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

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We present experimental evidence on the impact of a school choice program in the Indian state of Andhra Pradesh that provided students with a voucher to finance attending a private school of their choice. The study design featured a unique two-stage lottery-based allocation of vouchers that created both student-level and market-level experiments, which allows us to study the individual and

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the aggregate effects of school choice (including spillovers). After two and four years of the program, we find no difference between test scores of lottery winners and losers on Telugu (native language), math, English, and science/social studies, suggesting that the large cross-sectional differences in test scores across public and private schools mostly reflect omitted variables. However, private schools also teach Hindi, which is not taught by the public schools, and lottery winners have much higher test scores in Hindi. Furthermore, the mean cost per student in the private schools in our sample was less than a third of the cost in public schools. Thus, private schools in this setting deliver slightly better test score gains than their public counterparts (better on Hindi and same in other subjects), and do so at a substantially lower cost per student. Finally, we find no evidence of spillovers on public school students who do not apply for the voucher, or on private school students, suggesting that the positive effects on voucher winners did not come at the expense of other students. *JEL* Codes: C93, H44, H52, I21, O15.

I. INTRODUCTION

One of the most important trends in primary education in developing countries over the past two decades has been the rapid growth of private schools, with recent estimates showing that private schools now account for over 20 percent of total primary school enrolment in low-income countries (Baum et al. 2014). The growing market share of fee-charging private schools is especially striking because it is taking place in a context of increased spending on public education and nearly universal access to free public primary schools, and raises important questions regarding the effectiveness of private schools in these settings and the optimal policy response to their growth.

Opponents of the growth of private schooling argue that it has led to economic stratification of education systems, and weakened the public education system by causing the middle class to secede. They also worry that private schools compete by cream-skimming students and attract parents and students on the basis of superior average levels of test scores, but that they may not be adding more value to the marginal applicant.[1] Others contend that private schools in developing countries have grown in response to failures of the public schooling system, that they are more accountable and responsive to parents, that the revealed

1. This concern is supported by several studies across different contexts, which find that highly demanded elite schools do not seem to add more value to student learning. See Zhang (2014) in China; Lucas and Mbiti (2014) in Kenya; Cullen, Jacob, and Levitt (2006) in Chicago; and Abdulkadiroglu, Angrist, and Pathak (2014) in Boston and New York.

preference of parents suggests that they are likely to be better than public schools, and that policy makers should be more open to voucher-like models that combine public funding and private provision of education.[2]

There is very little rigorous empirical evidence on the relative effectiveness of private and public schools in low-income 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 they have not been able to rule out the concern that these estimates are confounded by selection and omitted variables.[3] Furthermore, even experimental studies of school choice to date (from anywhere in the world) have not been able to estimate spillover effects on students remaining in public schools or on students who were in private schools to begin with. For instance, Hsieh and Urquiola (2006) argue that Chile's school voucher program led to increased sorting of students among schools but did not improve average school productivity.

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 randomization of the offer of a voucher (across villages as well as students). The design creates a set of control villages, which allows us to experimentally evaluate the individual effects of school choice (using the student-level lottery) as well as its aggregate effects including the spillovers on nonapplicants and students who start out in private schools (using the village-level lottery). The experiment was a large one that led to 23 percent of students in public schools in program villages moving to a private school. Participation of private schools in the voucher program was voluntary, but they were not permitted to selectively accept or reject voucher-winning students.

The main operating difference between private and public schools in this setting is that private schools pay substantially lower teacher salaries (less than a sixth of that paid to public

2. See Tooley and Dixon (2007), Muralidharan and Kremer (2008), Goyal and Pandey (2009), and Tooley (2009).

3. 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 (Andrabi et al. 2011; Singh 2015).

school teachers) and hire teachers who are younger, less educated, and much less likely to have professional teaching credentials. However, private schools hire more teachers, have smaller class sizes, and have a much lower rate of multigrade teaching than public schools. Using official data and data collected 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 effect of school choice on test scores (if any) is likely to be due to changes in school as opposed to household factors.

At the end of two and four years of the school choice program, we find no difference between the test scores of lottery winners and losers on the two main subjects of Telugu (native language of AP) and math, suggesting that the large cross-sectional test-score differences in these subjects across public and private schools (of 0.65σ) mostly reflect omitted variables. However, analysis of school time use data reveals that private schools spend significantly less instructional time on Telugu (40% less) and math (32% less) than public schools, and instead spend more time on English and science and social studies (EVS). They also teach a third language, Hindi, which is not taught in public primary schools (Hindi is not the main language in AP, but is the most widely spoken language in India). We conduct tests in all these subjects after four years of the voucher program and find small positive effects of winning the voucher on English (0.12σ; $p = .098$) and EVS (0.08σ; $p = .16$), and large positive effects on Hindi (0.55σ; $p < .001$).

If we assume equal weights across all subjects, we find that students who won a voucher had average test scores that were 0.13σ higher, and the average student who attended a private school using the voucher scored 0.26σ higher ($p < .01$). This positive impact is mainly driven by Hindi (which is taught in private schools but not in public primary schools), and we find no effect of winning a voucher on average test scores excluding Hindi. However, even without assuming equal weights across subjects, we can still infer that private schools were more productive than public schools because they were able to achieve similar Telugu and math test scores for the lottery winners with substantially less instructional time, and use the additional time to generate large gains in Hindi test scores. Furthermore, the annual cost per

student in the public school system is more than three times the mean cost per student in the private schools in our sample. Thus, students who win a lottery to attend private schools have slightly better test scores (better on Hindi and same on other subjects) even though the private schools spend substantially lower amounts per student.

These gains in test scores for voucher-winning students do not come at the expense of other students who may have been indirectly affected by the voucher program. Comparing across treatment and control villages, we find no evidence of spillovers on public school students who do not apply for the voucher. We also do not find any significant difference between the test scores of applicants who are lottery losers across treatment and control villages. Finally, we find no evidence of any negative spillovers on students who started out in private schools to begin with. Taken together, we find no evidence of adverse effects on any of the groups of students who experienced a change in their peer group as a result of the voucher program.

Turning to heterogeneity, we find limited evidence of variation in program impact by student characteristics, but we do find suggestive evidence of heterogeneity as a function of school and market characteristics. In particular, instrumental variable (IV) estimates suggest that students who switched from attending a public school to a Telugu-medium private school did better than those attending an English-medium one (especially on nonlanguage subjects).[4] The IV estimates have large standard errors and are not precise, but they suggest that private schools may have been even more effective when students did not experience the disruption of changing their medium of instruction. They also suggest that switching to English-medium schools may have negative effects on first-generation learners' literacy in the native language and on their learning of content in other nonlanguage subjects. Finally, we find suggestive evidence that the impact of the vouchers may have been higher in markets with greater choice and competition.

Since Friedman (1962), the theoretical promise that greater school choice and competition may yield better education outcomes

4. We instrument for medium of instruction of the school attended (which is a choice variable) with the medium of instruction of the nearest private school to each applicant for the voucher and the interaction of receiving the voucher and the medium of instruction of the nearest private school. See details in Section IV.D.2.

has generated a large empirical literature, 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 on school choice 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). On the other hand, more recent studies have found significant positive effects of attending charter schools on test scores (Hoxby, Murarka, and Kang 2009; Abdulkadiroglu et al. 2011; Dobbie and Fryer 2011).

We add to this evidence base by providing the first experimental evidence on the impact of school choice, and the relative performance of public and private schools in a developing country.[6] Furthermore, our two-stage design allows us to conduct the first experimental analysis of the spillover effects of school choice programs on nonapplicants, lottery losers, and 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. Note that our inference regarding the relative productivity of private and public schools would have been wrong if we had not accounted for school time use patterns and had not measured outcomes on 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. Our suggestive evidence of heterogeneity of impact by medium of instruction further highlights the centrality of accounting for variation across schools' instructional

5. Studies of school choice and charter schools using lottery-based designs include Howell et al. (2002), Howell and Peterson (2002), Cullen, Jacob, and Levitt (2006), Abdulkadiroglu et al. (2011), Dobbie and Fryer (2011), and Wolf et al. (2013).

6. Angrist et al. (2002), and Angrist, Bettinger, and Kremer (2006) provide experimental evidence on vouchers in the middle-income setting of Colombia and find positive effects of the PACES program. However, the program allowed vouchers to be topped up and required students to maintain minimum academic standards to continue receiving the voucher. The estimates therefore reflect a combination of private school productivity, additional education spending, and student incentives.

programs for studying the relative productivity of public and private schools and the impact of school choice.

The rest of this article is structured as follows. Section II describes the AP School Choice experiment (design, validity, and data collection); Section III presents results on summary statistics of school, teacher, and household inputs into education; Section IV presents the test score results, and Section V discusses policy implications, caveats, and directions for future research. Tables A.1 to A.9 are available in an Online Appendix.

II. The AP School Choice Experiment

II.A. 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. Nevertheless, education quality is low with less than 40% of children aged 6–14 in rural India being able to read at the second-grade level (ASER 2013). The majority of children in rural India are enrolled in free government-run public schools (with additional benefits such as free textbooks and midday meals).[7] However, 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.[8]

A prominent trend in India in the past two decades has been that parents are enrolling their children in fee-charging private schools in increasing numbers. Annual data from the ASER reports show that 29% of children between the ages of 6 and 14

7. Government-run public schools are referred to as "government schools" in India, with the term "public school" often referring to elite private schools (following the British convention). We use the term "public school" throughout this article to refer to government-run public schools following the more standard use of the term.

