The Private School Premium:
Size and sources of the private school advantage in test scores in India

Abhijeet Singh*
University of Oxford

December 1, 2012

Abstract

Students in private schools have been shown to outperform children in government schools in India by several studies; however, these studies have been constrained by their use of only cross-sectional data, a lack of extensive information about school and household environments, and unavailability of detailed test data. In this paper, I use a unique longitudinal dataset collected by the Young Lives Project in Andhra Pradesh State between 2002 and 2011, which combines detailed school and household level information, to show that in rural areas the gap between the achievement of children in private and government schools (the private school premium) is present in English and receptive vocabulary (but not Mathematics or Telugu) in children between the age of 8-10 years and in math, receptive vocabulary and a Cloze test of Telugu competence for 15 year old children. I do not find evidence of better absolute performance by children in private schools in urban areas once background characteristics and previous test scores are accounted for. Results are robust to accounting for parental aspirations for children's education and their lagged assessments of the child's academic performance. Results for 9-10 years old children are very similar in incidence to emerging experimental evidence on a comparably aged cohort.

Decomposition of learning productivity across the two sectors reveals significant impacts of teacher absenteeism, teacher support (as reported by children), children's subjective assessments of school experience and children's non-cognitive skills (self-efficacy and locus of control) on test scores. Children in private schools also report much more positive assessments of their school experience.

*Preliminary draft. Please do not cite or circulate. Comments are welcome at abhijeet.singh@economics.ox.ac.uk

1 Introduction

The role of the private sector in school education has increasingly come into sharp focus in India. The share of private schools in total enrolment has risen substantially across both urban and rural areas in the past 15 years (Kingdon, 2007); students in these schools perform substantially better in test scores according to several studies (e.g. Pratham, 2006, 2007; Muralidharan and Kremer, 2009); and frequently it seems that private schools achieve this better performance even with much lower expenditure per pupil than government schools. This has led to calls by some scholars to look at the private education sector as a viable means of delivering quality education, even to children from the poorest households in society(Tooley and Dixon, 2003, 2007).

Two research questions are central to understanding the implications of these developments in the Indian educational sector and in guiding any policy response: is the better performance of the children in private schools attributable to the schools or is it merely a reflection of selection on household and child characteristics?; and secondly, what are the sources of greater effectiveness in private schools and which, if any, may it be possible to implement also in government schools?

The central issue in answering the first question is the familiar problem of selection into private schools: children in these schools are likely to systematically differ from children in government schools in socio-economic status, in the concern that parents may have for their education, and in the level of additional investment they receive at home[1]. Existing studies have tried to address these concerns as best as possible by controlling for available covariates and by restricting comparisons to children in the same village or the same household who go to different types of schools. However, all of these studies remain constrained by only having access to cross-sectional data on a limited range of range of controls, typically only either at the school level or the household level; together, these data limitations ensure that credible identification of any private school effect on test scores has remained elusive. For the same reason, it has not been possible to unpack the sources of productivity in different school types. Comprehensive attempts at fitting education production functions require extensive data at both school and household level and, preferably, with information on the same children over time; such data has not been available thus far.

In this paper, I use a unique longitudinal dataset collected by the Young Lives Project in the state of Andhra Pradesh which tracks two cohorts of children (born in 1994/95 and 2001/2) through household visits in 2002, 2007 and 2010 and

[1] These concerns are similar to those encountered in the evaluation of selective schools (e.g. Catholic schools, Charter schools or Grammar schools) in OECD countries.

school visits in 2011 and attempt to better identify a private school effect and to disentangle the sources of different productivity across sectors. Specifically, this paper aims to extend the literature on private schooling in India in three respects: using a value-added approach to modelling achievement production, I attempt to provide more convincingly causal estimates of the effect of private schooling in India; I attempt to show if and how this gap in test scores differs at different stages of the educational trajectory of children, across urban and rural areas, and across different cognitive domains; and finally, I attempt to unpack the sources of differing productivity across the two sectors.

In rural areas, where private schools account for about a quarter of the total enrolment in our data, I find that children in private schools do better than children in government schools in English and receptive vocabulary between the ages of 8 and 10 years and no worse in Telugu (the local language) and Mathematics even after controlling for a wide range of child and household characteristics and past performance of the child; at the age of 15 years, they significantly outperform government school children in Mathematics, receptive vocabulary and Cloze tests in Telugu. Differences in productivity across sectors are not explained by differences in school level infrastructure, class sizes, gender composition of the class, teacher qualifications, teacher knowledge or experience: indeed controlling for these increases the estimated size of the gap as government schools in our sample are, on average, better placed in these dimensions. Socio-economic status and household background variables explain a substantial portion of the variation, as does the greater time spent by private school students at school and studying after school. Teacher absenteeism, which is more prevalent in government schools, and a lower degree of teacher support have a substantial and significant negative impact. I also show uniquely that private and public school children differ not just in their cognitive skills but also in their non-cognitive skills and present suggestive evidence that these differences serve to exacerbate the gap in test scores.

In urban areas, where private schools are the dominant education providers and account for between two-thirds and four-fifths of the enrolment in our data, even though there is frequently a positive difference in the test scores of children in private and government schools, this premium invariably disappears upon the inclusion of the rich set of controls and past performance of the child; I do not find any evidence of a causal private school premium in urban areas.

Results in this study are complementary to emerging results from a school voucher experiment implemented independently by Muralidharan, Kremer and Sundararaman (2012, MKS hereafter). The MKS study offered school vouchers through random assignment to children in the last year of preschool (kindergarten) and Grade 1 for the entire duration of primary schooling up till Grade 5 which could be used to attend any private school in the village; this allows for clean identification of the magnitude of any private school effect. Results on the study available thus far indicate that children in private schools perform better in English and Hindi and no worse in Mathematics and Telugu even though up to 40% less instruction time is dedicated to these subjects in private schools than in government schools.

The MKS experiment is of direct relevance to our paper not only because of the fundamental similarity of the research question but also because the two projects have considerable geographical and temporal overlap; the MKS experiment was implemented between 2008 and 2012 across 180 villages in the same state as the Young Lives Project. The MKS study offers therefore an ideal comparison for the robustness of our results.

On all test dimensions comparable between the two studies, for children of the same age in rural areas, our results from a panel-based specification of a detailed education production function agree closely with the experimental estimates obtained by MKS. These results add to a recent emerging literature from the US which documents that value-added models (VAMs) of education production functions, which control flexibly for past performance, are indistinguishable from experimental estimates through school choice lotteries (Kane and Staiger, 2008; Deming et al., 2011) and from quasi-experimental estimates (Chetty et al., 2011) based on exogenous changes in teaching staff.

Given that some of the key concerns in the specification of VAMs centre around selection on unobservables and sorting across different schools or classrooms, which may reasonably be expected to significantly differ across different educational and labour markets, the robust performance of VAMs in the US do not necessarily provide evidence that such robustness would also be found in developing country contexts such as India; evidence of the robust performance of VAMs in a rural Indian setting thus makes a valuable contribution to ongoing methodological debates about the reliability of these observational, panel-based estimates of education production. This is the first study, to my knowledge, that can make such comparisons between experimental and panel-based non-experimental estimates in a developing country setting using two large independent samples[2]. In this respect, the paper is complementary to a recent paper by Andrabi et al. (2011)

[2]Not only does the close correspondence between the results in the two studies emphasize the robustness of estimating education production functions using VAMs, it also highlights the importance of careful selection of the samples on which experimental studies are conducted. Results from this paper, using an independently drawn sample, confirm that experiments using representative samples (such as the MKS study) may possess a high degree of external validity, beyond the undeniable strength of the experimental approach in establishing causality in the selected sample; this addresses some of the most potent criticism of experimental evidence (Heckman and Smith, 199x; Deaton 2007).

who document that estimates of private school effects on test scores using VAMs are identical to estimates using dynamic panel estimators.

Apart from demonstrating the robustness of VAMs, and the findings on the comparable set of children and indicators in both studies, this paper supplements the emerging evidence base on the private school effect from the MKS study in three dimensions: compared to the MKS study, it extends the comparison of the performance of public and private school students in learning also to urban areas; it extends the comparison also to older students in post-primary education by utilizing data on the older cohort of children; and it compares students not just on cognitive measures directly targeted by the schools (scores on Math, English,etc.) but also on measures of receptive vocabulary which are not explicitly focused on by schools but should nonetheless be responsive to school instruction and may partially answer criticisms that private schools 'teach to the test'.

Moreover, as Todd and Wolpin (2003) emphasize, experimental estimates of treatment effects and production function parameters are valuable in different respects: the former provide an estimate of the total policy effect (the total derivative) that subsumes responses to policy interventions made by parents and schools, while production function parameters provide ceteris paribus (partial derivative) effects. These parameters answer different questions and are each valuable separately, even if they do not agree (although they do in this case). For example, recent work on India and Zambia (Das et al., 2011) clearly shows that while the marginal impact of block grants to schools is positive (as would be identified by a production function parameter), the total positive effect after two years is zero because households offset anticipated increases in school inputs by a corresponding reduction in private expenditure on educational inputs. Thus the estimation of educational production functions remains important even with the availability of experimental estimates of the total policy effects of educational interventions.

In the context of this paper, estimating production function parameters allows a decomposition of the learning gains across successive time periods into its contributory sources i.e. it allows for an investigation of the relative contributions of the different educational inputs (both home-based and school-based) on learning. This is important for at least two reasons. Firstly, an assessment of the relative contribution of home inputs and school inputs into the low levels of student achievement is central to assessing how effective school-based or home-based interventions may be in raising achievement[3] or reducing inequalities in skill formation[4]. Secondly, private and government schools differ across a range of characteristics, not all of which may be easily transposable from one sector to the other given the differing institutional structures of the two sectors - for example, changing incentives of teachers in government schools is likely to be considerably more difficult than investment in infrastructure and school inputs; estimating separately the relative contribution of these different factors might allow us to identify which, if any, features of school processes or inputs in the private sector may also be possible to implement in government schools.

Results from the decomposition of the learning gains presented in the paper offer only limited answers to the second question posed above: which features of the organization of instruction in private schools may be practical for application in government schools in order to improve learning outcomes? Teacher incentives and effort or differing patterns of time use by children[5], which are a major source of better absolute performance in private schools, are not easy to manipulate in the government sector. Finally, I incorporate in the education production functions data on several subjective responses by children which proxy psychosocial variables such as locus of control (agency) and self-efficacy, which are prominent in the psychology literature as being important determinants of school performance, as well as their subjective responses on educational aspirations, school engagement, teacher support and peer support. I show that these measures are informative - they display variation and are predictive of test scores, even when controlling for the full range of controls available from the school and household level data collection including past performance of the child. This is important in relating to recent literature on the effect that non-cognitive skills may have on the production of learning outcomes(Cunha et al., 2010; Cunha and Heckman, 2008), in controlling for possible sources of bias (since private and government school students often differ on these measures in our data), and in highlighting gains from possibly targeting similar information in further data about children's education.

The rest of the paper is organized as follows: Section 2 provides a brief literature review and an overview of the education sector in India; Section 3 introduces the data; Section 4 presents the empirical strategy and the main results from VAMs

[3]The design and evaluation of possible interventions whether based at school or home is, of course, non-trivial and depends not only on the proportion of variance explained by inputs from these factors but also on the availability of policy levers to affect investment processes and the practicality and cost-effectiveness of different interventions.

[4]This is important because inequality in test scores in India is among the highest in the world. Using internationally comparable data on mathematics tests from two Indian states, Das and Zajonc (2010) note that the distribution of test scores in the sample is second in inequality only to South Africa with its particular history of institutionalized discrimination.

[5]Children in private schools spend more time in school and more time studying after school both of which have large effects on their test scores.

estimating the private school effect in rural and urban areas across different domain of learning and at different ages; Section 4 investigates sources of learning gains; and Section 5 presents a discussion of the results and concludes.

2 Private and Government Schools in India

As noted previously, the share of the private sector in total enrolment especially at the primary level has expanded very rapidly and a large literature finds significant difference in the test scores of children in these schools when compared to state school students. It has also been shown that government school teachers, although better-paid and more qualified than privat teachers, are also much more likely to be absent.

Merely noting that children in Indian private schools (or indeed Catholic schools or Charter schools in the US) do better on average in tests than children in government schools is clearly insufficient evidence of the greater effectiveness of private schools: children in these schools are likely to systematically differ from children in government schools in socio-economic status, in the concern that their parents may have for their education, and in the level of additional investment they receive at home which may affect learning. In order for any lessons to be drawn from results on a gap between the average test scores of children in private schools and government schools, it needs to be demonstrated that this gap is attributable to schools themselves and not merely a reflection of their confounding socioeconomic characteristics.

Studies in the Indian context have not been unmindful of these concerns and have adopted a series of econometric techniques to correct for this source of bias: by controlling for observed background characteristics of children (Muralidharan and Kremer, 2009; Kingdon, 1996; French and Kingdon, 2010; Desai et al., 2008); by running models with village fixed effects to isolate village level confounders; through household fixed effects (e.g. French and Kingdon, 2010); through propensity score matching (Chudgar and Quin, ming); and finally, through the use of Heckman selection models (Kingdon, 1996; Desai et al., 2008). The results in most cases seem to indicate that there is, in fact, a 'private school premium' in test scores which persists even when issues of selection have been dealt with as far as possible.

