Department of Quantitative Social Science

The relative effectiveness of private and government schools in Rural India: Evidence from ASER data

Rob French
Geeta Kingdon

DoQSS Working Paper No. 10-03
June 2010

DISCLAIMER

Any opinions expressed here are those of the author(s) and not those of the Institute of Education. Research published in this series may include views on policy, but the institute itself takes no institutional policy positions.

DoQSS Workings Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

Department of Quantitative Social Science. Institute of Education, University of London. 20 Bedford way, London WC1H 0AL, UK.

The relative effectiveness of private and government schools in Rural India: Evidence from ASER data

Rob French[*]**, Geeta Kingdon**[†‡]

Abstract. One of the many changes in India since economic liberalisation began in 1991 is the increased use of private schooling. There has been a growing body of literature to assess whether this is a positive trend and to evaluate the effects on child achievement levels. The challenge is to identify the true private school effect on achievement, isolating the effect of the schools themselves from other variables that might boost private school outcomes, such as a superior (higher ability) student intake. Using the ASER data for 2005 to 2007 a number of methodologies are used to produce a cumulative evidence base on the effectiveness of private schools relative to their government counterparts. Household fixed effects estimates yield a private school achievement advantage of 0.17 standard deviations and village level 3-year panel data analysis yields a private school learning advantage of 0.114 SD.

JEL classification: I21.

Keywords: Student achievement, private and public schooling, India.

[*]Department of Quantitative Social Science, Institute of Education, University of London. 20 Bedford Way, London WC1H 0AL, UK. E-mail: (rfrench@ioe.ac.uk)

[†]Department of Quantitative Social Science, Institute of Education, University of London. 20 Bedford Way, London WC1H 0AL, UK. E-mail: (g.kingdon@ioe.ac.uk)

[‡]**Acknowledgements**. We are most grateful to Pratham and ASER Centre for providing the ASER data and specifically to Dr. Wilima Wadhwa for help with queries about the data. Rob French gratefully acknowledges financial support from the ADMIN (Administrative data: methods, inference & network) Research Centre at the Institute of Education and Geeta Kingdon from DFID under its RECOUP Research Programme Consortium. Any errors are ours.

1 Introduction

Investment in human capital is a critical part of India's strategy for development. Cognitive test scores are an increasingly well accepted measure of human capital. Recent evidence suggests that it is not mere completion of given levels of schooling but rather what is *learnt* at school that matters to both individual earnings and to national economic growth (Hanushek and Woessmann, 2008). If private and public schools differ in terms of their effectiveness in imparting learning, then the choice of private or public school has implications for people's life-time earnings and for national growth. Thus, the question of the relative effectiveness of private and public schools is of considerable policy significance in India and elsewhere.

This human capital formation has traditionally occurred in government funded schools but since liberalisation in 1991, private schools increasingly offer an alternative. According to household survey data, private schooling participation in rural India has grown from 10% in 1993 to 23 percent of the student population in 2007 (Kingdon, 2007); this is much higher than in most developed countries. Private school participation is considerably higher in urban India. The high demand hints at dissatisfaction with government schooling and the superior results of private schools suggest that these schools do a better job than government schools.

Private schools in India have generally less qualified teachers than government schools and operate using much lower levels of capital. However, private schools operate within the market and as a result have strong incentives to be competitive. Private schools hire teachers who often do not have a teaching certificate and pay them a fraction of the salaries of government schools, but they hire more teachers to reduce class sizes. The heads have far greater control over hiring and firing of teachers and thus are able to exhibit tighter control, have higher attendance and only retain effective teachers.(Nechyba, 2000; Peterson *et al*, 2003)

The primary research question of this paper is to examine the relative effectiveness of private and public schools. Conceptually one models the education production function, where the output is cognitive achievement and the school type is included as one of the input variables. The main methodological issue with estimating these education production functions is that the choice of school type is related to unobserved variables that are also correlated with cognitive achievement which would bias estimation. This paper uses a series of different methodologies to control for these unobserved variables and reduce the bias.

The paper begins with a discussion of the literature, outlining the economics of private schooling; this is followed by a critical discussion of the methodologies used to investigate private school effects, outlining some of the results found in the empirical work. Section 3 explains the nature of the data used, outlining the strengths and weaknesses. Section 4 demonstrates how the methodologies were applied to this data and presents the results. The paper concludes with a discussion of the outcomes of this research.

2 Theory, Literature and Methods

2.1 Economic Theory

2.1.1 The Participation Decision

The decision for a child to participate in education, or in private school education, can be thought of as an outcome of household cost-benefit analysis. The costs may be opportunity costs (forgone wages, forgone domestic help) or direct costs such as tuition fees. Benefits would include increased human capital and higher wages. The participation decision is made in two stages, the first to attend any kind of school, and second given that one will attend school, whether to attend government or private school.

Depending on the fee level, the costs associated with fee-charging schools may be large or small compared to the opportunity cost of not being able to participate in the labour market or help at home. It is claimed that many Indian children combine some form of employment with study, and this may be more compatible with 'free' (government) schooling as the household does not pay for the schooling missed due to work (Campaign Against Child Labour, 1997). Additional costs of schooling include transport, uniforms, materials and books used in school and other less direct costs such as the effort of enrolling children, preparing them for school and motivating them to attend.

The primary benefit of schooling, and of private schooling, is the wage premium derived from higher levels of – or better quality of – education. In addition to potentially higher cognitive skills and thus higher economic returns from private (than government) schooling, there may be non-cognitive advantages to attending private schools, such as access to a superior peer group. Demand for private schooling could also be demand for a differentiated education since different religious and linguistic communities often run denominational or 'minority' schools which provide an acculturation in the desired language or religion.

It is an obvious statement that higher quality schooling would increase the returns from education yet concepts such as schooling quality are difficult to estimate, covering a number of different notions such as resources, teacher quality and the organisational structure.

Hanushek's (2003) meta analysis of school quality finds little evidence that increasing inputs results in increased outcomes, concluding that commonly used 'input' policies are inferior to 'incentive' related policies within schools. This suggests that it may not be material differences that make the private schools more effective/attractive, but more to do with their organisational structure, something that is far less easily observed. Krueger (2003) criticizes the meta-analysis methodology and in any case such issues may be compounded in analysis of education outcomes in developing countries due to the massive heterogeneity in their education sectors and education policies.

The teachers at private schools are different from those at state schools and face different recruitment and reward structures. Estimating the difference in teacher quality is a difficult process because what makes an effective teacher is not well defined or clearly understood. Teacher quality is usually judged by the qualifications of the teacher (both academic and professional qualifications), and also by the number of years of experience. As private schools in India often employ teachers that have somewhat lower academic qualifications and that typically do not hold a teaching certificate, superficially their teacher quality appears lower. However parameters such as effort and motivation of a teacher are much more difficult to measure, though most likely more pertinent to their level of effectiveness, and these less tangible measures of teacher quality may differ between the government and private schools because of private-public sector differences in reward, incentives and accountability structures.

Extant Indian studies are consistent in suggesting that private schools in India are, on average, more internally efficient than government schools. They are more cost-efficient on average costing only about half as much per student as public schools. Private schools are also more technically efficient, producing higher achievement levels (after controlling for student intake) and making more efficient use of inputs, for example having more students per class and lower teacher absenteeism.(Bashir, 1997; Govinda and Varghese, 1993; Kingdon, 1996; Muralidharan and Kremer, 2006; Tooley and Dixon, 2005). However, the existing studies are often based on data from particular regions of India (rather than national data), or use individual methods that do not yield convincing estimates of the private school effect. In this study, we use an extremely large national dataset on child achievement as well as a variety of econometric approaches to quantify the private school effect.

2.1.2 The 'Production' of Learning

In economics a production function is used to model how inputs are converted by a firm into outputs. In the same way an educational production function can be constructed to show how effective particular inputs into a child's education improve cognitive achievement (Monk, 1989).

The inputs of educational production can be divided into individual child level inputs, household inputs and school inputs. The child brings their natural aptitude, motivation and effort, maturity (measured by age), gender and health, and these will all have a bearing on his or her achievement. The household resources contribute to the child's education, financially, nutritionally and also through the home environment e.g. whether it is conducive to study. The parent's ability and motivation are also important, while their education, income and occupation will all have a bearing on the child's outcomes. School quality determines child's outcomes through a combination of infrastructure, resources, teacher quality and the organisational structure. Though individual and household factors may be more important than school factors in determining outcomes, school quality is the area of policy interest. The government can do relatively less about the child or household characteristics at least in the short to medium term, whereas policy changes can actually make some difference to school quality.

The output of education production function is the increase in human capital. In the long term this can be measured using wage returns, but while the child is still at school the output is cognitive achievement measured by a test. Such a measure is only a proxy for all the attributes of an individual that may be pertinent to the earnings of the student once they join the labour force.

The marginal benefits of private education over state education are hinted at by the increasing demand and better results of these schools. However such a result is not conclusive because these higher levels of achievement may be the result (partly or wholly) of self-selection of superior students into private education. These differences may include superior ability or higher motivation of parents and students. Such differences may drive the apparent superiority of results of private schooling. The following section outlines methods to overcome the problem of identifying a private school effect.

