Working Paper 133

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An International Study of Childhood Poverty

Do Schools Reinforce or Reduce Learning Gaps between Advantaged and Disadvantaged Students?

Evidence from Vietnam and Peru

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Summary

This paper examines whether disadvantaged children learn less than advantaged children when both types of children are enrolled in the same school for two developing countries, Vietnam and Peru. This is done by estimating education production functions that contain two school fixed effects for each school, one for advantaged children and one for disadvantaged children. The paper examines six different definitions of disadvantage, based on household wealth, having low cognitive skills at age 5, gender, ethnic minority group (Peru only), maternal education, and nutritional status. The results show no sign of discrimination against disadvantaged groups in Vietnam; indeed if anything one advantaged group, males, seems to do worse in school than the corresponding disadvantaged group, females. In contrast, in Peru ethnic minority students and students who enter primary school with low cognitive skills appear to learn less in school than ethnic majority students and students with relatively high cognitive skills who are enrolled in the same school, respectively.

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About Young Lives

Young Lives is an international study of childhood poverty, following the lives of 12,000 children in 4 countries (Ethiopia, India, Peru and Vietnam) over 15 years. **www.younglives.org.uk**

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1. Introduction

Economists and other researchers have shown that investments in education can increase the rate of economic growth and, at the individual level, can increase incomes and the standard of living (Krueger and Lindahl, 2001). Developing countries have increased enrolment rates at both the primary and secondary levels in the past 25 years (Glewwe et al., 2013), which should increase economic growth and raise living standards. Yet there is evidence that students in many developing countries learn much less in school than their counterparts in developed countries (Glewwe and Kremer, 2006). Equally disturbing is the fact that some children appear to learn much more than others within developing countries. While it is almost impossible to eliminate inequality in student learning within any country, it may be possible to reduce within-country gaps in learning by implementing education policies that are particularly beneficial to students from disadvantaged backgrounds. Yet in some countries schools, and education systems more generally, may reinforce and thus widen learning gaps between advantaged and disadvantaged students (see, *inter alia*, Banerjee and Duflo, 2011),

Much of the existing literature on these issues focuses on learning gaps created by disadvantaged students' attendance at lower-quality schools (for example, McEwan, 2004; McEwan and Trowbridge, 2007). Recent evidence (reviewed in the next section) suggests, however, that there may be important differences in learning between children from different backgrounds even within the same schools. This paper utilises rich panel data from Peru and Vietnam to investigate such *intra*-school differences in learning in two distinct settings. Using six different indicators of advantage and disadvantage, the paper assesses whether advantaged students learn more than disadvantaged students when both types of student attend the same school. The results indicate that whether or not this occurs depends on the type of disadvantage and on the country context. The paper also investigates which school characteristics appear to have positive effects on student learning and, more importantly, which appear to increase or reduce learning gaps between advantaged and disadvantaged students in these two countries. Comparison of Vietnam and Peru is of particular interest, given the stark difference in their performance on the recently released 2012 PISA comparisons (OECD, 2013): among the 65 countries in that study, Vietnam ranked 17th in mathematics and 19th in reading (ahead of both the USA and the UK), while Peru ranked last among all 65 countries in both maths and reading. This difference is particularly striking given that income per capita in Peru in 2012 ($6,060) was four times higher than in Vietnam ($1,550) (World Bank, 2013).

More specifically, this paper estimates the combined impacts of all school and teacher characteristics on student learning by estimating separate fixed effects for advantaged and disadvantaged students within each school. For both countries, the relative sizes of these two fixed effects are compared for six definitions of disadvantage – low wealth, low skills before entering Grade 1, being female, membership of an ethnic minority, low maternal education, and early childhood malnutrition – to examine whether advantaged children learn more (or less) than otherwise comparable disadvantaged children within the same schools. The differences in school fixed effects between advantaged and disadvantaged children are then regressed on school and teacher characteristics, to investigate which characteristics are more favourable to learning among disadvantaged children, and which are more favourable to advantaged children.

This paper advances the literature on student learning in developing countries in three ways. First, the rich data set, which links detailed individual- and household-level panel data extending back to early childhood to very detailed school surveys, reduces omitted-variable bias. Second, by estimating separate school fixed effects for advantaged and disadvantaged children, the paper allows for within-school heterogeneity in the impact of school and teacher variables on student learning, which is usually ignored in the literature. Third, the use of very similar data for two very different countries allows for another kind of heterogeneity, namely heterogeneity across countries in whether schools increase or reduce gaps in student learning.

The rest of the paper is organised as follows. Section II reviews the relevant literature, after which the next section describes the school systems in Peru and Vietnam, and Section IV describes the data. Section V explains the methodology, and Section VI presents the results. Section VII checks the robustness of the results, and the subsequent section offers extensions to the analysis. Section IX summarises the results and draws conclusions for education policy.

2. Literature review

Gaps in education outcomes between children from more and less socio-economically advantaged backgrounds are widely documented. Filmer and Pritchett (1999), using data from Demographic and Health Surveys conducted in 35 developing countries, show that, even in terms of basic education indicators such as enrolment and drop-out, most shortfalls in education are attributable to children in the bottom 40 per cent of the wealth distribution. The link between socio-economic background and schooling has also been found in both countries examined in this paper: Vietnam (Glewwe and Jacoby, 2004) and Peru (Jacoby, 1994).

A large literature considers why children from advantaged backgrounds almost always have more favourable educational outcomes than disadvantaged children. This literature focuses mainly on differences in the home and school environments, which can affect learning through a number of mechanisms, including the opportunities for good-quality education, household demand for and attitudes towards education, and the capacity of children to learn, which may be impaired by children's exposure to adversity (Duncan and Murnane, 2011).

Child-background characteristics that seem to matter include family income, parental education and early childhood nutritional status (Glewwe and Miguel, 2008; Behrman, 2010; Alderman and Bleakley, 2013). While there is ample evidence that school and teacher quality affect education outcomes across many contexts (for example, Aaronson et al., 2007; Altonji and Mansfield, 2011; Lai et al., 2011), there is little consistency in the available evidence on exactly what constitutes school quality. Glewwe et al. (2013), reviewing studies of developing countries conducted over the past 20 years, conclude that beyond the finding that basic inputs (such as desks, high-quality walls and roofs, a school library, teacher attendance and knowledge of the subjects that they teach) matter, much of what matters is unlikely to be observable.

Whatever it is that constitutes school quality, the literature finds that children's access to it varies by their socio-economic backgrounds, and that this explains at least some of the differences in their educational outcomes. For example, studies in the UK and USA find that gaps in learning between children from more and less advantaged backgrounds widen during their time in school, and that differences in school quality explain at least part of this trend (Currie and Thomas, 2001; Fryer and Levitt, 2004). There is also some developing-country evidence: two recent studies find that a significant proportion (for example, 50–69 per cent in Guatemala) of the large learning gap between indigenous and non-indigenous children is due to differences in school quality across several Latin American countries, including Guatemala, Bolivia and Chile (McEwan, 2004; McEwan and Trowbridge, 2007).

Much of the literature focuses on differences between schools attended by advantaged and disadvantaged children. Our paper builds on a newer, smaller literature which considers *within-school* differences in learning by children from different backgrounds. Banerjee and Duflo (2011) suggest that this may be an important source of education inequality in many developing countries. While we know of no studies which directly consider the effect on attainment gaps of within-school differences in the educational experiences of children from different backgrounds, there is evidence of within-school differences in the treatment of, and attitudes towards, children from different backgrounds that is consistent with such an effect.

For example, Hanna and Linden (2012) randomly assigned child characteristics to completed examinations in India and showed that exams assigned to low-caste children were scored significantly lower than exams assigned to high-caste children. Similar discrimination along lines of race has been found in Brazil by Botelho et al. (2010), and Dee (2005) shows that in the US South, especially among disadvantaged students, racial and ethnic distance between teachers and pupils affects teachers' perceptions of student performance. Other studies have found that discrimination may lower pupils' performance through 'stereotype' threat. Hoff and Pandey (2006) show that, in India, a significant caste gap emerges when the caste of school-age task participants is revealed. This is consistent with the emphasis on identify by Akerlof and Kranton (2010), *inter alia*, who argue that within a school the dominant group's norms can adversely affect the behaviour and performance of 'outsiders'. These striking findings motivate our focus on *within-school* differences in learning among children from advantaged and disadvantaged backgrounds in two very different developing countries.

Unfortunately, there are several challenges to robust identification of the effects of child background and school factors on educational outcomes. Perhaps the most pervasive challenge is that of fully capturing all relevant inputs. While some can be observed, many, such as child ability or teacher motivation, cannot; this may lead to omitted-variable bias in estimates of the effect of observable inputs. Another possible problem is endogenous programme placement, if differences in the provision of school inputs are driven by differences in education outcomes. Studies based on observational data often apply multiple approaches to reduce these concerns, including instrumental variables and using observable proxies for potential omitted variables (see Glewwe and Kremer, 2006, for an overview). A common method to fully capture school inputs is to use school, class, or teacher fixed effects to control for the contribution of these school characteristics (Rivkin et al., 2005; Aaronson et al., 2007; Rothstein, 2010); yet this is of little use to policy makers who must select specific school policies and interventions.

In this study, the design and richness of the Young Lives data enable us to minimise concerns for many of these identification challenges. We discuss our methodology in detail in Section V after an overview of education in Vietnam and Peru, and a description of our data.

3. Overview of the school systems in Vietnam and Peru

Vietnam and Peru provide a particularly interesting comparison of two education systems. Although both have experienced the recent expansion in primary-school enrolment that has occurred in many developing countries (Glewwe et al., 2013), Vietnam has been more successful than Peru in ensuring that the expansion has been equitable.

Primary-school attendance has been compulsory for many years in both Vietnam and Peru. In Vietnam, the Law on Universal Primary Education, adopted in 1991, made primary education (Grades 1–5) compulsory for all children aged between 6 and 14. The national target of universal access to primary education was essentially achieved by 2010, with the net enrolment rate reaching 98 per cent (World Bank 2012). Peru's 2003 General Education Law (*Ley General de Educación*) also set a goal for compulsory basic education, and Peru has also experienced striking increases in primary-school enrolment in recent years, reaching a net enrolment rate of 95 per cent in 2010 (World Bank 2012).

Education quality is a central concern in both counties. Vietnam has made substantial investments in primary-school facilities, teacher training, and curricular and textbook reform, and has increased the hours per day of formal instruction, which are among the lowest in Asia. Vietnam's high rank in the 2012 PISA tests suggests that these efforts have had some success, while Peru's recent low rank in those tests indicates less success in raising education quality.

There is also evidence of large inequities in educational provision in these two countries. In Vietnam, several new programmes, such as SEQAP (School Education Quality Assurance Programme) and PEDC (Primary Education in Disadvantaged Communes), which are intended to ensure provision of 'minimum standards' of quality to all, especially those in disadvantaged areas (World Bank 2004; UNESCO 2011), show a commitment to ensuring that the expansion in enrolment does not widen inequalities in student learning. Concern remains in Vietnam about growing inequity, especially in grade progression and learning, in terms of income, location and ethnicity (World Bank 2011); however, comparisons with other developing countries suggest that, for its income level, the distribution of educational outcomes in Vietnam is quite equitable (Holsinger, 2005). In contrast, in Peru there are significant gaps in pupils' access, grade progression and learning related to socio-economic group, ethnicity and location (urban/rural and accessible/remote) (Murray 2012). Further, Crouch et al. (2009) show, using the ratio of performance at the 95^{th} to performance at the 5^{th} percentiles to measure achievement inequality, that Peru has the largest ratio of all participating countries. They also find that inequality of student learning in Peru is higher than in other countries with similar levels of income inequality. The authors hypothesise that this educational inequality may be driven by racial discrimination and linguistic barriers.

These differences between Vietnam and Peru suggest that their education systems have different effects on learning gaps between advantaged and disadvantaged students. We use comparable data to examine these effects for a relatively high-performing and equitable education system (Vietnam) and a much more unequal, less well-performing system (Peru).

4. Data

This paper uses data from the Young Lives study, a multi-country longitudinal study of child poverty which tracks approximately 3,000 children in each of four developing countries: Ethiopia, India (Andhra Pradesh), Peru and Vietnam. In each country two cohorts of children are followed: 1,000 children born in 1994–95 and 2,000 children born in 2000–01. This study uses data only from the cohort born in 2000–01, which is known as the 'younger cohort', and it focuses on Peru and Vietnam, where school surveys have recently been conducted.[1]

The sampling of the 2,000 younger-cohort children in each country involved random selection of 100 children who were 6–18 months old in 2001–02, from each of 20 sites. Thus the data are statistically representative of the site-level populations, but strictly speaking they are not nationally representative. However, the 20 sites in each country were purposely selected to represent diversity within each country on key socio-economic, demographic and geographic dimensions, except that the wealthiest areas were excluded from both country studies.

