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Investing in Human Capital in Africa: A framework for research

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ABSTRACT

This essay argues that the existing paradigm in discussions of the acquisition of human capital has been focused on the drive to universal schooling and expanding access and grade attainment. This focus has been quite successful. The expansion of schooling in Sub-Saharan Africa (SSA) over the last decades has been impressively rapid, in percentage growth terms much faster than other regions of the world, because SSA at political independence began far behind most other regions.

However, the paradigm needs to shift as "invest in human capital", which implicitly focuses on the acquisition of valued skills, has mostly been treated as equivalent of "spend on school" and this conceptual elision has produced very mixed results on learning and the creation of cognitive skills, which were, and are, taken to be an important goal of schooling. This section therefore focuses on some facts about schooling and learning with an emphasis on both the question of whether: (i) "Sub- Saharan Africa" has been distinctive as a region; and (ii) the heterogeneity across SSA both in sub-regions and across countries that make generalizations about SSA problematic (if not outright unhelpful).

The conclusion is that there needs to be a shift from the crude "accumulationist" model of "invest in human capital" as exclusively: (i) more years spent in school; and (ii) more spend on school. "Invest" in human capital must mean: (i) acquisition of valued skills, capabilities, dispositions; and (ii) effective spending. This implies three major changes in the research paradigm: (i) stop using "year of schooling" as the major "outcome" to be pursued; (ii) stop using a naïve "education production function" to evaluate impact of inputs towards a systems approach; and (iii) as part of that, work towards a more realistic positive model of the politics of learning

1. Introduction

This article is the first of an overall work with three distinct parts. Together they are a proposed structure for a forward-looking approach to researching human capital in Sub-Saharan Africa.

Thomas Kuhn's classic book, *The Structure of Scientific Revolutions,* made a sharp distinction between "normal science" as the patient fleshing out of questions raised within the dominant paradigm (e.g. Newtonian gravity or pre-quantum mechanics particle physics). Also a part of "normal science" is seeing if the paradigm can be expanded, modified, and tinkered with to accommodate observed factual anomalies and to expand the range of phenomena explained by the dominant paradigm. But in Kuhn's terms, a "paradigm shift" changes the framework and basic ideas about the underlying phenomena that shifts the ways in which the anomalies are seen and then creates a whole new set of questions for a new "normal science" to address, while hopefully "encompassing" and hence explain new phenomena while also being able to explain everything the previous paradigm could. Obvious examples of paradigm shifts are the shift from Newtonian gravity to Einstein's General Relativity, the shift from classical to quantum mechanics, the idea of evolution through natural selection as the explanation of variations across species, the understanding of the structure of DNA.

This present work argues that the existing paradigm in discussions of the acquisition of human capital has been focused on the drive to universal schooling and expanding access and grade attainment. On one level, this focus has been quite successful as the expansion of schooling in Sub-Saharan Africa (SSA) over the last decades has been impressively *rapid,* in percentage growth schooling has expanded much *faster* in SSA than other regions of the world, in part because most countries in SSA at political independence lagged far behind

However, the paradigm needs to shift as "invest in human capital", which implicitly focuses on the acquisition of valued skills, has mostly been treated as equivalent of "spend on school" and this conceptual elision has produced very mixed results on learning and the creation of cognitive skills, which were, and are, taken to be an important goal of schooling. This section therefore focuses on some facts about schooling and learning with an emphasis on both the question of whether: (i) "Sub-Saharan Africa" has been distinctive as a region; and (ii) the heterogeneity across SSA both in sub-regions and across countries that make generalizations about SSA problematic (if not outright unhelpful).

The conclusion is that there needs to be a paradigm shift from the crude "accumulationist" model of "invest in human capital" as

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exclusively: (i) more years spent in school; and (ii) more spent on schools. "Invest in human capital" must mean: (i) acquisition of valued skills, capabilities, dispositions; and (ii) *effective* spending. This implies three major changes in the research paradigm: (i) stop using "year of schooling" as the major "outcome" to be pursued; (ii) stop using a naïve "education production function" to evaluate impact of inputs and move towards a systems approach to understanding why some schooling produces more learning than others; and (iii) as part of that, work towards a more realistic positive model of the politics of learning.

The anticipated second part of the work proposes a *life-cycle approach* as an overall framing of the issue of human capital in Africa. In this life-cycle approach, I propose two basic periods, one the "accumulation" phase and one the "utilization" phase. The accumulation phase divides the period of "conception/birth to adulthood" into sub-periods and transition (e.g. first 1000 days, starting school, primary to secondary) decisions that affect human capital accumulation. This raises the distinctive set of questions in each period. The second basic period is of "human capital utilization" (with some continued acquisition) which is, the much longer period of human life from youth-to-adulthood to old age to ultimately, in the jargon of economists, the inevitable "Cap T." This section starts with the "school to work" transition and the very tricky and differential across individuals in the "blend" or transitional ages from say, age 15–25 and onward into career paths, job transitions, etc.

What is very different about these periods (and their sub-periods) is to a large extent the "policy and programmatic" tools and levers for improving outcomes and their modes of engagement and contact with the child and the relative mix of engagement in three broad classes of "institutions" (either "social", "state/government" and "economic"). In particular, I think a human capital in Africa project should acknowledge the very important role in human capital of the overall ways in which the capabilities and competencies that augment human capital are gained when young (and these are across the array of both "soft" and "hard" skills, competencies, values, dispositions, attitudes, traits) and how these individual capabilities are embedded productively (or not) in various roles as adults: parents, community leaders, citizens, workers, entrepreneurs, political leaders, thought leaders, etc. That is, "Human Capital in Africa" should include both how human capital is *formed, developed,* and *created* **in** Africa but also how that human capital is *deployed, further shaped,* and *utilized* **in** Africa to allow individuals to promote their well-being, that of their families, communities, cities, regions and countries.

The anticipated third part addresses the "utilization" of human capital. While the first part is primarily about the acquisition of capabilities in youth and in particular acquisition of cognitive skills in formal K-12 schooling. The second part extends to a more "lifecycle" approach that includes the very young, before K-12, and also after. My argument is that the challenges Africa faces are at least as much in "utilization" as in the "accumulation" phase and, without improvements in the "utilization", how productively labour and human capital are deployed in the economy—further progress in accumulation might be of limited value (if not, one might fret, counter-productive).

This in turn suggests a shift in the research from the simple "accumulationist" views of human capital as represented by a simple aggregate of "factors" of "capital" and "human capital" in growth models such as the Solow/Swann model and expand research into the connections between measures and types of human capital and the deeper determinants of the evolution of economic productivity. We cannot simply ignore that the main reason human capital is low in Africa is because the human capital is embedded in people who work in Africa.

This article has four sections. Section I outlines the conceptual distinction between "invest in human capital" and "spend on school" and why the general conflation of "spend on school" with "invest in human capital" has led to large success in schooling but much more limited success in creating human capital. Section II displays the facts about the (mixed) success in expanding schooling in SSA. Section III discusses what is known about the extent to which "schooling", measured as "time served", actually translated into "education" and the accumulation of valued skills and capacities that could legitimately be called "human capital." Section IV articulates the need for a new research paradigm about schools, schooling, and education/learning.

Two caveats about this article. First, this is not a "review of the literature" that pretends to be a balanced summary: rather is one person's assessment on: (i) what are the future big, pressing, policy and practical research questions; and (ii) a take on a feasible path forward in researching those questions.

Second, this three-part work is an expression of my own distinct and distinctive view on the major issues and, as such, it differs from the "mainstream" view (of both economists and educationists) about education and about economic growth. My goal is not to express a "consensus" nor that the reader simply "adopts" my view. Rather my hope is that by presenting my sharply critical and distinctive view on important issues, I encourage the reader to shape their own view, a view based on their own judgment of what is important in their own country (and regional) context and hence not simply adopt the existing paradigm either of economics or of education on policy as the basis for their research (which is, understandably, a powerful professional temptation).[1]

2. I: "Invest in human capital" has been treated as "spend on school"—but it isn't

"Invest in human capital" has been an integral part of development advice since there was such a thing as "development advice" starting in the 1950s/1960s[2] One parsing "invest in human capital" is great development advice. On the parsing that "invest in human capital" means "develop a labour force with the capacities to sustain a high productivity economy and a generally informed and high capability citizenry" it is excellent advice. But the what became the dominant parsing and interpretation in practice supplanted "invest" with "spend" and "in human capital" with "on schooling." "Spend on schooling" became the dominant paradigm in development, and hence in Africa, for research, policy analysis, and advocacy and recommendations.

The "spend on school" paradigm is deeply problematic in many ways and, while it was perhaps a necessary stage, no further good can come from research or policy action that adopts that paradigm. Let me count the *seven* ways that "spend on school" is a conceptual mistake and leads to practical mistakes.

First, "spend on school" conflates "spend" with "investment" whereas current "spend" is "investment" if and only if it is *effective* at creating future value. "Spend on school" confuses "accounting cost", what gets allocated as expenditure to what budget head, with "economic cost", which is conceptually what is the *minimum* needed to be spent to achieve a given outcome (Pritchett and Aiyar, 2014). Budgetary spend

[1] Both caveats help explain what would be, in a paper with any pretense of being a "review", the striking degree of self-citation.

[2] It is a complete and total myth that "early" development efforts or development economics "ignored" human capital. The recurrent claims that this or that innovation "discovered" an important role of human capital (e.g. either Becker's microeconomics or macroeconomic "endogenous growth" models) are just false. For instance, Gunnar Myrdal's classic *Asian Drama* written in the late 1950 s already took it as the already settled conventional wisdom that expanding schooling was a necessary part of development. Although W. Arthur Lewis's 1955 Theory of Economic Growth is taken to have promoted rising savings and investment rates as they key to higher sustained growth, this discussion of savings and investment comes on page 256—well after extensive discussions of how institutional change and expanded education are part of the growth process—and he explicitly includes human capital in his definition of investment. Theodore W. Schultz's contribution on investment in human capital was published in 1961. By the time there was a "development" economics, human capital was seen as an integral part of it.

that is ineffective as producing outcomes is not really an "investment" as it does not contribute to useful "capital" (for an analogy to physical capital, see Pritchett, 2000). As a crude, perhaps inflammatory, example, suppose one had an effective school that was producing good learning outcomes for its students and then it was decided to hire an artist to paint a mural on the front of the school and the mural cost the same as the rest of the school. The spending on the mural undoubtedly increases the "spend" on the school but, unless it affects student outcomes, is conceptually not a "cost" of the production of human capital. The conflation and hence confusion of "accounting cost" with "economic cost" is deep and pervasive in policy discussions.

Second, "spend on school" conflates "time served" with "valued capabilities created." Schooling *can* create valued capabilities if there are effective teaching and learning practices enacted in the school. The assumption is that schooling and learning were causally connected by a "learning profile" such that "more schooling" reliably meant "more capabilities" hence more "human capital." But, we have painfully learned and now know that "schooling ain't learning" (Pritchett, 2013). If the (causal) learning profile for a child attending a given school is flat (or very shallow) then lots more schooling can lead to little or no additional human capital.

Third, "spend on school" assumed that all, or nearly all, of the relevant "investment" in human capital happened in a formal institution called a school and hence downplayed the household. It ignored the key role that decisions controlled by the household, especially but not exclusively in the early years, played in the creation of human capital. The creation of human capital is affected by the (often sharply constrained) choices of households that affect the biological outcomes (e.g., the effects of malnutrition on size, neurological development, motor skills), the social and emotional dispositions, and the preparedness for learning inside schools.

Fourth, the "spend on school" paradigm often (though not necessarily or always) assumed that the formal institution of the school could be separated from the communities (and hence parents and student) into which the schools were placed and that "schooling" could be treated as a strictly technocratic and neutral social process. Or, even more aggressively, "school" was seen as a social transformation independent of, perhaps even dismissive of, what parents and communities wanted.

Fifth, the "spend on school" paradigm often (though again, not necessarily) that the standard mechanisms of the "modern" state, a "civil service" organization that was top-down, hierarchical, rules and compliance oriented, was an adequate administrative structure to create an effective school system. That is, the assumption was that "spend on school" could be more or less completely conflated with "budget to a Ministry of Education" where a "Ministry of Education" followed the same "civil service" paradigm of service provision as the post office or any of the other branches of government (Pritchett, 2014).

In addition to these five limitations of the "spend on school" approach in actually leading to schooling that created skills, competencies, capabilities that lead to improved life outcomes (in all people's adults: parents, citizens, workers, community members), the "spend on school" paradigm also assumed away the "utilization" of skills question as a key determinant of "human capital" in two important senses.

Sixth, "spend on school" more or less assumed away the "school to work" transition and the accumulation of human capital outside of school and formal training. The assumption was made that everything that was needed to be an effective worker was teachable in the standard "school" paradigm (including "vocational" schooling) and hence "school to work" was the key transition. This assumed that (nearly) all skills needed in the workforce that were not taught in school were either general "characteristics" (and hence not teachable in school) or were "firm specific" knowledge and hence would be learned from employers who would have an incentive to invest in their workers.

Seventh, the "spend on school" approach assumed that "school" and "utilization of school acquired skills" were completely separable. In fact, far and away, the most important determinant of "human capital" in a total, absolute, monetary value sense is the productivity of the place in which a person's skills are used (Rosenzweig, 2006, 2010). Since "human capital" is often measured at the individual level as the expected net present value of wages/earnings/income over their lifetime, then the act of migration reveals starkly that an individual's human capital is dramatically affected by the "place premium" of the location in which the individual is working. Estimates show that low-skill migrants from African countries working in the US make many-fold higher wages than equal "intrinsic" productivity workers working in Africa (Clemens et al., 2019). This implies a person's "human capital" is hugely determined by not just "who" they are but "where" they work.

