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How to Improve Education Outcomes Most Efficiently?

A Comparison of 150 Interventions Using the New Learning-Adjusted Years of Schooling Metric

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Abstract

Many low- and middle-income countries lag far behind high-income countries in educational access and student learning. Limited resources mean that policymakers must make tough choices about which investments to make to improve education. Although hundreds of education interventions have been rigorously evaluated, making comparisons between the results is challenging. Some studies report changes in years of schooling; others report changes in learning. Standard deviations, the metric typically used to report learning gains, measure gains relative to a local distribution of test scores. This metric makes it hard to judge if the gain is worth the cost in absolute terms. This paper proposes using learning-adjusted years of schooling (LAYS)—which combines access and quality and compares gains to an absolute, cross-country standard—as a new metric for reporting gains from education interventions. The paper applies LAYS to compare the effectiveness (and cost-effectiveness, where cost is available) of interventions from 150 impact evaluations across 46 countries. The results show that some of the most cost-effective programs deliver the equivalent of three additional years of high-quality schooling (that is, schooling at quality comparable to the highest-performing education systems) for just $100 per child—compared with zero years for other classes of interventions.

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1 Introduction

The average child in a low-income country is expected to attend 5.6 fewer years of school than a child in a high-income country (World Bank, 2020).[1] By the age of 10, 90 percent of children in low-income countries still cannot read with comprehension, compared with only 9 percent in high-income countries (Azevedo et al., 2021). With limited resources, policymakers must make tough choices about what to invest in to improve education outcomes—from constructing schools to coaching teachers, from improving school management to deploying new educational software. Making these investment decisions requires comparable data on both the benefits and costs—i.e., the cost-effectiveness—of alternative approaches.

However, the impacts of educational interventions are often reported in ways that make these comparisons difficult. For example, policymakers must choose between interventions that increase the number of years a child stays in school and investments that deliver increased learning during those years, without a good way of comparing progress against these alternative outcomes. But policymakers want a combination of the two. Politicians like Boris Johnson and advocates like Malala have called for an increase in the number of "years of quality education," a single concept that incorporates both quality and quantity dimensions (Crawfurd, Evans, Hares, & Moscoviz, 2020; McKeever, 2020). There is evidence that some of the benefits of education, including economic growth, are more closely associated with learning (Hanushek & Woessmann, 2012), whereas others are associated with years of schooling (Baird, McIntosh, & Özler, 2011; De Neve, Fink, Subramanian, Moyo, & Bor, 2015; Duflo, Dupas, & Kremer, 2021). These two dimensions of education cannot be considered entirely separately. Improving the quality of education has more impact if more children go to school for longer, and programs that increase years of schooling lead to more learning if the underlying education system is of a higher quality.

In this paper, we analyze over 150 educational policies and interventions across 46 countries and identify the most efficient policies and interventions to improve education outcomes. We use a recently developed, unified measure—learning-adjusted years of schooling (LAYS)—that combines improvements in access and quality. By doing so, we make it possible to compare the effectiveness of a broad range of education interventions using a concrete and policy-salient metric.[2,3] For a subset of interventions for which cost data are available, we include cost-effectiveness analysis and comparisons, a critical component to assess trade-offs of the most efficient policy and program to invest in.

[1] We calculate this based on a measure of expected years of schooling using source data from the UNESCO Institute for Statistics (UIS) compiled for the World Bank Human Capital Index 2020.

[2] In previous work, the concept of LAYS has been applied to characterizing the differences in quantity and quality of education across countries (Filmer, Rogers, Angrist, & Sabarwal, 2020), which we refer to in this study as "macro-LAYS" to distinguish them from intervention- and policy-level effects expressed in terms of LAYS, which we refer to as "micro-LAYS."

[3] Since we adjust any increase in years of schooling for quality, one implication is that in the combined measure, improved access is valued in large part to the extent that schooling leads to at least some improvements in learning. This reflects an explicit value judgement that a central goal of schooling is to produce learning, and that other benefits of schooling (such as enhanced socio-emotional skills and reduced fertility) are not the only ones to take into account.

We find that while many interventions are not cost-effective, some of the most cost-effective interventions can deliver the equivalent of three years of high-quality education (i.e., three years of learning in a high-performing country such as Singapore) for as little as \$100 per child. This finding suggests that despite the huge challenges children and schools face in low- and middle-income countries, from poor health and nutrition of children to weakly performing teachers, the right investments can deliver huge returns, even against the benchmark of the best-performing systems. The three most cost-effective approaches are: targeted information campaigns on benefits, costs, and quality; interventions to target teaching instruction by learning level rather than grade (such as "Teaching at the Right Level" interventions and tracking interventions); and improved pedagogy in the form of structured lesson plans with linked student materials, teacher professional development, and monitoring (which includes multi-faceted interventions such as Tusome in Kenya). In India, targeted instruction yields up to 4 additional learning-adjusted years of schooling per \$100—a gain equivalent to the entire system-level education gap between India and Argentina.[4] Other interventions like providing school inputs alone (that is, without necessary complementary changes) perform poorly because they tend not to boost access or learning substantively. Shifting the marginal dollar of government expenditure from low-efficiency to high-efficiency educational investments could therefore yield very substantial benefits per dollar spent.

Another striking result from our analysis is that many interventions that increase participation in schooling are often less cost-effective than interventions that improve the *productivity* of schooling—that is, the amount of actual learning gained while in school. For example, prior reviews have shown that cash transfers can increase schooling. However, those results have not been compared to those of interventions that improve learning directly. We find that cash transfers are not a cost-effective tool to improve LAYS; while they have yielded gains in schooling in systems with low-quality education, they have done so without improving learning across the studies in our sample, all at a relatively high cost. By contrast, some policies that improve the productivity of each year of schooling, such as targeting instruction to a child's learning level or structured lesson plans, can yield on average of around 3 additional LAYS per \$100. This does not imply that cash transfers are not a useful tool to improve social welfare in general; indeed, research has shown they can be highly effective in achieving their primary aim of reducing poverty and increasing consumption. Rather, these results suggest that if the goal of governments is to achieve high-quality education, they should invest in policies that improve the productivity of schooling, instead of solely providing additional schooling.

Any attempt to compare education gains across studies requires a number of assumptions. Our results are robust to a series of tests and alternative choices in the construction of our measure, including alternative specifications of what constitutes high-quality learning, different scaling of test scores, tests for different distributions of performance within samples across countries, and

[4] This calibration does not imply that this intervention would necessarily close the gap between country-level education systems, since many interventions are less effective at scale and political economy factors may impede effectiveness at the system-level. Rather, this comparison is meant to illustrate and calibrate the magnitude of effects.

different assumptions about learning in the absence of an intervention or policy change.

This work contributes to three major literatures. The first concerns the use of summary measures to inform policy analysis. Such measures have become foundational in public health, macroeconomics, and welfare analysis. In public health, such measures include Quality-Adjusted Life Years (QALY) and Disability-Adjusted Life Years (DALY), which were first introduced in the 1970s and early 1980s (Pliskin, Shepard, & Weinstein, 1980; Torrance, Thomas, & Sackett, 1972; Zeckhauser & Shepard, 1976). While DALYs rely on many assumptions, today they are used widely as the reference standard in cost-effectiveness analysis (Drummond, Sculpher, Claxton, Stoddart, & Torrance, 2015; Murray & Lopez, 1996). In economics, summary measures such as the Multi-dimensional Poverty Index (MPI) (Alkire & Foster, 2011) have enabled researchers to understand poverty as a function of multiple measures, rather than focusing exclusively on income. Our work introduces a summary measure for impact measurement in education.

By setting out the benefits of using LAYS, we hope to encourage more researchers to express their results in common metrics to facilitate comparative analysis. Moreover, by providing a unifying framework with transparent assumptions, we hope to encourage researchers to make greater use of standardized learning assessments, which will in turn facilitate more meaningful comparisons across studies. To this end, introducing a common framework – even with imperfect data in the first instance – can make the best use of available data as well as set into motion a cycle of ever more comparable data and comparisons in education over time. This evolution mimics the progression of DALYs and QALYs in the health sector, which started with a framework, assumptions, and a first analysis; over time, the data inputs improved, enhancing the comparability of each underlying study as well as facilitating cross-study comparisons.

We also contribute to the literature synthesizing results from rigorous impact evaluations in education. Previous reviews examining the impact of educational interventions in low- and middle-income countries include Glewwe, Hanushek, Humpage, and Ravina (2011), Kremer, Brannen, and Glennerster (2013), Krishnaratne, White, and Carpenter (2013), Evans and Popova (2016b), Glewwe and Muralidharan (2016) Ganimian and Murnane (2016), and Snilstveit et al. (2015). Our study updates the literature with recent evaluations and provides cost-effectiveness analysis for more studies than previous work has covered. Many of these reviews are now over a decade old. In the years since these reviews, there have been hundreds of additional impact evaluations in education, necessitating an updated review of the literature.

Moreover, gains in prior reviews are often reported in standard deviations rather than against an absolute benchmark. In countries with different levels of inequality in learning, the same absolute increase in average learning on the same test would generate very different standard deviation improvements. When we compare studies using standard deviations as our metric, we impose the assumption that the difference in learning levels between the median and 66th-percentile student in a fourth-grade math class in Kenya is equivalent to the difference in learning levels between the median and 66th-percentile student in a twelfth-grade history class in Peru. A better and more transparent approach to comparing learning gains is to measure them against how long the

average student in a high-performing education system would take to make this learning gain (at the appropriate age). This yields a plausible cardinal measure for comparing different types of learning gains: a gain that would take a student in a high-quality system twice as long to achieve is one with twice the educational value.

A concrete example demonstrates the value of LAYS over standard deviation gains—the current standard unit used for comparative analysis in education. We find that deworming in Kenya yields .113 years of schooling, but only .018 standard deviation gains, while conditional cash transfers in Mexico yield .09 years of schooling, and 0.143 standard deviation gains. Thus, while both interventions have similar effects on schooling, standard deviations make it appear as if CCTs are five times more effective as an artifact of local variation. While expressing results in terms of standard deviations is typical, this approach can be potentially misleading.

We find that expressing outcomes in terms of LAYS yields substantive insight and new understanding relative to standard deviations. In the same example, deworming in Kenya yields .054 LAYS relative to .036 LAYS for CCTs in Mexico. This preserves the original ranking of years of schooling gains and reflects education gains in clear, transparent, and absolute terms. Moreover, we see the added value of capturing the quality of education using LAYS. CCTs in Malawi yield substantially more years of schooling (.270) than deworming in Kenya (.113). However, these estimates do not incorporate differences between the quality of the education in the two countries. This quality dimension is critical since in Kenya, each year of schooling produces more learning than in Malawi, where the quality of education is far worse. When we account for quality, we see deworming interventions yield around .054 LAYS, while CCTs in Malawi yield around .049 LAYS. Using LAYS, we see that deworming interventions enhance education outcomes similarly or slightly more than CCTs (and at substantially lower cost). Had we used years of schooling or standard deviations, our understanding of which education interventions are more effective would be flipped. By adjusting for quality and reducing the influence of local variation, using LAYS allows us to say something about how effective an education intervention is using an absolute, cross-country standard and with a unified education measure.

In addition, current metrics used in the literature make it hard to judge whether the results of a program are worth the cost. If $100 buys an additional 6 months of schooling for a child, is that a good buy if the quality of schooling is bad? Is $100 for an increase in test scores of 0.05 standard deviation a good investment? The answers depend on the underlying quality of the additional schooling in the first case and on the underlying heterogeneity in learning outcomes in the second. To this end, our analysis takes this literature a step further by using a metric (LAYS) that enables unified comparisons of studies across access and learning in education, increases comparability of results across studies, and provides clear interpretation of the results in concrete policy terms.

Finally, we relate to a literature attempting to inform government intervention through the use of cost-effectiveness and cost-benefit analysis across a broad range of potential government interventions. Much of this literature conducts cost-effectiveness analyses, but in different ways. For example, higher education analyses typically report the cost per enrollment (Dynarski, 2000; Kane,

2004), and early childhood education studies often report a social benefit-cost ratio (Heckman, Moon, Pinto, Savelyev, & Yavitz, 2010). Hendren and Sprung-Keyser (2020) propose a unified analysis using a new measure of Marginal Value of Public Funds (MVPF) and compare benefit and cost information (expressed in monetary terms) to prioritize among 133 social policies in the United States. Their analysis reveals that direct investment by governments in low-income children's health and education in the United States has historically had the highest return on investment, with many such policies paying for themselves. Our study similarly demonstrates that there are investments in education interventions in low- and middle-income countries that can deliver large gains at relatively low cost, even when compared against a benchmark of education gains made by children in high-income countries. Of note, many of the studies we review examine individual policies; however, some of the most effective policies we review combine interventions, consistent with recent evidence suggesting coupling interventions has complementarities (Mbiti et al., 2019).

This work, like other syntheses and summary measures, has limitations. First, outputs of this type are only as good as the inputs, and in this case data are still limited—especially on costs. Many education interventions have yet to be evaluated rigorously. As data inputs improve and the range of evaluated interventions expands over time, the outputs of comparative analysis will also improve. Second, in many cases, studies report learning outcomes only in standard deviations. We therefore have to use assumptions about the distribution of learning levels in the study area to translate the study findings into LAYS. However, if future studies use common, standardized tests, future comparisons will allow relaxation of that assumption. Third, because both impacts and costs are measured with imprecision (Evans & Popova, 2016a), it would be unwise to focus on small differences in cost-effectiveness. Rather, this analysis aims to inform broad trade-offs at the aggregate level in cases where there are large, consistent differences. For example, we consistently see that as a cost-effective tool for improving LAYS, cash transfers rank lower than investments in early childhood development. This pattern is robust to method, data inputs, and study or country contexts. Fourth, while access to school and learning proxied by test score performance capture important components of education, they do not capture all aspects of education (like socioemotional learning). However, the combination of these measures represents an improvement over the status quo where typically only one measure is used. Fifth, context matters. Even for the most cost-effective interventions, policymakers will have to consider whether contextual conditions support local adaptation of an intervention (Bates & Glennerster, 2017).

The rest of paper proceeds as follows. Section 2 provides a framework for the learning-adjusted years of schooling. Section 3 describes the set of studies and data included in the analysis of education policies and interventions. Section 4 presents the results in terms of both effectiveness and cost-effectiveness. Section 5 provides a series of robustness tests, and Section 6 concludes.

