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Girls' Schooling is Good, Girls' Schooling with Learning is Better

Mari Oye, Lant Pritchett and Justin Sandefur
Center for Global Development

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Mari Oye[*], Lant Pritchett[†,*] and Justin Sandefur[*]

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Abstract

An enormous empirical literature measures the benefits of increased schooling. The assumption, often implicit, is that schooling and learning–the acquisition of skills, competencies, abilities, dispositions–have a strong and tight relationship so that schooling is a good measure of learning. We use Demographic and Health Survey (DHS) data on women's self-reported schooling and a direct measure of literacy to disentangle the effects of schooling and literacy on both fertility and child survival. We show that schooling is not always learning: the probability a woman with five years of schooling can read a single sentence ranges from below 10% to over 90% in our sample of countries. Female schooling is good for child survival: each additional year of female primary schooling is associated with a reduction of roughly six deaths per thousand live births (U5MR) in DHS microdata. But schooling with learning is even better: the gains from a year of schooling are roughly two-thirds larger in countries with the highest versus lowest learning.

Keywords: return to schooling, gender, literacy, child mortality, fertility

JEL Classification Numbers: I1, I2, J1, O1

[*]Center for Global Development

[†]Harvard Kennedy School

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1 Introduction

It is well known that around the world schooling is associated with higher wages/income for women and men (e.g. Montenegro and Patrinos (2014) summarize estimates from 139 different economies and find estimated returns are higher for women). There are also strong associations between female schooling, and household outcomes such as child health, fertility, and child schooling performance in both household and cross-national studies. However, in the literature on wage gains there has been a massive debate about the causal mechanisms, how much of the gains are due to skills/competencies/abilities acquired from schooling and how much of the wage gain is due to schooling as a labor market signal (Spence (1973)). This paper investigates the gains to schooling *per se* versus the gains from learning for outcomes that are not market mediated (though not necessarily "externalities" as the gains are often internal to the household).

The usual case for schooling, explicit or implied in global declarations and the Sustainable Development Goals, relies on an association of schooling with both income and non-income welfare measures. Hanushek and Woessmann (2012) have shown that expansion of schooling has been much less predictive of countries' economic growth than the cognitive skills gained while in school have been. Are the other benefits cited for schooling itself (in domains as varied as health, democracy, and women's empowerment) similarly differentiated according to learning gains? If these non-income gains also appear to be dependent on learning and not just schooling, this provides another rationale for focusing on steepening the learning profile in addition to the expansion of schooling attainment.

We present a novel measure of school quality in Section 4, based on direct measures of literacy for adult women in the Demographic and Health Surveys (DHS) Woman's Questionnaire. The wide coverage of the DHS allows us to greatly expand the number of developing countries with comparable learning data. By comparing women with a fixed number of years

of schooling, we focus on the quality of schooling rather than the quantity of schooling. The cross-country heterogeneity in this data is enormous: among women who list fifth grade as their highest educational attainment, in Nepal and Rwanda more than 90% are literate, while in Nigeria and Sierra Leone fewer than 10% are literate.

Our core empirical question is whether these measured learning levels are the mechanism explaining the well-documented relationship between girls' schooling and non-pecuniary outcomes such as reduced fertility and child mortality. We present two frameworks to address this question: first using individual literacy as a mediator variable between schooling and women's outcomes, and second using aggregate school quality in the region or country as a moderator on the non-pecuniary return to schooling.

The mediation analysis in Section 5 suggests that somewhere between a third and a half of the relationship between schooling and these adult women's outcomes is mediated by a simple binary indicator of literacy. However this mediation finding is not robust to plausible magnitudes of omitted variable bias.

Turning to the role of school quality as a moderator in Section 6, we measure school quality using the DHS literacy index in a given region or country, conditional on highest grade attained. Interacting this school quality measure with individual years of schooling, we find that the returns to schooling in terms of child survival, for instance, are two-thirds larger at the highest level of school quality compared to the lowest. This finding is robust to alternative outcome measures and to an entirely independent alternative cross-country measure of school quality.

2 Background

While a child's schooling need not be justified by extrinsic benefits, policymakers, parents, and children all expect it to be a means to achieving goals. At a basic level, nearly all coun-

tries expected that expanding schooling would facilitate higher levels of labor productivity and raise access to good jobs and higher wages. Yet schooling alone does not achieve this; Hanushek and Woessmann (2012) demonstrate macroeconomic gains to education appear to be all through learning.

At a general level, the statement "schooling has this or that impact" (e.g. on economic productivity or improved child survival) usually carries the ancillary unstated assumption that schooling produces education via a learning profile in the relevant capability/skill/ability/knowledge. That is, nearly all of the causal mechanisms postulated involve education. The literature now suggests that the "schooling" economic effect is in fact an "education" effect; educational quality "matters both for wage levels (at the micro level) and for economic growth (at the macro level)" (Demeulemeester and Diebolt, 2011). We may expect the same pattern of benefits hinging on education quality to hold for other beneficial outcomes linked to schooling in the non-market space, such as reduced child mortality.

The causal link between mothers' educational attainment and decreased child mortality is "one of the most consistent and powerful findings in public health,' with a Lancet review estimating that the prevention of 4.2 million child deaths (51.2 percent) from 1979-2009 can be attributed to increased educational attainment in women of reproductive age (Gakidou et al., 2010). The authors note that "many hypotheses have been proposed for the mechanisms through which increased education could lead to reductions in child mortality rates, including individual level effects through improved use of health services, economic advantages, empowerment and independence of women, and community-level effects" (Gakidou et al., 2010).

A robust body of evidence associates education, and particularly the education of women, with positive effects "additional to the impact of schooling on household income" (Glewwe, 1999). Women's education has been found to play a role, for example, in mothers' determination of child malnutrition (Christiaensen and Alderman, 2004); in reducing women's

ideal and actual family size (Behrman, 2015); and in reduced child mortality and morbidity (Gakidou et al., 2010; Basu and Stephenson, 2005).