2.2 Identification of the Private School Effect

A 'full model' of the education production function is shown in equation 1. Where y is the cognitive outcome, α is an intercept, P is the private school indicator for each individual, W is a vector of *all* characteristics that affect cognitive achievement and ε is the individual deviation from the average effect. In this full model β can be interpreted as the true causal effect on achievement of an individual attending a private school.

$$y = \alpha + \beta P + \tau W + \varepsilon \qquad (1)$$

There are many factors that affect learning only some of which are observed. In equation 2, W is now decomposed into the observed variables X and unobserved variables Z.

$$y = \alpha + \beta P + \gamma X + \varphi Z + \varepsilon \qquad (2)$$

In practice the model we estimate can only include the vector of observed elements X, while the unobserved component Z is part of the error term (along with the individual shocks), as in equation 3.

$$y = \alpha + \beta P + \gamma X + \varepsilon \qquad (3)$$

Unobserved factors that determine child achievement – such as the child's and family's motivation, ability and ambition, teacher effort, headmaster quality, school ethos etc., are included in the error term ε. There may even be some factors such as child health which are potentially observable and measurable but in fact are not available in most datasets and are thus omitted from W, i.e. they are not part of X and are included in ε. If these variables merely influence achievement (y, the dependent variable) but are uncorrelated with the school-type indicator (private/public school), then the private school dummy variable P does not suffer from any omitted variable bias. However, if P is systematically correlated with factors included in ε that also affect student achievement, then P is an endogenous variable. In this case the coefficient β is not a measure of the true causal effect of attending private school on student achievement. A naïve model, such as that in equation 3 – including just a private school dummy – will give biased estimates as β picks up the effect of other factors associated with private schooling as well, rather than estimating a ‘pure’ private school effect.

The aim of this research is to estimate the effect of private schools on cognitive achievement. The challenge is do so in such a way that the effect is truly identified. The impact evaluation literature gives several tools to estimate the impact, on student achievement, of private schools attendance. We discuss these different approaches first and assess their strengths and weaknesses.

The earliest studies of the private school effect used a private school dummy variable and a series of controls to identify a private school effect (Halsey, A and Ridge, 1980) in the UK, (Psacharopoulos, 1987) in Colombia and Tanzania and (Govinda and Varghese, 1993) in India. The problem with this method is that it treats the private school dummy variable as exogenous, which it is unlikely to be since, in most societies, children from better off and presumably more educationally-oriented homes are more likely to attend private schools.

Most of the current studies that use OLS as a method for estimating the private school effect are aware of the endogeneity issue, but use these estimates as a baseline from which to make further estimates that try to control for this problem. The OLS private school dummy baseline provides an upper bound of the private school effect, because it includes the effect from other unobserved variables in addition to the ‘pure’ effect of private schooling on cognitive outcomes.

2.3 Experimental Estimates of the Private School Effect

In estimating the effect of private schooling on achievement, one can only observe an individual attending any one type of school. What one would like to do is measure the achievement level of a sample of school children in private school, and then to measure the achievement level of the same sample in state schools, and find the difference between the two averages. The crux of the issue is that one cannot observe the counterfactual i.e. the effect of another type of schooling on the same set of individuals.

If there are unobserved differences between individuals at different types of schools, then children in state schools do not form a valid comparator group for children in private schools and the estimates of the private school effect would be biased. One way to ensure that the groups of individuals being compared are similar is to randomly assign individuals to different school types, then observed and unobserved characteristics are randomly (and hence equally) distributed between the treatment and control groups. In this case – providing the randomisation worked as intended – comparing the average achievement of children in treatment and control groups would give the true causal effect of private school attendance.

School vouchers provide a convenient method of randomly assigning school choice. Peterson et al. (2003) outline three randomised voucher schemes in the U.S. If vouchers are distributed randomly, the group that receive them (and chooses private schooling) should not be different from the control group (who would be less likely to choose private schooling). The results showed that private school effects are not always significantly positive for all groups in society and there is a difference in effects between regions. For the developing world, Angrist et. al. (2002) and Angrist et. al. (2006) provides two examples of studies that use the randomized control trial (RCT) method to estimate the private school effect in Colombia. These show positive effects of private schooling on student achievement as well as on additional outcomes such as completion rates, though the magnitude of the benefits varies between groups.

2.3.1 Natural Experiment Alternative

In the absence of random allocation, one can exploit exogenous variation in treatment caused by an event such as a policy change. The variation could be a planned social experiment- or a natural experiment. As with the random controlled trial the 'effect' is calculated using the 'difference in difference' method i.e. by comparing achievement – before and after the intervention – of the treatment and control groups. However there are likely to be underlying differences in the unobserved characteristics between children in the control and treatment groups. These differences are mitigated using a matching strategy, or modelling participation in the treatment group using a Heckman two step approach.

The motivation behind matching is to improve the similarity between the treatment (private) and control (state) school groups of individuals; the objective is to find a good counterfactual (control) unit for each treated unit such that the control unit is as similar as possible to the treated unit. . One either selects or weights the control group according to their propensity of an individual to be in the treatment group for the analysis. The advantage of this method is that it pares the large comparator group down to only those units that are similar to the units in the treatment group (on the basis of their pre-treatment observed characteristics). However, the drawback of this approach is that matching of treatment and control units is necessarily done on their vector of observed characteristics – they could still differ in terms of their unobserved traits such as ability, ambition, motivation and effort.

2.3.2 Instrumental Variable Estimation

An alternative approach to estimating the impact of a variable is to use two stage least squares estimation. This uses an instrument (which may or may not arise from a natural experiment), a variable that is correlated with the endogenous variable but not otherwise correlated with the unobserved factors that affect the outcome of interest (cognitive achievement, in our case). Instrumental Variable (IV) estimation uses the common variation between the instrument and endogenous variable, and uses only this variation in determining the estimate of the effect of the variable of interest. (Wooldridge, 2002 chapter 18).

While the IV approach is sometimes used convincingly in the education production function literature, for example (Angrist and Lavy, 1999), it is often difficult to find good instruments, and there are many examples in the literature of weak instruments that only poorly predict the endogenous variable.

In the context of estimating a private school effect, it is difficult to think of variables that would affect choice of private (versus state) school but would not otherwise affect student achievement. Most factors that affect a child's choice of private or public school also affect his/her achievement outcome. A variable that has been used as an instrument for private school attendance in an achievement study on Nepal is 'the number of private schools available in the child's area of residence' (Sharma, 2009). Here the assumption is that the number of private schools in an area is plausibly exogenous to the private/public school choice of a given family, though the criticism of such an approach of course is that it will reflect the collective choice of the parents in the area.

In a specialised context the IV technique has been used in estimating a private school effect, namely in the literature on vouchers. Though the distribution of a voucher may be random, the expected effect of attending private school may still be endogenous. To allow for this, 'receipt of a voucher' (when vouchers are randomly allocated) can be used as an instrument for 'attending private school' since those who obtained a voucher in the Colombian voucher lottery were much more likely to choose to attend private school, yet the receipt of the voucher was not correlated with the unobserved characteristics of the children (Angrist *et al*, 2002). They show that the effect of 'using the voucher' (i.e. attending private school) was 50% greater than the estimate of simply 'winning the voucher' (but then not using it to attend a fee-paying school).

However, this type of an approach is available only where there is already randomised allocation of children to private and public schools, whether through vouchers or otherwise. In most developing countries in general – and in India in particular – there is no randomised allocation of students to private and public schools.

2.3.3 Heckman Selection Model

The classic application of correction for 'selection' (using the Heckman sample selectivity correction approach) is in the estimation of wage equations where the missing are the unemployed, for whom wage data is necessarily missing. This approach has also been used for estimating school effects where the outcome data (e.g. achievement scores) are not missing for different types of schooling, but where separate achievement production functions are estimated for private and state school student samples.

Sample selection bias refers to problems where the outcome equation is estimated for a restricted, non-random sample rather than for the population as a whole. Since in each of the separate achievement equations the sub-sample on which the equation is fitted (e.g. the private school sample and the state school sample) is not necessarily a random draw from the whole student population but rather a self-selected sub-sample, an important basic assumption of the classical linear regression model is violated, namely that the error term be independent of the included variables. Thus, simple OLS estimation of an achievement equation for private schoolers, and a simple OLS estimation of an achievement equation for state schoolers would both suffer from endogenous sample selectivity bias.

The choice of private school is endogenous if there are unobserved attributes of the individual and family that are related to the choice of school type that are also correlated with the cognitive achievement outcome. Thus, the problem of sample selection bias when achievement equations are separately estimated for private and public school sectors is akin to the problem of endogeneity of a private school dummy variable in an achievement equation estimated for the whole sample.

Heckman's approach involves two-step estimation. In the first step, a binary probit equation is estimated of choice of school-type (private or public). The parameters of this equation are used to estimate the predicted probability of attending private school. The researcher then calculates the Inverse Mills Ratio which is a monotonically decreasing function of the predicted probability of attending private school. In the second step, the achievement equation is estimated on the private school students' sub-sample, with the Mills Ratio as an extra term. Similarly, a separate selectivity-corrected achievement equation is fitted on the public school students' sub-sample. Finally, one can use the fitted private school achievement equation to predict the achievement score of the average student – with the mean characteristics of all students in the population as a whole – if he/she were to attend a private school and predict another achievement score for this same average student if he/she were to attend a public school, and examine whether this average student's score was higher in the private or the public sector.

The Heckman approach was used to the estimate the relative effectiveness of private and public schools in (Jiminez, Lockheed and Paqueo, 1991) and (Kingdon, 1996). Kingdon (1996) extends the more standard binary probit equation of school type choice (as between private and public school) into a multinomial logit model that allows choice between three different school types (private, aided, and government). The paper finds evidence of selection into private schooling, and presents estimates of the 'relative advantage' of private schooling that are lower than those from OLS. The advantage for private aided schools (over government schools) is eliminated, while the estimate of the private unaided school 'effect' is greatly reduced.

2.3.4 Selection on Unobservables

In the classical linear regression model adding additional variables to the equation reduces endogeneity by controlling for previously unobserved variables. Yet under any specification there are still unobserved characteristics. A method proposed by Altonji, Elder and Taber (2005) complements OLS by estimating the potential effect of remaining unobservables in such a model, by estimating how much greater the effect of unobservables would need to be relative to the observables, to eliminate the whole of the private school effect.