3. II: "Spend on school" has been successful in rapidly expanding schooling in Sub-Saharan Africa

The "spend on school" variant of "invest in human capital" has been adopted and implemented around the globe. It has been successful at expanding access, enrollments, and grade attainment around the world, pretty uniformly across regions and countries, including in SSA and its geographic sub-regions.

There are several facts about this expansion of schooling that need to be emphasized, as they are not widely known and hence play too little role in the discussion of education research and policy.

First, using the standard database of Barro and Lee (2010) and investigating the evolution of the average years of schooling of the labour force aged population (15−64), we can see that expansion of schooling in the developing world since 1960 has been so rapid that most developing countries in 2010 have more AYS (adult years of schooling) than many European countries had in 1960, at a time at which those countries had much, much, higher GDP per capita.

Panel A of Fig. 1 shows that Kenya in 2010 had more AYS (7.3) than either the UK or France in 1960—at a GDP per capita (GDPPC) about a fourth as high (P$2379 versus over P$10,000 for the UK or France). Even a very low performing country in terms of AYS, Cote d'Ivoire at 3.7, was in 2010 not that far behind France in 1960 even though, again, its GDPPC was only a fourth as high.

Second, some claim that economists over-emphasized economic growth and under-emphasized the expansion of schooling. While this might be true, this is not what happened. In fact, the years of schooling increased by much *more* than would have been expected from economic growth alone. That is, the cross-section relationship between years of schooling and GDPPC shows the "expected" (in the strict "conditional mean" sense) growth in AYS from a given growth in GDPPC. As shown in Panel C (the southwest graph), growth in average developing country GDP would have produced an "expected" increase in average years of schooling from 3.51 to 6.05 (about 2.5 years) but the actual 2010 AYS was 8.11 (2.1 years *higher* than "expected" for the 2010 level of GDPPC). Or, put another way, a country with the *same* GDPPC in 2010 as in 1960 (zero per capita economic growth) has about 2.4 more years of schooling in 2010 than in 1960.

Of course, one could reverse that relationship and say that the average GDPPC for a country with the same level of AYS has fallen. Panel C of Fig. 1 (northeast) shows that the average country with 6 years of schooling in 1960 had a GDPPC of around P$10,000, whereas a country with 6 years of schooling in 2010 had a predicted GDPPC of only a third as high (P$3144).

However, one frames the relationship, it is just not the case that many countries expanded GDPPC but did not expand schooling, rather the opposite, many countries expand schooling quite rapidly even in the face of sluggish economic performance.

Finally, countries have had very different trajectories of accumulating schooling and increased GDPPC. Panel D of Fig. 1 (southeast) shows the trajectory of Ghana and Thailand. Ghana had a much *larger* increase in AYS than Thailand, increasing AYS from 1.1 to 7.7 versus an increase in Thailand that started from a much higher level, from 4.2 to 7.5. Nevertheless, GDPPC in Ghana in 2010 was roughly what it was in

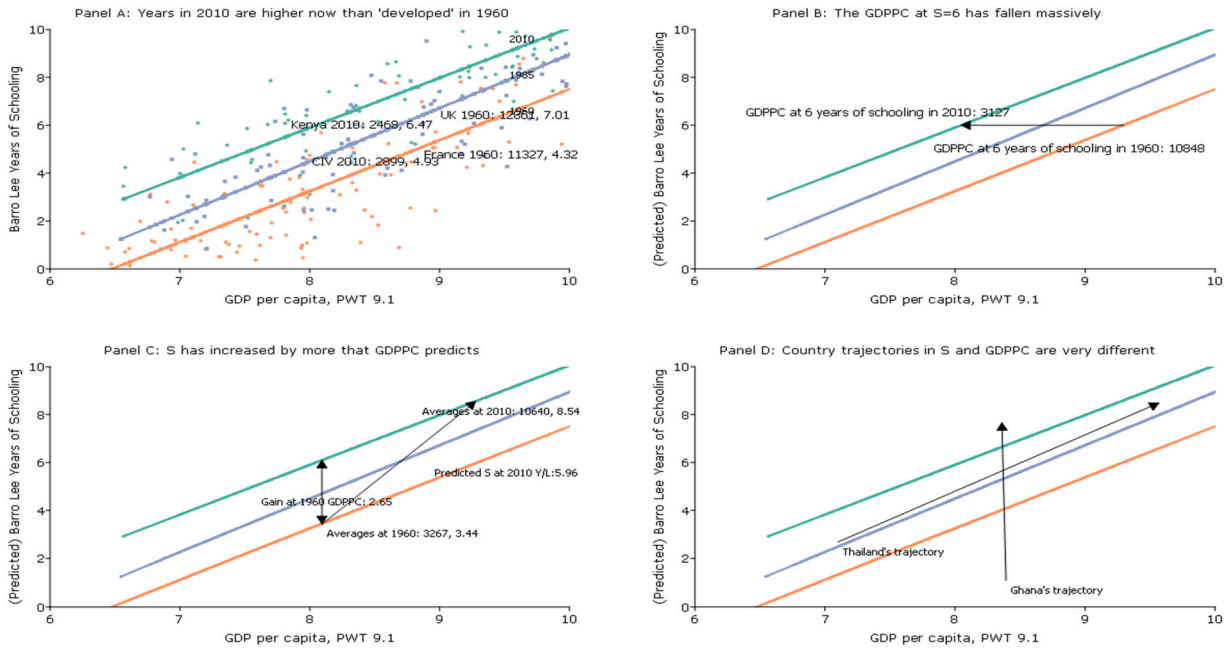

Fig. 1. The evolution of GDP per capita and adult years of schooling (AYS), schooling years completed have increased by much more than 'expected' from GDPPC. Source: Author's calculations with Barro and Lee (2011) and PWT9.1.

1960, whereas Thailand's GDPPC increased more than ten-fold between 1960 and 2010.

Often a standard part of the "why is Africa lagging?" narrative is that successful regions, such as East Asia, "invested in human capital" (parsed as "spend on school" and "expand enrollment and grade attainment") and Africa did not. This narrative has small nuggets of truth, but these nuggets are hidden in very large piles of tailings. Fig. 2 uses the standard Barro and Lee data on the average years of schooling, updated to 2015, and examines the percentage growth from 1950 to 2015 of the average years of schooling, this time focused only on those aged 25–34[3] and call this "average years of schooling of young adults" (AYS-YA).

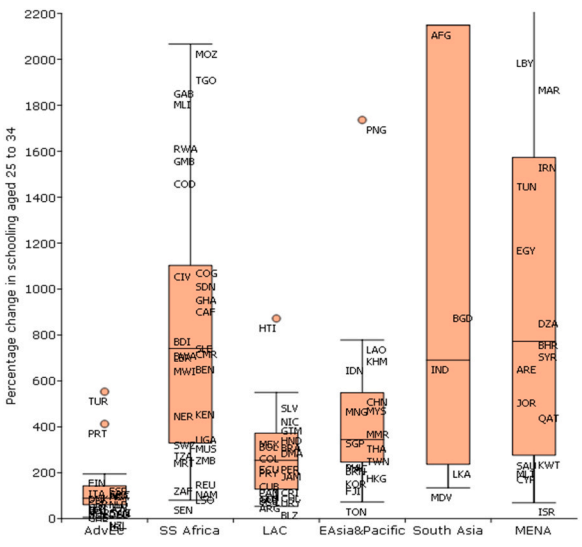

Fig. 2. The *percentage* growth in the average years of schooling of the 25–34-year-old cohort was actually faster in SSA than nearly all other regions of the world. Source: Author's calculations with Barro-Lee data on average years of schooling of young adults (25−34). Three letter country abbreviations are the ISO codes.

The *percentage* growth in the AYS-YA for the median country in SSA is *higher* than for all other regions in the world. Since many SSA countries began in 1950 with very low levels of schooling, the proportional rate of growth has been phenomenal. While most advanced countries slightly more than doubled the AYS-YA, and in the East Asia and Pacific region the AYS-YA increased by 344% (from 2.2 to 9.7), in Africa the median AYS-YA increased from only 0.78–6.64, which is by 744%—increasing by a factor of 8.

Showing the individual standard three letter country labels with the boxplot shows two additional points. One, that the percentage growth of AYS-YA was more rapid in the slower percent growth of schooling SSA countries 25th percentile), such as Uganda than in the very high economic growth countries of Korea, Taiwan or Hong Kong. This was because the base in 1950 was so much higher in those countries and hence the same absolute amount of increase was a lower percentage rate of growth. If one does the simple regression of the percentage rate of growth of AYS-YA on the initial level in 1950, there is no indication that SSA had slower percentage growth of AYS-YA than "expected" (again in the strict conditional mean sense of "expected") as the binary variable for SSA is modestly positive.

The second point is that the range of outcomes of growth in average years of schooling of the youth cohort is quite large within regions of the world, and especially within SSA. Because some countries such as Mozambique and the Democratic Republic of Congo - DRC (the ISO three letter code in the graph is COD) and Mali began from very near zero schooling their *percentage* rate of growth was over 1000% (levels increased by more than ten-fold) whereas countries with a higher base,

[3] I use this age range as the younger group of 15–24 masks the improvement of those in the OECD as many are still in school at those ages whereas the entire population is a weighted average of older and younger populations and does not reflect the increases in grade attainment as rapidly.

such as Zambia (ZMB), had a lower percentage growth. Therefore, on the face of it, it is not at all obvious there is anything distinctive about SSA as a region in the percentage growth of schooling but rather there are a number of sub-regional patterns.

Fig. 3 shifts the age group to the young (AYS-Y) and changes the time period to 1960–2015 (as many SSA nations only gained independence in the late 1950s and early 1960s and onwards). Here, one can see there are some nuggets of truth that SSA lagged behind other regions in its expansion of schooling in *absolute* terms. There has been an expansion of schooling that is massive in absolute terms—the AYS-Y of the median SSA country increased by 4.6 years from 1960 to 2015, which is a 500% expansion. However, this was slower than regions that started near the same level (e.g. MENA and South Asia) and, while it was near in absolute terms to other regions (e.g. LAC and East Asia), these regions started ahead (and the youth 15–24 age range understates their gains as tertiary is expanding in those regions).

But the most important take-away of Figs. 1, 2 and 3 together is that the recommendation "expand schooling" has been a constant of development advice since there was development advice, and that this advice has been followed in practice. There has been an expansion of enrollment and grade progression and hence grade attainment in the developing world that is massive in absolute and even more impressive in percentage expansion terms. Nearly all developing countries, and most in SSA, are at a *higher* level of grade attainment today than was the case in the typical advanced economy in 1950 or 1960 (when their incomes were much, much, higher than in SSA today).

An implication of that fundamental take-away is that, given these massive changes in years of schooling completed of the population, any claim of the type: "Expanding schooling will improve outcome Y" should be relatively easy to document as either true or to refute the claim. Two simple points about research is that: (a) any given research finding should also be able to embed an encompassing understanding of the relevant phenomena; and (b) in doing research, what leads to precision of the estimation of the association of X with Y is variation in X. Therefore, if someone says "Expanding basic education will reduce economic inequality", one should: (a) be able to reconcile that statement with a five-fold expansion in years of schooling and trends in economic inequality; and (b) recognize that the very large time series changes in years of schooling implies it should be possible to be quite precise about the association (and, since the change in economic inequality over the period has been small and mixed, have to be very small). Put another way, "expand schooling" is an experience that has been tried. Therefore, claims about the consequences of expanding schooling that were conjectural in the 1960s or 1970s (as so little expansion in schooling had occurred) need to be grounded in the reality that there has been massively expanded schooling and hence if the promised consequences have not been manifest, some re-thinking is needed before continuing with "more of the same."

4. III: The learning profile: Linking "spend on school" to "invest in human capital"

A massive limitation of the "spend on school" interpretation of "invest in human capital" is that, on a range of measures of learning, there are five key facts about the connection of "schooling" and "learning":

(i) there are massive variations across countries in the *learning profiles* (the relationship between a measure of skill/capability/competence and year of schooling or level of schooling completed),
(ii) and the typical developing (and typical African) country has a learning profile much less steep that was assumed would be the case and hence basic schooling completion did not (and does not) reliably produce basic education,
(iii) these differences in learning profiles lead to massive differences in the stock of learning from a given number of years of schooling,
(iv) there is evidence from the *descriptive* learning profiles that they have gotten *shallower* or *less steep* over time, that is, there appears to be much less learned per year of primary schooling currently that 30–40 years ago, and this downward trend is substantially larger in Africa than in other regions, and
(v) The challenge of low learning is not limited to the "marginalized" or "excluded" but affects all categories and all levels of socio-economic status.

4.1. III.A: Learning profiles: Connecting schooling ("time served") and learning "capabilities gained"

No one ever really believed that just sitting in a school year after year or even just moving from grade to grade would magically produce all of the economic (and other social and political) benefits expected from *education*. The assumption that schooling would lead to improved life outcomes has always been based on beliefs/assumptions about a "learning profile" and that the mechanism for schooling to lead to better outcomes was that in school people would learn and acquires skills, competencies, capabilities, attitudes, norms, etc. A *causal* learning profile is the connection between time spent in school and learning, where "learning" is broadly interpreted on a measure of learning outcomes from an incremental year of schooling in a specific school/classroom). Fig. 4 presents three possible hypothetical "learning profiles": high, medium, low.

4.2. III.B: There are massive variations in descriptive earning profiles across countries (literacy and numeracy)

The Demographic and Health Surveys (DHS) ask adult women (the primary respondent sampling frame is women aged 15–44), their highest completed year and level of schooling and then, only for those women whose highest schooling is that they completed primary school or less, asks each woman to read a single, simple, sentence in any language of their choice (the surveyors carry cards with the sentences in the range of languages they might encounter). The DHS data allows the construction of a retrospective, descriptive learning profile (which is *not* a prospective causal learning profile) by plotting the connection between the fraction of women who can read and the years of schooling completed by country, as in Fig. 5.