Moreover, a few studies have demonstrated a causal effect of schooling on these outcomes in a number of cases.Duflo (2000) and Breierova and Duflo (2004) exploit variation from a nationwide school construction program in Indonesia and find that increased parental schooling lowers child mortality. Likewise, the introduction of Universal Primary Education in Nigeria in 1976 and Uganda in 1997 provided researchers with a source of exogenous change; based on this analysis,Osili and Long (2008) suggests that increasing female schooling by one year reduces early fertility in Nigeria, while Keats (2014) finds that women in Uganda with more schooling prefer to have fewer children, delay having their first child,and reduce overall fertility at any age, while investing more in their children's health.

Various pathways are posited to mediate the effect of schooling, including the act of girls attending school, building familiarity with new social interactions and networks; knowledge actually transferred in school to future mothers; and finally, the acquisition of learning skills, literacy and numeracy, allowing women to accumulate knowledge both in and outside of school (Bongaarts and Watkins, 1996; Christiaensen and Alderman, 2004; Dearden, Pritchett and Brown, 2004; Glewwe, 1999; La Ferrara, Chong and Duryea, 2012). These are not mutually exclusive or necessarily universal. In an analysis of Malawi, Uganda and Ethiopia, Behrman (2015) finds some pathways to be common across contexts while others are country-specific.

However, a growing body of studies emphasize the role of skills (e.g. literacy) in mediating the association between schooling and beneficial social outcomes. Glewwe (1999) finds that "education improves child health primarily by increasing [mothers'] health knowledge" in Morocco. Though Moroccan schools did not include health knowledge as part of the curriculum, the impact of schooling on health behaviors appeared to be the result of skills

learned in school; literacy and numeracy allowed mothers to improve their health knowledge outside of school, for example via the ability to read medicine labels (Glewwe, 1999). Based on survey data and ethnographic research in Nepal, including basic literacy tests, LeVine et al. (2004) found that literate mothers had better comprehension of both print and radio health messages, and were better able to tell an organized health narrative to an interviewer; schooling had no significant effect separate from that mediated by literacy.

The role of education quality for social outcomes is all the more crucial as quality varies so drastically across systems. The 2012 UNESCO Global Monitoring Report's analysis of Demographic and Health Survey literacy data finds that "many children in poor countries have not become literate even by the time they have completed primary school," while at the same time, "curricula around the world expect children to learn to read by the end of the second year of primary school" (United Nations Educational and , UNESCO). This is itself a learning crisis. In Nigeria, for example, the percentage of young women still illiterate after six years of school increased from 41 percent in 2003 to 52 percent in 2008, illustrating what United Nations Educational and (UNESCO) termed a "chronic quality problem." Progress towards steepening students' learning profiles has been too slow (or at times nonexistent).

If the learning profile is flat, years in school only measure "time served," not skills gained. The Annual Status of Education Report (ASER) assessed over a half a million children in rural India in 2014, finding that the percentage of grade 5 students who can read a simple story fell from 54 percent to 48 percent from 2010 to 2014, and the percentage of grade 5 students who could do a simple division problem fell from 36 percent in 2010 to just 26 percent in 2014 (Programme, n.d.).

Even as enrollment levels have increased around the world, the education quality gap between most poor and rich countries remains wide, often so wide that students cannot be tested on the same international assessment scales. Most children in the developing world

are not included in the sampling frame of any of the well-known international learning assessments, e.g. Trends in International Mathematics and Science Study (TIMSS), Progress in International Reading Literacy Study (PIRLS), and Programme for International Student Assessment (PISA). Even including regional tests like Southern and Eastern Africa Consortium for Monitoring Educational Quality (SACMEQ), Programme d'Analyse des Systèmes Educatifs (PASEC), and Latin American Laboratory for Assessment of the Quality of Education (LLECE), most of the world's population is left out. In 2012, those non-OECD developing countries which participated in PISA (which were mostly upper middle income) came in about 100 points behind the OECD mean of 500 (Programme, n.d.).

If beneficial social outcomes commonly associated with schooling, such as reduced child mortality, do in fact depend largely on whether students learn in school, then these gaps in education quality carry far-reaching repercussions.

3 Data

Demographic and Health Surveys (DHS) have been conducted in over 90 countries, with more than 300 surveys carried out since 1984. (Demographic and Survey, n.d.).

We made use of the Woman's Questionnaire, carried out with a nationally representative sample of women in each country, and construct a sub-sample of women aged 25-34.The sample we use here covers 53 countries, spanning the years approximately 2000 to 2014 and including DHS Wave IV and above (Demographic and Survey, n.d.). 24 low-income, 24 lower-middle-income, and 5 upper middle-income countries are included (according to World Bank classification categories).

From the DHS data, we drew indicators of fertility expressed as births per woman; neonatal, infant and under-five mortality per 1,000 live births; and literacy.

After Duflo (2000) and Osili and Long (2008) we construct an indicator for fertility by age 25.

The DHS includes self-reported data on neonatal, infant, and under-five mortality per 1,000 live births.

For the literacy indicator, all respondents who have not attended secondary school are asked to read a card in their relevant language, with sentences such as:

- Parents love their children.
- Farming is hard work.
- The child is reading a book.
- Children work hard at school.

Respondents were coded as literate if they were able to read the complete sentence. The DHS did not administer this literacy test to those who had attended any secondary school.

4 Schooling and the production of literacy

We define school quality in functional terms, as the amount of learning achieved by pupils in a year of schooling, on average, in a given school system. This requires a measure of learning that is relatively comparable across countries. We propose a novel way to generate such a measure using already available literacy indicators from the DHS micro data.