The method is based on the condition that if "the relationship between private schooling and the mean of the distribution of the index of the unobservables that determine outcomes is the same as the relationship between P and the mean of the observable index after adjusting for differences in the variances of these distributions." (Altonji, Elder and Taber, 2005p. 13) as shown in equation 4. Put simply this means then the relationship between the indices of unobservables shown on the LHS of the equation is the same as the relationship between the observables shown on the RHS.

$$\frac{E(\varepsilon|P=1)-E(\varepsilon|P=0)}{var(\varepsilon)}=\frac{E(X^{'}\gamma|P=1)-E(X^{'}\gamma|P=0)}{var(\varepsilon)} \qquad (4)$$

There are three assumptions. Firstly, that the observed variables are a random selection from the full set of variables (both observed and unobserved) that affect cognitive outcomes. Second, the number of variables in both the observed and full set of variables are large. Finally that there is not any one variable that dominates the outcome effect. (Altonji, Elder and Taber, 2005)

Given these assumptions we can compute how large the omitted variables bias must be to make our results invalid (i.e. to cast the whole of the private school effect as being due to the unobservables). The question posed is 'how large would the ratio on the LHS of equation 4 have to be relative to the ratio on the RHS to account for the entire estimate of the private school effects under the null hypothesis that the private school effect is zero.'

The original paper to demonstrate this method was by Altonji, Elder and Taber (2005), it estimated the 'implied ratio' for identifying a catholic school effect. (Goyal, 2008) is the only application of this method to estimating the private schooling effect in India. This paper suggests the implied ratio if 9.81 for reading and 9.76 for maths. That is, the effect of unobserved factors on student achievement would have to be nearly 10 times as large as the effect of the observed factors, for the whole of the private school effect to be due to unobserved factors. This is unlikely. Thus one is confident that the private school effect cannot be attributed wholly to unobservables, and can conclude that some of the private school effect is a real causal effect.

2.3.5 Panel Data Approach

Longitudinal data provides repeated observations of the same individuals over time. As a result, one can take advantage of the multiple measures for each individual and net out time-invariant individual characteristics. By netting out both observed and unobserved characteristics that do not vary over time, one is able to control for variables that would otherwise be correlated with both private school participation and cognitive achievement. There are two principal methods for panel data, either fixed effects estimation or random effects estimation.

The fixed effects estimator controls for time invariant unobserved heterogeneity. The time invariant characteristics drop out because they have no bearing on any temporal change in the outcome. For example gender will be the same in all periods and is thus not part of the model, one is already controlling for all observed (and time-invariant unobserved) characteristics of an individual. In simple terms one might imagine this as a idiosyncratic dummy δ_i, shifting the equation of interest up or down by some individual specific amount, the model can be specified with i (individual) and t (time period) subscripts as in equation 5.

$$y_{it} = \alpha + \delta_i + \beta P_{it} + \gamma X_{it} + \varepsilon_t \qquad (5)$$

More correctly (but mathematically equivalently) this is a deviation from the mean dependent and independent variables, shown in equation 6.

$$(y_{it} - \bar{y}_i) = \beta(P_{it} - \bar{P}_i) + \gamma(X_{it} - \bar{X}_i) + (\varepsilon_{it-}\varepsilon_i) \qquad (6)$$

The traditional intercept term is now eliminated (the constant is determined by the individual level deviation) and time-varying unobservables now are the only unobservables that still remain in the error term. For instance, if ability does not change over time, it will be netted out in an individual fixed effects model. But if ability changes over time, the portion of ability that changes over time will still remain in the error term.

This method is not particularly helpful in estimating the private school 'effect' since typically few students change school-type mid-way through their school career. In any case, longitudinal data on achievement levels and school and teacher inputs etc. for a set of children – even when they change schools – is not usually available for most countries.

A random effect model is a special case of fixed effects, making the additional assumption that the individual effects are randomly distributed, drawn from some specified distribution. In this model the error term is split into the error term: u_{it} and the random effects v_i shown in equation 7. The random effects are distributed normally, with a mean of zero and constant variance.

$$Y_{it} = \alpha + \beta P_{it} + \gamma X_{it} + u_i + \varepsilon_{it} \qquad (7)$$

This approach requires no correlation between the regressors X_{it} and the random effects u_i. Random effects is more efficient than fixed effects, because it estimates a distribution of idiosyncratic effects rather than a different intercept for each individual, thus saving degrees of freedom. To benefit from the extra efficiency of the random effects model and obtain consistent estimates, one must be sure that the 'no correlation' assumption is satisfied both theoretically and empirically. Intuitively this requires one to justify how relevant unobserved characteristics are not related to the relevant observed variables. Empirically, this is tested using a Hausman test. To our knowledge there are no examples in the literature estimating the private school effect using longitudinal data.

While the most common use of fixed effects estimation is to control for within individual variation in a panel data setting, the same estimation method may be used to control for heterogeneity between any clusters in the data. In the longitudinal panel sense the cluster is the individual; in a cross-sectional sense individuals are clustered into households, schools and geographic areas that have heterogeneous effects on the outcome of interest which can be netted out using fixed effects estimation.

In practical terms, this approach is particularly promising in estimating a private school effect as it requires only cross-section data, which is more commonly available than longitudinal data on achievement and inputs. It is possible to use fixed effects at different levels – i.e. at the level of the state, the district, the village and finally, the household. Intuitively, estimating a household fixed effects achievement production function relates the difference in achievement score of siblings, on the type of school (private vs. public) attended by the siblings. Any unobserved characteristics of the household that affect achievement – such as parental taste for education and the home educational environment – would be netted out across the siblings since they would be the same for all children within the household. Of course, it remains possible that individual children within the household will differ to some extent in their unobserved characteristics (e.g. in terms of ability or ambition etc.) but, in general, a family fixed effects method will provide a tighter upper-bound of any private school effect (than an OLS method), since it controls for those unobserved individual-level characteristics that are shared among members within a household.

2.3.6 The Methods Used in this Study

This study uses a variety of techniques to build a picture of the private school effect on achievement in India, since no technique on its own has the capacity to yield perfectly convincing estimates of the private school effect. This comparative approach enables us to examine whether different approaches yield similar conclusions about the private school effect in Indian primary schooling. The randomized trial method is not possible as we do not have appropriate data. Similarly while 2SLS and Heckman sample selectivity correction approaches are in principle feasible, we do not have convincing identifying variables that could predict private school choice but not otherwise affect achievement. Hence, we start with the private school dummy variable approach as the OLS baseline. We then use cross-section fixed effects techniques, using progressively more stringent levels of fixed effects at the level of the state, the district, the village and finally household fixed effects. We supplement this with longitudinal data analysis by constructing a village level panel data over time and use village and time fixed effects. Finally we use the method suggested by Altonji, Elder and Taber (2005) to examine whether the size of the effect of the unobservables could be large enough to explain away the entire private school effect.

3 Data

3.1 The Surveys

The study uses three years of the Annual Status of Education Report (ASER) surveys, from 2005 to 2007. These surveys were conducted by a group of 776 NGOs and institutions under the banner of *Pratham*, an educational NGO from Mumbai. The motivation behind such a comprehensive study was to assess the state of learning and school enrolment in India. During this period the annual survey aims to cover about 400 households in each one of India's 580 odd districts, yielding a large national dataset of about 330,000 households, with learning achievement tests from over 1.1 million children aged 6-14.

The 2005 ASER data included a household survey and a separate survey of the main government school in the sample villages. The household survey focussed on the schooling of the children and tested each child's level of ability in maths and reading. For the survey of the village government school, measures were taken of school attendance and a series of indicators of school quality.

In 2006, there was a household survey but no school survey. The household survey collected the same individual-level information as in 2005. Additional information was collected in the household survey regarding the mother's characteristics, including on mother's education and reading ability. In 2006 all of the villages sampled in 2005 were sampled, and an additional set of villages were sampled, about half as many again as in 2005.

In 2007, both a household survey and a school survey were carried out. In the household survey there was more extensive testing of children and also data on some of the mother's characteristics collected in 2006 were also collected again. The survey of the largest government school in the village was more comprehensive than in 2005, collecting more information on school quality variables, and looking at some characteristics specific to certain grades. In 2007 the 'new' villages of 2006 were re-sampled, also half of those sampled in 2005 were re-sampled, the other half being replaced by a new sample of villages. Roughly speaking, if one split the entire sample of villages in the three years of the survey into four quarters, each of approximately five thousand villages, then one quarter are sampled in both 2005 and 2006 but then dropped for 2007; another quarter are surveyed in all three years; another quarter are not sampled in 2005, but are in 2006 and 2007; and a final quarter that are sampled only in 2007.

3.2 The Sampling Methodology

All Indian states were included in the sample, and within each state the rural parts of all districts were used. The sampling took place at the village and household level. To be cost effective Pratham needed a sample size that was sufficiently large to be able to draw statistically significant conclusions, yet at the same time minimise costs. Pratham calculated that reliable inference required 400 households for each district. Ideally these would be drawn as a random sample; however there was no complete list of households within districts. Instead there was an arbitrary decision to sample 20 households in 20 villages within each district. The village was randomly selected using probability proportional to size for each district. For sampling households within each village there were no lists of households to make a random sample. Instead the interviewer was asked to use a random sampling method. Each village was divided into four sections by the interviewer, in each section the interviewer chose a central household for the first survey. They then chose every fifth (in larger villages a larger interval was used) household in a circular fashion until they had selected five households for that section. This is repeated for each of the four sections yielding 20 households for each village. The advantage of this approach is that villages in India are often divided into separate hamlets and so interviewers may miss households on the periphery. By dividing villages into sections it ensures all parts of the village are covered.

3.3 Strengths and Weaknesses of the Data

The primary strength of the data set is the enormous sample size, with 265,460 children aged 6-14 in 2005, 433,972 in 2005 and 410,379 in 2007 used in this analysis. These samples represent the rural proportion of approximately 200 million school aged children for the country as a whole. In addition to the large number of students surveyed, there is data on the quality of thousands of schools providing a clear and representative picture of the state of rural schooling in India.

A second strength of the survey is the fact that children were tested at the household level. Such an approach is rare because it is much more expensive to test children in the home than in a school where there are large numbers of students, well organised into ages and ability and with the facilities for testing. This feature allows us to be much more confident in our findings as it prevents the bias associated with testing in schools from teachers putting their most able students forward.