The Young Lives study collects data at both the household and school levels. To date, three rounds of household level data have been collected (in 2002, 2006–7 and 2009), and school-level data were collected in 2011 for a sub-sample of the younger-cohort children in Vietnam and Peru,[2] as explained below. The 2011 school surveys had somewhat different designs in each country, in order to reflect differences in schooling systems and in policy and research priorities. Nonetheless, all school surveys include child-assessment tests in reading comprehension and mathematics; key indicators of school quality (infrastructure, facilities and resources); and teacher knowledge, training and experience.

In Vietnam, the school survey was conducted in October 2011 in all 20 Young Lives study sites. The sample consisted of all younger-cohort children enrolled in Grade 5 (the appropriate grade, given their age) of primary school in the 2011–12 school year; all schools attended by these children were surveyed, so the sample represents a grade cohort.[3] Data were collected from 92 schools and 1,138 children from the younger-cohort sample in Vietnam.

In Peru, the school survey was conducted from October to December 2011, in nine of Peru's 14 regions. The sampling design was stratified, using four school types as strata to ensure that each school type – private urban, public urban, public rural bilingual medium and public rural non-bilingual (Spanish only) medium – is represented. A random sample was drawn of younger-cohort children attending each of the four school types, and their schools constituted the school sample (all younger-cohort children in those schools were surveyed). The resulting sample in Peru consists of 132 schools, attended by 572 children from the younger-cohort sample. Most of these children are in Grade 4 (59 per cent), while 32 per cent are in Grade 5 and 9 per cent in Grade 3. After excluding a relatively small number of children for whom there were incomplete school or household data, this paper employs data for 547 younger-cohort children in 132 schools in Peru and 1,129 younger-cohort children in 90 schools in Vietnam.

1 School data are not yet available for Ethiopia; school data are available for India, but, due to the large number of small schools attended by the younger-cohort children, the data are not well suited to identify school fixed effects.

2 In 2011, these children were about ten years old. They typically started primary school in or around 2006.

3 Classroom peers of the younger-cohort children were also sampled in both countries. Those data are not used in this paper.

Child learning (the outcome of interest) is measured by using scores on two curriculum-based tests, one of mathematics and the other of reading comprehension, administered as part of the school surveys. In Vietnam, both the maths and Vietnamese tests consisted of 30 multiple-choice items which reflected the Grade 5 curriculum. They were designed to be similar to tests used in the Ministry of Education's Grade 5 Assessment Study (World Bank 2011). In Peru, grade-specific maths and reading tests with 30 to 35 items were developed by GRADE (*Grupo de Análisis para el Desarrollo*) to reflect the curricula in Grades 3, 4 and 5.[4]

Since the tests were designed to reflect each country's grade-specific curriculum, they are not comparable across countries. However, the Peru tests included common items across grades, thus allowing one to link tests in different grades using Item Response Theory to create scores that are comparable across grades.[5] The children's background characteristics for both countries include individual, parental and household characteristics measured in the first two rounds of the household survey, when the children were aged 1 and 5, respectively, and thus before they started school. These are discussed further, with some descriptive statistics, below.

5. Methodology

This paper uses data from two countries with very different school systems, Vietnam and Peru, to examine whether there is within-school variation in the impact of school characteristics by student background. This section explains the estimation methodology, starting with the equations to be estimated and then turning to specific estimation issues.

a) Equations to be estimated

To begin, consider a general production function for cognitive skills (S) for primary-school students:

$$S = S(S_p;\ N_p,\ N,\ PE,\ PT,\ EI,\ IA;\ \mathbf{SC},\ \mathbf{TC}) \qquad (1)$$

In this equation, S_p denotes 'preschool' skills, that is skills that students have when they start primary school around the age of 5; N_p denotes 'pre-school' nutritional status, that is nutrition from birth to age 5; N is current nutritional status (while in primary school); PE is parental education; PT is (current) parental time spent with the child on activities which foster cognitive skills when the child is in primary school; EI is purchased educational inputs when the child is in primary school, such as textbooks, school supplies and tutoring lessons; IA is the child's innate ability; and **SC** and **TC** are vectors of school and teacher characteristics, respectively.

The overview of the education-quality literature by Glewwe et al. (2013) shows how difficult it is to estimate the impacts of school and teacher characteristics on student learning. While in the poorest settings basic infrastructure (chairs, desks, roof, etc.) and trained teachers usually have explanatory power, once the basics are in place the marginal benefits of additional improvements in these dimensions appear to be relatively low. In contrast, teacher

4 The English versions of the complete set of tests and questionnaires used in the Vietnam and Peru school surveys are available from the Young Lives website: http://www.younglives.org.uk/what-we-do/school-survey.

5 See Das and Zajonc (2010) for details of the application of Item Response Theory for this purpose.

attitudes and motivation, as well as school-management processes and organisation, are more likely to be able to explain variation in school and classroom 'quality', but are very difficult to measure.

While these findings may seem discouraging for the first objective of this paper – to assess whether schools reinforce or reduce gaps in learning outcomes between advantaged and disadvantaged students – there is no need to estimate the impact of each school and teacher characteristic on student learning. Instead, one can use a summary measure of school quality which captures both easy and more difficult (if not impossible) to measure characteristics of the school, using school fixed effects. That is, one can use a linear approximation of equation (1):

$$S = \beta_0 + \beta_1 S_p + \beta_2 N_p + \beta_3 N + \beta_4 PE + \beta_5 PT + \beta_6 EI_3 + \beta_7 IA + \gamma_1'\mathbf{SC} + \gamma_2'\mathbf{TC} + u \qquad (2)$$

and then combine the impacts of school and teacher variables into school fixed effects:

$$S = \beta_1 S_p + \beta_2 N_p + \beta_3 N + \beta_4 PE + \beta_5 PT + \beta_6 EI + \beta_7 IA + \sum_{s=1}^{S} \delta_s D_s + u \qquad (3)$$

where $\delta_s = \beta_0 + \gamma_1'\mathbf{SC}_s + \gamma_2'\mathbf{TC}_s$, and D_s is a dummy variable for school s. Note that each δ_s includes β_0; this implies that one can estimate only the *relative* impact, not the *absolute* impact, of each school on student learning. The residual, u, includes two distinct phenomena: (a) errors due to the linear approximation, and (b) measurement errors in S; both are assumed to be uncorrelated with the explanatory variables in equation (3).

Equation (3) has two important characteristics. First, no additional assumptions were imposed on (2) to obtain (3); the latter simply converts each school's $\gamma_1'\mathbf{SC} + \gamma_2'\mathbf{TC}$ term into a school fixed effect. Second, these school fixed effects can incorporate all possible interactions between **SC** and **TC** variables, which implies that (3) is more general than (2).

The disadvantage of estimating equation (3) instead of equation (2) is that (3) does not reveal which specific school characteristics contribute to student learning. The advantage, however, is that (under certain assumptions) this measure captures both observable and unobservable school characteristics which explain variation in education outcomes and so can help one to answer questions about the role of school (and teacher) characteristics in explaining pupil attainment. This method has been used in the literature concerning the economics of education to study both school and teacher quality (Aaronson et al., 2007; Rivkin et al., 2005; Rockoff, 2004).

To date, the use of school fixed effects in the literature has assumed that there are no interaction terms between either observed or unobserved school variables and the student and household variables. Thus the validity of this method hinges on the assumption that within schools all students receive the same benefit from what these schools offer, regardless of the students' specific characteristics. To our knowledge, this assumption has never been relaxed. Yet it may not be valid in all settings, given the evidence, discussed above in Section II, of differences in performance among students within the same schools, due to discrimination by caste and race or to adverse effects of 'stereotype threat'.

One way to allow school and teacher characteristics to have different impacts across students in the same school is to divide students in each school into two or more groups and allow each group to have a distinct fixed effect for each school. In particular, suppose that children can be classified as 'advantaged' and 'disadvantaged'. Then rewrite equation (3) as:

$$s = \beta'\mathbf{X} + \sum_{s=1}^{S} \kappa_s D_s + \sum_{s=1}^{S} \theta_s D_s A + u_3 \qquad (4)$$

where A is a dummy variable indicating students who belong to an 'advantaged' group, and, to reduce clutter, $\beta'\mathbf{X}$ denotes $\beta_1 S_p + \beta_2 N_p + \beta_3 N + \beta_4 PE + \beta_5 PT + \beta_6 EI + \beta_7 IA$. The term κ_s measures the impact of school s on disadvantaged students, and $\kappa_s + \theta_s$ measures that school's impact on advantaged students. If schools contribute equally to the learning of advantaged and disadvantaged students, then $\theta_s = 0$ (and each κ_s in (4) equals the corresponding δ_s in (3)).

b) Estimation issues

Equations (3) and (4) will be estimated using the Young Lives data from Peru and Vietnam. However, several complications arise concerning these estimations. This section explains these problems, and the methods used to address them.

Perhaps the most pervasive estimation problem is omitted-variable bias. Any child and household variables in equations (3) and (4) that are not in the data become part of the error term and so may cause it to be correlated with the observed variables in these equations, which can lead to biased estimates of all the coefficients.

Fortunately, the Young Lives data contain detailed information dating back to when the students were about one year old, allowing one to include many, if not most, of the relevant variables. In addition, controls are included for household wealth before the child started school which is used to purchase many of the inputs that may not be captured in the data. Technically, this implies that equations (3) and (4) are no longer 'pure' production functions, because other inputs in them, such as parental education and child ability, may also affect the purchase of educational inputs. But this should have little effect on the estimates of the school fixed effects in these equations. A final point is that any remaining omitted child- or household-level variables that may be correlated with observed child and household variables will not lead to biases in estimated school fixed effects if they are uncorrelated with school fixed effects; such variables lead to biases only in the estimated impacts of child and household variables, the estimation of which is not the goal of this paper.

Perhaps the most important omitted variable is that the Young Lives data have no direct measures of innate ability (IA) in equations (3) and (4). The absence of this variable could bias estimates of the structural impacts of observed variables, because some of those variables may be correlated with IA, such as S_2 and PE, and, potentially, the type of school that the child attends. Among the more unique features of the Young Lives data is the availability of measures of children's cognitive skills *before* they started primary school. These are used as the S_2 variables; the approach taken here is to assume that S_2 includes most of the impact of innate ability on S_3. Omitted-variable bias due to lack of data on that variable should then be minimal.[6] We return to this issue below when discussing the specific measures of S_2.

6 The estimated model is similar to the combined cumulative-inputs value-added model favoured by Todd and Wolpin (2007) in their cognitive-achievement production-function estimates. A pure cumulative-inputs model includes only the history of inputs and assumes that observed inputs fully account for all inputs. In contrast, a value-added model includes only current inputs and last period's test scores. The identifying assumption is that the lagged test score fully captures the effect of the complete history of inputs and endowments. In the mixed cumulative value-added model, we estimate that identification relies on a weaker assumption than in either of these models – namely that combined lagged test scores and observed inputs fully capture effects of all past inputs.

Concerns about omitted-variable bias are further reduced because the aim of this paper is to examine differences between groups, rather than level effects. Thus unless the relevant omitted variables are systematically different between 'advantaged' and 'disadvantaged' children, they should not bias the estimates of θ_s. That is, our analysis is more robust to omitted variables than would be the case if we were estimating, say, the level effects of school inputs. One way to explore the sensitivity of our estimates to omitted variables is to compare them with those from models based on stronger assumptions, such as a pure value-added specification or a pure cumulative-inputs model (cf. note 6). This is done in the robustness analysis (Section VII).

There is another identification issue concerning equation (4) that has implications for interpreting the θ_s coefficients, and also relates to omitted-variable concerns. To see this issue, suppose that the dummy variable A in equation (4) equals one for all 'ethnic majority' children, who in Peru and Vietnam are advantaged (perform better in school), while ethnic-minority children are disadvantaged (have relatively low school performance). Even with a very rich set of **X** variables, it may be that a dummy variable for ethnic-majority children is statistically significant when included as an **X** variable, so that A should be part of **X**. Yet the A variable equals $\sum_{s=1}^{S} D_s A$, so the $\boldsymbol{\beta}$ coefficient corresponding to A is not identified.[7] In particular, each θ_s is the sum of the school fixed effect that affects *only* advantaged children and the general 'non-school' advantage that those children have after controlling for other **X** variables.

This paper focuses on the differential impact of school characteristics on advantaged and disadvantaged children, so the estimation strategy to minimise the extent to which θ_s includes a general 'non-school' advantage is to use as many **X** variables as possible to capture all 'non-school' advantages of being an advantaged child. Thus **X** includes conditions in the first year of life (collected in 2002) and in the next four years of life before starting school (collected in 2006), including time spent with mother, maternal assessment of child size at birth, maternal mental health, parental education, child nutrition, number of siblings, pre-school attendance, physical growth, and per capita expenditure (which may be spent on educational inputs). In addition, **X** includes scores of tests taken at age 5, to control for all unobserved 'advantages' of advantaged children that raise their cognitive skills by age five (before they start primary school). These cognitive assessments before the children started school are a novel feature of the data which allow us to capture much more of the pre-school advantage that raises subsequent educational outcomes than would be possible with only the observable inputs. This should minimise the component of each θ_s due to unobserved *non-school* advantages enjoyed by advantaged children.[8]

Another issue is that θ_s can be identified only for advantaged children who attend schools that are attended by at least one disadvantaged child from the school survey sample. This may lead to bias in the estimates of the school effects, due to unobservable selection into schools with both types of children, and may also have implications for the generalisability of the results. We examine this in Section VII, which checks the robustness of the main results.