Fig. 5 illustrates that the average among the 51 DHS countries (which skew towards the very poor countries in the world) is that only about *half* of women who completed six years of schooling can read a single simple sentence in any language. Many commitments to "universal primary schooling" were premised on the notion that the learning profile was steep enough that someone who completed primary schooling was enabled and equipped with at least the rudiments of literacy (and other skills). But this is not currently true. In some countries, the rates of reading acquisition are very low. In Nigeria, for instance, only about 11% of women surveyed with grade 6 (but no higher) completed could read.

Fig. 6, from Kaffenberger and Pritchett (2021a) shows the heterogeneity in the acquisition of reading ability from schooling across countries from the DHS in a different way. For each country, we show the predicted effect of six years of primary schooling (versus no schooling) on the DHS literacy indicator scaled from 0 (cannot read at all), to 1 (can read some words but needs help to read the whole sentence) to 2 (can read the sentence by themselves). Therefore, if universal literacy (at the very limited definition of just being able to read a sentence) were achieved from primary school this would imply a coefficient of 2. Fig. 6 shows that the gain on this two-point scale of literacy ranges from very low numbers, like less than 0.5 in Ghana (GH) to about 1.7 in Rwanda (RW) whereas Kenya (KE) is near the median of 1.33.

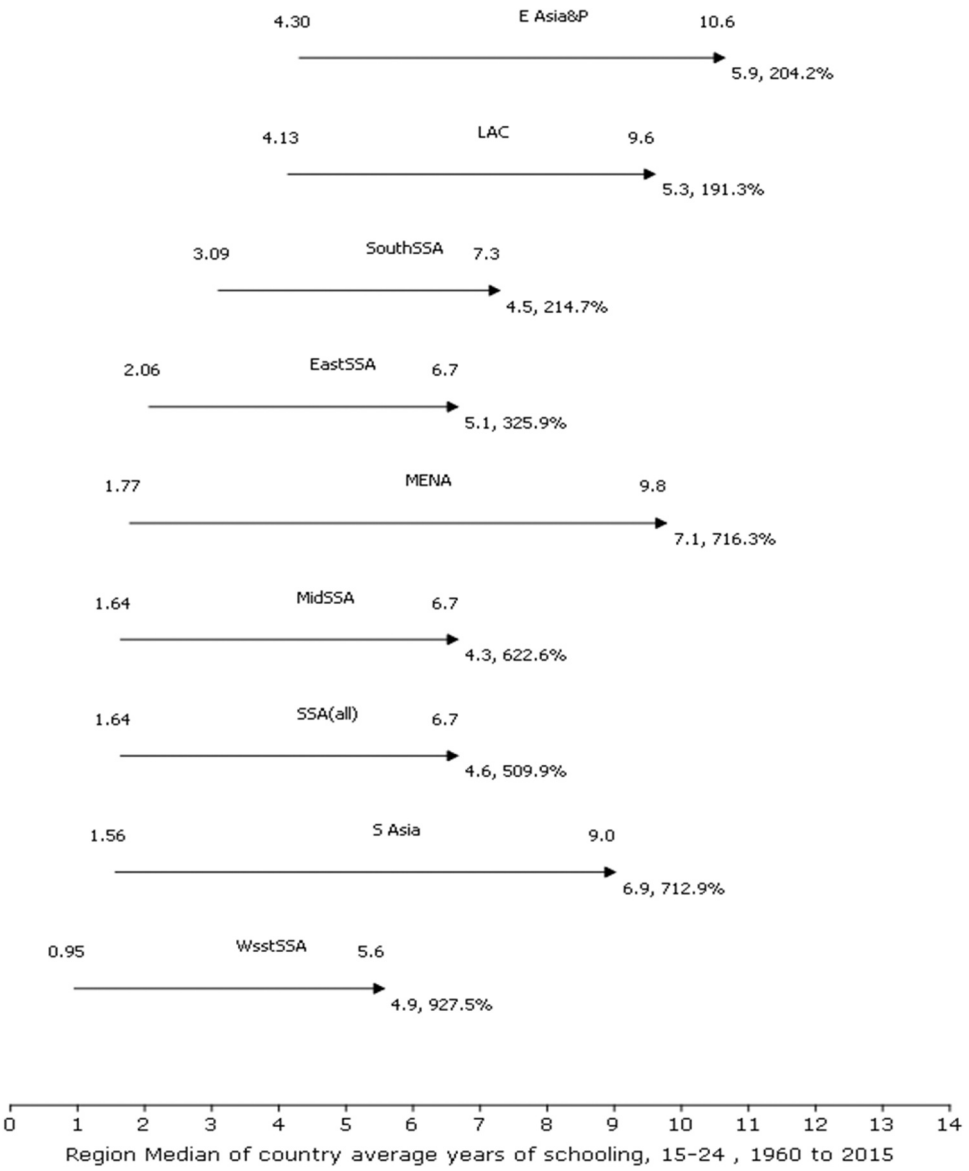

Fig. 3. Expansion in the absolute number of average years of schooling of youth 15–24 in regions and in sub-regions of SSA also shows massive expansions, but with SSA lagging. Source: Author's calculations with Barro and Lee updated data.

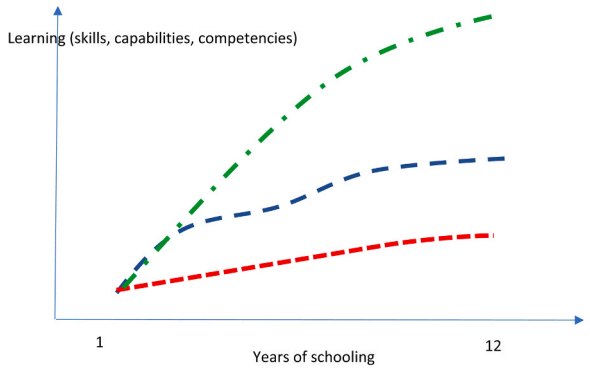

Fig. 4. Three hypothetical learning profiles illustrating possible causal connections between learning (acquisition of skills, competencies, capabilities, etc.) and schooling measured as grade attainment. Source: Author.

An important point from Figs. 5 and 6 is that *variation* across countries in the extent to which a year of schooling translates into increased likelihood of being able to read (which is the slope of the learning profile) is massive. Pretty much any learning outcome from primary school that could happen, from (near) zero impact to (near) universal ability to read, does happen in some country.

This massive variation across countries in learning profiles is also true of numeracy. The UNICEF MICS (Multiple Indicator Cluster Surveys) data measure the fraction of children reaching foundational numeracy by grade enrolled across a number of countries, which allows visualization of the descriptive learning profiles, grade by grade. Fig. 7 shows the results for 18 countries. Again, the range is huge as 70% of students in Thailand have reached "foundational numeracy" by grade 5 but 10% or less of students have done so in Togo, Central African Republic (CAR) and the DRC. The average across the 18 countries is that only around 40 percent of students have acquired even this standard of "foundational" numeracy by grade 9.

The massive range in descriptive data in learning gain per year of schooling does imply that "completed primary school" is not a very informative measure of "education." Therefore, one cannot measure

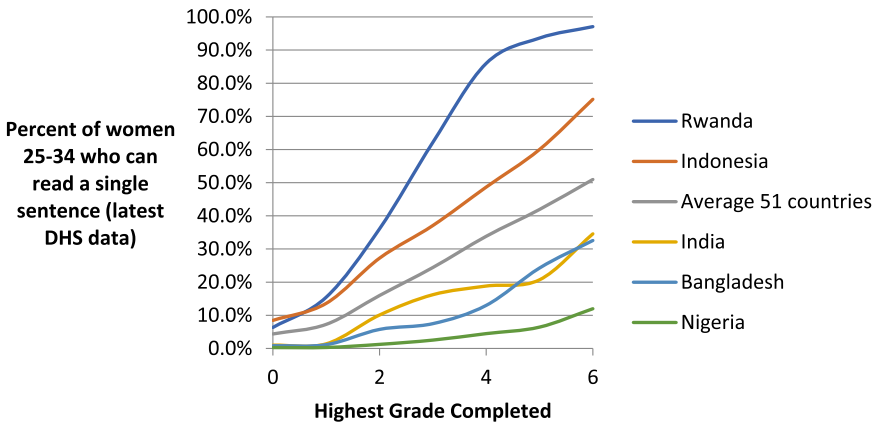

Fig. 5. Learning profile of reading and years of schooling, DHS data, selected countries. Source: Adapted from Pritchett and Sandefur (2017).

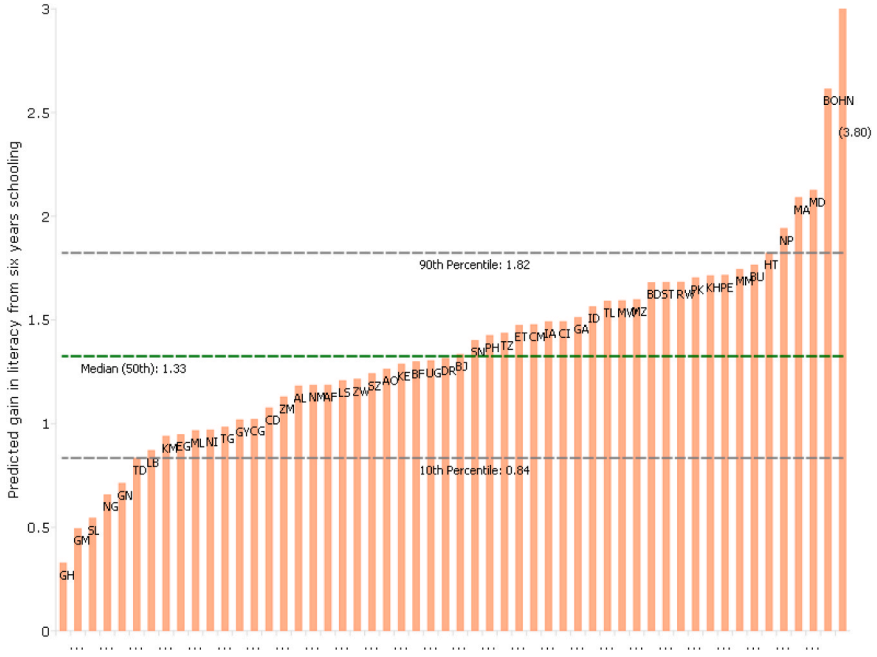

Fig. 6. The predicted gain from six years of schooling on a 0 (cannot read), 1 (read with assistance), 2 (read without assistance) scale of reading a single sentence (in a language of the surveyed woman's choice) ranges across countries from very low (very unlikely a woman gains literacy) to very high (nearly every woman gains literacy). Source: Kaffenberger and Pritchett (2021b).

country's commitments to, or achievements of, "education" (which implies gains in learning) solely from data on progress in "schooling."

4.3. III.C: Measured stocks of learning are low in Africa

There have been two recent attempts to measure cross-nationally comparable levels of the performance on assessments of cognitive skills of enrolled students in secondary school across nearly all countries. The technical challenge is to create a "concordance" that reliably maps assessment outcomes on one instrument, such as PISA to another instrument, such as the assessments used in the African regional assessments such as SACMEC or PASSEC and to estimate learning for those countries with no assessments.[4]

The World Bank's Human Capital Index (HCI) is one such effort. Table 1 shows the "Harmonized Test Score" from the October 2018 version of the Human Capital Index. Like most comparisons of learning outcomes, it adopts as a numerical "norm" (usually for the OECD countries) for the assessment of 500 (this is just a norm as the numerical level of any assessment is arbitrary). The table shows both the level and heterogeneity across SSA compared to other developing countries. On average, SSA is modestly behind other regions (a median of 373 versus 407), but there is substantial heterogeneity across SSA as the 25th

[4] A third effort, Patel and Sandefur (2020) uses a different method to establish the link between regional and global assessments and, rather than linking assessments via overlapping countries gave an assessment containing test items from the various assessments to a sample of students in India. This created a "Rosetta Stone" linkage directly between assessments. This limits itself only to those countries with an assessment.

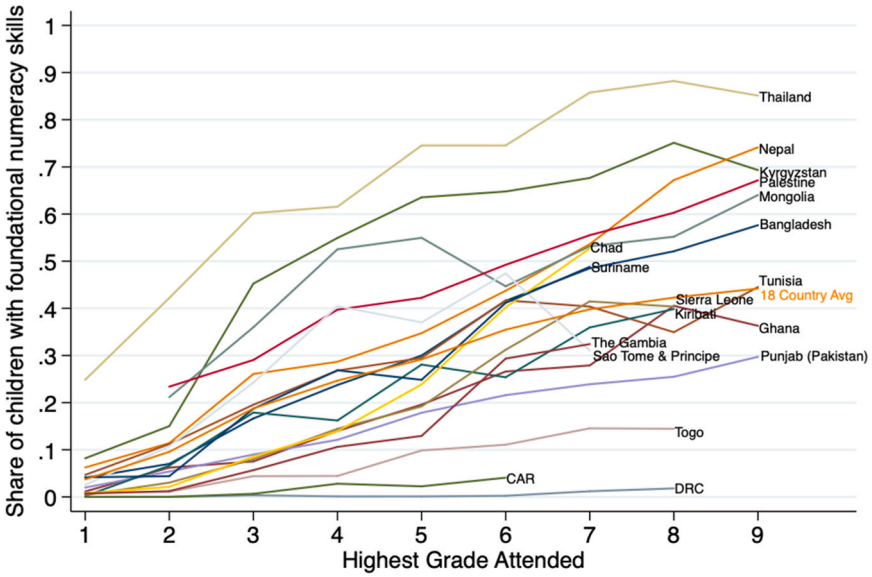

Fig. 7. There are also massive differences across countries in the descriptive learning profiles of currently enrolled students for foundational numeracy. Source: Silberstein (2021) based on UNICEF MICS6 data.