2 Learning-Adjusted Years of Schooling Framework

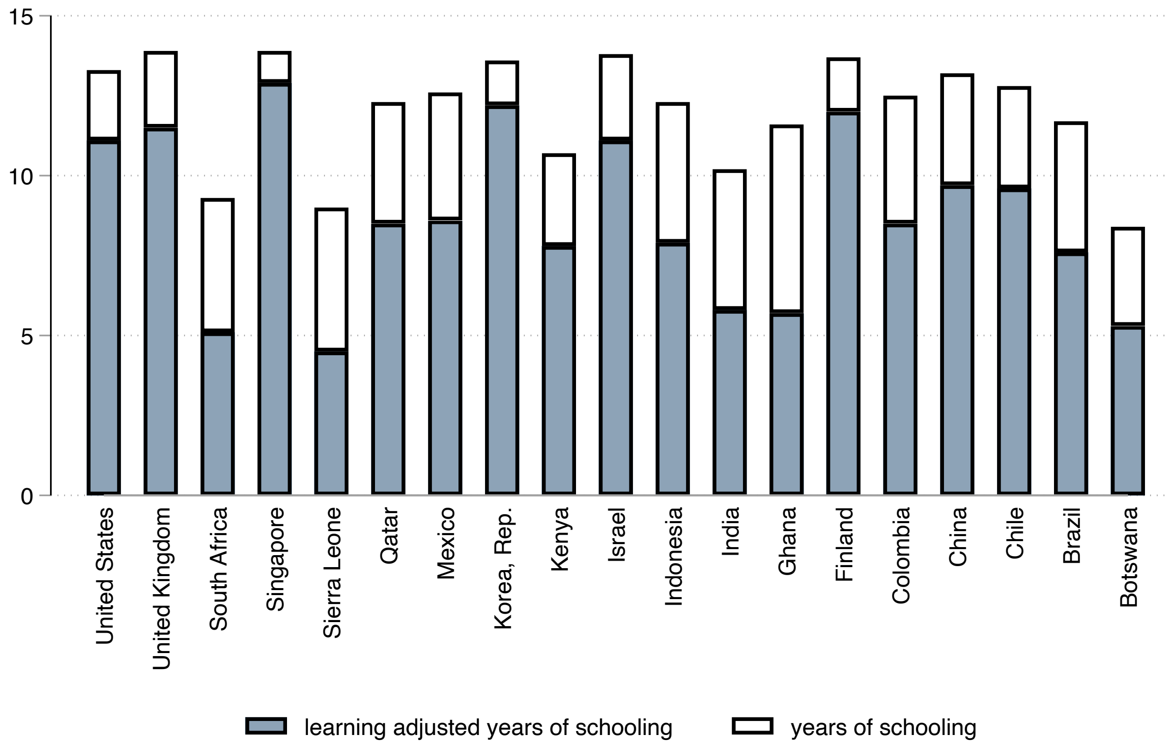
Learning-adjusted years of schooling for a given country—what we call macro-LAYS—are the product of years of schooling and a measure of schooling quality (Filmer et al., 2020). Specifically, they are produced by scaling the country's average schooling by its test-score performance relative to a global high-performance benchmark.[5] Figure 1 shows an example using data from the World Bank Human Capital Index. For example, Singapore's average student test scores are closer to the high-performance benchmark than any other country's scores. As a result, its 14 average years of schooling are discounted only slightly, to 13 LAYS. By contrast, South Africa has 10 years of schooling but only about 5 LAYS, because its test scores are only about half of Singapore's. In other words, macro-LAYS are produced at the country level by adjusting average schooling in a given country by the amount of learning in that country (relative to a high-performance benchmark). Expressing national education levels in terms of macro-LAYS provides a unified and user-friendly measure for a variety of education outcomes.

In this section, we show how LAYS can also be used at the micro level to compare education interventions. The number of rigorous studies evaluating the effect of interventions on educational outcomes is growing, with nearly 300 impact evaluations focused on learning outcomes in low- and middle-income countries as of 2016 (World Bank, 2018). An improved and more comparable metric would enable better evidence synthesis and clearer policy recommendations. As described below, we aim to address many of the challenges that limit current comparisons—most notably, that access and learning impacts are often discussed separately, and that learning gains can be expressed only relative to local performance. We do this by expressing education outcomes from interventions and policies in terms of LAYS units that offer a single, internationally comparable, and policy-salient metric. Hereafter, we will refer to LAYS gained from an intervention or policy as micro-LAYS.

If impact evaluation studies tested students, and reported results, against internationally agreed test scores such as the Trends in International Mathematics and Science Study (TIMMS), the Programme for International Student Assessment (PISA) or the Early Grade Reading Assessment (EGRA), the translation into LAYS would be straightforward. This is what we would hope to see in future studies. However, this is currently not the norm, and therefore a number of assumptions are needed to translate existing studies into LAYS.[6] To ensure a coherent unifying approach, the micro-LAYS methodology invokes assumptions similar to those used in constructing country-level

[5]The high-performance benchmark used in the World Bank Human Capital Index is an artificial benchmark of high performance of 625 as defined by the international assessment Trends in Mathematics and Science Study (TIMSS), which was chosen because that benchmark is stable over time and is apolitical (Kraay, 2019). Other high-performance benchmarks can also be used to construct LAYS estimates. For example, we can use the top-performing country. If Singapore is the highest-performing country in a given year, we can express every country's LAYS in Singapore-equivalent years. That is, we could say that a student in South Africa achieves 10 years of schooling, but 5 years of Singapore-quality schooling.

[6]Comparing education gains across age groups and learning levels is methodologically challenging. The learning jump from single-digit subtraction to long division is inherently different from the jump between recognizing letters to being able to read a sentence. But if we conclude these are fundamentally different concepts that cannot be compared, we forfeit the ability to make comparisons across impact evaluations or advise policymakers on the most cost-effective approaches to improving education.



Notes: Schooling data is based on UNESCO expected years of schooling and learning data is based on Harmonized Learning Outcomes (HLO).

Source: The Human Capital Index is described in Kraay (2019) and is based on Angrist, Djankov, Goldberg, and Patrinos (2021) learning data and UIS enrollment data.

Figure 1: Years of Schooling and Learning-Adjusted Years of Schooling (Macro-LAYS)

macro-LAYS estimates. In this section, we outline the approach to producing micro-LAYS for evaluations that report effects on schooling participation, such as attendance or years of school gained, and subsequently for evaluations that report effects on learning outcomes.

2.1 Micro-LAYS using schooling participation estimates

When studies report effects on schooling participation, micro-LAYS are the product of: (1) the access gains resulting from the intervention and (2) the schooling quality in the country where the intervention took place, measured relative to a global benchmark of high performance. We then multiply these gains by the duration over which the effects of the intervention are expected to persist. The construction of micro-LAYS derived from impacts on schooling participation, denoted

by superscript p, can be expressed as follows:

$$\text{LAYS}^p = \gamma_i * L_i^h * t$$

where L_i^h is a measure of learning for a cohort of students in country i relative to a high-performing benchmark h, such that $L_i^h = \frac{L_i}{L_h}$. Because interventions differ in the duration of their impacts, we include a multiplicative factor t that represents how long the intervention effects γ are expected to persist. In our analysis, we explore various options for the time over which the intervention is expected to be effective. These include per single year ($t = 1$); the length of the evaluation (g); and the remaining school life expectancy (s).

Consider a case where schools are built in a remote area of Afghanistan, and we observe that the intervention delivers on average an additional year of globally benchmarked high-quality schooling per child over the course of an evaluation. If we assume that students will stay in school once the school is built and that the quality of schooling remains constant, we can then adjust this estimate by the remaining school life expectancy (i.e., the number of grades in a given school system minus the grade at which the students entered the school), because we expect students to continue to benefit even after the evaluation period. If students entered in grade three and there are seven grades in primary school, then we would simply multiply the additional year of high-quality schooling by four. Thus, the intervention produced four years of high-quality schooling. In our main analysis we restrict parameter t to the observed gains over the length of the evaluation ($t = g$).

2.2 Micro-LAYS using learning estimates

When studies report effects on learning gains, we first express the learning gains from the intervention in terms of a quantity measure, the equivalent years of schooling gained in a given country with "business as usual" learning. For example, if students learned 0.25 standard deviation per year as a result of an intervention in South Africa, and if students typically learn 0.25 standard deviation per year in a given year in South Africa, then students will have learned a year's worth of South African schooling as a result of the intervention. Second, we then apply a global quality-adjustment factor to derive the corresponding LAYS. For example, if South African students learn half as much as the high-quality benchmark on an international test, we adjust the one year's worth of South African schooling to reflect that it is worth half a year of globally benchmarked high-quality schooling. In the third and final step, we introduce a multiplicative factor for the period of time over which effects are expected to last. As an example, if students had fallen behind grade level and an intervention enables students to catch up to grade level, they might now benefit from day-to-day schooling for the remaining school life expectancy.

Formally, we first express the intervention's learning impacts in terms of equivalent years of school gained. Drawing on the methodology used by Evans and Yuan (2019), we derive equivalent

years of schooling, e, by expressing learning gained relative to learning in the status quo:

$$e = \frac{\beta_i^{\sigma,\text{test}}}{\delta_i^{\sigma,\text{test}}}$$

where β is the learning gain produced by the intervention expressed in standard deviations (σ) per year in country i; *test* denotes the test used to measure learning; δ is the status-quo learning rate in standard deviations (σ) per year in country i; and x denotes the population for which this status-quo learning rate is calculated. This population x could represent the control group of the same study from which the β estimates are drawn; alternatively, x could be the student population in country i, in which case δ becomes the average learning trajectory for the country as a whole. The choice of x will affect our interpretation. If we choose the control group, then the resulting value for equivalent years of schooling gained is relevant to the study sample only. If we choose national-average learning trajectories, we can interpret the value as the equivalent years of schooling gained at the national level. In this paper's main calculations, we use national-level learning trajectories n, and in the robustness section, we explore the trade-offs involved in using a different measure of status quo learning. We estimate micro-LAYS derived from impacts on learning, denoted by superscript l. To derive these estimates, we adjust equivalent years of schooling, e, gained in country i by the quality of learning L_i^h in that country relative to learning in a high-performance global benchmark country h:

$$\text{LAYS}^l = \overbrace{\frac{\beta_i^{\sigma,\text{test}}}{\delta_i^{\sigma,\text{test}}}}^{\text{equivalent years of school}} \quad * \quad \overbrace{L_i^h}^{\text{learning adjustment}} \quad * \; t$$

We substitute in terms for $L_i^h = \frac{L_i}{L_h}$. This is analogous to the quality adjustment used in macro-LAYS. We further specify that both the numerator and denominator of the learning-adjustment term are expressed in terms of standard deviations (σ) on a test that is representative at national level n for each country, such that:

$$\text{LAYS}^l = \overbrace{\frac{\beta_i^{\sigma,\text{test}}}{\delta_i^{\sigma,\text{test}}}}^{\text{equivalent years of school}} \quad * \quad \overbrace{\frac{L_{i,n}^{\sigma,\text{test}}}{L_{h,n}^{\sigma,\text{test}}}}^{\text{learning adjustment}} \quad * \; t$$

For the next step we invoke two assumptions. First, we assume that learning is constant along a local trajectory. This assumption, explored in depth in Filmer et al. (2020) for macro-LAYS, enables conversion of relative levels $L_i^h = \frac{L_i}{L_h}$ into relative rates $L_i^h = \frac{\delta_i}{\delta_h}$, since the relationship is constant. (This assumption is discussed in more detail in Section 5.) Second, we assume that standard deviations across tests and samples are comparable. This assumption is strong, but not novel: it is implicitly invoked whenever any time standard-deviation effect sizes are compared across studies, which is the dominant practice in the literature on education interventions. We note that this assumption is most robust when learning gains in a given study are based on similar tests to

the ones used in computing the learning-adjustment factor. We further explore robustness to this assumption in Section 5. These assumptions simplify our conversion to:

$$\text{LAYS}^l = \overbrace{\frac{\beta_i}{\delta_{i,n}}}^{\text{equivalent years of school}} * \overbrace{\frac{\delta_{i,n}}{\delta_{h,n}}}^{\text{learning adjustment}} * \ t$$

The $\delta_{i,n}$ terms cancel, and we are left with the expression:

$$\text{LAYS}^l = \frac{\beta_i}{\delta_{h,n}} * \ t$$

This expression produces an intuitive metric: the years of h-quality learning from the intervention. For example, assume that an intervention in South Africa yields 0.25σ per year of learning ($\beta_{\text{South Africa}} = 0.25$), and that in Singapore, a high-performance benchmark on international learning assessments, students learn 1σ over the course of a given year ($\delta_{\text{Singapore}} = 1$). Then we have 0.25 LAYS^l; in other words, the intervention enabled South African students to gain a quarter of a year's worth of Singaporean-quality schooling.

Finally, we apply a multiplicative factor t for the length of time over which the intervention is expected to have lasting effects. Since micro-LAYS is based on participation estimates, t can take on a few values: a single year, such that $t = 1$; the length of the evaluation, g; and the remaining school life expectancy s. As an example, if students had fallen behind grade level and an intervention enables students to catch up to grade level, they might now benefit from day-to-day schooling for the remaining school life expectancy, s.[7] We find that micro-LAYS are robust to a range of sensitivity and robustness tests, outlined in Section 5.

2.3 Putting micro-LAYS estimates together

In summary, both participation- and learning-based LAYS tell us that a given intervention in a country produces a certain number of years' worth of globally benchmarked high-quality learning. Thanks to this common unit, the impacts of studies that measure these two different types of outcomes can be directly compared.

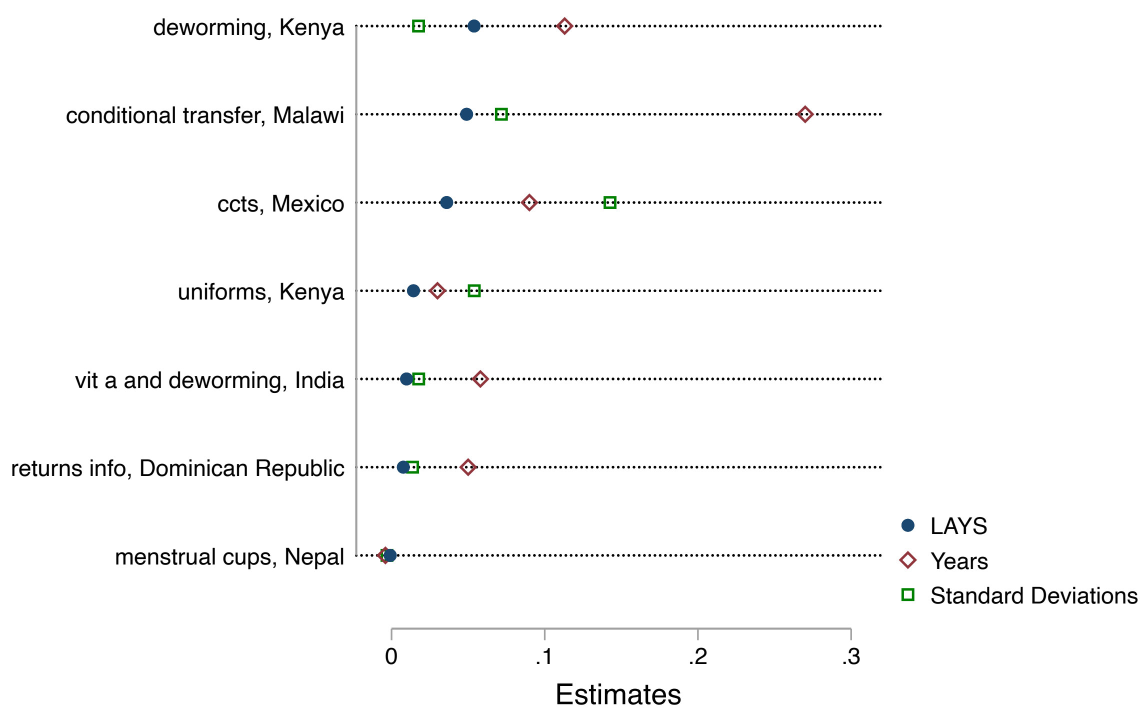
As an illustration of the insights gained by using LAYS, consider the following example. Figure 2 compares LAYS with the raw and standard deviations estimates per year for a subsample of studies that targeted school access and participation. Standardized effect sizes are the most typical measure used in the education literature to benchmark how large effects are relative to local variation. As an example, deworming in Kenya yields .113 years of schooling, but only .018 standard deviation gains, while conditional cash transfers in Mexico yield .09 years of schooling, and .143 standard deviation gains. Thus, while both interventions have similar effects on schooling, standard deviations make it appear as if CCTs are five times more effective as an artifact of local variation. While common,

[7] The value of t, the length of time an intervention's effect is expected to last, might vary by intervention and apply differently to quality improvements versus quantity improvements.

this approach can be potentially misleading.

We find that expressing outcomes in terms of LAYS yields substantive insight and new understanding relative to standard deviations of which interventions most efficiently improve education outcomes. In the same example, deworming in Kenya yields .054 LAYS relative to .036 LAYS for CCTs in Mexico. This preserves the original ranking of years of schooling gains and reflects education gains in clear, transparent, and absolute terms.

