling. As such, a synthesis of 20 evaluations reveals that school feeding programs are particularly impactful in improving the school participation of girls and overall enrollment in areas of high food insecurity and among a population of internally displaced people/refugees (UNESCO 2019). In poor communities where households may face a trade-off between sending girls to school and having them participate in household food production, take-home rations could be the deciding factor incentivizing parents to enroll girls in school, as Nikiema (2019) shows is the case in Burkina Faso.

Figure 2.4. Correlation between food insecurity and the effect of school feeding on math scores



Source: Author's compilation.

Note: Data on prevalence of underweight are for the year the study was conducted or are an extrapolation based on the closest years for which data are available.

To the extent that school feeding increases girls' school participation, it can also help to improve their learning outcomes. Recent studies show that girls can benefit more from school feeding programs than boys in terms of achieving higher test scores even though the evidence is more mixed than in the case of school participation. Aurino et al. (2018) report larger gains for girls in a nationwide school feeding program in Ghana. In Senegal, the impact of school meal interventions on girls' math scores is larger than on boys' scores, whereas the effect on French scores is larger for boys (Azomahou, Diagne, and Diallo 2019). A potential reason for the larger impacts on girls is that school feeding may induce steeper declines in the marginal opportunity costs of human capital investments for girls than for boys (Aurino et al. 2018).

The benefit of school feeding programs in keeping children in school in food-insecure areas can be further accentuated in the presence of systemic shocks such as droughts. For example, in Kenya, a randomized trial showed that the negative effect of a drought on school attendance was greater in schools that did not have school feeding programs than those that did (Omwami, Neumann, and Bwibo 2011).

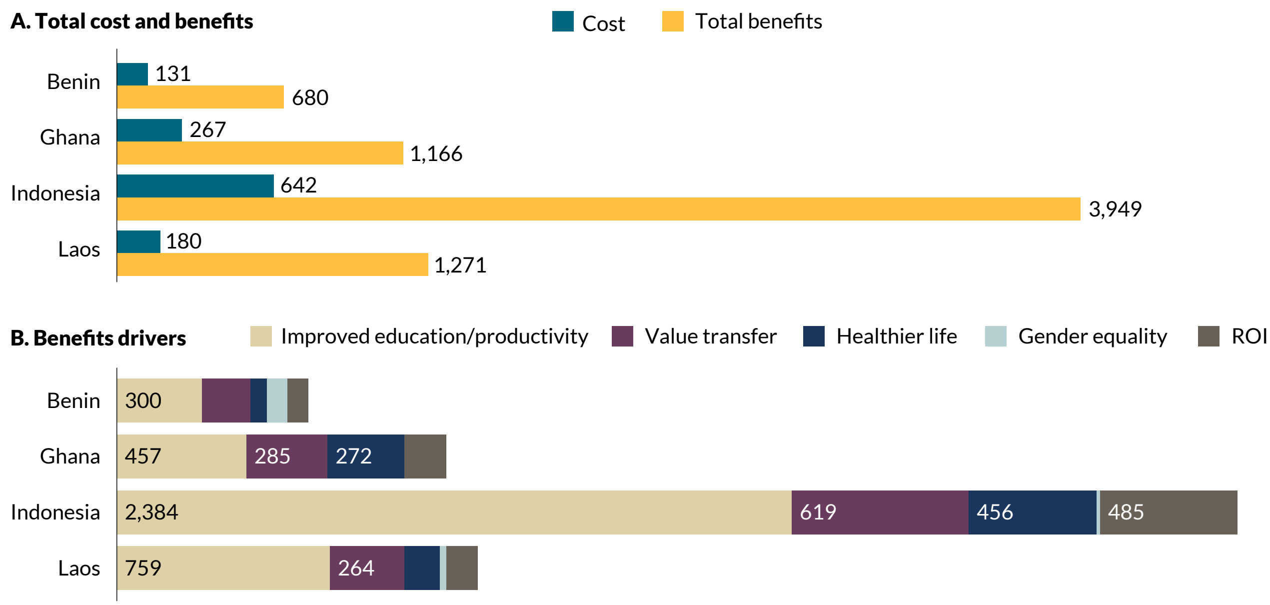
2.4 School feeding is cost-effective particularly in improving the educational outcomes of girls and children in food-insecure areas

In the presence of competing interventions that might potentially achieve comparable education outcomes, cost-effectiveness is key to picking a program that could lead to the best possible welfare gain. In this regard, the most convincing analysis would be based on side-by-side randomized field experiments of alternative programs (Adelman, Gilligan, and Lehrer 2008). Cost-effectiveness analysis is rather pertinent in the context of countries transitioning from dedicated external support from organizations such as the World Food Programme (WFP) to funding school feeding on budget (where resources are more fungible). Unfortunately, it is often difficult to obtain the requisite data to conduct basic cost-effectiveness analysis let alone full-fledged cost–benefit analysis that requires estimating the multidimensional benefits of school feeding interventions. However, there is an emerging effort to integrate rigorous cost-effectiveness exercises into the evaluation of school feeding interventions.

In analyzing the cost–benefit structure of school feeding programs in 14 countries, Verguet et al. (2020) attempt to explicitly compute the estimated benefits of interventions in a range of domains including education, health, social protection, and local agriculture. The main finding of the study is that "the overall benefit-cost ratio of school feeding programs could vary between 7 and 35, with particular sensitivity to the value of local wages" (1). In other words, a 1-dollar investment in school feeding could generate 7 dollars in combined human capital returns in the country with the lowest relative returns (of the 14 included in the study), whereas the return could be as high as 35 dollars in the case of the country with the highest relative returns. However, the study does not account for the joint effects of the education and health components in a manner that would allow cost-effectiveness comparison with specialized education and nutrition programs that are more narrowly targeted at specific objectives as suggested by Adelman, Gilligan, and Lehrer (2008). Moreover, the high sensitivity of the results to wage assumptions (considering that the benefit-to-cost ratio could decrease to as low as 3 to 1 with lower wages assigned) reduces the reliability of the results to aid allocation decisions.

The WFP and the Mastercard Foundation have led a more promising effort to perform rigorous cost–benefit analysis in selected countries using a common framework. Figure 2.5 presents the key figures compiled from cost–benefit analysis in four countries (Benin, Ghana, Indonesia, and Lao PDR). Overall, the benefit-to-cost ratio ranges between 4.3 in Ghana and 7.1 in Laos. The cost drivers considered in calculating total costs include cost of food, transport cost, personnel cost, cooking group incentives, overheads, and

Figure 2.5. Cost–benefit analysis of school feeding programs: average value per beneficiary (in US dollars)



Source: Author's compilation from various country reports.

Note: ROI = return on investment.

management cost. On the other hand, the benefits of school feeding consist of returns to lifetime productivity improvements due to greater school attendance and learning, reduced public and private health expenditures due to nutrition and health benefits, monetary value of food transferred, spillovers from better participation of women in education and the economy, and returns on investment on freed-up funds. In all countries, the highest contribution to total benefits comes from improved education and increased productivity. That component alone accounts for half of the benefits on average, whereas value transfer (which represents the social protection component) contributes an average of 21 percent.

Ideally, comparison of the cost-effectiveness of school feeding programs against alternative interventions is needed to facilitate informed policy choices. An analysis by Turkson, Baffour, and Wong (2020) represents a recent example of such an exercise juxtaposing the cost-effectiveness of school feeding and Teaching at the Right Level (TaRL) interventions in Ghana. The study concludes that, although there may be strong equity arguments for favoring school feeding that benefits girls and children from poor backgrounds more, broad-based TaRL is likely a more effective use of resources as it produces a benefit–cost ratio of 8 as opposed to 5 in the case of school feeding.

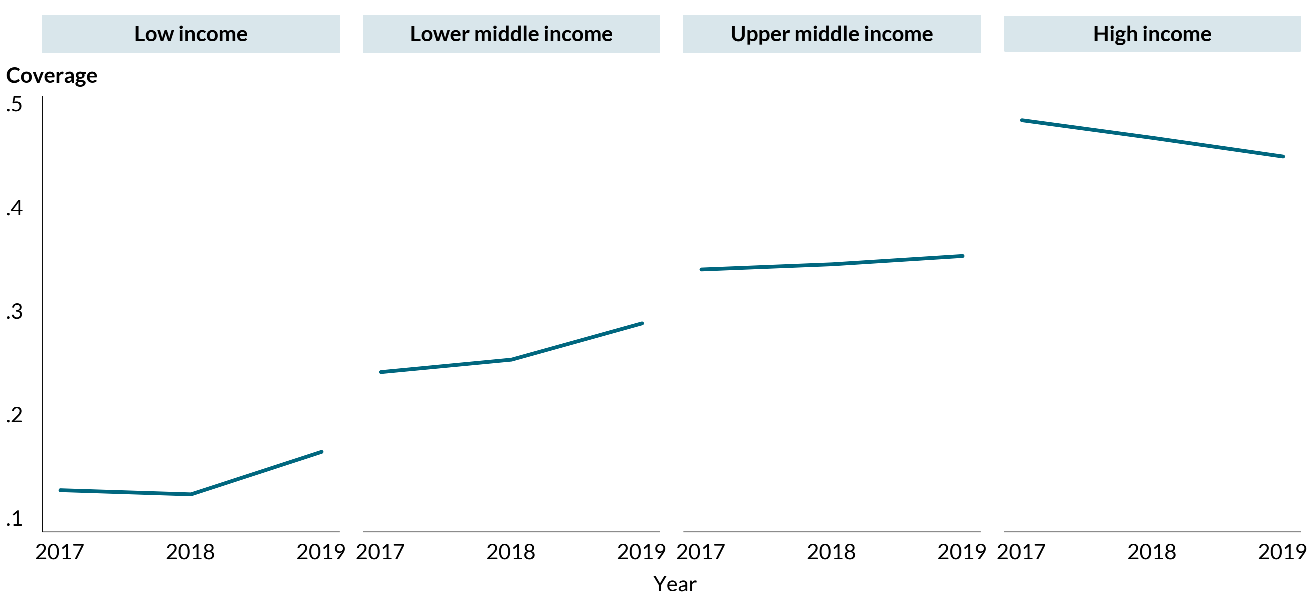
The shortcoming of the analysis comparing school feeding and TaRL is that it does not explicitly account for the equity implications of school feeding despite acknowledging the presence of an efficiency–equity trade-off. In contrast, the WFP studies attempt to factor in the positive externalities generated through improved gender equity. Moreover, traditional cost–benefit analysis is generally criticized for failing to take into account the disproportionate welfare gains of poor and disadvantaged individuals from a given intervention vis-à-vis wealthy and privileged individuals. In this regard, a modified cost–benefit analysis that reflects the higher marginal utility of groups who would otherwise be excluded from schooling without school meals could facilitate a more accurate appraisal of the cost-effectiveness of school feeding programs. This means school feeding could still be comparatively more cost-effective than TaRL once the equity implications are incorporated since its benefits are concentrated among girls and vulnerable children.

2.5 Despite scope to scale up school feeding in low-income countries, financing and lack of oversight could be key challenges

The 2019 Global Child Nutrition Foundation (GCNF) survey finds that 67 of the 85 countries that have responded to the survey have some kind of school feeding program in place. Not surprisingly, the coverage, types, and funding modalities of the programs vary widely from country to country. While some countries have had decades of experience of running large-scale school feeding programs, others are only starting to roll out such programs in certain parts of their territory. Geographically, school feeding programs are relatively evenly distributed across most regions of the world. However, there is significant disparity in terms of the level of coverage between different income groups of countries. Figure 2.6 shows that the average coverage of school feeding in low-income countries stands at 17 percent whereas the corresponding number for high-income countries is 45 percent. This means there is potentially significant unmet need for school meals in many places where it might have high marginal returns.

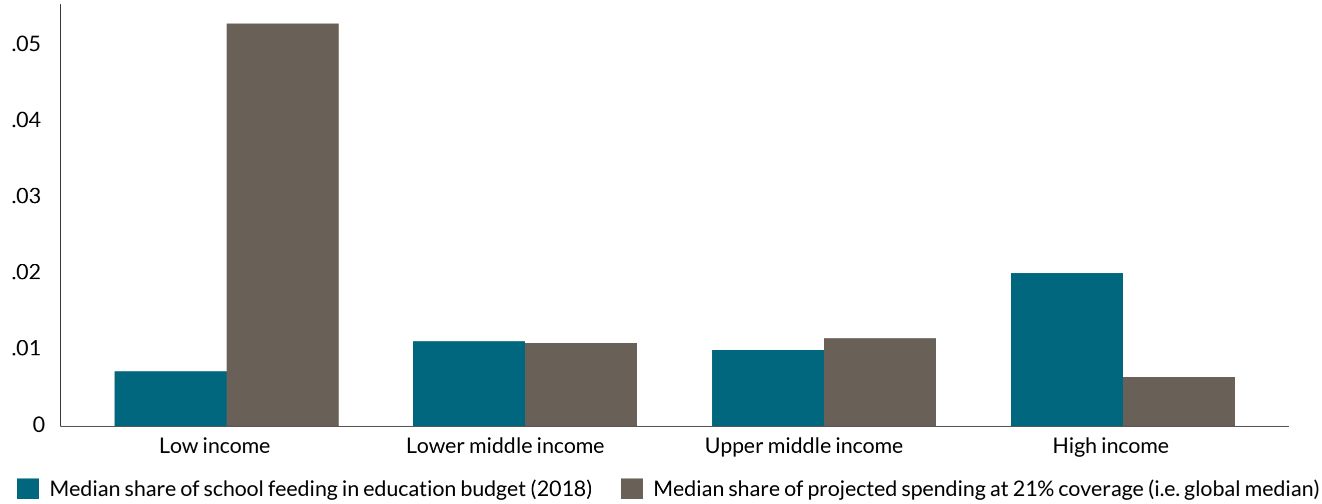
Figure 2.6 also shows that the highest year-over-year increase in the coverage of school feeding programs between 2018 and 2019 took place in low-income countries. Much of that is because many of those countries started from a low base. However, this is a good indication that state capacity might not be as significant a constraint as it is in some other interventions to scale up school feeding programs. Taken together with the evidence in Figure 2.3 showing that effect size with respect to learning outcomes does not decline with sample size, this can be viewed as providing support for the scalability of school feeding programs in countries where they may have the highest impact.

Figure 2.6. Trends in the coverage of school feeding across countries in various income groups



Source: Author's computation based on GCNF (2019).

Figure 2.7. Actual and projected share of school feeding in education budgets



Source: Author's computation based on GCNF (2019).

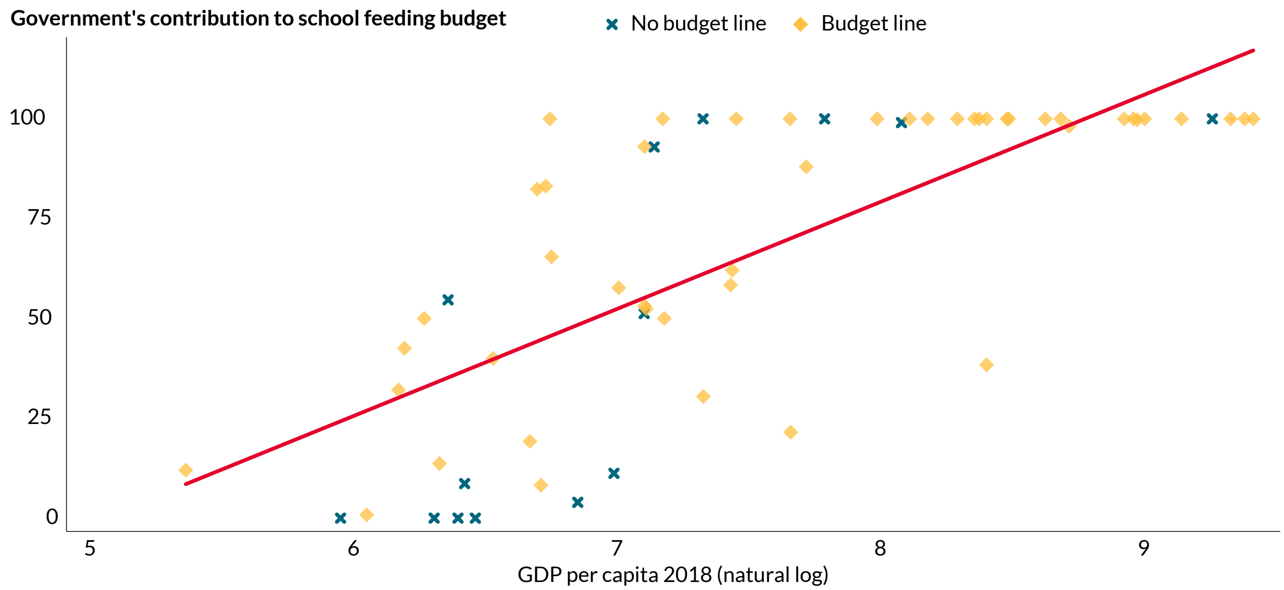
Note: Projections are based on the assumption that unit cost, overall education budget, and nongovernment share of school feeding spending remain constant.

Assuming that scaling is cost-effective and technically feasible, the next question would be whether it is financially viable to undertake significant expansion in countries that may benefit from it. As it stands, the median country in the GCNF sample spends just around 1 percent of its education budget on school feeding. Figure 2.7 shows that the median spending among low-income countries is 0.7 percent of education expenditure, whereas the corresponding figure for high-income countries is 2 percent. This means low-income countries may still have some budgetary space to increase school feeding spending. However, due to the existing gap in coverage, raising the scale of school feeding programs in low-income countries by a significant amount could eat into the budget quite rapidly. For instance, if one were to increase coverage in low-income countries to the current global median, that is, 21.3 percent, it would amount to committing 5 percent of the education budget in the median country to school feeding. This could imply that, in the short run, scaling in low-income countries should be carefully targeted in order to be fiscally tenable.

In most low-income countries international donors were or still are a significant contributor to the school feeding budget. Figure 2.8 shows that there is some correlation between the share of a government's contribution in the school feeding budget and per capita income. However, the contributions of governments appear to increase quickly with income with a number of lower-middle-income countries already covering 100 percent of their school feeding budget. Three-quarters of the sample countries have a dedicated line for schooling feeding in their national budget, which may facilitate transparency as well as sustainability. Not surprisingly, the higher the share of a government's contribution to the program, the more likely it is to have a line in the budget dedicated to school feeding.

Apart from financing, frontline capacity for consistent delivery of school meals and oversight capacity by supervising bodies are critical for the quality and sustainability of school feeding programs. Although clearly defining responsibilities at every level and setting performance parameters are important, enforcement of de jure rules and procedures might sometimes be overshadowed by de facto power relations and structural constraints, as demonstrated by the Mali case study presented in Box 2.1. Drawing on case studies from 14 countries, Drake et al. (2016) conclude that, in general, "there is strong political will to continue to fund school feeding and to expand programs further,

Figure 2.8. Government funding of school feeding programs



Source: Author's computation based on GCNF (2019) data.

Note: GDP = gross domestic product.

as far as possible through national funds" (xlvi). After all, school feeding is likely to have better electoral benefits to the incumbent than most other educational interventions. However, the relatively high demands on decentralized procurement and monitoring capabilities, which are often deficient in low-income countries, pose a challenge for the execution of school feeding programs.

2.6 Conclusion

We show in the chapter that policymakers in low- and middle-income countries design school feeding programs with the aim of achieving multiple social policy objectives. Survey data reveal that increasing school participation and improving education outcomes are the most common objectives policymakers cite. The strongest causal evidence with regard to the effect of school feeding on education pertains to the impact on school enrollment and attendance. But we have reviewed a set of evaluations from a range of countries that collectively show a non-negligible impact of school feeding on learning outcomes, particularly math scores. There is also suggestive evidence that such an effect might be stronger in more food-insecure countries. A nascent body of work based on multicountry impact evaluation shows that school feeding is highly cost-effective with the bulk of the benefits coming from improved educational outcomes and future productivity. There is significant scope to scale up school feeding in low-income countries where it might exert the most impact. However, the lack of fiscal space may entail careful targeting in the short run. A great majority of low- and middle-income countries have taken a step toward sustainability by incorporating school feeding into their national budgets. However, sustainability requires strong coordination and monitoring capabilities across all tiers of government and implementing parties including schools.

Box 2.1. The pitfalls of implementation: A case study of Mali's school feeding system

Chapter 1 noted that one advantage of school feeding as a policy option is that it scales easily: whereas more complex pedagogy programs have often seen diminishing effects as they expand their scope, even states with limited implementation capacity have run successful, large-scale school feeding programs. But things do not always go smoothly.

The government of Mali runs a national school feeding program alongside parallel programs run by the WFP and Catholic Relief Services. As of 2017, 1,183 schools in food-insecure and low-enrollment regions were targeted out of a total of 19,082 schools. The schools are located in 325 of Mali's 703 municipalities. The government allocated 2.4 billion CFA francs toward the school canteens program in the 2017 fiscal year. However, a monitoring mission uncovered that more than a third of the schools entitled to school feeding did not receive any funding. Those schools are spread across 128 municipalities, indicating that the challenge of budget execution was not isolated to a few areas. This had prompted the Ministry of National Education and the Ministry of Economy and Finance to freeze all funds allocated to school feeding in order to verify the effectiveness of the canteens established in each of the municipalities.

However, the problem has worsened in recent years with recurring political instability and economic downturn. In 2019, only 44 percent of school canteens received funds while 95 percent of appropriations were executed. At the time of budget monitoring, 42 percent (546 out of 1,301) of canteens were functioning, but only about 9 percent (115 out of 1,301) were providing the mandated five meals per student a week.

At the center of the implementation challenge is the undue discretion of authorizing officers in local administrations who often try to retain the management of the canteen funds contrary to the spirit of the inter-ministerial order that entrusts this responsibility to school management committees. An exercise to further deconstruct the root causes of the problem has revealed that the following issues have contributed to the irregularities and lack of efficiency in budget execution:

- a lack of whistleblowing on bad practices of some mayors involved in managing school canteen resources and by school management committees, school principals, and the community;
- inadequate training of school management committees on their roles and responsibilities in the management of school canteens;
- administrative officials (managers) who are not deeply involved in monitoring resources at the local level; and
- an inadequate procedures manual for managing school canteen funds.

Source: Collaborative Africa Budget Reform Initiative.

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Comment: Multifaceted benefits make school meals cost-effective, but more evidence is needed on long-term gains

Farzana Afridi

Historically, the immediate objective of school meal programs has been to improve child nutrition and health. This holds for both poor as well as higher-income countries. However, it is imperative to understand how and whether such programs can affect learning, since educational attainment has longer-term implications for labor market outcomes of children as they transition to adulthood.

To put the review's overarching objective in perspective, it would be worthwhile to first outline the pathways through which school meals can improve learning outcomes:

Direct impact: First, as a conditional transfer program, school meals can improve school participation (both enrollment and regular attendance), particularly of children in poor households and girls. Coming to school regularly may itself increase learning, conditional on some minimum quality of teaching. Second, school meal programs directly impact child nutrition through potentially higher nutrient consumption in low-income settings. Research shows that households are not purely altruistic in making resource allocation decisions, and hence targeted transfers such as school meals have a significantly higher marginal impact on children's daily nutrition than transfers to households (e.g., Afridi 2010 on India; Jacoby 2002 on the Philippines; Kooreman 2000 on the labeling effect of targeted transfers). This can have an impact on child cognition, especially when malnourishment is widespread and school meals are also fortified with nutrients to address micronutrient deficiency, such as anemia, which is often prevalent in low-income countries (Berry et al. 2020). Both school participation and nutritional improvements have short- and long-term implications on learning.

In the short term, meals provided during school hours can improve classroom attention, by reducing classroom hunger (Afridi, Barooah, and Somanathan 2019 on India). This was the rationale for introducing the breakfast program in the United States (Bhattacharya, Currie, and Haider 2006), but it is also relevant in poor and middle-income countries. Indeed, there is credible and strong evidence, from multiple contexts, of the positive effects of school meals on children's school participation, nutrition, and health. Surprisingly, while these first-order effects have been studied intensely, there is much, much less evidence on the short- and long-term learning impacts.

Indirect impact through externalities or spillover effects: Provision of school meals is a conditional, in-kind transfer program that reduces food insecurity in agrarian and poor economies (Singh, Park, and Dercon 2014 on India). Hence, the potential income effect on beneficiary households can also impact the educational outcomes of the child. Second, provision of school meals can also reduce disruptive behavior of peers who often come to school on empty stomachs, and have spillover effects, generating externalities that would suggest existing estimated impacts on classroom learning could be downward biased (e.g., see Kremer and Miguel 2004 on deworming in Kenya).

While there is evidence on reduction in food insecurity and household consumption smoothing, there has not been any research on spillover impacts within the classroom.

Overall, as the review article points out, direct evidence, on both short- and long-term learning outcomes, is surprisingly limited. This is particularly disappointing when it comes to large-scale, nationwide programs, such as India's school feeding program (except Chakraborty and Jayaraman 2019), which is the largest in the world and exists within a public education system that is well known to exhibit poor learning outcomes (ASER Centre 2018).

Based on the limited evidence on the effects of school meals on learning outcomes, the review article provides some estimates of the improvements in learning outcomes. While many of these estimates are also short term and primarily based on small-scale randomized control trials (RCTs), it is nevertheless instructive to point out the cost-effectiveness of school meals from a policy perspective. The review article highlights the cost-effectiveness of school meals, particularly when we account for its multidimensional impact on child outcomes and in the labor market in adulthood. It would be useful to also have more information on how learning outcomes were measured in these studies, in the context of either limited data availability or education systems that are incentivized to teach to the test (see World Bank 2019).

In terms of cost–benefit estimates, it would also be worthwhile to compare school meals with other interventions to improve learning outcomes in developing countries—school inputs, teacher incentives, remedial education, and more recently, EdTech (see Holla and Kremer 2009 for a review). However, the paucity of rigorous and more contextualized estimates of long-term benefits of school meals for comparison with other school-based interventions poses a challenge.

The review briefly touches upon the challenge of monitoring and scaling of school meals. This is of particular relevance in the low-state-capacity context of most developing countries, which also have greater need for an effective school meal program. Here it is relevant to mention that variation in the design of the program not only has implications for its impact but also on monitoring and leakages. For instance, India's school meal program transitioned from take-home rations once a month to daily provision of cooked meals during school days. Since the conditionality is weaker in the former than in the latter, evidence suggests that provision of cooked meals was more effective in improving both nutrition and school participation. At the same time, leakages are likely to be lower in the latter case. Increasingly, IT tools are being adopted to monitor the program (Debnath, Niayamgode, and Seekhri 2020), and more evidence on this issue is required.

To summarize, the review article is a timely reminder of the multifaceted benefits and high cost effectiveness of this targeted, conditional transfer program. One hopes that it brings greater attention to the need for more rigorous evidence on the program's learning effects in the longer term in low-income settings that go beyond small-scale RCTs and are instead based on at-scale, credible, standardized test scores and labor market returns data.

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Comment: School meals bring social protection and education to the table

Ugo Gentilini and Shwetlena Sabarwal

Social protection tends to attract passionate and heated debates worldwide. This is because the theme is a proxy for larger societal quandaries, such as how much redistribution is acceptable, who deserves it, and what forms it should take. And yet, school feeding programs tend to emerge relatively unscathed from these bitterly contested ideas.

Why? Various reasons are at play: to begin with, school feeding makes political sense. The nature of school meals evokes goals and beneficiaries—children, their nourishment, and schooling—that garner broad support among the public and across the political aisle. As Biniam Bedasso suggests, programs that provide food to children who may otherwise go hungry have an inherent moral appeal.

It is possibly for this reason that school feeding programs have a rich and deep history, in some cases dating back to the 18th century, and enjoy a large operational footprint. In fact, school feeding schemes are among the largest-coverage social protection programs, and among the top performers in reaching the most people (World Bank 2018). Such a powerful combination of noble objectives, tradition, and scale make school feeding programs a natural embodiment of society's social contracts.

Finally, school feeding has been subject to extensive empirical scrutiny across an array of social and economic dimensions. While cash transfers have been on the fast track of academic interest, high-quality research on school feeding has been a regular, if perhaps less "visible," feature of social protection literature over the past two decades. Bedasso makes reference to a UNESCO synthesis of 20 evaluations.

Bedasso's contribution provides an insightful, compact overview of those effects of school feeding as well as of key policy quandaries. His chapter illuminates a range of key choices and dilemmas that underpin school feeding programs across the country income spectrum. Within the overall discussion, four considerations stand out.

First, the strength of the evidence tends to move from "strong" for service utilization (e.g., school enrollment and attendance) to more mixed and limited for longer-term outcomes, such as learning. In fact, the chapter's Figure 2.3 shows that for math scores, there is ample heterogeneity in the direction of results as well as an overall small effect in magnitude (effect sizes tend to revolve around zero). This may suggest that one of the chapter's headline messages—"School feeding does not only keep children in school but also helps them learn better"—should be interpreted with a dose of caution and nuance.

Second, the chapter rightly underscores the need not just to evaluate school feeding in general but to do so in a comparative mode. In fact, on the whole, further research is needed to establish how much longer-term benefit school feeding has over other social safety nets (especially the easier-to-implement, more flexible, and more adaptive cash transfer programs). However, is that even the right lens to use? It can be argued that the benchmark used should be not just other demand-side social protection interventions like cash transfers, but the broader menu of supply-side measures. For instance, the work of the Global Education Evidence Advisory Panel (2020), Neelsen et al. (2021), Bergstrom and Ozler (2021), and Evans and Yuan (2019) points precisely to the need for widening the horizon of comparisons and adopting an outcome-oriented instead of a program-centric approach.

Third, there is a really fascinating discussion about what governments most value within school feeding programs. Implicit here is an interesting quandary—how to make programs that enhance welfare on multiple dimensions salient to ministries that care only about one dimension.

School feeding programs, if well designed and delivered, can improve both education and health outcomes. However, they may not be the most efficient at improving just one of these. A comparison of 150 education-focused interventions (Angrist et al. 2020) suggests that school feeding is relatively effective in improving Learning Adjusted Years of Schooling (Filmer et al. 2020), but with a high variance. However, there are other programs, such as information campaigns, Teaching at the Right Level, and structured pedagogy, that do better.

As Bedasso's review shows, governments most value school-feeding programs for education objectives. So if an education ministry was identifying policies to improve learning and retention, school feeding programs may not be its first choice. This decision making does not take into account what Bedasso calls the "economies of scope" of school feeding. In other words, the decision to implement school feeding programs may rest with line ministries that may not have the incentives to factor in the synergistic relationship between childhood nutrition, health, and education.

Admittedly, given the popularity and political salience of school feeding programs, this is not much of a constraint overall. But it would be interesting to see how much of a constraint this is on their scale-up in fiscally constrained environments.

Finally, the COVID-19 crisis has highlighted the limits of scaling up programs tied to particular institutions—in this case schools—which can occur when those same institutions are severely affected (i.e., school closures). Such a constraint might have been particularly acute for logistics-intensive programs like in-kind transfers. As a result, at least 27 countries have opted to replace meals served at school with vouchers and cash transfers provided directly to children's families (Gentilini et al. 2022). While such a switch has been temporary, it has also opened up new longer-term opportunities and challenges for connecting ministries of education with those of social protection (Waxman et al. 2021).

Ultimately, the discussion on school feeding mirrors broader themes in development economics. The contexts that need school feeding the most—the low-income, food-insecure contexts—are also the ones that can least afford and manage them.

School feeding programs are ingrained in the state–citizens social contract. They are well evaluated. And they offer an institutional platform for pursuing multiple goals and delivering different services. The social protection and education communities should look at school feeding as one of the key entry points for working even more and better together.

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Chapter 3.The Case for Free Secondary Education

The experience of free primary education shows how free secondary education could work—but it requires more than a stroke of a pen.

Lee Crawfurd and Aisha Ali

How can countries achieve global education goals? Scalable policies are of critical importance. Perhaps the most obvious proven scalable education intervention is the expansion of free schooling. In this chapter, we assess what we can learn from the experience of free primary school reforms for the expansion of free secondary school. The lessons are simple: talk is cheap, and money matters. There are hundreds of cases of governments declaring that school is free but to little effect. Achieving increases in enrollment requires additional resources to reimburse schools for lost fee income and to build new schools in areas where they haven't existed. Finally, we argue that concerns about the equity of spending on secondary school are misplaced. Removing fees for secondary school does provide a direct benefit to wealthier households who are already enrolled, but it also allows children from lower-income households to enroll for the first time. The costs are high but can pay for themselves through higher earnings in the long term.

