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THE AGGREGATE EFFECT OF SCHOOL CHOICE: EVIDENCE FROM A TWO-STAGE EXPERIMENT IN INDIA

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ABSTRACT

We present experimental evidence on the impact of a school choice program in the Indian state of Andhra Pradesh (AP) that featured a unique two-stage lottery-based allocation of school vouchers that created both a student-level and a market-level experiment. This design allows us to study both the individual and the aggregate effects of school choice (including spillovers). We find that private-school teachers have lower levels of formal education and training than public-school teachers, and are paid much lower salaries. On the other hand, private schools have a longer school day, a longer school year, smaller class sizes, lower teacher absence, higher teaching activity, and better school hygiene. After two and four years of the program, we find no difference between the test scores of lottery winners and losers on math and Telugu (native language). However, private schools spend significantly less instructional time on these subjects, and use the extra time to teach more English, Science, Social Studies, and Hindi. Averaged across all subjects, lottery winners score 0.13σ higher, and students who attend private schools score 0.23σ higher. We find no evidence of spillovers on public-school students who do not apply for the voucher, or on students who start out in private schools to begin with, suggesting that the program had no adverse effects on these groups. Finally, the mean cost per student in the private schools in our sample is less than a third of the cost in public schools. Our results suggest that private schools in this setting deliver (slightly) better test score gains than their public counterparts, and do so at substantially lower costs per student. More generally, our results highlight that ignoring heterogeneity among schools' instructional programs and patterns of time use may lead to incorrect inference on the impact of school choice on learning outcomes.

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1. Introduction

One of the most important trends in primary education in developing countries over the past couple of decades has been the rapid growth of fee-charging private schools that cater to the poor.[1] This growth is especially striking as it is taking place in spite of increasing government spending on public education, and near universal access to free public primary education. Annual data for rural India shows a steady growth in private schooling, with the most recent estimates being that the private school enrollment share is over 28% in *rural* India (Pratham 2012). The corresponding figure for urban areas was estimated at 58% in 2005 (Desai et al. 2009) and is likely to be over 65% in 2012 (Rangaraju et al. 2012).

Opponents of the growth of private schools argue that this phenomenon leads to economic stratification of the education system and weakens the public education system by causing elites to secede. They also worry that private schools compete by cream-skimming students, and attract parents and students on the basis of superior mean test scores and performance, but that they may not be adding more value to the marginal applicant.[2] Others contend that private schools in developing countries have arisen and grown in response to failures of the public schooling system, that they are more accountable and responsive to parents, that the revealed preference of parents suggests that they are likely to be better than public schools, and that they are likely to be more cost effective than public schools.[3] However, there is very little rigorous empirical evidence on the relative effectiveness of private schools in developing countries. Non-experimental studies have used several approaches to address identification challenges, and have typically found that private school students have higher test scores, but have not been able to rule out the concern that these estimates are confounded by selection and omitted variables.[4]

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[1] See Srivastava and Walford (2007) for a review of this phenomenon with a focus on South Asia and Africa.

[2] Several studies across different contexts find that elite schools that are in much demand from parents and have significantly higher *levels* of test scores do not seem to add more value to student learning. Zhang (2012) shows this in China, Lucas and Mbiti (2012) in Kenya, Cullen et al. (2005) in Chicago, and Abdulkadiroglu et al (2012) do so in Boston and New York. Hsieh and Urquiola (2005) argue that Chile's voucher program led to increased sorting but did not improve average school productivity across all students.

[3] See Tooley and Dixon (2007), Muralidharan and Kremer (2008), Goyal and Pandey (2009), and Tooley (2009)

[4] Existing approaches to identifying the causal effects of private schools in developing countries include controlling for observables (Muralidharan and Kremer 2008), incorporating a selection correction (Desai et al 2009), using family fixed effects and within household variation (French and Kingdon 2010), aggregation of test scores to district-level outcomes (Bold et al 2011; Tabarrok 2013), and using panel data (Singh 2013). Angrist et al. (2002 and 2006) provide experimental evidence on school vouchers in a middle-income setting by evaluating the PACES program in Colombia, and find positive effects of the program. However, the PACES program also featured student incentives for effort by requiring maintenance of grades, and non-repetition in order to continue receiving the voucher, and the estimates therefore reflect a combination of private school productivity and student incentives.

Reflecting the concerns of growing economic stratification in schooling, the recent Right to Education (RtE) Act passed by the Indian parliament includes a provision mandating that private schools reserve up to 25% of their seats for students from disadvantaged backgrounds, with a reimbursement of fees by the government (subject to a maximum of the per-child spending in the public schools). This provision in the RtE Act could lead to India having the world's largest number of children attending private schools with public funding, and may constitute the largest attempt to achieve school integration across economic classes anywhere in the world. These large-scale changes to the education system have however been proposed (and are starting to be implemented) with almost no evidence on their likely impacts.

In this paper, we present results from a four-year long experimental evaluation of a school choice program in the Indian state of Andhra Pradesh (AP) that was designed to closely resemble a scenario where this RtE provision is implemented. The AP School Choice Project provided children who were enrolled in free public primary schools with a voucher that allowed them to attend a private school of their choice. The project design featured a two-stage randomization of the offer of a voucher (across villages as well as students) and allows us to estimate the impact of the voucher on lottery winners, relative to lottery losers in *control villages*, thus creating an uncontaminated comparison group that is unaffected by the departure of voucher students. The presence of control villages allows us to simulate a *counterfactual school system* and thereby study the aggregate effects of such a program by comparing outcomes for non-applicants as well as students who start out in private schools across treatment and control villages.

We find that the main operating difference between private and public schools in India is that private schools pay substantially lower teacher salaries (less than a sixth of that paid to public school teachers), and hire teachers who are younger, less educated, and much less likely to have professional teaching credentials. However, they hire more teachers and have smaller class sizes and less multi-grade teaching than public schools. Using official data as well as data collected from direct observations conducted during unannounced visits to schools, we find that private schools have a longer school day, a longer school year, lower teacher absence, higher teaching activity, and better school hygiene. We find no significant change in household spending or in time spent doing homework among voucher-winning students, suggesting that the impact of the program (if any) is most likely to be due to changes in school as opposed to household factors.

However, in spite of the superior performance of the private schools on most measures of school processes, we find at the end of two and four years of the school choice program that lottery winners do no better than lottery losers on tests of Telugu (native language of AP) and Math. Our data from school time tables suggest that a likely explanation for these results is that private schools spend significantly less instructional time on Telugu and Math, and instead spend more time on English, Science, Social Studies, and Hindi. We conduct tests in these subjects at the end of four years of the program and find positive (but insignificant) effects of winning the voucher on test scores in English, Science, and Social Studies (of around 0.1σ each), and positive (and highly significant) effects on test scores in Hindi (of 0.5σ). Averaging across all subjects, we find that students who won a voucher scored 0.13σ higher, and students who attend private schools score 0.23σ higher.

We find no evidence of spillovers on students who do not apply for the voucher or students who start out in private schools to begin with, suggesting that there were no adverse peer effects on these groups. We also do not find any significant difference between the test scores of lottery losers who were in program villages, and lottery losers in control villages. Thus, even though we use the 'correct' (uncontaminated) comparison group for our estimates, using the typical comparison group would not have significantly altered our results. Finally (and crucially in the policy context of the Right to Education Act in India), we find no evidence of any negative spillovers on students who started out in private schools to begin with.

While the mean test score impacts of the voucher reported across subjects are positive and significant, there is no objective way to weight the different subjects in terms of their importance for labor market outcomes. However, even without weighting across subjects, the combination of test score results and school time table data already show that private schools are more productive than public schools because they are able to achieve similar Telugu and Math test scores for the lottery winners with substantially less instructional time, and use the additional time to improve outcomes on other subjects – especially Hindi. But the cost-effectiveness comparison is rendered stark by the fact that the annual cost per student in the government-school system is over three times the mean cost per student in the private schools in our sample. Thus, students who win a lottery to attend private schools do as well on some subjects and better on others even though the private schools spend substantially lower amounts per student.

Since Friedman (1962), the theoretical promise of increased choice and competition for better education outcomes has generated a large empirical literature trying to measure the impacts of school choice on education outcomes, with the best-identified studies typically using lottery-based designs to identify the impact of choice and better schooling options.[5] However, the results to date are quite mixed with most studies typically finding zero to modest positive effects of receiving a voucher or attending a more selective school on test scores (Rouse and Barrow 2009 review the evidence), though recent evaluations have found positive effects of attending charter schools on test scores (Abdulkadiroglu et al. 2011; Dobbie and Fyer 2011).

We add to this evidence base with one of the largest and most comprehensive (in terms of data collected on intermediate inputs) school choice experiments in the world. In addition to providing the first experimental evidence on school choice from a developing country setting, our two-stage design also allows us to conduct the first experimental analysis of spillover effects of school choice programs on non-applicants, on lottery losers, and on private school students. More generally, our results highlight that it is essential for the school-choice literature to recognize that schools provide vectors of attributes and may be horizontally differentiated in their offerings. Specifically, our inference regarding the relative productivity of private and government schools would have been wrong if we had not accounted for school time use patterns and had not tested students in additional subjects on the basis of analyzing the school time use data. Similarly, evaluating school choice and charter school programs on a limited set of test scores (typically in math and reading) may provide an incomplete picture of the impact of such programs if they do not account for the full pattern of time use in these schools.

There are several policy implications of our results and these are especially timely given the passing of the RtE Act in India, and we discuss them in detail (along with caveats) in the concluding section of the paper. The rest of this paper is structured as follows: Section 2

[5] Lottery-based designs have been used to study school voucher programs (Angrist et al. 2002, 2006; Mayer et al. 2002; Krueger and Zhu 2004; Howell and Peterson 2004), the impact of more selective schools (Cullen et al 2005; Zhang 2009; Lucas and Mbiti 2012; Abdulkadiroglu et al 2012), and more recently charter school programs (Hoxby and Murarka 2009; Abdulkadiroglu et al 2011). It is important to note that the likely mechanisms of impact are different across these three types of programs. In particular studies that evaluate the impact of going to a "better" school (typically defined in terms of observed outcomes) are typically not evaluations of school choice. But these studies are still relevant to the school choice literature, because one of the key mechanisms by which school choice is posited to work is that students can transfer from low-performing to high-performing schools thereby contributing to an expansion of market-share of good schools and a reduction in that of weak schools. However, if the observed cross-sectional differences in outcomes between 'good' and 'poor' schools are mostly driven by selection and unobservables (as opposed to school effectiveness), then the empirical case for school choice is less compelling.

describes the AP School Choice Experiment (design, validity, and data collection); section 3 presents results on summary statistics of school, teacher, and household inputs into education; section 4 presents the main results, and section 5 discusses policy implications, caveats, and directions for future research.

2. The Andhra Pradesh (AP) School Choice Experiment

2.1 Background and Context

India has the largest school education system in the world comprising around 200 million children. Primary school enrollments have steadily increased over the past two decades and over 96% of primary-school aged children are now enrolled in school (ASER 2012). Nevertheless education quality is low with less than 40% of children aged 6 to 14 being able to read at the second grade level. The public education system in India is characterized both by inefficient choices of inputs, as well as inefficient use of resources conditional on the choice of inputs.[6]

A prominent trend in India has been that parents are enrolling their children in fee-charging private schools in increasing numbers. Over 28% of children between the ages of 6 and 14 in *rural* India attend private schools (ASER 2012), with the corresponding fraction in urban India being over 50% (Desai et al. 2009).[7] The majority of these private schools are low-cost or 'budget' private schools that cater to non-affluent sections of the population, and have per-student spending that is significantly lower than that in public schools (Tooley 2009). However, since private schools charge fees and public schools are free, students attending private schools on average come from more affluent households with higher levels of parental education (Muralidharan and Kremer 2008; also see Appendix Table 1). Cross-sectional evidence finds that students in private schools significantly outperform their counterparts in public schools, even after correcting for observable differences between the characteristics of students attending the two types of schools (Muralidharan and Kremer 2008; Desai et al. 2009; French and Kingdon 2010). Nevertheless, these studies cannot fully address selection and omitted variable concerns with respect to identifying the causal impact of attending a private school.

[6] As an example of inefficient choice of inputs, Muralidharan and Sundararaman (2013) show that locally-hired contract teachers are at least as effective as regular civil-service teachers in spite of the latter being paid five times higher salaries. The most striking evidence on inefficient use of inputs is perhaps the high rate of teacher absence, with around 25% of government-school teachers in rural India being absent when observed during unannounced visits to schools (Kremer et al. 2005; Muralidharan et al. 2013).