A final estimation problem is that some explanatory variables in equations (3) and (4) could have measurement error, such as the S_p, N and PT variables. In principle, instrumental variables are needed for these variables, but finding suitable instruments is a challenge. Since

7 Even if A were a continuous variable, the school fixed effects multiplied by A, i.e. the DsA variables in (4), would still sum to that same continuous variable in the X vector, and the identification problem remains.

8 We return to this issue in discussion of the results below.

this paper focuses on the impact of school characteristics, not child or household characteristics, on student learning, IV methods are not used to estimate equations (3) and (4).

c) Extensions

While this paper focuses on estimating equations (3) and (4), the school fixed effects estimated in those equations can also be used to investigate which school and teacher characteristics contribute to student learning. If one constrains the school fixed effects to be identical for advantaged and disadvantaged children, the equation to estimate is:

$$\delta_s = \gamma_0 + \gamma_{11}SC_1 + \gamma_{12}SC_2 + \ldots + \gamma_{21}TC_1 + \gamma_{22}TC_2 + \ldots + u_s \qquad (5)$$

where SC_1, SC_2, etc. are the elements of the school-quality vector **SC**, and TC_1, TC_2, etc. are the elements of the teacher-quality vector **TC**.[9]

If advantaged children benefit, or suffer negative consequences, from being enrolled in particular schools, one would like to know which school and teacher characteristics are most likely to help, or hinder, advantaged children; those that favour advantaged children widen learning gaps between advantaged and disadvantaged children, while those that hinder them (that is, that favour disadvantaged children) reduce those gaps. This can be investigated by regressing, at the school level, the school effects estimated in equation (4) on observed school characteristics. Thus equation (6) investigates which school characteristics benefit disadvantaged children, and equation (7) does the same for advantaged children:

$$\kappa_s = \gamma_{d0} + \gamma_{d11}SC_1 + \gamma_{d12}SC_2 + \ldots + \gamma_{d21}TC_1 + \gamma_{d22}TC_2 + \ldots + u_{ds} \qquad (6)$$

$$\kappa_s + \theta_s = \gamma_{a0} + \gamma_{a11}SC_1 + \gamma_{a12}SC_2 + \ldots + \gamma_{a21}TC_1 + \gamma_{a22}TC_2 + \ldots + u_{as} \qquad (7)$$

Most relevant for this paper are the school characteristics that have *different* effects on advantaged and disadvantaged children, which can be estimated by:

$$\theta_s = \gamma_{\theta 0} + \gamma_{\theta 11}SC_1 + \gamma_{\theta 12}SC_2 + \ldots + \gamma_{\theta 21}TC_1 + \gamma_{\theta 22}TC_2 + \ldots + u_{\theta s} \qquad (8)$$

In equation (8), school (teacher) characteristics favouring advantaged pupils will have positive $\gamma_{\theta 1}$ ($\gamma_{\theta 2}$) coefficients, and those favouring disadvantaged pupils will have negative coefficients.

An important point about regression estimates of equations (5)–(8) is that the inability to distinguish between θ_s and unobserved non-school effects of advantage on student learning will not affect these estimates. To see why, note that if all θ_s coefficients in equation (8) contain not only the differential impact of school and teacher characteristics on pupil learning but also part or all of a ‘general’ impact of being advantaged unrelated to school and teacher characteristics, that simply adds the same constant to all the θ_s coefficients and so will not affect estimates of the γ_θ coefficients (except the constant term) in equation (8).[10] Note that the fact that δ_s, κ_s and θ_s are estimated values, not the ‘true’ values, does not necessarily bias OLS estimates of (5)–(8); random deviations from the true values simply add to the error terms.

9 The regression in equation (5) suggests that it may be simpler to insert this expression for $_s$ into equation (3) and estimate that regression, which essentially estimates equation (2). This is not done, because such a regression assumes that all relevant school characteristics are observed, while the school fixed effects in equation (3) include both observed and unobserved school and teacher characteristics. Another reason not to follow this suggestion is that it would complicate attempts to determine whether schools favour advantaged or disadvantaged children.

10 For the same reason, this will affect only the constant terms in equations (5), (6) and (7).

A more serious problem is omitted-variable bias in estimates of equations (5)–(8). Quite simply, many aspects of school and teacher quality may not be measured in the Vietnam and Peru school surveys, leading to biased estimates of those equations if unobserved teacher and school variables are correlated with the observed variables, which is likely. While such bias cannot be avoided, it should be minimal, since the school surveys in each country collected a vast array of information on teachers and schools, as described in discussion of the results.

6. Results

This section presents the estimates for Vietnam and Peru for the maths and reading-comprehension tests. After a description of the control variables used in the specifications, the first sub-section presents estimates of the determinants of learning (that is, equation (3)). The second sub-section then examines evidence of within-school differences in learning between children from more and less advantaged backgrounds, estimating equation (4) for different definitions of 'advantage'. Additional analysis in Section VIII examines the school (including peer), teacher and principal characteristics that underlie the school fixed effects in equations (3) and (4) by presenting estimates of equations (5) and (8).

Table 1 presents summary statistics for the outcome measures in equations (3) and (4), as well as for the main control variables.[11] As noted in Section V, this paper's primary aim is to explore differential impacts of school characteristics on children from advantaged and disadvantaged backgrounds. To minimise the extent to which estimates of the school effects include effects of non-school factors, the specifications for equations (3) and (4) include many time-invariant characteristics of the child and his/her environment, as well as conditions in the first year of life and in the next four years of life (before the child started school). Together, these variables should capture the main inputs in the skill-production function in equation (1), including 'innate ability', which (as discussed in the previous section) we control for by using pre-school measures of cognitive skills. The following paragraphs describe these variables.

11 Appendix Table 1 compares the characteristics of the sub-sample of children included in the school survey with the whole Young Lives sample. While the sub-sample of children from Peru appears similar to the entire younger-cohort sample for almost all background characteristics, the Vietnam school sample appears on average better off than the full sample: the children in the school-survey sample are less likely to belong to ethnic minorities, and are more likely to have better-educated parents, to have smaller households, to be a bit older, to have performed better on the cognitive tests at age 5, and to have had more time in pre-school. These differences are unsurprising, as the Vietnam school-survey sub-sample included only children in Grade 5 (the grade expected for the Young Lives younger cohort at the time) and so omits children less likely to be in that grade, who were the youngest children in the cohort, and children from less-advantaged backgrounds who thus were less likely to start school on time and not repeat a grade.

Table 1: *Sample Descriptive Statistics*

	Vietnam		Peru	
	Mean	SD	Mean	SD
From School Survey				
Mathematics IRT test score (whole sample mean = 500, sd=100)	499.19	96.48	508.60	102.65
Language IRT test score (whole sample mean = 500, sd=100)	497.44	96.69	505.77	99.69
Age in months at the time of the test	123.24	2.71	122.16	3.87
From Household Survey (time-invariant variables)				
Male (dummy)	0.51	0.50	0.48	0.50
Ethnic minority (dummy)	0.08	0.27	0.36	0.48
Dad: Years of schooling	7.69	3.53	9.23	3.77
Mum: Years of schooling	7.17	3.30	7.84	3.11
Measured during Infancy (Round 1)				
Birth size (maternal assessment: 1 (very large) to 5 (very small)	3.08	0.66	3.11	0.96
Health better than other children (dummy, maternal assessment)	0.26	0.44	0.41	0.49
Child care: regularly looked after by people outside the household / in crèche (dummy)	0.42	0.49	0.21	0.41
Maternal mental health (stress/depression: score out of 20, higher score indicates higher stress level)	4.4	4.02	5.78	4.26
Wealth index	0.47	0.20	0.43	0.19
Measured at Age 5 (Round 2)				
Height for age z-score	-1.30	0.97	-1.53	1.02
PPVT IRT Score (whole sample mean=300, sd=50)	305.33	44.33	301.34	44.84
CDA IRT Score (whole sample mean=300, sd=50)	306.80	46.56	299.35	46.39
Time spent in pre-school (hours per day)	5.79	2.45	3.44	1.83
Only child (dummy)	0.22	0.42	0.19	0.40
Household size	4.57	1.39	5.52	2.23
Log per capita real consumption (in local currency)	5.77	0.54	5.02	0.67
Area of land owned (in hectares)	0.42	1.07	1.09	3.34
Number of Observations	1,129		547	

Notes:
Wealth index constructed using measures of housing quality, access to key services and ownership of durables.
Peabody Picture Vocabulary Test (PPVT) is a measure of receptive vocabulary.
Cognitive Development Assessment (CDA) is designed to assess cognitive development of pre-school-age children.

In both countries the children were about ten years old when they took the tests used as outcome measures. The test scores were transformed using a three-parameter IRT (Item Response Theory) model, with means and standard deviations standardised to 500 and 100, respectively.[12] The tests differed across the two countries, so the scores are not comparable.

To control for circumstances in infancy, we include maternal assessment of the child's size at birth (a five-point scale from very small to very large) and of the child's health at age 1 relative to other children of a similar age.[13] We also add controls for time spent away from the mother at age 1 (whether the child was looked after by someone outside the household,

12 The scores and standard deviations in Table 1 are not exactly 500 and 100, respectively, as the standardisation was done for the whole school-survey sample, including school peers, who are not included in the analysis.

13 We exclude an indicator of child nutrition in infancy (height-for-age), as it is highly correlated with child nutrition at age 5, which is included as one of the child characteristics.

including attendance at a crèche), a practice more widespread in Vietnam than in Peru, as well as a measure for the mother's mental health. We also include a household-wealth index[14] to proxy for the economic situation of the household during the child's infancy.

The second round of data, collected in 2006, is used to capture conditions just before the children started school. Most importantly, we add their scores on two separate cognitive tests administered as part of the survey to control for their skills at the age of entry into primary school. The first is the Cognitive Development Assessment (CDA), which measures children's basic quantitative skills,[15] and the second is the Peabody Picture Vocabulary Test (PPVT), a widely used measure of receptive vocabulary. Both tests have been validated in many contexts and shown to be highly correlated with broad-based measures of IQ,[16] which implies that they are suitable proxies for 'innate ability' in equation (1). For both tests, an IRT model was used to generate test scores that (for the whole Young Lives sample) were standardised to have a mean of 300 and standard deviation of 50. We also include child nutrition (measured by height-for-age z-scores, computed using WHO standards), which is likely to be correlated with ability (Glewwe and Miguel, 2008), and time spent in pre-school. Finally, we control for some circumstances capturing the household's capacity to provide various non-school inputs, including whether the child had any siblings, household size, parental education, consumption (to measure permanent income), and land ownership.

a) Estimates of determinants of learning

Table 2 presents estimates of equation (3) for Vietnam and Peru, separately for maths and reading comprehension. Recall that this equation applies the same school fixed effects to all pupils in each school, while equation (4) has separate school fixed effects for advantaged and disadvantaged children in each school.[17]

14 This is constructed using measures of housing quality, access to key services, and ownership of durables.

15 The CDA is a 15-item instrument developed by the International Evaluation Association to assess cognitive development of 4-year-old children. The sub-scale administered to the Young Lives children links to perceptions of quantity by testing children's understanding of concepts such as *few, most, half, many, equal,* and *pair*.

16 For details of the tests, their psychometric properties and construction of the scores, see Cueto et al. (2009).

17 The analogous estimates for the different versions of equation (4) for different definitions of advantage yield very similar estimated impacts of non-school (child and household) variables.