Moreover, we see the added value of capturing the quality of education using LAYS. CCTs in Malawi yield substantially more years of schooling (.270) than deworming in Kenya (.113). However, these estimates do not incorporate differences between the quality of the education in the two countries. This quality dimension is critical since in Kenya, each year of schooling produces more learning than in Malawi, where the quality of education is far worse. When we account for quality, we see deworming interventions yield .054 LAYS, while CCTs in Malawi yield .049 LAYS. Using LAYS, we see that deworming interventions enhance education outcomes similarly to or slightly more than CCTs (and at substantially lower cost). Had we used standard deviations, our understanding of which education interventions are more effective would be flipped. By adjusting for quality and reducing the influence of local variation, using LAYS allows us to say something about how effective an education intervention is using an absolute, cross-country standard as well as a unified education measure.



Notes: a subsample of interventions retrieved from J-PAL studies that focus on school access. Estimates are reported per year. Standardized effects are calculated as $\frac{\mu_i}{s.e._i\sqrt{N_i}}$, *where* μ_i *is the raw estimate,* $s.e._i$ *is the standard error, and* N_i *is the number of observations in the raw estimate's regression for each intervention* i.

Figure 2: Comparing LAYS, Standardized Deviations, and Years of Schooling

One challenge in assembling micro-LAYS estimates is how to handle a study that reports impacts on both participation and learning. If we sum the estimates, we will double-count in cases where gains in learning resulted directly from gains in participation or where gains in learning led to gains in participation (e.g., because students had a greater incentive to attend schools that delivered more learning). As an alternative to adding the two estimates, we could choose to use only estimates from either participation or learning. However, under this approach we would be assuming that one is the central output, and that the other outcome dimension is largely captured within that central output. Instead, for the purposes of this paper, we use the LAYS impact that is greater—whether that was obtained through schooling or learning increases—for each evaluation. This approach places *a priori* equal weight on schooling and learning, introduces no new assumptions, and avoids double-counting.

3 Data and Analysis Framework

We compare impact estimates from over 150 evaluations of education interventions in 46 countries using a unified measure. In our comparison, we highlight findings from a subset of studies that have comparable cost data[8] and that therefore allow us to compare cost-effectiveness of interventions. We examine how many LAYS each policy or intervention delivers; how cost-effective those gains are; and how much of the gap between quality-adjusted years of schooling and actual years of schooling that intervention would close if it were scaled up, assuming that the effectiveness of the intervention remained constant. This assumption of scalability is not trivial, given that effectiveness at system scale is often substantially lower than effectiveness in even a large pilot study; we therefore carry out this calculation as a calibration exercise rather than a simulation exercise.

We start with over 300 studies compiled from a database produced by Evans and Yuan (2019), which draws studies from a wide range of reviews (Evans & Yuan, 2019; Ganimian & Murnane, 2016; Glewwe et al., 2011; Kremer et al., 2013; Krishnaratne et al., 2013). We then add 13 studies from the World Bank Strategic Impact Evaluation Fund (SIEF), as well as four additional recent studies that have rigorous evaluation methodologies and high-quality impact and cost data (Eble et al., 2021; Piper, Destefano, Kinyanjui, & Ong'ele, 2018; Romero, Sandefur, & Sandholtz, 2020; Sabates, Rose, Alcott, & Delprato, 2021).[9]

Our inclusion criteria are that the study should be based on a credible causal inference strategy, using either randomized controlled trials or quasi-experimental methods, such as differences-in-differences, instrumental variables, regression discontinuity, fixed effects, or propensity score matching. To aggregate across outcomes, we code outcomes such that positive impacts always represent an improvement; for example, a reduction in absenteeism is coded as an

[8]Given that there are substantial difficulties when comparing cost data across contexts and interventions, we believe the field will benefit from efforts to standardize how these costs are reported, for example by consistently using $PPP.

[9]We also check for robustness of our results with the new data from the 2023 GEEAP report (Akyeampong et al., 2023). These results are analysed in Appendix C.

increase in attendance. In addition, for studies that report impact on learning, we start with impact estimates that can be expressed in terms of standard deviations. The list of studies included in our analysis is illustrative rather than exhaustive, and in the future, we aim to continue adding more studies and build as comprehensive a database of education interventions as possible. In total, after applying our inclusion criteria, we analyze data from over 150 impact estimates across 46 low- and middle-income countries.[10]

In this analysis, we make several choices for parameters and data inputs. First, we assume that the intervention's effects (t) last only for the duration of the evaluation since this is a known quantity and requires no further assumptions. In Appendix Figure B1 we explore the alternative assumption that the impacts last just one year ($t = 1$). A second choice that we make is to set the high-quality benchmark learning rate ($\delta_{h,n}$) equal to 0.8 standard deviations per year. This is a conservative estimate for high rates of learning, drawn from year-on-year learning gains in high-performing education systems, policy-relevant differences across education systems, and standard benchmarks. We choose an artificial benchmark for this analysis, because unlike the actual learning rates of high performers, such as Finland or Singapore, it has the advantage of being stable over time and of being apolitical. This approach to defining high-quality learning rates is similar to the one used to define the high-performance benchmark learning level in the World Bank Human Capital Index, which sets the benchmark at 625 on the scale of TIMSS and PISA. In the robustness section, we explore four plausible approaches to validate this 0.8-standard-deviation high-performance benchmark.

We calculate how much to adjust improvements in access for the level of learning (i.e., the learning adjustment rate L_i^h) using Harmonized Learning Outcomes (HLO), which are global measures of learning introduced by Angrist et al. (2021) and used in the World Bank Human Capital Index. The paper generates comparable learning measures across 164 countries by linking psychometrically-designed international assessments to their regional counterparts to construct globally comparable learning outcomes at a national level.

We choose HLO data over alternative test score data for various reasons. First, these data enable us to use the same learning scale for interventions from 164 countries across the world, a wide range of countries from which we also draw impact evaluation education estimates. Second, these data are used in the World Bank Human Capital Index (HCI), which enables us to produce micro-LAYS that map directly to the macro-LAYS in the HCI. The international tests of student learning that are included in the HLO data are often scaled to a mean of 500 and standard deviation of 100. For micro-LAYS, we also derive a learning scale whose lower limit plausibly represents zero learning. We use data from early grade reading assessments (EGRA), where underlying test items have a plausible zero: no reading comprehension. In Appendix Figure B2 we show that the HLO score that corresponds to a floor of zero reading comprehension is 300. In accordance with this, in our analysis we scale the HLO data with a linear transformation of 300. In Section 5, we further

[10] LAYS is a unit of measurement and therefore any result, including descriptive and observational results, can technically be expressed in terms of LAYS. Thus, this approach has uses beyond comparison of effects of impact evaluations.

explore the sensitivity of results to the score scale.

4 Results

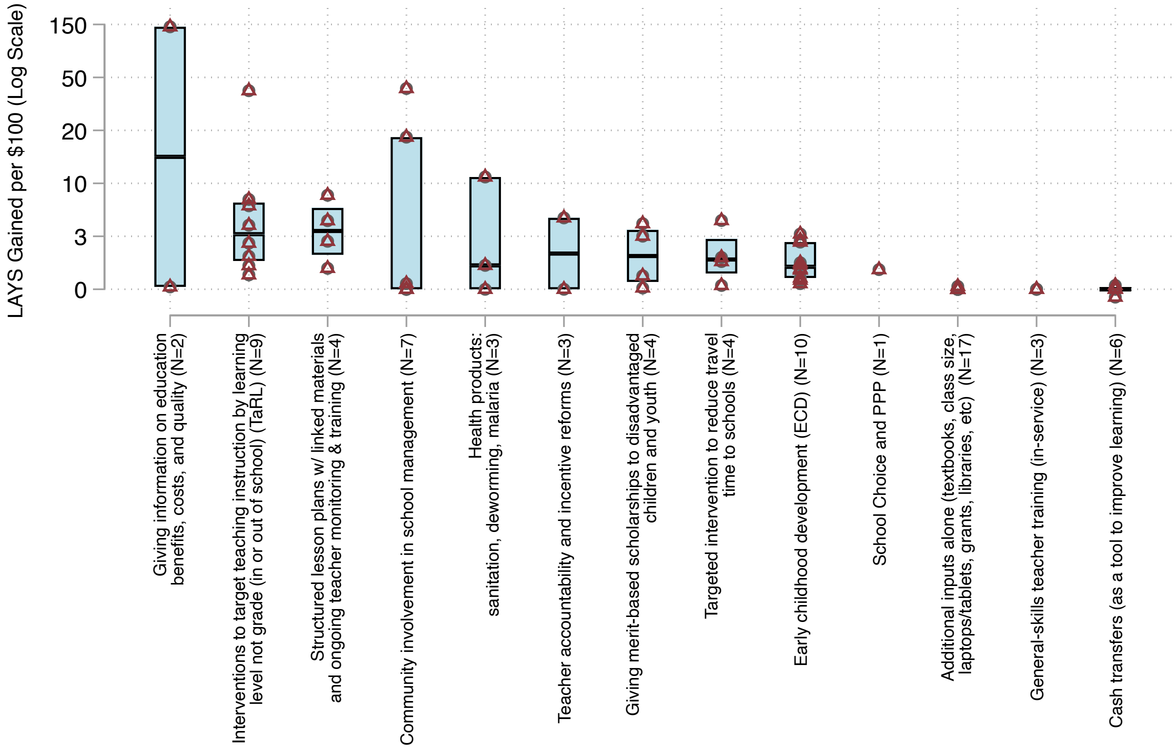
4.1 Aggregate categories of policies and interventions

We are interested in comparing the impacts of aggregate categories of policies and interventions. To this end, we summarize results by category of education intervention, such as Early Childhood Development (ECD) or instruction targeted to the child's level of learning rather than grade level. Intervention categories are based on original study designations, with a few adjustments. These adjustments help classify interventions more precisely based on the primary theory of change underlying them. First, we recategorize technology interventions into either "targeted instruction" or "additional inputs alone" based on whether they involved adaptive software or were largely a hardware-based intervention. Second, we classify interventions for ECD that involved building or opening of schools or classrooms as "targeted intervention to reduce travel time to schools." Third, we define teacher training interventions narrowly. Many interventions include training of teachers; for this analysis, when a program provides materials to help teachers target instruction to the level of the child and also provides training to those teachers, we classify that as a "targeted instruction" intervention. "General-skills teacher training" captures only general teacher training programs without other major elements. Fourth, for interventions with multiple components, we selected the central component and used that as a category. Later in the paper, we examine individual studies and so will characterize them more precisely.

Comparative information on effectiveness will be most useful to policymakers when it incorporates information about cost. Therefore, we start by analyzing cost-effectiveness of policies and interventions with a subset of studies where cost information is available. Figure 3 shows the LAYS gained per $100 per child.[11] To calculate this, we divide the per-student gains by the per-student costs, so the figure shows LAYS gained per student per $100 spent per student. Typical spending in education systems ranges between $208 per student in Sub-Saharan Africa to $7,908 in East Asia in primary school in terms of 2013 PPP USD (Bashir et al. 2018). Therefore, cost-effectiveness per $100 per student is a relevant unit for many systems, along the lines of status-quo spending benchmarks, even at the lower tail of spending. Of course, the share of overall investment that $100 represents will depend substantially on context.

The top performers, ranked by mean effect size, are: targeted information campaigns on benefits, costs and quality; interventions to target teaching instruction by learning level rather than grade (such as "Teaching at the Right Level" interventions and tracking interventions); improved pedagogy in the form of structured lesson plans with linked materials and monitoring (which includes combination interventions such as Tusome in Kenya); community involvement in school

[11] In Appendix C we complement our data with data from the 2023 GEEAP updated report (Akyeampong et al., 2023) and show cost-effectiveness results in Figure C1.



Notes: Each category of education intervention shows the learning-adjusted years of school (LAYS) gained from a given intervention or policy. Each red triangle represents a cost-effectiveness estimate. The boxplot is ordered from largest to smallest mean effects and the shaded boxplot describes the 25th and 75th percentile. The y-axis is reported on a natural log scale.

Figure 3: Learning-Adjusted Years of School (LAYS) Gained per $100 by Category

management (such as training for community members); health products (such as anti-malarial or deworming pills); scholarships for disadvantaged groups; teacher accountability and incentives (such as camera monitoring of teacher attendance or merit based pay); targeted interventions to reduce travel time to school (for example, constructing schools in remote underserved areas); merit-based scholarships provided to disadvantaged children and youth; early childhood development (ECD); and school choice and public-private partnerships (such as voucher schemes). The last three categories—cash transfers, additional inputs alone (such as textbooks, technology hardware, uniforms, school grants, or reducing class size without complementary reforms), and general skills teacher training—have zero effect on LAYS.

Some of these categories have moderate effects in absolute terms, but are extremely cheap, making them very cost-effective; an example is providing information on the returns to schooling. Other interventions are highly effective in absolute terms, but are expensive, and are thus moderately cost-effective; these include school construction to reduce travel times to school as well as scholarship schemes. Moreover, we observe that some categories have low variance—–as

in the case of class-size reductions and additional inputs, which are tightly concentrated around zero——while other categories have high variance. An example of the high-variance group is information campaigns on the costs and benefits of education: some of the impact estimates for this category are around zero, while others are at the highest end of the spectrum. Structured lesson plans produce large gains with relatively low variation, whereas community involvement has a lower average effect but high variation. This indicates that when considering interventions, we should consider not only the average effect but also the variance. This further points to the importance of contextual relevance: some interventions have similar effects across contexts, while others work extremely well in one context but not in others.

Moreover, context is essential to consider across all categories regardless of variation. For example, early childhood development might be most effective in contexts with strong primary education systems where these early investments translate into preparedness for primary school, thus enabling dynamic complementarities (Johnson & Kirabo Jackson, 2019); providing information on the returns to education may be highly cost-effective in one country but ineffective in a context where those returns are well known; and similarly, a deworming program is unlikely to be cost-effective in a place with low levels of intestinal worms.

It is important to consider these results in the context of how governments typically spend their budgets. They make substantial investments in textbooks, technology hardware, uniforms, school grants, class-size reductions, and general-skills teacher training. When not well integrated with other interventions, these categories of interventions consistently produce almost no effect. By contrast, investments such as early childhood development and merit-based scholarships to disadvantaged students can yield gains of up to 3 additional LAYS per $100 per child. To this end, shifting the marginal dollar of government investment from status-quo spending to more efficient educational investment could substantially improve education outcomes. These implications are consistent with the findings of prior cost-effectiveness reviews, such as Kremer et al. (2013).

Beyond reinforcing findings of prior reviews, our unified analysis also reveals new insights. One is that many interventions that increase participation in schooling are less cost-effective than interventions that improve the productivity of schooling—that is, the amount of actual learning gained while in school. For example, prior reviews have shown that cash transfers can increase schooling. However, those results have not been compared to those of interventions that improve learning directly. We find that cash transfers are not a cost-effective tool to improve LAYS; while they yield gains in schooling in systems with low-quality education, they have done so without improving learning across the studies in our sample, all at relatively high cost. By contrast, some policies that improve the productivity of each year of schooling, such as targeting instruction to a child's learning level or structured lesson plans, can yield on average of around 3 additional LAYS per $100. This does not imply that cash transfers are not a useful tool to improve social welfare in general; indeed, research has shown they can be highly effective in achieving their primary aim of reducing poverty and increasing consumption (Fiszbein et al., 2009; Haushofer & Shapiro, 2016). Rather, these results suggest that if the goal of governments is to improve learning, cash transfers

might not be the most efficient tool for this specific purpose. More broadly, our analysis reveals the importance of focusing on policies and interventions that improve the productivity of schooling, rather than solely providing additional schooling.