[7] The annual time-series data provided by the ASER reports show an increasing private school share in rural India (the urban trends are likely to be similar though there is no corresponding annual time-series available).

The growing popularity of private schools has led to concerns about increasing economic and social stratification in education, leading to calls for expanding access to private schools for all children, regardless of socioeconomic background – including experimenting with voucher-based school choice programs (Shah 2005). The recent Right to Education (RtE) Act passed by the Indian parliament includes a provision mandating that private schools reserve up to 25% of the seats in their school for students from disadvantaged backgrounds, with a reimbursement of fees by the government (subject to a maximum of the per-child spending in the public schools). While the specific implementation details have not yet been fully specified, the allocation of these places is likely to be based on a combination of location of residence and a lottery.[8] If implemented as per the letter of the law, this provision in the RtE Act could lead to India having the world's largest number of children attending private schools with public funding, and also be one of the largest attempts at school integration (across economic classes) that may have ever been attempted anywhere in the world.

2.2 Conceptual Overview of Experiment Design

Figure 1 (Panel A) shows the typical design used in experimental evaluations of voucher programs around the world. The key feature of this design is that a limited number of vouchers are offered that enable students currently enrolled in public schools to defray the costs of attending a private school. The program is typically oversubscribed and the limited slots are allocated by lottery. Such a program design creates four groups of students: those who do not apply for the voucher (group 1), those who apply and lose the lottery (group 2), those who apply and win the lottery (group 3) and those who were in private schools to begin with (group 4). The best studies to date on school choice estimate the impact of winning the lottery conditional on applying for it (i.e. they compare groups 3 and 2). The lottery allows researchers to estimate both the impact of winning the lottery (the 'intention to treat' effect) and the impact of attending a private school (using the lottery as an instrumental variable for attending a private school - the 'treatment on treated' estimate).

[8] The initial draft of the RtE Act that was distributed for comments in 2005 (on the basis of which this study was designed) envisaged an allocation mechanism based purely on a lottery. The final draft that was passed in 2009 introduced residential location as a criterion for the allocation of places in private schools under the "Economically Weaker Sections (EWS)" category. The specific rules under which the 25% reservation provision will be implemented have been left up to individual states to determine, and while there is uncertainty with respect to the final allocation rules that will be adopted, it is likely to involve a combination of residential location and a lottery.

However, even an experimental design of this sort (while better than the alternatives) ignores the potential spillover effects of the voucher program on the losers of the lottery. Thus, the departure of group 3 students may have additional effects on students in group 2. Some possible mechanisms include changing of the peer group (because motivated students may have left),[9] changes in per-student resources (for instance, class sizes may be smaller after some students leave because the teacher allocation is not proportionately reduced), and changes in behavior by public school teachers in response to the voucher program (such as a competitive response to improve quality and keep children from leaving the government schools). These confounding factors may bias a simple comparison between groups 2 and 3. In other words, the "control" group even in experimental studies is not truly a "business as usual" control group because of potentially unobserved spillover effects, and even the internal validity of the estimates from the literature to date can be questioned on this basis.

Moreover, existing studies typically cannot estimate the program's impact on students in group 1 (who did not apply for the voucher and who are subject to similar spillovers as group 2) or students in group 4 (who may be worse off because of an influx of low-performing students from public schools). Thus, even if group 3 is doing better than group 2 (which is what the traditional experimental studies focus on), this may have come at the cost of poorer performance for groups 1 and 4. Thus, a critical open question in the global literature on vouchers and school choice is that of the "aggregate impact" of such programs (Hsieh and Urquiola 2005).

The AP School Choice Experiment aims to address both these issues by employing a two-stage randomization design, where we first use a lottery to assign entire *villages* into control and treatment groups, and then conduct a second lottery to assign vouchers to applicants in the treatment villages. Figure 1 (Panel B) presents the conceptual overview of the experiment design. The key innovation in this design is that the control villages provide a 'system-level' counterfactual to the voucher program and hence provide the kind of control group that has not typically been found in the literature. Since villages are randomized into treatment and control status *after* baseline tests are conducted and after parents apply for the voucher, comparing the recipients of the voucher (3T) with applicants in control *villages* (2C) will provide an experimental estimate of the impact of the choice program *without being contaminated by the*

[9] While the vouchers are offered by lottery, not all winners will typically accept it and move to a private school. It is possible that the most motivated students may be the ones who accept the voucher to go to a private school.

spillovers. In other words, group 2C represents the "true" control group because they have applied for the voucher and lost the lottery (at the village level), but nothing else has changed for them because there is no voucher programs in these villages.

The design also lets us to do 3 additional comparisons, which have not been possible in the literature to date. First, comparing groups 2T ('control' students with spillovers) and 2C ('control' students without spillovers), will provide a sense of the extent to which ignoring spillovers may bias the estimates existing studies. Second, the comparison between groups 1T and 1C will let us estimate the impact of school choice programs on the children 'left behind' (who for reasons of limited information or motivation choose to not apply for the voucher). Third and finally, comparing outcomes between groups 4T and 4C will provide an estimate of whether students in private schools are adversely affected by an influx of students from the government school (which is exactly what will happen if the provision in the RtE Act regarding reserving 25% of places in private schools for disadvantaged students is implemented).

2.3 The AP School Choice Experiment

Andhra Pradesh (AP) is the 5th most populous state in India, with a population of over 80 million (70% rural). Recent estimates suggest that over 35% of students in rural AP are enrolled in private schools (ASER 2012), compared to an all India average of 28%. The Andhra Pradesh School Choice Project (that this paper is based on) was implemented by the Azim Premji Foundation (one of India's leading non-profits working on education).[10] The academic year in AP runs from mid-June to mid-April. The AP School Choice project started in the academic year 2008-09, with preparatory work starting in early 2008.

The project was carried out in five districts across AP over a universe of 180 villages that had at least one recognized private school.[11] Baseline tests were conducted for *all* students in 2 cohorts of *all schools* (public and private) in these villages in March-April 2008.[12] This was

[10] The AP School Choice Project was carried out under the larger program of the "Andhra Pradesh Randomized Evaluation Studies (AP RESt)" which was set up as a research partnership between the Government of Andhra Pradesh, the Azim Premji Foundation, and the World Bank.

[11] These were the same districts as in the overall AP RESt project (Muralidharan and Sundararaman 2010, 2011, 2013), but the AP School Choice Project was conducted in different sub-districts and so there was no overlap in the schools/villages across these studies.

[12] The cohorts covered were students attending kindergarten and grade 1 in the previous school year (2007-08), and the voucher covered the entire primary education of recipients from the school year 2008-09 (from grade 1 to 5 for the younger cohort and from grade 2 to 5 for the older cohort). Baseline tests were conducted in math and Telugu (native language of AP) for the older cohort and in Telugu for the younger cohort.

followed by an invitation to apply for a voucher to parents of students in government schools (who had taken the baseline test) in all 180 villages. The application specified the full terms of the voucher including the fact that it would be allocated by lottery and that applying did not guarantee receipt of the voucher. The communication regarding the voucher program and the application process was done by field staff of the Azim Premji Foundation during the summer break in May 2008.

Participation of both households and schools was completely voluntary. Households were told that they could go back to the public school at any time and there were no terms and conditions for participation beyond consent for answering surveys and taking tests. The voucher covered all school fees, textbooks, workbooks, notebooks and stationery, and school uniforms and shoes, but did not cover transport costs to attend a private school outside the village and did not provide any allowance in lieu of the free mid-day meals that the government schools provide. The value of the voucher was paid directly to the school, and the materials were provided directly to the voucher households by the schools.[13]

At the same time as the baseline tests, the Azim Premji Foundation (the Foundation) also invited participation in the project from private schools in the sample villages, and school participation was completely voluntary. The value of the voucher was set at the 90th percentile of the distribution of the all-inclusive private school fees in the sampled villages, and schools were asked to indicate if (a) they wanted to participate in the program by being willing to admit economically disadvantaged students who would be awarded a voucher by the Foundation, and (b) if so, how many seats they could make available to voucher students in each of the two cohorts.[14] The terms and conditions specified that the Foundation would directly pay the value of the voucher to the school's bank account (in three annual installments – which was the typical fee cycle of the schools). The only condition imposed on the schools was that they were not allowed to select students. If there was greater demand for a school than the number of places

[13] This was consistent with standard practice we observed in the field. The private schools had a recommended set of books, uniforms etc. which they procured in bulk and supplied to parents for a fixed fee. It was therefore easiest to have the voucher cover these payments directly as opposed to making cash payments to parents for other incidental education expenses.

[14] At the time of starting the project, the 2005 draft of the Right to Education (RtE) Act was already in circulation and private schools knew that the stipulation regarding reserving 25% of seats for economically disadvantaged children was likely to be implemented. Thus, the communications to schools regarding the project was along the lines that this was a pilot project being done by the Foundation to help the Government of AP understand the impacts and implications of implementing this clause in private schools.

offered, then the school could either admit all voucher recipients who wanted to attend the concerned school or the Foundation would conduct a lottery to allocate the places among the applicants (this is similar to the admission protocols of most charter school programs in the US).

All communications with schools (and elicitation of willingness to participate) was conducted before the village-level randomization took place.[15] Once the applications were completed, 90 villages (stratified by district) were assigned by lottery to be voucher villages (Figure 1 - Panel A), while the other 90 villages continued "as usual" with no voucher program (Figure 1 – Panel B). Conditional on being a "voucher village", a second lottery was conducted to offer the vouchers to a subset of applicants. The design therefore creates two lottery-based comparison groups – those who did not get the voucher due to their village not being selected for the program (group 2C in Figure 1), and those who did not get the voucher due to losing the individual level lottery conducted within voucher villages (group 2T in Figure 1).

Out of 10,935 eligible households, a total of 6,433 households applied for the voucher (59%). A total of 3,097 households had applied in the treatment villages, from which 1,980 were selected by lottery to receive the voucher (64%). 1,210 of these 1,980 households accepted the voucher and enrolled in a private school at the start of the project (61%). At the end of four years of the project, a total of 1,005 students continued to avail of the voucher. Figure 2 shows the program design with the actual number of students in each of the cells.

Appendix Table 2 shows that application for the voucher and acceptance conditional on being awarded one are not correlated with observable demographic characteristics like parental assets, education, or caste (Table A2). The only observables that are correlated with application are having a sibling in the government school (negative) and having a private school within a radius of half a kilometer (positive), which are as expected. The same patterns are observed in acceptance conditional on being awarded the voucher. Thus, while it is possible that the decision to apply and/or to accept may be driven by unobserved household characteristics, we do not see any correlation between household socio-economic characteristics and voucher application or acceptance.

[15] The initial frame for the project was 200 villages, which was reduced to 180 after dropping villages where there was no private school willing to participate, or where the private schools did not obtain recognition at the start of the 2008-09 school year (the sample initially included villages with unrecognized schools that said that they were in the process of getting recognized, but villages where there was no school that had obtained recognition were dropped from the study universe). This was done because the Foundation did not want to put voucher winning children in a situation where the school they went to would be shut down by the government (as the law entitles them to do).

The allocation of villages and students to the voucher program by lottery ensured that the treatment groups and the corresponding comparison groups are not significantly different on observable characteristics including baseline test scores, parental education, assets, and caste. Table 1 - Panel A shows the balance between lottery winners and losers – first showing the comparison with lottery losers in the treatment villages and then showing it with lottery losers in control villages. Panel B shows the balance for the groups of students who will be used for the spillover analysis – first showing the comparison between non-applicants across treatment and control villages, and then showing it between students who start out in private schools across these villages.

2.4. Data and Attrition

We collect a rich set of data on school and teacher characteristics through school and teacher surveys. Enumerators also conducted several unannounced visits to schools during the four years of the project and measured school processes such as teacher absence and activity, classroom practices and processes, and school hygiene. They also conducted household surveys to obtain data on household inputs into education – including expenditure as well as student time-use data. The school surveys were carried out once a year in all the schools in the 180 project villages, while the household surveys were carried out in a representative sample of households each year from all the four groups of students as indicated in Figure 1.

Data on learning outcomes was collected through independent student tests conducted at the end of two and four years of the project. Tests in Telugu (native language of AP and the medium of instruction in public schools), Math, and English, were conducted at the end of two and four years, while additional tests in Science, Social Studies and Hindi were administered at the end of four years. All subjects except Hindi were administered as written tests, whereas the Hindi tests were administered individually to students by enumerators. We attempted to administer the written tests to the full sample of students as identified in Figure 2 (the full set of students who had applied for the voucher, and a representative sample of students who had either not applied or who were in the private schools at the start of the project). The Hindi tests were administered to a representative sample of the students who applied for the voucher. We verify that the samples are balanced across treatment and control groups for all key observables in all cases where students were sampled for surveys or testing (tables available on request).