Table 1
The "Harmonized Test Score" from the World Bank Human Capital Index.

	SSA (40 countries)	All other developing (68 countries)
Lowest	305	321
25th percentile	338	368
Median of SSA	373	407
75th percentile	397	436
Highest	473	538

Source: https://datacatalog.worldbank.org/dataset/human-capital-index

percentile is 338 and 75th percentile is 397 so the within SSA 25th-75th range of 59 points is about twice as big as the average gap between SSA and other developing countries (34 points).

Another very recent effort at estimating learning outcomes across the world is Gust et al. (2023), which uses a different set of methods than the World Bank HCI to combine the existing international and regional efforts of student assessment, and uses different techniques to extrapolate to countries, which lack assessments. They combine this measure of "achievement of enrolled secondary students" with estimates of enrollment rates in secondary to create an estimate of students in a cohort achieving "basic skills" using a definition of the threshold for "global universal basic skills" that roughly corresponds to the Sustainable Development Goal - SDG 4 standard or achieving PISA level 2.

Table 2
Only six per cent of youth in Sub-Saharan Africa are reaching a threshold of "global universal basic skills" in math and science.

By region (and sub-regions of Africa)	Fraction of enrolled students in secondary education *not* reaching basic skills	Fraction of youth *not* enrolled in secondary education	Fraction of youth *not* reaching basic skills
World	0.617	0.355	0.657
Sub-Saharan Africa	**0.893**	**0.665**	**0.941**
East	**0.878**	**0.630**	**0.925**
Middle	**0.830**	**0.650**	**0.887**
West	**0.886**	**0.649**	**0.935**
Southern	**0.932**	**0.664**	**0.969**
South Asia	0.85	0.402	0.892
Middle East & North Africa	0.639	0.195	0.679
Latin America & Caribbean	0.612	0.21	0.652
Central Asia	0.4	0.094	0.421
East Asia & Pacific	0.252	0.219	0.291
Europe	0.259	0.102	0.284
North America	0.222	0.069	0.239

Source: Gust, Hanushek and Woessmann (2023), Table 2, sub-regions of Africa are author's calculations with data in Appendix Table A4

Table 2 in column 1 shows the fraction of enrolled *not* reaching basic skills. This varies from 22% in North America to 89% in SSA. This, combined with the fact that the net enrollment rate in secondary school is still only about 33% in SSA implies that 94% of youth in SSA are *not* reaching basic skills at around age 15.

This varies somewhat across the sub-regions of Africa as in, but not by much, as the best region is "middle" Africa where 88.7% (this region includes oil rich Gabon) do not reach and the worst region is Southern where 97% of youth do not reach basic skills. But even within these regions there are large variations, as for instance East Africa includes Kenya where 68.7% of students (those youth enrolled at age 15) do not reach basic skills and Zambia where 96% of enrolled students do not reach basic skills.

One suspects two-way causal association between learning outcomes and the level of GDP per capita (as higher learning allows higher output and higher output allows for higher learning outcomes). Fig. 8 shows the association of the average score of enrolled students from GHW (2023) and 2019 level of GDP per capita (PPP adjusted). The estimates use a quadratic functional form in GDPPC, which allows for a flexibly non-linear association. Fig. 8 shows that measured learning increases strongly with GDPPC but levels off at around P$30,000 (of course there is complex bi-directional causality between schooling, learning, and growth (Pritchett, 2001, 2006, 2024, Hanushek and Woessmann, 2012, Angrist et al., 2021).

Fig. 8 shows that most SSA countries are below even the predicted level of learning for the GDPPC. If one only allows a binary variable for SSA, the estimate is −40 points (on the 500 point scale). The box and whisker plot within Fig. 8 shows the distribution of the residuals from the GDPPC regression for SSA countries—how much countries are above or below the "expected" level for their income, separately by SSA sub-regions.

There are a number of East African countries who are (conditional on GDPPC) high learning performers (e.g. Tanzania, Kenya, Burundi) and the estimated regional dummy for East Africa is only −6 points (and not

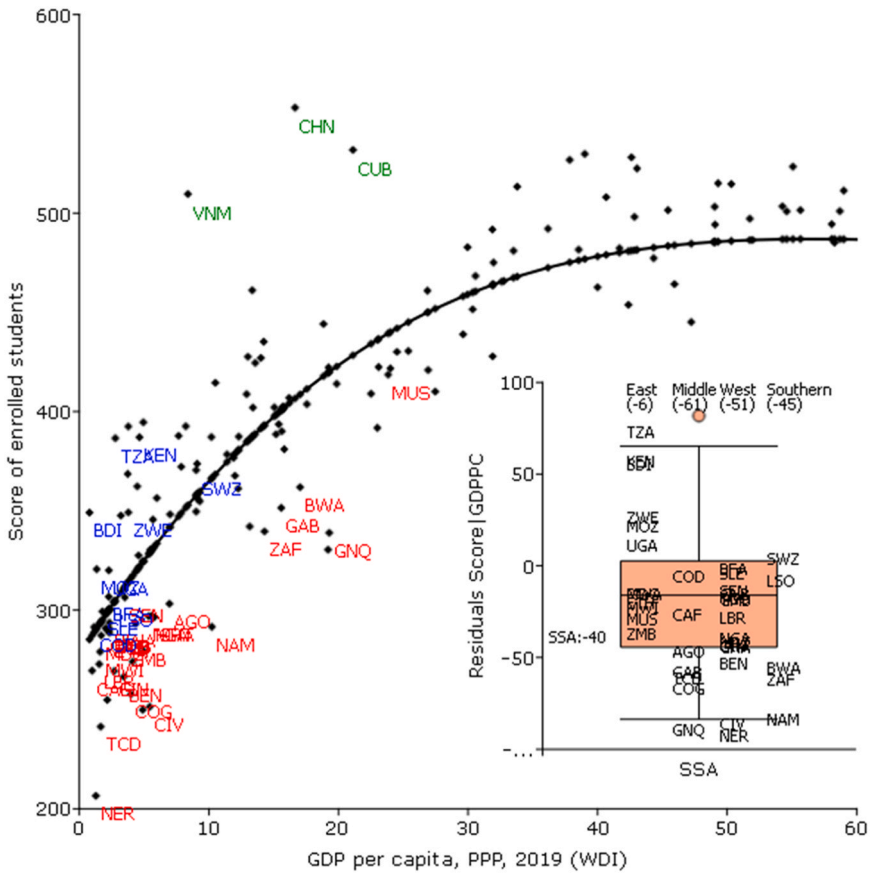

Fig. 8. Even adjusted for GDP per capita, Africa still modestly under-performs on measured math and science achievement, but with strong differences across regions and within regions, *Notes. Africa countries above their "expected" learning conditional on GDPPC are shown in blue, those below are shown in red. In the box and whisker diagram, the estimated dummy variables for SSA and for each region are reported in parenthesis. Countries above their predicted level are show in blue, those below are shown in red. Source*: Author's calculations with data from Table A4 of Gust, Hanushek and Woessmann (2023).

statistically significant), suggesting East Africa's learning performance is at par with other countries of similar GDPPC.

The other sub-regions of Africa are lower than conditionally expected based on GDPPC (between 45 (Southern) and 61 (Middle) points lower). Some countries with relatively high GDPPC, such as oil rich Gabon, have scores that are absolutely higher than many other SSA countries, but low for its income level.

The variance across countries is massive, even controlling for GDPPC is massive. South Africa (ZAF) has GDPPC three times higher than Kenya (P$14,269 vs P$4641) but, perhaps not surprisingly given its history of racial discrimination and inequality, South Africa has lower learning performance, on average, than Kenya (339 vs 387).

One other noticeable feature is that highlighted in green; three countries outside of Africa that are very high for their level of income. For instance, Vietnam has estimated learning performance of over 500 although, even after two decades of rapid growth, its GDPPC in 2019 was still only P$8381 (and started in the 1990 s with a level of GDPPC similar to African countries).

4.4. *IIII.D: There has been a long-term deterioration in learning outcomes at primary schooling*

Nestour et al. (2021) have used the fact, as described above, the DHS surveys measures reading and the fact that the DHS sample includes women of very different ages (their primary samples are of women of child bearing ages, 15–44) and that there are various waves of the DHS across the years for many countries to estimate how the likelihood a woman with five years of schooling complete can read. Using the different years of DHS allow them to adjust for cohort versus generational effects as there are multiple estimates for the same cohort—e.g. the cohort of women aged 25–30 in a survey in 2000 are aged 30–35 in a survey in 2005 and 35–40 in 2010). This allows something very important and unique, which is to measure the trend over very long periods of the likelihood that schooling produces literacy (reading). This is one possible *descriptive* measure of "school quality" (although of course the socio-economic composition of those in school and completing exactly grade 5 has changed).

Fig. 9 shows how Africa has performed relative to non-African countries in the expansion of women's enrollments (those who complete grade 5 or higher) (on the horizontal axis) and on the improvement/deterioration in whether primary school produces the ability to read (on the vertical axis). As the period spanned by the DHS data is different across countries, we take the annual rates of change over the available period for each country and estimate what the 35 year (the typical span in the sample) gain/loss in grade five or higher schooling and literacy for those completing grade 5 would have been of 35 years at each country's annual rate.

This figure shows that the average expansion in fraction of women with more than five years of schooling was very similar between the SSA and non-SSA countries, with both expanding by about 20 percentage points. As emphasized above, "spend on school" has produced massive expansion in grade attainment.

However, the fraction of women with grade 5 (but no higher) complete who can read *fell* substantially on average over the years covered by this data (from women born in the 1950s/early 1960s to women born in the mid to late 1990s and hence attending schooling in the 2000s. Across SSA, the fraction fell by about 13 percentage points, versus only about 6 percentage points in non-SSA countries (the DHS data is focused

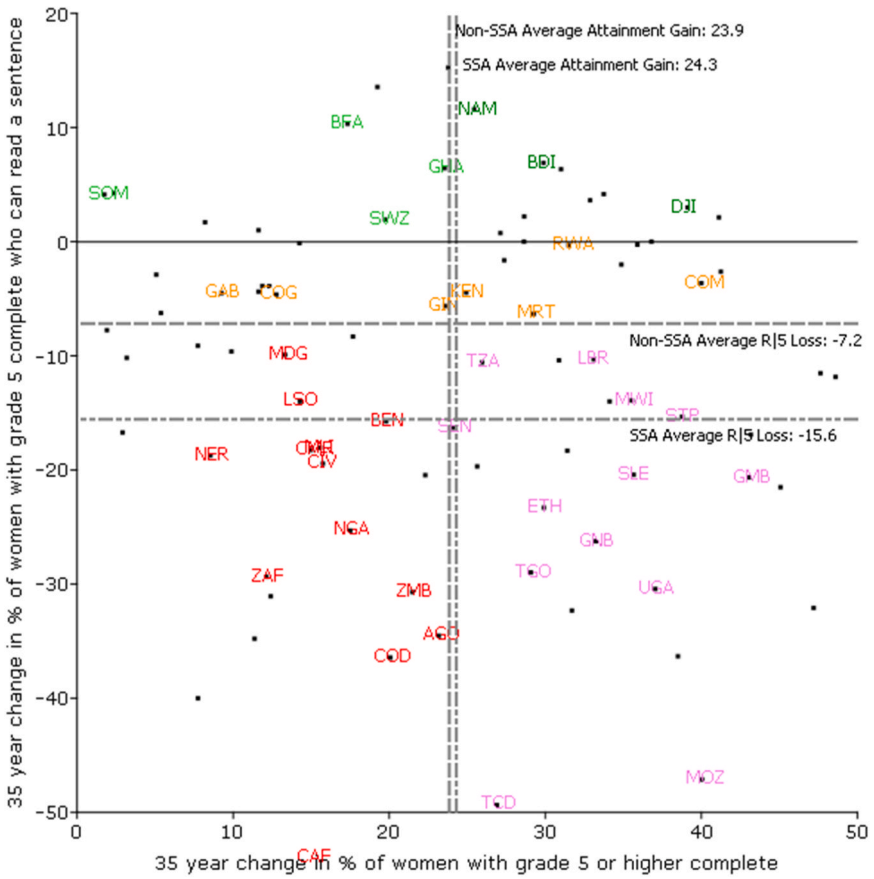

Fig. 9. The likelihood that five years of schooling produced the ability to read has *declined* over time, and massively so in some countries, *Notes: Data for all countries is shown with a symbol. Three letter ISO codes shown for SSA countries shown with color coding: Red: below non-SSA avg. on learning loss and expansion, Violet: below non-SSA avg. on learning loss, above on expansion, Orange: negative learning loss but better than non-SSA average, green: better than non-SSA average on expansion and positive learning gain, lime green: positive learning gain, less than non-SSA on expansion. Source: Author's calculations with data from Nestour, Moscoviz, and Sandefur (2021).*

on poor countries and hence not globally representative).

Fig. 9 has the country three letter acronyms color coded to show the large variation in outcomes across SSA.

In shades of red are those SSA countries with a *larger* learning loss than the non-SSA country average. In darker red in the "southwest) of the graph are those countries (e.g. Nigeria (NGA), Cote d'Ivoire (CIV), Niger (NER)) which have *both* less expansion in schooling *and* larger learning losses. In the lighter shade of red (in the "southeast" of the graph) are countries that had larger learning losses than average in non-SSA countries, but had larger expansion than average (e.g. Uganda (UGA), Ethiopia (ETH), Mozambique (MOZ)).