3.1 Introduction

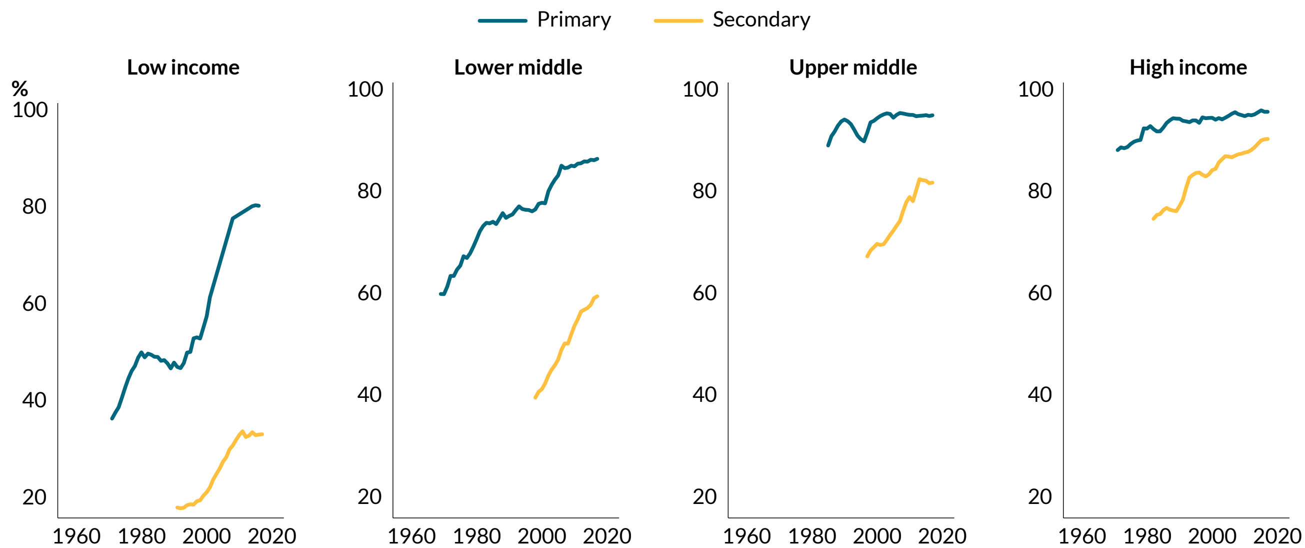
New global goals have raised objectives from aiming for universal primary schooling by 2015 to universal secondary by 2030. This increase in ambition is well founded, since most children now attend primary school, but secondary education still has much farther to go (see Figure 3.1). Attending secondary school has important benefits, even for students who are not as well prepared (Duflo, Dupas, and Kremer 2021). Many countries have now announced fee removals for secondary school, but, as we show in this chapter, the experience of free primary school shows that an announcement alone is not enough. Nonetheless, removing fees and providing schools with replacement funding has been shown to be a reliable means to expand enrollment, even in countries with weak government implementation capacity.

We focus on two critical policies that have played central roles in mass school expansion: (1) removing fees to attend school, and (2) building (and staffing) new schools. Both of these policies require significant investment. But with that investment, they are feasible and reliable.

Importantly, financial barriers are not the only obstacles to secondary school. In many countries, students must pass a primary school leaving exam to be admitted to secondary school. These exam requirements should probably be scrapped altogether. For example, in Ghana, where junior secondary school is now free, around 20 percent of children who pass the entrance exam and are admitted into senior secondary school

Figure 3.1. Secondary school enrollment remains very low in the poorest countries

Net school enrollment rates, by country income group



Note: Data from the World Bank Development Indicators.

do not register due to the cost (Ajayi 2014). And these exams are unfair; standards are insufficiently moderated across years, so that exams are more difficult in some years than others, leading to arbitrary changes in pass rates (Rossiter et al. 2021).

3.2 Removing fees

Making school free can both lead to an increase in enrollment and serve as a pure benefit to fee-paying parents of children who are already enrolled. The removal of school fees has been a major redistribution to the majority of households who have school-age children. In Africa, 75 percent of households have children under 15 (United Nations 2017). Fees are also an important policy lever in the minds of government officials in developing countries. A Center for Global Development survey of senior officials from 35 low- and middle-income countries found school fee removal was by far the most commonly cited "most important education reform of the last five years" (Crawfurd et al. 2021). By contrast, well-researched interventions such as cash transfer programs reach as few as 20 percent of Africans, according to the World Bank ASPIRE database (2019).

Existing literature on effects of fee removals on enrollment

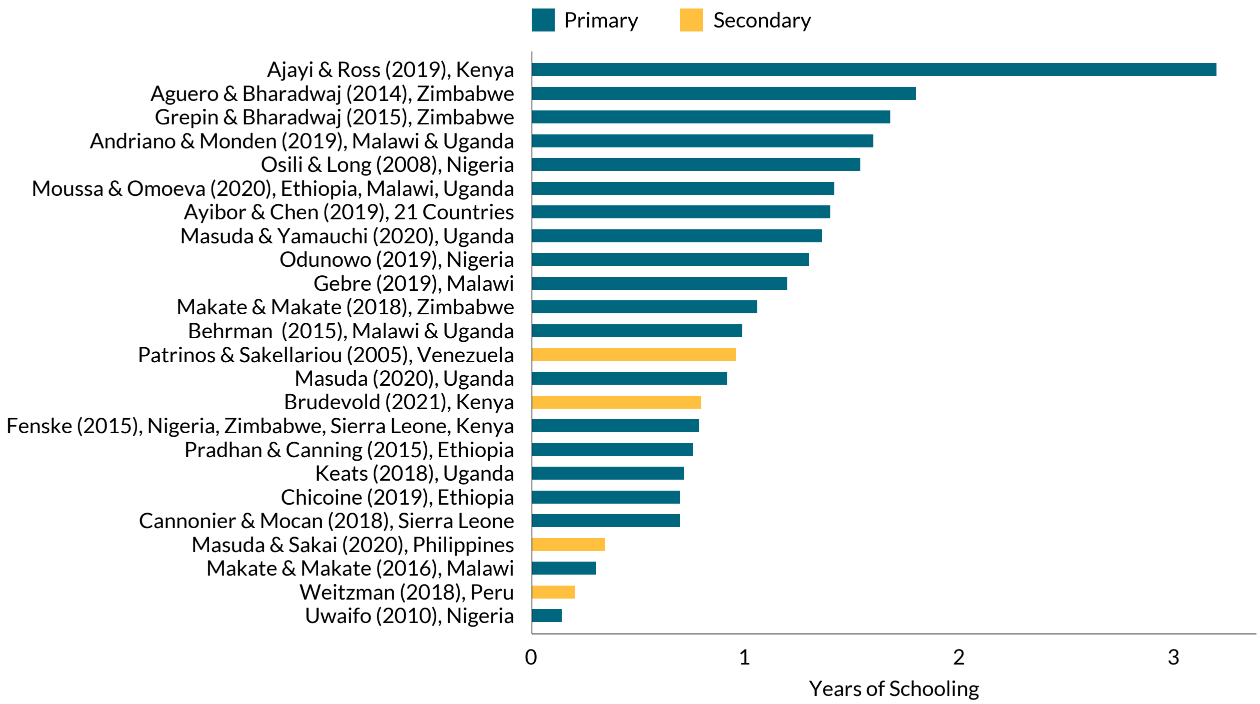
Most (96 percent) countries no longer charge fees for primary school, but fees are still charged for secondary school in 24 countries at the lower secondary level and in 40 countries at the upper secondary level.[1] In addition to these official or formal fees, informal fees for books, uniforms, and other costs quickly add up even where there is no tuition fee—and can be a large financial burden on families.

Removing fees for both primary and secondary school increases enrollment. We count 28 studies from over 21 different countries that estimate the effect of a free school or fee elimination reform. The average beneficiary gains one extra year of school, compared

1. World Policy Analysis Center, Education Database 2019.

Figure 3.2. Removing fees helps people get more school

Estimated effects of fee removal policies on schooling



Note: A full list of these studies is contained in Appendix 3.A.

with those in earlier cohorts who had to pay fees (Figure 3.2).[2]

The five studies that look at removing fees for secondary school mostly find smaller but positive effects on completed schooling. Four studies with positive effects are those from reforms in Venezuela in 1980, the Philippines in 1988, Peru in 1993, and Kenya in 2008 (Patrinos and Sakellariou 2004; Masuda and Sakai 2020; Weitzman 2018; Brudevold-Newman 2017). A fifth study on Uganda finds no gain in enrollment but finds a large (60 percent) decrease in household per student spending on education (Omoeva and Gale 2016). Why are effects smaller for secondary school than for primary school, despite larger pre-reform fees? This is partly due to higher hidden fees, and partly due to additional restrictions on enrollment into secondary school – namely passing national primary school leaving examinations. For example, the World Bank shows that two-thirds of African countries have high-stakes examinations at the end of primary school that determine entry into secondary school (Bashir et al. 2018). In Tanzania, in addition to direct effects on increasing enrollment, free secondary school has also had spillover effects on primary school performance (Sandholtz 2021). Students who could suddenly afford secondary school did better on exams at the end of primary school.

2. These studies mostly focus on the long-run increase in average years of schooling as a result of the policy, rather than the short-run change in enrollment rates. In order to get the short-run change in enrollment rates, we assume that increasing long-run average schooling by one year requires a short-run change in enrollment rates of one year divided across six grades, or 16.6 percent.

Box 3.1. Free primary and secondary education in Ethiopia led to more school, more earnings, and better health

The Ethiopian government published the Education and Training Policy in 1994, which abolished all user fees for students in public primary and lower secondary schools. Two aspects of this policy were key: fee abolition and the introduction of mother tongue instruction (Chicoine 2019). Together, the policy increased schooling for affected children by an average of 0.7 years. Consequently, each additional year of schooling then increased literacy by 12 percentage points, HIV knowledge by 5 percentage points, and the likelihood of knowing an HIV testing location by 6 percentage points. The policy also reduced a woman's preferred number of children by 0.8 and increased her likelihood of working professional or skilled jobs by 6 percentage points (Chicoine 2020). Other studies have found even greater post-policy increases in educational attainment for Ethiopian women, with increases of 0.24 years for those who were eligible for free primary school at 13 and an additional 1.5 years for women eligible by age 7 (Moussa and Omoeva 2020). The same women also experienced a 10-percentage point reduction in teen births and a 1.5 percentage point increase in salaried employment. Eligible women were 6 percentage points less likely to marry and have a baby before adulthood (Pradhan and Canning 2015).

Cross-country evidence from a new database of all fee removal policies

The empirical literature in the prior section focuses on a select sample of successful fee reforms. Most studies have focused specifically on successful reforms so that they can use the reform as a natural experiment for studying the causal effect of education on other outcomes.

In this section we review all free education reforms, both successful and unsuccessful. What makes some successful and some not? We address this question using a newly constructed database of national school fee removal reforms. Our original database contains 130 announcements of free education in 85 countries. These are a combination of laws (65), policies (54), programs (5), decrees (3), and court decisions (3). The resulting dataset includes multiple instances in which school fee removal has been announced multiple times per country. Overall, 29 countries have had at least two announcements of free education.[3] We compare two categories of fee removal: legal reforms and policies. The first category of legal reforms includes constitutional amendments, laws, or presidential decrees. In these cases, schools are banned from charging fees but are not necessarily compensated by the government with additional subsidies to replace fee income. The second category of policies includes announcements as part of a specific policy or program.

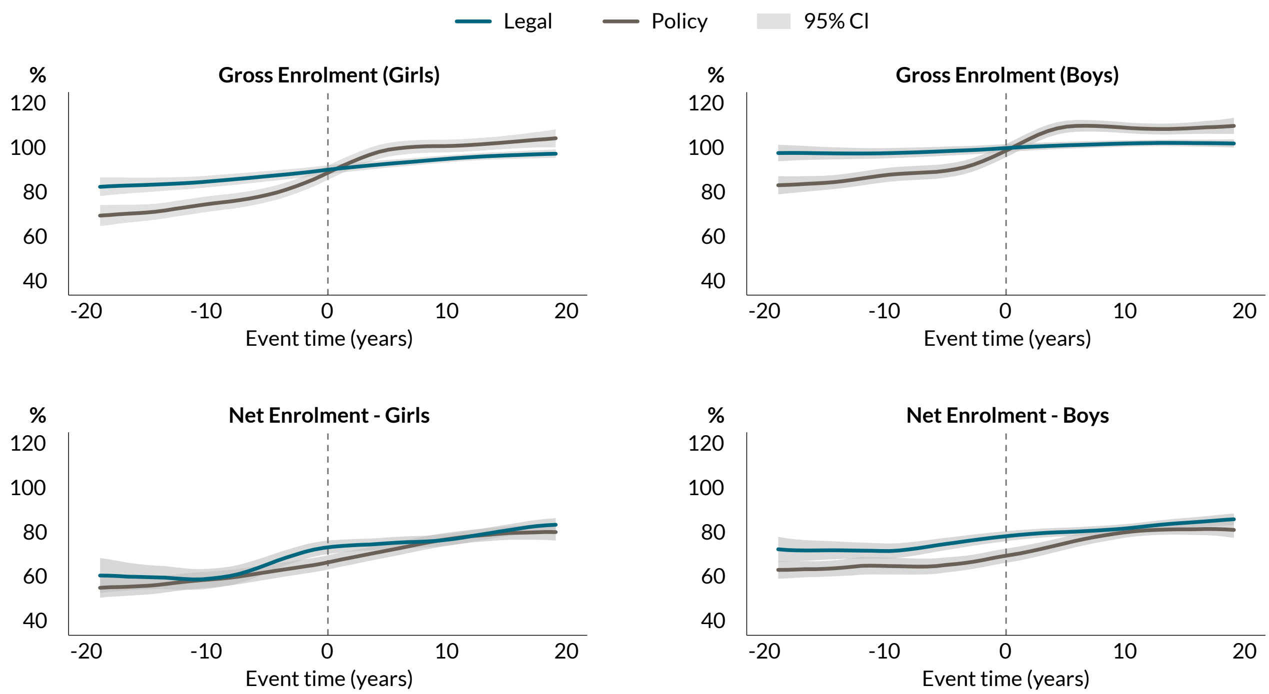
We merge the data on school fee reforms with national-level data on trends in school inputs and outcomes from the United Nations Educational, Scientific and Cultural Organization (UNESCO) Institute for Statistics (UIS) database.

First, we see essentially no change in enrollment rates as a result of legal (de jure) fee reforms but find a sharp rise in gross enrollment around fee policy (de facto) reforms, from 80 percent to over 100 percent (Figure 3.3). Enrollment increases for both girls and boys. Gross enrollment rises much more rapidly than net enrollment, implying that a large amount of enrollment in response to fee removal policies is of children above the usual expected age for primary school.

3. Full details on the construction of this database are included in Appendix B.

Figure 3.3. Action matters more than legal decrees

Gross and net primary school enrollment rates, for both girls and boys



Note: The shaded areas around each line represent the 95 percent confidence intervals (95% CI). The "legal" category includes 899 free primary education announcements, which are either constitutional amendments, laws, or presidential decrees. The "policy" category includes free primary education announcements associated with 59 policies and programs.

3.3 Building new schools

In addition to removing school fees, another important component of many efforts toward universal primary school has been building schools in areas where they had not previously existed. For example, Indonesia built over 61,000 primary schools between 1973 and 1978 (Duflo 2001). In Punjab (Pakistan), 29,000 schools were built between 1960 and 1989 (Khan 2021). Positive effects of school construction that has reduced students' travel distance have also been shown experimentally in Afghanistan (Burde and Linden 2013) and Burkina Faso (Kazianga et al. 2013). Effects can be more pronounced for girls and in areas where safety is a concern like Afghanistan (Burde and Linden 2013). Using observational estimates but a large sample of countries, Filmer (2004) estimated the elasticity of enrollment with respect to distance to primary school. This elasticity is relatively invariant with respect to the initial average distance to school. On average, living one kilometer closer to a school is associated with 5 percent higher enrollment rates.

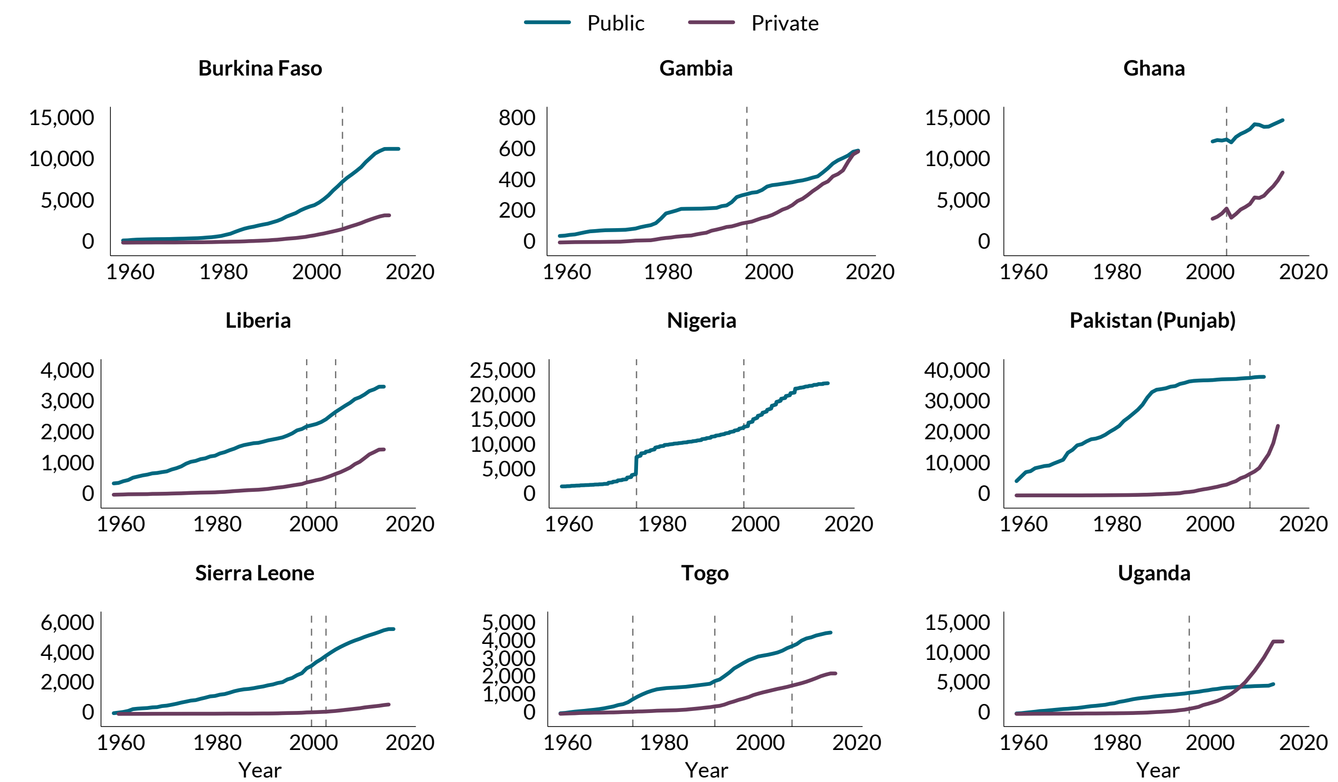
School building and free education announcements

Does school construction explain differences between countries in the effectiveness of universal schooling policies? Comparable cross-country data on school numbers is not published by UNESCO. We reconstruct estimates of growth in school numbers over time using recent education management information systems (EMIS) data, which indicates when schools that are currently open were first established. We have data for nine countries that includes the date of school establishment and any overlaps with free school reform. For most of these countries, there appears to be little change in trends of school construction around universal schooling policies (Figure 3.4). A notable exception is Nigeria, where there is a clear jump in construction coinciding with the 1976 free school policy (studied in Osili and Long 2008). In Uganda, around the time of the 1997 free primary education reform, construction of private schools increased but construction of public schools did not.

Unsurprisingly, building new schools is most important in areas where schools are scarce. In some countries, such as India, there is now arguably an oversupply of public primary schools, with the median school having just 62 students (Datta and Kingdon 2021). However, this is not the case in many other countries. Figure 3.5 shows cross-national data on the number of primary-age children (ages 5–14) per school and geographical area served (in square kilometers) per school, for all low- and middle-income countries for which data is available. South Asia has the most schools per student and per area of all regions. In sub-Saharan Africa, there are over three times more children per school.

Figure 3.4. Most free school reforms are not accompanied by more school-building

Total number of schools, by ownership and date of opening



Note: The y-axis indicates the total number of schools in each country. Dotted lines indicate the year a free school reform was instituted. The reforms in Burkina Faso, Gambia, and Pakistan were constitutional or legal reforms. Ghana and Uganda are classified as policy reforms. In Liberia, a law in 2002 was followed by a policy in 2006. Nigeria is categorized as a policy in 1976 and a law in 1999. Sierra Leone was a policy in 2002 followed by a law in 2004. Togo had legal reforms in 1975 and 1992, followed by a policy in 2008. School construction data are based on the year schools were reported as first opening, in the most recent available EMIS data (2018–2019 in Burkina Faso, 2020 in Gambia, 2015–2016 in Liberia, 2013–2016 in Nigeria, 2017–2018 in Pakistan, 2019 in Sierra Leone, 2017 in Togo, and 2014 in Uganda). The indicator does not therefore account for any schools that may have closed. Data for Ghana comes from contemporaneous EMIS data from 2001–2017.

Box 3.2. Major school construction programs in India, Indonesia, and Nigeria led to higher rates of schooling and literacy, as well as long-term intergenerational effects

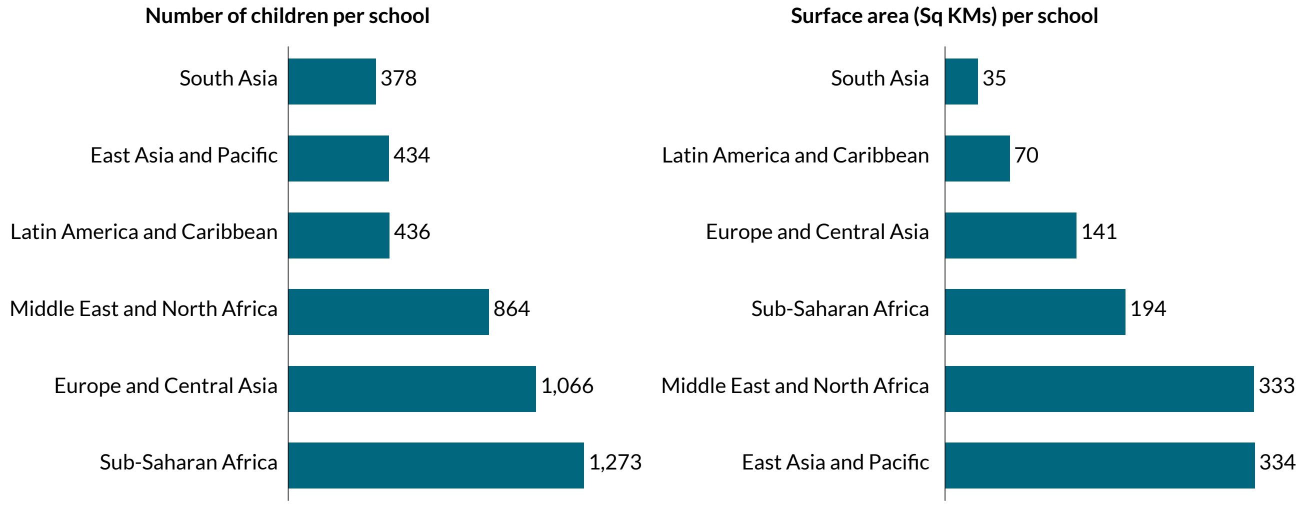
Beginning in 1994, **India** expanded access to primary school through the District Primary Education Program, focusing heavily on regions with low female literacy rates. The government constructed more than 160,000 new schools, trained 1 million teachers, and increased education expenditures in 271 recipient districts across 18 states. Children in receiving districts were roughly 2 percentage points more likely to both attend and complete primary school (Azam and Saing 2016). The program increased average schooling by 0.2 years and reduced the gender attainment gap. Importantly, the expansion has also been used to study general equilibrium effects (Khanna 2015). While the program increased literacy (by 3 percentage points) and schooling, it also depressed wage returns to schooling by 6.5 percentage points. The program also had positive intergenerational effects; the children of female beneficiaries had higher scores on math, English, and vernacular tests (Sunder 2018). These effects can be explained by those educated mothers having higher bargaining power, better health, and delayed marriage.

Between 1973 and 1979, the **Indonesian** government rapidly expanded access to primary education by constructing more than 61,000 schools, or an average of one school per 500 children under 14 years of age. The construction program led to a 0.25–0.40 increase in years of education and a 7–11 percent return on wages per additional year of schooling for men in the treated birth cohort (Duflo 2001). The program increased migration rates by 0.7 percentage points and household expenditure by 2–3 percent. It also had positive intergenerational effects, with the exposed cohort's children obtaining 0.1–0.2 additional years of education (Akresh, Halim, and Kleemans 2018).

Nigeria's 1976 Universal Primary Education reform abolished user fees for public primary schools and constructed over 16,000 new schools. Fee abolition led to an average increase of 1.5 years of schooling for the treated cohort, and school construction (at a cost of US$130 per student) increased the number of years of education by an additional 0.5 years (Osili and Long 2008). Each additional year of female education corresponded to a reduction in the number of early births by 0.26. Increases in educational attainment for women led to a reduction in the probability of genital cutting for beneficiaries' daughters by 0.3 percentage points (De Cao and La Mattina 2019) and an increase in the probability of completing secondary school by 3.3 percentage points (Odunowo 2019). Children of female beneficiaries also have higher educational attainment and are 5 percentage points more likely to attend secondary school. Finally, the reform also had long-term political effects, with a 3-percentage point increase in voter turnout and a 4-percentage point increase in community meeting attendance among beneficiaries (Larreguy and Marshall 2017).

Figure 3.5. Where are schools most needed?

Number of schools per child and per square kilometer, by region



Note: This figure uses data on the number of schools per country from Walter (2020), and on population ages 5–14 and surface area from the World Bank Development Indicators for 2018.

3.4 Cutting fees and building schools are not cheap

Fewer studies report the actual level of fees prior to their removal. Median fees in studies that did were US$6.5 per student per year (Table 3.1). A review of fee levels for secondary school in eight African countries finds an average of US$55 (Table 3.1). In some countries, this comprises a large share of public-school budgets. For example, in Senegal and Zambia, over two thirds of school budgets come directly from parents (Figure 3.6).

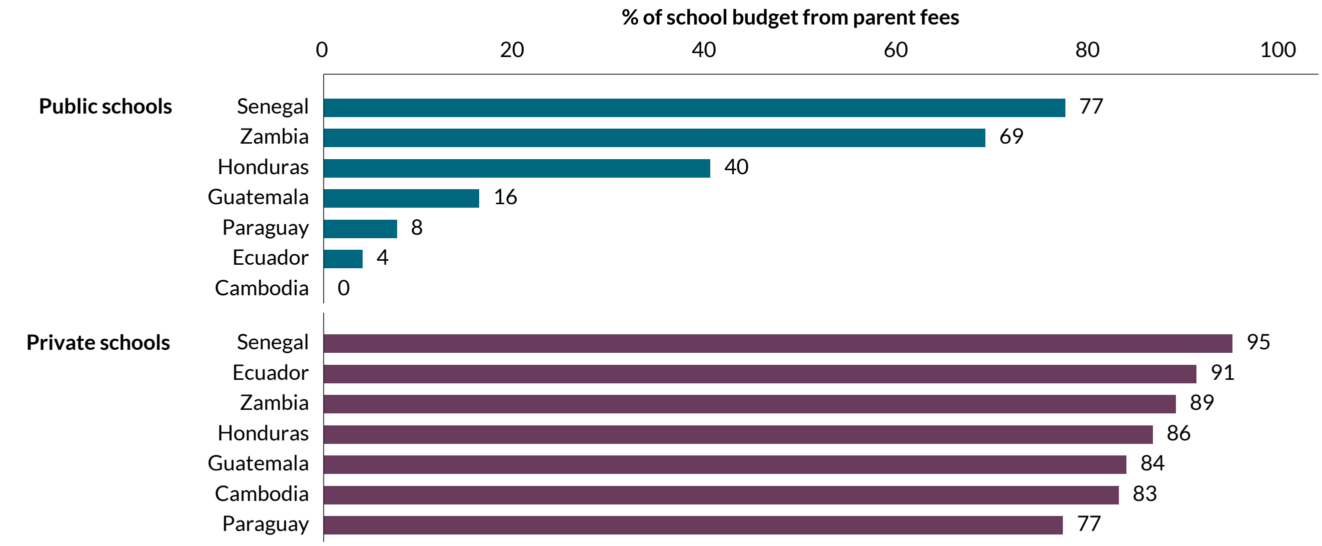
Why are policies more effective than legal changes? First, policies are associated with an increase in education spending (Figure 3.7). This increase actually begins before policies are announced. Despite spending increases, policies are associated with a temporarily worsening pupil-teacher ratio, as pupils enroll faster than new teachers are recruited. Spending per pupil therefore increases in the very short term before deteriorating in the years after a fee removal announcement.

Table 3.1. Average cost to households of public school (pre-reform)

Author	Country	School Level	Cost (US$)
Grogan 2009	Uganda	Primary	4
Tsimpo and Wodon 2014	8 countries	Primary	7
Lincove 2009	Nigeria	Primary	13
Lucas and Mbiti 2012	Kenya	Primary	16
Lincove 2012	Uganda	Primary	17
Borkum 2012	South Africa	Primary	19
Blimpo, Gajigo, and Pugatch 2019	Gambia	Secondary	44
Borkum 2012	South Africa	Secondary	49
Tsimpo and Wodon 2014	8 countries	Secondary	55

Figure 3.6. Parent fees provide much of secondary school budgets

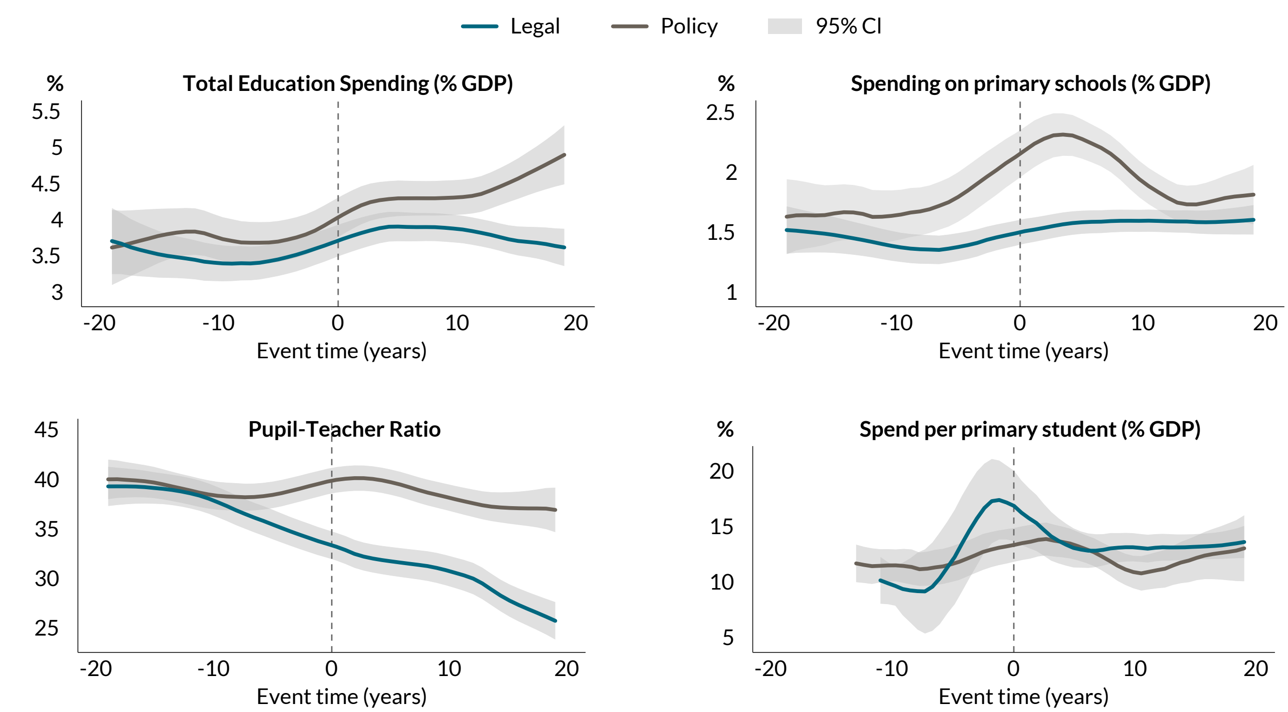
Share of public secondary funding from parent fees



Source for data: PISA for Development (PISA-D), 2017

Figure 3.7. Successful policies are accompanied by more spending

Spending and pupil teacher ratios, before and after free primary reforms



Note: The "legal" category includes 899 free primary education announcements, which are either constitutional amendments, laws, or presidential decrees. The "policy" category includes free primary education announcements associated with 59 policies and programs.

3.5 Expanding enrollment has not undermined learning

An important concern is whether removing fees and rapidly increasing enrollment reduces the quality of education. Le Nestour, Moscoviz, and Sandefur (2021) show for the period between 1950 and 2000 no change in school quality following the removal of fees. This finding is supported by other analyses from Kenya (Lucas and Mbiti 2012; Brudevold 2017), Tanzania (Valente 2019), and southern Africa (Taylor and Spaull 2013). Students who would have attended school anyway did no worse. However, some studies have shown reductions in quality where fees were removed in South Africa without any additional government financing (Garlick 2019).