Existing measures of learning levels across countries at a given age or grade level overwhelmingly exclude the poorest countries in the world, which are our focus here. For instance, in its latest round, the PISA exam conducted by the OECD sampled fifteen-year-olds in 63

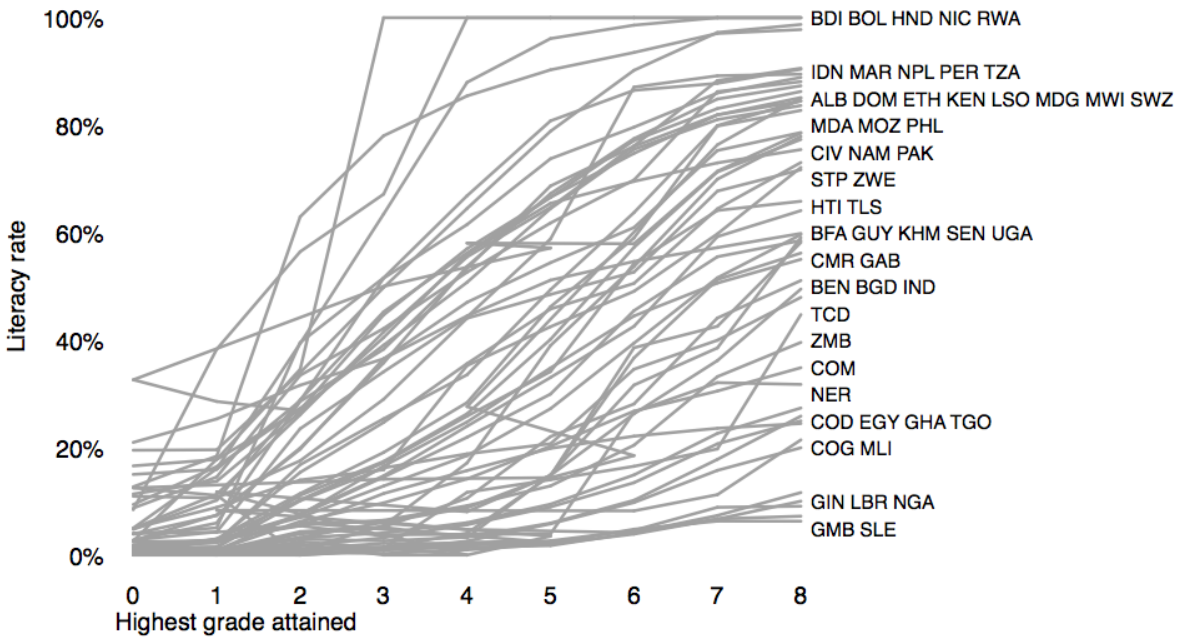
countries, of which only 3 were lower-middle income countries by the World Bank's classification (out of 52 lower-middle income countries total) and none were low-income countries (out of 31 total low-income countries). Similarly, in their latest rounds of data collection, the TIMSS and PIRLS initiatives testing primary school students by grade level included just 12 and 2 lower-middle countries, respectively, and neither test includes any low-income countries. The widest coverage of cross-national learning data for low-income countries comes from regional initiatives like SACMEQ in Southern and Eastern Africa, PASEC in Francophone West Africa, and LLECE in Latin America. While each initiative covers several low-income countries, these initiatives are not benchmarked for comparison to each other.

Our school quality measure covering 24 low-income and another 24 lower-middle income countries is based on a DHS indicator which records literacy on a three-part scale: cannot read at all; able to read parts of a sentence; and able to read the whole sentence. Enumerators show a simple sentence on a card to respondents in the field during interviews, and the literacy classification is done by the enumerator, rather than self-reported.

Average literacy rates among women are likely driven by both the quantity and quality of schooling in the population. To isolate the role of school quality, we calculate literacy conditional on years of schooling for a given age cohort, i.e., women age 25 to 34.

The relationship between literacy and schooling varies enormously across countries, as shown in Figure 1. The graph shows a simple local polynomial regression of literacy on years of schooling, separately for each of the 53 countries int he sample. In almost all cases, literacy is below 20% and often near zero for women with zero years of schooling. Among women who complete third grade, roughly two-thirds or more are literate in a few countries such as Nicaragua, Honduras, and Rwanda. But in other countries, such as Liberia and Mali, literacy rates remain at approximately zero after three years of schooling. Looking at women just shy of primary completion with seven years of schooling, several West Africa

Figure 1: Literacy among women age 25-34, by schooling level



Source: Authors' calculations based on DHS microdata for 53 countries, based on the most recent available round by country. Lines show fitted values from a local polynomial regression of the literacy score on years of schooling, limited to a sample of womean age 25-34 who did not complete primary schooling.

countries still report literacy rates of less than ten percent, including the largest country in the region, Nigeria. Estimates predict that a majority of women will remain illiterate after seven years of schooling in twenty-one of the fifty-three countries.

Note that in this analysis we must restrict our attention to primary schooling, as DHS administered this literacy assessment only to women who had not completed primary education.[1] The number of years of schooling corresponding to complete primary has a mode of eight, but varies by country and is as low as six in some cases. Thus in the regressions in the following sections, our principal measure of school quality is based on literacy among women 25 to 34 with five years of schooling.

[1]Note that in DHS data, women with complete primary, secondary schooling, or higher, are automatically classified as literate without taking the assessment. We exclude these imposed values, thus our calculations will systematically differ from literacy rates published in official DHS reports.