Though the data contain important control variables, a concern with making inference on any data set is that many variables that are important to achievement are omitted from the data. Firstly, income data or a socio economic status measure would have allowed us to distinguish any private school effect from the effect of family affluence. Unfortunately, such information was not collected. Secondly, data on the motivation, natural ability or prior achievement of the student would allow us to make more confident statements about any private school effect because these unobserved traits may be correlated with the private school choice (e.g. it may be that the more motivated children, e.g. from more motivated and educationally-oriented families go to private schools). The final variable that would have been useful is the caste and religion of the child, as these are major sources of discrimination in India and it would be important to see how this impacts on student achievement.

However, we will use econometric techniques that enable us to overcome these data deficiencies, at least to a large extent. For example, household fixed effects estimation will do away with the disadvantage of not having data on household income, SES, caste, religion etc. since these remain the same for all siblings within the household. Even the effect of unobserved traits such as motivation and ability are likely to be lower in a family fixed effects equation than in a simple OLS equation since motivation and ability are often genetically passed on from parent to child and are shared within the family, at least to some extent.

3.4 Regression Variables

Achievement was measured using tests of the students in both maths and language, and assigned a level of up to three or four respectively, as shown in Table 1. For the analysis, a single outcome measure was made by adding the maths and language scores. This was standardised within each year by first taking a child's achievement score, subtracting the mean achievement score of all students in that year and then dividing by the standard deviation of achievement for that year. Thus we work with the z-score of achievement mark rather than with absolute achievement mark.

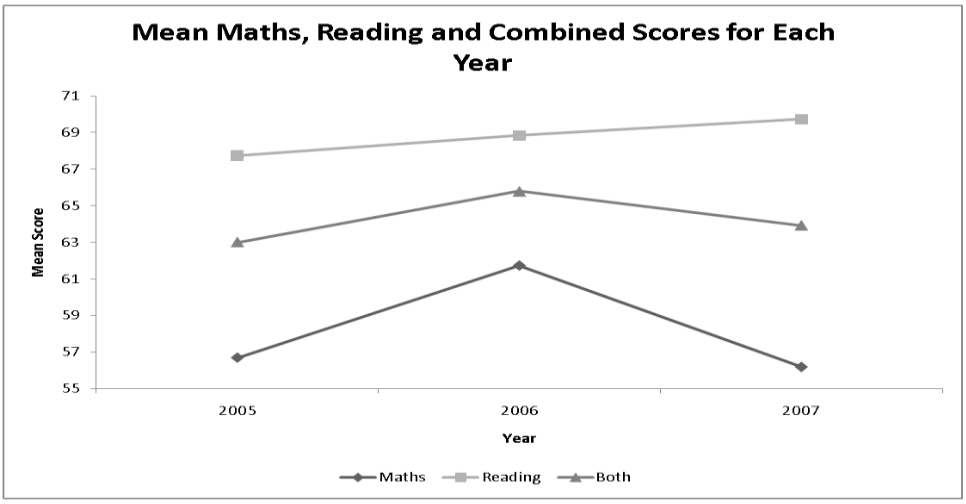
Table 1
Cognitive outcomes

Language	Mark	Maths	Mark
Could do nothing	0	Could do nothing in maths	0
Could read letters	1	Could recognise two digit numbers[a]	1
Could read words	2	Could do two-digit subtraction	2
Could read a paragraph	3	Could do three by one digit division	3
Could read a story	4		

[a] 'Could recognise numbers 1 to 9' was added in 2007. For our analysis, we have included this with 'could do nothing in maths'.

Figure 1 presents the mean maths, reading and total cognitive achievement mark in each year (the outcomes were converted to percentages to aid comparison). The scale of the graph was chosen to exaggerate the differences between years. Reading mark shows a small but consistent growth rate over time whereas Maths achievement fluctuates, with a large jump in 2006 and the 2007 estimate slightly below the 2005 estimate. It is possible that the reason that maths achievement fell in 2007 was because the maths test changed slightly in that year's survey. In 2007 an additional category was added of whether the child could recognise single digit numbers, this fell between 'being able to do nothing in maths' and 'being able to recognise 2 digit numbers'. For the purposes of our analysis this category was recoded with 'being able to do nothing' but it is possible that the presence of the this extra intermediate level affected outcomes in the category above depressing the results for that year.

Figure 1



The independent variables were organised into three levels: the child level, the household level, and regional level. Individual child characteristics include child's gender, age and current schooling. At the household level there is the household size in every year and there were additional characteristics regarding the mother in the 2006 and 2007 surveys. This included the mother's age, the highest schooling obtained (i.e. highest grade completed) by the mother and also a test of whether the mother was able to read. At the regional level the village, the district and the state in which the child lives were recorded.

The descriptive statistics of the variables used in the regression are presented in Table 3 and 4. The first table summarises the individual child level data, with columns for the mean and standard deviation of each variable for every year; in the following table this sequence is repeated for the aggregated village level data. The mean and SD of the achievement z-score are 0 and 1 (by construction) in each of the three years.

In the survey the school type was recorded into five categories, government, private, EGS/AEI (Education Guarantee Scheme / Alternative Education Institution); Madrasa and out of school. The primary concern of this paper was to distinguish between the government and private school. EGS/AEI were included with government schooling as these are simply special types of government school, these represent only 0.66 of one percent of the sample. Madrasa was not included because this is a religious rather than an academic type of schooling and as such the decision will be driven by religious preferences rather than the school quality differences that we are trying to isolate. Those who were out of school were also dropped from the analysis because such children are most likely engaged in other activities and so the schooling choice becomes irrelevant. Dropping the Madrasa and out of school children should not affect our results unduly because they represent such small part of the population, only 0.7 and 5.65 percentage points respectively. While removing about 6 percent of the sample could cause our achievement production function to suffer from sample selectivity bias, we have not dealt with this potential econometric problem for two reasons. Firstly, with such a small proportion of 0s (94% being 1s), it would be difficult to properly identify a first stage binary probit equation of enrolment choice. Secondly, even if these were not issues, we do not have any convincing identifying variables with which to identify the selectivity term lambda, i.e. there are no variables that affect enrolment choice but do not plausibly also affect the achievement outcome. It seems unlikely that the small percentage of excluded children will be a major source of sample selectivity bias in identifying the private school effect.

Table 3 and 4 show that the sex ratio in the 6-14 age population is an alarmingly low 0.45 across the three years, lending support to Amartya Sen’s (1992) ‘missing women’ hypothesis which suggests widespread male-child preference on the part of parents. Whether such pro-male gender bias in education manifests itself in lower learning achievement of girls is revealed in the regression analysis.

At the household level the mother’s schooling and literacy prove to be the most important. The proportion of the mothers that never received any formal schooling is fairly high at over fifty percent. Unsurprisingly this is correlated with whether the mother can read and again this represents more than half of the population.

4 Results

This section discusses findings from the four methods which were used to estimate the private school effect. Cross-sectional OLS regression as a baseline; state, district, village and household fixed effects to control for heterogeneity at each cluster level; longitudinal analysis to net out time-invariant heterogeneity; and finally estimating the effect of unobservables relative to the private school effect.

The full regression results are presented together in the appendix. Rather than always quote results from each year, the estimate from the pooled regression was used as a summary for the cross-sectional analysis, with the intention that the reader could cross-reference with the year specific results in the appendix.

4.1 Cross-Sectional Analysis

A simple OLS cross-section regression is summarised by equation 8, here the outcome measure y is the standardised achievement score. The independent variable P is the private school dummy and X is a vector of the control variables.

$$y = \alpha + \beta P + \gamma X + \varepsilon \tag{8}$$

Table 5, 6, 7 and 8 present cross-section regressions of the achievement production function for years 2005, 2006, 2007 and pooled respectively. The controls used were the gender and age of the child. Age was treated as a categorical variable because the age profile followed a complex pattern and the large sample size meant that saving degrees of freedom was not a concern. The full estimates from this specification are shown in the first column of Table 5 to 8.

First we take a brief look at results other than on the private school dummy variable. All variables are very precisely determined, due to our large sample sizes. In 2005, girls' achievement was about 0.038 SD lower than boys' but in 2006 this falls to 0.025 SD and further to 0.016 SD in 2007, suggesting an equalising trend in achievement levels. However, less benignly, the gender gap in achievement continues to exist even in the household fixed effects equation in the last column of each table, which suggests that the gender gap in achievement is an *intra-household* phenomenon.

Achievement increases monotonically with age: It increases by about 0.35 SD per year between ages 6 and 9 but then increases progressively more slowly each year after that. However this trend may be due to the test being designed to evaluate competencies more appropriate to the early grades and thus not able to show advances beyond basic arithmetic and reading a story.

Turning to the variable of most interest, the estimate of the private school effect was 0.247 in 2005, 0.201 in 2006, 0.246 in 2007 and 0.228 in the pooled data. That is, after controlling for age and gender, private school attendees have cognitive achievement between 0.20 and 0.25 standard deviations (SD) higher than government school attendees. This is about seven times the effect of gender, and almost equal to the effect of an extra year of education, on average over the age range 6-14.

The OLS estimates provide the ‘upper bound’ for the private school effect; one can refine the OLS estimates by using a cluster level fixed effect, to control for both observed and unobserved heterogeneity between clusters. We estimate equation 9, where the i subscript denotes the cluster and the t subscript the individual within that cluster.

$$(y_{it} - \overline{y_i}) = \beta(P_{it} - \overline{P_i}) + \gamma(X_{it} - \overline{X_i}) + (\varepsilon_{it-}\varepsilon_i) \qquad (9)$$

In the data we have four potential levels of clustering: the state, the district, the village and the household. This allows us to control for observable and unobservable differences between the different clusters and thus produce more accurate estimates of the effects of the independent variables. It also allows us to see how the private school effect changes when we use progressively lower and lower levels of geographical aggregation as our clustering variable: state, then district, then village, then household. As one moves to a more localised level of fixed effects, one is eliminating the differences due to the location of the individual and thus one can see which independent variable effects are consistent at all levels and those for which the regional effects formed some part of the estimate.