In aggregate numbers, we see an increase in pupil-teacher ratios in successful reforms, but not in those that are unsuccessful. This is consistent with a rapid increase in enrollment happening faster than recruitment of new teachers. Increases in class sizes are often viewed as an indicator of worsening quality, but as we show in Chapter 4, the empirical literature on the causal effect of class size on learning is decidedly mixed, with many results showing no effect at all.

3.6 Free secondary education is not regressive

Conventional wisdom in international development policy circles holds that free secondary and tertiary education policies are regressive. If the only students who reach secondary school are the most affluent, then removing fees could be seen as a subsidy for the rich. The standard benefit incidence analysis that underlies this consensus, however, undercounts the progressive effects of free school by ignoring behavioral response. Simply put, when fees fall, students from poorer families are more likely to enroll. This view is also flawed by valuing education only at the cost of supplying it, rather than the much higher net present value of education's future returns.

The static benefit incidence of abolishing fees for secondary or tertiary school is usually regressive. This point has been made from everyone from the Malala Fund to the IMF and World Bank. The premise is often conceded even by those who reject the call for fees (Lewin 2009). Analysis of household survey data from Uganda confirms that higher-income households received a disproportionate share of the benefits from President Museveni's Universal Secondary Education subsidies (Wokadala and Barungi 2015). A 2016 report on financing upper-secondary education in the developing world commissioned by the Malala Fund advocated against a blanket abolition of user fees, in favor of a phased approach targeting marginalized groups in the early stages (Malala Fund 2016). A key theme of the report is reflected in a pull quote from an anonymous expert: "I shudder at the thought that we are even supporting [universal] lower secondary education given these [poor learning] results at the basic level." A 2003 IMF review argued that "spending on secondary and tertiary education primarily benefits the nonpoor and there is strong evidence of middle-class capture" (Davoodi, Tiongson, and Asawanuchit 2003). Across sub-Saharan Africa, the wealthiest quintile captured 38.7 percent of the benefits of spending on secondary education, and 54.4 percent of the benefits of spending on tertiary education.

The World Bank's tertiary education coordinator declared in 2015 that free college is regressive, citing countries where "students from the lowest income quintile accessing higher education is only 4 percent, while 60–70 percent of college-age students belonging to the highest income quintile are enrolled in higher education" (O'Malley 2015). After Ghana rolled out universal free upper-secondary school, many voices (including the Minister of Finance), called for rolling back the policy to only target tuition waivers (Doe-Glah 2017). Pauline Rose of the University of Cambridge has promoted the concept of "progressive universalism" in education finance for the developing world, targeting the most vulnerable first and stressing the need "to be realistic about the pot of money we have" (UNESCO 2017). This same approach was endorsed by the report of the International Commission on Financing Global Education (2016), chaired by former UK prime minister Gordon Brown and by large philanthropies like the Mastercard Foundation (Zubairi and Rose 2019).

Simple benefit incidence analysis ignores the main benefits of free schooling: additional enrollment and education's long-run returns

For higher-income households who would always enroll their children in secondary school regardless of fees, any fee reduction is a pure gain. Meanwhile, there will be some lower-income households for whom fees are a barrier to secondary school enrollment and for whom a reduction in fees would lead them to enroll. The overall progressivity of a fee reduction depends on the relative magnitude of these two mechanisms: how regressive the benefit to currently enrolled children's households is (the intensive margin), and how progressive the enrollment effect is (the extensive margin).

Enrollment of students from lower-income households is more sensitive to changes in the price of schooling

How much more responsive are children from low-income households to changes in school fees than children from high-income households? Numerous studies have estimated the price elasticity of demand for education—how much enrollment changes in response to price or fee changes. They all show bigger enrollment changes for children from lower-income households—in Pakistan (Alderman, Orazem, and Paterno 2001), Peru (Gertler and Glewwe, 1990), Madagascar (Glick and Sahn 2006), and South Africa (Borkum 2012; Garlick 2013).

Perhaps the clearest evidence on the relative price sensitivity of school enrollment by high- and low-income households in the developing world comes from simple before-and-after comparisons spanning the abolition of primary school user fees. Two papers study the abolition of primary school fees in Uganda in 1997. Deininger (2003) finds the enrollment rose disproportionately among the poor. In a follow-up paper, Lincove (2012) finds differential impacts of fee reductions by wealth quintile, though with a non-monotonic pattern. The second and third quintiles respond the most, while the higher quintiles—as well as the lowest—do not (Figure 3.8).

Duflo, Dupas, and Kremer (2021) report on a randomized trial of scholarships covering the cost of fees for senior high school (SHS) in Ghana. They find much larger effects on enrollment and attainment, increasing the probability of ever enrolling in SHS by 25 percent and of completing SHS by slightly more. Notably, the study population here included students who passed the entrance exam to SHS but failed to enroll due to costs (i.e., students who one might expect to be the most price sensitive). Around half of these students had parents who had not attended secondary school.

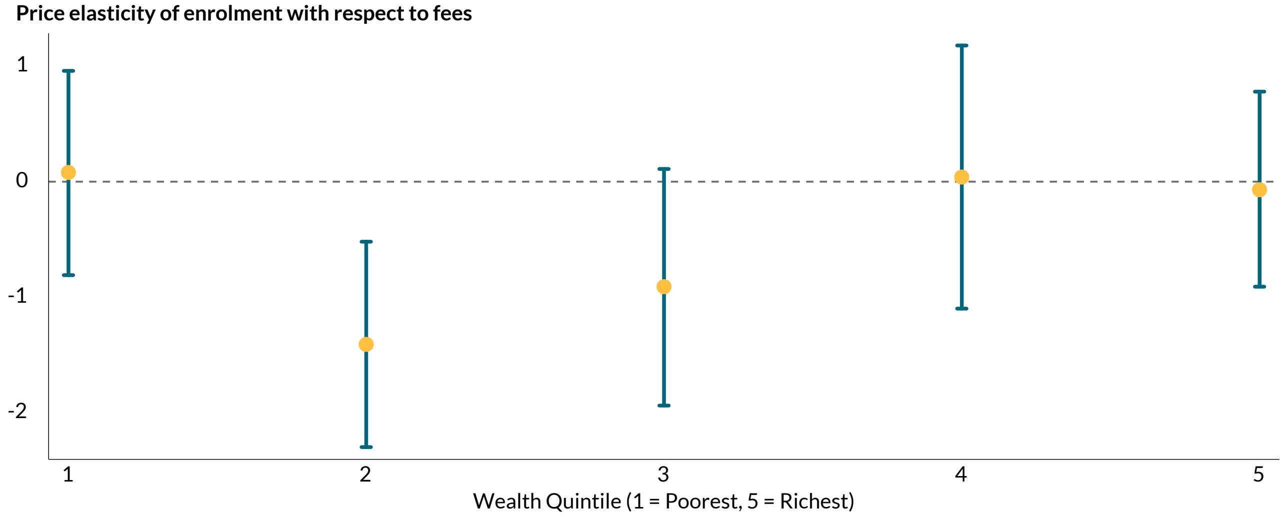
The rate of return to investment in education is high

Education is an investment with very real and concrete payoffs. Montenegro and Patrinos (2021) estimate the rate of return of an additional year of schooling for 142 economies using 853 harmonized household surveys. The average person earned 10 percent more over their lifetime for each additional year of education they complete. This is by no means uniform across all countries, but the rate of return is on average higher in poorer countries.

One concern about large expansions in schooling is that they may lead to reductions in education's estimated rate of return . One fact mitigating against this concern is that despite the large global expansion in schooling over the last 50 years, the rate of return has only declined modestly, implying that demand for skills is growing at a similar pace to supply. In India, Khanna (2015) has estimated the magnitude of any such general equilibrium effects. A major primary school expansion did lead to a reduction in wages for skilled workers and an increase in wages for unskilled

Figure 3.8. Higher fees lead to lower enrollment only for the poor

Elasticity of enrollment with respect to fees, by income quintile



Note: Figure reproduced from Lincove (2012). Q1 is the poorest 20 percent of households and Q5 is the richest 20 percent.

workers. This reduction in the returns of education reduced the benefits of schooling (by around 30 percent), but they remained positive.

3.7 Conclusion

Policies that subsidize higher levels of education will tend to be regressive, since children or young people from more affluent backgrounds are more likely to attend those higher levels and reap the benefit of that subsidy. This correlation between socioeconomic background and secondary school enrollment generally persists even where fees have been abolished.

On the other hand, abolishing fees and building new schools in areas where they had not existed often has a bigger impact on the enrollment decisions of lower-income households, who receive more schooling when schooling is cheaper. Higher-income households may attend regardless of fees or be less price sensitive.

The literature suggests that free primary education policies increased educational attainment, reduced teenage pregnancy, and had positive effects on intergenerational outcomes.

Taken together, expansive school construction and teacher hiring were critical aspects of designing a successful free primary education policy. Abolishing user fees was not enough to increase enrollment and attainment if children still did not have access to schools. We can expect the same in efforts to expand access to secondary school.

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Chapter 3 Appendices

Appendix 3.A. Studies on school fee removals included in Figure 3.2

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Appendix 3.C. School fee abolition database

We compiled this database from numerous sources, starting with a list of all low- and middle-income countries as defined by the World Bank. We then reviewed existing literature reviews of free school announcements in multiple countries.

- Bhalotra et al. (2015) compiled dates of school fees removal in 27 countries. They supplemented a list of episodes for 17 countries compiled previously by Tomasevski (2006), with an additional 10 countries, drawn from various sources.
- UNESCO's International Bureau of Education maintained a World Data on Education database, the seventh edition of which was published in 2011, covering 163 countries (IBE-UNESCO 2012).
- The Comparative Constitutions Project dataset contains 848 constitutional events (defined as a new constitution or constitutional amendment) in 137 countries in which education is announced to be free (Elkins et al. 2019).
- UNESCO published a series of 161 National Education for All 2015 review reports, in which countries were invited to assess their progress since 2000.
- Finally, we reviewed all national education system reviews published by World Education News & Reviews.
- For remaining countries, we did a Google search for combinations of the country name and "free education," "free primary education," "free compulsory education," "free secondary education," and "school fee removal."

Comment: Free schooling is a progressive policy if countries can afford it

Robert Osei and Kwabena Adu-Ababio

The chapter provides a review of public funding for universal primary education. More specifically it looks at what the effect of universal primary and secondary education is for developing countries. This is a very important topic as many developing countries grapple with persistent poverty and inequality coupled with the pressing need to improve human capital for growth and development. Additionally, funding for primary and secondary education requires significant fiscal sacrifices that impose additional pressure on the already constrained fiscal situation in most of these countries. Although international donor support may sometimes supplement a country's public expenditure on education to meet global development goals, it may not be targeted or consistent enough to make the required impact. The discussion in the chapter is therefore of real significance for policy as it helps bring out issues on providing universal primary and secondary education and what its implications are. The chapter, in discussing issues of the effects of universal primary and secondary education, touches on the following key points.

The effect of fee removal on enrollment: The chapter notes that generally the effect of fee removal has been to increase the enrollment rates, with the effect smaller for secondary schools than it is for primary schools. The argument with respect to why fee removal engenders lower response for secondary enrollment is put down to factors such as the rationing of places at the secondary level and also to "other or hidden fees." Since the direct effect of fee removal is to cater to those who have passed the national-level exams and cannot afford to enter secondary school, one will be excused to favor the hidden fees factor as more of a binding constraint for entry into secondary school. Indeed, the additional expenditure requirements needed for secondary-level education are much higher and so even where governments legislate for no additional charges, where schools have the option, they will still ration places. In Ghana, even where the government has insisted that no fee is charged to parents, various alumni have taken up many of the capital investments that are happening in senior high schools. Therefore in the absence of fees, enrollment will still be constrained by existing capacity, which is already stretched in many cases.

Progressivity or otherwise of free secondary education: The chapter argues that free secondary education is not regressive, a position that is contrary to what the literature generally suggests. This chapter makes a very good case for why this is the case, arguing that most of the literature has looked at the issue from a static point—if current secondary enrollment is dominated by the non-poor, then a fee removal will be a transfer to the non-poor. However, once you factor in behavioral responses, then you have a situation where the probability of the poor also benefiting from this transfer increases, with the potential to change the regressivity of free secondary results. Of course, the paper alludes to the fact that the result is dependent on the elasticity of the response with respect to the poor versus the non-poor. However, it goes on to argue that when one builds in the net future returns to education, it tilts the argument more in favor of the progressivity of free education. We have no doubt that this is a very strong argument and one that must be taken seriously. Indeed, some support for this position can be found in the literature. Adu-Ababio and Osei (2018), using a microsimulation for Ghana, find that poverty in Ghana decreased when the country implemented the free senior high school policy. Additionally, the results showed that the reduction in poverty was even higher for female-headed households. So there is some empirical support for the position of the chapter that the removal of secondary fees is not regressive or as regressive as thought.

Effect of fee removal on education quality: One of the more contentious issues that we would have loved to see the authors spend a bit more time on has to do with the effect of fee removal on the quality of education. The chapter notes that fee removal is positively associated with an increase in the pupil–teacher ratio but further argues that the results of the effect of class sizes on learning outcomes are mixed. While we cannot dispute those latter results, it is important to note that most developing countries have class sizes that are significantly higher than their counterparts in the more advanced countries and with educational outcomes that are lower. So is increasing class sizes an optimal policy when one holds everything else constant? This question of optimal class size will continue to engage researchers for some time to come. The one thing that cannot be disputed is what these class sizes mean for the complementary infrastructure required to maximize learning outcomes. Ultimately, and for any country, the requirement is to maximize the overall human capital given the resources at its disposal. Therefore, if increasing the numbers (for the given infrastructure) will be detrimental to learning outcomes, then some hard policy choices will have to be made. However, if the assumption is that countries have excess education infrastructure capacity and therefore can accommodate increased enrollment, then fee removal, in the long term, will improve growth and development.

To conclude, we note that the issues discussed in relation to fee removal are important issues that countries are confronted with, in the presence of a binding constraint in the limited fiscal space that countries face. Moreover, most developing countries today have significant fiscal challenges, with almost every facet of the social and economic infrastructure requirement being a priority. Improving learning outcomes, and therefore human capital, in the minds of many is something that must be given more prominence in the policies of developing countries. Therefore, the art of creating fiscal space to enable the removal of primary and secondary education fees is one that countries will continue to grapple with. It is for this reason that we are inclined to support the concept of "progressive universalism" in education finance in developing countries.

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Comment: Public secondary education spending needs to prioritize the poorest

Pauline Rose

Debates on free secondary schooling have become heated in the context of the Sustainable Development Goals. Whether to include "free" was one of the issues most fiercely debated in relation to Target 4.1—with international advocacy organizations such as the Mala Fund and ActionAid pushing for the word "free" to be included, with many international funding bodies and some national advocacy organizations suggesting greater caution, particularly in contexts that had yet to achieve universal primary education with quality. The chapter by Crawfurd and Ali appears to side with international advocacy organizations.

In an ideal world, who would disagree with free universal secondary education? The benefits of secondary education for individuals and societies, as outlined in the chapter, are well noted. But we are not in an ideal world. Governments have to make tough decisions about how they prioritize their spending, and face difficult trade-offs. In a context where additional public resources are not forthcoming, there is a need to identify who does and does not benefit from free secondary education.

In this short commentary, I highlight five key interconnected points in response to Crawfurd and Ali's chapter, identifying flaws in their arguments.

1. Fee abolition is a political win, but does this mean it is right for all countries now?

The authors begin by citing a Center for Global Development (CGD) survey of senior officials from 35 low- and middle-income countries that found "school fee removal was by far the most commonly cited 'most important education reform of the last five years.' " This is hardly surprising. Since the movement to abolish primary school fees in the 1990s and 2000s, it is widely documented that this is seen as a vote winner. Having achieved political success at the primary level, political parties have shifted attention to fee-free secondary education, more recently using the SDGs as justification (as in the recent cases of Ghana and Malawi, for example). This does not necessarily mean it is the right thing to do, as outlined in points below.

2. Muddling of evidence from primary and secondary fee abolition, and from widely different geographical contexts and time periods

It is beyond the time and scope that I have for this commentary to undertake a thorough review of the literature, nor does it allow time for a thorough reassessment of the literature that is referred to in the chapter. However, it is apparent that the chapter draws on evidence in inappropriate ways. Notably, it brings in evidence on fee abolition from experience at the primary level to justify fee-free secondary education. This evidence has been widely cited elsewhere, with near unanimous agreement that primary schooling should be fee free at the point of entry (even though there are still debates about whether this necessarily means it should be fee free or can also be achieved with the use of vouchers, for example). While some lessons can be learned from the primary experience, it cannot be simply extrapolated to secondary schooling.

One reason for differential effects is that, unlike primary schooling, access to public secondary schooling is often highly selective and so, in some countries, those from richer households are much more likely to gain access (i.e., they are more likely to have made it to the end of primary schooling, and have had the benefit of better-quality primary schooling). For those from poorer households who do complete primary school, given they are less likely to be selected for government secondary schools where there is a rationing of places, they face the choice of not continuing to secondary school at all or paying fees for private schooling (with the role of the private sector in secondary schooling being very different than in primary schooling—an area that has received too little attention).

Some issues of differential effects at primary and secondary levels are mentioned in passing in the chapter—for example, that the effects of fee abolition are smaller for secondary school than for primary school. The chapter also notes that secondary school fees make up a large share of public school budgets, and so cutting fees is not cheap. However, the chapter is not consistent in how it draws on evidence from primary school experience.

Another aspect of muddled evidence in the chapter is pulling together literature from very different geographical contexts and time periods—with, for example, Venezuela (1980), the Philippines (1998), and Peru (1993) in the same sentence as Kenya (2008)—to make the case of positive experience of removing secondary school fees. Toward the end of the chapter, there is reference to the effects on the poorest, referring to "price elasticity of demand" for education, indicating poorer households benefit more. Again, it uses a mix of countries and time periods, and at least some focusing on primary schooling, ranging from Peru (1990) to Madagascar (2006) and South Africa (2012/13). The chapter also looks at before/after evidence based on a couple of studies in Uganda focused on primary schooling, but, again, this evidence cannot be extrapolated to secondary schooling.

The chapter does not refer to other literature on Kenyan's secondary fee abolition experience that has been more skeptical on the effects for those from more disadvantaged backgrounds, nor other related evidence from countries such as Ghana and Malawi. Our initial analysis from these two countries identifies adverse effects on poorer households, including for reasons given above. In addition, in Ghana, there is evidence of a reallocation of government and donor spending from primary to secondary education in the context of the "natural experiment" before and after the secondary fee abolition. This is in a context where only around one-third of the poorest complete primary school.

3. Does expanding enrollment undermine learning?

The chapter includes a short paragraph to suggest that expanding enrollment from the removal of fees does not undermine learning. Probably one of the most important lessons from the more extensive evidence on primary schooling is that where fees have been abolished without additional resources, the implications have been adverse for quality. Indeed, this is the conclusion of one of the papers by fellow CGD colleagues cited in the chapter, which states: "FPE [free primary education] reforms are associated with an acceleration of the negative trend in school quality but school quality was decreasing before the introduction of these reforms and FPE reforms might also have been associated with a reduction of resources per students"

(Le Nestour, Moscoviz, and Sandefur 2021, 29). Similar points have been raised in other studies. However, the chapter uses this paper to argue there “was no change in school quality following the removal of fees” (and also does not note that it is based on primary school fee abolition).

While the evidence cited in the chapter on whether learning has been undermined is from primary schooling, the effect is likely to be even more extreme for secondary schooling given the higher cost. The impact could also be greater for more disadvantaged students who are more likely to attend secondary schools where resources become even more constrained.

Following the adverse effects on quality as a result of primary fee abolition without sufficient additional resources, some countries provided capitation grants to schools. The design of these grants had important implications for equity. For example, where grants are used on the basis of children enrolled, it disadvantages locations with large numbers out of school, which are likely to be poorer. Some countries have sought to address this by using a formula to ensure poorer parts of the country receive a larger share, or by weighting the formula according to characteristics of the population who are likely to need more resources. The chapter, however, does not consider these issues with respect to the implications for secondary schooling.

4. Omissions in barriers to universal secondary education

In addition to the focus on fee abolition, the chapter includes an assessment of the building of new schools. It does not consider the shift in some countries toward basic education (i.e., the combining of primary and lower secondary grades, which means that new secondary schools do not necessarily need to be constructed). Nor does it consider the role of the private sector, which, as mentioned, is likely to have quite different effects at the secondary level compared with primary.

There are other fundamental demand-side barriers that would need tackling, such as those faced by adolescent girls as they reach puberty. There is a mention in passing of the benefits of school construction for girls in Afghanistan given the reduction of the distance to school, but this is in the context of community schools serving primary-school-aged populations. There are also many other well-recognized economic, social, and cultural barriers that adolescent girls from poor households face which are not considered. While I appreciate that a chapter of this length cannot cover everything, the omission of these key areas that would need to be tackled to achieve universal secondary education is a concern. The omission of gendered effects is even more stark given the chapter’s dismissal of equity effects, to which I now turn.

5. Misunderstanding of equity effects and progressive universalism

The abstract concludes: “Finally, we argue that concerns about the equity of spending on secondary school are misplaced. Removing fees for secondary school does provide a direct transfer to wealthier households who are already enrolled, but it also allows children from lower-income households to enroll for the first time. The costs are high but can pay for themselves through higher earnings in the long term.”

The chapter pitches itself against what it refers to as “conventional wisdom” in relation to arguments of regressivity of fee abolition. Building on my points above, the authors’ casual dismissal at the end of the chapter of equity concerns that are highlighted in much of the existing literature is not justified. They suggest, “Simply, when fees fall, poorer students are more likely to enroll.” This is simply incorrect.

Importantly, the chapter fails to recognize that in countries making the most difficult decisions about resource allocation, many children do not make it to the end of primary school, so will not benefit from fee abolition. As Figure 3.1 in the chapter shows, in low-income countries, less than 40 percent are enrolled in secondary schools, and only around 60 percent in lower-middle-income countries. The majority who are not enrolled are from poorer households. In sub-Saharan Africa, for example, drawing on our analysis from the latest Demographic and Health Surveys data, only around one-quarter of the poorest make it to the end of the primary cycle, compared with around 85 percent of the richest in the survey. Making secondary schooling free and constructing more secondary schools will make little difference to them. And, as noted, even if they do make it to the end of the cycle, they are less likely to be selected to government secondary schools where there is a need to ration places. So a number of the poorest will have no chance of benefiting from the abolition of secondary fees. As such, in such contexts, fee abolition results in a redistribution of public funds from the poorest to the richest.

Finally and crucially, in the context of limited resources, governments face trade-offs between universal versus targeted approaches. Their criticism in the chapter of a targeted approach, noting that "much-studied interventions such as cash transfer programs reach as few as 20 percent of Africans," misses the point that cash transfer programs are about redistribution to those most in need. A targeted approach has the potential to remove secondary school fees together with other costs of schooling for the poorest (as experience from Bangladesh and CAMFED's program in Tanzania show, for example). This is important because the poorest face cost barriers beyond fees that can be prohibitive, such as clothing, stationery, transport, and so on. For this reason, evidence from cash transfer programs highlights that the size of the transfer matters, such that a balance needs to be considered between the amount provided to each beneficiary and the number of beneficiaries who can be reached.

More generally, the chapter shows a lack of understanding of a progressive universalism approach. This approach is about prioritization—it does not say that ultimately secondary schooling should not be free. Indeed, that is the desired goal.

There are other aspects of the chapter that could be further critiqued (e.g., technical arguments around benefit incidence analysis, rates of return, price elasticity of demand, etc.—all of which have been heavily debated for several decades). But to do so would be beyond the scope of this short piece. It suffices to say that sweeping statements made in relation to these in the chapter are often not justified.

If the intention of the chapter is to provoke people like me, it has been successful. But the flippant dismissal of arguments made in other research that has evolved over the last few decades in relation to the debates on the effects on equity of fee abolition implies, at best, naïveté and, at worst, danger signs, given the influence that CGD has in some international policy circles.

Finally, I appreciate CGD's offer for me to write this brief response. In going forward, I hope that we can work collaboratively to assess ways to further strengthen the evidence base that addresses the challenges and opportunities to financing secondary education in a way that ensures everyone has the chance to access a good-quality education.

References

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Chapter 4. How Much Should Governments Spend on Teachers?

The effects of both raising teacher pay and reducing class sizes are small; the scope to increase access without reducing quality is big.

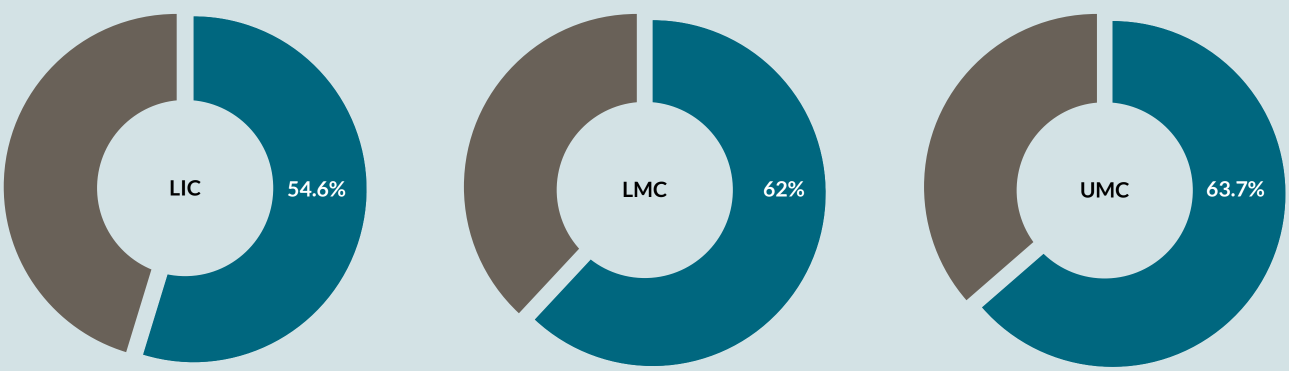
Lee Crawfurd and Alexis Le Nestour

More than half of all education spending goes on teacher salaries, so it is critical to consider how these funds are spent. In this chapter, we review the evidence on whether paying teachers more or hiring more teachers improves learning outcomes for students. Observed changes in teacher pay and class sizes have had little effect on student learning in low- and middle-income countries. This includes quasi-experimental evidence on salaries from Indonesia, Peru, and Zambia, and on class size from Bangladesh, Brazil, Chile, Kenya, India, and Uganda. Spending more on teachers seems more likely to raise student learning in richer countries, where teachers are better managed and supported. In the absence of effective management and support, increasing teacher pay seems unlikely to have large effects on increasing enrollment and learning. Allowing class sizes to rise may even be a low-cost way to increase student enrollment and time in school, with little deterioration in average learning levels. While additional spending on staffing is unlikely to improve learning for existing students, spending on new teachers will be necessary in most countries that still need to expand full-time enrollment to more students.

4.1 Introduction

How should countries spend their education budgets? Most education spending goes on teachers (Figure 1),

Figure 4.1. Share of education spending on teachers



Note: LIC stands for low-income countries, LMC for lower-middle-income countries, and UMC for upper-middle-income countries.

Source: Data from Crawfurd and Pugatch 2021.

so choices around how much to pay them and how many to recruit represent critical policy decisions. Ministers should base these decisions at least in part on the evidence of how much they matter for student outcomes. In this chapter, we summarize what we know from experimental and quasi-experimental studies on the causal effects of teacher pay and class sizes on student learning.

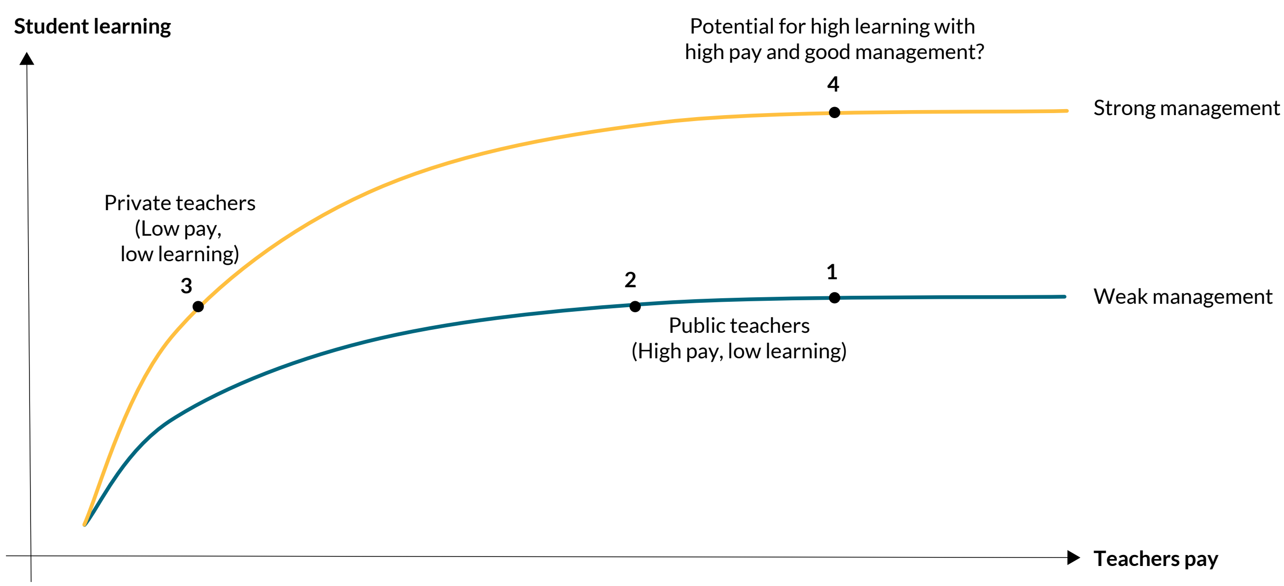
Increases in teacher pay have little short-run impact on student learning outcomes. Pay increases do allow for the recruitment of stronger new candidates over time, though the size of improvement is small.

Reductions in class sizes have led to only small average improvements in learning, and, in many studies, no improvements at all. Teaching style is a critical factor that mediates the effect of class size on student learning. The effect of marginal changes also depends on the initial class size and the type of contracts used to hire the teacher.

This does not mean that teacher pay and numbers don't matter. Rather, it means that effective management is a limiting factor. Paying teachers better is unlikely to improve outcomes unless it is accompanied by strong management and support. Most empirical studies on teacher pay and student learning involve marginal changes in pay in a context of weak management. Pay can make a difference in attracting better candidates and improving motivation, but only where this is a binding constraint. Figure 4.2 presents a simple stylized model of this relationship between teacher pay and student learning. Most empirical studies focus on movements along the flat section of the weak management curve, where teacher pay matters little (from point 1 to point 2).

Class size can matter in two cases: (1) when classes are already small, which occurs in high-income countries where school management is also more effective; and (2) when class size is reduced using teachers recruited and managed through a more effective process.

Figure 4.2. Diminishing effects of teacher pay



Note: This figure is a stylized representation of the evidence reviewed in more detail in the following section.

Points 1 and 2 represent the status quo for most public-sector teachers in developing countries—relatively high pay and poor learning outcomes, with diminishing gains to further salary increases (see de Ree et al. 2017). Point 3 represents the average contract teacher or low-fee private school teacher, who achieve similar student learning outcomes but at much lower pay through a stronger system of staff performance management (primarily because schools can make their own hiring and firing decisions). Point 4 speculates on what might be possible with high public-sector pay levels in a functional system of high support and accountability for teachers. One illustration of this might be East Asian countries where teachers are highly paid and students perform well on international assessments even with large class sizes.

If governance and management are poor, a government cannot improve the quality of education through spending on teachers alone. Inversely, marginally *cutting* spending on teachers could free up resources to reallocate to higher-return activities without substantially worsening learning outcomes. However, there is a limit to how far we can extrapolate from the marginal changes observed and studied in the empirical literature.

4.2 Teacher pay: More money doesn't improve learning

Teachers matter for student outcomes. Some teachers are much more effective than others (Bau and Das 2020; Crawfurd and Rolleston 2020). Raising teacher pay attracts more skilled individuals to the profession and increases the quality of teachers over time—but not by much. The main constraint to better teaching is not the quality of initial candidates, but the quality of their ongoing training and support. The effect of higher pay through attracting better candidates also clearly depends on the ability of recruitment systems to identify the best applicants, which remains a struggle for many systems–even those in high-income countries. On the other hand, if reducing teacher salaries does not affect the quality of teaching and learning, it might be possible to hire more teachers with the same budget.

We review the experimental, quasi-experimental, and cross-country evidence on the relationship between teacher pay and student outcomes. Evidence shows mixed effects of teacher pay on student learning. Studies from the US, UK, Norway, and Peru show that higher teacher pay *can* lead to improved student performance (Alva et al. 2017; Britton and Propper 2016; Gjefsen 2020; Nagler, Piopiunik, and West 2015). By contrast, several studies from low- and lower-middle-income countries show that (in some cases substantial) increases in teacher pay have led to zero change in student performance (Castro and Esposito 2017; Chelwa, Pellicer, and Maboshe 2019; de Ree et al. 2017; Greaves and Sibieta 2014; Pugatch and Schroeder 2018). One important contextual difference between high-income and lower-income countries is how pre-reform teacher pay compared to other workers. In most higher-income countries, teachers earn around an average wage, whereas in most lower-income countries, teachers earn two to three times average earnings (Figure 4.3).