In Figure 4 we show the full set of 150 studies from 46 countries, highlighting the subset of impact evaluations that include cost-effectiveness data.[12] The first important takeaway from this figure is that, by and large, the subset of interventions with cost-effectiveness data are not systematically biased towards high or low impacts, although within each category the studies with cost-effectiveness data may skew one way or the other. This figure further enables us to assess LAYS gains in absolute terms, rather than per $100, and decompose whether an intervention is cost-effective due to being effective, cheap, or both. For example, health products are moderately effective in improving outcomes, with up to 0.2 LAYS gains per child, but are cheap. Thus, in Figure 3 we see the modest absolute gains translate into up to 10 LAYS gains per $100 per child, marking these health interventions as highly cost-effective. Other interventions are highly effective but expensive, and therefore less cost-effective. Giving merit-based scholarships can yield up to 1 LAYS, but since this policy is relatively expensive, it is less cost-effective than the health programs, delivering 3 LAYS per $100 at the upper end of the distribution of studies. Finally, Figure 4 also includes a new category: nutrition interventions (such as school feeding), which did not enter the cost-effectiveness analysis in Figure 3 due to a lack of cost data. We observe that school feeding is relatively effective in improving LAYS, although with high variance. Taken together with the findings in Figure 3, this indicates that health and nutrition interventions can translate into meaningful education outcomes.

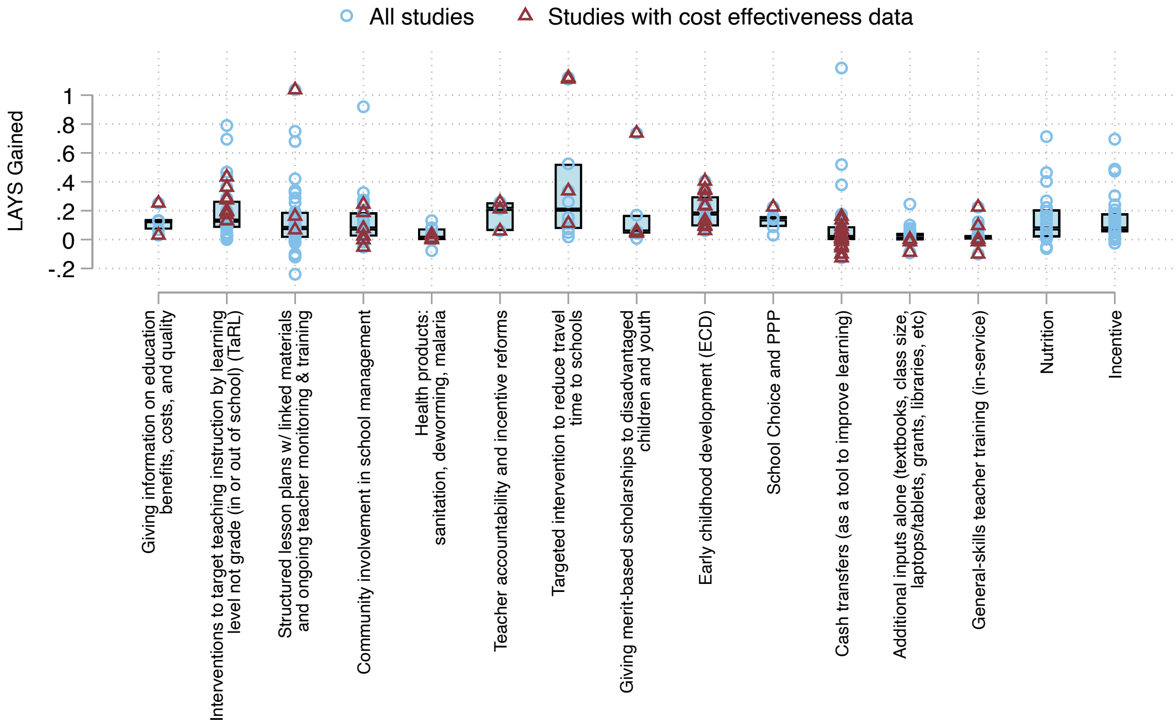
4.2 Specific cost-effectiveness studies

4.2.1 Effectiveness and cost-effectiveness

Next, we examine specific interventions to explore the degree to which aggregate patterns might parallel more granular ones or reveal underlying heterogeneity. Figure 5 shows results for absolute LAYS gained by intervention and country for the studies that include cost-effectiveness data. The top ten performers are: a combined intervention with improved pedagogy, para-teachers and targeted instruction in The Gambia (4 LAYS); the Campaign for Female Education (Camfed) program in Tanzania—a holistic program that includes scholarships and mentorship for girls along with school materials and training for teachers and parents (1.1 LAYS);[13] Tusome (the Kiswahili word for "Let's Read") in Kenya—a program that provides textbooks, teacher coaching, and teacher training (1.04 LAYS); building village-based schools in Afghanistan (0.74 LAYS); computer-assisted learning (CAL) in India (0.43 LAYS); community-based preschools in Mozambique (0.41 LAYS);

[12]In Appendix C we complement our data with data from the 2023 GEEAP report (Akyeampong et al., 2023) and compare impact sizes in Figure C2.

[13]CAMFED does not only provide financial support in its program; the breakdown by type of service provided by CAMFED in the paper does not reflect CAMFED's main offering but rather illustrates how various program offerings might operate together to inform other programs.



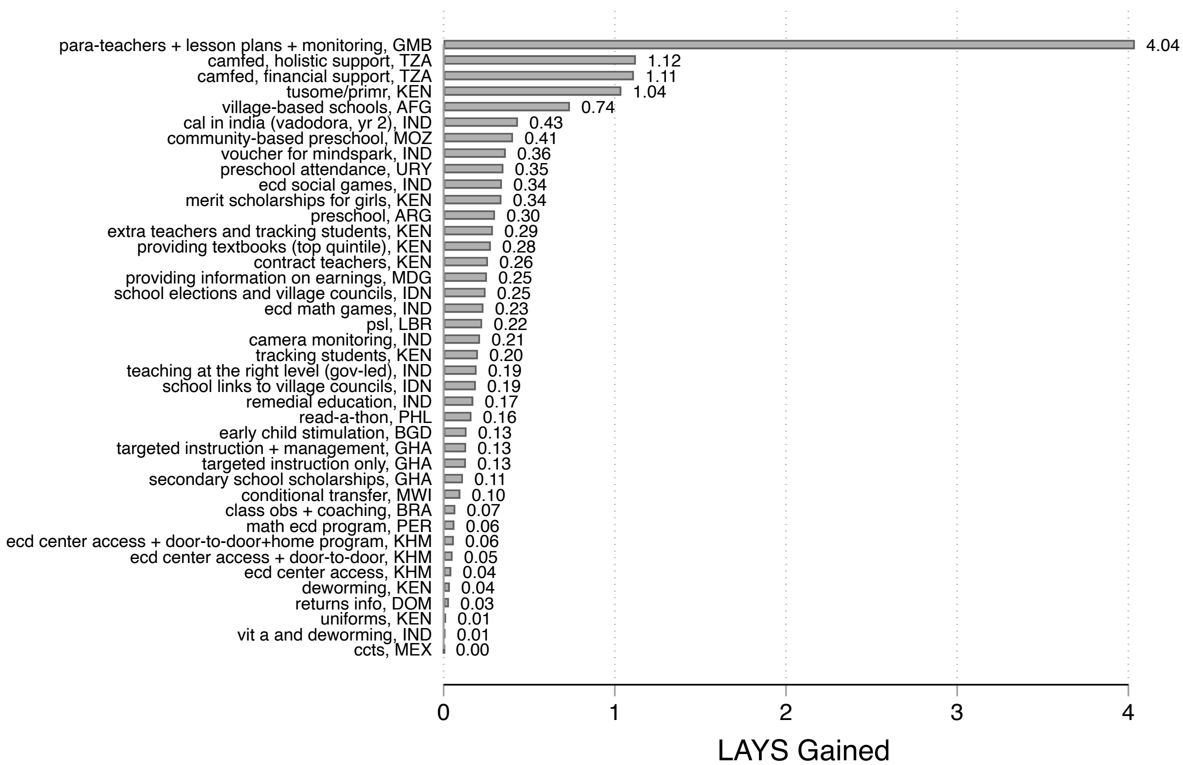
Notes: Each category of education intervention shows the learning-adjusted years of school (LAYS) gained from a given intervention or policy across over 150 impact estimates in 46 countries. The boxplot describes the 25th and 75th percentile. The boxplot is ordered in the same order as Figure 3 to provide a direct analogy, with the exception of the "nutrition" category which has no cost-effectiveness data and does not appear in Figure 3.

Figure 4: Learning-Adjusted Years of School (LAYS) Gained by Intervention Category

vouchers for mind-spark adaptive learning software in India (0.36 LAYS); preschool attendance in Uruguay (0.35 LAYS); merit scholarships for girls in Kenya (0.34 LAYS); and ability grouping using extra teachers in Kenya (0.29 LAYS). By contrast, about half of all interventions produce no significant effects; those interventions are not included in the figure.

These findings point to a few overall lessons. In this sample of studies, the most effective programs are: multidimensional programs (a combined intervention in The Gambia, Camfed in Tanzania, and Tusome in Kenya); programs that are carefully targeted to a local need, such as scholarships (for girls), information (when returns to schooling are not widely known), and school construction in under-serviced remote areas; pedagogical instruction that is pitched to students' levels of learning, not based on a rote syllabus or an over-ambitious curriculum; and programs that facilitate early childhood development.[14]

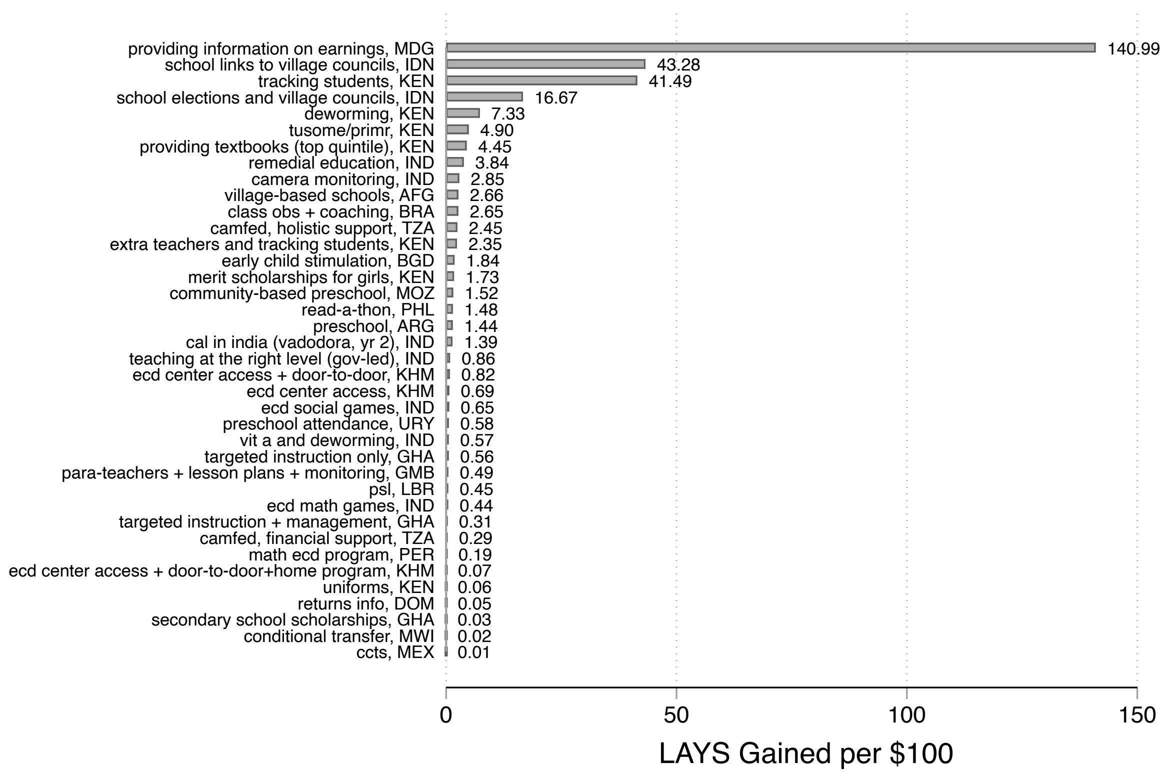
[14] Related approaches to teaching at the right level include reforming the curriculum so that it focuses on foundational skills and aligns better with students' actual pace of learning. Such reforms have been evaluated in Tanzania with promising effectiveness (Rodriguez-Segura & Mbiti, 2022).



Notes: We do not include interventions with null impacts, which by definition are not cost-effective.

Figure 5: Learning-Adjusted Years of Schooling (LAYS) by Intervention

Figure 6 shows cost-effectiveness estimates for these interventions, expressed in LAYS per \$100. When we take cost into account, several new interventions join the list of top performers: provision of information on the returns to schooling in Madagascar; school links to village councils in Indonesia, remedial education in India, camera-based monitoring of teachers with incentives in India, deworming in Kenya, and classroom observation and coaching in Brazil. By contrast, public-private partnerships, scholarship programs, targeted school construction and access, and technology-assisted adaptive instruction drop down the list because of their higher cost. However, these programs are still cost-effective, with LAYS per \$100 gained ranging between 0.24 to 2.66.

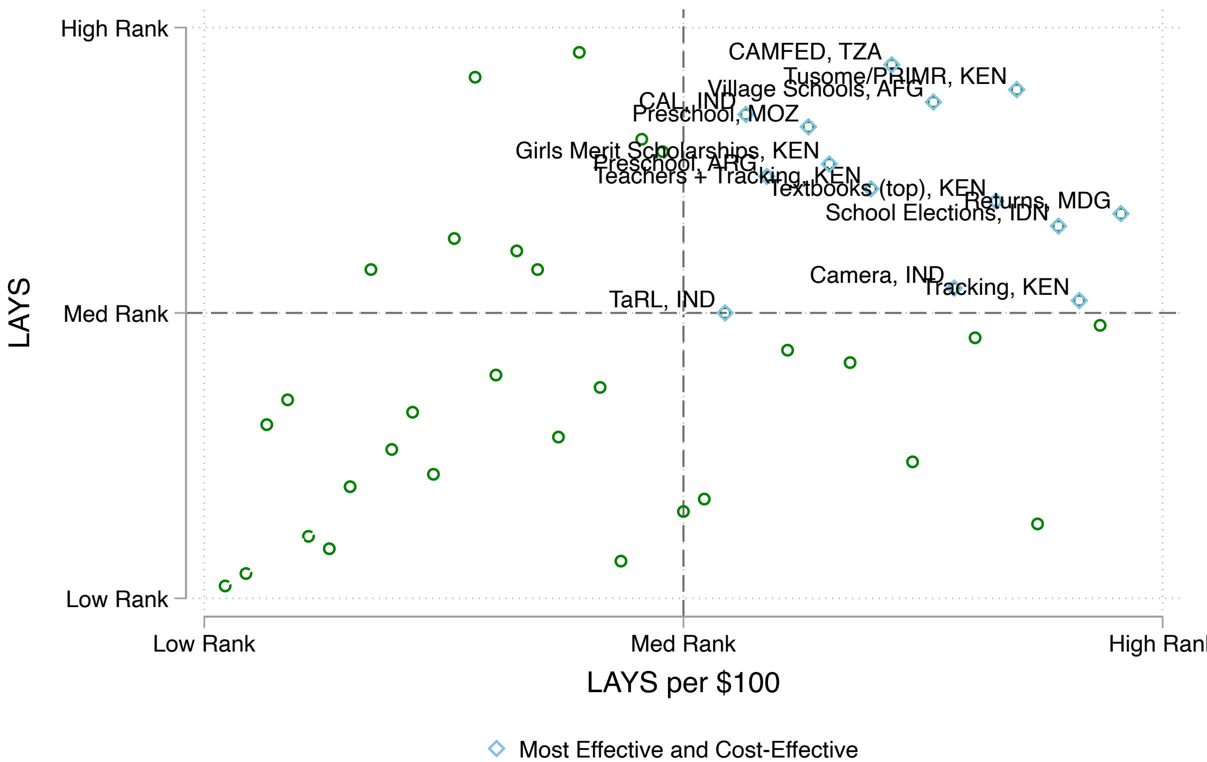


Notes: We omit interventions with a null effect.