In India, some of the states are larger than most countries of the world. Thus to net out unobserved differences between these states allows one to control for both the observed differences in education policies but also the more subtle unobserved or unmeasured differences between states. Using state fixed effects, the estimate for the private school effect is 0.281 SD in 2005, 0.245 SD in 2006, 0.257 SD in 2006 and 0.255 SD in the pooled data. The effect size is statistically significantly larger than the OLS estimate in all specifications and years but economically it is not all that larger.

The Indian districts, like the states, comprise vast areas and as such the district fixed effect is able to control for the social, political and geographic differences between these regions. The district fixed effects results do not change much from the state fixed effects results, with an estimate of the private school effect of 0.286 SD in 2005, 0.225 SD in 2006, 0.246 SD in 2007 and 0.241 SD in the pooled data, though these estimates are still somewhat above the OLS estimates.

Using a village level fixed effect one finally begins to control for the observed and unobserved difference at a level that really affects the everyday lives of the individuals in the survey. Including village fixed effects allows us to control for observables that affect achievement such as school quality, as well as less easily measured variables such as level of motivation and organisation within schools. A separate study (French, 2008) using the same data showed that the quality of the local government school had an effect on private school attendance and cognitive outcomes. Because school quality was only measured for government schools in the sample villages, the quality measures would be hard to interpret in the context of comparing government and private school attainment. By using village fixed effects one is effectively controlling for both government school and private school quality at the village level (though this effect is not quantified). The estimates of the private schooling effect using village level fixed effects was 0.273 in 2005, 0.217 in 2006, 0.240 in 2007 and 0.227 for the pooled data. These estimates are similar in magnitude to those from the plain OLS regression at the village level.

The household fixed effects are the most interesting and important of the cluster level fixed effects estimates. The strength of the statements one is able to make using such an approach rests on two assumptions. The first, that household level observables and unobservables such as parents' income and motivation have been controlled for and have equal effects for each child. Second, siblings within a household have equal unobserved characteristics such as ability and motivation. If these assumption hold the estimates of the private school effect using household fixed effects may be interpreted as though it was the same child attending different types of school and thus a true measure of the relative effectiveness of private and government schools.

While it is intuitive to argue that by using household fixed effects one controls for observable and unobservable parental factors, it is harder to justify the assumption that the children within a family have equal ability. School choice is not random and the fact that a parent has distinguished between the children by sending them to alternative schools suggests that there are differences (possibly in ability) between the children. Nevertheless, it is the case that on average an individual is more likely to be similar to their siblings than to a random other individual. To the extent that this is true, household fixed effects estimation provides a tighter upper bound of the true private school effect.

The household fixed effects estimate of the private school effect was 0.207 in 2005, 0.165 in 2006, 0.180 in 2007 and 0.180 using the pooled data for 2005 to 2007. In each sweep the household fixed effect is substantially below the OLS estimate, showing that this specification eliminates a large proportion of the previously unrevealed bias. This is the most stringent specification of the fixed effects analysis and shows that even when one has controlled for everything within the home there is still a large and significant private school advantage. These estimates compare with the household fixed effects estimates from Desai et al.(2008), which also used a national household survey from India but a much smaller one, with about 11,000 observations. Using the same controls as in this study they found household fixed effects estimates of the private school effect of 0.224 standard deviations for arithmetic skills and 0.307 for reading skills.

In 2006 and 2007 mother's characteristics were measured in the survey. These have been used in a separate set of regressions in Table 9. In both of these years the mother's age and the highest grade she achieved were included, in addition the squares of these values were used to capture the non-linear effect of these variables. The effect of private schooling was equal to about six years of mother's schooling. Clearly mother's age and education level are correlated with both private schooling and child cognitive achievement which is why their inclusion reduces the private school effect. For example the private school effect using village effects and the more parsimonious specification was 0.217 in 2006 (Table 6) and 0.240 in 2007 (Table 7).

Comparing these estimates with the equivalent specification but including the mother's characteristics reduced the estimate of the private school effect to 0.186 in 2006 and 0.208 in 2007.

In 2006 there was additional information from a test of whether the mother could read. This provided an interesting contrast to the specification using mere 'level of schooling'. Adding this to the regression proved to be more important that mother's age and schooling level. The effect of mother being able to read on child achievement was 0.057 SD, equivalent to about four years of mother's primary schooling, however when comparing mothers with higher levels of education the effect of being able to read fell to less than two years. Adding whether the mother could read had no effect on the private school effect estimate.

4.2 Longitudinal Analysis

4.2.1 Creating a Pseudo-Panel at the Village Level

The ASER data does not follow individuals over time and hence we are not able to make a longitudinal analysis at that level. The villages used in the survey in 2005 were included in subsequent waves and so one is able to construct a village level panel by averaging individual level variables within the village. Each village became a single observation in the dataset, with one, two or three years' worth of data on it, depending on how many years of the survey it was included in.

The cognitive outcome measure for each village was the village mean of the 'standardised achievement' used in the individual level analysis. While for each year the individual-level z-scores have a mean of zero and a year specific standard deviation of 1, when we take village-level mean of the z-scores of all 6-14 year olds in the sample village, the village mean of z-scores need not be zero. Similarly, the village level standard deviation is no longer equal to 1. In a longitudinal context this de-meaning of each year's sample takes away any trend in the data caused by a change in the overall scores for each year. Despite the de-meaning in terms of the outcome it was still important to use a dummy for each of the years to pick up the effect of any changes between the years, and indeed each of these year dummies proved significant, hinting that there are unobserved time variant factors that are affecting cognitive achievement but that are unobserved in the data.

The nature of the independent variables has also changed in the village panel. Where a variable was previously an individual child-level dummy variable, it would now be the proportion of 1s (mean of the 0/1 dummy variable) within the village, while continuous variables would now take the village mean. For example, where as in individual level analysis up to now, the variable 'female' took the value of 0 or 1, in the village-level panel data analysis, the variable 'female' represents the proportion of female children in the village. To aid comparison between the individual level cross-sectional analysis and village level longitudinal analysis, the following section contains the intermediate case of village-level cross-sectional analysis first.

4.2.2 Village Level Cross-Section

The results from the village level cross-sectional analysis are presented in Table 10. Before discussing the private school effect, we briefly show how the effects of age and gender at the village level support those found in the individual analysis. The estimate for the effect of gender in the pooled data suggests that in a village with all female children compared to a village with all male children mean village cognitive achievement is not significantly different from zero in all years in Table 10. As with the individual data, we see a trend of reducing gender bias, from -0.0315 SD in 2005, to -0.217 SD in 2006 and 0.0128 SD in 2007, but because these are not statistically significant, one can make no inference. When we add mother's characteristics, the gender bias becomes larger and statistically significant. The results with and without mothers characteristics tell us that the explanation for the gender bias effect (and the effect of mother quality) is different at the village level than at the individual level. At the village level the proportion of girls in the sample may increase when more girls go to school and hence are included in the sample for this analysis. The level of 'mother quality' at the village level may be more related to the degree of socialisation and development of the village, rather than having a direct effect on children's cognitive outcomes that we found at the individual level.

The effect of age is more consistent with the individual level analysis, with achievement increasing monotonically with age. It increases by an average of 0.4 SD between the ages of six and nine then grows much more slowly at 0.1 SD per year between nine and fourteen.

The village level results also reinforce the finding of a private school achievement advantage found in the individual level analysis. The beta values show the effect of a change in the private school attendance from none to all children in the village, on village mean standardised achievement; this is 0.199 SD in 2005, 0.219 SD in 2006, 0.289 SD in 2007 and 0.242 SD in the pooled data. There appears to be a slight positive trend, though one should be cautious about interpreting a trend from three years data, especially given that we found no trend under alternative specifications.

In 2006 and 2007 we are able to include mother's characteristics and these results are presented in Table 11. They show that the relationship between mother's education (M grade) and student achievement changes somewhat from that at the individual level. The inclusion of maternal education and literacy variables in 2006 causes the estimate of the private school effect to fall to 0.0549 (or 0.0353 if one includes 'mother can read'). In 2007 the private school effect falls to 0.141, not as large a fall as in 2006 but still a significant drop compared to that found in individual level analysis.

The reason that adding mother quality causes the private school effect to fall more in the village level analysis than in the individual child level analysis is due to changes in the nature of the data. In averaging the data at the village level the mother's quality changes from being an individual level variable whose main effect would be on their child's cognitive outcomes to a village level measure of mothers' education that will still affect child's cognitive outcomes but may also cause an increase in the probability of private schooling by signalling a demand for a private school.

The reason for the stronger effect of mother's education and literacy in village level than in individual-level regressions can be understood from Table 2. Table 2 show the correlation between the 'mother quality' variables and private schooling. The correlation with mother's age is low and does not change much; this variable makes little difference to the private school effect when added to the regression. However the correlation between education and literacy increases by half as much again, when the data is averaged at the village level. This suggests that at the village level while mother's quality still affects child's cognitive outcomes directly, it also has a greater effect on the propensity to attend private schooling. Thus there may be selection of private schooling into villages where mothers are more educated.

Table 2

Correlation Coefficients between the Private School Variable and Mother's Characteristics in the Individual Data and the Village Level Data

	Correlation With Private Schooling	
	Individual Data	Village Data
Mother's age	-0.0102	0.0477
Mother's Highest Grade	0.1919	0.2927
Whether Mother Can Read	0.1364	0.2002

4.2.3 Village Level Panel

While the panel data equations could be estimated using fixed effects or random effects, the more efficient random effects were rejected for three reasons. Firstly the distribution of village effects does not have a useful interpretation and is of little relevance. Second, it is hard to justify the assumption that private schooling is exogenous, because the objective of private schooling is to improve cognitive outcomes and making such a choice is related to many factors that are not observed in this survey. Finally, there is a clear empirical rejection of random effects comparing models and using the Hausman test.

Using village and time fixed effects with village level panel data (last column of Table 10), we estimate that a village moving from zero to one hundred percent private school attendance will result in a 0.190 SD increase in village mean attainment. This is similar to the effect found in individual child-level data, in the household fixed effects analysis, in Table 5 to 8. To put this in a more realistic context, if private schooling increases in a village by two standard deviations – say from 1 SD below its mean level to 1 SD above it (i.e. by about 49%, see Table 4), this would be associated with a 0.09 SD increase in mean achievement of children in the village.