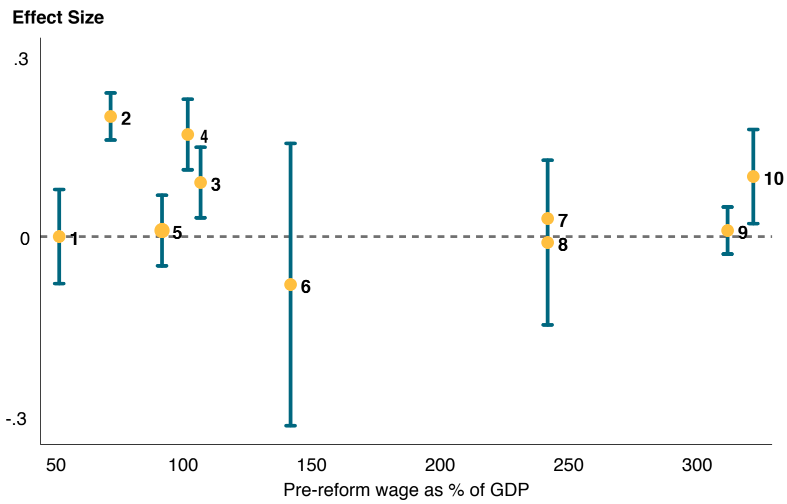
Experimental and quasi-experimental studies on teacher pay increases

First, a randomized controlled trial from Indonesia finds no effect of a doubling of teachers' salaries on student learning (De Ree et al. 2017). This increase focused on current teachers. Thus, this study rules out that higher pay leads to an immediate improvement in student learning—for example, through higher teacher motivation or a reduction in the need for a second job. However, it does not tell us if making teaching more attractive for new teachers will improve learning outcomes. Several studies instead use a regression discontinuity design to study differences in pay between geographic areas. For example, some countries pay teachers a hardship allowance for rural postings, which then puts individuals on either side of an arbitrary border that defines the edge of a rural location. These studies tell us more about the equilibrium relationship between pay and quality, accounting for the possibility that better pay attracts better teachers over time. These include studies from Zambia (Chelwa, Pellicer, and Maboshe 2019), the Gambia (Pugatch and Shroeder 2018), Peru (Alva et al. 2017; Castro and Esposito 2017), the UK (Greaves and Sibieta 2014), and Norway (Gjefsen 2020). Only two of these studies (from Peru and Norway) find a positive effect of teacher pay on learning outcomes, with the others showing statistically insignificant effects.

Finally, a third set of papers use within-country variation in relative teacher pay caused by economic recessions. Teacher pay is typically set centrally and changes slowly over time, whereas other private-sector wages have much greater variability both over time and in different local areas. These papers, mostly

Figure 4.3. Teacher pay has small effects on student learning

Meta-analysis of quasi-experimental teacher pay studies



1 = de Ree et al (2018), Indonesia
2 = Lazareva & Zakharov (2020), Russia
3 = Nagler et al. (2020), USA
4 = Gjefsen (2020), Norway
5 = Greaves and Sibieta (2014), England
6 = Cabrera & Webbink (2020), Uruguay
7 = Bobba et al (2021), Peru
8 = Castro & Esposito (2021), Peru
9 = Chelwa et al (2019), Zambia
10 = Tavares & Ponczek (2018), Brazil

Sources: Pre-reform wages as a percentage of GDP are taken from Lazareva and Zakharov (2020) for Russia; from Sandefur (2018) for Indonesia, US, Peru, and Zambia; and from the UNESCO Institute for Statistics (UIS) database for Brazil, Norway, and Uruguay. The full list of studies is included in Appendix 4A.

Note: This figure shows standardized effect sizes, adjusting the size of the treatment in each study to a 10 percent increase in salary.

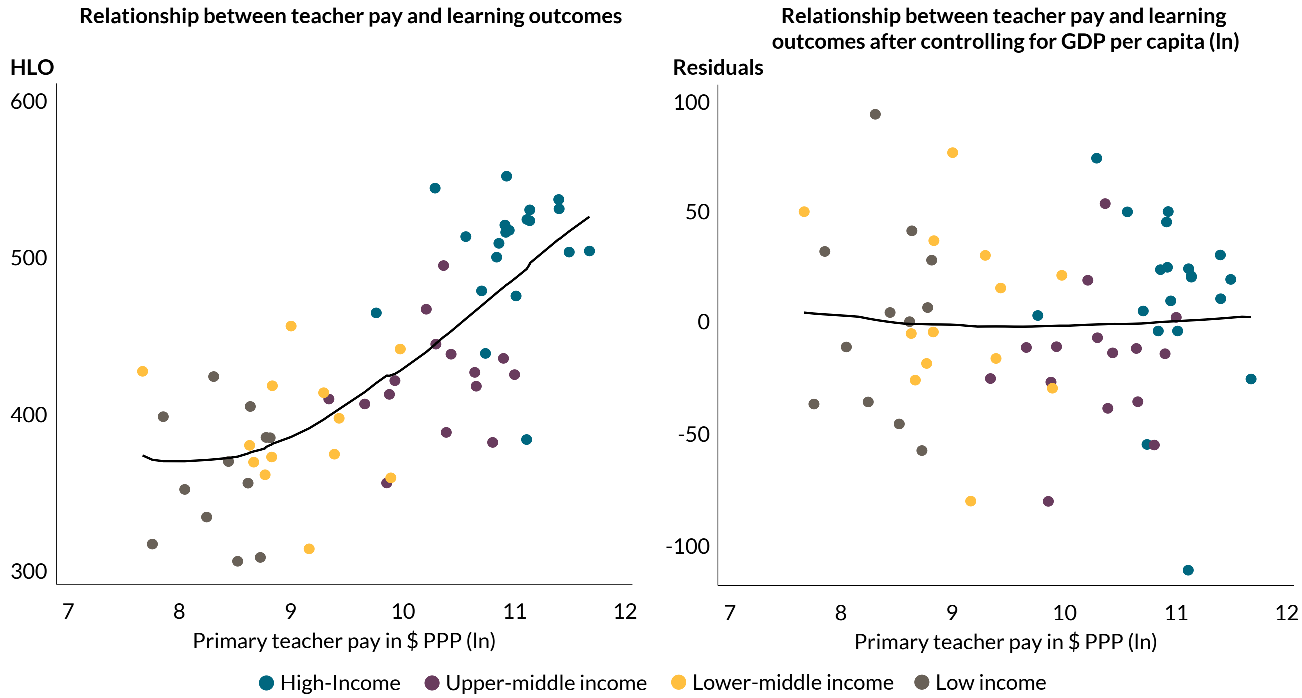
from high-income countries, show that during economic downturns, stable public-sector teaching jobs become more attractive, leading to higher-quality recruits and better student outcomes—in the UK (Britton and Propper 2016), US (Nagler, Piopiunik, and West 2020; Loeb and Page 1999) and Russia (Lazareva and Zakharov 2020).

Observational studies

Four studies consider the relationship across countries between teacher pay and student performance (Braga et al. 2020; Carnoy et al. 2009; Dolton and Marcenaro-Gutierrez 2011; Hanushek, Piopiunik, and Wiederhold 2019). Since they are based on large international learning assessments (PISA, TIMSS, and PIRLS), these studies are mostly limited to rich countries. Just one of them (Braga et al. 2020) includes any low- or lower-middle-income countries (Honduras, Indonesia, Iran, and Morocco). There may also be many omitted factors correlated with both teacher pay and student learning. However, bias aside, these studies might present a view of a long-term equilibrium between pay and outcomes after recruitment has had time to respond to changes in wages.

We extend this prior cross-country analysis to include 25 low- and lower-middle-income countries. We compare the average salary of teachers with harmonized learning outcomes, using UNESCO Institute for Statistics (UIS) and World Bank data (Figure 4.4). The unconditional relationship between teacher pay and learning outcomes is positive in our sample of 60 countries. However, this relationship disappears after controlling for (the logarithm of) GDP per capita. This suggests that, for a given level of income per capita, countries paying their teachers more do not achieve better results.

Figure 4.4. Cross-country variation suggests little relationship



Source: US dale for leacher pey and World Bank for Harmonized Laming Outcorne (HILO). Average of 2010's data. Average leacher pay is estimated by dividing total stall compensation by number of teachers. HLO scores are regressed on GDP per capila in log to obtain residuals for the right-side panel

Evidence from alternative contracts

Contract and private school teachers achieve similar learning outcomes to public-sector teachers, on much lower salaries. A substantial literature shows that teachers recruited at much lower salaries in developing countries achieve similar learning outcomes (Chudgar, Chandra, and Razzaque 2014; Bau and Das 2019). This includes public-sector teachers on temporary contracts and private-sector teachers receiving market wage salaries (usually well below civil service salaries). One concern with paying lower wages for contract or private school teachers is that they may only be willing to work for a lower salary temporarily, in the hope that they can then later obtain a permanent and higher-salary civil service job. One test of this hypothesis is to look at the observed distribution of teacher tenure by contract status. Data from the World Bank STEP Skills survey shows that this is the case in some but not all countries. In Bolivia, Colombia, Georgia, and Ghana, median teacher tenure is lower for teachers on temporary contracts than those on permanent contracts. But in Armenia, Sri Lanka, Vietnam, and China's Yunnan province, median tenure is longer for those on temporary contracts.

Overall, it is difficult to draw conclusions about the potential effects of increases in teachers' pay from this literature. Findings from higher-income countries tend to show positive outcomes, but these do not seem to be replicated in lower-income countries.

Why doesn't pay have bigger effects?

Differences in the effect of teacher pay reflect broader system features. In countries with weak or no systems in place to select, train, support, motivate, and reward teachers, pay alone is much less likely to be very important. Higher pay might attract better candidates, but if the school system is unable to select the best candidates or reward them when they are performing well, it is unlikely that raising pay will lead to better educational outcomes. For instance, six years after doubling teacher pay in Indonesia, only half of new recruits had a bachelor's degree, despite there being no shortage of college graduates with a teaching degree (De Ree et al. 2017). Thus, if the best applicants are not selected, increasing teachers' pay may not lead to better students' outcomes, even if the pool of applicants improves following the reform.

At least as important as the level of teacher salaries is their structure. One approach to understanding the structure of teacher pay is the World Bank Systems Approach for Better Education Results (SABER) project. The project scores countries according to four criteria, which include

1. the level of starting salaries,
2. whether pay varies according to performance,
3. whether there is scope for career progression, and
4. whether promotion is merit-based.

For each criteria, a country's policy is scored on a four-point scale, where the weakest score of 0 is "latent" or no effective practice, 1 is "emerging," 2 is "established," and the strongest score of 3 is "advanced." On this measure, high-income countries average a score of 1.8 (close to "established"), and low-income countries average 0.5, halfway between "latent" and "emerging."

Formal performance-related pay schemes have been shown to boost learning in low- and middle-income-countries, at least over relatively short time horizons (Leaver et al. 2021; Muralidharan 2011). It is unclear if such schemes linking teacher pay to student exam results could be sustained at scale and over time without succumbing to some form of cheating or gaming. One study of a large program implemented by the government in Punjab found no effect on student learning (Barrera-Osorio and Raju 2017). But relatively few performance pay schemes have been scaled or sustained nationally, even in areas where pilots showed positive impacts (Breeding, Béteille, and Evans 2021).

The SABER project also scores countries according to their broader teacher policy. This framework helps to understand the different policy levers that can be used to improve teacher's effectiveness, of which pay is just one. It is made up of eight categories: (1) clear expectations of teachers; (2a) attractive starting pay, (2b) competitive entry requirements, (2c) working conditions, and (2d) opportunities for career progression; (3) teacher training; (4) allocation of teachers where they are needed; (5) empowerment of school principals to support teachers and monitor their work; (6) systems in place to measure performance; (7) systems to support teachers who are in need of professional development; and (8) incentives to reward teachers and hold them accountable.

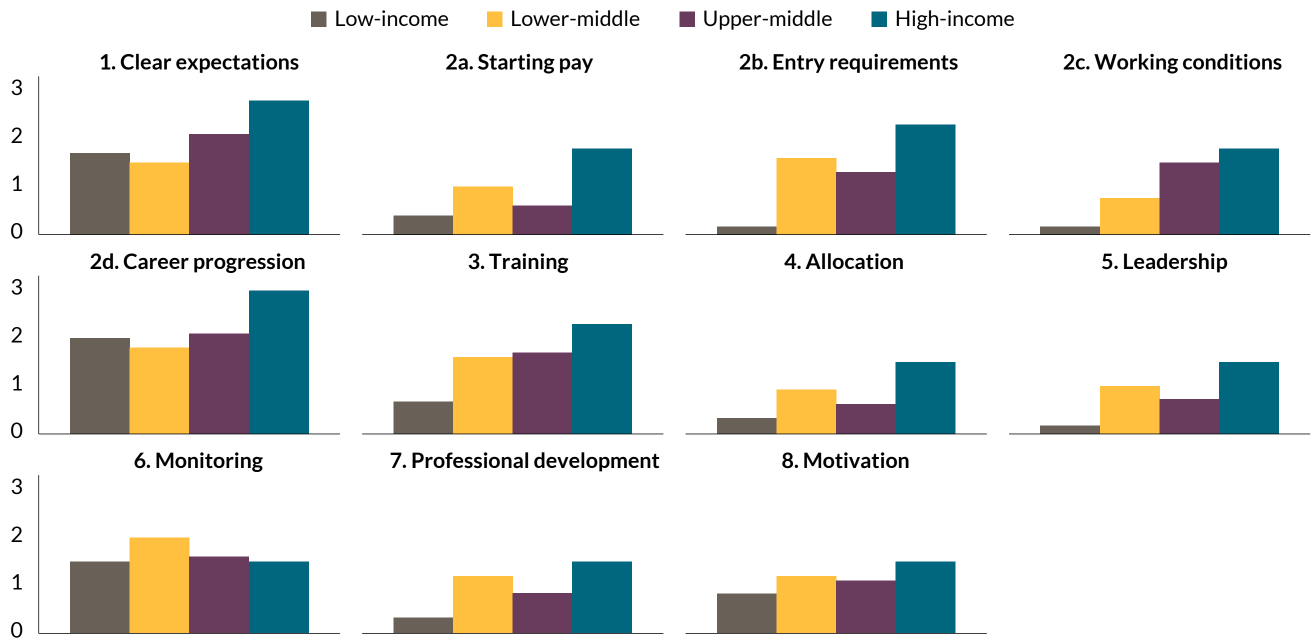
All of these factors are likely to contribute to the teachers' effectiveness. But there is not enough research to know the relative importance of each factor and how they interact with each other. What is clear is that low-income countries score worse than higher-income countries across a whole range of different aspects of teacher policy, not just pay (Figure 4.5). Across almost all components, lower-income countries score worse than higher-income countries. Countries scoring low on SABER aggregate score also tend to have less attractive teacher pay, but increasing teacher's pay may not be enough to improve these countries' scores without strengthening other components of teacher policies.

These issues might explain why increases in teacher pay do not seem to lead to better outcomes in low-income countries but do in high-income countries. Higher pay might only be effective when other features of the system are working properly.

Pay increases may also make little difference where other employment options are poor. Formal jobs are scarce in most low-income countries. This means that teaching is a relatively secure job, already attracting some of the best candidates. The World Bank STEP surveys show that teachers are already selected from the upper end of the skill distribution in developing countries (Figure 4.6). In addition, teachers are already paid well above GDP per capita (Figure A1).

Figure 4.5. Good teacher policy goes well beyond pay

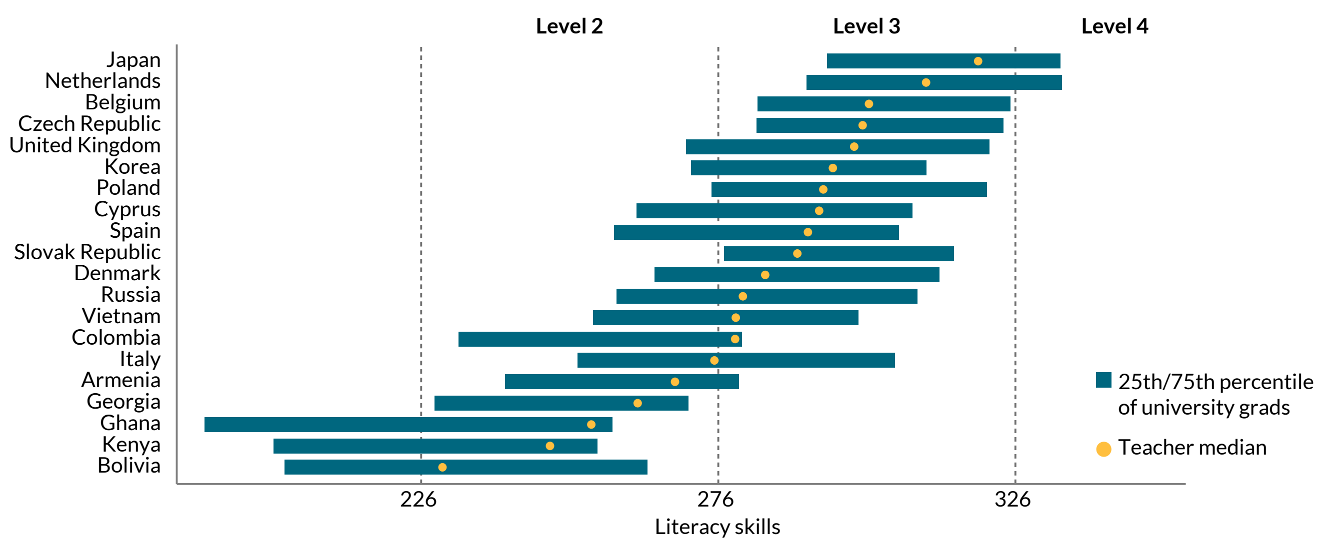
SABER teacher policy scores by country income level



Note: This figure shows the average score given by SABER to the different components of the framework broken down by income groups. The component "attracting the best into teaching" was broken down into its subcomponents to show the separate rating of other policies aiming at attracting skilled individuals into teaching. Individual policies are scored on a scale from 0 to 3 based on SABER assessment (0=latent, 1=emerging, 2=established and 3=advanced).

Figure 4.6. Teachers are already among the best educated

Literacy scores for teachers and other university graduates



Note: This figure shows the literacy skill level of teachers and university graduates in selected countries. Data for Armenia, Bolivia, Colombia, Georgia, Ghana, and Kenya are from the World Bank STEP survey. Data for other countries is from the Organisation for Economic Co-operation and Development (OECD) Programme for the International Assessment of Adult Competencies (PIAAC) survey.

There may be other reasons to pay teachers more: Indirect effects of changing teacher pay

Teachers can make up a large share of qualified jobs in poor countries—40 percent in the case of women. Increasing or decreasing teacher pay can change people's incentive to acquire education. An increase in teacher pay might increase the potential benefits to completing education. If we assume that increasing teacher pay will not change the number of available qualified jobs or change the wage of nonteaching qualified jobs, and that teaching jobs pay roughly the same as other qualified jobs, a 10 percent increase in teachers' wages would result in an increase of 4 percent of the returns on schooling. Increasing teacher pay should then be evaluated in relation to the opportunity cost of attracting talent from other sectors. Individuals attracted to the teaching profession because of higher wages might have been more productive in other public or private jobs.

Higher pay might increase retention. Because teachers improve with experience, reducing attrition is one route through which higher pay might improve student outcomes. Low teacher pay in Rwanda contributes to one in five teachers leaving their job each year (Zeitlin 2021). But in general, low-income countries have lower teacher attrition rates than high-income countries, so attrition is unlikely to explain the difference in performance between them. If the sole objective of raising teacher pay is to reduce teacher attrition rates and have a more experienced teaching force, it is unlikely that it will have a big impact on student performance.

In summary, our review of the evidence on the links between teacher pay and student performance finds mixed results. Higher pay can improve results in some contexts, but it is by no means a guarantee, particularly in education systems with weak management and support for teachers. In the next section, we put teacher pay aside to discuss what the evidence suggests is likely to be achieved by increasing spending on additional teachers to reduce class sizes.

4.3 Class sizes: Smaller isn't necessarily better

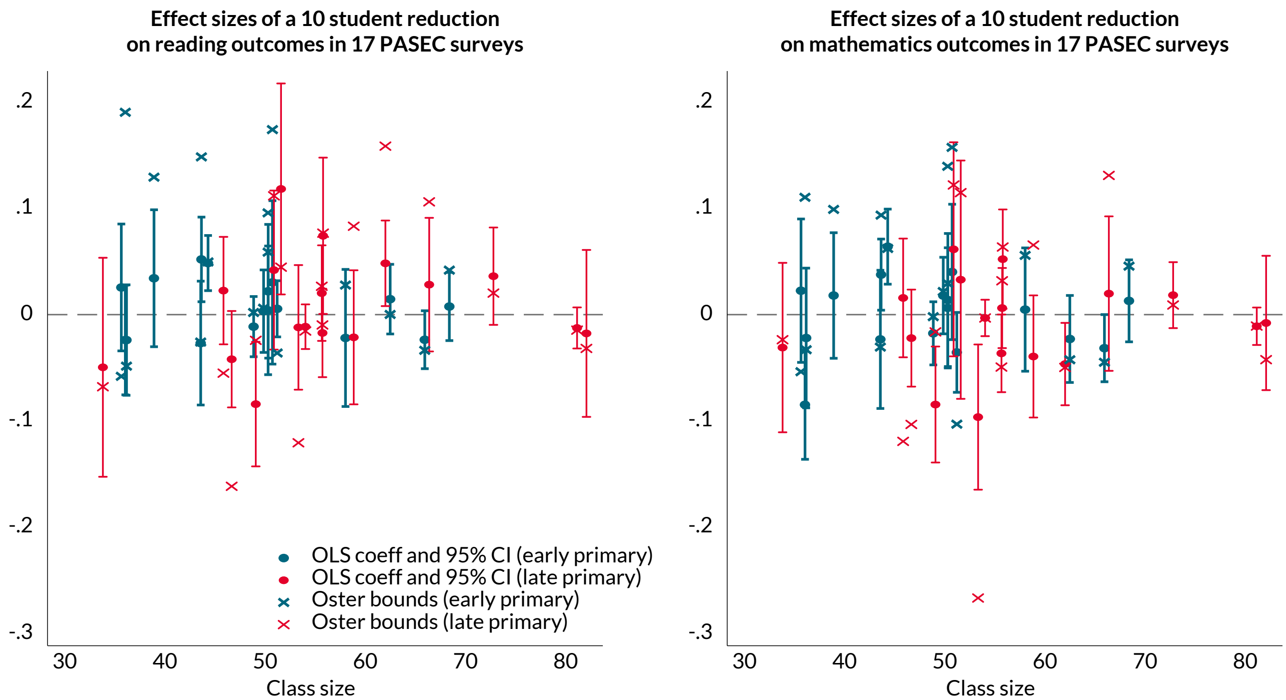
Class size reductions in developing countries have led to little improvement in learning. The average effect of a reduction of ten students (across 33 estimates) is a 0.05 standard deviation improvement in learning. This is a small effect by most standards.

We complement the literature review with new observational estimates of class size effects from 17 countries. These new estimates increase the overall number of estimates and the number of countries covered to support the findings of small class size effects. We used the 2014 Programme for the Analysis of Education Systems (PASEC) survey that includes 10 sub-Saharan African countries, as well as seven older PASEC surveys (Senegal 2007, Burkina Faso 2007, Burundi 2009, DRC 2010, Chad 2010, Vietnam 2010, and Lao 2010) that include test scores at the beginning and end of the year and allowed us to estimate value-added models (though there are no pretest data in DRC). We estimate the same model for all countries, explaining learning outcomes in mathematics and reading as a function of class size, adjusting for student, teacher, and school characteristics. PASEC collects data in grades 2 and 6 (grade 5 before 2014, and grade 4 in Lao), and models for mathematics and reading were estimated separately.

Just over half of the estimates are positive (36 of 68), and eight are significant at the 5 percent level (Table 4.7). Average effects are small and not statistically different from 0 for both reading (0.01) and mathematics (-0.01). Effects are highly variable between different countries, grade levels, and subjects.

Figure 4.7. Class size effects vary substantially

Observational estimates of class size effects for 17 surveys



Note: In this figure we present both ordinary least squares (OLS) estimates and Oster bounds (OBs). OLS estimates are likely to be biased by unobservable variables. Larger class sizes may reflect a higher demand for certain schools because they are better at raising learning outcomes. To estimate how this bias can affect our estimates, we used OB, a method that quantifies the potential bias of OLS estimates by comparing OLS estimates with and without controls and making assumptions on the relationship between the variance explained by observable and unobservable variables. We followed Oster (REF) and set the R2 to 1.3 the R2 of OLS estimates and the delta to 1. OBs give us an estimate of the maximum effect of class size we could expect if we could control for all unobservable variables. Here, OLS coefficients, their 95 percent confidence intervals, and Oster bounds are plotted against class size. In most cases (39 out of 68), OBs are larger than OLS estimates, suggesting that failure to control for unobservable variables might decrease the effect of class size on learning. There is a high level of variability in OB estimates and, overall, average effects for reading (0.02) and mathematics (0.01) are larger than with OLS estimates, but they do not suggest that the true causal effect of reduction of class size on learning outcomes is large.

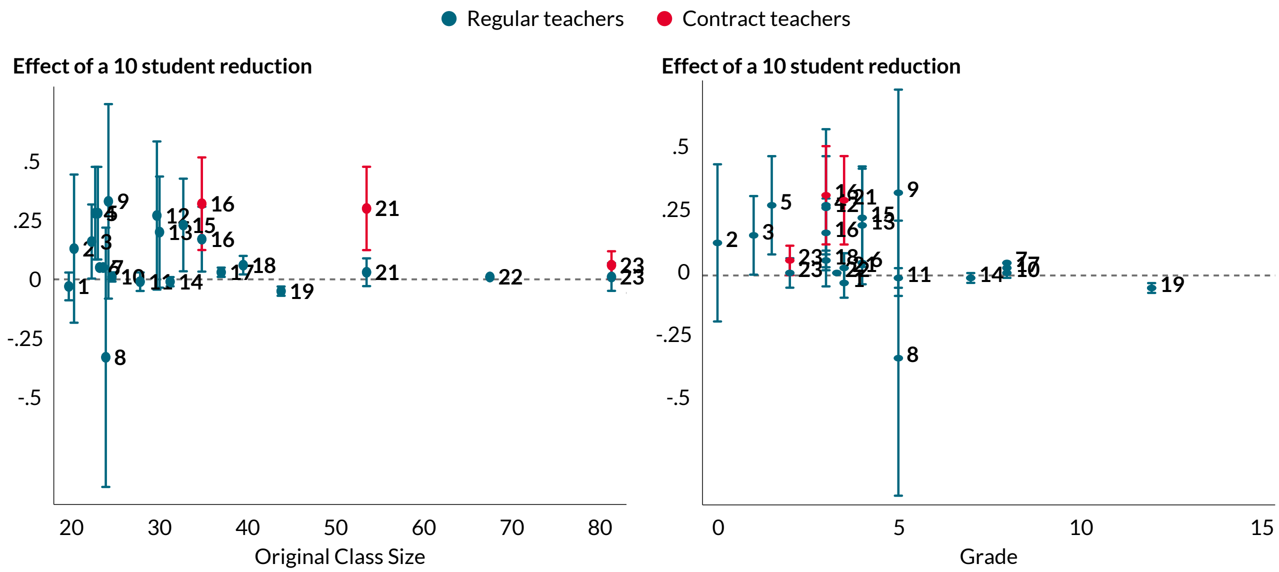
What mediates the effect of class size on learning?

The starting size of the class

Going from 80 to 40 students in a class may make little difference because it is impossible to engage students meaningfully in individualized instruction when class sizes remain very large, even after a reduction. In our meta-analysis (Figure 4.7), we find a nonlinearity, with class size reductions mattering more in smaller classes. The average effect is slightly larger for classes that had started with fewer than 40 students (0.07 standard deviations), and null or negative for classes larger than 40 students. This is consistent with a model in which class size matters for learning when teachers aim to check for understanding and give children individual feedback. When teachers simply lecture, class size doesn't matter. This is the opposite pattern to the one used in some modeling exercises, such as UNESCO Institute for Statistics (2015) and the International Commission on Financing Global Education Opportunity (2016). The Education Commission's Learning Generation report argues that the "marginal benefits [of further reductions] drop once a class size of around 40 is reached." A recent attempt to directly model these nonlinearities with data from India suggests that class size has little effect on learning below a threshold of around 50 students, but class size increases above this are associated with worse performance (Datta and Kingdon 2021a).

Figure 4.8. Class size reductions matter most in small classes

Meta-analysis of quasi-experimental studies on class size



1 = Angrist et al (2017), Italy
2 = Milesi (2006), US
3 = Ecalle (2006), France
4 = Bressoux et al (2009), France
5 = Krueger (1999), US (STAR)
6 = Cho et al (2012), US
7 = Shen & Konstantopoulos (2019), Hungary, Lithuania, Romania, Slovenia
8 = Gerritsen (2017), Netherlands
9 = Fredriksson, Äckert & Oosterbeek (2013), Sweden
10 = Leuven et al (2008), Norway
11 = Angrist et al (2019), Israel
12 = Urqiola (2000), Bolivia
13 = Angrist & Lavy (1999), Israel
14 = Matavelli & Menezes-Filho (2020), Brazil
15 = Urquiola & Verhoogen (2009), Chile
16 = Muralidharan & Sundararaman (2013), India
17 = Altinok & Kingdon (2011), 18 Developing Countrie
18 = Muralidharan & Sundararaman (2013), India
19 = Datta & Kingdon (2021), India
20 = Asadullah (2006), Bangladesh
21 = Banerjee et al (2007), India
22 = Jones (2016), Uganda
23 = Duflo, Dupas, Kremer (2015), Kenya

Note: This figure collects all estimates of class size effects from experimental and quasi-experimental studies. Effect sizes are adjusted to the expected effect for a reduction of 10 students. In practice the change in students in each study varies. Treatment in Banerjee et al. (2007) was combined with the application of a "teaching at the right level" curriculum developed by the NGO Pratham. The full list of studies is included in Appendix 4B.

The PASEC data does not show bigger effects in small classes. Class sizes are large in the 17 PASEC surveys, with an average of 53 students per class. The lack of effects of class size reductions in the PASEC data is thus consistent with the estimates from the literature in the meta-analysis, demonstrating that marginal reductions matter less when the starting point is large.

Grade level

Class size could matter more in earlier grades in which students need more guidance and less in higher grades in which children are more capable of independent learning. We see some support for this possibility from our meta-analysis, with large effects for studies covering primary school grades and no effect for the smaller number of studies in secondary schools. This could be a particular issue for some countries that have much larger enrollment in the first grades of primary school. For example, in Malawi class sizes average at over 150 students in grade 1 but "only" 75 students in grade 7 (Bashir et al. 2018).

Teaching style

If teachers are trying to engage in individual interaction with students, then class size clearly matters. But if a teacher is simply giving a lecture at a blackboard, then class size barely matters at all. In more effective school systems, evidence from the US, Israel, and Bolivia shows that reducing a class size by eight to ten students can improve average test scores by between

0.04 and 0.27 standard deviations (Figure 4.8). Where teachers deliver lectures—in Kenya, for example—reducing class sizes from as much as 82 to 44 students had no effect on average test scores (Duflo, Dupas, and Kremer 2015). By contrast, in the very same context, combining a smaller class size with an incentivized teacher who is trained in delivering individualized instruction by taking formative assessment seriously (using a "teaching at the right level" curriculum) can have large effects.[1] We don't have good evidence on whether it is the incentive and training or the type of teaching that is most important, just that the combination matters.

One (imperfect) measure of the degree of personalization of instruction is the policy on classroom assessment. The first step toward good, individualized instruction is an understanding of each child's learning level. The World Bank SABER project collects data on classroom assessment policies and practices. Countries are graded on the degree to which classroom assessment is practiced effectively. On average, high-income countries in the sample have moderate-quality classroom assessment practices (2.8 on a scale from 1 to 4), and low-income countries have weak classroom assessment practices (1.7 on the same scale).[2] This lack of classroom assessment is likely both a consequence of large class sizes and a reason that reductions in class size has little effect.

Poor deployment of teachers

In many countries, teachers are not well allocated in related to student numbers. For instance, one analysis suggests that in Ghana, as little as 40 percent of variation in teacher numbers across schools can be explained by variation in student numbers (Béteille and Evans 2021). Another analysis in India suggests that inflated student enrollment data and inefficient allocation of teachers has led to an excess of close to 350,000 teachers (Datta and Kingdon 2021b). Without a strong monitoring system and an enforcement of effective teacher allocation rules, a policy aimed at hiring more teachers to reduce class size may not even manage to do so. Any benefits of class size reduction might even be achieved by redistribution of existing teachers and/or students between schools. There are substantial differences in class sizes even within countries (Walter 2019). The net effect of any changes would depend exactly on the nonlinearities—whether effects of size change depend on the starting size. This again highlights the importance of effective management of teachers.