Table 2: *Determinants of Cognitive Skills at School in Vietnam & Peru (school fixed effects)*

	Vietnam		Peru	
	Maths score	Vietnamese score	Maths score	Spanish score
Age at the time of the test in (months after 9 years)	0.149	-1.803*	3.512***	0.843
	(0.992)	(0.962)	(1.139)	(1.155)
Male	-12.850**	-31.343***	6.723	-8.258
	(5.124)	(4.974)	(7.706)	(7.811)
Ethnic minority	-35.382**	-44.439***	-2.164	-4.659
	(16.570)	(16.138)	(13.443)	(13.627)
Dad's education: log years of schooling	-11.918	4.212	-47.258	-30.141
	(14.569)	(14.167)	(32.836)	(33.284)
Dad's education: log years of schooling (squared)	8.245	2.963	17.794*	11.655
	(5.361)	(5.210)	(9.449)	(9.578)
Mum's education: log years of schooling	-6.668	3.766	13.901	-15.080
	(16.452)	(15.992)	(19.014)	(19.273)
Mum's education: log years of schooling (squared)	7.859	5.867	4.542	15.183**
	(5.895)	(5.729)	(6.926)	(7.020)
Characteristics Measured in Infancy (Round 1)				
Birth size (maternal assessment)	-1.656	1.531	1.289	-4.465
	(3.992)	(3.876)	(4.328)	(4.387)
Health better than other children (maternal assessment)	-2.768	-5.701	4.071	6.679
	(6.091)	(5.912)	(7.752)	(7.857)
Child care: regularly looked after by people outside the household / in crèche (dummy)	-9.458*	-11.221**	-11.108	-27.086***
	(5.465)	(5.308)	(9.785)	(9.918)
Maternal mental health (stress/depression)	-1.244*	-1.006	-0.496	-0.151
	(0.692)	(0.674)	(0.946)	(0.958)
Wealth index	-82.015	-21.634	105.103	355.228***
	(63.822)	(62.287)	(126.608)	(128.334)
Wealth index squared^			-89.793	-357.871***
			(132.184)	(133.987)
Characteristics Measured at Age 5 (Round 2)				
Height for age z-score	1.845	7.099**	7.785*	8.345*
	(3.039)	(2.950)	(4.304)	(4.362)
PPVT Score	0.289***	0.397***	0.169	0.218**
	(0.075)	(0.073)	(0.103)	(0.105)
CDA Score	0.189***	0.134**	0.200**	0.351***
	(0.069)	(0.067)	(0.097)	(0.098)
Time spent in pre-school	-1.099	-0.999	2.306	-2.213
	(1.819)	(1.769)	(2.565)	(2.600)
Only child	-8.862	1.960	-0.020	-2.497
	(6.896)	(6.694)	(10.691)	(10.837)
Household size	-1.276	-4.602**	-0.936	0.465
	(2.161)	(2.098)	(2.005)	(2.033)
Log per capita real consumption (in local currency)	0.735	1.936	2.508	6.081
	(6.662)	(6.466)	(7.771)	(7.877)
Area of land owned (hectares)	-1.021	0.768	2.298*	-0.284
	(3.362)	(3.263)	(1.261)	(1.278)
Constant	378.443***	371.654***	285.566***	236.970***
	(51.612)	(50.121)	(72.216)	(73.200)
Number of observations	1,129	1,129	547	547
Number of schools	90	90	132	132

Notes: *** p<0.01, ** p<0.05, * p<0.1; ^ wealth index squared was not significant in any models estimated for Vietnam and has therefore not been included in any of the final specifications for Vietnam.

Consider first the results for Vietnam, shown in the first two columns of Table 2. The 1,129 children in 90 schools include all younger-cohort children in Grade 5 in the 2011–12 school year. The first result to note is that the coefficients on both cognitive-skill variables are positive and highly statistically significant. A one standard deviation increase in the PPVT score at age 5 raises the maths test score at age 10 by 0.14 standard deviations, and the language test score by 0.20 standard deviations.[18] A one standard deviation increase in the CDA score at age 5 raises maths and language test scores at age 10 by 0.09 and 0.07 standard deviations, respectively. Including these two test scores in the set of explanatory variables should reduce the magnitude and significance of the other explanatory variables that measure inputs provided up to age 5.[19] Indeed, of the 11 other variables that measure inputs in the first year, or the next four years, of life (up to age 5), none is significant at the 5 per cent level in the maths equation, and only three are significant at that level in the language equation.

A few other variables have significant effects on cognitive skills. Consider child age at the time of the test, seen in the first row of Table 2: one would expect older children, other things being equal, to have higher skills, as they have had more time for general cognitive development. Indeed, the estimated impact is positive, but statistically insignificant, for the maths test. Yet the impact is negative and marginally significant for the reading test; a possible explanation is that since only children in Grade 5 (the appropriate grade for most of the sample) were included in the Vietnam school survey, the younger ones among them (their age ranges from nine years and four months to ten years and ten months) may also be the more able ones, either starting school unusually early or skipping a grade. Another possibly surprising finding is that, other things being equal, boys had lower scores than girls on both tests. Given that the standard deviations on both tests were normalised to 100 (see Table 1), the negative impact of being male on the maths test is modest, -0.13 standard deviations, but the negative effect on the reading test, -0.31 standard deviations, is fairly large. Even larger negative effects are seen for ethnic-minority children: their maths and reading scores are 0.35 and 0.44 standard deviations lower, respectively. While mothers' and fathers' levels of education have no significant impacts on learning in this specification, parental education does have positive and significant effects in specifications that exclude controls for cognitive skills at age 5, which suggests that the highly statistically significant Round 2 tests capture most of the effect of parental education on school performance.

Turning to variables measured when the children were one year old, we note that two of the five have significant impacts. First, children who were enrolled in child care at this age have lower maths and Vietnamese scores, effects that are statistically significant (10 per cent level for maths and 5 per cent for Vietnamese). This may reflect reduced contact with parents in very early childhood. Second, there is some evidence that children of mothers who had elevated levels of stress or were depressed when their child was one year old also performed less well on the maths and reading tests, although only the former effect is significant, and only at the 10 per cent level.

18 Recall that the standard deviations for the PPVT and CDA tests are normalised to be 50. Thus, for example, a one standard deviation increase in the PPVT test increases the score on the maths test by 14.45 (0.289×50) points, which is about 0.14 standard deviations for that test (since it has a standard deviation of 100).

19 As discussed in the previous section, the motivation for including the pre-school measures of skills is to minimise the possibility that estimates of school learning also measure non-school effects.

Regarding child characteristics at age 5, the primary-school entrance age, a few appear to have significant impacts on maths and reading skills at age 10. One important characteristic is early childhood nutritional status, which several studies (reviewed in Glewwe and Miguel, 2008, and Alderman and Bleakley, 2013) have found to have strong causal effects on learning. Height-for-age is a useful indicator of nutritional status since birth, so it is not surprising that the coefficients on this variable are positive for both maths and reading, although the impact is significant only for the latter. A final variable measured at age five which has a significant impact on learning is household size; its negative impact on student learning may indicate that the number of siblings reduces resources and parental attention for the child, but this impact is statistically significant only for the reading test.

Turning to Peru (columns 3 and 4 in Table 2), we see that, as in Vietnam, both the PPVT and the CDA scores generally have significantly positive effects on the maths and reading scores (the one exception is the impact of vocabulary at age 5 on maths scores at age 10; while positive, it is not quite statistically significant). As in Vietnam, including these variables results in very few significant impacts for variables measured at age 1 and age 5; for maths, none of these 11 variables is significant, and for reading (Spanish) only two are significant, at the 5 per cent level.

Examining the other variables, we see that the age when the test was taken has a positive effect, but is significant only for the maths test. Boys score somewhat better on the maths test and somewhat worse on the reading test, yet in contrast with Vietnam neither of these effects is statistically significant. Ethnic-minority children perform slightly worse on both the maths and the reading tests, but not significantly so. Another contrast with Vietnam is that even after controlling for pre-school cognitive ability, parental education has a positive effect on cognitive skills.

Of the five variables measuring child characteristics at age 1, two are significant. As in Vietnam, children enrolled in child care at this age scored significantly lower on both tests, and the impact on Spanish skills is significant and large (-0.27 standard deviations). Second, the wealth index has a positive but diminishing impact on Spanish scores, but not on maths scores. Turning to variables measured at age 5, we see that, as in Vietnam, (cumulative) nutritional status has a positive and weakly significant impact on both tests at age 10. Of the remaining variables, only land owned has a significantly (10 per cent level) positive effect, and only on maths skills.

b) Differences in school impacts between advantaged and disadvantaged children

The estimates that are of greatest interest in this paper are those of the κ and θ parameters in equation (4), because they can be used to test the hypothesis that the impacts of school and teacher characteristics on learning are, on average, larger for advantaged children. We use six different definitions of advantage. As reviewed earlier, there is evidence of within-school ethnicity/race discrimination in several contexts, including India and the USA. Belonging to the dominant ethnic group is thus one dimension of advantage that we consider.[20] We also consider five other child or household characteristics that may lead to an advantage in school, through, for example, a combination of perceptions of own capabilities

20 We are able to explore this dimension of advantage only in the Peru sample, as only 8 per cent of the younger-cohort sub-sample included in the Vietnam school survey belong to an ethnic-minority group.

and/or perceptions of and treatment by others, as in Akerlof and Kranton (2010); these include being in the top two wealth quintiles, the top two 'ability' quintiles, being male, having a mother who at least completed primary schooling, and having adequate nutrition in early childhood.[21]

While most of these types of advantage are positively correlated, Appendix Tables 2 and 3 show that their overlap is only partial. That these indicators represent advantage with respect to skill acquisition is seen in Table 3. For all six types of advantage, in almost all cases, the advantaged group performed significantly better on the PPVT and CDA tests taken at age 5, before the children started school, suggesting that advantaged children benefit from more favourable conditions for cognitive-skill acquisition before enrolling in primary school.

Table 3: *Difference in Pre-school Test Scores between More and Less Advantaged Children (advantaged–disadvantaged)*

	Vietnam		Peru	
	PPVT	CDA	PPVT	CDA
Being richer (top two wealth quintiles)	21.89***	13.29***	24.80***	30.94***
Being more able (top two Round 2 CDA quintiles)	30.06***	77.35***	11.23***	76.39***
Being male	5.99**	4.13*	-2.29	-1.92
Being a member of an ethnic majority	NA	NA	21.17***	15.19***
Having a mother with at least completed primary schooling	16.68***	6.33*	22.81***	18.17***
Having not been malnourished in infancy	10.74***	2.82	13.91***	-0.53

Notes: *** p<0.01, ** p<0.05, * p<0.1; Note that difference in CDA scores between more and less able children is there by construction, since being more able is defined as having a CDA score in the top two quintiles of the distribution at age 5.

We now examine whether evidence suggests that school effects are more favourable for these children, controlling for several non-school advantages and pre-school differences in cognitive skills. As seen in Section V, it is impossible to identify the coefficient on the advantage category (for example, allowing school fixed effects to differ for ethnic-minority and ethnic-majority children precludes one from including an ethnic-minority dummy in the regression), which raises concerns that the estimated school effects may capture some of the 'non-school' advantage associated with the advantage category, since that cannot be captured in the 'non-school' control variables. For the six advantage categories listed above, one can include close correlates for all advantage categories except being male and being from the ethnic majority.

In addition to the large set of controls, non-school advantage through wealth is captured by a measure of consumption, ability by PPVT and CDA test scores, and maternal education by log of maternal years of schooling. It is hard to think of a direct effect that gender or ethnicity would have on school achievement due to non-school factors not captured by skills measured at age 5 and household circumstances. Yet in the presence of such an effect, the estimates of school effects for these two definitions of advantaged may be biased, as we are unable to find controls for them. We return to this issue when discussing the results below.

21 The wealth indicator is described in note 14. 'Ability' quintiles are based on the CDA score at age 5, as this is a more general test of child development than the PPVT. Children not stunted (height-for-age z-score > -2) at age 1 (in round 1) are considered to have had adequate nutrition in infancy.

Equation (4) is estimated for those schools with at least one child from the advantaged group and at least one from the disadvantaged group. The samples therefore differ across the definitions of advantage; Appendix Table 4 shows the number of observations and schools for each definition.[22] In the Vietnam sample, 82 of the 90 schools are attended by two or more children in the sample. Of these 82 schools, 48 have children from both the poorer and richer groups, 61 include both more able and less able children, 77 have both boys and girls, 61 have children with both more educated and less educated mothers, and 63 have children who were and were not malnourished in infancy. Of the 132 schools in Peru, 80 are attended by two or more sample children. Of these 80, 36 have both richer and poorer children, 47 have both more able and less able children, 63 include both boys and girls, 25 have both ethnic-minority and ethnic-majority children, 48 have children with both more educated and less educated mothers, and 54 have children who were and were not stunted as infants. Limiting the analysis to schools with both advantaged and disadvantaged children raises concerns about selection bias in the estimates of $\theta_{s.}$ We return to this issue in discussion of robustness of the estimates. A final point about estimation of equation (4): while school fixed effects are allowed to vary across advantaged and disadvantaged children, coefficients on child and household variables are not. Tests of whether those variables' impacts varied across advantaged and disadvantaged children rejected the null hypothesis of no difference in only three of 22 cases.[23]

Table 4 presents estimates of the differences between the average school fixed effect for advantaged children and the same average for disadvantaged children, as well as tests of the significance of these differences (that is, tests of the significance of the average of θ_s) for the five definitions of advantage in Vietnam. The first and second columns present results for the mathematics and Vietnamese tests, respectively. Below the estimates of the mean of θ_s, Table 4 also shows tests of the joint significance of the school fixed effects, separately for advantaged and disadvantaged children, for each definition of advantage. All these joint tests are highly significant, indicating that beyond non-school sources of advantage, schools vary substantially in their contribution to the cognitive-skills acquisition of the sample children.

Turning to the estimated means of θ_s, we note little evidence that the average size of the school impact in Vietnam varies much between more and less advantaged children. For all but one definition, the differences in the average school fixed effects between more and less advantaged children are statistically insignificant. The exception is when 'advantaged' is defined as being male: Vietnamese schools convey a sizeable *disadvantage* of being male. That is, the average contribution of schooling to boys' maths scores is 11.2 points (0.11 standard deviations) lower than for girls, a difference that is not quite significant at the 10 per cent level (p-value of 0.14), and 23.1 points (0.23 standard deviations) lower on the Vietnamese test, which is highly significant even after controlling for test scores at age 5.