8. As an example of inefficient choice of inputs, Muralidharan and Sundararaman (2013) show that locally hired contract teachers are at least as effective as 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: 26.2% of public-school teachers in rural India were found absent during unannounced visits to a nationally-representative sample of schools in 2003 (Kremer et al. 2005), and 23.6% were found absent in 2010 (Muralidharan et al. 2014).

in rural India attended fee-charging private schools in 2013 compared to 18.7% in 2006, pointing to a rapid growth in the market share of fee-charging private schools at a rate exceeding 1 percentage point a year (ASER 2013). Although annual data on private school market share are not available for urban areas, this figure was estimated to be 58% in 2005 (Desai et al. 2009) and was recently estimated to be over 65% for the medium-sized city of Patna (Rangaraju, Tooley, and Dixon 2012).

The majority of these private schools are low-cost or "budget" private schools that cater to nonaffluent sections of the population, and per student spending in these schools 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 Table II later). Cross-sectional studies (as well as our baseline data) find 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.[9]

The growth of private schools has led to concerns about increasing economic and social stratification in education (Srivastava 2013) and has led 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; Kelkar 2006). India's recently passed Right to Education (RtE) Act includes a provision mandating that private schools reserve up to 25 percent of the seats in

9. Beyond selection, a major limitation in the cross-sectional comparisons is that private school students typically have two years of preschool education (nursery and kindergarten) compared to public school students (who typically start in the first grade). Thus, comparisons of test score levels at a given primary school grade confound the effectiveness of private schools and the total years of schooling. Panel data approaches can mitigate this concern (Singh 2015) but are limited by the lack of annual panel data on test scores in representative samples.

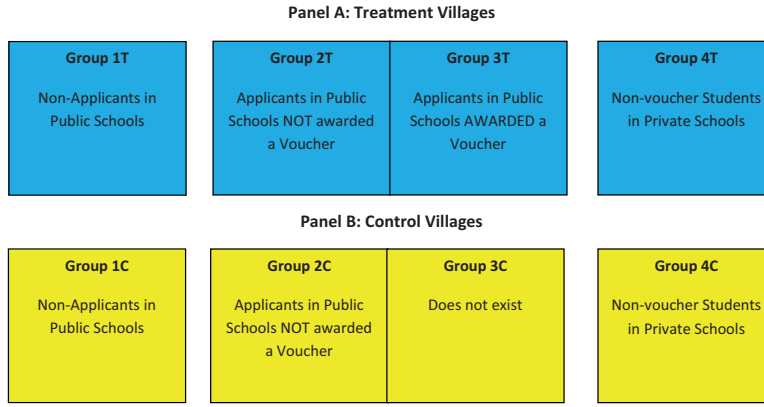

FIGURE I
Design of AP School Choice Program

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 public schools).

If implemented as intended, this provision could lead to India having the world's largest number of children attending private schools with public funding. It may also constitute the most ambitious attempt at school integration (across socioeconomic classes) that has ever been attempted (analogous to school desegregation in the United States). Estimating the relative productivity of public and private schools and the spillover effects of moving children from public to private schools is therefore especially policy relevant in this setting.

II.B. Conceptual Overview of Experiment Design

Experimental evaluations of school voucher programs to date typically feature excess demand for a limited number of vouchers, which are allocated among applicants by lottery. Such a design creates four groups of students as shown in Figure I (Panel A): non-applicants (group 1), applicants who lose the lottery (group 2), applicants who win (group 3), and students in private schools to begin with (group 4). The lottery is used to estimate the impact of winning a voucher conditional on applying for it (comparing groups 3 and 2), and the impact of attending a private school

(using the lottery as an instrumental variable for attending a private school).

However, even this experimental design faces two limitations: a contaminated control group and an inability to estimate spillover effects that may negate (potential) gains estimated for voucher winners. First, the departure of some voucher winners may have additional effects on lottery losers (group 2), including changes in the peer group, changes in per student resources (especially class size), and behavioral changes by public school teachers in response to the voucher program. These confounding factors could bias experimental studies to date, since the control group is not unaffected by the voucher program. Second, existing studies cannot experimentally estimate effects on students left behind in public schools who did not apply for the voucher and may be worse off from the departure of highly motivated peers (group 1), or the impact on students in private schools who may be worse off due to an influx of low-performing students from public schools (group 4). Thus, even if group 3 does better than group 2 (the focus of experimental studies to date), this may have come at the cost of poorer performance for groups 1 and 4. Hence, 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 2006).

The AP School Choice Project (which this article is based on) aims to address both these issues using a two-stage experiment, where villages are first randomized into control and treatment groups, after which some applicants in the treatment villages are offered vouchers using a second lottery (Figure I, Panel B). Since villages are randomized into treatment and control status after baseline tests are conducted and after parents apply for the voucher, comparing the lottery winners (3T) with lottery losers in control villages (2C) allows for an uncontaminated estimate of the impact of school choice. Thus, applicants in group 2C are a "pure" control group because they applied for the voucher and lost the lottery (at the village level), but nothing changed for them because there was no voucher program in their villages.

The design also allows us to estimate three sets of spillovers, which have not been possible to date. First, comparing groups 2T (control students with spillovers) and 2C (control students without spillovers), provides an estimate of the extent to which ignoring spillovers to the control group may bias existing voucher studies. Second, comparing groups 1T and 1C lets us estimate the

impact of school choice programs on the students "left behind" in public schools (who for reasons of limited information or motivation choose to not apply for the voucher). Third, comparing outcomes between groups 4T and 4C provides an estimate of whether students in private schools are adversely affected by an influx of students from public schools (which is what will happen under the school integration envisaged by the RtE Act). Overall, the key innovation in our design is that the control villages provide a "system-level" counterfactual to the voucher program enabling experimental comparisons that have not been possible to date.

II.C. The AP School Choice Experiment

AP is the fifth most populous state in India, with a population of 85 million (70% rural).[10] Recent estimates suggest that over 35% of students in rural AP are enrolled in private schools, compared with an all-India average of 28% (ASER 2013). The AP School Choice Project was implemented by the Azim Premji Foundation (one of India's leading nonprofits working on education).[11] The school year in AP runs from mid-June to mid-April, and the project started in the school year 2008–9, and continued for four years (preparatory work started in the prior school year of 2007–8).

The AP School Choice project was carried out in five districts across AP over a universe of 180 villages that had at least one recognized private school.[12] Baseline tests were conducted for all students in two cohorts of all schools (public and private) in these villages in March–April 2008.[13] This was followed by an

10. Note that the original state of AP was divided into two states on June 2, 2014. Since this division took place after our study, we use the term AP to refer to the undivided state.

11. 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 an education research partnership between the Government of Andhra Pradesh, the Azim Premji Foundation, and the World Bank.

12. These were the same districts as in the overall AP RESt project and were representative of all the three major regions of AP (Muralidharan and Sundararaman 2010, 2011, 2013). The AP School Choice Project was conducted in different subdistricts, so there was no overlap in the schools/villages across these studies.

13. The cohorts covered were students attending kindergarten and grade 1 in the previous school year (2007–8), and the voucher covered the entire primary education of recipients from the school year 2008–9 (from grade 1 to 5 for the younger

invitation to apply for a voucher to parents of students in public 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 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 midday meals that the public schools provide. The value of the voucher was paid directly to the school, and books and materials were provided directly to the voucher households by the schools.[14]

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 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 (i) 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 (ii) if so, how many seats they could make available to voucher students in each of the two cohorts.[15] The terms and conditions specified that the Foundation would directly pay the value of the voucher to

cohort and from grade 2 to 5 for the older cohort). Baseline tests were conducted in math and Telugu for the older cohort and in Telugu for the younger cohort.

14. This was consistent with the standard practice that private schools had a recommended set of books, uniforms, and so on, 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 these additional expenses. 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.

15. At the time of starting the project, the 2005 draft of the RtE Act was already in circulation, and private schools knew that the stipulation regarding reserving seats for economically disadvantaged children in private schools 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 provision of the RtE Act. The value of the voucher was set at the 90th percentile of the fee distribution to ensure that the reimbursement was above marginal cost for all schools (while still being considerably below the benchmark of per child spending in public schools).

the school's bank account (in three installments a year, which was the typical fee cycle of the schools). The only condition imposed on the schools was that they were not allowed to select voucher students. If there was greater demand for a school than the number of places 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 was similar to admission protocols of most charter school programs in the United States.[16]

Communication with schools, and elicitation of willingness to participate, was conducted before the village-level randomization took place. Once the applications were completed, 90 villages (stratified by district) were assigned by lottery to be voucher villages (Figure I, Panel A), while the other 90 villages continued "as usual" with no voucher program (Figure I, 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 created 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 I), and those who did not get the voucher due to losing the individual level lottery conducted within voucher villages (group 2T in Figure I).

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 I (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 nonapplicants across treatment and control villages, and then showing it between students who start out in private schools across these villages (for the representative sample of students in these groups who we track over time).

16. In practice, participating schools accepted all applicants who indicated a preference for the school, and the Foundation never needed to conduct any such school-level lotteries. Field interviews suggest that the private schools in this setting were not "selective" on any criteria other than ability to pay fees, and were happy to accept all voucher-receiving students, since the Foundation could be relied on to make full and timely fee payments.