One challenge with deployment of teachers is that there are many schools—especially those in remote or high-poverty areas—where many teachers are unwilling to teach at current levels of compensation. Independent of the impact of class size on student learning, certain schools struggle to keep enough teachers employed to stay open. In those cases, hiring more teachers or compensating them more to increase a willingness to offer any education in the most disadvantaged areas may be the only option (Box 4.1).

1. Duflo, Dupas, and Kremer (2015) find that experimentally lowering class size from 82 to 44 students in Kenya had no immediate effect on average learning outcomes. Banerjee et al. (2007) find similar results from reducing class size from 40 to 20 in Mumbai and Gujarat. Asadullah (2005) uses a government cap on class sizes at 60 as exogenous variation to estimate the effect of smaller classes, finding that at the margin smaller classes have worse exam results. These results stand in some contrast to earlier studies from the US STAR project, as well as rule-based quasi-experimental studies from Israel and Bolivia, that do find positive effects (Figure 4.8).

2. The high-income countries in the sample are Bahrain, Brunei Darussalam, Kuwait, Oman, and United Arab Emirates. Upper-middle-income countries are Armenia, Iraq, Jordan, Kazakhstan, Lebanon, Libya, North Macedonia, Serbia, and Sri Lanka. Lower-middle-income countries are Angola, Egypt, Ghana, Kyrgyz Republic, Mauritania, Morocco, Pakistan, Sudan, Tunisia, Vietnam, West Bank and Gaza, and Zambia. Low-income countries are Ethiopia, Mozambique, Nepal, Syrian Arab Republic, Tajikistan, Uganda, and Yemen.

Box 4.1. Making sure every student has a teacher—and what that means for student learning

This box was contributed by David K. Evans and Amina Mendez Acosta

While teacher pay or class size may not be binding constraints in many settings, there are schools where just getting enough staff to function is a challenge. For example, schools in urban neighborhoods with high concentrations of poverty and schools in remote areas are often last on teachers' list of choices. With secondary school enrollment rates of just 34 percent in low-income countries and 60 percent in lower-middle-income countries, achieving universal basic education will likely require more teachers in many countries. (According to the World Development Indicators for 2018, one in five school-age children in low-income countries remain unenrolled in primary school.) Furthermore, because rural areas have the highest concentration of children not enrolled in school, more teachers will be needed in schools and communities that lack the amenities that teachers want.

Beyond the likely need for more teachers as enrollment numbers expand, teachers are often not allocated according to where students are given the current student populations (Datta and Kingdon 2021b). In Ghana, only 40 percent of the variation in teacher allocation across schools can be explained by student numbers (Bashir et al. 2018). There are many contributing factors, but one is that many teachers do not want to teach in certain schools. This challenge manifests itself across countries and regions. A survey in Peru found that 24 percent of teachers currently working in urban areas would not accept a posting in a rural school under any circumstances (Castro and Esposito 2021). In Zambia, about twice as many teachers leave rural postings each year relative to urban postings (Chelwa, Pellicer, and Maboshe 2019). The problem extends beyond the quantity of teachers to teachers' characteristics; in the Gambia, there are far fewer female teachers in hardship schools, and in Chile, teachers in rural schools tend to have lower test scores (Pugatch and Schroeder 2014; Elacqua et al. 2019).

How can education systems staff these schools? Policy levers in use in different countries include financial bonuses for teachers, additional training, subsidized housing, a faster route to promotion, or mandatory rotations. Most of the evidence focuses on the impact of providing hardship pay to teachers in challenging schools. Within those programs, most yielded positive impacts on teachers, either increasing the placement of teachers in hard-to-staff schools or reducing turnover from those schools. Even more encouraging, many of these programs delivered positive impacts on student learning, sometimes driven by gains among particular groups (such as low performers in Brazil or boys in Zambia). But these programs aren't cure-alls; several struggled with implementation challenges such as late payments, out-of-date incentives that no longer held much value, or a failure to communicate about the program with potential applicants (Evans and Acosta 2021).

Recently, education systems in Ecuador and Peru have experimented with lower-cost interventions, such as providing more information about bonus programs, appealing to teachers' altruistic motivations, or simply placing hard-to-staff schools higher on the list of schools that teaching applicants can choose from (Ajzenman, Bertoni, et al. 2021; Ajzenman, Elacqua et al. 2021). Each of these had positive impacts on getting teachers to hard-to-staff schools and thus was extremely cost-effective, since the interventions cost very little.

Box 4.1. Continued

Whether we consider higher-cost (but higher-impact) interventions like hardship pay or lower-cost interventions, the supply increases they do induce—while making schooling accessible for more rural children—are unlikely to close the gap between urban and rural schools (Bobba et al. 2021).

Much of the evidence in this chapter suggests that blanket increases in teacher salaries or hiring new teachers to reduce class sizes are unlikely to result in large, immediate gains in student learning. Teacher salaries already make up the bulk of education spending. But one expansion of teacher spending that may be unavoidable includes benefits—whether financial or in-kind, such as housing—if education systems expect to provide basic education for every child.

4.4 Limits to the evidence

The evidence reviewed thus far suggests that overall, increases in teacher pay and reductions in class size are not reliable means to improve student learning, particularly in contexts of weak management and support systems. But there are limits to what we can learn from the evidence.

First, there may be nonlinear effects. A marginal 10 percent salary increase (or even a 100 percent increase, as in Indonesia) might have small effects. But this does not necessarily mean that larger changes would not have larger effects. (That said, few education systems—especially in low- and middle-income countries—have the fiscal space to incorporate dramatic increases to teacher salaries, so the question may be theoretical rather than practical.) These changes may not be symmetrical; salary cuts could have larger effects than increases. There are limits to how much we can generalize from the evidence to more extreme potential changes in pay or class size. In the case of class size, a smaller class may be necessary but not sufficient for improved teaching and learning.

Second, there may be longer-term composition effects. We argue that, at present pay levels, teachers are generally well represented in the overall distribution of skilled adults. Large changes in pay might have small effects on the performance of current teachers but change the composition of incoming recruits, or they could cause current teachers to leave the profession.

Third, the research reviewed so far focuses on the effects of teacher pay and class size on learning. There is little research of the effect of class size on other important outcomes, such as dropout or child well-being.

4.5 Conclusion and implications for policy

In this chapter we have reviewed the empirical literature on investment in teachers and student outcomes. Any policy analysis for a government trying to understand how it can achieve education goals should be informed by this evidence.

The main conclusions from the empirical literature reviewed suggest the following:

- Class size and average teacher pay make little difference to student outcomes in most low- and middle-income countries.
- Class size and teacher pay can improve outcomes more substantially, but only if effective systems of assessment and teacher management are in place (which they often aren't).
- The effect of teacher pay on learning is also effectively zero where systems of teacher management are weak. Where teacher management is

functional,[3] average effects may be on the order of 0.05 standard deviations per US$1,000 increase in pay (in purchasing power parity (PPP) terms), based on findings from richer countries. This effect is hardly competitive with other possible investments to improve educational outcomes.

- The effect of class size on learning is effectively zero in contexts with weak pedagogy. Where student assessment is functional,[3] effect sizes are on the order of 0.2 standard deviations per 10-student reduction in class size.
- There is too little research to determine whether effects vary by level of schooling (preschool, primary, or secondary), or by student socioeconomic status.

A government focused on improving student enrollment and learning outcomes could therefore afford at the margin to prioritize investment elsewhere. In fact, allowing class sizes to rise could therefore be a relatively low-cost means to expand access to schooling. While increasing spending on teachers is unlikely to improve learning for existing students, expanding access to school, particularly secondary school, will require investment in recruiting new teachers. The question of how this should be done—whether through new recruitment of teachers at existing public pay scales or through public-private partnerships that can reduce costs through recruitment at lower private salaries—is discussed in chapter 5 of this volume.

3. We define functional teacher management and student assessment as achieving an "established" score on SABER (an aggregate SABER score of 20 or above).

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Chapter 4 Appendices

Appendix 4.A. Studies on teacher pay included in Figure 4.3

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Appendix 4.B. Studies on class size included in Figure 4.8

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Comment: It’s probably premature to conclude that teacher pay or class size doesn’t matter

Tessa Bold

In this chapter, the authors review the evidence for the effect of spending on teachers to improve student learning. For this the authors focus on two margins of spending: the extensive margin—that is, hiring more teachers and thus reducing class size (or increasing enrollment)—and the intensive margin—increasing the pay of existing teachers (possibly tied to incentives).

The authors conclude that on both dimensions, the evidence shows that there is only a weak relationship—if any at all. The authors then offer some explanation for why there is no such relationship between teacher pay/class sizes and learning, when any sensible education production function suggests that there should be. The authors helpfully point out that teacher pay and class sizes can make a difference, but only in a context where they are binding constraints. Since the authors do not find large positive effects of reducing class size and increasing teacher pay, they suggest in the introduction that on the flip side it may be possible to reduce spending on teachers (either by employing fewer teachers, paying them less, or enrolling more students) without reducing learning levels.

Teacher pay rates

In the discussion of teacher pay, the authors mainly draw upon the experimental literature on teacher pay, namely, the Indonesian teacher pay experiment, which showed zero effect of an increase in teacher pay on student learning. While there is no immediate effect on teacher motivation in this experiment, it may be worth mentioning that Indonesian teachers had very low wages both before and after the reform (in contrast to the “average” evidence the authors cite that teachers in low-income countries tend to be in the upper part of the pay distribution).

Beyond these immediate effects, there may also be more long-term effects, where higher pay attracts better candidates/teachers into the sector. To illustrate this, the authors cite evidence from regression discontinuity designs, but I think both Brown and Andrabi (2021) and Leaver et al. (2020) are relevant experimental papers here that have important findings about the relationship between teacher (performance) pay and selection.

I liked the analysis in Figure 4.4 a lot. Sometimes it is good just to take a step back and examine the macro evidence, and I think the finding that there is effectively no relationship between teacher pay and achievement once GDP is controlled for is an interesting and important one. I would perhaps even start with this evidence (since it reflects the micro evidence quite well).

The authors then also examine the evidence for learning outcomes when teachers are on alternative contracts (often short-term or otherwise incentivized contracts) on substantially lower pay. The evidence shows that these teachers do not perform substantially worse. So, if increasing teacher pay does not do the trick, this evidence at least suggests that the same quality of education could be delivered at substantially lower cost. There are, however, good reasons to think that this may not be the case. The authors hint at this, but could elaborate this point further. For example, Figure 4.2 seems to suggest that countries can choose to locate at point 3 or 1 and 2, but there are good reasons to think that location 3 only exists because teachers in private schools are queuing for the positions in 1 and 2, so if 1 and 2 do not exist, neither does 3.

Why hasn't teacher pay had a larger effect on student learning? To address this question, the authors use the World Bank Systems Approach for Better Education Results (SABER) indicators, which try to give a system-level measurement of how well education systems around the world function. Here, they focus on the "good teacher policies," an indicator that consists of clear expectations, starting pay, entry requirements, working conditions, career progression, training, allocation, leadership, monitoring, professional development, and motivation. Low-income countries in general score lower than high-income countries on these dimensions and the different components of the indicator are correlated, so that countries that score low overall also tend to have low teacher pay.

Regarding the exposition, I think it would be useful to point out that the SABER indicators present de jure (rather than de facto) measures of teacher policy. In poorer countries, which may be characterized by "institutional isomorphism," there may be a big gap between the two. Second, as the authors point out, there is little evidence to show that their components of "good teacher policies" they examine are highly correlated with teacher effectiveness. For example, much evidence on teacher training suggests that it is not very effective. I'd therefore suggest to also draw on additional direct measures of effective school management as in the World Management Survey (WMS) and, for example, Lemos, Muralidharan, and Scur (2021), or other work by Scur and coauthors.

Class size

In the class size part of the chapter, the authors begin by reviewing evidence of class size effects in low- and middle-income countries, drawing on the PASEC surveys. Interestingly, and in contrast to perceived wisdom, the authors find that there is a class size effect in smaller classes, but not in larger classes.

The authors then speculate why there is no relationship between class size and student learning and note that this could have to do with teaching style. To support this explanation, they draw on SABER measurement of whether countries practice classroom assessment effectively, and note that there is variation across countries. Two comments: (1) as above, I would complement this evidence with direct measures of classroom practices in low-income countries as documented in Bold et al. (2017). (2) since the authors do not show that class size reductions and classroom assessment are correlated (or for that matter how classroom assessment is correlated with learning), the argument feels a little incomplete.

In general, I think it would be useful to not just draw on teaching practices. The same conclusion can be made for teacher knowledge—that is, if teacher knowledge is very low then reducing class size (or in general putting more teachers in schools by reducing absence for example) will have little impact on learning. There is information on this in Bold et al. (2017) and Bold et al. (2019).

Poor deployment of teachers

The authors argue that in many countries the allocation of teachers to schools is very inefficient. This can imply very unequal class sizes and that target class sizes could be achieved simply by redistributing teachers. This may well be true, but if class size does not have a significant effect on learning, then that seems to be a second-order concern.

A more important concern is that some (remote) schools may struggle to hire teachers at all. They cite a number of studies that attempt to remedy this problem and find impacts both from high- and low-cost studies. However, these impacts are not large enough to fill all the positions. From this, the authors conclude that "one expansion of teacher spending that may be unavoidable includes benefits—whether financial or in-kind, such as housing—if education systems expect to provide basic education for every child." This may well be the case, but it seems a bit of an ad-hoc conclusion given the arguments nd evidence presented in this section.

Conclusion and summary for policy

The authors conclude that class size and teacher pay can improve outcomes more substantially, but only if good systems of assessment and teacher management are in place. While this seems obvious, it is not clear to me how this follows from the evidence presented in the chapter.

From the class size effects, I did not get the sense that there is a strong relationship between assessment systems and class size effects, and even if this were the case, it is not possible to conclude from this that there are class size effects only if good systems of assessment and teacher management are in place. I am now wondering whether something is missing in the class size subsection; it seems the authors refer in the conclusions to results that have been established there, but that do not appear in the chapter.

Coming back to the opening statement "the scope to increase access without reducing quality is big." Yes, perhaps, bearing in mind that status quo levels of education quality in many countries are simply very low.

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Comment: Teacher allocation and teaching methods matter as much as class size and pay levels

Esme Kadzamira

The chapter raises important issues relating to teacher pay and class size which have policy implications for low-income countries who are faced with the challenge of whether to raise teacher salaries amidst industrial action demanding higher teacher pay. In some countries, there is also a widely perceived view that teachers are lowly paid compared to similar professions in the public sector. In addition, the need for additional teachers is huge in many low-income countries, especially those that still need to universalize their enrollments. The COVID-19 pandemic has also placed higher demand for extra teachers who are needed to decongest classrooms in countries like Malawi where class sizes above 100 are not uncommon.

Using available evidence from experimental and quasi-experimental and cross-national studies, the chapter explores the relationship between teacher pay and class size on student learning outcomes. The chapter is of great importance to policymakers as it brings together evidence from different empirical studies that have examined the effect of teacher pay and class size on student learning. From the evidence drawn from the reviews, it is clear that on their own neither increasing teacher pay nor reducing class size can lead to better student learning outcomes. Rather it is the effective management of teachers that is crucial in ensuring that teacher pay has a positive impact on student learning. Similarly, in the case of class size, the chapter argues that teaching styles might be the enabling factor that mediates the impact of class size on student learning.

The authors observe that "for a given level of income per capita, countries paying their teachers more do not achieve better results." This observation is particularly true in low-income countries because other than pay there are other equally important non-pecuniary factors that demotivate teachers and make them less likely to deliver. These include restricted career structure, lack of promotion opportunities, and unconducive work environment.

The authors observe that contract and private school teachers on much lower salaries achieve similar learning outcomes to public-sector teachers. One of the reasons why this might be so is that in developing countries private schools are more likely to have smaller class sizes than public schools. The smaller class sizes might be contributing to better student outcomes in private schools as students are more likely to get individualized instruction. However, as the chapter has pointed out, teaching style matters. Some teaching styles like lecture will not likely lead to improved performance even when class sizes are small.

On teacher deployment, a recent study carried out in Malawi found that deployment of primary school teachers was inequitable both between schools and within schools. One of the factors contributing to this was political interference in matters relating to teacher management (Zubairi, 2020).

Teacher management systems matter as much as class size and pay levels

Evidence on the impact of teacher pay and class size on student learning outcomes is mixed. A synthesis of the evidence, however, suggests that teacher pay is a necessary but not sufficient condition in raising student outcomes. Effective teacher management has been found to be a critical factor that impacts positively on students learning. This suggests that in low-income countries such as Malawi, which have ineffective teacher management systems, increasing teachers' pay will unlikely lead to any significant improvements in students learning. Malawi primary school students depict very low learning outcomes. There is strong evidence of learning poverty in early primary grades with a large proportion of students not acquiring basic reading and numeracy skills in the early years. Although this may be related to the very large class sizes in lower primary, it is not clear whether teacher pay has any bearing on the poor learning outcomes at this level. The prevailing evidence suggests that weak teacher management systems that are in place do contribute to the large classes observed in some schools. ineffective earning outcomes persist Recent evidence from Malawi reveals an extremely ineffective teacher management system at the primary school level (Asim & Gera, 2020; Zubairi, 2020; Asim et al., 2017). According to evidence from a recent study, poor utilization of teachers has resulted in the uneven distribution of teachers between schools with remote schools having higher pupil: teacher ratios (PTR) than schools near trading centers despite the system having an almost adequate supply of teachers (Asim & Gera, 2020). The study also found that within schools, teachers are unevenly distributed between grades with lower grades having larger classes than higher grades. Furthermore, the study revealed that teacher absenteeism was also high and as a result, time spent on teaching was low (ibid).

Evidence also shows that teacher data systems are weak and fragmented which makes it difficult to manage teacher placements effectively leaving the system to the mercy of teacher and political interests thereby making it almost impossible to target teachers to schools needing then mist (Asim et al., 2017). Similarly, Zubairi (2020) found the existence of political interests at both national and local levels in the deployment and placement of teachers at the primary level, which did not respond to schools' needs.

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Chapter 5. What Public–Private Partnerships Can and Cannot Do

The evidence on outsourcing education to the private sector is mixed. Results are better for increasing enrollment than for raising learning outcomes.

Maryam Akmal, Susannah Hares, and Rita Perakis

Outsourcing the delivery of education to the private sector is a popular policy option for governments looking to expand access to schooling, but many possible models for how to do that exist and their design and implementation matters a lot. In this chapter, we present original analysis on the cost of different models of public–private partnership for the provision of basic education. We suggest that governments considering such models should be cautious. Existing evidence suggests subsidizing private schools may be an affordable policy option for increasing access to secondary school in underserved areas, although questions remain about the political viability of such arrangements, as well as their full costs. In contrast, contracting private firms to manage existing public schools has shown mixed effects across all outcomes. The literature shows some low impacts on learning, but at high unit cost, making this modality unlikely to be a cost-effective policy option for governments in low- and middle-income countries.

5.1 Introduction

As demand for education increases and more countries implement universal secondary education, public school systems are not always equipped to meet this demand. Engaging private-sector education providers has become a popular policy option to increase education access, particularly at the secondary level. The approach may be promising although concerns have been raised about how feasible it is to effectively regulate private-sector delivery of education and the effects of the increase in private schooling on equity in education systems (UNESCO 2021). Is outsourcing to the private sector a viable and cost-effective way to achieve universal secondary education and other Sustainable Development Goal 4 (SDG 4) targets for basic education? To address this question, governments need to understand the evidence behind public–private partnership (PPP) models—including evidence of their costs relative to the alternative of government-only provision.

We define a PPP as an arrangement between a government and a private body—whether that body is for profit or not—whereby the government is providing financing and guiding policy decisions but the private entity (such as a nongovernmental organization [NGO], foundation, or business) is delivering education services. In theory, such partnerships bring private-sector expertise and resources to education systems while governments retain responsibility and stewardship of education. In practice, however, the evidence on whether outsourcing to the private sector can improve education outcomes—and can do so cost-effectively—is mixed, and the wide range of partnership design and implementation features makes it challenging to point to private-sector partnerships as a single, straightforward solution.

In this chapter, we review the cost and impacts of different PPP designs. Overall, such outsourcing models have had a better track record in improving enrollment than in improving learning outcomes. We find that subsidy-based models may be an affordable policy option for increasing access to secondary school in underserved areas, although questions remain about the political viability of such arrangements. Contract school partnerships have a more mixed record when it comes to enrollment, learning, or equity outcomes and are less likely to be an affordable policy option for governments in low-income countries. We discuss the main cost drivers for these various arrangements and considerations for governments that are exploring how they can leverage private providers to meet national education goals.

Growth in private schools and outsourcing to the private sector

Regardless of governments' roles, private schools are providing education to millions of children across the world, including poor children in low-and middle-income countries. The question for most governments is not whether private schools should exist but whether and how to leverage and regulate the private sector to expand education access and quality, potentially through partnership models that support private delivery of education with public financing.

Private enrollment has grown in primary and secondary schools in low- and middle-income countries in the last two decades. The global share of primary school students in private schools was 19 percent in 2019, up from 10 percent in 2000.[1] The share of secondary enrollment in private schools in low- and middle-income countries is higher than primary at 27 percent in 2019, up from about 20 percent in 2000. Figure 5.1 shows which countries have a higher concentration of private secondary schools. These numbers are likely to be lower than the actual figures of students in non-state schools because data may not capture religious or other non-state schools that are not registered with the government.[2]

In 2019, private primary enrollment was 14 percent in sub-Saharan Africa, 21 percent in Latin America, and 38 percent in South Asia, rising from less than 20 percent in South Asia in 2000.[3] Data from the state of Uttar Pradesh in India show steep growth in private schooling in both urban and rural areas, with a high proportion of these schools catering to the poor. Thirty-two percent of private school students pay fees of less than Rs. 100 per month (about US$1.35), and 84 percent pay less than Rs. 500 per month (about US$6.75) (Kingdon 2019).

The global data on enrollment in schools that fall under various models of PPP are less clear. "Private enrollment" refers to pupils or students enrolled in institutions that are not operated by a public authority, but, as the World Bank notes, in countries where private institutions are substantially subsidized or aided by the government, the distinction between private and public educational institutions may be less clear-cut, especially when certain students are directly financed through government scholarships.[4] However, as demand for education has grown, and governments have not been able to meet it or to achieve quality through the public system alone, there has been an increase in public engagement of the private sector to deliver and finance education in low- and middle-income countries, as a growing

1. World Bank, World Development Indicators.
2. World Bank DataBank, Metadata Glossary, https://databank.worldbank.org/metadataglossary/world-development-indicators/series/SE.SEC.PRIV.ZS.
3. UNESCO Institute for Statistics data, https://data.worldbank.org/indicator/SE.PRM.PRIV.ZS?name_desc=false&locations=8S.
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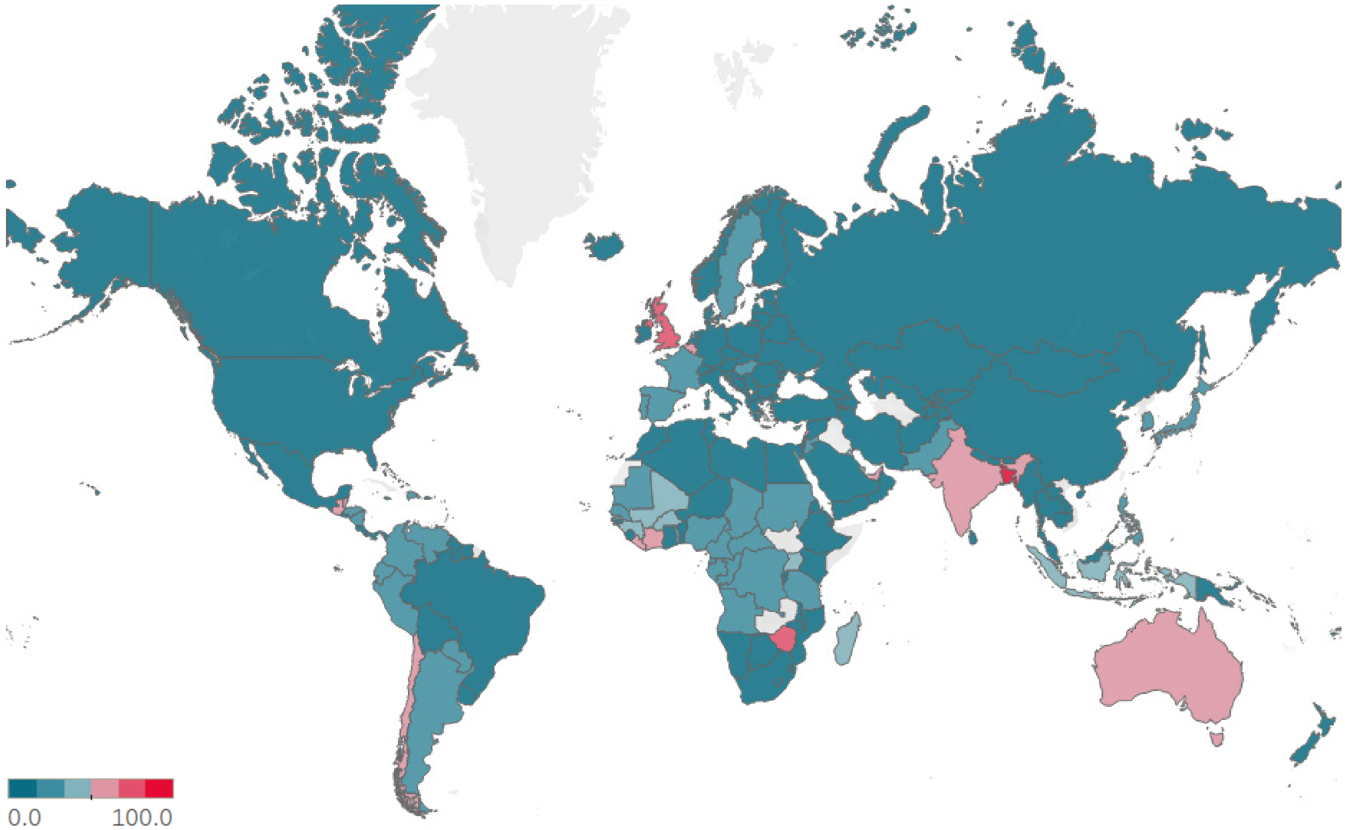
Figure 5.1. **Private secondary enrollment as percentage of total secondary enrollment**



Note: Map based on Longitude (generated) and Latitude (generated). Color shows average of Se Sec Priv Zs. Details are shown for Countryname.

body of research shows (Patrinos, Barrera-Osorio, and Guáqueta 2009; Epple, Romano, and Urquiola 2017; Aslam, Rawal, and Saeed 2017; Baum 2018).

Most countries have some mix of public and private provision and financing of education. Private schools, especially those serving the poor, have emerged where the public sector has not met demand for education for a number of possible reasons, including an insufficient supply of government school places (for example, there has been a rise in low-cost private schools in Kenya, despite the government's free primary education policy, due to excess demand [Oketch et al. 2010]); parents actively choosing low-cost private schools because of perceptions that they are of higher quality than public schools; and demand for specific types of schooling, mainly religious schools (Heyneman and Stern 2014; Zuilkowski et al. 2017; Härmä 2013). Government interest in outsourcing education delivery often emerges alongside the growth of private schools, perceived as a solution to help meet demand for universal schooling while governments retain a role in providing oversight of non-state entities.

Types of public–private partnerships

Beyond the need for governments to regulate the private school market, there are various policy options for partnerships with the non-state sector in education and specific design and implementation choices will have an impact on their outcomes. The models discussed in this report all involve some degree of public financing and private provision, but the extent of each of those varies. The type of PPP a government might launch should depend on its objectives: for example, the primary goal might be creating more school places or improving learning outcomes.

Broadly, these partnerships fall into three categories:

- Contract schools: the government contracts out provision or management of education to private providers in public schools.
- Subsidies: the government provides subsidies to existing private schools that are commonly used to fund student places.
- Vouchers: the government pays for a child to attend a fee-charging private school of choice.

Key features, and key cost drivers for governments, include whether a PPP involves government-owned or privately owned buildings and whether schools hire teachers who are on the public payroll or not. These decisions and other design features can have an impact on learning outcomes, equity, and costs. Table 5.1 summarizes the design and impacts of various PPPs that have been implemented.

Table 5.1. PPP design features and impacts

	Liberia PSL	Colombia Concessions	UK Academies	USA Charters	Punjab PSSP	Sindh PPRS	Chile SEP	India RTE	Uganda Secondary
Year started	2016	2000	2001	1991	2016	2007	2008	2012	2007
Number of schools	194	25	7,500	7,000	4,300	2,314	7,500	c. 91,000	800
Type of PPP	Contract management	Contract management	Contract management	Contract management	Contract management	Subsidy	Voucher	Subsidy	Subsidy
Design									
No fees /or top ups	Yes	Yes	Yes	Yes	Yes	–	–	Yes	–
Non-profit	–	Yes	Yes	Mixed	–	–	–	–	–
Non–selective	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	–
Teachers on Govt contracts	Yes	–	Mixed	–	–	–	–	–	–
Unionised teachers	Yes	–	Yes	–	–	–	–	–	–
Accountable for outcomes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	–
National curriculum	Yes	Yes	–	Mixed	Yes	Yes	Yes	Yes	Yes
Government buildings	Yes	Yes	Yes	Mixed	Yes	–	–	–	–
Impact									
Cost compared with govt schools	Higher	Equivalent	Equivalent	Equivalent	Lower	Lower	Lower	Lower	Lower
Enrolment	Lower	Higher	No effect	No effect	Higher	Higher	No effect	No effect	Higher
Learning outcomes	Small effect	Small effect	No effect	No effect	Small negative effect	Higher	Higher	No effect	Small effect
Equity	Negative	Unclear	No effect	No effect	Unclear	Positive	Positive	Unclear	Positive

More Public ⟵⟶ **More Private**

Note: This table summarises the design features of different public-private partnership schemes. Sources include: Romero et al., 2020; Romero and Sandefur, 2019; Barrera-Osorio et al., 2016; Andrews and Perera, 2017; Cheng et al., 2017; Crawfurd, 2017; Barrera-Osorio et al., 2017; Neilson et al., 2019; Crawfurd et al., 2021; Damera, 2017.

Source: Crawfurd and Hares (2021). Note that this chart attempts to summarize overall findings for some PPP types, such as US charter schools, that include within them a range of different models with varying results.

5.2 The controversial role of private schools in public education systems

To understand whether PPPs are a viable policy option for helping to achieve the goal of universal education, we need to consider their cost against the outcomes that they achieve. Proponents of PPPs argue that private providers are more efficient and can achieve better learning, access, or equity outcomes at a lower per unit cost than government schools (Tooley and Dixon 2003), and therefore more schooling should be outsourced to the private sector. They suggest that PPPs can combine the theoretical benefits of the private sector with government financing, offering a way to reach more children while benefiting from the private sector's ability to reduce costs more than public schools can.

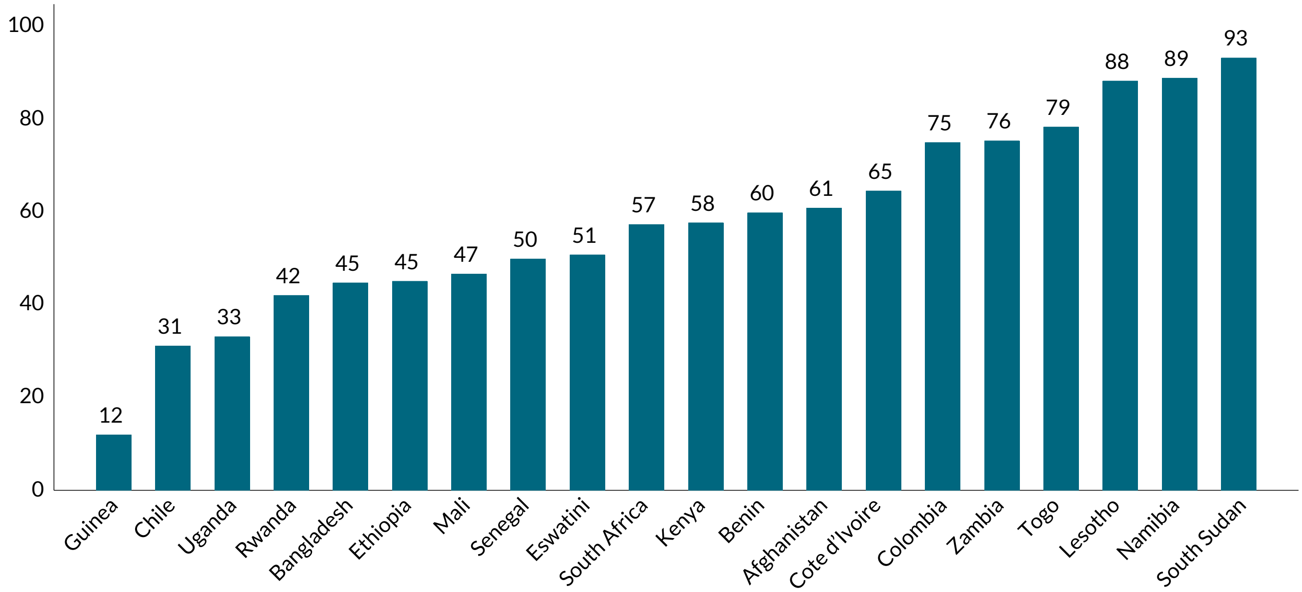
Indeed, Day Ashley et al.'s (2014) review of the evidence on private schools finds moderate evidence to suggest that the cost of education delivery is lower in private schools than public schools.