22 One could argue that the precision of estimates for the non-school variables could be increased, perhaps also reducing bias in the school-effects estimates, if we included the whole sample (not just the 'overlap' sub-samples that have at least one child belonging to the advantaged and disadvantaged groups), still estimating the school effects only for the 'overlap' sub-sample. It appears that this method has little to offer, as the main estimates of differences in school effects change little when the whole sample is included (estimates available upon request).

23 See Appendix Table 5 for F-statistics and p-values of these tests for each subject and disadvantage category.

Table 4: *Differences in School Effectiveness by Group in Vietnam*

	Maths	Vietnamese
Mean incremental effect, $\bar{\theta}_s$ in eq (4) (se)		
Being richer (top two wealth quintiles)	-11.32 (11.25)	-9.74 (10.66)
Being more able (top two round 2 CDA quintiles)	1.02 (11.81)	12.24 (11.30)
Being male	-11.27^ (7.58)	-23.15***(7.40)
Having a mother with at least primary education	-8.33 (14.43)	-22.14^ (13.58)
Having not been malnourished in infancy	11.46 (10.57)	-14.58 (10.02)
Joint significance of fixed effect, F-stat (p-value)		
Richer	2.40***(0.000)	2.30***(0.000)
Poorer	3.36***(0.000)	4.37***(0.000)
More able	2.34***(0.000)	2.91***(0.000)
Less able	4.02***(0.000)	4.16***(0.000)
Male	3.06***(0.000)	2.95***(0.000)
Female	2.78***(0.000)	2.44***(0.000)
More educated mums	3.66***(0.000)	4.37***(0.000)
Less educated mums	1.91***(0.000)	3.07***(0.000)
Stunted in infancy	2.53***(0.000)	2.38***(0.000)
Not stunted in infancy	3.75***(0.000)	3.50***(0.000)

Note that at age 5, before starting school, boys outperform girls on both cognitive tests (Table 3). While we cannot directly control for the non-school advantage enjoyed by boys, this suggests that if this advantage is not captured by the controls, especially age-5 test scores, it would downwardly bias estimates of the difference in school effects between girls and boys.

Overall, for Vietnam we find no evidence that schools or teachers favour advantaged children in the classroom. Indeed, the only significant effect is that something about Vietnamese primary schools favours girls, who are usually considered to be a disadvantaged group.

In contrast, analogous results for Peru in Table 5 show differences in school impacts between advantaged and disadvantaged children for several definitions, including pre-school ‘ability’ (‘school readiness’), ethnicity, and (possibly) early childhood malnutrition, although the estimates of this last difference are imprecise. While there are no significant differences in school impacts between children from richer and poorer households, significant differences emerge when advantage is defined according to children’s ‘ability’ or ‘school readiness’ (as measured by the CDA test): schools in Peru appear to heavily favour more able children in their Spanish skills, an impact of 32.3 (0.32 standard deviations).[24] The mean school fixed effect for more able children is also higher for maths, but this difference is not significant.

24 Note that this does not reflect that such children had better Spanish skills before starting school, since Spanish skill at age 5 is one of the control variables.

Table 5: *Differences in School Effectiveness by Group in Peru*

	Maths	Spanish
Mean incremental effect, $\bar{\theta}_s$ in eq (4) (se)		
Being richer (top two wealth quintiles)	13.95 (12.72)	5.83 (14.71)
Being more able (top two round 2 CDA quintiles)	17.02 (15.75)	52.78***(17.29)
Being male	2.40 (9.28)	-12.08 (9.38)
Being an ethnic majority	37.27**(16.17)	25.07^ (15.91)
Having a mother with at least completed primary schooling	-10.82 (20.98)	-4.50 (22.44)
Having not been malnourished in infancy	2.61 (12.42)	19.26^(12.47)
Joint significance of fixed effect, F-stat (p-value)		
Richer	1.49** (0.05)	1.08 (0.36)
Poorer	2.05***(0.001)	1.76***(0.007)
More able	1.47**(0.033)	1.69***(0.001)
Less able	2.53***(0.000)	1.74***(0.000)
Male	2.27***(0.000)	1.34* (0.058)
Female	1.98***(0.000)	1.60***(0.001)
Ethnic minority	2.41***(0.000)	1.57* (0.053)
Ethnic majority	1.16 (0.28)	1.84**(0.014)
Educated mums	1.53**(0.027)	1.11 (0.306)
Uneducated mums	1.92***(0.001)	1.00 (0.474)
Stunted in infancy	1.22 (0.155)	1.30* (0.094)
Not stunted in infancy	2.11***(0.000)	1.80*** (0.001)

The next set of results in Table 5 suggests that, in contrast to Vietnam, schools in Peru do not favour girls over boys, or vice versa, in either maths or reading scores. There is also no evidence of non-school differences between boys and girls at age 5; they had very similar CDA and PPVT scores before starting school (see Table 3). However, it appears that schools in Peru contribute more to learning among ethnic-majority children. Even after controlling for age-5 maths and reading skills, nutritional status and parental education (and all other Table 2 variables), the impact of school and teacher characteristics on ethnic-majority children's maths skills is 37.3 points (0.37 standard deviations) higher than the same impact for ethnic-minority children, and the analogous differential impact for Spanish skills is 25.1 points (0.25 standard deviations). While the effect on maths skills is significant, the effect on Spanish skills is less precisely estimated (p-value of 0.12). Note from Table 3 that ethnic-majority children already outperformed ethnic-minority children in cognitive skills in terms of age-5 test scores (before entering primary school). To the extent that ethnicity directly affects learning via non-school factors other than ability and household circumstances, these estimated differences in school effects may be biased upwards; yet it is difficult to think of mechanisms for such bias.

The next set of estimates in Table 5 shows, as in Vietnam, no significant differences in school impacts between children in Peru whose mothers have little or no education and those with more educated mothers. Yet the average school fixed effect for Spanish is 0.19 standard deviations smaller for children who were malnourished in the first years of life than for those who were not malnourished, although this difference is not quite significant (p-value = 0.12).

Overall, even after controlling for a large number of child and household variables, when advantaged children (defined as coming from a wealthier household, being more able at age 5, being male, not being a member of an ethnic minority, having a relatively educated mother, and being well nourished in the first years of life) and disadvantaged children attend the same schools, advantaged children often learn more in Peru. This stands in complete contrast with Vietnam, where advantaged children never appear to learn more than disadvantaged children when they attend the same schools, and for one type of advantage (male) appear to learn less.

7. Robustness

Having presented the main estimates, we now turn to robustness issues. As explained above, the main concern is omitted-variable bias. This was discussed at length in Sections V and VI, where we emphasised that we use an extensive set of controls for non-school inputs from the first two rounds of the Young Lives data, as well as cognitive test scores at age 5 as measures of ‘innate ability’, to capture the effect of non-school advantage on cognitive skills at age 10. Further, since the estimates of interest are differences in school effectiveness, we argue that our analysis is less vulnerable to omitted-variable bias than are analyses that focus on impacts of child and household variables. One way to test this claim is to check the main estimates for sensitivity to different assumptions about omitted variables. Todd and Wolpin (2007) review different models for estimation of the cognitive-skills production function, and the assumptions that each makes about omitted variables. The main model in their paper combines the cumulative-inputs model and the value-added model; it is identified under weaker assumptions than either of these models (see Todd and Wolpin, 2007, for details). In order to test the sensitivity of the main estimates in Tables 4 and 5 to omitted-variable bias, we compare them with estimates from the possibly less robust cumulative-inputs and value-added models.

Tables 6 and 7 show these estimates for Vietnam and Peru, respectively. For ease of comparison, the first set of estimates in both tables is the main estimates in Tables 4 and 5. The second set is estimates of a pure value-added model in which the only controls for non-school inputs are pre-school test scores. The third is estimates of a cumulative-inputs model that excludes pre-school test scores but controls for non-school circumstances; it assumes that inputs alone fully capture the contribution of non-school factors to cognitive skills at age 10.

Table 6: *Sensitivity of the Estimated Differences in School Effectiveness by Group in Vietnam*

	Maths	Vietnamese
Mean incremental effect, $\bar{\theta}_s$ in eq (4) (se) (main estimates)		
Being richer (top two wealth quintiles)	-11.32 (11.25)	-9.74 (10.66)
Being more able (top two Round 2 CDA quintiles)	1.02 (11.81)	12.24 (11.30)
Being male	-11.27^ (7.58)	-23.15***(7.40)
Having a mother with at least primary schooling	-8.33 (14.43)	-22.14^ (13.58)
Having not been malnourished in infancy	11.46 (10.57)	-14.58 (10.02)
Mean incremental effect, in eq (4) $\bar{\theta}_s$ (se) (Value-Added model)		
Being richer (top two wealth quintiles)	5.15 (9.50)	8.37 (9.31)
Being more able (top two Round 2 CDA quintiles)	1.23 (11.75)	13.60 (11.63)
Being male	-13.65* (7.55)	-28.45*** (7.48)
Having a mother with at least primary schooling	18.35** (9.3)	19.32** (9.09)
Having not been malnourished in infancy	15.74* (9.34)	4.51 (9.11)
Mean incremental effect, $\bar{\theta}_s$ in eq (4) (se) (Cumulative Inputs)		
Being richer (top two wealth quintiles)	-10.81 (11.38)	-10.78 (10.85)
Being more able (top two Round 2 CDA quintiles)	12.68 (9.19)	20.97**(8.85)
Being male	-8.34 (7.67)	-19.31*** (7.53)
Having a mother with at least primary schooling	-11.98 (14.62)	-26.43*(13.83)
Having not been malnourished in infancy	12.67 (10.64)	-13.49 (10.16)

Most estimates for both countries show little sensitivity to alternative assumptions about omitted variables in the different models. Starting with Vietnam, the difference in the school effect between wealthy and poor children is insignificant for all specifications. The difference between more and less able children is positive in all specifications and, in all but one, statistically insignificant. All estimates suggest that boys benefit less from school than girls, especially for Vietnamese, and all but one show no difference for those who were well nourished in infancy. The one exception to this overall robustness is the results for maternal education. The main estimates and those from the cumulative-inputs model suggest that children with more educated mothers gain less from school, but this is only marginally significant (10–12 per cent level). Yet the value-added model estimates suggest the reverse: a positive effect that is large and highly significant. This pattern of results also holds for Peru, as seen in Table 7.

For all categories of advantage but one, the consistency of the direction of differences in school effects across specifications is encouraging evidence of limited sensitivity to omitted-variable bias in the main estimates of $\bar{\theta}_s$. The sole exception is the estimates of differences when children are grouped by maternal education. In fact, the differences in the estimates are driven by controls for parental education: the significant positive difference in the school effect between children of more and less educated mothers in the value-added specifications for both Vietnam and Peru vanishes once controls for parental, especially maternal, education are added. Perhaps parental education continues to have a direct effect on children's learning beyond its effect on school readiness (captured by cognitive skills at age 5). The most obvious channel is that educated parents may be more able to help children with their school work.

Table 7: *Sensitivity of the Estimated Differences in School Effectiveness by Group in Peru*

	Maths	Spanish
Mean incremental effect, $\overline{\theta}_s$ in eq (4) (se) (VA+CI)		
Being richer (top two wealth quintiles)	13.95 (12.72)	5.83 (14.71)
Being more able (top two Round 2 CDA quintiles)	17.02 (15.75)	52.78***(17.29)
Being male	2.40 (9.28)	-12.08 (9.38)
Being an ethnic majority	37.27**(16.17)	25.07^ (15.91)
Having a mother with at least primary schooling	-10.82 (20.98)	-4.50 (22.44)
Having not been malnourished in infancy	2.61 (12.42)	19.26^(12.47)
Mean incremental effect, $\overline{\theta}_s$ in eq (4) (se) (VA)		
Being richer (top two wealth quintiles)	25.82** (12.35)	10.62 (13.68)
Being more able (top two Round 2 CDA quintiles)	27.13* (15.84)	53.23*** (16.84)
Being male	7.28 (9.81)	-5.89 (9.54)
Being an ethnic majority	50.13*** (15.78)	36.51**(15.93)
Having a mother with at least primary schooling	43.89***(11.92)	38.34***(12.59)
Having not been malnourished in infancy	0.29 (11.48)	24.70**(11.44)
Mean incremental effect, $\overline{\theta}_s$ in eq (4) (se) (CI)		
Being richer (top two wealth quintiles)	13.45 (12.76)	6.65 (14.70)
Being more able (top two Round 2 CDA quintiles)	21.91**(10.53)	50.69*** (11.53)
Being male	2.10 (9.36)	-12.32 (9.48)
Being an ethnic majority	35.22** (16.51)	21.04 (16.19)
Having a mother with at least primary schooling	-12.33 (21.19)	7.76 (22.65)
Having not been malnourished in infancy	3.59 (12.60)	18.84^(12.55)

Another concern is the effects of restricting the sample to the sub-set of schools with both advantaged and disadvantaged children. Firstly, there may be some unobserved selection into these schools which differs between advantaged and disadvantaged children.[25] For example, among children from richer households, perhaps those who are in the same schools as children from poorer backgrounds, rather than in more elite schools, are the less able or less motivated advantaged children, while the reverse is true for the children in those schools from poorer households. While this is possible, the analysis of the sensitivity of our estimates to different assumptions about omitted-variable bias suggests that most of our estimates are insensitive to assumptions about omitted variables.