TABLE I
VALIDITY OF DESIGN

	(1) Lottery winners (treatment villages)	(2) Lottery losers (treatment villages)	(3) *p*-value for difference	(4) Lottery winners (treatment villages)	(5) Lottery losers (control villages)	(6) *p*-value for difference
Panel A: Treatment and control students						
Normalized baseline Telugu score	0.03	0.05	.46	0.03	−0.04	.30
Normalized baseline math score	0.01	0.00	.75	0.01	−0.02	.74
Both parents have completed at least primary school	0.29	0.28	.44	0.31	0.28	.62
At least one parent has completed grade 10	0.34	0.34	.88	0.34	0.36	.16
Scheduled caste	0.34	0.33	.27	0.34	0.32	.45
Household asset index	3.17	3.15	.49	3.17	3.19	.71
Observations	1,980	1,119		1,980	3,334	

TABLE I

VALIDITY OF DESIGN

	(1) Nonapplicants in treatment villages	(2) Nonapplicants in control villages	(3) p-value for difference	(4) Students initially in private schools in treatment villages	(5) Students initially in private schools in control villages	(6) p-value for difference
Panel B: Students for spillover analysis						
Normalized baseline Telugu score	−0.02	0.04	.58	0.55	0.66	.16
Normalized baseline math score	−0.04	0.06	.41	0.72	0.59	.17
Both parents have completed at least primary school	0.28	0.27	.92	0.52	0.55	.45
At least one parent has completed grade 10	0.29	0.32	.39	0.50	0.54	.20
Scheduled caste	0.34	0.31	.43	0.12	0.13	.57
Household asset index	3.08	3.18	.28	3.84	3.85	.91
Sample observations	734	809		1,151	1,104	
Total observations	2,816	2,756		12,720	12,061	

Notes. All standard errors are clustered at the school level. Note that standard errors in the test score analysis are clustered (more conservatively) at the village level. Telugu and math scores are normalized relative to the distribution of public school students by subject and grade. The household asset index reported is a sum of five household indicators: whether a household owns its own home, has a nonmud house, has at least one covered room, has working water facilities, and has a toilet available. None of the baseline differences are statistically significant with clustering at either the school or village level.

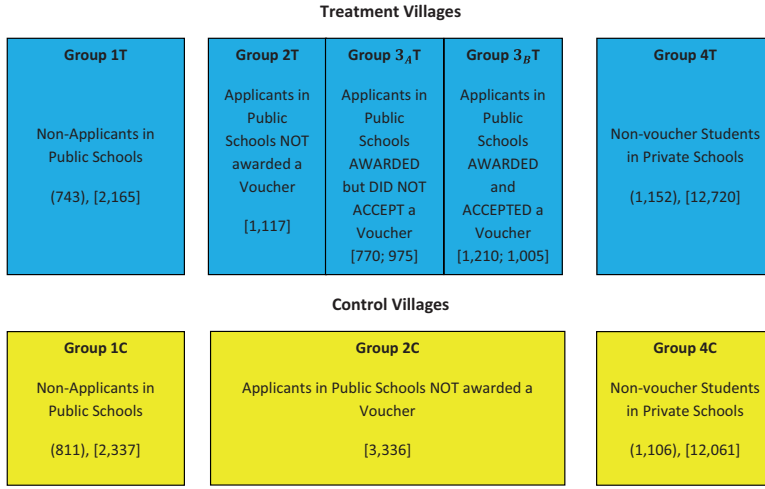

FIGURE II

Design of AP School Choice Program with Student Counts

All of groups 2T, 3T, 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

Out of 10,935 eligible households, a total of 6,433 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%). Of these 1,980 households, 1,210 accepted the voucher and enrolled in a private school at the start of the project (61%). Thus, a total of 23% of public school students in treatment villages accepted the voucher and moved to private schools, and around 8% of the students in private schools (in the two treated cohorts) were those who had transferred from the public school with the voucher. At the end of four years of the project, a total of 1,005 students continued to avail of the voucher. Figure II shows the program design with the actual number of students in each of the cells.

Table II presents summary statistics for the typical public and private school students (columns (1)–(3)); for applicants and nonapplicants from public schools (columns (4)–(6)), and for

TABLE II

BASELINE TEST SCORES AND SOCIOECONOMIC CHARACTERISTICS OF VARIOUS STUDENT SUBGROUPS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Private school students	Public school students	Difference	Voucher applicants	Voucher nonapplicans	Difference	Accepted conditional on winning voucher lottery	Did not accept conditional on winning voucher lottery	Difference
Normalized baseline Telugu score	0.604	0.000	0.604	−0.022	0.03	−0.052	−0.035	0.07	−0.105
	[0.810]	[1.000]	(0.039)***	[0.989]	[1.015]	(0.040)	[0.976]	[0.992]	(0.062)*
Normalized baseline math score	0.672	0.000	0.672	−0.034	0.044	−0.078	−0.117	0.116	−0.233
	[0.819]	[1.000]	(0.050)***	[1.001]	[0.997]	(0.047)*	[0.979]	[1.070]	(0.093)**
Both parents have completed at least primary school	0.535	0.284	0.251	0.284	0.283	0.001	0.275	0.315	−0.04
	[0.499]	[0.451]	(0.016)***	[.451]	[0.451]	(0.017)	[0.447]	[0.465]	(0.023)*
At least one parent has completed grade 10	0.656	0.379	0.277	0.394	0.35	0.044	0.366	0.403	−0.037
	[0.475]	[0.485]	(0.016)***	[.489]	[0.477]	(0.017)**	[0.482]	[0.491]	(0.026)
Scheduled caste	0.125	0.333	−0.208	0.33	0.328	0.002	0.309	0.391	−0.082
	[0.330]	[0.471]	(0.012)***	[.470]	[0.469]	(0.019)	[0.462]	[0.488]	(0.028)***
Household asset index	3.844	3.176	0.668	3.173	3.124	0.049	3.117	3.237	−0.12
	[0.978]	[1.023]	(0.039)***	[1.007]	[1.040]	(0.046)	[0.980]	[1.030]	(0.067)*
Observations	24,741	11,987		6,408	4,459		1,210	770	

Notes. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$. Standard deviations reported in brackets, standard errors in parentheses. All standard errors are clustered at the school level. The sample for columns (1)–(6) includes students from all villages at the baseline (2008) prior to the randomization. The sample for columns (7)–(9) is restricted to treatment villages at baseline. Telugu and math scores are normalized relative to the distribution of public school students by subject and grade. The household asset index reported is a sum of five household indicators: whether a household owns its own home, has a nonmud 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.

those who accepted the voucher conditional on winning it and those who did not (columns (7)–(9)). Overall, the students who applied for and accepted the voucher had lower baseline test scores, suggesting that students with lower test scores were more likely to leave the public schools if given the opportunity to do so. Students belonging to historically disadvantaged scheduled castes are equally likely to apply for the voucher but less likely to accept it if awarded. Nevertheless, the fraction of voucher accepting students who belonged to scheduled castes was considerably higher than the fraction of scheduled caste students in a typical private school (column (7) versus column (1)), suggesting that the provision of vouchers can significantly reduce socioeconomic stratification in private schools (which is one of the main concerns expressed regarding the growth of private schools; see Srivastava and Walford 2007, and Srivastava 2013 for illustrative discussions).

Table A.1 in the Online Appendix presents voucher application and acceptance rates by observable student characteristics.[17] The main observable that is correlated with application (and especially with acceptance) is distance to a private school. Students with a private school within half a kilometer were 6.5 percentage points more likely to apply for a voucher and 16.8 percentage points more likely to accept it if awarded. Students with an older sibling in the public school, in the older cohort, and without a parent who has completed 10th grade were slightly less likely to apply, but no less likely to accept if awarded. Scheduled caste students were no less likely to apply, but were less likely to accept if awarded. Of course, the differences reported in Tables II and A.1 do not affect our experimental estimates, but they help characterize the nature of selection on the nonexperimental margins of the study (application for and acceptance of the voucher).

II.D. Data and Attrition

We collect a rich set of survey data on school and teacher characteristics. Enumerators conducted unannounced visits to schools during the four years of the project and measured teacher absence and activity, classroom practices and processes, and school hygiene. They also conducted household surveys to

17. Note that Table II presents mean values of observable characteristics among students who applied/accepted, whereas Table A.1 presents mean application/acceptance rates for students with and without specific characteristics.

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 schools in the 180 project villages, whereas 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 II (in both treatment and control villages).

Data on learning outcomes were 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 EVS 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 set of students who had applied for the voucher (groups 2 and 3), and a representative sample of students who had either not applied or who were in the private schools at the start of the project (groups 1 and 4). The Hindi tests were more expensive to conduct (since they were done individually) and 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 variables in Table I in all cases where we survey/test a representative sample of students (tables available on request).

Field enumerators made extensive efforts to keep track of all students who were in the frame of the study at the beginning, but some attrition was unavoidable.[18] 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 (Online Appendix Table A.2, Panel A). These differences

18. Most of the attrition is due to students who had migrated and could not be found, as opposed to students still attending schools but not present for testing. The initial tests at the end of two years of the project were conducted in schools, but had high attrition rates (around 40%). This was followed by an intense effort by enumerators to track down all the students who had applied for the voucher and conduct an additional round of testing in each village outside school hours. 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 2.33 years into the program. A similar protocol was followed for testing after four years.

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 all observable characteristics reported in Table I (collected before the lottery) 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), the estimation sample is unlikely to be 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 reweighting as well as bounding (Lee 2009). The attrition rate in the sample that is used to test for spillovers is around 33% (Online Appendix Table A.2, Panel B), but the differences between treatment and control students are not significant.[19]

III. Results: School, Teacher, and Household Inputs

III.A. School and Teacher Inputs

Table III (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 usual differences and are not affected by the treatment).[20] On average, private schools in our sample are considerably larger than their public counterparts. They have a longer school year

19. There is a significant difference between treatment and control groups in this sample in 1 out of 24 comparisons, which is in line with expectations in a random sample (Table A.2, Panel B). We control for the variables compared in Table A.2 in all our estimates of program impact.

20. 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 mean private school characteristics relative to 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 mean 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 in response to the program (unlike what might be expected in a scaled-up steady-state implementation of the RtE Act or in a scaled-up voucher system like in Chile).