Opponents, on the other hand, suggest that private schools achieve such cost efficiencies by making compromises on the quality or equity of education—for example, by excluding harder-to-educate children, typically those who are the most marginalized and served only by the government sector. Driving for low cost can also exploit labor markets for less qualified and less experienced teachers—often women with restricted mobility—working for significantly lower salaries (Kingdon 2008; Muralidharan and Kremer 2008; Muralidharan and Sundararaman 2015). Bold et al.'s (2013) study in Kenya provides an account of a nationwide contract teacher program, with 18,000 new low-paid contract teachers, which provoked organized resistance from the national teachers' union. It illuminates how education reforms dependent on low-wage teachers can be derailed by unions and other political economy forces. Cost efficiencies driven by lower teacher salaries may not be sustainable in the long term.

Under some PPP agreements, teachers are on the government's payroll and paid their normal salaries. Under others, such as the subsidy or voucher models, governments pay for pupils' spots in private schools and teachers are paid private school salaries, which tend to be lower. This difference in labor costs is a primary driver of lower per pupil costs when children attend private schools. For example, in Pakistan, an average female teacher in a government primary school earns Rs. 5,897 per month (Rs. 6,408 for male teachers). In low-cost private schools, male teachers earn Rs. 1,789 per month, while female teachers earn just Rs. 1,069 (Andrabi, Das, and Khwaja 2008). In rural India, low-cost private primary schools pay teachers one-fifth (and often as low as one-tenth) of what public school teachers make (Muralidharan and Kremer 2008). In Kenya, identical teachers earn twice as much in public schools as in the private sector (Barton, Bold, and Sandefur 2017). In the public sector, teacher salaries tend to constitute the bulk of the education budget (Figure 5.2). At the secondary level, teacher salaries tend to constitute 55 percent of government education spending in sub-Saharan Africa (UNESCO 2011).

Apart from lower teacher salaries, the private sector may be able to build schools more cheaply than the government. The private sector could also perhaps mobilize capital for infrastructure investments faster and on better terms than the public sector. Furthermore, subsidy programs that are established using existing private premises can reduce or eliminate school construction costs and could enable the expansion of access to education more cheaply than through the public sector alone (Kim, Alderman, and Orazem 1999; O'Donoghue et al. 2018).

Figure 5.2. **Public teacher staff compensation as a percentage of total expenditure**



Source: Authors' analysis of UNESCO Institute for Statistics data estimates on teaching staff compensation as a percentage of total expenditure in public institutions for the latest available year (weighted by population size).

5.3 The cost and impact of different PPP programs vary widely

PPPs are not the same as private schools, and their costs may be different if, for example, they are required to meet particular regulations to be eligible to receive public funds. Therefore an assessment of the role of PPPs in achieving universal education requires us to look beyond the per unit cost of private schools.

Study sample and method of analysis

We draw on studies from previous reviews and more recent studies that provide impact data about subsidy, voucher, and contract school partnerships. Unlike existing reviews, we focus on the cost of PPPs in addition to their impacts on enrollment, learning, and equity. To analyze actual costs, we examine the cost of education provision through the PPP model versus government-only provision. The analysis covers 31 studies in 11 countries.

To select the studies used in this analysis, we began with those included in the rigorous review by Aslam, Rawal, and Saeed (2017). The review identified studies from 2009 onward that provided evidence of the impacts of voucher, subsidy, and contract school programs on education outcomes in low- and middle-income countries. Studies were assessed based on the quality of the evidence, and high- and medium-quality studies were included. For empirical studies published before 2009, we drew upon those included in Patrinos, Barrera-Osorio, and Guáqueta (2009), a comprehensive review of the evidence behind education PPPs up to that time. We also supplemented these sources with studies published since 2017 that were identified in Crawfurd and Hares (2021).

We ended up with a sample of 31 studies, which we analyzed and scored across two dimensions:

- Policy relevance: We assess how relevant the study is to education in low-income countries. Each study is rated according to *country* (with more points awarded to studies in low- and lower middle-income countries than richer countries); *year* (with more weight given to studies from the last 10 years compared to older ones); *sample size* (with more weight for studies that covered at least 10 schools, or an equivalent number of students); the study's *rigor* (using the rigor ranking from Patrinos, Barrera-Osorio, and Guáqueta and Aslam, Rawal, and Saeed); and whether it was a *government or NGO project* (with more points awarded to programs led by governments rather than NGOs).

- Effect: We assess (in terms of having a negative, null, weak positive or strong positive effect) how the PPP performed on
  - enrollment;
  - learning;
  - equity; and
  - cost

  Each study is rated by size of enrollment effect, learning effect, equity effect, and whether the PPP saved costs compared with the alternative government program. We summarize these results by indicating whether the program had positive, negative, null, or unclear effects (Figure 5.3).

An explanation of the policy relevance and weighting schemes is included in Appendix 5.1, and the full database of studies is available online.

Findings

In our model, six studies suggest that PPPs improve access at the same cost or lower, and seven studies suggest that PPPs improve learning at the same cost or lower. Of studies that show "strong positive" results on enrollment or learning, five studies show that PPPs that improved enrollment did so with cost savings, whereas two studies that demonstrated learning improvements involved cost savings. About half of the studies across all countries do not have sufficient information to allow us to compare costs with government-only provision.

Overall, most studies show small or no effects of engaging with the private sector to improve enrollment, equity, or learning. PPPs in Pakistan and Uganda seem to have had the most positive results when it comes to improving enrollment and with some cost gains, although this comes with mixed results on equity and learning outcomes, and questions about the politics and sustainability of the partnership programs (see Boxes 5.1 and 5.2). Even where positive impacts are observed, they are not always clear because studies do not all account for the number of children that would have been in school in the absence of the PPP or, in cases of learning improvements, for the children's social and economic backgrounds. For example, Crawfurd (2018) found an increase in enrollment in Pakistan but could not conclude whether it was due to students who were already enrolled in school. Barrera-Osorio et al. (2016) found positive enrollment and learning impacts of secondary school subsidies in Uganda but did not determine whether a change in the composition of children attending secondary schools accounted for some of the impacts.

When it comes to learning, findings are somewhat positive across many programs but the effect sizes are often small. The school voucher program in Chile has been well studied but studies have mixed findings on whether it led to improved learning, alongside limited or sometimes negative evidence when it comes to enrollment, equity, and cost savings. Reviews of the evidence (Day Ashley et al. 2014; Aslam, Rawal, and Saeed 2017) suggest that evidence of PPPs is inconclusive and limited in volume, but sufficient to conclude that PPPs are no silver bullet for global education. The newer literature covered in Crawfurd and Hares (2021) gives us reason to be less optimistic about PPPs' role in improving quality in education systems than previous reviews. As that study points out, causal effects of PPPs on education outcomes are often unclear or marginal, and the governance of partnership models—including procurement, monitoring, and evaluation—is too often ineffective.

While not always clear, the evidence base is stronger regarding PPPs' impacts on student enrollment, while also taking equity into consideration, with the evidence showing that some programs are reaching poorer

Figure 5.3. Summary of evidence from PPP studies

Country	PPP	Type	Study	Policy Relevance	Enrollment	Learning	Equity	Cost
Chile	Chilean SEP vouchers		Neilson (2021)	Weak positive	Unclear	Strong positive	Strong positive	Unclear
	Chilean vouchers		Anand et al. (2009)	Weak positive	Unclear	Strong positive	Weak positive	Unclear
			Contreras et al. (2010)	Weak positive	Unclear	No effect	Unclear	Unclear
			Elacqua et al. (2011)	Weak positive	Unclear	No effect	Unclear	Unclear
			Gallego (2004)	Weak positive	Weak positive	Strong positive	No effect	Unclear
			Hsieh & Urquiola (2006)	Weak positive	No effect	No effect	Negative	No effect
			Lara et al. (2011)	Weak positive	Unclear	Weak positive	Negative	Unclear
			McEwan (2001)	Weak positive	Unclear	Weak positive	Negative	Unclear
			Mizala & Torche (2012)	Weak positive	Unclear	Unclear	Negative	Unclear
			Sanchez (2018)	Weak positive	No effect	Unclear	No effect	Strong positive
			Sapelli & Vial (2003)	Weak positive	Unclear	Strong positive	No effect	Unclear
Colombia	Bogota Subsidy	Subsidy	Uribe et al. (2005)	Weak positive	Unclear	No effect	Weak positive	Unclear
	Colombia CEC	Contract	Barrera-Osorio (2006)	Weak positive	Weak positive	Strong positive	Strong positive	Unclear
			Bonilla-Angel (2011)	Weak positive	Unclear	Strong positive	Negative	No effect
	Colombia PACES	Voucher	Angrist et al. (2002)	Weak positive	Weak positive	Strong positive	Strong positive	Negative
			Angrist et al. (2006)	Weak positive	Strong positive	Strong positive	Strong positive	Unclear
	Fe y Alegria	Subsidy	Osorio and Wodon (2014)	Weak positive	Unclear	Weak positive	Strong positive	Strong positive
Haiti	Haiti Subsidy	Subsidy	Adelman & Holland (2015)	Weak positive	Weak positive	Unclear	Unclear	Unclear
India	India (AP) School Choice	Voucher	Muralidharan and Sundararaman (2015)	Strong positive	No effect	Weak positive	No effect	Strong positive
Liberia	Liberia PSL	Contract	Romero & Sandefur (2019)	Strong positive	Negative	Weak positive	Unclear	Negative
Pakistan	Pakistan SEF	Subsidy	Barrera-Osorio et al. (2017)	Strong positive	Strong positive	Strong positive	Strong positive	Strong positive
	Punjab FAS	Subsidy	Barrera-Osorio & Raju (2011)	Strong positive	Weak positive	Unclear	Strong positive	Strong positive
	Punjab PPPs	Multiple	Ansari (2021)	Strong positive	Unclear	Unclear	Weak positive	Unclear
	Punjab PSSP	Contract	Crawfurd (2018)	Strong positive	Strong positive	Negative	Unclear	Strong positive
	Quetta Urban Fellowship	Subsidy	Kim et al. (1999)	Strong positive	Strong positive	Unclear	Weak positive	Strong positive
Philippines	Philippines ESC	Contract	Jimenez et al. (2011)	Weak positive	Unclear	Strong positive	Negative	Strong positive
Sierra Leone	Sierra Leone faith-based schools	Subsidy	Wodon & Ying (2009)	Weak positive	Unclear	Weak positive	Strong positive	Unclear
Uganda	PEAS Uganda	Subsidy	EPRC (2018)	Strong positive	Strong positive	Weak positive	Strong positive	Strong positive
	Uganda USE	Subsidy	Barrera-Osorio et al. (2016)	Strong positive	Strong positive	Weak positive	Unclear	Strong positive
Venezuela	Fe Y Alegria	Subsidy	Allcott & Ortega (2009)	Negative	Unclear	Weak positive	Strong positive	Unclear
			Wolff et al. (1994)	Negative	Unclear	No effect	Unclear	Strong positive

Negative effect (low) | No effect | Weak positive effect (medium) | Strong positive effect (high) | Unclear or n/a

students. Reaching unenrolled students is a common objective for governments in working with the private sector, particularly for secondary education. Subsidy schemes in particular have had positive impacts, with a majority of studies in our sample showing positive effects on enrollment, equity, or cost-effectiveness. In Pakistan, although evidence on learning outcomes is weak, studies have shown some positive impacts on enrollment and evidence that children in PPP schools were from lower wealth quintiles (Ansari 2020, 2021; Barrera-Osorio et al. 2017). The subsidy program in Uganda (see Box 5.1) and a range of PPP models in

Punjab and Sindh have been cost-effective, as they all cost the government the same as—or in some cases substantially less than—it would cost to educate a child in the public sector (Crawfurd and Hares 2021). Box 5.2 examines cost-effectiveness under the Punjab Education Foundation, which manages various private-sector partnerships for education delivery in Pakistan.

The two main cost drivers discussed earlier are mechanisms through which PPPs may lower "per unit" costs while preserving or improving enrollment and/or learning, and hence improving overall efficiency: lower teacher salaries (Barrera-Osorio et al. 2017; Kim, Alderman, and Orazem 1999; Andrabi, Das, and Khwaja 2008; Muralidharan and Kremer 2008; Barton, Bold, and Sandefur 2017) and the lower cost of establishing a PPP school compared with a government school (Kim, Alderman, and Orazem 1999; O'Donoghue et al. 2018). Some studies report PPP schools hiring a larger number of teachers (Crawfurd 2018; Barrera-Osorio et al. 2017; Barrera-Osorio and Raju 2011), which could potentially be due to lower teacher salaries or greater flexibility in labor laws, but even with the cost of new hires, programs are able to maintain lower unit costs when compared with government-only schools.

However, cost efficiencies are not consistent across the board. Two studies suggest that PPPs cost more on a per student basis than government programs while delivering some gains in learning. These include one contract management partnership, the Partnership Schools for Liberia (PSL/LEAP) program, and one voucher program, Programa de Ampliación de Cobertura de la Educación Secundaria (PACES) in Colombia.

Under the partnership program in Liberia, the government delegated management of 93 public schools to eight private operators: BRAC, Bridge International Academies, Youth Movement for Collective Action, More Than Me, Omega Schools, Rising Academies, Stella Maris, and Street Child. Whereas ordinary public schools run on a budget of $50 per pupil per year, the contract schools were allowed to draw an additional $50 per pupil from a pool of philanthropic funds, and some operators brought in additional funding. The education ministry also made special staffing arrangements whereby partnership schools were allowed two additional teachers per pupil. In the first year, the average expenditure was roughly $300 per pupil, although it ranged from a low of approximately $57 per pupil for the Youth Movement for Collective Action to a high of approximately $1,052 per pupil for Bridge International Academies, on top of the ministry's normal expenditure of around $50 per pupil (Romero, Sandefur, and Sandholtz 2017). Unit costs fell after the first year as start-up investments and fixed costs declined. After three years, the average (self-reported) expenditure fell to $119 per pupil. However, Bridge International Academies and More Than Me continue to spend at least three times as much as the government target (Romero and Sandefur 2019).

PACES in Colombia provided more than 125,000 students with vouchers covering somewhat more than half the cost of private secondary school. Vouchers were awarded by lottery and were renewable contingent on satisfactory academic performance. The voucher program cost $24 more per winner than the cost of creating a public school placement (government cost of $350 per student), but the program reported positive gains in enrollment, learning, and equity (Angrist et al. 2002).

5.4 The cost drivers of different outsourcing models

A key factor affecting cost is whether the government is paying private entities to manage public schools, or whether it is paying private schools to educate students.

A contract model PPP, where the government pays a private actor to take over the management of the schools, will probably cost more money. These PPPs may be unaffordable for governments in the long term as they generally involve the private operator taking on full payroll costs—well over half of the total cost of educating a child in almost every country—as well as the private operator's additional management costs. Therefore, the model is unlikely to reduce the total cost of educating a child (Crawfurd and Hares 2021). As we saw in Liberia, costs range widely across providers (Romero and Sandefur 2019). In another contract model PPP in Pakistan, more than 4,000 government schools were contracted to private operators. Unlike the PSL in Liberia, operators were able to hire their own teachers at wages set by the operators and existing public school teachers were given the option to transfer to other public schools. Although the overall enrollment, learning, and equity effects from the evaluation of that outsourcing program by Crawfurd (2018) are inconclusive, the program was cheaper versus the government schools on a per child basis, but setting lower levels for teacher pay raises questions about the political economy and sustainability of different teacher pay structures.

The evidence points to the conclusion that a contract school PPP is probably not a good policy choice, as costs are high and quality gains are minimal. On the other hand, a voucher or subsidy PPP—where the government pays private schools to educate students—may be cheaper for the government, since school construction costs can be avoided and private schools, because they typically pay lower teacher salaries than public schools do, may have lower per pupil costs.

Uganda's Universal Secondary Education (USE) program and the Promoting Private Schooling in Rural Sindh (PPRS) program shed some light on how governments can use PPPs to increase access to education at a lower unit cost than the public sector can. In Uganda (Box 5.1), the cost of the subsidy paid to private schools—47,000 Ugandan shillings per term—is much less than the cost of educating a child in a public school and did not require upfront infrastructure investment by the government (Barrera-Osorio et al. 2016). However, it is worth noting that these figures do not show the full cost of the program, including any government costs of managing the subsidy program and hidden costs to parents. An additional report on the Uganda subsidy program shows that a majority of financing for secondary schooling in Uganda comes from households (O'Donoghue et al. 2018), largely because of the fees that private schools charge, although public schools charge fees as well.

In Sindh, Pakistan, the subsidy paid to private schools is low—less than half the cost of government schooling—although high start-up and administrative costs meant that after the first year the total cost to the government was broadly equivalent to the cost of educating a child in the public system. Economies of scale meant that non-subsidy costs fell from 70 percent of total costs to less than 30 percent of total costs over three years (Barrera-Osorio et al. 2017).

In Uganda and Sindh the subsidies paid to private schools to educate children are lower than the government cost per child. Even taking any setup costs into consideration, it seems likely that PPPs will deliver education at a lower cost than the government. That cost is also lowered by infrastructure savings: because both programs primarily aimed to increase access in underserved areas, they were able to do so at a lower cost than that of constructing new government schools.

However, in voucher and subsidy programs the cost savings are typically driven almost entirely by lower teacher salaries, and that may not be a palatable political option. For example, in India, where a subsidy PPP is operating at massive scale, private school teachers are mobilizing to be paid salaries at parity with their counterparts in public schools. Therefore, expecting teachers in private schools subsidized by the government to work at much lower wages creates great political tension and may not be feasible in the long term. A similar story has played out in Kenya with lower-paid contract teachers mobilizing for equal pay.

The politics of voucher and subsidy PPPs are challenging for other reasons. The public perceptions of PPPs can be polarizing. On the one hand, constituencies who believe that PPPs are a threat to public education may resist them. On the other hand, where the public does not perceive a PPP as a government intervention, lack of recognition for the government's role in the provision of public education can reduce political support. For example, the USE program in Uganda, a subsidy

PPP, was phased out by the government, despite evidence of cost-effectiveness. A factor in the decision to phase it out was that the important contribution the program plays in helping to deliver secondary education in Uganda was not fully understood by many key stakeholders, from government to parents (O'Donoghue et al. 2018). Even where it is possible to achieve lower per pupil costs, management costs, regulation of private providers, and political considerations make implementing these partnerships challenging and worth questioning carefully in comparison to improving public provision.

5.5 Conclusion

Governments looking to expand access to secondary education in an affordable manner should probably avoid contract school PPPs. They may well consider outsourcing schools through voucher or subsidy programs to take advantage of the differential and sometimes lower cost of delivery of the private sector. Evidence from Pakistan and Uganda (Barrera-Osorio et al. 2016; Barrera-Osorio et al. 2017; Crawfurd 2018) highlight that subsidy and voucher arrangements have the potential to rapidly increase access to education at an affordable cost and—within a limited set of policy options—may be worth pursuing to achieve such goals. However, these partnership arrangements often come with management and coordination costs that are not fully captured in the data, as well as political economy constraints. Policymakers should consider those factors for their own contexts in deciding whether it is worth embarking on these partnership models.

As discussed above, there are practical and moral constraints to paying all teachers the low salaries that they are paid in private schools. Such a policy might not only be immediately politically untenable in the short run but also affect the quality of teachers that choose to enter the profession in the longer term. Similarly, firing and replacing existing teachers is generally politically unfeasible and raises questions about the supply-side availability of qualified replacements, when even many adults (and in many cases existing teachers) lack basic literacy and numeracy skills (Crawfurd 2016).

In the wake of the COVID-19 economic crisis and with the SDG 4 targets for secondary education going increasingly off track, PPPs should remain a policy option to expand education to more children, especially currently out-of-school children. Important research questions about the role of PPPs in the delivery of public education remain outstanding—particularly whether outsourcing education to the private sector reduces support for public financing of education. In addition, a future research agenda could more closely examine the real costs of various partnership arrangements: we found that few studies provided cost information and even when cost data exist, those data do not always cover the full picture of costs, such as governments' costs of managing partnerships or additional funds that non-state providers may bring in from philanthropic sources. Policymakers need more complete cost data to understand the costs versus the benefits of any PPP model and whether it is likely to be a good choice for a government. Moreover, while most studies focus on the overall impacts of various partnership models, future research could focus on the specific features that contributed to those impacts—such as, for example, a provider's management approach or instructional practices—and the potential for any of these features to be replicated in public schools.

As long as these questions remain unanswered, governments would be wise to tread carefully if they embark on new PPP arrangements, with an understanding of what the objectives of the partnership are and accountability mechanisms in place to ensure that objectives are being met.

Box 5.1. Case study: Secondary school subsidies in Uganda

A former secondary school subsidy scheme in Uganda shows how partnership models can lead to enrollment and cost-effectiveness gains, but also illustrates the political challenges such programs face. This PPP was a part of Uganda's Universal Secondary Education (USE) program starting in 2007, when Uganda's secondary enrollment rate was only about 25 percent. Primary school completion rates had increased following the launch of free primary education in 1997, and the Ugandan government sought to fulfill a political commitment to USE and rapidly increase secondary enrollment by taking advantage of existing secondary school places in private schools.

Under the scheme, the government transferred subsidies to private schools that in turn enrolled more students. Schools received USh 47,000 (about US$13) per student per term to replace enrollment fees. The program grew from 363 schools in 2007 to more than 800 in 2016, at which point it covered nearly a third of all enrolled secondary school students in Uganda.

Although the per student costs appear low, they do not capture the full cost of secondary schooling. In Uganda— whether in private or public schools—a high proportion of costs are borne by households, as Table 5.1 shows. Primary research (O'Donoghue et al. 2018) showed that total fees per term for USE students—including tuition as well as lunch, uniforms, remedial classes, building fees, admission fees, exam fees, library fees, and any other cash or in-kind contribution—amounted to somewhere between USh 128,000 and USh 164,000 (US$35–US$45), or at least three times the amount of the government subsidy. While this is lower than the cost of fees at non-PPP private schools, it applies to students in either public or PPP schools who in theory should be benefiting from a free education. The program was administered through the education ministry's private schools division; additional management costs at the central level appear to be low but are not captured.

Table B5.1. Sources of finance for secondary education in Uganda

	Lower Secondary	Upper Secondary
Government of Uganda	16%	11%
Households	63%	78%
International	19%	9%
Generated by School	2%	2%
Total	100%	100%

Source: UNESCO IIEP National Education Accounts 2016 and O'Donoghue et al, 2018.

Using a value-added measure, one review found little difference in quality between different school types, with PPP schools only slightly outperforming government schools (O'Donoghue et al. 2018). An earlier study found increases in enrollment and performance attributed to PPP schools but also found that students in PPP schools were significantly more likely to come from better-off households (Barrera-Osorio et al. 2016).

Overall, the PPP arrangement helped more secondary-school-age kids access school, achieving similar quality to public schools overall at a lower cost. But it was not without implementation challenges: poor accountability mechanisms and poor communication about the public role in providing education through the PPP came at a high perceived cost to the Ugandan government, which began to phase out the subsidies beginning in 2018.

Despite the political decisions made about the future of Uganda's secondary school PPP, it remains a good example of how a PPP could be structured to fulfill the objective of expanding secondary school access, with relevant lessons about the political economy considerations of PPPs.

Box 5.2. Case study: Punjab Education Foundation

In Punjab—Pakistan's most populous province—partnership with the private sector has been an important part of reforms to overcome the challenge of low basic education enrollment levels. The Punjab Education Foundation (PEF) was established in 1991 as an autonomous body to promote education, including through loans and grants to private entrepreneurs as a way of increasing access. The establishment of PEF, and of the Singh Education Foundation in 1992, were a first formal step toward legitimizing the role of the private sector as a key player in education provision in Pakistan. A restructuring of PEF in 2004 further strengthened that role. While PEF has contributed to gains in student enrollment, a closer look reveals the challenges in defining the true costs of partnership models, and questions about sustainability.

Over the past two decades PEF has partnered with nonprofit and for-profit organizations to educate more than 2 million primary-school-age children in Punjab through its main programs. Foundation Assisted Schools (FAS), which pays fees for children educated in registered private schools, has had 1.7 million beneficiaries as of 2017; the Education Voucher Scheme (EVS) has supported 400,000 children; the New School Program (NSP) has contracted private organizations to start around 2,000 schools in remote and underserved areas, which have enrolled 193,000 children; and the Public School Support Program (PSSP), a contract management PPP contracting out poor-performing public schools, has enrolled approximately 120,000 children. All of these programs provide financial assistance to cover or partially cover tuition fees for children enrolled in private schools.[1]

Several studies have examined the impact of these PPP programs and other low-fee private schools on enrollment in Pakistan and have found generally positive effects (Amjad and MacLeod 2014; Barrera-Osorio and Raju 2011; Barrera-Osorio et al. 2017; Andrabi et al. 2010; Crawfurd 2018; Ansari 2020). There is, however, some inconsistency in the findings. One qualitative study conducted by Oxfam covering 31 PEF schools showed that out of 12,502 children in the schools only 158 were previously out of school, and many came from families who could afford paying fees for these low-fee private schools themselves (Afridi 2018). The study also found that schools were actively screening and selecting children for academic ability. One study included in our analysis (Crawfurd 2018) found a 60 percent increase in enrollment in 4,276 poorly performing public primary schools that were contracted out to private operators through PSSP but could not determine whether the students were previously out of school or enrolled in a different school. The study found no clear change in average test scores. Another study (Ansari 2020) used socioeconomic data of students collected from 812 public, PPP, and private schools and found that PEF schools appeared to have been located in districts where high shares of children were out of school.

Although costs vary widely across the programs, overall PEF appears to have achieved some increase in enrollment and quality cost-effectively (Table 5.2). The true cost of the programs to the government includes direct per pupil costs as well as indirect program management costs. Analysis of Punjab's School Education Department and PEF's expenditure in 2016/17 shows that on average it costs PEF PKR 638 (US$6.5)[2] per month to educate a child, compared with PKR 1,049 (US$10.5)[3] in public primary schools.

1. PEF beneficiary numbers from the 2016/17 school year, the latest year for which both enrollment and financial data are available.
2. This is calculated by dividing PEF's total expenditure (PKR 18,565,258,451) by total enrollment (2,424,097) and 12 months. Average yearly rate for 2016/17: PKR 98.98 per US$, https://www.ofx.com.
3. This is calculated by dividing the government of Pakistan's total expenditure (PKR 91,920,268,189) to total primary enrollment (7.3 million) and 12 months.

Box 5.2. Continued

Parents incur additional costs of educating their children that are not captured here. The premise of PEF was that families would not be responsible for the costs of their children's education, yet parents have in fact faced considerable out-of-pocket expenses for things like supplies, uniforms, and transport (Afridi 2018). However, the limited budget available for nonsalary expenses in the public school system would also indicate that parents are bearing similar out-of-pocket expenses for children to attend public schools. And some anecdotal evidence exists suggesting that organizations participating in PSSP—PEF's contract management PPP program—use their own financing to top up the PKR 700 per child per month provided by PEF. The contract does not impose any limits on how much organizations or individuals can spend per child.

The PEF story is overall a positive one as an example of a large-scale reform that has led to improvements in enrollment and quality. To build on PEF's success, more refined targeting is needed to make sure programs reach the most vulnerable populations. Moreover, regulation remains a challenge; existing partnership schools are under increased scrutiny by the district governments to ensure that schools are operating in buildings that are safe. The sustainability of PEF programs is in question, however, particularly due to concerns that its cost model relies on paying teachers rates below those of public school teachers and even below minimum wage. Whereas PEF helped expand the teaching workforce by creating opportunities for teachers from underserved areas, it has been criticized for the hiring of less qualified teachers. The future of the partnership remains uncertain as policymakers grapple with these challenges.

Table B5.2. Punjab Education Foundation audited expenditure for FY 2016/17

Direct Program Expenditure	PKR	USD
Foundation Assisted Schools (FAS)	10,513,576,148	106,219,197
New Schools Program (NSP)	1,093,514,754	11,047,835
Education Voucher Scheme (EVS)	2,685,755,920	27,134,329
Academic Development Unit (ADU)	60,508,302	611,318
Free Textbooks	638,485,852	6,450,655
CPDP	121,585,684	1,228,386
Public School Support Program (PSSP)	2,729,915,607	27,580,477
Inclusive Education	14,533,983	146,838
Early Childhood Education	10,336,853	104,434
Nestle Healthy Kids Program	26,850	271
Indirect Program Expenditure	**PKR**	**USD**
Monitoring Cost	74,433,663	752,007
Monitoring and Evaluation Assistants	48,536,030	490,362
Seminars, workshops and symposiums	3,475,966	35,118
Capacity building of staff	1,177,520	11,897
Total Program Expenditure	**17,995,863,132**	**181,813,124**
Other HR Costs	450,056,174	4,546,941
Total Expenditure	**18,565,258,451**	**186,360,065**

Average yearly rate for 2016-17: PKR 98.98 per USD, https://www.ofx.com.

Source: Authors' commissioned analysis by Ahmad Jawad Asghar

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Chapter 5 Appendix

Policy relevance and effect weighting for sample of studies

Policy relevance weighting

Country	Points
Low income	3
Low-middle income	3
Upper middle income	1
High income	0
Sample size	
More than 10 schools	1
Less than 10 schools	0
Implementer	
Government	3
Nongovernmental organization (NGO) with government	2
NGO	0
Study rigor (taken from the Aslam, Rawal, and Saeed [2017] report and Patrinos, Barrera-Osorio, and Guáqueta [2009] where available). When study is missing, we use the criteria in Appendix Table A3 of Aslam, Rawal, and Saeed.	
High	3
Moderate	2
Low	1
Year of study	
Last 10 years	1
Older	0

Policy relevance score is sum of points:

0–4 = low 5–8 = medium 9–11 = high

Effect scores

Enrollment score
Strong positive: High effect (above 15%) or clear evidence of enrolling students that would have been out of school
Weak positive: Low effect (under 15%) or undetermined but positive effect
No effect
Negative effect: decrease in enrollment
Learning score
Strong positive: positive effect of 0.2 SD +
Weak positive: effect is positive but less than .2SD
No effect
Negative effect: Decline in learning outcomes
Equity score
Strong positive: Successfully targeted poor children
Weak positive: Targeted poor children, outcome unclear
No effect: No or no reported target or effect for poor kids
Negative effect: Evidence of more positive impacts for better-off population
Cost score
Strong positive: Less than government
No effect: Equal with government
Negative effect: More than government

Comment: You can't regulate what you can't provide: The weakening case for education PPPs

Jishnu Das

Between 1995 and 2005, careful and painstaking field investigations uncovered pervasive deficits in the provision of public services in Low- and Middle-Income Countries. These deficits ranged from terrible conditions in schools to frequent absenteeism among healthcare workers and teachers to the sizable leakage of funds originally meant for schools. Together with research showing that the relationship between government funding and educational outcomes was tenuous to begin with, a consensus developed that public schools were failing children. At best they had been successful in raising enrollment, but not in improving learning (Chaudhury and Hammer 2004; Chaudhury et al. 2006; Reinikka and Svensson 2004; World Bank 2003).

Interestingly, these developments mirrored a changing landscape in the United States, where the narrative of failing public schools had already been part of the education discourse for some time. There, the publication of A Nation at Risk put the education system under the microscope as early as 1983, with subsequent developments predicated on the belief that teachers were part of a problem that could be fixed through greater accountability and the more flexible use of public resources. Buoyed by research that again showed little relationship between funding and outcomes, charter schools and vouchers became an important part of this landscape in the early 1990s.[1]

Twenty years later the pendulum, if not swinging back, seems to be somewhat stuck. Despite the fact that some types of charter schools have been shown to systematically raise test scores, vouchers and charters in the United States have arguably not yielded the large gains that were anticipated. A recent study suggests that charters in the state of Texas negatively impacted young adult earnings (Dobbie and Fryer 2020) and, Angrist, Pathak and Walters (2013) who have led multiple evaluations of charter schools, note:

> *"In a recent report evaluating roughly two dozen Massachusetts charter schools from around the state, we find little evidence of achievement gains at schools outside of high-poverty urban areas (Angrist, Pathak, and Walters 2011). Some of the estimates for nonurban Massachusetts charters show significant negative effects. These results echo findings from a multi-state study of 36 charter middle schools using admissions lotteries (Gleason et al. 2010). Here too, charter schools outside of urban areas seem to do little for achievement, though, as in our earlier work, urban schools with high-minority, high-poverty enrollment generate some gains."* (Page 2)

1. I note that the original idea of charters proposed by the president of the American Federation of Teachers, Albert Shanker, emphasized public schools that would be given a renewable "charter" to try innovative approaches toward pedagogy and teaching. This proposal quickly morphed into an emphasis on management and the strict control of teachers, very different from what was originally envisioned. See Kahlenberg and Potter (2015) and Shanker (1988).