However, all of the studies cited above use only cross-sectional variation in test scores and a limited range of characteristics of children, schools and households to arrive at their estimates of the private school premium. Their identification strategies, while perhaps the best that can be achieved given the data, also are still vulnerable to several sources of endogeneity: Ordinary Least Squares (OLS) regressions controlling for background characteristics cross-sectionally are unlikely to have observed all relevant dimensions in which these children differ; within-village comparisons are more convincing but neglect the potential bias caused due to unobserved characteristics that lead to households in the same village making different choices regarding the enrolment of their children; household fixed effects control for any characteristics that are constant across children in the same household and identify the private school effect across siblings who are enrolled in different school types at the same point in time – however, few households send children to different school types and when they do, it is quite likely that this differential enrolment is related to either unobserved ability differences across children or, even more plausibly, to other unobserved differences in investment across children, for example in parental attention or extra tuitions; propensity score matching methods are also constrained, similar to OLS regressions, by their reliance on a 'selection on observables' assumption which is unlikely to be maintainable given a limited set of conditioning variables; finally, variables used to control for selection in these studies using Heckman selection-correction estimators are unlikely to satisfy necessary exclusion restrictions[6].

Furthermore, existing studies on the private school premium have mostly focused on whether the premium exists and is causal but have not been able to investigate rigorously the sources of the premium; however, even if this gap is in fact causal, we need to know the sources of the private school advantage to draw any implications for recommended practice in government schools. Finally, while it is quite likely that any private school premium differs across urban and rural areas, across different stages of schooling and across different cognitive domains, existing studies have not been able to extensively study this heterogeneity in the premium[7].

[6]For example, Desai et al. (2008) use the presence of a private school in a village as a factor predicting selection into private schools but not test scores; this exclusion restriction is almost certainly untenable as villages which do have a private school will differ from villages that do not. In fact such a pattern has clearly been documented by Pal (2010) using the PROBE dataset covering five Indian states. Kingdon (1996) excludes variables such as mother's education and its square which are used to predict selection into private schools but not achievement thereafter: however, it seems quite plausible that more educated mothers wouldn't just influence school choice but also could provide greater support for learning (e.g. by supervising homework) which would directly affect learning scores.

[7]Most studies in the literature are focused either exclusively on urban areas(Kingdon, 1996; Tooley and Dixon, 2003, 2007) or on rural areas(French and Kingdon, 2010; Muralidharan and Kremer, 2009) due to the nature of the data available. The only exceptions to this are studies based on the India Human Development Survey (Chudgar and Quin, ming; Desai et al., 2008) which does cover both urban and rural areas. Further, they are often constrained by access to only very basic tests of reading, writing and numeracy which only allow for an assessment of whether, for example, a child can read a simple sentence and perform a division task but do not capture the full distribution of cognitive skills; this is true of both the ASER data and the India Human Development Survey which underpin most of the research in this area.

3 Data

3.1 Sampling

The data I use in this study were collected by the Young Lives Project[8] between 2002 and 2010 in the state of Andhra Pradesh. Andhra Pradesh is the fourth-largest state in India by area and had a population of over 84 million in 2011. It is divided into three regions – Coastal Andhra, Rayalaseema and Telangana – with distinct regional patterns in environment, soil and livelihood patterns. Administratively the state is divided into districts, which are further sub-divided into sub-districts (mandals) which are the sentinel sites within our sample[9].

The Young Lives study in Andhra Pradesh has collected data on two cohorts of children: 1008 children born between January 1994 and June 1995, and 2011 children born between January 2001 and June 2002. Data was collected from children and their families using household visits in 2002, 2007 and 2010. The study also collected extensive data through visits to the schools of a randomly-selected subsample of the younger cohort in 2011. Figure 1 presents graphically the timings of data collection, the age of the children at the time of the data collection[10]. Attrition rates in the data have been kept very low – 1930 children (96%) in the younger cohort and 976 children (97%) in the older cohort are still in the sample in 2009. This has been achieved in part by following children whose households migrated from their original communities to their destination of migration.

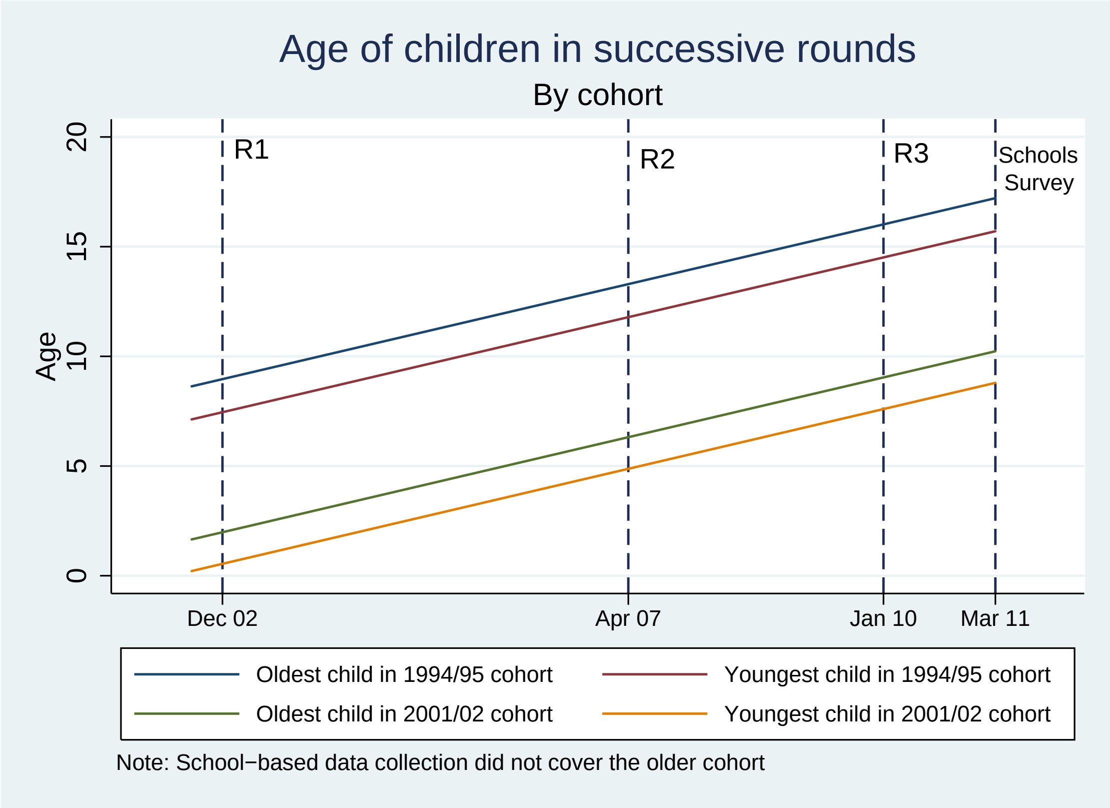
3.2 Data collected through household visits

The data have complete schooling histories of the 3000 index children being followed by the Project which were collected retrospectively in 2009. Extensive test data were collected from index children in all rounds of the survey. The

[8] Young Lives is a longitudinal study of child poverty which follows two cohorts of children in four countries: Ethiopia, Andhra Pradesh state (India), Peru and Vietnam. For details, please visit www.younglives.org.uk

[9] The Young Lives sample is distributed across the three main regions and covers about 100 communities (villages or urban wards) across 20 sentinel sites. The sentinel sites were chosen purposively on a well-defined set of socio-economic criteria to ensure that the sample captured the diverse conditions in different parts of the state; sentinel sites range in population between 30,000 and 240,000 people with the exception of one sentinel site in Hyderabad city which is much larger. Selection of communities within the sentinel sites and children within the community was random. A careful comparison with the DHS 1998/99 sample for Andhra Pradesh shows that the data in the Young Lives sample do contain the type of variation that is commonly found in larger representative surveys: a detailed explanation of the sampling methodology and the comparison of the characteristics of the Young Lives sample with the DHS sample on a range of observed characteristics is reported in Kumra (2008).

[10] The interviews were usually carried out over a period of four to six months for the bulk of the sample. The timing of interviews given in Figure 1 correspond to the end-period for the majority of the interviews which did not involve tracking children to different communities.



tests were designed collaboratively by experts from several disciplines including education, economics, child psychology and sociology. The tests differed in their focus on which dimension of cognitive achievement they attempted to capture and how closely they relate to the formal school curriculum in Andhra Pradesh; often, different tests were administered to children across rounds in order to ensure that they were appropriate for the age and the stage of education that the children were in. Box 1 lists the different test measures available in the data; details of each test are explained in Appendix 1[11]

[11]All test items used were adapted for use in local languages and validated separately in the study areas, including Andhra Pradesh: details of the validation are available in Cueto et. al.(2008), along with detailed descriptions of the tests which have been abridged for presentation here. For precise details of the contents of the test, please see the Young Lives questionnaires which are publicly available at www.younglives.org.uk.

There are some concerns about the adaptation and administration of the PPVT test in the 2009/10 round. The test was designed in English and incorporated items of sufficient difficulty levels to be administered to learners of all ages. However, in the process of translation into local languages, especially in the case of the Telugu translation, it appears that the difficulty levels of the highest items was severely reduced. This may have contributed to a ceiling effect in the PPVT scores in Round 3 as children who had scored very highly in 2006/7 do not seem to have progressed much. I have reported the results here as it is one of the only cognitive tests that are entirely comparable across rounds.

Box 1. Cognitive Tests in Young Lives

COHORT	ROUND 1 (2002)	ROUND 2 (2007)	ROUND 3 (2010)	SCHOOL SURVEY (2011)
Older Cohort	**8 years**	**12 years**	**15 years**	
	Raven's Test Reading Assessment Writing Assessment Numeracy	PPVT Math Achievement Reading Assessment Writing Assessment	PPVT Math Achievement Cloze test	
Younger Cohort	**6-24 months**	**5 years**	**8 years**	**9 years**
		PPVT CDA Quantitative	PPVT Writing Assessment Early Grade Reading Math Achievement	Mathematics Telugu English

Scores on the PPVT, the CDA Quantitative test, the mathematics tests in 2007, 2009/10 and 2011, and the Cloze test in 2010 were generated using Item Response Theory (IRT) models. The use of IRT models is standard in the education assessments and presents significant advantages: it allows for the accounting of difficulty of different items, for the detection and removal of test items that did not perform well in the field and, where the same test (or a subset) was administered over time, it allows for the computation of scores from the repeated tests on the same scale with cardinal meaning[12]. Scores were computed in Stata using maximum likelihood. Tests in which the same items were administered (PPVT in both cohorts in Rounds 2 and 3, and the maths test in the younger cohort in Round 3 and the school survey) were calibrated together which allows them to be put on the same scale[13]. I have normalized the test scores to have a mean of 0 and a standard deviation of 1[14].

[12]IRT models posit a relationship between a unidimensional latent ability parameter and the probability of answering a question correctly; it is assumed that the relationship is specific to the item but is constant across individuals. Further assuming local independence between answers to different items by the same person, and across persons for the same item, it is possible to recover estimates of ability based on standard maximum likelihood techniques. The estimation of the item scores in this paper was carried out using the openirt suite of commands in Stata written by Tristan Zajonc. For a detailed explanation of IRT models, please consult Das and Zajonc (2010); Van der Linden and Hambleton (1997).

[13]Math scores for the older cohort in Rounds 2 and 3 cannot be linked to a common scale due to the unavailability of adequate link items administered in both rounds. In contrast, the same items were administered in the PPVT in both rounds and a subset of items from the Round 3 math tests for the younger cohort were repeated in the school survey.

[14]In the case of tests in different rounds which were calibrated together, I have normalized scores to mean of 0 in the first period in which the test is administered by cohort. Test scores for PPVT and Cloze tests are only available for the children who gave the test in Telugu as it is not possible to reliably equate the difficulty of items across languages for the purpose of the IRT estimation of the latent ability.

The tests used in Young Lives are much more comprehensive in the domains of learning they capture and offer more variation than tests in previous studies in the literature; I feel that this is a considerable strength of the dataset in this study.

Data collection in 2002, 2007 and 2010 was at the households of the children. This data has particularly rich information about the socio-economic background of the children's households (for example, their castes, livelihoods, income and wealth, agricultural activities, economic shocks, literacy and information networks), parental expectation/aspirations for the children, and also detailed child-specific data (for example, anthropometric information, details on schooling, attitudes towards schooling, 'non-cognitive' psychosocial measures such as agency and educational and employment aspirations).

In the interest of clarity, I will explain individual variables being used in the estimation as part of the different empirical sections at the point they are actually being employed.

3.3 Data collected from schools

In 2011, the Young Lives project visited a random sub-set of 247 schools being attended by children in the younger cohort[15]. The schools were selected based on stratified random sampling with the objective that adequate variation was retained across urban and rural areas while keeping the exercise logistically and financially viable. The sampling frame consisted of all the Younger Cohort (YC) children who were still enrolled in school in Round 3 (2009) and were going to school within Andhra Pradesh[16].

The sampling was carried out within strata defined on whether the school was in an urban or a rural area, whether it was private or public and whether it

[15] It was not possible to visit all schools due to budgetary and logistical constraints. In total, 807 different schools were being attended by children in this cohort in 2009, 538 of those attended by only one Young lives child; logistical constraints and funding meant that we could at best survey 250-300 schools.

[16]

YC children outside AP were excluded from the frame as tracking them was going to be logistically unfeasible and because all questionnaires, tests and procedures were designed keeping the AP education system in context; this left 1880 children in the sampling frame.

was recognized or unrecognized, yielding a total of six strata[17]. The final sample includes 952 children across 249 schools.

The school-level survey was conducted between December 2010 and March 2011, i.e. in the school year immediately after the 2010 school year when the third round of household-level data collection was carried out. The survey attempted to capture in detail school-level differences in infrastructure and funding, in teacher qualifications and characteristics, in classroom characteristics, in teaching processes and in children's experiences of schooling. It administered questionnaires to all school principals (headmasters/headmistresses), to all Young Lives index children in the school and to the math teachers of the index children covered in the survey. Additionally, enumerators observed a math class for each of the index children keeping detailed records of what the teacher and each index child was doing at various points of time during the class; they also looked the notebooks of each Young Lives child to note the extent of work which had been seen/marked by the teacher and in what detail.