In orange are those SSA countries where learning fell, but by less than the non-SSA country average (e.g. Kenya (KEN), Rwanda (RWA)).

In shades of green are SSA countries where the learning did not fall. There are only seven SSA countries in which, on this measure, learning did not fall, and only three (all small) African countries in the "north-east" with both expansion in grade attainment higher than non-SSA country average and positive gains in learning: Namibia (NAM), Burundi (BDI), and Djibouti (DJI).

A common reaction to the *descriptive* facts about learning losses (adults with grade 5 complete less likely to read) over long periods of time shown in Fig. 9 is to argue that the *causal* explanation of this fact might not be a deterioration of learning conditions in schools but rather that the observed fall is simply due to the changing composition of women who completed grade 5 as the education system expanded enrollments. That is, perhaps learning of those with grade 5 complete fell because those who achieved exactly grade 5 (or any given grade) in school from the birth cohorts in the late 1990 s were very different from those women attending school and achieving grade 5 in the 1950 s and that student composition had a harder time learning. While this, in principle, is a possible factor, there are two important points.

One, if this "compositional" shift were a major explanation of the observed descriptive fact of learning falls, one would expect that the magnitude of expansion of attainment and the learning loss would be strongly correlated across countries (as countries with large expansions would have large learning losses)—but this is not true. There is only a very small (and not statistically significant) association of magnitude of expansion and learning loss. The dots for country observations in Fig. 9 are clearly not in a strongly downward pattern (with larger grade 5 attainment expansion countries having larger observed falls in learning).

Two, SSA countries generally have more learning loss at any level of expansion—there are more countries in the "south" (large learning losses) of the "west" (low attainment gains) and more SSA countries in the "south" (large learning losses) in the "east" (large attainment gains).

This very new information on the long-term changes in an important measure of learning is very important. Prior to this pioneering study, one could have assumed that the low performance of African countries on learning was a "constant" and just a chronic "feature" of Africa. But, instead, this study shows that the current low levels of learning are, to a large extent, because of larger *falls* in the learning in SSA over the decades than other countries.

For instance, in Fig. 8 above, relating learning levels and GDPPC one can see that Zambia (ZMB), at about 250, lags far behind Vietnam at about 500. These current levels could be because Zambia has just always been behind Vietnam, with both on the same long-term trend line. But in this study with DHS data, we can illustrate in Fig. 10 that the gap in learning was much *smaller* in the past (61.2% in Zambia, 72% in

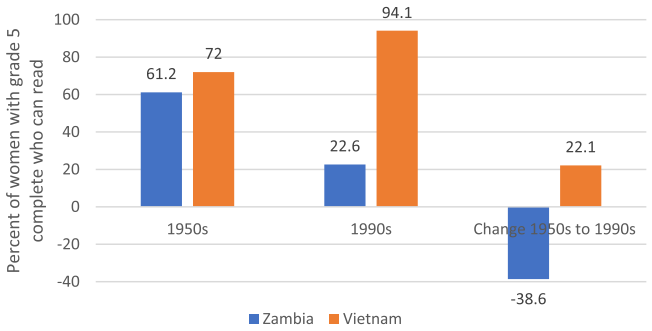

Fig. 10. The gap in learning between Zambia and Vietnam was small in the 1950 s and large in the 1990 s because learning *fell* in *absolute* terms in Zambia and *rose* in Vietnam. *Source: Author's calculations with data from Nestour, Moscoviz, and Sandefur (2021).*

Vietnam) and the has grown because Zambia's fraction who can read at grade 5 completion has *deteriorated* in absolute levels over time (to only 22.6% in the most recent cohort) whereas Vietnam has improved to reach nearly universal (94.1%) level.

This new information about the long-term evolution in learning is also important in framing the discussion of human capital in Africa and the challenges in improving learning outcomes. The default assumption should be that "more of the same will produce more of the same" (though there are exceptions). More of the same of the "spend on school" parsing of "invest in human capital" should be expected to produce: (i) more years of schooling completed; (ii) mixed (at best) contribution of these more years of schooling to sustained episodes of rapid growth; and (iii) very different, usually low, and declining (at best stagnant) levels of learning for those enrolled.

4.5. III.E) The low level of learning outcomes affects the elite as well

One last fact about learning outcomes in SSA is that while there is a great deal of legitimate and important attention to expanding access and learning for "marginalized" groups, the challenges of globally low levels of learning affect the elite as well. While this point can only be illustrated with precision for the two SSA countries that participated in PISA-D, the problem is quite general. There are three important facts.

First, we (Pritchett and Viarengo, 2023) can estimate the absolute number of children in a "learning performance elite"—how many children in the cohort of those aged 15 are reaching PISA (Programme of International Student Assessment) level 4 or above. This level is achieved by about 30% of youth in the OECD, which is roughly the fraction of youth who advance to a four-year university education in those countries and therefore one can think "how many children are on a path to enter a (global quality) university education?" The answer is that for Zambia, the PISA-D estimates suggest there are single digits of individuals for Math, Reading, or Science. In Zambia of the 360,000 15-year-olds in a given year, these results suggest one can count literally on one hand those reaching PISA level 4 or above. In Senegal, there are at most a few hundred youth in the learning levels of PISA 4 or above in any of the three domains of learning assessed.

This seems like a vitally important issue for a country's progress that goes largely unremarked in discussions of human capital. Many African countries are relatively small and have quite low learning performance, on average, and do not in fact have notably higher inequality in learning outcomes. Countries such as Vietnam have, each year, a quarter of a million students in the global "learning performance elite" because it is a large country with high learning performance. Indonesia has, across the three learning domains, between 50,000 and 100,000 students per cohort because, while it has mediocre learning performance, it is a very large country. While it is important for an education system to produce mass access and universal basic education, education systems also play the role of producing those who go on to fill the professions: engineers, scientists, doctors, accountants, and who go on to civic and thought leadership and into political leadership. It is hard to imagine how African countries can cope with the many challenges of national development in a globalized world without a larger and more robust learning performance elite. (Table 3)

While we can estimate this precisely and comparably only for the two PISA-D countries, this point applies more generally as the combination of very low average level of learning and inequality in learning outcomes that is about the same as other countries implies the upper tail (e.g. 95th or 99th) percentile of learning outcomes is also very low - a point comparisons using the micro data that estimates the whole distribution of learning has emphasized (e.g. Crouch and Rolleston 2017; Patel and Sandefur, 2020).

The second important fact is that the average learning performances even of children from advantaged and socio-economically elite conditions who are enrolled in public schools have very low levels of learning. While it is the case that children from more "marginalized" conditions—rural residence, non-native speakers of the language of instruction, migrants, girls—on average have lower learning and children from lower SES (socio-economic status) households do worse, it is not the case these differences are so large that this implies the advantaged SES elite children are doing well. In Pritchett and Viarengo (2023), we estimate that the average PISA result on Math for an advantaged (male, urban, native speaker, non-migrant) and SES elite student in Zambia is 332 and in Senegal is 311. This is on the PISA scale where reaching the SDG *minimum* level of learning is reaching 417. Therefore, even if all schooling and learning gaps in these two countries were *completely* eliminated on *all* of these five dimensions (sex, rural residence, mother language, migrant status, SES) it is still the case that less than 20% of children would reach the SDG learning goals.

The third important fact, which is much harder to interpret, is that the gradient of learning with respect to SES is *not* larger in the SSA countries than even in the high performing and socially equal countries such as Denmark and Finland. That is, of children enrolled at age 15 and participate in the PISA assessment, there are SES gaps for all countries and these gaps between "rich" and "poor" children are, in the two SSA countries absolutely smaller, even relatively smaller, in Zambia than in the developed countries. The reason this is hard to interpret is that in the OECD countries, enrollment is nearly universal and there is no "selection" on SES in being enrolled at age 15 whereas there is in the SSA countries. Therefore, part of the reason for the small SES gaps in learning of the enrolled is the differential enrollment, so that only relatively well performing children from low SES HHs in SSA stay in school until age 15.

But, overall, it is just not factually correct to characterize the low levels of learning performance of those enrolled in SSA as the result of a lack of inclusion, or marginalization, or inequality. There is a very small global learning performance elite and even the advantaged SES "elite" in public schools are receiving an education well below the global minimum levels of the SDGs.

5. IV: Elements of a "invest in human capital" versus "spend on school" research agenda

Sections I, II, and III show that SSA, by and large, has had massive expansions in access, enrollment, and grade attainment since independence, and that whether SSA was "fast" or "slow" in expanding schooling depends on whether one takes absolute gains or percentage gains. that "SSA lagged in expanding schooling" is not robustly true). We do show that, across countries, and even adjusting for level of income, the measured level of cognitive skills acquired from schooling in Africa is low and, moreover, there is evidence that in many countries, on some measures of learning, it has been getting worse for many decades.

The obvious, but to my mind, not yet fully acknowledged, implication is that if most SSA countries continue with their same conceptual,

Table 3
The estimated "learning performance elite" is very small in two SSA countries (Zambia and Senegal).

Country	Total number of 15 year olds in country	% taking PISA	Mathematics		Reading		Science	
			% at PISA Level 4 or above (>544.7)	Estimated total 15 year olds at PISA Level 4 or above	% at PISA Level 4 or above (>552.9)	Estimated total 15 year olds at PISA Level 4 or above	% at PISA Level 4 or above (>558.7)	Estimated total 15 year olds at PISA Level 4 or above
Zambia	360,000	36.0	0.0039	5	0.004	5	0.0017	2
Senegal	337,636	29.0	0.351	344	0.197	193	0.015	14
Cambodia	370,856	28.1	0.103	108	0.004	4	0.000	0
Paraguay	135,869	55.6	0.048	37	1.325	1000	0.198	150
Guatemala	387,167	47.5	0.077	141	0.695	1276	0.096	177
Honduras	193,268	41.4	0.649	519	1.172	937	0.339	271
Ecuador	352,702	60.6	1.174	2508	4.231	9038	1.414	3021
Denmark	68,174	89.0	35.0	21,249	28.4	17,255	27.2	16,492
Vietnam	1803,552	48.5	27.5	240,605	18.5	161,466	32.1	281,245
United States	4220,325	83.5	20.6	727,777	30.1	1060,945	27.6	973,884
Indonesia	4534,216	68.2	3.42	105,742	2.04	63,070	1.68	51,858

Notes: "Total number of 15 year old in country" refers to the number of individuals who are 15 years old in the country; "Percent taking PISA" refers to the coverage rate of the PISA sample with respect to the total population of 15-year-olds; "Percent at PISA Level 4 or above (>544.7)" refers to the share of students who take the PISA test and perform at a Level 4 or above of the PISA proficiency scale; "Estimated total 15 year olds at PISA Level 4 or above": absolute number of 15 year old in country who perform at a Level 4 or above of the PISA proficiency scale
The methodology to estimate the total number of 15 year olds at PISA Level 4 or above consists of the following calculation: "Total number of 15 year old in country" * "% taking PISA" * "Percent at PISA Level 4 or above". The table includes countries that participated in PISA-D and as comparators countries that participated in PISA.
Source: Table 2 *of* Pritchett and Viarengo (2023) *using PISA-D and PISA Database;* OECD (2018) *PISA Results,* Table 3,*9 (Reading), 30 (Mathematics), 51 (Science)*

policy, and practical approaches in their education systems, the mostly likely expectation is that grade attainment levels will continue to rise and learning levels will continue to fall (or at best stagnate). From "business as usual" one should expect "more of the same": it is hard to see how anyone would expect that the same institutions, laws, policies, programmes that have produced the outcomes we now observe would somehow begin to produce very different results from minor, project tweaks. As Albert Einstein (is reputed to have) said, "Problems are not solved at the same level of thinking that created the problem."

I suggest three very different approaches to research that, as they are different, at least might produce the kinds of findings and recommendations that could make a difference in accelerating progress in creating learning from schooling.

5.1. IV.A: Integrate learning outcomes directly into measures of expanded education

The first and obvious (and already partially adopted at the global cutting edge) is to stop using a "year of schooling" as the unique summary statistic of policy or programmatic actions. That is, some studies (explicitly or implicitly) take as their goal the estimation of a LATE (local average treatment effect) where the "effect" of the treatment is measured in "additional years of schooling." But once one acknowledges that the gain from learning from a year of schooling (of any type of learning, whether cognitive or non-cognitive) differs massively across countries, across schools, across individuals, across grades (and across grades by individual); that is, that the slope of the causal learning profile (incremental gain from a year of schooling) is wildly different, then it no longer makes any sense to judge a policy or programme or project solely by whether it produced an additional year of school.

This is important because additional schooling is typically justified instrumentally on the grounds that it increases the well-being of the individual who receives more schooling. But there is no reason to believe that the gain to well-being from a year of schooling is invariant with respect to what happens during that year of schooling.

For example, suppose one believed that women's education was important in expanding women's well-being because it led to greater women's empowerment and that claim was backed up by evidence of a relationship, perhaps even a credibly established causal relationship from some country or context, between years of schooling completed and empowerment. But suppose that in country Y, where the LATE of schooling on empowerment was established with evidence, the learning profile was steep and in country X, where one is contemplating a programme to extend women's schooling the learning profile is flat. Then there is zero reason to believe the impact of a "year of schooling" in country X is going to produce the same impact on female empowerment as it did in country Y as the LATE alone does not reveal the intermediating causal mechanisms, one of which could be, and plausibly is, the amount learned from the year of schooling.