The building evidence that charters are not a "miracle-gro" for test scores comes at the same time that newer studies demonstrate a clear relationship between funding and test scores; in fact, the older "null" relationship between funding and outcomes in the United States may have had more to do with faulty econometrics than with a genuine causal finding. See Jackson (2020).

Now, Akmal, Hares, and Perakis, henceforth AHP, argue in Chapter 5 for a similar reassessment in low-income countries. They first document that, on the surface, private–public partnerships or PPPs have considerable potential. The share of children enrolled in unsupported private schools increased sharply from 1990 onward, and by 2019, 20 percent (primary) to 27 percent (secondary) of children were being educated in such schools. In many cases the per student cost in these schools at the point of delivery was lower than in public schools, providing an obvious target of opportunity: policymakers argued that they could pay for private school attendance through public funds, thus allowing children to attend (possibly) higher-quality schooling at lower cost. This lower cost was presumed to reflect more efficient delivery, or, as AHP put it, "Proponents of PPPs argue that private providers are more efficient and can achieve better learning, access, or equity outcomes at a lower per unit cost than government schools (Tooley and Dixon 2003). They suggest that PPPs can combine the theoretical benefits of the private sector with government financing, offering a way to reach more children while benefiting from the private sector's ability to reduce costs more than public schools can."

AHP go on to argue that the promise of PPPs has not been realized. Careful evaluations of different PPP models have yielded few outright successes. With the exception of some models that increased both enrollment and learning (for instance, in Sindh, Pakistan), the general message is that the public funding of private schools did not appreciably improve test scores or generate greater equity in schooling. Again, in their words: "Overall, most studies show small or no effects of engaging with the private sector to improve enrollment, equity, or learning."

Despite the negative finding, in their conclusion they are more positive when it comes to voucher and subsidy programs, which their review of the evidence suggests could increase enrollment at low costs: "They [governments] may well consider outsourcing schools through voucher or subsidy programs to take advantage of the differential and sometimes lower cost of delivery of the private sector."

This is overly generous. Although the studies that AHP include are careful evaluations with credible counterfactuals, they are ultimately limited by the variation they can exploit. In what follows, I show that this variation often does not allow us to answer key questions around the costs and benefits of PPP models, and the errors are in the direction that strengthen the argument against their use. I highlight five problems with existing studies, showing that the estimated impacts of PPPs on three key outcome variables—costs, enrollment, and test scores—do not provide the full accounting we need for a comprehensive policy analysis. I then conclude with a brief discussion of alternate methodologies and lessons from AHP's review. Like AHP, I maintain an emphasis on narrowly defined costs and benefits at the point of delivery. This misses the kind of political capture and corruption that has been documented in the United States; arguably the consideration of these costs would lead to an even higher bar than what is being currently proposed for a successful model. See https://networkforpubliceducation.org/april-2021-scandals/.

Five persistent empirical problems

Problem #1: The "cost-advantage" of private schools

Much advocacy around private schools and PPPs points to the cost advantage of private schools, as do AHP (albeit more cautiously). By this they mean that the costs at the point of delivery divided by the number of students are substantially lower in private compared to public schools. I have shown previously (Andrabi et al. 2008), and as AHP correctly emphasize, this cost advantage primarily reflects lower teacher salaries. This implies that private schools are more "cost-effective." By no means does it show that they are *more efficient*.

Technical efficiency in production is a question of translating inputs into outputs—if labor is the only input and one firm takes 10 hours of work to make 100 widgets while another takes 20, the first firm is more efficient. This conclusion is unchanged if the wages for Firm 1 are five times higher than for Firm 2, even though the cost of 100 widgets is now greater for Firm 1. Just like fuel efficiency for cars, which measures how far a car will go on a gallon of gas, the price of gas is irrelevant.

The problem arises when there are differences in output or multiple factors of production, so that the optimal input mix changes with factor prices and firms compute the best way of combining inputs, given prices, to produce a given output. But how do we know how much it would cost the private sector to produce the same test scores if it faced the same higher wage structure as public schools? Established methods of estimating firm-specific efficiency, such as stochastic production frontier estimates, all require a fully specified model of firms and the economy. How to incorporate the public sector into these models is not at all obvious unless we take a stand on the objective function of government schooling. For instance, if the government cares about equity and builds schools in remote areas, these schools may be small and costly and standard models may suggest that they are highly inefficient. But that conclusion is not warranted from the data or a sophisticated frontier analysis—it just means that the government is not maximizing profits, while remaining silent on whether it is indeed meeting its objective of reaching poor people.

The fact that private schools are not necessarily more efficient (or, at the very least, we have no data to suggest that they are) is important because the cost-effectiveness of private schools now boils down to different factor prices, which will likely change if PPPs are widely used. If, as in India's "aided schools," teachers' wages are equalized to those in the public sector, an outcome that courts have been favorable to under the principle of equal pay for equal work, the cost advantage could just as easily become a cost liability. Indeed, this process of teachers using the courts to argue against dual wage structures for the same work is documented by AHP in the case of teachers hired on temporary contracts in Ghana.

Problem #2: The cost advantage of PPP programs

A second basis for establishing a cost advantage is to compare the per child costs for children in a voucher program with those in public schools. For instance, in the province of Punjab, Pakistan, AHP calculate the per child costs of PPP programs by dividing the total budget by the number of children enrolled. But consider a simple example with two schools in a village, one public and one private, with 100 children each. Suppose it costs the government $10 to educate each child in the public sector (as AHP document for Punjab), so the total fiscal outlay for the government is $1,000. Costs in the private sector are $6 (as in the study that AHP discuss), so we maintain the greater cost-effectiveness of the latter.

Suppose that a new voucher scheme moves one child from the public to the private sector. If children enrolled in the private sector are all subsidized, this implies that a total of 101 children in the private sector now have to be subsidized at $6 each. The per child cost is indeed lower in the voucher program ($6 versus $10) and so is the total cost of educating children, but the fiscal burden on the public sector increases from $1,000 to $1,596 ($990 for 99 children in the public sector + $606 for 101 children in the private sector). The reason for this increase is that the price elasticity of demand for private schooling is low—all the children who wanted to enroll in the private school were already doing so prior to the voucher program. Consequently, the voucher program funds a large number of inframarginal children, pushing up the costs to the public purse.

The cost escalation is not a foregone conclusion—if 50 or more children had transferred to the private sector, overall government spending would indeed have declined. What is therefore critical is the demand elasticity of private schooling—in this example if public spending is to decline, we would require a price elasticity of demand of -0.5 or higher.[2] In recent work from Punjab, Pakistan, Carneiro et al. compute that the sectoral demand elasticity is -0.27 for girls and -0.10 for boys. These low elasticities imply that, like in the example, many children who are being funded under the voucher program would have gone to private school even without the subsidy, pushing up the cost to the government beyond the $6 that represents the cost per participating child. In fact, what is striking about Punjab is that even as voucher enrollments have increased to 2.4 million children, the overall fraction of children in private schools has remained remarkably on trend, suggesting that most of the voucher funding goes to children who would have chosen to enroll in private schools in any case. While remaining agnostic of the overall welfare implications, the program does not fulfill its objective of providing higher quality at a lower fiscal burden.

Problem #3: The enrollment benefits of PPP programs

AHP correctly highlight that most studies find it difficult to pinpoint the enrollment benefits of PPPs. This is because the areas where PPPs are implemented often have other schools, and therefore an increase in the enrollment in PPP schools could reflect the displacement of children from other schools. Realizing that administrative data on participating schools alone are insufficient to estimate enrollment effects of the program, Barrera-Osorio et al. (2020) partially address this problem through a household survey and indeed find evidence of displacement in their data (see also Dinerstein and Troy 2021).

But the deeper problem is that the definition of the catchment area should include all schools whose cross-demand elasticity is non-zero with regard to the PPP school. That is, it should include all other schools that are likely to react in some fashion to the introduction of a PPP program in a given school. Unfortunately, the research on PPPs has been stymied when it comes to defining the boundaries of the market and it should be obvious that such market definitions will be hard—perhaps impossible—in urban areas where people may use different forms of transport to access schools that are miles away from their homes. Consequently, and this is a general problem, PPP studies are unable to provide a full accounting of enrollment increases and in particular, the extent to which these increases reflect switching from other schools versus new enrollment. Without this accounting, it is difficult to establish the true enrollment benefits of such programs.

Problem #4: Test score impacts of PPP models when children change schooling status

As with costs and enrollment, there are equally severe problems with estimating test score impacts if PPP models change the schooling status of children in the affected populations.

Romero, Sandefur, and Sandholtz (2020) find that schools under a PPP arrangement encourage weaker children to drop out—a strategy that has also been used in charter schools in the United States to improve performance. One concern is that test score impacts based on children who are still in the school are biased as the "weakest" children have been asked to leave. With a great deal of fortitude and patience, this problem can be solved (as Romero, Sandefur, and Sandholtz (2020) do) by tracking children to their homes and testing them regardless of their current enrollment status. But studies that do not track dropouts or are largely unsuccessful in doing so will overestimate the test score impacts of the program.

2. I abstract from the question of what elasticity to compute.

The truly insidious problem is that children dropping out could change the experience of those who remain behind. Edward Lazear (2001) proposes a theory leading to nonlinear class-sized effects. In Lazear's model, a single disruptive child reduces the time spent teaching but two disruptive children lead to even higher disruptions than the sum of each individual disruption. Based on my personal experience as a bona fide disruptive student I can vouch for the validity of this model; I can also vouch that teachers improve the class environment by removing disruptive children from their class, either by asking them to stand outside (in my case) or, more perniciously, by working with the principal to remove such children from school entirely. If PPP schools engage in such behavior, any test score impacts will be overstated due to these external effects, even if researchers are able to track every child regardless of where they are.

To be fair, the sorting induced by a PPP program need not overstate the test score impact in all cases. For instance, consider the problem of accretion, whereby there are new children in the PPP school who were either previously unenrolled or were enrolled in other schools, as suggested by positive enrollment effects (irrespective of whether these are net gains or displacement from other schools). In the (common) worst-case scenario, baseline scores on these children are not available so very little can be said about the impact of the program on this population. Suppose that baseline scores are available. We now have to define the affected population. One option is to compare test score gains among new enrollments in the treated versus control schools, which is the correct estimate of the treatment effect on the newly enrolled as long as there is no selection induced by the program itself. But if the PPP program changes the composition of new enrollees, researchers will have to (statistically) identify the children induced to move through the introduction of the PPP program separately from the regular churn across schools. In some programs, accretion could reflect the bulk of the gains from PPP programs, like in Crawfurd (2017). How to do so remains an open question; machine learning offers new opportunities here but requires stringent assumptions. See Dean and Jayachandran (2020).

The third issue when children change schools is that if other schools are trying to maintain enrollment or profits, they will react to the change in the quality of the PPP school. Indeed Andrabi et al. (2022a) show that such spillover effects on test scores are just as large as the original effects in the context of grants to public schools in Pakistan. But these results could also go in the opposite direction—Dinerstein and others (2021) show that in the Dominican Republic a massive public school construction program led to the exit of private schools and quality improvements among those who remained.

The thrust of my argument here is that while researchers have focused on the sanctity of the sample tested in the baseline, that is not necessarily the correct policy-relevant sample. For obvious reasons, we should care about the performance of all children who are potentially affected by the program. If these effects were small, we could safely regard them as "second-order." Unfortunately, new research suggests that, in fact, these effects are a central part of programs that change how schools function and we just know too little at the moment to come to a firm conclusion on how PPPs affect test scores in the wider population.

Problem #5: Heterogeneity across schools

Romero, Sandefur, and Sandholtz (2020) also showed that the impact on test scores differed across PPP operators. Ongoing work on schooling markets by Andrabi et al. (2022b) demonstrates that a striking feature of education markets is the vast within-market, within-sector variation in quality. They show that every village in their sample has better/worse public and private schools, and depending on precisely what movements a policy induces between public and private schools, they can recover private school effects that range from highly negative to highly positive.

This heterogeneity matters because the effect of a policy will depend not only on the effectiveness of the PPP school but also the effectiveness of the schools from which the children are drawn. A PPP or voucher program targeted at poorly performing public schools will have a larger effect relative to one targeted at well-performing public schools. Similarly, contracting out a poorly performing public school to private management may lead to different test score effects relative to contracting out a high-performing public school. Heterogeneity—across PPP operators, across public schools, and across private schools—has not been investigated sufficiently, and it is only now that we have the data and methods that allow us to drill down to school-specific (test score–based) measures of quality. Without this deeper understanding we may wrongly extrapolate from specific programs ("vouchers aimed at poorly performing public schools led to a gain in test scores" to "vouchers increased test scores") to a broader level of generalization than what the evidence suggests.

Concluding discussion

To be clear: I am more pessimistic than AHP about PPP programs and the extent of the supporting evidence, much of which is weaker than what is evident at first glance. The evidence on efficiency and cost-savings is incorrect in a way that favors PPP programs; the evidence on enrollment probably overstates "new" enrollment from the program and the evidence on test scores is insufficient for a full accounting. I conclude with two observations, the first related to methodology and the second to the substance of such programs.

As far as methodology goes, the approach of working with schools rather than markets cannot solve Problems #2 to #5 discussed above. An alternate approach is to work with markets. This is proposed in my work with Tahir Andrabi and Asim Khwaja (Andrabi et al. 2017, Andrabi et al. 2020 and Andrabi et al. 2022a), where we introduced the idea of "market-level randomizations." In these studies, we leveraged the fact that villages in Punjab, Pakistan, are closed markets where more than 90 percent of children attend schools in the village and more than 90 percent of the enrollment in the schools is drawn from the village itself. This is still not the perfect definition for the market boundary, as we have not demonstrated that the cross elasticity of demand is zero for schools outside the village, but it is perhaps as close as we will get.

With these markets, studies could experimentally vary the availability of a PPP program. For instance, (some) schools could be contracted out to private management, or children could be given vouchers. The impact of the program is then the impact on enrollment and test scores of children, not in the school but in the village, which is uncontaminated by sorting, attrition, accretion, and/or peer effects. Finally, the total fiscal outlay at the village level provides the correct measure of the public costs of the program, as opposed to the average cost per child, which is what is currently used.

This still leaves Problem #1 of how to estimate the productivity of private and public schools unresolved. I don't see any easy solution, as it would require (experimental) variation both in the factor prices that private schools face as well as the location of the school. A simpler—and reasonable approach—is to give up on arguing that private schools are more efficient and instead focus on program costs, realizing that factor price differentials will always play a major role and will be decided (hopefully) within a broader democratic process that includes courts and government pay commissions—a process that is totally outside the control of the researcher.

On substance. One argument advanced by policymakers is that we don't have the "enabling environment" for good public service delivery and, therefore, it is easier to sidestep the task of public provision by contracting out to the private sector through PPP arrangements. By "enabling environment," my guess is that they mean the specific problem of how to manage human resources, as we have seen dramatic improvements in basic public finance management tasks like getting salaries to teachers on time, getting midday meals to children, or the construction and upgradation of schools. We have not seen such improvements when it comes to dealing with people; for instance, teacher absenteeism remained virtually unchanged over a 10-year period in India between 2003 and 2010.

AHP's review convincingly demonstrates that the enabling environment is not greener on the PPP side. PPP arrangements shift the government function from provision to regulation and market design, which are two tasks that low-income country governments have little experience with when it comes to education. For all the reasons that I have discussed here, it is not clear that even the limited success that AHP document can be regarded as clear improvements; instead of a one-time operation that allows governments to wash their hands of the problem, governments need to think of PPPs as ongoing processes that require considerable investments in developing a regulatory state.

At this point, it becomes important to ask if it is really worth it. The original research that spurred our movement away from the emphasis on public services never argued for wholesale privatization. Instead, that literature asked governments to face up to the fact that things were not working and that they needed to invest substantially in improving the quality of their service delivery. New research is now helping us understand that the problem of poor public provision is not a generic issue, thus reducing the dimensionality of the problem. Instead, the public system produces high-quality public schools that are as good as the best private schools even as it also produces a long tail of poorly performing public schools, often in the same village. Similarly, the latest generation of school-financing studies finds that financing can improve test scores in public schools, and such improvements lead to knock-on improvements in private schools as well. My guess is that we need to shift our focus back to the public sector with a renewed emphasis on the identification and improvement of those schools that are not performing well.

At the same time, we should be cautious not to throw the baby of private schooling out with the bath water of PPPs. In a series of studies, my coauthors and I have established that the market for unsubsidized private schools "works" in the sense that quality is recognized and rewarded by parents, and schools in turn make strategic quality investments knowing that these will be reflected in prices and enrollments. This does not imply that the market works perfectly—parental information is not perfect; schools do not have access to finance; and in many cases the market for secondary inputs is sparse or nonexistent. Neither does it mean that there is no role for government actions. We have argued—and demonstrated empirically in randomized studies—that the government can play a critical role in improving the functioning of the market without putting its thumb on the scale by supporting specific schools. This approach of supporting public and private schooling—but not private schools—is a fundamental shift in emphasis. But it's one that is long overdue.

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Comment: Be cautious about public–private partnerships, and fund students not schools

Moses Ngware

My short and straightforward understanding of a public–private partnership (PPP) is a service delivery arrangement between a government and a private entity, whereby the private entity solicits for money to finance a public good, analogous to what they do in infrastructure development. The private entity recoups its investment over time, earns a self-determined profit, and the service or product may revert to the government and/or the private entity may continue to deliver it at a negotiated fee. However, it is important to note up front that PPPs could have different goals—for instance to raise funds, as in infrastructure development, or to provide a service as in health and education.

Some of my understanding of a PPP resonates well with Akmal, Hares, and Perakis's chapter, which evaluates "the evidence on outsourcing education to the private sector." From the assessment of available evidence the chapter concludes that though evidence of PPP in education is mixed, PPPs are better for enrollment and not for learning outcomes. The big takeaway from the chapter is that of all possible models and/or schemes of PPP, governments in low- and middle-income countries (LMICs) are better off considering outsourcing schools "through voucher or subsidy programs to take advantage of the differential and sometimes lower cost of delivery of the private sector." There is merit to such a policy message in view of the need to expand policy options available to expanding access to and quality of education. In any case, the chapter cautions about the need for careful evaluation of the choice of the option. In the context of LMICs, the evidence is scarce and weak. The chapter provides very useful case studies of PPP in Africa that provide good lessons, such as the one in Uganda, now discontinued, and in Punjab in Pakistan.

I want to introduce two perspectives here to enrich this takeaway message. First, is the time ripe for LMICs to embrace PPPs in education?—no right or wrong response to this question. Second, why should a government consider PPP models of education service delivery when it can actually do it? The two questions are related, so I have a joint response to them. My perception of delivery of education services in LMICs is influenced by the notion of a public good, reaching vulnerable populations, context, equity–efficiency trade-offs, effectiveness of the service, and human rights perspectives.

Most education systems in LMICs are not ready for PPPs at this time, but they are ready for private investment in education to close supply gaps. Private investment in education has always been there in LMICs and has traditionally played a vital role in providing education to both high-income households who make a choice to utilize private education and low-income households who have little or no choice as private schools, especially low-fee private schools, are the only ones accessible and easy to integrate with their daily routines. These facts are well acknowledged in the chapter. As to whether what we observe on private-sector involvement in education in many LMICs is a PPP or simply private investment is a debate for another day. Available literature from both developed and developing countries indicates that PPPs are problematic, especially in the social sector, which then raises the above questions on country preparedness and/or why they should consider PPPs. Four main points shed more light on the questions:

1. There is inadequate robust evidence on how PPPs work. For example, around the middle of the last decade, available evidence showed that of the 442 PPPs supported by the World Bank, only 2 percent had been evaluated for their impact on poverty.
2. PPPs are usually in the favor of the private sector at public cost—perhaps because the private sector are better negotiators. This negotiation power, supported by integrity issues on the part of the negotiators from both sides, casts doubts of PPPs being a suitable policy option in education.
3. PPPs are generally risky undertakings, and there is no evidence to suggest that the LMICs have the necessary and sufficient conditions and/or institutions to ensure PPPs have beneficial outcomes in both the short and long term. In fact, proponents of PPPs admit that these models left behind negative fiscal legacies in countries such as Ghana, Lesotho, Peru, Tanzania, and Uganda.
4. Increasingly, instruments that define public education provision embedded on a human rights framework continue to emerge. One such framework is the Abidjan Principles that spells out the obligation of states in providing public education, and how to regulate private involvement in education. A study by Civil Society Organizations in Malawi, Mozambique, Nigeria, and Tanzania raises concerns on the extent to which PPPs can comply with a country's equity goals, and/or the extent to which they can provide cheaper financing for education.

Based on the above, my four key takeaway messages for LMICs are:

- Since there is power in partnerships, and there is no panacea for the many education challenges, use PPP models only if it will benefit the poor directly, not implicitly.
- Use PPP models if they enhance inclusion in access to education as alluded to in the chapter; for example, if we introduce a PPP model, will the previously excluded population now have affordable access to schooling and education given the prevailing conditions?
- There should be compelling and contextual reasons why an LMIC would want to adapt or adopt a PPP in education—e.g., a totally "failed" system of education.
- There are good and traditionally available models of private provision of education including subsidies and scholarships that target individuals and households, and not schools. These collaborative models can be enhanced through direct capitation grants to support individual students and/or their households to access low-fee private schools, especially in low-resourced environments. These models operate outside formalized PPPs, and should be allowed to thrive.

Chapter 6. Finance: Ambition Meets Reality

To achieve universal primary and secondary schooling, unit costs are going to have to come down—dramatically.

Jack Rossiter

The United Nations' Sustainable Development Goals (SDGs) set ambitious targets for high-quality, universal education by 2030. But existing efforts to "cost the SDGs" return unattainable price tags. In this chapter, we first review approaches to costing the SDGs in the education sector. Then, we estimate realistic domestic expenditure levels to 2030 and work back to policy options. Even if international financing comes in line to meet targets, governments are not going to have anything like the sums that costing exercises require. We can choose to ignore this shortfall, stick with plans, and watch costs creep up. Or we can see it as a serious budget constraint, redirect our attention toward finding ways to push costs down, and try hard to get close to universal access in the next decade.

6.1 Introduction

The United Nations' Sustainable Development Goal 4 (SDG4) sets ambitious targets for high-quality, universal preprimary, primary, and secondary education by 2030. But existing estimates of the cost to get all young people into school and learning exceed any plausible level of available finance in low- and middle-income countries. In this chapter, we pivot from how much money countries might need to achieve SDG4, to how much money countries might have for their education sectors over the next decade.

Many organizations have put considerable expertise into efforts to "cost the SDGs". The standard approach begins with the design of a service package intended to achieve certain access and quality standards. The price tag for each package can then be built up from its contents. High price tags tend to be driven by low pupil-teacher ratios, high (relative) wages and additional budget for new materials and equipment, or an expanded education workforce.

A financing plan is paired with the investment case, outlining expectations for international and domestic actors. Across the board, plans call for large increases in international financing. Headline figures for the international financing gap run into the tens of billions but remain within ranges that may be attainable. In contrast, and quantitatively far more significant, reaching plan goals requires substantial increases in domestic public spending on education that do not seem fiscally feasible.

Even if international finance and household spending come in line to meet plan targets, governments are not going to have anywhere near what SDG4 costing plans require. We estimate that low- and middle-income country governments might spend US$1.9 trillion on education in 2030.[1] That is $750 billion (28

1. To simplify direct comparison with existing spending estimates, all values reported in this chapter are in constant 2014 US dollars unless otherwise stated.

percent) lower than well-publicized costing estimates for domestic public spending on education (Education Commission 2016). We then look at how spending changed during periods of rapid educational expansion and use this to stretch the rate of spending growth for all countries. Even if all countries increase education spending in line with this speculative upper bound, domestic spending in 2030 falls $350 billion short of the level required to get all young people into school and learning.

We can choose to ignore this shortfall and watch costs creep up. Or we can accept it as a serious budget constraint, redirect our attention toward finding ways to push costs down, and try hard to get closer to universal access in the next decade. Countries (and their international partners) will need to prioritize or find new ways of delivering if they want universal primary and secondary education. This will involve reducing unit costs by tackling assumptions regarding resource requirements—including teachers, wages, and other recurrent costs.

This chapter proceeds in two sections. In the first section, we review attempts to estimate the cost of providing a quality education for every child. We outline approaches and suggested sources of finance to fill in gaps. In the second section, we project countries' expenditure levels and contrast these with existing cost estimates.

Box 6.1. Can developing countries afford to feed kids and abolish secondary school fees?

This box was contributed by Biniam Bedasso, Lee Crawfurd, Jack Rossiter, and Justin Sandefur

There's an obvious tension across the various chapters of this report. Chapters 1, 2, and 3 make the case for ambitious new investments, while chapter 6 paints a slightly dour budget picture. Chapter 4 advocates containing expenditure growth on teacher salaries to make room for other initiatives, but it's unclear whether the overall budget arithmetic works out. This box outlines very rough, back-of-the-envelope calculations that suggest the biggest ticket items described in this report—namely, universal school meals and free secondary school—are plausibly affordable for low- and lower-middle income countries by 2030.

How much would it cost to remove secondary school fees? In some sense, how long is a piece of string? As we show in chapter 3, there have been very many instances in which governments have announced free education but not spent any money at all, and accordingly seen no change in enrolment. If we instead focus on the cases in which fees were removed as part of a program or policy with associated spending plans, we can see that overall spending on primary school increased by around 0.3 percentage points of GDP as part of a successful reform (chapter 3, Figure 7). This expansion was associated with around a 25-percentage point increase in gross enrolment (chapter 3, Figure 4). Of course, secondary school is usually more expensive than primary school. According to World Bank estimates, the ratio of per pupil secondary spending to per pupil primary spending is an average of 2.05 in low-income countries and 1.35 in lower-middle-income countries. So successfully removing secondary school fees might cost between 0.6 percent of GDP in low-income countries to 0.4 percent in lower-middle-income countries.

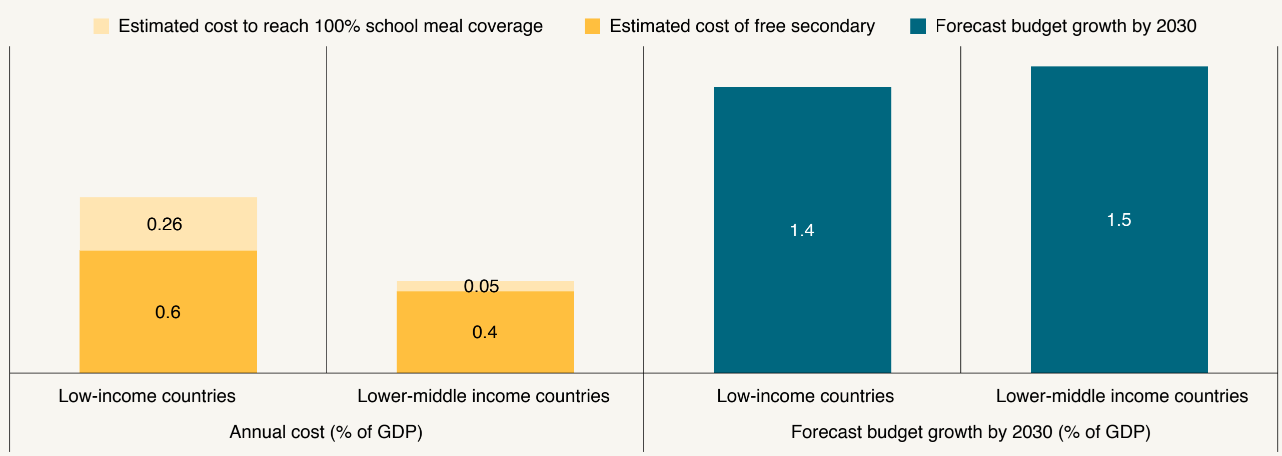
Box 6.1. Continued

How much would it cost to expand free school meals to 100 percent of primary school students? The content of a school meal varies dramatically across countries. Ideally, one would standardize unit costs by calories and the number of days food is delivered. However, for the purpose of calculating the cost of expanding existing programs, "as is," it suffices to obtain the current unit cost. Based on the data presented in chapter 3, the median cost of feeding a child is $37 per pupil per year for a low-income country and $32 for a lower-middle income country. Given baseline levels of school feeding, expanding meals to reach 100 percent of pupils would cost the median low-income country 0.26 percent of GDP, and just 0.05 percent of GDP in the median lower-middle income country where baseline levels of school feeding are higher and unit costs are lower.

On the other side of the ledger, how much budget room are low- and lower-middle income countries likely to have between now and 2030? Based on the budget projections in chapter 6, assuming historical patterns hold, a typical low-income country would see education expenditure increase by about 1.4 percent of current GDP by 2030. So one thought experiment is to imagine that current spending levels are locked in, in real terms, and discretionary planning is restricted to that marginal 1.4 percent. If low-income countries beat historical trends and expanded education spending in line with the more ambitious examples of recent decades, they might add another 0.7 percent of GDP, but we'll ignore that here. In the case of lower-middle income countries, the benchmark projection in chapter 6 is a budget increase of 1.5 percent of GDP by 2030.

In short, the total cost of free secondary school and universal free school meals is far less than the anticipated expansion in public expenditure on education between now and 2030 in both low- and lower-middle income countries, as shown in Figure B6.1. Of course, doing these things would require difficult cost containment on other fronts. And we want to reiterate how crude these calculations are. But the purpose of our very rough exercise here is simply to demonstrate the costs implied are not obviously beyond reach, even under a business-as-usual spending scenario.

Figure B6.1. The cost of school meals and free secondary vs. forecast budget increases



Source: Authors' calculations. See text for underlying assumptions and data.

6.2 SDG4 reflects big expansionary goals with huge price tags

Many organizations have put a price on providing a quality education for every child (Table 6.1). Each package differs in content, educational levels, and countries covered. Yet common assumptions include declining pupil-teacher ratios[2] and teacher salaries moving to the levels of the top-paying half of countries at the same income level. With a goal of increasing quality and equity, packages also earmark a sizable budget for materials and administrative support and anticipate additional per-pupil costs for attracting marginalized students (Wils 2015; Education Commission 2016). A total cost for each package can then be built up from its contents, with universally high price tags.

A plan of how to finance the investment is prepared alongside each package. For instance, UNESCO's financing plan for low- and lower-middle-income countries requires total spending to rise from 3.5 percent to 6.3 percent of GDP between 2012 and 2030 (UNESCO 2020). The Education Commission estimates total spending to rise from $1.2 trillion in 2015 to $3 trillion in 2030 (Education Commission 2016). And several others have indicated similar annual costs (e.g., Manuel et al. 2018) or shares of GDP for education (e.g., Sachs 2018). More recent estimates from UNESCO (2021) and Theirworld (2021) extend these projections to account for COVID-19-related impacts on financing. In principle, these costs are achievable with the right blend of domestic prioritization and international support.

Most international discussion has focused on the so-called financing gap, which can be understood as the shortfall between total cost and total projected domestic spending. In UNESCO's original plan, the gap was an annual $39 billion,[3] close to the Education Commission's estimation of $44 billion each year. Awareness of this gap has encouraged efforts to increase aid for education and finance from other international sources, including novel lending mechanisms.

The equivalent domestic financing gap is harder to see in plans but is quantitatively far more important. It can be thought of as the shortfall between the plan's projected level of domestic spending and some business-as-usual level of domestic spending. This is an important distinction because SDG plans build assumptions for education prioritization into their projections. For instance, UNESCO assumed that low-income country governments would see basic education spending rise from 2.6 to 3.9 percent of GDP from 2015 to 2030 (excluding aid). And the Education Commission expected lower-middle-income country governments to increase total education spending from 4.1 to 6.0 percent of GDP over the same period.