The longitudinal private school effect estimate of 0.190 SD (Table 10) is smaller than those found in the cross-sectional analysis. Approximately four fifths of the individual child level estimate of 0.227 (using the pooled village fixed effects estimates from Table 8) and four fifths of the village-level estimate of 0.242 (using the pooled estimates from Table 10). This longitudinal approach allows one to find the effect of a change in private schooling over time on change in achievement over time, while controlling for all of the time-invariant unobserved village level characteristics that are associated with private school choice and cognitive achievement which biased the cross-sectional estimates.

Having mother's characteristics for two years permits the construction of a two year village panel (Table 11). As one would expect, including mothers characteristics and a panel approach leads to the lowest estimate of the private school effect of 0.114. This is lower for three reasons. Firstly because adding mother quality eliminates the effect of the mother quality omitted variable bias; second because of the bias associated with the increase in the correlation between mother quality (as shown in Table 2) and finally the longitudinal analysis examines only the effect of temporal changes within the same village so controls for village level unobservables in a more stringent way than a cross-sectional village fixed effect. It is remarkable that with just one year of change one is still able to identify an economically sizeable and statistically significant private school effect.

4.3 Selection on Unobservables

The Altonji, Elder and Taber (2005) method was applied to a number of specifications for the cognitive achievement model, but only the most stringent is reported here. This uses the fullest specification possible, including age, gender, school type and the mother's age, the highest grade that the mother achieved and whether the mother could read. These mother's characteristics are available only in the 2006 data, so this is the sample used. The estimates are calculated using village fixed effects and are presented in Table 12.

In this specification the implied ratio is 5.29. This means the effect of unobservables must be more than five times that of the observables to eliminate the entire private school effect. This suggests that there is a positive and highly statistical effect of private schooling that is unlikely to be wholly the result of selection on unobservables.

5 Conclusions

This paper used the ASER data in attempting to estimate the 'true' effect of private schooling on children's cognitive by using a series of approaches to deal with the endogenous nature of the private school choice. There is consistent evidence of a private schooling advantage throughout the methodologies. Our best estimate of the private school effect from individual child level cross-section data is from our household fixed effects method. This yields a private school effect on child achievement of about 0.17 SD. Using village level panel data including mother quality and the longitudinal methodology, there is a still a sizeable private school effect of 0.114 SD. All of the methodologies listed attempted to control for the level of unobservables, the method of Altonji, Elder and Taber (2005) showed that the whole private school effect is unlikely to be explained by unobservables.

The limitation of any study of private schooling is the extent to which one is able to control for the endogeneity of the school choice. The potential to deal with this issue depends on the quality and nature of the data. There is a shortage of data on developing countries, it is expensive to collect and depends on an infrastructure that is not necessarily in place. By choosing such an ambitious target as producing a representative dataset of rural India, the survey compromised on richness of variables in order to make the enormous sample size feasible. Further data on the individuals, such as a measure of innate ability and further data on households, particularly income would strengthen estimation. However the methodologies used here to tackle the effects of endogeneity makes one confident that the private school advantage is 'real'. This implies that there is a shortfall in the government schooling output that may be reduced by adapting some of the processes used within the private sector.

One could build upon the existing analysis by regressing different outcomes or separate estimates for different groups. Finding the private school effect for maths and reading scores separately to find if the private school impact is different for these subjects. One could also estimate the effects for primary and secondary aged children separately as the primary and upper-primary school level issues are likely to be different. Estimating the private school effect for boys and girls would be particularly interesting, in the cross sectional estimates this paper shows the gender bias reduces substantially, from -0.0401 standard deviations in 2006, to -0.0241 in 2006 and to -0.0235 in 2007. For the gender bias to reduce by almost half in only three years is a substantial achievement and separate estimations may reveal the different effects of private schooling between genders. Finally, it would be useful to do the analysis by state since educational policies of the various states differ in India and this may impinge on the relative effectiveness of private and government schools in the different states. These are useful research agendas for the future.

6 References

ALTONJI, J. G., ELDER, T. E. & TABER, C. R. (2005) Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools. *Journal of Political Economy,* 113**,** 151-184.

ANGRIST, J., BETTINGER, E., BLOOM, E., KING, E. & KREMER, M. (2002) Vouchers for Private Schooling in Colombia: Evidence from a Randomised Natural Experiment. *American Economic Review,* 92**,** 1535-1558.

BASHIR, S. (1997) The Cost-Effectiveness of Public and Private Schools: Knowledge Gaps, New Research Methodologies, and an Application in India. IN COLCLOUGH, C. (Ed.) *Marketising Education and Health in Developing Countries: Miracle or Mirage?* Oxford, Clarendon Press.

CAMPAIGN AGAINST CHILD LABOUR (1997) *Public Hearing on Child Labour, Reference Kit,* New Delhi, Document prepared for the 2nd National Convention on Child Labourers.

DESAI, S., DUBEY, A., VANNEMAN, R. & BANJERI, R. (2008) Private Schooling in India: A New Educational Landscape. *Brookings-NCAER India Policy Forum*

FRENCH, R. J. (2008) Schooling Participation and Pupil Achievement in India. *Department of Quantitative Social Science.* London, Institute of Education, University of London.

GOVINDA, R. & VARGHESE, N. V. (1993) Quality of Primary Schooling in India: A Case Study of Madhya Pradesh. *International Institute for Educational Planning, National Institute of Educational Planning and Administration*.

GOYAL, S. (2008) Inside the House of Learning: The Relative Performance of Public and Private Schooling in India. *Education Economics,* 17**,** 315-327.

HALSEY, A., A, H. & RIDGE, J. (1980) *Origins and destinations: Family , Class and Education in Modern Britain,* Oxford, Clarendon Press.

HANUSHEK, E. A. (2003) The Failure of Input-Based Schooling Policies. *The Economic Journal,* 113**,** F64-F68.

HANUSHEK, E. A. & WOESSMANN, L. (2008) The Role of Education Quality in Economic Growth. *World Bank Policy Research Working Paper* 4122.

JIMINEZ, E., LOCKHEED, M. & PAQUEO, V. (1991) The Relative Efficiency of Private and Public Schools in Developing Countries. *World Bank Research Observer,* 6**,** 205-18.

KINGDON, G. G. (1996) The Quality and Efficiency of Private and Public Education: A Case Study of Urban India. *Oxford Bulletin of Economics and Statistics,* 58**,** 25.

KRUEGER, A. B. (2003) Economic Considerations and Class Size. *The Economic Journal,* 113**,** F34-F63.

MONK, D. H. (1989) The Education Production Function: It's Evolving Role in Policy Analysis. *Educational Evaluation and Policy Analysis,* 11**,** 31-45.

MURALIDHARAN, K. & KREMER, M. (2006) Public and Private Schools in Rural India. *Harvard*.

NECHYBA, T. J. (2000) Mobility, Targeting, and Private-School Vouchers. *American Economic Review,* 90**,** 130-146.
PETERSON, P. E., HOWELL, W. G., WOLF, P. J. & CAMBELL, D. E. (2003) School Vouchers: Results from Randomised Experiments. IN HOXBY, C. M. (Ed.) *The Economics of School Choice.* Chicago, University of Chicago.
PSACHAROPOULOS, G. (1987) Private versus Public Schools in Developing Countries: Evidence from Colombia and Tanzania. *International Journal of Educational Development,* 7**,** 59-67.
SEN, A. (1992) Missing Women. *British Medical Journal,* 304**,** 916-7.
SHARMA, N. (2009) Public and Private Schools in Nepal: A Comparative Perspective.
TOOLEY, J. & DIXON, P. (2005) An Inspector Calls: The Regulation of 'Budget' Private Schools in Hyderabad, Andhra Pradesh, India. *International Journal of Educational Development,* 25**,** 269-285.
WOOLDRIDGE, J. (2002) *Econometric Analysis of Cross-Sectional and Panel Data,* Cambridge, Massachusetts, MIT Press.

7 Appendix

Table 3

Descriptive Statistics for Regression Variables across Each Year at the Individual Level

	2005		2006		2007	
	Mean	SD	Mean	SD	Mean	SD
Score	0	1	0	1	0	1
Female	0.445	0.497	0.450	0.497	0.455	0.498
Age	9.739	2.424	9.942	2.508	9.835	2.443
Private	0.174	0.379	0.215	0.411	0.206	0.404
HH size	6.787	3.037	7.198	3.363	6.429	2.622
Mother age			34.948	7.991	33.544	5.807
Mother age^2			1285.248	662.755	1158.904	422.179
Mother grade			3.164	4.063	3.231	4.067
Mother grade^2			26.512	41.420	26.979	41.238
Mother read			0.369	0.482		
Observations	265460		433972		410379	

Table 4

Descriptive Statistics for Regression Variables Across Each Year at the Village Level[a]

	2005		2006		2007	
	Mean	SD	Mean	SD	Mean	SD
Score	0.007	0.491	0.016	0.494	-0.001	0.503
Female	0.445	0.128	0.453	0.139	0.455	0.134
Age	9.745	0.855	9.978	0.784	9.839	0.811
Private	0.171	0.228	0.216	0.245	0.205	0.242
HH size	6.629	1.834	6.839	2.018	6.257	1.534
Mother age			34.785	3.881	33.392	3.087
Mother age^2			1275.305	315.784	1149.077	223.062
Mother grade			3.349	2.525	3.386	2.586
Mother grade^2			28.323	25.600	28.410	26.086
Mother read			0.489	0.315		
Observations	9037		15616		14895	

[a] The mean ‘Score’ here is the average (across villages) of the village-level mean of individual children’s achievement z-scores. i.e. first we calculated the mean of the achievement z-scores of individual children within a village, and then have taken the mean of that village level achievement variable. That is why the mean and SD of the achievement z-score here are not equal to 0 and 1 respectively.