25 As noted above, any unobservables that are the same across the two groups are differenced out, since we look at the difference in school fixed effects.

Secondly, the sample restrictions may limit the external validity of our findings. If schools with both advantaged and disadvantaged children are very different from other schools, our findings may be relevant only to a specific sub-set of schools in each country. To examine this possibility, Tables 8 and 9 show a few general school characteristics for the whole sample and the overlap sub-samples, with significance levels of t-tests of differences between the means of the whole sample and of each of the sub-samples.[26] The results show almost no significant differences in these means for Vietnam for all definitions of advantage. In contrast, some differences emerge for Peru. This is unsurprising, as, on average, schools in Peru are smaller than in Vietnam; hence a smaller proportion have both advantaged and disadvantaged children for each definition of advantage. More specifically, schools in the overlap samples tend to be bigger than in the overall sample. This is to be expected, since larger schools are more likely to have more Young Lives children, other things equal, so these schools have a higher chance of being in the overlap sample. The sub-sample most different from the main sample is the schools with both wealthier and poorer children. On average, these are larger schools, with higher assets, and lower proportions of ethnic-minority pupils and grade-repeaters. To the extent that these differences indicate that these schools are better than average, the external validity of the findings for this definition of advantage may be affected. Overall, however, other than school size there is little to suggest systematic selection of schools into the overlap samples, especially for the definitions of advantage for which we find significant differences in school effectiveness.

Table 8: *Main Characteristics of Schools in Vietnam in the Whole Sample and Schools in the Analysis Overlap Sub-samples[1]*

		Overlap sub-sample1 (means, sd's in parentheses, asterisks indicating significance level of t-test of difference with mean for all schools)				
	All (mean, sd)	Wealth	Ability	Sex	Maternal education	Malnutrition in infancy
Pca score on school assets scale	0.00 (1.64)	0.33 (1.36)	0.25 (1.59)	0.26 (1.57)	0.29 (1.46)	0.07 (1.45)
School size (number of students)	481.73 (347.04)	535.81 (387.28)	533.77 (387.38)	502.63 (367.53)	501.75 (375.76)	445.71 (196.72)
School offers free full-day schooling	0.24 (0.43)	0.17 (0.38)	0.13 (0.34)	0.25 (0.44)	0.23 (0.43)	0.26 (0.44)
School offers free lunch	0.88 (0.33)	0.91 (0.28)	0.83 (0.38)	0.87 (0.34)	0.88 (0.04)	0.90 (0.30)
Years as principal	10.78 (7.08)	10.91 (7.27)	10.1 (6.98)	10.76 (7.35)	10.15 (7.10)	10.48 (6.83)
Mean years as teachers	17.58 (6.37)	17.10 (6.26)	16.68 (5.39)	17.81 (6.33)	17.88 (6.45)	17.49 (6.72)
Mean score on Vietnamese pedagogy test	70.27 (12.23)	69.97 (13.10)	71.17 (13.57)	70.68 (12.96)	70.13 (12.79)	71.57 (11.18)
Mean score on Maths pedagogy test	68.84 (8.44)	68.30 (6.74)	70.61 (8.32)	68.93 (8.34)	67.90 (7.89)	68.38 (7.38)
Mean number of days absent	1.94 (6.82)	1.57 (6.51)	2.52 (8.16)	2.18 (0.84)	2.46 (8.18)	1.94 (7.53)
Proportion ethnic minority pupils	0.26 (0.37)	0.09*** (0.19)	0.12** (0.24)	0.19 (0.31)	0.16* (0.28)	0.20 (0.32)
Proportion of grade repeaters	0.05 (0.05)	0.04 (0.04)	0.04 (0.05)	0.05 (0.06)	0.05 (0.05)	0.05 (0.05)

Notes: *** p<0.01, ** p<0.05, * p<0.1 ; 'Overlap sub-sample' at the school level includes all the schools in the sample that have both advantaged and disadvantaged children for each definition of advantage.

26 Overlap sub-samples at the school level include all schools in the sample with both advantaged and disadvantaged children for each definition of advantage.

Table 9: *Main Characteristics of Schools in Peru in the Whole Sample and Schools in the Analysis Overlap Sub-samples[1]*

	All (mean, sd)	Overlap sub-sample1 (means, sd's in parentheses, asterisks indicating significance level of t-test of difference with mean for all schools)					
		Wealth	Ability	Sex	Ethnicity	Maternal Education	Malnutrition in infancy
Pca score on school assets scale	0.00 (1.74)	1.33*** (1.47)	0.52 (1.83)	0.28 (1.88)	0.27 (1.76)	-0.27 (1.70)	-0.09 (1.89)
School size (number of students)	378.0 (398.6)	785.9*** (444.4)	553.7*** (449.2)	491.1 (456.9)	557.7** (504.4)	417.3 (413.3)	410.3 (415.1)
School private	0.14 (0.35)	0.08 (0.28)	0.11 (0.31)	0.10 (0.30)	0.04 (0.20)	0*** (0)	0.06* (0.23)
School offers free lunch	0.66 (0.48)	0.58 (0.50)	0.62 (0.49)	0.67 (0.48)	0.80 (0.41)	0.71 (0.46)	0.72 (0.45)
Years as principal	12.14 (8.67)	13.6 (9.37)	11.66 (8.06)	11.76 (8.34)	9.92 (7.40)	11.57 (8.40)	11.81 (8.12)
Mean years as teachers	17.23 (7.69)	19.52 (6.40)	18.05 (7.22)	18.92 (7.05)	18.57 (6.32)	19.59* (6.34)	17.81 (7.38)
Mean score on maths pedagogy test	7.74 (2.02)	8.23 (1.44)	7.62 (2.15)	7.57 (2.08)	7.99 (1.42)	7.25 (2.35)	7.67 (2.38)
Mean number of days absent	0.47 (1.06)	0.30 (0.47)	0.31 (0.47)	0.43 (0.76)	0.36 (0.50)	0.50 (0.86)	0.45 (0.79)
Proportion of ethnic-minority pupils	0.19 (0.32)	0.05** (0.11)	0.15 (0.29)	0.17 (0.30)	0.08 (0.170)	0.21 (0.32)	0.22 (0.34)
Proportion of grade-repeaters	0.26 (0.29)	0.14*** (0.13)	0.19 (0.18)	0.22 (0.19)	0.23 (0.19)	0.24 (0.16)	0.27 (0.21)

Notes: *** p<0.01, ** p<0.05, * p<0.1 ; 'Overlap sub-sample' at the school level includes all the schools in the sample that have both advantaged and disadvantaged children for each definition of advantage.

8. Extensions: What school characteristics explain variation in school fixed effects?

To design better education policies, one needs to know not only what the aggregate effect of schooling is, but also which school, teacher and principal characteristics account for inter- and intra-school variation in school effectiveness. As an extension to the main analysis in this paper, we offer estimates of equations (5) and (8) to explore these questions.

Estimates of equation (5), where school fixed effects are constrained to be the same for all students, examine which school and teacher characteristics explain the *inter*-school differences in school effectiveness. Factors underlying *intra*-school differences in school effects between advantaged and disadvantaged groups are examined by estimating equation (8) for all dimensions of disadvantage for which significant within-school heterogeneity in school effects was found in Section VI (i.e. the estimate of $\overline{\theta}_s$ was statistically significant).

This analysis is more speculative, for two reasons. First, the school-level samples are small: the Vietnam and Peru samples have 90 and 132 schools, respectively. Thus, despite the very rich school, class, teacher and peer data in the school surveys, the number of explanatory variables that we can use in the estimates is limited. This is especially true for estimating equation (8), which requires sub-samples of schools with both advantaged and disadvantaged children (according to each definition). Second, while the school data have many variables, there may still be few patterns in the data; while this could partly reflect a small sample, it could also be due to the more general difficulty of measuring the underlying determinants of school quality.

Table 10: *School-Level Descriptive Statistics*

	Vietnam		Peru	
	Mean	**SD**	**Mean**	**SD**
School Infrastructure				
Pca score on school assets scale	0.00	1.64	0.00	1.74
School Organisation				
School size (number of students)	481.73	347.04	378.04	398.59
School choice in the area	0.66	0.48		
School free all day	0.24	0.43		
School private			0.14	0.35
School offers free lunch	0.88	0.33	0.66	0.48
Principal Characteristics				
Male	0.70	0.46	0.61	0.49
Years as principal	10.78	7.08	12.14	8.67
Has highest level of training	0.63	0.49	0.15	0.36
In-service training*	17.72	19.66	0.68	0.47
Mean Teacher Characteristics				
Proportion male	0.30	0.40	0.41	0.43
Mean years as teachers	17.58	6.37	17.23	7.69
Proportion with highest level of teacher training	0.38	0.42	0.28	0.40
In-service training*	8.04	9.71	0.60	0.44
Mean score on language pedagogy test	70.27	12.23		
Mean score on maths pedagogy test	68.84	8.44	7.74	2.02
Mean number of days absent*	1.94	6.82	0.47	1.06
Pupil Characteristics				
Mean home assets index (principal components)	-0.67	1.61		
Proportion repeating a grade	0.05	0.05	0.26	0.29
Proportion ethnic minority	0.26	0.37	0.19	0.32
Proportion with fathers who have none/incomplete primary education			0.19	0.28
Proportion with fathers who have complete primary/secondary education			0.54	0.31
Proportion with fathers who have higher education			0.15	0.24
Number of Observations	90		132	

Notes: The recall period for these variables differs in the Vietnam and Peru data-sets:

(a) In-service training (principal and teachers): in Vietnam this is asked as the number of days in the last academic year, while in Peru it is a dummy variable indicating whether the principal or teacher had at least 20 hours of training in the last two years.

(b) Mean number of days absent: in Vietnam this is asked with reference to the last academic year, while in Peru the reference period is the last 30 days.

Table 10 presents descriptive statistics for the school characteristics that we selected as key determinants of school effectiveness. They were identified on the basis of the Glewwe et al. (2013) review of studies published since 1990 on the effects of school characteristics on student learning; that study grouped school characteristics into three types: school infrastructure and pedagogical materials; teacher and principal characteristics; and school organisation. We use all variables in each of these groups that are available in both the Vietnam and Peru school surveys.[27] We also control for basic pupil (peer) characteristics. These are not discussed in Glewwe et al., yet they have been found to be a vital component of the school learning environment (see Sacerdote, 2011, for an overview). We use peer data collected in both the Peru and Vietnam surveys to construct school-level averages of pupil wealth, ethnicity and grade repetition. Since no household-wealth data were collected for peers in the Peru survey, we use dummies for father's education as an indicator of peers' household wealth.

Tables 11 and 12 present estimates for Vietnam and Peru, respectively. The first two columns in both tables are estimates of equation (5), i.e. school-level regressions of 'average' school fixed effects (derived from estimates of equation (3) using tests in mathematics (column 1) and reading comprehension (column 2) as the outcomes) from the 90 schools in Vietnam and the 132 schools in Peru on school, teacher, principal and pupil (peer) characteristics.

27 We constructed a school-asset index using principal-components analysis to measure school infrastructure and pedagogical materials, based on schools having a library, computer facilities, internet, electricity, and working toilets. School-organisation controls include school size, whether the school is private (for Peru), whether free full-day schooling is offered (for Vietnam, where only half-day schooling is free at the primary level), and whether the school offers a free lunch. Teacher and principal characteristics include gender, experience, level of training, and recent in-service training. Finally, for teachers we include scores on pedagogy tests administered in Vietnam and Peru and on teacher absenteeism (self-reported in Vietnam and based on school records in Peru).