Finally, four tests were administered as part of the school survey: each child completed a test in mathematics and both Telugu and English (if these languages were taught as part of the curriculum in the school); mathematics teachers of the Young Lives children were also administered a test of competency in teaching mathematics.

[17] The R3 (2010) HH survey data itself did not have details about whether the school was recognized or unrecognized. To construct the above categories and then sample within them, the lack of a school census code (DISE code) was taken as a proxy for unrecognized schools. In each stratum, a pre-determined number of children were drawn randomly and all other Young Lives YC children in the school were covered as well: this structure of the sampling was administered because the marginal effort of surveying additional Young Lives children in schools which are being surveyed anyway is low and as importantly, within-school variation (which this maximizes) is essential for several analytical purposes. The initial sample covered 1111 children in 299 schools which were well distributed geographically and (by design) across the different school types. Where the child(ren) enrolled in a particular school had shifted schools since 2009, they were dropped from the school-based survey and were not followed to their new school unless this school was also already in the sample. The different number of initial draws per stratum in the sample, combined with the higher probability of being in the sample if a larger number of Young Lives children were in the same school, means that different children in the sampling frame had different initial probabilities of selection. Given the systematic nature of the sampling, it is possible to calculate these probabilities of selection a priori and assign sampling weights to each child. Deaton (1997) discusses whether sampling weights need necessarily be employed in regression analysis and concludes that, for the purpose of causal analysis, running weighted regressions is not usually necessary. In all of the analysis in this section, I do not employ sampling weights and all regressions are unweighted.

4 Size of the Private School Premium

4.1 Empirical Framework

Following Todd and Wolpin (2003, 2007), it is possible to write the achievement production function in a general form:

$$y^*_{ist} = F[X_i(t), S_i(t), \mu_{is0}, \epsilon_{ist}] \tag{1}$$

where the achievement (y^*_{ist}) of child i in school s at time t is expressed as a function of the whole history of home-based inputs $X_i(t)$, school-based inputs $S_i(t)$, student endowments μ_{is0} (such as ability), and a time-varying error term ϵ_{ist}. While useful for conceptualizing the production technology for achievement, direct estimation of Eq (1) is not typically possible because the whole history of home and school inputs, as well as individual-specific endowments, are not observed by the researcher.

Following the initial specification provided by Andrabi et al. (2011), which imposes some restricions on the function in Eq(1), I model the education production function as follows:

$$y^*_{it} = \alpha^{'}_1.x_it + \alpha^{'}_2.x_{i,t-1} + ... + \alpha^{'}_t.x_{i1} + \sum_{s=1}^{s=t} \theta_{t+1-s}\mu_{is} \tag{2}$$

where x_{it} is a vector of inputs for child i at time t, y^*_{it} is true achievement at time t measured without error, and summed μ_{is} are cumulative productivity shocks. In the absence of longitudinal data, some studies choose to estimate the contemporaneous specification of the education production function which only relates current achievement to current characteristics:

$$y_i = \alpha^{'}_1.x_{it} + \mu_{it} \tag{3}$$

This specification however relies on several strong assumptions: either current learning needs to be entirely a product of current characteristics or the characteristics producing learning have been the same at all points of time for all children; additionally, observed inputs need to be entirely uncorrelated to any unobserved ability. Instead, adding and subtracting $\beta y^*_{i,t-1}$to equation (1) and assuming that coefficients decline geometrically[18] yields the lagged value-added model:

$$y^*_{it} = \alpha^{'}_1.x_{it} + \beta y^*_{i,t-1} + \mu_{it} \tag{4}$$

[18]If data on past inputs is available, then it is possible to relax this assumption by including them directly in the specification. This yields the ‘value-added plus’ model estimated in Todd and Wolpin (2007).

The lagged test score in the above specification measure is expected to capture the contribution of all previous inputs and any past unobservable endowments and shocks. This specification is believed to be a significant improvement over the contemporaneous specification. The estimates are still possibly subject to bias from two sources – measurement error in the lagged achievement measure and any unobserved heterogeneity affecting learning between children, whether or not correlated with the inputs in the vector x_{it}.

Andrabi et al. (2011) document, while analyzing the effectiveness of private schools in Pakistan (a setting very similar to the one in this paper), that biases from measurement error and unobserved heterogeneity are countervailing and aggregate bias on the private school coefficient does not seem to be significant in practice; in fact, they also show that merely correcting for the bias due to measurement error is likely to make the aggregate bias worse and, particularly in the private schooling analysis, severely bias coefficients downwards[19]. They correct for the twin sources of bias through the use of dynamic panel methods (e.g. Arellano and Bond, 1991) where they estimate a restricted value added specification after differencing it and then use the scores in other subject as the instrument[20]. In this paper, I will largely be adopting the lagged value-added specification (Eq. 4) to obtain estimates of the public school premium.

Using a value-added modelling approach, Deming et al. (2011) compare the effects of a school choice lottery in the US and find no significant differences between experimental estimates of school effects based on the school lottery and estimates from a value-added model that controls for previous test scores; Kane and Staiger (2008), analyzing results from a different experiment in Los Angeles that assigned children randomly across classrooms, similarly report that teacher effect estimates that controlled for prior student test scores yielded unbiased predictions

[19] Specifically, they report from their application in Pakistan: "Despite ignoring measurement error and unobserved heterogeneity, the lagged value-added model estimated by OLS gives similar results for the private school effect as our more data intensive dynamic panel methods, although persistence remains overstated. The relative success of the lagged value-added model can be explained by the countervailing heterogeneity and measurement error biases on β (their persistence parameter) and because lagged achievement can also act as a partial proxy for omitted heterogeneity in learning."

[20] The application of these methods require two things: that there are at least three measures over time and that, in the case of evaluating a dummy variable such as enrolment in public school, there should be some movement across categories between every round. While I would have liked to attempt addressing the two biases similarly, the data available do not enable me to do so even with multiple rounds of data. The older cohort did not have very comparable tests across the three rounds; in particular, in the 2002 round of the study, only a basic reading and writing test and a simple numerical calculation were asked and as a result I only have two rounds of strictly comparable test data (from the 2007 and the 2010 rounds). Similarly, in the younger cohort, no test was administered in all three rounds of test data collection (from 2007, 2010 and 2011). As a result, in neither cohort can I use dynamic panel estimators to simultaneously correct for these problems.

of test scores after randomization; finally, Chetty et al. (2011) find no evidence of bias when comparing estimates of teacher effectiveness using a value-added approach to estimates using previously unobserved parent characteristics and a quasi-experimental research design based on changes in teaching staff[21]. Specifications in these recent papers differ from (Eq. 4) and use a more general form of the lagged value-added model where they use a control function based on the lagged score (including the level of the lagged score but also its square and cube); I will also show the robustness of results to the use of the third-degree polynomial of lagged test scores instead of only using the level[22].

4.2 Results on 8-year old children

In this sub-section, I present the estimates of the effects of public and private schools on learning achievement for the younger cohort, born in 2001/02, using scores from 2010 as the outcome variables.[23].

Children in this cohort were, on average, aged about 8 years at the time of the survey in 2010.At this point in their lives, nearly all children in the sample are enrolled in school: of the 1929 children in the sample, only 16 children are reported as being out of school. In the analysis below, I drop the non-enrolled children. Table 1 presents some descriptive statistics about the sample in this cohort, separately for rural and urban areas. In rural areas, 27% of children in the sample are enrolled in private schools; in urban areas, over four-fifths of the

[21]A note of caution is sounded by Rothstein (2010) who documents that there may be a possibility of bias due to unobserved heterogeneity. However he does document that the lagged value-added model performs considerably better than cross-sectional estimates or a gain-score model (similar to results in Andrabi et al. (2011)) and that using multiple scores from previous years, the evidence of remaining bias is low. Based on Rothstein's (2009, 2010) recommendations, I also estimated specifications which include multiple lagged scores; the results do not change substantially in sign or significance; they are sometimes a little smaller in magnitude but not significantly so. These results are not incorporated in the current draft but are available on request. In the section on robustness of the main results, I engage directly with Rothstein's key concern - that achievement measured through test scores may still exclude much information that is available to relevant decision-makers (headmasters in his case) which could be used to sort students; specifically, I show that controlling for the parent's lagged assessment of the child's academic performance, or parental aspirations about the child's educational levels, do not change the results on the effect (or lack thereof) of private school enrolment on test scores.

[22]In a recent paper, Angrist et al. (2011) also show how their estimates of Charter school estimates are identical when estimated on the same sample of children using lottery outcomes and separately using observational data (including baseline scores). In their estimation, they also take great care to match the sample on a range of common support on a limited combination of characteristics. While I have not attempted to similarly match observations here, it may be possible to do this with the data available.

[23]I have not used scores from 2007 as outcome variables in this paper. My choice in this regard is guided by the fact that tests in Rounds 2 (2007) and 3 (2009) are richer in the variation they offer and much more comparable to each other than tests administered in Round 1 (2002). In the specific case of the younger cohort, children in my sample were aged about 5 years and less than half the sample had started formal schooling in 2007; the estimation would thus have confounded the causes of early enrolment (which differs by school type) and the effect of the school types themselves.

children in the sample are. In both rural and urban areas, there are significant differences in the observable characteristics of children in government and private schools: children in private schools are likely to be from richer households with more educated parents and are much more likely to be male and the first-born child. Given the large differences in the share of private and public schools in enrolment, the differing socio-economic composition of the student body and the probable differences in the institutional arrangements of these schools in urban and rural areas, I separate the analysis for urban and rural communities throughout the paper[24].

In this cohort, as in the older cohort, we have the math and the PPVT tests as outcomes [25]. The PPVT is the only test administered in both the 2007 and the 2010 rounds; the scores are expressed on the same cardinal metric, having normalized the score in 2007 to have mean 0 and standard deviation 1. For the math test, I use the score on the CDA quantity subscale score as the lagged achievement measure.

The specifications estimated are as follows:

$$
\begin{aligned}
Y_{it} &= \alpha + \beta_1.Private_{it} + \beta_2.site_i + \epsilon_{it} && (5)\\
&\quad +\beta_3.X_{it} && (6)\\
&\quad +\beta_4.Y_{i,t-1} && (7)\\
&\quad +\beta_5.schooltime_{it} && (8)
\end{aligned}
$$

where $Private_{it}$ is an indicator variable equaling 1 if the child is enrolled in a private school in 2009/10 with enrolment in a government school is the base category. $site_i$ is a vector of sentinel site (mandal) fixed effects. X is a vector of background characteristics that includes standard information about the socio-economic background of the child (caste and wealth index), maternal and paternal years of schooling, the sex of the child and whether he/she is the eldest child in the household and the number of hours spent on a typical day in various activities;

[24]Estimating all results separately for rural and urban areas also helps me benchmark my results from rural areas to the MKS study.

[25]Additionally, we have scores from the Early Grade Reading Assessement but I cannot use them for the purpose of this analysis. The test administration in the survey allowed children to choose the language they wanted to take the test in - about a quarter took the test in English while the rest took it in Telugu. The difficulty of items cannot be directly compared across languages and therefore it is not possible to put the test scores of all children on the same metric; this is a serious issue for my analysis because the choice of test language by the child is directly correlated with the type of schooll he/she attends. Given the large proportion of children choosing to take the test in English, I cannot restrict the scores to only the Telugu test-takers as in the case of the PPVT and the Cloze tests (where non-Telugu test takers accounted for less than 10% of all observations).

specifically, I control for the time use on caring for others, domestic tasks, studying outside of school time (including extra tuition), tasks on the family farm or other family business and paid work outside of the household. $Y_{i,t-1}$ is the lagged test score. $schooltime_{it}$ is the time spent at school on a typical day[26]. In all regressions in this paper, I cluster standard errors at the sentinel site level[27].

Results from this exercise are given in Table 2 for rural areas and Table 3 for urban areas. As can be seen in Cols. 1 and 5, there is a substantial cross-sectional private school premium in test scores with only mandal fixed effects of about 0.35 SD in mathematics and 0.42 SD in PPVT. Controlling for the socio-economic background of the child and the time use outside of school (with time spent sleeping or in play/leisure being the omitted category)reduces the premium significantly in Cols. 2 and 6; the effect of private schools is essentially halved for PPVT at 0.22 SD and is insignificant for mathematics although still positive at 0.1 SD. Time use patterns outside school are predictive: an extra hour studying after school raises both math and PPVT scores by about 0.1 SD while an extra hour working on the family farm reduces math scores by a third of a standard deviation. Controlling for the lagged test scores makes the coefficients on the private school dummy even smaller, although not by much (Cols. 3 and 7). Finally, controlling for the time spent in school essentially drives the coefficient to zero for mathematics, where school time has a strong impact (with an hour of extra school time translating into a 0.12 SD increase in test scores), but not for PPVT where time spent in school does not seem to be associated in any significant way with the test score.

In urban areas, the raw within-mandal private school premium in is large in both math (0.5 SD) and PPVT (0.36 SD). Adding controls for socio-economic background, child characteristics and time use reduces the premium substantially in both indicators and they are no longer statistically significant. Controlling for the lag and for time spent in school reduces the coefficients on the private school dummy further although they remain positive (but not statistically significantly different from zero) at around 0.15 SD. Hours per day spent caring for others is significantly negatively associated with math scores; there is a strong wealth effect;

[26]Hours spent in school on a typical day are a factor within control of school management. As such, even if they explain a portion of any private school effect on test scores, that does not indicate a bias in the preceding specification but merely the channel through which such an effect is being created; this is in contrast to other controls in the estimation and therefore, I include this variable in a separate step.

[27]I control for site-fixed effects and cluster standard errors at the site level in order to be consistent with the empirical approach of the MKS study and also because mandals were the primary sampling units in the survey design; selection of villages within mandals and households within villages was random. Most results are not sensitive to the choice of whether we control for fixed effects at the mandal level or at the village level; the only exception is in the mathematics result for the younger cohort (8-year olds) in 2010 where the current specification shows no impact but controlling for village fixed effects does show a significant positive impact (although zero effect remains within the 95% CI).

and hours spent at school seem to have an effect on math (although this is only significant at the 11% level) but not on PPVT.