This concern that the LATE of additional schooling and the LATE of additional learning are not the same is far from hypothetical. For instance, a recent study evaluated the impact of two different modes of targeting girls for scholarships. One program design targeted girls who were from poor households, a different program design targeting girls who were high academic performers. A short-term evaluation showed that both approaches increased enrollment. However, a long-term follow-up found that the "poverty" targeted scholarship increased schooling but had no detectable effect on cumulative learning—and no detectable effect on any measured life outcome for the targeted children. But the "achievement" targeted recipients had more schooling and had more learning and had, on some measures, better life outcomes (Barrera-Osorio et al., 2018).

The very famous conditional cash transfer programme in Mexico with the RCT impact evaluation that showed the conditional cash transfer (CCT) expanded grade attainment also showed that the additional grade attainment did not lead to significantly greater learning. In a study of different modalities of giving scholarships, an RCT had two different treatment arms, one of which targeted children based on their poverty status and one of which targeted children based on their academic performance. Just to be clear that this criticism has "edge", we would argue it implies that nearly all of the research on "conditional cash transfers" has, from a point of view of "human capital" been largely a waste as researchers devoted the latest RCT techniques to examining the LATE on enrollment/grade attainment without any real attention to whether this increased "human capital" versus just "time served" in school. One can make the argument that this was even worse as it shifted attention from where it should have been—"are schools effective at creating useful learning?"—to act as if parent and student decisions were the main problem, thus shifting accountability from where it should have been.

Moreover, if dropout or schooling transition decisions are endogenous to academic performance and to anticipated learning (and optimally they should be, and there is strong empirical evidence in some circumstances they are) then the children induced to stay in school by programmes that reduce costs of schooling are those who are the least likely to learn. In contrast, programmes that induce additional enrollment by raising anticipated learning induce the incremental enrollers on the basis of increased learning benefit. There is no plausible economic theory that suggests that those two acquired "years of schooling" should have equal impact on well-being outcomes.

A simulation model that allows for concave learning profiles (learning declines by year) and endogenous dropout shows that programmes that expand enrollment without changing the learning profile could have massive impacts on years of schooling and next to zero impact on learning (Pritchett, 2021).

At the aggregate level, a recent paper from Indonesia has shown that it is possible to have very large expansions in schooling attainment and yet, if the learning profile (i) shifts inwards and (ii) is flat in the later years, one can have no change at all in the cognitive skills of a cohort. Beatty et al., 2018 show that between 2000 and 2014, the fraction of the cohort of youth 18–24 completing senior high school (grade 12) increased by about 20 percentage points and yet the measured mastery of simple primary school arithmetic of the cohort actually *fell.* One reason was that the learning profile shifted inwards (less learning per year), such that the average seventh grader in 2014 only had the same competence in arithmetic that fourth grade had in 2000. Fig. 11 explicates the point that the LATE on schooling of two programmes could be exactly the same, but if the learning profile of one treatment, say, "A" were flat and the learning profile of another intervention, which raised the learning profile for the student was "B", then the LATE measured exclusively on S would risk getting policy questions completely wrong. Suppose that all of the benefits from an additional year of S were from the learning acquired (on a very broad measure of learning), then one could find that the "cost-effectiveness" of A in expanding schooling was high but the "value effectiveness" was zero. And supposing that intervention B, which improved learning, was (much) more costly than a treatment that just kept kids in school, then it would appear on a comparison of just LATE(S) that A was more "cost-effective" (same gain in S for less cost) but the "value effectiveness" or "human capital creation" effectiveness would be much higher for B than for A.

This pervades the analytics of alternative interventions as if we think that a child stays enrolled for an additional year if the marginal benefit is greater than the marginal cost (MB>MC), then one can either reduce costs or increase benefits. Benefits that lower marginal cost (e.g. reduction of distance, reduced tuitions, free schooling, or conditioning cash transfers) can induce children to remain in school even if marginal benefit is low (or, in the case of CCTs if the MB is zero, or even negative). Interventions that induce additional years by raising benefits are less likely to simply push children out a flat (or nearly flat) learning profile, but measuring programme impact in research only by measuring its gain in years attended cannot reliably be used to differentiate the true cost-benefit of alternative interventions.

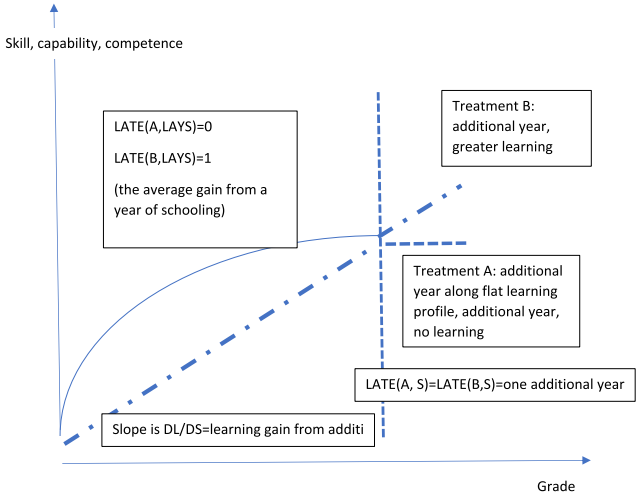

Fig. 11. The LATE of enrollment alone and the LATE of "learning adjusted years of schooling" can be very different for different programmes (or different treatment arms within programmes) and future research needs to acknowledge that difference. *Source: Author, as described in text.*

The latest summary of evidence on education from the Global Education Evidence Advisory Panel uses a "year of learning equivalent" measure to compare different "interventions", as some may increase years of schooling and some may increase learning per year of schooling. In adjudicating "cost effectiveness", one needs a common unit. This exact measure is almost certainly flawed in various ways (as it implicitly assumes all of the benefits of school come through standard measures of learning) but is a large step in the right direction. The World Bank's Human Capital Index at the country level also adjusts the "human capital" for not just years of schooling but also for the learning per year, again, a step in the right direction in the aggregate "macro" measurement of human capital (Angrist et al., 2021).

5.2. IV.B: A system approach to school

A central question for research motivated by the facts above, is: "why in some countries (regions) are learning profiles steep (and improving) and hence children acquire skills at an acceptable pace and hence nearly all children emerge from basic schooling prepared for their adult roles, while in other countries there are shallow learning profiles (and falling?) and even completing basic schooling does not produce the needed foundational skills?"

The key difference between a "spend on school" and "invest in human capital" approach to school is that in the "spend on school" approach, the performances of schools are seen as *organization* level issues that can be handled as (more or less purely) *technocratic* and *management* issues. That is, there is the (largely incorrect) view that the efficacy of the schooling system at achieving the goal of:

(a) every child attending a school,
(b) schools implement effective teaching and learning practices;
(c) schools are inclusive

is primarily (exclusively?) about the operation of a Ministry of Education and the "policies" it adopts and the Ministry implementation of those policies.

Moreover, it is nearly always assumed that organizational success in creating effective education can be reduced to a "logistical" task based on "thin input" and "process compliance", of the type a typical Weberian bureaucracy is capable of producing. In this view, success at *education* can be achieved by more effective organizations more or less autonomously by achieving higher levels of the "proximate determinants", which are "thin inputs" of learning.

In complete and sharp contrast, the *system* view sees the outcomes of a country's schooling as the result of the *endogenous* operation of a system, within which a Ministry of Education (and/or private schools) are embedded as one (important, but just one) element. In a system view, the "proximate determinants" of effective learning in schools and classrooms are the *endogenous* result of how the entire system operates, not an fully autonomous and exogenous "choice" of an organization (and much less the choice of a single "leader" such as a Minster of Education).

The need for a system approach to research on improving learning is three-fold:

First, the "proximate determinants" approach, especially that based on the standard measures of "thin inputs" such as class size, input

availability, infrastructure quality, and formal qualifications of teachers, cannot really explain much of the observed cross-national differences in learning outcomes. For instance, we saw above in Fig. 8 that the performance of Vietnamese students was roughly 200 points higher than students in countries with similar GDP per capita. Pritchett and Viarengo (2023) show with PISA-D data that students Senegal, Zambia (or Cambodia or Guatemala) with similar personal and household characteristics score 200 points higher in Vietnam. Efforts at "decomposing" this Vietnamese success into the standard "proximate determinants" largely fails. Dang, Glewwe, Lee, and Vu (2020) use PISA data and find that the available student and household and school characteristics explain very little of Vietnam's superior performance. Even more strikingly, using panel data from the Young Lives survey that followed children in four locations (Ethiopia, Peru, Andhra Pradesh in India, and Vietnam) from very young ages through their schooling lives, Glewwe, James, Lee, Rolleston, and Vu (2021) find that the very large difference in learning between Vietnam and India cannot be explained at all with standard student and school/teacher "thin inputs", as only 1–2% of the gap can be accounted for. Only when they introduce an endogenous and sophisticated measure of "math teacher pedagogical skills" (not teacher formal qualifications but in practice assessed) do they even explain 10–12% of the difference in outcomes. It is a common finding that the huge dispersion in learning outcomes as seen in Fig. 8, with gaps of 100 points of more between countries at similar levels of GDPPC, are only weakly accounted for by observed inputs.

The large array of evidence that "thin inputs" explains very little of observed learning variation (across schools, across regions, across countries) is discussed more in depth in Pritchett (2013).

Second, individual "interventions" aimed at improving learning outcomes show very different outcomes, and the most plausible explanation of why there are often very low (zero) impacts even of seemingly attractive and "common sense" interventions is that features of the system lead to the intervention having little or no impact.

We provide three quick examples, followed by a more extended example of how impact, even of the same "intervention" varies widely across systems.

> One, it is often thought that low education quality is the result of low teacher pay, but an impact evaluation of a programme in Indonesia that roughly *doubled* teacher pay found zero impact on teacher performance or student learning (de Ree et al., 2017), which the authors conjecture was because nothing about the system generated any connection between pay and performance.
>
> Two, an impact evaluation of a scaled "school improvement plan" in Madhya Pradesh India found that although the schools actually did do a diagnostic and did prepare a school-specific improvement plan, nothing else happened, as neither the school nor the education support bureaucracy appeared to actually act on the plan and hence, not surprisingly, there was zero impact on learning outcomes (Muralidharan and Singh, 2020). The authors point out that the education system was actually geared around "process compliance" and was not actually conducive to programmes/interventions aimed at improving actual learning outcomes.
>
> Three, in a paper based on a very early impact evaluation carried out in the 1990 s (although the paper came out much later) Glewwe et al., 2009) provided textbooks to children in Busia, Kenya, fully expecting that providing additional textbooks so that each child had their own book would increase learning, but they found zero average impact on learning, which they eventually discovered was because the textbooks were too hard for the actual learning level of the students and hence only the most advanced students benefitted from the increased "inputs."

A more detailed example is a recent paper Angrist and Meager (2022) that reviews the rigorous estimates of the LATE of doing TaRL (teaching at the right level). They find that while all of the estimates find positive impacts, the range of those impacts is enormous, from 0.07 to 0.78 effect sizes. This implies the "rigorous" evidence supports that TaRL "works" (has positive impact) but the range of estimated outcomes varies by a factor of 10, which would imply the practical importance and cost effectiveness differ widely and, with a range this wide, knowing the "average" impact is only of modest value. They investigate the source of this wide variation across studies and discover that nearly all of the variation is due to differences in implementation. In technical terms, studies usually measure only the programme impact (LATE) as the "intention to treat", which is the relevant impact measure if future implementation is going to be of the same fidelity (and uptake) as in the evaluated programme. They find that the "treatment on the treated" estimates are remarkably similar across countries. They also find that the mode of implementation (with regular teachers or with volunteers) plays a large difference in the resulting estimated impact. They also undertake a new experiment aimed not at changing the intervention itself but rather solely at increasing the fidelity of implementation. They find that this "meta" intervention raises the impact (LATE as 'intention to treat') by 0.22 effect size gain from the "same" intervention (in a literature where a 0.10 effect size is considered a "large" impact). This study reveals that the usual published literature about interventions only contains a small part of the relevant information needed to be known in attempting to adopt or adapt an intervention. Knowing that somewhere, someone found an effect size of 0.78 does not imply the best prediction of doing the "same" programme will be similar to that unless one can recreate in the context fidelity of implementation and that, in turn, depends on the education system.

The third reason a system is needed is that, even if one understands the proximate determinants that lead to good outcomes, if the existing proximate determinants of learning are endogenous chosen by existing actors in the system, then there may be little or no scope for changing outcomes without changing the system level proximate determinants. The strength of economics and economists it not at being better informed about technical relationships in the production function than others (e.g. there is a huge difference between the roles of agronomists (knowing the production function) and agricultural economists (knowing about markets)), but rather about understanding how a decentralized system produces, endogenously, emergent properties and outcome through the choices made by actors.

There are a number of ways to model an education system, but one of them, which is the World Development Report 2004 approach, extended to education (Pritchett, 2015), is to model the schooling system as the operation of set of accountability relationships between actors, including citizens in general (and associations of citizens), politicians, the organizations of the State, the organizational providers of schooling (including a Ministry as provider), and parents, students and communities. Actors in this systems approach are engaged in accountability relationships that can be strong or weak at achieving outcomes depending on the design elements. In this view, schooling systems that achieve high performance in learning outcomes are those with overall systems that are coherent for learning, both across and within the four key accountability relationships (politics, compact, management, and voice and choice).

A system approach brings new concepts, such as "coherence", that are not obvious at the unit/organizational level or from an "input by input" proximate determinants approach. One can use the concept of "coherence" within an accountability relationship. Let me give three examples of the concept of coherence.