Table 11: *Correlates of the Vietnam School Fixed Effects for Maths and Vietnamese*

	Math	Vietnamese	Maths $\bar{\theta}_s$ (girls)	Vietnamese $\bar{\theta}_s$ (girls)
Pca score on school assets scale (comp)	-4.057	-7.449	-4.351	-11.297*
	(5.201)	(6.369)	(5.940)	(6.156)
School: size	0.007	-0.019	-0.014	0.007
	(0.023)	(0.029)	(0.026)	(0.027)
School: school choice in area	38.594***	13.336	-20.471	-36.683**
	(14.351)	(17.574)	(15.372)	(15.930)
School: free full day	12.506	-23.491	-15.981	27.335
	(16.682)	(20.430)	(18.075)	(18.730)
School: offers free lunch	2.303	33.916	-8.417	3.386
	(22.602)	(27.679)	(26.114)	(27.061)
Principal: male	-5.858	10.584	-21.239	-5.299
	(18.043)	(22.097)	(21.835)	(22.627)
Principal: years as principal	1.275	0.916	-0.559	1.469
	(0.927)	(1.136)	(1.042)	(1.080)
Principal: top training	2.820	-21.743	-6.549	28.712*
	(15.481)	(18.958)	(16.525)	(17.124)
Number of days of professional/in-service training in the last academic year	0.210	0.443	-1.252	0.962
	(0.458)	(0.561)	(0.997)	(1.033)
Teachers: proportion male	6.341	-5.022	-12.587	-9.891
	(16.550)	(20.268)	(20.172)	(20.904)
Teachers: mean years teaching	-0.252	1.065	-1.153	-1.754
	(1.055)	(1.292)	(1.242)	(1.287)
Teachers: proportion with top teacher training	-12.557	21.983	19.672	59.363***
	(15.845)	(19.404)	(18.790)	(19.472)
Teachers: proportion receiving in-service training	-0.555	0.259	-0.080	-1.115
	(0.665)	(0.814)	(0.732)	(0.758)
Teachers: mean score on Vietnamese pedagogy test	-0.567	-0.110	-1.101*	-0.420
	(0.556)	(0.680)	(0.605)	(0.627)
Teachers: mean score on maths pedagogy test	-0.042	-0.090	-0.761	0.032
	(0.784)	(0.960)	(0.924)	(0.958)
Teachers: mean days absent in the last academic year	0.209	-1.087	0.390	-0.174
	(1.051)	(1.287)	(1.125)	(1.166)
Pupils: mean home assets index	17.656*	36.989***	5.977	17.505
	(9.306)	(11.397)	(11.470)	(11.886)
Pupils: proportion repeating a grade	-140.535	-113.937	41.201	-154.700
	(115.640)	(141.617)	(131.840)	(136.620)
Pupils: proportion ethnic minority	18.609	64.147*	-3.478	31.319
	(29.530)	(36.163)	(36.332)	(37.649)
Constant	390.490***	336.501***	234.403***	49.066
	(74.586)	(91.340)	(86.449)	(89.583)
R^2 (Adjusted R^2 in parentheses)	0.38 (0.21)	0.32 (0.13)	0.27 (0.02)	0.34 (0.12)
Tests of joint significance (F-statistics with p-values in parentheses)				
All	2.23*** (0.01)	1.68*(0.06)	1.08 (0.40)	1.54 (0.11)
School infrastructure	0.61 (0.44)	1.37 (0.25)	0.54 (0.47)	3.37* (0.07)
School organisation	1.91 (0.12)	1.01 (0.41)	0.68 (0.61)	2.20* (0.08)
Principal and teacher characteristics	0.70 (0.73)	0.75 (0.69)	1.33 (0.23)	1.72* (0.09)
Pupil (peer) characteristics	2.01 (0.12)	3.77*** (0.01)	0.18 (0.91)	1.18 (0.32)
Number of observations	89	89	76	76

note: *** p<0.01, ** p<0.05, * p<0.1

Table 12: *Correlates of the Peru School Fixed Effects for Maths and Spanish*

	Maths	Spanish	Spanish $\overline{\theta}_s$ (ability)	Spanish $\overline{\theta}_s$ (nutrition)
Pca score on school assets scale (comp)	0.929	9.384	6.887	3.065
	(6.033)	(6.566)	(19.613)	(14.231)
School: size	0.010	-0.026	-0.007	0.000
	(0.024)	(0.027)	(0.052)	(0.055)
School: private	-20.997	-23.321	257.811***	72.898
	(29.165)	(31.743)	(78.646)	(85.505)
School: offers free lunch	3.650	27.616	-31.993	42.417
	(16.070)	(17.491)	(40.553)	(40.227)
Principal: male	-18.971	-8.405	-15.749	6.445
	(13.411)	(14.597)	(32.735)	(28.159)
Principal: years as principal	0.320	-0.047	2.505	2.106
	(0.763)	(0.831)	(2.249)	(2.260)
Number of years as principal missing	49.439	40.783	-19.495	103.600
	(55.260)	(60.146)	(109.736)	(78.113)
Principal: top training	-8.791	-1.574	26.503	9.480
	(19.186)	(20.882)	(47.151)	(35.617)
Principal in-service pedagogical training: 20 hours for the last 2 years	-13.439	13.652	41.992	-7.191
	(14.108)	(15.355)	(36.200)	(31.751)
Teachers: prop male	-17.911	-15.168	-0.968	-70.200
	(16.587)	(18.053)	(44.677)	(44.249)
Teachers: mean years teaching	-0.854	0.272	3.062	1.968
	(1.029)	(1.120)	(2.853)	(2.028)
Teachers: proportion with top training	-18.822	-23.355	-20.469	-28.258
	(17.629)	(19.187)	(55.965)	(44.116)
Teachers: proportion with in-service training	27.140*	24.558	-44.951	51.671
	(14.860)	(16.174)	(52.114)	(44.355)
Teachers: mean score on maths pedagogy test	-0.345	1.471	8.161	-3.241
	(3.271)	(3.560)	(11.125)	(6.940)
Teachers: mean days absent in the last 30 days	-5.474	-10.190	59.187	-38.030**
	(6.360)	(6.923)	(37.952)	(18.959)
Pupils: proportion whose fathers have none/incomplete primary education	-21.224	32.721	177.561	-33.554
	(38.335)	(41.724)	(151.017)	(100.964)
Pupils: proportion whose fathers have complete primary/secondary education	6.446	-1.036	25.864	-107.257
	(38.672)	(42.091)	(144.265)	(91.240)
Pupils: proportion whose fathers have higher education	13.471	51.215	-54.314	136.863
	(47.117)	(51.283)	(150.241)	(126.749)
Pupils: proportion ethnic minority	-50.827*	-60.811*	41.297	-148.577**
	(29.097)	(31.669)	(70.663)	(70.514)
Pupils: proportion of grade-repeaters	-3.368	-4.512	126.347	220.055*
	(30.698)	(33.412)	(153.096)	(117.268)
Constant	321.627***	217.912***	-175.330	-3.975
	(55.096)	(59.967)	(164.216)	(116.582)
R^2 (Adjusted R^2 in parentheses)	0.18 (0.03)	0.18 (0.03)	0.54 (0.19)	0.55 (0.28)
Tests of joint significance (F-statistics with p-values in parentheses)				
All	1.22 (0.25)	1.19 (0.28)	1.55 (0.15)	2.01** (0.04)
School infrastructure	0.02 (0.89)	2.04 (0.16)	0.12 (0.73)	0.05 (0.83)
School organisation	0.47 (0.71)	1.36 (0.26)	5.28*** (0.01)	0.53 (0.66)
Principal and teacher characteristics	1.10 (0.37)	0.93 (0.52)	0.76 (0.68)	1.57 (0.16)
Pupil (peer) characteristics	1.68 (0.15)	1.29 (0.27)	1.20 (0.34)	3.51*** (0.01)
Number of observations	132	132	47	54

note: *** p<0.01, ** p<0.05, * p<0.1

The R^2 coefficients indicate that the variables in these regressions explain 30–40 per cent of the variation in the 90 school fixed effects for Vietnam and 18 per cent of the variation in the 132 fixed effects in Peru. Yet for both countries most of these characteristics' impacts are statistically insignificant. For Vietnam, three have significant predictive power. First, schools appear to contribute more to maths learning when parents can choose between two or more schools in the local area. Second, schools that offer peers from homes with higher assets appear to raise both maths and reading scores; note that this effect is conditional on students' own family wealth. This peer effect is larger and more statistically significant for Vietnamese. Third, learning of Vietnamese is positively related to the proportion of ethnic-minority pupils in the school (although significant only at the 10 per cent level), perhaps because such schools put more emphasis on teaching Vietnamese. For both maths and Vietnamese, the full set of explanatory variables is jointly significant, though only at the 10 per cent level for Vietnamese. Tests of the joint significance of each of the four sub-sets of regressors (school infrastructure, school organisation, principal and teacher characteristics, and peer characteristics) suggest that only peer variables are (somewhat) significant for both tests (only at the 12 per cent level in the maths test).

For Peru, the results in the first two columns of Table 12 yield only two significant variables: the share of teachers with in-service training has a positive, marginally significant effect on the school fixed effects for maths, and the proportion of ethnic-minority pupils has a marginally significant negative effect on school fixed effects for both tests. However, none of the F-tests for the joint significance of variables in the Peru regressions is significant, whether for all variables combined or separately for any of the four sub-sets of variables.

Turning to the second question (what school characteristics seem to reduce, or raise, differences in learning between advantaged and disadvantaged students?), we should recall that the only dimension in Vietnam with significant heterogeneity between advantaged and disadvantaged groups was the difference by gender; the third and fourth columns of Table 11 present estimates of equation (8) to investigate this difference for maths and Vietnamese scores. The outcome is the mean incremental school effect for the 'advantaged' group, defined in this case as being a girl (recall that results in Table 4 suggest that schools favour girls' learning); school characteristics that contribute more to the learning of girls will have positive values.

None of the school, teacher, principal, or peer variables has an impact significant at the 5 per cent level that can explain why schools in Vietnam contribute more to girls' learning in maths. The null hypothesis of the joint insignificance cannot be rejected for all the variables combined, or for any of the four sub-sets of variables. Indeed, the adjusted R^2 is very low, at 0.02. Thus the data do not explain differences in the contribution of schools to learning maths among boys and girls in Vietnam. The results are somewhat more promising for explaining differences by gender in school contributions to learning Vietnamese (Table 11, Column 4). The two characteristics with significant impacts that favour girls are principals with the highest level of training, and the fraction of teachers with the highest level of training. There are also some characteristics that favour boys: a higher level of school assets and having a choice among local schools. Interpreting these differences is not easy.[28] An admittedly

28 The size of the coefficients is comparable only for variables that are dummy variables (see Table 5 for the means of these variables). For other variables the impact of a one standard deviation change in the variable can be obtained by multiplying the coefficients by the standard deviations in Table 10.

speculative explanation is that 'classroom' training of principals and teachers favours girls, while situations that favour flexibility in pedagogy (such as a choice of schools) favour boys.

The model also performs poorly in explaining within-school heterogeneity in effectiveness across different definitions of advantage and disadvantage in Peru. Table 5 shows that significant learning differences exist between children who are more and less prepared for school at age 5, between children from ethnic-minority and ethnic-majority groups, as well as children who were better nourished and less well-nourished in infancy (although the last difference is significant only at the 12 per cent level). The last two columns in Table 12 examine which school characteristics seem to favour learning of Spanish among students who are better prepared for school at age 5, relative to less prepared students (column 3), and students not malnourished in infancy, relative to those who were (column 4). While we would also like to examine differences between ethnic-majority and ethnic-minority students, only 25 schools attended by both types of children are in the sample, which is too few to estimate equation (8).

As for Vietnam, almost all variables are insignificant, yet a few are significant. First, students who were better prepared for school at age 5 benefit significantly more from being in a private school than do less prepared students. On the other hand, differences in the school contribution to learning Spanish among children who were and were not malnourished in infancy seem to be affected by teacher absenteeism and class composition. The former has a significantly negative effect, in that teacher absenteeism has a more negative effect on well-nourished children. Regarding class composition, the proportion of grade-repeaters in a class appears to favour children not malnourished in infancy, but the fraction of ethnic minorities favours those who had been. These results are also difficult to interpret, and, given that the differential impact of school fixed effects on well-nourished and malnourished children is not quite significant at the 10 per cent level, it seems unwise to draw conclusions from these results.

In summary, despite the richness of the school data, the estimated school-level models clearly lack power to explain both the variation in school fixed effects across schools and the differences between advantaged and disadvantaged children within schools. This result is not surprising, for three reasons. First, the estimated fixed effects are likely very noisy (especially for Peru), since they are based on few students: on average there are four pupils per school in the Peru sample and 12 in the Vietnam sample. Secondly, the school-level regressions have relatively few schools: a maximum of 90 in Vietnam and 132 in Peru, and fewer for estimation of correlates of fixed-effects heterogeneity (equation (8)). Finally, as Glewwe et al. (2013) explain in their review of the literature on school determinants of cognitive skills, easily observed school and teacher characteristics often have low explanatory power after a basic threshold of school quality has been reached (which is probably the case in both countries). Those authors conclude that what probably matters most is the way in which schools are organised and the incentives experienced by teachers, administrators, parents and students – all of which are very difficult to measure and compare across contexts. Our findings support this view, although a few significant correlates do suggest future lines of enquiry to understand differences in school effectiveness for more and less advantaged children, such as what it is about the way children are taught in private schools in Peru that makes them more effective at teaching children who begin with more skills, and why teacher and principal training matter for girls' skill acquisition in Vietnam.

9. Conclusions

Economists and policy makers generally agree that education is a valuable investment in developing countries, but evidence indicates that students in many of those countries learn far less their counterparts in developed countries, and that within developing countries some children appear to learn much more than others. This paper has investigated whether schools in Vietnam and Peru reinforce or reduce gaps in learning between advantaged and disadvantaged students. It does so for six different definitions of advantage, using a methodology which estimates separate school fixed effects for advantaged and disadvantaged students. This methodology allows us to focus on *intra-* rather than *inter*-school differences in schools' contribution to learning among students from different backgrounds.