Field enumerators made extensive efforts to keep track of all children who were in the frame of the study at the beginning, but some attrition was unavoidable.[16] The two year attrition rate was 10% and 15% in the treatment and control groups respectively, and the four-year attrition rate was 15% and 19% in the two groups (Table 2 – Panel A; columns 4, 5, 10, and 11).[17] These differences are statistically significant (columns 6 and 12), but we find no difference in observable characteristics between the attritors across the treatment categories. We also estimate a model of the probability of attrition from the sample using a rich set of observable characteristics collected before the lottery (including baseline test scores, and household socioeconomic indicators) and cannot reject the null that the same model predicts attrition in both the treatment and control samples. Given the balance of attrition on all observable characteristics (both individually and jointly) it is likely that the estimation sample is not imbalanced on unobservables that may be correlated with test score gains over the period of the study. Nevertheless, we test our results for robustness using both inverse probability-weighting as well as bounding (Lee 2009). The overall attrition rates in the sample that is used to test for spillovers are around 33% (Table 2 – Panel B), but the differences between treatment and control students are not significant.[18]

3. Results – School, Teacher and Household Inputs

3.1 School and Teacher Inputs

Table 4 (Panel A) presents key summary statistics on private schools in our sample (using data from only the control villages to ensure that the descriptive statistics represent 'business as

[16] The initial tests conducted at the end of two years of the project were conducted in schools, but we had not just high rates of attrition (over 40%), but also had a high-level of differential attrition (with the difference in attrition rates between treatment and control groups being high enough (around 20%) to render the sample almost useless for estimating the impact of the voucher program. This was followed by an intense effort by enumerators to track down all the students who had applied for the voucher and the conducting of an additional round of testing in each village. This was conducted in November 2010 (around a third of the way into the third year of the program), and so the test score results corresponding to "two years" as described in the text are based on tests conducted around two and a half years into the program.

[17] Note that the main treatment effects will be calculated with respect to the lottery-losers in the control villages. Columns 1-3 and 7-9 present the attrition rates relative to the lottery-losers in the treatment villages

[18] There is a significant difference in baseline Telugu scores between treatment and control groups in the sample used for estimating spillover effects on students in the treatment villages, who start out in the private schools, but this is one of 10 comparisons presented in columns 1-6, and none of the other differences is significant. We also control for baseline test scores in all our estimates of program impact.

usual' differences and are not affected by the treatment).[19] On average, private schools in our sample are considerably larger than their government-run counterparts. They also have a longer school year (2 working weeks or 11 days longer per year), and have considerably lower pupil-teacher ratios (around a third lower) than government schools. They are also more likely to have drinking water, functional toilets (as well as separate toilets for girls), functional electricity, and to have a computer, with the differences being quite stark on some of these measures. Government schools are more likely to have a functioning library and radio.[20]

Government school teachers are more likely to be male, are considerably older, have more years of teaching experience, are more likely to have completed a college degree, and are much more likely to have completed a teacher training course (Table 3 - Panel B). However, they are less likely to be from the same village as the schools that they are assigned to, and are paid six times higher salaries. This calculation understates the differences in total pay, because it does not include the discounted value of the pension and other retirement benefits that government civil service teachers obtain that are typically not available to private school teachers.

The total spending per-child spending in the government schools is over 3.5 times the mean per-child spending in the private schools in our sample (Table 3 - Panel C).[21] As the discussion above makes clear, the main driver of these differences in costs is the much higher salaries paid to government school teachers. However, private schools hire more teachers per student, and also have better infrastructure, as a result of which the differences in per-child expenditure are not as stark as the differences in teacher salaries.

[19] There are no significant differences in mean private school characteristics across treatment and control villages, but we use only the control villages for the purposes of the summary statistics. We verify that being in treatment villages does not change the average of several key school characteristics between treatment and control villages over the course of the study (results available on request). In other words, it appears as if schools used the additional resources provided by the voucher payments to either keep overall enrollments constant (by accepting voucher recipients instead of other students) or by hiring enough staff so that their average characteristics (such as class size) did not change on average. More broadly, since this was a one-off experiment that was not repeated for later cohorts, we do not expect to see a significant supply-side response from private schools.

[20] The libraries referred to here are typically not separate rooms dedicated to being libraries, but are more typically a collection of books kept in a cupboard that students can use. The large prevalence of radios reflects a policy to facilitate distance education in public schools, through the distribution of radios to schools.

[21] Note that since salary expenditures are not reported at the school level, we compute average per-child spending in public schools from reports of official spending (Dongre 2012). For the private schools, we collect detailed data on both income and expenditure, and report the mean total income of the schools in Table 3 – Panel C. The expenditure figures are typically lower than the income because schools did not impute the rental value of their infrastructure when they owned their premises. The income figures (which include donations and other philanthropic grants) therefore represent an upper bound on per-child expenditure in the private schools.

In addition to reporting on measures of school and teacher quality based on their characteristics, we also measure school quality on the basis of direct observations of schools and teachers conducted during unannounced visits to the schools during the four years of the project (a representative sample of schools and teachers were observed each year). Private schools significantly outperform government schools on all measures of observed classroom processes (Table 4 – Panel A). Classrooms in private schools are significantly more likely to be engaging in active teaching (51% vs. 34%), have a greater likelihood of a teacher being in the classroom (97% vs. 92%), and are much less likely to be multi-grade classrooms where more than one grade is taught simultaneously by the same teacher (24% vs. 79%). Moreover, enumerators observed teachers in private schools as being more likely to be in complete control of the class (69% vs. 41%) and as more effective in teaching and maintaining discipline (50% vs. 36%).

We find from observations at the teacher level (Table 4 – Panel B) that government school teachers were considerably more likely to be absent than private school teachers (24% versus 9%) and less likely to have been actively teaching at the point of observation (35% versus 50%).[22] Finally, enumerators also coded measures of school hygiene based on their observations when they entered the schools and we find that private schools are less likely to have indicators of poor hygiene such as having garbage dumped on the school premises, having stagnant water (breeding ground for mosquitos), or having a heavy presence of flies on the school premises (the most common carrier of pathogens from open human and animal waste).

3.2 Household Inputs

In addition to school-level factors, receipt of a voucher may also change household inputs into education (Das et al. 2012; Pop-Eleches and Urquiola 2012). We collect data on time use as well as household expenditure on education from a representative sample of students, and compare these across treatment and control households. Columns 1 and 2 of Table 5 present the average patterns of child time use and household education expenditure among children attending private and government schools. Columns 4 and 5 present means of these same metrics for students who were awarded the voucher and those who were not. Column 6 presents the estimate of the intent-to-treat (ITT) effect of receiving a voucher on time use and

[22] The discrepancy between the difference in teacher absence rates (15 percentage points) and the difference in the probability that a classroom does not have a teacher (5 percentage points) is partly explained by the fact that the most common response to teacher absence in government schools is to combine grades and have all students taught by the same teacher (as seen in the much higher rate of multi-grade teaching in government schools).

expenditure, while column 7 presents the estimate of average treatment-on-treated (ToT). Comparing columns 7 and 3 provides a measure of the extent to which time use and household expenditure patterns of voucher receiving students have converged to the typical patterns of students attending private schools.

The typical private school student spends 43 minutes more per day in school, and an additional 23 minutes per day on studying and doing homework at home (Table 6, Panel A, Columns 1-3), which adds up to over an hour of extra school and study time per day and over 250 hours per year. Comparing columns 3 and 7, we see that the voucher receiving students who attend a private school have completely caught up with the typical private school student in terms of time spent in school. However, a striking result is that they do not appear to have caught up in terms of time spent studying and doing homework at home, suggesting that attending a private school did not have an impact on changing study habits at home. It is also worth noting that the typical private school student spends 20 minutes less per day playing with friends, while there is no reduction in time spent playing with friends for the voucher winners.

The extra time needed to attend the private school (45 minutes a day) mainly comes from a reduction in household chores comprising of helping with cooking and caring for children and elderly members of the household (23 minutes) and smaller reductions in free time, working outside the home, and watching TV (10, 7, and 6 minutes each). The last three are not significant due to the smaller sample sizes, but the point estimates in columns 7 and 3 are similar. Overall, around two thirds of the 'cost' of the extra time spent in school seems to have been borne by parents (30 minutes of reduced time on chores and work outside the home), while the remaining one third was borne by the student (15 minutes less of watching TV and free time).

Households of children attending private schools spend over five times as much money on their child's education (Table 7, Panel B, columns 1-3), which is to be expected given that the private schools charge fees and require additional expenditures on textbooks and uniforms, while the government schools are free, provide free textbooks, and do not require uniforms. However, the households of voucher winning children spend slightly less on the education (of the winning child) relative to those in the control group (column 7), which is consistent with the fact that the voucher pays for school fees, books, and uniforms/shoes.

In summary, household expenditure on education is slightly lower for voucher winning children, and we find no evidence of a change in home study habits of the voucher winners.

However, the average time spent in school does go up for voucher winners. These results suggest that the main mechanism of any impact on test scores is likely to be due to changes in school-level inputs as opposed to increases in household inputs.

4. Results – Test Scores

4.1 Impact of winning a voucher and attending a private school

Our main estimating equation for the impact of receiving the voucher takes the form:

$$T_{isv}(Y_n) = \beta_0 + \beta_1 \cdot T_{isv}(Y_0) + \beta_2 \cdot Voucher_i + Z_i + \varepsilon_{isv} \qquad (1)$$

where $T_{isv}(Y_n)$ represents normalized test scores for student *i* in subject *s* in village *v*, at the end of *n* years of the experiment. Since test scores are highly correlated over time, we control for baseline test scores to increase the precision of our estimates.[23] We also include a set of district fixed effects (Z_i) to absorb geographic variation and increase efficiency, and to account for the stratification of the village-level lottery at the district level. The main estimate of interest is β_2, which provides an unbiased estimate of the impact of winning a voucher on test scores (the 'intent to treat' estimate) since the voucher was assigned by lottery.

As described in section 2, a key feature of our design is the ability to estimate the impact of winning the voucher relative to the control group in control villages. The estimation sample therefore includes the applicants who won the voucher lottery, and applicants whose villages were not selected (by lottery) to receive the voucher. The estimation sample does not include the applicants who lost the lottery but were in treatment villages (we use this sample later when analyzing spillover effects). Test scores are normalized relative to the distribution of the government-school students in the control villages on each test, since these students represent the 'business as usual' distribution of test scores. Standard errors are clustered at the village level to account for common shocks to test scores that may occur at the village level.

We estimate the impact of attending a private school using the offer of a voucher as an instrumental variable for attending a private school, where the second stage equation is:

$$T_{isv}(Y_n) = \beta_0 + \beta_1 \cdot T_{isv}(Y_0) + \beta_2 \cdot Private\_School_i + Z_i + \varepsilon_{isv} \qquad (2)$$

and the endogenous regressor $Private\_School_i$ is instrumented for with the first-stage equation:

$$Private\_School_i = \gamma_0 + \gamma_1 \cdot Voucher_i + Z_i + \varepsilon_{isv} \qquad (3)$$

[23] The default baseline score that we control for is the score on the same subject, but in cases where no baseline test was conducted in the same subject, we control for the mean normalized test score across all subjects for which a baseline test was available (which provides a measure of baseline ability).

These results are presented in Table 6 for test scores at the end of 2 years and 4 years of the program, with Panel A showing the impact of being awarded a voucher and Panel B showing the impact of attending a private school. Since there is no obvious way to weight the outcomes across subjects, we treat each subject as a separate outcome and report the results for each subject separately. But we also present results for the average program impact across subjects to provide a sense of the overall program impact.[24]

At the end of two and four years of the program, we find that voucher winners had slightly lower scores on Telugu and Math than lottery losers (all four point estimates are negative but none of them are significant – columns 1, 2, 5, and 6). They have higher scores in English (though significant only in the 2-year estimates – columns 3 and 7). The average program impact across the three subjects that were assessed at the end of 2-years was close to zero (Table 6 – Column 4). These results might seem surprising given the findings that private schools appear to perform better than government schools on several measures of process – including having a longer school year and school day, substantially lower pupil-teacher ratios, and higher levels of teacher attendance and effort. On the other hand, the teachers in the private schools are less likely to have a college degree or a teacher training credential. They are also less experienced, and paid much lower wages. So it is possible that these factors offset each other and produce a net effect of close to zero. Overall, these results would suggest that the cross-sectional differences in test scores shown in Appendix Table 1 are mostly due to omitted variables and not due to differential effectiveness of public and private schools.