4.3 Results on 9-10 year old children using school data

For the younger cohort, I also have access to the school-based data collection in the school year 2010/11 which followed a subset of the children in the sample to their schools. The characteristics of this sample are given in Table 4.

The characteristics of the children in this sample are similar to those in Table 1; this is not surprising given random selection of a subset for school visits. However, in addition to the variables already included in the estimation for the full sample in 2010 (Tables 2 and 3), I also have additional controls available for this subsample: I have much more extensive information about home investments into childen's studies (collected as part of a battery of questions in the child questionnaire of the school visit); and I can use lagged measures which proxy psychosocial variables such as aspirations, motivation and a sense of self-efficacy[28].

As can be seen, children in private schools in both rural and urban areas, but especially so in the latter, report a higher level of support for learning (especially through homework support) at home. Similarly, students in private schools are more likely to report that they 'like to make plans for future study and work' and that 'if [they] try hard, they can improve their situation in life' compared to children in government schools (although the magnitude of the difference is very small for the latter statement).

In this sample, I have three test measures: a math test (which had common items with the test administered in 2010), a test of Telugu competence and a test on English language competence[29]. I estimate the following specifications:

$$\begin{aligned} Y_{it} &= \alpha + \beta_1.Private_{it} + \beta_2.site_i + \epsilon_{it} && (9) \\ &\quad +\beta_3.X_{it} + \beta_4.noncog_{i,t-1} + \beta_5.homesupport_{it} && (10) \\ &\quad +\beta_4.Y_{i,t-1} && (11) \end{aligned}$$

[28] These measures were collected in 2009/10 for the younger cohort but not in 2007 as the children were only 5 years old on average. I have not used responses to these questions as controls in the previous sub-section (Tables 3 and 4) because contemporaneous non-cognitive skills may well be an outcome of the type of schooling. While this possibility cannot entirely be ruled out by using lagged psychosocial measures, the risk of simultaneous determination of non-cognitive and cognitive skills is reduced.

[29] At this point, children in the younger cohort were aged about 9-10 years which is exactly analogous to the age of the children in the MKS study four years after their intervention (offer of scholarships). Furthermore, they test the children on all of these three dimensions (Math, Telugu and English). Therefore results on this sub-sample are the most comparable to their experimental estimates.

$Private_{it}$, $site_i$, X_{it} [30]and $schooltime_{it}$ are defined as previously. $homesupport_{it}$ is an index of academic support at home, as reported by the child[31].$Y_{i,t-1}$ is the lagged achievement measure; for the mathematics test, this measure is a lagged test score which is on the same metric as the school survey score; for Telugu, I use the score from the lagged PPVT test; for English I use the PPVT score from 2010 as well as dummy variables for whether the child could read a simple sentence and write a simple sentence in 2010[32].

As can be seen in Table 5, the coefficient on the private school dummy variable is positive, substantial and strongly significant for mathematics and English (although not for Telugu) when only controling for mandal fixed effects cross-sectionally; the magnitude of the coefficient is very large at about 0.37 SD for math and 0.9 SD for English. Controlling for background characteristics in X_{it}, the home support index and lagged non-cognitive measures reduces the premium sharply towards zero for mathematics (and the coefficient is not statistically significant) and reduces it for English substantially by 0.25 SD. Finally controlling for the lagged measures, the results suggest no impact of private schools on test scores in math and Telugu and a substantial positive impact on English of over 0.5 SD. The signs and statistical significance of these estimates are very similar to results documented by MKS: when comparing four-year impacts of their intervention on children aged 9-10 years by the time of their assessment, they find no impact on math or Telugu abilities with coefficients close to zero impact and a substantial positive impact on English test scores[33].

[30]All controls in X_{it} are from the 2010 round since a separate household questionnaire was not administered at the time of the school visits. Most of these characteristics (like sex, caste and birth order) are time-invariant. Others, such as time use are not expected to have changed very much in the course of a year.

[31]The index is based on the following statements: 'There is no one at home to help me with my school homework', 'If I need help with my school homework I can ask someone at home', 'at least one of my parents or household members knows my rank in class', 'My homework is regularly checked by my parents or other household members' and 'No one at home is able to help me with my studies'. The index is created by recoding all statements to be positive (i.e. implying greater home support), adding up the number of positive responses, averaging by the number of non-missing statements and normalizing to have a mean of zero and a standard deviation of 1

[32]Since an English test was not administered in previous rounds, I do not have a lagged English score. However, to the extent the PPVT score as well as ability to read and write in the previous year als proxy past investments and child ability, the estimates should be robust.

[33]The English test score effect is statistically significant in their sample at an assessment 2.5 years after the intervention but not at the 4-year assessment (with a t-statistic of about 1.6).

MKS document, and results in this paper reaffirm, that private schools seem to be providing higher level of skills in English (and also Hindi in the MKS study which is not assessed here) without any corresponding lower gains in Telugu and mathematics. This indicates that students in private schools learn more on aggregate (across domains) than children in government schools, indicating higher productivity of private schools in terms of learning gains across the school year. MKS also document that private schools spend between 30-40% less instruction time on Telugu and mathematics than government schools indicating that even the null result in Table 5 (and in their study) indicates greater productivity of private schools in terms of learning gains per hour of instruction time.

The time students spend studying outside of school time seems very predictive of test scores: an increase in study time after school by one hour is associated with an increase in the math, Telugu and English language competence by between 0.07 to 0.09 SD. The statement “I like to make plans about future studies and work”, on which there is substantial difference between private and government school students in the sample, seems to be informative; even conditional on a full set of controls, it has a significant effect of about 0.09 SD in math and 0.12 SD in Telugu (although the latter effect is not statistically significant). Finally, the home support index also seems to significantly impact learning in this sample in all test scores.

I do not report on results for urban areas for this subsample: the very low number of children in urban government schools (27 children across 18 schools) implies that the sample has very low power and that I cannot say much that is reliable about the presence or size of a private school premium in urban areas.

4.4 Results on 15-year old children in 2010

In this sub-section, I present the estimates of the effects of public and private schools on learning achievement for the older cohort, born in 1994/95, using scores from 2010 as the outcome variables.

Children in this cohort were, on average, aged about 15 years at the time of the survey in 2010. At this time, 77% of the sample (756 out of 976 children) were still in school; of those in school, about 63.5% were in government schools (480 children), while the rest were in private schools (276 children). Table 6 presents the descriptive statistics for the children in the three groups – not in school, government schools and private schools - separately for urban and rural areas. Private schools account for about a quarter of enrolment in rural areas but about two-thirds in urban areas. Children in private schools differ significantly from the other groups along socio-economic dimensions as in the younger cohort. Importantly, the Raven’s test for general intelligence, which is available for this cohort but not for the younger cohort, does not display any quantitatively meaningful difference across the different enrolment groups or across rural and urban areas; *prima facie*, while it seems that there is selection by housheold characteristics and the sex and birth order of the child, it does not seem as if there is selection by ability.

In this cohort, the data have three outcome measures available: scores on the mathematics achievement test, on the Peabody Picture Vocabulary Test and on the Cloze test. The PPVT and a math achievement test are also available from 2006 for use as lagged achievement measures; since the Cloze test was not administered in 2007, I use the lagged PPVT score and a reading test from 2007 as proxy for the lagged measure of the Cloze test.

For each of the outcome variables, I estimate the following specifications:

$$\begin{aligned} Y_{it} &= \alpha + \beta_1.noschool_{it} + \beta_2.Private_{it} + \beta_3.site_i && (12) \\ &\quad + \beta_4.X_{it} + \beta_5.raven_i + \beta_6.noncog_{i,t-1} && (13) \\ &\quad + \beta_7.Y_{i,t-1} && (14) \\ &\quad + schooltime_{it} + \epsilon_{it} && (15) \end{aligned}$$

where $noschool_{it}$ is an indicator variable equaling 1 if child i is not enrolled in school at time t and $Private$ is an indicator variable equaling 1 if the child is enrolled in a private school; enrolment in a government school is the base category.

X is a vector of background characteristics that includes standard information about the socio-economic background of the child (caste and wealth index), maternal and paternal years of schooling, the sex of the child, whether he/she is the eldest child in the household and time use outside of school. $raven$ is the score from the Raven's test taken by the child in 2002 which is included here as a control for ability. $noncog$ is a vector of five measures of aspirations and non-cognitive skills: the first is the educational aspirations of the child (a binary variable for whether the child wants to attend university[34]); the other four are indicator variables which equal 1 if the child agreed with the statements 'I am proud of my achievements at school', 'If I try hard, I can improve my situation in life', 'I like to make plans about future studies and work' and 'If I study hard, I will be rewarded with a better job in the future'. Due to concerns that learning achievement and these non-cognitive skills might be simultaneously determined, I use the values of the non-cognitive variables from the 2007 round of the survey to predict the 2010 scores[35]. Finally, $schooltime_{it}$ refers to the hours spent at school.

Results from the estimation are presented in Table 7 for rural areas and Table 8 for urban areas. Across the three test measures, there is a substantial positive and strongly significant private school premium when only controlling for mandal fixed effects, which is progressively reduced upon controlling for background characteristics, the Raven's test scores, lagged non-cognitive indicators, lagged achievement and hours spent in school on a typical day. However, unlike in the younger cohort,

[34] The precise wording of the question was: 'Imagine you had no constraints and could study for as long as you liked, or go back to school if you have already left. What level of formal education would you like to complete?'. The attempt is to see aspirations of education, not constrained expectations.

[35] Note that if private schools also create higher non-cognitive measures, perhaps by motivating children more, the resulting estimates of the private school premium understate the effectiveness of private schools.

a significant positive effect survives across all three test measures: in the most restrictive specifications, the premium is about 0.2 SD in math and 0.12 SD in PPVT and the Cloze test. The Raven's test score appears as a significant positive determinant across all specifications (although sometimes insignificant for mathematics) indicating that it does capture some element of ability that might remain even upon controlling for lagged achievement. The statement "If I study hard, I will be rewarded with a better job" is strongly related with academic performance, especially in mathematics. Hours spent working outside the household for pay or tasks on family farm and family business seem to have a negative association with test scores.

4.5 Robustness

Results reported on the three samples above - the younger cohort children in 2009/10 and 2011, and the older cohort in 2009/10 - display considerable heterogeneity. In rural areas for the younger cohort, there is strong evidence of a substantial premium in test scores for English but not for mathematics or Telugu. In the older cohort, while a substantial portion of the within-mandal differences in the test scores is explained away by controlling for background characteristics and lagged performance measures (about three-quarters of the gap in Cloze and PPVT and over half for mathematics), a significant gap remains suggesting that private schools have a significant positive impact on test scores in the post-primary stage of schooling.

While previous studies on the robustness of VAMs have been encouraging, and indeed our own results on a comparable cohort and indicators agree closely with the experimental evidence presented by MKS, the possibility of bias in the estimates cannot be definitively ruled out; this may especially be a concern for indicators/cohorts for which external validation through the MKS study is not available. Analogous to Rothstein's (2010) criticism (delivered in the context of tracking of students into different classrooms by headteachers), while VAMs may deliver unbiased estimates of the private school effect if selection was only on the variables controlled and past achievement, it is plausible that parents observe more or different information on child achievement which is used as basis for selecting whether the child is enrolled into private or government schools[36]. Furthermore,

[36] Parent's assessments of the child's academic performance may contain information other than that contained in test scores for at least two reasons. Parents may observe much more about their children than our survey measures can capture; and parental assessments may have significant measurement error of their own (if, for example, parents cannot reliably assess a child's actual progress i.e. how well the child *should* have done as opposed to actual progress). The precise reason for (possible) divergence of parental assessments from achievement data on our test measures is not central to the issue; what is important is that selection on ability, if any, depends on the former (parental) measure and not the latter (test scores). If there is divergence between the two, it is plausible that bias may still exist.

it is always possible that parents differ in their degrees of aspirations for children and the preferences they have towards their education; if these preferences lead to a greater propensity to select into private schools (as they are perceived to be of higher quality) and also lead to higher home-based investment which is not captured in our range of controls or proxied by past achievement, then our estimate of the private school effect might be biased.

I attempt to test directly for these sources of bias by using unique proxies available in the Young Lives data for these sources of bias. In 2007 and 2009/10, in both cohorts, the household survey collected parents' assessments of how they thought the child (if enrolled at the time) was performing in school; the measure was collected on a five-point scale with 1 being "Excellent" and 5 being "Very bad". Furthermore, in 2007 the survey asked parents what they would desire as the highest level of education for their child, in the absence of any constraints[37]. These measures seem to be meaningful: average test scores in mathematics seem to increase incrementally for each point of the parental assessment scale; similarly, parental aspirations about a child's education (reduced to a dummy variable for whether the parent would like the child to go to university) seem to be associated with private school attendance. As a robustness check on this possible source of bias, I estimate the lagged VAMs from Sections 4.3 and 4.4 (i.e. on the school survey sample and the older cohort sample[38]) supplementing the specification with a vector of dummy variables for each point of the parental assessment scale (with "Excellent" as the omitted category) and a dummy variable for whether the parent desires the child to go to university.

Results from this analysis are given for rural areas in Table 9; as can be seen, even though there is information in the parental assessments and their educational aspirations which is related to test scores, the coefficients on the private school dummy variable seem to be unchanged from the main estimates[39]. I find no evidence of additional bias in the VAM specification estimated in previous subsections.

Finally, I estimate specifications which test for the possibility of a different lag stucture in the VAMs: specifically, I estimated the main regression specifications including a third-order polynomial of the lag (as in Kane and Staiger (2008);

[37] The precise question was....

[38] I cannot run similar regressions for the younger cohort children using 2010 test scores because lagged parental assessment of performance in school is not available for over half of the sample; in 2007, children in this cohort were aged between 4.5 to 6 years and only about 44% had joined formal schooling.