TABLE III
SCHOOL AND TEACHER CHARACTERISTICS

	(1) Private schools	(2) Public schools	(3) Difference
Panel A: School characteristics			
Total enrollment	296.21	74.04	222.17***
Total working days	229.81	218.66	11.15***
Pupil-teacher ratio	17.62	25.28	−7.67***
Drinking water available	0.99	0.92	0.07***
Functional toilets	0.86	0.68	0.18***
Separate functional toilets for girls	0.77	0.40	0.37***
Functional electricity	0.88	0.61	0.28***
Functional computers	0.52	0.05	0.48***
Functional library	0.80	0.97	−0.18***
Functional radio	0.13	0.81	−0.68***
Observations	289	346	
Panel B: Teacher characteristics			
Male	0.24	0.46	−0.21***
Age	27.58	40.00	−12.42***
Years of teaching	5.14	14.96	−9.82***
Completed at least college or masters	0.69	0.88	−0.19***
Teacher training completed	0.34	0.99	−0.65***
Come from the same village	0.44	0.13	0.32***
Current gross salary per month (Rs)	2,606.66	14,285.94	−11,679.27***
Observations	2,000	1,358	
Panel C: School expenditures			
Annual cost per child (Rs/child)	1,848.88	8,390.00	−6,542***
Observations	211	325	

Notes. *$p < .1$; **$p < .05$; ***$p < .01$. All standard errors are clustered at the school level. The sample for Panels A and B are restricted to schools and teachers in control villages across 2008 through 2012. In cases of multiple observations per school or teacher across different years, variable means are used (so each teacher/school is one observation). All expenditures are measured in rupees per student per year, trimmed at the 1st and 99th percentiles to remove outliers. The actual number of observations for each regression may vary slightly within columns based on the dependent variable. Estimates of annual cost per child in government schools come from government budget documents for 2010. The private school figures for Panel C are therefore limited to private schools in control villages in 2010.

(2 working weeks or 11 days longer per year) and have considerably lower pupil-teacher ratios (around a third lower) than do public schools. They are also more likely to have drinking water, functional toilets (as well as separate toilets for girls), functional electricity, and a computer, with the differences being quite stark for some of these measures. Public schools are

more likely to have a library (usually a collection of books and not a separate room) and radio.

Public 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 III, 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 civil service teachers obtain that are typically not available to private school teachers.

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

In addition to reporting on measures of school and teacher quality based on their characteristics, we also measure school quality using 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 public schools on all measures of observed classroom processes (Table IV, Panel A). Classrooms in private schools are significantly more likely to be engaged in active teaching (51% versus 34%), have a greater likelihood of a teacher being in the classroom (97% versus 92%), and are much less likely to be multigrade classrooms where more than one grade is taught simultaneously by the same teacher (24% versus 79%). Moreover, enumerators coded teachers in private schools as being more likely to be in complete control of the class (69% versus 41%) and as more effective in teaching and maintaining discipline (50% versus 36%).

21. Note that since salary expenditures are not reported at the school level, we compute average per child spending in public schools from analysis of budget documents at the state-level (Dongre 2012).

TABLE IV
TEACHER AND SCHOOL EFFORT

	(1) Private schools	(2) Public schools	(3) Difference
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***
Teacher is 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			
Teacher is absent	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. *$p < .1$; **$p < .05$; ***$p < .01$. All regressions include a constant and 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. All data are collected through unannounced surveys of schools administered during the project (2008 through 2012). The unit of observations are classrooms (Panel A), teachers (Panel B), and schools (Panel C). The actual number of observations for each regression may vary slightly within columns based on the dependent variable.

We find from observations at the teacher level (Table IV, Panel B) that public 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

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 public schools is to combine grades and have all students taught by the same teacher (as seen in the much higher rate of multigrade teaching in public schools).

when they entered the schools (Table IV, Panel C) 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 (a common carrier of pathogens from open human and animal waste).

III.B. Household Inputs

In addition to school-level factors, receipt of a voucher may also change household inputs into education (Das et al. 2013; Pop-Eleches and Urquiola 2013). 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 V present the cross-sectional comparison of average child time use and household education expenditure between children attending private and public 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 expenditure, whereas column (7) presents the estimate of average treatment-on-treated (ATT) effect on time use of attending a private school. Thus, comparing columns (7) and (3) provides a measure of the extent to which time use and household expenditure patterns of voucher receiving students who attended a private school had converged to the typical patterns of private school students.

The typical private school student spends 43 minutes more a day in school, and an additional 23 minutes a day on studying and doing homework at home (Table V, Panel A, columns (1)–(3)), which adds up to over an hour of extra school and study time per day and over 250 hours a 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. Also, the typical private school student spends 20 minutes less each day playing with friends, while there is no reduction in time spent playing with friends for the voucher winners. These results suggest that

TABLE V

HOUSEHOLD INPUTS

	(1) Private schools	(2) Public schools	(3) Difference	(4) Applicants offered voucher	(5) Applicants in control villages	(6) Intention to treat estimate	(7) Treatment on the treated estimate
Panel A: Student time use diaries (minutes per day)							
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	−12.84***	−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	1,839		885	1,212		2,097

TABLE V
(CONTINUED)

	(1) Private schools	(2) Public schools	(3) Difference	(4) Applicants offered voucher	(5) Applicants in control villages	(6) Intention to treat estimate	(7) Treatment on the treated estimate
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	216.27***	171.14	237.07	−65.94***	−121.77***
Notebooks/textbooks	554.46	228.57	325.89***	209.05	278.35	−69.29***	−127.40***
Special events	15.91	7.29	8.62***	5.30	9.04	−3.74*	−6.93*
Transportation	113.61	13.59	100.02***	46.55	43.57	2.99	5.51
Private tuition	71.07	32.51	38.56***	34.80	39.55	−4.75	−8.75
Total expenditure	2,910.36	566.73	2,343.64***	774.94	892.69	−117.75	−215.95
Observations	634	1,815		858	1182		2,040

Notes. *$p < .1$; **$p < .05$; ***$p < .01$. All regressions include a constant and district fixed effects. All standard errors are clustered at the village level. In all columns in Panels A, the sample is based on student time-use diaries from a normal, nonsick school day. The sample for columns (1)–(3) is restricted to students and schools in control villages. The samples for columns (4)–(7) is applicants offered vouchers in treatment villages and applicants not offered vouchers in control villages. Data for both panels come from the household 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 the categories listed and all other school-related expenditures. The actual number of observations for each regression may vary slightly within columns based on the dependent variable.

study and play habits of voucher winners at home did not change over this period.[23]

Households with children attending private schools spend over five times as much money on that child's education (Table V, Panel B, columns (1)–(3)), which reflects private school fees and additional required expenditures on textbooks and uniforms. Public schools are free, provide free textbooks, and uniforms are optional. However, voucher-winning households spend a little less on the education (of the winning child) relative to 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. Thus, any impact on test scores for voucher winners is likely to be due to changes in school-level factors as opposed to increases in household inputs.

IV. Results: Test Scores

IV.A. Impact of Winning a Voucher and Attending a Private School

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

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

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.[24]

23. Overall, around two thirds of the cost of the extra time spent in school (45 minutes/day) seems to have been borne by parents (30 minutes of reduced time on chores and work outside the home), and the remaining one third was borne by the student (15 minutes less of watching TV and free time).

24. 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 and increases precision).

We also include a set of district fixed effects (Z_i) to absorb geographic variation and increase efficiency and 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 ITT estimate) since the voucher was assigned by lottery. We estimate β_2 both with and without controlling for household socioeconomic characteristics (X_i) shown in Table I.

As described in Section II, 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 public 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.

While we focus our discussion on the ITT estimate, we also present the ATT impact of attending a private school by scaling up the ITT estimate above by the inverse of the voucher take up rate (defined as the fraction of voucher recipients who accepted the voucher and stayed in a private school for two and four years, respectively).[25]

25. IV estimates using the voucher (awarded by lottery) as an instrument for attending a private school yield similar estimates of the causal impact of attending a private school. However, there is one further challenge in interpreting this estimate, which is the differential attrition rate in our sample between compliers and noncompliers (among voucher recipients). This does not affect the ITT estimates but necessitates a further set of assumptions to yield unbiased IV estimates. We therefore prefer to present ATT estimates by scaling up the ITT estimates by the inverse of the take-up rate, and focus most of our discussion on the ITT estimates. Note that there were no lottery losers who went to private school on their own and stayed there throughout the study, and the take-up rate of the voucher is therefore equivalent to the differential take-up rate across treatment and control groups. Finally, note that we do present an instrumental variable estimate in Table V (column (7)) on time use because those data are collected continuously every year and the private school attending status of voucher-winning students changes over this period (from 61% at the start to 51% at the end of the project). Thus, the scale-up

These results are presented in Table VI for test scores at the end of two and four years of the program, with Panel A showing the impact of being awarded a voucher and Panel B showing the average causal impact of attending a private school for those who accepted the voucher and enrolled in a private school for two and four years. Results in Table VI includes the controls shown in Table I (for greater precision), but are unchanged without the controls.

At the end of two and four years, we find that voucher lottery winners had slightly lower scores on Telugu and math than lottery losers (not significant; columns (1), (2), (5), and (6)), and higher scores in English (0.19σ after two years, $p = .02$, and 0.12σ after four years, $p = .098$; columns (3) and (7)). The average impact across the three subjects that were assessed at the end of two years was close to zero (Table VI, column (4)). These results suggest that the large cross-sectional differences in math and Telugu test scores (of 0.65σ) shown in Table II are mostly driven by omitted variables and not by differential effectiveness of public and private schools.

However, a key determinant of education outcomes by subject is the allocation of instructional time across different subjects.[26] We present data from school timetables in Table VII, and see that private schools have sharply different patterns of time allocation than public schools. In particular, they allocate a lot less time per week to Telugu and math, which are the two main subjects taught in the public schools, accounting for over 500 minutes/week, and around 28% of total instructional time each. Private schools spend around 200 minutes less on Telugu and 160 minutes less on math a 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

factor between columns (6) and (7) in Table V is slightly lower than that between Panel A and Panel B in Table VI.