However, in addition to facilities, teachers, and teacher activity levels, a key determinant of education outcomes is instructional time, and in particular the allocation of instructional time across different subjects.[25] We present data from school time tables in Table 7, and see that private schools have sharply different patterns of time allocation than government schools. In particular, they allocate a *lot less time per week* to Telugu and Math, which are the two main subjects taught in the government schools - accounting for over 500 minutes/week and around 28% of total instructional time each. Private schools spend around 200 minutes less on Telugu

[24] This procedure is similar to that of Kling, Liebman, and Katz (2007) for the analysis of programs with multiple outcomes. Implementing the procedure is straight-forward in our case, because treatment effects for each subject are already calculated and reported in normalized terms. Another recent application of the same approach in the context of a field experiment in a developing country with multiple outcomes is Olken, Onishi, and Wong (2012).
[25] We thank Mark Jacobsen for this comment while discussing the two-year results, which prompted us to collect and analyze school time table data, and test additional subjects at the end of Year 4 based on the time table data.

and 160 minutes less on Math per week (40% and 32% less instructional time respectively). On the other hand, they spend significantly more time on other subjects such as English (~90 minutes/week), Social Studies (~65 minutes/week), Science (~100 minutes/week), Hindi (~215 minutes/week), and Computer use (~45 minutes/week). They also spend an hour/week more on "other" periods which include arts, crafts, sports, and study hall. Overall, we see that the three subjects that were tested at the end of two years of the program account for 70% of the instruction time in the government school curriculum, but account for less than 50% of that in the private schools.

Thus, limiting our analysis to these subjects may not provide a complete picture of the impact of the voucher. Based on the time table data, we conducted additional tests in Science/Social Studies (EVS) and Hindi.[26] While this still does not account for all the subjects (computer use for instance), the tested subjects now account for over 80% of instructional time in both types of subjects and are also closer to being equal across school types (81% for private and 85% for public schools). The full set of test score results are presented in columns Table 7 – columns 5 to 10, and we see that voucher winning students score slightly better on EVS (though this is not significant). The most striking result though is that they do dramatically better in Hindi – scoring over 0.5 standard deviations better than students who did not win the voucher, and the impact on Hindi scores of actually attending a private school is even more pronounced with students who attend private schools scoring nearly 0.9 standard deviations (SD) better. Averaging across all subjects, students who won a voucher score 0.13 SD better than those who did not, and the causal impact of attending a private school is estimated as 0.23 SD (column 10), and both estimates are significant at the 1% level.

Since the overall program effects are mainly driven by gains in Hindi, and since government schools do not teach Hindi we analyze the Hindi results in more detail at the individual question level (by skill) to better understand what the program impact means in terms of actual ability to

[26] Under the government syllabus for primary schools, science and social studies are taught jointly under the subject title of "environmental studies" (EVS). The tests we conduct follow the curriculum and are therefore analyzed and reported jointly. The EVS tests were administered in a standard written format. Hindi is not taught in the government schools, and so we could not administer a written test (which would result in more children being coded as scoring zero in Hindi relative to their true level of competence). Enumerators therefore administered individual oral tests to a representative sample of the universe of voucher applicants (which was balanced between treatment and control categories on all observables). The test follows the same format as that administered by the non-profit Pratham in their annual surveys of learning levels implemented across India and published in the ASER reports (Pratham 2012) and is therefore comparable with a benchmark measure of competence that has been widely used in India in the recent past.

use Hindi. We present these results in Appendix Table 3, and see that attending a private school more than doubles the probability of students reading letters correctly, and more than triples the probability of being able to read words, sentences, and paragraphs.

While we follow Kling, Katz, and Liebman (2007) in reporting mean normalized test score impacts across multiple subjects and find a positive and significant impact of the program on this composite score, it is not clear that the subjects should be weighted equally. Nevertheless, the results unambiguously indicate that the private schools are more productive, because they are able to deliver similar outcomes in Telugu and Math with considerably less instructional time, and are able to use the extra time to improve test scores in other subjects and especially Hindi.

4.2 Robustness to attrition

The main threat to the results above is from the differential attrition noted in Table 3. As discussed in section 2.4, we verify that our results are robust to this concern using two different procedures. In Table 8 – Panel A, we report the ITT effects of winning a voucher using inverse probability reweighting to account for the differential probability of attrition based on observables, and see that doing so barely changes the estimated effects presented in Table 8 – Panel A. As we will see in the next section, there is very limited evidence of heterogeneous treatment effects by baseline student characteristics, and it is therefore not surprising that inverse probability reweighting does not change the main estimates.

A more conservative approach to the differential attrition rates between treatment and control groups is to compute bounds based on Lee (2009). We calculate these bounds and show the widened 95% confidence intervals as a result of the procedure in Table 9 – Panel B. The results are all robust to implementing these bounds – the point estimates of the impact on Math, Telugu, English, and EVS continue to be insignificant, and the estimated impact on Hindi is large enough that its significance is not affected by the using the more conservative confidence intervals implied by the Lee (2009) bounds. The overall ITT estimate (averaged across subjects – column 10) also continues to be significant (though at the 5% and not at the 1% level as in Table 6).

4.3 Heterogeneous effects

4.3.1 Heterogeneous effects by student characteristics

We test for heterogeneity of the impact of the voucher program along several student characteristics including baseline scores, gender, caste, parental literacy and affluence, age, and religion, using a standard linear interaction specification of the form:

$$T_{isv}(Y_n) = \beta_0 + \beta_1 \cdot T_{isv}(Y_0) + \beta_2 \cdot Voucher_i + \beta_3 \cdot Characteristic_i + \beta_4 \cdot Voucher_i \cdot Characteristic_i + Z_i + \varepsilon_{isv} \quad (4)$$

where the parameter of interest is β_4 which estimates the extent to which the impact of the vouchers is different for students with the concerned characteristic.

Table 9 reports β_4 from estimating (4) over two and four years over various characteristics and the main result is the lack of any consistent evidence of heterogeneous effects along most student characteristics. In particular, the baseline score can be treated as a summary statistic of educational inputs that students had received up to the point when they enter the study, and the lack of any differential treatment effects by baseline score suggests that the impacts of the program were broad based. The one demographic group that seems to benefit significantly more from the voucher program is Muslim students, who are one of the most educationally disadvantaged groups in India (Sachar Committee Report 2006).[27] These results are consistent with those found in the US by Peterson and Howell (2002) who report that educationally-disadvantaged groups gain the most from school choice programs.

4.3.2 Heterogeneous effects by market characteristics

The market-level experimental design allows us to study a key question in the school-choice literature, which is whether students who have greater choice among schools have superior education outcomes (Hoxby 2000). We use the distance data described above to calculate the number of private schools within a 0.5 kilometer radius and within a 1 kilometer radius of each student. Our measure of choice and competition is constructed separately for each student, and can therefore generate variation at the student-level even when two students might live in the same village. Since there is no obvious functional form between the number of schools in a choice set and outcomes, we estimate this relationship both parametrically, and non-parametrically. For the first, we use a linear interaction of voucher receipt and the number of schools in the choice set (in both levels and logs) in a specification similar to (4). For the second, we estimate equation 4 with the 'characteristic' being whether the number of schools a student has in her choice set is in the top 25%, top 10%, or top 5% of the distribution of the

[27] Since we are testing heterogeneity across several covariates in Table 10, we need to be cautious in inferring heterogeneity since significant results could simply be reflecting sampling variation. However, we can be more confident in the inference that Muslim students benefit more from the vouchers because we see significant positive effects for Muslim students in both the two-year as well as the four-year data and this is seen for every subject at the two-year point, and three out of five subjects after four years.

number of schools (with the absolute number of schools varying based on the distance that the choice set is defined within).

These results are presented in Table 10, and we find no significant effect of competition when estimated with a linear interaction between voucher receipt and the number of schools in a student's choice set within a half and one kilometer (in both levels and logs). However, while conducting the study in a rural sample allows us to study spillovers (see next section), a limitation is that over 60% of voucher applicants have only 0 (40%) or 1 (21%) private school within a half kilometer radius and nearly 50% have only 0 (27%) or 1 (21%) private school within a kilometer radius. Thus, the extent of choice and competition between private schools is quite limited for many of the voucher applicants.

The non-parametric estimates might therefore be more appropriate in this context, and they provide some suggestive evidence of the benefits of greater choice and competition, since we find that voucher winners do significantly better when there are four or more schools within a half kilometer distance from their homes or when there are six or more schools within a one kilometer radius. We find evidence of larger impacts in areas with more choice and competition in both the two-year and the four-year results suggesting that the heterogeneity is likely to be real and does not just reflect sampling variation.

However, while suggestive, these are not very robust, and the rural setting may not be the best one to study the effects of competition. Urban India however has much greater population and school density and a recent census of schools (with geo-coding) in the city of Patna[28] found that there are between 9 and 93 private schools within a one kilometer radius of every government school, with the median being greater than 50 (Rangaraju et al 2012). Our results therefore suggest that the effects of choice and competition may be considerably larger in such a context. This is an important area for future research.

4.4 Estimating Spillover Effects

An important concern in the global school choice literature is that positive estimated effects of vouchers from experimental studies may be overstating the benefits of private schools because these estimates do not account for potential negative spillovers to students in the public schools who do not apply for the voucher or for potential negative spillovers on the students who start in

[28] Patna is the capital of the state of Bihar with a population of 1.7 million and population density comparable to other large cities in India.

the private schools, and who are exposed to lower-scoring peers from public schools as a result of the voucher program (Hsieh and Urquiola 2005). Much of the protest from existing elite private schools against the 25% reservation provision in the Right to Education Act in India has also been based on this concern.[29]

The principal motivation for the two-stage design of the AP School Choice Experiment was to estimate these spillovers. We calculate three different sets of spillovers as described in section 2.2, and the estimating equations all take the same form as (4). The right-hand side variable of interest in each case is an indicator for being in a treatment *village* and the estimation sample comprises the concerned group (lottery losers, non-applicants, and students attending private schools before the school choice program) from both treatment and control villages. The village-level lottery ensures that we obtain unbiased reduced form estimates of these three spillovers.

Table 11 - Panel A compares the within-village control group to the across-village control group. Note that the former is the 'traditional' control group used in typical experimental studies of school choice (the lottery losers in the treatment villages) and that this sample has not been used so far in any of the analysis due to the possibility of spillovers as discussed in section 2.2. We find no difference whatsoever between the groups and the combined effects across subjects are not only insignificant, but close to zero.[30] Panel B estimates if there were any spillovers on non-applicants and we again find no significant effects on either individual subjects or on the aggregate test scores across subjects. Thus, even though the literature has often worried about the possibility of negative spillovers on students who are 'left behind' in public schools in response to voucher programs, these spillovers were not empirically salient in our setting.[31]

In the Indian context, a greater concern has been the possibility that the Right to Education Act clause on quotas in private schools would lead to negative spillovers on the students who start out in the private schools. We estimate these spillovers in Panel C and find that there are no significant negative spillovers on the students who were in private schools to begin with. We

[29] See Shah (2012) in the *New York Times* for an example.
[30] Our not finding any significant spillovers here suggests that the 'typical' control group that would have been created if we did not have a two-stage experiment would have also provided an unbiased estimate of the impact of the voucher program. We present these results in Appendix Table 4 and as expected, we find the same results as in Table 6, though the treatment effects on English are now significant.
[31] Of course, the estimated 'non-effect' is a reduced form estimate that combines factors which could potentially hurt the students left behind (loss of motivated peers) as well as those that could help them (smaller class sizes and potentially teachers becoming more responsive in the face of competition). We do not have enough power to explore these channels with adequate precision, but we do provide the first experimental reduced form estimates of these spillovers.

explore the possibility of negative spillovers further by estimating if the spillovers are a function of the *number* of voucher receiving students who join a particular private school. Since this is a choice variable, we construct an instrumental variable estimate using the number of voucher-winning students for whom a given school is the nearest private school as an instrument for the actual number of students who move into the school. We again find no significant impact on the test scores of students who started out in private schools (Appendix Table 5).