[39] This is true for most coefficients in the regressions apart from the lagged achievement measures. This indicates that parent's assessments of child performance, although informative, do not seem to bias the estimates and probably reflect information similar to the lagged achievement measures.

Deming et al. (2011); Chetty et al. (2011)) instead of the lag only in levels (as in all specifications heretofore) and, separately, by including also lagged measures from time $t-2$ instead of just a single period lag; coefficients on the private school premium seem stable and unchanged. This is reported in Appendix B.

5 Decomposing learning production in schools

5.1 What differs across government and private schools?

Table 10 presents the descriptive statistics about school, class and teacher characteristics in the sample, and student-level observations/reports of school experience, by school type across rural and urban areas.

Private schools differ from government schools on several dimensions: they typically have more students and more teachers, are more likely to have access to amenities like toilets, drinking water, electricity connection, and libraries, and mostly report using English as the medium of instruction. Teachers in private schools are much more likely to be female, younger, less experienced, less likely to hold a teaching qualification, paid a fraction of their state school counterparts and less likely to hold a permanent contract; these teachers are much more likely to use a textbook during class observations by survey interviewers, are more likely to have marked most or all of the work in the homework books of the children in the sample, and are much less likely to be reported as being frequently absent by their students. Government schools are much more likely to have multigrade teaching (i.e. children of more than one grade being taught in class at the same time) and typically have a single teacher teach all subjects across for the grade. However, private schools have worse student-teacher ratios on aggregate, larger class sizes and a larger proportion of boys in class. This broad stylized picture seems to be true across both rural and urban areas.

The school-based data collection collected responses on a range of questions, often involving subjective responses, administered to children in the school survey subsample. Reports on home support for learning were previously used in Table 5.

5.2 Decomposing school productivity

In Table 10 we saw that not all differences in schooling were in favour of private schools: how do these differing factors determine productivity of schools in the two sectors in production of learning achievement?

My estimation strategy for answering the above question takes Eq (11) as the base and adds factors at the school, class and teacher level to estimate the relative contribution of these factors in promoting achievement. Given that incentives for teachers and teacher characteristics differ considerably between the two sectors, I allow a range of teacher characteristics to differ across the two sectors. Specifically, I estimate the following specification for the test scores in Telugu, English and Math:

$$\begin{aligned} Y_{it} &= \alpha + \beta_1.Private_{it} + \beta_2.site_i + \beta_3.X_{it} + \beta_4.noncog_{i,t-1} + \beta_5.homesupport_{it} + \beta_6.Y_{i,t-1} \\ &\quad + \beta_7.S_{it} + \beta_8.C_{it} + \beta_9.T_{it} + \beta_{10}.Private * \tau_{it} + \epsilon_{it} \end{aligned} \tag{16}$$

where S_{it} is a vector of school variables that includes an index of school facilities and the student-teacher ratio in the school, C_{it} is a vector of class level controls which includes whether the class was observed to be using a textbook during the observation of the math lesson, T_{it} is a vector of controls relating to teachers which include dummy variables for whether the teacher is male, the level of education, experience, and whether the teacher is permanent or temporary, whether the child had a notebook which with all or most of the work marked by the teacher, whether the child reported that the teacher was frequently absent and whether he/she attended extra classes with his teacher after school. A subset of these teacher controls - education levels, experience, sex and tenure - are allowed to differ in their impact across the two sectors. The survey also included a test of the teacher's pedagogical knowledge in mathematics which is included in the regressions on math scores. Other controls - $Private_{it}$, $site_{it}$, X_{it}, $noncog_{it}$, $homesupport_{it}$ and $Y_{i,t-1}$ - are defined as in Eq(11). Given that the Round 3 (2010) data collection and the school-based data collection are separated by less than a full academic year, there may be concerns as to whether adequate progress on learning has been made which can be captured through these specifications. Accordingly I also use specifications whih control for the lag from 2007 instead of 2010; the results do not change.

Table 11 presents the results from this exercise. Of school-level variables, infrastructure seems to be associated with a significant effect on test scores - gaining an additional amenity is associated with a 0.07 SD increase in test scores in Math and Telugu - but the coefficient on student-teacher ratio is both statistically insignificant and very small. Class size has a very strong negative effect: coefficients across the three tests imply that a difference in class size by 15 children (about the difference between the average class in a government school and an average class in a private school in our sample) results in roughly a difference in test scores by about 0.15 SD. Teacher absenteeism has a strong negative impact on math and Telugu scores (although not significant on the latter). Home support, wealth and hours per day studying outside of school time have large and statitically significant, even controlling for the various school-based inputs. Finally, we see surprisingly little heterogeneity in which teacher characteristics matter: the only exception is possessing a Bachelor's or postgraduate degree which seems to have a positive impact in government, but not private, schools. Teacher training, teacher experience and teacher tenure do not seem to have any effect on test scores.

5.3 Do student perceptions of schooling matter?

The analysis of achievement production in government and private schools focused on traditionally measured inputs. In this subsection, I investigate whether students' perceptions of their schooling experience and their own beliefs about their agency and efficacy affect their test scores, even conditional on the other school and home based investments examined previously.

The school survey data allows us to construct five measures, in addition to the home support index previously described, based on these attitudinal items[40]: an index of locus of control which measures the degree to which a student feels that outcomes in their life are under their control; an index of self-efficacy/academic self-concept which reflects an individual's self-assessment of their ability to achieve favourable outcomes; an index of peer support which is a measure aggregating over a child's subjective reponses to questions on several domains of support from peers; an index similarly measuring teacher support; and finally an index of school experience which aggregates responses to several dimensions of a child's experience of the school.

There is variation in these measures, even though most of them are skewed rightwards. Students in rural private schools report significantly higher degrees of self-efficacy, peer support as well as a much more positive assessment of their school experience (Table 12). They are significantly more likely to report being happy going to school, enjoying all their lessons at school and feeling safe at school. Students in private schools are much more likely to report self-assessments of being good in math and English (but not Telugu), being proud of their achievements at school, and being able to do class work without help. Finally, and somehwat surprisingly, children in private schools also give more positive reports of support from peers; they are more likely to report that they can approach other students

[40] The indices were computed using an identical procedure. Negative statements on the same dimension were rescored to be positive, each statement was converted to have a mean of zero and a standard deviation of 1, and then an average was taken across all questions in the domain. The specific questions that were included in each index and their means by school type in rural and urban areas are given in Appendix C.

It is possible that results are sensitive to aggregation methods. I intend to investigate such robustness in the future.

for help, that all other students in class are their friends, and less likely to report that children in their class tease them.

They also report somewhat higher levels of teacher support and locus of control, but these differences are not statistically significant. an exception in the degree of teacher support is in questions around fairness: children in private schools are much less likely to report that their teacher behaves 'unfairly' in all three statements assessing child's perceptions of fairness.

My method of investigating any effects of these characteristics on student achievement is straightforward: using Eq. (16) as the base, I sequentially add the assessments of peer support and teacher support, indexes of agency and self-efficacy, and finally the index of school experience. As can be seen in Table 12 for rural areas, while peer support does not seem to matter in our estimation, assessments of teacher support are strongly predictive of learning gains in math and Telugu: a 1 SD increase in teacher support is associated with a rise in math scores by about 0.1 SD. Both agency and efficacy matter as well. And finally, children's assessments of their schooling experience is also very strongly significantly predictive with a 1 SD change being associated with a 0.1-0.2 SD improvement across the three test scores.

Interpreting these estimates requires care. It is conceivable that that there is an endogenous relationship between attitudes such as self-efficacy and school experience and actual achievement in the form of test scores: it could be, for example, that doing better in school prompts greater happiness with the schooling experience and that is captured in the subjective assessments of school experience. There are two important things to note however: all regressions in Table 12 control for academic achievement in the previous session which should guard substantially against simple versions of the bias noted above - to the extent we worry that these attitudes may themselves be products of the past achievement history, controlling for this history should allay some of these concerns. Furthermore, all regressions also control for the full range of school, class and teacher characteristics and interactions of a subset of inputs with private school as in Eq (16)which should guard against the possibility of these characteristics being a mere reflection of standard school inputs and bolster the case that these attitudes and non-cognitive skills independently affect future outcomes[41].

[41] I do not investigate the correlates of these psychosocial variables but merely control for schooling inputs to avoid confounding effects of, for example, teacher characteristics. To the extent that we may care about psychosocial outcomes as outcomes of interest on their own, for example caring about children's happiness about school inpendently of their test performance in school, such an investigation may also be worthwhile. Patterns here do suggest cross-productivity across these different domains of child wellbeing and perfomance in school.

Measures of psychosocial variables in the school based data seem to be informative: they show important variation between individuals, this variation seems to be predictive of test achievement, and this association is robust to the inclusion of a rich set of controls at the school and household level and the past achievement of the child himself. This presents, in my opinion, strongly suggestive evidence for the possibly large effects of these psychosocial variables on achievement and possible gains in attempting to also measure them in other data collection in schools in developing countries. This is important to note because our current knowledge of which, if any, interventions might be able to shift these variables remains limited[42].

6 Conclusion

In this paper, I investigated the extent of test score gaps between students of private and government schools across several cognitive domains for children aged 8 years, 9 years and 15 years in rural and urban areas; I have tried to isolate the extent to which any gaps might be causal effects of private schools; and I have attempted to understand the sources of learning achievement at the school level.

Raw differences between children in private and government schools in test scores are invariably substantial, statistically significant, and favour private school students in every test score. However, much of this variation seems to be a reflection of greater home investment and socio-economic background. Upon controlling for a wide ranging set of controls and prior achievement, for younger children I find evidence of substantially better performance only in English and a somewhat smaller effect on receptive vocabulary. For older children, I do find significant impacts of going to private schools on their scores in mathematics and a cloze test in Telugu; while these differences are substantial and consistently significant, they are only between 25-50% of the average within-community difference in test scores. In urban areas, I find no evidence of a significant private school effect.

Two features may account for the differences in results between the younger and the older cohorts and between rural and urban areas. In urban areas, the degree of school choice is considerably greater, private schools are the dominant education provider, and it is plausible that parents only send their children to a government school if they know it to be a good school; as the core survey sample is based on a random sample of households, and not a random sample of schools, this would be consistent with finding no effects of private school on aggregate when compared to public schools similar children attend, but also consistent with (possibly many)

[42] A rare exception is Krishnan and Krutikova (2010) who report an intervention in an urban Mumbai slum to raise psychosocial skills and report finding substantial effects of the intervention on self-esteem and self-efficacy.

undersubscribed and ineffective government schools in urban areas. In comparing the younger and the older cohort, the most important difference is that the former are still in primary school whereas the latter are now in middle and secondary schooling. The characteristics of the school sectors at these different points of the educational trajectories, across both government and private sectors, are very different. Much of what we know about Indian education is based on studies focusing exclusively on primary schools. Heterogeneity in results between prmary and later schooling should not, therefore, be a cause of much concern.

Results on comparable indicators for a comparably aged cohort agree across this paper and recent experimental evidence presented by Muralidharan et al. (2012). Furthermore, I do not find any evidence of any bias arising in the main results as a result of tracking to private schools of or a more intensive application of home inputs based on either parental assessments of a child's school performance and their aspirations about the child's education. These tests, as well as broad agreement with independent experimental evidence, provide further evidence of the robustness of value-added models as a mechanism for investigating causal differences across school effects or teacher effects, which echoes a recent literature from the US and Pakistan. This is important since experiments may not be uniformly feasible across contexts and convenient convincing natural experiments may not be available in many situations where evaluating a relevant policy question remains important.

The availability of matched school data and rich household level data from multiple rounds allows me the possibility of estimating the role of different inputs in the production of learning. I find little evidence of characteristics such as tenure, teacher experience or teacher performance on a test of pedagogy affecting test scores but the effect of teacher absence seems to be strongly negative and substantial. Uniquely, I present strongly suggestive evidence that children's subjective reports of their schooling experience, their locus of control and their self-efficacy, and their reports of teacher support are associated with large changes in test scores even conditional on a rich set of school and home characteristics and past achievement history of the child. Private school students report substantially more positive degrees of school experience and self-efficacy.

The results highlight, in keeping with previous literature, that teacher accountability remains one of the core problems in the delivery of public education in India. Government school teachers are much better paid than teachers in private schools, have much greater job security, are much more likely to have received specialist teacher training and, as I uniquely document, are more knowledgeable on average than teachers in private schools; yet, they are more likely to be absent from school and their students report that they are less approachable and less fair; results in

Section 5 indicate that these factors have a direct impact on the performance of children.

This study does not offer any direct policy recommendations although it does highlight the important issues of incentives in the public sector (such as tenure and salary levels) and the poorer perception of school experience by the children in public schools themselves. The results do suggest that flat public sector pay increases and in-service trainings would be at best very blunt, and more probably entirely ineffective, in addressing the issue of teacher motivation and incentives; descriptive results that were presented regarding the inability of observations of class teaching by interviewers to capture significant differences in the quality of teaching (most likely due to a Hawthorne effect) suggest that mere monitoring also holds little promise.

Perhaps the most promising intervention thus far has been reported by Muralidharan and Sundararaman (2011) who present experimental evidence, from the same state as this study, that even small performance related incentives can end up with large gains across test scores and that these learning gains persist. Duflo et al. (ming) report on a field experiment from the state of Rajasthan where they report a small per-day incentive to teachers to come to school, which was verifiable through time-stamped photographs of the teachers, led to a significant decline in teacher absence. These interventions potentially hold promise for tackling the issue of teacher underperformance in government schools[43].

Results in this paper, combined with previous work highlighting that the average cost per child in rural private schools is less than a third of the average cost in the state schools and that private schools dedicate less instructional time to Telugu and mathematics (the dimensions in which I find no significant effect of private school attendance) suggests strongly that private schools are considerably more productive than government schools.