Figure 6: Learning-Adjusted Years of Schooling (LAYS) Gained per \$100, by Intervention

There are two broader key takeaways from these figures. First, note that relatively few interventions have any positive impact at all. Indeed, over half of interventions reviewed had null effects and are omitted from these figures. Thus, any intervention identified as effective is already near the 50th percentile. Second, the cost-effectiveness of some interventions is an order of magnitude greater than the median. These highly cost-effective interventions include providing information on the returns to schooling in Madagascar, creating school links to village councils in Indonesia, and grouping students by ability level in Kenya. These interventions stand out for being both effective and extremely cheap.

The upper-right quadrant of Figure 7 highlights the interventions that are both effective and cost-effective. The programs that do well on both measures include: targeted information (on future earnings) and targeted scholarships (for girls); accountability reforms, such as camera monitoring of teachers with incentives, and school elections and community engagement; instruction targeted to student levels through pedagogical interventions, grouping, and technology; school construction in remote areas that otherwise lack school access; structured pedagogy interventions; and early childhood development programs.



Notes: "High rank" means more effective or cost-effective, respectively.

Figure 7: Effective and Cost-Effective Interventions

Overall, this exploration of specific interventions reveals broadly consistent patterns with the aggregate categories in Figures 2 and 3. Rather than delivering precise estimates or identifying specific interventions to invest in, this analysis is most useful for the aggregate patterns that it reinforces, such as the relative efficiency of input-alone interventions versus targeting instruction to children's level. Results for these aggregate categories of policies should help to inform prioritization by governments among specific policies for improving quality education.

4.2.2 Calibrating gains from specific interventions and policies to system-level gaps

What could the macro, systemwide effects of these highly effective interventions be? To answer this, we explore first how many LAYS a given intervention could contribute toward closing the gap to achieve a full and globally benchmarked quality education at scale in a given country—assuming, as mentioned before, that the nationally scaled-up version of the program were as effective as the evaluated version. Of course, this is rarely the case, and this exercise is meant as a calibration rather than as a simulation. An alternative approach would be to apply a "discount rate" to intervention effectiveness as they go to scale. In essence, in this exercise we map micro-LAYS onto macro-LAYS. Figure 8 takes cost-effectiveness into account, making the interventions comparable by showing the gap that each could close at a cost of \$100 per child. It shows that highly cost-effective programs like Tusome and ability grouping could substantially narrow the learning gap separating the children in Kenya from their peers in higher-quality systems. Moreover, policies that improve the productivity of each year of schooling, such as targeting instruction to a child's learning level, can yield up to 4 additional LAYS per \$100 in India. This calibration reveals that shifting the marginal dollar of government expenditure from low-efficiency to high-efficiency educational investments could help countries make the most out of the years of education they offer. While this cost analysis is useful for comparing interventions on a common scale, both the cost and the effectiveness of interventions can change at varying scales of implementation.

5 Robustness

In this section, we present sensitivity analyses of our assumptions and parameter choices. We focus on four main areas: the high-quality learning benchmark, scaling of the learning assessments, standard deviations across tests and samples, and status-quo learning trajectories.

5.1 High-quality learning benchmark

We use 0.8σ as a benchmark for high-performing learning rates. As noted above, this value is an artificial high-performance benchmark chosen because it is stable (unlike benchmarks based on actual performance of leading countries) and non-political. This approach to defining high-quality learning rates is similar to the approach to defining the high-performance benchmark learning level in the World Bank Human Capital Index (Kraay, 2019). We explore three approaches to validating this high-performance benchmark: (a) average annual learning trajectories in high-performance cases; (b) policy-relevant learning changes; and (c) rules of thumb and a range of effect sizes in reviews of multiple studies.

The first approach draws on high-performance learning trajectories. Although there is surprisingly little year-on-year raw data on learning, one notable example where there is longitudinal data is from the Young Lives survey. That survey follows students in India, Vietnam, Peru, and Ethiopia over 15 years and uses learning assessments based on Item-Response Theory (IRT).

Using this data and a combination of value-added estimates, instrumental variables, and regression discontinuity methods, Singh (2020) finds that the causal effect of an additional year of primary school in Vietnam is 0.76σ, the largest value among the four countries. This is likely a lower bound for "high performance" on a global scale, since Vietnam—while an excellent performer for its income class—ranks in the second decile of average Harmonized Learning Outcomes (which, as noted above, covers 164 countries from 2000-2017). We can compare these results to an alternative high-benchmark year-on-year comparison: changes analyzed in the United States by Bloom, Hill, Black, and Lipsey (2008), building on methods used by Kane (2004). The largest year-on-year learning gains are between grade 1 and 2, and range from 0.97σ in reading to 1.03σ in math. Finally, we can derive approximate year-on-year changes for global high performers. We assume that the appropriate high-performance rescaled HLO benchmark is a score of 325 at the primary level. This score is assumed to be obtained over four years, since most primary international assessments occur in grade 4; average high-performance learning per year is thus 81.25 points. We then assume a within-country standard deviation of 85 points, based on the values for the five highest-performing countries using 2006 PISA microdata. Taking the ratio of these two values yields a year-on-year gain of 0.96σ.

The second approach examines large, system-level gains. Here, we explore what would constitute a large learning gain in systemic terms, as a way to benchmark what high-performing learning progress would look like. One example is to consider cross-country learning gaps in terms of HLO scores used for the World Bank Human Capital Index. A gain of 0.8σ would enable the United Kingdom or Vietnam to catch up to Singaporean learning levels: because the cross-country standard deviation is equivalent to 70 HLO points, a 0.8σ gain for the United Kingdom (517) or Vietnam (519) translates into nearly closing the gap with Singapore (581). In another example, consider that the black-white achievement gap in the United States in math ranges from 0.99σ to 1.04σ in grades 4 and 8 (Bloom et al., 2008). A gain large enough to nearly close either of these gaps would be highly meaningful in policy terms.

The final approach uses rules of thumb. Cohen (1988) proposed the following standardized effect-size benchmarks: at least 0.20 for "small" effects, 0.50 as "medium" effects, and 0.80 for "large" effects. This framework has been broadly applied across interventions and contexts for decades. However, there is debate about the relevance of these indicators to education interventions, given that almost all interventions in high-, middle-, and low-income countries have much smaller impacts. For high-income countries, the 90th-percentile effect size is 0.47 (Kraft, 2020); for low- and middle-income countries, it is 0.38 (Evans & Yuan, 2022). Both of those fall below the traditional Cohen benchmark for even medium effects.

In summary, these various approaches—particularly those focused on high-performance learning trajectories and meaningful systemic improvements—yield high-performance benchmark learning rates ranging from around 0.8σ to 1.0σ. In this paper, we use an artificial benchmark of 0.8σ for learning gains, which is a conservative high-performance benchmark consistent with this range.

5.2 Test-score scaling

Next, we explore sensitivities to score scales, comparing our results based on scores rescaled via a linear transformation of 300 points to the original HLO score scale. This enables us to use a scale that starts at zero, which has useful statistical properties. In Appendix Figure B2, we corroborate this *de facto* floor with data from EGRA, which shows that an HLO score of 300 corresponds roughly to zero percent reading comprehension.

Appendix Figure B3 compares the L_i^h value using the two score scales. While the scale that we use largely does not affect relative ranks, it does affect the degree of the absolute learning adjustment. Using the original scale (y-axis), the distance between Mexico and Ghana is 0.2; by contrast, under the re-scaled version (x-axis), the distance is closer to 0.5. Appendix Figure B4 compares results using the two score scales. In Panel A, we see that the main effect of the rescaling is to reduce the micro-LAYS values that are based on participation impacts—for example, conditional cash transfers in Malawi. This is because under the original scale, the maximum learning adjustment discounted a year of school by about half, since the *de facto* floor of the HLO scale was 300, which produced a learning-adjustment factor, L_i^h, of 0.48 relative to the high-performance benchmark of 625. Under the rescaling, the minimum learning factor converges to zero, and the learning adjustments drop substantially, reducing participation-based LAYS estimates. As an example, the learning adjustment in Kenya shifts from an original L_i^h of 0.73 to 0.48, while countries on the lowest tail of distribution, such as Malawi, shift from a learning-adjustment of 0.57 to 0.18. The re-scaling does not affect the computation of learning-based micro-LAYS, since those values are derived relative to an artificial high-performance benchmark of 0.8σ. However, as an added sensitivity test, we can use the old scale to derive a new corresponding high-performance benchmark of 1.8σ. Panel B plots LAYS using this new benchmark.

These sensitivity tests show that the unscaled scores yield higher values for micro-LAYS based on participation estimates. Also, although unscaled scores largely preserve rank, they understate the degree of learning adjustment, especially for countries on the lower tail of the distribution. For these reasons, in the main results presented in this paper we use micro-LAYS based on re-scaled scores. Since the lowest-performing countries are already far behind, re-scaling of scores is unlikely to yield major new insights and will not change ranks; however, we think re-scaling is important for capturing the full degree of the learning gap.

5.3 Standard deviations across tests and samples

We also test sensitivity of our results to differences in standard deviations across tests and samples. This is relevant because, for the interventions included in our comparisons, we use effect sizes expressed in standard deviations of each intervention's test and sample as inputs into LAYS. Note that this issue is not inherent to LAYS, which are just a unit of measurement that can be applied to any assessment results. In an ideal world (ideal from a comparability perspective), all interventions and national-level assessments would use the exact same test to measure learning

progress, and therefore there would be no issue with LAYS conversion. However, in practice studies and interventions vary widely in the test used, making comparability an issue. LAYS can currently be constructed only with the data as they are, so a separate effort is needed to produce comparable underlying learning assessments.[15] Once that effort bears fruit, it will enhance the comparability of LAYS.

For now, therefore, we rely on standardized effect sizes. Standardized effect sizes are used to account for differences across measurement scales and express those effects in relative terms. This should prove useful when comparing effect sizes in education across various assessments and scales. However, standard deviations will not account for whether a given test is either "too hard" or "too easy," causing floor or ceiling effects. We test for this possibility empirically by comparing standard deviations from tests on nationally representative samples, chosen to ensure that the same underlying population is represented. We focus on primary-level tests for countries that have participated in multiple tests and that have interventions featured in this paper. Appendix Figure B5 compares standard deviations for Tanzania, Malawi, and Indonesia using various assessments: HLO scores derived from EGRA, Progress in International Reading Literacy Study (PIRLS), raw EGRA or Southern and Eastern Africa Consortium for Monitoring Educational Quality (SACMEQ) tests. We find only small differences of a few points, and as a result, the estimated learning rates per year across assessments are quite similar, as shown in Appendix Figure B6. As a robustness check, Appendix Figure B6 also shows learning rates per year using raw data from the assessments before they are converted to HLO scores.

In terms of standard deviations across samples, it is well known there is high variation across sample populations. As a result, when students answer five questions correctly in a country with less variation, this will appear as a larger standardized effect size than if they had gotten the same questions correct in a country with more variation. This is a feature of standard deviation comparisons rather than a bug, since standardized effect sizes produce relative comparisons to derive a sense of magnitude. Therefore, while LAYS conceptually have the advantage that they do not need to use standard deviations as inputs, in cases where in practice they do use standardized effect sizes as inputs (as in this paper), comparisons of relative performance and rank orders should be prioritized over precise magnitudes of absolute performance.

5.4 Status-quo learning

When we compute LAYS from learning estimates, we first convert learning into equivalent years of school gained. To do this, we express learning relative to status-quo learning trajectories. For

[15] Short of a universal test, a number of efforts currently underway may improve comparability. One approach focuses on a set of common test items that can be inserted into assessments and linked to regional and international assessments, such as TIMSS and PISA. The Jameel Poverty Action Lab has launched an initiative to produce an "item bank" of questions for inclusion in education interventions to produce comparable assessment. The Rosetta Stone initiative of the International Association for the Evaluation of Educational Achievement (IEA), the UNESCO Institute for Statistics (UIS), and other organizations are also working to improve comparability across regional and global assessments by having the same samples of students take both types of assessments (Sandoval Hernandez, 2022).

example, if students learned 0.25σ per year in an intervention in South Africa, and students typically learn 0.25σ in a given year in South Africa, impacted students will have learned a year's worth of schooling in South Africa. A few options exist for possible status-quo learning trajectories: for example, we might use the national average learning trajectory, or the learning gains in the control group of the same evaluation from which effect sizes are drawn. Alternatively, we could use an average learning profile across all countries being compared. This choice of status quo will affect our interpretation. If we choose the experimental or quasi-experimental control group, then the resulting figure for equivalent years of schooling gained is relevant to the study sample only. If we choose national average learning trajectories, we can interpret the figure as the number of equivalent years of schooling gained at the national level, with the embedded assumption that the standard deviations are comparable. In the main analysis reported above, we use national-level learning trajectories; in this section, we explore the alternative of using the study's control group to measure status-quo learning.

An advantage of using control-group status-quo learning is that this estimate is drawn from the same sample as the learning gains from the intervention are, so the two are directly comparable. If the study sample is not representative of the nation, however, then when we later apply a national-level learning adjustment to compute LAYS in globally comparable terms, the adjustment will be less reliable. We can test the assumption of representativeness of study samples by examining the degree of variation within a country. If variation within a country is large, this means that any given study sample is likely to diverge from the national average. We test this assumption using a uniform test, EGRA. Two advantages of EGRA data are that (1) EGRAs are included in the World Bank Harmonized Learning Outcomes database and (2) they are often used to assess the impact of interventions and policies. In Appendix Figure B7, we compare variation within a country to variation across countries as a benchmark of whether variation within a country is large. We find that for a sample of 39 countries for which we have EGRA HLO scores, the average cross-country standard deviation is 53, whereas the within-country standard deviation is often higher than 53, with a density skewed to the right tail. This finding indicates that within-country variation is often quite large, casting doubt on the assumption that a sample would necessarily represents the nation. As a specific example, control-group status-quo gains in India in our sample of studies range from 0.5σ to 0.76σ, implying high variance from study to study.

In summary, we find that the assumptions for using control-group learning trajectories as our measure of the typical status-quo learning in a country are unlikely to be robust. We instead rely on national-level learning trajectories, which are easily interpretable and can be converted to a global metric. An additional advantage of using national-level learning trajectories is a practical one: greater data availability. Whereas control-group information is often missing from published papers, national learning trajectories can be calculated using HLO scores, which are available for 164 countries.