Our results indicate that schools vary enormously in terms of their effects on their students' learning, even after controlling for a variety of child and household characteristics, including cognitive skills measured by tests taken at age 5, before entering primary school. When we allow for separate school fixed effects for advantaged and disadvantaged children, we find no evidence that schools in Vietnam favour advantaged children. Indeed, the one significant effect is that girls, who are often considered to be a disadvantaged group, appear to pull ahead of boys between the ages of 5 and 10.

In contrast, for two definitions of disadvantage in Peru it appears that schools favour advantaged students: students with higher skills at age 5 acquire more Spanish skills than do less well prepared students, and ethnic-majority students learn more maths than ethnic-minority students, even after conditioning on skills at age 5. We also find weakly significant evidence that Peruvian schools favour well-nourished over malnourished children.

These findings are consistent with existing evidence on differences between school systems in Vietnam and Peru. In Vietnam, rapid expansion in primary education over the last two decades was accompanied by effective investment in education quality (as seen in the recent PISA results) as well as equity, through an emphasis on the need for all pupils to attain 'minimum standards'. In contrast, existing evidence suggests that Peru's school system suffers from high inequality in student learning, with evidence of gaps in pupils' access, grade progression and learning outcomes by background characteristics such as socio-economic group and ethnicity. Our results indicate that such gaps exist even among students attending the *same* schools, with pupils from more advantaged backgrounds learning more than pupils from less advantaged backgrounds, even after controlling for many pupil characteristics, including skills at age 5. While it is beyond the scope of this paper to explore the mechanisms behind these effects, the recent literature on within-school discrimination against less advantaged groups and biases in the school curriculum in favour of more advantaged children in a number of contexts are possible explanations for at least part of our findings.

The paper also presents a methodology to investigate which school, teacher, principal and peer characteristics appear to favour advantaged children over disadvantaged children (or vice versa). Regrettably, these school-level regressions yielded few significant results; larger samples are needed to obtain results that are useful for policy making. Classroom observations to see whether teachers discriminate against disadvantaged children should be a high priority. Yet one conclusion for policy research is clear: estimates that assume that schools have the same impacts on different types of student may overlook a major source of inequality in student learning in developing countries.

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Appendix Table 1: *Differences in Main Characteristics between Sub-sample of Children Included in the School Surveys (analysis sample in this paper) and Whole Young Lives Sample of Younger-Cohort Children*

	Vietnam					Peru				
	Analysis sub-sample		Whole sample in Round 3		Diff (Std Err)	Analysis sub-sample		Whole sample in Round 3		Diff (Std Err)
	Mean	SD	Mean	SD		Mean	SD	Mean	SD	
From Household Survey (time invariant variables)										
Male	0.51	0.50	0.52	0.50	-0.00 (0.02)	0.48	0.50	0.50	0.50	-0.02 (0.02)
Ethnic minority	0.08	0.27	0.14	0.35	-0.06*** (0.01)	0.36	0.48	0.31	0.46	0.05** (0.02)
Dad: Years of schooling	7.69	3.53	7.41	3.88	0.29** (0.14)	9.23	3.77	9.15	3.95	0.09 (0.19)
Mum: Years of schooling	7.17	3.30	6.82	3.81	0.35*** (0.13)	7.84	3.11	7.81	4.54	0.02 (0.21)
Measured during Infancy (Round 1)										
Birth size (maternal assessment: 1 (very large) to 5 (very small)	3.08	0.66	3.07	0.68	0.02 (0.03)	3.11	0.96	3.12	0.99	-0.00 (0.05)
Health better than other children (maternal assessment)	0.26	0.44	0.21	0.41	0.02 (0.02)	0.41	0.49	0.40	0.49	0.02 (0.02)
Age in months	12.82	2.61	11.64	3.17	1.18*** (0.11)	11.93	3.60	12.04	3.56	-0.11 (0.17)
Child care: looked after by others / crèche	0.42	0.49	0.38	0.49	0.04** (0.02)	0.21	0.41	0.24	0.43	-0.04* (0.02)
Maternal mental health (stress/depression: score out of 20, higher score indicates higher stress level)	4.4	4.02	4.36	3.91	0.04 (0.15)	5.78	4.26	5.68	4.29	0.10 (0.21)
Wealth index	0.47	0.20	0.44	0.22	0.02*** (0.01)	0.43	0.19	0.43	0.19	0.01 (0.01)
Measured at Age 5 (Round 2)										
Height for age z-score	-1.30	0.97	-1.34	1.04	0.05 (0.04)	-1.53	1.02	-1.54	1.12	0.01 (0.05)
PPVT Rasch Score (normalised: whole sample mean=300, sd=50)	305.33	44.33	300.50	48.09	4.84*** (1.75)	301.34	44.84	300.10	48.95	1.24 (2.24)
CDA Rasch Score (normalised: whole sample mean=300, sd=50)	306.80	46.56	300.17	49.42	6.63*** (1.79)	299.35	46.39	300.00	49.99	-0.65 (2.28)
Time spent in pre-school (hours per day)	5.79	2.45	5.05	3.07	0.74*** (0.10)	3.44	1.83	3.60	1.87	-0.16 (0.09)
Only child	0.22	0.42	0.24	0.43	-0.02 (0.02)	0.19	0.40	0.19	0.39	0.00 (0.02)
Household size	4.57	1.39	4.67	1.51	-0.09* (0.05)	5.52	2.23	5.51	2.08	0.01 (0.11)
Log per capita real consumption (in local currency)	5.77	0.54	5.76	0.61	0.01 (0.02)	5.02	0.67	5.01	0.66	0.01 (0.03)
Area of land owned (hectares)	0.42	1.07	0.48	1.03	-0.06 (0.04)	1.09	3.34	2.54	19.69	-1.45*** (0.47)
Number of Observations	1,129		1,965			547		1,962		

Notes: *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 2: *Overlap in Deprivation Domains (Vietnam)*

	Wealth		Ability		Gender		Maternal education		Nutrition	
	Rich	Poor	More able	Less able	Male	Female	Mother more educated	Mother less educated	Not stunted in infancy	Stunted in infancy
Richer	100	0	38.0	62.0	51.1	48.9	94.5	5.5	86.8	13.2
Poorer	0	100	26.9	73.1	51.3	48.7	72.2	27.8	74.7	25.3
More able	50.7	49.3	100	0	53.2	46.8	83.5	16.5	81.0	19.0
Less able	38.0	62.0	0	100	50.3	49.7	80.7	19.3	79.2	20.8
Male	41.8	58.2	32.8	67.2	100	0	83.2	16.8	75.0	25.0
Female	42.2	57.8	30.3	69.7	0	100	79.9	20.1	84.8	15.2
Mother more educated	48.7	51.3	32.3	67.7	52.2	47.8	100	0	81.6	18.4
Mother less educated	12.5	87.5	28.4	71.6	46.6	53.4	0	100	71.6	28.4
Not stunted in infancy	45.7	54.3	32.0	68.0	48.1	51.9	83.5	16.5	100	0
Stunted in infancy	27.5	72.5	29.7	70.3	63.3	36.7	74.2	25.8	0	100

Notes: Table shows the proportion of children in each of the row categories who are in each of the column categories. Proportions add up to 100 for each of the advantage/disadvantage column pairs. For example, in Row 1, columns 3 and 4 show that among the children in the 'rich' category, 38% are in the 'more able' category and 62% are in the 'less able' category.

Appendix Table 3: *Overlap in Deprivation Domains (Peru)*

	Wealth		Ability		Gender		Ethnicity		Maternal education		Nutrition	
	Rich	Poor	More able	Less able	Male	Female	Ethnic Majority	Ethnic Minority	Mother more educated	Mother less educated	Not stunted in infancy	Stunted in infancy
Richer	100	0	49.8	50.2	50.2	49.8	88.6	11.4	94.1	5.9	83.6	16.4
Poorer	0	100	21.0	79.0	46.7	53.4	47.3	52.7	58.5	41.5	69.2	30.8
More able	61.2	38.8	100	0	46.1	53.9	70.2	29.8	85.4	14.6	72.5	27.5
Less able	29.8	70.2	0	100	49.1	50.9	60.7	39.3	66.7	33.3	76.2	23.8
Male	41.8	58.2	31.2	68.8	100	0	65.8	34.2	75.3	24.7	70.3	29.7
Female	38.4	61.6	33.8	66.2	0	100	62.0	38.0	70.4	29.6	79.2	20.8
Ethnic majority	55.6	44.4	35.8	64.2	49.6	50.4	100	0	88.5	11.5	81.1	18.9
Ethnic minority	12.6	87.4	26.8	73.2	45.5	54.5	0	100	44.9	55.1	64.1	35.9
Mother more educated	51.8	48.2	38.2	61.8	49.7	50.3	77.6	22.4	100	0	77.6	22.4
Mother less educated	08.7	91.3	17.4	82.6	43.6	56.4	26.8	73.2	0	100	67.8	32.2
Not stunted in infancy	44.6	55.4	31.5	68.5	45.1	54.9	69.0	31.0	75.4	24.6	100	0
Stunted in infancy	26.3	73.7	35.8	64.2	56.9	43.1	48.2	51.8	65.0	35.0	0	100

Notes: Table shows the proportion of children in each of the row categories who are in each of the column categories. Proportions add up to 100 for each of the advantage/disadvantage column pairs. For example, Row 1, columns 3 and 4 show that among the children in the 'richer' category, half are in the 'more able' category and half are in the 'less able' category.

Appendix Table 4: *Individual and School Sample Sizes in 'Overlap Group'[1] in each of the Advantage-Disadvantage Categories*

	Vietnam		Peru	
	Individuals	Schools	Individuals	Schools
Richer	420 (474)	48	175 (219)	36
Poorer	510 (655)		127 (328)	
More able	346 (357)	61	144 (178)	47
Less able	660 (772)		223 (369)	
Male	568 (578)	77	212 (263)	63
Female	540 (551)		232 (284)	
Ethnic majority			128 (198)	25
Ethnic minority			90 (349)	
More educated mother	816 (921)	61	235 (309)	48
Less educated mother	185 (208)		145 (238)	
Not stunted in infancy	775 (900)	63	264 (410)	54
Stunted in infancy	217 (229)		120 (137)	

Notes: 1: 'Overlap group' at the child level includes children who are in schools that have both advantaged and disadvantaged children for each definition of advantage. 'Overlap group' at the school level includes all the schools in the sample that have both advantaged and disadvantaged children for each definition of advantage. The total number of children in each of the advantage and disadvantage groups is included in brackets.

Appendix Table 5: *Tests for Equality of Non-school Variable Coefficients in Equation (4) across Advantaged and Disadvantaged Children*

	Maths	Vietnamese	Maths	Spanish
Richer and poorer	0.97 (0.51)	1.48* (0.07)	0.57 (0.94)	0.52 (0.97)
More and less able	0.89 (0.61)	1.66** (0.03)	0.81 (0.72)	0.96 (0.52)
Male and female	0.59 (0.94)	0.70 (0.84)	0.92 (0.57)	1.13 (0.32)
Ethnic majority and ethnic minority			1.14 (0.32)	1.28 (0.20)
More and less educated mother	0.63 (0.91)	0.93 (0.56)	1.07 (0.38)	0.90 (0.60)
Stunted and not stunted in infancy	0.73 (0.81)	1.19 (0.25)	1.92*** (0.001)	1.04 (0.41)

Do Schools Reinforce or Reduce Learning Gaps between Advantaged and Disadvantaged Students? Evidence from Vietnam and Peru

This paper examines whether disadvantaged children learn less than advantaged children when both types of children are enrolled in the same school for two developing countries, Vietnam and Peru. This is done by estimating education production functions that contain two school fixed effects for each school, one for advantaged children and one for disadvantaged children. The paper examines six different definitions of disadvantage, based on household wealth, having low cognitive skills at age 5, gender, ethnic minority group (Peru only), maternal education, and nutritional status. The results show no sign of discrimination against disadvantaged groups in Vietnam; indeed if anything one advantaged group, males, seems to do worse in school than the corresponding disadvantaged group, females. In contrast, in Peru ethnic minority students and students who enter primary school with low cognitive skills appear to learn less in school than ethnic majority students and students with relatively high cognitive skills who are enrolled in the same school, respectively.

Young Lives
An International Study of Childhood Poverty

About Young Lives

Young Lives is an international study of childhood poverty, involving 12,000 children in 4 countries over 15 years. It is led by a team in the Department of International Development at the University of Oxford in association with research and policy partners in the 4 study countries: Ethiopia, India, Peru and Vietnam.

Through researching different aspects of children's lives, we seek to improve policies and programmes for children.

Young Lives Partners

Young Lives is coordinated by a small team based at the University of Oxford, led by Professor Jo Boyden.

- *Ethiopian Development Research Institute, Ethiopia*
- *Pankhurst Development Research and Consulting plc*
- *Save the Children (Ethiopia programme)*
- *Centre for Economic and Social Sciences, Andhra Pradesh, India*
- *Save the Children India*
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- *Centre for Analysis and Forecasting, Vietnamese Academy of Social Sciences, Vietnam*
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