[43]There are, of course, significant concerns of external validity. It is possible that these interventions are ineffective once scaled up and put in charge of local authorities. Banerjee et al. (2008) report how a similar incentive scheme for nurses in Rajasthan was subverted by collusion between the nurses and the local administration. Similarly, even with performance-related pay, the incentives are only in place if collusion between the teachers and the testing authority can be guarded against; this cannot be taken for granted in the Indian context. Nonetheless, these are probably the most promising ideas that have been tested yet on how to improve incentives for public sector workers.

References

Andrabi, T., Das, J., Khwaja, A., and Zajonc, T. (2011). Do value-added estimates add value? accounting for learning dynamics. *American Economic Journal: Applied Economics*, 3(3):29–54.

Angrist, J. D., Pathak, P., and Walters, C. R. (2011). Explaining charter school effectiveness. NBER Working Papers 17332, National Bureau of Economic Research, Inc.

Arellano, M. and Bond, S. (1991). Some tests of specification for panel data: Monte carlo evidence and an application to employment equations. *The Review of Economic Studies*, 58(2):277–297.

Banerjee, A., Duflo, E., and Glennerster, R. (2008). Putting a band-aid on a corpse: Incentives for nurses in the indian public health care system. *Journal of the European Economic Association*, 6(2-3):487–500.

Chetty, R., Friedman, J., and Rockoff, J. (2011). The long-term impacts of teachers: Teacher value-added and student outcomes in adulthood. Technical report, National Bureau of Economic Research.

Chudgar, A. and Quin, E. (forthcoming). Relationship between private schooling and achievement: Results from rural and urban india. *Economics of Education Review*.

Cunha, F. and Heckman, J. (2008). Formulating, identifying and estimating the technology of cognitive and noncognitive skill formation. *Journal of Human Resources*, 43(4):738–782.

Cunha, F., Heckman, J., and Schennach, S. (2010). Estimating the technology of cognitive and noncognitive skill formation. *Econometrica*, 78(3):883–931.

Das, J., Dercon, S., Habyarimana, J., Krishnan, P., Muralidharan, K., and Sundararaman, V. (2011). School inputs, household substitution, and test scores. Technical report, National Bureau of Economic Research.

Das, J. and Zajonc, T. (2010). India shining and bharat drowning: Comparing two indian states to the worldwide distribution in mathematics achievement. *Journal of Development Economics*, 92(2):175–187.

Deaton, A. (1997). *The analysis of household surveys: a microeconometric approach to development policy.* Johns Hopkins University Press.

Deming, D., Hastings, J., Kane, T., and Staiger, D. (2011). School choice, school quality and academic achievement. *NBER Working Paper*, (17438).

Desai, S., Dubey, A., Vanneman, R., and Banerji, R. (2008). Private schooling in india: A new educational landscape. In *India Policy Forum*, volume 5, pages 1–58. Global Economy and Development Program, The Brookings Institution.

Duflo, E., Hanna, R., and Ryan, S. (forthcoming). Incentives work: Getting teachers to come to school. *American Economic Review*.

French, R. and Kingdon, G. (2010). The relative effectiveness of private and government schools in rural india: Evidence from aser data. *DoQSS Working Papers*.

Kane, T. and Staiger, D. (2008). Estimating teacher impacts on student achievement: An experimental evaluation. Technical report, National Bureau of Economic Research.

Kingdon, G. (1996). The quality and efficiency of private and public education: A case-study of urban india. *Oxford Bulletin of Economics and Statistics*, 58(1):57–82.

Kingdon, G. (2007). The progress of school education in india. *Oxford Review of Economic Policy*, 23(2):168–195.

Krishnan, P. and Krutikova, S. (2010). Skill formation in bombay slums. *Cambridge Working Papers in Economics*.

Muralidharan, K. and Kremer, M. (2009). Public and private schools in rural india. *School choice international: exploring public-private partnerships*, page 91.

Muralidharan, K., Kremer, M., and Sundararaman, V. (2012). The aggregate effect of school choice - evidence from a two-stage experiment in india. *unpublished*.

Muralidharan, K. and Sundararaman, V. (2011). Teacher performance pay: Experimental evidence from india. *The Journal of Political Economy*, 119(1):39–77.

Pal, S. (2010). Public infrastructure, location of private schools and primary school attainment in an emerging economy. *Economics of Education Review*, 29(5):783–794.

Pratham (2006). Annual status of education report 2005. Technical report, Pratham, New Delhi.

Pratham (2007). Annual status of education report 2005. Technical report, Pratham, New Delhi.

Rothstein, J. (2010). Teacher quality in educational production: Tracking, decay, and student achievement. *The Quarterly Journal of Economics*, 125(1):175–214.

Todd, P. and Wolpin, K. (2003). On the specification and estimation of the production function for cognitive achievement*. *The Economic Journal*, 113(485):F3–F33.

Todd, P. and Wolpin, K. (2007). The production of cognitive achievement in children: Home, school, and racial test score gaps. *Journal of Human capital*, 1(1):91–136.

Tooley, J. and Dixon, P. (2003). Private schools for the poor: A case study from india.

Tooley, J. and Dixon, P. (2007). Private schooling for low-income families: A census and comparative survey in east delhi, india. *International Journal of Educational Development*, 27(2):205–219.

Van der Linden, W. and Hambleton, R. (1997). Item response theory: Brief history, common models, and extensions. *Handbook of modern item response theory*, pages 1–28.

Table 1: Descriptive statistics - Younger Cohort (2010)

	Rural Areas			Urban Areas		
	Government	Private	Total	Government	Private	Total
CDA score (2007)	-0.2083	0.2144	-0.0936	-0.0551	0.3728	0.2935
PPVT score (2007)	-0.2551	0.16	-0.1417	0.0962	0.6058	0.5049
PPVT score (2010)	0.4717	0.8914	0.567	0.8011	1.082	1.0048
Math score (2010)	-0.1086	0.262	-0.008	-0.3163	0.1117	0.0325
Mother's education	3.1124	5.266	3.6967	3.7529	8.1337	7.3224
Father's Education	4.8219	7.8772	5.6509	5.1059	9.3556	8.5686
Male	0.4952	0.6164	0.5281	0.4824	0.5535	0.5403
First-born child	0.3371	0.4834	0.3768	0.2706	0.4572	0.4227
Scheduled Caste	0.2257	0.1483	0.2047	0.2	0.0802	0.1024
Scheduled Tribe	0.1905	0.087	0.1624	0.0706	0.008	0.0196
Other Backward Classes	0.4914	0.4834	0.4892	0.4	0.4679	0.4553
Other castes	0.0914	0.2813	0.143	0.3294	0.4385	0.4183
Monthly per capita expenditure (2010)	725.6993	1100.5204	825.4001	780.9076	1094.6817	1035.3972
Time use - hours spent on caring for others	0.2229	0.1893	0.2137	0.0941	0.1765	0.1612
Time use - hours spent in household chores	0.4	0.2506	0.3595	0.2588	0.2513	0.2527
Time use - hours spent studying after school	1.7505	2.0818	1.8404	1.6471	1.9652	1.9063
Time use - hours spent on no-paid work outside household	0.0152	0.0026	0.0118	0	0	0
Time use - hours spent on paid work outside household	0.0048	0.0051	0.0049	0.0118	0	0.0022
Time use - hours spent at school	7.6038	8.0153	7.7155	7.6353	7.9385	7.8824
N	1050	391	1441	85	374	459

Table 2: Private School Effect for 8 year olds (2010) - Rural Areas

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	r3_math_theta_mle				r3_ppvt_theta_mle			
Private school in 2009/10	0.35***	0.100	0.046	0.0068	0.42***	0.22***	0.15**	0.15**
	(0.097)	(0.079)	(0.072)	(0.077)	(0.078)	(0.064)	(0.058)	(0.062)
mother's education level		0.0069**	0.0052**	0.0056**		0.0076**	0.0064**	0.0065**
		(0.0026)	(0.0024)	(0.0022)		(0.0033)	(0.0029)	(0.0029)
father's education level		0.0066*	0.0043	0.0046		0.0020	-0.0020	-0.0020
		(0.0035)	(0.0035)	(0.0036)		(0.0031)	(0.0030)	(0.0030)
Male		0.096	0.088	0.085		0.13**	0.12***	0.12***
		(0.055)	(0.054)	(0.053)		(0.046)	(0.039)	(0.039)
wealth index		1.11***	0.95***	0.89***		0.75***	0.57***	0.57***
		(0.14)	(0.14)	(0.14)		(0.11)	(0.15)	(0.14)
hours per day - caring for others		0.094	0.074	0.070		-0.0049	-0.048	-0.048
		(0.065)	(0.068)	(0.067)		(0.051)	(0.043)	(0.043)
hours per day - domestic tasks		0.11**	0.098*	0.10*		0.024	0.022	0.022
		(0.048)	(0.049)	(0.048)		(0.034)	(0.033)	(0.033)
hours per day - at school				0.12***				0.014
				(0.036)				(0.033)
hours per day - studying outside of school time		0.10***	0.096***	0.11***		0.10***	0.095***	0.097***
		(0.028)	(0.026)	(0.026)		(0.024)	(0.025)	(0.027)
hours per day - tasks on family farm or other family business		-0.33**	-0.32*	-0.20		-0.0065	-0.029	-0.016
		(0.15)	(0.17)	(0.15)		(0.093)	(0.089)	(0.096)
hours per day - paid work outside of household		-0.042	-0.071	-0.074		-0.083	-0.049	-0.050
		(0.050)	(0.093)	(0.079)		(0.11)	(0.14)	(0.14)
Theta (MLE)			0.20***	0.20***				
			(0.028)	(0.028)				
Theta (MLE)							0.24***	0.24***
							(0.044)	(0.044)
Constant	-0.10***	-0.90***	-0.76***	-1.73***	0.47***	-0.12	0.10	-0.0040
	(0.026)	(0.13)	(0.13)	(0.27)	(0.018)	(0.12)	(0.13)	(0.30)
Observations	1,441	1,441	1,441	1,441	1,308	1,308	1,278	1,278
R-squared	0.271	0.322	0.349	0.356	0.210	0.262	0.317	0.317

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at mandal level
All regressions control for mandal fixed effects

Table 3: Private School Effect for 8 year olds (2010) - Urban Areas

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES		Math scores (2010)				PPVT scores (2010)		
Private school in 2009/10	0.53**	0.22	0.17	0.15	0.36*	0.15	0.15	0.17
	(0.14)	(0.13)	(0.095)	(0.099)	(0.16)	(0.24)	(0.22)	(0.20)
mother's education level		0.018**	0.016**	0.015**		0.019	0.013	0.013
		(0.0063)	(0.0052)	(0.0053)		(0.012)	(0.014)	(0.015)
father's education level		0.0056	0.0070*	0.0064*		0.022	0.021*	0.021*
		(0.0032)	(0.0026)	(0.0028)		(0.013)	(0.0093)	(0.0095)
wealth index		1.42**	1.36***	1.30***		0.40	0.19	0.33
		(0.34)	(0.23)	(0.26)		(0.46)	(0.55)	(0.61)
hours per day - caring for others		-0.15***	-0.15**	-0.16**		-0.051	-0.10	-0.094
		(0.022)	(0.037)	(0.041)		(0.15)	(0.14)	(0.15)
hours per day - domestic tasks		0.044	0.051	0.054		-0.17	-0.21	-0.23
		(0.11)	(0.090)	(0.096)		(0.17)	(0.17)	(0.17)
hours per day - at school				0.066				-0.11
				(0.033)				(0.098)
hours per day - studying outside of school time		0.036	0.035	0.045		0.035	0.036	0.014
		(0.049)	(0.049)	(0.048)		(0.048)	(0.044)	(0.054)
hours per day - paid work outside of household		-0.17	-0.10	-0.100		-0.17	0.17	0.18
		(0.20)	(0.20)	(0.19)		(0.21)	(0.34)	(0.35)
CDA Quantitative score (2007)			0.14***	0.14***				
			(0.027)	(0.028)				
PPVT score (2007)							0.16	0.16
							(0.084)	(0.080)
Constant	-0.40**	-1.39***	-1.35***	-1.82***	0.74***	0.14	0.30	1.14
	(0.12)	(0.25)	(0.20)	(0.29)	(0.12)	(0.18)	(0.20)	(0.60)
Observations	459	459	459	459	284	284	273	273
R-squared	0.175	0.266	0.300	0.304	0.117	0.208	0.235	0.246

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Standard errors clustered at mandal level
All regressions control for mandal fixed effects

Table 4: Descriptive statistics - Older Cohort (2010)