26. 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 timetable data.

TABLE VI

TEST SCORE IMPACTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Year 2 assessments				Year 4 assessments						
	Telugu score	Math score	English score	Combined across tests	Telugu score	Math score	English score	EVS score	Combined across tests excluding Hindi	Hindi score	Combined across tests
Panel A: Impact of winning a voucher (intention to treat effects)											
Offered voucher	−0.079	−0.053	0.185**	0.016	−0.017	−0.031	0.116*	0.083	0.036	0.545***	0.133***
	(0.055)	(0.065)	(0.079)	(0.061)	(0.051)	(0.052)	(0.070)	(0.060)	(0.048)	(0.068)	(0.045)
Total observations	4,620	4,620	4,525	13,765	4,385	4,385	4,217	4,243	17,230	1,696	18,926
Treatment observations	1,778	1,778	1,738	5,294	1,674	1,675	1,607	1,628	6,584	867	7,451
Control observations	2,842	2,842	2,787	8,471	2,711	2,710	2,610	2,615	10,646	829	11,475
Panel B: Average treatment on the treated (ATT) effect of attending a private school (scaling up intention to treat effect by inverse of voucher take-up rate)											
Voucher recipient in private school	−0.156	−0.104	0.364**	0.032	−0.033	−0.061	0.229*	0.164	0.071	1.074***	0.262***
	(0.108)	(0.128)	(0.156)	(0.120)	(0.100)	(0.102)	(0.138)	(0.118)	(0.095)	(0.134)	(0.089)
Total observations	4,620	4,620	4,525	13,765	4,385	4,385	4,217	4,243	17,230	1,696	18,926
Voucher recipients	997	997	982	5,294	945	946	911	920	6,584	510	7,451
Nonrecipients	3,623	3,623	3,543	8,471	3,440	3,439	3,306	3,323	10,646	1,186	11,475

Notes. *$p < .1$; **$p < .05$; ***$p < .01$. 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 relative to the distribution of public school students in control villages by subject and grade. Telugu, math, English, and EVS (science and social studies) test scores are from written end-of-year tests; Hindi test scores are from an individual assessment administered to a representative sample of students. 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 sampling probability of a student being selected to take the test from the universe of students. Controls include indicators for both parents having completed at least primary school, at least one parent having completed grade 10, and the household being a scheduled caste, as well as the household asset index defined in Table I. Students with a missing value on any control have all controls coded as 0, and an additional dummy is included in all regressions indicating these students to ensure that no observations are lost due to missing data on any individual control. Results are unchanged without controlling for these socioeconomic variables. The only result whose significance changes without socioeconomic controls (that improve precision) is the four-year impact on English that moves from a p-value of .098 (with controls) to a p-value of .113 (without controls). Panel B reports coefficients and standard errors from Panel A scaled up by the inverse of the year 2 and year 4 voucher take-up rate of 1,005/1,980 (51%).

TABLE VII

SCHOOL TIME USE: INSTRUCTIONAL TIME BY SUBJECT (MINUTES PER WEEK)

	(1) Private schools	(2) Public schools	(3) Difference
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)
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	2,005.87	1,796.47	209.4***
	(13.73)	(6.86)	(14.46)
Break	461	473.18	−12.18
	(9.14)	(3.05)	(10.58)
Total school time	2,466.87	2,269.65	197.22***
	(17.46)	(8.25)	(19.79)
Observations	325	200	

Notes. *$p < .1$; **$p < .05$; ***$p < .01$. All regressions include a constant and district fixed effects. All standard errors are clustered at the village level. The sample for this table is restricted to schools in control villages. All numbers are in minutes per week. "Other" includes sports/PE, arts and crafts, and study hall.

the program account for 70% of the instruction time in public schools, but for less than 50% of that in the private schools.

Thus, limiting our analysis to these subjects may provide an incomplete picture of the impact of the voucher. Based on the time table data, we also conducted tests in EVS and Hindi after four years of the voucher program.[27] Although this still does not

27. Science and social studies are taught jointly under the subject title of environmental studies (EVS). Our EVS tests followed the public school curriculum and were administered in a standard written format. Hindi is not taught in the public

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 Table VI, columns (5)–(8) and (10). Voucher-winning students score slightly better in EVS (0.08σ; $p = .16$), and much better in Hindi (0.55σ, $p < .001$).

Since the test score gains are mainly found in Hindi, and since public 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 use Hindi. We present these results in Online Appendix Table A.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.

If we weight all subjects equally (as in Kling, Liebman, and Katz 2007) and exclude Hindi (which is not taught in public schools), the mean test score impact of winning a voucher is not significantly different from zero (Table VI, column (9)). If we include Hindi, we find that students who won a voucher scored 0.13σ higher, and the average student who accepted the voucher to attend a private school scored 0.26σ higher (column (11)). Whereas views on optimal weights across subjects (and on whether Hindi should be treated equally with other subjects given that it is not taught in public schools) may vary, we can still unambiguously infer that private schools are more productive, because they deliver similar outcomes in Telugu and math with less instructional time and use the extra time to improve test scores in Hindi.

schools, 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 nonprofit Pratham in their annual surveys of learning levels implemented across India (ASER 2013) and is therefore comparable with a benchmark measure of competence that has been widely used in India in the recent past.

IV.B. Robustness to Attrition

The main threat to the results is from the differential attrition noted in Online Appendix Table A.2. We verify that our results are robust to this concern using two different procedures. First, 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 VI, Panel A (Online Appendix Table A.4, Panel A). Second, we present conservative confidence intervals using the bounding procedure suggested by Lee (2009), and find that the overall results are unchanged (Online Appendix Table A.4, Panel B).

IV.C. 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 the private schools, and who are exposed to lower-scoring peers from public schools as a result of the voucher program (Hsieh and Urquiola 2006). Our two-stage design allows us to estimate these spillovers. We calculate three different sets of spillovers as described in Section II.B, and the estimating equations all take the same form as equation (1), but the right-hand-side variable of interest is now an indicator for being in a voucher village. The estimation samples comprise the concerned group for whom we want to estimate the spillovers (lottery losers, nonapplicants, 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 VIII, 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 II.B. We find no difference between the groups, and the combined effects across subjects are not only insignificant but close to

TABLE VIII

SPILLOVERS (ITT ESTIMATES)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Year 2 assessments				Year 4 assessments				
	Telugu score	Math score	English score	Combined across tests	Telugu score	Math score	English score	EVS score	Combined across tests
Panel A: Comparing the within-village to across-village controls									
Lottery loser in treatment village	0.010	0.011	0.041	0.020	0.011	−0.002	−0.048	0.093	0.013
	(0.060)	(0.071)	(0.090)	(0.069)	(0.058)	(0.057)	(0.067)	(0.071)	(0.055)
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 nonapplicants from public schools									
Treatment village	−0.022	0.056	0.129	0.054	0.064	0.012	0.039	−0.004	0.028
	(0.071)	(0.066)	(0.089)	(0.067)	(0.061)	(0.067)	(0.069)	(0.071)	(0.055)
Total observations	1,030	1,030	1,008	3,068	1,173	1,174	1,145	1,149	4,641
Treatment observations	490	490	476	1,456	555	555	541	542	2,193
Control observations	540	540	532	1,612	618	619	604	607	2,448
Panel C: Impact on nonvoucher students from private schools									
Treatment village	0.067	0.028	−0.112	−0.000	0.043	0.038	−0.019	0.029	0.024
	(0.060)	(0.073)	(0.073)	(0.060)	(0.061)	(0.059)	(0.098)	(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. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$. 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. Test scores are as defined in Table VI.

zero.[28] Panel B estimates if there were any spillovers on nonapplicants 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 even though a large fraction (23%) of public school students moved out to private schools.[29]

In the Indian context, a greater concern has been the possibility that the RtE Act clause on quotas in private schools would lead to negative spillovers on the students who start out in the private schools (see Shah 2012 for an example). We estimate these spillovers in Table VIII, Panel C and find that there are no significant negative spillovers on the students who were in private schools to begin with. In addition to these average spillover effects on private school students in the exposed cohorts, we also estimate the extent of spillovers as a function of the number of voucher-winning students who join a particular private school.

Since this is endogenous, we first construct a measure of potential exposure to voucher students for each private school (in both treatment and control villages) as the number of voucher applicants for whom it is the nearest private school. We then construct an instrumental variable for the number of voucher-winning students who join any given private school by interacting the "potential exposure" with the (randomly determined) fraction of these students who win a voucher. The instrument will take a value of 0 for all private schools in control villages (where no vouchers were awarded) and can vary across private schools in treatment villages. We present the IV estimates of spillovers on private school students as a function of the number of voucher students received by the school in Online Appendix Table A.5, and again find no impact on the test scores of students who

28. Our not finding any significant spillovers here suggests that the potential contamination of the "typical" control group (as discussed in Section II.B) in existing voucher studies is likely to be an empirically second-order issue.

29. Of course, the estimated noneffect is a reduced-form estimate that combines factors that could potentially hurt the students left behind (loss of motivated peers) as well as those that could help them (smaller class sizes if teacher allocations did not fully adjust to the departure of the voucher students and potential positive teacher effort response to 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.

started out in private schools.[30] Although set in India, these results are consistent with those reported in Angrist and Lang (2004), who similarly find negligible impacts on white students from the school desegregation conducted under the Boston Metco Program.