Taken together, our results suggest that while spillovers are an important theoretical concern in the school choice literature, they do not appear to be a first-order issue empirically in our context.[32] Note that our results do not imply that peer effects and sorting do not matter in the context of school choice. Rather they suggest that these may not be first-order concerns for lottery-based studies of school choice and for school choice programs that do not allow for private schools to select their applicants.[33]

4.5 Cost Effectiveness

The combination of test score results (Table 6) and school time table data (Table 7) already show that private schools are more productive than public schools because they are able to produce similar levels of test scores in math and Telugu using substantially less instructional time and use the extra time to produce higher test scores in other subjects - especially Hindi. The results in Table 11 suggest that private schools may be even more productive when students attending them are not experiencing the disruption of switching their medium of instruction. Finally, it is worth highlighting that the average cost per-student in the private schools in our sample is less than one-third of the per-student costs in the public schools (Table 3C) and that the value of the voucher was only around 40% of the per-student costs in the public schools. Thus,

[32] It is likely that the extensive media attention paid to this issue reflects the concerns of a very small number of elite private schools in New Delhi and other large metropolitan cities, and it is possible that adverse spillovers may be a legitimate concern for these schools. However, our estimates based on a representative sample of rural private schools in a large Indian state suggest that the spillovers to students in private schools is unlikely to be significant on average. Note that Angrist and Lang (2004) similarly find negligible evidence of peer effects in the US from the school desegregation conducted under the Boston Metco Program.

[33] Macleod and Urquiola 2012 develop a model of school choice under different selection regimes and show that many of the potential gains of choice and competition may not materialize in systems where private schools are allowed to select students, while also showing that choice and competition will typically improve outcomes if private schools are *not* allowed to select their students.

private schools produce better academic outcomes at lower cost and are unambiguously both more productive and cost-effective than public schools in India.[34]

5. Discussion and Conclusion

We present evidence from the first experimental evaluation of the impact of a school choice program and the first experimental evidence on the relative effectiveness of private and public schools in a low-income country. The two-stage experimental design allows us to not only study the impact of receiving a voucher and attending a private school, but also allows us to estimate spillovers on non-applicants and students in private schools. In addition to being directly relevant to current policy debates in India, our design and results also speak to important issues in the global literature on the effectiveness of private, and charter schools; and on the impacts of school choice more broadly.

Our results on private school productivity suggest that it may be possible to substantially increase human capital formation in developing countries like India by making more use of private provision in the delivery of education. The differences in productivity by type of school management are consistent with the evidence in Bloom and Van Reenen (2010) documenting that "government-owned firms are typically managed extremely badly" and that developing countries typically have lower management quality across the board. The costs of low productivity in education may be especially high in low-income settings where low levels of human capital are likely to be barriers both to economic growth and to the inclusiveness of growth, and where fiscal constraints limit the total spending on education.

Our results showing no significant spillovers on private-school students from receiving voucher recipients from government schools suggest that it may be possible to achieve greater levels of social integration in private schools, as envisaged by the RtE Act, without the efficiency costs that opponents of the integration are concerned about. While the point estimates are clearly most relevant to the Indian context, they are consistent with similar findings showing low academic costs to advantaged students from school integration policies in the US (Angrist and Lang 2004).

[34] A similar result is found in Kenya by Bold et al. (2011) who cannot fully account for selection, but infer the effects of private schools using district-level aggregate data.

Finally, our demonstration of the centrality of accounting for patterns of time use in evaluating the effectiveness of private schools are perhaps the most general result for the global literature on school choice. On the one hand, studies of vouchers and school choice that find no effects on test scores may understate the benefits by not accounting for other subjects that the private schools may be teaching. On the other hand, studies of charter schools finding positive effects on test scores may overstate the benefits if charter schools focus more on scores on high stakes tests and divert instructional time away from other subjects. More broadly, schools provide vectors of outcomes and may be horizontally differentiated in their offerings, which makes it difficult to compare outcomes across school types. In the absence of data on long-term outcomes such as employment and wages, it is important for education research to devise, test, and validate more content-neutral measures of learning that may enable meaningful comparisons of outcomes across varying instructional programs.

The policy implications of our results for education in India are particularly timely. The provision in the RtE Act for 25% reservation in private schools for disadvantaged students (with the government reimbursing the fees up to the per-child spending in public schools) has been highly controversial and contested all the way up to the Supreme Court of India. Our results suggest that this provision is likely to not only reduce social stratification at limited cost to current students in private schools, but also likely to increase average productivity in the education sector by increasing the share of private schooling. This may thus be a rare example of a policy that improves equity, and efficiency, and does so at a lower cost than the status quo.[35]

Nevertheless, there are important caveats to the broad implication that greater private sector participation in education production (supported by public funding as envisaged in the RtE Act) would improve the productivity of human capital creation. The first is that the private schools in our sample did not on average improve outcomes in math and the native language (even though they spent less time and money, and were as a result more productive). It is important to highlight that our results *do not* imply that increasing the time or money spent on instruction in these subjects in private schools will lead to a linear (or even concave) increase in learning outcomes (we have no evidence on this). For instance, if the voucher value were to be increased to equal the level of per-student spending in the public schools, it is possible that the private

[35] Reimbursements to private schools are bounded at the upper end by the per-child spending in government schools.

schools may respond by improving aspects of the school that are more visible to parents and improve their marketing prospects rather than more effective teaching.

We see an illustration of this issue when we consider the question of *why* private schools choose the allocation of instructional time that they do (which produces low levels of knowledge in three languages as opposed to grade-appropriate reading levels in at least one language).[36] Detailed qualitative interviews with head teachers suggest that the main reason for this is that the low-cost private schools in our sample typically copy the curriculum of elite private schools, which reflect the three-language formula that is typical of the education that elites in India have received.[37] Given the socially aspirational nature of private school attendance, the management of private schools we interviewed also stated that it would be difficult for them to remain competitive if they did not follow the curriculum that was 'standard' among more elite private schools (even if this curriculum was not optimal for the learning of the typical student attending a low-cost private school).[38]

This discussion points to the second caveat clear, which is that there may be a trade-off between a libertarian approach to school choice that believes that parents will make optimal schooling choices for their children and a paternalistic approach that believes that parents may make misguided evaluations of school quality based on factors that may not contribute much to more effective learning (such as buildings and facilities, and *levels* of test scores of other students). Our results unambiguously establish that private schools are more productive and cost effective than government schools from the perspective of a social planner. But, it is not obvious that they represent a better value for the marginal parent who is paying out of pocket for private schools over a free public school. Since test scores did not improve in math and Telugu, parents would have to place a high value on Hindi scores to justify paying out of pocket for the typical

[36] This assessment is based on the typical public school student being substantially behind grade level competences in the native language and math (Pratham 2012, Muralidharan and Zieleniak 2013). Since the mean impact on Telugu scores of going to private schools is zero, it is likely that the absolute level of competence in any language among voucher winners is low.

[37] The three language formula aims to teach the state language, the national language (Hindi) and a global language (English) that also serves as a lingua-franca between Indian states, given the history of resistance to Hindi in some non-Hindi speaking states of India. This is a more onerous expectation in terms of the number of languages that a school student is expected to know than found in most countries around the world, but is a standard expectation among most Indian elites – especially those in non-Hindi speaking states such as Andhra Pradesh.

[38] See Srinivas (1962) for the classic reference in Indian sociology on the phenomenon of 'Sanskritization' and the processes of transmission of socially aspirational behavior. Of course, it is also likely that knowledge of an additional language like Hindi would have returns in the labor market, but it is less clear that these returns are higher than increasing competence in the native language to enable better learning of other subjects.

private school in our sample. While, we cannot rule out this possibility, it is likely that parents were not able to easily determine the effectiveness of schools at *improving* learning outcomes, and our results highlight the importance of providing better information on determinants of education quality to schools and parents based on careful research.

A final caveat is that the social efficiency gains from the greater productivity of private schools can be negated if the steady state system of allocation of students to schools features high degrees of selectivity by schools (see Macleod and Urquiola 2012 for a theoretical treatment of this issue). This insight is already incorporated in the rules that most charter schools in the US operate under (that they cannot be selective in who they accept), but it is important to apply it to the way that the RtE will be implemented.

Our results and discussion point towards several avenues for future research on school choice. The first is to better estimate education production functions with a specific focus on the relationship between instructional time per subject and test scores, and on the role of the language of instruction (including positive and negative spillovers to other subjects, and heterogeneous impacts of medium of instruction as a function of home characteristics). Second, the analysis in this paper (and in most of the school choice literature) has focused exclusively on the impacts of choice on test scores and learning outcomes, and has ignored welfare gains to households from enhanced choice and match quality. A natural extension therefore is to estimate a structural model of school choice using revealed preference of program take up, and estimate the welfare gains to households from introducing new schools into their feasible choice set by bringing their price down sharply (Bresnahan and Gordon 1996).

Three further sets of research questions are first order in the Indian context. First, it would be important to replicate this experiment with the value of the voucher set equal to the per-student spending in public schools. Second, our rural setting was not ideal for studying heterogeneous effects of voucher programs as a function of the extent of choice and competition. While our results in this area are suggestive, more conclusive evidence will require running a similar experiment in urban areas in India – where the greater population density allows for much more choice and competition between schools. Finally, the theoretical properties of the unique hybrid system envisaged in India (where private schools can select the fee-paying 75% of their students, while allocating the remaining 25% of slots – most likely by lottery – to disadvantaged students) are unclear and it would be greatly beneficial for policy to formally

model the properties and characterize the equilibria that may result from such a structure. Indian states are currently in the process of drafting the rules for implementing the RtE, and there is much fertile ground for future research to better understand education markets in low-income settings and directly contribute to better education policy.

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Figure 1: Design of AP School Choice Program

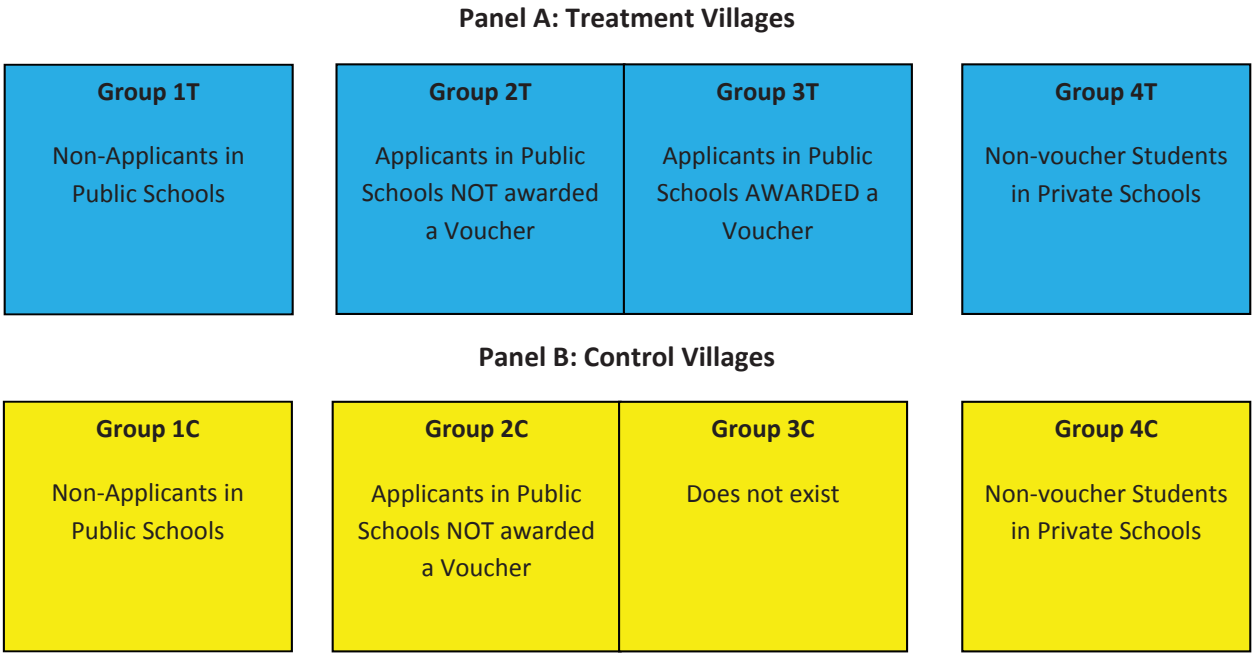
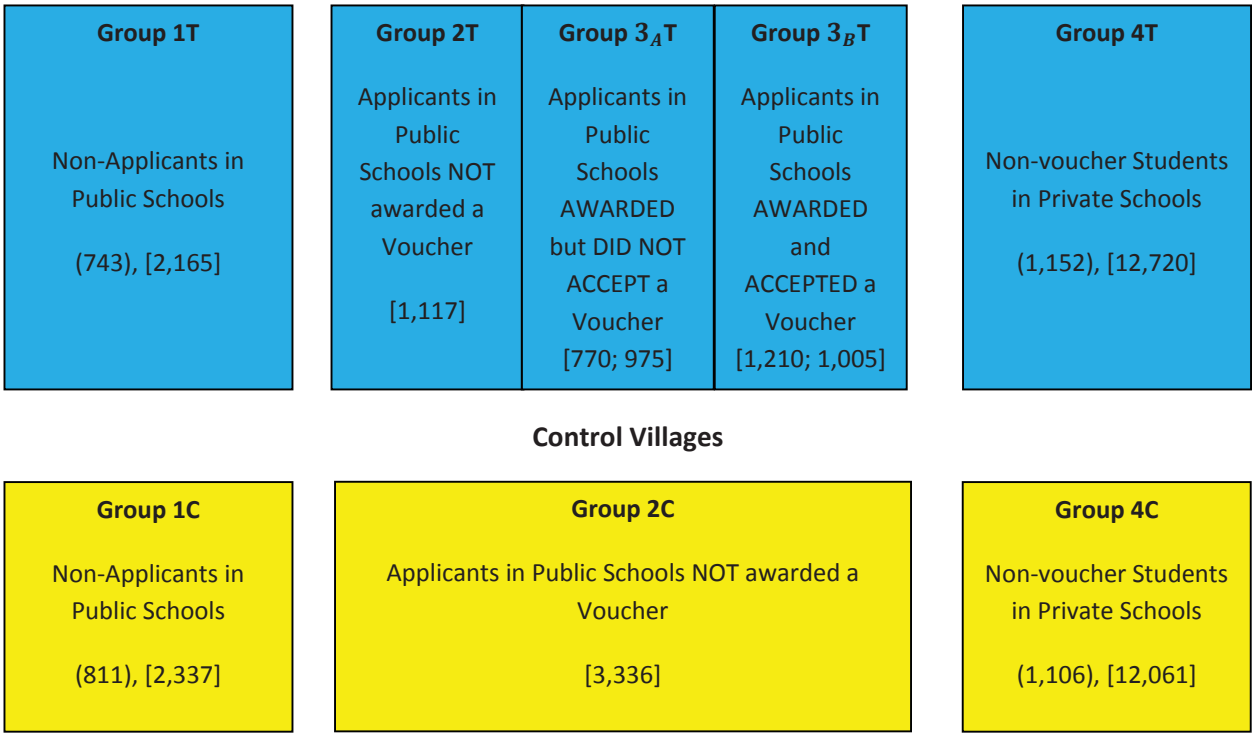