	Rural Areas			Urban Areas		
	Not enrolled	Public School	Private School	Not enrolled	Public School	Private School
Can read sentence in 2002	0.3333	0.4539	0.6619	0.3226	0.5735	0.781
Could write sentence in 2002	0.3439	0.4539	0.6403	0.4839	0.5441	0.8102
Math score (2002)	-0.7729	0.0587	0.4409	-0.4583	0.2425	0.4259
Math score (2010)	-0.9342	0.0905	0.5857	-0.6435	0.1395	0.5151
PPVT score (2007)	-0.7314	-0.0624	0.4056	-0.1963	0.5489	0.6725
PPVT score (2010)	-0.9082	-0.1868	0.4241	-0.3172	0.1609	0.439
Cloze scre (2010)	-0.7858	0.1129	0.5545	-0.7392	0.2172	0.3653
Raven's test score (2002)	22.4439	22.5	23.4783	23.3548	22.8235	24.562
Child aspires to attend university	0.2222	0.6359	0.7986	0.2581	0.6618	0.8905
If I try hard, I can improve my situation in life	0.8201	0.9053	0.9856	0.6129	0.8088	0.8686
I like to make plans about future studies and work	0.5344	0.6723	0.741	0.3871	0.6324	0.708
If I study hard, I will be rewarded with a better job in the future	0.3968	0.8277	0.9065	0.3548	0.75	0.8905
I am proud of my achievements at school	0.2434	0.5801	0.6978	0.2581	0.5882	0.7299
Mother's education (years)	1.709	4.4684	5.0288	3.4194	4	8.2701
Father's education (years)	2.455	5.0073	7.2734	4.7742	6.9559	10.1168
Male	0.4127	0.4854	0.5755	0.4839	0.3971	0.5766
Eldest child	0.2275	0.2621	0.4317	0.2258	0.3235	0.4745
Scheduled Castes	0.2222	0.2694	0.1079	0.2258	0.2206	0.0511
Scheduled Tribes	0.127	0.1335	0.1079	0	0.0294	0.0146
Other Backward Classes	0.5503	0.4757	0.4604	0.3226	0.5294	0.4964
Other Castes	0.1005	0.1189	0.3237	0.4516	0.2059	0.438
Monthly per capita real expenditure (2010)	1018.8392	963.02	1195.4145	990.7905	972.3836	1442.7581
Time use: Hours spent at school	0	7.8738	8.5612	0	7.6912	8.7956
Time use - Hours spent caring for others	0.5798	0.2136	0.1871	0.4194	0.1912	0.146
Time use - Household chores	2.4574	1.4393	0.8705	2.0968	0.9412	0.7591
Time spent on family farm, family business etc.	1.9415	0.1772	0.1295	0.5806	0	0.0438
Hours spent working for pay outside the household	4.4787	0.0583	0	4.7097	0.0294	0
Time spent studying after school	0.1543	2.4005	2.8417	0.4194	2.6471	2.6058
N	189	412	139	31	68	137

Table 5: Private school effect - 15 year olds, 2010 - Rural Areas

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES		r3_math_theta_mle				r3_ppvt_theta_mle				r3_cloze_theta_mle		
Private School in 2009/10	0.44***	0.25***	0.20**	0.19**	0.53***	0.26***	0.13*	0.11	0.43***	0.23**	0.12*	0.12*
	(0.062)	(0.069)	(0.070)	(0.075)	(0.097)	(0.073)	(0.069)	(0.074)	(0.11)	(0.088)	(0.061)	(0.064)
Not enrolled in 2009/10	-1.01***	-0.34**	-0.27**	-0.11	-0.74***	-0.12	-0.046	0.17	-0.90***	0.12	0.17	0.23*
	(0.10)	(0.13)	(0.12)	(0.12)	(0.092)	(0.14)	(0.11)	(0.11)	(0.11)	(0.17)	(0.15)	(0.12)
Male		0.23***	0.22***	0.23***		0.26***	0.20**	0.21**		0.061	0.019	0.023
		(0.049)	(0.054)	(0.053)		(0.067)	(0.080)	(0.079)		(0.070)	(0.071)	(0.068)
Child wants to attend university		0.17***	0.087	0.084		0.18***	0.089	0.084		0.20**	0.076	0.074
		(0.054)	(0.052)	(0.051)		(0.049)	(0.050)	(0.050)		(0.078)	(0.070)	(0.069)
If I study hard, I will be rewarded with a better job in the future		0.42***	0.33***	0.33***		0.34***	0.19**	0.18**		0.37***	0.14	0.14
		(0.11)	(0.088)	(0.089)		(0.092)	(0.079)	(0.077)		(0.10)	(0.087)	(0.085)
on a typical day hours spent - at school				0.032				0.044**				0.013
				(0.022)				(0.016)				(0.016)
hours spent - caring for others		-0.080	-0.084*	-0.076		0.018	0.0065	0.018		-0.067	-0.053	-0.050
		(0.054)	(0.045)	(0.045)		(0.026)	(0.018)	(0.018)		(0.040)	(0.045)	(0.046)
hhours spent - doing domestic tasks		0.044	0.056**	0.072**		0.014	0.0039	0.026		0.016	-0.0018	0.0053
		(0.030)	(0.026)	(0.025)		(0.021)	(0.017)	(0.020)		(0.037)	(0.034)	(0.032)
hhours spent in - tasks on family farm, cattle herding, other fa		-0.050*	-0.034	-0.024		-0.032	-0.033*	-0.018		-0.13***	-0.098***	-0.094***
		(0.025)	(0.022)	(0.024)		(0.018)	(0.016)	(0.019)		(0.020)	(0.022)	(0.023)
hhours spent in - activities for pay or for money outside of hou		-0.051***	-0.033**	-0.023		-0.022	-0.019	-0.0056		-0.083***	-0.059***	-0.054***
		(0.017)	(0.014)	(0.015)		(0.015)	(0.016)	(0.018)		(0.015)	(0.014)	(0.017)
hhours spent in - studying at home/extra tuition outside the hom		0.029	0.028	0.029		0.060**	0.016	0.018		0.044	0.019	0.019
		(0.030)	(0.029)	(0.026)		(0.025)	(0.017)	(0.016)		(0.033)	(0.030)	(0.030)
Raven's test score		0.017**	0.011	0.011		0.031**	0.020**	0.020**		0.026***	0.019***	0.019***
		(0.0065)	(0.0064)	(0.0065)		(0.013)	(0.0087)	(0.0084)		(0.0053)	(0.0041)	(0.0041)
Math score (2007)			0.38***	0.38***								
			(0.048)	(0.049)								
PPVT Score (2007)							0.54***	0.54***			0.32***	0.32***
							(0.061)	(0.059)			(0.053)	(0.053)
Can read without difficulty in R2											0.76***	0.76***
											(0.14)	(0.14)
Constant	0.096***	-0.99***	-0.57**	-0.85***	-0.17***	-1.67***	-0.88***	-1.26***	0.12**	-1.08***	-0.90**	-1.02***
	(0.030)	(0.26)	(0.23)	(0.28)	(0.033)	(0.38)	(0.27)	(0.34)	(0.040)	(0.28)	(0.30)	(0.28)
Observations	740	734	734	734	725	721	720	720	715	711	704	704
R-squared	0.322	0.412	0.494	0.495	0.302	0.462	0.633	0.637	0.228	0.366	0.485	0.485

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Coefficients on caste dummies, parental education variables and three non-cognitive indicators not presented in Cols. 2,3,5,6,8,9
Standard errors clustered at mandal level
mandal fixed effects included in all regressions

Table 6: Private school effect - 15 year olds, 2010 - Urban Areas

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES		r3_math_theta_mle				r3_ppvt_theta_mle				r3_cloze_theta_mle		
Private School in 2009/10	0.47**	0.071	0.074	0.079	0.31**	0.011	-0.0076	-0.064	0.14	-0.13	-0.23	-0.14
	(0.13)	(0.18)	(0.16)	(0.17)	(0.086)	(0.11)	(0.11)	(0.11)	(0.24)	(0.40)	(0.33)	(0.33)
Not enrolled in 2009/10	-0.74**	-0.34	-0.083	-0.14	-0.47**	-0.31	0.019	0.66	-0.96**	-1.01	0.11	-0.75
	(0.19)	(0.36)	(0.33)	(0.26)	(0.16)	(0.28)	(0.12)	(0.46)	(0.29)	(0.49)	(0.64)	(0.55)
Male		0.088	0.046	0.046		0.23*	0.18	0.18		0.0098	0.0059	-0.0091
		(0.17)	(0.22)	(0.22)		(0.084)	(0.10)	(0.097)		(0.18)	(0.12)	(0.17)
Child wants to attend university		0.28	0.22	0.22		0.067	-0.019	-0.033		0.27	0.081	0.098
		(0.16)	(0.15)	(0.15)		(0.15)	(0.16)	(0.14)		(0.31)	(0.26)	(0.27)
If I study hard, I will be rewarded with a better job in the future		0.081	0.059	0.059		-0.087	0.012	-0.0015		-0.094	0.036	0.032
		(0.21)	(0.17)	(0.17)		(0.24)	(0.16)	(0.16)		(0.36)	(0.27)	(0.28)
Eldest child in the household		0.11	0.048	0.049		0.21	0.080	0.062		0.15	0.048	0.086
		(0.13)	(0.12)	(0.12)		(0.13)	(0.092)	(0.100)		(0.095)	(0.097)	(0.092)
wealth index		0.94	0.34	0.35		1.49***	1.20***	1.13***		0.93	0.56	0.62
		(0.45)	(0.54)	(0.53)		(0.19)	(0.18)	(0.11)		(0.48)	(0.70)	(0.71)
on a typical day hours spent - at school				-0.0069				0.083				
				(0.020)				(0.061)				
hours spent - caring for others		0.092	0.039	0.037		-0.054	-0.018	0.0037		-0.039	0.030	-0.000021
		(0.043)	(0.049)	(0.051)		(0.065)	(0.096)	(0.11)		(0.20)	(0.21)	(0.20)
hours spent - doing domestic tasks		-0.11	-0.14	-0.14		-0.066	-0.12*	-0.11*		0.032	-0.0060	-0.026
		(0.077)	(0.079)	(0.082)		(0.054)	(0.045)	(0.041)		(0.080)	(0.10)	(0.095)
hours spent in - tasks on family farm etc.		0.033	0.012	0.011		0.034	0.018	0.030		0.072	0.077	0.058
		(0.047)	(0.039)	(0.041)		(0.043)	(0.033)	(0.037)		(0.059)	(0.073)	(0.071)
hours spent in - activities for pay outside of hhh		-0.023	-0.042	-0.042		-0.0086	-0.034	-0.030		0.061	0.059	0.056
		(0.056)	(0.046)	(0.046)		(0.052)	(0.032)	(0.028)		(0.033)	(0.039)	(0.036)
hours spent in - studying at home/extra tuition outside the hom		0.040	0.026	0.024		0.029	0.018	0.040		0.11	0.13	0.093
		(0.042)	(0.039)	(0.040)		(0.073)	(0.036)	(0.024)		(0.056)	(0.100)	(0.076)
Raven's test score		0.026	0.014	0.014		0.018**	0.0092**	0.0092**		0.023***	0.012	0.011
		(0.013)	(0.010)	(0.010)		(0.0054)	(0.0025)	(0.0028)		(0.0044)	(0.011)	(0.011)
Theta (MLE)			0.34**	0.34**								
			(0.092)	(0.092)								
Theta (MLE)							0.39***	0.38***			0.25**	0.26*
							(0.067)	(0.064)			(0.087)	(0.11)
Can read without difficulty in R2											0.62	0.65
											(0.34)	(0.31)
Constant	0.082	-1.48***	-0.74*	-0.69	0.15*	-1.29**	-0.93***	-1.55**	0.22	-1.47**	-2.31**	-1.48**
	(0.093)	(0.096)	(0.31)	(0.41)	(0.059)	(0.33)	(0.15)	(0.39)	(0.14)	(0.39)	(0.59)	(0.51)
Observations	236	236	236	236	170	170	165	165	144	144	138	138
R-squared	0.258	0.448	0.520	0.520	0.157	0.378	0.546	0.560	0.218	0.375	0.449	0.431

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Coefficients on caste dummies, parental education variables and three non-cognitive indicators not presented in Cols. 2,3,5,6,8,9.
Standard errors clustered at mandal level. Mandal fixed effects included in all regressions

Table 7: Descriptives - 9 year olds (2011)

	Rural Areas			Urban Areas		
	Government	Private	Total	Government	Private	Total
Math score (2011)	0.0227	0.4535	0.1229	0.4932	0.2663	0.3013
English score (2011)	-0.3614	0.5351	-0.1436	0.1595	0.6119	0.5417
Telugu score (2011)	-0.0829	0.3062	0.0077	0.4111	-0.1182	-0.0366
CDA score (2007)	-0.2211	0.2827	-0.1039	0.0459	0.2729	0.2379
PPVT score (2007)	-0.264	0.1681	-0.1625	0.0656	0.4521	0.3851
PPVT score (2010)	0.4611	0.8479	0.5375	1.0939	0.9727	0.9971
Math score (2010)	-0.0371	0.3589	0.055	-0.0662	-0.0253	-0.0316
Mother's education	3.1629	5.1734	3.6304	4.3704	7.25	6.8057
Father's Education	5.1734	7.9249	5.8132	5.5556	8.4865	8.0343
Male	0.5044	0.6647	0.5417	0.5185	0.4932	0.4971
Urban	0	0	0	1	1	1
First-born child	0.3082	0.4624	0.3441	0.1481	0.4595	0.4114
Scheduled Caste	0.2119	0.1329	0.1935	0.2963	0.0946	0.1257
Scheduled Tribe	0.1331	0.0867	0.1223	0	0	0
Other Backward Classes	0.5639	0.5145	0.5524	0.3704	0.4865	0.4686
Other castes	0.0893	0.2659	0.1304	0.3333	0.4189	0.4057
Monthly per capita expenditure (2010)	738.687	1071.3841	815.0063	772.8651	1032.7854	991.7454
cmotivation 1	0.91	0.96	0.93	0.95	0.97	0.97
cmotivation 2	0.6	0.71	0.63	0.55	0.69	0.67
cmotivation 3	0.91	0.94	0.91	0.95	0.93	0.93
z home support index	-0.14	0.02	-0.1	0.14	0.45	0.41
N	571	173	744	27	148	175