Taken together, our results suggest that the (small) test score gains for voucher winners are not achieved at the cost of negative outcomes for any other group of students who may have been indirectly affected by the voucher program. Of course, our results do not imply that peer effects and sorting never matter for evaluations of school choice. But they do suggest more broadly that although spillovers are an important theoretical concern in the school choice literature, they do not appear to be empirically first-order in our context and may not be so for lottery-based studies of school choice in programs that do not allow private schools to select students.[31]

IV.D. Heterogeneous Effects

1. Heterogeneous Effects by Student Characteristics. We test for heterogeneity of the impact of the voucher program along several student characteristics, including baseline scores, sex,

30. The instrument is relevant because distance is a strong predictor of primary school choice (first-stage *F*-statistic is over 45 in Online Appendix Table A.5). We present the first-stage regression in Table A.6 and see that the instrument strongly predicts the number of voucher students attending any given private school. Furthermore, the village-level randomization (after inviting applications) allows us to define the "potential exposure" variable for every private school in both treatment and control villages. Thus, an alternative approach to using a linear IV for estimating spillovers (as shown in Table A.5) is to restrict our spillover analysis in Table VIII, Panel C to private schools (in treatment and control villages) with higher potential exposure to voucher students. We reestimate the spillover effects reported in Table VIII, Panel C in samples restricted to the top 50% of private schools (by potential exposure to voucher students) and also the top 25%, and still find no evidence of spillover effects (results available on request).

31. 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.

parental literacy, and affluence, using a standard linear interaction specification of the form:

$$\begin{aligned} T_{isv}(Y_n) = {} & \beta_0 + \beta_1 \cdot T_{isv}(Y_0) + \beta_2 \cdot Voucher_i \\ & + \beta_3 \cdot Characteristic_i + \beta_4 \cdot Voucher_i \cdot Characteristic_i \\ & + \beta_{Z_i} \cdot Z_i + \varepsilon_{isv}, \end{aligned} \tag{2}$$

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 IX (Panel A) reports estimates of β_4 over two and four years. 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.[32]

We also estimate a similar specification to test for heterogeneity among the public school students who did not apply for the voucher (group 1 in Figure I), and find no evidence of differential spillover effects on non-applicants (Table IX, Panel B). Overall, we find limited evidence of student-level heterogeneity for either the main effects or spillovers (this is also true when we test for heterogeneity nonparametrically as a function of baseline test scores).

2. Heterogeneous Effects by School Characteristics. Our experiment was not designed to identify heterogeneous effects by school characteristics,[33] but we report some suggestive results that are likely to be important for future research designed explicitly to study such heterogeneity. In particular, a key feature of

32. We do find that Muslim students, who are one of the most educationally disadvantaged groups in India (Sachar et al. 2006), benefited significantly more from receiving a voucher. These results are consistent with those found in the US by Howell and Peterson (2002) who report that educationally-disadvantaged groups gain the most from school choice programs. See the working paper version for details (Muralidharan and Sundararaman 2014).

33. Note that this is true of the experimental school choice literature in general, because even when a voucher is randomly assigned, the school attended is typically not. Indeed, the logic of school choice is based to a considerable extent on enabling better student-school matching on unobserved characteristics.

TABLE IX

HETEROGENEOUS TEST SCORE IMPACTS BY STUDENT CHARACTERISTICS (ITT ESTIMATES)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Year 2 assessments				Year 4 assessments						
	Telugu score	Math score	English score	Combined across tests	Telugu score	Math score	English score	EVS score	Combined across tests excluding Hindi	Hindi score	Combined across tests
Panel A: Test score impacts on voucher recipients											
Offered voucher * covariate											
Baseline test score	−0.056	−0.054	−0.074	−0.062	0.004	0.000	−0.022	−0.022	−0.010	−0.107*	−0.031
	(0.042)	(0.043)	(0.052)	(0.041)	(0.037)	(0.037)	(0.047)	(0.043)	(0.033)	(0.061)	(0.033)
Female student	0.013	0.069	0.117	0.065	0.010	−0.037	0.013	0.017	0.001	0.173*	0.035
	(0.055)	(0.054)	(0.076)	(0.054)	(0.064)	(0.064)	(0.071)	(0.071)	(0.056)	(0.101)	(0.053)
Parents literate indicator	0.043	−0.022	−0.011	0.003	−0.031	−0.006	0.132	−0.138*	−0.012	−0.240**	−0.059
	(0.065)	(0.060)	(0.089)	(0.062)	(0.068)	(0.070)	(0.120)	(0.077)	(0.066)	(0.121)	(0.064)
Household asset index	0.014	0.031	−0.018	0.010	−0.028	−0.001	0.009	−0.019	−0.010	0.018	−0.002
	(0.031)	(0.033)	(0.045)	(0.032)	(0.033)	(0.031)	(0.038)	(0.035)	(0.028)	(0.063)	(0.030)
Observations	4,620	4,620	4,525	13,765	4,385	4,385	4,217	4,243	17,230	1,696	18,926

TABLE IX
(CONTINUED)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Year 2 assessments				Year 4 assessments						
	Telugu score	Math score	English score	Combined across tests	Telugu score	Math score	English score	EVS score	Combined across tests excluding Hindi	Hindi score	Combined across tests
Panel B: Test score impacts on nonapplicants (in treatment versus control villages)											
Treatment village * covariate											
Baseline test score	−0.042	−0.074	−0.036	−0.050	−0.019	−0.012	−0.022	−0.007	−0.014	−0.080	−0.028
	(0.055)	(0.056)	(0.069)	(0.055)	(0.049)	(0.048)	(0.074)	(0.049)	(0.047)	(0.081)	(0.045)
Female student	0.026	−0.020	0.007	0.004	−0.064	−0.050	0.015	−0.010	−0.027	0.270*	0.019
	(0.074)	(0.080)	(0.095)	(0.073)	(0.074)	(0.070)	(0.116)	(0.081)	(0.070)	(0.157)	(0.065)
Parents literate indicator	−0.057	−0.134	−0.048	−0.078	0.004	−0.043	−0.083	0.041	−0.021	−0.178	−0.056
	(0.088)	(0.099)	(0.126)	(0.094)	(0.094)	(0.089)	(0.146)	(0.105)	(0.093)	(0.172)	(0.091)
Household asset index	0.001	−0.049	−0.020	−0.021	−0.054	−0.023	−0.115*	−0.022	−0.053	−0.096	−0.065*
	(0.039)	(0.044)	(0.054)	(0.041)	(0.040)	(0.042)	(0.065)	(0.046)	(0.039)	(0.065)	(0.038)
Observations	2,416	2,297	2,354	7,186	2,695	2,695	2,608	2,617	10,615	788	11,403

Notes. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$. Each cell reports the coefficient from a separate regression testing for heterogeneity in impact of the voucher program along several characteristics. In Panel A, we test for heterogeneous impacts on voucher recipients (regressions include controls for the characteristic in question, an indicator for whether a student was offered a voucher, and the student's normalized baseline test scores). In Panel B, we test for heterogeneous spillover effects on nonapplicants left behind in the public schools (regressions include controls for the characteristic in question, an indicator for whether the student is in a treatment village, and the student's normalized baseline test scores). All regressions include a constant and district fixed effects and standard errors are clustered at the village level. All test scores are as defined in Table VI, and indicators for assets and literacy are as defined in Table I. The actual number of observations for each regression may vary slightly within columns based on the dependent variable.

private school heterogeneity in India is the medium of instruction. All public schools in our sample teach in Telugu, whereas over half the private schools use English as the medium of instruction. The high actual and perceived returns to English in India have led to growing demand for English-medium private schools.[34] At the same time, it is possible that switching to being taught in English may be disruptive to the learning of voucher-winning students (many of whom are first-generation learners with illiterate parents). Thus, studying heterogeneous effects of attending private schools as a function of the medium of instruction is especially important in this context.

Since the choice of school attended (and its medium of instruction) is endogenous, we use the medium of instruction of the nearest private school to each applicant household, and its interaction with the receipt of the randomly assigned voucher as instruments for the medium of instruction of the private school attended. We define the following variables of interest:

$A\_EM\_PS_i$ = student i attends an English medium private school
$A\_TM\_PS_i$ = student i attends a Telugu medium private school
$N\_PS\_EM_i$ = nearest Private school to student i teaches in English medium
$N\_PS\_TM_i$ = nearest Private school to student i teaches in Telugu medium

and are interested in estimating β_2 and β_3 in the second-stage equation:

$$T_{isv}(Y_n) = \beta_0 + \beta_1 \cdot T_{isv}(Y_0) + \beta_2 \cdot A\_EM\_PS_i + \beta_3 \cdot A\_TM\_PS_i + \beta_{X_i} \cdot X_i + \varepsilon_{isv}, \quad (3)$$

34. Munshi and Rosenzweig (2006), Azam, Chin, and Prakash (2011), Chakraborty and Kapur (2012), and Shastry (2012) all find significant positive labor market returns in India to knowledge of English. Several journalistic accounts and qualitative studies have noted the high perceived returns to English among parents and the growing demand for English medium schools in India (see Bajaj and Yardley 2011, and Meganathan 2011 for examples).

where the endogenous variables are $A\_EM\_PS_i$ and $A\_TM\_PS_i$, and the first-stage equations are:

$$\begin{aligned} A\_EM\_PS_i (or\ A\_TM\_PS_i) = {} & \gamma_0 + \gamma_1 \cdot T_{isv}(Y_0) \\ & + \gamma_2 \cdot N\_PS\_EM_i + \gamma_3 \cdot N\_PS\_TM_i \\ & + \gamma_4 \cdot Voucher_i \cdot N\_PS\_EM_i \\ & + \gamma_5 \cdot Voucher_i \cdot N\_PS\_TM_i \\ & + \gamma_{X_i} \cdot X_i + \varepsilon_{isv} \end{aligned} \qquad (4a, 4b)$$

We use equations (4a) and (4b) to instrument for the two endogenous variables in equation (3), and present the two first-stage regressions in Online Appendix Table A.7. The main parameters of interest (β_2 and β_3) from the IV estimation of equation (3) are presented in Table X, Panel A. A more conservative approach is to use only the interactions as instruments, and we present the results from this specification in Table X, Panel B.[35] The estimation sample is the same as that in Table VI, and comprises the voucher lottery winners in the treatment villages, and the lottery losers in the control villages.[36]

At the end of four years of the voucher program, we find that the causal impact of attending an English-medium private school varies sharply by subject, with students doing worse (than staying in the public school) in Telugu, math, and EVS but much better in English and Hindi. The mean impact across subjects is positive (0.22σ) but not significant. On the other hand, the estimated impact of attending a Telugu-medium private school is positive for every subject, and the mean impact across subjects is positive (0.53σ) and significant (Table X, Panel A). The IV

35. Since the location decisions of English and Telugu medium schools may vary, the most conservative IV strategy is to use only the interactions as instruments (because these are comparing similarly located schools across treatment and control villages, which are chosen randomly). However, as we see in Online Appendix Table A.8, there is no difference in mean baseline scores of voucher applicants as a function of whether their nearest private school is in English or Telugu medium. So our default specification uses all four instruments (for greater precision) and includes all variables in Table A.8 as controls (Panel A), but we also report results with the more conservative IV strategy (Panel B).