Figure 2: Design of AP School Choice Program with Student Counts



Notes: All of groups 2T, 2T, and 2C were sampled for tests of learning outcomes after two and four years of the project. For other groups, numbers in parentheses are the sample size that was tracked (with the total population in brackets). The two numbers under group 3_BT represent those who first accepted and started in a private school (1210) and those who were still in a private school at the end of 4 years (1,005). Conversely in group 3_AT, 770 initially rejected the offer, while 975 were no longer availing the voucher at the end of 4 year

Table 1: Validity of Design

Panel A: Treatment and Control Students

	Lottery winners [treatment villages]	Lottery losers [treatment villages]	P-value for difference	Lottery winners [treatment villages]	Lottery losers [control villages]	P-value for difference
	[1]	[2]	[3]	[4]	[5]	[6]
Normalized baseline Telugu score	0.01	0.03	0.44	0.01	-0.05	0.31
Normalized baseline math score	-0.02	-0.03	0.75	-0.02	-0.05	0.74
Both parents have completed at least primary school	0.3	0.28	0.29	0.3	0.28	0.68
At least one parent has completed grade 10	0.34	0.33	0.76	0.34	0.36	0.14
Scheduled caste	0.35	0.33	0.3	0.35	0.32	0.43
Household asset index	3.17	3.14	0.41	3.17	3.19	0.68
Observations	1,980	1,119		1,980	3,334	

Panel B: Students for Spillover Analysis

	Non-applicants in treatment villages	Non-applicants control villages	P-value for difference	Students initially in private schools in treatment villages	Students initially in private schools incontrol villages	P-value for difference
Normalized baseline Telugu score	-0.03	0.09	0.12	0.57	0.64	0.21
Normalized baseline math score	-0.01	0.09	0.28	0.68	0.66	0.75
Both parents have completed at least primary school	0.28	0.29	0.69	0.51	0.56	0.1
At least one parent has completed grade 10	0.31	0.34	0.19	0.52	0.55	0.11
Scheduled caste	0.34	0.34	0.98	0.12	0.13	0.64
Household asset index	3.16	3.2	0.49	3.85	3.85	0.96
Observations	2,165	2,337		12,720	12,061	

Notes:

All standard errors are clustered at the school level. This table is based on baseline household surveys and assessments (2008). Telugu and math scores are normalized across treatment and control students with respect to students in control villages by subject and grade. The household asset index reported is a sum of five household indicators, including whether a household owns its own home, has a proper house, has at least one covered room, has working water facilities, and has a toilet available.

Table 2: Attrition

Panel A: Voucher Winners and Losers

	Year 2 assessments						Year 4 assessments					
	Lottery winners [treatment villages]	Lottery losers [treatment villages]	P-value for difference	Lottery winners [treatment villages]	Lottery losers [control villages]	P-value for difference	Lottery winners [treatment villages]	Lottery losers [treatment villages]	P-value for difference	Lottery winners [treatment villages]	Lottery losers [control villages]	P-value for difference
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Characteristics												
Present during the test	0.90	0.84	0.00	0.90	0.85	0.00	0.85	0.80	0.00	0.85	0.81	0.02
Comparison of attritors												
Normalized baseline telugu score	0.05	0.14	0.37	0.04	0.09	0.70	0.03	0.14	0.21	0.02	0.10	0.27
Normalized baseline math score	0.06	0.03	0.79	0.06	0.10	0.82	0.05	0.01	0.68	0.05	0.10	0.61
Both parents have completed at least primary school	0.29	0.25	0.43	0.29	0.25	0.44	0.28	0.24	0.24	0.28	0.30	0.49
At least one parent has completed grade 10	0.33	0.32	0.78	0.33	0.34	0.78						
Scheduled caste	0.39	0.34	0.38	0.39	0.32	0.15	0.38	0.41	0.42	0.38	0.34	0.29
Household asset index	3.15	3.01	0.15	3.15	3.09	0.59	3.15	3.10	0.54	3.15	3.21	0.48
Observations	1,980	1,117		1,980	3,336		1,980	1,117		1,980	3,338	

Panel B: Students for Spillover Analysis

	Year 2 assessments						Year 4 assessments					
	Non-applicants in treatment villages	Non-applicants control villages	P-value for difference	Students initially in private schools in treatment villages	Students initially in private schools incontrol villages	P-value for difference	Non-applicants in treatment villages	Non-applicants control villages	P-value for difference	Students initially in private schools in treatment villages	Students initially in private schools incontrol villages	P-value for difference
Characteristics												
Present during the test	0.66	0.67	0.84	0.62	0.60	0.48	0.66	0.67	0.84	0.70	0.65	0.11
Comparison of attritors												
Normalized baseline telugu score	-0.03	0.07	0.41	0.58	0.72	0.10	-0.03	0.07	0.41	0.51	0.75	0.00
Normalized baseline math score	-0.03	0.11	0.36	0.76	0.69	0.53	-0.03	0.11	0.36	0.75	0.72	0.81
Both parents have completed at least primary school	0.26	0.25	0.81	0.57	0.61	0.30	0.26	0.25	0.81	0.57	0.63	0.13
At least one parent has completed grade 10	0.27	0.29	0.68	0.51	0.55	0.27						
Scheduled caste	0.38	0.28	0.06	0.14	0.13	0.83	0.35	0.29	0.28	0.16	0.15	0.75
Household asset index	3.13	3.29	0.20	3.88	3.90	0.80	3.13	3.21	0.49	3.94	3.94	1.00
Observations	743	811		1,149	1,109		743	811		1,152	1,106	

Notes:

All standard errors are clustered at the school level. Telugu and math scores are normalized across treatment and control students with respect to students in control villages by subject and grade. The household asset index reported is a sum of five household indicators, including whether a household owns its own home, has a proper house, has at least one covered room, has working water facilities, and has a toilet available.

Table 3: School and Teacher Characteristics by School Type

	Private schools	Government schools	Difference
	[1]	[2]	[3]
Panel A: School Characteristics			
Total enrollment	301.71	83.31	218.4***
Total working days	229.42	218.40	11.02***
Pupil-teacher ratio	16.86	26.37	-9.514***
Drinking water available	0.99	0.92	0.0730***
Functional toilets	0.89	0.68	0.205***
Separate functional toilets for girls	0.79	0.43	0.364***
Functional electricity	0.90	0.59	0.305***
Functional computers	0.53	0.04	0.484***
Functional library	0.81	0.98	-0.169***
Functional radio	0.14	0.80	-0.660***
Observations	926	1,183	
Panel B: Teacher Characteristics			
Male	0.25	0.44	-0.19***
Age	35.47	47.04	-11.57***
Years of teaching	5.61	14.82	-9.21***
Completed at least college or masters	0.69	0.86	-0.16***
Teacher training completed	0.34	0.98	-0.64***
Come from the same village	0.46	0.14	0.32***
Current gross salary per month (Rs.)	2310.10	13720.90	-11410.85***
Observations	2,868	2,370	
Panel C: School Expenditures			
Annual cost per child (Rs./child)	2334.03	8390.00	-6055.97***
Observations	695	1,052	

Notes:

* p<0.1; ** p<0.05; *** p<0.01. All regressions include district fixed effects. All standard errors are clustered at the school level. The sample for this table is restricted to schools and teachers in control villages across years 2008 through 2012. All expenditures are measured in Rupees per student per year. Actual observations for each regression vary in small amounts within panels based on the dependent variable. Estimates of per child annual expenditure in government schools come from government budget documents

Table 4: Measures of Teacher and School Effort

	Private schools	Government schools	Difference
	[1]	[2]	[3]
Panel A: Measures of Classroom Activity			
Class is engaged in active teaching	0.51	0.34	0.17***
A teacher is present in class	0.97	0.92	0.048***
Effective in teaching and maintaining discipline	0.50	0.36	0.14***
Teacher has complete control over class	0.69	0.41	0.28***
Teachers teaching mutliple classes at the same time	0.24	0.79	-0.55***
Observations	2,738	2,784	
Panel B: Measures of Teacher Activity			
Cannot find the teacher (absent) before the class starts	0.09	0.24	-0.15***
Teacher is actively teaching	0.50	0.35	0.15***
Teacher is in school and not teaching	0.01	0.03	-0.02***
Observations	6,577	5,552	
Panel C: Measures of School Hygiene			
Flies heavily present on premises of the school	0.14	0.19	-0.05**
Stagnant water present on premises of the school	0.18	0.28	-0.10***
Garbage dumped on premises of the school	0.33	0.44	-0.11***
Observations	426	614	

Notes:

* p<0.05; ** p<0.01; *** p<0.001. All regressions include district fixed effects. All standard errors are clustered at the school level. The sample for this table is restricted to classrooms, teachers, and schools in control villages. Measures of classroom activity are from school short surveys administered in years 2008 and 2009. Measures of teacher activitiy are from teacher short surveys administered across years 2008 through 2012. Measures of school hygiene are from school short surveys administered across years 2010 through 2012. Actual observations for each regression vary in small amounts within panels based on the dependent variable.

Table 5: Changes in Household Inputs and School Processes as Reported by Children

	Private schools	Government schools	Difference	Applicants offered scholarship	Applicants in control villages	Intention to treat estimate	Treatment on the treated estimate
	Panel A: Student Time Diaries (Minutes per Day)						
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Activity:							
Time spent in school	423.53	380.25	43.28***	409.34	383.38	25.96***	46.93***
Studying and doing homework at home	75.99	52.72	23.27***	59.83	56.86	2.97	5.38
Private Tuition	25.15	16.62	8.53**	21.95	17.43	4.52	8.17
Bathing/Toilet/Getting ready	55.11	61.7	-6.59***	57.82	61.24	-3.42	-6.19
Time traveling to school	23.5	20.92	2.58*	23.51	21.43	2.08	3.75
Working (outside/inside the house)	1.51	11.05	-9.54**	5.46	9.36	-3.90	-7.14
Chores	16.82	31.18	-14.36***	21.62	34.45	-12.83**	-23.51**
Watching TV	75.88	83.38	-7.50**	80.57	84.04	-3.47	-6.28
Playing with friends	82.34	101.99	-19.65***	100.88	99.73	1.15	2.08
Eating	43.57	44.69	-1.12	43.78	44.12	-0.34	-0.61
Free time	53.38	64.38	-11.00**	56.69	62.13	-5.44	-9.96
Observations	652	1839		885	1212		2097
	Panel B: Household Student Expenditure (Rupees per Year)						
Household expenditure on student:							
School admissions	140.58	14.95	125.63***	34.35	31.23	3.12	5.76
Uniforms	416.68	200.41	216.26***	171.14	237.07	-65.94***	-121.7698***
Notebooks/textbooks	554.46	228.57	325.89***	209.05	278.35	-69.29***	-127.39***
Special events	15.91	7.29	8.62**	5.30	9.04	-3.74	-6.92
Transportation	113.61	13.59	100.02***	46.55	43.57	2.98	5.51
Private tuition	71.07	32.51	38.56***	34.80	39.55	-4.75	-8.75
Total expenditure	**2910.36**	**566.73**	**2343.64*** **	**774.94**	**892.69**	**-117.75**	**-215.95**
Observations	634	1815		858	1182		2040

Notes:

* p<0.05; ** p<0.01; *** p<0.001. All regressions include district fixed effects. All standard errors are clustered at the village level. In all columns in Panels A and B, the sample is restricted to those students who reported activities and processes from a normal, non-sick school day. The sample for columns [1] through [3] is restricted to students and schools in control villages. The samples for columns [4] through [7] is applicants offered scholarships in treatment villages and applicants not offered scholarships in control villages. Data for both panels come from the parent child surveys administered between 2008 and 2012. The chores activity consists of preparing meals, caring for other children, and caring for the elderly. Total expenditure includes those other categories lsited and all other school-related expenditures. Actual observations for each regression vary in small amounts within panels based on the dependent variable.