One, in a well-structured relationship between employer and employee (say) the elements of the relationship (delegation, finance, support, information and motivation) align, are coherent, so that the agent acts in the interests of the principal. But in dysfunctional bureaucracies (both private and public), there can be disconnects at each state: delegation can be unclear (or overambitious) and not related to available finance/resources. Support (training) can be given that is not useful to achieving the purposes or goals of the organization. The

information collected about performance can be merely procedural and unconnected to actual outputs or outcomes. Therefore, there can be a lack of coherence within an accountability relationship.

Two, there can be incoherence between the curriculum (what is formally stated as the learning objectives), the actual classroom capability and practices of the teachers, and what the high stakes examinations assess. For instance, in a "survey of the enacted curriculum" in Uganda and Tanzania, Atuhurra and Kaffenberger (2020) show a massive disconnect between the topic coverage and depth of understanding in each topic in the curriculum, which expected deep understanding of advanced topics) and the teaching, which mainly focused on rote learning/memorization of very few basic topics.

Three, as an education system is part of an overall political and social system, there can be a lack of alignment or coherence between the various "principals." For instance, at the "front-line" level, classroom teachers and school head teachers and principals in public schools are typically civil service employees of a large bureaucracy and hence accountable in that relationship but are also intended (at least rhetorically) to be accountable to the students, and the parents of the students, and communities in which they teach. But the two "principals", the Ministry of Education and the students/ parents/ communities/ local governments often had quite different views on what is important about education and what teachers should be doing. Qualitative work in Africa, Malawi (Watkins and Ashforth, 2019) and Nigeria (Bano, 2022), and also Pakistan (Siddiqi, 2022) and Indonesia (Bano and Dyonisius, 2022) reveal that the lack of coherence of the goals of the various actors in education leads to tensions and often leave local actors alienated and frustrated with the local public schools. This lack of coherence implies that "bottom up" efforts at "community based management" as a means of improving schools often fails.

The main point is that the research agenda of showing "what can, in principle, work?" with specific, limited scale, often NGO or researcher implemented, interventions is played out and has reached the limits of its general usefulness. For instance, Kerwin and Thornton (2021) demonstrate the extreme sensitivity to RCT findings about programme effectiveness. One version of a mother-tongue reading programme in Northern Uganda produced very large positive gains in literacy, but that version was very expensive and hence a reduced cost version was developed, trying as best as possible to maintain the principles and design of the programme "rigorously demonstrated to be effective." But the reduced cost version had zero (or even negative effects on some learning indicators). Therefore, the first RCT finding about "what works" was of only academic interest as its cost made scaling infeasible and even what the designers thought was the "same" but reduced cost version did not work to improve literacy/reading.

A second example is from Busia region of Kenya where an RCT impact evaluation implemented by an NGO of reducing class size by either hiring an additional teacher in civil servant status or on a renewable contract found that there were reliable learning gains *only* when the additional teacher hired was hired on contract basis (Duflo et al., 2015). Because reducing class size in early grades is a pressing issue for the Kenyan government, they decided to take this rigorous evidence on board and scale up this "intervention" of hiring teachers on contract. Given the lack of capability to implement this nation-wide, it was decided to engage an non-governmental organization (NGO) to implement in some regions and have the government implement in other regions. This allowed the possibility of RCT impact evaluation of the impact on learning of the "contract teacher" intervention when scaled, and when scaled by NGO vs government. The results were both entirely predictable and striking. When the programme was implemented by an NGO, it had roughly the same impact as in the original study but when it was implemented by the government it had zero impact on learning (Bold et al., 2018).

This result was predictable as the politics and reality of government implementation of a "contract teacher" scheme are radically different from when an "intervention" of this type is done by an NGO. The politics are radically different because government implementation of a scheme of "contract teachers" is (rightly?) perceived by existing teachers and teachers unions as a threat to the civil service modality of government employment (and of the wage (Barton et al., 2017) and tenure security that come with that) and hence government adoption and implementation, predictably, created massive political push-back. The result was also predictable as the hypothesized mechanism of action whereby the contract teacher path led to more learning because teachers perceived that if they did not perform well their contract would not be renewed. This hinges on the credibility of the lack of renewal both among the community/school and of the hired teachers. There were good reasons to believe that the government, whatever it said *ex ante*, would not (and in some sense deeply could not) actually fail to renew contracts and hence the mechanism of action was credible when done by NGOs but not credible when done by governments.

More generally, the public administration scholar Mark Moore (Moore, 2019) argues that the idea that one can "innovate" in the NGO sector and then "scale" this innovations by transplanting them into government (or even government-financed) programmes is fundamentally flawed. This approach to innovation assumes away the hard part. The answer to the question: "can we, freed of the inevitable (and in many ways legitimate) constraints of implementation of doing things in the government, do something more effective than what the government is doing?" is "of course you can." The reason that answer is obvious is that it is built into the question, almost by definition if you are "freed of constraints" in a constrained optimization problem you will be able to achieve better results unless the constraint was not binding (technically this is built into the Kuhn-Tucker conditions of optimization in that the only way relaxing a constraint does not lead to better results is that if it was a "slack "constraint whose Lagrange multiplier was zero). The hard question is: "What can be done by the government, with either the constraints it now faces or with politically and administratively feasible modifications to those constraints, that will make things better?" For this latter question, RCT impact evaluations of boutique, NGO (or researcher) implemented "interventions" may teach us little or nothing (Vivalt, 2020).

The path forward needs to move towards "how works?" or "what will work to raise learning outcomes that is possible to be implemented in this particular context/system?" or, alternatively, "How can a system changes in coherence of accountability (in delegation, finance, support, information, and in motivation) make possible/induce organic innovations that raise learning to endogenously emerge and endogenously scale?"

This kind of research will have to take the contextual embedding of implementation in existing organizations that themselves are embedded in existing systems (with their associated "institutional" features) as a key feature of the research itself. This could involve impact evaluations, perhaps with RCTs, "at scale" (Muralidharan and Niehaus, 2017) but these are a rare form of existing RCT research because they are so much harder to arrange, fund, and implement than "field experiments" or impact evaluations of small-scale NGO-implemented interventions.

A promising line of research are studies that are themselves embedded into the implementation process. Aiyar et al. (2023), for instance, studied the implementation of a "teaching at the right level" reform programme in Delhi India using multiple methods, including having a research in selected schools every day for extended periods to get the "feel" of the pressures school leaders and administrators were under and how they, and teachers, responded on a day to day basis to the new demands of this pedagogical reform. Levy et al. (2018) produced a fascinating study of the challenges of producing quality education in South Africa, comparing the political and bureaucratic conditions between the Eastern Cape and Western Cape provinces.

Another kind of research is doing case studies of "success cases" that trace not just "what" was done but "how" this was accomplished. This can take a variety of forms. It can come from memoirs of those involved, such as Jaime Saavedra's (2023) recent memoir of his time as Minister of

Education in Peru, or cross-national case studies of success cases, often of places that implemented similar reforms, such as Crouch's (2020) study of the implementation of core instructional support in three country/regional contexts, or Stern et al., 2022 study conditions for successful implementation of instructional support for reading.

5.3. IV.C: A realistic politics of learning: End of the naïve confusion that "Education production function" estimates lead directly to "policy" recommendations

A considerable amount of research by economists in education has been taken up with the estimates of what is called the "education production function" (EPF), which is intended to provide guidance into the causal connections between "input" and "outputs" of the process of education. The underlying idea is that these estimates of the education production function provide useful inputs to "policy makers" or the "managers" of schools (or organizational collections of schools).

However, this underlying research agenda is deeply conceptually confused about what is a "normative" model and what is a "positive" model of the behaviour of an education "producer", and this confusion is sufficiently pervasive that this research may, in the end, be not just useless, but worse than useless (Pritchett, 2009). That is, one can estimate an education production under the presumption the producers are, as a positive descriptive model, following the normative prescriptions for maximization. Either they are not or they are.

If the hypothesis that "normative is positive" is rejected, systematically so, then this line of research rejects the notion that producers are efficient at maximizing the measured outputs. This is what nearly all empirical studies do, implicitly rejecting the conditions of maximization, often by multiple orders of magnitude (Filmer and Lant, 1999). In this case, this line of research leads to the question, "if producers are not maximizing the measured outputs/outcomes then what is it they are doing?" and also the question: "if producers are not maximizing the measured outputs under the existing system (where the structure of the system is the proximate determinant of the proximate determinants ("inputs") measured in EPF estimates) then why not, and what would be the changes in the system that would lead producers to make other choices?"

If the hypothesis that "normative is positive" is accepted, that is, that producers are producing efficiently then there is no "guidance" to be given to the producers (as they are already doing the best they can with what they have).

What is not logically coherent, but is nevertheless very common, is to reject that producers are maximizing (routinely, and by large amounts) and then use those same estimates to give "advice" as to how to maximize something the relevant decision-makers are demonstrably not maximizing (Pritchett, 2009). This is like saying "Here is what a person should do if they were trying to lose weight." Then I observe that people's consumption choices are wildly at odds with what they would be doing if they were trying to lose weight. The obvious conclusion is that they are not, in fact, trying to lose weight. If so, then telling them: "if you ate less of X you would lose weight" is not, in any sense, "advice" to people who are not trying to lose weight.

We stress this because it is a clear example of the difference between a methodological innovation within an existing paradigm versus a paradigm shift. That is, the onset of the fad of using randomized control trial (RCT) techniques in development is a (kind of) methodological innovation. One idea would be to use RCTs to recover estimates of the causal impact of specific inputs in a quasi-EPF way. This is obviously just a better method for the same paradigm (normal science). But if the underlying paradigm of giving advice from EPF estimates under the presumption that "normative is positive" is wrong, then the RCT estimates of EPF, even if they do identify a causal impact better than estimates using observational data, are no more useful for policy and practice than non-RCT EPF estimates.

One needs better guidance about *system* features and design RCT experiments to help understand the operation of the system. But here the paradigm shift comes first, then the method: method does not lead. More bluntly put, the methodological "innovation" of RCTs has set the economics of education *back* by 20 years.[5] Instead of realizing that the economics of education needed a radically better overall framing (that was embedded in a system approach), people just used the new tools for old paradigm questions, thus putting off the reckoning that the old paradigm of EPF where "normative as positive" was itself conceptually flawed. The many new "systematic reviews" of the literature that supposedly provide guidance for "evidence based" choices in education policy are only marginally better than the old reviews of the empirical literature from nearly 40 years ago (e.g. Hanushek, 1986; 1995) as, without being embedded in a coherent theory that allows for contextual differences in the estimated LATE of various inputs that encompasses the observed variation and lack of external validity (and some progress is being made on this front, e.g. Glewwe and Muralidharan, 2016) and without an understanding of the serious issues with "construct validity" of estimates of LATE from specific interventions (Kerwin and Thornton, 2021; Pritchett, 2017) more RCT estimates are, in and of themselves, of little value.

While I don't want to get too distracted by questions of RCTs per se, it is important in formulating a new, forwarding looking, research agenda in asking what the "value added" to the existing stock of knowledge would be from any given use of resources devoted to education (for broader overviews on the role of RCTs in development see Basu, 2014, Deaton and Cartwright, 2018, Pritchett, 2020). Whereas, say, 25 years ago, when there were very few RCTs in development and very few RCTs in education in development, one might have been able to make the case that an incremental new RCT would be of high value; this is no longer the case, for two reasons. First, just declining marginal product. The first RCT in a domain or about some topic might be useful but, as with nearly everything, the marginal product is declining and the n^{th} is of much less worth.

Second, and more importantly, as there have been more and more RCTs, it has become clear that the problem of "external validity" is just overwhelming empirically as the variability across RCT estimates is so large as to make any given RCT of limited value (Vivalt, 2020). Moreover, the proponents of RCTs have never been able to give even a minimally logically coherent account of how the evidence from RCTs was to be used to update beliefs (in any sense, not just an "optimal" or Bayesian mode) (Pritchett and Sandefur, 2014; Pritchett and Sandefur, 2015). My recent paper shows that the suggested approach of doing 'systematic reviews' of 'rigorous estimates' of LATE does *worse* (and potentially much worse) at predicting programme impact (in the standard lowering the RMSE (Root Mean Square Error) across a variety of contexts than the simplest possible "old school" approach of just using Ordinary Least Square - OLS context by context (Pritchett, 2023).

On a more constructive note, this implies that tackling the question of the politics of learning is a promising domain for new research. Mostly, economists and educationists have ignored the politics of learning and adopted a "spend on school" approach where it was imagined that the key problem was that politicians were not expanding access to education fast enough because they were insufficiently "committed" to education, or alternatively, because they were insufficiently committed to the expansion of education to "marginalized" groups. But the facts in Sections I,II and III suggest that is an egregiously inadequate characterization of the nature of the questions to be asked and answered.

The really puzzling question is not: "why do some countries expand schooling and others not?" or "why do some countries do 'free'

[5] "Innovation" is in scare quotes as the application of RCTs in development contexts was the application of methods in the use of RCTs known for decades and decades, there was no technical or method "innovation" that led to more RCT experiments being done in developing countries.

schooling and others not?" or even "why do some countries spend higher fractions of government revenue on schooling than others?" These are not particularly interesting questions because the extent and pace of expansion in schooling has been so large and so uniform.

The puzzling question is: "Why do some governments spend so much to expand schooling and when the result is such low learning outcomes from that schooling (relative to other countries)?" And, even more pointedly: "What is the nature of the politics such that some governments have expanded both schooling and learning very rapidly (e.g. Vietnam) whereas other governments have expanded schooling with stagnant learning outcomes, and others have expanded schooling while learning within schools was deteriorating consistently over decades?"

The simplistic answer that just extends the "spend on school" paradigm to claim that expansions of enrollments cut "resources per pupil" and hence "lack of commitment to spend" is still the answer is not even first order plausible as a general explanation. Many governments, as in Indonesia and India, have actually increased real spending per pupil by a factor of 2 or 3 over recent decades and yet still suffered sustained deterioration in measured learning per year of schooling.