Table 5

2005 Cross-Sectional Regression of Cognitive Achievement Using Fixed Effects Estimation

	No FE	State FE	District FE	Village FE	HH FE
Female	-0.0379***	-0.0411***	-0.0381***	-0.0370***	-0.0401***
	(0.00365)	(0.0147)	(0.00444)	(0.00303)	(0.00304)
Age 7	0.398***	0.400***	0.401***	0.402***	0.400***
	(0.00780)	(0.0150)	(0.00950)	(0.00667)	(0.00643)
Age 8	0.745***	0.750***	0.755***	0.763***	0.775***
	(0.00762)	(0.0259)	(0.0107)	(0.00680)	(0.00604)
Age 9	1.098***	1.098***	1.095***	1.099***	1.105***
	(0.00873)	(0.0280)	(0.0130)	(0.00755)	(0.00655)
Age 10	1.342***	1.349***	1.356***	1.365***	1.381***
	(0.00838)	(0.0268)	(0.0133)	(0.00748)	(0.00625)
Age 11	1.557***	1.560***	1.571***	1.583***	1.599***
	(0.00909)	(0.0268)	(0.0145)	(0.00791)	(0.00685)
Age 12	1.680***	1.683***	1.694***	1.708***	1.725***
	(0.00859)	(0.0245)	(0.0149)	(0.00777)	(0.00657)
Age 13	1.796***	1.800***	1.813***	1.827***	1.836***
	(0.00907)	(0.0265)	(0.0153)	(0.00825)	(0.00714)
Age 14	1.882***	1.890***	1.900***	1.912***	1.923***
	(0.00897)	(0.0331)	(0.0163)	(0.00847)	(0.00727)
Private	0.247***	0.281***	0.286***	0.273***	0.207***
	(0.00806)	(0.0438)	(0.0127)	(0.00702)	(0.00700)
HH size	-0.0104***	-0.00435**	-0.00311***	0.000404	
	(0.00118)	(0.00198)	(0.00118)	(0.000719)	
Constant	-1.078***	-1.127***	-1.144***	-1.174***	-1.168***
	(0.0108)	(0.0237)	(0.0135)	(0.00751)	(0.00487)
R^2	0.372	0.380	0.402	0.440	0.471
Observations	265460	265460	265460	265460	265460
States		28			
Districts			486		
Villages				9037	
Households					78321

Standard errors in parentheses. *** p < 0.01 . ** p < 0.05 . * p < 0.1 .

Table 6

2006 Cross-Sectional Regression of Cognitive Achievement Using Fixed Effects Estimation

	No FE	State FE	District FE	Village FE	HH FE
Female	-0.0248***	-0.0232**	-0.0235***	-0.0233***	-0.0242***
	(0.00275)	(0.0107)	(0.00344)	(0.00220)	(0.00225)
Age 7	0.365***	0.360***	0.360***	0.364***	0.375***
	(0.00596)	(0.0143)	(0.00735)	(0.00503)	(0.00505)
Age 8	0.721***	0.718***	0.719***	0.722***	0.739***
	(0.00570)	(0.0215)	(0.00881)	(0.00495)	(0.00459)
Age 9	1.067***	1.059***	1.061***	1.061***	1.079***
	(0.00685)	(0.0211)	(0.0112)	(0.00578)	(0.00512)
Age 10	1.335***	1.332***	1.331***	1.335***	1.362***
	(0.00624)	(0.0260)	(0.0114)	(0.00548)	(0.00473)
Age 11	1.572***	1.567***	1.567***	1.569***	1.591***
	(0.00676)	(0.0253)	(0.0116)	(0.00584)	(0.00516)
Age 12	1.698***	1.696***	1.696***	1.699***	1.729***
	(0.00612)	(0.0251)	(0.0113)	(0.00554)	(0.00479)
Age 13	1.835***	1.835***	1.832***	1.829***	1.854***
	(0.00627)	(0.0340)	(0.0119)	(0.00567)	(0.00507)
Age 14	1.924***	1.919***	1.915***	1.913***	1.942***
	(0.00613)	(0.0413)	(0.0122)	(0.00571)	(0.00505)
Private	0.201***	0.245***	0.225***	0.217***	0.165***
	(0.00574)	(0.0392)	(0.00903)	(0.00450)	(0.00447)
HH size	-0.0120***	-0.00465***	-0.00377***	-0.000714	
	(0.000837)	(0.00153)	(0.000848)	(0.000539)	
Constant	-1.108***	-1.167***	-1.169***	-1.191***	-1.204***
	(0.00813)	(0.0196)	(0.0111)	(0.00576)	(0.00374)
R^2	0.402	0.414	0.431	0.472	0.515
Observations	433972	433972	433972	433972	433972
States		31			
Districts			555		
Villages				15616	
Households					127139

Standard errors in parentheses. *** p < 0.01 . ** p < 0.05 . * p < 0.1 .

Table 7

2007 Cross-Sectional Regression of Cognitive Achievement Using Fixed Effects Estimation

	No FE	State	District FE	Village FE	HH FE
Female	-0.0164***	-0.0206	-0.0200***	-0.0218***	-0.0235***
	(0.00277)	(0.0123)	(0.00339)	(0.00221)	(0.00225)
Age 7	0.364***	0.365***	0.363***	0.362***	0.367***
	(0.00579)	(0.0160)	(0.00741)	(0.00508)	(0.00501)
Age 8	0.736***	0.741***	0.736***	0.739***	0.752***
	(0.00581)	(0.0269)	(0.00946)	(0.00515)	(0.00464)
Age 9	1.106***	1.109***	1.101***	1.099***	1.113***
	(0.00686)	(0.0287)	(0.0120)	(0.00593)	(0.00512)
Age 10	1.385***	1.388***	1.383***	1.391***	1.416***
	(0.00646)	(0.0335)	(0.0123)	(0.00580)	(0.00483)
Age 11	1.620***	1.624***	1.620***	1.628***	1.652***
	(0.00678)	(0.0279)	(0.0123)	(0.00600)	(0.00521)
Age 12	1.743***	1.746***	1.744***	1.755***	1.782***
	(0.00629)	(0.0260)	(0.0121)	(0.00579)	(0.00496)
Age 13	1.877***	1.881***	1.877***	1.886***	1.910***
	(0.00644)	(0.0279)	(0.0121)	(0.00590)	(0.00525)
Age 14	1.965***	1.964***	1.963***	1.970***	1.986***
	(0.00632)	(0.0288)	(0.0124)	(0.00604)	(0.00537)
Private	0.246***	0.257***	0.246***	0.240***	0.180***
	(0.00608)	(0.0390)	(0.00983)	(0.00469)	(0.00456)
HH size	-0.0110***	-0.00188	-0.000504	0.00231***	
	(0.00108)	(0.00216)	(0.00113)	(0.000637)	
Constant	-1.147***	-1.208***	-1.211***	-1.232***	-1.221***
	(0.00847)	(0.0229)	(0.0110)	(0.00591)	(0.00383)
R^2	0.415	0.427	0.444	0.498	0.543
Observations	410379	410379	410379	410379	410379
States		30			
Districts			562		
Villages				14895	
Households					124749

Standard errors in parentheses. *** p < 0.01 . ** p < 0.05 . * p < 0.1 .

Table 8

Pooled Cross-Sectional Regression of Cognitive Achievement Using Fixed Effects (Pooling 2005, 2006 and 2007 data)

	No FE	State FE	District FE	Village FE	HH FE
Female	-0.0247***	-0.0268**	-0.0276***	-0.0255***	-0.0275***
	(0.00175)	(0.0116)	(0.00283)	(0.00157)	(0.00141)
Age 7	0.372***	0.371***	0.371***	0.372***	0.377***
	(0.00368)	(0.0124)	(0.00508)	(0.00341)	(0.00311)
Age 8	0.732***	0.736***	0.737***	0.737***	0.752***
	(0.00364)	(0.0208)	(0.00652)	(0.00340)	(0.00287)
Age 9	1.089***	1.089***	1.087***	1.087***	1.097***
	(0.00428)	(0.0208)	(0.00790)	(0.00394)	(0.00317)
Age 10	1.355***	1.358***	1.358***	1.361***	1.386***
	(0.00399)	(0.0240)	(0.00834)	(0.00375)	(0.00298)
Age 11	1.585***	1.586***	1.585***	1.590***	1.615***
	(0.00428)	(0.0191)	(0.00833)	(0.00398)	(0.00324)
Age 12	1.711***	1.712***	1.713***	1.719***	1.748***
	(0.00391)	(0.0199)	(0.00828)	(0.00371)	(0.00305)
Age 13	1.842***	1.844***	1.842***	1.846***	1.871***
	(0.00409)	(0.0254)	(0.00880)	(0.00388)	(0.00325)
Age 14	1.930***	1.929***	1.927***	1.932***	1.954***
	(0.00398)	(0.0303)	(0.00899)	(0.00383)	(0.00328)
Private	0.228***	0.255***	0.241***	0.227***	0.180***
	(0.00381)	(0.0383)	(0.00792)	(0.00348)	(0.00292)
HH size	-0.0113***	-0.00380**	-0.00288***	-0.00481***	
	(0.000590)	(0.00144)	(0.000782)	(0.000486)	
2006	-0.0449***	-0.0460	-0.0450***	-0.0170***	
	(0.00566)	(0.0419)	(0.0139)	(0.00582)	
2007	-0.0326***	-0.0254	-0.0233	-0.00898	
	(0.00634)	(0.0404)	(0.0142)	(0.00669)	
Constant	-1.085***	-1.144***	-1.148***	-1.152***	-1.202***
	(0.00670)	(0.0382)	(0.0131)	(0.00606)	(0.00235)
R^2	0.399	0.408	0.418	0.434	0.514
Obs.	1109811	1109811	1109811	1109811	1109811
States		32			
Districts			584		
Villages				17920	
Households					330180

Standard errors in parentheses. *** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.