Table 6: Test Score Impacts

Panel A: Impact of Winning a Voucher ("Intention to Treat" Effects)

	Year 2 assessments				Year 4 assessments					
	Telugu score	Math score	English score	Combined across tests	Telugu score	Math score	English score	Science and social studies score	Hindi score	Combined across tests
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Offered scholarship	-0.079	-0.053	0.179**	0.014	-0.017	-0.031	0.114	0.084	0.526***	0.129***
	(0.055)	(0.065)	(0.079)	(0.061)	(0.052)	(0.053)	(0.072)	(0.061)	(0.068)	(0.046)
Total observations	4,620	4,620	4,525	13,765	4,385	4,385	4,217	4,243	1,691	18,926
Treatment observations	1,778	1,778	1,738	5,294	1,674	1,675	1,607	1,628	867	7,451
Control observations	2,842	2,842	2,787	8,471	2,711	2,710	2,610	2,615	824	11,475

Panel B: Impact of Attending a Private School ("Treatment on the Treated" Effects, using lottery-based voucher award as an Insturmental Variable)

	Year 2 assessments				Year 4 assessments					
Scholarship recipient in private school	-0.140	-0.094	0.317**	0.025	-0.030	-0.055	0.201	0.149	0.891***	0.227***
	(0.098)	(0.115)	(0.139)	(0.108)	(0.092)	(0.093)	(0.127)	(0.108)	(0.103)	(0.081)
Total observations	4,620	4,620	4,525	13,765	4,385	4,385	4,217	4,243	1,691	18,926
Scholarship recipients	997	997	982	5,294	945	946	911	920	510	7,451
Non-recipients	3,623	3,623	3,543	8,471	3,440	3,439	3,306	3,323	1,181	11,475
First-stage F-stat	362	362	360	362	354	356	376	369	401	384

Notes:

* p<0.1; ** p<0.05; *** p<0.01. All regressions control for baseline normalized test scores and include district fixed effects. All standard errors are clustered at the village level. All test scores are normalized across treatment and control students with respect to students in control villages by subject and grade. Telugu, math, English and social studies test scores are from endline exams, Hindi test scores are from a special assessment. Combined scores are obtained by running a pooled regression across all test scores in each year, with Hindi test score observations weighted up by the inverse of the probability within a given group of a student being selected to take the test from the universe of students.

Table 7: School Time Tables

	Instructional Time by Subject (Minutes per week)		
	Private schools	Government schools	Difference
	[1]	[2]	[3]
Telugu	307.72	511.52	-203.81***
	(6.36)	(3.60)	(6.99)
Math	339.75	500.69	-160.94***
	(7.50)	(3.36)	(8.63)
English	322.68	235.52	87.17***
	(7.96)	(5.39)	(9.69)
Social studies	239.21	173.24	65.96***
	(6.29)	(6.89)	(9.84)
General science	205.52	104.58	100.94***
	(9.09)	(5.78)	(9.44)
Hindi	215.78	0.01	215.77***
	(6.08)	(0.89)	(6.41)
Moral science	16.85	20.11	-3.26
	(4.82)	(3.20)	(5.56)
Computer use	46.7	0.51	46.19***
	(6.50)	(1.02)	(6.80)
Other	311.66	250.29	61.37***
	(14.55)	(6.70)	(16.20)
Total instructional time	**2005.87**	**1796.47**	**209.4*** **
	(13.73)	**(6.86)**	**(14.46)**
Break	461	473.18	-12.18
	(9.14)	(3.05)	(10.58)
Total school time	**2466.87**	**2269.65**	**197.22*** **
	(17.46)	**(8.25)**	**(19.79)**
Observations	325	200	

Notes:

* p<0.1; ** p<0.05; *** p<0.01. All regressions include district fixed effects. All standard errors are clustered at the school level. The sample for this table is restricted to schools in control villages. All numbers in minutes per week. Other includes sports, arts and crafts, and study hall.

Table 8: Robustness to Attrition of Estimates of Winning a Voucher ("Intent to Treat" Effect)

Panel A: Inverse Probability Weighting

	Year 2 assessments				Year 4 assessments					
	Telugu score	Math score	English score	Combined across tests	Telugu score	Math score	English score	Science and social studies score	Hindi score	Combined across tests
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Offered scholarship	-0.081	-0.055	0.177**	0.012	-0.017	-0.031	0.113	0.083	0.522***	0.134***
	(0.055)	(0.065)	(0.079)	(0.061)	(0.052)	(0.053)	(0.072)	(0.061)	(0.068)	(0.046)
Total observations	4,620	4,620	4,525	13,765	4,385	4,385	4,217	4,243	1,696	18,926
Treatment observations	1,778	1,778	1,738	5294	1,674	1,675	1,607	1,628	867	7451
Control observations	2,842	2,842	2,787	8471	2,711	2,710	2,610	2,615	829	11475

Panel B: Lee Bounds

	Year 2 assessments				Year 4 assessments					
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Lower bound estimate	-0.148	-0.142	0.076	-0.079	-0.076	-0.112	0.001	-0.010	0.479	0.050
	(0.031)	(0.031)	(0.039)	(0.020)	(0.036)	(0.035)	(0.046)	(0.040)	(0.062)	(0.019)
Upper bound estimate	0.028	0.041	0.262	0.115	0.059	0.045	0.199	0.200	0.575	0.217
	(0.030)	(0.030)	(0.039)	(0.019)	(0.034)	(0.033)	(0.042)	(0.041)	(0.060)	(0.018)
Confidence interval low	**-0.208**	**-0.202**	**-0.001**	**-0.118**	**-0.145**	**-0.180**	**-0.090**	**-0.089**	**0.358**	**0.013**
Confidence interval high	**0.087**	**0.099**	**0.337**	**0.153**	**0.125**	**0.109**	**0.281**	**0.280**	**0.694**	**0.253**

Notes:

* p<0.1; ** p<0.05; *** p<0.01. All regressions in Panel A control for baseline normalized test scores and include district fixed effects. All standard errors are clustered at the village level. All test scores are normalized across treatment and control students with respect to students in control villages by subject and grade. Telugu, math, English and social studies test scores are from endline exams, Hindi test scores are from a special assessment. Inverse probabilities calculated using a probit regression of whether a student attrited on whether they were offered a scholarship and their baseline test scores, clustered at the school level. Combined scores are obtained by running a pooled regression across all test scores in each year, with Hindi test score observations weighted up by the inverse of the probability within a given group of a student being selected to take the test from the universe of students.

Table 9: Heterogeneous Test Score Impacts (By Initial Student Characteristics)

	Year 2 assessments				Year 4 assessments					
	Telugu score	Math score	English score	Combined across tests	Telugu score	Math score	English score	Science and social studies score	Hindi score	Combined across tests
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Offered scholarship * covariate										
Baseline test score	-0.056	-0.054	-0.074	-0.062	0.004	0.000	-0.022	-0.022	-0.105*	-0.030
	(0.042)	(0.043)	(0.052)	(0.041)	(0.037)	(0.037)	(0.047)	(0.043)	(0.060)	(0.033)
Female indicator	0.013	0.069	0.117	0.065	0.010	-0.037	0.013	0.017	0.169*	0.034
	(0.055)	(0.054)	(0.076)	(0.054)	(0.064)	(0.064)	(0.071)	(0.071)	(0.098)	(0.053)
Scheduled caste indicator	-0.012	0.042	-0.025	0.001	0.029	0.099	0.006	0.056	-0.108	0.014
	(0.069)	(0.066)	(0.082)	(0.066)	(0.070)	(0.070)	(0.090)	(0.083)	(0.121)	(0.064)
Parents literate indicator	0.043	-0.022	-0.011	0.003	-0.031	-0.006	0.132	-0.138*	-0.234**	-0.058
	(0.065)	(0.060)	(0.089)	(0.062)	(0.068)	(0.070)	(0.120)	(0.077)	(0.117)	(0.064)
Parents laborers indicator	-0.006	0.042	0.018	0.016	0.050	0.144**	0.148	0.019	-0.123	0.052
	(0.069)	(0.069)	(0.083)	(0.066)	(0.069)	(0.071)	(0.102)	(0.081)	(0.115)	(0.064)
Household asset index	0.014	0.031	-0.018	0.010	-0.028	-0.001	0.009	-0.019	0.017	-0.002
	(0.031)	(0.033)	(0.045)	(0.032)	(0.033)	(0.031)	(0.038)	(0.035)	(0.062)	(0.030)
Muslim indicator	0.232**	0.258**	0.404**	0.298**	0.364***	0.290**	0.151	0.288**	0.111	0.263***
	(0.116)	(0.137)	(0.177)	(0.126)	(0.112)	(0.128)	(0.147)	(0.140)	(0.168)	(0.097)
Christian indicator	-0.099	-0.085	-0.208	-0.130	-0.154	-0.232**	-0.113	-0.109	-0.193	-0.172
	(0.122)	(0.126)	(0.157)	(0.122)	(0.130)	(0.111)	(0.121)	(0.159)	(0.248)	(0.112)
Indicator for older cohort at baseline	0.020	-0.062	0.107	0.019	-0.045	-0.055	0.101	-0.051	0.116	0.013
	(0.072)	(0.083)	(0.089)	(0.071)	(0.082)	(0.087)	(0.116)	(0.097)	(0.104)	(0.069)
Observations	4,620	4,620	4,525	13,765	4,385	4,385	4,217	4,243	1,691	18,926

Notes:

* p<0.1; ** p<0.05; *** p<0.01. Each cell reports the coefficient from a separate regression that also includes controls for whether a student was offered a scholarship and the student's normalized baseline test scores. All regressions include district fixed effects and standard errors are clustered at the village level. All test scores are normalized across treatment and control students with respect to students in control villages by subject and grade. Telugu, math, English and social studies test scores are from endline exams, Hindi test scores are from a special assessment. The household asset index reported is a sum of five household indicators, including whether a household owns its own home, has a proper house, has at least one covered room, has working water facilities, and has a toilet available. Actual observations for each regression vary in small amounts within columns based on the covariate. Combined scores are obtained by running a pooled regression across all test scores in each year, with Hindi test score observations weighted up by the inverse of the probability within a given group of a student being selected to take the test from the universe of students.