I believe there are two key elements to constructing a realistic politics of learning:

First, models of why governments control the means of production of learning need to incorporate that governments, pretty much everywhere and always, whether democracies or not, seek to control the socialization process in schools. The political scientist Agustina Paglayan (2021); (2022) has shown generally historical myth that governments expanded public schooling as a response to public pressure due to expanding democracy is that, a myth. She shows that governments expanded schooling as a mechanism to deflect and defray opposition to State control by expanding their control of socialization. This implies that what those in the "political settlement" that controls the State think is the objective of education will have a large role in shaping the true objectives around which the education system is coherent. Opalo (2023) traces how debates within the ruling party in Tanzania have shaped how the purpose of expansion of access to schooling was framed and reframed over time as power shifted. In particular, the combination of a commitment to rapid expansion of primary education but very sharp limitations in expansion of secondary schooling emerged from Nyerere's view to education in producing Tanzania citizens with basic skills, but the attempt to avoid an "over-educated" population.

Second, the commitment of governments to universally high levels of learning in basic education will depend on a strong commitment of the elites who constitute the "political settlement" that their vision of the future actually depends on an educated population. While this is a common rhetorical commitment, many political settlements that depend on the control and redistribution of a narrow pool of economic rents (e.g., point source minerals or resources) have elites which do not see a realistic link between a highly educated (as opposed to just schooled) population and their vision of the national future (and their role in that future). A set of case studies of the politics of learning in twelve (12) developing countries illustrate the diverse ways in which "learning" has to compete with other political priorities and the reasons why it has been easy to construct a "high access/low learning" equilibrium (Gershberg and Spindelman, 2023). In contrast, the political case study of high performing Vietnam (London and Duong, 2023) reveals that the strong and deep political and social commitment to learning managed to produce impressively high levels of learning, not through a well-implemented top-down plan, but rather through what might be called "pressured chaos."

5.4. IV.D: Two topics: technology and private sector

In discussions of this paper generally, and in particular in its presentation to generalist policy audiences (e.g. non-education specialists), two themes consistently emerge which I have not yet addressed: the role of technology and the role of the private sector. I have a similar view with respect to both, which is that if and when these are integrated into an overall systematic approach, they have the potential to be "enabling" factors that can contribute but that, in and of themselves, if pursued as general or major "solutions" or contributions to the learning crisis they will fail (as have many other non-system approach fads).

5.4.1. IV.D.1i: *"Technology" and education*

There are two major ways in which "technology" is seen as crucial to the future of education. One is that the application of technology will help solve the learning crisis and improve teaching and learning practices. The other is that the education that children receive should focus more on teaching them how to use technology, for instance that the education should enable children with skills in using computing and media to be more productive.

The latter concern is easy to address with a simple answer: yes and no, no!, NO!.

Yes, children being educated in 2023 and in the future should have higher levels of adeptness with, and skills in, the use of modern computing and information technology. But, no, no, no, this is not the key problem for most SSA schooling systems. That is, the learning crisis implies that most children do not acquire foundational literacy, numeracy, scientific, and critical thinking skills early in their schooling experience and hence arrive at very late levels of schooling with very low stocks of needed cognitive skills. Simply adding curricular objectives at the late stages of schooling (e.g. junior and senior secondary) about computing and information technology and their uses cannot be effective as the students lack the foundation that is needed for those to be effective.

The first question of whether "technology" is, or can be, a major contributor to solving the learning crisis comes up against the brutal facts laid out above, contrasted with the facts about progress in computing. That is, we have seen that: (a) learning per year of schooling appears to be on either a stagnant or declining path in most developing countries; (b) the levels of learning are very much lower in some countries whereas some developing countries have managed consistently very high levels of measured learning (e.g. Vietnam); and (c) (though this is less documented here) the Organization for Economic Co-operation and Development - OECD countries achieved their current levels of PISA-like assessed learning at least by the early 1970 s and have made near zero progress since.

These facts line up very awkwardly with the fact that Moore's law describes the rapid progress in computing power, which cumulatively has led to a 10 to the 10th power increase in computing power. This means that, if by "technology" one means information and communication technology and the capability to process, store and communicate bits and bytes, this has increased a near trillion-fold over the last 60 years.

These combined facts imply at least four things. One, "technology" was not a necessary condition for achieving very high levels of cognitive skills in the now developed countries, which are very high compared to those in SSA today. Two, "technology" was not a necessary condition for the currently high performing countries to achieve their high levels of performance today (or of those that achieved high levels in the past at low incomes, such as Japan and Korea). Three, the trillion-fold improvement in computing power has not been a sufficient condition to improve learning outcomes in the OECD. Four, the trillion-fold improvement in computing power has, as yet, been consistent with secular *decline* in learning in the developing world.

Therefore, to my mind, the view that somehow technology is going to play a large role in solving the learning crisis seems a bit like the old saying about second marriages being the triumph of hope over experience. There is a constant hope that "technology" can lead to "leapfrog" approaches that accelerate learning, but there is as yet no evidence that this has been true in any of the cases of sustained success, and a long string of well-evaluated failures for "computers in classrooms" to make a positive difference.

That said, if governments do the other needful things to make their education system coherent around learning, then there is no reason why technology cannot be a modest enabling factor in helping that overall system reform.

5.4.2. IV.D.2) Private schools

There is a tendency to see rapid expansion in private sector schooling as a promising feature and that this indicates possibilities of "public private partnership." I believe there is a powerful "liberty" case for allowing people the freedom to start and run private schools (with modest regulation) and a "liberty" case for allowing people the freedom to choose the school their child will attend (this view is reflected in the UN Declaration of Human Rights, Article 26.3). Therefore, there is a strong case for *allowing* private schools to operate.

I also believe that no country has produced a high-quality system of basic education without high quality publicly produced schooling. Therefore, there is a strong case for *producing* high quality education in the public sector. Where I am dubious is whether there is much scope for the third "p" in public-private-partnership. There are two types of envisioned PPP. One is that "money follow the student" to either public or privately owned and operated schools. The second is that the public sector should "contract in" management of schools from private sector operators.

There is a historical case of "money follows the student" leading to high quality education systems, which is that the countries such as Belgium and the Netherlands resolved their social and political conflicts between secular, Protestant, and Catholic systems by creating a three-pillar approach where all three pillars were publicly funded on an equal basis, subject to some regulation of content (including socialization). These countries have maintained these systems and hence have the highest levels of "private" enrollment at basic levels in the world. But this "private" is tightly regulated and seen as an integral part of an overall system.

There are four quite powerful arguments against seeing "money follows the student" as *in and of itself* as key to building a successful education system.

First, it is not at all clear that a rapid expansion of enrollment in the private sector is because the private sector has some sector-specific "secret sauce" or whether there are significant issues with the public sector. My reading of the cross-national evidence is that reasonably well-functioning public sector education systems are able to produce learning as cost-effectively as private schools. Much of the apparent superiority of private sector schooling in learning outcomes is due to powerful "selection effects" in which students who would have otherwise had high learning attend private schools (e.g. high SES students) and not that private schools have higher value added in learning than public schools in well-functioning systems.

Fig. 12 shows the estimates of selection corrected estimate of the private sector learning premium adjusted to student SES using both standard PISA data and the Patel and Sandefur (2020) data. This data shows both the level and variance of the private sector premium for countries with different levels of learning performance in the public sector, with three obvious points. One, when the public sector learning outcomes are very low, the "private sector learning premium" tends to be very high. Two, when the public sector performance is moderate (say in the range of 360–480) one sees that, on average, the PSLP is very high—but, at the same time, there are a significant number of countries for which the SES adjusted premium is low, or even negative (private schools have lower learning value added by these SES-adjusted estimates). Third, when public sector learning performance is high there is still large variance and some countries with high estimated PSLP but, on average it is low and, for a significant fraction of the countries there is no SES-adjusted positive learning premium at all.

Second, recent research on the underlying demand for quality

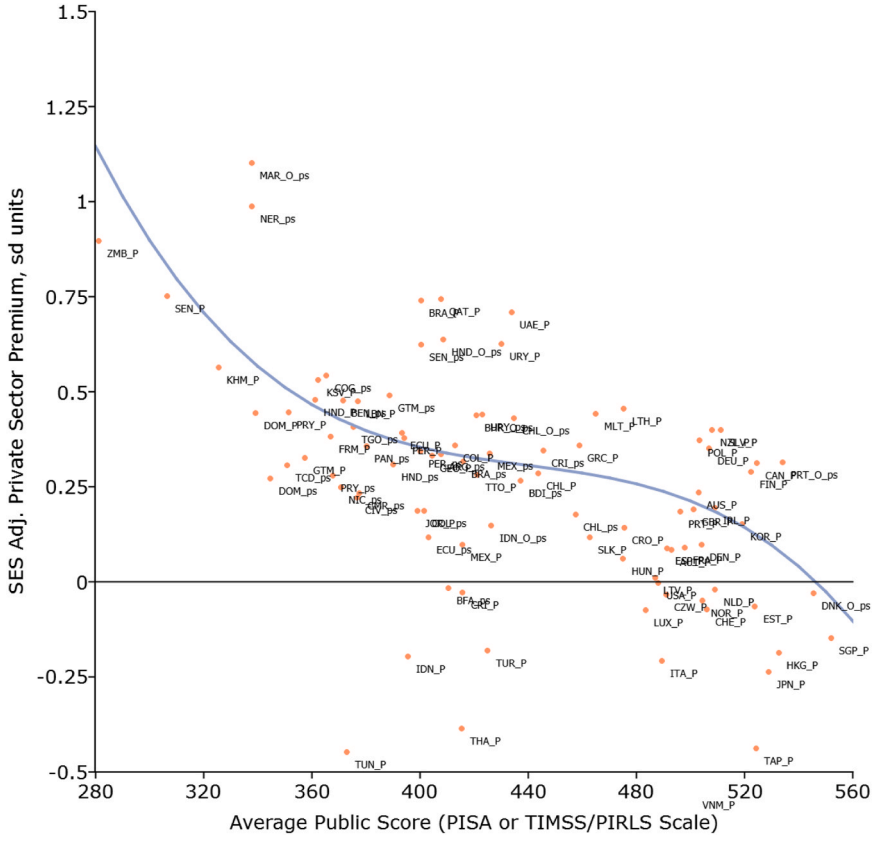

Fig. 12. Estimates of the private sector learning premium (adjusted for student household socio-economic status) vary strongly by public sector learning outcomes. Source: Author's graph based on Patel and Sandefur (2020).

adjusted schooling implies that schooling quality is quite price inelastic. This implies that while a "voucher" like system would shift some students from lower to higher value-added schools, the fraction shifted would be relatively modest compared to current enrollments, and hence a main feature of a voucher system would be "infra-marginal" as it would just compensate parents who would have had children enrolled in private schools in any case. This means from a public finance view that, even if a voucher were to increase scores, it might be a very cost-ineffective way of doing so as the learning gains to the movers come at the expense of large transfers to the non-movers, who were always in private school and hence are inframarginal (Das, 2023).

Third, it is not at all obvious that for private schools the cost of "partnership" with the public sector to receive public monies would not dilute precisely the features that allowed whatever higher performance in "cost effectiveness" they had. For instance, in South Asia, a huge cost advantage of the private sector is that they pay teachers much lower wages (only a fifth to a tenth as high as the public sector wage) as the public sector wage has a huge element of politically negotiated 'rent' (Pritchett and Aiyar, 2014 on India). Therefore, if receiving public sector finances came at the condition of paying wages which are the same as the public sector, it is not clear this would be attractive for the private sector. Or, there are other regulations in the operation of the school that may reduce its flexibility or modes of instruction that eliminate its attractiveness. In a number of countries which "nationalized" significant groups of private schools by leaving them private but subsidizing them on a per student basis, the "private with public money" schools are the least cost-effective. In Chile's "voucher" system nearly all of the traditionally "elite" private schools opted out of receiving the public sector voucher as the conditions for receiving the voucher were unattractive.

Fourth, if the overall education system does not have reliable data on learning performance and cannot manage the system to improve, it is not obvious that the conditions for "money follows the student" are present for the public sector to make this put positive pressure on the private schools. If, on the other hand, the public sector is learning-oriented and dynamic, and performance pressured, it is not clear "money follows the student" is needed.

6. Conclusion

This is an exciting time for research into human capital in Africa. The reigning paradigm for the last 50 years or so in global education has been to conflate "invest in human capital" with "spend on school." This has brought about the paradox of the combination of very rapid expansion in the access, enrollment, and grade attainment and hence much more schooling, but with generally low (and, on new evidence, long-term secularly falling) levels of learning.

The paradigm has shifted so that it is increasingly recognized that the future of "invest in human capital" has to focus on the creation of valued skills (and these skills are of personal, community, social and economic value; this is not reducing learning to only what is valued instrumentally in a market while in school. This shift in the overall paradigm requires shifts in the priorities for research about basic schooling and education and I have outlined three: (i) a shift to the use of "output" and "outcome" measures that are not just "time served" but "human capital gained"; (ii) away from an attention on proximate determinants, particularly "thin inputs" to the system level, to put attention on the "why" and "how" and the realities of implementation, how the proximate determinants of the proximate determinants structure endogenous choices and outcomes; and (iii) to an explicit consideration of the politics of learning and an explicit rejection of the naïve use of "normative as positive" as a basis for "policy recommendations" and the formation of better models of why governments do what they do in the domain of basic education.

Author statement

I could not find easily on the site what this was supposed to contain.

I am the sole author of this work and it is original work, not elsewhere published or submitted

I read the Ethics and AI acknowledgements and there are not issues and no AI was used at any stage.

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