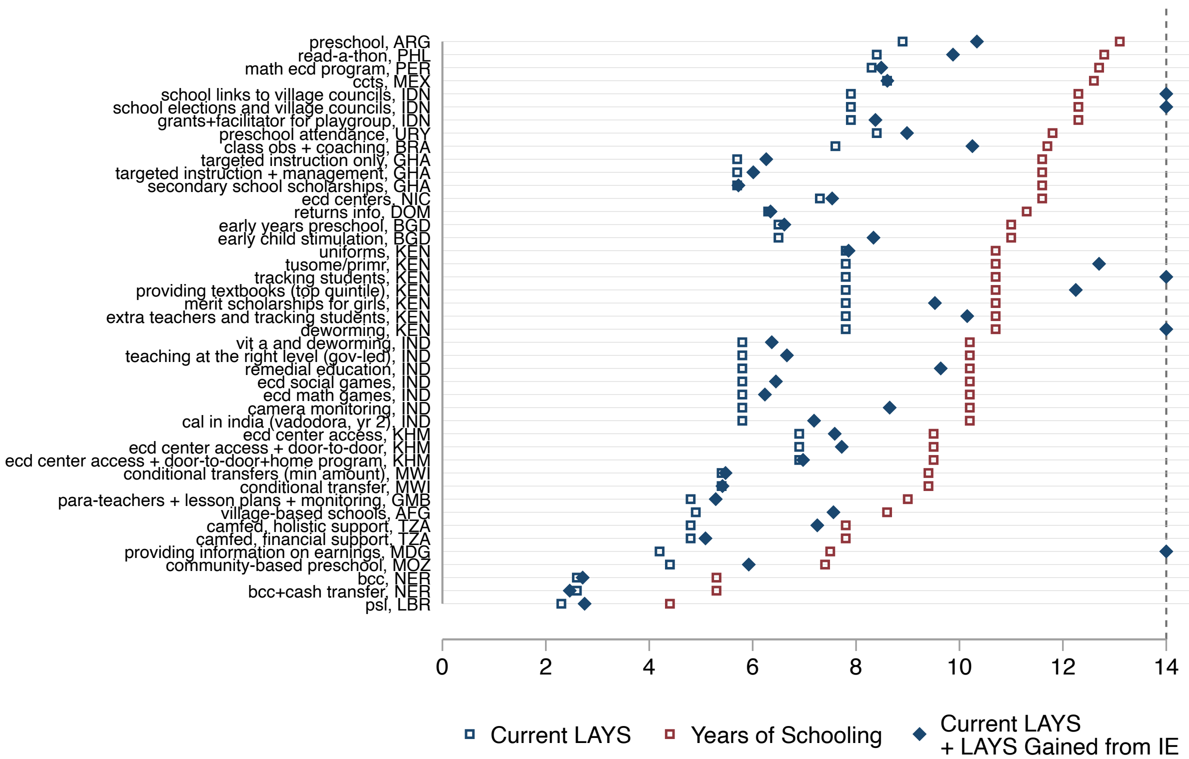
6 Conclusion

In this paper, we analyze which investments most efficiently improve education outcomes. We expand on previous education reviews and analyze 150 interventions and policies across 46 countries using a recently developed unified education measure: learning-adjusted years of schooling. A central insight from this analysis is that many interventions that increase participation in schooling are less cost-effective than interventions that improve the productivity of schooling—–that is, the amount of actual learning gained. Policies that improve the productivity of each year of schooling, such as targeting instruction to a child's learning level or improving pedagogy through structured lessons plans and coaching, can yield large gains in LAYS, narrowing the gap between high- and low-preforming education systems globally. These results should be interpreted with context in mind: challenges should be identified locally, and the global evidence base should then be used to identify possible cost-effective solutions, which should then be carefully adapted to the local context.

This analysis strengthens the foundation for the use of LAYS as a common metric for the economic evaluation of education interventions. Similar unified metrics have played important roles in public health, macroeconomics, and economic welfare analysis, but to date no reference standard exists for education cost-effectiveness analysis, and approaches to comparative analysis have been *ad hoc*. Using micro-LAYS to express impact sizes achieves three goals: (a) it places attainment and learning outcomes on a unified scale, allowing interventions to be compared directly; (b) it expresses educational outcomes in terms of an easy-to-interpret measure that improves incentives for policymakers to promote both quantity and quality of schooling; and (c) it identifies levers for countries to close gaps between their current performance and the full years of high-quality schooling that they aspire to. Recent research suggests that policymakers may not reap political benefits from learning gains alone (Habyarimana, Opalo, & Schipper, 2020), yet an additional year of schooling can lead to very different levels of learning (Singh, 2020; World Bank, 2018). Using LAYS as a metric of progress allows a focus on additional years and learning together. At the same time, the LAYS framework also helps make a clear case for future impact evaluations to use standardized approaches to measuring learning, for example by using test scores from internationally applied assessments such as the Trends in International Mathematics and Science Study (TIMMS), the Programme for International Student Assessment (PISA), or the Early Grade Reading Assessment (EGRA). This shift would greatly improve the likelihood that policymakers can make informed decisions about which interventions to prioritize and scale up.

In summary, this paper uses learning-adjusted years of schooling to provide guidance on which policies and interventions are the most efficient investment in education, given the state of evidence and data available today. The LAYS metric has recently been incorporated into large-scale policy efforts to improve education. It is a component of the World Bank's recently launched Human Capital Index (World Bank, 2019), and is being used by the World Bank, UNICEF, and United Kingdom's Foreign, Commonwealth & Development Office (FCDO) to prioritize cost-effective

education investments. These efforts demonstrate the value of the analysis in this paper to provide a useful tool and synthesis for policymakers, researchers, and decisionmakers who are seeking to address learning and access gaps.



Notes: This calibration assumes no loss of effectiveness once an intervention operates at national scale, which often is not the case. Alternative calibrations could apply a discount factor to account for weaker effects at scale. For the purposes of this exercise, which are designed only as a calibration of effect sizes, we provide a single estimate. We include years of schooling and learning-adjusted years of schooling (LAYS) from publicly available data used in the World Bank's Human Capital Index for each country. The LAYS gained from the impact evaluation (IE) indicates how much a given intervention or policy helps a country close its country-specific LAYS gap as well as bridge the global LAYS gap. The dashed red line at 14 years of schooling indicates the "distance to the frontier" as defined by the HCI as 14 years of high-quality schooling. Where the LAYS gained from the IE result in a LAYS estimate that exceeds the global benchmark of 14 of high quality schooling, we set the LAYS gained from IE estimate to the value needed to close the global LAYS gap fully.

Figure 8: LAYS Gained per \$100 per Intervention, Calibrated to Country-Level LAYS Gaps

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Appendices

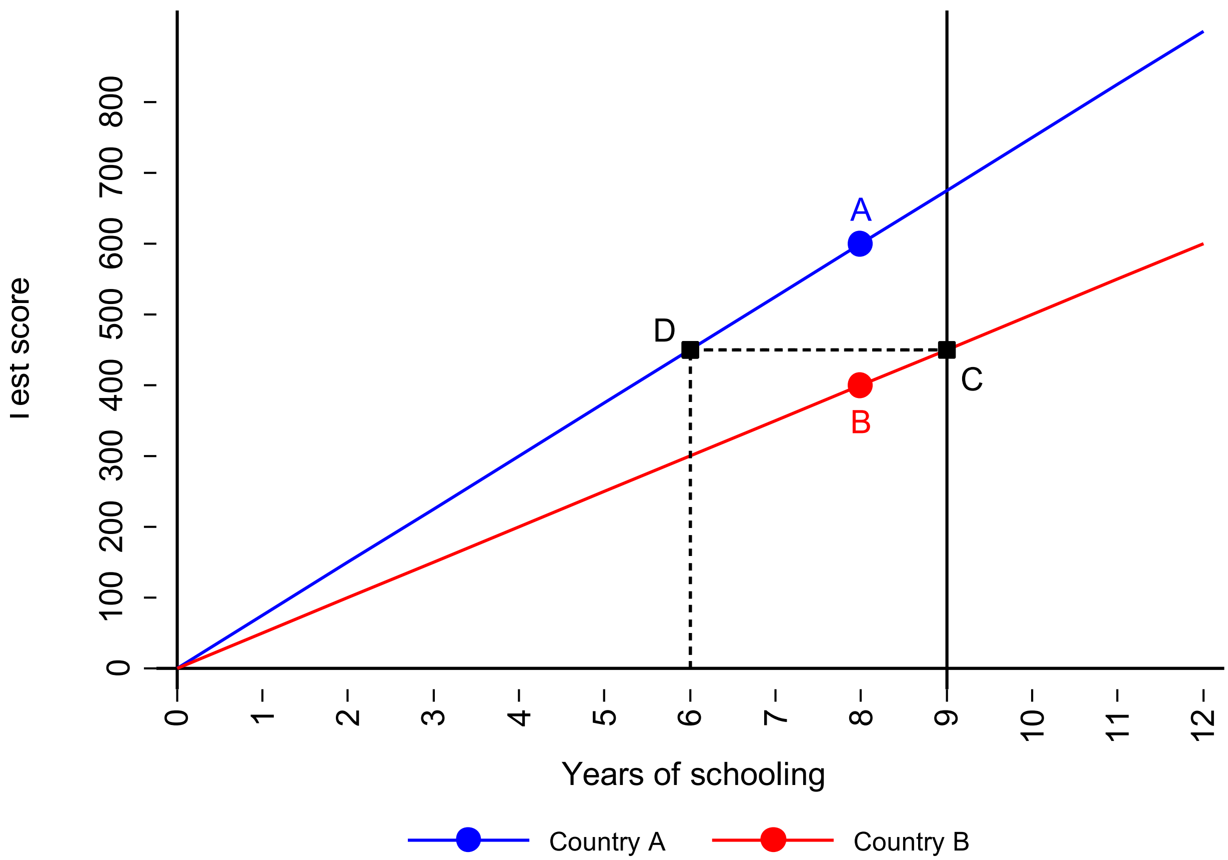
Appendix A The assumption of constant average learning trajectories

The assumptions invoked in the construction of macro-LAYS are explored in depth in Filmer et al. (2020). Here, we highlight one assumption: constant average learning trajectories, or the idea that students learn the same amount with each additional year of schooling.[16] Figure A1 shows the utility of this assumption using a hypothetical example. Assume that we observe Grade 8 test scores of 600 for Country A and 400 for Country B and that individuals in Country B average 9 years of schooling. LAYS allow us to "convert" the 9 years of schooling in Country B into the number of years of schooling in Country A that would have produced the same level of learning. Moving along the average learning profile from Grade 8 allows us to infer what Country B's average score would be if its students were tested in Grade 9. This calculation is represented by the move from point B to point C, or from a test score of 400 to 450. The next step is to go from point C to point D, to find the number of years of schooling that it would take in Country A to produce that level of learning (450) given the average learning profile in Country A. In this example, it takes 6 years, so the resulting LAYS measure in Country B is 6. Both steps of the calculations rely on the linearity assumption, because we do not have data on the actual learning trajectories but rather on learning at one point in time for each country.

How realistic is this assumption? Filmer et al. (2020) explore this question with a series of empirical tests on whether learning trajectories are constant on a locally defined interval. Figure A2 showcases one example using data from India's Annual Status of Education Report (ASER), which administers the test consisting of the same questions to students from ages 5 to 16, covering Grades 1 to 12 (ASER Centre, 2018). The ASER data enable us to assess the rate of learning with a stable, comparable metric across grades and over time. To allow us to map out the specific trajectory for learning in school, we restrict our sample to school-going children.[17] In the case of a mathematical skill, division, Figure A2 shows that students learn along an "S-shaped" learning trajectory, but with a locally linear interval from Grades 5 to 10. Other, more complex skills than division are likely to have a linear learning trajectory across an even wider interval because they cannot be mastered so quickly. The other empirical tests in Filmer et al. (2020) also yield results consistent with the linearity assumption (at least over a significant local interval).

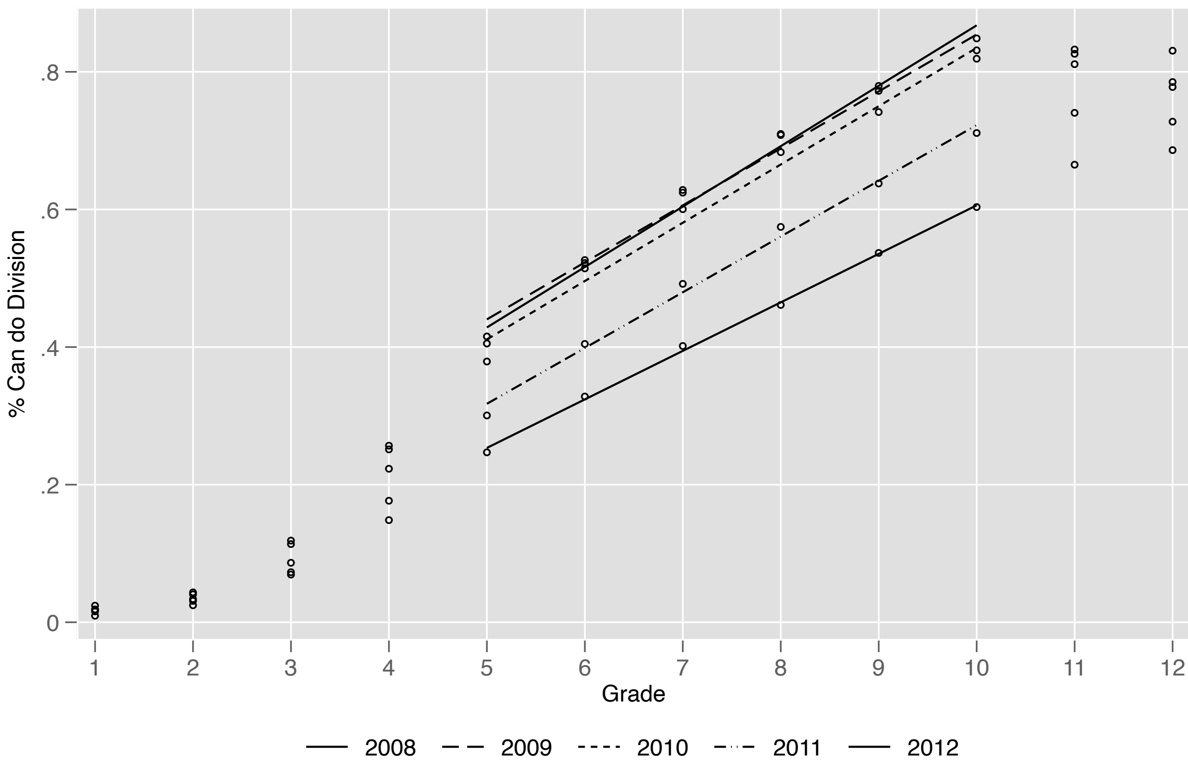
[16] This framework focuses on learning within schools. Clearly, not all learning happens within schools. However, learning outside schools is beyond the scope of this exercise. For a fuller discussion see Filmer et al. (2020).

[17] This comparison is conducted across different cohorts of students at different grades.



Notes: We map hypothetical learning trajectories in countries A and B to demonstrate the utility of the assumption of constant learning trajectories.

Figure A1: Constant Learning Trajectories

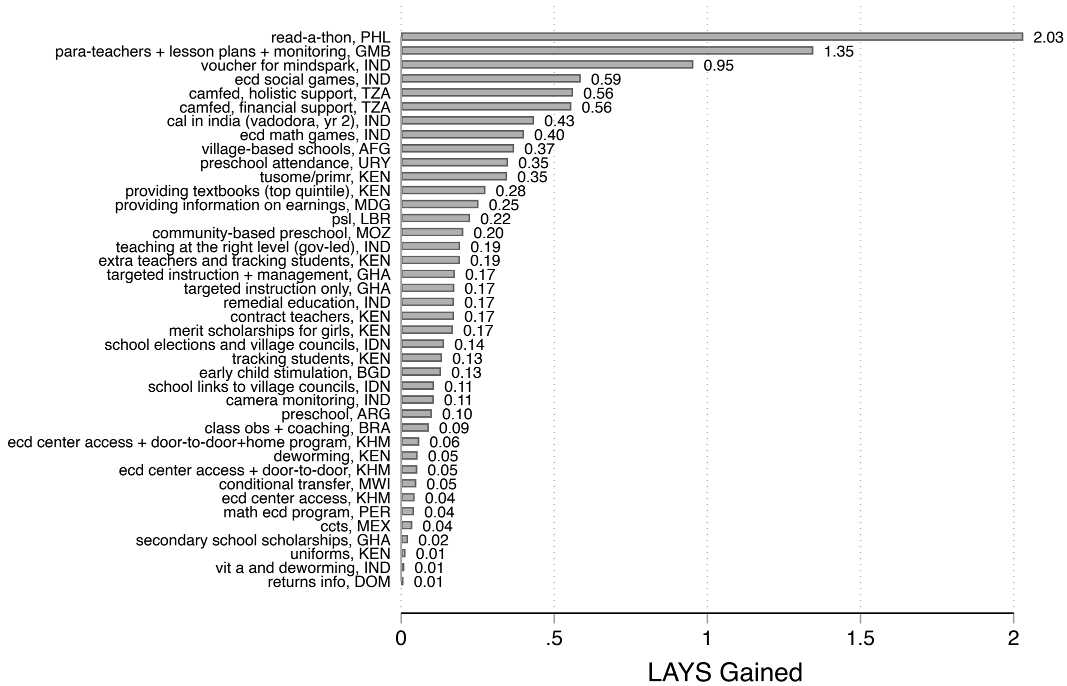


Notes: We derive learning trajectories using empirical data from a national survey conducted in households in India for students aged 5 to 16 in grades 1 through 12. We include only students at the household who are in school.