Table 1: Literacy by highest grade attained

	(1)	(2)	(3)	(4)	(5)
S_{ict}	0.100***	0.100***	0.086***	0.101***	0.102***
	(0.006)	(0.006)	(0.017)	(0.005)	(0.008)
t	-0.003***	-0.003***	-0.003***	-0.010***	-0.004**
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
S_{ict}^2			0.002		
			(0.002)		
$S_{ict} \times t$				-0.003***	
				(0.001)	
Mills ratio					-0.020
					(0.052)
Country FE	✓	✓	✓	✓	✓
Age FE	✓		✓	✓	✓
Birth-cohort FE		✓			
Obs. (women)	64683	64683	64683	64683	64672
Countries	53	53	53	53	53

The dependent variable is a binary indicator of literacy. The sample is restricted to women without complete primary schooling. In addition to the controls listed, all regressions include controls for a cubic polynomial of the woman's age and the interaction of schooling with the cell average (country-year or region-year) of the dependent variable, per column (3) of Table 3. Regressions use survey weights within country-year cells and are weighted so that each country-year receives equal weight. Standard errors are clustered at the level of the fixed effects (country-year cells or subnational region-year cells). Variables marked with a tilde are demeaned: individual-level variabls (e.g., $\tilde{S}_{ic}$) are demeaned at the country level while country-level varialbes (e.g., $\tilde{L}_c$) are demeaned at the global level. The control for $\tilde{S}_{ic} \times \tilde{Y}_c$ denotes a control for the schooling term interacted with the average of the dependent variable at the country level.

We avoid drawing causal inferences here, but it is useful to contemplate the source of these patterns in the data. The association between schooling and literacy evident in Figure 1 may be a biased estimate of the true causal relationship for a variety of reasons. The primary concern in the literature on the economic returns to schooling is confounding due to unobserved ability which contributes to grade progression and directly affects outcomes. Empirically, quasi-experimental research has shown the magnitude of bias due to unobserved ability is relatively modest (Card, 2001), but this conclusion rests mostly on rich-country samples, focuses on earnings outcomes, and is not undisputed in the literature (Heckman, Humphries and Veramendi, 2016). Notably for our purposes, however, the direction of bias here is generally agreed to be upward – making the very low literacy rates we observe after several years of schooling in many countries all the more troubling from a policy standpoint.

A second and closely related bias that may arise in these estimates stems from sample selection. The proportion of women who complete primary varies greatly across countries, and these women are excluded from the literacy measure. Countries could achieve high female completion rates by genuinely educating more girls or by lowering standards for grade progression. The latter would be particularly worrying for our school quality estimates. Theoretically, one could imagine that as primary completion rises in a given country, the average academic caliber of pupils who fail to complete primary goes down – essentially the bar for graduation is lowered, and the average non-completer is a weaker student. This would create a distribution of measured literacy rates that was unrelated to true school quality.

While we cannot rule out sample selection bias in our results, we can provide a limited empirical test. If high completion rates are driven by low thresholds for grade progression, we would expect to find a negative correlation between completion rates and literacy among non-completers. This might be particularly apparent in countries where grade attainment has expanded rapidly, as is the case in much of sub-Saharan Africa. In fact, we find the opposite. In the sample of 106 country-years with available data, the correlation between

literacy rates among non-completers and the rate of primary completion is 0.73, and when looking at annual changes in both rates across survey rounds, the correlation is 0.69 (Figure 4). As more women finish primary school, the literacy rate among those who don't goes up. This is suggestive that sample selection is not driving the distribution of conditional literacy rates in Figure 1.

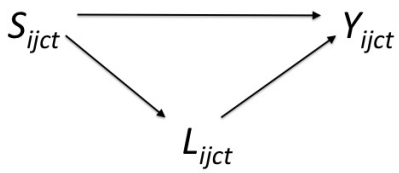
5 Individual literacy as a mediator between schooling and outcomes

This section and the following section explore the evidence for whether learning, as captured in the DHS literacy measures, provides a plausible mechanism explaining the association between female schooling and non-pecuniary outcomes, specifically reduced fertility and increased child survival. To test this mechanism we present two distinct models of the relationship between schooling, S_{ijct} and learning, L_{ijct}, where subscripts denote inidvidual i in region j and country c in period t. In the first model, individual literacy mediates the relationship between schooling and outcomes (Figure 2a; in the second, average literacy rates conditional on schooling in a given population are used as a metric of school quality, which we treat as a potential moderator of the return to schooling (Figure 2b).

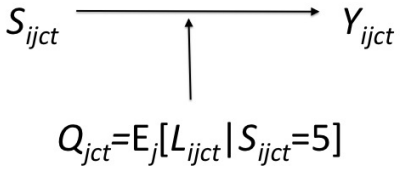
Th first model provides a test of whether literacy mediates the association between girls' schooling and outcomes for adult women and their children. A simple graphical clue to the answer to this question is provided by examining outcomes such as child survival for women at each level of highest grade attainment, and dividing the sample into groups by literacy. The first group in Figure 3 contains all women, whether literate or not. The second group contains those who were deemed partially literate or not at all, and the third group includes only illiterate women. As seen in the figure, child survival is steeply increasing with highest

Figure 2: Literacy as a mediator or moderator of the non-pecuniary returns to schooling

(a) Indivial variable: mediator



(b) Group variable: moderator



grade attained for the full sample of women, but when we restrict the sample to women who did not acquire full literacy in school, the slope is considerably less steep, and when we restrict it to women who remained completely illiterate, it is still less steep. The graphs provide prima facie evidence for literacy mediating the return to schooling as defined in terms of both child survival and fertility outcomes.