Table 9

Cross-sectional Regression of Cognitive Achievement Using Village Fixed Effects Estimation and Including Mother's Characteristics

	2006[a]	2006	2007	Pooled
Female	-0.0245***	-0.0245***	-0.0233***	-0.0246***
	(0.00218)	(0.00219)	(0.00220)	(0.00170)
Age 7	0.366***	0.366***	0.362***	0.365***
	(0.00501)	(0.00501)	(0.00506)	(0.00378)
Age 8	0.726***	0.726***	0.739***	0.734***
	(0.00494)	(0.00494)	(0.00515)	(0.00375)
Age 9	1.065***	1.065***	1.097***	1.083***
	(0.00577)	(0.00577)	(0.00593)	(0.00438)
Age 10	1.341***	1.341***	1.390***	1.367***
	(0.00551)	(0.00551)	(0.00584)	(0.00416)
Age 11	1.574***	1.574***	1.625***	1.599***
	(0.00586)	(0.00586)	(0.00605)	(0.00442)
Age 12	1.705***	1.705***	1.752***	1.730***
	(0.00561)	(0.00561)	(0.00589)	(0.00417)
Age 13	1.836***	1.836***	1.882***	1.858***
	(0.00576)	(0.00576)	(0.00604)	(0.00432)
Age 14	1.919***	1.919***	1.966***	1.946***
	(0.00583)	(0.00583)	(0.00622)	(0.00434)
Private	0.185***	0.186***	0.208***	0.174***
	(0.00442)	(0.00442)	(0.00461)	(0.00366)
HH size	-0.00112**	-0.00112**	0.00210***	-0.00458***
	(0.000527)	(0.000527)	(0.000632)	(0.000530)
M age	0.00516***	0.00514***	0.0197***	0.00764***
	(0.000925)	(0.000925)	(0.00166)	(0.000933)
M age^2	-0.0000331***	-0.0000329***	-0.000237***	-0.0000624***
	(0.0000110)	(0.0000110)	(0.00002.25)	(0.0000115)
M grade	0.00842***	0.0197***	0.0155***	0.0272***
	(0.00155)	(0.00110)	(0.00114)	(0.000980)
M grade^2	0.000973***	0.000361***	0.000694***	0.000179**
	(0.000119)	(0.000104)	(0.000108)	(0.0000904)
M read[b]	0.0570***			
	(0.00569)			
2007				0.00931
				(0.00573)
Constant	-1.399***	-1.394***	-1.676***	-1.448***
	(0.0191)	(0.0191)	(0.0301)	(0.0189)
R^2	0.480	0.479	0.505	0.463
Observations	433972	433972	410379	844351
Villages	15616	15616	14895	17895

[a] Whether the mother could also read was included in these estimates, but not in the subsequent regressions, because it was not available.

[b] For 'Whether the mother could read' an additional 'missing dummy' was included, this effectively makes this into a categorical variable where the mother could read, couldn't read, or was missing. This allowed me to include this variable without which there would have been too many missing to make it feasible.

Standard errors in parentheses. *** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.

Table 10

Regression of Village Mean Cognitive Achievement, for Each Year and for Pooled Data (Cross-Sectional) and using Village and Time Fixed Effects(Longitudinal)

	2005	2006	2007	Pooled	Longitudinal
Female	-0.0315	-0.0217	0.0128	-0.0118	-0.0841***
	(0.0369)	(0.0257)	(0.0279)	(0.0168)	(0.0246)
Age 7	0.266***	0.323***	0.351***	0.323***	0.352***
	(0.0855)	(0.0651)	(0.0628)	(0.0399)	(0.0568)
Age 8	0.460***	0.613***	0.617***	0.586***	0.745***
	(0.0827)	(0.0660)	(0.0626)	(0.0397)	(0.0557)
Age 9	1.172***	1.056***	1.131***	1.118***	1.175***
	(0.0775)	(0.0661)	(0.0628)	(0.0391)	(0.0559)
Age 10	1.078***	1.257***	1.243***	1.208***	1.331***
	(0.0722)	(0.0585)	(0.0576)	(0.0355)	(0.0496)
Age 11	1.180***	1.455***	1.431***	1.378***	1.342***
	(0.0768)	(0.0641)	(0.0638)	(0.0387)	(0.0536)
Age 12	1.394***	1.516***	1.463***	1.454***	1.556***
	(0.0744)	(0.0591)	(0.0586)	(0.0361)	(0.0490)
Age 13	1.512***	1.810***	1.674***	1.687***	1.681***
	(0.0812)	(0.0607)	(0.0631)	(0.0383)	(0.0514)
Age 14	1.619***	1.977***	1.920***	1.880***	1.837***
	(0.0852)	(0.0607)	(0.0619)	(0.0385)	(0.0532)
Private	0.199***	0.219***	0.289***	0.242***	0.190***
	(0.0209)	(0.0146)	(0.0156)	(0.00950)	(0.0166)
HH size	-0.0368***	-0.0303***	-0.0350***	-0.0335***	-0.0104***
	(0.00261)	(0.00179)	(0.00244)	(0.00126)	(0.00203)
2006				-0.0368***	-0.0334***
				(0.00605)	(0.00577)
2007				-0.0449***	-0.0260***
				(0.00604)	(0.00670)
Constant	-0.695***	-0.922***	-0.909***	-0.831***	-1.002***
	(0.0573)	(0.0467)	(0.0460)	(0.0285)	(0.0395)
R^2	0.174	0.191	0.182	0.182	0.175
Observations	9037	15616	14895	39548	39548
Unique obs.					21433

Standard errors in parentheses. *** p < 0.01 . ** p < 0.05 . * p < 0.1 .

Table 11

Regression of Village Mean Cognitive Achievement Including Mother's Characteristics. For 2006, For 2007, For Both Years Pooled (Cross-Sectional) and Using Time Fixed Effects (Longitudinal)

	2006[a]	2006	2007	Pooled	Longitudinal
Female	-0.127***	-0.104***	-0.0557**	-0.0810***	-0.0635*
	(0.0250)	(0.0246)	(0.0268)	(0.0182)	(0.0330)
Age 7	0.266***	0.303***	0.321***	0.321***	0.283***
	(0.0627)	(0.0621)	(0.0603)	(0.0432)	(0.0779)
Age 8	0.664***	0.690***	0.669***	0.688***	0.663***
	(0.0646)	(0.0631)	(0.0602)	(0.0435)	(0.0754)
Age 9	0.995***	0.993***	1.044***	1.032***	1.071***
	(0.0644)	(0.0632)	(0.0605)	(0.0436)	(0.0784)
Age 10	1.248***	1.265***	1.217***	1.244***	1.302***
	(0.0577)	(0.0560)	(0.0557)	(0.0394)	(0.0700)
Age 11	1.308***	1.326***	1.347***	1.345***	1.364***
	(0.0632)	(0.0614)	(0.0617)	(0.0435)	(0.0755)
Age 12	1.476***	1.491***	1.460***	1.486***	1.482***
	(0.0575)	(0.0568)	(0.0569)	(0.0401)	(0.0690)
Age 13	1.583***	1.628***	1.556***	1.604***	1.639***
	(0.0594)	(0.0585)	(0.0612)	(0.0422)	(0.0703)
Age 14	1.796***	1.803***	1.841***	1.831***	1.737***
	(0.0599)	(0.0587)	(0.0605)	(0.0420)	(0.0742)
Private	0.0353**	0.0549***	0.141***	0.0951***	0.114***
	(0.0148)	(0.0149)	(0.0160)	(0.0109)	(0.0247)
HH size	-0.0172***	-0.0184***	-0.0191***	-0.0188***	-0.0110***
	(0.00175)	(0.00176)	(0.00244)	(0.00143)	(0.00278)
M age	0.000439	-0.00202	0.0440***	-0.000327	0.0200**
	(0.00532)	(0.00523)	(0.00989)	(0.00435)	(0.00812)
M age^2	0.0000444	0.0000760	-0.000653***	0.0000209	-0.000174*
	(0.0000648)	(0.0000639)	(0.000136)	(0.0000553)	(0.000101)
M grade	0.0467***	0.0928***	0.0702***	0.0797***	0.0482***
	(0.00716)	(0.00645)	(0.00628)	(0.00449)	(0.00841)
M grade^2	-0.000933	-0.00348***	-0.00191***	-0.00251***	-0.000315
	(0.000656)	(0.000635)	(0.000628)	(0.000446)	(0.000822)
M read	0.235***				
	(0.0159)				
2007				-0.00260	0.00905
				(0.00522)	(0.00627)
Constant	-1.173***	-1.122***	-1.815***	-1.114***	-1.595***
	(0.110)	(0.108)	(0.177)	(0.0855)	(0.161)
R^2	0.279	0.266	0.247	0.255	0.205
Villages	15206[b]	15616	14895	30511	30511

[a] Whether the mother could also read was included in these estimates, but not in the subsequent regressions, because it was not available.
[b] The N is slightly smaller because missing values of 'mother can read' were ignored in calculating the proportion of mothers that could read within the village, resulting in some villages for whom this value was missing and hence not included in this regression.
Standard errors in parentheses. *** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.

Table 12

Estimates of the Effect of Unobservables Using 2006 Data with Village Fixed Effects

	2006 Village FE
Female	-0.0245***
	(0.00218)
Age 7	0.366***
	(0.00501)
Age 8	0.726***
	(0.00494)
Age 9	1.065***
	(0.00577)
Age 10	1.341***
	(0.00551)
Age 11	1.574***
	(0.00586)
Age 12	1.705***
	(0.00561)
Age 13	1.836***
	(0.00576)
Age 14	1.919***
	(0.00583)
Private	0.185***
	(0.00442)
HH size	-0.00112**
	(0.000527)
M age	0.00516***
	(0.000925)
M age^2	-0.0000331***
	(0.0000110)
M grade	0.00842***
	(0.00155)
M grade^2	0.000973***
	(0.000119)
M read	0.0570***
	(0.00569)
Constant	-1.399***
	(0.0191)
R^2	
Observations	433972
Bias	0.0341738
Implied Ratio	5.289288

Standard errors in parentheses. *** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.