Source: ASER India data from 2008 to 2012 as analyzed by Filmer et al. (2020).

Figure A2: Learning Trajectories in India

Appendix B Additional Figures



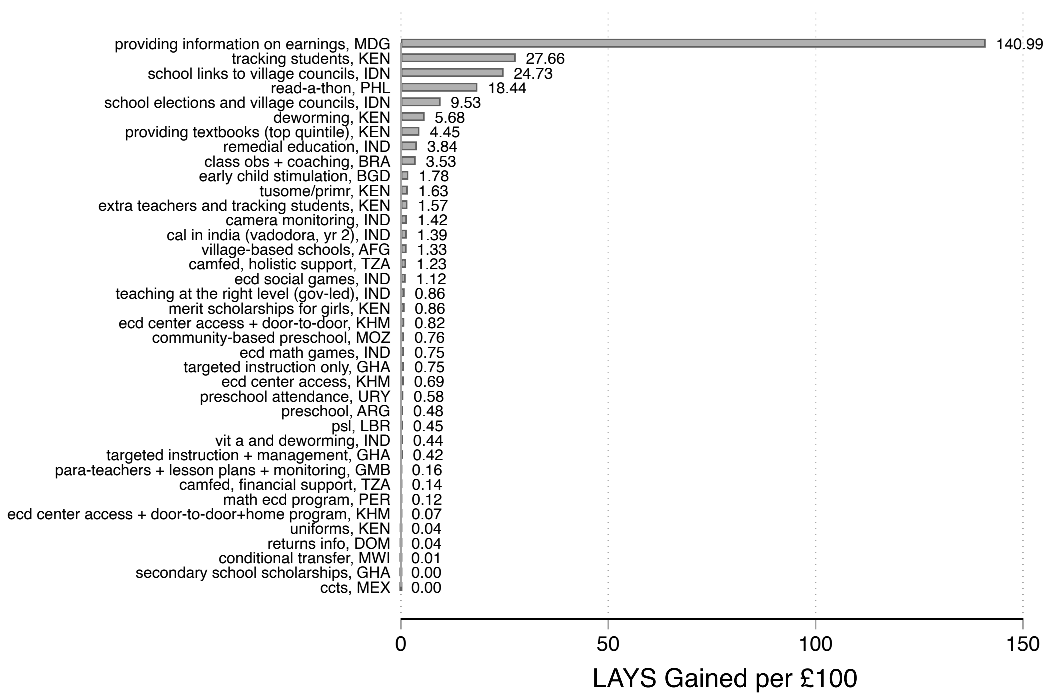
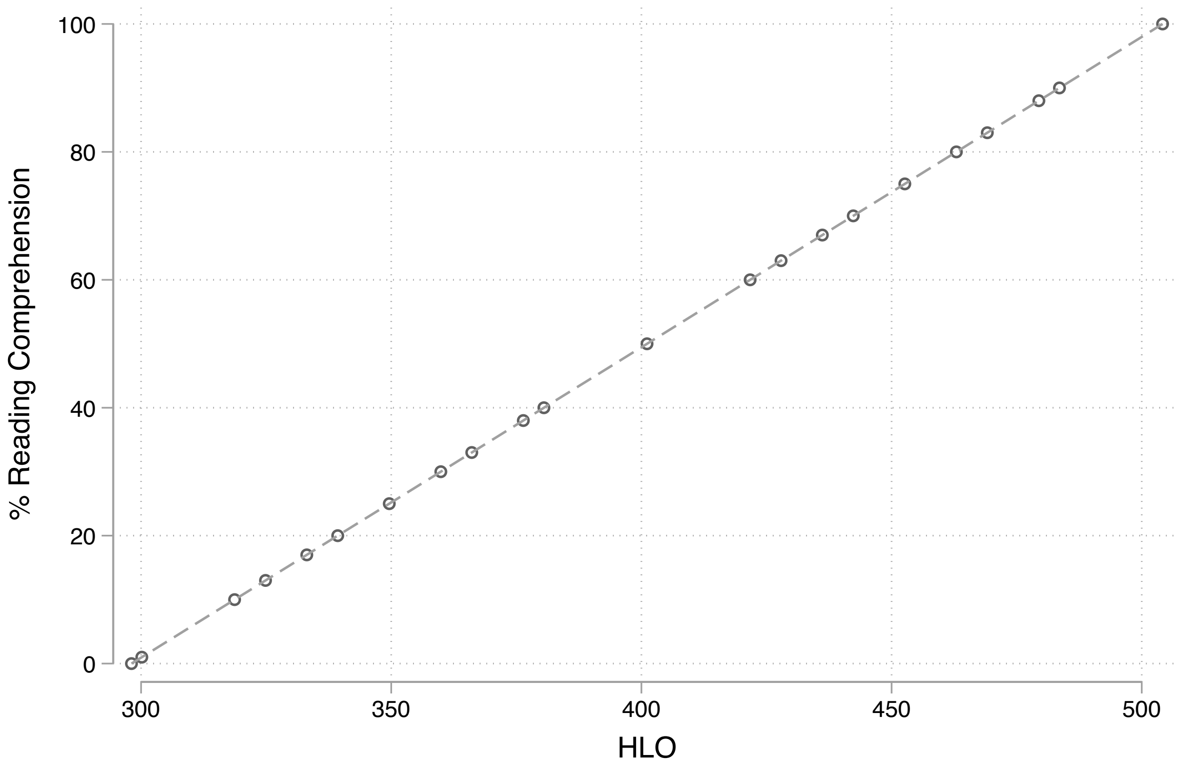
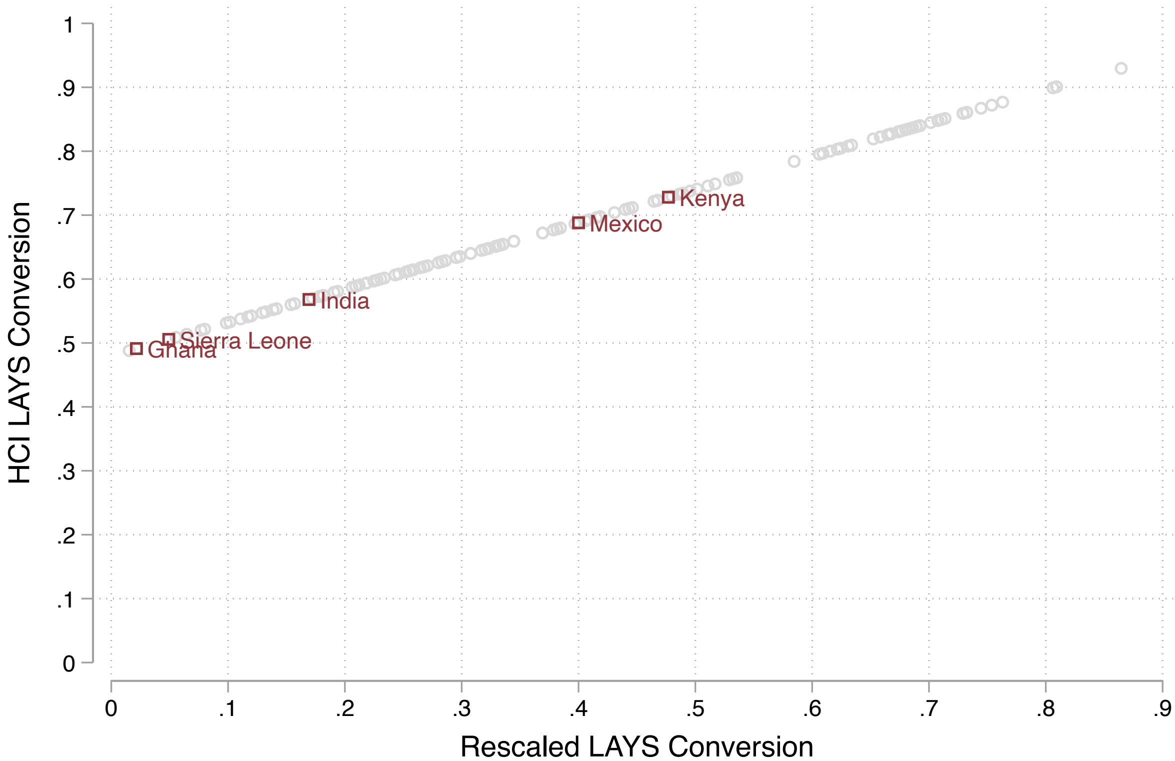


Figure B1: Expressing LAYS gained per year (t = 1)



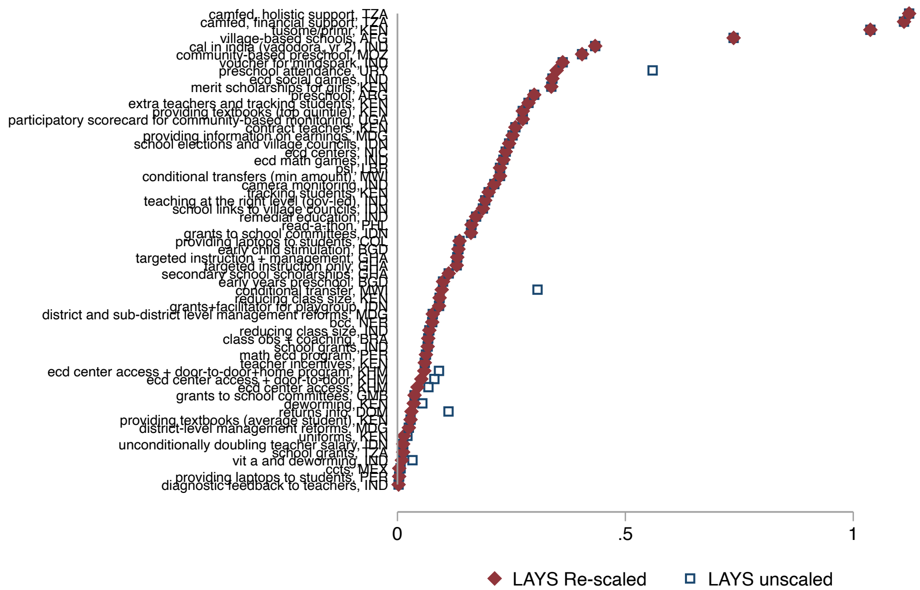
Notes: We analyze EGRA data across 39 countries and match raw score on reading comprehension modules with the Harmonized Learning Outcome (HLO) scores used for the World Bank Human Capital Index.

Figure B2: EGRA raw reading comprehension relative to HLO score

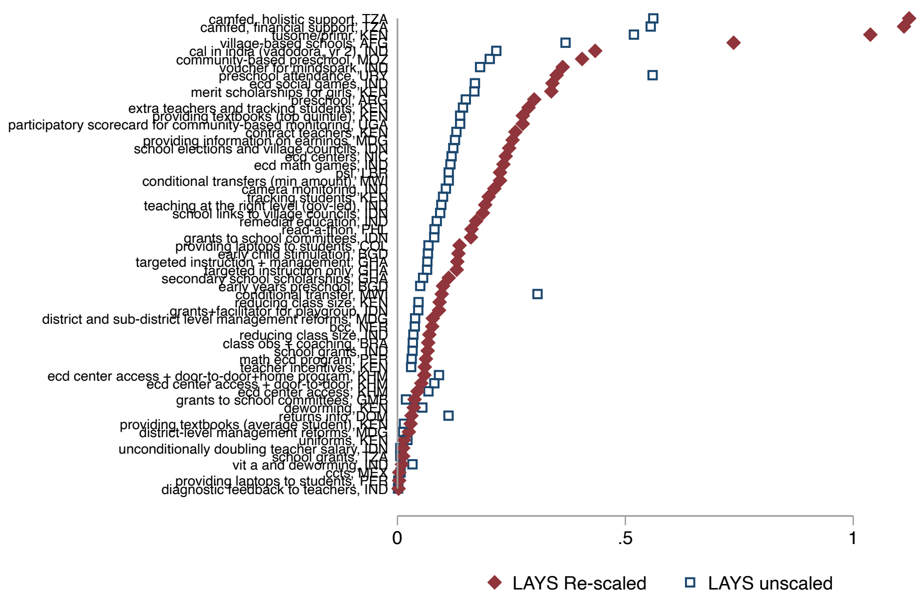


Notes: We rescale LAYS conversion rates. Initial conversion rates are based on scores which often floor around 300 due to underling test scores scales. Since the LAYS conversion rate is calculated out of 625, this produces a floor conversion rate of .48. However, when learning levels are very low this conversion will under-adjust learning. We rescale LAYS exchange rates to range from 0 to 1.

Figure B3: Learning Adjustment Rates

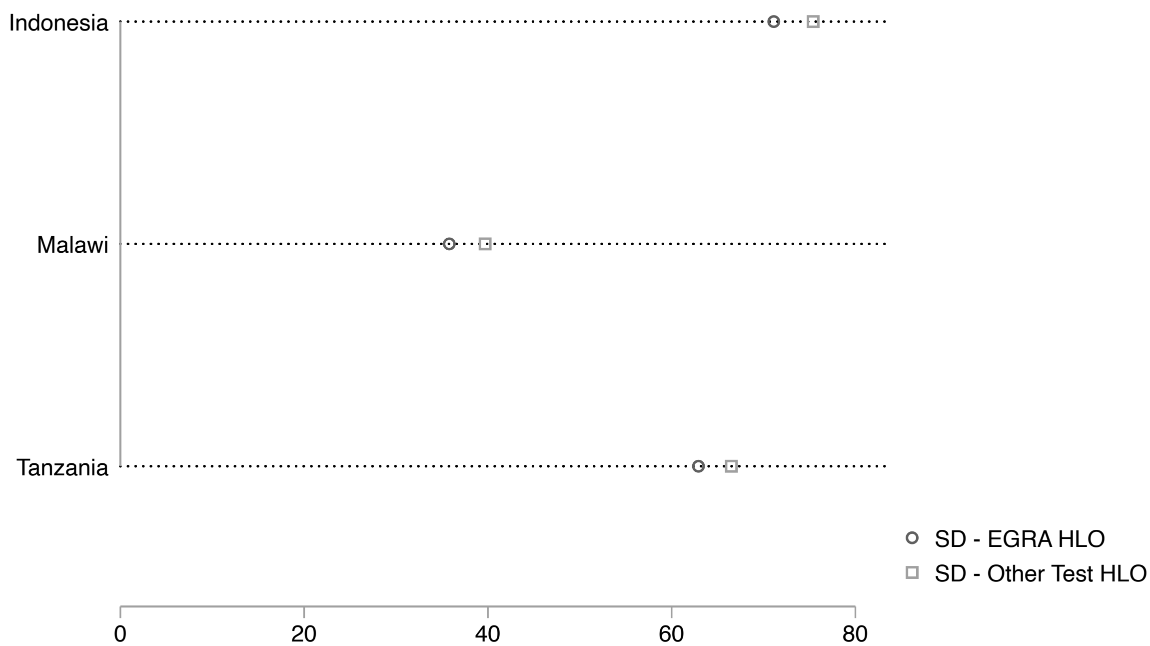


(a) *Notes: We rescale LAYS conversion rates for participation-based estimates to adjust for a quality factor that accounts for the new HLO scale. We do not adjust learning-based estimates in this figure since they are not adjusted by a learning factor. Rather, they are based on an assumption of high-benchmark learning trajectories of .8σ.*