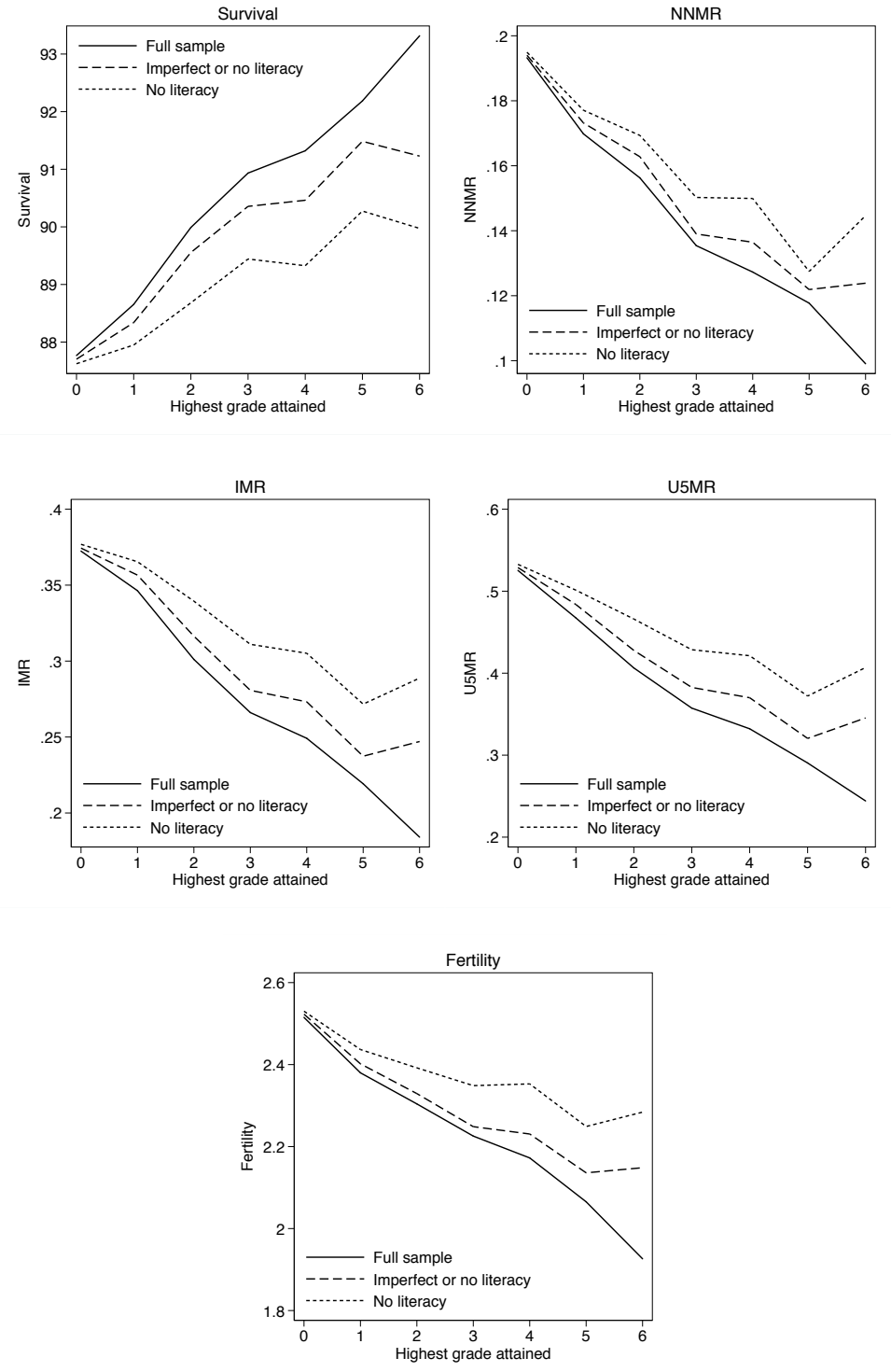
Looking at this mediation question more formally using the terminology of the treatment effects literature, the average treatment effect of schooling on a given outcome can be decomposed into the average causal mediation effect (ACME) of learning, and the average direct effect (ADE) of schooling unrelated to learning. Following **?**, we can estimate the ACME in a linear regression framework as the coefficient, γ_2 on the mediator in an outcome regression controlling for schooling:

$$L_{ict} = \alpha_1 + \beta_1 S_{ict} + \epsilon_1 X_{ict} + u_{ct} \quad (1)$$

$$Y_{ict} = \alpha_2 + \beta_2 S_{ict} + \gamma_2 L_{ict} + \epsilon_2 X_{ict} + v_{ct} \quad (2)$$

While this approach to testing mechanisms is widespread in the social sciences, it rests on a set of assumptions that are not testable in observational data such as ours.**?** characterize the assumptions required to consistently estimate the ACME, which they refer to as sequential

Figure 3: Does literacy mediate the return to schooling?



Lines represent the sample average for each outcome at each level of schooling, averaging over all survey years. The sample is restricted to women ages 25-34.

ignorability. The first step in sequential ignorability is a standard exogneity assumptions about schooling, known in the treatment effects literature as the ignorability of treatment. The second step in sequential ignorability is an assumption that the mediator is ignorable conditional on the treatment status.

Because we cannot test for violations of sequential ignorability, we follow the method proposed by **?** to assess the sensitivity of our results to such violations. Define ρ as the correlation between the errors in equations (1) and (2). **?** show how for a given estimate of γ_2 the true ACME depends on ρ, and suggest reporting cut-off values of ρ at which one can no longer reject the null that the ACME is zero.

We emphasize that all estimates here are associations in observational data, and the sensitivity tests are the only formal grounds for discussing causality in the context of our results.

Taking that major caveat on board, the estimated ACME of individual literacy based on equation (2) in Table 2 is roughly zero and statistically insignificant for overall child survival, as well as the three mortality age categories: neo-natal, infant, and under-five. The ACME for total fertility is statistically significant at the 1% level, but of modest magnitude. The implied ACME of literacy is roughly 0.1 fewer total children, roughly equivalent to the estimated ADE of one additional year of schooling. For all of the outcomes, estimates imply that a minority of the total relationship between schooling and women's outcomes is mediated by our individual literacy measure.

Turning to the sensitivity tests, the only relevant case is fertility, where the estimates suggest a statistically significant ACME from literacy. The **?** procedure suggests that given our estimate of γ_2, the true ACME is distinguishable from zero only if ρ is less than -0.09. In short, any positive correlation between the errors in the literacy regression and the fertility regression would overturn this result.