36. However, we have around 7% fewer observations in Table X than in Table VI due to missing household GPS data. The results in Table VI are unchanged when estimated in this truncated sample (available on request).

TABLE X

HETEROGENEOUS TEST SCORE IMPACTS (BY MEDIUM OF INSTRUCTION, IV ESTIMATES)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Year 2 assessments				Year 4 assessments						
	Telugu score	Math score	English score	Combined across tests	Telugu score	Math score	English score	EVS score	Hindi score	Combined across tests	Combined across nonlanguage subjects (math & science/social studies)
Panel A: Treatment on the treated effects by medium of instruction (all four instruments)											
Students attending private English-medium schools	−0.078	−0.010	0.683***	0.198	−0.272	−0.349	0.677**	−0.197	1.379***	0.223	−0.274
	(0.164)	(0.175)	(0.232)	(0.174)	(0.244)	(0.248)	(0.339)	(0.298)	(0.250)	(0.204)	(0.255)
Students attending private Telugu-medium schools	−0.033	0.062	0.408	0.143	0.259	0.255	0.043	0.746**	1.384***	0.532***	0.496*
	(0.168)	(0.197)	(0.285)	(0.202)	(0.245)	(0.254)	(0.171)	(0.313)	(0.230)	(0.181)	(0.275)
Total observations	4,291	4,291	4,209	12,791	4,070	4,070	3,922	3,935	1,576	17,573	8,005
First-stage *F*-stat on first regressor	33.2	33.2	32.9	33.2	12.1	12.2	12.4	12.1	11.2	12.6	12.2
First-stage *F*-stat on second regressor	38.3	38.3	39.4	38.7	21.5	21.5	22.1	20.5	20.6	22.2	21.1
p-value of equality by medium	.80	.72	.33	.78	.15	.13	.09	.06	.99	.29	.07

TABLE X

(CONTINUED)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Year 2 assessments				Year 4 assessments						
	Telugu score	Math score	English score	Combined across tests	Telugu score	Math score	English score	EVS score	Hindi score	Combined across tests	Combined across nonlanguage subjects (math & science/social studies)
Panel B: Treatment on the treated effects by medium of instruction (only interactions as instruments)											
Students attending private English-medium schools	−0.242	−0.173	0.595**	0.060	−0.523	−0.591	0.978**	−0.104	1.426***	0.213	−0.352
	(0.231)	(0.239)	(0.289)	(0.232)	(0.385)	(0.381)	(0.460)	(0.384)	(0.365)	(0.285)	(0.360)
Students attending private Telugu-medium schools	0.201	0.294	0.535	0.339	0.513	0.501	−0.274	0.651	1.337***	0.542*	0.575
	(0.252)	(0.260)	(0.417)	(0.291)	(0.450)	(0.458)	(0.376)	(0.446)	(0.328)	(0.320)	(0.442)
Total observations	4,291	4,291	4,209	12,791	4,070	4,070	3,922	3,935	1,576	17,573	8,005
First-stage *F*-stat on first regressor	38.4	38.4	38.2	18.3	17.7	17.8	18.1	17.6	16.2	38.4	17.8
First-stage *F*-stat on second regressor	30.6	30.6	31.1	25.1	25.4	25.4	25.2	25.1	20.2	30.8	25.4
p-value of equality by medium	.3	.2	.9	.5	.2	.2	.1	.3	.9	.5	.2

Notes. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$. In Panel A, we instrument for the medium of instruction of private school attended using the medium of instruction of the nearest private school and the interaction of receiving a voucher and the medium of instruction of the nearest private school. First stage regressions are shown in Online Appendix Table A.7. In Panel B, we instrument for the medium of instruction of private school attended using only the interaction of receiving a voucher and the medium of instruction of the nearest private school, and we control for the the closest private school being English medium and for the closest private school being Telugu medium. In both panels, we also control for mother's and father's education, scheduled caste status, and a household asset index as defined in Table I. Students with a missing value on any control have all controls coded as 0, and an additional dummy is included in all regressions indicating these students to ensure that no observations are lost due to missing data on any individual control. All regressions control for baseline normalized test scores. The results in this table do not include district fixed effects, because one of the districts has very few English-medium schools. All standard errors are clustered at the village level. The sample is around 7% smaller than that in Table VI (due to missing location data). All test scores are defined as in Table VI.

estimates in Table X have large standard errors and are much less precise than the main estimates in Table VI. Nevertheless, some suggestive patterns emerge in the results.

The first is that the causal impact of attending a Telugu-medium private school (for students who start out in public schools, which all teach in Telugu medium) on test scores in Telugu, math, and EVS appears to be greater than that of attending an English-medium private school (p-values of .15, .13, and .06, respectively). The second is that there appears to be a negative impact of switching the medium of instruction on the learning of content in nonlanguage subjects. We see this most clearly in the last column of Table X, Panel A, where we present the mean treatment effects across math and EVS (which are the two content subjects, while the other three are language subjects). The difference in mean test scores across medium of instruction is 0.77σ, which is a very large effect ($p = .07$). The third is that private schools appear to be even more effective than suggested by the estimates in Table VI (a mean treatment effect of 0.53σ across subjects as opposed to 0.26σ), when their students are not also going through the disruption of switching their medium of instruction.[37]

These results are only suggestive and have several caveats. First, they are highly imprecise. Using a more conservative IV strategy (reported in Table X, Panel B), we find similar estimates, but the standard errors are too large for meaningful inference. Second, even with a precise IV estimate, the medium of instruction will be correlated with other school characteristics. However, we see that on average, the English-medium schools have superior indicators of school quality – including facilities; teacher experience, qualifications, and salary; and annual fees charged per child (Online Appendix Table A.9). Since our main result in

37. Recall that the point estimates are relative to attending public schools, and the effects on Telugu and math reflect both the potentially higher private school productivity effect and the lower instructional time allocated to these subjects in the private schools. The results on EVS, on the other hand, reflect both the productivity effect and additional instructional time, and it is therefore not surprising that the estimated effects on EVS are much larger (when the medium of instruction is not disrupted). However, our focus in this table is on the relative impact of Telugu and English medium private schools (and less on the subject-level point estimates relative to public schools).

Table X is that Telugu-medium private schools appear to be more effective than English-medium ones, the superior input-based quality indicators of the English-medium schools suggest that the differences shown in Table X may be a lower bound on the relative advantage of native-language versus English-medium instruction (for the population of public school students who applied for the voucher).

These results are consistent with the education psychology literature, which suggests that first-generation learners may be better off being taught in their native language, which can be reinforced at home (see Abadzi 2006 for a summary). Qualitative interviews by enumerators with teachers and parents suggest that one plausible reason for adverse effects of shifting to an English-medium private school is that these schools use textbooks written in English for teaching nonlanguage subjects such as math and EVS, and that reading textbooks in English was much more difficult for students shifting from public schools.

In the development economics literature, Ramachandran (2012) provides recent evidence from Ethiopia showing that a switch to mother-tongue instruction for primary school led to a significant increase in education attainment. Jain (2014) examines historical data from colonial India and finds that linguistically mismatched districts (where the official language did not match the local language) had lower rates of literacy and college graduation results. Our results are consistent with these findings in aggregate data and highlight the importance of more well-identified research to directly estimate the impact of the medium of instruction on test scores.

3. Heterogeneous Effects by Market Characteristics. The market-level experimental design allows us to test whether students who have greater choice among schools benefit more from a voucher (Hoxby 2000). We use the distance data described above to calculate the number of private schools within a 1 km radius of each voucher applicant. Our measure of choice and competition is constructed separately for each student and can therefore generate variation at the student level even for students living in the same village. We estimate the relationship between the number of schools in an applicant's choice set and test scores, both parametrically and nonparametrically. For the first, we use a linear interaction of voucher receipt and the number of schools in the

choice set in a specification similar to equation (2). For the second, we estimate equation (2) 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 number of schools.

These results are presented in Table XI, and we find no significant effect of choice and competition when estimated with a linear interaction between voucher receipt and the number of schools in a student's choice set within 1 km. However, while conducting the study in a rural sample allows us to study spillovers by randomizing across villages, a limitation is that around 50% of voucher applicants have only 0 (27%) or 1 (21%) private school within a 1 km radius. Thus, the extent of choice and competition between private schools is quite limited for many of the voucher applicants.

The nonparametric 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 they have six or more schools within a 1 km radius of where they live (Table XI). 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. Furthermore, we find similar results when we consider heterogeneous impacts as a function of the number of private schools in a half-kilometer radius and with the total number of schools in a half or 1 km radius and not just the number of private schools (available on request).