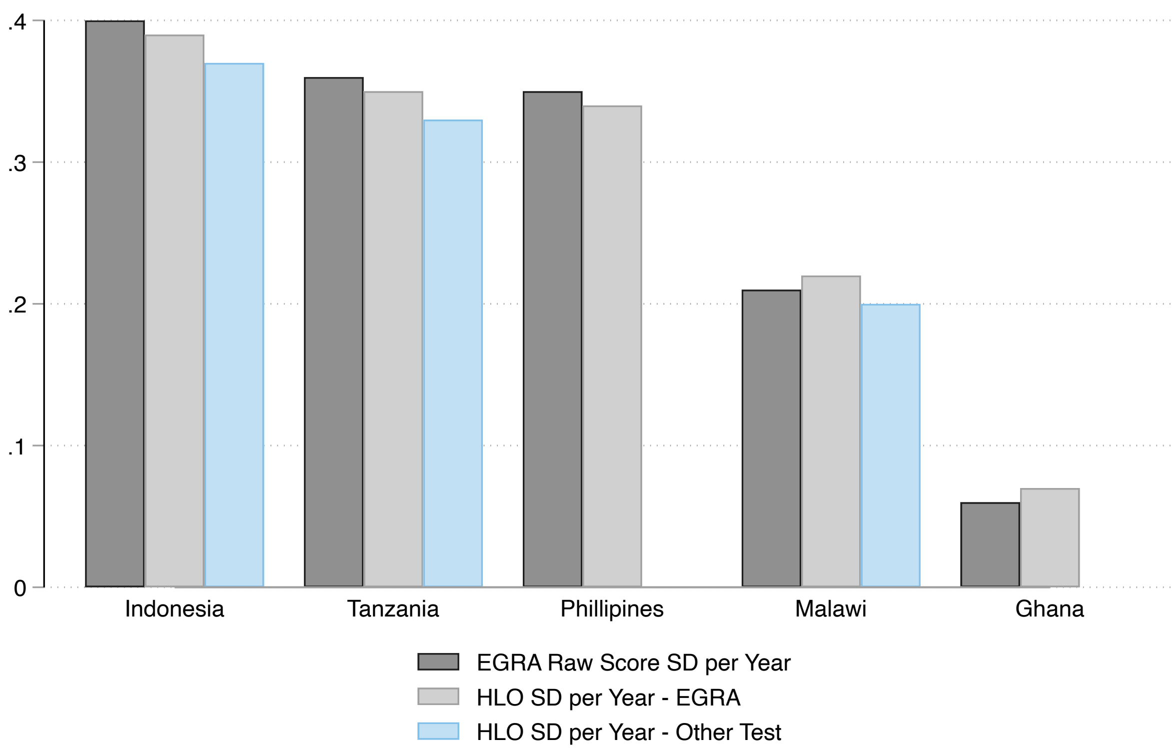
(b) *Notes: We rescale LAYS conversion rates and apply this rescaling to participation-based estimates to adjust for a quality-adjustment factor that accounts for the new HLO scale. In this figure, as a robustness test we adjust learning-based estimates by deriving a new high-benchmark learning trajectory based on a new scale, which yields 1.8σ.*

Figure B4: Comparing LAYS using scaled and unscaled test scores



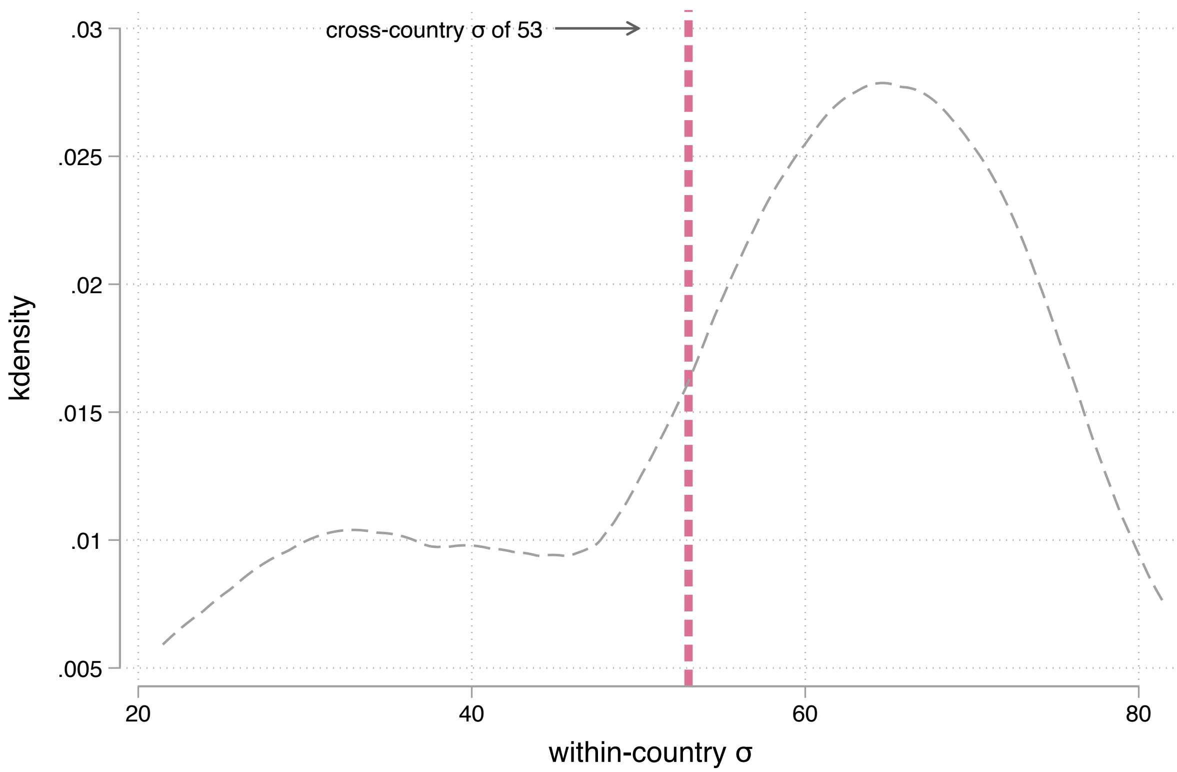
Notes: For Indonesia, the "other test" is PIRLS 2011; for Tanzania and Malawi it is SACMEQ 2007.

Figure B5: SD Comparisons, by Source Test



Notes: For Indonesia, the "other test" is PIRLS 2011; for Tanzania and Malawi it is SACMEQ 2007. We assume all scores were obtained in Grade 4 as a placeholder for primary school scores.

Figure B6: Learning Per Year (in SD), by Source Test



Notes: We use micro EGRA data across 39 countries and include country-year observations. The x-axis represents within-country variation. The vertical line represents the cross-country standard deviation: 53 for the cross-country variation of this EGRA as a benchmark. Variation is often greater within country than across countries, with most-within-country SDs falling to the right of the vertical line.

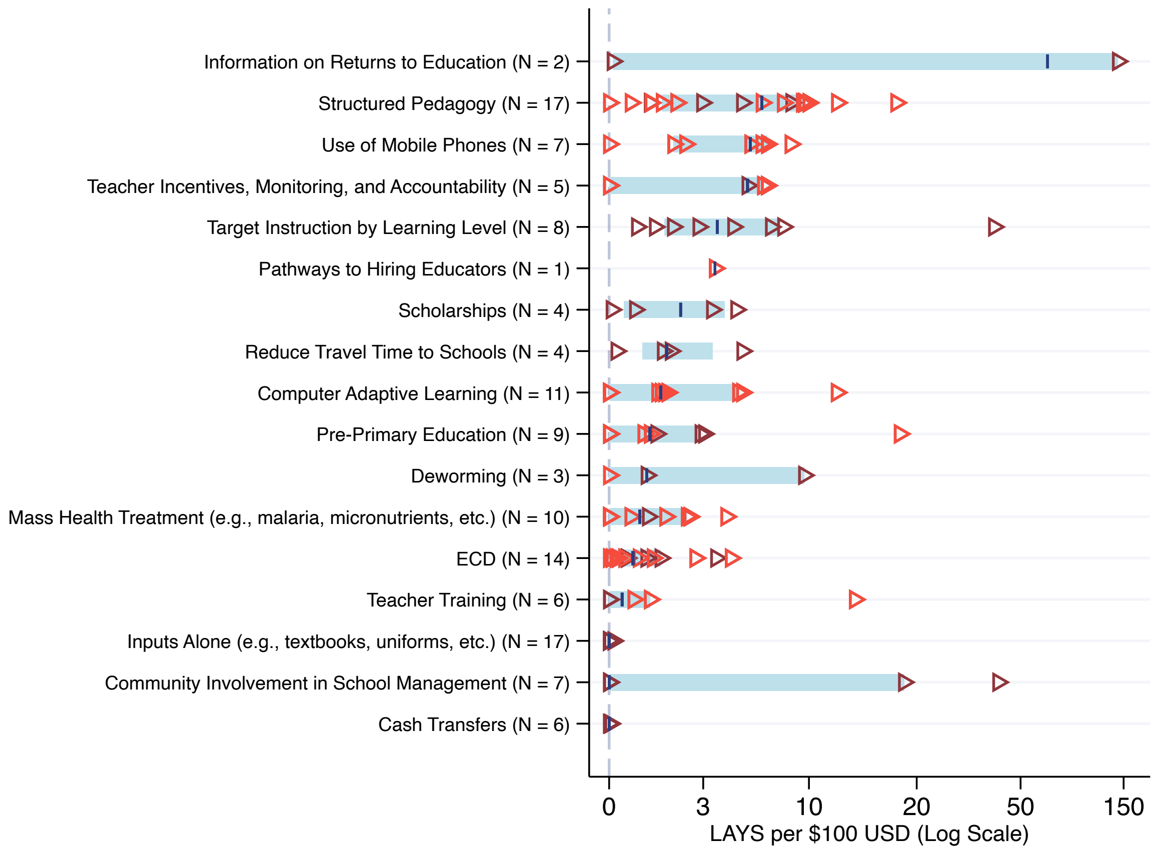
Figure B7: Within- vs. Cross-Country Variation in Test Scores

Appendix C Additional Cost Effectiveness Estimates

We complement our dataset with cost-effectiveness data from the 2023 GEEAP report (Akyeampong et al., 2023). These data were collected after performing a systematic search of the literature. This search process yielded over 13,000 papers, which were later screened and synthesized using similar inclusion criteria to the ones for our paper. This reduced their number of studies to 235, out of which 91 had some cost information. After cleaning the data to ensure compatibility, we added 60 data points to our dataset. Outcomes were also coded so that positive impacts represent improvements on learning. Estimates were reported in terms of standard deviations per $100 USD, and later transformed to LAYS per $100 USD.

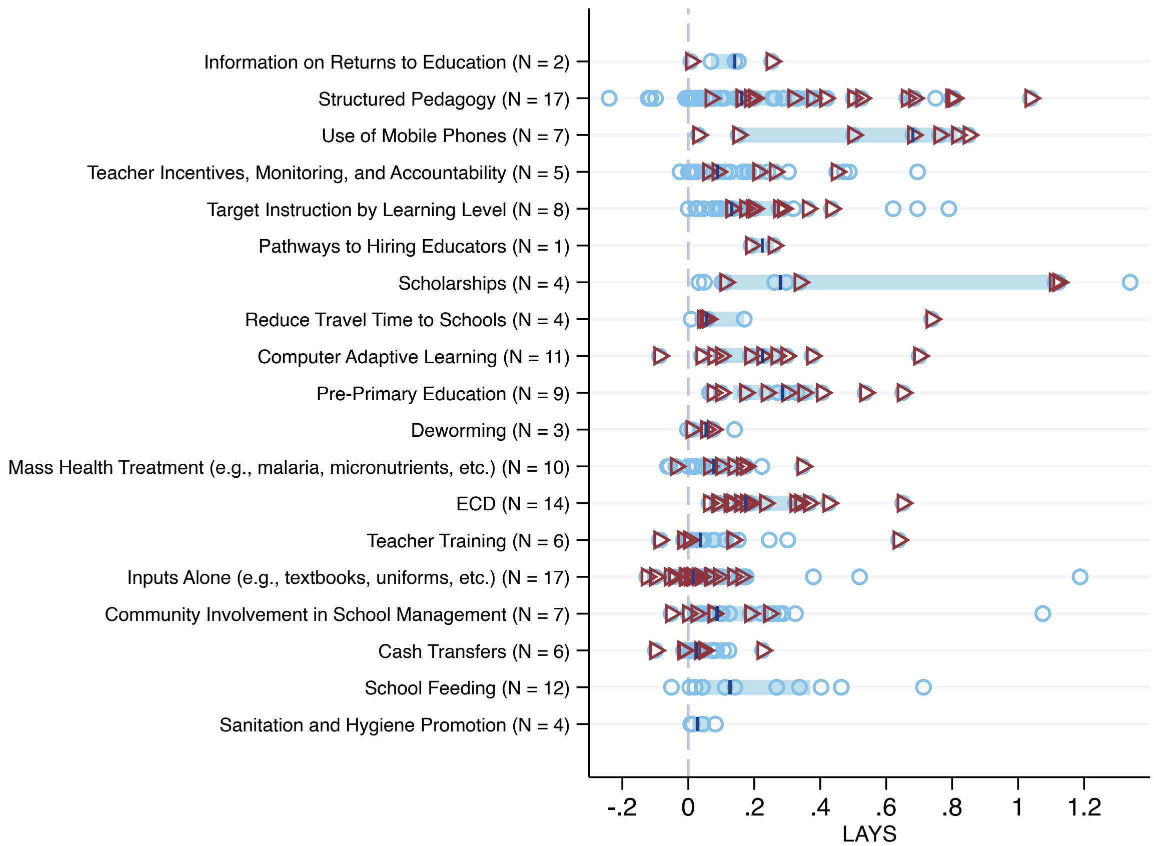
We replicate the cost-effectiveness analysis from Figure 3 with this extended dataset in Figure C1. Overall, the findings of our paper are robust to the inclusion of these additional data. In particular, some of our findings are strengthened, such as the high cost-effectiveness of interventions that provide structured lesson plans. Additionally, it is worth noting the inclusion of some intervention categories that were not part of our original dataset and that rank highly in terms of cost effectiveness. Notably, interventions that leverage the use of mobile phones and technology that is already in place come out to be highly cost-effective. This is consistent with one of the main findings of our paper in that interventions that improve the productivity of schooling (for instance, via mobile phone tutoring) tend to be more cost-effective than interventions that focus exclusively on increasing schooling.

To complement the analysis we provided in Figure 4, we show in Figure C2 the set of interventions that include cost-effectiveness data along with the rest of the data points for which we do not have data on costs. Once again, our findings are robust to the inclusion of these data, as we confirm that studies that have cost information are not biased towards high or low impacts upon visual inspection. Further, we confirm that our findings on cost-effectiveness can be explained by the interaction of impacts and costs for each intervention category. For instance, while interventions that focus on pre-primary education and ECD have high impact, they also tend to be relatively expensive to implement and therefore rank low on cost-effectiveness. Interventions such as deworming and mass health treatments in general have low impacts but are relatively inexpensive, which makes them highly cost-effective policies. Finally, interventions that rely on the use of mobile phone technology have both high impacts and relatively low costs, which makes them a highly cost-effective intervention category.



Notes: Each category of education intervention shows the learning-adjusted years of schooling (LAYS) gained from a given intervention or policy. Each triangle represents a cost-effectiveness estimate. Red triangles are our original sample, while orange triangles are the estimates integrated from the 2023 GEEAP report. The boxplot is ordered from largest to smallest median effects and the shaded area describes the 25th and 75th percentiles. The x-axis is reported on a natural log scale.

Figure C1: Learning Adjusted Years of Schooling (LAYS) Gained per $100 by Category



Notes: Each category of education intervention shows the learning-adjusted years of schooling (LAYS) gained from a given intervention or policy. Each triangle represents a cost-effectiveness estimate and comprise the full sample showed in Figure C1. The boxplot describes the 25th and 75th percentiles. The boxplot is ordered in the same order as Figure C1 to provide a direct analogy, with the exception of the "school feeding" and "sanitation and hygiene promotion" categories which have no cost-effectiveness data and do not appear in Figure C1.

Figure C2: Learning Adjusted Years of Schooling (LAYS) Gained by Intervention Category

Appendix D Full List of Citations

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