Table 2: Mediation analysis: Does literacy mediate the association between schooling and women's outcomes?

	(1) Survival	(2) NNMR	(3) IMR	(4) U5MR	(5) Fertility
S_{ijct}	0.311***	-0.008***	-0.015***	-0.018***	-0.021***
	(0.056)	(0.002)	(0.002)	(0.003)	(0.004)
L_{ijct}	0.980***	-0.016***	-0.040***	-0.057***	-0.046***
	(0.118)	(0.003)	(0.005)	(0.006)	(0.008)
Country-year FE	✓	✓	✓	✓	✓
Age controls	✓	✓	✓	✓	✓
Obs. (women)	100524	100524	100524	100524	100524
Country-year cells	105	105	105	105	105
Total effect	0.569	-0.012	-0.025	-0.033	-0.033
% mediated	45	34	41	45	36
H0: ACME=0	0.00	0.00	0.00	0.00	0.00
Threshold ρ	0.02	-0.06	0.01	-0.02	-0.07

The dependent variable is listed in the top row. The sample is restricted to women with twelve or fewer years of schooling. The total effect in column (4) is equivalent to the sum of β_2 (i.e., the coefficient on schooling in the table) and $\beta_1 \times \lambda_1$ (i.e., the ACME or mediated effect which is the product of the coefficient on learning in the table and the coefficent on schooling in the learning regressions in Table 1). The p-values reported are from a χ^2 test that the ACME is zero. The last row reports the threshold value of the unobersvable ρ correlation, above which the true ACME would be zero.

In sum, schooling is a strong predictor of literacy, and of women's outcomes including fertility, child survival, and child mortality in various age bands. Simple regression estimates in the spirit of **?** suggest that between a third and a half of the relationship between schooling and women's outcomes can be explained by our simple literacy indicator. A large caveat applies here though: sensitivity analysis suggests the 'effect' of this mediator is quite fragile to the presence of unobserved confounders that are common to the literacy and outcome regressions.

Taking that caveat on board, we find that literacy mediates a minority of the school-outcomes relationship, and possibly none. This finding is consistent with either of two hypotheses: the true effect of schooling is a direct effect, or our binary literacy measure simply contains too little information to capture the role of learning in mediating the returns to schooling. The next section turns to a model of literacy in the aggregate as a moderator, rather than a mediator, which provide a higher signal to noise ratio from the DHS literacy data.

6 Education quality as a moderator of the non-pecuniary returns to schooling

The previous section tested whether individual literacy moderates the relationship between girls' schooling and adult outcomes; this section tests whether school quality – as measured by the average propensity of schooling to generate literacy – moderates the relationship between girls' schooling and adult outcomes. This is an alternative approach to answering the same core question: is learning the mechanism linking schooling to reduced fertility and child mortality? If the social return to schooling is significantly higher where school quality is better, this is indicative of a learning channel.

Concretely, we use microdata on over one million women from over one-hundred rounds of DHS surveys spanning more than fifty countries to regress a measure of child survival at the individual-woman level on geographic and demographic controls, years of schooling, and the interaction of years of schooling with our aggregate school quality measure from the previous section.

Let Y_{ic} denote the woman-specific child-survival rate for individual i in country c at time t. This is defined as the simple ratio of currently living children over total live births. We regress this measure on years of schooling, S_{ict}, and its interaction with average learning levels in the country, $\bar{L}_{ct}$.

$$Y_{ict} = \alpha S_{ict} + \beta(\tilde{S}_{ict} \times \tilde{L}_{ct}) + \gamma X_{ic} + \mu_{ct} + \varepsilon_{ict} \tag{3}$$

where the tilde denotes that the variable has been de-meaned by subtracting the country-year average in the case of individual level variables (such that $\tilde{S}_{ict} \equiv S_{ict} - \bar{S}_{ct}$) and aggregate variables are de-meaned by subtracting the overall sample average (such that $\tilde{L}_{ct} \equiv L_{ct} - \bar{L}$). This adjustment is purely to aid interpretability and ensure that the α coefficient is not changed by the inclusion of interaction terms. Additionally, because the school quality indicator is measured at the country level, in the analysis we cluster the standard errors at the country level.

To allow for non-linearities in the return to schooling, we also estimate a version of equation (3) replacing the linear schooling term with a spline function, with a single knot at five years of schooling, such that

$$\begin{aligned} S_{ict}^{0-4} &= \min(S_{ic}, 4) \\ S_{ict}^{4-8} &= \max(S_{ic}, 4) - 4. \end{aligned}$$

Table 3: Moderator analysis: Does school quality moderate the relationship between schooling and child survival?

	Linear schooling				Spline (knot at $S = 5$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Schooling								
S_{ic}	0.599***	0.600***	0.621***	0.621***				
	(0.027)	(0.026)	(0.018)	(0.018)				
S_{ic}^{0-4}					0.664***	0.666***	0.608***	0.613***
					(0.038)	(0.035)	(0.029)	(0.030)
S_{ic}^{4-8}					0.498***	0.504***	0.647***	0.649***
					(0.034)	(0.034)	(0.026)	(0.026)
Moderators								
$\tilde{S}_{ic} \times \tilde{L}_c$		0.142	0.341***	0.347***				
		(0.103)	(0.064)	(0.063)				
$\tilde{S}_{ic}^{0-4} \times \tilde{L}_c$						0.346**	0.519***	0.545***
						(0.141)	(0.105)	(0.100)
$\tilde{S}_{ic}^{4-8} \times \tilde{L}_c$						-0.167*	0.056	0.044
						(0.089)	(0.089)	(0.088)
Obs. (women)	1202310	1202310	1202310	1202310	1202310	1202310	1202310	1202310
Country-year cells	105	105	105	105	105	105	105	105

The dependent variable is the woman-specific child survival rate, i.e., total number of living children over total live births, multiplied by 100. The sample is restricted to women with twelve or fewer years of schooling. Regressions are weighted so that each country-year receives equal weight. Standard errors are clustered at the country level. Variables marked with a tilde are demeaned: individual-level variabls (e.g., $\tilde{S}_{ic}$) are demeaned at the country level while country-level varialbes (e.g., $\tilde{L}_c$) are demeaned at the global level. The control for $\tilde{S}_{ic} \times \tilde{Y}_c$ denotes a control for the schooling term interacted with the average of the dependent variable at the country level. $\tilde{S}_{ic} \times$ (% $S > 8$) is a control for schooling interacted with the share of women with more than eight-years of schooling who were thus excluded from calculation of the literacy measure.