However, these results are suggestive, because they are only significant in markets representing the top 5% of the distribution of the extent of choice and competition, and the rural setting may not be the best one to study heterogeneous impacts of school choice as a function of choice and competition. However, urban India has much greater population and school density. A recent geocoded school census in the city of Patna found between 9 and 93 private schools within a 1 km radius of every public school, with the median being greater than 50 (Rangaraju, Tooley, and Dixon 2012). Our results therefore suggest that the impact of a school voucher program may be considerably larger in high-density urban settings. This is an important area for future research.

TABLE XI

HETEROGENEOUS TEST SCORE IMPACTS BY MARKET COMPETITION (ITT ESTIMATES)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Year 2 assessments				Year 4 assessments						
	Telugu score	Math score	English score	Combined across tests	Telugu score	Math score	English score	EVS score	Combined across tests excluding Hindi	Hindi score	Combined across tests
Number of private schools within 1 km (linear)	0.024	0.015	0.019	0.020	0.027	−0.001	−0.021	−0.001	0.001	−0.007	−0.003
	(0.022)	(0.028)	(0.034)	(0.025)	(0.024)	(0.027)	(0.032)	(0.030)	(0.023)	(0.031)	(0.021)
3 or more schools within 1 km (top 25%)	0.042	−0.065	−0.127	−0.048	0.046	−0.051	−0.067	−0.084	−0.038	−0.108	−0.065
	(0.110)	(0.126)	(0.150)	(0.118)	(0.102)	(0.104)	(0.158)	(0.115)	(0.094)	(0.139)	(0.085)
5 or more schools within 1 km (top 10%)	0.192	0.251*	0.270	0.238*	0.230*	0.125	−0.151	0.178	0.099	−0.004	0.061
	(0.117)	(0.137)	(0.199)	(0.134)	(0.129)	(0.151)	(0.177)	(0.160)	(0.135)	(0.166)	(0.123)
6 or more schools within 1 km (top 5%)	0.183	0.295	0.452	0.309	0.480***	0.435***	0.208**	0.470***	0.401***	−0.099	0.290**
	(0.136)	(0.198)	(0.314)	(0.205)	(0.125)	(0.138)	(0.099)	(0.142)	(0.101)	(0.228)	(0.116)
Observations	4,612	4,612	4,518	13,742	4,378	4,378	4,215	4,237	17,208	1,689	18,897

Notes. *$p < .1$; **$p < .05$; ***$p < .01$. Each cell reports the coefficient from a separate regression that also includes controls for whether a student was offered a voucher and the student's normalized baseline test scores. We also control for mother's and father's education, scheduled caste status, and a household asset index as defined in Table I. Students with a missing value on any control have all controls coded as 0, and an additional dummy is included in all regressions indicating these students to ensure that no observations are lost due to missing data on any individual control. All regressions include a constant district fixed effects and standard errors are clustered at the village level. All test scores are defined as in Table VI.

IV.E. Cost-Effectiveness

The combination of test score results (Table VI) and school timetable data (Table VII) 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. Furthermore, the results in Table X 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 a third of the per student costs in the public schools (Table III, Panel C) and that the value of the voucher was only around 40% of the per student costs in the public schools. Thus, private schools produce (slightly) better academic outcomes at lower cost and are unambiguously both more productive and cost-effective than public schools in India.

V. 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. Furthermore, the two-stage experimental design also allows us to estimate spillovers on nonapplicants and students in private schools.

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 costs of low productivity in public education delivery may be especially high in low-income settings where low levels of human capital are likely to be barriers to both economic growth and 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 public 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 integration are concerned about.

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 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 (or elite public) 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. In the absence of data on long-term outcomes such as employment and wages, it is important for education researchers 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, given the provision in the RtE Act for 25% reservation in private schools for disadvantaged students (with the government reimbursing private school fees). 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 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.[38]

Nevertheless, there are important caveats to the broad implication that greater private sector participation in education production (supported by public funding and featuring enhanced school choice) would improve the productivity of human capital formation. The first caveat is that the private schools in our sample did not on average improve outcomes in math and Telugu (though they spent less time and money, and were more productive as a result). In particular, private schools did not do better on math and Telugu in spite of having a longer school year

38. Note that reimbursements to private schools are capped at the per child spending in public schools. The increased social integration across economic classes may have broader social benefits as well. For instance, Rao (2014) finds evidence in Delhi that exposure to economically disadvantaged students increased prosocial preferences and behavior among privileged children attending private schools.

and school day, substantially lower pupil-teacher ratios, and higher levels of teacher attendance and effort. We point to four possible explanations for this result.

First, the total instructional time on math and Telugu was lower in private schools.[39] Second, private schools spend much less per student and hire teachers with lower levels of education, training, and experience, paying them much lower salaries. Thus, higher teacher effort in private schools may be offset by lower teacher knowledge and experience. Third, we find suggestive evidence that private schools are more effective if students are not also experiencing a disruption in the medium of instruction (Table X) and thus the lack of an overall impact may reflect the negative impacts of switching to English-medium schools for those who did so. Fourth, there is suggestive evidence that a first-order binding constraint in Indian schools (both public and private) is that the pedagogy mechanically follows the textbook as opposed to the level of the students, who are typically way behind grade-level competencies (Banerjee and Duflo 2011; Pritchett and Beatty 2012; Muralidharan 2013). In such a setting, the additional effort of private school teachers in transacting the curriculum may not translate into much additional learning. To the extent that this last explanation is true, the productivity advantage of private schools may mainly reflect their ability to pay lower teacher salaries as opposed to superior effectiveness of instruction.

Thus, although it is plausible that increasing per child spending in private schools to the same level as public schools will lead to an increase in learning outcomes (perhaps by enabling the hiring of better teachers), our results do not imply that increasing the time or money spent on instruction in these subjects in private schools will lead to improved learning outcomes. 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 schools may respond by improving school characteristics that are more visible to parents and

39. This is true in spite of the longer school year and lower teacher absence. Private schools have 5% more working days in a year (Table III) and 20% higher teacher attendance (Table IV). However, with 32% and 40% less time spent per week on math and Telugu, the total instruction time in these subjects is still only 85% of that of public schools for math and 75% for Telugu.

improve their marketing prospects rather than investing in 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. In particular, students may be better off if private schools used their higher productivity to spend more time on the native language to help students reach grade-appropriate reading levels in at least one language, as opposed to low levels of knowledge in three languages.[40] Interviews with head teachers suggest that an important reason for this is that the low-cost private schools in our setting typically copy the curriculum of elite private schools, which feature the three-language formula (comprising of the state language, English, and Hindi) that is typical of the education that elites in India receive. Given the socially aspirational nature of private school attendance (see Bajaj and Yardley 2011), the management of private schools we interviewed stated that it would be difficult for them to remain competitive if they did not follow the standards of elite private schools (even if this curriculum was not optimal for the typical student attending a low-cost private school).[41]

This discussion points to the second caveat, 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 one that believes that parents (especially poor and uneducated ones) may make misguided evaluations of school quality based on visible factors

40. The literature on early childhood development in low-income countries suggests that returns to language competence are convex for the first language, because of the importance of being able to "read to learn" in at least one language (Abadzi 2006). The majority of public school students in AP are far behind grade-level competences in Telugu and math (Muralidharan and Zieleniak 2014) and cannot typically read fluently even in Telugu, which is their native language (ASER 2013). Since the mean impact on Telugu scores of going to private schools was 0, it is likely that the absolute level of competence in any language is low for voucher winners.

41. See DiMaggio and Powell (1983) for the classic reference on "isomorphism" between organizations where it is common to copy organizational form without delivering on the corresponding function. Of course, it is also likely that knowledge of an additional language like Hindi (the most widely spoken Indian language, and the fourth most spoken language in the world with over 500 million speakers) would have positive labor market returns (especially with growing migration of workers across Indian states). But it is not clear that these returns are higher than those from increasing competence in the native language to enable better learning of core content subjects. This is an important area for future research.

that may not contribute to more effective learning (see Bau 2014 for an illustration). While we find that private schools are much more productive than public schools from the perspective of a social planner, it is not obvious that they represent a better value for the marginal parent who is paying for private schools over a free public school. Since test scores did not improve in math and Telugu, the marginal parent would have to place a high value on Hindi scores to justify paying for the typical private school in our sample. Although we cannot rule out this possibility (or that parents valued other nonacademic aspects of private schools), it is also possible that parents were not able to easily determine the effectiveness of schools at improving learning outcomes, and it may be important to provide better and easily understandable information on determinants of education quality to schools and parents (Hastings and Weinstein 2008; Andrabi, Das, and Khwaja 2012).

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 United States operate under (they cannot be selective in whom they accept), but it is important to apply it to the way that the RtE Act will be implemented.

Our results and discussion point toward several avenues for future research. The first is to better estimate an education production function relationship between instructional time per subject and test scores, and on the role of the language of instruction. Second, the analysis in this article (and in most of the school choice literature) has focused exclusively on the impacts on test scores and learning outcomes and has ignored welfare gains to households from enhanced choice and match quality. A natural extension for future work therefore is to use our market-level experiment 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 through vouchers (Bresnahan and Gordon 1996; Carneiro, Das, and Reis 2013).

Three further sets of research questions are of the 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 to measure the extent to which the greater productivity of private schools can translate into better absolute learning outcomes. Second, although we find suggestive evidence on the positive effects of greater choice and competition, more conclusive evidence will require running similar experiments in urban India—where the greater population density allows for much more choice and competition between schools. Third, our estimates are based on a voucher experiment with two cohorts of students and do not capture the long-term dynamic impacts of a school choice system. Doing so would require modeling (and measuring) school entry and exit, as well as the endogenous price and quality responses of private schools in response to a steady stream of public funding for students to attend private schools in (Nielson 2013 conducts such an exercise in Chile). Indian states are currently starting to implement the RtE Act, 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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SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at QJE online (qje.oxfordjournal.org).

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