Table 8: Private school effect - 9 year olds (2011) - Rural Areas

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ss_math_theta_mle			ss_telugu_theta_mle			ss_english_theta_mle		
Private School	0.37***	0.058	0.034	0.14	-0.072	-0.12	0.92***	0.68***	0.60***
	(0.10)	(0.081)	(0.067)	(0.095)	(0.095)	(0.089)	(0.15)	(0.16)	(0.15)
Home support index - normalized		0.076*	0.055*		0.12**	0.096**		0.084*	0.044
		(0.039)	(0.030)		(0.043)	(0.036)		(0.040)	(0.035)
wealth index		1.11***	0.74***		0.79**	0.31		0.75	0.092
		(0.22)	(0.22)		(0.35)	(0.33)		(0.46)	(0.40)
hours per day - caring for others		-0.035	-0.075		0.0087	-0.0048		0.049	0.018
		(0.057)	(0.050)		(0.065)	(0.054)		(0.081)	(0.080)
hours per day - domestic tasks		0.050	0.015		0.017	0.0092		-0.044	-0.089
		(0.052)	(0.048)		(0.042)	(0.043)		(0.064)	(0.059)
hours per day - studying outside of school time		0.095***	0.078**		0.11***	0.075**		0.14***	0.093***
		(0.026)	(0.026)		(0.032)	(0.033)		(0.041)	(0.030)
hours per day - tasks on family farm or other family business		-0.20	-0.15		-0.050	0.10		-0.11	-0.040
		(0.19)	(0.19)		(0.17)	(0.12)		(0.23)	(0.22)
hours per day - paid work outside of household		0.18**	0.15**		0.53***	0.48***		0.096	-0.020
		(0.075)	(0.065)		(0.058)	(0.082)		(0.33)	(0.35)
If I try hard, I can improve my situation in life		0.039	0.038		0.046	0.063		0.11	0.11
		(0.11)	(0.096)		(0.14)	(0.14)		(0.17)	(0.18)
I like to make plans about future studies and work		0.16***	0.085*		0.15**	0.12		0.11	0.054
		(0.055)	(0.044)		(0.065)	(0.076)		(0.078)	(0.076)
If I study hard, I will be rewarded with a better job in the future		0.098	0.12		-0.16	-0.16		-0.028	-0.13
		(0.12)	(0.10)		(0.15)	(0.15)		(0.13)	(0.13)
Theta (MLE)						0.47***			0.20***
						(0.080)			(0.067)
Can read sentence in R3									0.36***
									(0.088)
Can write without difficulty in R3									0.38***
									(0.080)
Theta (MLE)			0.30***						
			(0.072)						
Constant	0.026	-0.67**	-0.41	-0.031	-0.32	-0.22	-0.40***	-0.98***	-0.87***
	(0.026)	(0.27)	(0.25)	(0.025)	(0.31)	(0.28)	(0.041)	(0.25)	(0.20)
Observations	743	743	743	742	742	687	707	707	652
R-squared	0.169	0.257	0.351	0.147	0.221	0.340	0.283	0.340	0.432

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Robustness to selection on parental assessments and aspirations - Rural areas

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	8-year old (2010)		9-year old (2011)			15-year old (2010)		
VARIABLES	Math	PPVT	Math	Telugu	English	Math	PPVT	Cloze
Private	0.018	0.12**	0.012	-0.15	0.56***	0.22***	0.18***	0.13**
	(0.059)	(0.052)	(0.085)	(0.11)	(0.18)	(0.061)	(0.051)	(0.050)
_Iperfr3_2	-0.23*	-0.12*	-0.27*	-0.38	0.0064	-0.16	-0.14	-0.13
	(0.11)	(0.063)	(0.13)	(0.29)	(0.20)	(0.093)	(0.082)	(0.11)
_Iperfr3_3	-0.34**	-0.22**	-0.49***	-0.61*	-0.27	-0.48***	-0.12	-0.16
	(0.16)	(0.090)	(0.15)	(0.30)	(0.22)	(0.11)	(0.083)	(0.100)
_Iperfr3_4	-0.74***	-0.44***	-0.69***	-1.00**	-0.48*	-0.68***	-0.15	-0.64**
	(0.15)	(0.10)	(0.17)	(0.36)	(0.26)	(0.20)	(0.12)	(0.23)
_Iperfr3_5	-0.87***	-0.56***	-0.49***	-0.59**	-0.058	-1.18	-0.34*	-1.56***
	(0.25)	(0.13)	(0.14)	(0.27)	(0.23)	(0.87)	(0.17)	(0.22)
Parent Aspiration: Child will go to university	0.057	0.031	-0.0032	0.0087	0.020	-0.0070	0.063	0.091*
	(0.043)	(0.032)	(0.071)	(0.073)	(0.075)	(0.044)	(0.053)	(0.043)
Observations	1,348	1,199	695	642	607	625	612	608
R-squared	0.368	0.325	0.370	0.382	0.393	0.375	0.585	0.391

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Differences between government and private schools

	Government School	Private School	Total	Government School	Private School	Total
School Characteristics						
English medium	0	0.59	0.23	0.06	0.72	0.59
Highest grade taught	5.49	8.46	6.67	5.76	8.67	8.11
Number of students (I - V)	73.68	273.44	152.81	126.35	331.89	292.63
Number of teachers (I-V)	3.34	9.51	5.79	4.59	11.33	10.04
Proportion of teachers with permanent contracts	0.73	0.24	0.53	0.84	0.5	0.56
Proportion of male teachers	0.62	0.44	0.55	0.32	0.18	0.21
Proportion of teachers with teaching qualification	0.83	0.65	0.76	0.96	0.62	0.68
Student-Teacher ratio	20.7	29.06	24.01	25.98	29.56	28.87
One teacher teaches all subjects in Grade V	0.94	0.05	0.59	0.88	0.17	0.3
Has a library	0.03	0.33	0.15	0.06	0.42	0.35
Has a playground	0.75	0.82	0.78	0.47	0.56	0.54
Has an electricity connection	0.74	0.95	0.82	0.94	0.97	0.97
Has drinking water availability	0.71	0.95	0.8	0.82	0.97	0.94
Multigrade teaching observed	0.81	0.08	0.52	0.65	0.13	0.22
Number of separate rooms	2.88	10.74	5.99	4.53	12.43	10.92
Has toilets	0.63	0.82	0.71	0.88	0.99	0.97
N	**93**	**61**	**154**	**17**	**72**	**89**
Class Characteristics						
Proportion of boys in class	0.47	0.61	0.52	0.49	0.53	0.52
Class used textbook during math observaton	0.59	0.71	0.63	0.58	0.79	0.76
Total number of enrolled children	15.85	30.16	20.79	23.95	33.63	32.26
N	**222**	**117**	**339**	**19**	**115**	**134**
Teacher characteristics						
Teacher: Age	32.9	28.22	31.43	37.89	29.89	31.13
Teacher: Experience	7.49	4.83	6.66	11.28	5.27	6.2
Teacher: Salary	12106.33	3463.54	9409.52	16295.39	3906.53	5828.94
tch caste1	0.16	0.14	0.16	0.17	0.15	0.16
tch caste2	0.14	0.02	0.11	0.06	0	0.01
tch caste3	0.54	0.45	0.51	0.33	0.34	0.34
tch caste4	0.16	0.39	0.23	0.44	0.51	0.5
Teacher: Male	0.68	0.48	0.62	0.44	0.17	0.22
Teacher education: Upto Senior Secondary	0.3	0.22	0.27	0.06	0.11	0.1
Teacher Education: Bachelor's Degree	0.53	0.53	0.53	0.67	0.72	0.72
Teacher Education: Postgraduate	0.17	0.25	0.2	0.28	0.15	0.17
Teacher: Has teaching qualification	0.81	0.67	0.77	1	0.53	0.6
Teacher: Permanent Contract	0.68	0.18	0.52	0.78	0.27	0.34
Teacher: teaches all subjects to grade	0.81	0.07	0.58	0.83	0.15	0.26
N	**183**	**83**	**266**	**18**	**98**	**116**
Student level variables						
Has homework book	0.85	0.97	0.88	0.95	0.97	0.96
All/most of work in notebook is marked	0.38	0.81	0.5	0.71	0.84	0.82
My teacher is frequently absent from school	0.39	0.26	0.36	0.23	0.36	0.34
I attend extra classes with my teacher after school	0.54	0.61	0.56	0.45	0.49	0.49
Home support index (normalized)	-0.14	0.02	-0.1	0.14	0.45	0.41
Agency index (normalized)	-0.07	-0.02	-0.06	0.21	0.24	0.23
Efficacy index (normalized)	-0.15	0.2	-0.06	0.03	0.27	0.23
Peer support index (normalized)	-0.12	0.05	-0.08	0.35	0.31	0.32
Teacher support index (normalized)	-0.09	0.01	-0.06	0.54	0.18	0.23
School experience index (normalized)	-0.15	0.16	-0.07	0.38	0.26	0.28
N	**549**	**194**	**743**	**22**	**147**	**169**

Table 11: Decomposing school productivity - rural areas

VARIABLES	(1) ss_math_theta_mle	(2) ss_telugu_theta_mle	(3) ss_english_theta_mle	(4) ss_math_theta_mle	(5) ss_telugu_theta_mle	(6) ss_english_theta_mle
Private School	0.28	0.43*	1.38***	0.29	0.40*	1.34***
	(0.17)	(0.23)	(0.23)	(0.20)	(0.23)	(0.24)
Infrastructure index	0.34**	0.38*	0.11	0.32*	0.34*	0.19
	(0.16)	(0.21)	(0.26)	(0.17)	(0.21)	(0.26)
Student Teacher Ratio	-0.00025	0.00064	0.0033	0.0013	-0.0014	0.0024
	(0.0038)	(0.0040)	(0.0041)	(0.0042)	(0.0043)	(0.0040)
did this class incorporate the use of textbooks?	0.11	0.11	0.023	0.15**	0.15*	0.076
	(0.068)	(0.083)	(0.083)	(0.066)	(0.075)	(0.079)
Proportion of boys in class	-0.32	-0.12	-0.17	-0.19	-0.21	-0.20
	(0.22)	(0.24)	(0.26)	(0.24)	(0.23)	(0.25)
No. of children enrolled	-0.0076**	-0.018***	-0.0074*	-0.012***	-0.019***	-0.0092**
	(0.0038)	(0.0049)	(0.0040)	(0.0042)	(0.0049)	(0.0040)
Teacher education: Bachelor's Degree	0.19**	0.0082	0.22	0.20**	-0.035	0.20
	(0.075)	(0.10)	(0.14)	(0.084)	(0.11)	(0.13)
Teacher education: Post-graduate Degree	0.17	0.068	0.13	0.21*	0.075	0.16
	(0.11)	(0.12)	(0.14)	(0.11)	(0.14)	(0.14)
Teacher qualification: Diploma or qualification in teaching	0.020	0.22	0.19	-0.076	0.25	0.14
	(0.12)	(0.17)	(0.20)	(0.14)	(0.18)	(0.21)
Govt. school contract teacher	-0.066	-0.052	-0.024	-0.11	-0.087	-0.10
	(0.11)	(0.11)	(0.15)	(0.12)	(0.12)	(0.15)
Teacher: Experience	0.0031	-0.0025	0.0017	-0.0019	-0.0045	-0.00071
	(0.0055)	(0.0053)	(0.0086)	(0.0055)	(0.0055)	(0.0080)
my class teacher often does not come to school	-0.16***	-0.089	0.011	-0.17***	-0.067	0.021
	(0.058)	(0.066)	(0.068)	(0.059)	(0.060)	(0.064)
Teacher score on pedagogy test	0.047			0.055		
	(0.047)			(0.051)		
Private*teacher_theta_mle	-0.065			-0.032		
	(0.087)			(0.088)		
Private*tchr_edn_2	-0.48***	-0.38*	-0.36*	-0.62***	-0.26	-0.33*
	(0.17)	(0.20)	(0.20)	(0.19)	(0.18)	(0.19)
Private*tchr_edn_3	-0.30	-0.42*	-0.26	-0.53**	-0.36	-0.22
	(0.21)	(0.25)	(0.24)	(0.22)	(0.23)	(0.22)
Private*tch_trg	0.061	-0.13	-0.40	0.098	-0.13	-0.32
	(0.18)	(0.22)	(0.25)	(0.21)	(0.22)	(0.26)
Observations	726	670	636	726	714	680
R-squared	0.405	0.393	0.401	0.367	0.346	0.405

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 12: Effect of subjective experience of schooling and psychosocial variables on test scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	ss_math_theta_mle			ss_telugu_theta_mle			ss_english_theta_mle		
Private School	0.18	0.19	0.15	0.32	0.34	0.29	1.31***	1.34***	1.28***
	(0.17)	(0.16)	(0.16)	(0.22)	(0.21)	(0.21)	(0.23)	(0.24)	(0.23)
Peer support index	0.048	0.0057	-0.013	0.12***	0.058	0.035	0.044	-0.0030	-0.032
	(0.032)	(0.031)	(0.032)	(0.038)	(0.035)	(0.034)	(0.043)	(0.046)	(0.044)
Teacher support index	0.14***	0.088***	0.071**	0.10***	0.038	0.018	0.078**	0.027	0.00061
	(0.033)	(0.032)	(0.031)	(0.035)	(0.035)	(0.034)	(0.037)	(0.038)	(0.038)
Agency index - normalized		0.066**	0.051*		0.094**	0.075		0.083*	0.055
		(0.030)	(0.031)		(0.043)	(0.047)		(0.043)	(0.044)
Efficacy index - normalized		0.14***	0.12***		0.20***	0.17***		0.15***	0.12***
		(0.030)	(0.031)		(0.039)	(0.037)		(0.043)	(0.043)
School experience index			0.11***			0.14***			0.19***
			(0.031)			(0.050)			(0.048)
Infrastructure index	0.33**	0.29*	0.37**	0.42*	0.35*	0.43**	0.12	0.078	0.17
	(0.16)	(0.15)	(0.15)	(0.22)	(0.20)	(0.20)	(0.26)	(0.26)	(0.26)
Constant	-0.41	-0.29	-0.39	-0.71	-0.52	-0.62	-1.68***	-1.51***	-1.70***
	(0.38)	(0.36)	(0.36)	(0.47)	(0.43)	(0.43)	(0.49)	(0.47)	(0.46)
Observations	726	726	726	670	670	670	636	636	636
R-squared	0.428	0.451	0.459	0.414	0.448	0.458	0.408	0.428	0.443

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1