These two spline terms are also interacted with our school quality measure in later specifications.

Averaging overall all countries and primary grade levels, the results in Table 3 show a modest but statistically robust association between years of schooling and child survival, after controlling for both country fixed effects and a cubic polynomial of the mother's age. Over the first eight years, an extra year of education is associated with an increased probability of child survival of roughly 0.6% (column 1), relative to an average (unweighted) survival rate of 90.5% in the sample. The spline regressions (column 5) show a slightly larger relationship in the first four years of schooling (0.66%) than in the latter four years (0.50%).

Turning to the coefficient of primary interest, β on the interaction term in equation (3), we see a positive but statistically insignificant coefficient across all years of primary schooling (column 2). However, this interaction term is much larger (and statistically significant at the 5% level) for the first four years of primary schooling and negative in the latter four years (column 6).

Note that so far we have only controlled for characteristics of the individual woman and fixed effects at the country level, allowing for no other determinants of the variance in the return to schooling across countries besides our index of school quality. Equation (3) could be re-cast as a bivariate cross-country regression where the independent variables are the country-specific coefficients on S_{ict}. Controlling for other basic factors in this interaction space significantly increases the magnitude of the β coefficient on the schooling-quality interaction. For instance, once we control for the interaction of schooling and the average child survival rate in a country-year cell, the coefficient of interest increases to 0.31 in the linear specification (column 3), and to 0.52 for lower-primary in the spline specification, while the coefficient for upper primary falls to zero (column 7). An additional control for the interaction of individual schooling with the share of women who complete primary school has no appreciable effect on the results (columns 4 and 8).

6.1 Robustness checks

To test the robustness of the results for child survival in Table 3, we present two alternative specifications and test results using four alternative outcome measures in Table 4.[2] The alternative dependent variables are all closely related: child survival, total fertility by age twenty-five, neonatal mortality, infant mortality, and under-five mortality.

Using our benchmark model (panel A), we find statistically significant and fairly large coefficients on the interaction of schooling and school quality with all five dependent variables: The size of the coefficient on the moderator relative to the pure schooling term is smallest for fertility and largest for neo-natal mortality, where the estimated association between schooling and neo-natal mortality roughly doubles when going from the lowest to the highest school quality.

In panel B we present an alternative specification which replaces our preferred measure of school quality based on DHS literacy data with a measured based on a variety of international learning assessments, including PISA, PIRLS, TIMSS, and others. Altinok, Diebolt and Demeulemeester (2013) present a method of pooling these learning assessment results, based on standardizing scores using results from countries that administered more than one test. This approach is far from perfect, as different tests cover different topic areas, are administered to different grades, etc., but for our purposes the Altinok, Diebolt and Demeulemeester (2013) index allows us to test the robustness of our results using a completely independent measure of school quality. The same size falls from 105 country-year cells down to 79 country-year cells due to data availability for the school quality index. Nevertheless, the coefficient on the interaction of individual years of schooling and aggregate school quality remains robust and is statistically significant for all five outcome measures. The size of the coefficients is

[2]Note that column (1) of panel A in Table 4 replicates column (3) of Table 3.

not directly comparable when using the two different school quality measures without taking into account the different variance in the two indices. Our DHS-based measure has a cross-country standard deviation of 0.26, whereas we normalize the standard deviation of the Altinok, Diebolt and Demeulemeester (2013) measure to one. Thus the interaction term coefficient for child survival appears nearly identical using the two school quality measures; in column (1) panel A the interaction coefficient is 0.341, and in column (1) of panel B it is 0.082 (which is indistinguishable in our data from $0.341 \times 0.26 = 0.089$).

So far we have measured school quality at the national level. Panel C uses a sub-national measure instead, based on our DHS literacy index aggregated at the regional level. This yields 1,171 region-year cells with complete data. The higher granularity of this sub-national index must be weighed against the small sample sizes in some cells and the potential for measurement error. Regression results in panel C show that both individual schooling and its interaction with school quality are significant at the 5% level for all outcome variables. The size of the interaction coefficient relative to the pure schooling term is comparable, but if anything somewhat smaller, than in the benchmark model using national-level school quality.