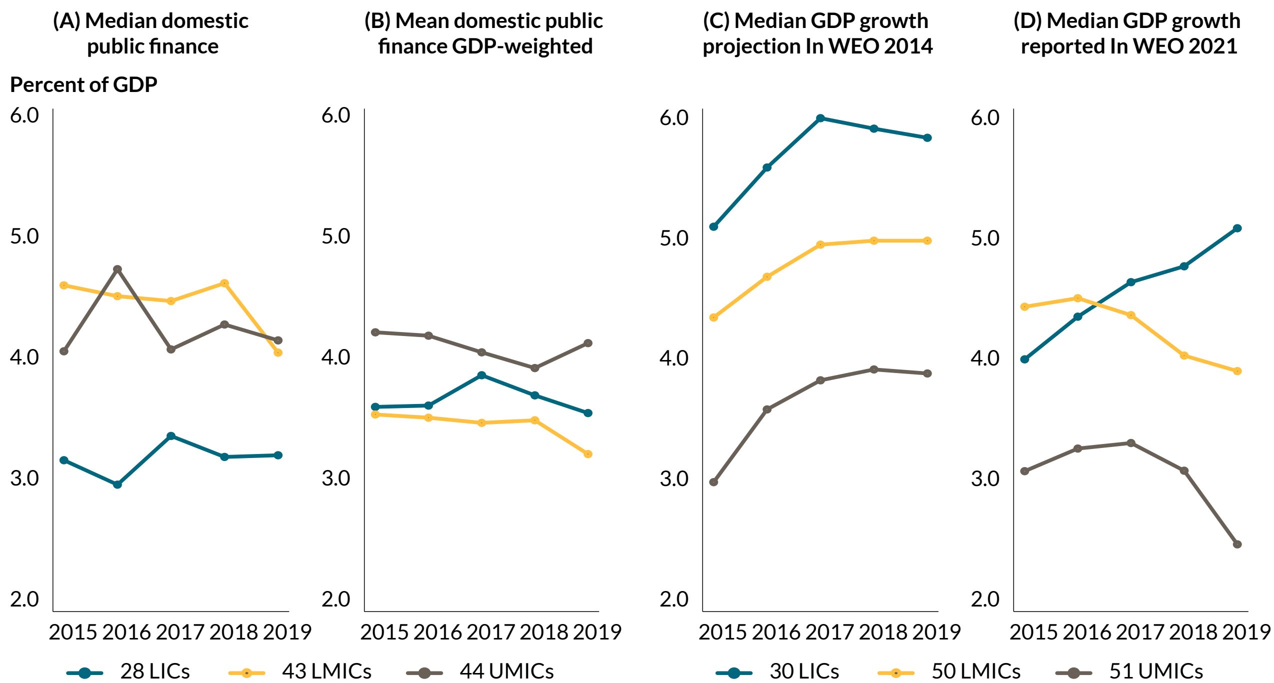
However, these spending goals now appear unattainable, especially in light of COVID-19. UNESCO (2020) revised its estimate for the annual financing gap upwards from $39 billion to $148 billion. This revision was among the first warnings that countries were not able to meet spending targets assigned to them in costing plans.

Six years into the SDG period, we can take stock of recent education financing patterns. Our analysis benefits from data that did not exist in 2015, showing that education prioritization within government spending was failing to keep pace with plan ambitions even before COVID-19 (Figure 6.1, Panels A and B). In addition—and this is relevant to all models that rely on International Monetary Fund (IMF) data for GDP growth—looking backwards in 2021, low- and middle-income economies have not expanded at the rates anticipated for the 2015–2019 period (Figure 6.1, Panels C and D).

2. Target levels for pupil-teacher ratios are not specified. Instead, as GDP per capita grows over the projection period, the pupil-teacher-ratio will approach the international trend line. Wils (2015) suggests that this approach might return a pupil-teacher ratio of 29 in primary and 27 in secondary schools across low- and lower-middle-income countries by 2030.

3. In 2012 US dollars.

Figure 6.1. Domestic finance for education hasn't lived up to SDG4 plan aspirations



Sources: World Bank, UNESCO Institute for Statistics (UIS), and IMF World Economic Outlook (WEO).

Note: Countries grouped by World Bank 2015 income groups, namely: low-income countries (LIC), lower-middle-income countries (LMIC) and upper-middle-income countries (UMIC).

In the remainder of this chapter, we try to shift the emphasis. We start by projecting business as usual country expenditure levels to 2030 and work back from there. To do this, we estimate the historical relationship between growth in real GDP and growth in education expenditure. By looking at how this varies by income group and during periods of rapid educational expansion, we can project budgets for each country before considering policy options.

6.3 What are realistic expenditure levels for education?

In this exercise, we attempt to estimate business as usual domestic expenditure levels for education, through 2030. This can then be used as a budget envelope for considering unit costs and policy options.

Estimating a relationship between economic growth and education spending

Education spending in any year is a function of GDP, government revenue and the share of education in government spending. Our budget estimates are built from historical data on

- GDP for 182 countries, since 2000;[4]
- public education expenditure for the same countries and years;[5] and
- country income classifications by country and year.[6]

4. World Bank World Development Indicator (WDI) data on GDP, https://data.worldbank.org/indicator/NY.GDP.MKTP.KD, accessed March 2022; IMF World Economic Outlook public expenditure data, October 2021, https://www.imf.org/en/Publications/WEO/weo-database/2021/October.
5. World Bank WDI data (UNESCO UIS via World Bank) on education spending as share of public expenditure, September 2021, accessed March 2022, https://data.worldbank.org/indicator/SE.XPD.TOTL.GD.ZS.
6. World Bank Country and Lending Groups, Historical Classifications, accessed October 2021, https://datahelpdesk.worldbank.org/knowledgebase/articles/906519-world-bank-country-and-lending-groups.

Table 6.1. Costing estimates for providing a quality education for every child by 2030

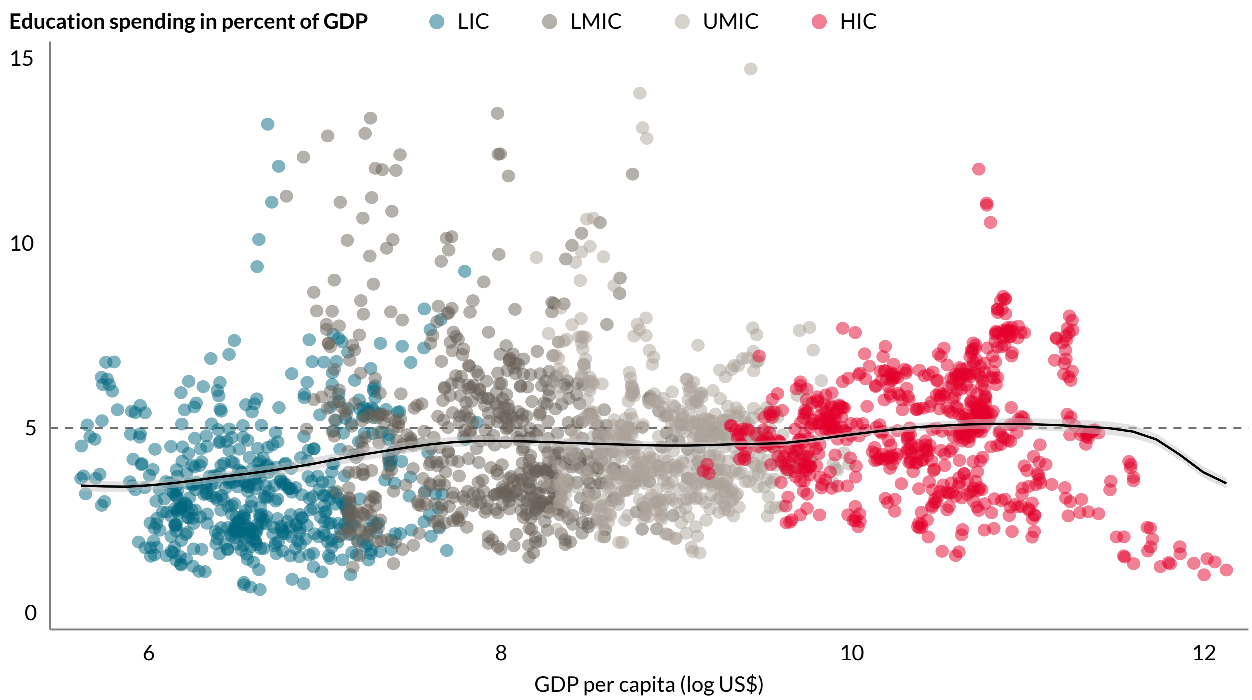
	Education Commission (2016)	IMF (Gaspar et al. 2019)	ODI (Manuel et al. 2018)	Move Humanity / SDSN (Sachs et al. 2018)	Schmidt-Traub (2015)	UNESCO (UNESCO 2015; Wils 2015)
Title	The Learning Generation: Investing in Education for a Changing World	Fiscal Policy and Development: Human, Social, and Physical Investment for the SDGs	Financing the End of Extreme poverty	Closing the SDG Budget Gap	Investment Needs to Achieve the Sustainable Development Goals	Pricing the Right to Education: The Cost of Reaching New Targets by 2030
Headline cost	Total LIC and MIC spending to rise from $1.2 trillion per year to **$3 trillion by 2030**	Education and health combined require additional spending of 8.3 percentage points of 2030 GDP in LIDCs (**$284 billion**) and 2.0 percentage points of 2030 GDP in EMEs (**$1 trillion**).	LIC and MIC annual education spending of **$1.1 trillion** average for the period 2015-30.	Total education requirement: **10% of GDP** in LICs and **7% in OLIDCs.**	LICs and LMICs average annual education spending of **$194 billion** on top of current spending.	LICs and LMICs average annual cost of **$340 billion** over 2015–2030.
Unit	Cost in 2030	Cost in 2030	2015–2030 average	2018–2030 average	2015–2030 average	2015–2030 average
Cost per student (in LMICs, unless stated)	In 2030. Preprimary: $571 Primary: $605 Secondary: $886 Postsecondary: $3,631	Not reported	2015–2030 average. $41 median per person in LICs and LMICs (per person, not per student as exercise estimates for all sectors at the population level).	2018–2030 average. $525 per student in OLIDCs	Not reported	In 2030. Preprimary: $842-$1,069 Primary: $510 Lower secondary: $573-$639 Upper secondary: $811
Price base	Constant 2014 US$	Constant 2016 US$	Constant 2017 US$	Constant 2018 US$	Constant 2013 US$	Constant 2012 US$
Education levels covered	Preprimary to postsecondary	Preprimary to postsecondary	Preprimary to secondary	Preprimary to secondary (ages 4–18)	Preprimary to secondary	Preprimary to secondary
Other sectors covered	None	Health Infrastructure	Health Social Protection	Health Infrastructure Social Protection Environment	Health Infrastructure Agriculture Environment	None

Table 6.1. Continued

	Education Commission (2016)	IMF (Gaspar et al. 2019)	ODI (Manuel et al. 2018)	Move Humanity / SDSN (Sachs et al. 2018)	Schmidt-Traub (2015)	UNESCO (UNESCO 2015; Wils 2015)
Approach	Macro-model with intervention component. Variation of UNESCO/Wils. Uses enrollment growth paths of top performers. Includes adjustments to account for learning improvements.	Input-outcome benchmarking exercise, assigning inputs (e.g. teachers per student) observed in well-performing countries today to countries in 2030. Uses this to quantify the annual cost of achieving high performance.	Variation of UNESCO/Wils. Adapted to include UMICs, where costs are assumed to be 4% of GDP.	Built up from unit costs per student. $330 per student for LICs and $525 per student in OLIDCs. Based on an analysis of the costs of providing particular SDG-based goods and services.	Variation of UNESCO/Wils. Rebased to 2013.	Macro-model. Selects inputs required to reach full enrollment with quality. Hinges on pupil teacher ratio, teacher wages, additional costs to reach marginalized; projected population and enrollment.
Domestic financing	Domestic spending rises from 4.0% to 5.8% of GDP between 2015 and 2030. GDP growth rates follow IMF until 2020 and then the earlier average up to 2030, subject to a maximum of 5%.	Financed through an increase in tax-to-GDP ratio of 5 percentage points of GDP. Sufficient for EMEs, not for LIDCs. Greater efficiency of spending assumed throughout.	Not clearly stated. In some instances, domestic spending on education reaches 20% of government spending.	Annual 7% GDP growth rate for LICs and 5% for OLIDCs, over 2018-30. Domestic revenue assumed to rise to 24% and 29%, respectively.	Annual GDP growth rates of 8% in LICs, 7% in LMICs and 5% in UMICs. Assumes annual growth rate in public spending on the SDGs of 12.5% in LICs and 11.4% in LMICs.	Domestic spending rises from 3.5% to 6.3% of GDP between 2012 and 2030. GDP growth rates follow IMF to 2016 then converge at a long-term average of 5%.
Major means of financing	Increase tax revenue Increase education as percentage in budget (e.g., reallocate energy subsidies) Continue household and private contributions to domestic spending Increase official aid and philanthropy Other international financing for education including innovations such as the International Financing Facility for Education.	Increase growth Increase tax to GDP ratio by 5 percentage points Improve spending efficiency Provide debt financing (public debt or guarantees) Increase foreign direct investment Deliver current ODA commitments Private philanthropy	Increase tax effort to approach theoretical potential. Assumes 50% of total potential tax revenue for three social sectors	Setup novel tax channels (e.g., on off-shore accounts, high-net worth, tech giants, and conspicuous consumption) Increase and better target ODA Generate new ODA from novel taxes on carbon and financial transactions Provide debt relief to open fiscal space	Increase tax revenue and SDG prioritization Increase ODA to meet country commitments Provide concessional public finance from non-Development Assistance Committee HICs Provide concessional public finance from UMICs	Increase tax revenue Prioritize education spending. If less than 6% of GDP on education then tax revenue and education share of government spending increase to 30% and 20%, respectively.

Note: Country groupings as follows: LICs = low-income countries; MICs = middle-income countries; LMICs = lower-middle-income countries; UMICs = upper-middle-income countries; LIDCs = low-income developing countries; EMEs = emerging market economies; OLIDCs = other low-income developing economies.

Figure 6.2. Relationship between GDP per capita and education spending (all years, 2001–2019)



Source: World Bank and UNESCO UIS data.

Note: Shown for country-by-year World Bank income group classifications.

These data are used to estimate the relationship between growth in real GDP and growth in education expenditure, which is allowed to vary by income group and country (Table 6.A1). This analysis considers only on-budget aid that is included in public spending. Given education spending data are not available for each country-year, the relationship was tested on annual data and for three-year periods.

Overall, a 1 percent increase in GDP generates a 0.86–1.00 percent increase in education spending depending on model chosen. And this relationship varies according to income level. The growth elasticity of education spending reaches 0.99 for low-income countries but is just 0.57 for high-income countries.

A cross section of education spending in GDP and GDP per capita illustrates this growth–spending relationship. Our chart doesn't track individual countries but is consistent with the regression results. Figure 6.2 indicates an increase in spending among low-income countries and upper-middle income countries, but with a flatter profile at lower-middle income and high income levels. Countries are classified by their income level in that year, and a horizontal line is included at 5 percent of GDP, showing how few country–year points have exceeded that level of spending.

Predicting budgets to 2030

Business as usual

We pair parameters from our historical spending analysis with IMF growth projections to estimate education spending to 2030. We use the October 2021

World Economic Outlook projections[7] of GDP growth, which extend to 2026. For simplicity, we carry 2026 growth rates forward through 2030.[8]

We predict each country–year change in education spending based on country income level and projected changes in GDP. From there, we estimate future levels of GDP and education spending, which we convert into estimates of education spending as a percentage of GDP. For countries for which we know education spending levels up to 2019, we use those rates. For countries with unknown spending between 2015 and 2019, the projection starts sooner, and we estimate spending levels following the same approach.[9]

We include 135 low- and middle-income countries in our analysis. For 121 of these countries, we have GDP and historical education spending data and can project a country-specific estimate. For 8 countries, there are no education spending data available since 2001, so we estimate their spending rate as the median among countries in the same income group.[10] There are 6 countries for which we lack GDP estimates or GDP growth projections, so we are unable to forecast education spending.[11]

Our projections suggest only minor changes to education spending in GDP over the SDG4 period (Table 6.2). The largest increases are registered in low-income and upper-middle-income countries. Among lower-middle-income countries, changes in education spending as a share of GDP are more modest. These estimates are quite different from the core assumptions in existing costing models. For example, the UNESCO model assumes that countries will converge toward a spending level of 6 percent of GDP in 2030 (Wils 2015). And the Education Commission projects a simple average of 5.8 percent of GDP across all low- and middle-income countries in 2030 (Education Commission 2016).

Rapid spending growth

An alternative way of thinking about countries' potential to increase education spending is to look back at large-scale reforms and see how government spending changed alongside them. We consider reforms to primary schooling in low- and middle-income countries. Many of these sought to universalize primary education, with changes to legislation or education policy and, in several cases, the removal or reduction of user fees.

Table 6.2. Projected education spending as a percentage of GDP by income group and year

	2015 income groups		
	Mean (median) low-income country	Mean (median) lower-middle income country	Mean (median) upper-middle income country
Education as % GDP (2015)	3.4 (3.2)	4.8 (4.6)	4.6 (4.1)
Education as % GDP (2030)	3.7 (3.3)	4.7 (4.1)	5.4 (4.7)

Source: Authors' calculations based on World Bank, UNESCO, and IMF data.

Note: Projections are shown by World Bank 2015 country income classification. For completeness, two low-income countries (Afghanistan and Ethiopia) are included in these projections using April 2021 growth projections, despite their omission from IMF October 2021 forecasts.

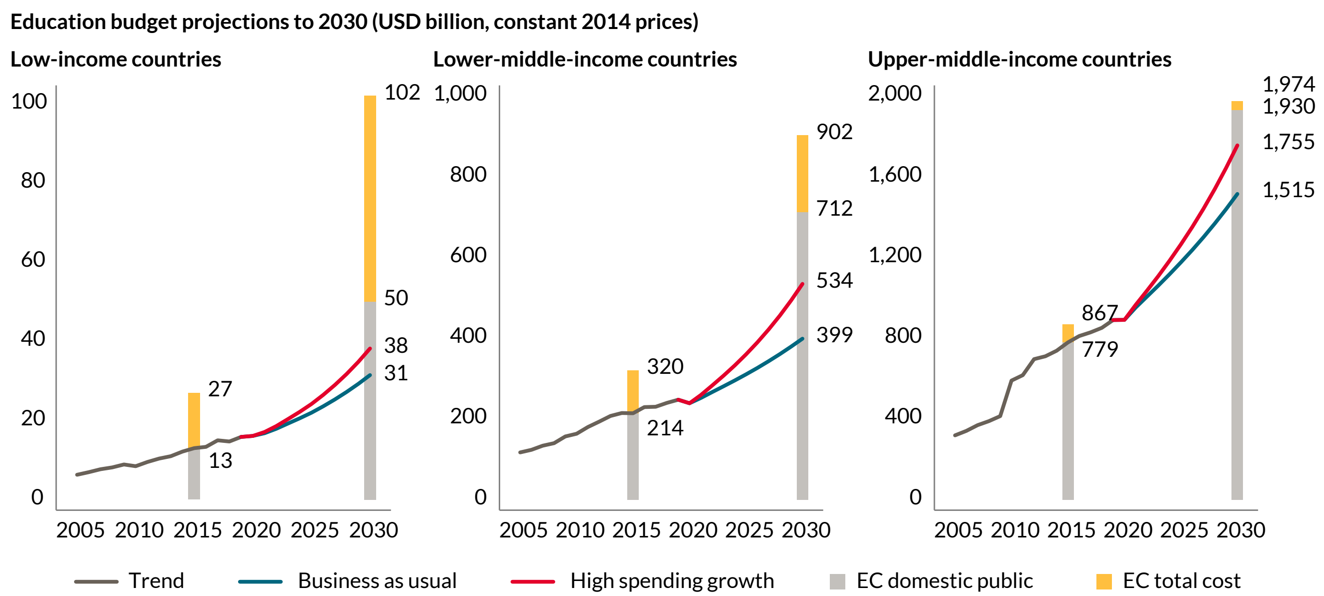
7. The IMF's October 2021 World Economic Outlook included no forecast for Afghanistan or Ethiopia. To retain these economies in our projection, we use their April 2021 projections, but with the understanding that these countries are unlikely to achieve rates released at that time. We note this choice in the accompanying dataset.
8. We use GDP growth rates rather than levels. This assumes that economies will return to their pre-COVID growth trajectory rather than their pre-COVID output level.
9. Summary statistics from this model, for each country, can be found at: https://docs.google.com/spreadsheets/d/1KQ9Jg4a2XudI_fbWFBay7akQsDCazlB6ZIy1saxLDB0/edit#gid=54084408.
10. These countries include Bolivia, Bosnia and Herzegovina, Iraq, Kosovo, Montenegro, Nigeria, Somalia, and Tuvalu.
11. These are American Samoa, Cuba, Eritrea, Democratic People's Republic of Korea, Lebanon, and Syrian Arab Republic.

We create an optimistic projection ("high spending growth") based on 10 large-scale education reforms. Starting from a database of 210 reforms since 1960 (Crawfurd and Ali 2022), we identify 48 that have sufficient data to assess the change in education spending around the time of the reform. Among these, 10 reforms coincide with increases in national primary enrollment, which we take as an indication of policy success. Each reform is listed in Figure 6.A1, alongside changes in spending and changes in enrollment.[12]

Finally, we estimate the average annual growth rate of education spending across these 10 reforms. Each country is assumed to meet this annual rate of education spending growth; if a country already exceeds this annual rate, the higher value is retained. The resulting "high spending growth" scenario accompanies the business-as-usual model.

Total domestic finance can then be calculated for each projection. We aggregate by 2015 country income classifications, to align with the grouping used in most existing costing exercises. We rely on the Education Commission (2016) model for this comparison because it includes all levels of education and thus relates to total government spending on education, which our data cover, and because it covers all low- and middle-income countries, which we also include in our analysis. Figure 6.3 shows, for each income group, the historical trend in total spending, the rate of change implied by the Education Commission model, and the estimated total budgets from our scenarios.

Figure 6.3. Even optimistic budget projections fall far short of costing estimates



Source: Education Commission (2016) and authors' calculations based on World Bank, UNESCO, and IMF data.

Note: EC = Education Commission (2016). Projections are shown by World Bank 2015 country income classification. "Business as usual" shows the predicted level of education spending to 2030 based on a model of historical education spending and GDP growth. "High spending growth" shows a more ambitious curve, which assumes that each country reaches or exceeds a rate of spending growth seen among 10 large-scale and successful education reforms since 1960. For completeness, two low-income countries (Afghanistan and Ethiopia) are included in these projections using April 2021 growth projections, despite their omission from IMF October 2021 forecasts. As a reference, the yellow and gray bars together show the education spending levels estimated for 2015 and 2030, as reported in Education Commission (2016); gray indicates the portion to be covered by domestic governments, while yellow is the amount assumed to be filled by households, the domestic private sector, or from one or more international sources.

12. Education spending data are noisy, so we take averages of three or five years either side of the reform year. We retain episodes where we have multiple data points in each bin, on either side of the reform. This choice is an attempt to avoid noise or error in a single estimate; different approaches could loosen this constraint.

Under a business-as-usual assumption, by 2030, low- and middle-income countries might spend $1.9 trillion on education. This figure is around $750 billion lower than well-publicized SDG costing estimates for government spending in 2030. Even if all countries rapidly increase their education spending as a share of GDP, as indicated by the "high spending growth" curve, a $350 billion gap in domestic spending remains. And this is in addition to the $290 billion that is expected from household spending and international finance in that year (Education Commission 2016).

6.4 Conclusion

Since 2015, organizations have been trying to put a price on providing a quality education for every child. Considerable effort and expertise have gone into developing plans and projection models, which have greatly improved sector knowledge of what it might take to deliver SDG4 and how countries can get there.

However, we argue that estimates of required domestic financing from these initiatives far exceed—by around $750 billion in 2030—any plausible level of available financing in low- and middle-income countries. Even optimistic budget projections for the education sector fall short of costing estimates. International financing may reach the $40–$50 billion levels expected of it each year, but these sums pale in comparison to the cost of meeting global education targets at current unit costs.

At this stage, we may need to accept that we're not going to achieve SDG4 or an inclusive and equitable quality education by 2030 (UNESCO 2020; Savage 2021). And we can choose to ignore this, watch costs creep up, and double down on efforts to fill a financing shortfall.

Or we can redirect some of our attention toward finding ways to push costs down and try hard to get close to universal access in the next decade. Back-of-the-envelope calculations suggest that by 2030, the average lower-middle-income country government may have domestic education resources of around $550–$650 per child. This may seem incredibly low, particularly given typical per student costs at higher education levels, but it may also be a suitable place to start in considering policy options across and within educational levels.

Countries and their international partners will need to work together to prioritize and find new ways of delivering. If we take this as a budget constraint, then we need to go back to our investment plans and consider what we may need to sacrifice if we want to reach all children. This will likely require us to tackle the main cost drivers, including lowering pupil-teacher ratios, moving teacher salaries to the levels of the top-paying half of countries at the same income level, and adding substantial recurrent budget for materials and administrative support (issues that are explored in detail in Chapters 1, 3, and 4).

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Chapter 6 Appendix

Table 6.A1. Growth of education spending on growth of GDP, for 182 countries since 2000

Growth	0.86*** (0.072)	0.57*** (0.13)	0.42** (0.16)	1.00*** (0.071)	0.76*** (0.12)	0.69*** (0.17)
Growth x UMIC		0.39*** (0.15)	0.62*** (0.20)		0.12 (0.14)	0.38 (0.21)
Growth x LMIC		0.24* (0,.14)	0.34* (0.20)		0.19 (0.12)	0.34 (0.21)
Growth x LIC		0.42*** (0.14)	0.54** (0.22)		0.40*** (0.12)	0.39* (0.22)
Constant	0.0087** (0.0035)	0.0090** (0.0035)	0.010** (0.0039)	0.0097 (0.0094)	0.013 (0.0094)	0.012 (0.013)
Observations	2,182	2,182	2,175	839	839	824
Country FE	No	No	Yes	No	No	Yes
3-yr Averages	No	No	No	Yes	Yes	Yes

Table 6.A2. "Successful" large-scale expansions used to model "high spending growth" scenario

Country	Reform year	Education as % GDP			Primary Gross Enrollment Rate (%)	
		Before (%)	After (%)	Change (pp)	1 year pre-reform	1 year post-reform
Togo	2008	3.3	4.3	1.0	111	125
Burundi	2006	3.3	6.1	2.8	81	103
Central African Republic	2005	1.6	1.3	-.3	58	63
Ghana	2005	6.4	5.5	-.9	87	91
Kenya	2003	5.5	7.1	1.6	88	100
Madagascar	2002	2.1	2.9	.8	103	118
Lesotho	2000	10.8	11.2	.4	96	112
Niger	1998	2.2	2.4	.2	29	31
Kenya	1979	5.4	5.4	0.0	98	118
Kenya	1974	4.4	5.5	1.1	71	108

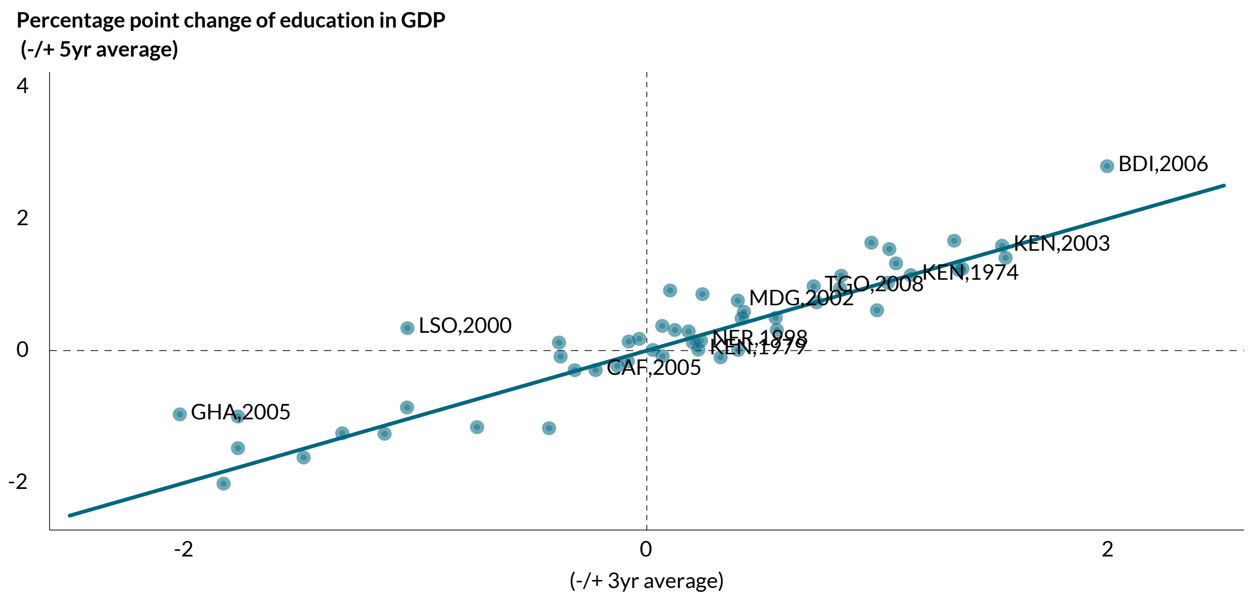
Note: Spending values are based on five-year averages before and after the reform year. The pattern is the same, albeit with slightly different values for each country, if three-year averages are used (see Figure 6A.1).

We create an optimistic projection ("high spending growth") based on 10 large-scale education reforms (Table 6.A2). In selecting these reforms, we take the following steps:

1. Start from a database of 210 reforms since 1960 (Crawfurd and Ali 2022)
2. Identify 48 of these that have sufficient data to assess the change in education spending around the time of the reform, defined as
    - a. 3 or more data points within the three years before and three years after the reform year and 6 or more data points within the five years before and five years after the reform year; or
    - b. 4 or more data points within the three years before and three years after the reform year and 4 or more data points within the five years before and five years after the reform year.
3. Identify 10 of these which have been shown to be accompanied by jumps in national primary enrollment
4. Estimate the average annual growth rate of education spending across these 10 reforms.

Figure 6A.1 shows reform episodes for which we have sufficient spending data, with 10 "successful" cases labeled. The figure shows the percentage point change in spending according to the three-year pre/post average and the five-year pre/post average.

Figure 6.A1. Changes in education spending around historical large-scale reforms



Note: shown for all large-scale reform episodes (e.g. FPE) with sufficient education spending data points. Ten labeled countries are the 'successful' subset for which an enrolment jump accompanied the reform.

Comment: Cost cutting is critical, but not a panacea

Daouda Sembene

The COVID-19 pandemic led many countries to reassert their objectives in terms of universal healthcare coverage, particularly in the developing world. At the same time, the crisis contributed to reversing progress that had been made in recent years toward other targets set forth by the international community, notably the critical need for every child to benefit from high-quality education through the end of secondary school by 2030 (Sustainable Development Goal 4). According to the United Nations, over a billion children and youth were left out of school at the onset of the pandemic. This strengthens even more the case for investing in education that has already been convincingly made. Not only is access to education a fundamental right for every child, but it also has positive ramifications in several priority areas in developing countries, notably in terms of social welfare and economic development as well as peace and stability.

But a key precondition for making progress toward SDG 4 is to make realistic and accurate cost estimates available to policymakers, as the choice of effective policy options primarily depends on it. Jack Rossiter must be credited for his timely attempt to fulfill this imperative and explore ways to move forward. His chapter provides a welcome review of approaches to "cost the SDGs," while illustrating how standard cost estimation techniques tend to produce astronomical and unfillable financing gaps potentially facing low- and middle-income country governments in their efforts to ensure high-quality primary and secondary education in 2030. Alternatively, the chapter proposes an innovative approach that aims to produce more realistic estimates of the unit costs of primary and secondary education, based on the use of country-by-country budget projections.

This work paves the way for the next steps that need to be carried out going forward to make inroads toward universal education. Based on realistic cost estimates, developing country governments must step up efforts to boost the share of domestic revenue allocated and the contribution of the private sector to education. In parallel, the international community must provide continued support to help meet funding gaps, notably by fulfilling aid commitments in the education sector and developing innovative external financing mechanisms. In this connection, due consideration should be given to existing proposals such as taxes on carbon and financial transactions and debt relief. In my view, there is also merit in considering education reform among the potential goals of rechanneling IMF special drawing rights through multilateral development banks.

That said, it is worth noting that a cost-cutting approach is not a panacea though it is critical. There is ample scope for exploring more effective ways to overcome the considerable spending inefficiencies that are typically prevalent in the education sector of many developing countries. While lower unit costs are key to reducing potential financing gaps, eliminating wasteful spending in this sector will be critically needed to further enhance the prospects for meeting SDG 4 sooner than later. This will require inter alia taking forceful steps to address costly behaviors by key education actors, including corrupt practices, rent seeking, and politically motivated maneuvering.

Beyond the focus on costing and financing, a multidimensional effort by the international community will ultimately be needed to fulfill every child's right to be provided with primary and secondary education. The Education Commission rightly called for education transformations, not only in finance but also in performance, innovation, and inclusion. In this process, strengthening accountability would remain key. In addition to making progress toward universal primary and secondary education, further achievements will be needed to achieve SDG 4. Those include improving access to quality early childhood development and affordable and quality technical, vocational, and tertiary education, eliminating gender disparities in education, and ensuring equal access to all levels of education and vocational training for the vulnerable.

Finally, there is a need to be mindful of the potential limitations inherent to the proposed methodological approach. For instance, the "high-spending growth" scenario may raise concerns over potential selection bias, and its exclusive focus on primary schooling outcomes leaves questions about its applicability to secondary and tertiary education unanswered. In addition, while the paper estimates that low- and middle-income countries might spend US$1.9 trillion on education in 2030—which is at least $800 billion lower than available costing estimates—comparing costing figures remains a challenging exercise, given methodological differences across studies. For instance, the paper focuses on primary and secondary, while some other modeling approaches referenced in the paper estimate total education spending.

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