Table 10: Heterogeneous Impacts by Market Competition

Panel A: Heteroegeneous Effects as a Function of Total Number of Private Schools within 0.5 km

	Year 2 assessments				Year 4 assessments					
	Telugu score	Math score	English score	Combined across tests	Telugu score	Math score	English score	Science and social studies score	Hindi score	Combined across tests
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Offered scholarship * covariate										
Number of Private Schools (Linear)	0.031	0.007	0.006	0.015	0.041	0.010	-0.009	0.002	-0.025	0.003
	(0.026)	(0.028)	(0.034)	(0.026)	(0.025)	(0.027)	(0.035)	(0.029)	(0.034)	(0.021)
Number of Private Schools (Log)	0.046	-0.010	-0.008	0.011	0.066	-0.027	-0.051	-0.069	-0.078	-0.034
	(0.069)	(0.079)	(0.094)	(0.073)	(0.071)	(0.071)	(0.093)	(0.082)	(0.090)	(0.058)
2 or more Schools (Top 25%)	-0.022	-0.070	-0.093	-0.061	0.011	-0.093	-0.121	-0.148	-0.139	-0.105
	(0.089)	(0.100)	(0.116)	(0.091)	(0.089)	(0.086)	(0.134)	(0.101)	(0.122)	(0.075)
4 or more Schools (Top 10%)	0.203	0.159	0.222	0.195	0.264**	0.198	0.214	0.232*	0.081	0.196*
	(0.123)	(0.123)	(0.166)	(0.123)	(0.115)	(0.129)	(0.245)	(0.121)	(0.167)	(0.105)
5 or more Schools (Top 5%)	0.206	0.182	0.306	0.232	0.284*	0.198	-0.006	0.272*	-0.243	0.108
	(0.186)	(0.168)	(0.192)	(0.166)	(0.162)	(0.153)	(0.183)	(0.163)	(0.271)	(0.133)

Panel B: Heteroegeneous Effects as a Function of Total Number of Private Schools within 1 km

Number of Private Schools (Linear)	0.024	0.016	0.021	0.021	0.029	0.001	-0.017	0.001	-0.012	-0.001
	(0.022)	(0.027)	(0.034)	(0.025)	(0.024)	(0.026)	(0.032)	(0.030)	(0.030)	(0.021)
Number of Private Schools (Log)	-0.069	-0.045	-0.027	0.019	-0.079	-0.030	0.023	-0.039	0.002	-0.043
	(0.052)	(0.064)	(0.072)	(0.082)	(0.049)	(0.052)	(0.067)	(0.055)	(0.059)	(0.060)
3 or more Schools (Top 25%)	0.038	-0.066	-0.128	-0.051	0.046	-0.048	-0.063	-0.082	-0.127	-0.064
	(0.111)	(0.126)	(0.152)	(0.119)	(0.104)	(0.106)	(0.167)	(0.115)	(0.136)	(0.090)
5 or more Schools (Top 10%)	0.188	0.250*	0.278	0.240*	0.228*	0.127	-0.142	0.175	-0.030	0.061
	(0.118)	(0.135)	(0.197)	(0.133)	(0.126)	(0.146)	(0.174)	(0.157)	(0.163)	(0.118)
6 or more Schools (Top 5%)	0.170	0.283	0.427	0.292	0.457***	0.410***	0.170	0.449***	-0.156	0.264**
	(0.143)	(0.202)	(0.323)	(0.212)	(0.116)	(0.130)	(0.113)	(0.136)	(0.228)	(0.106)
Observations	4,612	4,612	4,518	13,742	4,378	4,378	4,215	4,237	1,689	18,897

Notes:

* p<0.1; ** p<0.05; *** p<0.01. Each cell reports the coefficient from a separate regression that also includes controls for whether a student was offered a scholarship and the student's normalized baseline test scores. All regressions include district fixed effects and standard errors are clustered at the village level. All test scores are normalized across treatment and control students with respect to students in control villages by subject and grade. Telugu, math, English and social studies test scores are from endline exams, Hindi test scores are from a special assessment. Combined scores are obtained by running a pooled regression across all test scores in each year, with Hindi test score observations weighted up by the inverse of the probability within a given group of a student being selected to take the test from the universe of 10,245 students.

Table 11: Estimating Spillover Effects

	Year 2 assessments				Year 4 assessments				
	Telugu score	Math score	English score	Combined across tests	Telugu score	Math score	English score	Science and social studies score	Combined across tests
Panel A: Comparing the Within-Village to Across-Village Controls									
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Lottery Loser in Treatment Village	0.009	0.010	0.033	0.017	0.014	0.001	-0.049	0.095*	0.015
	(0.041)	(0.044)	(0.056)	(0.069)	(0.044)	(0.045)	(0.059)	(0.051)	(0.056)
Total observations	3,784	3,784	3,705	11,273	3,606	3,605	3,472	3,488	14,171
Treatment observations	942	942	918	2,802	895	895	862	873	3,525
Control observations	2,842	2,842	2,787	8,471	2,711	2,710	2,610	2,615	10,646
Panel B: Impact on Non-applicants from Government Schools									
Treatment village	-0.025	0.046	0.119	0.046	0.049	-0.002	0.024	-0.023	0.011
	(0.072)	(0.068)	(0.089)	(0.068)	(0.063)	(0.069)	(0.071)	(0.073)	(0.057)
Total observations	1,030	1,030	1,008	3,068	1,173	1,174	1,145	1,149	4,642
Treatment observations	490	490	476	1,456	555	555	541	542	2,194
Control observations	540	540	532	1,612	618	619	604	607	2,448
Panel C: Impact on Non-scholarship Students from Private Schools									
Treatment village	0.065	0.025	-0.114	-0.003	0.040	0.037	-0.026	0.029	0.022
	(0.062)	(0.074)	(0.076)	(0.061)	(0.062)	(0.059)	(0.104)	(0.073)	(0.057)
Total observations	1,386	1,386	1,346	4,118	1,522	1,521	1,463	1,468	5,974
Treatment observations	721	721	708	2,150	802	802	777	773	3,154
Control observations	665	665	638	1,968	720	719	686	695	2,820

Notes:

* p<0.1; ** p<0.05; *** p<0.01. All regressions control for baseline normalized test scores and include district fixed effects. All standard errors are clustered at the village level. All test scores are normalized across treatment and control students with respect to students in control villages by subject and grade. Telugu, math, English and science and social studies test scores are from endline exams. Combined scores are obtained by running a pooled regression across all test scores in each year, with test score observations for the non-applicants and the students who started out in private schools to begin with being weighted up by the inverse of the probability within a given group of a student being selected to take the test from the universe of students.

Appendix Table 1: Baseline Test Scores and Socio-Economic Characteristics

	Private schools	Government schools	Difference	Difference with village fixed effects
	[1]	[2]	[3]	[4]
Normalized baseline Telugu score	0.639	0.004	0.635***	0.677***
Normalized baseline math score	0.661	0.015	0.646***	0.678***
Both parents have completed at least primary school	0.558	0.285	0.273***	0.308***
At least one parent has completed grade 10	0.547	0.352	0.195***	0.219***
Scheduled caste	0.128	0.329	-0.201***	-0.193***
Household asset index	3.846	3.193	0.653***	0.646***
Annual school fees paid (Rs./month)	1330.37	3.79	1326.57***	1326.92***
Observations	14,541	8,538		

Notes:

* p<0.05; ** p<0.01; *** p<0.001. All standard errors are clustered at the school level. The sample for this table is restricted to students in control villages at the baseline (2008). Telugu and math scores are normalized across treatment and control students with respect to students in control villages by subject and grade. The household asset index reported is a sum of five household indicators, including whether a household owns its own home, has a proper house, has at least one covered room, has working water facilities, and has a toilet available.

Appendix Table 2: Scholarship Take-up (Application and Acceptance)

	Correlates of Application and Acceptance		
	Applied==1	Accepted==1 (conditional on winning)	Accepted==1 (relative to those who did not apply)
	[1]	[2]	[3]
Normalized baseline Telugu score	-0.006	0.013	-0.000
	(0.009)	(0.014)	(0.018)
Scheduled caste	0.005	-0.036	0.005
	(0.013)	(0.022)	(0.029)
Muslim	-0.014	-0.027	-0.059
	(0.023)	(0.047)	(0.047)
Both parents literate	0.022	-0.004	0.035
	(0.014)	(0.020)	(0.028)
Household asset index	0.008	0.017	0.015
	(0.007)	(0.011)	(0.016)
Older sibling in government school	-0.026**	-0.056***	-0.042*
	(0.011)	(0.018)	(0.022)
Private school exists within 0.5 kilometers	0.054***	0.102**	0.106**
	(0.021)	(0.039)	(0.048)
Observations	7,951	1,975	3,182

Notes:

* p<0.1; ** p<0.05; *** p<0.01. Each cell reports the coefficient from a binary separate regression that also includes a constant and district fixed effects. All standard errors are clustered at the village level. The sample for all regressions is restricted to those students that were eligible to apply for the scholarship. The household asset index reported is a sum of five household indicators, including whether a household owns its own home, has a proper house, has at least one covered room, has working water facilities, and has a toilet available. The actual number of observations for each regression may vary slightly within columns based on the dependent variable. Column 1 compares applicants to non-applicants. Column 2 compares those who accepted the voucher to those who did not (conditional on winning). Column 3 compares those who accepted the voucher to those who did not apply.

Appendix Table 3: Hindi Test Score Impacts by Question Type

Panel A: Intention to Treat Effects

	Student score (fraction correct) by question type				
	Letters	Words	Sentences	Paragraph	Advanced
	[1]	[2]	[3]	[4]	[5]
Offered scholarship	0.232***	0.172***	0.122***	0.121***	0.026***
	(0.029)	(0.026)	(0.023)	(0.023)	(0.009)
Mean in control	0.23	0.14	0.08	0.08	0.02
Total observations	1,691	1,691	1,691	1,691	1,691
Treatment observations	867	867	867	867	867
Control observations	824	824	824	824	824

Panel B: Treatment on the Treated Effects

	Student score (fraction correct) by question type				
	Letters	Words	Sentences	Paragraph	Advanced
	[1]	[2]	[3]	[4]	[5]
Attended a private school (using scholarship as an instrument)	0.393***	0.291***	0.206***	0.204***	0.044***
	(0.046)	(0.041)	(0.036)	(0.036)	(0.015)
Total observations	1,691	1,691	1,691	1,691	1,691
Scholarship recipients	510	510	510	510	510
Non-recipients	1,181	1,181	1,181	1,181	1,181

Notes:

All regressions include a constant and district fixed effects. All standard errors are clustered at the village level. Hindi test scores are from a special assessment administered in June 2012. Panel B instruments for scholarship reciept using the offer of scholarship.

Appendix Table 4: Test Score Impacts of Winning a Voucher (Intent to Treat) by Control Group

	Panel A: Within-village Controls						
	Year 2 assessments			Year 4 assessments			
	Telugu score	Math score	English score	Telugu score	Math score	English score	Science and social studies score
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Offered scholarship	-0.088**	-0.063	0.140***	-0.032	-0.033	0.160***	-0.016
	(0.041)	(0.041)	(0.043)	(0.037)	(0.035)	(0.045)	(0.046)
Total observations	2,719	2,719	2,655	2,568	2,569	2,468	2,501
Treatment observations	1,777	1,777	1,737	1,673	1,674	1,606	1,628
Control observations	942	942	918	895	895	862	873
	Panel B: Across-village Controls						
	Year 2 assessments			Year 4 assessments			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Offered scholarship	-0.079	-0.053	0.179**	-0.017	-0.031	0.114	0.084
	(0.055)	(0.065)	(0.079)	(0.052)	(0.053)	(0.072)	(0.061)
Total observations	4,620	4,620	4,525	4,385	4,385	4,217	4,243
Treatment observations	1,778	1,778	1,738	1,674	1,675	1,607	1,628
Control observations	2,842	2,842	2,787	2,711	2,710	2,610	2,615

Notes:

* p<0.1; ** p<0.05; *** p<0.01. All regressions control for baseline normalized test scores and include a constant and district fixed effects. All standard errors are clustered at the village level. All test scores are normalized across treatment and control students with respect to students in control villages by subject and grade. Telugu, math, English and social studies test scores are from endline exams, Hindi test scores are from a special assessment.

Appendix Table 5: Spillover effects as a function of number of scholarship students

Panel A: OLS Estimate of the impact of the number of voucher students who join a school on the students who started out in the same private school

	Year 2 assessments			Year 4 assessments			
	Telugu score	Math score	English score	Telugu score	Math score	English score	Science and social studies score
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Number of scholarship students	0.007	-0.005	-0.007	0.012**	0.000	-0.010	0.010
	(0.007)	(0.008)	(0.010)	(0.005)	(0.006)	(0.013)	(0.008)
Observations	554	554	544	668	668	657	653

Instrumental Variable Estimate of the impact of the number of voucher students who join a school on the students who started out in the same private school

	Year 2 assessments			Year 4 assessments			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Number of scholarship students	-0.008	-0.000	-0.041	0.003	-0.003	0.020	0.007
	(0.017)	(0.023)	(0.025)	(0.014)	(0.018)	(0.024)	(0.019)
Observations	554	554	544	668	668	657	653
First-stage F-stat	20.5	20.5	22.0	35.0	35.0	36.2	35.7

Notes:

* p<0.1; ** p<0.05; *** p<0.01. All regressions control for baseline normalized test scores and include a constant and district fixed effects. All standard errors are clustered at the village level. The sample in both panels restricted to those students in treatment villages for which location data is available with at least one private school in their village. All test scores are normalized across treatment and control students with respect to students in control villages by subject and grade. Telugu, math, English and social studies test scores are from endline exams. In panel B, number of scholarship students is instrumented by the number of scholarship-eligible students within a 1km radius for which each private school was the closest or second closest private school.