Table 4: Moderator analysis: Alternative outcomes and specifications

	Survival rate	Fertility by 25	100-NNMR	100-IMR	100-U5MR
	(1)	(2)	(3)	(4)	(5)
A. Benchmark model					
S_{ic}	0.621***	-0.089***	0.178***	0.396***	0.580***
	(0.018)	(0.003)	(0.006)	(0.011)	(0.017)
$\tilde{S}_{ic} \times \tilde{L}_c$	0.341***	-0.026***	0.184***	0.271***	0.303***
	(0.064)	(0.010)	(0.026)	(0.037)	(0.062)
Obs. (women)	1202310	1202310	1202310	1202310	1202310
Country-year cells	105	105	105	105	105
Country-year FE	✓	✓	✓	✓	✓
B. Alternative school quality measure					
S_{ic}	0.638***	-0.094***	0.177***	0.397***	0.592***
	(0.020)	(0.003)	(0.008)	(0.014)	(0.019)
$\tilde{S}_{ic} \times \tilde{L}_c$	0.082**	-0.007*	0.029**	0.052**	0.069**
	(0.032)	(0.004)	(0.012)	(0.022)	(0.031)
Obs. (women)	969897	1284171	969897	969897	969897
Country-year cells	79	79	79	79	79
Country-year FE	✓	✓	✓	✓	✓
C. Sub-national school quality measure					
S_ic	0.303***	-0.112***	0.167***	0.204***	0.274***
	(0.066)	(0.006)	(0.038)	(0.056)	(0.057)
$\tilde{S}\_ij \times \tilde{L}\_j$	0.200***	-0.014**	0.102**	0.119**	0.170***
	(0.069)	(0.007)	(0.041)	(0.051)	(0.066)
Obs. (women)	1194129	1194129	1194129	1194129	1194129
Region-year cells	1171	1171	1171	1171	1171
Region-year FE	✓	✓	✓	✓	✓
Country-year slopes	✓	✓	✓	✓	✓

The dependent variable is listed in the top row. The sample is restricted to women with twelve or fewer years of schooling. In addition to the controls listed, all regressions include controls for a cubic polynomial of the woman's age and the interaction of schooling with the cell average (country-year or region-year) of the dependent variable, per column (3) of Table 3. Regressions use survey weights within country-year cells and are weighted so that each country-year receives equal weight. Standard errors are clustered at the level of the fixed effects (country-year cells or subnational region-year cells). Variables marked with a tilde are demeaned: individual-level variabls (e.g., $\tilde{S}_{ic}$) are demeaned at the country level while country-level varialbes (e.g., $\tilde{L}_c$) are demeaned at the global level. The control for $\tilde{S}_{ic} \times \tilde{Y}_c$ denotes a control for the schooling term interacted with the average of the dependent variable at the country level.

7 Conclusion

An enormous literature demonstrates both the economic and broader social returns to girls' schooling. Yet school systems in many developing countries deliver very low quality education, as measured by learning gains. This juxtaposition suggests girls' schooling may be transformative even in the absence of real learning. Using DHS data and a variety of international learning assessments, we show that where learning levels are extremely low, the predicted social returns to schooling are still non-zero. Girls' schooling in and of itself appears to be a social good. However, where girls' schooling is synonymous with real learning, as measured by adult female literacy, the returns to schooling in terms of reduced fertility and child mortality are considerably higher.

The link between women's learning levels and their fertility rate or the mortality rate of their children is consistent with human capital theory, but the evidence here cannot entirely rule out alternative explanations based in signaling or credentialing in the labor market. In future revisions and extensions of this work, we hope to pursue two strategies to distinguish these theoretical mechanisms more clearly. The first is to exploit DHS data on socio-economic outcomes in women's adulthood to control for the labor market channel that might link schooling to fertility and child outcomes via increased incomes. Controlling for income differences would aim to 'turn off' the signaling route and thus focus on a pure human capital mechanism relating women's learning to their children's outcomes. Second, we note that our results here represent only associations in observational data. Future versions will explore techniques proposed by Altonji, Elder and Taber (2005) and Oster (2013) to bound the degree of selection bias in this relationship and pin down the causal inferences possibly suggested by the associations we document here.

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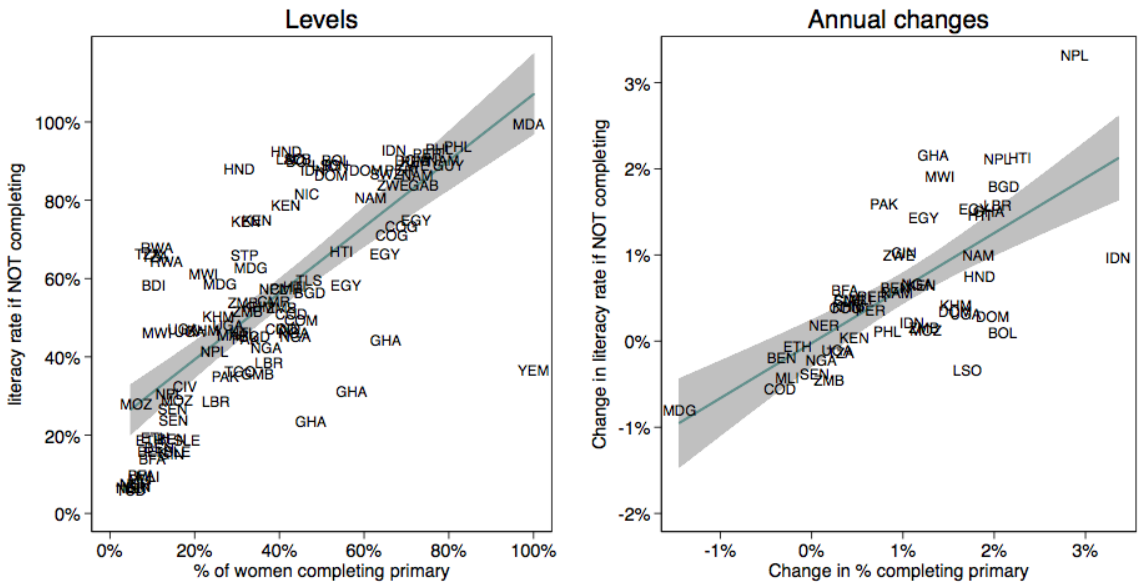
Figure 4: Evidence on the direction of sample selection bias in literacy rates measured among women with incomplete primary



The vertical axis measures the level or change in the percentage of women who are literate among those who did not complete primary school. The sample is restricted to women age 25-34. Each country appears up to three times, corresponding to waves 4-6 of the DHS. Survey dates vary by country; median survey year for wave 4 is 2004, 5 is 2007, and 6 is 2013. Annual changes are calculated as the percentage point change between waves divided by the (country-specifc) timespan between waves.

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